

Manfred Bietak, Ernst Czerny (Eds.)

The Synchronisation of Civilisations in the Eastern Mediterranean in the Second Millennium B.C. III

Proceedings of the SCIEM 2000 – 2nd EuroConference, Vienna
28th of May – 2nd of June 2001

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MANFRED BIETAK, ERNST CZERNY (EDITORS)

THE SYNCHRONISATION OF CIVILISATIONS IN THE EASTERN MEDITERRANEAN IN THE SECOND MILLENNIUM B.C. III

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and Hermann Hunger

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MANFRED BIETAK and ERNST CZERNY

Editorial Committee: Irene Kaplan and Angela Schwab

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Contents

Abbreviations 9

Manfred Bietak, Ernst Czerny, Preface by the Editors 11

Introduction: High and Low Chronology

Manfred Bietak and Felix Höflmayer 13

Science and Chronology

Malcolm H. Wiener
Times Change: The Current State of the Debate in Old World Chronology 25

Max Bichler, Barbara Duma, Heinz Huber, and Andreas Musilek
Distinction of Pre-Minoan Pumice from Santorini, Greece 49

Max Bichler, Heinz Huber, and Peter Warren
Project Thera Ashes – Pumice Sample from Knossos 59

Hendrik J. Bruins
Charcoal Radiocarbon Dates of Tell el-Dab^{c}a 65

Hendrik J. Bruins, Amihai Mazar, and Johannes van der Plicht
The End of the 2nd Millennium BCE and the Transition from Iron I to Iron IIA: Radiocarbon Dates of Tel Rehov, Israel 79

Sturt W. Manning
Clarifying the 'High' v. 'Low' Aegean/Cypriot Chronology for the Mid Second Millennium BC: Assessing the Evidence, Interpretive Frameworks, and Current State of the Debate. 101

Nicolas J.G. Pearce, John A. Westgate, Sheri J. Preece, Warren J. Eastwood, William T. Perkins, and Joanna S. Hart
Reinterpretation of Greenland Ice-core Data Recognises the Presence of the Late Holocene Aniakchak Tephra (Alaska), not the Minoan Tephra (Santorini), at 1645 BC. 139

Ilan Sharon, Ayelet Gilboa, and Elisabetta Boaretto
^{14}C and the Early Iron Age of Israel – Where are we really at? A Commentary on the Tel Rehov Radiometric Dates 149

Uroš Anderlič and Maria G. Firneis
First Lunar Crescents for Babylon in the 2nd Millennium B.C. 157

Chronological and Archaeological Statements: Egypt

Kenneth A. Kitchen
Egyptian and Related Chronologies – Look, no Sciences, no Pots! 163

Rolf Krauss
An Egyptian Chronology for Dynasties XIII to XXV. 173

Katherina Aslanidou
Some Ornamental Scenes on the Wall Paintings from Tell el Dab^{c}a: Iconography and Context 191

David A. Aston
Kom Rabi^{c}a, Ezbet Helmi, and Saqqara NK 3507. A Study in Cross-Dating. 207

Bettina Bader
A Tale of Two Cities: First Results of a Comparison Between Avaris and Memphis 249

Manfred Bietak
Bronze Age Paintings in the Levant: Chronological and Cultural Considerations 269

Perla Fuscaldo
Tell el-Dabᶜa: Some Remarks on the Pottery from ᶜEzbet Helmi (Areas H/III and H/VI, Strata e/1 and d) . . . 301

Helen Jacquet-Gordon
A Habitation Site at Karnak North Prior to the New Kingdom . . . 317

Teodozja Rzeuska
Some Remarks on the Egyptian *kernoi* . . . 325

Chronological and Archaeological Statements: The Levant and Syria

Sandra Antonetti
Intra moenia Middle Bronze Age Burials at Tell es-Sultan: A Chronological Perspective . . . 337

Michal Artzy
Tell Abu Hawam: News from the Late Bronze Age . . . 357

Frans van Koppen
Syrian Trade Routes of the Mari Age and MB II Hazor . . . 367

Mario A.S. Martin
A Collection of Egyptian and Egyptian-style Pottery at Beth Shean . . . 375

Mirko Novák
Mittani Empire and the Question of Absolute Chronology: Some Archaeological Considerations . . . 389

Luca Peyronel
Late Old Syrian Fortifications and Middle Syrian Re-Occupation on the Western Rampart at Tell Mardikh-Ebla . . . 403

Uwe Sievertsen
New Research on Middle Bronze Age Chronology of Western Syria . . . 423

Jean-Paul Thalmann
A Seldom Used Parameter in Pottery Studies: the Capacity of Pottery Vessels . . . 431

Chronological and Archaeological Statements: The Aegean, Cyprus and Adjacent Areas

Lindy Crewe
The Foundation of Enkomi: A New Analysis of the Stratigraphic Sequence and Regional Ceramic Connections . . . 439

Walter Gauss and Rudolfine Smetana
Early and Middle Bronze Age Stratigraphy and Pottery from Aegina Kolonna . . . 451

Peter Pavúk
New Perspectives on Troia VI Chronology . . . 473

Jacke Philipps
The Amenhotep III 'Plaques' from Mycenae: Comparison, Contrast and a Question of Chronology . . . 479

Peter M. Warren
A New Pumice Analysis from Knossos and the End of Late Minoan I A . . . 495

Section: Mycenaeans and Philistines in the Levant

Sigrid Deger-Jalkotzy
Section "Mycenaeans and Philistines in the Levant": Introduction . . . 501

Paul Åström
Sinda and the Absolute Chronology of Late Cypriote IIIA . . . 505

Tristan J. Barako
Coexistence and Impermeability: Egyptians and Philistines in Southern Canaan During the Twelfth Century BCE 509

Israel Finkelstein
Is the Philistine Paradigm Still Viable? 517

Elisabeth French
The Impact on Correlations to the Levant of the Recent Stratigraphic Evidence from the Argolid . . . 525

Marta Guzowska and Assaf Yasur-Landau
The Mycenaean Pottery from Tel Aphek: Chronology and Patterns of Trade 537

Sophocles Hadjisavvas
The Public Face of the Absolute Chronology for Cypriot Prehistory 547

Reinhard Jung
Tell Kazel and the Mycenaean Contacts with Amurru (Syria) 551

Amihai Mazar
Myc IIIC in the Land Israel: Its Distribution, Date and Significance 571

Penelope A. Mountjoy
The Dating of the Early LC IIIA Phase at Enkomi 583

Constance von Rüden
Exchange Between Cyprus and Crete in the 'Dark Ages'? 595

David Ussishkin
Lachish and the Date of the Philistine Settlement in Canaan 601

Assaf Yasur-Landau
Let's Do the Time Warp again: Migration Processes and the Absolute Chronology of the Philistine Settlement 609

Sharon Zuckerman
Dating the Destruction of Canaanite Hazor *without* Mycenaean Pottery? 621

Abbreviations

Ä&L *Ägypten und Levante*
ÄA *Ägyptologische Abhandlungen*, Wiesbaden
AAA *Archaiologika analekta ex Athenon*, Athènes
AAAS *Les Annales Archéologiques Arabes Syriennes,*
AASOR *Annual of the American Schools of Oriental Research,* Cambridge Mass.
ÄAT *Ägypten und Altes Testament.* Studien zu Geschichte, Kultur und Religion Ägyptens und des Alten
ABSA *The Annual of the British School at Athens*, London
ActaArch *Acta Archaeologica,* Kopenhagen
ADAJ *Annual of the Department of Antiquities of Jordan*, Amman
Aegaeum *Aegaeum.* Annales d'archéologie égéenne de l'Université de Liège, Liège
ÄF *Ägyptologische Forschungen*, Glückstadt
AfO *Archiv für Orientforschung*, Wien
AHL *Anistoriton History Library.* Electronic Journal of History, Archaeology and Art History, http://www.anistor.co.hol.gr/index.htm
AHw W. von Soden, *Akkadisches Handwörterbuch*, Wiesbaden
AION *Annali dell'Istituto universitario orientale di Napoli*, Neapel
AJA *American Journal of Archaeology*, New York, Baltimore, Norwood
Akkadica *Akkadica.* Périodique bimestriel de la Fondation assyriologique Georges Dossin, Bruxelles
American Anthropologist *American anthropologist: Journal of the American Anthropological Association*, Arlington, Va.
AnatSt *Anatolian Studies.* Journal of the British Institute of Archaeology at Ankara, London
Antiquity *Antiquity: a quarterly review of archaeology*, Oxford
AOAT *Alter Orient und Altes Testament. Veröffentlichungen zur Kultur und Geschichte des Alten Orients und des Alten Testaments,* K. Bergerhof, M. Dietrich et O. Loretz (eds.), Münster.
ArchEph Αρχαιολογική Εφημερίς, Athen
ARE *Ancient Records of Egypt*, *Volumes I–IV* (Reissue), translated by J.H. Breasted, New York, 1962. Original publication: Chicago, 1906.
ARM *Archives Royales de Mari,*
AS AnSt *Anatolian Studies. Journal of the British Institute of Archaeology at Ankara*, London
ASAE *Annales du Service des Antiquités de l'Égypte,* Kairo
ASAE *Annales du service des antiquites de l'Égypte*, Caire
ASAtene *Annuario della Scuola Archeologica di Athene e delle Missioni Italiane in Oriente,* Rome
ASE *The Archaeological Survey of Egypt,* London
ASE Archaeological Survey in Egypt, London.
ASN *Archaeological Survey of Nubia*, Kairo
ASOR *American Schools of Oriental Research*
cAtiqot *cAtiqot. Journal of the Israel Department of Antiquities*, Jerusalem
AV *Archäologische Veröffentlichungen.* Deutsches Archäologisches Institut, Abteilung Kairo, Berlin/Mainz am Rhein
BA *The Biblical Archaeologist.* American Schools of Oriental Research, Michigan, New Haven
BAR *Biblical Archaeological Review*
BAR Inter.Ser. *British Archaeological Reports, International Series*, London
BASOR *Bulletin of the American Schools of Oriental Research*, New Haven
BCH *Bulletin de correspondance hellénique*, Athens
BdE *Bibliothèque d'Étude,* Institut Français d'Archéologie Orientale, Kairo
BdL Bulletin de Liason, Le Caire
Berytus *Berytus.* Archaeological Studies, Musée d'archéologie et université américaine de Beyrouth, Beirut
BES *Bulletin of the Egyptological Seminar.* Columbia University, Brooklyn, New York
BICS *Bulletin of the Institute of Classical Studies,* London
BIE *Bulletin of the Israel Exploration Society,* Jerusalem
BollMonMusPont *Monumenti Musei e Gallerie Pontificie, Bollettino*, Città del Vaticano
BSA British School of Archaeology at Athens
BSAE *British School of Archaeology in Egypt*, London
CAJ *Cambridge Archaeological Journal*, Cambridge
CCE *Cahier de la Céramique Égyptienne*, Kairo
CChEM Contributions to the Chronology of the Eastern Mediterranean, Vienna
CMS †Matz, F., Pini, I., and Müller W. (eds.) 1964–. Corpus der Minoischen und Mykenischen Siegel (22 vols +). Berlin; 2002–. Mainz am Rhein
CNIP *The Carsten Niebuhr Institute of Ancient Near Eastern Studies*, University of Copenhagen, Museum Tusculanum Press, Copenhagen
CRIPEL *Cahiers de recherches de l'institut de Papyrologie et d'Egyptologie de Lille; Société Urbaines en Égypte et au Soudan,* Lille
CurrAnthr *Current Anthropology,* Chicago
DE *Discussions in Egyptology*, Oxford
Demography *Demography :a publ. of the Population Association of America*, Washington D.C.
E&L see *Ä&L*
EEF *Egypt Exploration Fund,* London
EES Excav.Mem. *Egypt Exploration Society Excavation Memoir*, London
EI *Eretz Israel,* Jerusalem
Eos *Eos.* Commentarii Societatis philologiae polonorum, Wroclaw
ERA Egyptian Research Account, London.
FIFAO *Fouilles de l'institut français d'archéologie orientale du Caire*, Le Caire
GM *Göttinger Miszellen*, Göttingen
HÄB *Hildesheimer Ägyptologische Beiträge*, Hildesheim
Hesperia *Hesperia.* Journal of the American School of Classical Studies at Athens, Princeton

HortScience *HortScience. A publ. by the American Society of Horticultural Science*, St. Joseph, Mich. [u.a.]
IAA Reports *Israel Antiquites Authority Reports*, Jerusalem
IEJ *Israel Exploration Journal*, Jerusalem
Iraq *Iraq.* British School of Archaeology in Iraq, London
JACF *The Journal of the Ancient Chronology Forum*
JAOS *Journal of the American Oriental Society*, New Haven, Conn.
JAS *Journal of Archaeological Science*, London, New York
JCS *Journal of Cuneiform Studies*, New Haven - Baltimore
JdI/ JdAI *Jahrbuch des Deutschen Archäologischen Instituts, Berlin*
JEA *Journal of Egyptian Archaeology*, London
JHA *Journal for the History of Astronomy*, Cambridge
JMA *Journal of Mediterranean Archaeology*, London
JMS *Journal of Mediterranean Studies.* History, Culture and Society in the Mediterranean World, Msida
JNES *Journal of Near Eastern Studies*, Chicago
JSOT *Journal for the Study of the Old Testament*, Sheffield
KMT *KMT.* A *modern journal of ancient Egypt*, San Francisco
Ktema *Ktema: civilisations de l'orient, de la Grèce et de Rome antiques*, Strasbourg
Kush *Kush.* Journal of the Sudan Antiquities Service, Khartum
KVHAA *Kungliga Vitterhets Historie and Antikvitets Akademien Konferense*, Stockholm
LÄ *Lexikon der Ägyptologie*, ed. by W. Helck, W. Westendorf, 7 vols. Wiesbaden 1972 ff.
LAAA *Liverpool Annals of Archaeology and Anthropology*, Liverpool
LD Erg. K.R. Lepsius, *Denkmaeler aus Aegypten und Aethiopien*, Ergänzungsband ed. by E. Naville, Leipzig 1913.
LD K.R. Lepsius, *Denkmaeler aus Aegypten und Aethiopien*, 12 vols., Berlin 1849-58, reprinted Osnabrück 1970; Text, 5 vols., ed. by E. Naville, Leipzig 1897-1913.
Levant *Levant.* Journal of the British School of Archaeology in Jerusalem and the British Institute at Amman for Archaeology and History, London
MDAIK *Mitteilungen des Deutschen Archäologischen Instituts,* Abteilung Kairo, Berlin, ab 1970: Mainz
MDOG *Mitteilungen der Deutschen Orientgesellschaft*, Berlin
MMJ *Metropolitan Museum Journal, New* York..
MSAE *Materiali e studi archeologici di Ebla*, Rom
OBO SA *Orbis Biblicus et Orientalis, Series Archaeologica*, Freiburg (Swizerland)
OBO *Orbis Biblicus et Orientalis*, Fribourg - Göttingen
OIP *Oriental Institute Publications*, University of Chicago, Chicago
OJA *Oxford Journal of Archaeology*, Oxford
ÖJh *Jahreshefte des Österreichischen Archäologischen Institutes in Wien*, Wien
OLA *Orientalia Lovaniensia Analecta*, Leuven
OpAth *Opuscula atheniensia.* Annual ofthe Swedish Institute at Athens. Acta Instituti Atheniensis Regni Sueciae, Lund
Or *Orientalia, Nova Series*, Rome
Oriens Antiquus *Oriens Antiquus.* Rivista del Centro per le antichità e la storia dell'arte del Vicino Oriente, Rome
PAE *Praktika tes en Athenais Archaiologikes Etaireias*, Athens
PEQ *Palestine Exploration Quarterly*, London
PoM I–IV A.J. Evans, *The Palace of Minos*, London 1921–1935
PSBA *Proceedings of the Society of Biblical Archaeology*, London
PZ *Prähistorische Zeitschrift*, Berlin
QGer *Quaderni di Gerico*, Roma
RDAC *Report of the Department of Antiquities of Cyprus*, Nicosia
RdE *Revue d'Égyptologie*, Paris
RecTrav *Recueil de Travaux rélatifs à la philologie et à l'archéologie égyptiennes et assyriennes*, Paris
RlA *Reallexikon der Assyriologie*, München - Berlin.
SAGA *Studien zur Archäologie und Geschichte Altägyptens*, Heidelberg
SAOC *Studies in Ancient Oriental Civilization*, Chicago
SCCNH *Studies in the Culture and Civilization of Nuzi and the Hurrians*, Bethesda
SDAIK *Sonderschriften des Deutschen Archäologischen Instituts*, Abteilung Kairo, Mainz
SIMA *Studies in Mediterranean Archaeology.* A Handbook of Archaeology, Göteborg, Jonsered, Sävedalen
SIMA PB *Studies in Mediterranean Archaeology, Pocket-book*
SJE T. Säve-Söderberg (ed.), *The Scandinavian Joint Expedition to Sudanese Nubia Publications*, Lund
Syria *Syria.* Revue d'art oriental et d'archéologie, Paris
TA *Tel Aviv.* Journal of the Tel Aviv University Institute of Archaeology, Tel Aviv
Thera I–VII S. Marinatos, *Thera I–VII*, 1967–1973, Athens 1968–1976
TUAT *Texte aus der Umwelt des Alten Testaments*, Gütersloh
TÜBA-AR *Turkish Academy of Sciences Journal of Archaeology*, Ankara
UAVA *Untersuchungen zur Assyriologie und Vorderasiatischen Archäologie, Ergänzungsbände zur ZA*, München - Berlin
UF *Ugarit-Forschungen.* Internationales Jahrbuch für die Altertumskunde Syrien-Palästinas, Neunkirchen - Vluyn
UZK *Untersuchungen der Zweigstelle Kairo des Österreichischen Archäologischen Institutes*, Wien
WA *World Archaeology*, London
WVDOG *Wissenschaftliche Veröffentlichungen der Deutschen Orientgesellschaft*, Berlin
WZKM *Wiener Zeitschrift für die Kunde des Morgenlandes*
ZA *Zeitschrift für Assyriologie*, München - Berlin.
ZÄS *Zeitschrift für ägyptische Sprache und Altertumskunde*, Leipzig, Berlin
ZDMG *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, Leipzig
ZDPV *Zeitschrift des Deutschen Palästina-Vereins*, Stuttgart - Wiesbaden

Preface by the Editors

The phenomenon of time is looked upon within this conference from very different angles and backgrounds as well as from different regions of research. The picture that each of us has formed is therefore different and it is only natural that groups with similar backgrounds share similar ideas. Putting our knowledge and experiences together we may achieve a major breakthrough. This cannot be done within one, two or three conferences and the research work in between. But, step by step, progress is achieved in detailed work and even if our results are not the same as the outcomes of groups working with other methods, we should not be vexed but should see differences as a phenomenon that we can learn to understand. We believe that the international co-operation in methods and evaluation of chronologies was never before so intensive and great as it is at present and, even if major differences still exist they are milestones on the rocky path to a solution. The SCIEM 2000-team would like to thank wholeheartedly all of our colleagues who have participated and contributed to this conference.

This volume contains no less than 45 articles, all based on lectures given during the 2nd SCIEM 2000 Euro-conference. As in the two previously published "The Synchronisation of Civilisations" volumes, the articles are arranged in two main groups, viz "Science and Chronology" and "Chronological and archaeological statements", the latter group being divided according to different regions of the Eastern Mediterranean. The last chapter, "Section: Mycenaeans and Philistines in the Levant", includes the proceedings of a special meeting on that topic within the frame of the SCIEM conference, headed by Sigrid Jalkotzy-Deger.

Atop of all these manifold papers, an introductory statement by M. Bietak and F. Höfelmayer pinpoints the crucial question concerning the ongoing research in Eastern Mediterranean chronology. Their statement suggests possible ways for future research to reconcile the different chronological schemes obtained by the means of historical chronology and scientific research, which, at first glance seem to lead to aporia.

The editors hope that the following articles, which cover a wide range of different aspects of chronological and archaeological research both in detailed studies and in more general surveys, may find a broad acceptance among many readers and scholars.

Manfred Bietak as a First Speaker of SCIEM 2000 would like to thank the European Commission, the Austrian Research Fund and the Austrian Academy of Sciences who have financed this conference and the printing of this volume. The Austrian Academy also hosted the conference at its premises in Vienna. The City of Vienna and the Austrian Academy gave receptions for the participants of this conference and thus set an agreeable and stimulating atmosphere for the meetings. We would also like to especially thank Dagmar Melman and Angela Schwab who organised the conference with élan and initiative. Angela Schwab also produced the layout. For counselling and overseeing of the manuscript we would like to thank Hannes Weinberger of the Austrian Academy.

Manfred Bietak
Ernst Czerny

Introduction: High and Low Chronology

Manfred Bietak and Felix Höflmayer

Since the founding of the international research programme SCIEM 2000, which led to this conference, we have been working toward establishing a general framework of interregional chronology of the second millennium BC. In the course of the discussions and the previously and recently published exchanges, it became clear that this aim could be achieved in reasonable time only within the relative chronology, which means that the periodisation of the different regions in the Eastern Mediterranean could be shown through their relationships to each other. In order to apply such a scheme with absolute dates, we still have the problem of two chronologies: the historical chronology, based mainly on Egyptian and Assyrian chronologies and their interrelationship and radiocarbon chronology.[1] Despite all attempts to discuss these differences away or at least to minimize them, one has to realise that there are periods with a considerable difference between radiocarbon- and historical chronology which cannot be denied nor be reconciled at the moment.[2] However, other sciences being involved, we hope for decisive results.

It can already be considered progress – last but not least within this congress – that in the dating of the Thera eruption, the package of ^{14}C, the Greenland ice-core- and/or dendrochronology – which looked in combination very impressive for some time and had stimulated alliances – has been dissolved for various reasons. Raising the radiocarbon dates for the Thera eruption from the late towards the middle of the 17th century and thus breaking it away from the 1628/27 dendro-signal in the northern hemisphere,[3] brought them very near to the date of tiny volcanic glass particles found in a Greenland ice layer (GRIP core), identified by SIMS as originating from the Minoan eruption and dated according to the count of the yearly ice deposition to ±1645 BC.[4] A rapprochement was said to have been a coincidence after the identification of the particles with the Thera eruption could not be proven sufficiently.[5] Nevertheless, after the ice particles were abandoned as an anchor and external proof, the radiocarbon determination for the Thera eruption came down again to c. 1620 BC.[6] A flirt with the 1628/27 BC dendrosignal is not repeated for the time being and this way is good. The high chronology rests now on the strength and weakness of the radiocarbon dating alone.

The alternative to Radiocarbon dating, the Egyptian chronology, is based on a combination of astrochronology (Sothis- and lunar dates), incomplete or corrupted king lists, incomplete regnal data, genealogies of officials and time estimates based on them and even such records as the stelae of the holy Apis bulls, recording their lifespan and the kings under which they were born or have passed away.[7] Such dead reckoning from undisputed dates of the first millennium backwards, such as the conquest of Egypt by Cambyses at 525 BC, is today the preferred method of arriving at a historical chronology of the New Kingdom. Historical Egyptian chronology also relies on the interrelationship with other chronologies such as the Assyrian one,[8] which offers with its eponyms' lists a framework which is considered with-

---

1 See Bietak 2003: 23–34.

2 Manning 1999: *passim*; Manning *et al*. 2002: 733–744; Manning & Bronk Ramsey 2003: 111–133; Bronk Ramsey *et al*. 2004: 325–344; last Manning *et al*. 2006: 565–569.

3 LaMarche & Hirschboek 1984: 121–126; Baillie & Munro 1988: 344–346; Manning *et al*. 2001: 2532–2535.

4 Hammer 2000: 35–37; Hammer *et al*. 2003: 87–94.

5 Pearce *et al*. 2004; Pearce *et al*. in this volume showed that the trace elements of the particles of the GRIP core would fit even better to the Aniakchak volcano in Alaska. See also Keenan 2003: 1097, who refutes the identification of the particles in question on statistical grounds. Max Bichler from the Atomic Institute of the Austrian Universities and SCIEM 2000 (personal communication) made it clear that the particles are too small to allow at present a reliable identification with a specific volcano.

6 Manning & Sewell 2002: 264–291; Manning *et al*. 2006: 565–569.

7 Hornung 1964; Bierbrier 1975; Kitchen 1986; 1987; 1996; von Beckerath 1994; 1997. For a recent reappraisal see Müller 2006; Krauss & Hornung 2006.

8 Brinkman 1972: 271–281; 1976: 6–7; De Martino 2004: 38–39.

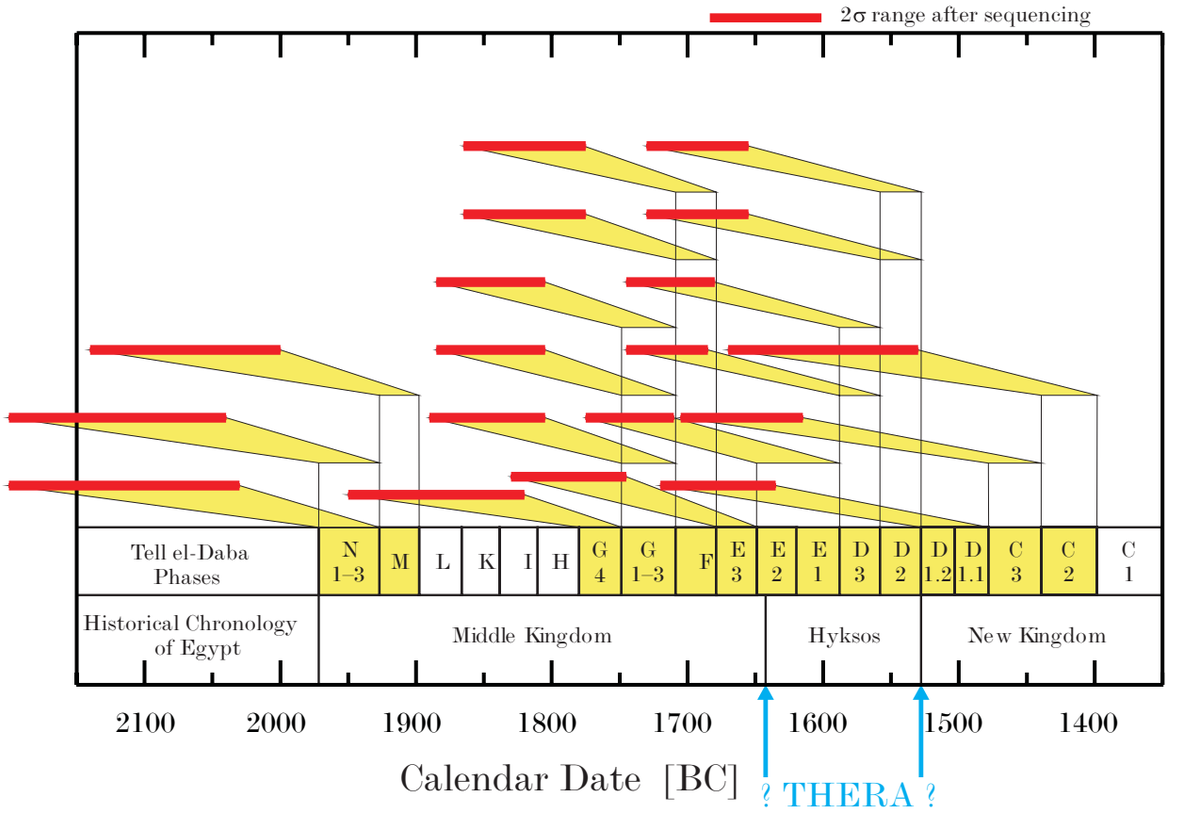


Fig. 1 Preliminary results of radiocarbon dates taken from the stratigraphy of Tell el-Dab^{c}a and their offset towards historical chronology (courtesy of Walter Kutschera)

in minor margins of error fairly accurate. The chronological experts in Egyptology agree on a date for the beginning of the New Kingdom from c. 1550 to 1540 BC. There is also an understanding that the margin of error may be within 20 years both ways, but this would put pressure on the genealogies and on some specific lengths of reign (eg. Tuthmosis II, Tuthmosis IV, Horemheb).

The two systems – the radiocarbon method and the historical chronology – have periods of agreement such as the 14th and 13th centuries BC. It is, however, wrong to claim the time from the 18th century BC backwards as a period of agreement again, thus limiting the disagreement to two to three centuries. For the time before the New Kingdom we don't have such a close control over the historical chronology as we do for the New Kingdom, especially not for the time of the Old Kingdom. The radiocarbon dates obtained from this period are also not consistent.[9] For the Middle Kingdom, we have a disagreement within the historical chronology between a high and a low chronology, which are about 42 years apart. Therefore we are not in the position to say if the radiocarbon dates are in agreement with the historical chronology or not.

On the contrary, the recent investigation of radiocarbon dates from short-lived samples throughout the stratigraphy of Tell el-Dab^{c}a by the VERA laboratory, covering successive strata from the 20th to the 15th centuries BC, shows a series of time consistent dates with an offset between 100 and 150 years higher than the historical chronology when using the high chronology of the Middle Kingdom to cover the first part of this stratigraphy (Fig. 1). The dates of the successive strata are anchored by two historical datum lines to the year 5 of Sesostris III (1868 BC high chronology) and the conquest and abandonment of Avaris c. 1530 BC.[10] Eleven phases of occupation (K–D/2) are sandwiched evenly in between. The space of "flexibility" as demanded by W. Dever, J.

[9] Zdiarsky 2005: 129–158.

[10] Bietak 2002: 28–38, fig. 2.

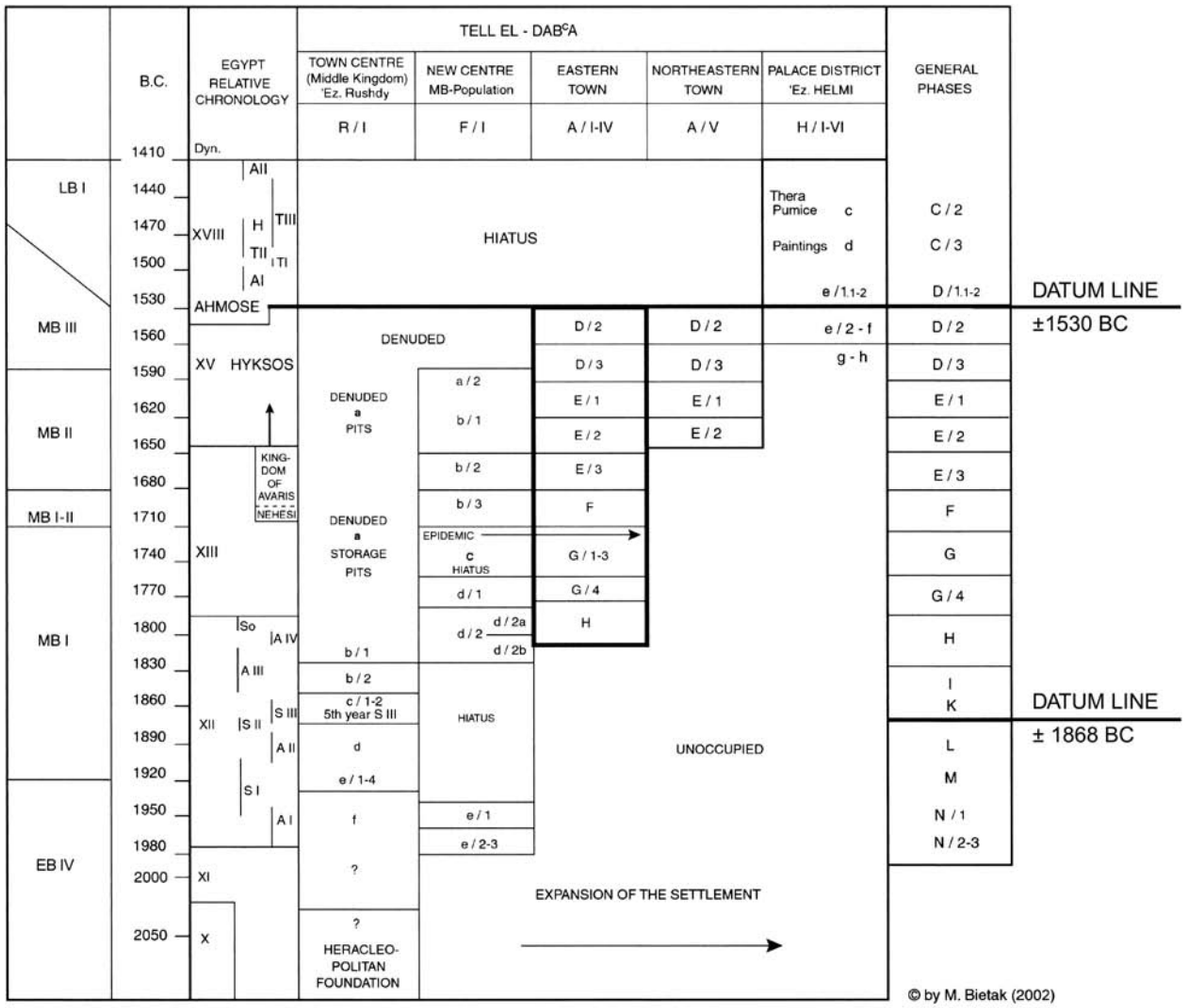


Fig. 2 The phasing, the stratigraphy of Tell Tell el-Dabᶜa and the two historical datumlines (after Bietak 2002, fig. 1)

Weinstein and S. Manning,[11] is very limited. If one would lengthen the time span of one stratum one has to squeeze the others to an extent that is not acceptable. Within certain limits, such adjustments have been made from the beginning of the system when sub-phases appeared (ph. G/1–3) (Fig. 2).

Besides this, there are cross dates to other sites such as a combination of ceramic types of the early 13th Dynasty from phase G/4 (allways dated according to pottery seriation to the beginning of the 13th Dynasty) which could be related to the moat between phases 14/13 at Ashkelon with a large number of Egyptian seal impressions of the early 13th Dynasty in the course of a *stratigraphie comparée* project[12] (Fig. 3). Those seals definitely proved the precision of the Tell el-Dabᶜa chronology which is recognized now largely by specialists of MB research and even by low chronology's strongest critic W. Dever.[13]

The establishment of Avaris as an interregional centre and as the seat of a major kingdom, 108 years (time span of the 15th Dynasty according to the Turin Canon) before the New Kingdom occupation

[11] Dever 1992: 6–10; Weinstein 1992: 28–32; Manning 1999: 328.

[12] Stager 2002: 353–363; Bietak, Kopetzki & Stager forthcoming.

[13] D. Ben-Tor 1994: 11; 1997: 163–64; A. Ben Tor 2004: 52–53 see also the rapprochement with similar chronologies of Weinstein 1992: 38; 1995: 84–90; Cohen 2002: 134–136.

B. C.	TELL EL-DABA	ASHKELON
	D/2	10
	D/3	
1600	E/1	11
	E/2	?
	E/3	
1700	F	12
	G/1-3	13
		13/14
	G/4	14
1800	H	

Fig. 3 The synchronisation of the phases of Tell el-Dab^{c}a and Ashkelon (after Bietak, Kopetzky and Stager forthcoming)

(c. 1640 BC) can be recognized by the sudden enlargement of the town to c. 250 ha and by the industrialisation of the pottery production during phase E/2 at Tell el-Dab^{c}a.[14] In addition, the seriation of pottery types helps to establish cross relationships to other sites with great precision.[15]

Of course Tell el-Dab^{c}a alone cannot establish interregional chronology, but the above paragraphs on its local chronology should show that the offset between the series of radiocarbon dates from Tell el-Dab^{c}a and the Egyptian historical chronology is indeed real as well as significant. This time difference of ca. 100 years or more repeats the offset between traditional (low) chronology (Thera eruption around 1500 BC) and the new radiocarbon-based high Aegean chronology (middle to second half of 17th century BC). Therefore, it would not make sense to try to remedy this situation by unilaterally raising the Aegean chronology by 100 to 150 years, claiming that a new proportion of the relationship between Egypt and the Aegean has been found.[16] The previous generation of scholars who have established the historical chronology by comparative methods of prehistoric archaeology were certainly no fools and have done their best to establish a timeframe based on exports and imports, with all the difficulties such as time lags and heirloom effects involved. Even if the mutual exports between Egypt and the Aegean world are scarce or questionable in the 17th and the 16th century BC, one can successfully work out a relative chronology for the time before and after those centuries and is able to fill the gap in between by a mutual assessment of Cypriot pottery in Egypt and Egyptian exports to Cyprus.

To keep the unilateral rise of Aegean chronology versus Egyptian is most difficult and leads repeatedly to results, which would need a lot of explanation to be even minutely possible. For example, one has to put a MC III-tomb at Arpera Mosphilos with three Tell el-Yahudiya/Lisht Ware jugs dating to the first half of the 16th century BC[17] (dangerously near the supposed high Theran eruption date in the second half of the 17th century BC) to the end of MC III if not to the transition to LC I[18] without taking into account the time lag between production, transportation to northern Cyprus (which is claimed to have no connection to Egypt) and the deposition of the jug into the tomb. This should however be the time when the LC Bronze Age should have already started, according to a LC IA2 WS I bowl in pre-eruption Thera.[19] Such a chronological scenario is very difficult to accept, even if we adjust Phase E/3,

[14] Bietak, Forstner-Müller & Mlinar 2003: 171–181.

[15] Bietak 1991: 31–47; 2002: 30–42.

[16] Manning *et al.* 2006: 565–569.

[17] Merrillees 1974: 49, 52, fig. 31/14–16, fig. 38–40. All jugs are of the Levanto-Egyptian group of TY Ware, Piriform 1b and c. no. 14, with kettle rim, and three zones of decoration typologically fall into the Phase F at Tell el-Dab^{c}a and may have been produced in the Levant, no. 15 has a rolled rim and is therefore late in this series, falling into Phase E/3 and no. 16 with a candlestick rim and segmented striped decoration is equally late and typologically anticipates already the Piriform 2 jugs of the Hyksos Period. It has two good parallels in Phase E/3 in Tell el-Dab^{c}a, which is the period shortly before the Hyksos time, i.e. first half till middle of the 17th century. The three jugs fit together and form an assemblage.

[18] Åström 1957: 197, n. 6, dates the tomb to the middle of the MC III period, in a later publication into mature MC III (Åström 1965: 120, pls. VI; XV: E.11.; Merrillees 1974: 43–77) because of a flattened base of a WP III jug and parallels of a spouted RP III bowl suggests a date in a late stage of MC III but definitely excludes a LC I date.

[19] On the bowl and its bibliography see Merrillees 2001: 195–202.

when the jug was produced, in a flexible way 20–30 years backwards and squeeze the phases F and G/1–3 towards the rock solid phase G/4. Also such an adjustment would lead to highly unlikely results. One has to inflate the regional development and to explain why specific Middle and Late Cypriot wares would appear first in northwestern Cyprus more than 100 years later in the same succession in south-eastern Cyprus and finally more than 100 to 150 years later in the same succession in Egypt. *Such a time lag may be credible within a shorter delay of 25 years or so, but such a succession of ceramic type groups, which reflects a production and market chronology, cannot be expected to have been kept up after a delay of over one hundred years or more.* This is an entirely unrealistic scenario, especially as we have to assume that exports accommodate the demands of the consumers. It seems that the succession of Middle and Late Cypriot wares, as observed in Cyprus for example at Maroni,[20] can also be found in a very similar succession in the stratification of Tell el-Dabᶜa, Ashkelon and in the new excavations of Peter Fischer at Tell el-ᶜAjjûl (Fig. 4). This would contradict a long delay between production and deposition at the above mentioned sites in Egypt and the Levant.

Trying to make a case for the high chronology, Manning also had to explain without a detailed typological treatment and material analysis that the Theran WS I bowl is of northern Cypriot production, despite leading experts like Karageorghis and Merrillees having different opinions believing it to be from the southern part of the island.[21] Also, the LB Canaanite jars found in Thera must be declared as MB.

Furthermore, one has to deny various strong synchronisms for the Aegean LM IA and LM IB period with Egypt. There are good typological reasons for an early 18th Dynasty date of an Egyptian calcite ointment jar found in a LH I-shaft-grave in Circle A in Mycenae,[22] showing that LM IA (which is more or less contemporary with LH I) must have ended *after* the beginning of the 18th Dynasty in Egypt. In addition to that, the fact that the vessel was reworked to a bridge-spouted jar shows that this import already had a history: it was produced in Egypt, exported to Crete, reworked on Crete, transported to the mainland, used for an unknown period, and then deposited in the shaft-grave.

On the other hand, evidence for LM IA in Egypt is scarce at best, but the transition from LM IA to IB can be narrowed down between the date of the youngest Egyptian object found in a LM IA-context in the Aegean and the first appearance of LM IB in Egypt. There are at least some useful contexts with LM IB material, that have been discovered in Egypt. The dating of the context of the much-discussed LM I-sherd found at Kom Rabiᶜa[23] is part of the contribution of David Aston in this volume. It is sufficient here to state that he provides evidence that the context of that sherd should be regarded as contemporary with strata c or d at ᶜEzbet Helmi and therefore should be dated to the Tuthmoside period.[24] From the Saqqara Teti Pyramid tomb NE 1, there is a LM IB-alabastron and a LH IIA ring-handled cup. Together with the Aegean imports, BR I and RLWM were found, and the Egyptian pottery from this tomb should be dated to the time of Hatshepsut and Tuthmosis III.[25] Other contexts of LM IB pottery are either inconclusive (like Abydos or Sedment) or confirm the first appearance of LM IB in the time of the Tuthmosides in Egypt (e.g. Gurob tomb 245, where a LH IIA-alabastron was found[26]). Therefore, it seems clear to us that the transition to LM IB should be placed around 1480, the time of the early Tuthmosides, considering the unknown time between production and deposition of the above-mentioned, reworked Egyptian jar from Mycenae.

Also, the massive first appearance of Theran pumice in archaeological contexts (thus far nearly 400 samples) in the Late Bronze Age in the Levant and in the Tuthmoside Period in Egypt and not before,[27] would have to be explained as lingering for two centuries on the beaches of Egypt and the Levant before being used, while thus far all pumice found in MB-contexts and in Egypt in the SIP were from other volcanoes. This is mounting evidence in favour of the traditional relative or even lower chronology, which cannot be easily brushed aside.

In toto, there are too many extreme explanations

[20] Cadogan *et al.* 2001: 75–88. See also Manning *et al.* 2006: 471–488.

[21] Merrillees 2001: 93; Karageorghis 1990: pls. VI; XV: E.11.

[22] Warren 2006: 308.

[23] Bourriau & Eriksson 1997: 95–120.

[24] Aston this volume.

[25] See Warren 2006: 311 with references.

[26] Warren & Hankey 1989: 144; Warren 2006: 313.

[27] Bichler *et al.* 2002: 55–70; Bichler *et al.* 2003: 11–21.

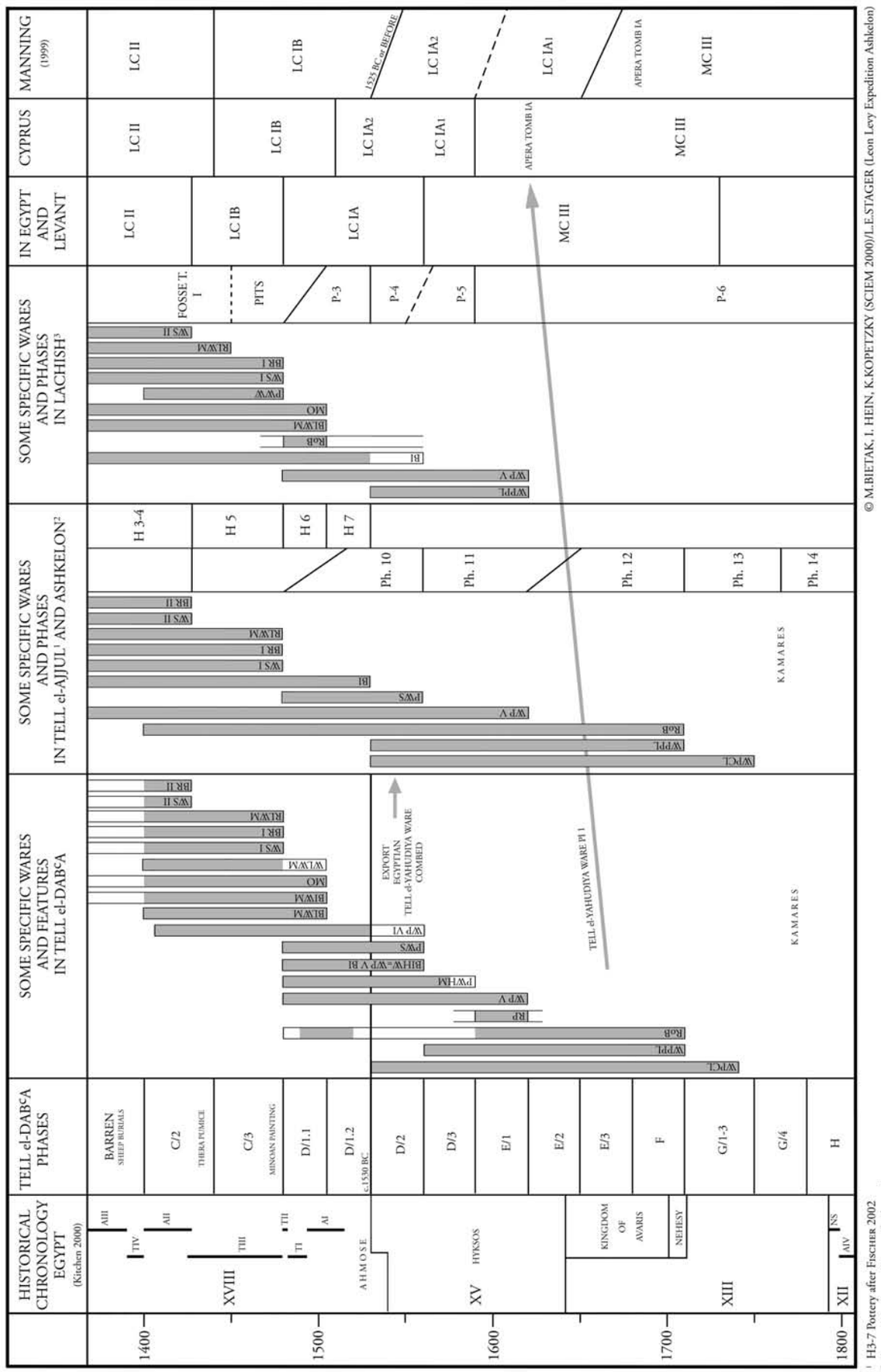


Fig. 4 Patterns of occurrencies of Middle and Late Cypriot pottery and other special wares in the stratigraphies of Tell el-Dab'a, Ashkelon, Tell el-'Ajjûl, and Lachish

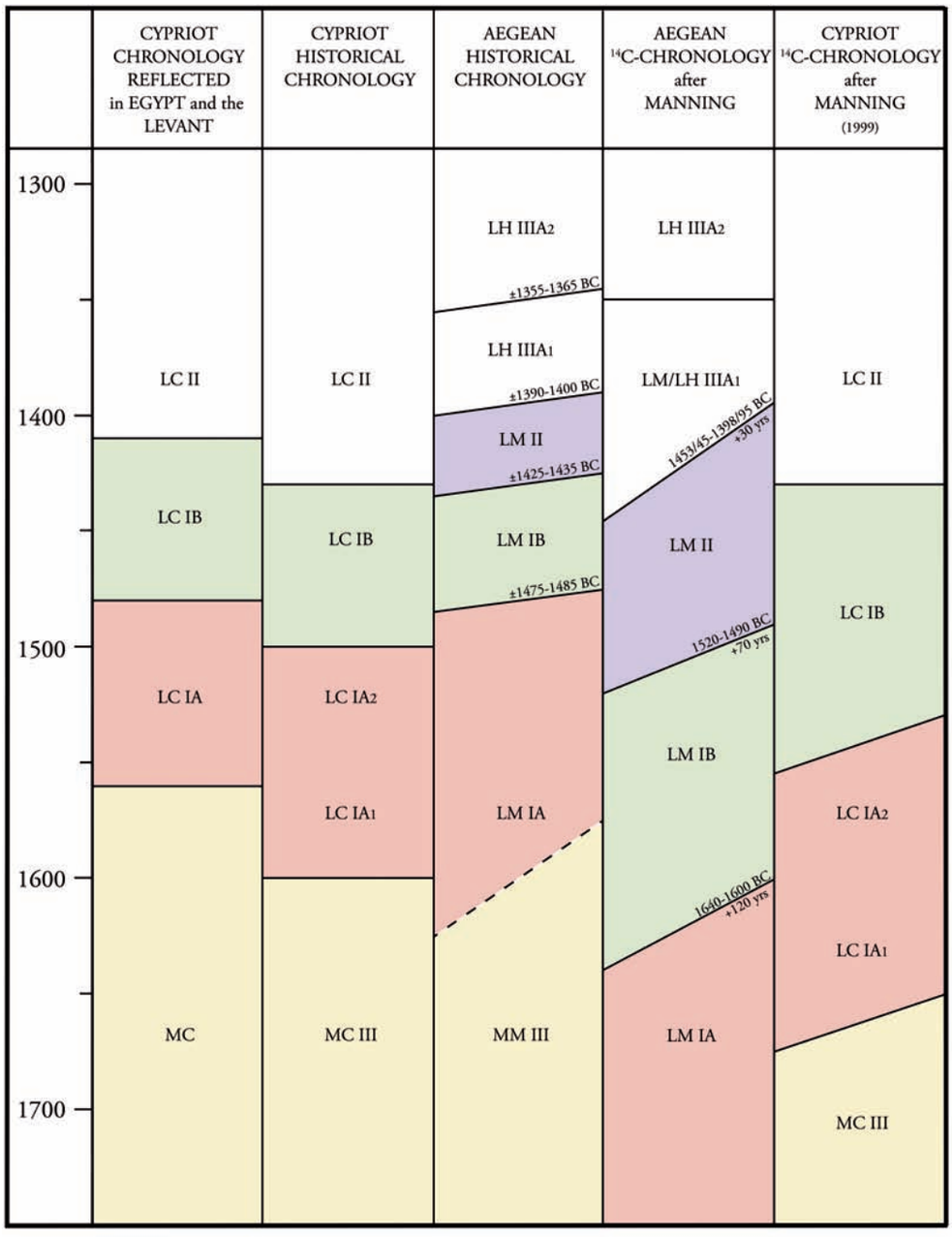


Fig. 5 Differences in the periodisation of the Minoan and Cypriot chronologies based on the historical chronology of Egypt, showing the offset towards the radiocarbon based periodisation

necessary to accept the unilateral rise of Aegean Late Bronze Age chronology. This construction is based only on a large number of implausible situations and is therefore not credible. Such a unilateral rise is also not necessary, as it seems that for the 17th to the 15th centuries the offsets of radiocarbon versus traditional chronology are the same in Egypt and the Aegean. Therefore one can come only to the conclusion that either the radiocarbon chronology or the historic chronology is wrong, or both have a defect. In such a case, the mutual control would not be possible without the help of an independent absolute dating method such as dendrochronology. As we have not yet succeeded in closing the floating dendrochronologies in Asia Minor and in the eastern Mediterranean, we may only compare the results of the two systems starting from a point of reasonable agreement, the dating of the latest ring of the keel of the Ulun

Burun shipwreck to 1364+15/–26, using the 2σ-range.[28] As there was also a scarab of queen Nefertity (1353–1341 BC) found in this ship , the lifespan of the ship and of the queen seem to fit perfectly together.[29] Also, the calibrated radiocarbon dates of Tell el-Amarna do not contradict the historical chronology, having no observable offset, as the second half of the 14th century lies perfectly within the margin of error.[30]

Working our way backwards, we can observe an increasingly higher date by the radiocarbon technology versus the historical chronology (see table in Fig. 5). With the end of LM IA we arrive at a dating difference of 120 to 130 years. In order to harmonise the two chronologies, one would be obliged to inflate the regnal years of kings of the 15th and early 14th century. This would be possible with the kings Tuthmosis II and Tuthmosis IV reaching a practically unsupportable maximum of 20 years and creating among the high officials of this time unusually high ages. But, to reach the age of 100 or even more is completely out of the question. This shows that the major reason (or fault) for this offset cannot be blamed on historical chronology. That there is an offset and not a false understanding in the relationship of the Egyptian and the Aegean periodisation (so the thesis of Sturt Manning)[31] is shown by the Tell el-Dabᶜa-series of Walter Kutschera *et al.* (Fig. 1) and by the late first appearance of Thera pumice in the archaeological contexts of Egypt and the Levant i.e. not before the Late Bronze Age in the Levant and not before the Tuthmoside Period, i.e. 15th century in Egypt.[32] This phenomenon cannot be explained by a change in technology, as pumice was also found before, however only in much smaller amounts. The fact that this pumice lay along old beaches of the 2nd millennium BC in North Sinai would explain the sudden and massive appearance at some sites (in addition to Tell el-Dabᶜa, Tell Hebwa and Tell el-ᶜAjjûl) and the sudden availability of large quantities of pumice, which formerly had to be imported.

In summation, the agreement between ^{14}C and historical chronology in the 14th century and the sharp rise of an offset a century earlier of up to 100 to 150 years as well as in the preceding centuries only shows that the calibrated radiocarbon dates presented by Manning, Bronk Ramsey *et al.* cannot be considered as a series of chronometric precision, but as a series where the precision seems to deviate considerably from the 15th century backwards. This conclusion is the more cogent one as within the historical chronology of the 18th Dynasty with its dense network of regnal and genealogical data nobody could claim that a mistake of more than 100 years could have mounted up from the Amarna period to the early Tuthmosides (within a century).

Under such auspices, one has to ask if it would not be worthwhile to investigate if a systemic failure in the Mediterranean ^{14}C evaluation could be discovered, or if the absorption of ^{14}C was, for environmental reasons, different from the 15th century BC backwards. Probably, we do not know enough about what may affect radiocarbon and its evaluation process. For this reason it, would be very important to close the gaps in Anatolian dendrochronology and to do the same with the cedar tree from Lebanon. Such new standards could be used to build up regional calibration.

In the nearer future we may collect more ^{14}C-samples from Tell el-Dabᶜa, especially to see, if the offset slows down in the 14th century, for which we do not yet have strata, as the occupation of the Amarna and post Amarna Period are denuded. The new project of the Oxford University laboratory under Christopher Bronk Ramsey, intending to measure well-dated Egyptian samples, is most important for enlarging the experience with Egyptian samples. The same is true of the project of sampling well-dated papyri by Ezra Marcus. According to our opinion, the relationship between historical dates and ^{14}C-dates of the New Kingdom would be of particular interest in order to see if the offset from the 15th century backwards could be verified also on new material.

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[28] Newton *et al.* 2005: 115–116.

[29] Weinstein in Bass *et al.* 1989: 17–29.

[30] Switsur 1984: 179–188; Hassan & Robinson 1987: 133.

[31] Manning 1999: *passim*; Manning *et al.* 2002: 733–744; Manning & Bronk Ramsey 2003: 111–133; Bronk Ramsey *et al.* 2004: 325–344; Manning *et al.* 2006: 565–569.

[32] The transition between MB and LB is put for convenience sake at 1550 BC because for the destruction of the MB cities for a long time Ahmose was made responsible, who hardly proceeded beyond southern Palestine. Also the Ahmose activities at Sharuhen only happened after the conquest of Avaris c. 1530 BC. In the meantime it became clear that many of those destructions happened later and possibly as late as from the year 22 = 1557 BC of Tuthmosis III onwards (Dever 1992: 14; Bietak 1991: 57–62). In the meantime objects from Egypt, dating into the 18th Dynasty were found in MB IIC contexts at Beth Shean (Mazar 2003: 328, fig. 5) and at Kabri (Black Lustrous Wheelmade Ware in tomb 902, see Kempinski 2002: 117–119, fig. 5.61/8–12).

In Egyptian chronology there are also problems in the first half of the first and the whole second millennium as well as the time before which also have to be worked out in respect to maximal margins of errors. A special conference was organised in Vienna (2005)[33] to address this theme and more work on these issues is being pursued in the meantime.

In respect to a realistic timetable to achieve a breakthrough, archaeologists could continue to refine the regional relative chronologies and establish, with mutual exports and datum lines of first appearances, especially of wide spread artefacts, a general relative chronology of the Eastern Mediterranean. One should do this without being biased by absolute chronologies. One may expect that at least achieving the relative interregional timetables could solve some problems in absolute chronology. Most of the contributions to this conference were parts of this collective endeavour. Above all, it seems to be most important that scientists should take the difference between Radiocarbon and historical chronology as seriously as we do.

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Times Change:
The Current State of the Debate in Old World Chronology

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Questions of chronological contemporaneity are at the heart of current discussions of the interaction and reciprocal influence between the early civilizations of the Mediterranean world. In order to consider such interactions, whether in the broad terms of world systems theory and core-periphery analysis or with respect to more precise modalities of interaction, it is necessary to establish what phase of Civilization A was in contact with what phase of Civilization B. No wonder, then, that chronology exercises its fascination. However, as Kenneth Kitchen has observed, chronology is not an academic discipline but a disease (Kitchen, pers. comm. of 1 February 2003, for which I am most grateful), or as I would say, an addiction, and indeed once one is hooked, it is hard to recover, whatever the cost to the historical work for which the chronological information was initially sought. Moreover, one pursues chronology knowing that whatever is said may be obsolete by the time it is published.

Chronological progress with respect to the ancient Mediterranean civilizations of the third and second millennia B.C. requires bridging the gap between the "two cultures", scientific and humanistic, described half a century ago by the British scientist, novelist and distinguished civil servant C.P. Snow. According to Snow, the two cultures existed in a state of mutual disdain and in almost total ignorance of the basic premises of the other (Snow 1959; 1967; see also Muhly 2003). In Old World archaeology and ancient history the problem has been particularly acute at times, with some practitioners trained in art history, classics and Near Eastern or Egyptian studies lacking basic knowledge of the strengths and limitations of the relevant sciences or of statistics. Conversely, many scientists working on archaeological material are unable to gauge the strengths or partial limitations of Egyptian and Near Eastern text-based dating, or the current state of understanding of Egyptian astronomy, or the reliability of data and interpretations from sciences other than their own. Indeed, even with the broad disciplines of Old World archaeology and linguistics, an information explosion has resulted in many cases in increasing specialization and concomitant difficulties in communication across geographic and material-based specializations. Communication shows signs of improvement, however, as archaeometry develops as a major subdiscipline and more students are trained in archaeological science. Growing sophistication in science among archaeologists is accompanied, however, by growing complexity and the arrival of information, some of potential critical chronological importance, from new and unfamiliar sources and sciences.

Egyptian Astronomy, Texts and Interconnections

It seems appropriate to begin a synopsis of the current state of the debate in Old World chronology with the first of the sciences harnessed to the task, astronomy, and in particular the astronomical dates from Egypt. These of course come in two forms: Sothic dates, *i.e.,* observations of the first rising of the dog star Sirius, and lunar dates, based in Egypt on the day when the crescent moon is no longer visible (unlike Babylonia where lunar dates are measured from the first visibility of the new moon). A recent paper by the late Patrick O'Mara casts doubt on the reliability of the basic critical assertion of Censorinus, an Egyptian third century A.D. Roman grammarian, who reported that a heliacal rising of Sothis had occurred on Egyptian civil new year's day in A.D. 139. O'Mara notes that the date asserted was the birthday of Censorinus' great patron and that, uniquely in this case, Censorinus gave no data to support his statement. O'Mara accepts another text, the Canopus Decree, but argues that the resultant Sothic dates can vary by twelve years (O'Mara 2003). The Sothic calendar question is primarily relevant to Middle Kingdom Egypt, because of the importance of the date proposed by Parker (1950) of 1872 B.C. for a heliacal rising of Sirius recorded on a particular date in the seventh year of Senwosret III. R. Krauss

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believes that the Parker date is untenable because the attributions of the Illahun lunar dates on which Parker based his computations were incorrect and that on the basis of lunar dates the seventh year of Senwosret III falls in 1831–30 B.C. Krauss' lunar calculations imply an end date for the Twelfth Dynasty of 1760–59 B.C. (Krauss, this volume).

Lunar dates remain controversial in some respects. Calculation of crescent visibility is difficult because of the complexity of the moon's orbit and the requirement of extreme accuracy – Kepler thought the computation of the exact time of conjunction impossible – and because of the difficulty of developing impartial criteria of visibility at various possible locations. R.A. Wells argues that the original Egyptian lunar observations yield so large a number of alternative readings and dates that no determinate calendrical system can be demonstrated. Recent experimental archaeology shows that it is difficult to obtain agreement among observers as to the day on which the old lunar crescent is no longer visible (Wells 2002; 1992; Spalinger 1992a). R. Krauss (this volume) remarks that the experiments in question did not involve experienced professional observers as would likely have been the case in ancient Egypt, but also notes that dust storms or overcast skies can make observations difficult, particularly at certain times of the year.

R. Krauss believes, however, that 1479 B.C. can be established as the precise year of the accession of Tuthmosis III with a high degree of probability. He argues that it is possible in almost all cases to eliminate many of the alternative interpretations of recorded lunar sightings stressed by Wells by virtue of their incompatibility with historical and other data at various points in the chain of dates which the discarded alternative readings would require (Krauss, this volume). Confirmation is found in a historical chain buttressed by astronomical observations from various reigns which R. Krauss believes requires an accession date between 1479–76 B.C. for Tuthmosis III on independent grounds (I am most grateful to R. Krauss for making his cogent analysis available to me prior to publication).

If, however, astronomical uncertainty could be shown to exist, how significant a difference would it make for Egyptian absolute chronology, particularly for the New Kingdom? Recall that without reference to astronomy K. Kitchen, by adding the last known regnal years of rulers and analyzing other data, was able to affirm an accession date for Ramses II not later than 1270 B.C. at the very latest, but more likely between 1274 and 1279 B.C. (Kitchen 1987; 1996, 1–13; 2002, 9). This result fits independently-derived Assyrian/Babylonian regnal dates considered accurate to within about a decade back to 1400 B.C., which are securely connected to Egyptian chronology through correspondence between Egyptian and Near Eastern rulers (Knudtzon 1915; Moran 1992; Rainey 1978; Cohen and Westbrook 2000; Dietrich and Loretz 1985; Rohl and Newgrosh 1988; Campbell 1964; Albright 1975). The prevailing Near Eastern chronology, as set forth by Brinkman, positing an eight-year overlap between the reigns of Ninurta-apil-Ekur of Assur and Meli-Shipak of Babylon (Brinkman 1972, 272–273; 1976, 31–33) has recently received confirmation through the discovery at Assur of tablets containing correspondence between these rulers (Frahm n.d.; Brinkman, pers. comm.). The Kitchen schema is also consistent with the date of 925/26 B.C. proposed by Thiele half a century ago for the invasion by Shishak in the fifth year of Rehoboam reported in the Hebrew Bible (Thiele 1983), a date which cannot be moved by more than about a decade given the secure date of 853 B.C. in the Assyrian annals for the battle of Qarqar during the reign of Ahab. R. Krauss believes that Egyptian lunar observations independently establish 926/25 B.C. as the date of the invasion by Shishak/Shoshenq I as set forth in his contribution to this volume.

Because the lunar date closest to 1270–79 B.C. for the accession of Ramses II was already believed to be 1279 B.C. on the earlier analyses of Krauss, von Beckerath and Hornung, Kitchen reasoned that one or more pharaohs or high priests might have ruled or served slightly longer than their last known year. He accordingly accepted 1279 B.C. for the accession of Ramses II, and hence 1479 B.C. for the accession of Tuthmosis III via a series of texts covering the intervening two centuries. These dates thus form the basis of the current standard, widely accepted Egyptian Chronology for the New Kingdom (Kitchen 1987; 1992; 1995; 1996, 1–13; 2002). If the lunar dates could no longer be maintained as suggested by R.A. Wells, then it would seem preferable to cite the critical Tuthmosis III accession date as c. 1475 B.C., rather than as 1479 B.C. (K. Kitchen has kindly informed me that he concurs with this suggestion in a pers. comm. of 25 February 2003, for which I am most grateful). The difference is accordingly minor. It is, however, far from clear that any change is necessary, for if R. Krauss is correct, then 1479 B.C. remains the exact year for the accession of Tuthmosis III and hence constitutes the earliest exact year date for any civilization at the present time. (Astronomy apart, the earliest certain annual date estab-

lished via continuous written records is the year 911 B.C. from the Assyrian annals.) Both K. Kitchen and R. Krauss concur that if the accession year of Tuthmosis III is 1479 B.C., then texts and inscriptions suggest c. 1539 B.C. as the most likely date for the accession of Ahmose and the beginning of the New Kingdom. The conquest of Avaris and the expulsion of the Hyksos from Egypt between the eleventh and twenty-second year of Ahmose, but more likely closer to the latter (Bietak 1996, 81; Bourriau 1997, 159) would thus occur between 1528 and 1517 B.C., followed by the first campaign of Ahmose in the Near East, including his three-year siege of the important site of Sharuhen. The appearance of considerable numbers of New Kingdom artifacts in the Near East presumably follows these events in time (see below).

Ice-Core Dating

Let us now consider proposals for dating the Aegean Bronze Age by recent scientific observations, beginning with the argument from the Greenland ice cores. Work at the frontier of science with respect to the difficult extraction of the cores, the counting of their annual laminations, and the chemical analysis of glass shards, each much smaller than the width of the human hair, has led Hammer *et al.* to propose that the eruption of Thera, in the mature-to-final phase of Late Minoan IA, can be dated to 1645 ±4 B.C. (Hammer *et al.* 2001; 2003). While some have expressed surprise that ice-core dating could be so precise, Hammer and Clausen note that the layers have been counted separately by themselves and two students, and that the counting was repeated years later with the same results (Hammer, pers. comm.). Moreover, the ice-core record contained shards identified as coming from the eruption of Vesuvius in A.D. 79 in an ice-core lamination counted to be only one year away from the known date. The putative one-year error – A.D. 79, the actual year of the eruption, rather than A.D. 80, when the tephra should have reached Greenland, in Hammer's view – is attributed by Hammer to a difficult-to-read ice-core lamination in the year A.D. 1936, when one year could be read as two (Hammer, pers. comm. of 21 March 2001, for which I am most grateful). Accordingly, it appears that the 1645 ± 4 B.C. date range for the glass shards in question is likely to be correct. Leading proponents of the Aegean Long Chronology at one point advocated combining this date range with the dates resulting from a proposed upward shift of 22 +4/–7 years in the dates for the Anatolian floating tree-ring chronology (see below) which would place the growth spurt evident in the logs from Porsuk near the Cilician Gates in 1650 +4/-7 B.C. (Manning *et al.* 2001). A date between 1650 and 1643 B.C. for the eruption is consistent with both claims.

While the date of the particles in the Greenland core seems reasonably secure, the Theran origin proposed for these glass shards is not. Only volcanic shards no larger than a few microns are light enough to reach Greenland from Thera before falling out of the stratosphere, but such minute particles are challenging to source chemically with sufficient precision to distinguish between similar eruptions, although it is possible to exclude clearly dissimilar sources. (See Hammer *et al.* 2003; Keenan 2003; Pearce *et al.* 2004.)

It is clear from the papers cited that the terms of discussion have moved considerably since the presentation of the "Extended Abstract" by Hammer *et al.* (2001) at the 2001 SCIEM EuroConference, which should not be regarded as surprising given the enormous challenge presented by the task. Proponents of the identification of the Greenland glass shards as Theran no longer believe that similarity in the bulk elements supports the proposition, or that a similar europium anomaly exists in the Theran and Greenland shards, but rather that the abundances of the remaining rare earth elements, with the exception of strontium, are sufficiently similar to constitute compelling evidence for Thera as the source of the Greenland shards (Hammer *et al.* 2001; 2003). D.J. Keenan contends, however, that the similarities are no greater than those between the Greenland shards and tephra from the eruption of Toba in Indonesia in 75,000 B.P. with respect to the twelve Toba rare earth elements tested, and moreover, that differences in composition in four of the bulk elements between the Greenland and Theran shards preclude Thera as a likely source (Keenan, pers. comm. with accompanying data, for which I am most grateful; now see Keenan 2003. For the Toba eruption, see Pearce *et al.* 1999). The Dawson eruption in Alaska about 25,000 years ago also produced very similar tephra (Pearce *et al.* 2003; Keenan 2003). Indeed, the empirical evidence suggests that massive rhyolitic mantle-sourced eruptions tend in general to produce tephra with similar rare earth element compositions (Keenan 2003).

Pearce *et al.* (2004) after detailed study also declare that the dissimilarity in chemical constituents shows that the Theran eruption was not the source of the Greenland shards. Indeed, Pearce *et al.* go further, concluding that not Thera but Aniakchak in the Aleutian Chain, which experienced a mid-second millennium B.C. eruption, is the highly likely source of the Greenland shards on the basis of extremely close similarity of chemical composition (see also Wiener

2003, 875–876, n. 65; for Aniakchak see Begét *et al.* 1992). S. Manning has kindly informed me that in light of the recent analysis he regards the ice-core data as irrelevant to the date of the Theran eruption (pers. comm., for which I am most grateful; see Manning, this volume; Ramsey *et al.* 2004).

A program of analysis of all glass shards in the ice-core record for the period between 1650 and 1500 B.C. would be desirable, but even a massive eruption would not necessarily leave an acidity signal on every square meter of Greenland (Wiener 2003; Robock 2000 and pers. comm., for which I am most grateful; Robock and Free 1995).

Dendrochronology

The potential contribution of dendrochronology to Old World chronology is evident. Because of the century-long history of data collection and analysis in the southwest United States, excavators of Native American sites sometimes know the exact year each room in a building and each building at a site was constructed. It has even been possible in some instances to determine which communities shared common logging areas, and which did not (Nash 2000, 60–82). In England, Ireland and Germany, trees with recognizable overlapping years because of similar weather patterns enable us to construct continuous chronologies back to the Neolithic. In the Near East and Aegean the sequence is not yet complete, due in part to gaps during the Classical era and particularly during the Roman Empire, when the wood used was imported from the entire Roman world from the Baltic to the Levant and hence often reflects climate patterns that cannot be compared. A "floating chronology" has been constructed, however, that forms a continuous sequence from the twenty-seventh to the seventh century B.C., incorporating at its core a 1,028-year sequence of juniper logs from Gordion and including the juniper from Porsuk previously mentioned. The absolute dates of the floating chronology are now fixed in all likelihood to within +8/–10 years, as established by a comparison between fifty-two decadal radiocarbon determinations from the Anatolian junipers and the 1998 international calibration curve (INTCAL98) which is based on measurements of German and Irish oak, and by the fact that component parts of the Anatolian sequence end in closely dated historical contexts at Gordion in Phrygia and particularly at Ayanis in Urartu (Newton and Kuniholm 2004).

A prior suggestion that the floating chronology could be anchored absolutely by the appearance in the Anatolian sequence of tree-ring events 469 years apart, just as in the oaks of Ireland and England where the events are dated to c. 1628 and 1159 B.C., has been withdrawn in light of subsequent research (Manning 1999, 313–314; 2004b; Manning *et al.* 2001; Renfrew 1996). The event previously placed at c. 1159 B.C. has not been found in logs subsequently examined, indicating that the event may have been local. Moreover, radiocarbon measurements of the Anatolian juniper sequence by Manning, Kromer, Kuniholm and Newton against the European oak-based INTCAL98 calibration curve determinations and against their own measurements of German oak for the seventeenth and sixteenth centuries B.C. led to the conclusion that a better fit is obtainable by shifting the Anatolian floating sequence back 22 +4/–7 years, as described above. The adjustment would shift the previously proposed date for the major growth spurt experienced by all sixty-one of the Porsuk trees from the year 1628 B.C. to 1650 +4/–7 B.C. (Manning *et al.* 2001). The result would thus fit the proposed date for the appearance of the glass shards in the Greenland ice core (whose relevance to the Theran eruption, however, is now widely debated, even by its former proponents, as noted above), while removing the Anatolian floating chronology from the 1628 B.C. date for a major climate event reflected in tree rings in the pines of California and the oaks of Germany, Ireland and England, previously identified with the Theran eruption by proponents of the Aegean Long Chronology, but now also deemed by most to be irrelevant (Manning and Sewell 2002; Manning, this volume). Dendrochronological research thus far has revealed no indication of a significant growth anomaly in the trees of California, Ireland, England or Germany around 1650–40 B.C. While the Greenland Ice Core Project (GRIP) ice core witnesses a volcanic event around 1626 B.C., the signal is clearly much less pronounced than the 1645 ±4 B.C. event (Clausen *et al.* 1997). It should be noted, however, that the magnitude of an acidity spike is only loosely correlated to the Volcanic Explosivity Index (VEI) of the eruption causing the spike (Keenan forthcoming, table 1). Among the other factors which influence the magnitude are the sulfur content of the eruption, the prevailing circulation conditions in the stratosphere and atmosphere, and the location of the eruption (Robock 2000). Similarly, it now appears doubtful that any of the B.C. events visible in the Irish oaks are the result of volcano-induced weather anomalies (Manning and Sewell 2002; Manning, pers. comm. of 17 February 2003).

As to the growth spurt in the Porsuk trees now placed at 1650 +4/–7 B.C., however, Kuniholm argues that whatever the effects of the Theran eruption elsewhere, the trees used at Porsuk, 820 km. downwind of Thera, would probably have responded to the additional moisture resulting from rainstorms caused by the eruption. The local effects of non-local eruptions can vary significantly, however. The logs of Gordion, from a semi-arid zone in Anatolia where additional moisture might be expected to have a significant effect, do not show any unusual effect near this date (Kuniholm *et al.* 1996, 780–782; Kuniholm and Newton, pers. comm. of 19 November 2002, for which I am most grateful). Of course the identification of the Aniakchak eruption as the source of the glass shards in the Greenland ice core at 1645 ±4 B.C. suggests the possibility that the Porsuk log growth spurt of 1650 +4/–7 B.C., if caused by a weather-forcing volcanic event, may possibly be attributable to this Alaskan eruption also. Recent work on the Porsuk dating, however, suggests that there may be no overlap in dates (1653–50 B.C. best dates for Porsuk anomaly, 1645 ±4 B.C. for Aniakchak [Kuniholm, pers. comm.; Manning, pers. comm.]). It is particularly worth noting that the raising of the Porsuk logs' last ring dates to 1573 +4/–7 B.C. (pursuant to the revised radiocarbon/historical analysis discussed above) also removes the Porsuk evidence from the argument of silence against a post-1570 B.C. eruption of Thera consistent with the Aegean Short Chronology.

To date no indication of a growth anomaly possibly related to a volcanic eruption has been reported in the trees of Ireland, England, Germany or California for the period 1570–1470 B.C. (The initial California bristlecone pine database was severely limited with respect to the number of trees and extent of area examined for the relevant period [LaMarche and Hirschboeck 1984]. Work now underway at the University of Arizona Laboratory of Tree-Ring Research seeks to expand the database with respect to the period 1675–1450 B.C. [unpublished report of Director T.W. Swetnam of 7 June 2002].) The absence of any such indication during the period favored by advocates of the Aegean Short Chronology for the Theran eruption was once a major tenet of the Long Chronology position, but further research and reflection have led to reconsideration of the value of this negative dendrochronological evidence. It is now well understood that many factors other than the degree of explosivity affect whether an eruption is represented in the tree-ring record. These include the proximity of the volcano, the amount of aerosol released, its sulfur content, prevailing circulation conditions in the stratosphere and atmosphere, offsetting or reinforcing weather factors such as El Niño or La Niña conditions, the time of year in relation to the growing season of the trees in question, whether the trees exist in a robust or marginal environment with respect to temperature and water, and the age and condition of the trees at the time of the event (Grattan 2002; Jacoby 2002; Brunstein 1996; Irwin and Barnes 1980; Allard *et al.* 1991). Conversely, many non-volcanic climate – and weather – related factors, including highly local conditions, can cause growth spurts or interruptions in trees.

Radiocarbon Dating

Recent years have seen major progress in the science and art of radiocarbon dating. Improved techniques of measurement at high precision laboratories, including lengthened counting periods at accelerator mass spectrometry facilities and periods of measurement of up to ten days at radiometric measurement laboratories where feasible and affordable, more stringent pretreatment protocols, and increased cooperation between laboratories to reduce inter-lab measurement discrepancies, have all contributed to a narrowing of proposed date ranges published, which after calibration combine statistical and judgmental factors (Kromer *et al.* 2001, 2530; see in general Scott 2003, esp. 287; Manning 2004a; forthcoming).[1] Within the past decade, however, high precision laboratories have

[1] The "State of Research" is well described by O. Cichocki *et al.* (2003, 102): "In order to measure radiocarbon ages it is necessary to find the amount of radiocarbon in a sample. This can be achieved either by measuring the radioactivity of the sample (the conventional beta-counting method) or by directly counting the radiocarbon atoms using a method called accelerator mass spectrometry (AMS). The main advantage of AMS over the conventional beta-counting method is the much greater sensitivity of the measurement. In AMS the radiocarbon atoms are directly detected instead of waiting for them to decay. The physical sample sizes required are typically 1000 times smaller, allowing much greater choice of samples and enabling very selective chemical pre-treatment. However, handling small amounts of sample material increases the danger of contamination. Since minute additions of non-genuine carbon can lead to

sometimes provided quite different date ranges for materials divided between them, as in the case of the reported dates a century apart for the Turin Shroud as well as for the control material of known first century B.C./A.D. date (Taylor 1997, 84–85). Examples of recent and more limited inter-laboratory measurement differences are provided in Manning (2004a). Some high precision laboratories acknowledge slight ongoing biases, *e.g.*, "a conservative upper limit of an additional unknown laboratory error in the Heidelberg facility is eight radiocarbon years" (Kromer *et al.* 2001, 2530).

The one standard deviation bands in which both radiocarbon ages and calibrated dates are normally stated (pursuant to the conventional Gaussian bell curve distribution) by definition provide only a 68% statistical chance of encompassing an accurate radiocarbon age or calibrated date under the best of collection and pretreatment circumstances. While the two sigma, 95% probability, bands for uncalibrated ^{14}C ages are twice those of the one sigma bands – *e.g.*, sixty years instead of thirty years – the calibrated bands quoted take into account various factors, including the number and duration of determinations, their precision (*i.e.*, similarity to one another) and the nature of their fit to the calibration curve. Such judgments are necessarily partly subjective (see *e.g.*, Manning 1995, 126–129) and are sometimes open to dispute (Wiener 2003).

All statements of probability made in the course of analyzing ^{14}C determinations refer in the end to the degree of overlap between the one or two sigma ranges of radiocarbon determinations from selected samples of seeds or wood and the one or two sigma ranges of decadal calibration curve measurements taken from trees of known dates, both of which are subject to the uncertainties of regional variation, intra- and inter-year variation, and the potential presence of old carbon. The probabilities presented are, however, only measurement probabilities, not date probabilities (see *e.g.*, van der Plicht and Bruins 2001), contrary to what readers or conference participants unfamiliar with the discourse of dating by radiocarbon may surmise. A measurement of the last rings of recently cut Theran wood which produced a ^{14}C age of 1,390 years was a high precision determination of high probability that correctly measured the overall carbon present, which included ^{14}C-depleted carbon absorbed from a nearby fumarole (Bruns *et al.* 1980), thereby providing a wholly erroneous date.

A. Gilboa and I. Sharon (2003, 60, n. 14) observe that:

"*Precision is not the same as accuracy.* The ± figure provided by the lab with radiometric dates merely denotes the internal variation, *i.e.*, the standard deviation of a number of individual counting-periods on the same vial or accelerator runs on the same target. There are a host of other factors that could (minutely) affect the result: the microenvironment around the sample in the ground; postrecovery storage conditions; differences in chemical protocols for pretreatment; differences in the counting protocols; differences in equipment and its calibration, etc. Some of these sources of possible error are removed in the cleaning process or are neutralized by the appropriate use of standards and backgrounds (blank samples) – but are all? These issues are the subject of ongoing investigations. Finally, even when different labs do agree, the calendar age depends to a large extent on the accuracy at which the calibration curve for the relevant period has been determined and such factors as regional differences in the radiocarbon reservoir. Recent studies (*e.g.*, Manning *et al.* 2001) indicate such inaccuracies exist, but they are small (*i.e.*, in the order of magnitude of individual decades)".[2]

incorrect results, the entire sample preparation has to be performed with utmost care, which is time consuming".

The rigor with which contaminants can be removed from seeds through pretreatment depends on the extent to which the sample consists of purely elemental carbon. Some Theran seeds, for example, may be more akin to humic acid compounds than to reduced elemental carbon, and hence must receive special treatment (Housley *et al.* 1999, 160). Rigorous pretreatment is also a necessity where seeds have been carbonized together with the baskets or sacks in which they were carried and stored, or with the wood or cloth tops of jars in which they were stored.

[2] The Gilboa and Sharon paper is noteworthy in its explicit statement of aims and procedures, a practice which should be made standard in all reports of radiocarbon dates: "With the aim of reaching measurement accuracy within the ±25 to ±40 (radiocarbon) year range, conventional radiometric counting was the preferred analytic method, rather than the less-practiced AMS (atomic mass spectrometry) technique. We set an acceptability threshold of 3 g clean carbon (after treating the specimen with hydrochloric acid and sodium hydroxide) while aiming for an ideal of 7 g carbon per specimen. The specimens were analyzed at the Weitzmann Institute's ^{14}C facility (WIS; for lab procedures, see Gupta and Polach 1985). To assure the desired precision, each sample was counted for 3,000 minutes" (Gilboa and Sharon 2003, 58).

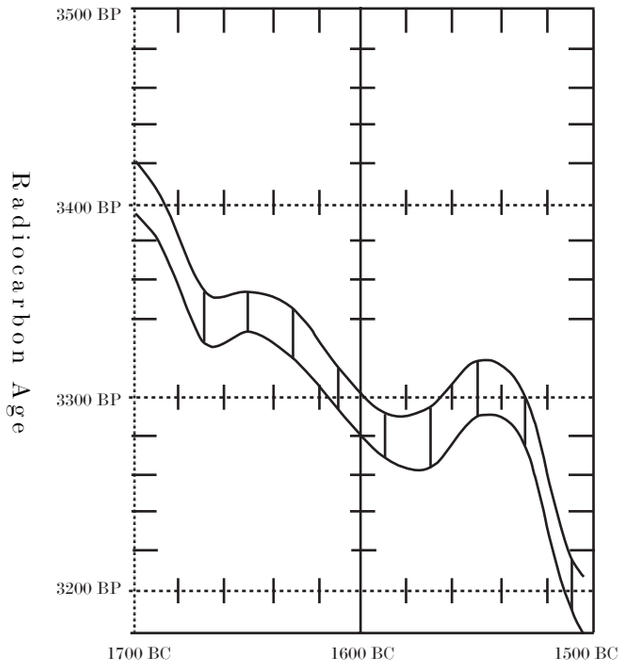


Fig. 1 Calibration curve, one standard deviation band from 1700 B.C. to 1500 B.C. (after W. Kutschera, pers. comm.). Of course the two standard deviation band depicts a still greater oscillation

The Manning *et al.* judgment cited is discussed in various contexts below, and may prove somewhat optimistic. In any event, it is important to note that differences "in the order of magnitude of individual decades" can be of critical importance in determining whether radiocarbon measurements fall within the oscillating portion of the calibration curve, consistent with either the Aegean Short Chronology or Aegean Long Chronology or an earlier date consistent with only the Aegean Long Chronology (Fig. 1).

In general, "halving the error limits on a radiometric age requires at least four times the number of determinations, or four times as much counting time . . ." (Buck and Millard 2003, VI). Such time-intensive measurements by either method are expensive and, given constraints on budgets, a practical question may often exist as to whether better overall results are likely to be achieved by obtaining less intensive determinations on a larger number of samples or more precise, but fewer determinations. In any event, differences of one to two decades strain the limits of the system of radiocarbon dating by measurement and calibration.

Of course neither the Gaussian standard bell curve distribution which determines the one and two sigma probability bands prior to calibration nor the stated calibrated ranges encompass such possible sources of error as stratigraphic disturbance of carbonized matter before recovery or contact with old carbon. Moreover, the Gaussian distribution is neither designed to reflect nor sufficient always to encompass such known variables as intra- and inter-year differences within the decadal calibration measurements, differing patterns of absorption in separate tree species or regional variation in the distribution of ^{14}C. The international calibration curve assumes uniform distribution of radiocarbon at the Earth's surface. Any known regional offsets such as those for the Southern Hemisphere, where F.G. McCormac *et al.* and M. Stuiver both recommend that 41 ±14 years be added to all measurements, must be applied to ^{14}C determinations prior to calibration. Moreover, such regional variations are not constant but rather vary over time, with differences in solar activity suspected as the cause (McCormac *et al.* 2002, 641; Knox and McFadgen 2001, 87; Stuiver *et al.* 1998, 1046).

The irregular absorption of ^{14}C because of solar, climatic and regional factors presents a wide array of issues. Changes in climate affect the growing season during which trees and seed-producing plants absorb most of their radiocarbon. For example, recent work by Manning *et al.* suggests that a marked climate change in Anatolia in the ninth-eighth centuries B.C. delayed the growing season of trees, resulting in aberrant ^{14}C determinations in this period as determined by comparison to rings of the same known date from the trees in Ireland and Germany used to establish the calibration curve (Reimer 2001; Kromer *et al.* 2001; Manning *et al.* 2001). P. Reimer has described succinctly the process at work:

"^{14}C is primarily produced at high latitudes in the lower stratosphere by the collision of cosmic ray-produced neutrons with nitrogen. During periods of high solar activity, distortion of Earth's geomagnetic field by the solar wind prevents charged particles from entering the atmosphere and little ^{14}C is produced, whereas ^{14}C production peaks during periods of low solar activity (solar minima). The atomic ^{14}C is quickly oxidized to $^{14}CO_2$ and enters the troposphere during the late spring, a period of high stratospheric-tropospheric exchange. By the next spring, the higher ^{14}C concentration in the atmosphere has been well mixed and diluted by exchange with other carbon reservoirs, particularly the surface ocean. The German trees, which grow mostly in the mid to late summer, take up more $^{14}CO_2$ during photosynthesis than do the Mediterranean trees, which grow in the spring and early summer" (Reimer 2001, 2495).

California bristlecone pines in one study consistently produced older ages than German and Irish oaks of the same known date, with an average offset of 35 years, due perhaps to differences in the growing season of the trees, with differences in species and altitude of the trees as other potential relevant variables (McCormac *et al.* 2002). Research in the course of establishing the 1998 calibration curve disclosed a mean difference of 24.2 ±6 years between Belfast measurements of Irish oak and Seattle measurements of German oak for the critical years 1700 to 1500 B.C. Eight measurements of sections of the same Irish trees by the two laboratories for the years 655 to 565 B.C. in a generally flat part of the calibration curve showed an inter-lab mean difference of only 3.6 ±5.2 years, however. Accordingly, unless the situation is markedly different for the oscillating calibration curve years of 1675 to 1525 B.C., much of the 24.2 ±6 year difference between the Irish and German oak from the 1700 to 1500 B.C. period is likely to be the result of regional variation rather than inter-laboratory measurement differences (Reimer, pers. comm. of 4 April 2002; see also Damon 1995b). M.G.L. Baillie and D.M. Brown at Belfast caution that "the full implications of the regional calibration off-sets have not yet been fully appreciated by the archaeological community" (Baillie and Brown 2002, 499). Because the Irish oak ages measured by Belfast are bidecadal and the German oak ages measured by Seattle decadal, the measurements of German oaks are heavily overweighted on the 1998 curve now in standard use. The statistical method used to smooth the data introduces a further variable. Moreover, for much of the INTCAL98 calibration curve, only one log of known calendar age was tested for a given period to obtain the corresponding level of ^{14}C (or in the case of the German oak as distinguished from the Irish and North American trees, one log for each laboratory). Accordingly a single aberrant determination by one laboratory can have a significant effect on the shape of the calibration curve for a particular decade (Wiener 2003 and sources cited therein).

Measurements also are affected by the intra-year variation in radiocarbon described by Reimer. One study observed an intra-year variation between the winter low and the spring-summer high of up to thirty-two years, with an average difference of fourteen years (Levin *et al.* 1992; Levin and Hesshaimer 2000; see also Kromer *et al.* 2001). A study of plants in Canada (Milton *et al.* 1993, 489) noted ". . . the difficulty of attempting to correlate the ^{14}C signal in growing vegetation with an average annual, or even average summer, emission rate". The study reports, moreover, that more than 5% of a plant leaf's carbon content may be replaced in the course of a day (Milton *et al.* 1993, 492–493; I am indebted to D.J. Keenan for calling my attention to this research). Measurements of seeds from early crops – *e.g.*, winter wheat or barley – could thus be misleading when calibrated against trees whose major growth period during which they acquire most of their ^{14}C occurs in late spring. Many Mediterranean plants have growing seasons that include winter, differing in this respect not only from the European trees on which the calibration curve is based, but also to some degree from the trees of central Anatolia where winters are colder and cold weather lasts longer. Egypt of course has a much warmer climate and a different growing season, which must be considered in connection with ^{14}C measurements such as those now underway of seeds from Tell el-Dab^{c}a. In all cases where a difference in growing season exists, Aegean seeds are likely to give older radiocarbon age measurements than European or Anatolian trees, although the difference is likely to be within a decade except in unusual cases (see Keenan 2004). Egyptian seeds, however, may be in a separate category in this regard.

Inter-year variation within the decadal calibration determinations is potentially more troublesome still. Just as radiocarbon measurements can show no significant change over several centuries during which the intake of ^{14}C balances the half-life loss, conversely major differences in ^{14}C ages exist between adjacent decades at sections of the calibration curve where the slope is vertical or near vertical. Where dates proposed for major archaeological horizons are dependent on such sections of the calibration curve, the relevant decadal determinations should first be confirmed. In one such area of the INTCAL98 curve relevant to our understanding of the major Late Minoan IB destruction horizon in Crete, a supposed vertical portion of the calibration curve proved to be an illusion resulting from a single aberrant measurement of one log in one laboratory (Wiener 2003, 382). The example given is not unique; a calibration curve measurement has had a major impact on the study of the prehistory of Sweden (Shak-Nielsen 2003, 122).

The fact that adjacent decadal determinations from trees can produce significantly more widely spaced radiocarbon ages in vertical or sharply sloped portions of the calibration curve raises a question about mid-points within the decades. When a decadal measurement includes in the sample tested wood from both narrow rings representing years of little

tree growth and wood from wider rings representing years of greater growth, the wider rings will provide a greater amount of the cellulose containing radiocarbon. Seeds absorbing their ^{14}C during a few weeks at differing points within the decadal spans may produce measurements different from the decadal averages. (Of course in the odd case where seeds are roasted and stored for export or as protection against a poor harvest, they may be recovered from a destruction level of a year falling in a decade different from the year of roasting, and thus also give a misleading indication of the date of the destruction.)

The decadal determinations comprising the INTCAL98 calibration curve are generally reliable and reproducible, but were often the product of a single measurement on one or few logs as noted and subject to shorter counting periods than those current today. I.U. Olsson notes that "it is important that a good pretreatment is applied on the wood used for calibration" (Olsson 2003, 23). The INTCAL04 calibration curve now in preparation by an international team chaired by P. Reimer will make a significant advance in terms of precision and accuracy. New puzzles are likely to arise, however. For example, current planning envisages some smoothing of the calibration curve measurements to limit the effect of individual (and potentially misleading) decadal determinations by weighting each decadal measurement with the two preceding and two following decadal determinations. The result will reduce the risk of significant error for any single decade, while reducing the extent of individual decadal peaks and valleys employed for purposes of "wiggle-matching", as for example, in the comparison of the fixed European and floating Anatolian dendrochronological sequences. (I am grateful to S. Manning and P. Reimer for discussing these issues with me. An interesting approach to the problem of smoothing ^{14}C determinations is presented in Gómez Portugal Aguilar *et al.* 2002.)

However precise the uncalibrated measurements, they remain dependent on the calibration curve adopted and the regional, time and climate sensitive data they encode (Bruins and van der Plicht 1995, 218–219). Small differences in sample and calibration curve measurements can have major implications for Aegean Bronze Age chronology and history. For example, because of the steepness of the slope of the calibration curve in the period in question, the twenty-year span between 3340 and 3320 B.P. in uncalibrated ages can move calibrated central dates of the ± range (sometimes called the "mode of distribution") from about 1620 to about 1525 B.C., and a spike at 1675 B.C. can be included in the oscillating portion of the calibration curve by a further extension of twenty additional radiocarbon years. The addition of only 0.25% ^{14}C-depleted carbon to the total carbon of a sample, either because the old carbon is absorbed by the plant or tree in its lifetime or acquired from later contact and not removed through rigorous pretreatment, will result in a twenty-year shift.

With regard to regional variation, radiocarbon determinations from Southern Hemisphere tree-ring samples giving markedly earlier ages at times than samples from the Northern Hemisphere of the same known age have been variously attributed to the fact that more of the Southern Hemisphere is covered by water, which contains fifty times the amount of ^{14}C of the atmosphere (Lerman *et al.* 1970; Olsson 1979; 1987), to the gradual release of a sink of old carbon in the Weddell Sea in Antarctica, southeast of South America (Lerman *et al.* 1970; Knox and McFadgen 2001) and/or to upwellings of old carbon from major Pacific Ocean underwater volcanic vents, with the upwellings occurring in particular during El Niño events (Keenan 2002). The major increase in the Southern/Northern Hemisphere gap during the Maunder Minimum, the period from about A.D. 1645 to 1715 when sunspots were extremely rare, corresponding to the middle and coldest part of the "Little Ice Age" during which Europe and North America at least were subject to bitterly cold winters, suggests the presence of a significant climate component in the hemispheric differences noted.

Measurements of modern, pre-nuclear test period shells of known age from the Mediterranean show a water reservoir radiocarbon addition of around 400 years, which then must be subtracted from marine determinations (Siani *et al.* 2000; Reimer and McCormac 2002). Whether upwelling of ocean or seawater containing old carbon is a plausible cause of anomalously early radiocarbon ages for short-lived terrestrial samples either in the Pacific or Mediterranean regions is a subject of dispute (Keenan 2002; Manning *et al.* 2002a). Keenan notes that the Mediterranean exhales carbon dioxide intermittently as a result of natural processes. Given the large concentration of CO_2 in seawater compared to the atmosphere, and its relative depletion in ^{14}C (resulting in the 400-year difference), the potential effect of episodic upwelling on coastal sites could be significant. Manning *et al.* conclude that there is "currently little evidence anywhere for a sustained large amplitude depletion of ^{14}C in terrestrial samples due to the influence of old CO_2 from the surface ocean and maritime air carried onshore" (2002a, 746). The question

requiring consideration, however, is not the existence of "sustained large amplitude depletions", but rather the possibility of episodic, small amplitude effects. (The addition of only one-half of one percent of old carbon to a sample results in a forty-year increase in radiocarbon age, as noted above.)

The quotation from Manning *et al.* continues as follows:

"A limited number of measurements directly on maritime air show highly localized and variable results (Bhushan *et al*. 1997; Dutta *et al*. 2000); such small-scale depleted air parcels would be expected to dissipate rapidly over short distances with atmospheric mixing, as is observed in air-sampling stations in the Southern Ocean/Antarctica. Where differences of up to a few ‰ (or a few tens of ^{14}C yr) do occur in tree-rings, they appear to vary on a relatively short timescale and may be partly or wholly due to other causes (McCormac *et al*. 1995; Damon 1995a; Stuiver *et al*. 1998; Knox and McFadgen 2001; Kromer *et al*. 2001; Hogg *et al*. 2002; Hua *et al*. 2002)".

Of course for certain critical decades and events such as the date of the eruption of Thera, "a few tens of ^{14}C years difference" would be sufficient to cause major uncertainty, as noted above.

A further question arises as to whether the analysis cited takes into account reports of the release of significant amounts of ^{14}C-depleted carbon from major vents or fields of vents on land and under the sea, particularly those in plentiful supply in the Hellenic Arc, including at least one close to Thera. While a number of single-source volcanic eruptions have resulted in emissions of carbon-depleted gases over small distances measured only in meters or hundreds of meters as stated (Kromer *et al.* 2004), others (particularly those with multiple vents) have been shown to produce strong effects over greater distances (*e.g.*, Rogie *et al.* 2001 re carbon dioxide emission at Mammoth Mountain in California; Pasquier-Cardin *et al.* 1999 re venting in the Azores), sometimes to a distance of over ten kilometers (Olsson 1987, 20 re Kamchatka in the Aleutian Chain). Within the Mediterranean, Allard *et al.* (1991) report carbon dioxide production of 70,000 tons per day from the gas vents around Mt. Etna and strong effects measured fifteen kilometers away. The extensive gas field in northern Italy running between Florence and Naples includes one vent east of Naples which emits 280 tons of carbon dioxide per day, while the average diffuse CO_2 degassing from an area of 23,000 km.^{2} is about 18,000 tons per day (Rogie 1996; Cardellini *et al.* 2003; Rogie *et al.* 2000; Chiodini *et al.* 1999). These sources may affect Italian ^{14}C determinations and hence Italian prehistoric chronologies.

The critical question, however, involves sources in the Aegean, particularly those close to Thera. Of course we cannot know of sources that have disappeared or become quiescent as a result of eruptions or earthquakes since the Bronze Age. Moreover, still active vents may be invisible, with their gas escaping through the soil.

Dando *et al.* (1995a) report that:

"Greece and the Aegean is one of the seismically most active regions on Earth and has the highest seismic activity in Eurasia (Bath 1983; Makropoulos and Burton 1984). Geothermal areas are known in the northern and central Aegean as well as along the Hellenic Volcanic Arc (Dominico and Papastamatoki 1975; Holm 1988; Fytikas *et al.* 1989; Varnavas 1989; Varnavas and Cronan 1991)".

The Hellenic Arc of volcanoes stretches from Methana on the coast of the Peloponnese through Melos and Thera to Yali-Nisyros. The vent near Melos is today one of the most active sources of hypothermal fluxes in the world. The seafloor around Melos has "the world's largest known concentration of vents in shallow water" (Pain 1999, 39), and, although not an active volcano, its output of ^{14}C-free carbon is estimated at two to ten kilotons per day (Botz *et al.*1996; Dando *et al.* 1995b; 2000). An earthquake near Melos in 1992 caused the eruption of gas vents all around the island and showed an increase of 65% in the number of vents (Pain 1999, 41). " 'Every fumarole on the shore blew out. And the sea boiled as the gas came out with such force. Stunned fish came to the surface' " (P.R. Dando, as quoted in Pain 1999, 41).[3]

Systematic surveys of submarine hydrothermal venting have not yet been conducted in the Aegean

[3] Discharges from the vicinity of Melos, however massive, would dissipate long before reaching Thera, particularly given the strength of Aegean winds. It is conceivable that in the special case of possible upwelling of old carbon from the Mediterranean, the old carbon content of the water would be augmented by massive discharges from underwater vents such as those described near Melos. On calm days such as occur during the spring and early summer growing season for plants the discharge of moisture against the hills of Thera from low clouds drifting in from the west is sufficiently frequent to be known as "the Poseidon irrigation system" (Doumas, pers. comm. of 1 December 2003). The chance that any particular radiocarbon determination would be affected in this manner is very slight, however.

(Dando *et al.* 1995b, 661), but gas vents producing ^{14}C-depleted CO_2 have been noted in many places, for example near the islands of Thasos and Lesbos, with others undoubtedly existing elsewhere (Dando *et al.* 2000). Vents rather than volcanism may be the source of much of the world's carbon (Rogie 1996). The principal recent study notes that "only recently, it has been recognised that the quiescent, non-eruptive diffuse degassing is the principal mode of gas release from volcanoes, and that volcanic flanks play a fundamental role" (Mörner and Etiope 2002, 189). (The total extent of outgassing of old carbon worldwide is also discussed in Gerlach 1991, 249.) While dilution over space will render negligible the effects of isolated points of low magnitude discharges, higher magnitude, more numerous, distributed sources may combine to produce a regional effect. Degassing from vents has been known to increase markedly with seismic activity, as in the case of Melos (Dando *et al.* 1995b) and numerous other earthquakes (Gold and Soter 1985; Soter 1999).

Whatever the potential effect of outgassing elsewhere in the Aegean, a far more significant question in regard to ^{14}C measurements of Theran seeds and wood is presented by sources of old carbon on Thera itself. Today on Thera fumaroles, hydrothermal springs and higher than normal concentrations of ^{14}C-free CO_2 in soils persist (McCoy and Heiken 2000a; 2000b; Holm 1988, 203; Bruns *et al.* 1980). Five kilometers to the northeast of Thera a major underwater source of ^{14}C-depleted carbon exists at the Columbo Bank (McCoy and Heiken 2000a). F.W. McCoy describes Columbo Bank as a very dangerous volcano, perhaps the most dangerous in the southern Aegean today, about whose eruptive history little is known (pers. comm. of 21 February 2004, for which I am most grateful). Non-volcanic earthquakes also often trigger the release of ^{14}C-depleted gas from vents. Earthquakes are of course frequent on Thera; 109 earthquakes, most with a Richter magnitude of one to three, were reported in the four-year period from 1985–1988 (Delibasis *et al.* 1990, cited in McCoy and Heiken 2000a, 48). F.W. McCoy and G. Heiken report that:

"[F]umaroles and hot springs are present on and around the Kameni islands, as well as on Thera near Athenios, Cape Exomiti, and Cape Therma (thermal springs at Cape Exomiti are now buried under a new harbor; those at Cape Therma with greatly diminished flow rate and temperature following the 1956 earthquake, according to local residents). High concentrations of helium and CO_2 are present in soils on central Thera and on southern Thera near Cape Exomiti, documenting gas emissions along fault lines presumably from active magma chambers (Barberi and Carapezza 1994; Delibasis *et al.* 1990)".

The publication by F. Barberi and M.L. Carapezza cited above reports the results of a soil gas survey conducted on Thera in 1993 that found several anomalous degassing sites, some along the Kameni and Columbo lines responsible for the historically documented volcanic activity on Thera, but also some related to a gas-leaking fault cutting a geothermal field in southern Thera. H.W. Hubberton *et al.* (1990) report that their tests did not show effects beyond 100 m. from a gas source (but without specifying wind conditions), while acknowledging that general conditions and sources of ^{14}C-depleted carbon at the time of the eruption are unknown. They contend that the scattered radiocarbon dates known prior to the 1990 date of their publication are unlikely to have been affected by the presence of old carbon and hence validate a high date for the eruption, finding possible support for their view in the purported evidence of Theran shards in the Greenland ice core at 1645 ±4 B.C. and the 1628–26 B.C. frost rings in trees, arguments since largely abandoned by proponents of the Aegean Long Chronology as noted above.

Carbon dioxide is 50% heavier than air and, if emitted in sufficient concentration, can easily accumulate in low-lying areas of cereal growth if not dissipated by winds; a few days per year of modest degassing in flat, calm weather is all that is required to affect plants. S. Soter gives the following highly simplified model as illustration:

"The growing season in Greece is about 180 days. Suppose the concentration of volcanic CO_2 near the ground equals that of the normal atmospheric CO_2 during one day in the growing season and is zero for the other 179 days. That brief exposure would provide 0.55%, or 1 part in 181, of volcanic CO_2 to the plants, which would produce an apparent radiocarbon age increment of about 40 years. The pre-industrial atmospheric CO_2 concentration was about 275 parts per million, and doubling it for one day would not be noticed. The amount of CO_2 needed before people even begin to feel ill effects is about 5000 ppm" (pers. comm. of 28 February 2004, for which I am most grateful).

Indeed, a few hours' exposure to ^{14}C-depleted carbon might suffice, for a plant leaf may replace over 5% of its carbon in a single day, as noted above. A plant growing in 1550 B.C. with only 0.25% of its carbon from volcanic CO_2 would have a calibrated date of about 1620 B.C., given the shape of the cali-

bration curve – *i.e.*, a ^{14}C age increment of only twenty years in the critical span results in a calibrated increment of seventy years. Twenty-year differences challenge the best of radiocarbon measurements and the limits of the method. Of course not all ^{14}C determinations suggesting the possibility of an early date for the Theran eruption fall within this twenty-year period.

The sequence of earthquakes and eruptions at Thera in the Late Bronze Age is of particular relevance. At the beginning of Late Cycladic I/Late Minoan I, a massive earthquake struck Thera, the southern Aegean and most of Crete, causing such destruction at Akrotiri that ground floors in some places became basements beneath the leveled rubble (Nikolakopoulou 2003; Warren 1991; Marthari 1984; Palyvou 1984). Earthquakes are likely to cause the release of quantities of ^{14}C-free carbon from both terrestrial and underwater vents in the Hellenic Arc (as the 1992 earthquake at Melos demonstrated). "Earthquake swarms" precede most volcanic eruptions (Heiken and McCoy 1990, 29). Results of recent excavations suggest the occurrence of further earthquakes within LM IA between the initial major event and the Theran eruption (Rehak and Younger 1998, 101). S. Soter notes that "terrestrial outgassing in tectonically active areas is highly time-variable. In the years leading up to the paroxysmal Minoan eruption of Thera, the volcanic region may well have been emitting enormous quantities of 'dead' carbon dioxide" (pers. comm. of 15 May 2003).

The period immediately preceding the great eruption of Thera was marked by "precursor events" in the form of more than one large earthquake, most likely volcanic in origin (Cioni *et al.* 2000; McCoy, pers. comm., for which I am most grateful). The excavators believe that during the year or two before the final eruption, the settlement of Akrotiri suffered for a short time from minor seismic tremors and finally, toward the end, from a major and destructive tremor, causing the populace to abandon their houses and begin a massive reconstruction effort (C. Doumas, pers. comm. of 11 March 2004; Nikolakopoulou 2003). Then, a precursor eruptive phase "some weeks or months prior to the major phases of the eruption deposited a thin tephra layer extending to 8 cm. over southern Thera" (McCoy and Heiken 2000b, 1235). I. Nikolakopoulou (2003) describes a period of "intense seismic activity" for a brief interval before the final eruption, after which there exists evidence for outdoor work including "arrangement of debris and organization of workspaces, collecting and sorting building materials, gathering tools, transport of objects, food supplies and consumption" and indoor work including "abolishing accesses to parts of complexes, abolition of rooms, supports, protection and storing of objects, discarding of objects, repairs and renovations". Nikolakopoulou notes particularly the recovery of sacks and baskets full of barley and concludes that it is now evident that during the rearrangement of the settlement, provisions were made for food and fire. Accordingly, the possibility exists that some of the seeds and twigs found in the Volcanic Destruction Level (VDL) at Akrotiri from which ^{14}C measurements were taken had been grown and collected for those who remained after the precursor events, including not only the clearly identifiable events in the resettlement phase, but also the earthquakes which are believed to have occurred in the preceding decade (McCoy and Heiken 2000b), and which may have been accompanied by an increase in the number of fumaroles and release of old carbon.

Fumaroles send ^{14}C-depleted carbon into the atmosphere where it can be absorbed by the leaves of plants and trees. The soil gas in geothermal areas has elevated concentrations of old CO_2. Plants are known to acquire small amounts of carbon dioxide directly from the soil through their roots (Geisler 1963; Yorgalevitch and Janes 1988; Stolwijk *et al.* 1957; Skok *et al.* 1962; Splittstoesser 1966; Arteca *et al.* 1979). For example, one experiment used labeled $^{14}CO_2$ to demonstrate that eggplant roots are capable of taking up CO_2 from the soil environment and that the carbon so acquired can be translocated to the shoots (Baron and Gorski 1986). However, there seems to be no consensus regarding the magnitude of CO_2 uptake by plant roots. Whether the effect can add significant increments to the radiocarbon ages of plants grown in volcanic areas is yet to be determined. (I am grateful to S. Soter for informing me of the literature regarding root uptake of carbon.)

Further questions, apart from those related to the release of old carbon by gas venting, arise with regard to possible sources of old carbon in samples measured. River water used for washing storage jars to remove insect infestations may produce a "freshwater reservoir" or "hardwater" effect biasing ^{14}C determinations upward (Fischer and Heinmeier 2003, 449). N.-A. Mörner and G. Etiope (2002, 193) note that in the "Tethyan belt [which includes the Mediterranean region], high CO_2 fluxes are related to important crustal formations of . . . carbonate rocks [causing] high levels of CO_2 concentration in ground and groundwater". Finally, they also report that "the Precambrian bedrock includes stromatolites, marble

and other carbonate bearing rocks [which] ... may give rise to the escape of CO_2" (197). The basement rocks of Thera and the other Aegean islands include metamorphosed limestone. The term "cretaceous" is derived from Crete, and one of its definitions is "chalky" (Soter, pers. reminder). Heating of limestone by metamorphism and volcanism generates old carbon dioxide, which can escape into the soil and atmosphere through fissures and faults, especially in connection with seismic activity.

The existence of known and potential sources of old carbon necessarily raises the question of whether there is a significant likelihood that such old carbon will be incorporated by the trees or seeds subsequently measured. S. Manning believes the possibility slight and undemonstrated, contending that it is unlikely that cereals and pulses dated by radiocarbon would have been planted or the trees grown in the vicinity of fumaroles or limestone sources (pers. comm.). The possibility exists, however, that the key seed samples, such as those from the jars found in the destruction deposit in rooms 3c and 5 of the West House and elsewhere, were all collected at the same time from the same location. C. Doumas (pers. comm.) notes that barley is grown today on the small hill across the road from the site of Akrotiri. Instances of old carbon effects extending over distances measured in kilometers are described above.

S. Manning contends in particular, however, that old carbon would not be present in short-lived samples in relatively consistent, limited amounts and hence would not explain ^{14}C determinations putatively supporting a date 50 to 150 years earlier than the date range proposed for the Theran eruption in accordance with the Egyptologically-based Aegean Short Chronology (Manning 1999, 236, and personal discussions for which I am most grateful). Here three caveats may be appropriate: (1) the atmospheric old carbon effect may have a restricted upper limit based on the magnitude and distance of the source if all the seeds tested come from plants growing in one area, with only a few outliers recording greater concentrations; (2) the studies which support the uptake of ^{14}C-depleted carbon through the root system of plants suggest that the uptake may saturate at a low but significant level; (3) a consistent upward bias may be caused by pretreatment which succeeds in removing almost all old superficially adsorbed carbon present but leaves a minute residue, a possibility that cannot be entirely excluded (see note 1). Nevertheless, the Manning argument will require serious consideration if it can be shown that samples from the VDL regularly provide dates earlier than the oscillating portion of the calibration curve, or samples from post-destruction periods, such as LM IB, regularly provide radiocarbon dates earlier than dates consistent with a post c. 1550 B.C. eruption.

The evidence to date, however, does not seem to support such a position. Most determinations fall within the oscillating portion of the calibration curve and are consistent with an eruption date between 1550 and 1525 B.C., or are so marginally higher as to fall easily within the range of measurement variation or error.[4] A threshold question exists regarding which determinations are accepted into the dataset and how these are combined. Biasing results, for example by including in the database a sample with 1% old carbon which adds about 80 years to a measurement while rejecting a sample containing 3% old carbon which adds about 245 years on the ground that the latter is an obvious outlier, must be avoided. Issues presented in the combining of determinations from archaeological samples are considered in Ashmore (1999).[5]

In the course of the attempt to provide dates for the Theran eruption and the major phases of the early Late Bronze Age in the Aegean, many radiocarbon measurements have been obtained in recent years from deposits other than the Theran VDL, for example from early LM/LH I, mature LM/LH I, LM IB/LH IIA and LM II contexts. These measurements have been placed in sequence and compared to the calibration curve, a process recently termed "sequence seriation" or "seriated sequence" analysis in place of "wiggle-matching" (a term now properly reserved for comparisons between series with known

[4] Early studies in particular produced "outliers" both earlier and later than the possible range of dates (Hubberton *et al.* 1990; Biddle and Ralph 1980; Michael 1978; 1980; Weinstein and Betancourt 1978; Weninger 1990). Some of those measurements did not come from short-lived samples or samples clearly associated with the VDL, and many of the samples did not undergo pretreatment of the kind now standard, however.

[5] The preceding pages have dealt in detail with the nature and extent of sources of old carbon and of its proposed paths of transmission to radiocarbon samples in order to redress partial and potentially misleading accounts in the current archaeological literature. Even if no old carbon were present in a sample, the problems posed by regional, inter- and intra-year variation and by the oscillating calibration curve would of course still be present.

intervals, such as decadal measurements of dendrochronological sequences of trees from different areas, species and/or measurement methods/laboratories). A detailed, informative description of the process and its utilization by the widely employed OxCal program is provided by its progenitor, C.B. Ramsey (2001). All such seriation analyses, where the time intervals between the various phases cannot be established independently, are highly dependent statistically on the correctness of the "boundaries" incorporated in the seriation, such as "start LM IA" or "LM IB end". (Potential hazards in the application of Bayesian analysis to radiocarbon dates are considered in Steier and Rom 2000; Wiener 2003.) Establishing the locations (dates) of the boundaries, however, encounters many of the same difficulties as establishing the date of the VDL directly, unlike the situation where a "boundary" date is fixed by dendrochronology or recorded history (*e.g.*, the tephra layer from the eruption of Vesuvius in A.D. 79). For example, attempting to establish an end of LM IB "boundary" on the basis of a set of dates at one site where the relationship in time of the particular deposit to other LM IB destructions is uncertain, other LM IB radiocarbon measurements of destruction levels are later and the INTCAL98 calibration curve at the relevant decade contains an erroneous measurement which could result in the lowering of the date by a decade or more (Wiener 2003, 54), appears to lack a solid basis. (See contra, Manning, this volume; Ramsey *et al.* 2004.) The consequence is historically significant, for the purported "boundary" would remove any overlap between the reign of Tuthmosis III and the LM IB period, a somewhat surprising prospect in light of the archaeological evidence.[6]

In the final analysis, and separate from the problems posed by the possible presence of old carbon and by regional, inter- and intra-year variation, we are left with the hurdle of the oscillating calibration curve of the late seventeenth and sixteenth century B.C. An analysis of radiocarbon measurements from Thera conducted by the VERA Laboratory of the University of Vienna (Kutschera, pers. comm.) focuses on the twenty-five critical Akrotiri samples (after culling from the data bank all samples with a one sigma range greater than 100 years and all those whose destruction layer origin could not be established). The study, which corrects the data presented at the 1998 SCIEM conference, concludes:

"But now we obtain – as a result of the calibration curve – two ranges, the first from 1640 B.C. to 1600 B.C. and the second from 1570 B.C. to 1530 B.C. . . . [T]he two peaks have almost the same size, so none of them has a preference over the other. That means in other words that measuring a lot of new samples from the destruction layer of Thera will not result in finding out which range is the true one. Thus ^{14}C dating – because of the shape of the calibration curve at that time – is not capable to distinguish between high and low chronology for the Thera event".

Similarly, W. Cavanagh, coauthor of the standard text on the *Bayesian Approach to Interpreting Archaeological Data* (Buck *et al.* 1996), in discussing the study of the Eastern Mediterranean radiocarbon data published by Manning *et al.* (2002b) concludes that the analysis ". . . in no way rules out the Aegean Low Chronology" (quoted in Wiener 2003, 391, n. 148). P. Reimer, the lead investigator in the INTCAL04 project, reaches the same conclusion: "It would indeed be difficult to distinguish dates between 1615 and 1525 B.C." (pers. comm. of 8 December 2003).

In the six intervening years since the completion of the VERA Laboratory study cited, many additional ^{14}C measurements have been added to the database at VERA and elsewhere. Studies under way utilizing the additional data will provide important new information and may alter the picture presented by the 1998 study. Unless significant numbers of precise measurements from relevant, rigorously collected, treated and analyzed samples provide dates outside the oscillating decades of the calibration curve,

[6] The site of Chania, where a destruction produced radiocarbon dates possibly earlier than the radiocarbon dates of other LM IB destructions (Manning 2002b; but *cf.* Wiener 2003) contained Alternating Style pottery, sometimes thought to represent a mature phase of LM IB on the evidence of the stratigraphy of one small plot at Kastri on Kythera (Coldstream and Huxley 1972). The Stratigraphic Museum site at Knossos, however, contained Alternating Style pottery throughout the LM IB sequence (Warren 1981). The example of error in decadal calibration measurements cited in connection with the Chania measurements is not unique; for example, in a few instances decadal measurements of trees in Anatolia and in Germany produced radiocarbon ages that were significantly at variance, probably as a result of measurement problems. (Newton and Kuniholm 2004. I am grateful to the authors for allowing me to see their important paper prior to publication; see also Kromer *et al.* 2001, fig. 3.)

however, radiocarbon dating will remain unable to provide convincing evidence of the date of the Theran eruption and the LM IA–IB transition.

Archaeological Evidence Based on Egyptian Chronology

Other papers in this volume present the Egypto-archaeological evidence for a mid-to-late sixteenth-century eruption of Thera. Foremost is the evidence from Cypriote pottery. The VDL at Thera contained the now well-known Cypriote White Slip I (WS I) bowl. While the vessel came from the A.D. 1870 exploration and not the current excavations, Merrillees' careful study concludes that "there can be no doubt about the provenance and stratification of the Cypriote White Slip bowl" (Merrillees 2001, 92). The absence of Cypriote pottery in the material studied to date from the ongoing excavation of Akrotiri is not surprising, given the fact that only six examples of WS I have been reported for the entire Aegean (Merrillees 2001, 98–100). While WS I eating and drinking vessels were attractive practically and aesthetically in the Near East in comparison with local wares, in the Cyclades WS I could not compete with Minoan pottery and its place within the general attraction of Minoan palatial culture of the New Palace Period, or indeed with the excellent Late Cycladic I pottery of Thera itself. The contemporaneity of WS I and the mature Late Minoan I pottery of the Theran VDL is also evident in the material excavated at Toumba tou Skourou and Ayia Irini on the northwest coast of Cyprus (Wiener 2003, 367 and sources cited therein). Of course the creation of the bowl in Cyprus could predate its destruction in the Theran eruption by many years, and in fact the bowl showed evidence of use and ancient repair.

The earliest certain appearance of WS I pottery in Egypt and the Near East comes in the Tuthmoside era, not before c. 1500 B.C., with the possible exception of WS I sherds found at Tell el-ᶜAjjul whose context, while somewhat uncertain, makes them potential candidates for an earlier arrival (Bergoffen 2001; Wiener 2003, 369). However, even if (1) WS I bowls of the type recovered from the VDL at Thera existed for two generations, or sixty years, before their first stratified appearance to date at Tell el-Dabᶜa, around 1500 B.C.; (2) one of the first examples produced arrived in Thera; (3) the use, repair in antiquity and destruction of the bowl in the Theran eruption all occurred within a decade and (4) there is some overlap between early stages of WS I production and Proto White Slip (which is stratified at Tell el-Dabᶜa in the final Hyksos stratum), then the date of the eruption would still move no earlier than 1550 B.C. Finally, it is important to recognize that the challenge to a seventeenth or early sixteenth century B.C. date for the Theran eruption posed by the contexts of Cypriote pottery is not limited to the position of WS I, but encompasses a sequence including Cypriote White Painted VI, Proto White Slip, Basering I and Red Lustrous Wheel-made wares, consisting of thousands of sherds from Tell el-Dabᶜa in Egypt, Tell el-ᶜAjjul in Canaan and various sites throughout the Eastern Mediterranean. The Tell el-ᶜAjjul sequence is notable for the numbers of Cypriote sherds of each phase of the sequence present. (See, for example, Bietak 2001; 2003a; Bietak and Hein 2001; Fischer 2003; Fischer and Sadeq 2002; Oren 1997; 2001; Bergoffen 2001; Wiener 2003, 369).

S. Manning has argued forcefully that the delay of at least a century between the arrival of a WS I bowl at Thera and its first documented appearance elsewhere could result from regionalism in Cyprus during Late Cypriote IA and from exclusive exchange relationships linking the major Cypriote southeast coast site of Enkomi with places in Egypt and the Near East including Tell el-Dabᶜa (Manning *et al.* 2002c). Manning regards the bowl from Thera as an early example of WS I produced on the northwest coast of Cyprus, an example of a type of pottery which does not reach Enkomi in any quantity until several generations later (Manning 1999, 119–129; see contra, the extended discussions in Bietak 2003b and Wiener 2003). Other Cypriote pottery specialists believe the Theran bowl comes from the west of Cyprus or the south coast (Popham 2001, 217; Merrillees 2001, 93; Karageorghis 1990, 57, n. 28; Bietak 2003a, 26–27) and, moreover, that it is not particularly early. (For the position of the decoration of the Theran bowl in relation to the Tell el-ᶜAjjul sequence, compare in the same volume the illustration in Merrillees 2001, 91 with Bergoffen 2001, 153, placing the decorative pattern of the Theran bowl about one-third of the way through the WS I sequence.) Merrillees (2001) suggests that the bowl belongs in the LC IA–IB transition. L. Crewe in her dissertation and article in this volume examining the limited surviving records of the Enkomi excavations concurs with S. Manning that the examples of WS I whose contexts there can be firmly identified are probably from late LC IB rather than LC IA deposits and hence later than the first appearance of WS I in the west of Cyprus. A delay of 100 or more years between the time a WS I bowl reaches Thera and the time the ware reaches the Near East and Egypt

appears unlikely, however, particularly in light of the fact that the putative delay in question must also somehow affect all other wares in the sequence, including Proto White Slip, as noted above (discussion in Bietak 2003a; Bietak and Hein 2001; Wiener 2001; 2003; see contra, Manning *et al.* 2002c; Manning, this volume).[7]

In addition to the pottery evidence, the contexts of waterborne pumice from the Theran eruption found in Egypt and the Near East have been considered as evidence relating to the date of the eruption. At Tell el-Dab^{c}a Theran pumice in large quantities was found at five locations, three of which the excavator believes were workshops active during the reign of Tuthmosis III and not abandoned until late in his reign or the following reign of Amenophis II, in any event after c. 1450 B.C. (Bietak *et al.* 2001, 37, 89, 91; Bichler *et al.* 2003; 2002; Warren 1996, 287–288, *cf.* below). Pumice from the Theran eruption has also been found in Eighteenth Dynasty, likely post-1525 B.C., contexts at Tell el-^{c}Ajjul and Tell Nami in Canaan (Bichler *et al.* 2003; 2002; Fischer 2003; Fischer and Sadeq 2002; Manning 1999, 145–150). Of course waterborne pumice could have been collected from the Nile Delta/Mediterranean Sea or imported as an abrasive at any time between the eruption and the date of use of the pumice. An eruption c. 1525 B.C., at the latest point in the indicated radiocarbon calibration range for the eruption, would mean a delay of two to three generations between the eruption and the date proposed for the abandonment of the Tell el-Dab^{c}a workshops containing Theran pumice, whereas an eruption c. 1600 B.C. would require a delay of five to six generations. A difference of this nature would not be of great significance to the chronological debate. It is worth noting, however, that in one context, Workshop N in area H/I, the pumice may appear as early as the reign of Tuthmosis I, around 1500 B.C. (Bietak *et al.* 2001). Accordingly it is possible that Theran pumice was first used at Tell el-Dab^{c}a within a generation of a putative c. 1525 B.C. eruption, and continued to be collected or imported for two generations thereafter.

At Tell el-^{c}Ajjul, forty-eight samples of pumice from the Theran eruption were collected from various strata, beginning with a stratum containing material from the early Eighteenth Dynasty and Cypriote pottery of the Late Cypriote I A2/I B period, no earlier than c. 1540 B.C. and likely post-dating the conquest of Tell el-^{c}Ajjul by Ahmose after 1525 B.C. This stratum and the one following contain examples of Cypriote White Slip I, Base-ring I and Red Lustrous Wheel-made wares, together with most of the pumice samples. No samples of Theran pumice have been found in the earlier Hyksos period strata at Tell el-^{c}Ajjul. A small number of samples, however, were found in strata extending into the Iron Age (Fischer 2003, 289–290). Other sites provide examples of Theran pumice in use covering more than a millennium, from LH II through the Hellenistic period (Wiener in Wiener and Allen 1998, 25–27), but only in small amounts, not large quantities such as that found at Dab^{c}a. Accordingly, while it is possible that the Theran eruption occurred much earlier than the date of the strata in which the Theran pumice at various places has been found, with the pumice collected or imported at the date of use, the fact that Theran pumice has yet to be found in pre-Eighteenth Dynasty strata remains significant, though clearly not conclusive.

Summation

The difficulties faced by archaeologists and prehistorians in following the ongoing discussions of Egyptian astronomy, confronting the purported ice-core and tree-ring evidence for a 1650 to 1628 B.C. date for the Theran eruption and understanding the nature and complexity of the problematic radiocarbon/statistical argument for the Aegean Long Chronology, on the one hand, and the reciprocal perplexity sometimes expressed by physical scientists concerning the textual plus archaeological evidence for the Aegean Short Chronology, on the other hand, indicate that the problem of the division between the "two cultures" is still with us. Even specialists in chronology who follow closely all developments hold differing positions, however. Those who regard the Egypto-archaeological evidence and in particular the generally prevailing Cypriote pottery analysis as unpersua-

[7] Of course the archaeological problems caused by the proposed Aegean Long Chronology extend beyond the Cypriote pottery sequence. The earlier the date proposed for the mature LM IA eruption of Thera, the greater the difficulties, including the required compression of both the MM III period in Crete and the Hyksos period in Egypt and the Egyptian contexts of Middle Minoan imports (see, *e.g.*, Warren 1984; Bietak *et al.* 2002; Wiener 2003). Similarly, the concomitant required extension of the LH I period of Mycenae from three to five or more generations poses difficulties in terms of, for example, the continuity of hands believed present in the metalwork through the sequence of burials (Graziado 1991; Dickinson 1977, 50–51; Kilian-Dirlmeier 1986).

sive, and the radiocarbon evidence for a Theran eruption date significantly earlier than 1550 B.C. as substantial, favor the Aegean Long Chronology, even though deprived of support from ice-core or tree-ring evidence. Conversely, those who believe the radiocarbon evidence unpersuasive because of some or all of the areas of uncertainty noted in this paper, and the Egypto-archaeological arguments for an Eighteenth Dynasty eruption date compelling, naturally favor the Aegean Short Chronology. At the least, the papers from this conference mark a major step forward in clarifying the issues.

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Distinction of Pre-Minoan Pumice from Santorini, Greece

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Abstract

The pumiceous products of the large scale explosive eruptions at Santorini like the Lower Pumice eruptions 1 and 2 about 200 000 years ago and the "Minoan eruption" show a high grade of similarity as well in their optical appearance as in their composition. The demand for a clear classification of Santorinian eruption products raised from archaeological research, where the widely used abrasive pumice can be interpreted as a "post-eruption" time marker. The aim of this work was to find elements that underwent significant changes during the geochemical evolution of the volcano and therefore could be indicative for a distinction of pumice produced by the five major explosive eruptions (Lower Pumices 1 and 2, Middle Tuff sequence, Cape Riva Tuff and the Minoan Tuff). INAA of 25 elements, in particular As, Ba, Ce, Co, Cr, Cs, Eu, Fe, Hf, K, La, Lu, Na, Nd, Rb, Sb, Sc, Sm, Ta, Tb, Th, U, Yb, Zn, and Zr allows a clear classification and contributes new information on the chemical evolution of Santorini volcano.

Introduction

The use of pumice as a time marker for post-eruption-dating in archaeology led to the demand for a clear classification of Santorinian eruption products. This problem became evident as the pumiceous products of some of the lare scale eruptions like the

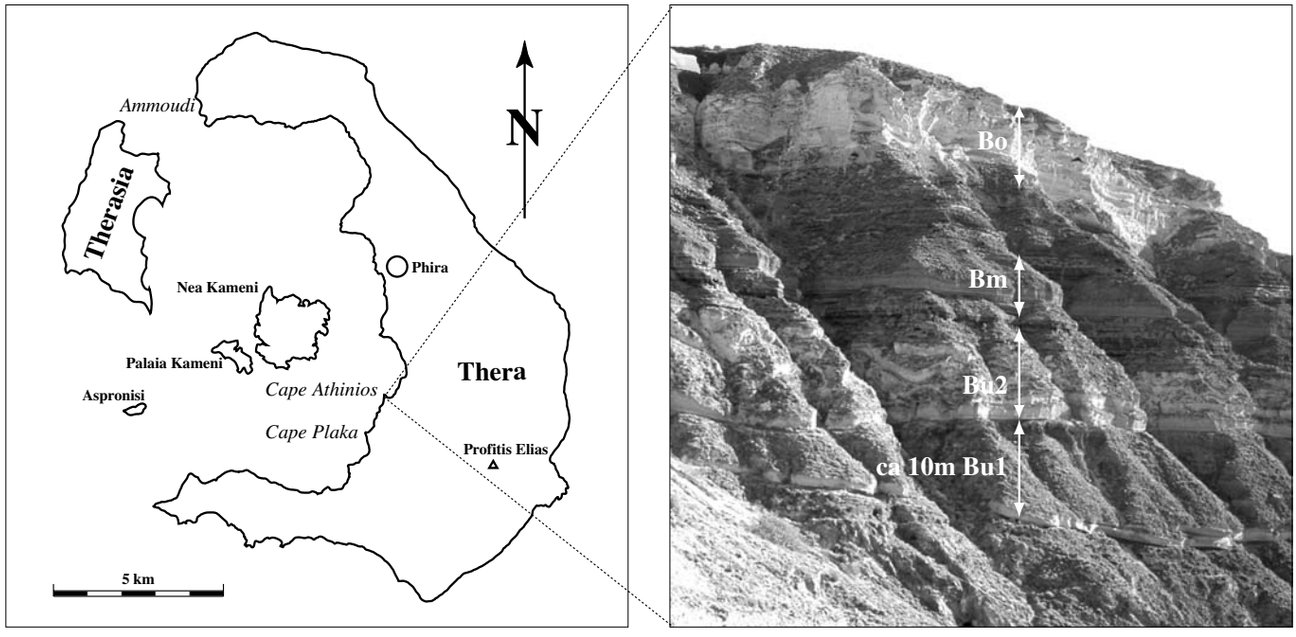


Fig. 1 Santorini volcanic complex, geographic situation of the sampling sites and a typical volcanic sequence near Athinios: Lower Pumice Tuff units 1 and 2 (Bu1, Bu2), Middle Tuff (Bm) and Minoan Tuff (Upper Pumice, Bo). The Cape Riva Tuff is not visible in that part of the caldera cliff

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Lower pumice 1 and 2 eruptions and the Minoan eruption show a high grade of similarity as well in their optical appearance as in their chemical composition. Instrumental neutron activation analysis (INAA) already has been applied successfully to distinguish Minoan eruption products from material produced by other volcanoes in the Aegean archipelago – such as Milos, Kos, Giali, and Nisyros.[1] The least compositional differences were found in material from Santorini itself, in particular the Lower Pumice Tuff eruptions 1 and 2 ("unterer Bimsstein" Bu1 and Bu2, 203 000 ± 24 000 years and ca. 180 000 years), the Middle Tuff ("mittlerer Bimsstein"; Bm, ca. 100 000 years), and the Cape Riva pumice (ca. 21 000 years). The investigation of the composition of pumice produced during these large scale explosive events revealed not only crucial differences but also a geochemical evolution that has not been reported during years of intensive volcanologic research at Santorini. Sample series from the eruption cycles mentioned above were collected at the caldera cliffs at Cape Plaka, Cape Athinios, and Ammoudi. The geographical situation and a view of the typical appearance of the volcanic sequence is shown in Fig. 1.

Samples and analytical techniques

The samples were taken during a field trip to the island of Thera in March 2002. From literature and previous geochemical investigations the optimized sampling sites were chosen and samples of about 1 kg were taken after removing the weathered surfaces to a depth of about 10 centimeters.[2] For a detailed description of the sampling sites see Duma.[3]

Pumice and pumice dust underwent a thorough cleaning by submersion in aqua destillata and application of an ultrasonic bath. By sonicating the samples for several minutes, impurities (e.g. remnants of plants, microorganisms or soil particles etc.) were removed thoroughly. For this procedure the samples were transferred into a beaker containing distilled water. Due to the low density of pumice it is necessary to place a second beaker with a smaller diameter above the samples to make sure that all parts are submerged totally. After the sonicating was finished, the pumice pieces were removed, rinsed with fresh distilled water and dried at 110°C for at least 24 h. For INAA, about 100 g of pure, xenolith-free pumice were used. The samples were ground in an agate mortar to a grain size <3 µm and dried to constant weight at 110°C. To avoid contamination of the samples, every step of the sample preparation was done using exclusively polyethylene and quartz-glass tools. Traces of these materials would not interfere with INAA as the neutron irradiation of their constituents C, Si, O and H produces only very short-lived or practically no gamma-emitting activation products.

Then ~150 mg were weighed into Suprasil™ quartz glass irradiation vials. The samples were irradiated together with international certified reference material for 100 hours in the KFKI-reactor of the Atomic Energy Research Institute, Budapest, Hungary, at a thermal neutron flux of about 10^{13} $cm^{-2}s^{-1}$. The multielement standards used for the quantitative analysis were the CANMET reference soil SO1, NIST SRM 1633b Coal fly ash, light sandy soil BCR No. 142, and MC rhyolite GBW 07113. The sample vials were decontaminated and packed into polyethylene vials fitting the automatic sample changer facilities of the Atominstitut. After a decay time of 7 days, a first gamma-spectrum was measured to obtain the activities of the short and medium-lived activation products. Three weeks later, a second measurement was started to detect the long-lived activation products. The measuring times were 1 800 s and 10 000 s, respectively. All samples were measured using an automatic sample changer with a fixed measurement position at a distance of 4 cm beside the detector. The whole gammaspectroscopic analysis was performed with a 151 cm^3 HPGe-detector (1,79 keV resolution at the 1332 keV ^{60}Co peak; 50,1% rel. eff.), connected to a PC-based multichannel analyser with preloaded filter and loss free counting system.[4]

Results

The results have been published recently and are presented in Table 2 and agree well with previously published analyses.[5] Slight but reproducible deviations are observed for the elements La and Ce, where the reported values obtained by XRF (for X-Ray-Fluo-

[1] Peltz *et al.*1999.

[2] Druitt *et al.* 1999; Vitaliano *et al.* 1978; Vitaliano *et al.* 1990.

[3] Duma 2002.

[4] Westphal 1982, Westphal 1995.

[5] Peltz *et al.* 1999; Druitt *et al.* 1999; Vitaliano *et al.* 1978; Vitaliano *et al.* 1990; Eastwood *et al.* 1999; Pearce *et al.* 1999; Bichler *et al.* 2007.

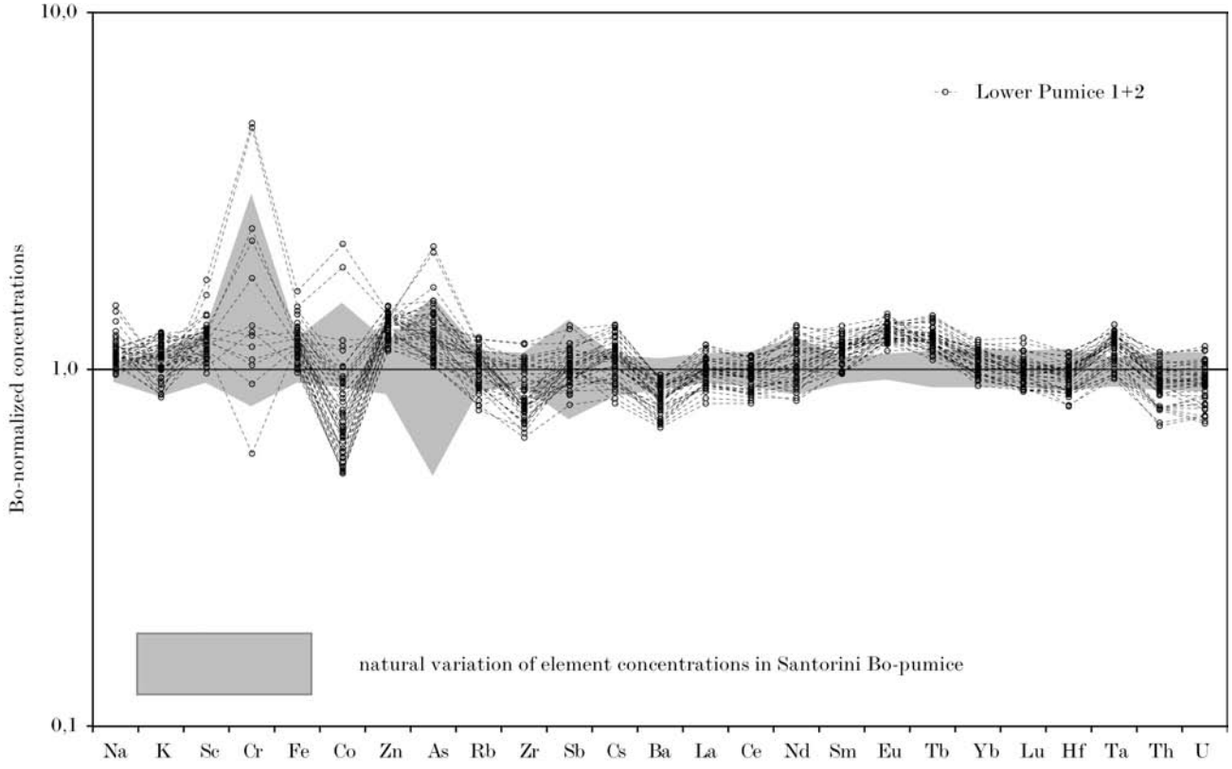


Fig. 2a Santorini Bo-normalized element abundance pattern of pumice samples from the Santorini Lower Pumice Tuff units 1 and 2 (Bu1, Bu2). The natural variation of the Minoan Tuff (Bo) is inserted as shaded area

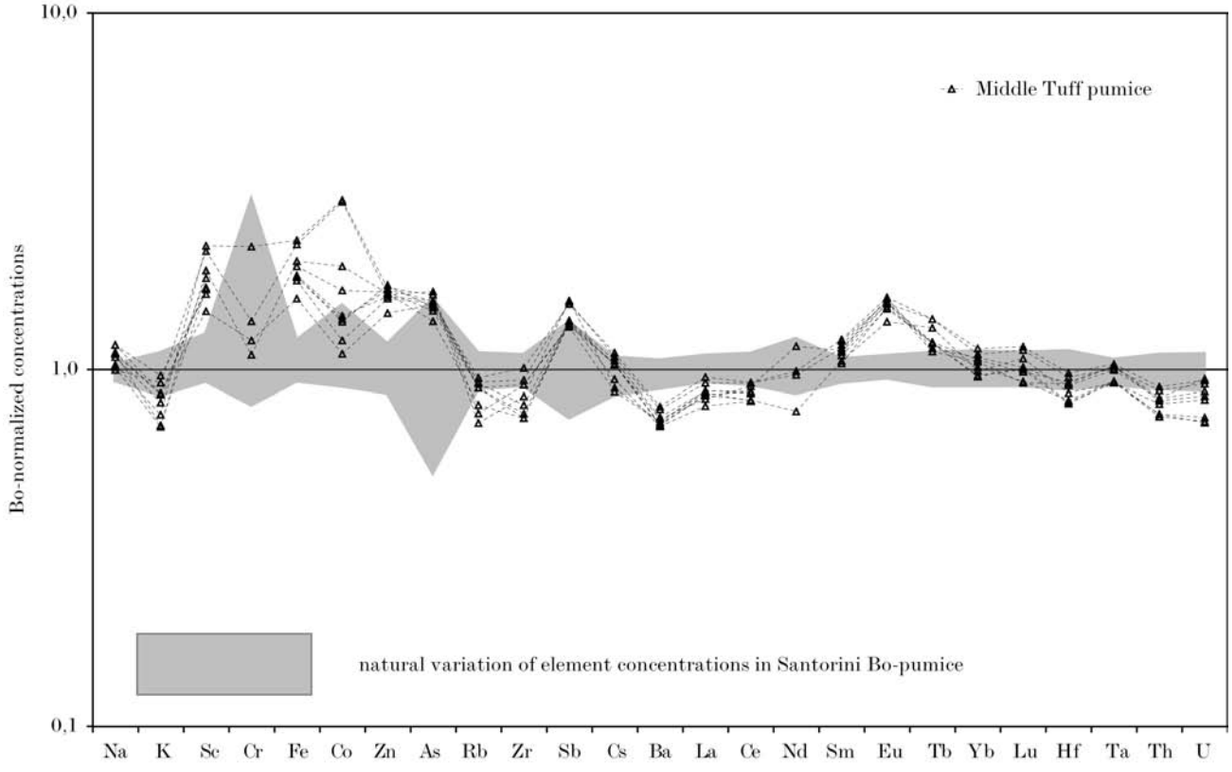


Fig. 2b Santorini Bo-normalized element abundance pattern of pumice samples from the Santorini Middle Tuff sequence (Bm). The shaded area shows the natural variation of the Minoan Tuff (Bo)

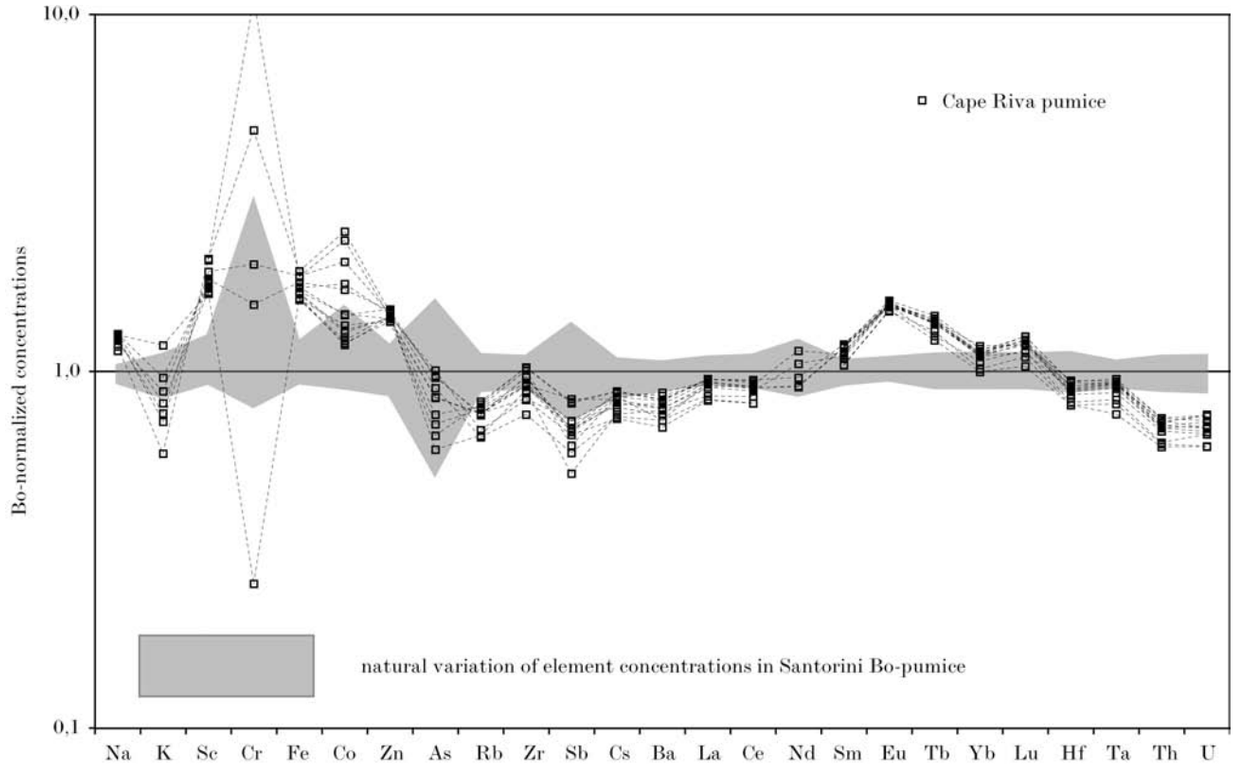


Fig. 2c Santorini Bo-normalized element abundance pattern of pumice samples from the Santorini Cape Riva Tuff. The shaded area gives the natural variation of the "Minoan" Tuff (Bo)

rescence Analysis) or ICP-MS (for Inductive Coupled Plasma – Mass Spectrometry) are about 10–20%rel lower than the INAA results. However, other INAA data on "Minoan pumice" are in perfect agreement with this work.[6] Blank values for the Suprasil™ quartz glass irradiation vials were measured and found negligible. Errors due to counting statistics are calculated by ± 1σ and range between 5 to 10% rel.

Discussion

The primary aim of this work was to find reliable arguments for a distinction of pumices from the different Santorinian eruption cycles. Instead of applying statistical analysis, it has been necessary to find a set of the most suitable elements, as statistic programs usually cannot include element properties such as volatility or solubility in hydrothermally active environments. The elements should meet the following requirements: different concentrations in the deposits in question, homogeneous distribution throughout the deposit, low solubility and volatility during weathering, and, last but not least, nuclear properties that are suitable for INAA. A comparison of the element distribution patterns is given in Fig. 2. A normalization is applied in order to exclude the atomic number depending odd-even effect due to nuclear stability. Elements with an even number of protons (Z) are typically more abundant than those with an odd number. Exceptions are hydrogen (Z = 1) and Be (Z = 4). The reason for this effect is that our terrestrial element inventary has been produced by stellar burning and supernovae. During such processes nuclear properties dominate the production rates of elements. One of these properties is that nuclei are especially stable if nucleons can "pair up" with their spins in opposite directions. This works for neutrons as well as for protons, but if Z is even, the protons can pair up, while if Z is odd, there must always be an unpaired proton. This lead to the obsereved abundance alternation which therefore is usually called the odd-even effect. Additionally, the normalization to the mean concentrations of the respective ele-

[6] Warren and Puchelt 1999; Bichler *et al.* 1997.

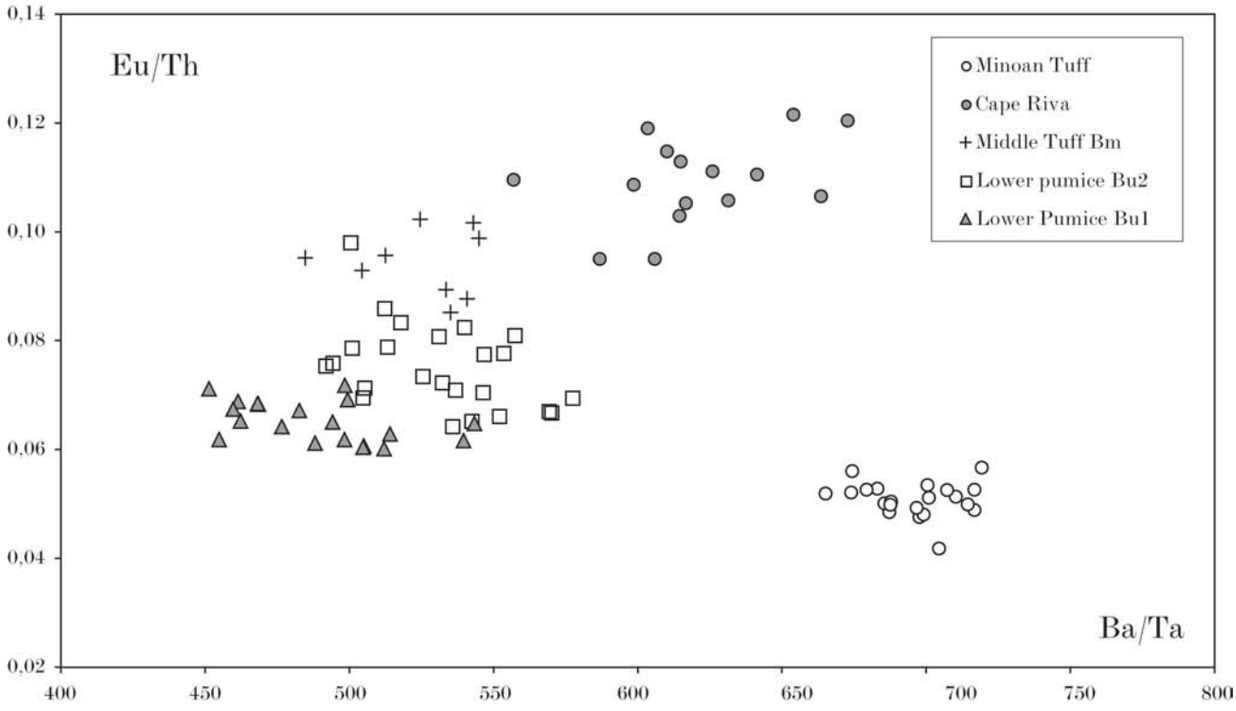


Fig. 3a Concentration ratios of Eu/Th versus Ba/Ta for the distinction of Minoan pumice

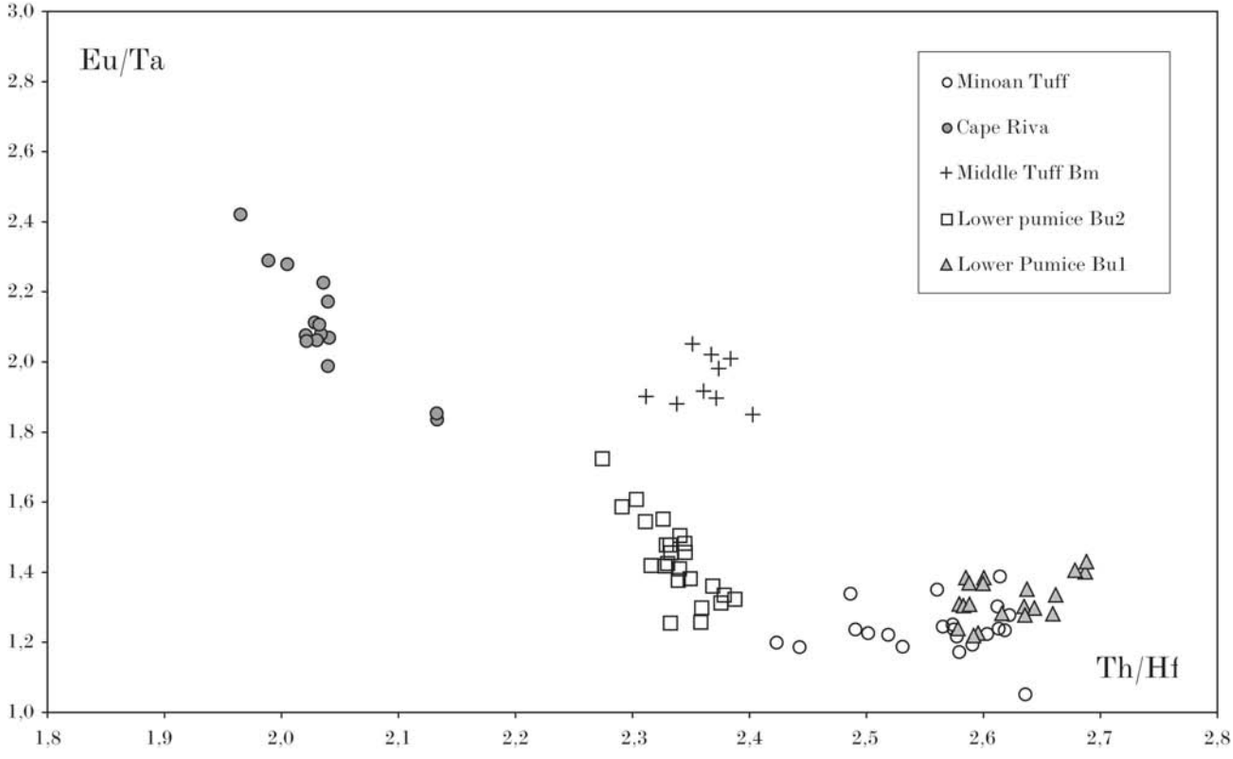


Fig. 3b Concentration ratios of Eu/Ta versus Th/Hf for the differentiation of Lower pumices Bu1 and Bu2, Middle Tuff Bm and Cape Riva pumice

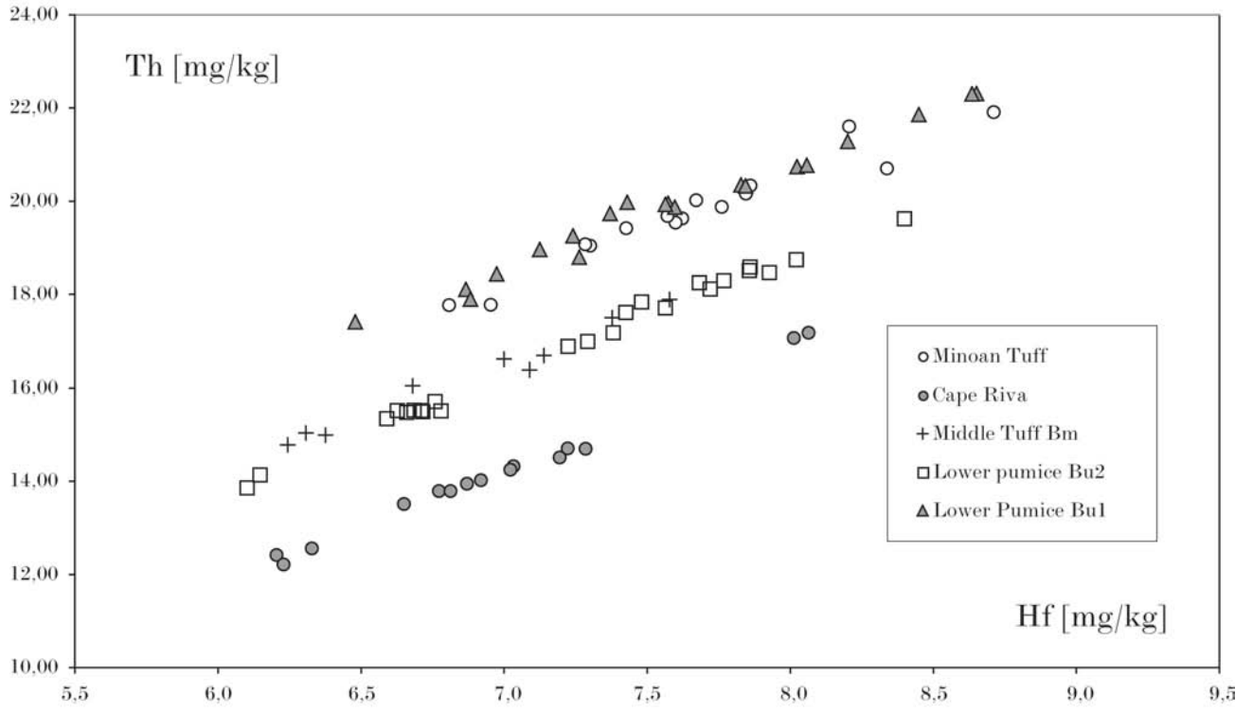


Fig.4 Concentrations of Th and Hf in pumice from Santorini

element	activation product	half-life	γ-energy [keV]
As	^{76}As	25,87 h	559
Ba	^{131}Ba	11,50 d	496
Ce	^{141}Ce	32,50 d	145
Co	^{60}Co	5,27 a	1173
Cr	^{51}Cr	27,70 d	320
Cs	^{134}Cs	2,07 a	796
Eu	^{152}Eu	13,50 a	1408
Fe	^{59}Fe	44,50 d	1099
Hf	^{181}Hf	42,40 d	482
K	^{42}K	12,40 h	1525
La	^{140}La	40,30 h	1596
Lu	^{177}Lu	6,70 d	208
Na	^{24}Na	15,00 h	2754
Nd	^{147}Nd	11,00 d	531
Rb	^{86}Rb	18,60 d	1077
Sb	^{124}Sb	60,20 d	1691
Sc	^{46}Sc	83,80 d	1121
Sm	^{153}Sm	46,30 h	103
Ta	^{182}Ta	114,40 d	1221
Tb	^{160}Tb	72,30 d	879
Th	^{233}Pa #	27,00 d	312
U	^{239}Np #	56,60 h	278
Yb	^{169}Yb	32,00 d	177
Zn	^{65}Zn	244,30 d	1116
Zr	^{95}Zr	64,00 d	757

… ^{233}Pa and ^{239}Np are produced by the b-decay of ^{233}Th ($T_{1/2}$ = 22.3 min) and ^{239}U ($T_{1/2}$ = 23.5 min), which were formed by neutron capture of ^{232}Th and ^{238}U, respectively

Table 1 Activation products, half-lives, and gamma-energies used for quantitative analysis

ments in Minoan pumice (Bo) greatly improves the visibility of compositional differences. It can be seen, that only a few elements such as Sc, Fe, Sb, Ba, Eu, Hf, Ta and Th show differences acceptable for a distinction. These elements are highly suitable for INAA, but some of them are not very significant as their concentration could easily be influenced by mobilization like sulfide decomposition or contamination from infiltrating solutions under weathering conditions. The best differentiation is obtained by using Ba, Eu, Hf, Ta and Th. Fig. 3a. shows the binary plot of the ratios of Eu/Th against Ba/Ta, which allows a clear distinction of the Minoan Tuff from the other eruptiva. The plot Eu/Ta versus Th/Hf separates the rest of the Theran deposits investigated (Fig. 3b). In agreement with their geochemical properties, a perfect correlation was found for the elements Hf and Th. Figure 4 shows a binary plot of their concentrations and there are three clearly separated distributions in the five eruptive cycles observed. The correlation coefficients are 0,987 (Bu1), 0,997 (Bu2), 0,982 (Bm), 0,996 (Cape Riva), and 0,902 (Bo). These extremely high values indicate geochemical processes like melt differentiation and magma mixing. This results are in perfect agreement with petrogenetical assumptions published in Druitt (1999) and are discussed in another publication.[7]

Conclusions

The chemical evolution of the volcanic products of Santorini with respect to the major explosive cycles led to only minor changes in their composition. The determination of 25 elements by INAA revealed differences that can be used for geochemical interpretation. Additionally, this allows the identification of these products if they are found beyond their natural environment. The choice of a set of suitable elements such as Ba, Eu, Hf, Ta, Th and the application of INAA allows a clear classification and can be used for the identification of pumice. This possibility contributes valuable information for the intensive discussion on the date of the Minoan eruption at Santorini by classifying archaeologically stratified findings of pumice. The presence of a certain material gives evidence for the decision that a stratum has been deposited after the respective eruption. The applicability to archaeological findings is demonstrated in this volume.[8]

[7] Bichler *et al.* 2007.

[8] Bichler *et al.* 2004

Table 2: Analytical results of elemental concentrations in pre-Minoan pumice from Santorini, all values in mg/kg
The analytical error due to counting statistics is less than 10 rel.% for the elements Cr, As, Nd, Sm, Yb, and U, less than 5 rel.% for all others

	sample	Na	K	Sc	Cr	Fe	Co	Zn	As	Rb	Zr	Sb	Cs	Ba	La	Ce	Nd	Sm	Eu	Tb	Yb	Lu	Hf	Ta	Th	U
Serie Athinios																										
	Bu1																									
	Sat 30b-1	32401	28448	12,23	n.d.	31823	4,01	104	3,8	117	236	0,30	3,27	451	34,0	66,4	31,9	8,18	1,40	1,27	5,36	0,79	7,37	0,999	19,74	5,65
	Sat 30b-2	33745	26760	12,12	n.d.	31119	3,76	100	3,3	118	229	0,27	3,38	500	32,6	66,5	31,0	7,27	1,43	1,17	5,39	0,83	7,43	1,002	19,97	5,69
	Sat 30c-1	33279	24186	10,71	n.d.	28113	3,59	93	4,3	113	221	0,28	3,23	461	31,3	59,8	26,8	6,88	1,24	1,14	4,81	0,72	6,97	0,955	18,44	5,61
	Sat 30c-2	32166	26514	11,12	n.d.	28580	3,66	95	4,2	118	213	0,27	3,31	490	32,1	61,9	28,3	6,97	1,31	1,18	5,09	0,78	7,12	0,982	18,96	5,27
	Sat 30c-3	35164	28762	10,97	n.d.	28237	3,51	91	4,0	115	209	0,28	3,29	460	32,1	60,3	26,2	7,22	1,29	1,16	4,88	0,76	7,26	0,983	18,79	5,42
	Sat 30c-4	32784	26486	11,43	n.d.	29446	3,71	96	3,3	120	219	0,30	3,36	466	31,8	63,0	32,8	7,11	1,30	1,20	5,19	0,81	7,24	1,013	19,26	5,71
	Sat 30d-1	35260	28587	10,92	n.d.	27005	2,62	99	3,6	133	263	0,28	3,67	535	33,7	68,9	34,1	7,66	1,32	1,27	5,75	0,86	8,20	1,073	21,28	5,96
	Sat 30d-2	35085	30101	10,38	2,0	25799	2,63	97	5,9	125	238	0,30	3,59	501	33,5	64,5	28,3	7,36	1,27	1,29	5,49	0,83	8,06	1,026	20,77	6,21
	Sat 30d-3	36100	30648	10,23	2,1	25384	2,55	95	6,1	127	233	0,35	3,55	509	33,8	63,4	32,3	7,26	1,23	1,23	5,32	0,79	7,84	1,008	20,33	6,13
	Sat 30d-4	36184	24391	10,56	n.d.	26236	2,73	95	4,7	122	224	0,32	3,45	501	32,3	63,1	29,3	7,13	1,30	1,25	5,31	0,81	7,56	1,014	19,93	6,26
	Sat 33-1	36212	30449	11,21	2,5	27743	2,81	98	3,9	122	315	0,34	3,69	528	37,1	65,8	28,4	7,81	1,34	1,37	5,65	0,85	8,02	0,971	20,74	6,30
	Sat 33-2	33623	27619	11,04	n.d.	26585	2,71	92	3,7	132	343	0,39	3,86	522	35,7	68,5	30,3	7,75	1,35	1,41	6,12	1,02	8,65	1,034	22,31	6,67
	Sat 33-3	36797	24672	10,93	n.d.	26139	2,47	95	3,2	131	341	0,35	3,84	527	37,2	68,0	30,5	7,93	1,34	1,43	5,93	0,98	8,63	1,029	22,30	6,69
	Sat 33-4	36837	29385	10,88	n.d.	26696	2,94	95	4,0	128	307	0,34	3,78	531	36,3	67,9	33,7	7,92	1,35	1,38	5,83	0,93	8,45	0,984	21,86	6,86
	Bu2																									
	Sat 31a-1	45393	23077	9,30	n.d.	24079	2,57	94	3,4	96	193	0,29	2,65	442	28,5	54,3	30,6	6,41	1,22	1,14	4,91	0,73	6,66	0,862	15,49	5,00
	Sat 31a-2	40798	27117	10,07	2,6	26147	3,61	98	4,1	98	203	0,32	2,78	480	29,5	56,1	23,7	6,66	1,27	1,20	5,13	0,76	6,76	0,862	15,73	4,71
	Sat 31a-3	42549	20658	9,77	1,1	25859	2,67	104	3,5	106	225	0,30	2,90	484	30,0	60,4	28,9	6,83	1,41	1,18	5,51	0,80	7,22	0,936	16,90	4,97
	Sat 31a-4	48348	24746	9,70	1,8	25257	2,97	97	3,5	97	207	0,29	2,61	449	27,9	55,4	26,0	6,36	1,26	1,14	4,94	0,73	6,62	0,846	15,53	4,23
	Sat 31b-1	38498	26159	10,59	2,3	26636	4,45	90	2,9	98	205	0,28	2,63	410	28,9	53,3	23,5	6,72	1,18	1,10	4,97	0,73	6,71	0,830	15,53	4,92
	Sat 31b-2	37760	25798	10,27	3,6	25534	3,98	105	4,3	112	222	0,40	2,97	471	31,1	59,2	25,4	7,16	1,23	1,22	5,46	0,83	7,42	0,934	17,63	5,45
	Sat 31b-3	34675	20634	13,86	9,4	32714	7,60	99	3,5	83	198	0,31	2,32	387	25,5	50,5	n.d.	6,00	1,22	1,08	4,73	0,74	6,14	0,756	14,15	4,39
	Sat 31b-4	34205	20610	15,27	9,7	36142	8,84	100	2,9	86	187	0,24	2,40	394	26,3	51,5	n.d.	6,42	1,36	1,11	5,04	0,81	6,10	0,788	13,87	4,35
	Sat 31c-1	36717	29965	8,72	2,5	21977	2,36	97	3,4	122	226	0,31	3,18	507	31,1	61,8	26,2	7,26	1,19	1,23	5,66	0,79	7,92	0,946	18,48	5,45
	Sat 31c-2	34890	26822	8,95	n.d.	22150	2,05	93	2,9	113	241	0,33	3,10	515	30,6	60,5	22,9	7,28	1,20	1,23	5,52	0,79	7,77	0,949	18,31	5,35
	Sat 31d-1	35508	26436	8,91	n.d.	22502	2,10	90	3,6	117	227	0,29	3,13	544	31,7	62,3	29,4	6,95	1,24	1,25	5,76	0,83	7,85	0,956	18,53	5,52
	Sat 31d-2	38981	24450	9,82	n.d.	25256	2,31	98	3,1	107	228	0,28	2,97	460	31,1	59,9	30,4	7,10	1,34	1,21	5,52	0,86	7,29	0,919	17,00	5,39
	Sat 31e-1	38140	30638	9,19	n.d.	23480	2,31	94	3,6	115	234	0,33	3,13	521	31,3	63,5	25,1	6,93	1,32	1,24	5,84	0,83	7,86	0,970	18,60	5,62
	Sat 31f-1	38191	30855	9,12	n.d.	22844	2,01	86	3,9	117	303	0,33	3,31	543	34,2	64,7	28,2	7,48	1,31	1,40	6,03	0,94	8,40	0,952	19,64	6,03
	Sat 31f-2	41422	24511	9,42	n.d.	23731	2,24	84	3,5	114	293	0,34	3,19	534	33,8	62,3	25,7	7,34	1,30	1,33	5,77	0,94	8,02	0,924	18,76	5,82
	Bm																									
	Sat 32a-1	39010	23403	13,97	n.d.	38823	4,75	110	4,3	103	293	0,46	3,22	445	30,2	58,0	25,4	7,51	1,60	1,41	5,80	0,96	7,58	0,834	17,89	5,52
	Sat 32a-2	36793	20730	14,46	n.d.	39741	5,36	120	4,1	100	271	0,46	3,15	437	29,1	57,8	24,8	7,10	1,53	1,40	5,59	0,94	7,38	0,809	17,50	5,40
	Sat 34-1	33227	22336	18,43	2,7	49070	11,62	113	4,6	82	263	0,39	2,52	404	27,4	51,6	n.d.	6,44	1,50	1,20	4,88	0,83	6,24	0,743	14,78	4,25
	Sat 34-2	34309	18227	19,10	4,4	50334	11,76	117	4,5	77	244	0,47	2,58	403	27,8	54,1	n.d.	6,77	1,49	1,21	4,83	0,84	6,31	0,740	15,04	4,35
Bo mean composition																										
		33281	24287	8,58	2,0	21839	3,93	69	2,8	108	290	0,30	2,89	564	31,7	63,0	25,7	6,18	1,00	1,01	5,06	0,83	7,75	0,804	19,97	5,91

Table 2 continued: Analytical results of elemental concentrations in pre-Minoan pumice from Santorini, all values in mg/kg
The analytical error due to counting statistics is less than 10 rel.% for the elements Cr, As, Nd, Sm, Yb, and U, less than 5 rel.% for all others

	sample	Na	K	Sc	Cr	Fe	Co	Zn	As	Rb	Zr	Sb	Cs	Ba	La	Ce	Nd	Sm	Eu	Tb	Yb	Lu	Hf	Ta	Th	U
Serie Cape Plaka																										
	Bu1																									
	Sat 23a-1	36179	24508	11,43	n.d.	28747	3,36	86	3,2	119	289	0,25	3,33	495	33,7	64,1	27,3	7,39	1,25	1,26	5,15	0,86	7,57	0,963	19,96	5,78
	Sat 23b-1	39896	20280	10,80	n.d.	26591	3,26	81	2,9	96	237	0,27	2,72	398	29,1	53,4	22,5	6,39	1,19	1,17	4,55	0,73	6,48	0,849	17,41	4,55
	Sat 23b-2	36830	22461	10,38	n.d.	26792	3,19	84	3,1	99	255	0,27	2,74	404	29,2	54,7	22,9	6,07	1,18	1,16	4,74	0,76	6,87	0,874	18,11	4,70
	Sat 23b-3	35753	23008	10,84	n.d.	27434	3,35	83	3,0	101	254	0,26	2,85	411	30,9	55,2	24,4	6,63	1,23	1,20	4,70	0,74	6,88	0,890	17,90	5,38
	Sat 23c-1	36314	27674	10,37	n.d.	25841	2,53	85	3,4	117	294	0,31	3,32	455	32,2	62,7	25,9	6,90	1,31	1,26	5,37	0,85	7,83	0,955	20,35	5,56
	Sat 23c-2	38041	28255	9,98	n.d.	24609	2,08	84	3,4	111	300	0,29	3,17	435	32,3	59,0	26,8	6,98	1,23	1,20	5,14	0,82	7,60	0,957	19,87	5,33
	Bu2																									
	Sat 24a-1	37166	26859	9,14	n.d.	23863	2,91	85	3,5	94	271	0,27	2,72	424	28,5	55,4	24,0	6,11	1,21	1,20	5,11	0,82	6,68	0,776	15,54	4,36
	Sat 24a-2	39520	22090	9,58	n.d.	24778	3,03	88	3,0	92	271	0,26	2,54	436	28,6	54,2	20,9	6,08	1,28	1,20	4,86	0,79	6,78	0,807	15,52	4,74
	SAT 24b-1	36409	n.d.	9,60	n.d.	24989	2,31	87	4,0	110	295	0,32	3,08	500	31,8	61,0	26,8	7,17	1,35	1,32	5,66	0,86	7,48	1,017	17,85	5,39
	SAT 24b-2	35531	n.d.	9,19	n.d.	23653	2,17	82	3,5	112	311	0,34	3,11	493	31,5	61,1	25,5	6,94	1,30	1,28	5,70	0,87	7,68	0,976	18,26	5,58
	Sat 24c-1	50263	24254	10,68	4,9	25865	4,57	80	2,8	86	252	0,30	2,49	401	27,5	52,5	21,3	6,06	1,13	1,16	4,87	0,77	6,59	0,763	15,36	4,17
	Sat 24c-2	36625	21490	11,10	4,5	27778	4,74	85	2,8	89	258	0,31	2,60	432	28,1	54,1	25,4	6,03	1,21	1,21	4,89	0,82	6,71	0,780	15,51	4,60
	Sat 24d-1	35229	28033	8,66	n.d.	21800	2,03	78	3,0	108	311	0,31	3,05	479	32,1	59,6	22,0	7,04	1,20	1,29	5,62	0,88	7,72	0,867	18,13	5,55
	Sat 24d-2	35219	29846	8,36	n.d.	21382	2,05	79	3,2	104	282	0,29	2,93	465	30,6	57,3	23,7	6,65	1,21	1,24	5,37	0,86	7,38	0,850	17,19	5,13
	Sat 24e-1	35051	26312	8,92	n.d.	22864	2,22	89	2,9	105	300	0,29	2,95	468	30,7	59,8	24,3	6,83	1,28	1,26	5,51	0,90	7,56	0,880	17,73	5,30
	Bm																									
	Sat 26a-1	39064	20929	12,51	2,4	34564	4,36	100	4,2	96	263	0,40	2,99	396	26,9	52,0	19,7	6,54	1,37	1,33	5,02	0,89	6,68	0,739	16,05	4,87
	Sat 26a-2	37063	17006	14,40	n.d.	39634	5,45	116	4,3	96	213	0,40	3,01	398	27,0	54,6	n.d.	7,26	1,56	1,14	5,32	0,76	7,09	0,820	16,39	5,00
	Sat 26b-1	36077	19704	15,48	n.d.	42540	6,56	114	4,1	96	219	0,41	2,99	411	26,3	56,6	29,9	6,99	1,59	1,16	5,47	0,84	7,00	0,803	16,61	5,15
	Sat 26b-2	37252	20776	14,58	n.d.	40122	5,57	111	4,3	99	231	0,41	3,07	416	27,6	54,9	n.d.	7,44	1,55	1,21	5,38	0,82	7,14	0,825	16,69	5,57
	Sat 26c-1	33783	16866	16,26	2,2	43996	7,67	114	3,8	86	213	0,41	2,72	392	25,2	51,6	n.d.	6,91	1,53	1,20	5,13	0,77	6,38	0,747	14,99	4,21
Serie Ammoudi																										
	Cape Riva																									
	Sat 27-1	39019	24120	13,25	n.d.	34485	4,78	100	3,0	99	319	0,25	2,96	522	36,8	68,9	29,5	8,40	1,64	1,57	6,15	1,03	8,06	0,890	17,19	5,17
	Sat 27-2	38748	28324	13,47	n.d.	35108	4,90	95	2,6	100	303	0,25	3,03	530	36,1	68,4	36,4	8,07	1,62	1,50	5,93	1,00	8,01	0,876	17,08	5,08
	Sat 35-1	41904	28668	14,19	n.d.	34541	4,71	95	2,0	84	263	0,20	2,44	490	29,6	56,9	n.d.	6,98	1,53	1,39	5,69	1,01	7,03	0,738	14,34	4,41
	Sat 35-2	42279	23248	15,06	n.d.	36092	5,11	97	2,3	82	242	0,22	2,45	409	29,4	56,5	23,1	7,17	1,53	1,41	5,56	0,93	6,87	0,735	13,96	4,25
	Sat 36-1	39536	21306	15,51	3,0	38703	6,64	102	2,7	82	265	0,25	2,40	443	28,5	57,5	29,2	6,85	1,53	1,37	5,67	1,01	6,92	0,740	14,04	4,07
	Sat 37a-1	40427	n.d.	14,24	n.d.	34924	4,65	98	n.d.	89	295	0,24	2,53	467	30,1	58,2	n.d.	7,21	1,54	1,42	5,66	1,04	7,19	0,740	14,53	4,07
	Sat 37a-2	42218	19722	14,29	n.d.	34689	5,28	96	n.d.	82	278	0,20	2,36	441	29,0	56,0	n.d.	6,94	1,56	1,40	5,61	0,98	6,77	0,717	13,80	4,14
	Sat 37a-3	41245	n.d.	15,19	n.d.	36508	5,67	97	2,3	82	262	0,22	2,28	426	28,5	55,2	26,9	6,81	1,55	1,37	5,62	1,01	6,65	0,698	13,53	3,93
	Sat 37b-1	41660	n.d.	14,60	n.d.	35866	5,03	98	2,1	86	291	0,18	2,44	464	30,0	59,4	24,6	7,34	1,55	1,40	5,94	0,98	7,28	0,752	14,72	4,28
	Sat 37b-2	41612	17495	15,25	n.d.	37248	6,89	100	2,7	81	279	0,20	2,32	454	29,5	56,4	23,3	7,18	1,54	1,38	5,55	0,99	6,81	0,726	13,81	3,99
	Sat 37b-3	40839	18484	16,27	3,9	40252	7,95	101	1,7	71	255	0,15	2,19	436	26,9	53,3	n.d.	6,72	1,53	1,27	5,37	0,95	6,33	0,668	12,58	3,92
	Sat 37c-1	37902	n.d.	17,52	21,8	39976	9,13	98	2,8	74	240	0,20	2,13	392	26,2	51,1	n.d.	6,43	1,48	1,23	5,03	0,85	6,20	0,649	12,43	3,63
	Sat 37c-2	41946	n.d.	15,33	n.d.	37484	5,64	103	n.d.	86	268	0,21	2,51	480	29,2	57,8	n.d.	7,24	1,58	1,45	5,84	0,98	7,02	0,748	14,26	4,46
	Sat 37c-3	41350	n.d.	14,12	n.d.	34890	4,78	99	1,8	87	297	0,24	2,54	468	29,9	59,0	n.d.	7,32	1,52	1,39	5,75	1,03	7,22	0,763	14,72	4,43
	Sat 37c-4	38960	14241	17,68	9,3	41591	9,67	101	2,5	71	219	0,18	2,15	409	26,4	51,1	n.d.	6,40	1,47	1,31	5,12	0,91	6,23	0,609	12,23	3,62
Bo mean composition																										
		33281	24287	8,58	2,0	21839	3,93	69	2,8	108	290	0,30	2,89	564	31,7	63,0	25,7	6,18	1,00	1,01	5,06	0,83	7,75	0,804	19,97	5,91

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Project Thera Ashes – Pumice Sample from Knossos

Max Bichler,[*] *Heinz Huber,*[**] *and Peter Warren*[***]

Introduction

The project "Thera Ashes" is the volcanological-radiogeochemical part of the Austrian Special Research Program SCIEM 2000 (Synchronization of Civilizations in the Eastern Mediterranean in the 2nd millennium B.C.). The aim of this project is to use the eruption products of the so-called Minoan eruption of the Thera volcano (Santorini) in the 2nd millennium B.C. to establish a datum line in the stratigraphies of the Eastern Mediterranean region. During this strong explosive event a large volume of magma was erupted in a short time-span of not more than a few days, and the eruption products were distributed over a large area (for an overview see[1]). The majority of this material consists of chemically homogeneous pumice tephra,[2] the volume estimations range from 16 to 35 km^3. The major part of the erupted material was deposited directly into the sea.[3] Pumice, which consists mainly of highly vesicular silicate glass, floats on water. Depending on the size of the pumice lumps it takes some time until the fine glassy bubble walls break, the pumice gets soaked and finally sinks. In the meantime marine currents and wind could transport the floating pumice over large distances all over the Eastern Mediterranean region.[4] It can be assumed that within weeks after the eruption large amounts of pumice accumulated along the shorelines.

Pumice is a useful abrasive and has been collected and traded since prehistoric times. This is well documented by finds from archaeological excavations.[5] The applicability for chronological purposes has been checked in earlier studies by demonstrating that the "Minoan" pumice can be distinguished from other pumice sources by its trace element distribution pattern.[6] Its first appearance in an archaeological context can therefore be used for relative dating and can contribute valuable information for the synchronization of the civilizations in the Eastern Mediterranean Region. The aim of this work was to identify a pumice sample found with vase SEX/97/P2423 in the Stratigraphical Museum Site excavations directed by P.M.Warren for the British School at Athens.

Analytical Techniques

First the sample was analyzed microscopically to check if crystalline minerals ("phenocrysts") such as pyroxene, feldspar, or ore-minerals are present in the glassy matrix. The investigations were performed on whole samples using a ZEISS STEMI SV8 stereomicroscope with variable magnification from 8 to 128.

Instrumental Neutron Activation Analysis (INAA) was used to determine the major and trace element abundances. INAA is a suitable technique to detect simultaneously a large number of geochemically significant trace elements, and was therefore used to enable the identification of such eruption products by their element distribution patterns, the so-called "chemical fingerprint". In particular, the elements Na, K, Sc, Cr, Fe, Co, Zn, As, Rb, Zr, Sb, Cs, Ba, La, Ce, Nd, Sm, Eu, Tb, Yb, Lu, Hf, Ta, Th, and U were determined. The applicability of this technique for the distinction of Santorini "Minoan" pumice (Bo, Upper pumice) from other, chemically rather similar eruption products of various potential pumice sources, such as the older Santorini eruptions (Middle tuff [Bm], Lower pumice tuff [Bu], Cape Riva, Cape Therma), or Kos, Giali, Nisyros, and Milos from the southern Hellenic volcanic island arc and Lipari from the Aeolian Islands, has been demonstrated by earlier studies.[7] The first application to stratified pumice from excavations at Tell-el-

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[1] Bichler 2003.

[2] Pichler and Schiering, 1980; Vitaliano *et al.*, 1978; Bichler *et al.*, 1997; Peltz *et al.*, 1999.

[3] Sigurdsson *et al.*, 1990; Friedrich, 1999.

[4] Sutherland, 1965.

[5] Faure, 1971; Warren and Puchelt, 1990.

[6] Peltz *et al.*, 1999.

Dab^{c}a showed that even long storage under wet conditions like in the Nile-Delta sediments does not affect the proper classification by chemical fingerprinting.[8]

The pumice sample from Knossos (Warren, this volume) and a control sample of "Minoan" Upper Pumice (Bo) were routinely prepared for INAA including a thorough cleaning procedure in distilled water in an ultrasonic bath, a microscopical investigation, and homogenization of a representative amount using an agate mortar and pestle. Quantities of about 100 mg each were weighed into Suprasil™ quartz glass vials, sealed, and irradiated together with internationally certified standard reference materials in the neutron flux of the TRIGA Mark-II reactor at the Atominstitut in Vienna. After decay times of one and four weeks, the activation products were measured by gamma-spectrometry and the concentrations of the respective elements were calculated by comparative measurements of the standards (BCR 142 light sandy soil, CANMET reference soil SO-1, NIST SRM 1633b Coal fly ash, and rhyolite GBW-07113).

Results and Discussion

The results of the geochemical investigations are presented in Table 1. The data indicate that the sample is clearly not from the Upper Pumice (Bo) produced by the "Minoan eruption". Figure 1 shows the element distribution pattern of all elements determined, normalized to mean "Minoan Pumice" composition[9] together with the results obtained from a control sample of „Minoan Pumice" analyzed in parallel. The results clearly show the small but significant differences in the concentrations of indicative elements such as K, Sc, Fe, Cs, Sm, Eu, and Ta.

The results were also compared to the compositions of the Lower Pumice (Bu; about 200 ka BP), the Middle Pumice (Bm; about 100 ka BP) and the products of the Cape Riva eruption (about 21 ka BP).[10] Figs. 2–4 show the normalized element distribution patterns. A very sensitive indication is also given by plotting the ratios of element concentrations such as Eu/Th versus Ba/Ta. The ratios allow to decide whether a sample is a product of the Minoan eruption or an earlier event at Santorini. If the latter is the case, Eu/Ta versus Th/Hf clearly separates preminoan pumices (see Figs. 5, 6).

Conclusion

According to the chemical fingerprinting, the source of the sample in question is the Lower Pumice 2 eruption. The element As shows large inhomogenities („nugget effect") due to its presence in sulphidic ore particles that liberate As during corrosion. The Cr enrichment is supposed to be environmental contamination introduced by dust particles as the primary concentration of Cr in the Santorinian pumices is extremely low. The immobile elements, and especially the most indicative rare earth elements (La – Lu) fit satisfactorily within their natural distribution range in Bu-pumice. The results of the stereomicroscopical investigation agree perfectly with the chemical analysis (presence of clear feldspar and greenish pyroxene phenocrysts in the vesicular glassy matrix, accessorial ore particles).

	Na [wt.%]	K [wt.%]	Fe [wt.%]	Sc	Cr	Co	Zn	As	Rb	Zr	Sb	Cs
Knossos	3.56	3.00	2.92	11.8	84.8	4.08	91.2	11.2	117	271	0.38	3.85
Control Bo	3.59	2.60	2.25	8.77	2.08	4.05	56.4	2.85	109	278	0.25	2.91

	Ba	La	Ce	Nd	Sm	Eu	Tb	Yb	Lu	Hf	Ta	Th	U
Knossos	483	35.4	64.4	28.6	7.94	1.33	1.15	5.20	0.72	9.05	1.02	19.9	6.64
Control Bo	566	30.1	61.8	26.2	5.85	1.02	1.05	5.03	0.83	7.61	0.84	19.6	5.49

All data in mg/kg; except as noted

Table 1 Concentrations of major and trace elements in a pumice sample from Knossos and a control sample of "Minoan" Upper Pumice (Bo

[7] Peltz *et al.*, 1999; Duma 2002.
[8] Peltz and Bichler 2001.
[9] Peltz *et al.*, 1999.
[10] Druitt *et al.*, 1999.

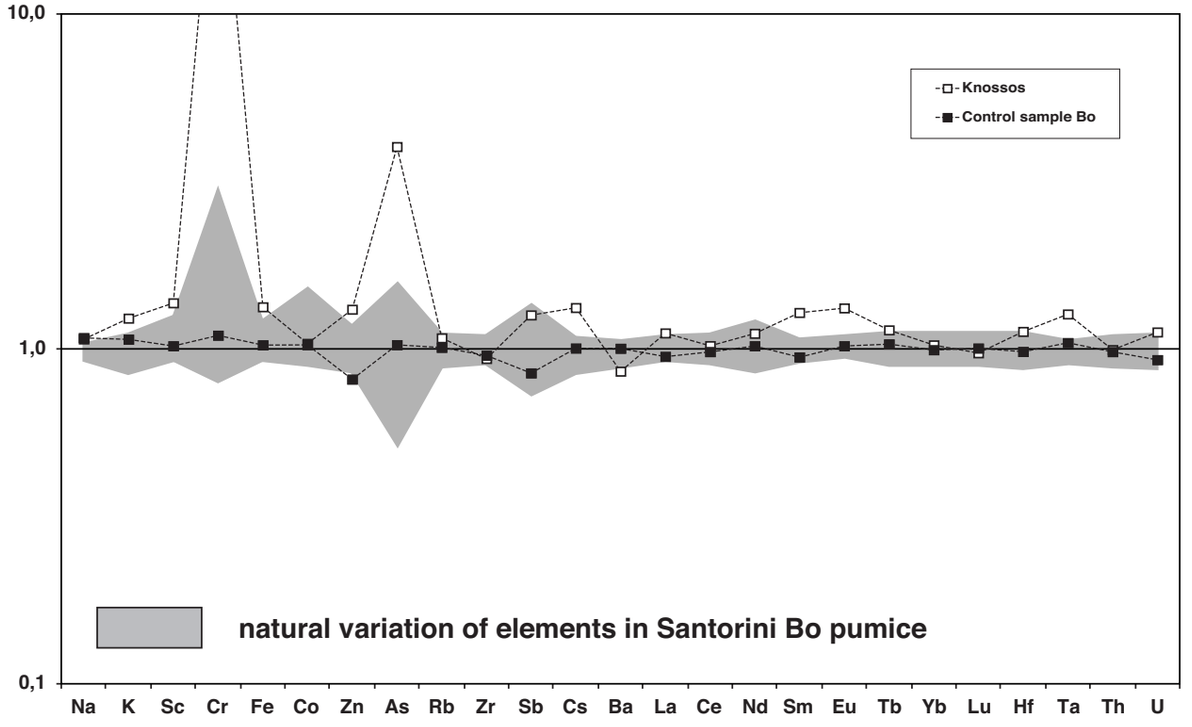


Fig. 1 Distribution of elements in a pumice sample from Knossos and a control sample of „Minoan" pumice (Upper Pumice, Santorini Bo-pumice). All values are normalized to the average concentrations of elements in Santorini Bo-pumice (data from Peltz *et al.*, 1999)

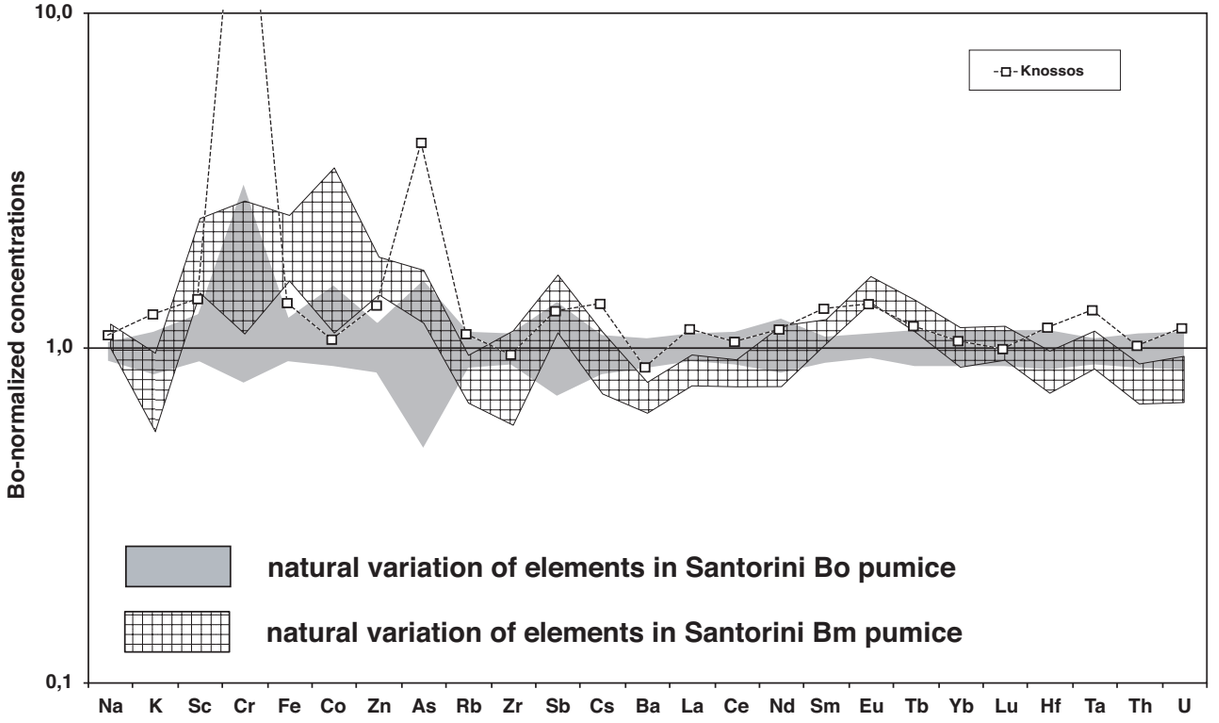


Fig. 2 Distribution of elements in a pumice sample from Knossos compared to Santorini Middle Pumice (Santorini Bm-pumice). All values are normalized to the average concentrations of elements in Santorini Bo-pumice (data from Peltz *et al.*, 1999)

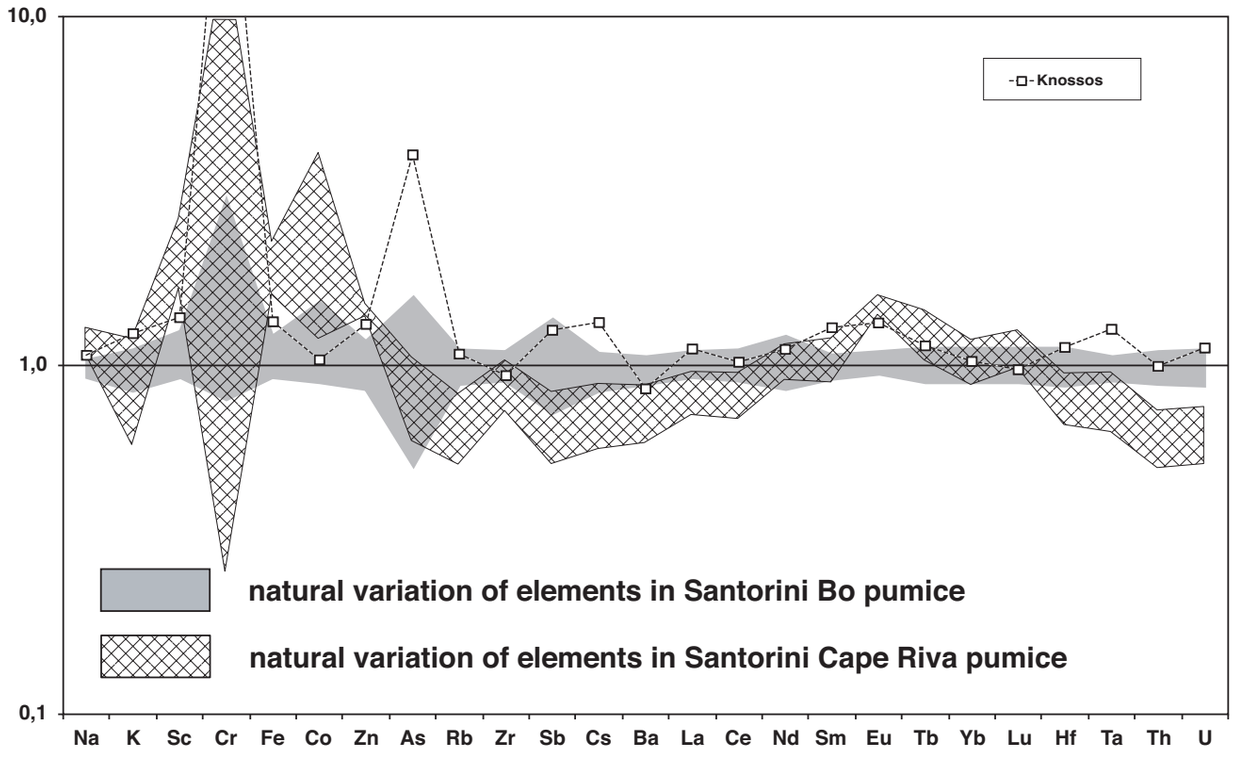


Fig. 3 Distribution of elements in a pumice sample from Knossos compared to Santorini Cape Riva Pumice. All values are normalized to the average concentrations of elements in Santorini Bo-pumice (data from Peltz *et al.*, 1999)

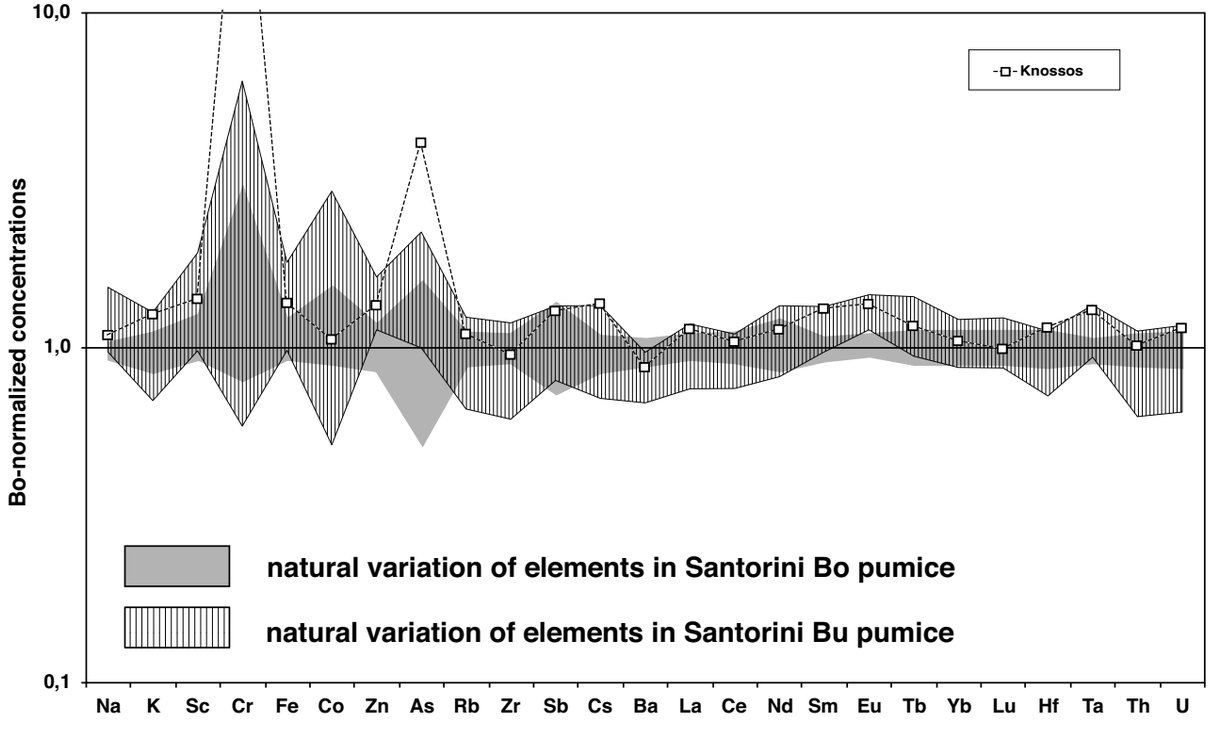


Fig. 4 Distribution of elements in a pumice sample from Knossos compared to Santorini Lower Pumice (Santorini Bu-pumice). All values are normalized to the average concentrations of elements in Santorini Bo-pumice (data from Peltz *et al.*, 1999)

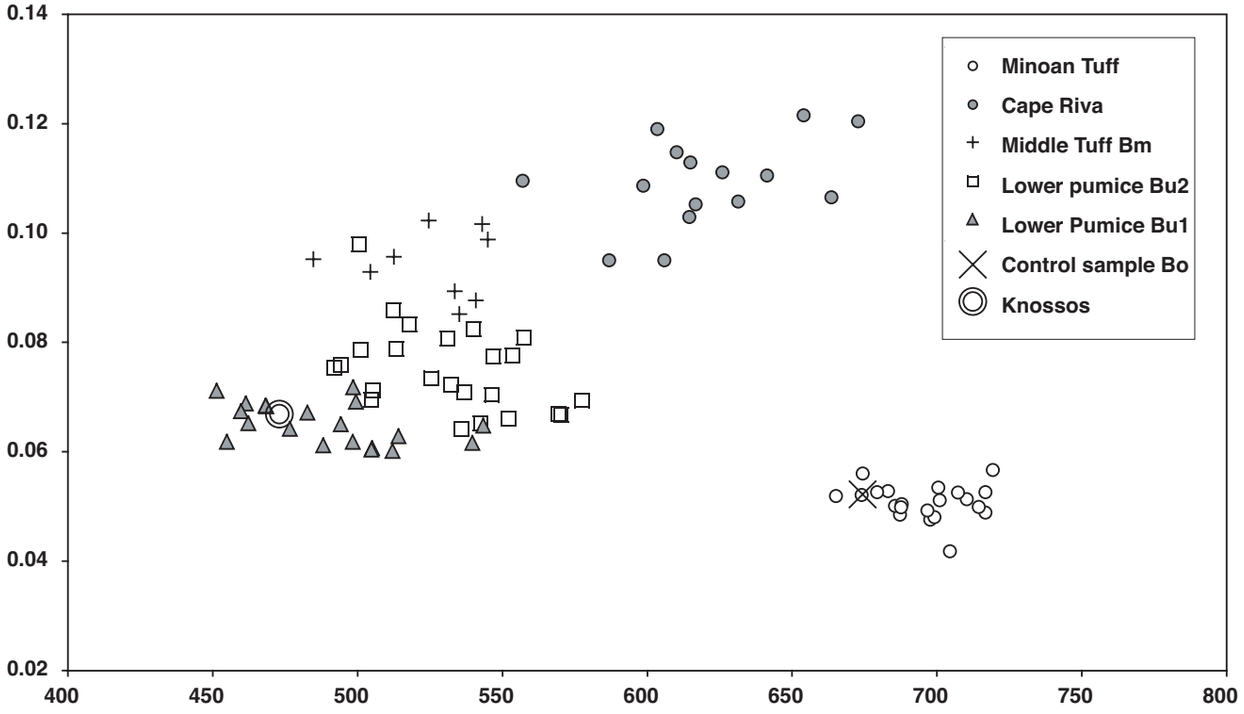


Fig. 5 Binary plot of concentration ratios of the elements Eu/Th versus Ba/Ta

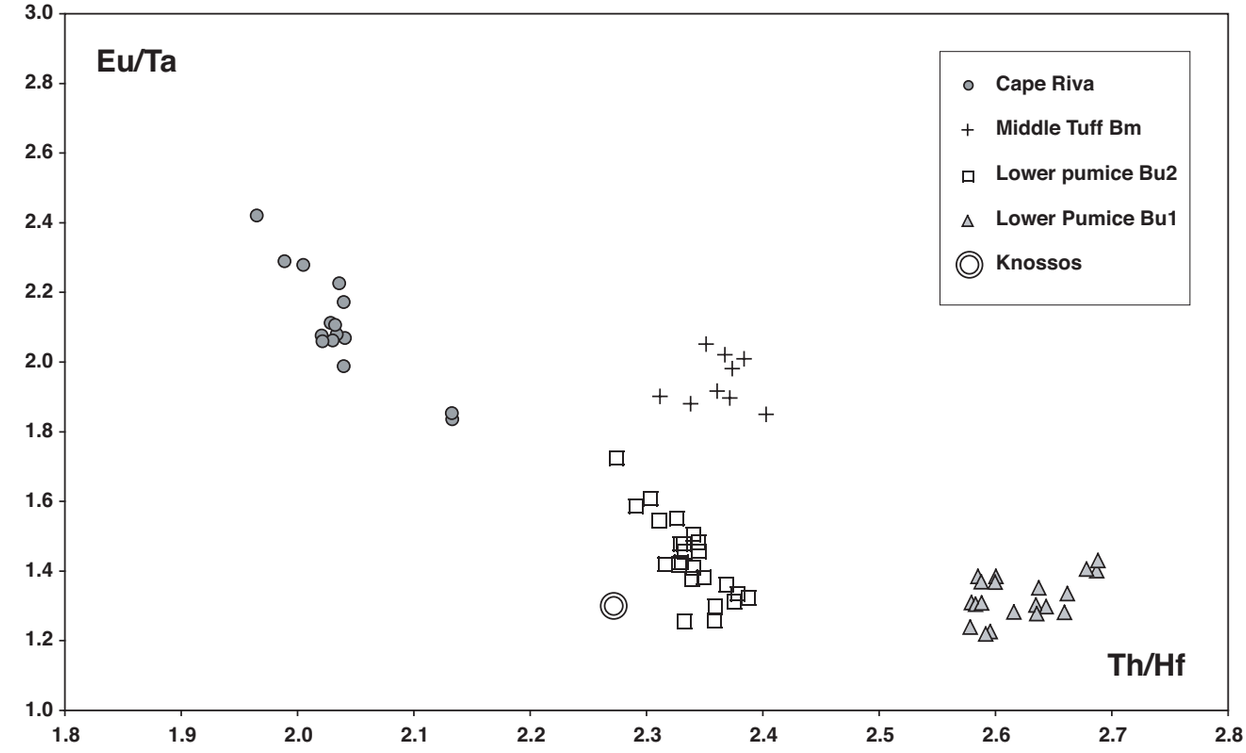


Fig. 6 Binary plot of concentration ratios of the elements Eu/Ta versus Th/Hf

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Charcoal Radiocarbon Dates of Tell el-Dab^{c}a

Hendrik J. Bruins[*]

Introduction

Tell el-Dab^{c}a, situated in the north-eastern Nile Delta, occupies a crucial position in the study of both Egyptian and regional chronologies in the 2nd millennium BCE (Bietak, 1991, 1997, 2003). As the ancient capital of the Hyksos – Avaris – the site provides key archaeological information about the Second Intermediate Period and the transition to the 18th Dynasty. The stratified archaeological remains of Tell el-Dab^{c}a facilitate essential associations with Egyptian literary sources and their unique historical chronological information.

How can we know that these historical data and the related chronological interpretations are correct? How can we verify the "Egyptian Calendar", which forms the *de facto* absolute chronological basis for archaeology in the entire Eastern Mediterranean Region, including the Aegean and the Levant? Such verification, which is a legitimate scientific request, must come from an independent outside source in order to avoid circular reasoning. Radiocarbon dating is the most obvious basis for verification and even for possible modification of the "Egyptian Calendar" (Hassan and Robinson, 1987; Bruins and Mook, 1989; van der Plicht and Bruins, 2001). However, hardly any ^{14}C dating of 2nd millennium Egypt has been carried out (Bruins and van der Plicht, 2003).

The chronological controversy concerning the date of the Minoan Thera eruption (Manning, 1999; Manning and Bronk Ramsey, 2003; Hammer *et al.*, 2003; Bietak, 2003, Wiener, 2003) is a serious problem. The archaeological interconnections between various sites in the Eastern Mediterranean region form a material cultural network with intrinsic chronological significance. The pinning down of this network to calendar years should not be based solely on Egyptian literary sources. Radiocarbon dating of stratigraphic sequences of key sites throughout the region may provide the chronological linchpin to solve the problem.

This article presents and evaluates a series of 16 radiocarbon dates of Tell el-Dab^{c}a, which were measured in 1987–1988 by Dr. Edwin Pak at the Institut für Radiumforschung und Kernphysik (University of Vienna). All samples consisted of charcoal, mainly from Acacia trees, which was used as fire wood in the different strata at Tell el-Dab^{c}a (Bietak, personal communication). At first face the results looked rather inconsistent and were, therefore, never published.

However, statistical options available in the OxCal program (Bronk Ramsey, 1995, 2001, 2003) enable verification (ranking) of individual dating results through Markov Chain Monte Carlo (MCMC) sequencing analysis within the constraints of stratigraphic models. Since the Tell el-Dab^{c}a charcoal dates can be related to each other, according to the detailed stratigraphy of the site (Bietak, 1991, 1997), such evaluation is possible and worthwhile. A new series of radiocarbon dates based on short-lived samples from Tell el-Dab^{c}a, measured by AMS (VERA Laboratory, University of Vienna), will soon be published (Kutschera, in preparation). These dates are undoubtedly of a better quality. Nevertheless, it is important to have an account also of the series of 16 multi-year charcoal dating results.

Some background comments made by Dr. Edwin Pak (letter 23-02-1988) concerning the 16 charcoal dates are quoted: "Konventionsgemäß sind die Daten auf das Jahr 1950 bezogen und unter Zugrundelegung der seinerzeit von Libby angenommen ^{14}C-Halbwertszeit von 5568 Jahren berechnet. Die statistische Unsicherheit des Alterswertes ist als (einfache) Standard-abweichung angegeben ... Jedes Zitieren der Datierung sollte immer samt der statistischen Unsicherheit und der VRI-Nummer erfolgen". This basic information concerning the meaning of BP in radiocarbon dating and the half-time used in the calculation of the age values, though a more precise value exists, as well as the convention about citation of ^{14}C dates, are concisely explained in the monograph by Mook and Waterbolk (1985).

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Sixteen ^{14}C charcoal dates of Tell el-Dab^{c}a: Stratigraphic Evaluation

Most charcoal samples were derived from strata in Area F (the new centre of the Middle Bronze Age population), except for one sample (#38, VRI-942) coming from Area A (the eastern town) (Bietak 1991, 1997). The radiocarbon dating results are summarised in Table 1 in stratigraphic order, putting the youngest layer on top. The calibrated dates, using the OxCal program (Bronk Ramsey, 1995, 2001, 2003), appear in Column 10. The next column, 11, shows the mid-probability point (50% p), besides the 16% p and 84% p values, calculated with the Cal25 program (van der Plicht, 1993). The rather large σ of the individual ^{14}C dates in BP years results in a roughly Gaussian calibrated age, which justifies the use in this case of the mid-probability point as an approximation of the highest relative probability. The last column shows the narrower ranges of calibrated dates, selected by Markov Chain Monte Carlo (MCMC) sampling (Gilks *et al.,* 1996; Bronk Ramsey, 2003) within the constraints of a stratigraphic model, explained below.

Figure 1 shows the series of 16 normally calibrated dates in stratigraphic order, using the OxCal program. The results of alternating old and young dates appear at first face to carry no sensible pattern of useful information. Should these 16 dates be simply rejected as a non-coherent series?

The acceptability of the dates within the constraints of the stratigraphic order can be tested by statistical analysis. Figure 1 shows the make-up of the sequence model for analysis with MCMC sampling in the OxCal program. The propositions put into the model are entirely based on the Tell el-Dab^{c}a stratigraphy (Bietak, 1991, 1997). The dates are organized in stratigraphic phases separated by boundaries. Two hiatus intervals (Bietak, 1991, 1997) are also included. Neither historical dates nor historical information have been put into the model. The MCMC sampling method generates probabilities for the ^{14}C dates within the constraints of the stratigraphic propositions and also evaluates the agreement of individual dates within the model, as well as the overall agreement. Statistical background information for these procedures can be found in the OxCal manual (Bronk Ramsey, 2003) and other publications (Buck *et al.,* 1992; Gilks *et al.,* 1996; Bronk Ramsey 2001; Steier *et al.*, 2001).

The sequencing results of all dates, according to this model, are graphically displayed in Figure 2. The overall agreement is poor, only 31.3%, which is well below the critical value of 60% (OxCal manual, Bronk Ramsey, 2003). Looking at each individual date within the sequence, it appears that this is caused by 4 dates that show poor agreement lower than 60%: #36 VRI-940, #119 VRI-951, #34 VRI-938, and #120 VRI-952. Removing these 4 radiocarbon measurements results in a series of 12 remaining dates, which appear to be reasonably coherent (Figure 3). Two dates with a very large σ (#44 VRI-948 and #50 VRI-949) are a bit problematic but are kept in the stratigraphic sequence as both received MCMC agreement above 60% (Figure 2).

Running the sequence model with MCMC sampling analysis – without the four "bad" dates – gives a dramatic improvement of the overall agreement, which is now 136% (Figure 4). All individual dates show agreement above 60%. The selected calibrated age ranges generated by MCMC sampling within the constraints of the model are numerically presented in Table 1, column 12. A more detailed description and evaluation of the results in comparison with archaeological-historical age assessments follows in stratigraphic chronological order, beginning with the oldest layer and going successively to younger strata.

Stratum e

The earliest known town at Tell el-Dab^{c}a is purely Egyptian in character, composed of a royal domain and an orthogonally planned settlement further to the south, represented by Stratum e. The latter settlement belongs, according to ceramic studies, to the beginning of the 12th Dynasty (ca 1963–1934 BCE), to the period of Amenemhat I and Sesostris I (Czerny, 1999; Bietak, 1997). There is one charcoal sample (#43) from stratum e, which yielded a radiocarbon date of 3760 ± 130 BP (VRI-947). This is indeed the oldest ^{14}C date obtained in the entire series (Table 1), thus fitting well with the stratigraphic sequence. The large standard deviation (σ) of 130 results in a very wide calibrated age range covering the 1σ period of 2400–1979 BCE. This result is older than the archaeological-historical date of 1963–1934 BCE. The even wider 2σ calibrated age range of 2563–1812 does accommodate the historical date in its younger margin. However, the midpoint of the calibrated date (50% probability), 2072 BCE (Table 1, column 11), is more than a century older than the historical date, according to the Cal25 program (van der Plicht, 1993). The 1σ MCMC sampling calibrated age range, according to the above stratigraphic model, is 2233–1978 BCE (Table 1, column 12), which is also older than the historical age. This MCMC date is narrower – more precise – and somewhat younger than the regular 1σ calibrated

# No	Stratum	Area	Characteristics	Culture	Dynasty	Archaeo-Historical Age	Lab No	^{14}C Date (yr BP)	Calibrated Date BCE OxCal 3.9 r:2 1) Standard Calibration	Calibrated Date BCE Cal25 2) Mid-(50%) probability	Calibrated Date BCE OxCal 3.9 r:2 3) Sequencing 12 agreeable results with MCMC sampling
120	b/1–2	F	b/1 is Hyksos	MB IIB	Late 13 to15	1680–1620	VRI-952	3680 ± 130	1σ: 2278–2252 (3.8%) 2230–2220 (1.5%) 2206–1883 (62.9%) 2σ: 2458–2416 (2.2%) 2408–1742 (93.2%)	16%p: 2263 50%p: 2072 84%p: 1898	
38	F (~b/3)	A	Kingdom of Nehesy	MB IIA–IIB	13	1710–1680	VRI-942	3420 ± 110	1σ: 1880–1839 (9.9%) 1830–1603 (56.4%) 1552–1547 (1.0%) 1542–1538 (0.9%) 2σ: 2016–1997 (1.0%) 1980–1490 (92.9%) 1479–1451 (1.5%)	16%p: 1868 50%p: 1727 84%p: 1587	1σ: 1745–1613 (68.2%) 2σ: 1781–1522 (95.4%) Agreement 116.9%
32	b/3	F	Kingdom of Nehesy	MB IIA–IIB	13	1710–1680	VRI-936	3280 ± 120	1σ: 1728–1723 (0.8%) 1688–1429 (67.4%) 2σ: 1879–1839 (2.8%) 1829–1366 (88.6%) 1363–1295 (4.0%)	16%p: 1707 50%p: 1570 84%p: 1442	1σ: 1742–1607 (68.2%) 2σ: 1783–1506 (94.2%) Agreement 102.5%
33	c/1 b/3	F	Mass burials	MB IIA	13	ca. 1710			1σ: 1879–1840 (8.7%) 1828–1791 (7.6%) 1783–1600 (44.8%) 1566–1529 (7.2%) 2σ: 2024–1996 (1.5%) 1980–1434 (93.9%)	16%p: 1869 50%p: 1717 84%p: 1568	1σ: 1784–1679 (68.2%) 2σ: 1832–1608 (95.4%) Agreement 126.0%
39	c/1 b/3	F	Mass burials	MB IIA	13	ca. 1710	VRI-943	3560 ± 170	1σ: 2138–2075 (8.2%) 2072–1688 (60.0%) 2σ: 2352–1513 (95.4%)	16%p: 2147 50%p: 1916 84%p: 1703	1σ: 1787–1684 (68.2%) 2σ: 1836–1614 (95.4%) Agreement 105.7%
34	c	F	Canaanite settlement	MB IIA	13	1740–1710	VRI-938	3310 ± 110	1σ: 1736–1714 (4.9%) 1691–1492 (58.0%) 1478–1454 (5.3%) 2σ: 1881–1837 (3.8%) 1832–1383 (91.6%)	16%p: 1728 50%p: 1600 84%p: 1478	
40	c	F	Canaanite settlement	MB IIA	13	1740–1710	VRI-944	3560 ± 120	1σ: 2115–2099 (2.5%) 2037–1740 (65.7%) 2σ: 2204–1603 (95.4%)	16%p: 2081 50%p: 1907 84%p: 1754	1σ: 1834–1741 (68.2%) 2σ: 1888–1684 (95.4%) Agreement 112.1%
42	c	F	Canaanite settlement	MB IIA	13	1740–1710	VRI-946	3560 ± 130	1σ: 2124–2097 (4.1%) 2090–2084 (0.7%) 2039–1738 (61.4%) 1708–1694 (2.0%) 2σ: 2283–2250 (1.4%) 2208–1600 (92.4%) 1570–1528 (1.6%)	16%p: 2095 50%p: 1908 84%p: 1745	1σ: 1834–1740 (68.2%) 2σ: 1888–1684 (95.4%) Agreement 115.3%

#No = archaeological sample number; VRI = ^{14}C Lab acronym of the Institut für Radiumforschung und Kernphysik (University of Vienna); 1) Individual calibration of each date with the OxCal v3.9 program, using a resolution of 2 (Bronk Ramsey, 2003). 2) Mid-point probability of the calibrated date (50% p), as well as (16% p and 84% p) with the Cal25 calibration program (van der Plicht, 1993). 3) Sequence MCMC analysis of all 12 agreeable radiocarbon results, without VRI-938, VRI-940, VRI-951, VRI-952, according to the stratigraphic model with phase boundaries and two hiatus intervals, based on the stratigraphy of Area F (Bietak, 1991, 1997). The MCMC analysis used 908,693 iterations. Overall Agreement is 135.9%, which is well above the minimum standard of 60.0%

Table 1 Charcoal radiocarbon dates of Tell el-Dab^{c}a, their stratigraphic relations, archaeo-historical age assessment, and ^{14}C calibrated age ranges

# No	Stratum	Area	Characteristics	Culture	Dynasty	Archaeo-Historical Age	Lab No	^{14}C Date (yr BP)	Calibrated Date BCE OxCal 3.9 r:2 1) Standard Calibration	Calibrated Date BCE Cal25 2) Mid-(50%) probability	Calibrated Date BCE OxCal 3.9 r:2 3) Sequencing 12 agreeable results with MCMC sampling
44	d/1	F	Egyptian palace	MB IIA	Early 13	1780–1750	VRI-948	3270 ± 250	1σ: 1882–1836 (4.1%) 1833–1295 (63.0%) 1232–1217 (1.1%) 2σ: 2146– 914 (95.4%)	16%p: 1870 50%p: 1557 84%p: 1237	1σ: 1912–1808 (68.2%) 2σ: 1978–1753 (95.4%) Agreement 88.5%
50	d/1	F	Egyptian palace	MB IIA	Early 13	1780–1750	VRI-949	3270 ± 220	1σ: 1877–1841 (3.4%) 1814–1797 (1.6%) 1779–1294 (62.2%) 1275–1264 (1.0%) 2σ: 2135–2079 (1.4%) 2060– 997 (94.0%)	16%p: 1833 50%p: 1556 84%p: 1276	1σ: 1911–1808 (68.2%) 2σ: 1977–1755 (95.4%) Agreement 77.5%
119	d/1	F	Egyptian palace	MB IIA	Early 13	1780–1750	VRI-951	3740 ± 120	1σ: 2330–2323 (1.0%) 2308–2008 (60.5%) 2003–1974 (4.7%) 1971–1957 (1.9%) 2σ: 2472–1874 (94.0%) 1842–1810 (1.4%)	16%p: 2333 50%p: 2154 84%p: 1986	
122	d/1	F	Egyptian palace	MB IIA	Early 13	1780–1750	VRI-953	3500 ± 110	1σ: 2008–2003 (0.9%) 1957–1685 (67.3%) 2σ: 2135–2080 (3.7%) 2049–1597 (87.7%) 1591–1525 (4.1%)	16%p: 1971 50%p: 1826 84%p: 1694	1σ: 1914–1812 (68.2%) 2σ: 1976–1758 (95.4%) Agreement 127.4%
51	d/1–2	F		MB IIA	Late 12 – Early 13	1800–1760	VRI-950	3520 ± 130	1σ: 2029–1992 (6.1%) 1982–1686 (62.1%) 2σ: 2199–2159 (1.9%) 2148–1521 (93.5%)	16%p: 2029 50%p: 1855 84%p: 1696	1σ: 1976–1857 (68.2%) 2σ: 2048–1792 (95.4%) Agreement 120.5%
36	d/2	F	Canaanite settlement	MB IIA	Late 12	1830–1780	VRI-940	3290 ± 120	1σ· 1731–1721 (1.9%) 1689–1434 (66.3%) 2σ: 1882–1836 (3.4%) 1833–1366 (89.0%) 1363–1308 (3.0%)	16%p: 1719 50%p: 1580 84%p: 1452	
37	d/2	F	Canaanite settlement	MB IIA	Late 12	1830–1780	VRI-941	3680 ± 120	1σ: 2271–2257 (2.1%) 2203–1885 (66.1%) 2σ: 2404–2362 (2.4%) 2353–1744 (93.0%)	16%p: 2250 50%p: 2071 84%p: 1910	1σ: 2050–1892 (68.2%) 2σ: 2179–1858 (95.4%) Agreement 118.5%
43	e	F	Planned orthogonal settlement, Egyptian culture	MBI	12	1980–1920	VRI-947	3760 ± 130	1σ: 2400–2380 (2.9%) 2348–2013 (62.4%) 1998–1979 (2.9%) 2σ: 2563–2522 (1.5%) 2497–1876 (92.8%) 1841–1812 (1.1%)	16%p: 2377 50%p: 2182 84%p: 1998	1σ: 2233–1978 (68.2%) 2σ: 2402–1907 (95.4%) Agreement 112.4%

#No = archaeological sample number; VRI = ^{14}C Lab acronym of the Institut für Radiumforschung und Kernphysik (University of Vienna); 1) Individual calibration of each date with the OxCal v3.9 program, using a resolution of 2 (Bronk Ramsey, 2003). 2) Mid-point probability of the calibrated date (50% p), as well as (16% p and 84% p) with the Cal25 calibration program (van der Plicht, 1993). 3) Sequence MCMC analysis of all 12 agreeable radiocarbon results, without VRI-938, VRI-940, VRI-951, VRI-952, according to the stratigraphic model with phase boundaries and two hiatus intervals, based on the stratigraphy of Area F (Bietak, 1991, 1997). The MCMC analysis used 908,693 iterations. Overall Agreement is 135.9%, which is well above the minimum standard of 60.0%

Table 1 continued Charcoal radiocarbon dates of Tell el-Dab^{c}a, their stratigraphic relations, archaeo-historical age assessment, and ^{14}C calibrated age ranges

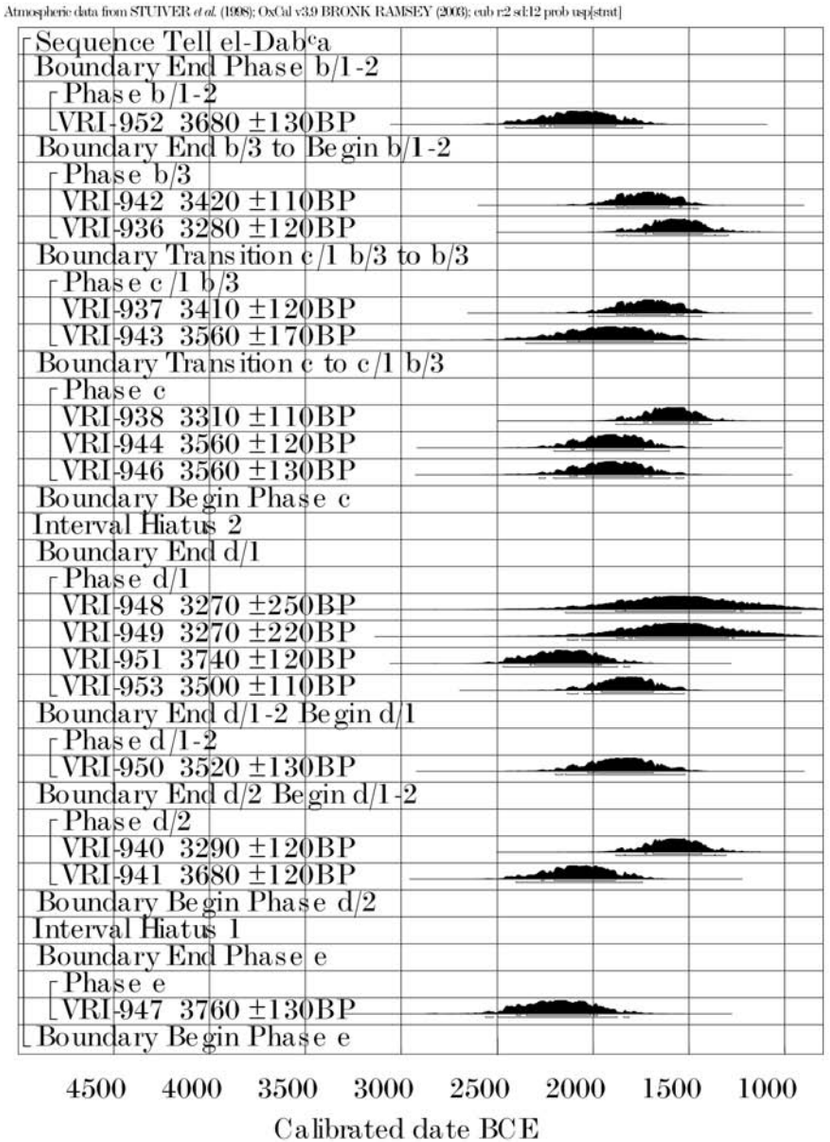


Fig. 1 All standard calibrated dates of Tell el-Dabᶜa in stratigraphic order, showing an apparent non-coherent alternation of older and younger dates

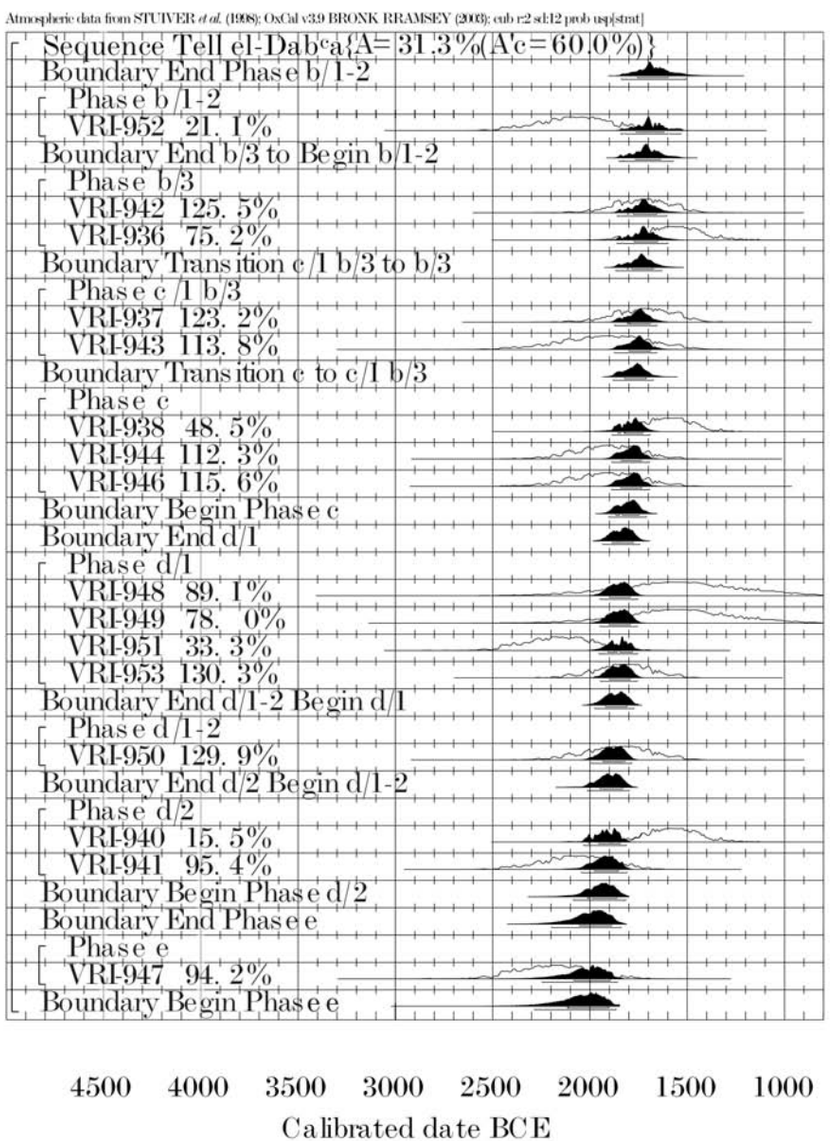


Fig. 2 Sequence analysis with Markov Chain Monte Carlo (MCMC) sampling of all 16 radiocarbon dates. The overall agreement (31.4%) is well below the minimum required 60%. Four individual dates (VRI-952, VRI-938, VRI-951, VRI-940) are the main cause, having poor agreement below 60%, and will be excluded from the series

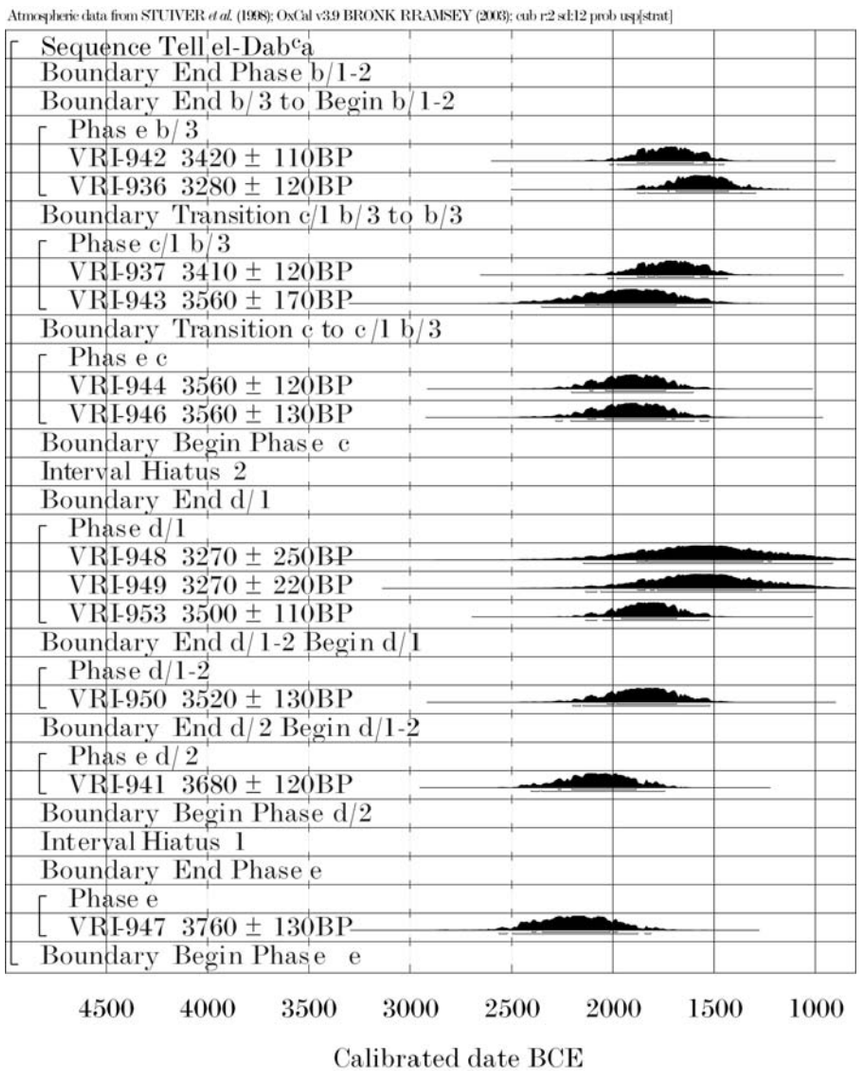


Fig. 3 The 12 agreeable dates in the Tell el-Dabᶜa series in stratigraphic order show a reasonably consistent pattern. The comparatively young dates VRI-948 and VRI-949 are somewhat problematic but nevertheless agreeable in the sequence due to their large σ

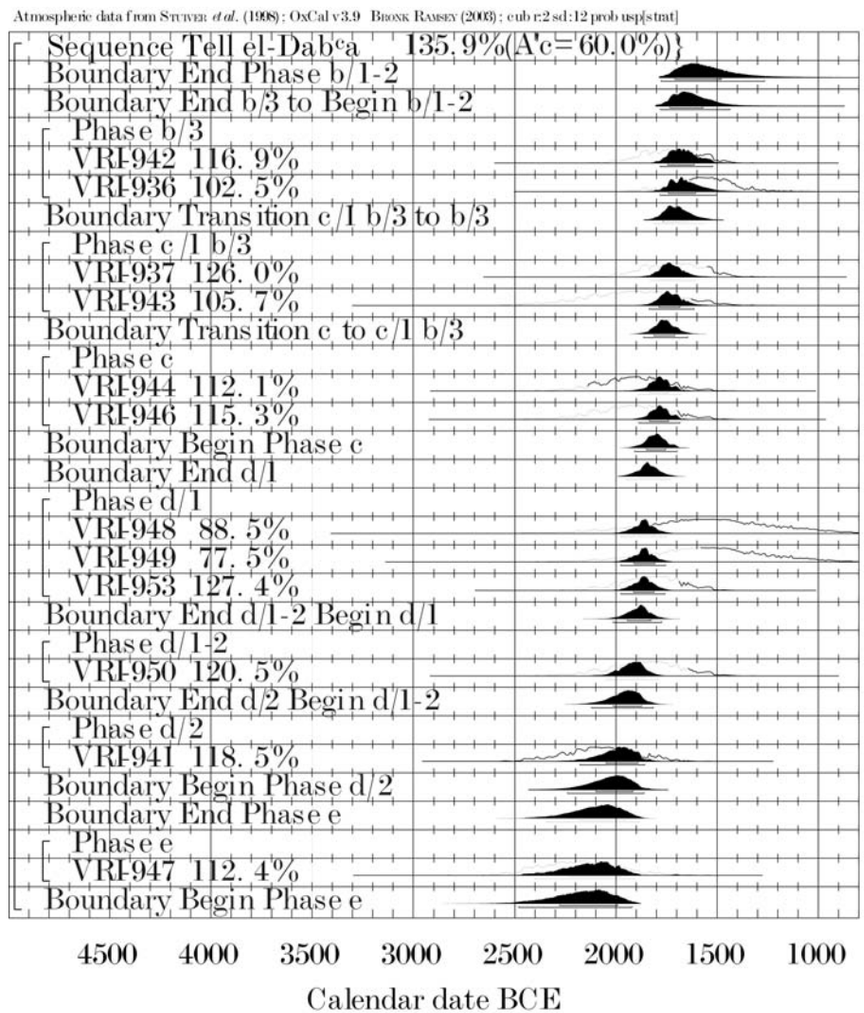


Fig. 4 Sequencing of the remaining 12 radiocarbon dates with Bayesian statistics using Markov Chain Monte Carlo (MCMC) sampling. The sequence model now has a good overall agreement (135.9%), well above the minimum required 60%

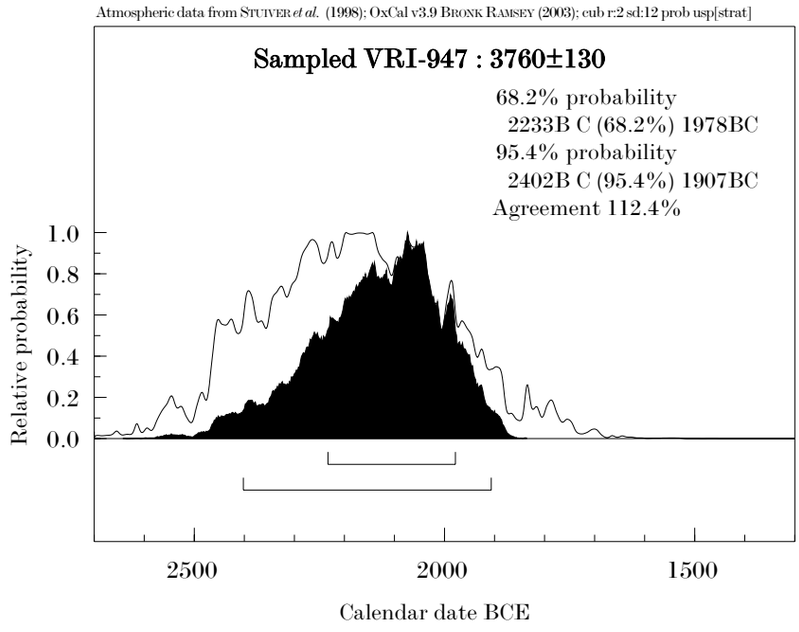


Fig. 5 Selected date by MCMC sampling (solid black) of Stratum e, being somewhat younger and narrower than the regular calibrated date (area below irregular black line)

range, modified by the stratigraphic sequence constraints, as shown graphically in Figures 4 and 5. However, as the internal age of the charcoal (old-wood effect) is unknown, no firm conclusions may be drawn until results of short-lived samples from Stratum e may be available.

Hiatus 1

MCMC sampling also gives an indication for the duration of the Hiatus interval between Strata e and d/2. The 1σ sampled duration of the interval is 0–79 years (Figure 6), which seems to fit well with the archaeological-historical assessment of about 90 years (Bietak, 1991, 1997).

Stratum d/2

A Canaanite settlement with *Mittelsaal* house type characterises Stratum d/2 (Bietak, 1997). Though most pottery is composed of Egyptian types, about 20% is Levantine Middle Bronze Age in style. The settlement is dated on archaeological-historical grounds, based on the seriation of Egyptian ceramics, to the end of the 12th Dynasty, approximately 1830–1780 BCE (Bietak, 1997).

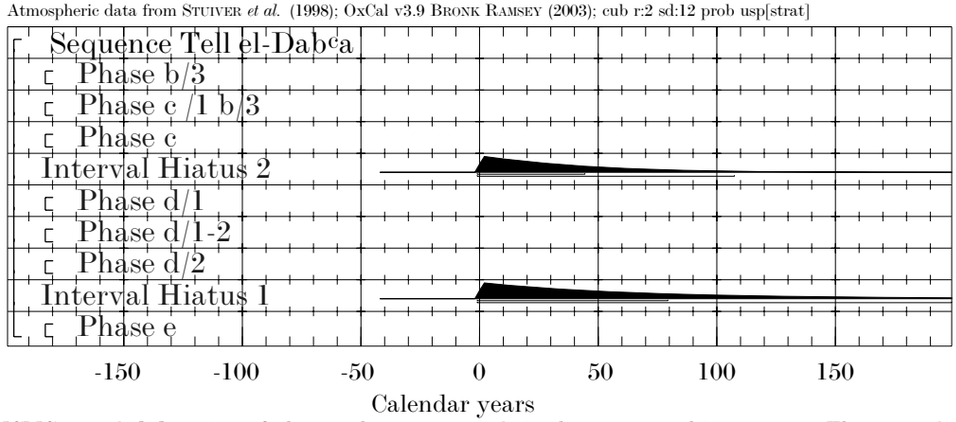


Fig. 6 MCMC sampled duration of the two hiatus intervals in the stratigraphic sequence. The upper line beneath and part of the solid black graphics of Hiatus 1 and Hiatus 2 indicates the 1σ range and the lower line the 2σ range

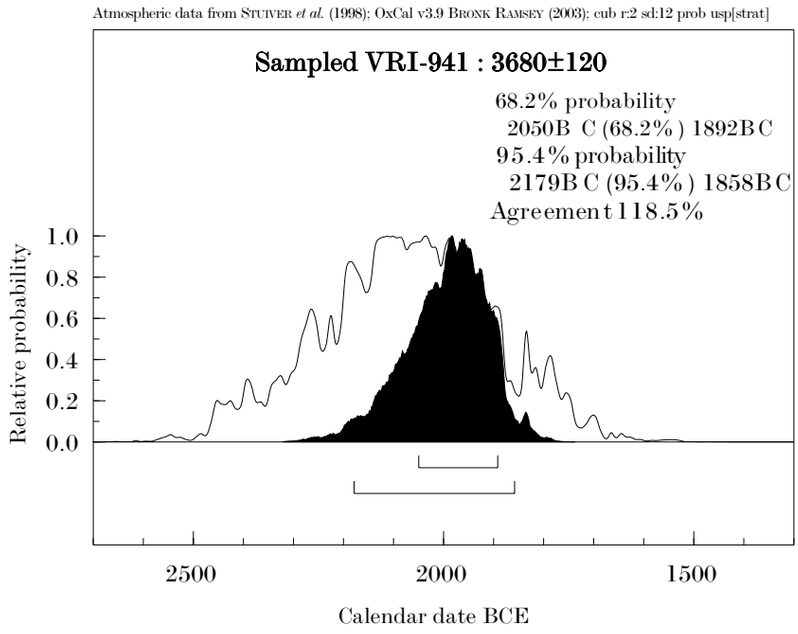


Fig. 7 MCMC sampling selects a narrower and somewhat younger part of the full calibrated age range for Stratum d/2, according to the stratigraphic model

There are two ^{14}C dates for Stratum e, which differ considerably in age: 3290 ± 120 BP (VRI-940) and 3680 ± 120 BP (VRI-941). It is statistically not correct in this case to calculate a weighted average, because the chi-squared test is not passed: T=5.3 (5% 3.8). The sequence analysis of all ^{14}C dates with MCMC sampling, using a stratigraphic model (Figure 2), rejects the young date (VRI-940). Its poor agreement of only 15.5% within the sequence is well below the acceptable limit of 60%. The agreeable older date has a 1σ calibrated age of 2271–1885 BCE, which is older than the historical age. Likewise, the mid-probability point of 2071 BCE is considerably older by more than 200 years. The selected narrower age range following MCMC sampling (Figure 7) is 2050–1892 BCE, also older than the historical date (Table 1).

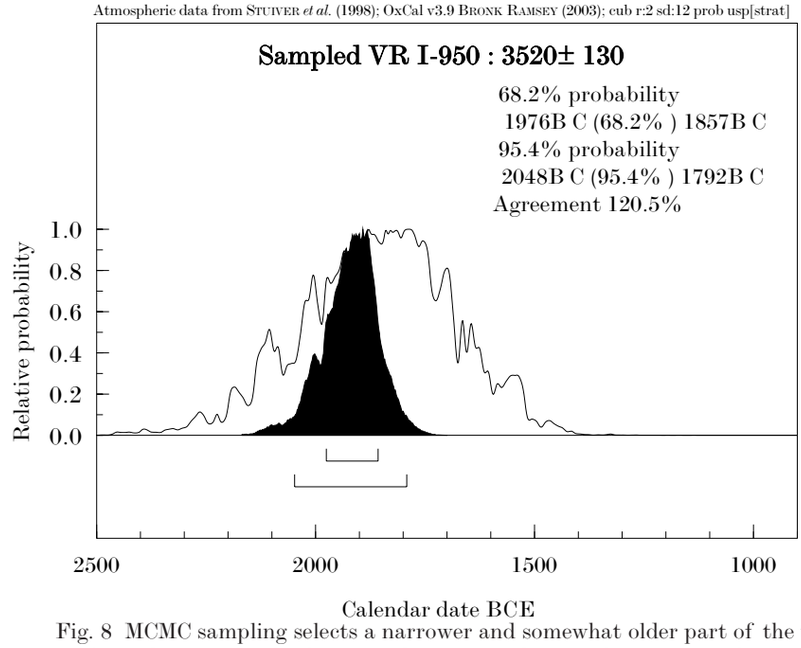


Fig. 8 MCMC sampling selects a narrower and somewhat older part of the full calibrated age range for Stratum d/1-2, according to the stratigraphic model

Stratum d/1–2

The dated sample (VRI-950, 3520 ± 130 BP) is from a layer transitional between d/2 and d/1. The latter Stratum is characterised by an Egyptian palace of the 13th Dynasty and associated tombs. Stratum d/1 is archaeologically-historically dated to the period of about 1780–1750 BCE (Bietak, 1997, Figure 4.3). The acceptable ^{14}C dates for Strata e, d/2 and d/1–2 show a coherent relationship, becoming gradually younger (Table 1). The 1σ calibrated date of Stratum d/1–2 (VRI-950) is 2029–1686 BCE with a mid-probability point of 1855 BCE (Table 1). The younger part of the calibrated age range includes the archaeological-historical date around 1780 BCE. However, the MCMC selected 1σ age range, 1976–1857 BCE, is clearly older, and also the 2σ MCMC age of 2048–1792 BCE.

Stratum d/1

The 13th Dynasty Stratum d/1 of the Egyptian palace was the source of four charcoal samples. The ^{14}C results vary considerably (Table 1, Fig. 1). The oldest result (VRI-951, 3740 ± 120 BP) is rejected by MCMC sampling (Fig. 1), showing poor agreement (33.3%) in the sequence. The weighted average of the three remaining dates is 3549 ± 73 BP, passing the chi-squared test: T=5.5 (5% 7.8). The detailed calibrated 1σ age of the average is 2010–2001 (2.4%), 1976–1857 (40.1%), 1845–1770 (25.7%) BCE. This result is basically older than the archaeological-historical date of 1780–1750 BCE (Bietak, 1997, fig. 4.3), although there is minimal overlap at the youngest margin. The calibrated 2σ age range {2128–2083 (3.4%), 2041–1730 (88.7%), 1722–1689 (3.3%)} does include the historical date in its younger part. However, the narrower calibrated age range of 1914–1808 BCE, obtained with MCMC sampling for the three agreeable dates of Stratum d/1 (Table 1, column 12), according to the stratigraphic model, is substantially older than the historical age.

Hiatus 2

A hiatus interval of about 10 years duration is indicated by Bietak (1997, fig. 4.3) in Area F after the end of Stratum d/1 and before the beginning of Stratum c. This hiatus interval was placed into the stratigraphic model (Figure 3 and 6), but without any indication of its time duration. MCMC sampling of the model suggests a 1σ duration of 0–44 years (Fig. 6). Hence the suggested 10 year hiatus is certainly probable within this range.

Stratum c

On the ruins of the Egyptian palace of Stratum d/1 a new Canaanite settlement was built by an Asian population during the 13th Dynasty. Stratum c exhibits an increase in Middle Bronze Age ceramics (40%), as compared to the two previous strata (20%), which might indicate a new wave of immigration from Canaan. The indicated archaeological-historical date for Stratum c is 1740–1710 BCE (Bietak, 1997).

Three radiocarbon dates are available for Stratum c (VRI-938, 3310 ± 110 BP; VRI-944, 3560 ± 120 BP; and VRI-946, 3560 ± 130 BP). The two latter dates are almost exactly the same, while the young outlier is rejected by MCMC sampling (Fig. 2), showing poor agreement (48.5%) within the full sequence. Combining the two similar dates gives a weighted average of 3560 ± 88 BP for Stratum c. The 1σ calibrated age of the average is 2026–1995 (7.8%), 1980–1856 (37.7%), 1846–1769 (21.5%), 1757–1752 (1.1%) BCE. The 2σ calibrated age is 2141–1684 BCE. The highest relative probability is for the age range of 1980–1856 BCE.

MCMC sampling of the stratigraphic model selects a somewhat younger part of the calibrated range (Figure 9): a 1σ age range of 1834–1741 BCE and a 2σ range of 1888–1684 BCE. The historical age assessment would fit the youngest part of the 2σ MCMC selection, but the section with the highest relative probability (1σ range) is older

Stratum c/1 b/3

The end of Stratum c is characterised by mass graves, often shallow, perhaps related to an epidemic, dated to about 1710 BCE on archaeological-historical assessment (Bietak, 1997). There are two radiocarbon dates of this phase. One date (VRI-943, 3560 ± 170 BP) is very similar as two previous dates of Stratum c. The other date (VRI-937, 3410 ± 120) is somewhat younger.

Both dates are acceptable within the sequence of the stratigraphic model, according to the MCMC sampling analysis, receiving agreement of 115.6% and 122.5%, respectively. Combining both samples is allowed with respect to the chi-squared test: T=0.5 (5% 3.8). The resulting weighted average is 3461 ± 98 BP, giving a 1σ calibrated age of 1915–1901 (2.5%), 1890–1680 (60.3%), 1670–1658 (2.5%), 1651–1637 (2.9%) BCE and a 2σ age range of 2027–1994 (2.4%), 1981–1522 (93.0%) BCE. The MCMC sampling analysis, considering both radiocarbon dates, selects a narrower range of 1787–1679 BCE within the constraints of the stratigraphic model. The archaeological-historical date of about 1710 BCE fits well in this case with the radiocarbon

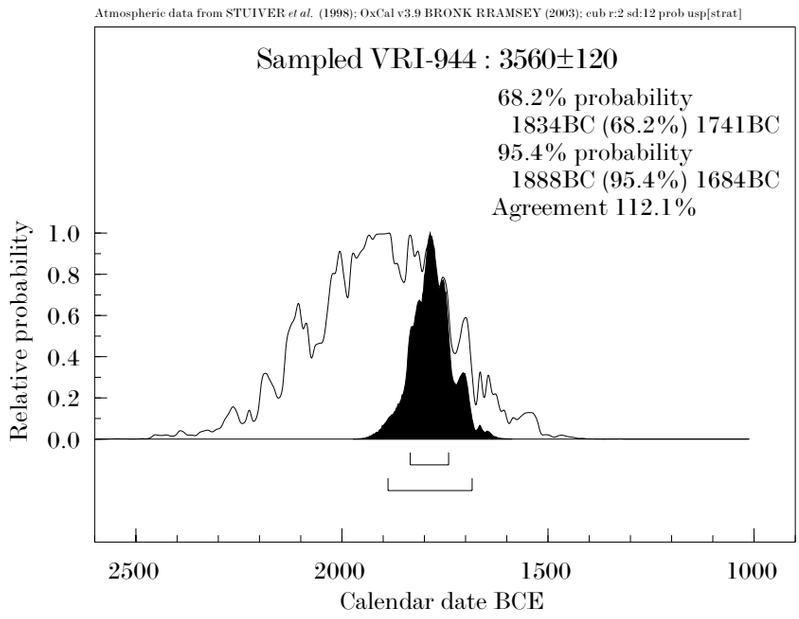


Fig. 9 MCMC sampling selects a narrower and somewhat younger part of the full calibrated age range for Stratum c, according to the stratigraphic model

age evaluations, particularly for sample VRI-937 (Fig. 10).

Stratum b/3

During this phase, the site of Tell el-Dab^{c}a had become the capital of the kingdom of Avaris, established by Nehesy. The archaeological-historical date for Stratum b/3 is ca 1710–1680 BCE (Bietak, 1997). There are two ^{14}C dates for this phase: 3280 ± 120 BP (VRI-936) derived from Stratum b/3 in Area F, and 3420 ± 110 BP (VRI-942) derived from the similar Stratum F in the eastern town of Area A. Both dates are acceptable in terms of MCMC sampling analysis within the constraints of the stratigraphic model,

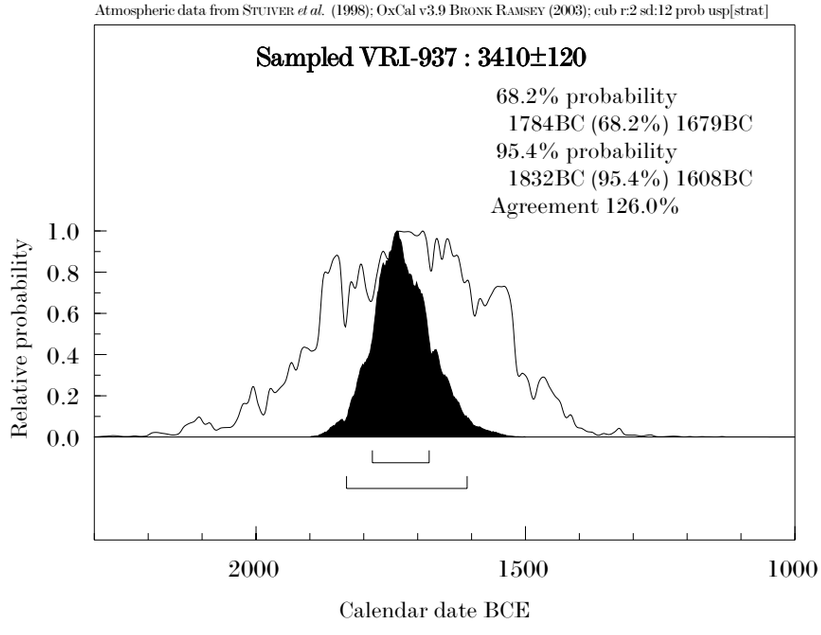


Fig. 10 MCMC sampling shows how well this date (VRI-937) of Stratum c/1 b/3 fits with the stratigraphic model and the historical age of ca 1710 BCE

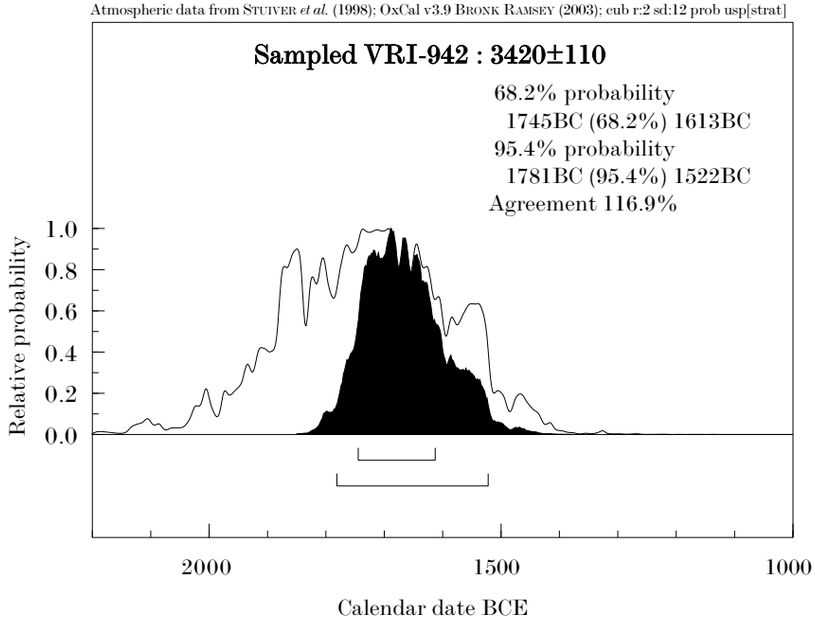


Fig. 11 MCMC sampling selects a narrower and somewhat younger part of the full calibrated age range for Stratum b/3, according to the stratigraphic model

MB Phases	Archaeo-Historical Date Year BCE	Egypt Relative Chronology	New Center Area F	Calibrated Date BCE Sequence MCMC sampling
LB I	1530–1410	XVIII		
MB II C	1580–1530	Hyksos	Denuded	
MB II B	1600–1580	XV	a / 2	
	1650–1600	Avaris Kingdom XIV	b / 1	
	1680–1650		b / 2	
MB II A–B	1710–1680			
MB II A	1740–1710	XIII	Burials c/1 b/3	1787–1679
			c	1834–1740
	1750–1740		Hiatus	
	1780–1750		d / 1	1912–1808
	1830–1780	XII	d/1–2	1976–1857
			d / 2	2050–1892
	1920–1830		Hiatus	
MB I	1940–1920		e / 1	2233–1978
	1970–1940		e / 2–3	
	1970–2050+	XI X		

Fig. 12 Calibrated radiocarbon dating results with MCMC sampling according to the stratigraphic model in relation to archaeological and historical age assessments (Bietak, 1991, 1997)

showing agreement of 103% and 117%, respectively. On an individual basis, the older date (VRI-942) fits well with the archaeological-historical age assessment. The highest relative probability of VRI-942 in the 1σ range is 1830–1603 (56.4%) BCE. The sampled MCMC 1σ selection is the calibrated period 1745–1613 BCE (Fig. 11), also fitting well with the historical date of 1710–1680 BCE (Table 1).

Stratum b/1–2

The radiocarbon date for this phase, which represents the beginning of the Hyksos period, is clearly too old: 3680 ± 130 BP (VRI-952). The date is rejected by MCMC sampling analysis within the constraints of the stratigraphic model, receiving a poor agreement of 21% (Fig. 2).

Conclusions

This evaluation should only be regarded as preliminary, because the data-series involved has its distinct limitations: 1) Multi-year charcoal samples, 2) Not enough samples per individual stratum to test the repeatability and reliability of the radiocarbon measurements, 3) No ^{14}C dates of the younger phases in the stratigraphic sequence that cover the Hyksos period and the beginning of the 18th Dynasty.

The evaluation indicates that the radiocarbon dates for the older strata (e, d/2, d/1) are older by 100 to 200 years than the archaeological-historical age assessment. The radiocarbon results for stratum c are also considerably older, but slight overlap exists between the youngest part of the ^{14}C dates and the historical age assessment.

Radiocarbon dates for the transition towards the Nehesy Kingdom of Avaris (Stratum c/1 b/3) and the first part of this kingdom (Stratum b/3) match very well with archaeological-historical dates.

An overview of the principal results of MCMC sampling of this charcoal radiocarbon series of Tell el-Dab^{c}a, according to the stratigraphic model, is presented in Figure 12 in relation to Middle Bronze Age archaeological cultural periods, Egyptian historical periods and their age assessments, based on Bietak (1991, 1997).

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The End of the 2nd Millennium BCE and the Transition from Iron I to Iron IIA: Radiocarbon Dates of Tel Rehov, Israel

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Introduction

The Iron Age in Israel covers approximately the last two centuries of the 2nd millennium BCE and continues until the Babylonian conquest in 586 BCE. Division of the Iron Age is based on ceramic styles, architecture and associated relations with literary data. Various systems have been proposed. Until the late 1960s many scholars used the following subdivision: 1200–920/900 BCE for Iron Age I and 920/900–586 BCE for Iron Age II (Wright 1961:96–101).[1] Following the excavations at Hazor (Yadin 1961), a new division was proposed by Aharoni and Amiran (1958). They suggested a tripartite division of the Iron Age (which they called "Israelite Period") into three subperiods: I) 1200–1000; II) 1000–84; III) 840–587. Henceforth, the date 1000 BCE for the transition from Iron I to Iron II was accepted in all major publications.

The term Iron IIA was used by some to include both the 10th and 9th century, such as Ben-Tor (1992:2) and Barkay (1992:305), as well as Mazar since 2001 (Mazar and Carmi 2001:1340–1341; Coldstream and Mazar 2003:40–44). Others confined the Iron IIA period only to the 10th century, mainly referring to the time of the Israelite United Monarchy of David and Solomon (Stern 1992:1529; Mazar 1990; Herr 1997).

The discovery of a ceramic assemblage similar to Iron IIA in the destruction layer of Jezreel (Ussishkin and Woodhead, 1994), a royal citadel of the Omride Dynasty (9th century, *ca* 885–843 BCE), led to the renewal of an old debate concerning the attribution of pottery and architectural assemblages to either the 10th or the 9th century BCE. Finkelstein (1996) renewed the ideas of Crowfoot (1940) and to some extent Kenyon (1964) – the latter only concerning the pottery – that the cultural horizon attributed by most scholars to the 10th century should be associated with the Omride Dynasty in the 9th century. He also claimed that the boundary between Iron I and Iron IIA should be lowered by about 80 years, from *ca* 1000 BCE to *ca* 920 BCE. The historical implications are obvious: all the buildings and cultural assemblages related by various scholars to David and Solomon should be attributed to the Omride Dynasty. Thus the "archaeology of the United Monarchy" is allegedly eliminated. The lowering of the Iron Age chronology gave legitimacy to skeptic evaluations of the historicity of the United Monarchy (Finkelstein 1999). This suggestion raised fierce archaeological controversy, in which chronology is at the heart of the debate (Mazar 1997; Ben-Tor 2000; Ben-Tor and Ben-Ami 1998; Finkelstein 2002).

Radiocarbon dating did not play any role in these archaeological time assessments and divisions of the Iron Age. The need for an independent, high-precision, ^{14}C chronology in Near Eastern archaeology of the Bronze and Iron Ages was advocated in the 1980s by Bruins and Mook (1989). Gradually a growing awareness developed concerning the potential significance of such impartial chronological measurements for the Iron Age dilemma. However, chronological questions within a time resolution of one century are at the limit of single calibrated ^{14}C dates, due to the wiggles in the calibration curve. Stratified series may have a far better time-resolution potential, but quality control through repeated measurements and interlaboratory comparison are essential in the high precision and accuracy demands of the historical periods of the Near East (Van der Plicht and Bruins 2001).

Investigations by Mazar and Carmi (2001) gave ambiguous results concerning ^{14}C dates from Iron Age layers at Tel Beth Shean and Tel Rehov. Some dates measured in the Rehovot Radiocarbon Laboratory

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[1] The date 920 or 918 BCE was related to the military campaign by Shoshenq I (biblical Shishak) in the Land of Israel, documented both at Karnak (Egypt) and in the Hebrew Bible.

appear too young by a large margin of a few centuries, too low even for the Low Chronology (such as those from Phase D-3; Mazar and Carmi 2001:1336; concerning the 18 dates from Locus 2425 see below, p. 91). Gilboa and Sharon (2001) published the results of radiocarbon measurements of charcoal samples from Tel Dor (Sharon 2001), which were accepted by them as evidence to support the Low Chronology, because many of the radiometric dates were up to a century lower than the conventional ceramic chronology. We note that all these dates were measured by the Rehovot Radiocarbon Laboratory. In addition, the Tel Dor samples in the above investigation consist of multi-year charcoal with a possible old wood effect. Hence the age difference of the young radiocarbon dates in comparison with the conventional chronology is even more pronounced! Gilboa and Sharon (2001:1343) succinctly described some principal repercussions of the Low Chronology: (a) "11th century archaeological strata and various material phenomena should be assigned to the 10th, and 10th century ones to the 9th"; (b) This "would by and large 'rob' the United Monarchy of material remains compatible with an organized state". This claim is based on ^{14}C dates of charcoal samples, measured by only one laboratory.

Quality control of radiocarbon dating is not a minor issue, particularly when high temporal resolution is required. Radiocarbon laboratories are usually aware of the need to evaluate the quality of the dates they produce. "The two questions of reliability and reproducibility of routinely acquired ^{14}C dates have been and continue to be of interest to both providers and users. One of the most direct means of assessing these properties has been through organized interlaboratory comparisons" (Scott et al. 1998:331). Results of the Third International Radiocarbon Intercomparison (TIRI) and the Fourth International Radiocarbon Intercomparison (FIRI) have recently been published (Scott, 2003).

A detailed, high-quality series of radiocarbon dates from Tel Rehov, was obtained at the Centre for Isotope Research, University of Groningen (The Netherlands), using two different ^{14}C laboratories: (1) Conventional measurements of radioactive carbon using Proportional Gas Counting (PGC) and (2) Accelerator Mass Spectrometry (AMS). All samples were pretreated with the acid/alkali/acid (AAA) method (Mook and Waterbolk 1985). The purified organic matter of each sample was subsequently converted into CO_2. For AMS dating (Van der Plicht *et al.* 2000), the CO_2 gas underwent additional treatment to convert it into solid graphite. The consistent series of radiocarbon dates, based on charred short-lived samples, enabled stratigraphic wiggle matching (Bruins, Van der Plicht and Mazar 2003a). The results show a sequential time-series that ranges from the 12th to the 9th centuries BCE. The calibrated radiocarbon dating sequence gives independent evidence, based on scientific methods and independent from historical or biblical data, that Iron Age IIA pottery continued to be in use for a rather long period, covering large parts of *both* the 10th and 9th century BCE, thus supporting the time frame suggested by Aharoni and Amiran and accepted by Barkay, Ben Tor and lately by Mazar (references above).

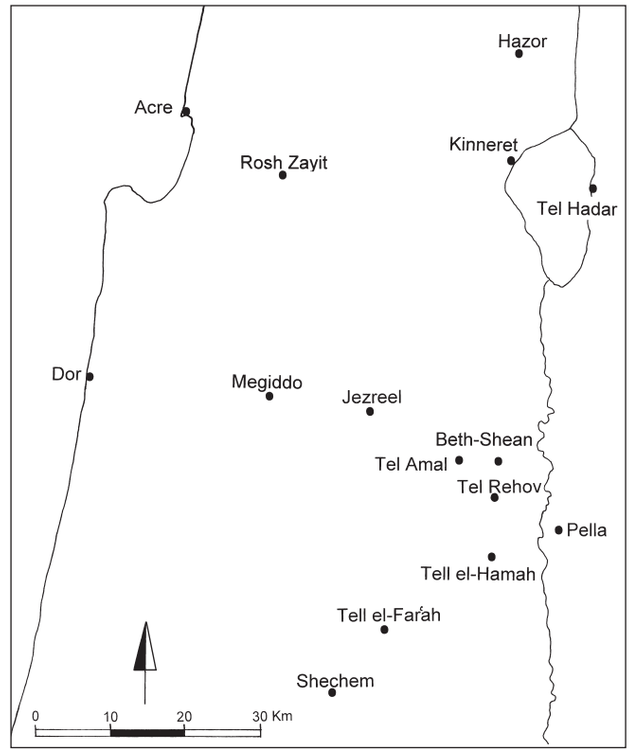


Fig. 1 Location map of Tel Rehov

Finkelstein and Piasetzky (2003a, 2003b) contested our conclusions, trying to place the Groningen radiocarbon dates into the Low Chronology system. We rejected their arguments in a technical reply (Bruins, Van der Plicht and Mazar 2003b). In the current article we have the opportunity to present our results in much more detail than was possible in the articles published in Science. Following the arguments by Finkelstein and Piasetzky (2003a, 2003b), we need to compare the Groningen radiocarbon results in detail with the Rehovot radiocarbon dates. In this article, we focus our attention on the radiocarbon chronology of the Iron I period and the transition to the Iron Age IIA period. The reasons are twofold (1) The SCIEM 2000 Program is directed at the 2nd mil-

Final Stratum	Area D	Area C	Area E	Area F	Area G	Area B	Area A	Period	Age assessment
	–	–	E-0			B-0	A-1a	Late Arab	Undated burials
I	–	–				B-1	A-1b	Early Arab	8–12th century CE
II	–	–				B-2	A-2	Iron Age IIC	Late 8th early 7th(?) century BCE
III	–	–				B-3	A-3a A-3b	Iron Age IIB	Until 732 BCE.
IV	–	C-1a	E-1a	F-1	G-1	B-4*	A-4	Iron Age IIA	Until ca. 830–840 BCE
V	D-1(?)	C-1b	E-1b	2	2	5a* 5b			From ca. 980
VI	D-1(?)–D-2	C-2	E-2	F-3-4	G-3	B-6			
VII	D-3 D-4 D-5	C-3						Iron Age IB	Until 990–980 BCE ca. 1130 (?) BCE
	D-6 D-7							Iron Age IA	12th century BCE
	D-8 D-9a D-9b							Late Bronze IIB	13th century BCE
	D-10							Late Bronze I–IIA	15th–14th century BCE
	D-11							MB/LBI	16th century BCE

* Needs further clarification in the future. It is possible that 5a should be correlated with general Stratum IV

Table 1 Stratigraphic table of Tel Rehov, showing the correlation between local phases in the different excavation areas and the general strata numbers

lennium BCE. (2) The amount of radiocarbon dates and required figures, as well as the archaeological context, are too much for detailed treatment in one article. A future paper will deal more extensively with the Iron Age IIA period (Mazar, Bruins, Panitz-Cohen and Van der Plicht in preparation).

Radiocarbon Dates of Tel Rehov

Tel Rehov is a large site, covering 10 hectares, situated at an important geographic junction, 28 km south of Lake Kinneret and 5 km south of Beth Shean (Figure 1). Six excavation seasons in the years 1997 to 2003 revealed complex stratigraphic sequences in seven excavation areas, yielding rich material assemblages, mainly from the Iron Age IIA period (Mazar 1999; Mazar 2002; Mazar in press).[2] The first two series of radiocarbon measurements, consisting altogether of 24 dates from seven loci, were measured during the years 1998–1999 in the ^{14}C Laboratory of the Weizmann Institute in Rehovot and the AMS Laboratory of the University of Arizona (Mazar and Carmi 2001). Another series of more than 30 dates from 14 loci were measured during 2001–2002 at the University of Groningen (Bruins, Van der Plicht and Mazar 2003a, 2003b). Both conventional and AMS measurement techniques were used at the latter institution, according to sample size and for quality control. All these dates together constitute the largest group of radiometric measurements from a single Iron Age site in the Levant.

[2] The excavations at Tel Rehov, directed by A.Mazar, are conducted under the auspices of the Institute of Archaeology of the Hebrew University and sponsored by Mr. John Camp. Our thanks to Mr. Harm-Jan Streurman, Mrs. Anita T. Aerts-Bijma and Mr. Stef Wijma for carefully preparing and measuring the radiocarbon samples at the Centre for Isotope Research of Groningen University.

The great majority of the organic materials used in the ^{14}C determinations consist of short-lived charred grains and olive pits. Most samples came from the best stratigraphic contexts available in each stratum, as explained in detail below.[3] The relative stratigraphy in each of the excavation areas at Tel Rehov is shown in Table 1. Iron Age I layers were excavated so far only in Area D and thus the local phase numbers of this area were retained. The Iron II layers were excavated at several areas of the tell, each area with its own respective stratigraphic phase numbers. An attempt was made to correlate these local stratigraphic phases into a comprehensive overall strata framework (marked in Roman numerals in the 1st column of Table 1). This correlation may become further refined in future excavations.

The Iron Age I

The excavations at Tel Rehov exhibited successive layers of the Iron Age I period in Area D (Figure 2 and 3), a 5 m wide step trench on the western slope of the lower city (Mazar 1999:9–16). This trench yielded 11 stratigraphic phases (D11–D1), as summarised in Table 1. The lower four phases are characterised by Late Bronze Age pottery. The five following phases yielded Iron Age I pottery while the uppermost two phases yielded Iron IIA pottery.[4] Area D is attached to Area C, and thus the last two phases can be correlated with the large exposures of Iron IIA strata in Area C.

Phase D-6

Phases D-7 and D-6 yielded pottery similar to the ceramic horizon of nearby Tel Beth Shean Level VI (University of Pennsylvania excavations) or Strata S-4 and S-3 (the Hebrew University excavations; Mazar 1993; 2003a: 324, 333–337). These levels at Beth Shean can thus be securely dated to the time of the 20th Dynasty, most probably related to the reigns of Pharaohs Ramesses III to VI. Phase D-7 exhibited several walls, floors and two foundation deposits with lamp and bowls, typical for the period under consideration, yet no charred organic remains were found. The later Phase D-6, which yielded charred seeds for radiocarbon dating, displayed several floor surfaces but very little architecture. The small amount of pottery from Phase D-6 included also two sherds of imported Mycenaean IIIC pottery, just like at Tel Beth Shean in the correlated levels.

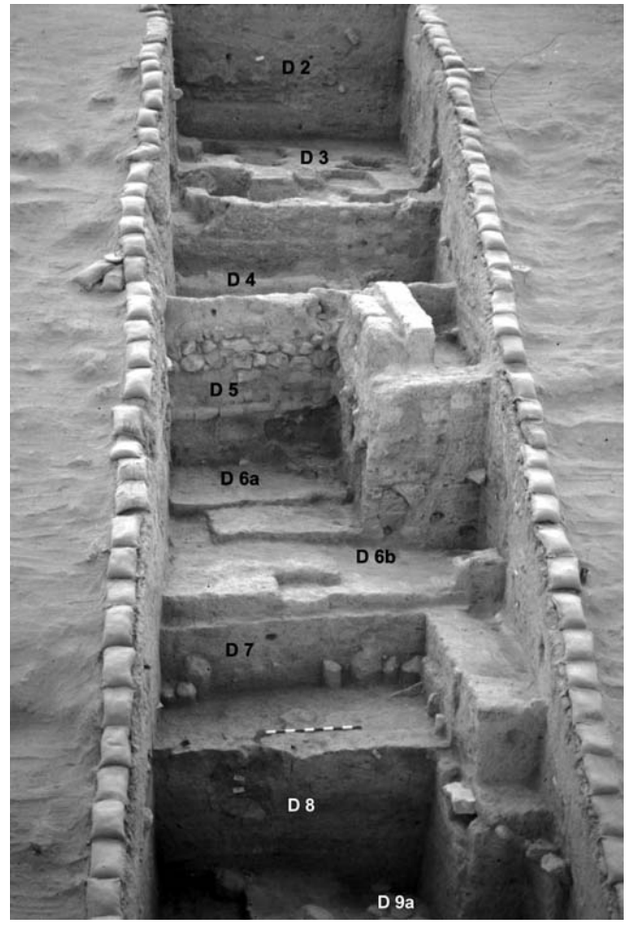


Fig. 2 Area D of Tel Rehov; the Late Bronze Age is exhibited in the lower part of the section, while the Iron Age phases 6, 5, 4, 3 and 2 (discussed in the text) appear in the upper part

Charred olive pits found in Locus 2874 and Locus 2836 of Phase D-6 were dated in Groningen by both Conventional (Proportional Gas Counting – PGC) and AMS techniques (laboratory codes GrN and GrA, respectively). Locus 2874 constitutes a plastered basin sunk from a floor in Square P4, sealed by brick debris. The other Locus 2836 from Square N4 is a layer of occupation debris that includes organic deposits and ash patches at levels 83.39–82.86. The Groningen AMS and PGC dates of Locus 2874 were 2935 ± 45 BP (GrA-19034) and 2880 ± 30 BP (GrN-26120), respectively (Table 2). The results are well

[3] Finkelstein and Piaseztky (2003a, 2003b) criticize some of our contexts as unreliable. We reject this criticism, as we explain the nature of each context in some detail in the following paragraphs.

[4] We use here the term Iron IA for the period of the Egyptian 20th Dynasty, following the terminology in Stern (ed.) 1993. Some Israeli scholars include this period in the Late Bronze Age (Ussishkin 1985; Gilboa and Sharon 2003).

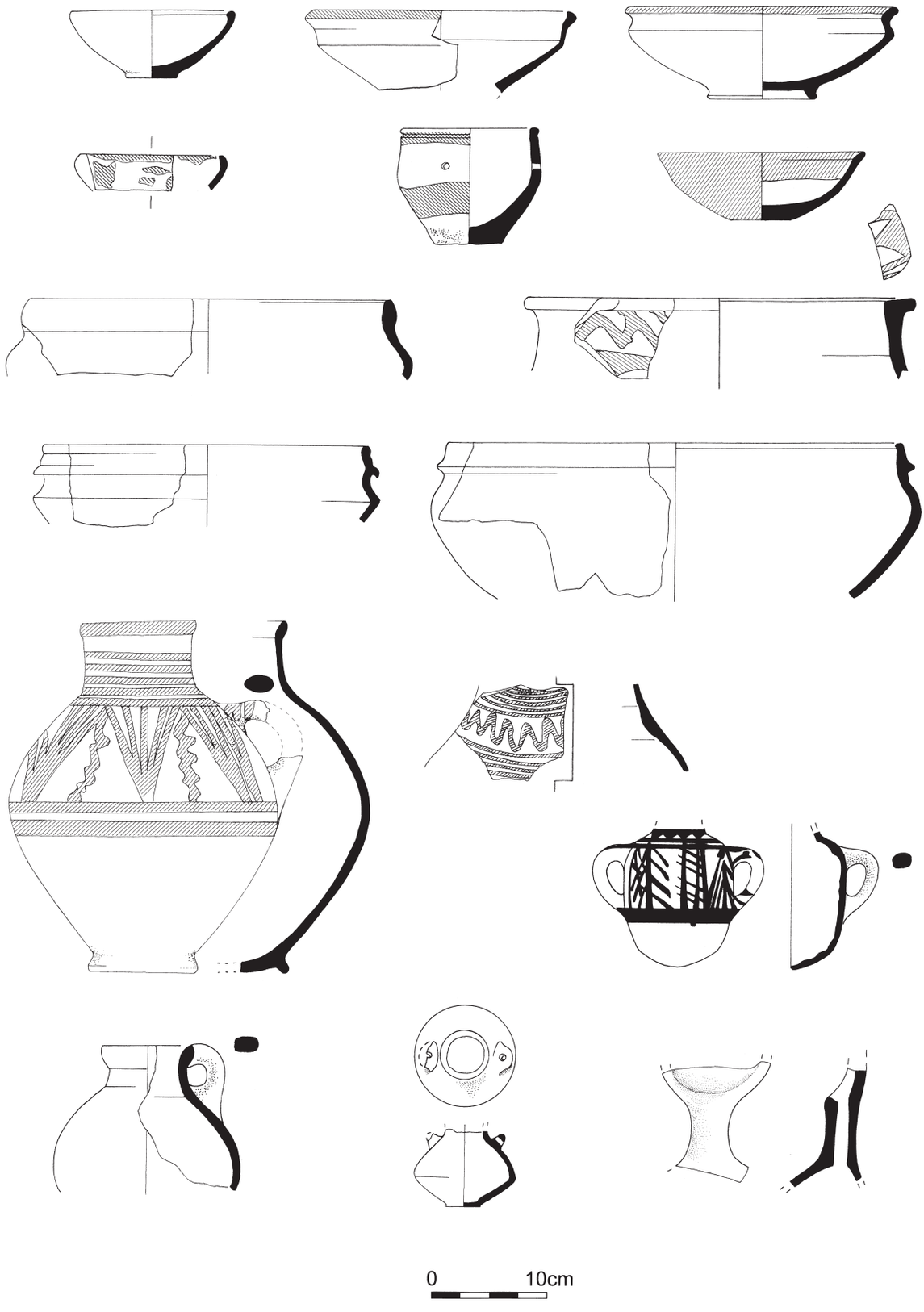


Fig. 3a Selected pottery forms from Phase D-4

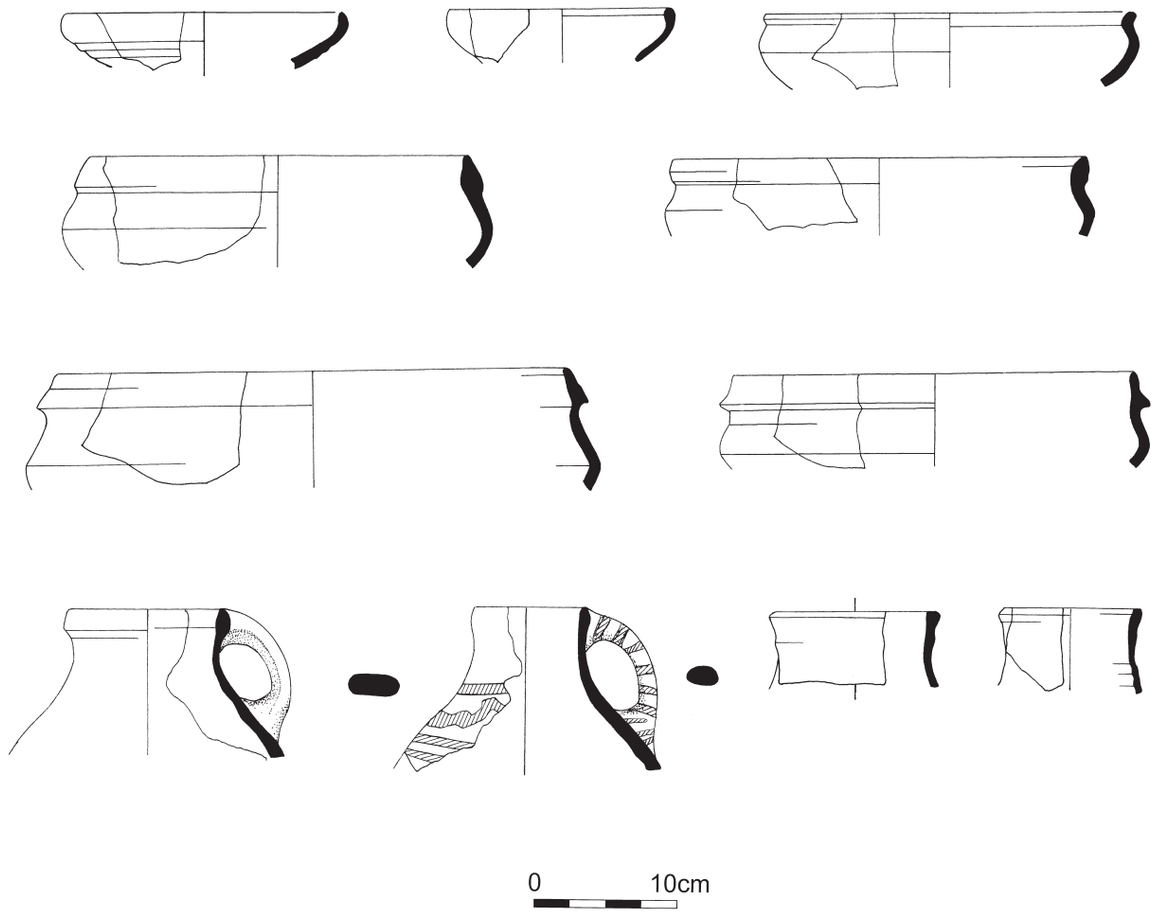


Fig. 3b Selected pottery forms from Phase D-3

within 2σ from each other and therefore acceptable. The weighted average has a smaller standard deviation 2897 ± 25 BP. The Groningen dates of Locus 2836 were 2950 ± 50 BP (GrA-18826) and 2920 ± 30 BP (GrN-26118), respectively by AMS and PGC. The two results are even closer, within 1σ from each other. The average date is 2928 ± 26 yr BP.

The calibration curve remains at a rather similar level, albeit with many wiggles, for much of the 13th to 11th century BCE (radiocarbon year period of about 3020–2880 BP). Therefore, the calibration of individual loci will inevitably result in wide age ranges. The undifferentiated 1σ calibrated range for the average date of Locus 2874 is 1125–1020 BCE, while the more precise average result of Locus 2836 results in a 1σ calibrated range of 1209–1050 BCE. Looking again at the four individual dating results for Phase D-6 in conventional BP years, three are almost similar (2920 ± 30, 2950 ± 50, 2935 ± 45), while one is somewhat younger (2880 ± 30). The average of Locus 2836, therefore, may reflect Phase D-6 more accurately. Its full calibrated range is graphically shown in Figure 4. Considering the stratigraphic sequence and the ceramic assemblage, the period 1130–1090 BCE within the calibrated result seems most likely for this stratum.

A sample of charred olive pits found on a floor surface in the same phase (Locus 1876) was dated at the Weizmann Institute in Rehovot (RT-3119; Mazar and Carmi 2001:1336). The resulting date of 2685 ± 40BP is 243 years younger than the Groningen average. Its calibrated date in the 9th century BCE is much too young by all standards as already mentioned (Mazar and Carmi 2001:1336).

Phases D-5 and D-4.

Phases D-5 and D-4 constitute two distinct architectural periods, characterised by a north-south oriented street flanked by buildings on both sides. The occupation debris of D5, sealed by that of D-4, has hardly been excavated. However, living surfaces of Phase D4 were excavated in the street as well as in a courtyard paved with cobble stones during its initial phase. The original floor surfaces in both cases were

Phase	Locus	Basket	Charred Organic Material	Lab No	^{14}C Date (BP)	d^{13}C (‰)	^{14}C Date BP or Average used in calibration	1σ Calibrated Date 1998 Curve OxCal v3.9 (year BCE)	2σ Calibrated Date 1998 Curve OxCal v3.9 (year BCE)
D-2 Iron IIA	1802	18119	Olive pits	GrN-26112	2805 ± 15	–22.46	same	997–989 (8.5%) 974–954 (25.9%) 944–919 (33.8%)	999–913 (91.8%) 912–905 (3.6%)
D-3 Iron IB–IIA transition	1858	28395	Olive pits	*RT-3120*	*2670 ± 40*	*–20.80*	*same*	*892–880 (11.4%)* *836–799 (56.8%)*	*900–795 (95.4%)*
D-3 Iron IB–IIA transition	2862	28493	Olive pits	GrA-19033 *GrN-26119*	2835 ± 45 *2720 ± 30*	–23.03 *–22.33*	2835 ± 45	1046–920 (68.2%)	1128–896 (93.5%) 877–857 (1.9%)
D-3 Iron IB–IIA transition	4815	48105	Olive pits	GrA-16757	2820 ± 50	–22.51	same	1042–1031 (3.8%) 1022–901 (64.4%)	1126–891 (87.0%) 881–835 (8.4%)
D-3 Iron IB–IIA transition	4816	48103	Olive pits Charcoal	GrA-12889 GrA-16848	2870 ± 70 2895 ± 40	–25.29 –24.41	2870 ± 70	1188–1181 (2.1%) 1150–1144 (1.6%) 1128– 970 (55.2%) 960– 928 (9.3%)	1260–1228 (3.8%) 1221– 894 (88.8%) 878– 839 (2.9%)
D-3 Iron IB–IIA transition	4830	48115	Olive pits	GrA-21044 GrA-21056 GrA-21183	2845 ± 35 2825 ± 35 2820 ± 50	–22.05 –23.30 –23.35	2832 ± 22	1004–970 (40.0%) 959–934 (28.2%)	1044–918 (95.4%)
D-4 Iron IB	1836	48450	Olive pits	GrN-26121 GrA-18825	2890 ± 30 2870 ± 50	–22.95 –22.99	2885 ± 26	1125–1119 (3.3%) 1112–1098 (10.0%) 1087–1059 (18.0%) 1053–1005 (36.9%)	1189–1179 (2.9%) 1154–1143 (2.2%) 1129– 996 (86.3%) 990–974 (2.5%) 955–943 (1.6%)
D-4 Iron IB	1845	48556	Seeds	GrA-21046 GrA-21057 GrA-21184	2905 ± 35 2945 ± 35 2920 ± 50	–22.49 –23.10 –24.12	2924 ± 22	1208–1202 (3.6%) 1189–1179 (7.2%) 1154–1142 (8.1%) 1129–1108 (15.1%) 1102–1067 (23.7%) 1065–1050 (10.4%)	1254–1244 (2.3%) 1212–1199 (6.2%) 1192–1139 (25.7%) 1132–1020 (61.2%)
D-4 Iron IB	1845	28243	Olive pits	*RT-3121*	*2800 ± 40*	*–21.10*	*same*	*999– 902 (68.2%)*	*1044–887 (81.6%)* *884–833 (13.8%)*
D-6 Iron IA	1876	28536	Olive pits	*RT-3119*	*2685 ± 40*	*–20.70*	*same*	*896–876 (18.1%)* *858–852 (4.2%)* *841–802 (45.9%)*	*905–797 (95.4%)*
D-6 Iron IA	2836	28352	Olive pits	GrN-26118 GrA-18826	2920 ± 30 2950 ± 50	–22.28 –22.46	2928 ± 26	1209–1201 (5.1%) 1190–1178 (7.9%) 1159–1141 (11.3%) 1130–1108 (13.8%) 1102–1067 (20.8%) 1066–1050 (9.2%)	1257–1239 (4.9%) 1213–1197 (7.4%) 1194–1138 (28.1%) 1133–1018 (55.0%)
D-6 Iron IA	2874	28901	Olive pits	GrA-19034 GrN-26120	2935 ± 45 2880 ± 30	–22.14 –22.36	2897 ± 25	1126–1040 (60.6%) 1032–1020 (7.6%)	1210–1200 (2.0%) 1191–1177 (4.5%) 1161–1141 (4.6%) 1130–1000 (84.3%)

Table 2 Radiocarbon dating results from Area D at Tel Rehov, showing stratigraphic relationships. Most dates are from Groningen (GrN signifies measurement by PGC and GrA by AMS). All three dates from Rehovot (RT) are much younger and should be rejected in this intercomparison of three labs. The 18 Groningen dates in this list had only one outlier (GrN-26119), which is also rejected (all rejections in *italics*). The other 17 Groningen dates constitute a coherent set of measurements

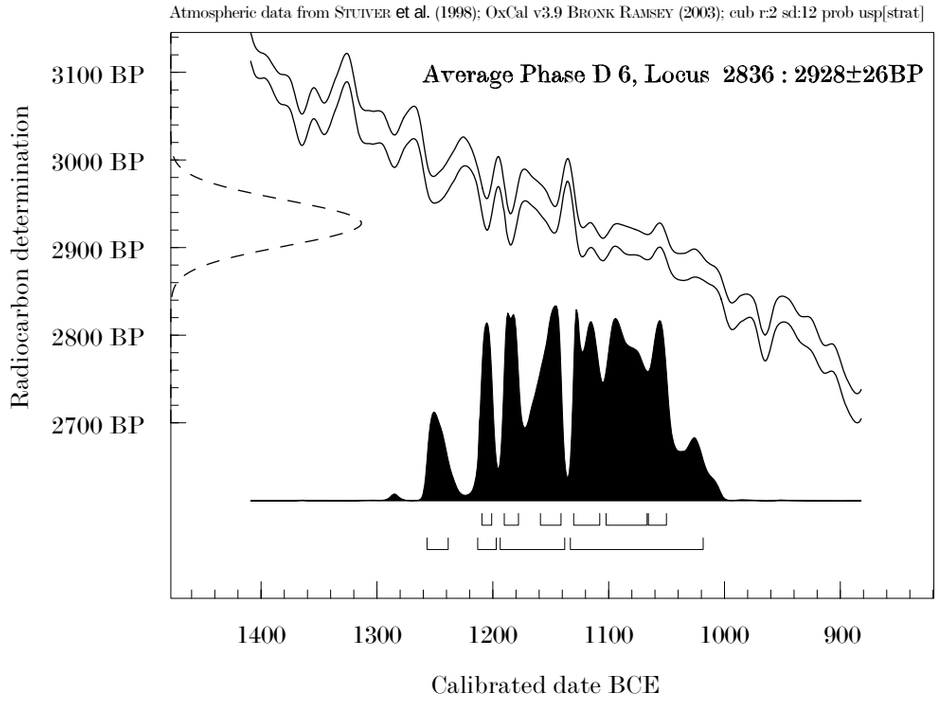


Fig. 4 Calibrated date of Iron IA, Phase D6, Locus 2836, using OxCal v3.9 (Bronk Ramsey 2003) and the INTCAL 98 calibration curve (Stuiver and Van der Plicht 1998). (Concerning the subsequent calibration figures, OxCal and INTCAL 98 are also used, see always above graph, but not mentioned repeatedly in the lower caption)

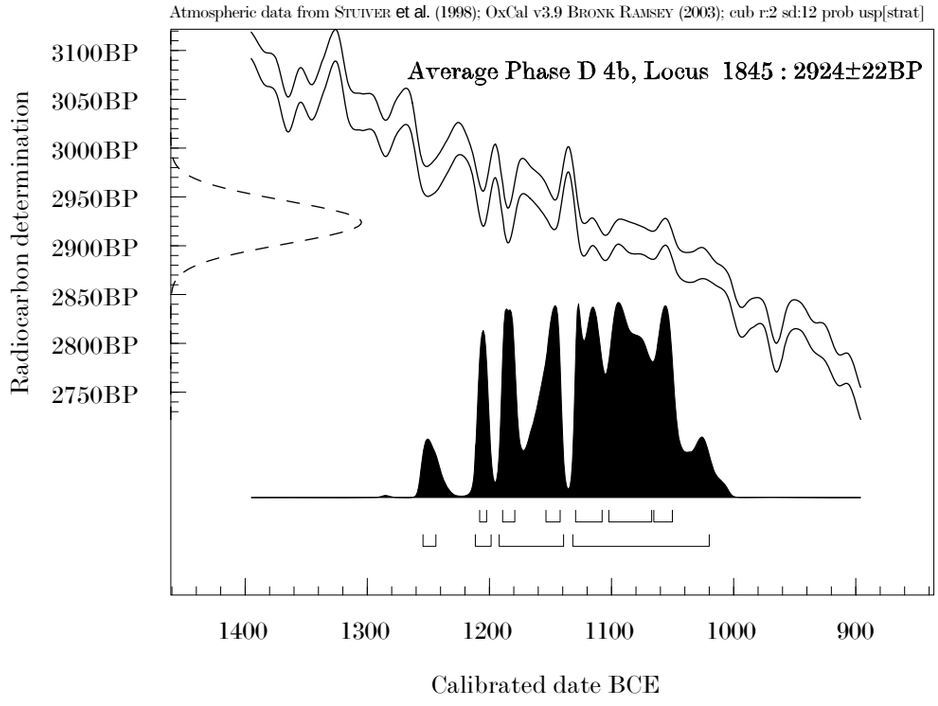
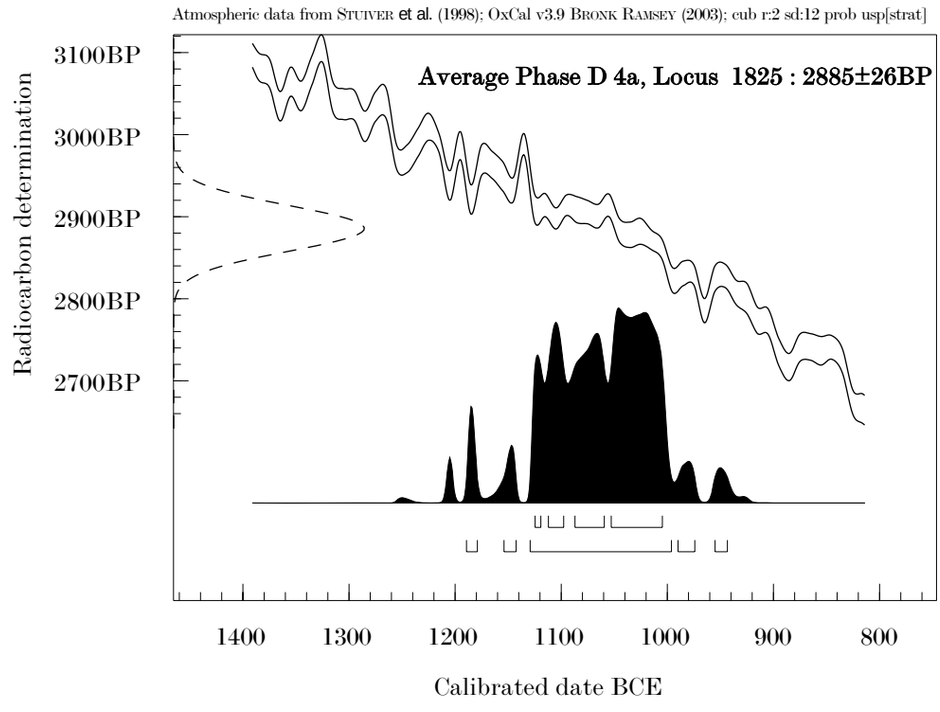


Fig. 5 Calibrated date of Iron IB, Phase D4b, Locus 1845



Fig, 6 Calibrated date of Iron IB, Phase D-4a, Locus 1825

raised from time to time during the duration of Phase D-4, resulting in a succession of striated surfaces. The ceramic assemblage from this phase is typical for the Iron Age IB period(Fig. 3a; for further discussion of the pottery see Mazar *et. al.* 2005).

A sizable cluster of charred seeds was found on the cobble floor of the courtyard (Locus 1845) of Phase D-4b, below thick accumulation of occupation striations. Therefore, the archaeological context of the seeds is first grade. The seeds were dated at the Weizmann Institute in Rehovot and also at the University of Groningen. The ^{14}C determination at Rehovot gave a date of 2800 ± 40 BP (RT-3121) (Mazar and Carmi 2001:1336 Table 3), while three AMS dates of seeds from the same locus in Groningen gave consistently older dates: 2905 ± 35 BP (GrA-21046), 2945 ± 35 (GrA-21057) and 2920 ± 50 BP (GrA-21184). The Groningen results are close to each other, mostly within 1σ, indicating high-quality dates with robust repeatability (Scott 2003). The Groningen weighted average for Locus 1845 of Phase D4 is 2924 ± 22 yr BP. The single Rehovot date is about 124 radiocarbon years younger, which would result in a calibrated age range in the 10th century BCE, which is generally considered too young for Iron IB. However, the consistent Groningen results give an older calibrated 1σ age range of 1208–1050 BCE (Fig. 5) for Phase D-4b.

The principle of stratified archaeological wiggle matching (Bruins, Van der Plicht and Mazar 2003a) demands that successive layers cannot have the same position on the calibration curve but must follow each other in time. Taking into consideration the stratigraphic sequence of Phase D-4 above the unexcavated Phase D-5 and the position of the latter above the dated Phase D-6, a date of ca. 1100–1050 BCE seems most likely for Phase D-4b.

In Locus 1825 (Phase D-4a), a cluster of charred olive pits was found on the highest floor surface among those that occur above cobble floor 1845. The olive pits were dated in Groningen by both the PGC and AMS lab facilities. The results of the same sample by these two different ^{14}C measurement techniques gave very similar dates, which underline the outstanding reproducibility (Scott et al. 1998; Scott 2003) status of both facilities in Groningen. The PGC result is 2890 ± 30 yr BP (GrN-26121) and the AMS result is 2870 ± 50 yr BP (GrA-18825), giving a weighted average of 2885 ± 26 yr BP which is about 39 radiocarbon years younger than the Phase D4b sample found on the cobble floor below. This may provide a reasonable time range for the accumulation of floor surfaces in the same place during phases D-4b and D-4a.

A reasonable date for Phase D-4a within the calibrated age range (Fig. 6), considering the BP difference (40 yr) with cobble floor 1845 and the principle of

stratified archaeological wiggle matching, would be *ca* 1060–1010 BCE. The Groningen radiocarbon dates agree very well with the conventional archaeological dates for the Iron IB pottery, found in Phase D-4.

Phase D-3

Phase D-3 in Area D included a very peculiar feature: more than 30 small and shallow pits were found concentrated in an accumulation layer less than one meter thick. Some of the pits penetrated the underlying destruction debris of Phase D-4 and also cut each other. The human activities recorded by these pits seem to represent a considerable time span. Some of the pits were plastered with thin white plaster. The pits contained only small amounts of pottery sherds of homogeneous types, which can be classified to the end of Iron I (Fig. 3b). The layer of pits is sealed by Locus 1802 (Phase D-2), a thick layer of refuse that is characterised by distinct Iron IIA pottery, related to Strata V and VI in the adjacent Area C.

The pits were probably used for refuse disposal and/or food storage. Each pit of Phase D-3 represents a homogeneous feature from a stratigraphic viewpoint, situated in between D-4 and D-2. It does not mean of course that each pit was made and used exactly at the same time, yet the entire accumulation of pits is sealed from above and below by distinct stratigraphic features. The organic samples found in these pits are not isolated single seeds but clusters of olive stones, albeit in small concentrations. Thus there is no foundation for the charge by Finkelstein and Piasetzky (2003a) that these pits are an unreliable context for our ^{14}C dates (Bruins, Van der Plicht and Mazar 2003a, 2003b).

Phase D-3 – Pit 4830

A cluster of charred olive stones was found (basket 48115) at the bottom level (85.15) of a shallow pit (Locus 4830) in Square Q4 within levels 85.35–85.15. The sample was investigated in Groningen, where, following standard pre-treatment, three sub-samples were prepared for measurement by AMS. The triplicate results are as follows: 2845 ± 35 BP (GrA-21044), 2825 ± 35 BP (GrA-21056), 2820 ± 50 BP (GrA-21183). The three measurements are very close, within 1σ from each other, which underlines the measurement repeatability quality of the AMS facility. The weighted average date of the three measurements is 2832 ± 22 BP.

The calibrated age (Fig. 7) gives two possible options in the 1σ range: 1004–970 (40.0%) and 959–934 (28.2%). The first period 1004–970 has clearly the highest relative probability. This result fits very well with the suggestion by Mazar (above) to place the boundary for the transition from Iron I to Iron IIA at about 980 BCE. Both calibrated radiocarbon date options are clearly too old for the Low Chronology theory, which places the above transition approximately at 920 BCE.

Phase D-3 – Pit 4816

This is a plastered pit cut into mudbrick wall 4859 of Phase D-4. It had two plastered surfaces inside, and the sample (Basket 48103) of charred olive stone fragments and fine charcoal was found between these two surfaces at level 85.67m. Thus this is a closed context. The charred olive stone fragments were dated in Groningen by AMS, giving a date of 2870 ± 70 BP (GrA-12889). The amount of material was small, even for AMS, resulting in a somewhat larger standard deviation. The fine charcoal was dated separately, also by AMS, giving a date of 2895 ± 40 BP (GrA-16848). The results are close to each other, within 1σ, which gives confidence in their reliability, while the charcoal is slightly older than the olive pit fragments, as might be expected. Looking closer at the date for the olive pit fragments, the BP result is the same as the youngest date (GrA-18825) for Phase D4a. Therefore, this pit (Locus 4816) is older than the former (Locus 4830) and reflects human activities in the 11th century BCE.

Phase D-3 – Pit 4815

This is a non-plastered pit within levels 85.53–84.76, in which a cluster of 15 charred olive stones was found at level 85.11 (Basket 48105). Some of these olive stones were measured in Groningen by AMS, yielding a date of 2820 ± 50 BP (GrA-16757). This dating result is very similar as those for Pit 4830. As we have only one AMS date, the relatively large standard deviation of 50 BP years results in a calibrated date with a wide age range (Fig. 8). The 1σ ranges are 1042–1031 (3.8%) and 1022–901 (64.4%) BCE.

The time resolution of the radiocarbon date in this case is not sufficient to distinguish unambiguously between 980 or 920 BCE as possible dates for the transition between Iron I and Iron IIA, as both options fall within the 1σ result. Nevertheless, 980 BCE is clearly situated in the central most likely part of the calibration result, whereas 920 BCE is located at the young margin of the ^{14}C outcome (Fig. 8), which has a lower probability level.

This particular result underlines the significance of high-precision dates with a low standard deviation (Bruins and Mook 1989; Van der Plicht and Bruins 2001). Such results can be obtained with the PGC tech-

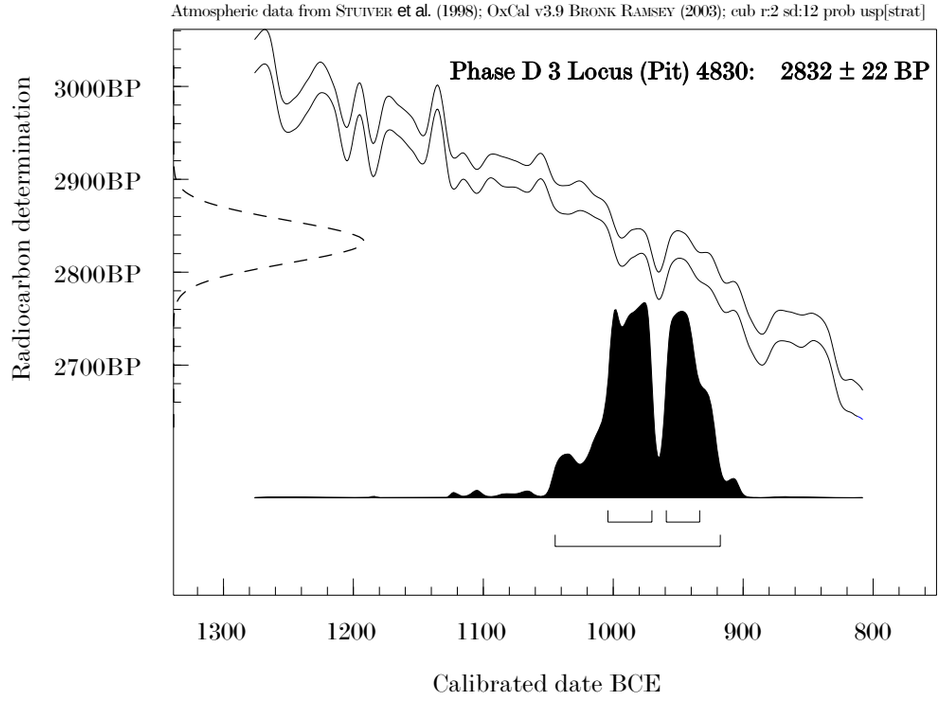


Fig. 7 Calibrated date of one of the storage/refuse pits (Locus 4830) in Phase D-3

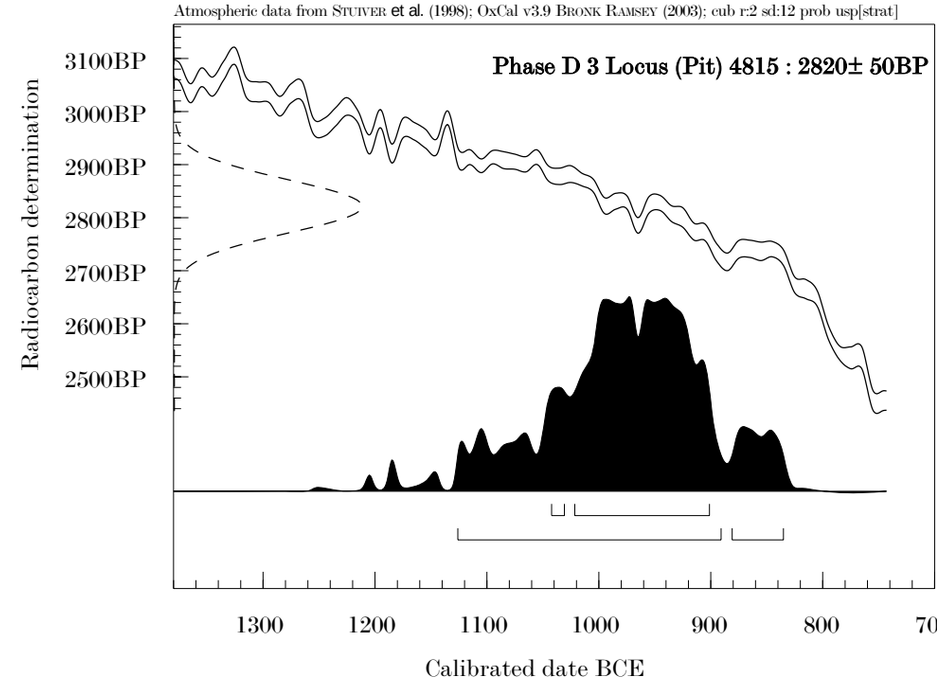


Fig. 8 Calibrated date of one of the storage/refuse pits (Locus 4815) in Phase D3

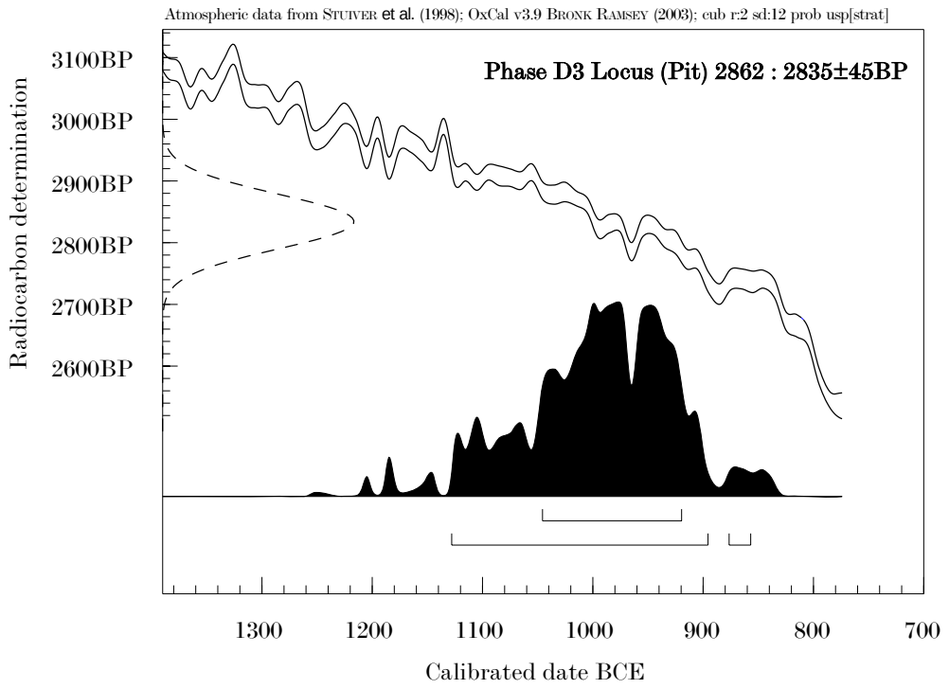


Fig. 9 Calibrated date of one of the storage/refuse pits (Locus 2862) in Phase D3

Stratum	Area and Local Phase	Locus	Lab No	^{14}C Date (BP)	^{14}C Date BP or Average used in calibration	1s Calibrated Date 1998 Curve OxCal v3.9 (year BCE)	2 s Calibrated Date 1998 Curve OxCal v3.9 (year BCE)	Ceramic type and context
IV	C-1a	5498	GrA-21152 GrA-21154 GrA-21267	2770 ± 50 2730 ± 50 2760 ± 35	2755 ± 25	918–893 (25.0%) 879–837 (43.2%)	970–959 (6.0%) 934–830 (89.4%)	Building F Stratum IV Destruction
V	B-5	4218	GrA-21034 GrA-21047 GrA-21179	2760 ± 35 2820 ± 35 2770 ± 50	2786 ± 22	995–991 (1.9%) 973–956 (19.4%) 942–899 (46.9%)	999–895 (83.7%) 877–841 (11.7%)	Stratum V Destruction
V	C-1b	2422	GrN-27361 GrN-27362 GrN-27412	2764 ± 11 2777 ± 13 2785 ± 28	2771 ± 8	969–961 (11.2%) 925–897 (55.7%) 872–870 (1.3%)	970–959 (12.7%) 935–894 (62.0%) 878–840 (20.7%)	Stratum V Destruction
V	C-1b	2425	GrN-26114 GrN-26115	2775 ± 20 2800 ± 20	2788 ± 14	971–959 (20.7%) 935–903 (47.5%)	998–981 (5.7%) 976–896 (86.9%) 876–860 (2.8%)	Building G Silo room Stratum V Destruction
V	C-1b	2441	GrN-26116 GrN-26117 GrN-27385 GrN-27386	2810 ± 20 2775 ± 25 2771 ± 15 2761 ± 15	2776 ± 9	969–960 (15.2%) 925–899 (53.0%)	971–958 (18.2%) 938–895 (64.6%) 877–843 (12.6%)	Building G Stratum V Destruction
VI	C-2	4426	GrN-27366	2761 ± 14	2761 ± 14	919–896 (34.5%) 877–857 (21.4%) 853–841 (12.3%)	969–960 (5.9%) 929–888 (43.4%) 883–834 (46.1%)	Building A Stratum VI
VI	C-2	4426	GrA-21043 GrA-21054 GrA-21182	2755 ± 35 2805 ± 35 2800 ± 50	2784 ± 22	995–991 (2.0%) 973–956 (18.8%) 942–898 (47.4%)	998–981 (6.9%) 977–894 (74.1%) 878–839 (14.4%)	Building A Stratum VI
V/VI	D-2	1802	GrN-26112	2805 ± 15	2805 ± 15	997–989 (8.5%) 974–954 (25.9%) 944–919 (33.8%)	999–913 (91.8%) 912–905 (3.6%)	Refuse deposits from Strata VI and V

Table 3 Calibrated Groningen radiocarbon dates of Tel Rehov Iron IIA Strata, in relation to the stratigraphy and archaeological context

nique if the sample is large enough, usually 20 grams or more. Multiple measurements with AMS of the same organic sample will also enable the calculation of a weighted average with a relatively low standard deviation. The previous Pit 4830 is a good example. The triplicate results 2845 ± 35 BP (GrA-21044), 2825 ± 35 BP (GrA-21056), 2820 ± 50 BP (GrA-21183) gave a weighted average date of 2832 ± 22 BP. Notice that GrA-21056 within this series is exactly identical with the single date for Pit 4815. This suggests that both pits were used in the same time period.

Phase D-3 – Pit 2862

This is a shallow plastered pit, in which a cluster of several charred olive stones was found at level 85.62 (Basket 28493). The sample was dated in Groningen both by AMS and PGC. The AMS measurement gave a date of 2835 ± 45 BP (GrA-19033), which is very similar to the dating results for Pit 4815 and Pit 4830. The Proportional Gas Counter (PGC) produced in this case a rare outlier (difference of about 4σ), which is clearly too young: 2720 ± 30 BP (GrN-26119). Since the difference between the two dates is beyond 2σ, it is not acceptable to calculate a weighted average. The AMS date should be regarded as reliable, fitting many of the other dates for Phase D-3.

The calibrated result of this date (Fig. 9) is again rather wide, due to the standard deviation of 45 years of the BP date (GrA-19033). The 1σ age range is 1046–920 (68.2%) BCE. The time resolution of this result is not sufficient to separate unambiguously between the 980 and 920 BCE options for the transition between Iron I and Iron IIA. However, 980 BCE is again located in the most probable central part of the calibrated date, while 920 BCE is situated at the lower boundary of the 1σ range.

Phase D-3 – Pit 1858

Pit 1858 in Square P4 is a 0.94 m deep pit in which four successive plastered floor surfaces were defined. One sample of olive stones found at the middle level of this pit was measured by the ^{14}C laboratory in Rehovot (RT-3120). The date of 2670 ± 40 BP (RT-3120) is definitely too young and the calibrated age in the 9th century BCE is much too low for the pottery assemblage found in Phase D-3 (Mazar and Carmi 2001:1336).

The Iron Age IIA

The Iron Age IIA period will only be treated in this article in relation to our age assessment for the transition between Iron I and Iron II. The excavations at Tel Rehov exhibited widespread exposures of the Iron Age IIA period in six different excavation areas. Separate stratigraphic phases were counted in each one of these areas (Mazar 1999, for results of the first two seasons). The correlation of the various local phases into a comprehensive stratigraphic frame is not easy, but our current understanding is represented by three general strata numbers: VI, V and IV (Table 1). Both the local phases and general strata numbers are mentioned in Table 3, in relation to the 20 coherent Groningen radiocarbon dates of Iron Age IIA layers (Bruins, Van der Plicht, Mazar 2003a): 16 from Area C, three from Area B and one from Area D (Phase D-2). Another 18 radiocarbon dates from Locus 2425 in Area C were measured at Rehovot and Arizona and published earlier, as well as three dates of charred beams from this period measured at Rehovot (Mazar and Carmi 2001:1337–1339).

Area D – Phase D-2

This phase in Area D included a one meter thick layer of refuse debris (levels 87.19–86.10 m) close to the slope of the mound, containing homogeneous Iron IIA pottery. This refuse was probably dumped from the nearby buildings in Area C (Phases C-2 and C-1b; general Strata VI and V). A cluster of olive stones came from the lowest level of this debris layer (Square D2, Basket 18119, level 86.11).

The single PGC radiocarbon measurement of the relatively large sample of olive pits, conducted in Groningen, resulted in a high-precision date with a low standard deviation: 2805 ± 15 BP (GrN-26112). The calibration of this precise date into calendar years (Fig. 10) gives, however, a wider range, due to the wiggles in the calibration curve. Both the 1σ and 2σ range cover the entire 10th century BCE. This result is, nevertheless, significant, because Iron IIA in the Low Chronology view should be mainly situated in the 9th century BCE, while the above result places Phase D-2 exclusively in the 10th century BCE. Although the last 20 years of the 10th century BCE are still acceptable in the Low Chronology view, this range has a probability of only 3.6% out of a total of 95.4% (= 2σ).

Area C – Stratum VI

The town and associated buildings of Stratum VI in Area C (local Phase C-2) belong to the earlier part of the Iron IIA period. The pottery from this stratum is somewhat different from that of the following two strata (V and IV), yet it definitely belongs to the Iron IIA period. Red slip and burnish are abundant here, but entirely absent in the earlier Iron I period, including the pits of Phase D-3. The samples dated at

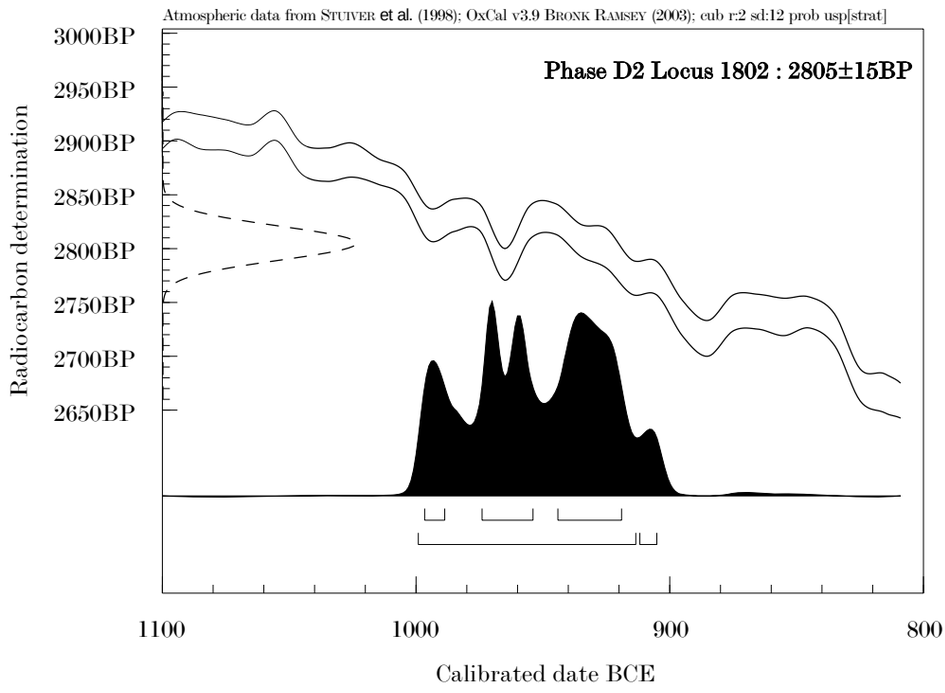


Fig. 10 Calibrated date (GrN-26112) of a large cluster of charred olive stones from the Iron IIA layer in Area D (Phase D-2, Locus 1802)

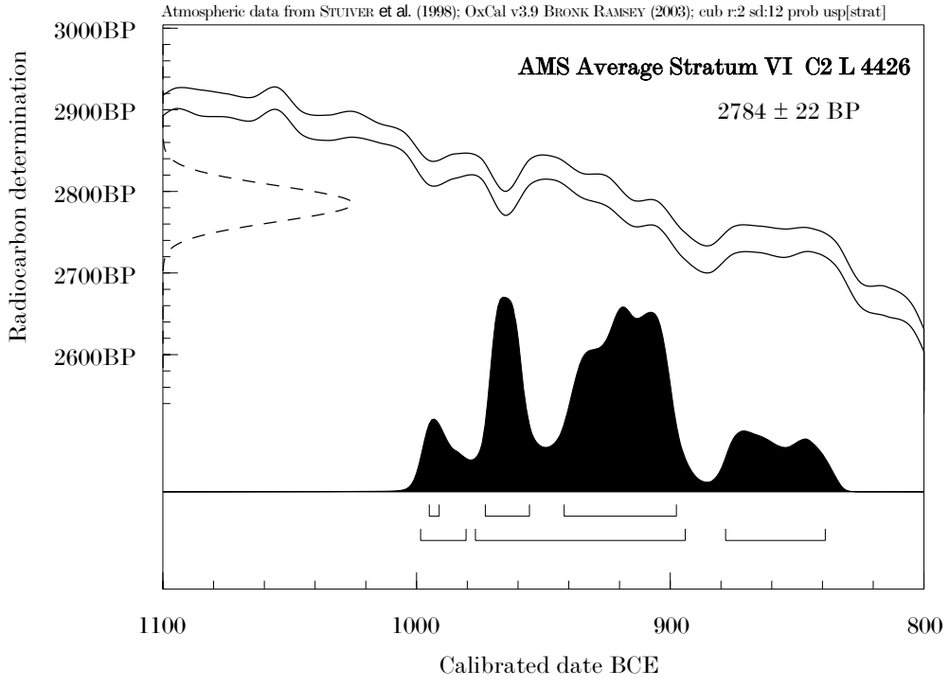


Fig. 11 The average of three AMS dates (2784 ± 22 BP) of Iron IIA Stratum VI in Area C (Phase C2), calibrated into calendar years

Groningen came from Locus 4426 in Building D (Basket 44166, level 85.45), a beaten earth floor in a room, covered by a 0.85 m thick layer of occupation debris and decayed bricks. The sample consisted of a cluster of charred grains found above the floor. The amount of grains was large enough to make one measurement in Groningen with PGC, which resulted in a high-precision date with a small standard deviation: 2761 ± 14 yr BP (GrN-27366).

However, a remaining fraction of the sample, composed of charred broken grains and fine charcoal was dated in triplicate by AMS. One should not be alarmed here by the word "charcoal", because the fine black powder amongst the broken grains is most likely derived from the seeds or associated short-lived material (threshing remains, husks). No wood remains were seen. Two of the AMS dates gave similar results: 2805 ± 35 yr BP (GrA-21054) and 2800 ± 50 yr BP (GrA-21182). A third AMS measurement yielded a somewhat younger date: 2755 ± 35 yr BP (GrA-21043). Since the younger AMS date is still within its 2σ range from the two older dates, the result is statistically acceptable. The two older dates are practically the same as the date for Phase D-2, described above. The weighted average of the three AMS dates is 2784 ± 22 BP.

The difference between the above average AMS date (based on three measurements) and the single PGC date (2761 ± 14 yr BP, GrN-27366) is only 23 radiocarbon BP years. Such a small difference is fully acceptable in the physical sense of radiocarbon dating, as 2761 ± 14 and 2784 ± 22 overlap well within 1σ. However, in archaeological terms, the difference becomes, in this case, significant after calibration in calendar years. The 1σ calibrated age range of the average AMS date is 995–991 (2.0%), 973–956 (18.8%), 942–898 (47.4%) BCE (Fig. 11). This result is almost entirely situated in the 10th century BCE. On the other hand the 1σ calibrated age range of the single PGC date is significantly younger: 919–896 (34.5%), 877–857 (21.4%), 853–841 (12.3%). This gives two distinct options in the 9th century BCE, while the range with the highest relative probability is in the last 20 years of the 10th century BCE.

Looking at the calibration curve (Figure 11), it is clear that the BP date of the AMS average "hits" the calibration curve at two principal spots: (1) the wiggle between 980–950 BCE and the younger area between 940–900 BCE. The latter option has the highest relative probability (47.4%) as compared to 18.8% for the older option of 973–956 BCE. The last 40 years of the 10th century BCE are statistically favoured, because this section represents more "probability volume" in the calibration curve than the section of the wiggle around 970 BCE.

Nevertheless, stratified archaeological wiggle matching does give preference to the "first hit" with the calibration curve around 970 BCE, because successive layers cannot have the same position on the calibration curve but must follow each other in time (Bruins, Van der Plicht and Mazar, 2003a:316). Stratum VI is to be placed on the calibration curve between the position of Phase D-3 (Stratum VII) and Stratum V. The many high-quality dates from the destruction layer of Stratum V (Table 3) clearly favour the last 40 years of the 10th century BCE (Figure 12 and 13). Moreover, the dates for Stratum V relate to the very end of this city. Its period of existence must predate these radiocarbon dates by a certain time span. Hence it seems inconceivable in stratigraphic terms to date Stratum VI to the same period as V. There are also clear differences between the pottery assemblages of these two strata, as the ceramic remains of Stratum VI are somewhat older than Stratum V. Therefore, putting all the criteria together, Stratum VI (Phase C-2 in Area C) would fit best in the period 975–950 BCE, the second most likely period in the 1σ range of the calibrated age of the average AMS date.

Radiocarbon BP dates for the calendar period 975–950 are *younger or similar* in age as compared to BP dates of the *later* calendar period 950–910, due to the wiggle of 975–950 BCE. The somewhat younger PGC date (GrN-27366) may, therefore, be compatible with this wiggle. It is important to realize that the wiggle is based on dendrochronological data through radiocarbon measurements of groups of 10 tree-rings (decadal) or 20 tree-rings (bidecadal) (Stuiver *et al.*, 1998). The radiocarbon measurements of Stratum VI are based on a short-lived sample –seeds– with a time span of about one year or even less (growing season). Past ^{14}C variations in the atmosphere are not captured in such detail by the calibration curve itself, due to the comparatively "coarse" decadal and bidecadal measurements. Therefore, it might be important in this case to make a special primary study of this wiggle through ^{14}C measurements by AMS of each individual tree ring in the available dendrochronological series for the period 980–940 BCE or even for the entire 10th century. Such detailed measurements have been carried out for older Bronze Age periods (Vogel and Van der Plicht, 1993).

Area C – Strata V and IV

These younger strata within the Iron Age IIA period are outside the focus of this paper, which deals with Iron Age I and the transition to Iron IIA. Extensive

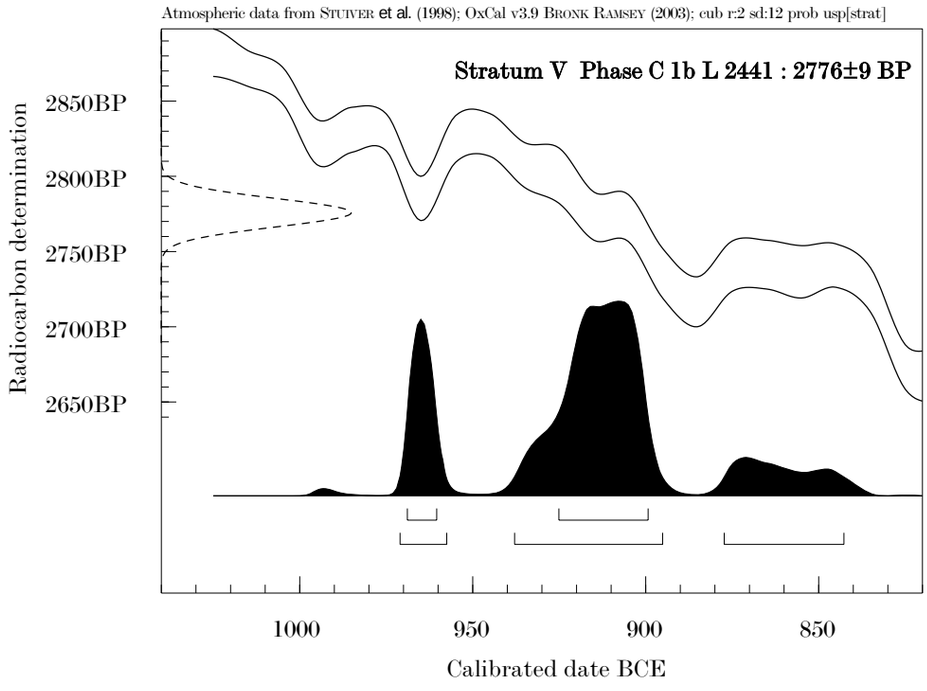


Fig. 12 The average of three Groningen AMS dates (2776 ± 9 BP) of Iron IIA Stratum V in Area C (Phase C-1b)

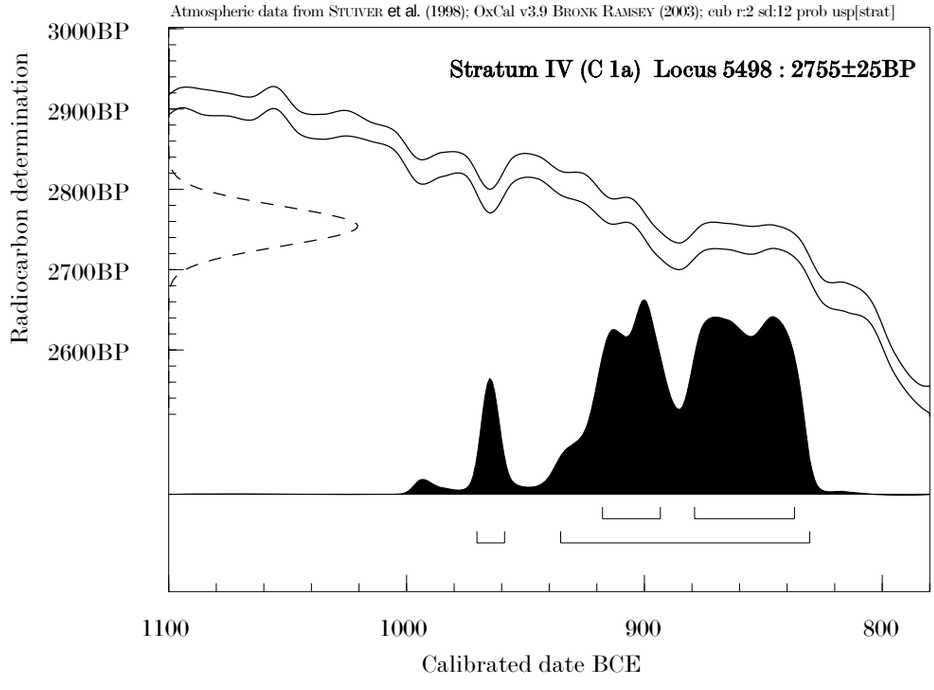


Fig. 13 The average of three Groningen AMS dates (2784 ± 22 BP) of Iron IIA Stratum VI in area C (Phase C2)

treatment of the Tel Rehov radiocarbon dates of these latter strata (shown in Table 3) will be published elsewhere (Mazar *et al.* 2005). For the sake of comparison with radiocarbon results from older layers, the calibration of the average date of Stratum V Locus 2441 is shown in Fig. 12. Four PGC dates from relatively large, charred, cereal grain samples found above a floor under a thick destruction layer gave a weighted average of 2776 ± 9 BP. The very small standard deviation results from the averaging calculation of the included dates, which have standard deviations in the range of 14 to 25. The 1σ calibrated age gives the ranges 969–960 (15.2%) and 925–899 (53.0%) BCE, but excludes the 9th century. The 2σ calibrated range of course gives a wider spectrum: 971–958 (18.2%), 938–895 (64.6%), 877–843 (12.6%). Thus the 9th century has a probability of only 13% out of 95%. The most likely 2σ range for the destruction of city V is the period 938–895 BCE with a probability of 65%.

As Rehov is mentioned in the list of places raided by Pharaoh Shoshenq I (Shishak) during his military campaign (Kitchen 1986: 187–239; B. Mazar 1986), we cannot ignore these literary sources in the evaluation of our ^{14}C time series. It is quite fascinating that our radiocarbon dates for the destruction of Stratum V are more or less in the same time span as historical assessments for the period of Shoshenq I (Bruins, Van der Plicht and Mazar 2003a). The precise time of Shoshenq's campaign is unclear; the year 925 suggested by Kitchen is most widely cited, but his argumentation depends to a large extent on biblical chronology and leaves the door open for somewhat higher or lower options (Kitchen 1986: 187–239; 2000:40–41). It is difficult for intrinsic reasons to prove a correlation between a destruction layer and a historical event. Yet in our case the association seems logical and fitting the circumstances.[5]

Concerning the comparison between the dates of Locus 2425, as dated in Rehovot and Arizona, and in Groningen by both PGC and AMS, see Bruins, Van der Plicht, Mazar (2003b). A more detailed treatment of these differences will be presented in a future paper dealing extensively with Iron Age IIA. For a more detailed treatment of these differences see Mazar *et. al.* 2005.

Stratum IV (local Phase C-1a) is the youngest Iron IIA Stratum at Tel Rehov. A sample of charred cereal grains from this Stratum was investigated in Groningen by AMS dating. The sample came from Locus 5498, of Building F in Area C, a room in a very well preserved building with mudbrick walls standing to a height of over one meter. The grain sample (Basket 54702) was found inside a pottery jug above the floor at level 86.10 m, covered by 1.18 m thick destruction debris. Many restorable pottery vessels and a unique cult object were found in this room. The three AMS dates 2770 ± 50 (GrA-21152); 2730 ± 50 (GrA-21154) and 2760 ± 35 (GrA-21267) form a robust series, as the results show excellent repeatability within one standard deviation from each other. The weighted average date of 2755 ± 25 yr BP gives a calibrated age in the 1σ range with two options: 918–893 (25.0%) and 879–837 (43.2%). Though the last 18 years of the 10th century BCE have still a relatively high relative probability, the 9th century period of 879–837 is clearly favoured with 43.2%. The calibrated results in the 2σ range are 970–959 (6.0%) and 934–830 (89.4%).

The destruction of Stratum IV and the abandonment of the lower city at Tel Rehov most probably occurred during the 9th century prior to *ca* 830 BCE. This event may be related to one of the historical episodes during this period. The wars between Israel and Aram during the time of Hazael *ca* 840–830 BCE are reasonable options, though earlier events in the 9th century should not be excluded.

Conclusions

Quality control in radiocarbon dating is important in order to have meaningful results in the historical archaeological periods of the Near East (Van der Plicht and Bruins 2001; Scott 2003). Both archaeological and physical elements are involved. The Groningen dates of Tel Rehov come from a detailed stratigraphic series of reliable sequenced contexts, each providing short-lived samples. In physical scientific terms, internal quality assessment and lab intercomparison of the Groningen dates are based on two fundamentally different radiocarbon techniques, (pre)treated and measured in two independent laboratories – Conventional PGC (GrN) and AMS (GrA). The repeatability of the measurements, which is a most important indicator of quality and accuracy, is generally outstanding. There are only few exceptions. Sometimes, the difference is slightly problematic, as the resolution of radiocarbon dating is pushed to the limit. This was the case for the lower part of Phase

[5] Finkelstein and Piasetzky (2003b: 286–277) don't accept this correlation. Instead, they suggest an absurd historical reconstruction. A detailed reply will be published elsewhere.

D6 (Locus 2874) and for Stratum VI. However, the age difference in both cases between the PGC and AMS dates is still within 2σ, which shows that we are really touching upon system resolution rather than apparent mistakes.

Our research resulted, incidentally, also in an unplanned intercomparison with radiocarbon dates from Rehovot, as the same samples or loci were measured in certain cases by three labs: Groningen PGC, Groningen AMS and Rehovot. It is indeed worrying that all Rehovot dates of short-lived Iron I samples from Tel Rehov are younger than the two Groningen labs by a century or more. A similar difference between Groningen and Rehovot was already noted for most of the Iron Age IIA dates of Stratum V, Locus 2425 (Bruins, Van der Plicht and Mazar 2003b).

It does not mean that other dates measured in Rehovot suffer from a similar bias.[6] However, the serious discrepancies outlined above, make it inevitable that the Rehovot dates on samples from other sites like Dor and Megiddo (Sharon 2001; Gilboa and Sharon 2001; 2003; Finkelstein and Piasetzky 2003a, 2003b, 2003c) should now be examined also by other ^{14}C laboratories, including pretreatment and all steps involved.

Large deviations are not uncommon in radiometric measurements (Scott *et al.* 1998; Scott 2003). For example, the Radiocarbon Laboratory of the British Museum issued a detailed statement in 1990 that its radiocarbon dates measured in the period 1980–1984 were on average too young by 200–300 years (Bowman et al., 1990). A retroactive re-evaluation of the dates was conducted, as far as possible, and the new, corrected dates were published, albeit with a necessary large standard deviation. Radiocarbon measurements in Groningen of short-lived Early Bronze Age samples from Jericho were indeed 200–300 years older than dates on similar samples measured in the British Museum Lab during 1980–1984 (Bruins and Van der Plicht 1998). This is another case of unplanned intercomparison, on EB samples, which incidentally underlines the accuracy of the Groningen dating record.

The Groningen ^{14}C series on short-lived charred organic material from Tel Rehov, presented together in stratigraphic order in the concluding Figure 14, lead us to the following summary and conclusions concerning Iron Age I and the transition to Iron Age IIA:

1. The Iron Age IA of Phase D-6 in Area D has a calibrated 1σ age range that covers the entire 12th and part of the 11th centuries BCE. The slightly lower date for Locus 2874 is related to one date being somewhat younger, as compared to the other three dates of this phase, which are very coherent. Yet this date is still within the 2σ measurement range of the other D-6 samples. Ceramic types in both Phases D-7 and D-6 seem to favour a date in the 12th century BCE, possibly related to the time of Egyptian presence in the region during the 20th Dynasty, and to Strata S-4 and S-3 at Beth Shean (Mazar 2003a: 324 Table 1; 333–337). Phase D-6 probably belongs to the last phase of this period.

2. Phase D-4 of the Iron Age IB has a calibrated 1σ age range that covers both the 12th and 11th centuries BCE. The many wiggles, coupled with the rather similar BP position of the calibration curve for this time period, make precise dating difficult. However, Phases D-6 and D-4 cannot occupy the same position on the calibration curve and thus Phase D-4 should be located in the younger part of the calibrated age range, *i.e.* in the 11th century BCE.

3. Phase D-3 has a calibrated 1σ age range that covers the last part of the 11th century BCE and the early 10th century BCE. This phase marks the transition from Iron I to Iron IIA.

4. Phase D-2 contains unambiguous types of Iron IIA pottery. Its 1σ calibrated age range covers the first 80 years of the 10th century BCE.

5. Phase C-2 (general Stratum VI) is stratigraphically the oldest Iron IIA city in Area C. The single PGC date is slightly younger than the average of three AMS dates. Taking the latter average, the 1σ calibrated age range covers the entire 10th century BCE. However, considering the detailed calibrated options (Table 3, Figure 11), coupled with the fact that successive Strata cannot occupy the same position on the calibration curve, the period 975–950 BCE, is favoured. A considerable time duration for phases C-2 (=VI) and C1b (=V) also fits the differences in the pottery assemblages of these two strata.

[6] The 13 dates from Beth Shean measured in Rehovot (Mazar and Carmi 2001; Mazar 2003:332) appear to be acceptable, though most of them have a chronological range after calibration that is too wide for effective evaluation. Attention that the criticism of our results by Finkelstein and Piasetzky (2003a, 2003b) is based to a significant extent on dates measured in Rehovot during the 1990's..

6. The calibration results of the various loci from Phase C-1b (=general Stratum V) place the highest relative probability within broadly the last 40 to 20 years of the 10th century BCE. The time span of the city must predate the radiocarbon results, based on charred grains found in the final destruction layer of this city. Hence the approximate period 950–910 BCE for Stratum V would fit well with the calibrated dates and stratigraphy.

7. Stratum IV is the last Iron Age IIA city at Tel Rehov. The dated charred cereal grains come again from a sealed destruction layer. The 1σ calibrated age range for the end of city IV has one option in the last part of the 10th century BCE and a more likely option in the 9th century BCE, but not later than 830 BCE.

8. Our radiocarbon results show that the transition from Iron I to Iron IIA occurred during the first half of the 10th century BCE (Figure 14), though there is some overlapping between the end of the Iron Age I (Stratum D-3) and the suggested date for the Iron IIA (Phases D-2 and C-2 (=general Stratum VI), as should be expected for a transition boundary. The suggestion by Finkelstein that the transition from Iron I to Iron IIA took place around 920 BCE, indicated by the blue line in Figure 14, clearly does not fit the radiocarbon dates.

9. The Iron IIA period had a rather long duration, including most of the 10th as well as the 9th centuries. This was already proposed by Aharoni and Amiran, by Ben-Tor, by Barkay and now also by Mazar, who suggests a time frame between 980–830 for this archaeological period.

10. The significance of our results for the debate concerning the archaeological nature of the United Monarchy is obvious: cultural assemblages traditionally dated to the 10th century (the assumed time of David and Solomon) can remain in this time period and don't have to be lowered to the 9th century, as proposed by Finkelstein's "Low Chronology". On the other hand, the longevity of the Iron IIA assemblages and their continuity into the 9th century leaves the door open for various interpretations to date specific buildings, such as the Megiddo palaces, either in the 10th or in the 9th century. Yet these issues are beyond the scope of this paper.

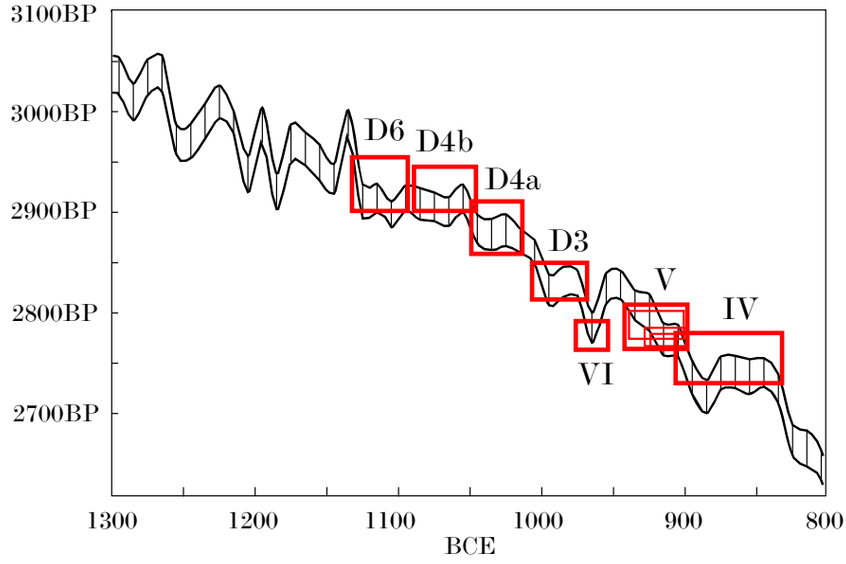


Fig. 14 Concluding graph (based on Bruins, Van der Plicht and Mazar, 2003a) showing the radiocarbon dating results (BP scale on y-axis) of the various strata on the calibration curve in stratigraphic order to indicate the calibrated range on the historical timescale (x-axis). Notice that the Iron I / Iron II transition occurs between Stratum D-3 and VI, approximately around 980 BCE.

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Clarifying the 'High' v. 'Low' Aegean/Cypriot Chronology for the Mid Second Millennium BC: Assessing the Evidence, Interpretive Frameworks, and Current State of the Debate

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An expert is a person who has made all the mistakes that can be made in a very narrow field (Niels Bohr)

Introduction

In the proceedings of the first SCIEM 2000 EuroConference, Manfred Bietak (2003) presented an analysis entitled "Science versus archaeology: problems and consequences of high Aegean chronology". He outlined what he saw as the problems of science versus archaeology in terms of the 'high' Aegean chronology, and its incompatibility with the archaeological evidence as he reviewed it. In this paper for the proceedings of the 2nd SCIEM 2000 EuroConference, I wish to address a number of Manfred Bietak's concerns (and also those of Wiener 2003; this volume; and Kitchen 2002), and to consider what exactly are the real problems involved in this area. I wish to consider where and to what extent 'science' really is at odds with 'archaeology' – to isolate what really is a problem requiring attention, versus what is merely a non-meeting of interpretative frameworks and viewpoints. This is an important issue: at present facts and interpretations are being confused and conflated in the literature, and there is more than a little misunderstanding of some arguments. Mistakes and misjudgements have also been made by many – including several by the present author – and there is perhaps some comfort to be drawn from the well-known quotation of Niels Bohr (above)! Various pieces of independent information that are valid and useful by themselves are also being associated wrongly with other issues; and, as always in *both* science and archaeology, some views and hypotheses must be revised or discarded as new and better evidence, and new and better analyses, come to hand.

The key 'point' I wish to make in this paper is that in reality we do *not* have a simple science v. archaeology situation. All the 'problems' noted by critics of the Aegean high chronology (here taken as Bietak 2003; Wiener 2003) exist if one analyses only the traditional (archaeological) evidence. The scientific evidence – and I mean solely radiocarbon (see below) – merely exaggerates (or highlights) these problems – it does not in fact create them.

I have tried not to burden this short paper completely with endless references to what is now a very considerable body of literature from the last 30 years; I try to cite just various key and where possible recent publications (which then have relevant further references). Readers seeking further bibliography should consult the papers and books cited for more details on specific areas and views.

Prior assumptions and mentalité – the need to be open minded

A significant problem with much of the debate and literature on the chronology of the middle second millennium BC east Mediterranean, and on the date of the Thera eruption, is that various authors begin any study with a largely pre-determined position. They believe some set of views, or set of data, are effectively right or paramount and everything else is then analysed accordingly – thus alternative evidence receives intense critical comment and or dismissal (even is ignored), while confirmatory evidence or scholarship is simply stated and or praised with little critical consideration or self-reflection. Some of the evidence we have is only partial or less than explicit and unambiguous – the temptation (often even unconscious) is to interpret/manipulate such data to serve a pre-conceived point of view. Arguments on such matters abound across our research fields, even in the hallowed world of Egyptian chronology, where Luft (2003:202) makes exactly such criticisms of some other leading scholars – concluding that: 'each scholar had a pattern to interpret the texts, while I prefer tracing a pattern inside the texts'. The outcome of such pre-conceived positions and assumptions, the resultant selective filtering of information, and the not unimportant role of the

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academic ego, is that only small and incremental changes and revisions are made to the 'right' basic position. Radical revision is avoided where possible, and the approximate status quo is maintained almost on principle.

Nearly everyone is guilty here – the present author included. The number of leading scholars in this field who have significantly changed their minds and published positions (once made) over the last three decades is very small. Instances of new evidence prompting a leading scholar to wonder if all the evidence can be re-assessed and interpreted in a significantly different way are very rare: one especially thinks of the remarkable paper by Philip Betancourt (1987). This is an indictment of us and of the way the modern academic 'industry' works: people are often not encouraged, or given the time and opportunity, to reflect and to re-assess.

What is worse is that this topic involves inter-disciplinary research and cross-overs. It is difficult or impossible to be expert in all areas, and difficult to be even handed to all evidence and to judge and criticise it appropriately on its merits. Sometimes a person from outside a field will in fact offer better critique and analysis than those within who have lost some perspective as they struggle with minutiae and tradition. Other times specific expertise is necessary even to be qualified to offer a worthwhile opinion. And so on.

How did we get to where we are? And is this whole debate a case of science versus archaeology? No.

By 1980 the 'conventional' archaeological chronology of the mid-second millennium BC had been challenged on solely archaeological grounds – different interpretation of the archaeological evidence. The key scholar was Robert Merrillees, who proposed a synthesis of the Cypriot evidence that led to a 'high' chronology, and who, with Barry Kemp, also did the same for the Minoan evidence (key publications: Merrillees 1968; 1977; Kemp and Merrillees 1980). There was of course a vigorous critical response, especially in the Levantine/Egyptian and Minoan spheres (e.g. Oren 1969; Warren 1985) – whereas Merrillees' Cypriot chronology in many ways went on to become the standard and mainstream one in its field. Merrillees did *not* use radiocarbon or any other scientific evidence – indeed he was highly critical at that time of the value or utility of radiocarbon evidence.

Independently in the mid 1970s, and onwards, sufficient radiocarbon evidence became available from mid second millennium BC contexts to indicate that radiocarbon dates seemed to point to a somewhat earlier chronology. But, and entirely plausibly given the poor precision and accuracy of radiocarbon dating at that time, and the limited evidence, it at first seemed that something must be wrong with these new dates and that preference should still be given to the conventional archaeological chronology (Betancourt and Weinstein 1976). But the radiocarbon evidence continued to mount. Then, in the 1980s, it also seemed that tree-ring and ice-core evidence combined to indicate a series of 'packages' of major volcanic eruptions in the past (Baillie and Munro 1988; Hughes 1988). It seemed plausible (possible anyway) that one of these packages in the 17th century BC might be Thera.

In 1987 Betancourt published, noting the science-dating evidence in favour of a 17th century BC date for the Thera eruption, and so also for the mature Late Minoan IA period; he then proposed that a re-assessment of the archaeological evidence was possible which could yield a compatible archaeological chronology. Manning, who was a student at this time in Australia, had likewise noted this situation, and published a similar independent case in 1988. The critical thing is that such an archaeological re-assessment was *already* possible (pre-science evidence), and the case had been argued by Merrillees, and Kemp and Merrillees. As Hallager (1988:12) noted, the flexibility possible for the Middle Minoan III–Late Minoan IA periods existed because there was in fact almost no archaeological evidence – its date was a best interpretation/estimate between better synchronisms and from some fairly loose stylistic associations. Thus he concluded that 'it is important to stress that the renewed investigations of the traditional synchronisms of the MMIII/LMIA material have shown the contexts – both the Egyptian/Near Eastern and Aegean – so dubious that a revised high chronology for the beginning of the LMIA is possible'.

Therefore, we did and do not have a science versus archaeology split: instead there were two existing views of the archaeological evidence and some scholars argued that the science evidence seemed to support one of these more than the other. This alternative case was then developed over the following 11 years up to Manning (1999). However, this was undoubtedly the road less travelled by.

A great deal has changed since 1988 and indeed since 1999 (and much has not). I note below (Sections 1–3) several key things, which may, for the present (AD2004), be put aside – but which seemed important in previous years (the tree-ring and ice-core evidence) – so a key change. Much new evidence

of much better quality is now available from radiocarbon measurements. This needs to be considered and is another key change to the past situation. And much new archaeological evidence and study is also available – and in particular data emanating from the important Tell el-Dabᶜa project led for many decades by Manfred Bietak. This too needs to be carefully considered. And so on.

What the field needs is honest and open re-assessment of all of the relevant evidence now (AD2004) pertaining, and of its interpretation(s) – without a pre-determined answer. The following is my attempt.

1. The Aegean Bronze-Iron Dendrochronology (the Aegean Dendrochronology Project datasets from the later 3rd through earlier 1st millennium BC built up over 30 years by Peter Ian Kuniholm and collaborators)

This 1503 year tree-ring sequence has been nearly absolutely dated through the measurement of a great many high-precision radiocarbon dates on specific decadal blocks of wood within that chronology (Manning *et al.* 2003; 2001a; and further work in progress). A previous – and now replaced – dating published in 1996 was based on just 18 dates (Kuniholm *et al.* 1996). This 1996 paper placed ring 854 (for example) of the chronology at c.1641BC as the 'best' fit from the radiocarbon data (this does not mean the correct fit – merely the one most likely given the then available data). The error range on this fit was quite large. For various reasons we then proposed, within this 1996 error range from the radiocarbon data, a date some 13 years later given information and our interpretation/viewpoint in 1995–1996. In 2001 using 52 radiocarbon data, and then in 2003 using 58 data, we found that the 1996 date (and the underlying hypotheses) was no longer possible. Moreover the date of the Dendrochronology could now be defined at good confidence levels within quite narrow boundaries even allowing for the range of data and the variations at issue. This led to a date for ring 854 now somewhere between 1653–1650BC, give or take small errors (and note this is in fact just +9 to +12 years from the 1996 radiocarbon date and well within the error range specified in 1996 from the radiocarbon evidence). This new 2001 onwards dating had nothing to do with any other evidence or other hypotheses. It is independent. This dendrochronology is a fact and its dating is very near absolute; it forms a key chronological framework for the Near East and associated regions. A probable link with the previously floating Early Bronze Age chronology means that we are close to having a nearly absolute 2009 year long tree-ring chronology for the entire Bronze Age of the Aegean-east Mediterranean region (Newton and Kuniholm 2004).

As is well known, a remarkable growth anomaly occurs over a few years in this Aegean dendrochronology starting in ring 854 (in 61 constituent trees as of early 2004). It has been suggested that this anomaly *could* be consistent with the impact of a massive low-mid latitude northern hemisphere volcanic eruption, and in particular Thera (Santorini). However, there is at present absolutely no positive evidence that connects the two events. The tree-ring anomaly, while extraordinary and at present unique in the seven thousand years of Aegean Dendrochronology available, could be something else. We do not know. A plausible/possible suggestion (and no more) has been made. However, any connection with Thera is *not* a fact and has *no* worth in strict analysis concerning east Mediterranean chronology. On present stated dating errors, there is possibly (but not necessarily) a temporal overlap with the very large volcanic signal in the Dye 3/GRIP ice-core c.1645BC – however this is not certain (they could be a few years even a decade apart), and, moreover, this volcanic signal seems not to be related to Thera on current evidence (see Section 3. below). (I note that there is just the one, solitary, tree-ring growth anomaly – Bietak 2003:23 implies we abandoned one and then chose another – no – the absolute date of the same ring 854 growth anomaly, and the whole Dendrochronology, moved with the re-dating based on many more and better radiocarbon determinations as reported in Manning *et al.* 2001a; 2003).

2. Other Tree Ring Information

A widely attested significant tree-ring growth anomaly occurs in the northern hemisphere at 1628/1627BC (Amarche and Hirschboeck 1984; Baillie 1995 and references; Grudd *et al.* 2000). It has been suggested that this phenomenon could be compatible with the impact of a large volcanic eruption, and Thera has been suggested as a candidate. However, there is no positive evidence for either proposition. The putative correlation with ice-core evidence previously suggested as possible (e.g. Hughes 1988; Baillie 1996) now seems on dating grounds to be impossible (Hammer 2000; Hammer *et al.* 2003) or unknown (Southon 2002). By itself this evidence tells us nothing *firm* about Thera and the dating thereof.

The seemingly strong circumstantial case of the late 1980s through early 2000s, where tree-ring and ice-core evidence offered a set of compatible 'pack-

ages' of evidence around several likely major volcanic events of the last several thousand years (e.g. Baillie 1995), has since been disproved and shown to be irrelevant. First, the better dating of the ice-core data broke apart most of the suggested 'packages', and negated the circumstantial case (Manning and Sewell 2002). Second, with regard to the Thera volcanic eruption, recent work has shown that it is unlikely that Thera can be identified with the proposed ice-core volcanic signal – again negating the supposed package of events discussed in the later 1980s through early 2000s (see next Section).

3. Ice-Core Information

At the time of writing there is no satisfactory evidence for the identification of, and thus the date of, the great Thera volcanic eruption in any ice-core. There are various acid signals in various ice-cores across the period c.1700–1450BC; these represent various volcanic eruptions – but there is no positive link at present for any of these with Thera. The proposal that the very large volcanic signal c.1645BC in the Dye 3/GRIP ice-cores be identified with Thera, on the basis of analyses of the composition of tiny volcanic glass fragments recovered (Hammer *et al.* 2003), has been shown to be incorrect (Pearce *et al.* 2004; n.d. this volume) – indeed Pearce *et al.* argue that the glass shards recovered from the c.1645BC GRIP core layer appear compatible with an Aniakchak provenance.[1] Thera is thus *not* dated at present from ice-core evidence. Clearly, various other candidate volcanic signals exist in the several ice-cores, and it is to be hoped that future work will provide both a robust identification and a precise date (although one may speculate in advance that, with the likely limited number of tiny glass shards available, and the available [or presently foreseeable] analytical possibilities, it may not be possible to get a truly definitive outcome). Significant problems with dating for some cores (e.g. GISP2) have also to be overcome (Southon 2002). This body of evidence can be set aside at present, until future work brings it back into play.

4. Radiocarbon evidence for Aegean Late Bronze I–II chronology

With the tree-ring and ice-core evidence currently *irrelevant* to the dating of the Thera eruption, radiocarbon offers the only high-precision, direct, and independent science-dating source for the timing of this event, and for the associated archaeological phases. A large high-quality body of new radiocarbon evidence has been produced in the period 2000–2004 directed at the dating of the Thera eruption and the associated Late Minoan IA, IB and II periods, developing previous work. One recent summary was presented at the May 2003 SCIEM Euro-Conference 2, and another up-dated one at a meeting in Vienna in January 2004.

A publication of all the data from this project produced at the Oxford Radiocarbon Accelerator Unit, and a robust set of analyses of these data (and previous Oxford data – from Housley *et al.* 1999; 1990) via a set of holistic Bayesian models is in Bronk Ramsey *et al.* (2004a). Numerous known age data run at Oxford across this time demonstrate the good accuracy and precision of the laboratory (Manning *et al.* 2002b:735 caption to fig.1; Bronk Ramsey *et al.* 2002:2–4; Bronk Ramsey *et al.* 2004b). The Bronk Ramsey *et al.* (2004a) paper uses the (then) current internationally recommended radiocarbon calibration curve (INTCAL98; Stuiver *et al.* 1998a) – a further paper (presentation in Vienna January 2004, written text in preparation) will discuss these data, and additional data run at the VERA laboratory in Vienna (final samples still in progress as this text is written), against both the new INTCAL04 calibration curve, and a variety of other radiocarbon calibration datasets in order to assess the robustness of the calibration (cf. Wiener 2003:384–386). (Such analyses include calibration against Aegean data where *no* significant regional/growing season offset can apply *even*

[1] Keenan (2003) previously disputed the Hammer *et al.* (2003) claim on statistical grounds. His conclusion has been shown to be correct. But the statistical method employed by Keenan is probably not appropriate for the type of data in question, as Pearce *et al.* (this volume) make clear:
Recently, Keenan (2003) employed t-tests on the standard errors of the analyses of Hammer *et al.* (2003) (standard error = standard deviation / $\sqrt{}$number of analyses) to show that the Minoan Bo-1 sample and the A1340-7 glass cannot be the same. This approach, where the numbers of analyses are large (i.e. n=174 for the ASEM analyses of the ice-core glass) may however reduce the errors to unattainably small values, far less that the true analytical reproducibility attainable by multiple analyses of homogeneous materials (Pearce *et al.* 1997). In doing so, when comparing different materials, the standard error approach may enhance the apparent differences between samples.

during the few major solar minima cooling episodes where such regional factors may create a *small* short-term offset as discussed in Kromer *et al.* 2001; Manning *et al.* 2001a – at other times no systematic detectable/significant offsets appear to exist within mid latitudes of the same hemisphere: also see Manning *et al.* 2002c; Talamo *et al.* 2003.) These new papers supersede previous preliminary discussions using only a few of the initial data from this project – although the general findings remain similar.

When we consider the date of the Thera eruption, we can begin with the 23 determinations at present available from internally consistent sets of defined final Volcanic Destruction Level (VDL) contexts on Thera from short-lived samples from standard modern pre-treatment and processing regimes – comprising (i) the Copenhagen laboratory n= 4 set (Friedrich *et al.* 1990 – see comments by Manning and Bronk Ramsey 2003:128–129), the Oxford Laboratory 1990 published series using only the standard pre-treatment samples from the final VDL n=8 set (stages 2/3 in that paper) (Housley *et al.* 1990), the new Oxford measurements n=8 set (published in Bronk Ramsey *et al.* 2004a), and the first half of a VERA set with n=3 at present (publication forthcoming once dating programme finished), then all these data (all on *different* seed or short-lived plant matter samples – even if from the same pot – and one twig) can combine satisfactorily within 95% confidence limits to offer a weighted average – this can then serve as a best estimate for the real average or typical radiocarbon age representative of the overall range of radiocarbon values within the set of short-lived samples from the VDL horizon: see Figure 1.

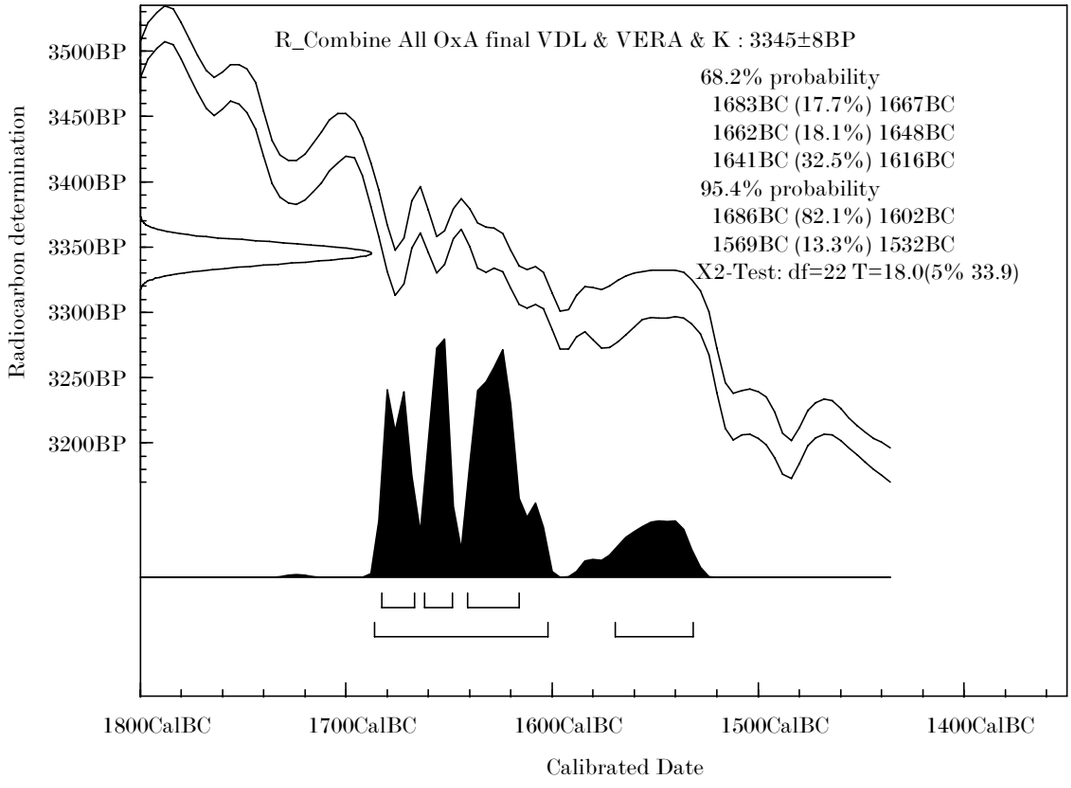


Fig. 1 Calibrated age probability distribution for the best current weighted average radiocarbon age for the Volcanic Destruction Level at Akrotiri, Thera. Data from the 23 determinations at present available from defined VDL contexts on Thera from short-lived samples from standard pre-treatment regimes – comprising (i) the Copenhagen laboratory n= 4 set (Friedrich *et al.* 1990 – see comments by Manning and Bronk Ramsey 2003:128–129), the Oxford Laboratory 1990 published series using only the standard pre-treatment samples from the final VDL n=8 set (stages 2/3 in that paper) (Housley *et al.* 1990), the new Oxford measurements n=8 set (published in Bronk Ramsey *et al.* 2004), and the first half of a VERA set with n=3 at present (publication forthcoming once dating programme finished – I thank Walter Kutschera and the VERA laboratory team for their collaboration). Calibrated data from OxCal 3.9 (Bronk Ramsey 1995, 2001 and later versions) with curve resolution set at 4 and INTCAL98 (Stuiver *et al.* 1998a) – note: the calibrated age ranges will need revision in light of INTCAL04 (and also assessment in light of other relevant calibration datasets); such issues will be considered in a future publication formally reporting the final data from the New Palace chronology project and their analysis

Note: the fact that all 23 data *can* combine satisfactorily together within 95% confidence limits is quite impressive and very much indicates that the *same* narrow dating horizon is being sampled (and then combined) and that the data represent a spread around the typical age (despite some likely issues concerning humic acid contamination for some samples [Manning 1999:237–239] – but the effect is clearly small as the overall set offers a consistent analysis and so, overall, is probably insignificant). The calibration of this average tells us what is already well known: the radiocarbon evidence indicates a most likely age range in the 17th century BC (82.1%), but a mid-16th century BC (13.3%) range is also possible.[2]

To try to move beyond the limitations of the single case analysis, the New Palace project analysed data from LMIA through LMII contexts from before, around, and after the Thera eruption. On the basis of a Bayesian sequence analysis of the current Oxford data for the New Palace period (in all 108 determinations – and only these data – the paper does not discuss other data or issues still in progress), and so including the constraints available from the archaeological seriation for the phases before and after the VDL on Thera, and using INTCAL98, the date range of the eruption of Thera is further refined in the study of Bronk Ramsey *et al.* (2004a), and is placed at 95% confidence level between c.1663 and c.1599BC (i.e. the mid-16th century BC range is largely excluded by the analysis of the New Palace data sets together). A date for the close of Late Minoan IB at two sites is suggested to lie roughly within the period c.1520–1490BC (give or take), and the close of Late Minoan II destruction at Knossos (Unexplored Mansion) is placed roughly c.1420BC give or take a decade or so. It must be noted that this is a large and mostly high-quality dataset. It cannot simply be dismissed as a few odd measurements.

These data, produced in the context of frequent known age tests, and consistently on two different accelerators at Oxford, offer a significant and internally robust chronological framework. They suggest an Aegean chronology for the Late Minoan IA and IB periods that is rather 'higher' and 'longer' than the conventional chronology. This evidence is dependent on the archaeological stratigraphy and relative phasings provided by each of the site excavators with regard to each specific sample; it is *independent* of any preconceived 'high' or 'low' chronological syntheses.

In the course of wide-ranging reviews of Bronze Age chronology, Malcolm Wiener (2003:380–395, this volume) raises a number of possible or theoretical concerns with the accuracy or precision of radiocarbon dating, and also makes critical comments on the preliminary papers of Manning *et al.* (2002b), and Manning and Bronk Ramsey (2003). I thank him as always for his close interest and critical attention to our work, and for a number of stimulating conversations. All his points deserve consideration; but, at the same time, it has to be noted at the outset that the level of ultra-scepticism directed now at the radiocarbon evidence is not also so directed at the archaeological evidence and its synthesis. The playing field is not level; nonetheless, radiocarbon is starting to make a real, robust, and relatively precise and accurate contribution to the debate (Bronk Ramsey *et al.* 2004a).

In general, I refer the reader to the Bronk Ramsey *et al.* (2004a) paper where a large body of data and a robust analysis over a set of analytical models

[2] Data not employed in this Akrotiri VDL assessment and reasons are:
(i) The old Pennsylvania data employed in e.g. Manning (1988 with references) – many of these data lack either NaOH pre-treatment or ^{13}C correction; further a number of these samples are not clearly or necessarily 'short-lived', and/or short-lived directly relevant to the final VDL (versus roofing matter and so on).
(ii) The Simon Fraser data (Nelson *et al.* 1990) – since this group did not publish their data (so impossible to analyse) and there remains no evidence to support their novel sample preparation strategy (Manning 1999:237–238).
(iii) The Zurich data – although a presentation was made at the Thera and the Aegean World Conference in 1989, these data were not then published in the proceedings (mention with sample details occurs later in another paper: Wölfli 1992:40–41) and are impossible to analyse.
(iv) Heidelberg data – the two dates on short-lived samples published by Hubberten *et al.* (1990:184 and table 2) do not offer a consistent set, as the two measurements are widely varying(N.B. while p.184 identifies just two samples as short-lived, table 2 indicates that sample 7092-6795 of "peas" is also short lived. Its radiocarbon age of 3360±60 BP would agree with the short-lived average in the text very happily). However, as the only two other available data (with details) with standard pre-treatment and correction (using just the two samples identified on p. 184), it is worth noting that they could in fact be added to the 23 date set employed in the main text without making the set fail to combine satisfactorily within 95% confidence limits (and their inclusion/exclusion hardly changes the weighted mean): weighted average of all 25 data 3344±8 BP with Chi-Squared test statistic of 29.7 less than the 95% confidence limit value of 36.4 for 24 degrees of freedom. Calibrated calendar age ranges: 1686–1602BC (81.0 %), and 1569–1532BC (14.4%). (OxCal 3.9 and INTCAL98)

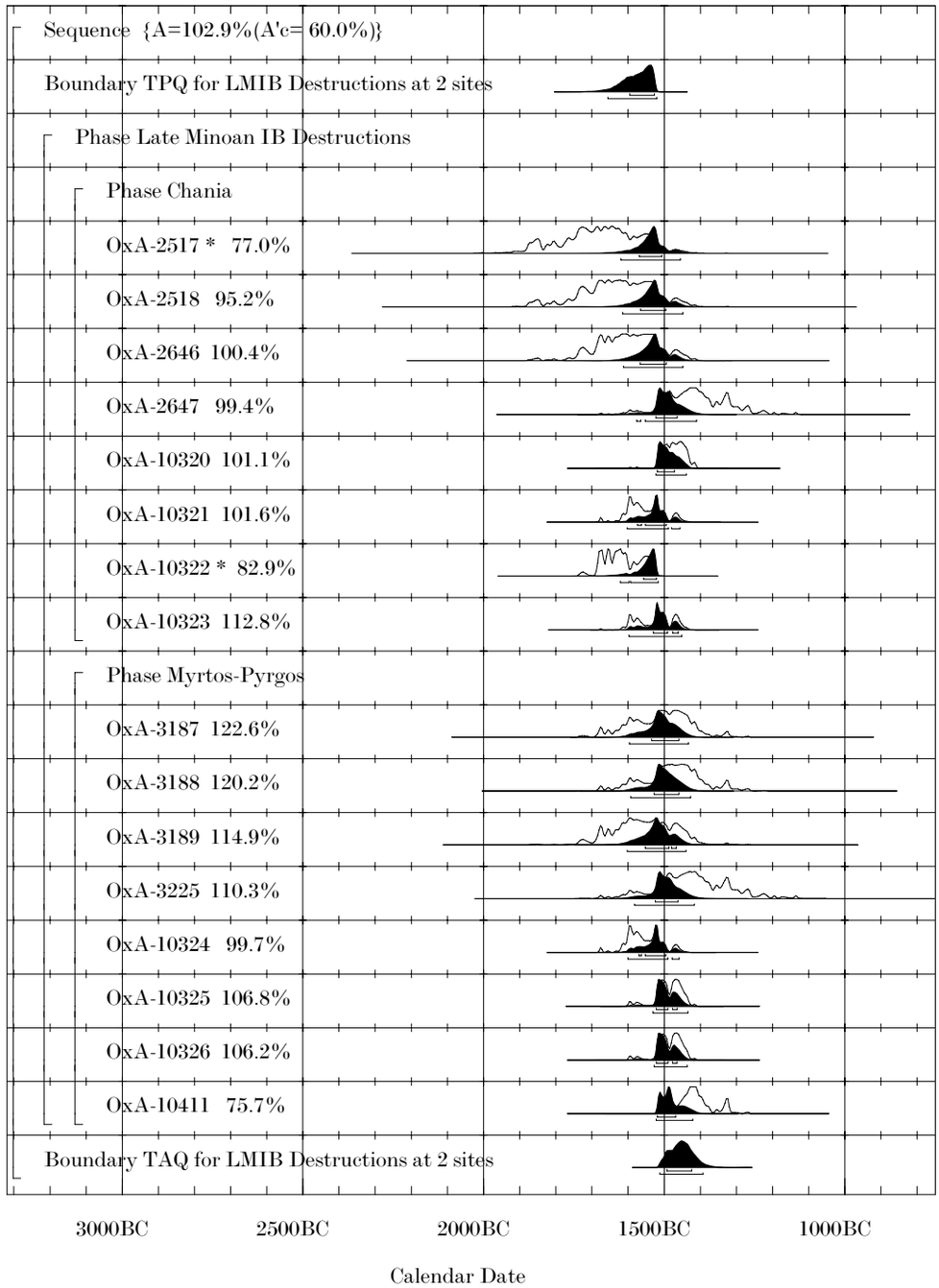


Fig. 2 Sequence-Phase analysis of the radiocarbon ages on short-lived samples from the close of Late Minoan IB destructions at Chania and Myrtos-Pyrgos, Crete (for data, see Manning *et al.* 2002b:737). Individual calibrated ranges are shown by the hollow histograms, calculated calibration ranges given Phase and Sequence constraints are indicated by the solid histograms. The % number indicates an agreement index between the former and the latter – a value over about 60% indicates satisfactory agreement at the 95% confidence level. The data centre around c.1500BC give or take a couple of decades. Each pair of determinations on similar sample matter (sample from same pot/context) combine satisfactorily, but two Chania samples, marked with an *, do not then combine with the other Chania data within 95% confidence limits under a Chi-Squared test (12.6 v. 7.8 for df3). Data from OxCal 3.9 (Bronk Ramsey 1995; 2001; and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD (68.2%) and (lower lines) 2SD (95.4%) calibrated ranges

offers our best current evidence. A number of other suggested problem issues were discussed and dismissed as significant by Manning and Bronk Ramsey (2003:124–129). Overall, and since Wiener (2003) was written, a considerably greater number of data now exist, including new data from the VDL on Thera, and a number of these samples, or similar samples, have been measured twice (and yield consistent outcomes) – a number of samples have also been measured at the VERA laboratory in Vienna. Further considerations of suggested issues of intra and inter year variation, and volcanic source carbon dioxide, were offered in the January 2004 Vienna presentation, and will be included in a written text in preparation at present (again I thank Malcolm Wiener for on-going discussions). Some of Wiener's concerns require additional research (in progress). I take this opportunity, however, to admit/concede to some failings in preliminary publications, as highlighted by Wiener (2003).

(i) Manning *et al.* (2002b) was a little over confident and sweeping in its conclusions – 'excited preliminary report syndrome'. Mea culpa. Nor did it consider variation in calibration dataset issues (something which is not considered in almost any other archaeology/radiocarbon work anywhere either – work on this topic as part of the East Mediterranean Radiocarbon Intercomparison Project [e.g. Kromer *et al.* 2001; Manning *et al.* 2001a] in fact places the Aegean-east Mediterranean at the forefront of current radiocarbon research). Much more robust and reflective analyses are offered in Bronk Ramsey *et al.* (2004a) and especially – dealing with calibration robustness – in the January 2004 Vienna presentation (written paper in preparation). The actual conclusions reached do not vary significantly from those in Manning *et al.* (2002b) and Manning and Bronk Ramsey (2003) – but they are much more securely based both in terms of quantity of data and in terms of robustness of analysis. The only point of relevance (versus refinement) concerns the close of the LMIB period as dated at Chania and Myrtos-Pyrgos. Here use of the INTCAL98 curve favours a date around c.1520BC where there is a steep slope in the calibration curve – but this slope's existence/scale is strongly influenced by one date on Irish Oak for a bi-decadal sample centred 1510BC (Wiener 2003:392). Removal of this datum removes the slope here and would seem to allow the LMIB data to move downwards, and back perhaps nearer 1490BC (compare the conclusions of Housely *et al.* 1999, who in their main text employed just the Seattle dataset on German Oak – see also Figure 5 and 6 below). In fact, the effect is fairly marginal. Much more critical is that one of the Chania samples could also be considered to be problematic (the sample of peas from TR10, Room E), which in two measurements yielded rather 'high' ages – and which do not combine with the other Chania data at the 95% confidence level) – these two determinations are marked with an * in Figure 2; removal significantly helps to favour a slightly lower date centred c.1500–1490BC.[3] For a new review of the dating of the Late Minoan IB destructions at Chania and Myrtos-Pyrgos, see Figures 2–6. This revised analysis finds that the majority of probability favours a date c.1520–1490BC for the close of LMIB destructions at both sites (and so a gap between the respective destructions of only a few years to a couple of decades). A rather lower probability could allow a date for one site (most easily Myrtos-Pyrgos – and so also a longer gap between the destructions) or (marginal probability) both sites within c.1475–1460BC. Either position requires a significant revision to the conventional chronology where LMIB ends c.1425BC (Warren and Hankey 1989:169) or c.1430BC (Warren 1999:902).

(ii) Wiener (2003:391 and n.148) quoting William Cavanagh criticises Manning *et al.* (2002b) for citing some modes or peak probability regions in their text/captions, rather than complete 1SD or 2SD ranges. I accept this criticism and regret the phrasing. The figures showed the 1SD and 2SD ranges, but the text perhaps implied greater precision than reasonable (versus suggesting as a commentary the most

[3] Why this sample yields a significantly older radiocarbon age is not clear. There is no reason to believe that its context is not similar, and the peas will not have been stored for more than a few years (I thank Erik Hallager for pers. comms. on this issue). Rapidly changing atmospheric radiocarbon levels over a few years might help explain the range in the Chania set – but this approach, based on the INTCAL98 calibration curve, has now been shown to be insecure (Wiener 2003:392). Growing season, or typical inter-annual, variations are unlikely to account for such a significant difference. We are thus left with a problem and no clear explanation. Since the two data from this sample are significantly different from the six data from the other Chania samples, the best course on review seems to be to exclude them. Since the excluded data are 'high' data, this means the provisional conclusions reached now are conservative and doing everything to help the 'low' chronology. There is no high chronology bias at all.

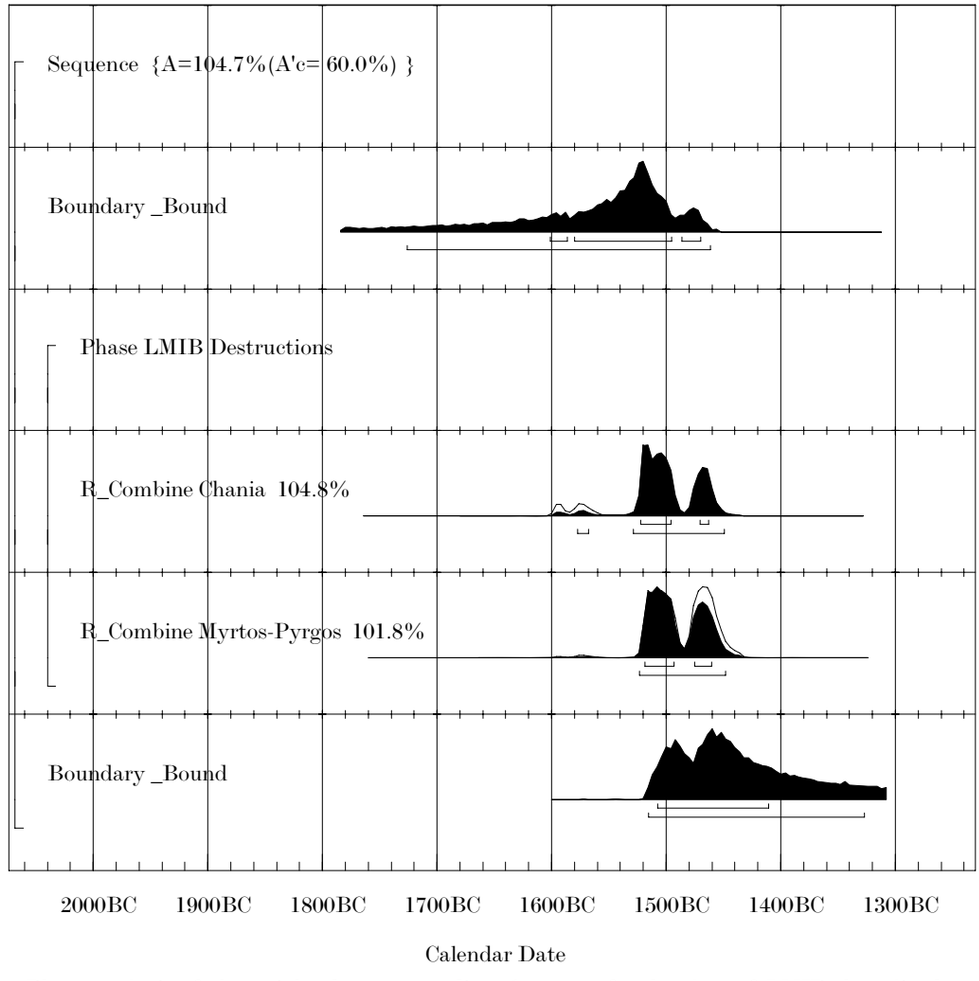


Fig. 3 Sequence-Phase analysis of the weighted average radiocarbon ages for the close of LMIB short-lived sample destruction datasets from Chania (excluding OxA-2517 and 10322 – see Figure 2) and Myrtos-Pyrgos. The calculated 1SD ranges for Chania are:1522–1496BC (56.3%), 1470–1463BC (11.9%), and for Myrtos-Pyrgos: 1518–1493BC (46.2%), 1475–1460BC (22.0%). The 2SD ranges end in 1449BC and 1448BC respectively. Thus both datasets clearly prefer a fit around or just before c.1500BC, but could date c.1475/70–1463/60 at rather lower probability. A date after c.1450BC seems ruled out. Data from OxCal 3.9 (Bronk Ramsey 1995; 2001; and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

likely tendency within the probability ranges). Mea culpa. The reader will note that for the quantified calibrated ages given above, I quote the 68.2% (1SD) or 95.4% (2SD) confidence total ranges. These are shown also in Figures 1–6.

The pattern in the radiocarbon evidence as a whole seems to be that radiocarbon and archaeological/historical dating work to give compatible information in the late 15th through 12th centuries BC (e.g. Manning *et al.* 2001b; 2002c; Manning and Weninger 1992). Acceptable match ups exist also in the Early Bronze Age (e.g. Kromer *et al.* 2003; Manning 1995; 1997) and into the first few centuries of the second millennium BC (e.g. Marcus 2003). The 'problem' is the 17th–16th centuries BC. Here radiocarbon dates from the Aegean have, for three decades, pointed towards a chronology rather 'higher' than the conventional archaeological interpretation. It is notable – and I think important – that this period is also 'thin' in plural (replicated) good, tight, archaeological synchronisms (as Hallager 1988 noted) – it lies between the multiple well-dated 14th–13th centuries BC synchronisms (Warren and Hankey 1989:146–162; Hankey and Aston 1995), and the pretty well-dated Kamares-Middle Kingdom synchronisms (Merrillees 2003). The limited or ambiguous to non-existent information available can plausibly be interpreted in different ways (either high

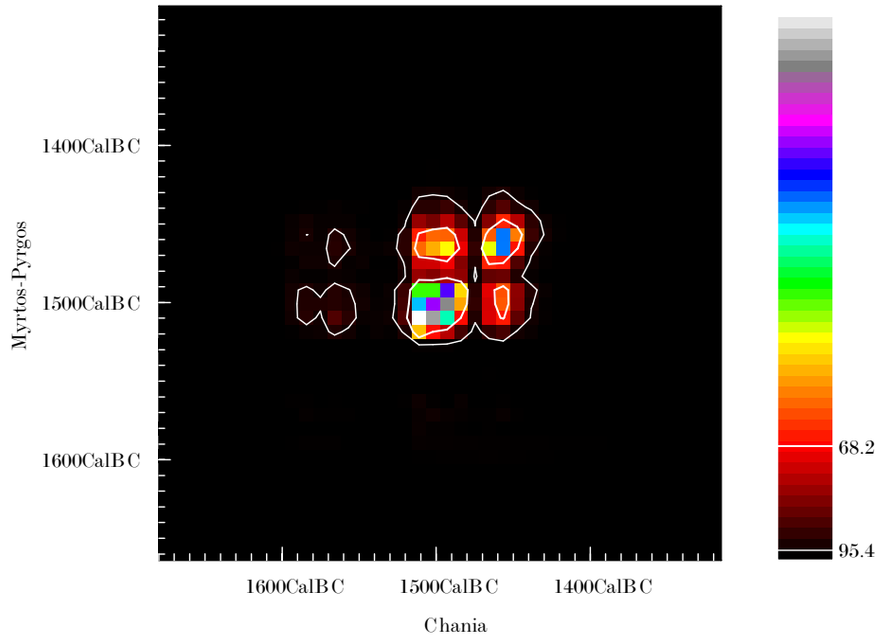


Fig. 4 Correlation plot from the analysis in Figure 3 showing the correlation of the calibrated calendar ages for the weighted average radiocarbon ages for Chania (excluding OxA-2517 and 10322) and Myrtos-Pyrgos for the LMIB destruction short-lived data. The inner (white) contour lines denote the limits of the 1SD ranges, the outer (white) contour lines denote the limits of the 2SD ranges. What we see is that the single most likely region for both sites to date is within the range c.1520–1480BC. This would also mean a relatively short gap between the respective destructions. However, it is also possible for (especially) the Myrtos-Pyrgos destruction to be later, around c.1470–1450BC. This would be most easily achieved if a gap of some 20–30+ years was permissible between the respective destructions. It would be possible at the lower margins of the 1SD limits to place both destructions here, but the Chania dataset is less consistent with this. Data from OxCal 3.9 (Bronk Ramsey 1995; 2001; and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The probability scale is shown on the right – from highest probability in white at the top to the lowest probability in black at the bottom

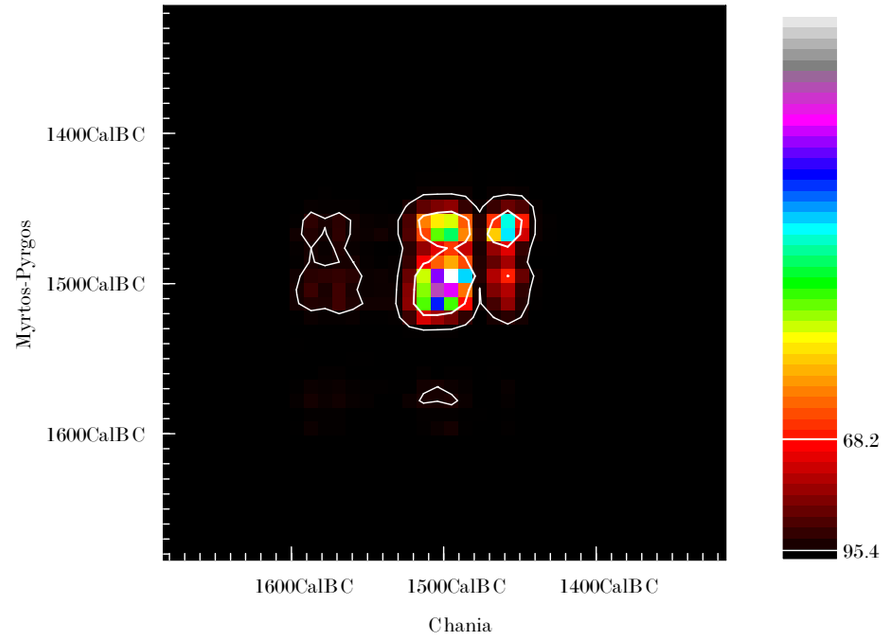


Fig. 5 As Figure 4, but using only the Seattle calibration dataset UWTEN98 (Stuiver *et al.* 1998b) (on German Oak for this period) and so avoiding the influence of the Belfast dataset (cf. Wiener 2003:392). The outcome is very similar. The probability scale is shown on the right – from highest probability in white at the top to the lowest probability in black at the bottom. The 1SD and 2SD limits are shown – these are the contours on the plot (inner contour = 1SD range limits, outer contour = 2SD range limits)

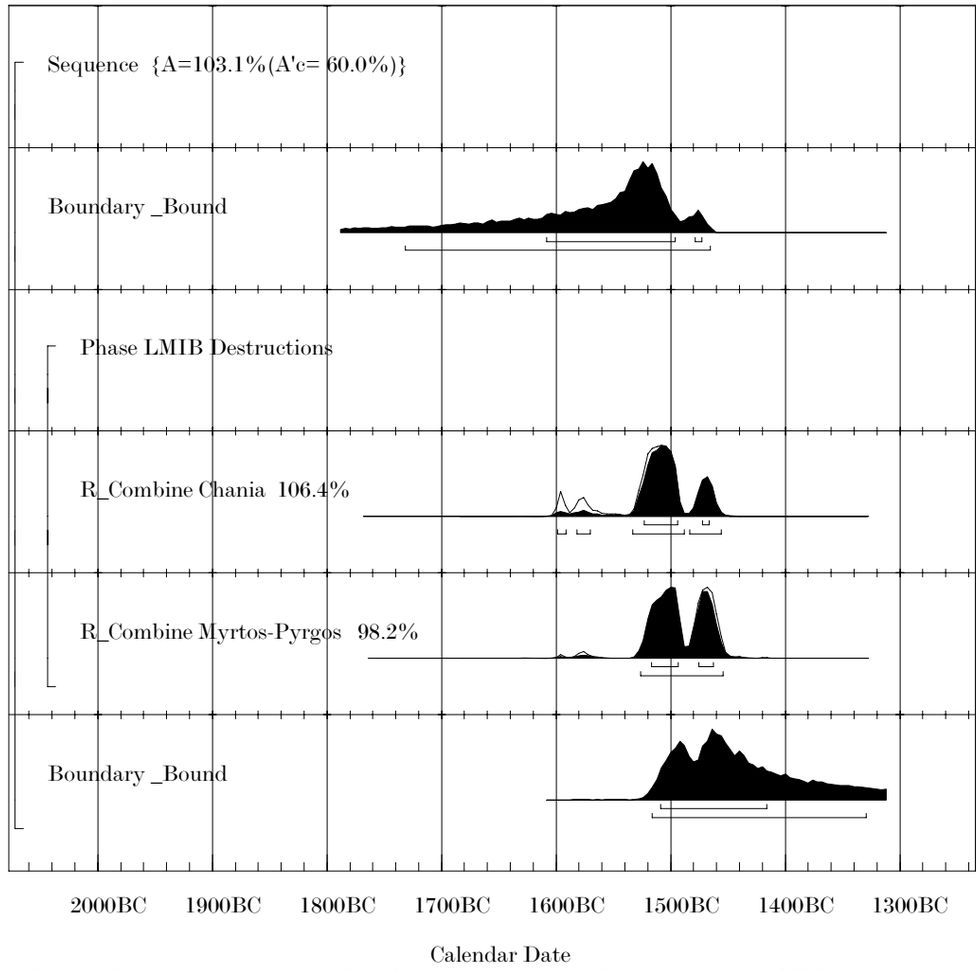


Fig. 6 As Figure 3, but using only the Seattle calibration dataset UWTEN98 (Stuiver *et al.* 1998b) (on German Oak for this period) and so avoiding the influence of the Belfast dataset (cf. Wiener 2003:392). The outcome is very similar. The calculated 1SD ranges for Chania are:1523–1494BC (60.9%), 1472–1466BC (7.3%), and for Myrtos-Pyrgos: 1517–1493BC (44.7%), 1476–1463BC (23.5%). The 2SD ranges end in 1456BC and 1454BC respectively. Thus both datasets clearly prefer a fit around or just before c.1500BC, but could date c.1476/72–1466/63 at rather lower probability (and especially for Chania). A date after c.1455BC seems ruled out. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

or low dates or in between), or are vague, as highlighted by Merrillees (1972; 1977); Kemp and Merrillees (1980); Betancourt (1987; 1990); Manning (1988), etc.

Now one approach to the problem is to argue that for some unknown reason(s) radiocarbon dates in the Aegean are wrong (too old) just for the 17th and 16th centuries BC. And not just for one site or island, but for the whole central-southern Aegean region, and not just for a short interval of a few years (i.e. *not* just in the few years leading up to and around the Thera eruption), but for all of Late Minoan IA and right through to mature/late LMIB. And yet radiocarbon dates must also 'work' in the same region for the previous millennia, and for those following. Wiener (this volume; 2003:383) speculates along these lines, raising a variety of potential or possible suggestions – though none with any actual positive evidence of applicability to the Aegean data at issue – and with only some of actual potential significance when one follows through the data. The problem is that at present there seems no even vaguely satisfactory explanation that could plausibly account for such a small and consistent/systematic 'old' age error/contamination for radiocarbon dates for the whole region at this time (and only this time) (Manning *et al.* 2002c). The crunch is that cited instances of major effects creating anomalous radiocarbon ages (e.g. releases of volcanic ^{14}C depleted CO_2) are highly localised and/or time varying and would (i) leave a clear signal in a pattern of significantly affected data tailing away to non-affected data by distance and/or against time (and *not* the more or less consis-

tent small, ca. 50 radiocarbon years or so, offset over a wide area and significant time span as in effect sought by Wiener, this volume; 2003), and (ii) cannot explain a consistent large area minor significant effect as the 'effect' is rapidly diffused and eradicated against both the scale of the atmosphere and the scale of other $^{14}CO_2$ inputs. The much smaller effects, or specific geology/soil speculations raised, could never explain a large area consistent effect on radiocarbon dates – and the former would indeed be unlikely to produce any detectable effect against the scale of other factors creating the consistent atmospheric ^{14}C signal recorded by plants growing across the southern Aaegean (Thera, Crete, Rhodes, Miletos in western Anatolia).

If we consider the possibilities of major effects, then no sapropel event is known at this time in the east Mediterranean (last one known is several thousand years earlier) and a significant up-welling event in the mid second millennium BC seems implausible (Manning *et al.* 2002c:744), and, since the situation must apply both in the 50–100 years before and c.100 years after the Thera eruption, localised and time-varying volcanic ^{14}C depleted (old) carbon dioxide from this eruption (or leaked in the lead up), or from other volcanic sources, cannot easily offer an explanation. Thera is not the type of volcano that continuously produces a very large diffuse CO_2 output (contrast Etna and some others: e.g. Allard *et al.* 1991) – and there is no evidence from modern comparanda for a significant volcanic effect that *consistently* covers many thousands of square kilometres at crop/tree leaf height (as required to account for consistent LMIA radiocarbon ages on plant and wood samples ranging from Thera to Rhodes and to Miletos: for full Oxford data, see Bronk Ramsey *et al.* 2004a; for partial preliminary data see Manning *et al.* 2002b; Manning and Bronk Ramsey 2003). Instances of volcanic or earthquake (etc.) related CO_2 emissions tend instead to be highly time variable (as clear from assorted examples cited by Wiener, this volume; 2003:383). Nor do the recent VDL or other Akrotiri samples exhibit the likely signs of such an effect operating (for which, see e.g. Bruns *et al.* 1980; Hubberten *et al.* 1990; Calderoni and Turi 1998; Pasquier-Cardin *et al.* 1999) – where very significant old-age biases can be observed close to a vent or hot spring source (and low to the ground only) and these then fall off quickly to more or less zero with distance (a few hundred meters), or are highly time and/or location variable (even close to a source). As Olsson (1987:22) concludes of work on Iceland: 'it is generally very difficult to see any effect except on the lava from 1973 on Heimaey and very close to some hot springs'. When we look at the more recent LMIA radiocarbon data from Thera from secure contexts with standard pre-treatment and processing and correction, we do not see any samples with massively old ages, and no pattern of fall-off from these to 'normal' ages. Instead, the data are pretty consistent over all samples, and across different crop types and a twig (see above and Figure 1 – the ability to be able satisfactorily to combine the whole set of 23 data on normally pretreated/processed short-lived samples from the very final VDL horizon demonstrates this point clearly). The palaeobotanical research of Sarpaki (1990) also indicates that the samples dated probably came from different fields, and so cannot all have been affected by a localised consistent effect (this, for Thera, and then the consistent LMIA data for Rhodes and Miletos, and data from Crete from LMIB also supporting a raising of the conventional low chronology, all indicate that no local ^{14}C depleted CO_2 source on or near Thera feeding either the atmosphere or root systems can satisfactorily account for the overall pattern of consistent data – suggestions to the contrary are special pleading with no positive evidence as of the present time). The usually (or on average) windy nature of Santorini (and so rapid atmospheric mixing), and the likelihood of some (most) crops growing on the non-caldera surfaces/slopes of the island (and so not in an imagined trapped reservoir of old carbon dioxide inside the caldera), further supports such a view. One may also note, for example, that radiocarbon satisfactorily dates the archaeology at Pompeii buried by the eruption of Vesuvius (Vogel *et al.* 1990; Nelson *et al.* 1990:202). Whatever 'effect' is proposed, it is also clear that no such 'effect' applied in the Levant (and so whole east Mediterranean?) generally, since, for example, good quality data from Tell Es-Sultan (Jericho) for the late MBA provide entirely satisfactory data (Bruins and Van der Plicht 1995). But, at the same time, it also seems that it is not only in the Aegean that we find earlier dates than expected from the conventional archaeological chronology in the 17th–16th century BC; it seems that a key site at the other end of some of the synchronisation discussions might also prove interesting in this regard. Kutschera *et al.* (2004) report radiocarbon data from the Second Intermediate Period (SIP) strata at Tell el-Dab'a, which, on average, suggest dates rather earlier than those assigned by the excavator, and quite compatible with some suggestions of a somewhat earlier dating of some of these strata and with the general high Aegean/ Cypriot chronology synthesis (e.g. Manning 1999). More data are needed to investigate the Tell el-Dab'a case.

The other approach is to accept that perhaps the radiocarbon data are more or less accurate (whatever minor problems and errors may apply). In the case of the dating of the Thera VDL, roughly the same age determination give or take a few decades has been found repeatedly over many years by several teams following standard pre-treatment procedures on relevant short-lived samples: see Figure 7 (and compare Figure 1) – with the recent work yielding much increased precision. All the data offer relatively similar outcomes. Thus the finding appears relatively robust within errors. If we regard these radiocarbon data as roughly accurate, then it is the conventional interpretation of the very limited archaeological-historical data for Aegean-Egyptian correlations c.1700–1500BC which needs some reconsideration. This would be along the lines proposed by the high chronology, or at least the compromise high chronology.

If we do follow this line and accept the radiocarbon evidence, then what scale of up-dating is at issue? The 'conventional' chronology has rather fragmented of late and has become a bit of a moving target. This alone almost demonstrates the value of the high chronology challenge, as new better positions are being developed; it also reveals the lack of evidential strength to the original standard consensus (much though it lasted for most of the 20th century AD). For example, when does the conventional chronology believe that the eruption of Thera occurred? I review just a handful of publications by prominent scholars:

Bietak (1997:125)	1515–1467BC
Bietak (2003)	early 18th Dynasty, before Tuthmosis III, so within c.1540–1479BC
Driessen and Macdonald (1997)	1550/1530BC
Eriksson (1992:219)	1460BC; c.1479BC in SCIEM2000 Euro-Conference 2 paper May 2003
Warren (1984)	1500BC
Warren (1999:900–902)	1520BC
Warren (2001:116)	1520/1500BC
Warren and Hankey (1989:215)	1535/25BC or 1560/50BC
Wiener (2003:363)	1560–1480BC, some preferring before 1530BC, perhaps 1560–1550BC (p.364 n.4)

A full 93 years is encompassed within the dates quoted, or 80 years if one takes Wiener's starting point. This is hardly a precise date. It is also important to note that some already seem prepared to concede one of the original starting points of the 'high' chronology challenge: that the eruption perhaps occurred before the 18th Dynasty began, in the SIP. A date c.1560–1530BC can just about be argued to be potentially compatible with the radiocarbon evidence – it is unlikely/less likely, but possible (see Figures 1, 7). A date from c.1520BC or later cannot be accommodated with the radiocarbon data – even at a considerable push.

A date of c.1560–50BC is some 30–60 years earlier than the Warren conventional position. It is just 38–48 years short of the lower limit of the 95% confidence most likely date range found for the Thera VDL in the study of Bronk Ramsey *et al.* (2004a). In other words, it is about halfway – and has to accept most of the high chronology synthesis concerning LMIA = SIP, LCIA (1 and 2) = SIP. Manning (1999) referred to this position as the compromise early chronology, Wiener (2003:364 and n. 4) instead claims it as the 'modified Aegean short chronology'. Critically, this position *must* adopt some version of the high chronology arguments about LCI regionalism and differences in trading partners in order to explain a pre 1560/50BC WSI bowl on Thera and yet no WSI at Tell el-Dab^{c}a (etc.) until early 18th Dynasty contexts (and generally Tuthmosid ones – and note that the most recent assessments of the material at Tell el-Dab^{c}a tend to place the appearance of WSI, BRI, etc. *later*, no longer initial 18th Dynasty as in e.g. Bietak 2000:fig.1, but now down into the Tuthmosid and mainly Tuthmosis III period: Bietak 2003:24, fig.1). Please note. Thus the compromise early/modified short chronology will end up employing most of the arguments of the 'high' chronology synthesis, especially with regard to Cyprus (i.e. as proposed first in Manning 1999; developed in Manning 2001; Manning *et al.* 2002a). However, this position has the big advantage of offering a much easier explanation for the early WSI bowl on Thera and the early WSI at late MBA Tell el-^{c}Ajjul, versus the general earlier 18th Dynasty finds of (mature) WSI, since only a few to several decades are at issue, and not about a century. As Bergoffen (2001a:146) notes, it of course seems inherently unlikely that early WSI pottery was in use at Tell el-^{c}Ajjul decades before its appearance at Tell el-Dab^{c}a. Bietak (2003:25–27) notes and dwells on this same 'gap' problem – the compromise early chronology seems to offer a way around – but from the perspective developed by the 'high' chronology synthesis.

The weakness of the compromise early chronology is that no actual suite of evidence as a whole really supports this specific date – it is just a compromise trying to best accommodate everything. In contrast, we do have some quite significant radiocarbon evidence more clearly supporting a date within the

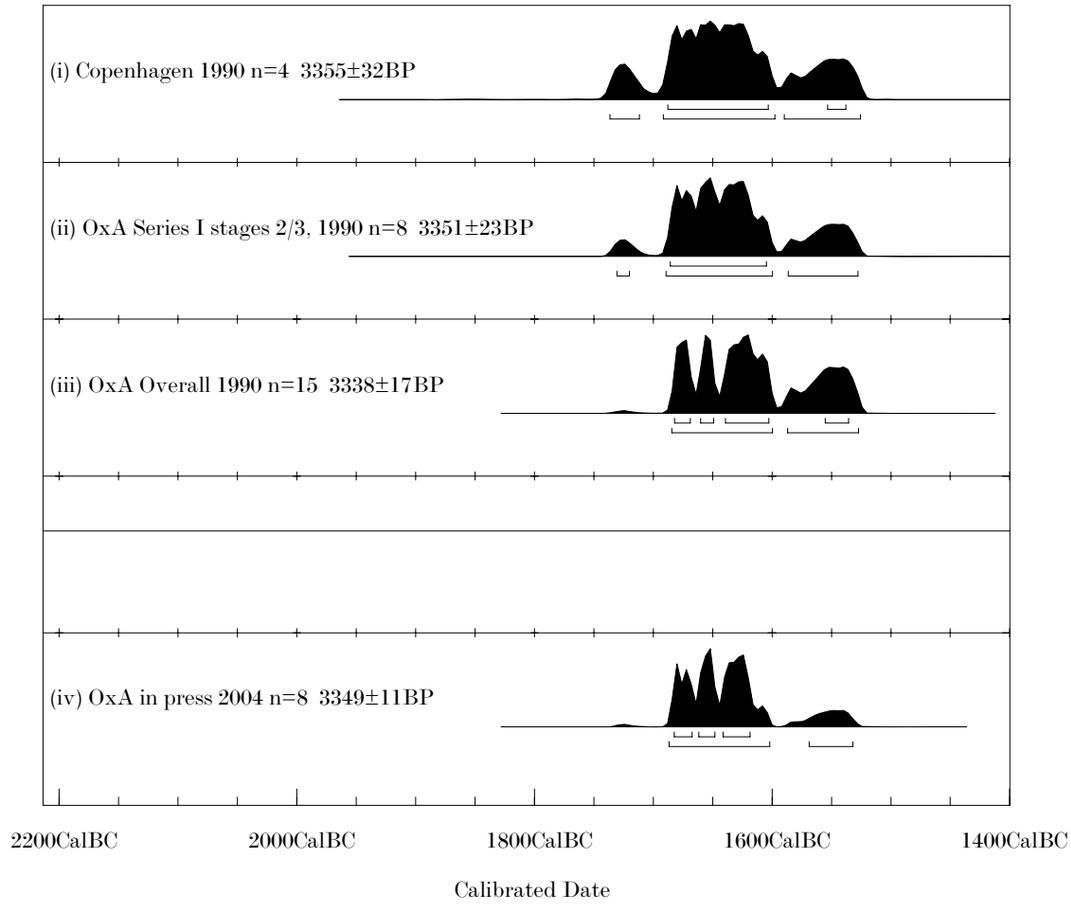


Fig. 7 Comparison of the calibrated age probabilities for each of the weighted averages on a variety of published or in press Akrotiri VDL data on short-lived samples from standard laboratory pre-treatment and correction regimes (except (iii) which includes the attempt by Oxford to replicate the 'novel' Simon Fraser protocol – which they could not – but nonetheless includes Oxford's Series II 'residue' data as the best overall estimate reached by Housley *et al.* 1990). For data see Friedrich *et al.* (1990), Housley *et al.* (1990); Bronk Ramsey *et al.* 2004). The in progress and not yet in press VERA data included in Figure 1 are not included here. Calibration using OxCal 3.9 (Bronk Ramsey 1995; 2001 and later versions) and INTCAL98 (Stuiver *et al.* 1998a). Curve resolution set at 4. The lines under the distributions indicate the (upper lines) 1SD and (lower lines) 2SD calibrated ranges

bounds 1663–1599BC. Note: The final published data in Bronk Ramsey *et al.* 2004a in fact yielded an average age of 3350±10 BP – versus the 3349±11 BP figure used above. There is no noticable difference.

The major gap in the radiocarbon data at present is the lack of any earlier LMIB datasets (just as correlation evidence from Egypt for LMIB is in fact for the classic *late* LMIB material known from the close of LMIB destructions on Crete). Such urgently needed earlier LMIB (or early to mid LHIIA) data now offer a critical test. Do they lie (also) in the late 16th century BC running into the current LMIB destruction data (and so indicating a relatively short, or later-dating, LMIB period), or do they lie in the earlier to mid 16th century BC, consistent with and requiring the high chronology? We need such data to find out: short-lived samples from closely defined contexts of early/earlier LMIB. Let us hope that excavators in the region can supply some suitable contexts and samples therefrom (recognising such material is a problem – what is needed are some overall LMIB stratigraphic sequences where samples may be taken from early through late to build up a chronological range for the overall period – one notes for example the reports of up to three building/stratigraphic phases within LMIB in House X (room 2) at Kommos, and similar reports of several building phases within LMIB at Palaikastro and Pseira (Manning 1999:334 with references).

5. Aegean Archaeological Evidence and Egypt for the LMIA–IB/LHIIA periods

Despite a lot of activity and 15 years of time, not that much has really changed in the archaeological situa-

tion since Warren and Hankey (1989); much of the potential ambiguity (e.g. Betancourt 1990) remains. It was subsequently suggested that finds of LHIIA or LMIB ceramics in earlier 18th Dynasty contexts in Egypt seem, plausibly, to require some revision of the Warren and Hankey chronology (and see already Warren and Hankey 1989 'postscript' p. 215). In particular: the LHIIA items from Tomb NE 1 near the Teti Pyramid at Saqqara were suggested to date from a burial during the reign of Amenhotep I *or* earlier (e.g. during the reign of Ahmose) (see for discussion Manning 1999:204; for correction of dating of the alabastron and cup to 'classic LH II A, contemporary with LM I B', see Macdonald 2001:530). Thus a date for deposition of classic LHIIA no later than c.1494BC and perhaps anywhere c.1550–1494BC was required. The LMI rim from Kom Rabi^{c}a could be argued to offer similar evidence (Manning 1999:203–204 and references). A perhaps early LHIIA squat alabastron from Gurob from an early 18th Dynasty context might also be called into play (Manning 1999:206 with references; Macdonald 2001:529 for early LHIIA suggestion). Wiener (2003:364 n.4) notes this evidence and observes that these archaeological data are hard to reconcile with suggestions of a date for the Thera eruption c.1500BC, let alone one even later. Another relevant item is the early BRI jug from Saqqara (unnumbered intrusive burial in Mastaba 3507), for which Merrillees (2001a) argues that 'a date of around 1525 B.C. for the deposit cannot be too far wrong' – i.e. very early 18th Dynasty. Reviewing the jug and associated typology, Merrillees argues that this perhaps places the LCIA2–LCIB transition around 1525 BC *or before* – Merrillees (p. 27) notes, as he has many times, that the issue of unknowns in point of production for the type within its period of production (from LCIA2 to LCIIA1 – Merrillees suggests LCIA2 for this vessel) *and* possible time-lag in deposition, 'gives all such chronological calculations an intrinsically conservative character, which, if any adjustment were to be made, would require the figures to be raised, not lowered'. A few other finds of BR also seem to confirm that BRI began to be deposited in Egypt *no later than* the very early 18th Dynasty (Eriksson 2001:58 with references – in a paper mainly arguing for a low chronology). The much debated but unpublished (early) BRI juglet said to be from a SIP context at Kom Rabi'a may offer even slightly earlier *terminus ante quem* evidence for a point in LCIA2 (at a minimum) in the SIP (Merrillees 2001a:27–28 and references; but cf. Eriksson 2001:56 and 58).

But against such evidence and analyses/interpretations, Aston (2003:141–143) finds little or less solid evidence to support the necessity of early dates for the Saqqara and Kom Rabi^{c}a contexts, and indicates that such material could also date down to the reign of Tuthmosis III. Of course, the paper of Aston leaves one wondering what the contexts and ceramics of Ahmose to Tuthmosis II look like, as most contexts/material previously held to be early seem to be being pushed down into the Tuthmosid period – *some* material one feels must be early 18th Dynasty (which, as Aston notes p.140, 'is the least well known of the four defined pottery phases, since very few tombs can be unequivocally dated to this period'). Thus we are rather left with ambiguity – the evidence could seem to be potentially compatible with a higher chronology taken in one light (and the LMIB radiocarbon evidence); but it can also be argued to be compatible with the low chronology (subject to the observation, when reading Aston's study, that we do need to reflect on the fact that some early 18th Dynasty material has to be found for the c.50 odd years before the accession of Tuthmosis III!). Aston (2003:145) observes, as others have before, that the reign of Tuthmosis III marks the change from LMIB to LHII imports (and by no later than late in his reign to LHIIB imports: Warren and Hankey 1989:145–146; Macdonald 2001:530 who revises the Lachish stemmed cup to LHIIB) – where within his reign is of course important, but not clear on current data.[4]

[4] The interpretation of the wall paintings at Thebes showing Keftiu (Minoans) is problematic. Manning (1999:209–220) offered an attempt at a 'high' chronology interpretation, but Macdonald (2001:529) makes some fair criticisms. How to interpret the transmission of the item depictions and clothing styles is not clear. The early Tuthmosis III Senmut tomb undeniably has LMIA/LHI style representations (and broadly the Senmut and Useramun tombs depict types that could date from here to the end of LMIB/LHIIA). We then have later links for the late Tuthmosis III paintings in the Rekhmire and Menkheperraseneb tombs. Some see these as LHIIB/LMII to LMIIIA/ LHIIIA, others have pointed to LMII–IIIA1 design elements (and Warren 1998 was prepared to consider LMIIIA1 as starting by the end of the reign of Tuthmosis III) (in general for diagnosis of the objects depicted, see Matthäus 1995:esp. 184–186). Macdonald's cautions about transference from one medium to another are relevant, but we have little data to work with to control this one way or the other. The likely use of copybooks by the artists further introduces possible time delay (more plausible for the earlier paintings – whereas the re-painting in the Rekhmire

Late Minoan IA remains notably *not* dated by any finds in Egypt, and there has been little evidence recovered for any direct contacts between Crete and Egypt at this time. Some fairly general stylistic linkages have been noted, such as suggested comparisons of Egyptian 'imitations' of Minoan rhyta (Koehl 2000), but these lack close chronological control. It is not actually known that they are imitations – and the decorations are not Aegean – nor, if they are imitations, what they were imitating or how (Koehl 2000:97). Time-lags and other issues involved are unknown. Koehl proposes an early LMIA style for SIP/early 18th Dynasty examples and LMIA comparanda generally for examples through to the mid-18th Dynasty (p.96) – but we potentially see mature LHIIA and LMIB material current also in the early or earlier 18th Dynasty in Egypt (above); at present there appears to be limited useful/precise chronological value to be derived from the rhyta. The Tell el-Dab^{c}a wall paintings are discussed below (see Section 9 below).

An excellent recent review of Kamares imports to Egypt and the associated chronological issues finds a chronology entirely compatible with, and rather in support of, the Aegean/Cypriot 'high' chronologies for the mid-second millennium BC (Merrillees 2003). It is wrong to imagine that the MBA evidence does not permit the Aegean/Cypriot 'high' LB1 dates (and important indeed to consider the evidence of the Middle Cypriot/Middle Minoan chronologies: a point made by Merrillees 2001a:29; 2003); similarly, the Aegean 'high' chronology rejoins the 'conventional' chronology around the close of the 15th century BC. The question and issue is the dating and cultural synchronisms for LMIA and IB, and Late Cypriot IA and IB, and how these relate to and inform the wider east Mediterranean picture. The evidence here is not clear-cut from the archaeology alone (contrast the 14th–13th centuries BC) – that is why we are now in the third decade of vigorous discussion surrounding the dating and synchronisms of the LMI and LCI periods.

For the high chronology to be possible, the LMIB period must be long.[5] The LCIA2 period must be likewise. An obvious problem is that earlier LMIB would be placed in the late Hyksos period through to the start of the 18th Dynasty (one can argue that the archaeological evidence could be compatible with mature LMIB and early to mature LHIIA and LCIB occurring in Egypt from early in the 18th Dynasty (and not just from about the reign of Tuthmosis III)) – but there is little positive evidence for this (the debatable Abydos sherd and tomb excepted – see Manning 1999:204 and references). But, in reverse, as Betancourt (1998:292) reminds us, 'one must remember that the LMIB pottery we have as intact vases in Crete is from the very end of the period, at its destruction'. It is these very late LMIB assemblages which offer the comparanda for most of the known LMIB items from Egypt (and in support we may note that this late LMIB is in fact being replaced by mature LHIIA and LHIIB Mainland products in many cases) – thus one could argue that the LMIB items from Egypt be regarded as mature/late LMIB (Manning 1995:220–221; a point agreed upon by Mountjoy 1999:16). If so, we are at present simply lacking material from the earlier part of the overall LMIB/LHIIA phases in the available synchronisa-

tomb implies greater immediacy and need to revise the images of Keftiu – and the choice of the different clothing style, kilts, and their decoration, is perhaps relevant in this regard and might be linked to a real embassy or similar to the Egyptian court that informed both this and the Menkeherraseneb tomb paintings). When and how long to apply for such time-lags is of course unknown. The conclusion at present is probably that these data as they stand could be argued to conform to either high, compromise, or low chronologies.

[5] Wiener (2003:394 n.161) raises as a problem for the high Aegean chronology the issue of the length of the Shaft Grave period at Mycenae, suggesting that this covers 'approximately three generations' on the basis of studies then cited by him. I am afraid I fail to see the evidence which supports this view, and why it affects the high chronology case. The Shaft Graves at Mycenae cover the MHIIIA to LHIB periods (Dietz 1997:fig. 1 usefully compares the two recent chronologies of the graves) – the latter runs to around and perhaps just after the eruption of Thera (it is possible to argue that LHIIA starts pretty much around or shortly after the eruption: Macdonald 2001:527; Manning 1999:17, 19, fig. 16). Dietz (1991; 1997), for example, in detailed studies of the chronology of the Shaft Grave assemblages, has been happy enough to work with the high chronology. No total contradiction is evident from the Shaft Grave material itself. What *is* the issue, as noted in the main text, is the length of the LMIB and LHIIA phases. Can the earlier through mid parts of these phases stretch to the start of the 18th Dynasty? This is the issue or problem for the high chronology. Can the overall (start to end) LMIB/LHIIA periods be around or a little over 100 years in length? We must instigate research to try to find out. At present, I can merely quote a leading Minoan ceramic specialist who states that the length of the LMIB period 'must surely be well over a century' (Betancourt 1998:293). The compromise high chronology/modified Aegean short chronology of course avoids this issue/problem.

tion matrix (and again one may note the scarcity of agreed upon very early 18th Dynasty contexts in Egypt [see above], and the quite sharp change in the character of the Hyksos versus New Kingdom levels at Tell el-Dabᶜa: Maguire 1995:54). On Crete, the evidence would seem to indicate that LMIB is a long phase (Betancourt 1998:293; Manning 1999: 330–335; only the classic/late LMIB material from the last part of the phase defines the short LMIB period of Popham, e.g. 1990). For Cyprus, we see classic/mature LCIB material deposited mainly from around the reign of Tuthmosis III, but perhaps as early as Amenhotep I (Aston 2003:140, 145 and references). Earlier 18th Dynasty imports of Black Lustrous and WPVI *could* be earlier LCIB (they die out on Cyprus during the course of LCIB) presumably from the east of Cyprus where WPVI lingers on longest, but they could also be LCIA (presumably LCIA2). The former with a decent length for the LCIA2 phase could be compatible with the high chronology, the latter favours the compromise high chronology or the low chronology.

6. The White Slip I Discussion

Here there has been some significant misunderstanding of the Cypriot data and literature, although progress has been made in recent years. The two key issues are:

1. The regional-temporal development processes of LCI as outlined by Merrillees (1971; see also discussions in Baurain 1984) and refined since (see further discussion in Manning *et al.* 2002a). This point is now increasingly noted and recognised (e.g. Bietak 2003) – debate is starting to move to the temporal scale and impact of this situation – versus ignoring it. This is positive. The regionalism of LCI Cyprus only comes to an end, and island-wide approximate homogeneity of (now all) mature LCI assemblages occurs (with, e.g., WSI of the framed wavy line style, as found all over Cyprus), by the end of the LCIB period – and not from the start of LCIB.

2. The tendency to ignore the actual range of production/occurrences of Cypriot wares (Åström 1972:675–705) and the application just of an initial date to foreign finds – where one might often expect them to in fact derive from later during the ware's lifetime of currency. Merrillees has noted this problem several times (e.g. Merrillees in Karageorghis 2001a:159, 217–218; Merrillees 2002:2, 5). Thus PWS appears in LCIA1 and then continues in LCIA2 and into LCIB (where it tails out). There is no reason for a given foreign export example or group to always be LCIA1; they could very well be LCIA2. WSI appears from the beginning of LCIA2 and then is dominant/standard in LCIB. There is some overlap with WSII. The length of this overlap and whether to see it as LCIB/IIA, or earlier LCIIA, is not clear. Appearances of earlier style WSI might thus be likely to be LCIA1/2 or LCIA2, and mature WSI most likely LCIB. PBR also occurs in both LCIA2 and into LCIB. Base Ring (BR) I occurs from LCIA2 and right through LCIIA and perhaps later for some classes like the jugs employed in funerary contexts (Manning and Monks 1998). There is significant overlap with BRII.

The value and interpretations of certain patterns observed in finds are of limited value without consideration of the appropriate Cypriot context, and the possible ranges involved.

ᶜEzbet Helmi/Tell el-Dabᶜa and the first appearance of White Slip (WS) I issue

On current evidence PWS makes its appearance in Stratum D/2 and continues through to the very start of Stratum C/2, and WSI makes its first stratigraphically securely attested appearance at 'Ezbet Helmi/Tell el-Dabᶜa in the Tuthmosid period (Stratum C/3) and stops by the end of this phase (end of Tuthmosis III) Bietak (2003:24, fig. 1, 27). Very little PWS/WSI overlap is noted (c.5 years in Bietak 2003:24, fig. 1). But this should be the whole of the LCIA2 period and at least a significant part of the LCIB period! Five years cannot represent the overlap attested on Cyprus (LCIA2 to during LCIB). Even on the ultra-short Åström (1972:762) chronology, this represents a significant span within the total time estimated as 115 to 135 years for LCIA2 and LCIB together.

Thus it is immediately clear that the sequence at Tell el-Dabᶜa does not represent the entirety of the relevant Cypriot record, or has specific biasing factors that lead to over-compression of the Cypriot import sequence. The quite sharp break between the late Hyksos levels with MC to LCIA1(–LCIA2) imports, versus the New Kingdom levels and imports, has been noted several times (e.g. Maguire 1995:54), and may have some bearing on this issue. Similarly-WPVI occurs in LCIA1, LCIA2, and LCIB on Cyprus; yet it too is shown with a c.5 year overlap with WSI and RLWM at Tell el-Dabᶜa. Again we seem to miss almost the entirety of the LCIA2 and LCIB overlap in this record. Similar problems exist earlier also. White Painted (WP) Pendent Line Style (PLS) occurs in MCIII and right through LCIA. It occurs at Tell el-Dabᶜa from Stratum F and ceases after the middle of Stratum D/3 (Merrillees

2002:3; Bietak 2003:24 Fig.1 shows it through to the end of Stratum D/3). Other evidence from Egypt suggests a similar range from the 18th century BC through to no later than the end of the Hyksos period (Merrillees 2002:4). This implies that LCIA2 was mainly if not all before the end of the Hyksos period also, with LCIA1 before this, and then MCIII. We are left with a general scheme of MCIII from the mid 18th through mid–17th century BC, and LCIA (1 and 2) from the mid-17th through mid-16th century BC (Merrillees 2002). Indeed this is exactly the consensus Cypriot chronology for the Cyprus Museum recently circulated by Dr. Sophocles Hadjisavvas: LCIA c.1650–1550BC, LCIB c.1550–1450BC. Bietak (2003:24, fig. 1), in contrast, has LCIA2 entirely in the 18th Dynasty and ending between about 1500 to 1460BC. But we lack a variety of expected LCIA2 evidence in the New Kingdom strata.

The situation with WPPLS and WP Cross Line Style (CLS) versus WPV at Tell el-Dabᶜa further illustrates the problems. On Cyprus WPPLS appears at the MCII/III transition and WPCLS in MCIII, both then continue right through the LCIA2 period. WPV likewise appears no earlier than the MCII/III transition and is a MCIII and LCIA ware (Manning *et al.* 2002a:152–153 and references). WPPLS, WPCLS and WPV have more or less the same spans on Cyprus. But, at Tell el-Dabᶜa, finds only partly overlap, with WPV appearing three or more strata *after* the others, and then continuing on for an additional stratum (Bietak 2003:24, fig. 1). We are clearly *not* seeing the entirety of Cypriot production attested. Roughly the first half of the WPPLS and WPCLS representation is plausibly MCIII and perhaps the second half is LCIA. In which case, more or less all the WPV must be assumed to be LCIA, since it only overlaps with the *final* ? to ¼ of WPPLS and WPCLS presence, with most of the MCIII production of this ware not (yet) attested at Tell el-Dabaᶜa (only recently did WPV even get extended back into (mid) Stratum E/1: Forstner-Müller 2003:170 and n. 23, fig. 6; cf. Manning *et al.* 2002a:153). This in turn suggests a close for the LCIA2 period around the end of the Hyksos period as represented by finds overseas. Allowing for even some time-lag effect, the real date on Cyprus can only be older (whether by a very short interval or possibly longer). Again the evidence is consistent with and/or supports the approximate Cypriot chronology of Merrillees (1977; 1992; 2002; 2003).

The implication of such observations is that the appearance of WSI at Tell el-Dabᶜa does not date the first appearance of WSI in Cyprus (or elsewhere – and it seems to occur in the MBIII period at Tell el-ᶜAjjul: Bergoffen 2001a).

Stylistic date of the Thera WSI bowl?

At present much of the 'high' versus 'low' chronology debate hinges on a now *lost* WSI bowl from Thera (Merrillees 2001b). The irony is impressive. Bietak (2003) argues that this bowl is not early style WSI but instead states that 'on the contrary, there are other assessments putting this bowl late in the WSI development' (p. 26). This claim shows a mistaken understanding of the evidence and previous analyses. Bietak correctly cites Merrillees (2001b:93) as the best recent study of the Theran bowl, but then significantly misrepresents Merrillees' views. Merrillees (2001b) suggests a stylistic date of LCIA–LCIB transition for the Thera bowl. Merrillees sees WSI production covering Late Cypriot (LC) IA2 and *all of* LCIB (compare Åström 1972:700 Chart). Thus an LCIA–LCIB transition date is in the *earlier* phase of WSI production (maybe broadly about ⅓ the way through). Merrillees' text (2001b:93) clearly confirms such an 'earlier' view when he commends Niemeier's (1990:122) observation of 'some Proto White Slip features, such as Popham's (1962, 283) 'rope' pattern with oblique cross lines, whereas for developed White Slip I Popham's 'ladder' pattern with cross lines at right angles is characteristic'. (The reader may indeed note that 30 years ago Merrillees 1974:6 and n. 16 in fact classified the Theran bowl as Proto White Slip – it is undeniably 'early' looking WSI.) Merrillees goes on to note that Niemeier therefore placed the bowl early in the WSI sequence and Merrillees writes 'Niemeier has identified the crucial element in the decoration which enables it to be placed ... in its proper chronological horizon …' (Merrillees 2001b:93).

Elsewhere (Manning *et al.* 2002a:98–106, 160–162; Manning 1999:153–157), I have argued that the Theran bowl is 'early' style WSI (so earlier WSI phase of production) – very much consistent with Merrillees (2001b). The analysis of Bergoffen (2002:34 n. 45, 36 n. 55) reaches a similar position. We can quickly review the salient issues and note a couple more with regard to the Thera WSI bowl:

(i) The decoration is early style WSI (Manning *et al.* 2002a:160–162) with the 'rope' pattern oblique cross lines in particular arguing for placement early in the WSI sequence (Merrillees 2001b:93);

(ii) The paired vertical lozenge chains are shown as *not* joining and with quite large irregular dots on either side (i.e. more PWS in inspiration than classic WSI) (Merrillees 2001b:fig. 2);

(iii) The decoration exhibits none of the features of later or mature WSI as typical of the LCIB period. It lacks the space and clean linear styling of classic/mature WSI and instead has early features only, including single pendant lozenge chains framed by single vertical lines. The best comparanda come from contexts most likely dated LCIA, such as *Toumba tou Skourou* Tomb VI (Vermeule and Wolsky 1990:317 TVI.25, fig. 46 – a context most likely to be LCIA since the rest of the assemblage is early LCI and there are no classic WSI items). And, in contrast, contexts of mainly LCIB (and onwards) date have lots of mature WSI but none of the 'rope' lattice early style WSI – with the tombs at Stephania and Ayia Irini not that far from *Toumba tou Skourou* nicely illustrating this clear distinction (noted first by Padgett in Vermeule and Wolsky 1990:374). This distinct, later, material very often features the two parallel line styles (framed … styles, into metope style) (Hennessy 1963; Pecorella 1977). The association with LHIIA vessels at Ayia Irini (Tomb 3 where all WSI bowls are of the classic style – nos. 24, 37, 38, 61, 107, 110, 126, 127, 128 and 129 – and there are also two LHIIA imports – nos. 16 and 29: Pecorella 1977) may also indicate a likely approximate LHIIA/LMIB–LCIB linkage, just as the LMIA and Late Cycladic finds at *Toumba tou Skourou* indicate a LMIA–LCIA linkage (Vermeule and Wolsky 1990:381–383; Cadogan 1990:95).[6]

(vi) The Thera bowl is said to have had a brown fabric (Merrillees 2001b:90, 93). This is not distinctive with regard to provenance, as fabrics for WSI vary quite widely, but does run counter a likely provenance in southwest Cyprus, such as around Palaepaphos *Teratsoudhia*, where fabrics are typically whitish, yellowish, grey, dark grey and to reddish (Karageorghis 1990 – the few WSI sherds with 'brownish hard gritty clay', e.g. pl.VII (i), p.42, do not come from bowls offering stylistic comparisons to the Thera one). In contrast, among other possible loci, *Toumba tou Skourou* (Vermeule and Wolsky 1990) provides a number of brown or brownish fabric WSI bowls (e.g. T I.105, 505, 512, 522 532, 533, 544, 546, 547, 519, 537, 531, 521, 522, 543, Lo II.25).

(v) The Theran bowl is distinct in style and fabric from the mature WSI bowl (in special white fabric WSI) known from Phylakopi (contra Bietak 2003:27–28). I argue it is also separate in time. One is LCIA, the other LCIB. The Phylakopi bowl quite possibly did come from southern and probably southwestern Cyprus (e.g. *Teratsoudhia* as Karageorghis 1990:57 n. 28 has suggested).

Archaeological date/context on Cyprus?

This is less clear-cut as we lack good LCIA to LCIB stratigraphic data from most sites, and rely mainly on evidence from tombs used often in both periods. Thus one can offer a 'higher' view by associating the early style WSI with only the other earlier material (i.e. LCIA), or a 'lower' view by suggesting association also with later LCIB material. One cannot on current evidence prove the former position; one can only argue that it makes sense in terms of the observed PWS-WSI evolution, and that early WSI occurs in contexts that can most plausibly be dated LCIA only, whereas classic/mature WSI occurs either in contexts covering LCIA and IB or just IB. There is of course overlap. Thus PWS occurs in LCIA1 through LCIB1, early style WSI in LCIA2 through IB, and classic/mature WSI mainly in LCIB.

The time problem?

Bietak (2003:25–27) argues that, while he partly accepts the regional development of LCI Cyprus scenario, he cannot see how this provides more than a few years (10 or 20, maximum 25 years), and thus he argues that one cannot have WSI on Thera in the 17th century BC *and* first showing up in Egypt in the Tuthmosid period.

Moreover, in recent years, the Tell el-Dab^{c}a evidence has actually been *widening* this gap, or time problem, as the appearances of WSI and BRI have been if anything pushed later at the site. Thus whereas in Bietak (2000:fig. 1; Bietak 2001:fig. 1) WSI and BRI were shown as starting from the beginning of the 18th Dynasty and PWS was shown as stopping at the end of the Hyksos period, now in Bietak (2003:24, fig. 1) PWS is shown as continuing well into the 18th Dynasty to the start of the reign of Tuthmosis III (into Stratum D/1: p. 27), and WSI and BRI have been pushed back to the reign of Tuthmosis III (BRI perhaps starting about Tuth-

[6] This picture is reinforced by early LCIA finds of Aegean material at Maroni: Cadogan *et al.* (2001) – Macdonald (2001:529) suggests that the Cadogan *et al.* (2001:fig. 3) sherd could in fact be MMIIIB–LMIA transition or even LMIB but for context – point accepted, but the sherd still offers consonant data for a general LMIA–LCIA linkage – since the context is early LCIA and a MMIIIB–LMIA transition to earlier LMIA date would be perfectly acceptable.

mosis I: Hein 2001:242–243). The 'problem' is therefore being made more and more acute. But the evidence is also very problematic as noted above. If the PWS represents the full range on Cyprus (as determined in Åström 1972:675–682, 700–701 chart), then earlier (to mid) LCIB ends (PWS declines) around the end of Stratum D/1 or around the accession of Tuthmosis III. In which case WSI and BRI of the LCIA2 and earlier LCIB phases is missing at the site – as is the substantial (LCIA2 and earlier LCIB) overlap of PWS and WSI. In turn, the LCIA2/IB transition is at an unknown earlier time. If the PWS is argued not to include material from the very end of the ware's history on Cyprus, then the problem is worse. It seems unlikely too large a time delay in transmission is involved in the later material, since the evidence comprises a reasonable number of samples (Bietak 2003:27). Is there an answer? Did WSI come relatively late as a dominant fashion to the main export area to Dab^{c}a – only *during* (even later) LCIB? One might wonder about the relevance of the remarks about LCI Cyprus and its exports to Egypt made over 30 years ago by Merrillees (1971) – they seem to have some relevance in view of the fact that most of the relevant MC and earlier LC Cypriot imports to Tell el-Dab^{c}a seem to come from eastern Cyprus (Manning *et al.* 2002a:103 and n. 15 and references there – odd possible exceptions notwithstanding: Bietak 2003:27 and n. 43).

This apparent 'time gap' is probably the biggest problem at present, and divide, between the high and low chronologies. The 17th century BC date for at least initial early style WSI stems from the radiocarbon evidence (Section 4 above). *If* we did not have the radiocarbon data, then one could more easily accommodate the LCIA2 to earlier LCIB periods in the 16th century BC, consistent with the analyses of e.g. Merrillees (1977; 1992; 2002; 2003); Kemp and Merrillees (1980). And the lengths of apparent time involved, and gaps, etc., would be reduced.

But I submit that current review of both the archaeological and scientific evidence indicates that the time problem is less extreme than envisaged by some scholars (while still an issue).

Bietak (2003) argues that the eruption of Thera occurred in the early 18th Dynasty, probably before the reign of Tuthmosis III – i.e. within the period c.1540–1479BC. And he notes the finds of Theran pumice from the Tuthmosid period and supports a linkage (Bietak 2003:28), so by implication a date closer to c.1494–1479BC. One view of the archaeological evidence indicates that the subsequent LHIIA and LMIB periods were perhaps already underway before the Tuthmosid period and possibly as early as the reigns of Ahmose or Amenhotep I (see above Section 5). If this turned out to be correct, then there is no reason at all to regard these deposits in Egypt as dating the *start* of either LHIIA or LMIB in the Aegean. These data would instead offer *termini ante quos* – with the length of the *ante* being unknown, but quite plausibly of several decades duration on any understanding (and potentially more). Thus an end for LMIA later than about the very start of the 18th Dynasty (18th Dynasty begins c.1550/1540BC) appears difficult from this view of the archaeological evidence, and this transition could well be several decades earlier (we simply have no robust archaeological evidence to constrain it in the upwards direction). But another view pushes several of these contexts/data down and probably also into the reign of Tuthmosis III, and so offers a synthesis entirely compatible with an early 18th Dynasty Thera eruption.

Is there any archaeological evidence to question the compact, short view? I return to the problem of the Cypriot sequence at Tell el-Dab^{c}a discussed above in this Section. We seem to have some gaps or compressions or biases; LCIA2 plausibly ends by the close of the Hyksos period, and there is thus the question of what happens in the 18th Dynasty until Tuthmosis III. Missing or unrecognised earlier LCIB and some LCIA2? Placing LCIA2 in the SIP as the evidence appears to support (also Merrillees 1992; 2002) would make such a date also required for the early WSI bowl on Thera (and the early WSI in a likely/possible MBIII context at Tell el-^{c}Ajjul (Bergoffen 2001a), and also BRI in a similar context at the same site (Bergoffen 2001b), can be seen as compatible). In turn, given LCIA–LMIA linkages, this would suggest a similar SIP date for the eruption. Whether it is late SIP (i.e. mid-16th century BC) as the compromise early chronology/modified Aegean short chronology allows, or earlier (late 17th century BC) as the radiocarbon evidence and Aegean 'high' chronology suggests, is then a matter for a choice: between (i) the easiest and – I agree – most plausible construction of the archaeological evidence given no other constraints, or (ii) a view regarding the significant body of radiocarbon data as requiring a way to be found to accommodate a late 17th century BC eruption date (and given that much of the archaeological evidence could be interpreted in a consistent light: e.g. Betancourt 1987; 1990; 1998; Manning 1988; 1999; Merrillees 1977; 1992; 2002; 2003).

The important point is that *either* position means

that early style WSI as found at Thera must *at a minimum* have been around before c.1540/1530BC – that is already some 50 or 60 years *before* WSI is reported from the C/3 Stratum at Tell el-Dabᶜa which is dated to the reign of Tuthmosis III (reign commences c.1479BC). Thus some significant 'gap' is present with no reference at all to radiocarbon evidence (i.e. the 'divide' is *not* just between archaeology and science). This gap can only be explained in terms of, first, the Cypriot regional development process tied into predominant trade associations (Merrillees 1971), and, second, in terms of some gaps, biases, or other factors affecting certain periods of Cypriot-Tell el-Dabᶜa linkages and artefact deposition. Therefore, even without the radiocarbon evidence, some model similar to the high chronology synthesis as in Manning (1999); Manning *et al.* (2002a) = Merrillees (1971) LCI Cyprus scenario, is required; with the radiocarbon evidence we definitely need a model like this.

At the other end, the suggested dates of 1628BC or 1645BC or 1650BC for the Thera eruption, based on hypothesises trying to associate tree-ring anomalies or ice-core signals (and the original compelling scenario where the ice-core and tree-ring data seemed able to be linked together in a package consistent with the radiocarbon evidence), have been set aside at present, since none can be demonstrated to have any firm link to Thera (see Sections 1–3 above). Yes, the eruption was very large, and yes its tephra and sulphur output may have been larger than some minimum estimates (Manning *et al.* 2002a:156–157 and n. 240; Stuart Dunn and Floyd McCoy, pers. comms.), but at the time of writing there is no tie between the eruption of Thera and any given ice-core volcanic signal, nor other absolutely dated environmental proxy such as tree-rings (see Sections 1–3 above). This is a major change in the background mentalité of much 'high' chronology work – the present author very much included. Things change. This also creates more flexibility. The key and only directly relevant scientific dating evidence at present is the significant body of radiocarbon data (see Section 4 above). Previous work indicated a most likely date for the Thera eruption in the 17th century BC (Manning 1999:232–246; Manning and Bronk Ramsey 2003), but with a lesser probability in the mid-16th century BC. This remains the case looking at the data for the Volcanic Destruction Level at Thera in isolation (e.g. see Figures 1 and 7). But, incorporating seriated sets of radiocarbon data from before, around, and after the eruption of Thera, new work in press (Bronk Ramsey *et al.* 2004a) indicates that a most likely date range at 95% confidence level may be calculated as c.1663–1599BC for the eruption (and a further, future, paper will consider an even larger database of information and will compare this against the new INTCAL04 calibration curve and a range of other calibration datasets – so, as always, the statement just made in the text is provisional pending further information). A date in the upper part of this range clearly adds in quite a few decades; a date lower in this range (late 17th century BC) is not really that far away from one view of the archaeological evidence, or is even compatible with it. The latter would be the much easier situation to accommodate.

For the high chronology, early style WSI would thus have to begin being made in an evolution out of PWS (and with several PWS-style elements in the decoration) by around 1630–1600BC at the latest (and maybe a few decades earlier – earlier half of the radiocarbon range) and an example is exported to pre-eruption Thera (and compatible contemporary return imports from Crete, and Thera itself, have been found on Cyprus, including at a plausible home for early-style WSI at *Toumba tou Skourou*: see above). LCIA would begin (depending on the length of LCIA1) some additional few decades earlier. There are limits here, especially as indicated by the pattern of WPPLS abroad (Manning 2001:78–80 [but note: now with the Middle or a low-Middle to Low Babylonian chronology applying, see Manning *et al.* 2001a]; Merrillees 2002 [ditto note re-revision to Middle or low Middle to Low Babylonian chronology]) – so somewhere c.1700–1650BC depending on choices and lengths assigned. WSI is then produced through to the reign of Tuthmosis III. LCIA2 would run from the later/late 17th century BC through to the mid-16th century BC, LCIB from the early New Kingdom through into the reign of Tuthmosis III.

On Cyprus, WSI and the 'LCI' package starts in the northwest, it is then found in the west and the southcoast. The east, especially, sees the 'MC' styles linger and really only becomes fully 'LC' by the *end* of LCIB. If the vast majority of Cypriot exports to Egypt and other centres in the southeast Mediterranean derived from eastern Cyprus, as seems to be the case (and seems plausible), then WSI (and other 'LC' wares [except Bichrome Wheelmade Ware, which was an eastern Cypriot invention as first argued by Artzy *et al.* 1973; Åström 2001:135 – it then spreads to other parts of Cyprus – and here I view the situation slightly differently to Karageorghis 2001b:144] would mainly not show up until *during* the LCIB period, even late in the LCIB period. This

roughly matches the picture at Tell el-Dabᶜa (Manning *et al.* 2002a:148–154). There undoubtedly were exceptions; but we see a reflection of the main pattern. The northwest saw some contacts into the Levant (perhaps MBIII Tell el-ᶜAjjul: Bergoffen 2001a, 2001b), and in reverse there are some Hyksos/MBIII influences/contacts evident from the material at e.g. *Toumba tou Skourou* (see Manning *et al.* 2002a for discussion and references).

The compromise early/modified Aegean short chronology has the archaeological advantage that it does not require such a long LCIA2 phase. It must however either rely on low/very low radiocarbon probabilities (or considerable subjective selectivity in data accepted), or even largely ignore this evidence altogether. Unless some significant problem can be identified with the current radiocarbon evidence (see Section 4 above), this seems a problem at present for the mid-16th century BC date. Of course, the situation will have to be reassessed in 2005 when the new INTCAL04 calibration curve is available, and when all relevant data from the current round of radiocarbon research are available. I am keeping an open mind on a possible re-think here, depending on the final data/calibration situation.

WSII and BRII

Attention recently has concentrated on the appearances of WSI and BRI at Tell el-Dabᶜa and elsewhere in the region. Here the current view is that none appears before the earlier 18th Dynasty, and indeed the majority of finds occur from the reign of Tuthmosis III (so from 1479BC) (Bietak 2003:24 fig. 1; Aston 2003:143, 145; Hein 2001:242–243 noting earliest appearance perhaps from about Tuthmosis I but typically Tuthmosis III; Fuscaldo 2003:71–72). This evidence, by itself, clearly seems to support a relatively low chronology, even if all this evidence is regarded as LCIB (or later – BRI continues well into earlier LCII). But we might also think about the appearances of WSII and BRII. At Tell el-Dabᶜa these wares are indicated as appearing during Stratum C/2, just after the Thera pumice, at about the transition from the reigns of Tuthmosis III and Amenhotep II. This is therefore c. 50 years after WSI and BRI first occur. This implies a very short LCIB and LCIA2 period if Tell el-Dabᶜa is regarded as the arbiter of Cypriot chronology. The question of when LCIIA began and WSII first appears in the Levant/Egypt is also not totally clear. Merrillees (1977:42) made a good case that the LCIB/IIA transition occurs before the end of the reign of Tuthmosis III in Egypt, and thus a little earlier on Cyprus, while a reported WSII vessel was found (with a BRI vessel and a likely LHIIA jar [revising previous LMIB attribution: see Hankey and Leonard 1998:33 n.30]) in the LB1 cache at Tell Taᶜannek near Megiddo often (if not totally securely) dated to Tuthmosis III year 23 (see Warren and Hankey 1989:142; Manning 1995:224–225 with references; 1999:206–207 and references). And, although disputed, some evidence may also indicate that BRII occurs before the end of Tuthmosis III's reign (Eriksson 2001:65 and references). Such indications might suggest that LCIB ends during the reign of Tuthmosis III – and not at its very end. Allowing for time-lags in transmission and then deposit, even if small, this could see LCIB ending on Cyprus perhaps around the middle of Tuthmosis III's reign – say c. 1450BC. This in turn requires that the LCIB and then LCIA2 periods be pushed 'up' at least somewhat from the very low dates determined by finds at Tell el-Dabᶜa.

7. Theran Pumice

Theran (Minoan eruption, Bo) pumice has been identified at Tell el-Dabᶜa and several other sites (for the latest on the identification of Theran pumice at Tell el-Dabᶜa and elsewhere, and for work towards establishing a robust approach to identifying the natural range of values for analyses of such finds of Theran Bo pumice/glass, see Huber *et al.* 2003). The Tuthmosid date for the appearance of this pumice at Tell el-Dabᶜa, and indeed its appearance mainly in the later part of the reign of Tuthmosis III in Stratum C/2 (based on Bietak 2003:28 and esp. 24, fig. 1), is held by Bietak to support an earlier 18th Dynasty date for the Thera eruption. The pattern of finds is indeed interesting. But the evidence as now understood, on solely archaeological grounds, in fact nicely disproves any relevance to the date of the Thera eruption. Stratum C/2 is dated to the later part of the reign of Tuthmosis III, and the words 'Thera pumice' appear *after* c. 1450BC in Bietak (2003:24, fig. 1). This is 50–110 years after even the mainstream 'conventional' chronology scholars place the Thera eruption (Driessen and Macdonald, Warren, Wiener cited above Section 4) – let alone returning to the discussions above in Sections 5 and 6. Thus this pumice most clearly does *not* show up in archaeological contexts at Tell el-Dabᶜa in Egypt until, at a minimum, many years after the eruption. The pumice thus merely sets a very loose *terminus ante quem*, with the length of the *ante* known to be at least 40–50 years and very possibly much more. There is no immediacy at all!

Bietak is right to note that Theran pumice suddenly appears and seems to be available from the mid-15th century BC. Why? This might be to do with either (i) new technology/practice in the region which now exploits a resource (perhaps linked with the Egyptian campaigns, building, and expansion into the Levant at this time), or (ii) the likelihood that Aegean traders (royal, or downwards) began supplying this 'special' resource as part of the well known Aegean (Keftiu) contacts evident from the reign of Tuthmosis III (especially).

8. Tell el-Yahudiyeh (TY) juglet at *Toumba tou Skourou*

Bietak (2003:29) argues that a TY juglet with lotus design from *Toumba tou Skourou* Tomb V chamber 1 disproves a high chronology. Let us therefore examine the situation. Bietak states that this type of TY was not made before Stratum E/3 at Tell el-Dabᶜa. Stratum E/3 is currently dated by Bietak at about 1685/80–1655/50BC (Bietak 1992; 1997; 2000; 2003:fig. 1). Somehow Bietak (2003:29) turns this into c. 1640BC. And it has to be remembered that these dates for Stratum E/3 depend on a variety of other interpretations – they are flexible, not fixed. Others have proposed dates a few decades or more earlier (Weinstein 1992; 1995; Dever 1997). Radiocarbon evidence from Tell el-Dabᶜa itself may also point in this direction (Kutschera *et al.* 2004).

This TY vessel is deposited in a late Middle Cypriot (MC) III tomb context. Thus this should be potentially at least a little before somewhere in the c.1700–1650BC range suggested for the start date for LCIA in the 'high' chronology (see above), if there is to be no problem. Merrillees (2002) has placed MCIII from c. 1750BC. Bietak argues that the TY juglet is early MBIIB. MBIIA overlapped into at least the start of MCIII, and ends about 1700BC give or take. Middle Minoan III on Crete starts also about 1750BC (Merrillees 2003). All these 'dates' are of course round numbers and approximate. We thus have a TY juglet perhaps of around the first few decades of the 17th century BC deposited on Cyprus close to the end of MCIII. Thus we might place the MCIII/LCIA1 border perhaps just after, c.1675–1650BC. We should remember also, of course, that the TY vessel date could go up by a few years given other interpretations of Stratum E/3's dating (just as, in reverse, it may not necessarily have been buried until a few years after production, nor be from the earliest phase of manufacture in Egypt).

There is thus no necessary problem (unless one tries to force the early MBIIB date down), nor are the 'dates' cited rock solid, but all are a little flexible. The synchronism can clearly work with a high Cypriot chronology; it certainly does not disprove it.

9. Tell el-Dabᶜa paintings and other LMIA linkage suggestions?

Bietak (2003:29) argues that the wall paintings from the Tuthmosid palace district at Tell el-Dabᶜa show some very close parallels to the Thera paintings, and he thus argues that Thera cannot have been too far away in time. Some similarities are indeed striking; but other elements of the Tell el-Dabᶜa paintings do not match so well with LMIA work and instead seem better dated to subsequent LMIB/LHIIA influences as a number of scholars have commented (e.g. Manning 1999:101–103 and references). There is also the problem that the wonderful and unique corpus of mature LMIA art at Thera leads scholarship to focus on it; we lack such full evidence from the LMIB period for example; ditto the Mainland which was increasingly important during the course of Tuthmosis III's reign. If we had more such evidence, then the Thera link might seem less definitive. We also do not know how chronologically stable representative traditions were with regard to wall painting, and certain key elements/motifs (and especially at élite centres) might vary from such normal rules even if we could provide such rules from the Aegean evidence (whether increased stability of tradition, or the reverse). It is notable in the Aegean that griffins, and bull-leaping, form a fairly stable tradition over quite a long period (much though we have only a few dispersed pieces of extant evidence). The range of possible dates and circumstances leads even a scholar prepared to accept a more LMIA stylistic link to end up stating that 'the value of the frescoes for Aegean chronology is very limited' (Macdonald 2001:529). And Macdonald wrote before it became evident that the frescoes date to Stratum C/3 and the earlier Tuthmosid period. The paintings could link the Tuthmosid palace with later LMI (i.e. mature LMIB) and derivatives and so be consistent with the high chronology, or the compromise high/modified Aegean short chronology, or they could fit into a low chronology.

Bietak (2003:29) raises the link made by Manning (1999) of some earrings shown in the Theran frescoes with examples found at Tell el-ᶜAjjul. Bietak states that Manning claimed these items were MB to fit his chronology; Bietak instead says they come from LB contexts. Bietak has not read the text of Manning carefully: he cites pages 55–59, and on p. 55 with reference to these items the text says in the

parenthesis 'see discussion in Chapter IV' – in Chapter IV on pages 138–139 there is discussion and here the text says that 'in past literature, the Tell el-ᶜAjjul examples have been dated LBI, but the correct date for these City II/Palace II, or earlier, finds (and the general tradition to which they belong) is later MBII in Syro-Palestinian terms'. There is then a footnote (668) citing references to the scholars who argue this, and a cross-reference to footnote 658, which also states the same thing with references and some related discussion. Of the scholarship cited I quote here Oren (1997:271):

Tell el-ᶜAjjul has yielded some of the largest gold hoards ever discovered in the eastern Mediterranean … Although most of the hoards were assigned to Palace II and City II of the Late Bronze Age I, thus postdating the "1570 B.C.E. destruction", some objects were recorded in Palace I, under its destruction debris. Typological considerations suggest that the assemblages of gold objects were collected over a long time and *subsequently deposited* [my italics] in the Late Bronze Age. Analogous examples from Megiddo, Gezer, and Ugarit confirm the MBIII–LBI horizon of the Tell el-ᶜAjjul hoards.

10. Egyptian Chronology

Here there is consensus among the standard range of scholarship, as Bietak (2003:23) states with references to the main studies (see also review of Egyptian and Mesopotamian dating by Wiener, this volume). All modern discussions of the last two decades place the beginning of the 18th Dynasty around 1550–1540BC. These studies all employ the same inscription/textual historical and genealogical data, and some make use also of the available astronomical records and their best retro-calculations to yield absolute calendar dates (e.g. recently Krauss 2003 with references, Krauss this volume). Given the information to hand, these studies are plausible and logical.

Contrary to claims by some critics, Manning (1999) employed this standard Egyptian chronology in his main text. In a couple of sentences of the main text and in Appendix 1 of the book, Manning explored whether perhaps there were any errors in Egyptian chronology that might allow another c. 11–25 years of time between the fixed point of 664/663BC and the start of the 18th Dynasty. It was thought that this might perhaps help synthesise science data and archaeological data. But there was no attempt to ignore nor undermine the worth of Egyptian chronology. Some criticism in print has been, to put it mildly, extreme and wrongly-based in most aspects: see the Appendix to this paper below.

So, is Egyptian chronology correct (for further details and references regarding the following, see the Appendix to the present paper)? This is a different question and one to which we cannot – at present – know the answer, precisely. However, it is unlikely to be far wrong, and some points may even be fixed (i.e. absolute). Both genealogy/history and astronomy seem to converge satisfactorily within the framework of conventional date ranges (see Rolf Krauss' paper in this volume). But it is also not unreasonable to argue for at least some flexibility at various points – pending general agreement on some astronomical absolute date fixes. The key thing to note is that one core element of current Egyptian chronology is built around highest attested reign years of a number of the pharaohs/kings – in few cases do we have a clear statement that so and so reigned a specific period, rather we have extant records up to a certain number of years. The question thus is whether there are unattested years (especially if there is little available data for a ruler, and thus more likelihood of missing information)? There is every reason to believe that there must be at least some: Kitchen himself has to accept that 'dead-reckoning' of such royal records leaves him a few years short between 664BC and 1279BC (an astronomically derived date). Therefore, in a simple logical progression, if we know that we have unknown information, then we cannot really quantify that unknown without other parameters being available. One could point to the astronomy, and yes, this seems to offer some key parameters, but there are at best a few precise data for the Middle Kingdom and then New Kingdom through Third Intermediate Period, and there remain disputes concerning the records (the interpretation and use of the texts), observation location and method, and various practicalities, and how best to analyse all these data and to which data one should give priority (see e.g. Wells 2002; Luft 2003; Wiener 2003:365 and n.7; O'Mara 2003). Taking a positive view we may be able to confirm the standard chronology (e.g. Tuthmosis III accession 1479BC) – taking a critical/sceptical view we are left with very few fixed points – for example Kitchen (2002:11) writes '…the lunar dates are all now to be discarded – see Wells 2002'. Wiener (2003:365) also reports such scepticism and concludes that such problems leave 'Egyptian texts as the sole chronological guide'. Such scepticism, and claims to reject for example all lunar data, have been shown to be based on incorrect or partial understanding of the data and their analysis (Krauss 1989; 2003; forthcoming; this volume). Nonetheless, such statements highlight the present lack of total consensus. Kitchen points to the genealo-

gies of various priests recorded at temples as confirming that his chronology cannot be extended more than marginally. Again, yes, these data offer general support, but nothing precise; contrary Kitchen, they do not rule out some small amount of flexibility (up or down) (see the Appendix to this paper below). We thus have an *approximately* 'fixed' chronology.

The radiocarbon evidence from Egypt has been limited in literature published up to 2003. Nonetheless, one may observe that calibrated radiocarbon dating in general is compatible with standard Egyptian chronology – even some apparent problems in the Old Kingdom period seem not really to be so once allowance is made for both the shape of the radiocarbon calibration curve through this period, and for the clear/likely indications that old and recycled wood are involved in many cases (Manning n.d.).

Prediction is very difficult,
especially about the future. Niels Bohr

11. Conclusion

Wiener (2003) referred in his title to the 'current impasse' in Bronze Age chronology. Bietak (2003) saw a conflict between science and archaeology; he did not think this conflict could be bridged. In light of the foregoing discussion, I suggest that both views need some modification.

First, review of the evidence indicates that only two positions are now plausible, either:

(i) The compromise high chronology/modified Aegean short chronology with a Thera eruption date in the earlier to mid-16th century BC and LCIA ending by about the end of the Hyksos period. Here the WSI bowl from Thera lies in the LCIA2 period (even LCIA2–LCIB transition as Merrillees 2001b argued) and is dated a little before c.1560/40BC. This can easily be compatible with other indications of LCIA2 ending around the end of the Hyksos period, of WSI and BRI from late MBA contexts at Tell el-^{c}Ajjul, and of BRI first occurring no later than the very early 18th Dynasty in Egypt if not in fact in the SIP (see Sections 4, 5 and 6 above).

(ii) The high chronology with the eruption of Thera determined as within the most likely span indicated by the current radiocarbon dating evidence (at present this best-dated range at 95% confidence level is c.1663–1599BC – but future work will of course modify) and perhaps most conveniently (given archaeological issues) in the late 17th century BC. Here the WSI from Thera has to be interpreted as early LCIA2 (even LCIA1/2 border) – and Bergoffen (2001a:155) even raises the idea of such early style WSI being introduced on Cyprus in LCIA1. LCIA2 is then a relatively long phase lasting until the late Hyksos period/New Kingdom transition, ending c.1550BC give or take.

As discussed above, either position requires much of the high chronology synthesis and Cypriot regionalism model. Choice between them depends on the weight given to the radiocarbon evidence. It should be noted that the quality and precision of the radiocarbon evidence has improved very significantly in the last couple of years, as will be evident from reading Bronk Ramsey *et al.* (2004a) and a further publication in preparation. But the situation remains flexible, and further data and/or analysis might yet increase the prospects of the compromise high/modified Aegean short chronology. This author for one is paying close attention to this possibility. Should radiocarbon data and analysis change to give a reasonable (or better) probability to the mid-16th century BC, then the compromise high/modified Aegean short chronology becomes the obvious best prospect and route to explaining all the data we have – and will I imagine be rapidly agreed to by nearly all in the field in such a case. But, at present, the radiocarbon evidence more clearly favours the high chronology – leaving us with the two choices above. What is not possible is the 'low' chronology; it must reject all the radiocarbon data with no good reason, and it must also avoid some key archaeological evidence, especially as relates to LCI Cyprus.[7]

[7] The good quality modern (standard pre-treatment and processing and correction) radiocarbon data, as they stand, clearly favour the high chronology, and permit at lower probability the compromise high/modified short Aegean chronology (see Section 4, see Figures 1 and 7). It is difficult to justify simply ignoring these data. Unlike the situation in the 1970s (compare and contrast conclusions then of Betancourt and Weinstein 1976; Hood 1978), the accuracy and precision of these data (and the quality controls for the laboratories producing them, see e.g. Bronk Ramsey *et al.* 2002:1–4) are good and robust (see also discussion of Manning and Bronk Ramsey 2003:124–129).
Wiener (2003; and especially this volume) thus tries to move the agenda to a suggestion that there might be some offset effect leading to older radiocarbon ages for the period around the Thera eruption (see discussion in Section 4). The strategy is to note all sorts of possible sources of some form of offset inducing circumstance – without in a single case demonstrating that any such effect actually applies to the Aegean radiocarbon dates at issue (and note: the range of

Second, review of the evidence indicates that there is no real science versus archaeology split. Both plausible positions require the same kinds of cultural synthesis and explanatory scenarios (e.g. regarding LCI Cyprus); the high chronology just requires a more stretched out version. The concentration of key evidence at Tell el-Dabᶜa into the Tuthmosid period in the latest assessments (WSI, BRI, RLWM, paintings, pumice), and downwards in time from original placements in the Late Hyksos period (in 1992), then early New Kingdom (1994–1995 onwards), requires significant distance from the Thera eruption – the question is just how much?

Overall, it is chastening to note with regard to East Mediterranean chronology both how little has changed in some regards over the last several decades (cf. Cadogan 1978), and at the same time how some other things have changed enormously (and sometimes then vanished). Today we await (as we have for 3+ decades!) the beginnings of a new relatively precise and robust radiocarbon framework as the best immediate hope outside of the artefactual evidence and stylistic comparisons (and of course traditional archaeology may yet produce fresh decisive evidence for several points which are currently ambiguous). And some day an ice-core layer that is soundly dated may also produce tephra closely comparable with Thera (and then preferably a second core, replicating this finding – noting the variability in ice-core records noted by Wiener 2003:376) – but not yet.

Finally, one might ask whether the present chronological debate is worthwhile? I would argue that the 'high' chronology challenge has significantly improved Bronze Age Mediterranean archaeological chronology by requiring careful scrutiny of previous loose assumptions, and by prompting much new research and data (even if often aimed at disproving the high chronology). It has also promoted the useful integration of science-dating methods into archaeological chronology. Already, it would seem that several scholars of the pre-existing conventional (or low) chronology are moving upwards to some extent to a 'modified Aegean short chronology' – just as the high chronology camp have noted for some years the plausible and basically identical 'compromise high chronology' position. Some form of paradigm shift is underway. Eventually, we will reach a consensus and an agreed best position. But even now we have much increased, and better, and more wide-ranging, data with which to disagree. This is good. And the plausible positions are narrowing. Even if finally disproved, the high chronology challenge will be able to claim some credit for the building of a new and better conventional position. This new, refined, chronology can then form the basis to a new generation of studies of the wider cultural relations and processes of the second millennium BC east Mediterranean.

Acknowledgements

I am very grateful as always for the generous hospitality of the SCIEM 2000 project in Austria, this time in Vienna. The SCIEM 2000 venture under Manfred Bietak has come to lead the research agenda in its field, and its meetings offer the major international forum of the current time. This is a wonderful achievement and I congratulate Manfred warmly. I

fluxes in the literature cited is huge – some are so small as to be irrelevant – some do not distinguish volcanic and biogenic sources – and there is little data on how consistent and/or dispersed a signal is and at what distances and heights). One must also stress the time and location variability of such effects *even in* the localised areas where they apply – thus in the Azores case (Pasquier-Cardin *et al.* 1999) totally uncontaminated samples were also found *within* the caldera. As noted in the main text in Section 4, the recent and good quality radiocarbon data from the LMIA and LMIB periods do not seem to exhibit the types of evidence consistent with such suggestions – and in fact the reverse.

The situation in simple terms is that, before and after the LMIA–LMIB periods, radiocarbon dating seems to offer data consonant with the historical-archaeological chronology (i.e. expectations). But, in the LMIA–IB period, it does not – it is higher. The archaeological dating evidence for the LMIA period is also very thin. So, do you regard the LMIA–LMIB radiocarbon dates as wrong, or do you wonder about the conventional interpretation of scarce hard archaeological correlation evidence?

If one does, nonetheless, wish to wonder about the radiocarbon data, it is important to note that the 'effect' sought for the 17th–16th centuries BC is in fact small – and not really compatible with most of the significant, but highly localised, or time-varying, effects noted by Wiener (this volume). And it must apply consistently over a wide area, and for some significant time period, but only for this time period (LMIA–LMIB or thereabouts), and not it seems over the entire eastern Mediterranean either. Attempts to suggest, and, better to investigate and establish, other hypotheses of possible very minor radiocarbon age offset factors will I am sure continue, and are worthwhile and useful (and some of these effects are very interesting and have great potential relevance for studies in various fields outside archaeology). However, there is no positive evidence at present that any such effects apply to the relevant Aegean radiocarbon data. A positive, but critical, approach appears the correct position with regard to the radiocarbon evidence as of 2004.

am pleased to offer this on-going discussion document concerning how we best seek both to synchronise the civilisations of the east Mediterranean and to include science-dating evidence. I greatly appreciate and respect Manfred's attempts to include all views and evidence types, and his engagement with those interpretations and data which pose problems. This text offers my current perspective on some of the key issues, and thus contrasts with Bietak (2003). Through the working out of problems and different interpretations progress will be made. Often neither side is completely right, nor completely wrong.

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APPENDIX: Some comments on 'Ancient Egyptian Chronology for Aegeanists' (Kitchen 2002)

Introduction

Kitchen (2002) criticises a portion (Appendix 1) of Manning (1999). A review/response highlights a number of issues usefully and complements the discussion in Section 10 in the main text..

Manning (1999) is a book about Aegean and east Mediterranean chronology and cultural relations in the mid-second millennium BC. In its main text (pp. 1–366), where pertinent, it uses (and clearly states this – see p. 66) the standard Egyptian chronologies of Kitchen and von Beckerath. All the main discussion and conclusions of the book as they relate to Egypt are so based. There are then two appendices to the book. Appendix 2 argues that various recent attempts radically to re-date (lower) Egyptian chronology are incorrect – i.e. it supports/defends the standard position. Rather than simply approve, Kitchen instead chooses to misrepresent Manning's position, as if to imply he supports radical chronological shortening (p. 7); whereas Manning has for a decade been a critic of such scholarship (e.g. Manning and Weninger 1992). Appendix 1 investigates just how secure is standard Egyptian chronology. It is this appendix that particularly vexes Kitchen.

Kitchen accuses Manning of 'hypercriticism' (p. 5), 'needless delusions' (p. 5), and 'unnecessary foul-mouthing of the proper state of Egyptian chronology' (p. 11). There is in fact only a small to non-existent chronological dispute between Manning and Kitchen. Kitchen (2002:11) concludes that Egyptian chronology is '…within a decade back to c.1480 BC, and within some 20 years back to 1550/1530 BC' (which oddly becomes '1550/1520 BC' on p.12). All Manning (1999:Appendix 1) sought to investigate was whether there might be a possible error range of 11 to 25 years at c.1550BC, and all I concluded was that 'the conventional [i.e. Kitchen/von Beckerath] chronology of second millennium BC Egypt is sound in general terms, but … is not known with total precision and accuracy'. This is hardly 'foul-mouthing'! Moreover, for the record, I am happy to note here (AD2004) – and superseding text written in the late 1990s – that based on current evidence and assessment I continue to regard standard (Kitchen, von Beckerath, Krauss, et al.) New Kingdom Egyptian chronology as closely dated within small errors given the evidence we currently have to hand. And, further to, and notwithstanding, a couple of possible questions (versus proposals) raised in Manning (1999) – discussed below – I regard a start date for the New Kingdom c.1550/1540BC and an accession of Tuthmosis III c.1479BC as the most likely current positions. I used these dates in the chronological synthesis in Manning 1999 (p.339 Fig.62) and in more recent papers (e.g. Manning et al. 2002a). There is no attempt radically to change Egyptian chronology.

Kitchen's paper does two things. First, it makes a number of supposed criticisms of Manning (1999), often misrepresenting what is actually written. Kitchen is in fact criticising various extreme views – none of which are Manning's and none of which Manning supports. I respond to these points below. Second, Kitchen's paper offers an up-dated summary (polemical) review from his perspective of Egyptian chronology. This is of course (as always) useful, and requires no comment here.

Responses

(1) Kitchen p. 5 begins by alleging that Manning wishes to raise the start of the New Kingdom to 1575BC. Nowhere in the 494 pages + xxxiii of Man-

ning (1999) does Manning say this. Manning merely wonders (p. 338) whether an 11 or 25 years adjustment to the current low chronology standard view is in any way possible (and refers readers to Appendix 1 for elaboration – where, as laid out on pp. 367–368, he 'seeks to review … (i) exactly how secure is the conventional chronology of the 18th Dynasty, and (ii) whether … a higher chronology is feasible?'). This is hardly odd when noted scholars, both Egyptologists and Aegeanists (Kitchen, Wiener, Ward – cited p. 338), have acknowledged some slight movement being 'conceivable'. Manning's text nonetheless uses Kitchen's/von Beckerath's standard dates for his (then) proposed chronological synthesis (pp. 335–340 and esp. fig. 62). From the start, Kitchen is determinedly ascribing false views to Manning. Kitchen then enthusiastically attacks such asserted claims.

(2) Kitchen pp. 5–6 is annoyed with Manning (1999:373) which states that, although 664BC is usually agreed as the earliest fixed date for Egypt, in fact 'if one were strict' the earliest 'truly fixed' date is 525BC. Kitchen's own text proves Manning was correct. Kitchen writes 'There has never been any dispute over the beginning of the 26th Dynasty, except as to whether it began (accession of Psamtek I) in 664 or 663 BC, this turning on whether Amasis II reigned 43 or 44 full years. As there is good reason to accept 44 years, not 43 … 664 BC should be retained'. A 'truly fixed' (i.e. absolute) date means there cannot be any doubt at all. Hence there was nothing wrong with Manning's (1999) statement p.373.

(3) Kitchen p. 7 re- Shoshenq I states that 'Over this man, Manning (1999:378) blunders horribly'. Kitchen tries to impute that Manning subscribes to the James et al. and Rohl (and others) revisionism whereby Ramesses II or III is referred to. Manning (1999:378) says no such thing – he merely refers to their criticism of Kitchen and points out that Kitchen had not fully responded to all their points in a documented way. Continuing, Kitchen refers to 'Manning (and the incompetents he chooses to cite)' (my italics). The scholars cited in the relevant section pp. 378–380 as expressing views which raise various uncertainties in the evidence, apart from James et al. and Rohl, are: Wente, Van Siclen, Cryer, Barnes, Hayes, Hooker, Tadmor and Cogan. Kitchen's definition of 'incompetents' is wide. (I ignore here the 'alternative' so-called New Chronology literature in places such as Journal of Ancient Chronology Forum or the book edited by Van der Veen and Zerbst 2002). Finally, Kitchen complains that Manning 'cavalierly' dismisses the work of Thiele. Manning did no such thing. Manning (p. 378) merely cites a scholarly critique of Thiele and the involved matters (Barnes), and points to the fact that several leading scholars have noted that 'an independent check on Biblical data for the reign lengths of Israelite kings is lacking' (Cryer, Tadmor, Cogan). I am happy to excise any reference to Shoshenq II.

(4) Kitchen p.8 complains that his date of 'either 1068, 1069 or 1070 BC for the death of Ramesses XI (and the New Kingdom) … must [be] treated more seriously than Manning does'. Not surprisingly, Kitchen fails to cite where Manning commits this sin. Why? Manning does not actually discuss the 21st Dynasty (and, as most readers will have noted: Manning 1999 is not about the Third Intermediate Period – contra the apparent tone of Kitchen 2002 – and issues regarding it occur at best on only a half dozen pages of 527!). But even so, Manning's basic point all through his 1999 Appendix 1 was that these sorts of 'Kitchen' dates are simply not truly absolute (as in fixed and with any associated uncertainty precisely quantified), and any stated error ranges are guesstimates and not solidly quantified. Kitchen himself confirms this point repeatedly in previous publications. Writing of 1069/1070BC, Kitchen (1996a:3) admits this date is really to be seen as 'within some very narrow limits (not exceeding about 5 years)' (my italics).

Manning's (1999:376–377) point in his discussion of Egyptian chronology from 525–1279BC is that Kitchen (in publications of 1987 and 1996a) has to admit that he cannot find enough attested years in records to produce an acceptable date for Ramesses II simply from dead-reckoning. He had to make up 11–17 or 9–15 years between 664BC and 1279BC (accession of Ramesses II) (see also Kitchen 2000:42, where Kitchen continues to note that 'clearly, the minimum and even "probable" dead-reckoning dates for Ramesses II are too low' – from his Table 4 by some 13 or 17 years). I note that Kitchen (2002:9) now argues this discrepancy is reduced to 0/1 or 4/5 or 8/9 years, but at the same time Kitchen admits that 'one might smuggle in 11 [additional] years piecemeal'. And just one paragraph later, Kitchen says there might be a 14 year variation by the start of the reign of Ramesses I. Kitchen then writes: 'So, by accepting the higher date, Dr. Manning would gain more than his 11-year minimum gain!'. Fine. This was exactly the point Manning (1999:Appendix 1) was making. I sought to highlight that there was some small flexibility in the dates.

(5) Kitchen (2002), although very critical at

length about things mentioned in brief, or tangentially, or not at all in Manning (1999), is then notably silent about the reign of Ramesses II and an instance of an actual discussion by Manning (1999:380–388). Here Manning investigated the Egyptian links with Babylonia, Assyria and Egypt and argued that this evidence could be compatible with a 1290BC or even perhaps a 1304BC accession date. My point then was that 1279BC had not been established beyond doubt (versus a probable argument). Here is an instance for Kitchen to prove his case. He chooses to avoid the topic. He instead only (p. 10) reviews the Amarna period Near Eastern linkages. But Manning (1999:390) had already concluded that 'whereas the evidence surrounding Ramesses II perhaps favours an earlier Egyptian chronology, the Amarna evidence is more consistent with the middle or lower Egyptian chronology (although there is greater uncertainty over the Babylonian kinglist and chronology at this time…)' (my italics). Kitchen thus discusses the example where both he and Manning reach the same position.

If we return to Ramesses II, we may observe that previously Kitchen (2000:42–43) noted the lunar dates from Ramesses year 52 as the key evidence. Analysis of these could lead to an accession date in 1279BC or 1290BC (or in earlier literature 1304BC). Kitchen's dead-reckoning had got him to 1262BC or 1266BC (2000:Table 4). Thus he happily accepts the need for (then) 13/17 extra (non-attested) years to make the lowest of these lunar options, and sets about distributing some of these about. In turn, Kitchen ignores the obvious logical point that if one has incomplete minimum knowledge with no constraints (an extra 13/17 non-attested years are necessary even for Kitchen), then one cannot constrain what one does not know without other linked information. But notwithstanding, Kitchen goes on to assert 'there is no warrant whatever to add over a decade back to 1290 BC', and criticises the 'scepticism of Manning 1995, 16, n.5'. He writes 'so, 1279 B.C. alone will fit all the data'. Now I wish to state that 1279 BC is probably correct as a best fit, and that the Amarna period evidence would support this view against 1290BC and especially 1304 BC. But Kitchen's published arguments/logic re-Ramesses II fail to prove this point. Instead, Kitchen (1987:39) more correctly described the situation when he said that the data and calculation of extra years beyond those attested 'do not suit 1290 BC so well …[and it] is rather less realistic … [or it is] unlikely but just possible'. Nothing substantive has changed since then.

(6) Kitchen (2002:9) finds a total of 184 years between the accession of Sethos I and the accession of Tuthmosis III. He dates Sethos year 1 at 1295 (max) to 1281 (min). Hence we can have Tuthmosis III acceding in 1479 BC – the current standard date (NB. The '*1475 BC' of Kitchen 2002:9 should be *1465BC from 184 +1281). Adding the earlier 18th Dynasty rulers we might reach 1550 BC (Kitchen 2002:9) or at a minimum 1540/1531BC. Fine. For the record, I am perfectly happy with c.1550/1540BC! I used this approximate date in Manning (1999:1–366) and in work since (e.g. Manning *et al.* 2002a). Similarly, I accept on evidence to hand that 1479BC represents the likely date for the accession of Tuthmosis III as argued by von Beckerath (1992; 1994; 1997) and Kitchen (1996a:6) and Krauss (2003:195) (and e.g. Warburton 2000:56, 58; etc.). Again, this is the date I used in my 1999 chronological synthesis, and have used in work since then.

The point of Manning (1999:Appendix 1) was to argue that, although this is the now standard view, there are some flexibilities in the evidence assembled and interpreted to yield these dates. Simply reading the work of Kitchen – even just his most recent 2002 publication – nicely demonstrates this. Manning sought to highlight that any dead-reckoned chronology based on what is undeniably only partial evidence must – at best – only establish a minimum chronology. Of course, this may be the correct chronology. But this will only be known when proven by either comprehensive evidence, or other independent means – e.g. science-dating such as dendrochronology. Already science-based dating has recently narrowly defined the range of second millennium BC Syro-Mesopotamian chronology (Manning *et al.* 2001a), and hopefully progress will be made with regard to Egypt in the not too distant future. Indeed, Kitchen may wish to note that the latest work combining radiocarbon dating and dendrochronology supports a middle to low Old Babylonian chronology (Manning *et al.* 2001a, revising Kuniholm *et al.* 1996) and that, therefore, Kitchen might consider returning again (cf. Kitchen 2000:46) to the potential linkage between Neferhotep I of Egypt, Yantin of Byblos and Zimri-Lim of Mari and a Yantin-Ammu of Byblos (Kitchen 1967).

(7) Kitchen pp. 10–11 rejects criticism of his (and Bierbrier's) generation counts as decisive evidence. Manning (1999:377, 387–388) had cited the study of Henige. Here Kitchen believes in a rigidity of evidence that is not possible from such limited demographic data (one only needs to read Kitchen 1996b and Bierbrier 1975 to see various flexibilities and best reconstructions and interpretations in their

analyses of the genealogies of various families of dignitaries – and even some disagreements). Kitchen also ignores the basic rule that limited examples do not discount the possibilities of some exceptions in the total population of data. I stand by the paragraph in Manning (1999:377). Indeed, Kitchen's (2002:10–11) own examples more or less demonstrate the point. These selected examples offer average generations ranging from 23/24 to 27/28 years – over let us say even 6 generations this implies a sensible error range of ±15 years on the average – hardly insignificant. And of course the time-line against which the calculations are made is Kitchen's, and this itself has some small flexibilities. Compound errors should be calculated. But even from what Kitchen states, we see quite a bit of range. The real overall population will, by definition, exhibit at least a slightly larger range. And Kitchen still has no defence against the possibility (even likelihood over long time periods) of a few exceptional individuals who could throw his averages. Bryan (1991:23) sums things up nicely: 'the average lifespan for ancient Egypt is unknown, and individual variations are great in all populations'. Thus there is some flexibility. As Henige (1981:184) wrote: 'the counting of generations in undated or partly dated genealogies, especially over a long period of time, cannot help in establishing exact dates'. Kitchen might argue his evidence can be compatible with such and such a view, but it in no way proves it, nor requires it. The genealogical data in no way rule out a 1290BC or even 1304BC date for the accession of Ramesses II (also Henige 1981:182). And note: I am not saying I believe in a 1290BC or 1304BC date (see above) – I am merely saying that the genealogical data by themselves do not rule out a very slightly longer chronology. (An interesting and slightly circular case of logic should be noted at this point. A key reason that Krauss proposed Elephantine as the observation point for the risings of Sothis (Sirius) was the chronological work of Bierbrier (Krauss 2003:184). But if this evidence were to be considered more critically within realistic errors, perhaps the case that needed to use the Elephantine observation location is slightly less strong.)

(8) Kitchen p. 11 reviews Middle Kingdom and Second Intermediate chronology. He offers dates consistent with Manning's review (1999:402–411). The sole issue Kitchen raises is that Manning 'overlooked' the evidence of the time-span provided by the number of rulers at Thebes regardless of who was 16th or 17th Dynasty (cf. Kitchen 2000:44–46). OK. Kitchen himself nicely avoids discussion of the problems in his previous work introduced by the book of Ryholt (1997 – cf. Allen *et al.* 1999). Manning (1999:409) estimated from the Middle Kingdom and Second Intermediate Period evidence a likely range of 1564–1541BC for the accession of Ahmose – i.e. more or less the standard chronology. There is little to object to in the end result.

(9) Kitchen p. 11 reaches the Thera eruption. He seems to regard dates from 1528BC to 1180BC as feasible. As noted above, any date after c.1530/1520BC must entirely ignore a considerable body of radiocarbon evidence (Kitchen of course has no interest in such matters). Kitchen correctly states – if the Aegean 'high' chronology is correct – that the eruption had nothing to do with the 18th Dynasty and was instead in the mid Second Intermediate Period. Yes: see text above, Manning et al. (2002a), etc. This is the high chronology synthesis.

Kitchen then accuses Manning of creating 'unnecessary fuss to try to insert 25 more years into the 18th Dynasty', contra the text of Manning (1999), which uses the standard Kitchen/von Beckerath chronology. Only in Appendix 1 did I investigate whether there was any flexibility to the standard chronology and whether dates 11 or 25 years earlier were conceivable. I argued (as above) that there clearly is a little potential flexibility. I did not claim that the higher dates were a fact, nor recommend them, nor use such dates in my text and analysis. And I do not now – see statements above.

Kitchen (p. 11) then states that 'Science cannot solve the intricate problems of detailed Egyptian successions, and the cross-links with the neighbouring Near East'. Yes, but only to a point. Dendrochronology is starting to do exactly the opposite (e.g. see Manning *et al.* 2001a). And Kitchen contradicts himself when on p. 12 he too admits that dendrochronology may prove useful. Sophisticated high-quality radiocarbon sequence analyses from Egypt may also in the future prove useful.

Finally, Kitchen admonishes Aegeanists for thinking pots can give absolute dates and reminds them that 'time-spans of pottery-style use are matters of (gu)es(s)timate'. If this is meant to be criticism of Manning, then Kitchen did not read the main text of Manning (1999); this is after all more or less the point of many pages in the book! (and several previous studies by several scholars, including the present one over the last 15 years – e.g. Manning 1988).

Conclusions

The basis of the Aegean 'high' chronology case is entirely independent of Egyptian chronology. And

it need not have any direct impact on Egyptian chronology (compare also WARBURTON's 2000 reasoned assessment). MANNING (1999) employed standard Egyptian chronology. However, as part of a critical review of all evidence, Manning also reviewed just how secure the standard Egyptian chronology was (as of 1998–1999 evidence). He also noted, if conventional linkages between the Late Minoan IB period and Tuthmosis III are maintained, that a slightly higher chronology might be more convenient (pp. 338, 412). He asked whether this was even possible. But MANNING also stated (p. 412) that 'the middle (or even low) Egyptian chronologies can easily be considered compatible' (and this position was the one used in the figure 62 synthesis on p. 339). He also made clear that this 'issue' only arose if the conventional art-historical views were maintained (p. 412). In fact, as noted above in Sections 4 and 5, there seem to be reasons from both radiocarbon and archaeology to in fact revise and somewhat raise the conventional linkages – hence there is perhaps no problem at all and no conflict between the Aegean and Egyptian dates at this point. MANNING (1999:412) concluded by writing that 'Nor, as I seek to stress, is there any actual conflict between the … early Aegean LBA chronology, and the conventional (middle or low) Egyptian chronology'. Contra KITCHEN (p. 5), I do not 'wish to adjust Egyptian chronology'; I merely investigated its accuracy and precision.

Between when MANNING (1999) was written and now, some 5 years, a number of things have of course changed (and significantly): new evidence, new analyses, and so on. Issues with regard to science-based evidence have been briefly reviewed in the main text above. Some of the new evidence strongly enhances the case for the 'high' Aegean chronology, but other discussions (mainly archaeologically-derived) create new problems or complications. Some new scientific evidence from Egypt from Tell el-Dab'a itself raises potential issues about the chronology of that site especially in the SIP (KUTSCHERA *et al.* 2004), and further work is anticipated to investigate this situation (Bietak pers. comm. Jan. 2004). Egyptian chronology is separate so far to current debates. The chronology for Egypt painstakingly built up, and as represented by the corpus of work by scholars such as Kitchen and von Beckerath, is more or less correct. The (inadequate) radiocarbon evidence available from Egypt offers, broadly, support, or at least compatible evidence (MANNING n.d.). There is, nonetheless, some small element of flexibility in even the best existing analyses, because we do not have comprehensive and/or fully replicated data, nor evidence which is entirely contradiction-free.

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Reinterpretation of Greenland Ice-Core Data Recognises the Presence of the Late Holocene Aniakchak Tephra (Alaska), not the Minoan Tephra (Santorini), at 1645 BC

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Abstract

The precise age of the Minoan eruption of Santorini (Thera) volcano, which erupted some time during the Second Millennium BC, is in debate. A range of dates between 1600–1650 BC has been proposed and the relative merits of these estimates have been widely argued. Recently, chemical analyses of small glass shards recovered from the GRIP ice-core from Greenland have been used to confirm a 1645±4 BC date for the Minoan eruption of Santorini (Hammer *et al.*, 2003), but marked differences in the concentrations of Si, Ti, Mg, Ba, Sr, Nb, REE (particularly La/Sm) deny this correlation. Instead, glass shards from the GRIP ice-core have a composition which is indistinguishable from glass shards from the late Holocene Aniakchak tephra, Alaska, which is proposed as the source for the material in the GRIP ice-core. This provides a precise date of 1645 BC for the caldera-forming eruption of Aniakchak, but leaves the issue of the precise age of the Minoan eruption of Santorini unresolved.

Introduction

The date of the Minoan eruption of the Santorini (Thera) volcano which erupted some time during the Second Millennium BC is of major importance in placing time constraints on the evolution of civilisations in the Eastern Mediterranean. In attempts to derive precise dates for the Minoan eruption, several proxy records have been linked to this event, including accurate dates derived from studies of acid spikes in ice-cores or anomalous growth recorded in tree rings (Hammer *et al.*, 1987; Kuniholm *et al.*, 1996; Manning *et al.*, 2002; Manning *et al.*, 2001). These proxy records often correlate over large distances (Manning and Sewell, 2002), and must reflect major and widespread environmental impacts. These proxies, however, do not identify unequivocally the cause of a particular environmental disturbance. The combination of various proxy records with ^{14}C dating, has seen a range of dates between 1600–1650 BC proposed for the Minoan eruption of Santorini, and the relative errors/merits of these ages have been widely argued (Hammer *et al.*, 1987; Manning, 1998; Manning and Sewell, 2002; Zelinski and Germani, 1998). While the acid spikes in the ice cores record an atmospheric response to a volcanic eruption, they do not record which eruption. Similarly, the tree ring records are equally uncertain. Thus, to be certain of linking a particular eruption with the ice-core record requires the presence within the ice of juvenile volcanic materials which can be chemically correlated with a particular source (e.g. Zelinski and Germani, 1998). Recently, analyses have been published for a suite of minute (~ 5μm) glass shards recovered from the GRIP ice-core from Greenland (Hammer *et al.*, 2003). These analyses are claimed to confirm a 1645 ± 4 BC age for the Minoan eruption of Santorini. However there are serious problems with the correlations suggested by Hammer *et al.* (2003). Indeed, their interpretations have already been challenged on statistical grounds (Keenan, 2003). Here, the analytical data of Hammer *et al.* (2003) are evaluated and compared to data from other large eruptions from the early-mid Second Millennium BC, including new analyses of tephra from the Aniakchak volcano on the Alaska Peninsula, which is proposed as the source for the glass shards from this layer of the GRIP ice-core.

Volcanic glass shards from the GRIP ice-core

Hammer *et al.* (2003) recovered hundreds of microscopic particles from the 1645 BC acid spike layer of the GRIP ice-core (sample A1340-7). Analyses of these particles revealed 174 to be silica-rich volcanic glass. Major element analyses were performed by

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	5Rj125 B	5Rj126 B	5Rj126 C	Hayes A	Hayes B	Hayes C	Hayes D	Hayes E1	Hayes E2	Hayes F	Hayes G	MSH Yb	MSH Yn	MSH Ye
SiO_2	63.44	62.82	59.19	75.45	76.61	74.49	72.98	74.52	71.74	76.48	74.83	73.77	74.81	75.42
TiO_2	1.23	1.21	1.36	0.24	0.22	0.24	0.27	0.25	0.24	0.21	0.25	0.26	0.17	0.16
Al_2O_3	15.50	16.51	16.52	13.80	13.44	14.65	14.89	14.41	16.05	13.68	14.53	14.78	14.38	13.93
FeOt	6.69	6.23	8.04	1.72	1.23	1.63	1.99	1.83	1.85	1.13	1.69	1.51	1.36	1.28
MnO				0.06	0.05	0.04	0.05	0.06	0.06	0.04	0.04			
MgO	1.86	1.92	2.87	0.53	0.28	0.49	0.70	0.53	0.67	0.22	0.50	0.62	0.55	0.53
CaO	4.29	4.84	6.32	2.29	1.93	2.22	2.86	2.23	3.22	1.79	2.24	2.41	1.89	1.79
Na_2O	4.87	4.67	4.11	3.46	3.39	3.64	3.81	3.44	3.84	3.62	3.68	4.53	4.63	4.65
K_2O	2.13	1.79	1.59	2.44	2.84	2.59	2.46	2.74	2.34	2.82	2.23	1.99	2.11	2.12
Cl												0.13	0.10	0.12
F														
O=F,Cl														
Cr_2O_3														
Total	100	100	100	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
H_2Od	3.6	1.9	1.9	3.65	3.28	4.42	3.94	7.00	4.65	2.06	7.79	2.20	1.88	0.96

	A1340-7 (n=174)	Bo-1 (n=38)	Relative difference A1340-7/ Bo-1	Minoan tephra Gölhisar Gölü (n=67)	Minoan Bo, Basal surge, Vitaliano et al 1990	Aniakchak UT2011 (n=14)	A1340-7 calibrated and normalised	Aniakchak normalised	Relative difference A1340-7/ Aniakchak
SiO_2	70.34 (1.82)	73.88 (1.62)	–5%	73.62 (0.59)	73.9	71.23 (0.19)	70.53	71.37	–1%
TiO_2	0.89 (0.60)	0.60 (0.52)	49%	0.29 (0.06)	0.2	0.47 (0.08)	0.44	0.47	–7%
Al_2O_3	14.65 (0.98)	13.83 (0.84)	6%	14.02 (0.18)	13.9	15.21 (0.11)	14.95	15.24	–2%
FeOt	3.34 (1.11)	2.22 (0.88)	50%	2.04 (0.12)	2.2	2.45 (0.08)	3.08	2.46	25%
MnO	0.36 (0.40)	0.18 (0.29)	100%	0.07 (0.04)		0.13 (0.05)	0.13	0.13	3%
MgO	0.63 (0.48)	0.34 (0.33)	83%	0.28 (0.04)	0.29	0.52 (0.02)	0.51	0.52	–2%
CaO	2.12 (0.60)	1.82 (0.55)	17%	1.40 (0.10)	1.4	1.74 (0.09)	1.65	1.75	–6%
Na_2O	3.74 (0.88)	3.23 (1.21)	16%	4.76 (0.64)	4.7	5.13 (0.13)	5.54	5.14	8%
K_2O	3.64 (0.54)	3.73 (0.96)	–3%	3.24 (0.12)	3.4	2.93 (0.10)	3.17	2.94	8%
Cl				0.31 (0.04)		0.20 (0.03)			
F				0.07 (0.09)					
O=F,Cl				–0.09 (0.04)					
Cr_2O_3	0.29 (0.46)	0.17 (0.24)							
Total	100.00	100.00		100.00	100	100.00	100.00	100.00	
H_2Od	1.05	0.92		2.36 (1.53)		0.89			

Table 1 Major element data for tephra deposits considered in this study: Units A–G from Hayes volcano (Riehle, 1994); Mount St Helens (MSH) Y tephra set (Westgate, 1977); Bo-1 glass from Santorini (Hammer *et al.*, 2003); Minoan tephra from Gölhisar Gölü, SW Turkey (Eastwood *et al.*, 1998; Eastwood *et al.*, 1999; Pearce *et al.*, 2002); A1340-7 from the GRIP ice-core (Hammer *et al.*, 2003); Veniaminof (samples "5Rj…") (Riehle *et al.*, 1999); and Aniakchak (UT2011, this study). All analyses have been recalculated to 100% excluding H_2Od. H_2Od is water by difference. The A1340-7 analyses have also been recalibrated using the reported Bo-1 analyses (Hammer *et al.*, 2003) and the analyses for the Minoan tephra (Eastwood *et al.*, 1999) for those elements common to both data sets, and the UT2011 analyses have then been recalculated to 100% for the same elements for comparison. Numbers in parentheses are 1 standard deviation. Concentrations in wt%

analytical scanning electron microscope (ASEM), for which no details are given, although Hammer *et al.* (2003) state that the "small sizes and irregularity of the particles prevents ideal calibration". ASEM analyses are less precise than conventional electron probe microanalyses, although, when calibrated correctly have the potential to be accurate (www.cameca.fr). Hammer *et al.* (2003) also determined the trace element composition of 8 larger (about 10 μm) shards from the ice-core sample by secondary ion mass spectrometry (SIMS) using a Cameca ion microprobe. For comparison, Hammer *et al.* (2003) determined the major element composition of 38 grains of glass from a comminuted sample of the Upper Pumice layer of the Minoan eruption (Bo-1) collected from Santorini, but only 3 grains of this material were analysed for their trace elements. These data are presented in Table 1.

Silica-rich (rhyolitic) magmas have high viscosities which, when combined with high dissolved

Source volcano	^{14}C age	Calibrated ^{14}C age (2σ) or suggested age	Age Sources	Comments and sources of compositional data
Veniamin of Alaska Peninsula	3700±100 BP	2460–1780 BC	(Miller & Smith, 1987)	Eruption >50 km^3 magma (Riehle et al., 1999)
Aniakchak, Alaska Peninsula	3430±100 BP 3380±80 BP	2012–1515 BC 1889–1449 BC	(Miller & Smith, 1987) (Vogel *et al.*, 1990)	Eruption >50 km^3 magma, tephra NE of source (Begét *et al.*, 1992)
Hayes volcano, Alaska		1850–1550 BC	(Riehle *et al.*, 1990)	7 or 8 tephra beds, totalling ~10 km^3 magma, tephra NE of source (Riehle, 1994; Riehle *et al.*, 1990)
Mount St Helens Yb, Yn and Ye tephras	3410±80 BP	1917–1518 BC 1560 BC	(Luckman *et al.*, 1986) (Pringle, 1993)	Eruptions total ~4 km^3 of magma. Yn tephra NE of source, deposits in NW Canada (Westgate, 1977)

Table 2 Sources, ages and comments on large North American volcanic eruptions from the early–mid Second Millennium BC. Calibrated ^{14}C ages calculated after Stuiver and Reimer (1993) and Stuiver *et al.* (1998)

volatiles in the magma, give rise to violent eruptions. From these eruptions widespread tephra deposits may be produced which commonly have similar major element compositions (Westgate *et al.*, 1994). Broad or general similarities in the composition of glass from separate deposits are thus insufficient to erect robust correlations: by virtue of their origins, rhyolitic tephras are often broadly similar in composition. Instead, for correlation purposes in tephrochronological studies, the major element chemistry must be essentially identical between widely separated deposits, and as such, acceptable variations are ~1–2% relative for SiO_2 and Al_2O_3, (i.e. SiO_2 within around ±1 wt%, Al_2O_3 within about ±0.25 wt%) and in the 5–10% relative range for the remaining, less abundant elements (Pearce *et al.*, 2004). Variations of this scale can readily be produced in large single eruptions that drain voluminous, chemically differentiated magma chambers.

Table 1 shows that large differences exist in major element chemistry between the ice-core glass (A1340-7) and the Bo-1 Minoan (Hammer *et al.*, 2003). Differences of 3.6 wt% SiO_2 (~5% relative) and 0.8wt% Al_2O_3 (~6% relative) are reported between these two deposits, and these are unacceptably large to propose a correlation between these samples. Additionally, Hammer *et al.* (2003) report FeOt (total Fe represented as FeO) and TiO_2 as being 50% higher in A1340-7 than Bo-1, MgO is almost twice as high in A1340-7 than Bo-1. K_2O and CaO are similar, and Na_2O is marginally higher in A1340-7, although because of the small sample size and irregular shape, Na is unlikely to have been determined accurately (Hunt and Hill, 2001). Considering the analyses presented by Hammer *et al.* (2003), it is clear that the relative differences are too great to allow a correlation to be proposed between the GRIP ice-core glass and the Minoan Bo-1 deposit. Recently, Keenan (2003) employed t-tests on the standard errors of the analyses of Hammer *et al.* (2003) (standard error = standard deviation / number of analyses) to show that the Minoan Bo-1 sample and the A1340-7 glass cannot be the same. This approach, where the numbers of analyses are large (i.e. n=174 for the ASEM analyses of the ice-core glass) may however reduce the errors to unattainably small values, far less that the true analytical reproducibility attainable by multiple analyses of homogeneous materials (Pearce *et al.*, 1997). In doing so, when comparing different materials, the standard error approach may enhance the apparent differences between samples. Nonetheless, the conclusions of Keenan (2003) agree with those here based on acceptable variations in composition between comparable tephra beds, *i.e.* the glass from the GRIP ice-core is not the same as the glass from the Minoan eruption of Santorini.

Correlation of the GRIP ice-core glass

Many major element analyses of glass from the Minoan eruption (Eastwood *et al.*, 1998; Eastwood *et al.*, 1999; Vinci, 1984; Vitaliano *et al.*, 1990; Warren and Puchelt, 1990) compare favourably with the Bo-1 major element analyses of Hammer *et al.* (2003), i.e. similar SiO_2, Al_2O_3, FeOt, MgO, K_2O and

CaO (see Table 1). However, the analyses in HAMMER *et al.* (2003) are generally higher in the minor components TiO_2 and MnO (see Table 1). These differences probably result from calibration problems in ASEM analyses where irregular shard geometry may give rise to problems in the corrections for mass absorption, fluorescence etc. These errors will also be incorporated into the analyses of the GRIP ice-core glass. Thus, to enable a more accurate comparison of the ice-core glass with other possible sources for this material, the ASEM analyses need to be recalibrated. In analysing 38 shards of Bo-1 glass, HAMMER *et al.* (2003) analysed a compositionally well know material (in essence a "standard") and this can be used to recalibrate their analyses. A correction factor can be calculated for the ASEM analyses, taking the average of the ASEM analyses of the Bo-1 glass and comparing this with published analyses of the Minoan tephra (EASTWOOD *et al.*, 1999; VINCI, 1984; VITALIANO *et al.*, 1990). Here, this correction has been made using the average of 59 glass shards by EPMA from EASTWOOD *et al.* (1999) to give a recalibrated analysis of the glass in A1340-7. This has then been further normalised to 100% for those elements common to both data sets (i.e. excluding F, Cl and Cr_2O_3, see Table 1). Normalisation to different published analyses makes only a minute difference to the corrected A1340-7 data. This recalibrated analysis can now be compared with known volcanic sources active at the time of deposition of the ice-core glass.

Several large volcanic eruptions occurred in North America in the mid-Second Millennium BC, their ages making them candidates for correlation with the glass from the GRIP ice-core. Details of these are summarised in Table 2, and include caldera-forming eruptions from Veniaminof and Aniakchak volcanoes, both on the Alaska Peninsula and smaller eruptions of Hayes volcano Alaska and Mount St Helens, Washington. Major element analyses of tephras from these eruptions are listed for comparison in Table 1. All the Hayes volcano and Mount St Helens tephra beds of the appropriate age are too siliceous, and the analyses from Veniaminof are all too silica-poor, to correlate with A1340-7. Differences in FeOt, alkalis and Al_2O_3 likewise exclude the possibility of correlation. It is clear that the Aniakchak tephra (UT2011) and the recalibrated ice-core glass (A1340-7) are extremely similar, showing only minor differences for all elements except FeOt. FeOt has the worst precision of the major elements determined by ASEM, with a relative standard deviation of around ±40% in Bo-1 (HAMMER *et al.*, 2003). This may in part be responsible for the larger difference in FeOt between A1340-7 and UT2011. The major element chemistry thus strongly suggests that the glass in the ice-core is derived from Aniakchak.

Comparisons of trace element analyses

HAMMER *et al.* (2003) produced ion microprobe (SIMS) trace element analyses of 8 glass shards from the A1340-7 ice-core sample and compared these with 3 analyses of glass from Bo-1. In samples that are only a few microns in diameter this is a remarkable achievement. These analyses are listed in Table 3. PEARCE *et al.* (2002) have recently defined the extent of trace element variation within the glass from the Minoan tephra using laser ablation LA-ICP-MS analyses, showing a remarkable compositional range between individual glass shards (see Table 3). This reflects small-scale heterogeneity within the magma caused by the combined effects of fractional crystallisation and diffusion giving rise to compositional gradients around crystallising phases (PEARCE *et al.*, 2002). The average of the singe-shard analyses compare extremely well with published bulk analyses, and provide compositional fields as defined on bivariate plots. Unfortunately, with their known range of compositions, the analysis of only 3 grains of Minoan glass by HAMMER *et al.* (2003) is statistically insufficient to gain an impression of the accuracy of the SIMS analyses. Despite this observation, the analyses of many trace elements in the Bo-1 glass (HAMMER *et al.*, 2003) comparable favourably with other published analyses of Minoan material for many elements (EASTWOOD *et al.*, 1999; PEARCE *et al.*, 2002; VINCI, 1984; VITALIANO *et al.*, 1990; WARREN and PUCHELT, 1990), although some elements fall above the range of single shard compositions, e.g. Nb, Ba which may reflect calibration problems (see Table 3). Additionally, the SIMS Rb data fall at the lower end of the range of single shard compositions, and Sr is at the upper end of the range, indicating that the SIMS analyses may not be truly representative. There are considerable differences between the trace element analyses of Bo-1 and A1340-7 given by HAMMER *et al.* (2003), with factors of up to 2.6 for the difference in Sr, ~1.5 for Ba, Sm, Nb and Rb and ~1.2 for La, Ce and Nd. The La/Sm ratio (defining the slope of the LREE) also differs by a factor of ~2 between the two samples. Differences in trace element concentrations of this magnitude cannot be generated by any realistic magmatic process within a single eruption, and in single grain analyses are not a result of contamination of the sample by detrital material. The differences unequivocally rule out a correlation. At the reported concentrations (a few tens to a few hundred

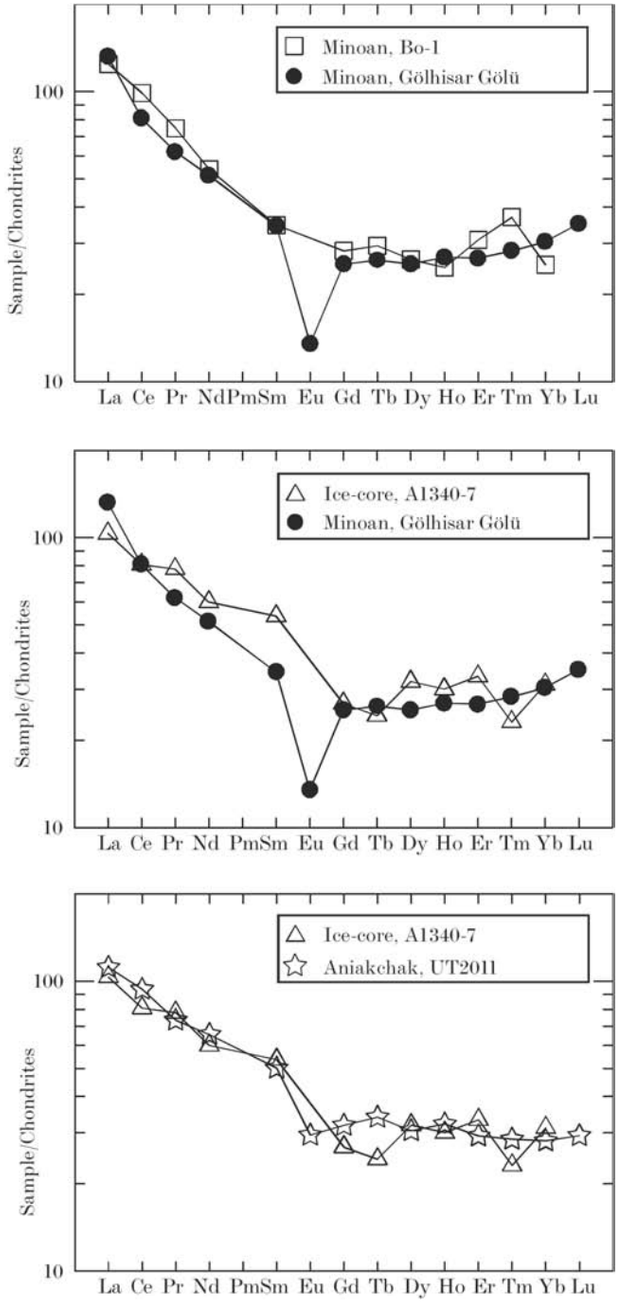


Fig. 1 Chondrite normalised REE spidergrams for glass from A1340-7 from the GRIP ice-core (Hammer *et al.*, 2003), the Minoan Bo-1 deposit on Santorini (Hammer *et al.*, 2003), the Minoan tephra deposited at Gölhisar Gölü, Turkey (Pearce *et al.*, 2002) and tephra from the caldera-forming eruption of Aniakchak (UT2011, this study). Normalisation factors from Sun and McDonough (1989)

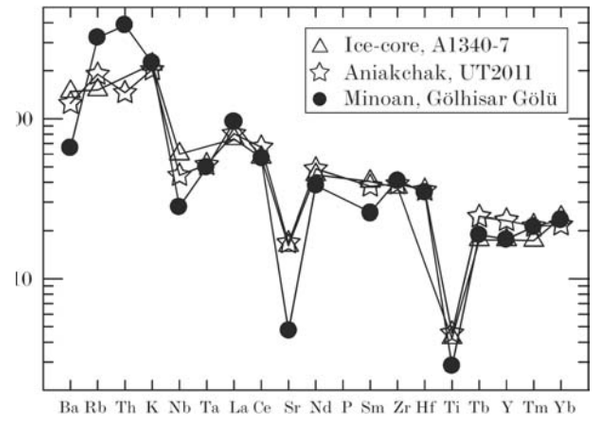


Fig. 2 Chondrite normalised incompatible element spidergram for glass from the GRIP ice-core (A1340-7)(Hammer *et al.*, 2003), the Minoan tephra deposited at Gölhisar Gölü, Turkey (Pearce *et al.*, 2002) and Aniakchak (UT2011, this study). Normalisation factors from Thompson (1982).

ppm), elements such as Ba, Sr, Rb and the LREE should be determined with good accuracy and precision by SIMS (perhaps ±10%) even in such small and difficult materials, but the differences between the Bo-1 and A1340-7 analyses are also well beyond any realistic estimates of analytical error, and again are far too great for these deposits to be considered the same. The differences between A1340-7 and the Bo-1 Minoan ash are further highlighted when compared with the single shard LA-ICP-MS analyses from Pearce *et al.* (2002) (Table 3). The most apparent differences are in the concentrations of Ba, Sr, Rb and the LREE. It is clear that the SIMS and LA-ICP-MS REE concentrations for the Minoan deposit are similar (Fig. 1), but both data sets differ markedly from the ice-core glass, the latter having a much lower La/Sm ratio (see above). Unfortunately, Hammer *et al.* (2003) did not report Eu, which may have helped in comparisons of the two samples. Despite the large difference in these data sets, Hammer *et al.* (2003) believed the Bo-1 and A1340-7 analyses show "a remarkable resemblance" and that it is "hard to believe that this resemblance should be coincidental". Unfortunately, any broad similarity which may exist for some elements in the SIMS analyses (but notably this is only *some* elements) is indeed coincidental. This is illustrated by Keenan (2003) who shows that analyses of the 75ka Toba eruption (Westgate *et al.*, 1998) compare well for many elements with both the Minoan and A1340-7 samples (equally as well as the A1340-7 data are claimed by Hammer *et al.* (2003) to compare with the Bo-1 data), where clearly these cannot be correlatives. For a robust correlation *all* elements (not just some) must compare extremely well, with the only permissible differences, outside analytical variation, being relatively small-scale variations which are caused and explained by magmatic process-

	Aniakchak, UT2011, solution ICP-MS (n=4)	A1340-7 (n=8), SIMS	Bo-1 (n=3), SIMS	Minoan tephra, Gölhisar Gölü (n=56), LA-ICP-MS	Minoan tephra, Gölhisar Gölü, range LA-ICP-MS (min–max)	Minoan tephra, Gölhisar Gölü, solution ICP-MS	Knossos, solution ICP-MS	QLO-1 accepted conc.	QLO-1 Solution ICP-MS (this study) Jun. 2003	QLO-1 Solution ICP-MS (this study) Oct. 2003	RGM-1 accepted conc.	RGM-1 Solution ICP-MS (this study) Jun. 2003	RGM-1 Solution ICP-MS (this study) Oct. 2003
Sc	10.8 (0.65)			13.9 (6.8)	2.57–34.3	6.76		**8.9**	8.43	8.75	**4.4**	4.71	4.62
V	12.2 (0.20)	12 (2)	13 (2)			39.4		**54**	43.3	49.0	**13**	14.4	14.2
Cr	2.02 (0.16)	9.6 (1.7)	1.7 (0.5)			6.20		3.2	3.40	3.43	3.7	3.50	3.46
Rb	66.5 (2.29)	53 (9)	77 (12)	113 (21.9)	56.4–173	91.4	94.8	**74**	73.3	71.1	**149**	150	154
Sr	199 (3.62)	190 (29)	72 (11)	55.5 (9.17)	37.1–76	77.4	103	**336**	340	316	**108**	112	110
Y	46.3 (2.54)	35 (5)	32.5 (5)	35.4 (6.33)	24.3–48.3	35.4	35.1	**24**	23.1	24.6	25	22.9	24.5
Zr	267 (3.92)	253 (39)	292 (44)	281 (48.8)	201–401	260	258	**185**	176	178	**219**	222	227
Nb	15.5 (0.15)	21 (4)	15 (3)	9.85 (1.91)	6.58–14.4	11.2	8.33	**10.3**	10.3	10.2	**8.9**	9.13	9.06
Cs	3.11 (0.11)	3.8 (0.9)	4 (0.9)	3.46 (0.81)	1.87–5.36	2.41	2.71	**1.75**	1.61	1.68	**9.6**	9.67	10.1
Ba	861 (12.5)	1020 (150)	690 (100)	450 (79.6)	297–658	504	489	**1370**	1334	1357	**807**	829	820
La	26.4 (0.19)	24.5 (3.8)	29.5 (4.5)	31.3 (6.24)	20.2–46.0	28.6	26.3	**27**	26.4	26.8	**24**	23.3	23.8
Ce	57.4 (1.96)	49.4 (7.5)	60.5 (9.2)	49.3 (8.40)	32.4–68.5	55.0	55.9	**54.6**	53.5	53.7	**47**	46.4	49.1
Pr	6.96 (0.19)	7.4 (1.2)	7.1 (1.2)	5.88 (1.24)	3.28–9.16	6.62	6.17	**6**	6.01	5.59	4.7	4.86	5.11
Nd	30.6 (0.09)	28 (4)	25.2 (3.9)	24.0 (4.91)	15.5–36.8	25.1	23.2	26	21.7	22.0	**19**	18.8	18.9
Sm	7.66 (0.24)	8.2 (1.6)	5.3 (1.2)	5.26 (1.37)	2.49–8.90	5.58	5.06	**4.88**	4.60	4.87	**4.3**	4.17	4.32
Eu	1.71 (0.10)			0.78 (0.28)	0.00–1.47	1.07	1.02	**1.43**	1.29	1.39	**0.66**	0.67	0.69
Gd	6.54 (0.20)	5.5 (1.6)	5.8 (1.4)	5.22 (1.73)	2.43–9.60	6.48	5.76	4.7	4.49	4.11	**3.7**	3.75	3.70
Tb	1.27 (0.06)	0.91 (0.31)	1.1 (0.3)	0.98 (0.32)	0.00–1.82	0.98	1.02	**0.71**	0.69	0.70	**0.66**	0.63	0.67
Dy	7.74 (0.19)	8.1 (1.4)	6.7 (1.2)	6.46 (1.61)	3.31–10.1	6.79	5.98	**3.8**	3.88	4.04	**4.08**	3.78	3.91
Ho	1.82 (0.08)	1.7 (0.4)	1.4 (0.3)	1.52 (0.32)	0.99–2.31	1.55	1.37	0.86	0.88	0.92	0.95	0.89	0.91
Er	4.84 (0.08)	5.5 (1)	5.1 (0.9)	4.41 (1.21)	2.53–7.99	4.53	4.23	**2.3**	2.38	2.44	**2.6**	2.42	2.55
Tm	0.73 (0.03)	0.59 (0.2)	0.94 (0.24)	0.72 (0.28)	0.30–1.40	0.72	0.75	**0.37**	0.33	0.37	**0.37**	0.37	0.37
Yb	4.78 (0.19)	5.3 (1.1)	4.3 (0.9)	5.17 (1.75)	1.59–8.76	5.00	4.48	**2.32**	2.41	2.41	**2.6**	2.35	2.59
Lu	0.74 (0.02)			0.89 (0.34)	0.30–1.61	0.80	0.79	**0.37**	0.37	0.38	**0.41**	0.38	0.41
Hf	7.12 (0.20)			6.88 (2.34)	3.39–13.3	7.20	6.73	**4.6**	4.59	4.59	**6.2**	6.14	6.36
Ta	1.03 (0.01)			0.99 (0.87)	0.24–5.52	1.25		**0.82**	0.86	0.90	**0.95**	1.01	0.97
Th	6.13 (0.07)			16.2 (3.79)	8.73–26.5	16.5	16.0	**4.5**	4.51	4.52	**15.1**	14.5	14.2
U	2.84 (0.12)			5.39 (1.05)	3.30–7.60	5.16	4.64	**1.94**	1.83	1.88	**5.8**	5.77	5.87
Ba/Rb	12.94	19.25	8.96	3.98		5.51							
Ba/Sr	4.32	5.37	9.58	8.11		6.31							
Rb/Sr	0.33	0.28	1.07	2.04		1.18							
La/Nb	1.70	1.17	1.97	3.18		2.55							
La/Sm	3.45	2.99	5.57	5.95		5.13							

Table 3 Trace element analyses of A1340-7 from the GRIP ice-core and Bo-1 glass from Santorini (Hammer *et al.*, 2003), the Minoan ash from Gölhisar Gölü, SW Turkey (Eastwood *et al.*, 1998; Eastwood *et al.*, 1999; Pearce *et al.*, 2002), Minoan ash from Knossos (Warren and Puchelt, 1990) and Aniakchak (UT2011, this study). Figures in parentheses for the data of Hammer *et al.* (2003) are errors at ±1 standard deviation, and for the LA-ICP-MS data of Pearce *et al.* (2002) are ±1 standard deviation of the individual glass shard analyses. The latter represents the range of compositions of the individual shards and is not a measure of the analytical precision, which would be approximately ±5% for Ba and Zr; ±20% for Tb, Ho, Tm, Lu and Ta; and ± 10% for the remaining elements. Also included are analyses of the USGS rhyolitic reference materials QLO-1 and RGM-1, which were analysed alongside the Aniakchak tephra (UT2011), with accepted concentrations for comparison (bold = certified, normal = suggested (Govindaraju, 1994)). All concentrations in ppm

es such as fractional crystallisation (Pearce *et al.*, 2002; Pearce *et al.*, 2004).

Table 3 also presents new trace element analyses of a sample Aniakchak volcano (UT2011). This analysis has almost identical Sr, Rb, Zr, REE to A1340-7, as well as very similar Ba, Nb, Y and Cs. Any differences between the two data sets can be related in part to the possible accuracy of the SIMS analyses, particularly when allowance is made for the differences between the SIMS analyses of Bo-1 and other analyses of Minoan tephra (e.g. Ba, Nb, see above). The Aniakchak REE analyses are compared with the A1340-7 analyses in Figure 1, where it is evident that the 2 data sets have an indistinguishable

Comparison of samples...		$D^2_{calculated}$	Accept/ reject Ho (D^2 critical 99% = 26.22)
Bo-1 (Hammer *et al.*)	Minoan tephra, Gölhisar Gölü, (Pearce *et al.*)	10.78 (17.46)	**Accept**, samples are possible correlatives
Bo-1 (Hammer *et al.*)	A1340-7 (Hammer *et al.*)	26.39	**Reject**, samples are statistically different
Bo-1 (Hammer *et al.*)	Aniakchak (UT2011, this study)	136.8	**Reject**, samples are statistically different
A1340-7 (Hammer *et al.*)	Aniakchak (UT2011, this study)	14.40	**Accept**, samples are possible correlatives
A1340-7 (Hammer *et al.*)	Minoan tephra, Gölhisar Gölü, (Pearce *et al.*)	47.84 (67.02)	**Reject**, samples are statistically different
Aniakchak (UT2011, this study)	Minoan tephra, Gölhisar Gölü, (Pearce *et al.*)	259.8 (810.9)	**Reject**, samples are statistically different

Table 4 Distance function calculations for a range of possible correlative pairs of samples in this study for the 12 trace elements Rb, Sr, Zr, Nb, Y, Cs, Ba, La, Ce, Pr, Nd and Sm using the methods described by (Perkins *et al.*, 1998; Perkins *et al.*, 1995). $D^2_{critical}$ at a 99% confidence level is 26.22. Where $D^2_{calculated} > D^2_{critical}$, the null hypothesis (that the two samples are identical) is rejected. In comparisons including the LA-ICP-MS analyses of the Minoan tephra from Gölhisar Gölü (Pearce *et al.*, 2002), the first $D^2_{calculated}$ uses the standard deviation of all single shard analyses (which reflects the natural variation between individual shards, not the analytical precision); the $D^2_{calculated}$ in parentheses uses an estimate of the analytical error for the LA-ICP-MS analyses (i.e. 10% relative for all elements, except Zr and Ba at 5%, see Pearce *et al.* 2004)

REE profile. Figure 2 shows a Thompson-type incompatible element spidergram (Thompson, 1982) that compares the Minoan (from Pearce *et al.* 2002) and Aniakchak (UT2011) tephras with the A1340-7 ice-core glass. The Aniakchak tephra and A1340-7 glass are essentially identical, and they both have markedly different Rb, Sr, Ba, Ti, and Sm from the Minoan tephra. It is clear that the trace element analyses concur with the major element analyses and deny the suggested correlation between the ice-core glass and the Minoan tephra from Santorini; instead they clearly show that the ice-core glass (A1340-7) correlates with the late Holocene Aniakchak tephra (UT2011).

Comparing samples using statistical distance

Perkins *et al.* (1998; 1995) have developed methods to test the possibility of a correlation between silicic tephra beds using a measure of the statistical distance between pairs of samples. Perkins *et al.* (1998) define the statistical distance between samples as:

$$D^2 = \sum_{k=1}^{n} (x_{k1} - x_{k2})^2 / (\sigma_{k1}^2 + \sigma_{k2}^2)$$

where n is the number of elements used in the comparison, x_{k1} and x_{k2} are the concentrations of the kth element in the first and second samples, and s_{k1} and s_{k2} are the precisions of the determinations of the kth element in each sample (i.e. the standard deviations of analyses). The value of $D^2_{calculated}$ has a Chi-squared distribution amongst compositionally identical samples (Perkins *et al.*, 1995). If $D^2_{calculated} > D^2_{critical}$ for a given confidence level (defined at the outset of the comparison), then the null hypothesis (that the two samples are identical) is rejected and the two samples can be considered to be different at that confidence level. The trace element analyses given by Hammer *et al.* (2003), Pearce *et al.* (2002) and the new analyses of the Aniakchak tephra (this study) have been compared using this measure of statistical distance. In this comparison, the heavy REE have been excluded from these calculations because of their relatively large analytical uncertainties. In the LA-ICP-MS data from Pearce *et al.* (2002) the standard deviation of all single shard trace element analyses (which reflects the wide, natural, inter-shard variation) has been used in the calculations, as well as an estimate of the real analytical error (see Pearce *et al.* 2004). The results are presented in Table 4, and show that, at the 99% confidence level, neither the Bo-1 (Hammer *et al.*, 2003) nor the Gölhisar Gölü Minoan (Pearce *et al.*, 2004) samples can be correlated with A1340-7, the ice-core glass. The data for the Aniakchak tephra (UT2011) and A1340-7 can, by this measure, be regarded as possible correlatives.

Conclusions

Small volcanic glass shards recovered from the 1645±4 BC layer of the Greenlandic GRIP ice-core are chemically distinct from glass from the Minoan

eruption of Santorini. Instead, they can be confidently correlated with the late Holocene Aniakchak tephra. The precise chronology which can be obtained from the ice-cores thus places a firm date of 1645±4 BC on the eruption of Aniakchak, and this is in good agreement with the published age ranges from ^{14}C dating. The eruption of Aniakchak at 1645 BC is also consistent with the suggestion that the acid volcanic signal in the Greenlandic ice-core results from a major eruption south of Greenland, but north of 30°N (Hammer *et al.*, 2003). Indeed, the prevailing atmospheric circulation has the potential to readily carry glass north and east from Alaska towards and across Greenland, as exemplified by the Aniakchak tephra fall. The presence of glass shards trapped within the ice-core in this case directly links the Aniakchak eruption with the acid peak at 1645 BC, and while this does not rule out a coincidental eruption of Santorini at the same time, it leaves the debate over the exact age of the Minoan eruption open.

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Methods

Major element analyses of UT2011 (Aniakchak tephra) were performed using a Cameca SX-50 WDS electron microprobe at the University of Toronto. Trace elements were determined by solution ICP-MS at the University of Wales, Aberystwyth for UT2011. Full descriptions of all methods used are given elsewhere (Pearce *et al.*, 1997; Pearce *et al.*, 2004).

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^{14}C and the Early Iron Age of Israel – Where are we really at? A Commentary on the Tel Rehov Radiometric Dates

Ilan Sharon,[*] *Ayelet Gilboa,*[**] *Elisabetta Boaretto*[***]

Abstract

Recently-published radiometric dates from Tel Rehov (Israel) have been claimed to prove the traditional, high Levantine Iron Age chronology. Reconsideration of these, alongside other published dates indicates that at present the available data is much more compatible with the low chronology and that the controversy is far from being over.

Introduction

The chronology of the early Iron Age in Israel has turned into one of the major archaeological controversies of the last decades. The 'low' chronology, proposed by I. Finkelstein (1996) is only about 75 years lower than the traditional biblically-derived 'high' chronological scheme, but with tremendous implications for nearly every issue of archaeology, history and historiography around the Mediterranean (see references in Gilboa and Sharon 2001; 2003).

Some scholars consider the matter decided (in one direction or the other), on the basis of historical considerations (Dever 2001; Ben-Tor 2001). For those of us, however, who are willing to concede that the archaeological scenario can be stretched to fit either historical narrative, radiometric dating presents the best hope of escaping from the chronological limbo we were cast into.

Several years ago we published a preliminary set of data, based on 22 radiocarbon measurements from Ephraim Stern's excavations at Tel Dor, on Israel's Mediterranean coast (Gilboa and Sharon 2001; Sharon 2001). These, we then claimed, form the first empirical support for the 'low chronology'. We did, however, caution against hanging the entire chronology of the Eastern Mediterranean over several 'centuries of darkness' on a single limited data set, from a single site, mostly analyzed by a single laboratory.

The Tel Rehov Dates

A second data set, lately published by Bruins, Mazar and van der Plicht (2003) was, however, claimed to clinch the case in the other direction. Based on a series of 34 ^{14}C dates from Mazar's excavations at Tel Rehov, in the Jordan Valley, analyzed by the Groningen Center for Isotope Research, the authors claim to have proven the traditional high chronology. The dates originate from a well stratified sequence: Stratum VII at Rehov, defined as Iron Age I (or alternatively, the Iron Age I/II transition in the supporting online material to that article), and Strata VI, V, IV – all defined by Mazar as Iron Age IIA. Two main conclusions were drawn from these dates: (a) That the Iron Age I/II transition at Tel Rehov occurred ca. 980 BCE; and (b) That the dates from the Stratum V destruction are the ones which best fit a postulated destruction of the site by the Egyptian king Shoshenq I, conventionally dated around 925 BCE. If these conclusions are accepted, extrapolating the Tel Rehov ceramic sequence and its accompanying dates to other sites in Israel, would indeed render a substantial support to the high chronology. Indeed, these conclusions were hailed by Harvard's Lawrence Stager as "the nail in the low chronology's coffin" (cited in Holden 2003). Let us, then, have a closer look at this nail.

Reassessment

We conducted a very simple formal test to assess the Rehov team's main conclusions. Using the same software package used by them, OxCal v.3.5 (Bronk Ramsey 2001), we constructed the probability distribution for the boundaries between Stratum VII (Iron Age I) and VI (earliest Iron Age IIA); between Stratum VI and V (both within Iron Age IIA) and between Stratum V and IV (also within Iron Age IIA, Shoshenq's visit according to Bruins *et al.* 2003).[1]

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[1] The analysis is based on 32 out of the 34 published dates, as it is unclear whether two measurements from local phase D-2 should be attributed to either Stratum V or VI (A. Mazar, personal communication).

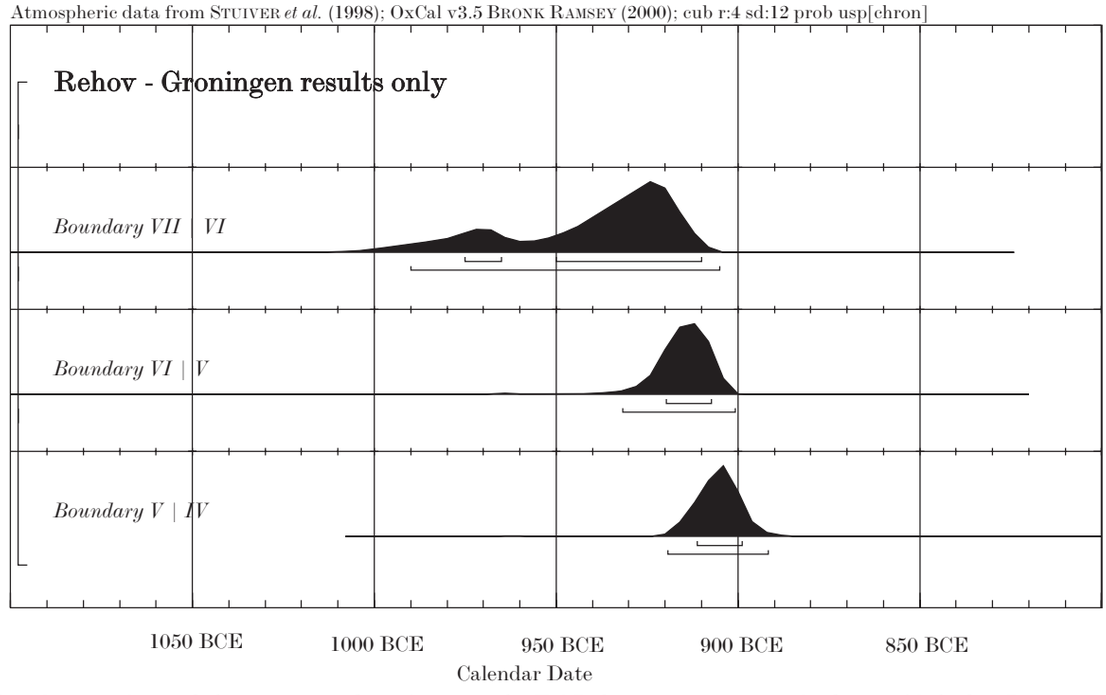


Fig. 1 Oxcal 3.5 analysis of the transition dates between the Tel Rehov strata, using only data published in Bruins *et al.* 2003

The upper plot in Fig. 1 presents the Iron I/II transition at Tel Rehov, placed by Bruins et al. ca. 980 BCE. Most of the distribution indeed falls in the 10th century BCE; the transition could have occurred anywhere in that century. But while the distribution *does* have a peak ca. 980, this peak is secondary and the more likely option is in the second half of that century, peaking between 930–920 BCE. This, let us be reminded, is exactly the claim of the low chronology, namely that Iron IIA begins in the late 10th century at the earliest, and that, if anything, Shoshenq I terminated Iron Age I strata (e.g., Finkelstein 1996, 183).

The dates for the boundary between Strata VI and V at Tel Rehov (the middle plot in Fig. 1) fall in the last third of the 10th century BCE, peaking between 920 and 910 BCE. The transition between V and IV (the postulated 925 Shoshenq destruction) falls actually, with 95% probability, between 919–892 BCE (lower plot in Fig. 1). This means that if one seeks the occupation at Tel Rehov that might have been destroyed by Shoshenq I, Strata VII and VI at Rehov are also eligible candidates; in fact, they are better candidates than Stratum V.

This last suggestion can be demonstrated in a very simple visual manner:

Fig. 2 has been extracted from Bruins *et al.* 2003: fig. 2. It is important to pay attention to the fact that one distribution plot in this figure is not an actual ^{14}C date: It is a simulated ^{14}C date for a sample whose 'real' date would be ca. 925. As may be seen – it was placed between the dates of Stratum V and those of IV – asserting the authors' conviction that this transition is the one chronologically compatible with Shoshenq. In reality, they were placed after the Stratum V dates in spite of the fact that in general the distributions of the latter have later 'tails'.

We suggest alternative placements of Shoshenq and his simulated date – between Strata VI and V (Fig. 3) and between VII and VI (Fig. 4). The advantage of these placements, especially the latter, is that now the simulated Shoshenq date is located before *all* the dates which have a somewhat later distribution – a much better positioning in the sequence. As claimed above – this means that both these strata are better nominees for the towns called upon by Shoshenq.

Moreover, in their 2003 paper, the Tel Rehov investigators chose to present and take into consideration only the (34) dates obtained at the Groningen lab. They ignore other dates from the same contexts at Tel Rehov, recently published by Mazar himself: 9 dates analyzed at the ^{14}C laboratory at the University of Arizona and 12 analyzed at the Weizmann Institute, Rehovot (Mazar and Carmi 2001). When *all* published dates from Tel Rehov are considered in the manner outlined above, the categorical conclu-

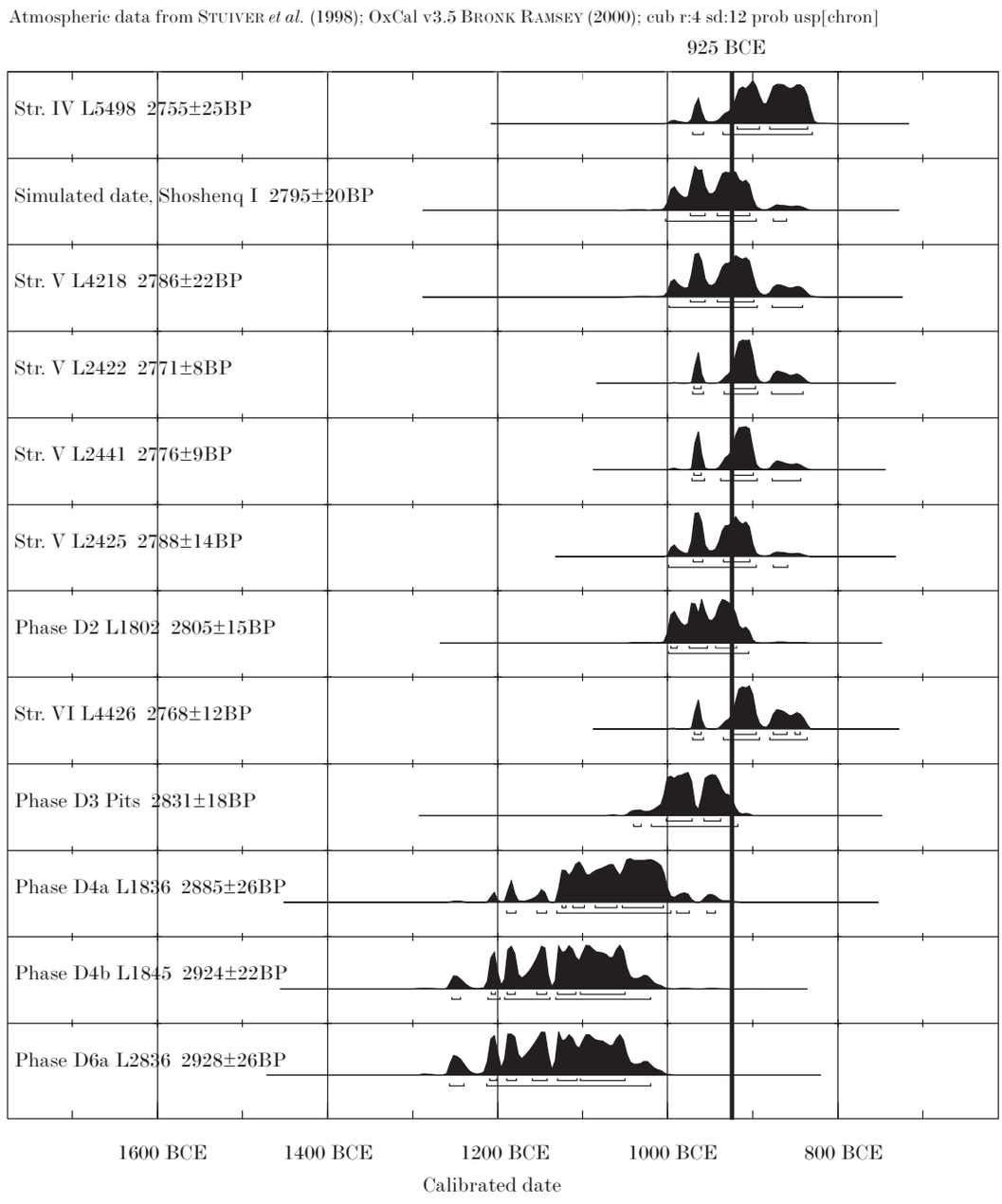


Fig. 2 Tel Rehov sequence after Bruins *et al.* 2003: fig. 2 with Shoshenq I simulated date placed between Strata V and IV

sions presented in Bruins et al. 2003 become even less plausible (Fig. 5):[2]

The transitions between Strata VII and VI and between VI and V are similar to the above, but the transition between V and IV ('Shoshenq's 925 campaign') is now placed between 880 and 850 BCE with 95% probability. These dates, again, squarely agree with the low chronology.[3]

[2] We have considered in this analysis only measurements on short-lived samples. Several other dates, on constructional wood, are cited in Mazar and Carmi 2001.

[3] After the publication of Bruins *et al.* 2003, Mazar (personal communication) avers that he is no longer sure that L2425, from which 20 of the dates were obtained (2 in Groningen, 9 in Arizona, 9 in Rehovot) is indeed in Stratum V – it may belong to IV. If the analysis is redone with all the L2425 dates in IV, the results very closely resemble those of the first run presented here.

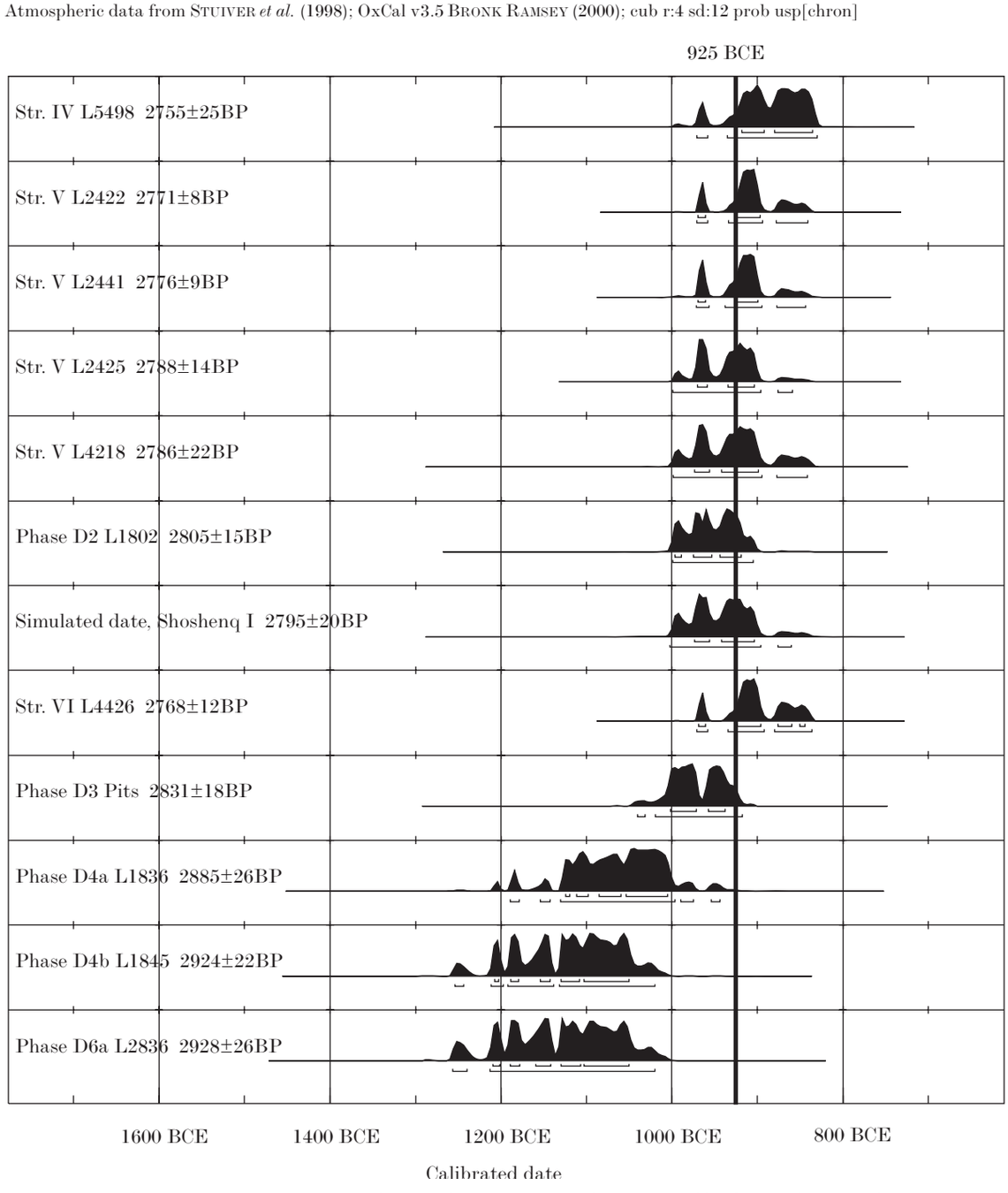


Fig. 3 Tel Rehov sequence in Bruins *et al.* 2003: fig. 2 re-arranged, option 1: Shoshenq I between Strata VI and V

Summary

As opposed to the conclusions reached by the Tel Rehov team, we maintain that the Rehov dates can much better be accommodated in a low chronology framework (see similarly Finkelstein and Piasetzky 2003). This has also been our conclusion regarding the other major relevant sequence of radiometric dates – that from Tel Dor (Gilboa and Sharon 2001; 2003; Sharon 2001: fig. 1), which was dismissed in Bruins et al. 2003 as "ambiguous".

Nevertheless, the Tel Rehov and Tel Dor dates are not easily reconciled – but the discussion of this discrepancy is outside our scope here.

So – where are we really at – and more importantly – where do we go from here?

Picking some dates and rejecting the rest, without explicit and well-founded reasoning will simply

Atmospheric data from Stuiver *et al.* (1998); OxCal v3.5 Bronk Ramsey (2000); cub r:4 sd:12 prob usp[chron]

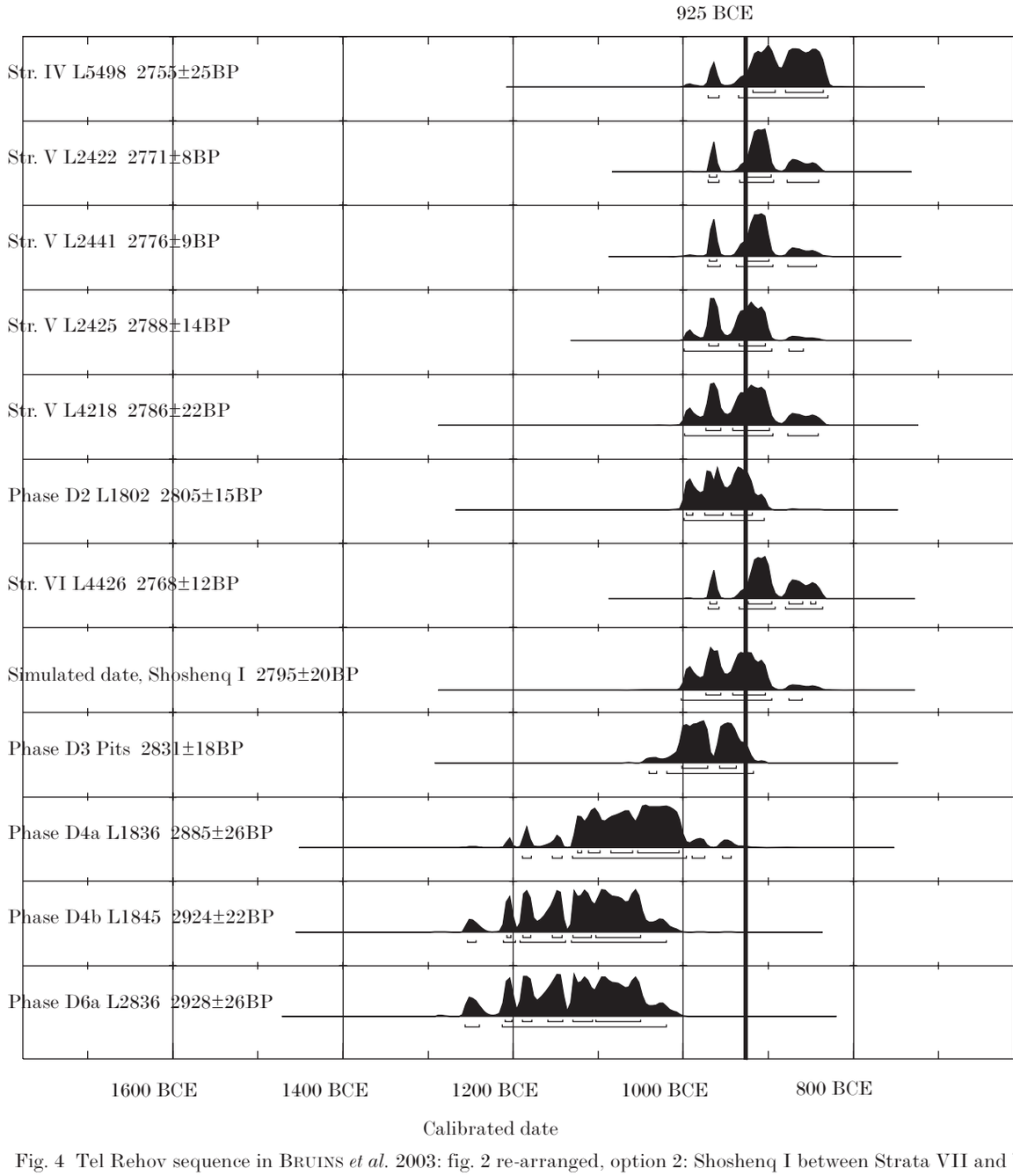


Fig. 4 Tel Rehov sequence in Bruins *et al.* 2003: fig. 2 re-arranged, option 2: Shoshenq I between Strata VII and VI

not do. Even with state-of-the-art radiocarbon dating, differentiating between chronological hypotheses separated by a less than a century can only be achieved with *hundreds* of dates from many sites, collated on the basis of careful stratigraphic and comparative artifactual sequences, to form a statistically-significant data base. These should be dated by several labs, using different pretreatment protocols and dating methods, in order to investigate the possibility of biases in any of these. Four years ago the three of us initiated such a project, involving some 25 excavations in Israel (including Rehov). It is our conviction that it will take many more nails before we may faithfully proclaim to have sealed this or other coffin.

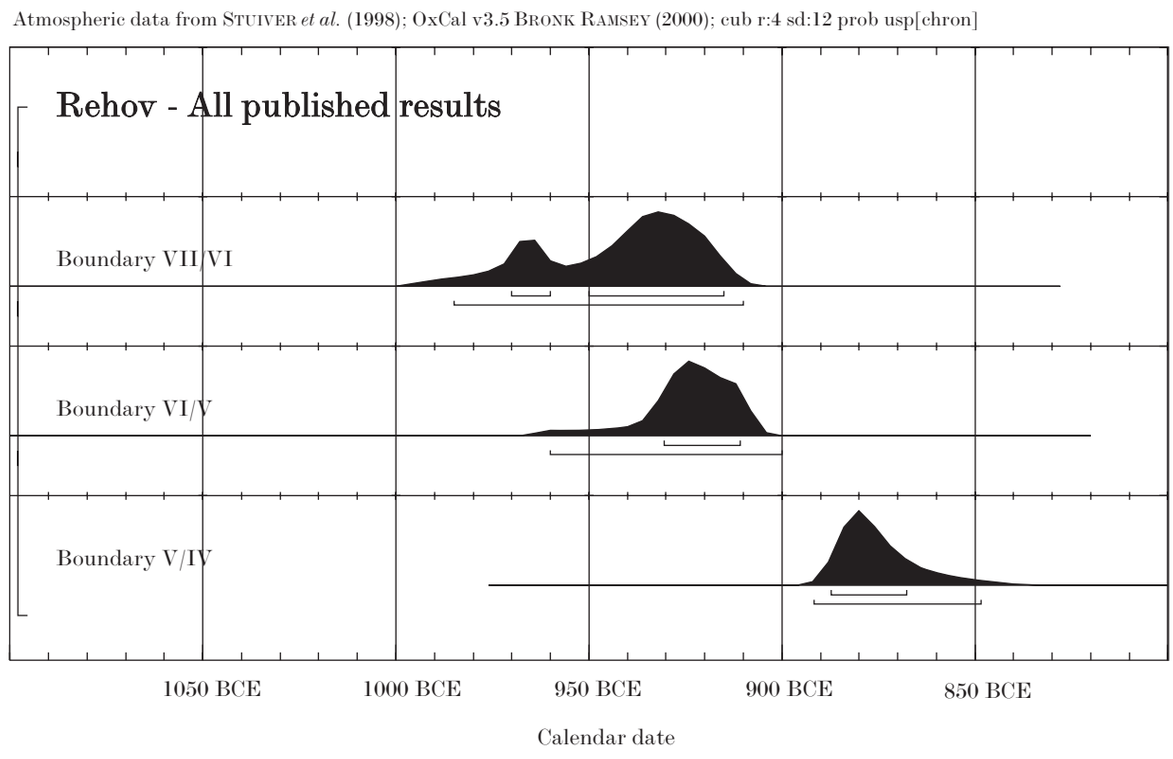


Fig. 5 Oxcal 3.5 analysis of the transition dates between the Tel Rehov strata, using all published data

Acknowledgements

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First Lunar Crescents for Babylon in the 2nd Millennium B.C.

Uroš Anderlič[1] *and Maria G. Firneis*[2]

Babylon's position in absolute chronology is established via the citations of astronomical phenomena on cuneiform tablets and as astronomical phenomena repeat more or less cyclically it is of vital concern to fit these data optimally to the framework of absolute astronomical chronology.

Astronomical research methods and data applied to ancient Mesopotamia have now been carried out for almost 130 years for ancient observations as well as divination – started by Johann Nepomuk Strassmaier and Joseph Epping.[3] A special task is the application of the value of the mean synodic month leading to a forecast of the visibility of the first lunar crescents for each month, especially for the city of Babylon. For modern verifications it is thus of great importance for historians to possess tabulated values for the first lunar crescent phases near the western horizon. As a matter of fact these earliest lunar crescent's visibilities are still an unsettled matter among modern astronomers. Observations of the young crescent depend on astronomical and atmospherical factors as well as human vision. Thus a reconstruction of the actual first lunar crescent observations can yield to uncertainties of at least 1 day.

According to H. Thurston[4] recorded astronomical data of the Babylonians date back almost 3800 years from now. Further, as Babylonian "Astronomical Diaries" comprise recorded and somehow crudely predicted astronomical events – available in written down form starting from the 8th century B.C. – by advice of H. Hunger[5] it seemed reasonable to create a list of the first lunar visibility from –2000 until 1.[6] The commercially available astronomical package UraniaStar[7] and the applied algorithms of J. Meeus[8] used therein have both already been tested on reliability for astronomical calculations in M. Firneis and M. Rode-Paunzen[9] using the semi-analytical lunar theory of J. Chapront:[10] ELP 2000.

In this treatise the first lunar crescents for Babylon are only given for the 2nd millennium B.C. while in Anderlič' master's thesis[11] values from 2001 B.C. until A.D. 1 were calculated. The complete data set for the 2002 year lunar crescent visibilities may be accessed via http://www..univie.ac.at/EPH/ Geschichte/First_ Lunar_Crescents/Main.htm. In these tables the date of the crescent phase is given in the Julian calendar – using the astronomical and historical notation. Though the setting time of the sun was calculated for the Eastern European time-zone, the usual day fraction is given in the Julian day fraction starting at 12h Universal Time; this is an astronomical convention. Further the topocentric apparent geometrical altitude of the moon at the beginning of civil twilight[12] – refraction included – is given as well as the altitude of the moon at the end of civil twilight.[13] Also the parameters delta azimuth, illuminated fraction of the moon and the so called lunar age are given in the tables of this publication. Because of the uncertainties explained above the list includes all important astronomical data needed to find the first visibility of the lunar crescent with the help of the cuneiform texts where – as the city

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[1] Institute of Astronomy, University of Vienna/Austria.
[2] Austrian Academy of Science, Commission of Astronomy.
[3] O. Neugebauer, *A History of Ancient Mathematical Astronomy*, (Berlin 1975), 348.
[4] H. Thurston, *Early Astronomy*, (New York 1994), 64.
[5] H. Hunger personal communication 2001.
[6] In historical notation 2001 B.C. until A.D. 1 since the year called zero is missing in the historical notation.
[7] M. Pietschnig and W. Vollmann, UraniaStar (version 1.1, November 1995).
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[10] M. Chapront-Touzé and J. Chapront, *Lunar Tables and Programs from 4000 B.C. to A.D. 8000*, (Richmond 1991).
[11] U. Anderlič, *First Lunar Crescents for Babylon (2001 B.C. to A.D. 1) and Nineveh (701 B.C. to 600 B.C.)*, Master's thesis (Vienna University 2003).
[12] The centre of the sun exactly sets on the western horizon.
[13] The sun is 6° below the western horizon.

Anderlič-Firneis Criteria	Boundaries		Extreme boundaries	
	Minimum value	Maximum value	Minimum value	Maximum value
Illuminated fraction [%]	0.01	0.03	0.01	0.1
Age [days]	0.6	1.9	0.5	3.1
Altitude (Beginning) [°]	5.79	20.48	5.79	20.48
Altitude (End) [°]	1.90	15.65	1.90	15.65

Table 1 Explanation for the different values used for the first lunar crescent visibilities

of Babylon lies in a very flat plain – the first lunar crescent could have been observed, assuming clear weather, in a very early phase within an accuracy of about 2°[14] by the Ancients.

Contrary to the lunar crescent visibility boundaries used by Firneis and Rode-Paunzen[15] some of these limits have been exaggerated in cases where the small differences in the criteria can be explained with the smaller number of cases in the data-base used for Egypt as well as the different geographical position of Egypt. This has been proved by comparing the data-set for Nineveh and Babylon with each other, where the dates for the first lunar crescent observation were in accord in more than 93%. The different dates occurred because of the different geographical longitude as well as latitude of both cities.[16] The difference between the criteria used by Firneis *et al.*[17] and the ones used in this treatise is that a visibility of the young moon closer to the horizon has been allowed for. The dates for the first lunar crescents – obtained with the present criteria – give the theoretical possibility of a first lunar crescent observation under optimal conditions where in some cases the possibility of the first lunar crescent sighting lies very close to the boundary of invisibility. Nonetheless at least such a date is a good starting point for a trial correlation (Table 1).[18]

Detailed analysis on the actual variation of the synodic month – that is the real time span from the new moon phase to the next one – shows a "Gaussian" distribution as expected (Fig. 1). On the other side the variation of the time-span from the first lunar crescent to the next one at the beginning of civil twilight shows two primary maxima and a secondary one in its distribution (Fig. 2). The difference between first crescent visibilities shows the typically 24 hour spaced peaks, because either the first crescent can be seen in a low altitude or in a higher one on the next day. Even a small 3rd peak may arise 48 hours later because the illuminated lunar fraction may escape the human vision 24 hours earlier. As the shortest time-span found between two first lunar crescent appearances lies at 28.9736 days the basic value for the graphs was chosen to be 28.9000 days.

When looking at the graphs for the *lunar age* after the conjunction at the beginning and end of civil twilight two rather similar shapes appear (Fig. 3) – only half-day steps are used for this age-parameter since ancient times. At the end of civil twilight the decline of the curve is more pronounced as the lunar age closer to the horizon will increase, but – as a logical conclusion – the histogram area is more or less constant. A similar feature emerges when comparing the two histograms of the *illuminated fraction* of the moon at the beginning and end of civil twilight (Fig. 4). The *altitude* of the moon in crescent phase at the beginning and end of civil twilight as expected shows a nearly equal shape shifted by a mean value of 4.86° for the crescents near the end of civil twilight (Fig. 5 and 6). As a minimal lunar height a value of 1.90° was permitted. Of course a higher number of low-altitude events show

[14] J.M. Steele, F.R. Stephenson and L.V. Morrison, The accuracy of eclipse times measured by the Babylonians, *Journal for the History of Astronomy* 28, Part 4 (November 1997), 345.

[15] See note 9.
[16] See note 11.
[17] See note 9.
[18] See note 11.

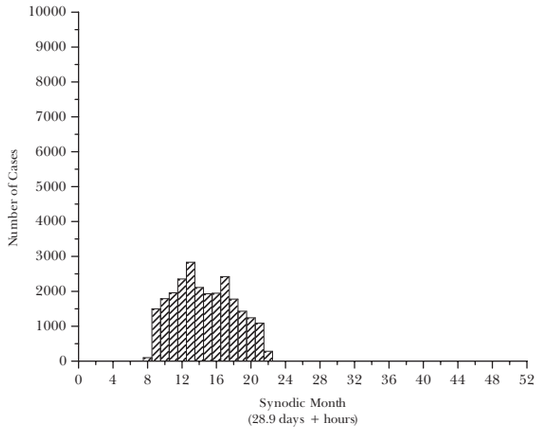


Fig. 1 The length of the synodic month shown in hours with a start-off value of 28.9 days

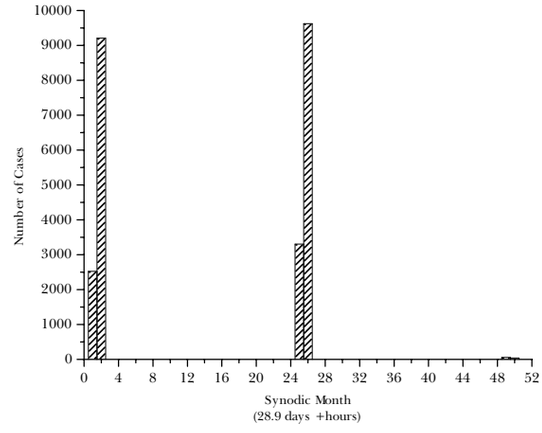


Fig. 2 Time-interval between two successive first lunar crescents at the beginning of civil twilight shown in hours with a start-off value of 28.9 days for the city of Babylon (44.42° E; 32.53° N)

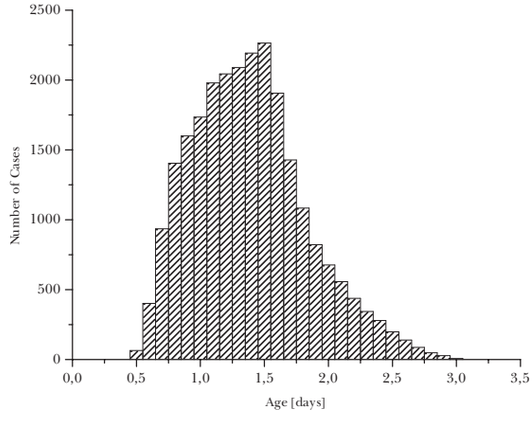


Fig. 3 The age of the moon at the time of first lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

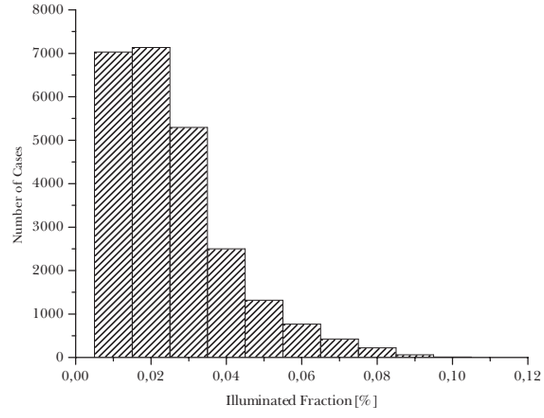


Fig. 4 The illuminated fraction of the moon at the moment of first lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

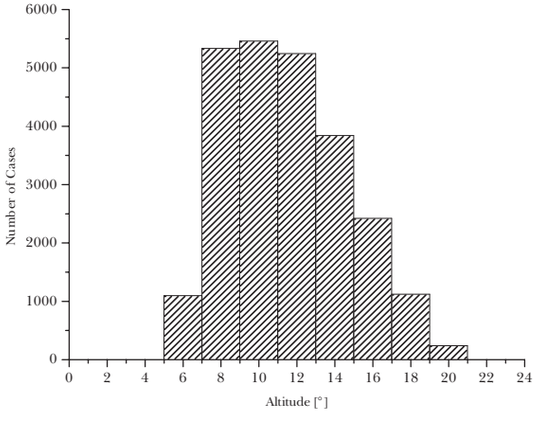


Fig. 5 The altitude of the moon at the time of first lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

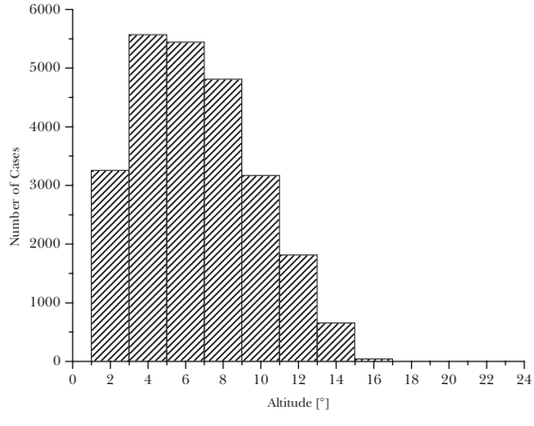


Fig. 6 The altitude of the moon at the time of first lunar crescent visibility at the end of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

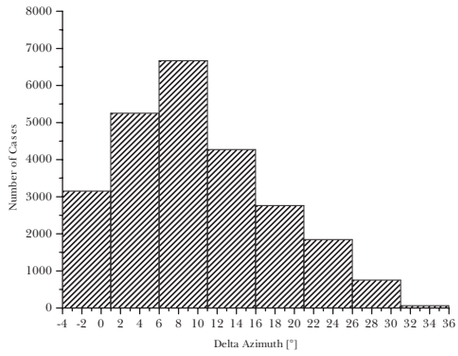


Fig. 7 Delta azimuth at the time of first lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

up at the end of civil twilight. Actual *azimuthal differences* (always reckoned in the sense sun-centre minus moon-centre) may range from –3.99° to +36° with a maximum between 6° and 11° (Fig. 7). By plotting the lunar age against the altitude of the moon or the altitude of the moon against the illuminated fraction of the moon no random distribution can be found (Figs. 8 and 9). Further, for a better master screen of the number of cases' distribution a 3-dimensional plot has been generated for the lunar age against the altitude of the moon and another 3-dimensional plot for the altitude of the moon against the illuminated fraction of the moon (Figs. 10 and 11).

Dates given for the first lunar crescent occurrences require optimal observing conditions in the evening of the first lunar crescent. Under non optimal conditions – for instance like dust, which greatly effects observability due to the low altitude of the moon – the next day should be taken as the day of the first lunar crescent observation. Sometimes the first lunar crescent can be seen one day earlier – even below our chosen altitude of 1.90° – because for instance hot air may cause extraordinary refraction. As L. Fatoohi *et al.*[19] has provided 209 first crescent visibilities for Babylon for the time-interval between 568 B.C. and 74 B.C., our values where counter-checked against his data-base. Nearly 80% of identical dates resulted. The rest of his first lunar crescents occurred one day later or earlier – mostly later. Details on this and further facts may be found in Anderlič' master's thesis[20] yielding an explanation that in some cases Fatoohi allowed for a lunar height above the horizon of less than 1.90° when using our calculation algorithm. Anyway agreement was well achieved within the error bars.

As mentioned before the main parameters of the first lunar crescent are: the lunar age, the illuminated fraction and the altitude. To find the main peri-

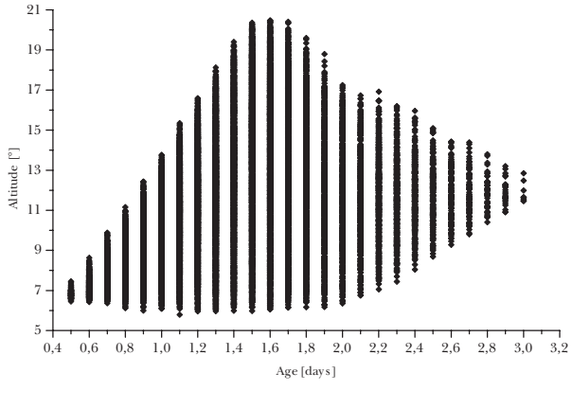


Fig. 8 First lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

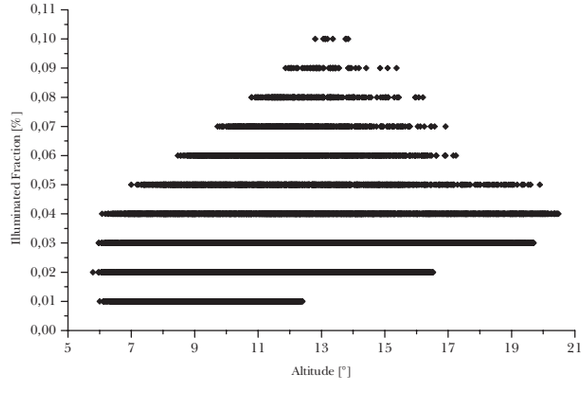


Fig. 9 First lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

[19] L.J. Fatoohi, F.R. Stephenson and S.S. al-Dargazelli, Babylonian First Visibility of the Lunar Crescent, *Journal for the History of Astronomy* 30, Part 1 (February 1999), 51.

[20] See note 11.

[21] Wolfram Research Inc. Mathematica, Version 5.0, Champaign Il (2003).

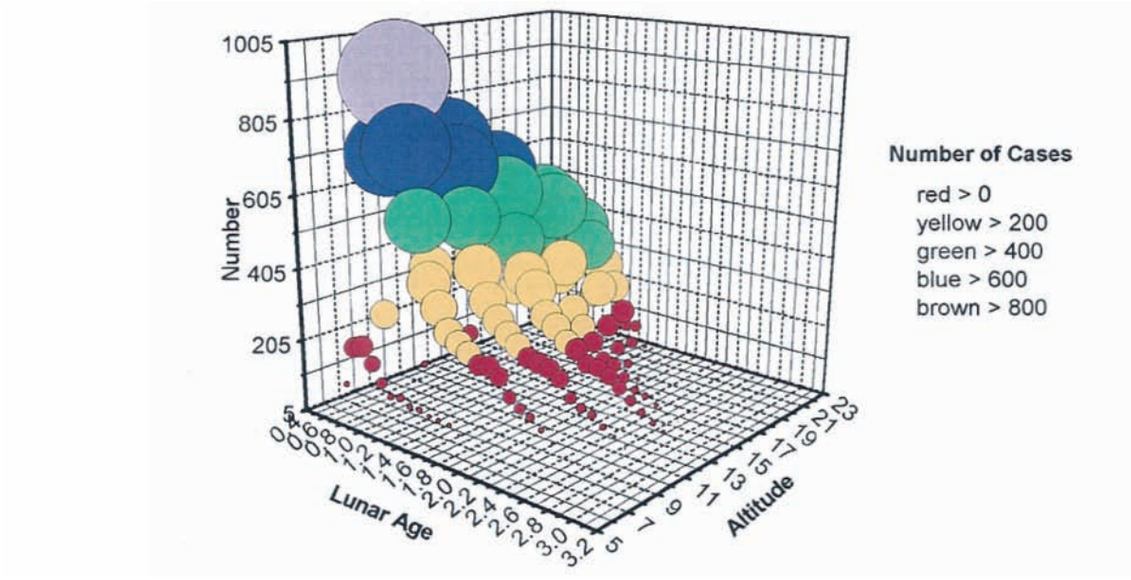


Fig. 10: First lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

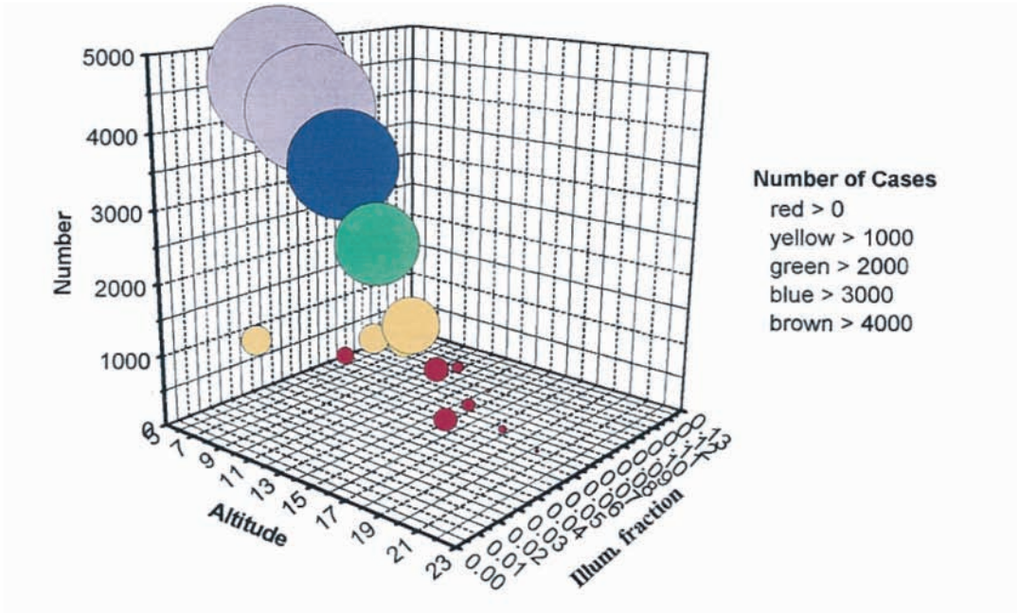


Fig. 11: First lunar crescent visibility at the beginning of civil twilight for the city of Babylon (44.42° E; 32.53° N) for the years 2001 B.C. until A.D. 1

od of these characteristic values of the first lunar crescents a Fourier analysis has been executed using the program Mathematica[21] (Fig. 12). The x-axis represents the synodic months of the whole calculation time-span (only the first half of the whole graph was plotted because of aliased frequencies at higher values on the x-axis) and the y-axis gives the magnitude of the parameter, respectively. The peak with the highest magnitude was found at a value of 2003 on the x-axis which represents the 2003rd part of the whole calculation time-span for these 2002 years. Dividing the peak value of this point on the x-axis through the whole calculation time-span yields the frequency. Taking the reciprocal of the frequency for all three parameters a period of 12.36 synodic months has been achieved. This is clear as

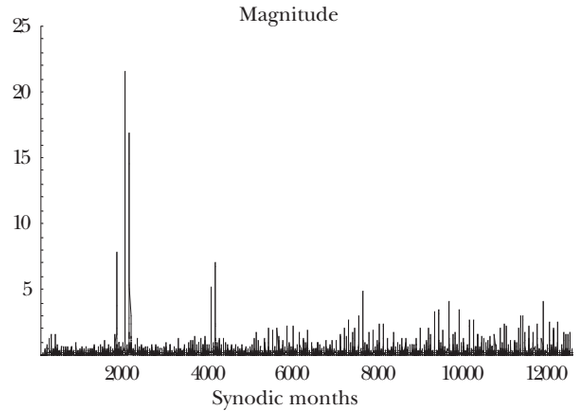


Fig. 12 Fourier analysis of the lunar age

one year later the positions of the sun and moon relative to the earth are nearly the same.

A general look at the lunar age-components – concerning first crescent visibility for Babylon over 2000 years – shows the typical 18.6-year peaks well known since ancient times as the Metonic cycle.

The various criteria for first crescent visibility, for instance the Anderlič-Firneis criteria, thus seem reasonable. However, the problem with ΔT = TD – UT (Delta T = Terrestrial Dynamical Time minus Universal Time) still resides and is immanent in all calculations dating back actual ancient lunar observations and this is a problem still not perfectly solved in modern astronomy.

Egyptian and Related Chronologies – Look, no Sciences, no Pots!

Kenneth A. Kitchen[*]

§1.

At the end of the day, the chronology of the Ancient Near East/Ancient East Mediterranean has to depend on written sources for the periods when and where these exist. In their absence, archaeological sequences of assemblages using stratigraphy give us sequence, but not absolute dates. During last century, highly ingenious "scientific" procedures have been developed to try to overcome the problem of fixing absolute dates, especially when explicit written records are lacking, including use of astronomy, radio-carbon, tree-rings, ice-cores, and so on. However, each of these is subject to various flaws that prevent attainment of absolutely reliable results, so far.[1] Therefore, it may here be of service to indicate where we stand, if one makes careful use of our written historical evidence on its own, until the other methods can be freed of the problem of inherent sources of error.

§2.

Let us start with Alexander the Great, seemingly well fixed at 336–323 BC. In 331, he ended the Persian Empire with the death of Darius III Nothus. From the traditional sources of Ptolemy's Canon, the Saros Canon and tablet, and other classical references, it is possible to set up dates BC for the entire Achaemenid Empire from Cyrus II down to Darius III. What is specially important is that these figures can be confirmed almost throughout, from the take-over of Babylon by Cyrus (539 BC) down to the accession of Artaxerxes III in 359 BC, using a whole series of date-lines of accessions and deaths (in their last regnal years) of these kings from original documents. Thus, earlier local regimes ended in 539 BC, of Neo-Babylon (Nabunaid), and in 525 BC of 26th-Dynasty Egypt (Psamtek III). For direct data in Babylonia during 626–359 BC, see listings in Parker and Dubberstein 1956, 10–24 and 25–47. On Cambyses in Egypt from 525, see Depuydt 1996, and on Ptolemy's Canon during 747–30 BC, see Depuydt 1995. No change is required in the dates here.

§3.

Beyond 539 and 526 respectively, we can go back appreciably further in both Mesopotamia and Egypt, using principally first-hand sources and good-quality records (such as the Babylonian chronicles, Assyrian eponym lists, etc.), without any recourse to the natural sciences. Let us visit Mesopotamia first.

Thus, the reigns of the Neo-Babylonian Empire from the eclipse of Nabunaid back to the accession of Nabopolassar in 626 BC[2] are correctly given in the Canon (omitting only the few months of Labashi-Marduk), as is – again – demonstrated by the attestation of the beginnings and ends of their reigns in original business tablets of the time in each case. Before 626, in both sources (Canon; and ancient, especially the Babylonian chronicles) the enigmatic Kandalanu and a brief interval between him and Nabopolassar total 22 years, 648–626 BC. The year 648 saw the fall of Shamash-shum-ukin to his brother Assurbanipal of Assyria, after they had both reigned 20 full years to that point, after their parallel accessions in 669 BC at the death of Esarhaddon. The Years 18, 19 and 20 of Shamash-shum-ukin occur in business tablets (cf. Millard 1964, 29, with references), For Kandalanu's reign-end and Years 21/22 "after Kandalanu", see Parker and Dubberstein 1956, 11.

At this point, Assyria takes the lead. The Eponym Canon (well-transmitted back in time, for the years 649 to 910 BC) enables us to date the reigns of all Assyrian kings back to Adad-nirari II, whose 21

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[1] On the elimination of Egyptian lunar and Sothic dates as invalid because of the problems of observation, see Wells 2002.

[2] In this paper, we use true accession-dates for all Mesopotamian rulers, not the misguided modern convention of ignoring accession-years and counting only from Year 1 (in pointless imitation of the book-keeping usage of ancient lists, not applicable today).

years of reign (in the Assyrian king-lists) ended in 891 BC, and thus began in 912 BC; for the eponym lists, see Millard 1994. In the 10th year of Assur-dan III, which thus fell in 763 BC, *using exclusively our historical sources*, the Eponym canon records an eclipse of the sun (Millard 1994, 2, 58). Our astronomers believe that just such an eclipse happened on 15/16 June (Julian) in 763 BC – how nice, that our ancient historical sources can now prove by independent calculation that our modern astronomers actually got it right!

Beyond 612 BC, the Assyrian king-lists carry us reliably back still further, to the accession of Tiglath-pileser I's father, Ashur-resh-ishi I, in 1133 BC.[3] Before that, there are (briefly) problems ancient and modern. Between Ashur-resh-ishi I and Ashur-dan I are listed two kings with no regnal years, labelled *tuppi-shu*, a remark not yet fully understood. The practical effect is that they both are assigned zero years, and the reign of Ashur-dan I then immediately precedes Ashur-resh-ishi I, chronologically. Most scholars accept 46 years for Ashur-dan I, rather than the 36 years possibly in the Nassouhi list and preferred by Boese and Wilhelm 1979. Immediately before him, Ninurta-apil-ekur also shows a 10-year discordance, at either 3 or 13 years, the higher date being accepted by most authorities. Either way, as Boese and Wilhelm (1979, 28–29, §V) point out, the joint total of the two kings together is 49 years (3 + 46; or 13 + 36). This is comparable with similar variants (but only of 1 year) of 35 + 33 years varying with 36 + 32 years, for Assur-uballit I plus Adad-nirari I respectively (Brinkman, 1973, 311, n. 29). Thus, we have a possible 10-year margin of error, depending on whether we adopt the two higher figures (13 + 46 years = 59 years total) for Ninurta-apil-ekur and Ashur-dan I, or else treat the variants as cumulative evidence for 49 years for the two reigns combined (13 + 36, versus 3 + 46). So, before 1192/82 and 1179/69 BC, we now have a 10-year margin of error for all earlier Assyrian dates; we will give here both options at this stage.

Going further back in time, there are consistent figures for Ninurta-apil-ekur's precursors back to Enlil-nasir II, from 1431–1425 (or: 1421–1415) in the later 15th century BC.[4] We thus gain maximum and minimum dates for Assur-uballit I as 1364–1328 BC (or: 1354–1318 BC). This result is important, because this king had correspondence with two pharaohs: Akhenaten (Amenophis IV) reigning 16 years, and Tutankhamun at 9 years (thus skipping the very brief reign of Smenkhkare, 1 year of sole rule, before the last-named). So, Akhenaten could not die before 1364/1354 BC, and Tutankhamun could not accede to the throne later than 1328/1318 BC, a point of possible importance below (cf. §13).

§4.

So much for Assyria; what of Babylonia? Here, before 669 BC, we have a continuous series of kings with year-dates back to Nabunasir (Nabonassar) of 748–734 BC, resting upon a combination of Ptolemy's Canon, Mesopotamian king-lists, and other sources. Before him 17 kings are known, but not their reign-lengths, and two more before these, reigning 36 years and 8 months respectively, from the beginning of the "Dynasty of E". Before all that, the Second Dynasty of Isin and three lesser lines (partly contemporary) have figures preserved, but fixed dates BC cannot yet be established. Before this lot, we have the remarkable Kassite Dynasty of "36 kings, (for) 576 years". Here, things are somewhat better, because lengths of reigns can be largely established for kings 18–36, covering the early 14th century down to the mid-12th century BC. Here, we have a double problem: (i) the figures from the king-list are often one year more than the highest year yet found in contemporary economic tablets (so, for Kurigalzu II, Kadashman-Turgu, Kudur-Enlil and Shagarakti-Shuriash), and in one case (Nazi-Maruttash) 2 years more. Thus, if we had a base-line date for the end of the Dynasty, then the king-list-based figure for the period back to the accession of Burnaburiash II would give dates 6 years higher than trusting to the economic texts on their own. This Burnaburiash (within his reign of 27 attested years) corresponded with three pharaohs: Amenophis III, Akhenaten (Amenophis IV) reigning 16 years, and Tutankh-amun at 9 years (thus skipping the very brief reign of Smenkhkare,

[3] Not 1134; and so Tiglath-pileser I acceded in 1115, not 1116 BC; this comes from recognition that Tiglath-pileser II reigned 32 years, not 33, cf. Brinkman 1973, 310.

[4] Assur-nadin-apli's 4 years can occur as 3 (Brinkman 1973, 311, A.2), but the impact of this 1-year variant is minimal, and can be largely set aside. Thus, our result for Assyria before 912 BC comes out as identical with that in the table computed by Brinkman 1976, 31, with 13 years for Ninurta-apil-ekur (1192–1179). At this point, we keep open the 3 years option for that king (1182–1179), which entails merely subtracting 10 years from all preceding Assyrian royal dates, should it prove to be correct.

1 year of sole rule, before the last-named). Which has consequences, if we can date either him or these three kings. Just over 50 years after Burnaburish II there reigned Kadashman-Turgu for either 17 years (contemporary tablets) or 18 (king-list). He at one point volunteered to help Hattusil III by marching with him against Egypt – such hostilities would have been unthinkable, after the Egypto-Hittite treaty in Year 21 of Ramesses II, and therefore predate it; this may be a useful control factor to add to the others.

§5.

But (so far) we do not have a base-line for these Babylonian rulers. Instead, we have to turn to synchronisms with Assyria. From the "Synchronous History", one may derive a long series of synchronisms between pairs of named Assyrian and Babylonian kings; see Rowton 1959, 7, for a list. The dating of the Kassite Dynasty depends on its links with the Assyrian kings, not on the defective dates for Babylonia during c. 1160–748 BC, as clearly pointed out by Brinkman (1976, 29–30, nn. 85, 86). We may use these links at both ends of the second half of that Dynasty, to gain usable dates from c. 1400 to about 1150 BC. First, the earlier phase, with Assur-uballit I at c. 1364–1328 (high dates; low, 1354–1318). He had married-off his daughter to the reigning Babylonian king earlier in life, and they had a son, Kara-hardash. At the death of Burnaburiash II, this son succeeded him, and began pushing back hostile tribes, settling fortresses, etc., when he was suddenly overthrown and killed, a local usurper briefly replacing him until his enraged Assyrian grandfather Assur-uballit I swept down with an army, killed the interloper, and put another prince on the throne, namely Kurigalzu II. Clearly, young Kara-hardash was already old enough to command or send out army-forces, organize public works, etc., at his accession; he was not a babe in arms. The picture would well suit a young man of about 25 or so. If so, he would be born early in the 27-year reign of Burnaburiash II, and his princess-mother sent to that king by an Assur-uballit already reigning, ultimately for 36 years. These figures give us a bracket for the relative dating of the two monarchs, as Burnaburiash II's reign had to have ended within that of Assur-uballit, for the latter to avenge his swiftly-murdered grandson. The difference in reigns is 9 years. At the upper extreme, one might assume that Burnaburiash II ascended his throne about the same time as Assur-uballit, or even shortly earlier, receiving the girl in alliance soon after the latter king's accession. In which case, Kara-hardash could have been born soon after, in about Year 2 of his father's reign, to succeed him 25 years later; that would leave 8 years' minimum interval from his death and due succession of Kurigalzu II (1336 or 1326 at latest) to the death of old Assur-uballit I himself (1328 or 1318). The opposite solution is to place the whole episode only a year or so before the old Assyrian king's own death (1328/1318), and date father and son accordingly. Thus, Burnaburiash II would reign at c. 1364–1337 (or slightly earlier), high dates; or 1354–1327 (or slightly earlier) on low dating, for the higher option. On the lower option, he might be set at c. 1356–1329 (high dates) or c. 1346–1319 (low dates).

We now turn to rulers much later, nearer the Kassite Dynasty's end. Its 32nd king, Adad-shuma-usur (or, -nasir) reigned for 30 years, and so was a contemporary of five Assyrian rulers: (i) the last years of Tukulti-Ninurta I, (ii) 4 years (unless just 3) of Assur-nadin-apli, (iii) 6 years with Assur-nirari III, (iv) 5 years with Enlil-kudurri-usur, and (v) with Ninurta-apil-ekur (who reigned either 13 or 3 years, hence our 10-year variants for the Assyrian dates before him). From the death of Assur-uballit I (1328/1318) down to the accession of Ninurta-apil-ekur totals 10+12+32+30+37+4+6+5 years, *i.e.* some 136 years all told, to which some overlap with the last-named must be added. If we accept the likeliest figures for the Babylonian kings from the accession of Kurigalzu II to the death of the long-lived Adad-shuma-usur (30 years), we have some 146 years in total, as follows. Kurigalzu II, 25 years; Nazi-maruttash, 26 years (King-list; only up to 24 in contemporary tablets so far); Kadashman-Turgu, 18 years (17 in tablets); Kadashman-Enlil II and Kudur-Enlil, 9 each; Shagarakti-Shuriash, 13 years (last one, only days); Kashtiliash IV, 8 years; no separate rule of Tukulti-Ninurta I over all Babylonia, only in certain districts(?); then Enlil-nadin-shum and Kadashman-harbe II, 1 year each; Adad-shuma-iddina 6 years; and finally the 30 years of Adad-shuma-usur; total, 146 years. The 10 years' difference from the Assyrian total (136 years) can then cover (i) some overlap needed between Ninurta-apil-ekur at the end, and (ii) between the accession of Kurigalzu II and the last years of Assur-uballit I at the beginning. With Brinkman, one could split these years evenly, with 5 to give overlap between the first years of Ninura-apil-ekur (1192 ff.) and the last years of Adad-shuma-usur (consequently, 1217–1187). The other 6 years can then push back into Assur-uballit I's reign, to set the reign of

Burnaburiash II at c. 1360–1333 BC. On a 3-year reign of Ninurta-apil-ekur, Assur-uballit I would reign c. 1354–1318, and the Babylonian dates reduce by 10 years, from Burnaburiash II (1350–1323 BC) down to Adad-shuma-usur (c. 1207–1177 BC), the Kassite line ending 1155/1145 BC. But for other possibilities, cf. §17 below.

§6.

Now, we return to Egypt. There is no reason to doubt the date of 664 BC for the commencement of the 26th Dynasty with Psamtek I, after Tantamani of Nubia had slain his (Ps I's) father Necho I, and raised general revolt against Assyria, resulting in Assurbanipal's fierce punitive campaign in 663. The year 664 also saw the death of Tantamani's predecessor Taharqa after a full 26 years of reign, which thus began in 690 BC, a date which should now be given equal status with 664, and to all intents and purposes with 525. In 716, Sargon II took tribute from the east-Delta ruler, Shilkanni of Egypt, who was Osorkon IV, last of the name. Thus, the Nubian 25th Dynasty did not secure 'permanent' control of Egypt until after that incident. Between 716 (*i.e.*, from 715 at earliest), down to Taharqa's accession in 690, we have two kings senior to him: Shabako, with a 14-year reign, and his successor Shebitku (Shabataka) of 3+*x* years; for details, see Kitchen 1996, updates to 1995. Thanks to Sargon II's Tang-i-Var text, we now know that Shebitku was ruler of Nubia by 706 BC, before he succeeded Shabako.[5] The synchronism of Sennacherib's campaign in Palestine against Hezekiah of Judah in 701 BC found the latter backed by "Tirhakah, king of Kush" – NOT of Egypt (as most of us had mistakenly considered). Just as Shebitku had been ruler of Nubia for Shabako, so in turn Taharqa was ruler of Nubia under Shebitku, who evidently sent him to war against Assyria in 701. On the stela Kawa IV, Taharqa tells of Shebitku ordering him to bring an army north to himself in Memphis. To bring an army (not a mere platoon!) of several thousand men most of 2,000 miles from Upper Nubia to Memphis and the Delta is a major undertaking, not done for fun! It could only be done for a war about to be waged. And 701 is the time the only relevant one happened. Thus, we may date Shebitku to 702–690 with 12 years' reign [as in the Eusebian text of Manetho], which neatly accommodates Shabako's 14 years in 716–702 BC, with his taking over Egypt in his Year 2, in 715 BC (date-line of an Apis-burial in Memphis, after slaying Bakenranef, 24th Dynasty), with Osorkon IV also thus disappearing after 716.

§7.

Before 716 and Shabako, we have his predecessor Piye ("Piankhy"). That king invaded Egypt in his 20th year, commemorating this with a stela in Year 21; later regnal dates run up to Year 30. During that invasion, he found Osorkon IV in Bubastis (22nd Dyn.), and Iuput II in Leontopolis (23rd Dyn.); then, Tefnakht was Great Prince of the West, but NOT a king. Thus, Piye's Year 20 was some 10 years before Shabako's accession in 716 (in 726 or before), and preceded the 7/8-year reign of Tefnakht as king, and 5/6-year reign of his son and successor Bakenranef, which is a 12 to 14 years' span, ending in 715 BC (Shabako's conquest of Egypt, with death of Bakenranef), putting Piye's invasion at 715 + 12/14 = 727/729 BC. Let us say 727 BC, as absolute minimum, although 728 would be safer. In 725, So of Egypt in 2 Kings 17:4 (to whom Hoshea of Israel appealed) is also Osorkon IV (despite squawks to the contrary by ignoramuses). Thus, we have 727 (better, 728), 725, 716 as datelines for Osorkon IV.

§8.

Before him, we have the entire 22nd Dynasty back to Shoshenq I in impeccable good order, which I list in almost unrealistically minimal terms at this stage: Shoshenq V, 37 years (up to Yr 38), Pimay, 6 years (up to Year 6; Apis-sequence), (the new) Shoshenq IV plus Shoshenq III, together 52 years (40+12; on Apis sequence), Takeloth II, 25 years (not less; definitely 22nd, not 23rd Dyn.); Osorkon II, up to Year 23, but 24 needed; Takeloth I, 14 years' minimum (15?); Osorkon I, 32 absolute minimum (Year 33 bandages); and Shoshenq I, 21 years. Adding up 37+6+52+25+ 24+14+32+21 = 211 years unrealistically absolute minimum; from 727 = an irreducible and unrealistic bottom date of 727+211 = 938 BC for the accession of Shoshenq I. If we allow 728 as proper date for Piye's invasion, and Osorkon IV reigning

[5] But not then in Egypt! See briefly my remarks (Kitchen 2000 in: M. Bietak (ed.), 2000, 58–59, also Kitchen 2002, 6–7, pointing out that Assyrian *sharru* of foreign potentates did not always mean "king", but any local bigwig; and the same (for Sargon II) is most likely true here; Shebitku did not rule *in Egypt* from 706 BC, only in Nubia.

from 730 BC (and not just 5 minutes before Piye arrived on his doorstep!), Takeloth a 15th year in full, and Osorkon I, 35 years (correcting Manethonic *15), then 1+2+1+3 = 7 years more, bringing Shoshenq I's accession to c. 945, close to maximum. A 50-year range of error, that some have suggested,[6] is absolutely excluded. Alongside the sequence of kings, we have the dated sequence of Apis-bulls, and of a complex series of genealogies of dignitaries in both Thebes and Memphis (full details, Kitchen 1996), which underline the reliability of the royal sequence cited here.

§9.

We also have an Assyrian-backed Near-Eastern synchronism to check upon these Egyptian dates, themselves wholly derived from dead-reckoning on purely Egyptian data, other than the 702/701 synchronism for Shebitku. In the biblical 1 Kings 14:25–26, and supplementary 2 Chronicles 12:3–4, we find that Shishak (better, Shushaq) king of Egypt raided Palestine in the 5th year of Rehoboam, king of Judah. The Assyrian annals of Shalmaneser III note opposition from Ahab of Israel in 853, and the submission of Jehu of Israel in 841 BC. This 12-year span included the 2 + 12 years of two intervening kings, Ahaziah and Jehoram (Ahab's sons; 1 Kings 22:51, and 2 Kings 3:1) = 14 years. But Israel's first king, Jeroboam I, had come from Egypt, where non-accession dating was used; on that basis 2 + 12 nominal years = 1 + 11 full years; that principle ran back all the way to Jeroboam I, an interval of six kings in "84" years = 84–6, or 78 full years, putting the accession of Jeroboam I in 853 + 78 = 931 BC – also the starting-date for his rival, Rehoboam of Judah (931/930); details on these, see Kitchen 2003a, 26–32. The 5th year of Rehoboam (and on the opposing accession-year usage) was consequently in 926/925 BC. As Shoshenq I most likely reigned 945–924 BC (Egyptian non-accession usage), his 20th year =926 BC, in Rehoboam's 5th. On return, in Year 21, Shoshenq I commissioned a massive celebratory building-programme (date, from the Silisila stela), including his great triumphal relief and list of places in Palestine (Kitchen 1997, 118–120, 124–125).

Thus, we can better our absolute minimal date from 938 to 945 for the start of the 22nd Dynasty and end of the 21st, by a mere 7 years, with the indirect Assyrian synchronism, itself the result of straight dead-reckoning, with which an eclipse happens to fit (it would be wrong if it didn't!)

§10.

In turn, the 21st Dynasty goes back beyond 945 BC. As shown long ago, it had 7 kings, *not overlapping* any other dynasty or king (cf. Kitchen 1996, xix–xxii, §§O–T, contra Dodson 1987), with Smendes I at 26 years (cf. stela, Year 25); Amenemnisu, reputedly at 4 years (ephemeral); Psusennes I up to Years 48, 49, but possibly co-regent with his successor, hence only 46 years in Manetho; Amenemope with Years 5 and 10(?), agreeing with Manetho's 9 years; Osochor assigned 6 years (Yr 2 known); Siamun, a Year 17 known, but Manetho <1>9 years; and Psusennes II 13 to 15 years = 123 to 125 years. This total added to 945 would give a range of c. 1068 to 1070 BC, with 1069 as the mean. Let us temporarily adopt the lowest option, 1068 BC.

§11.

We now reach the New Kingdom. The 20th Dynasty on absolutely minimum figures for Setnakht and Ramesses III to Ramesses XI inclusive = 1+31+6+4+7+7+1+18+3+29 years = 107 years basic. The probability is that Setnakht should have 2 years, either Ramesses VI or VII (or both, each) 8 years; R. VIII might merit 3 (Book of Sothis), and R. X 7 or 8 years, if not also R. IX 19 years, giving us potentially 10/11 years more, and 117/118 years total (up to c. 1185/86 BC). But these possibilities we leave aside at present; 1068 + 107 = 1175 BC, for the barest minimal start of the 20th Dynasty and end of the 19th.

For the 19th Dynasty, we have two problems: whether or not the 3 years of Amenmesses are his sole reign, or wholly enveloped within that of Sethos II – a 3-year difference. For Sethos I, the absolute minimum is 10 or 11 years, but 14 or 15 are possible. So, we have 3+5 = 8 years possible range above the minimum. Before 1175, that is 8 years for Siptah + Tewosret, 6 years for Sethos II, 9 years for Merenptah (10 likelier), 66 years for Ramesses II, and 1 full year for Ramesses I. Thus, in chronological order, 1+10+66+9+6+8 = a rock-bottom minimum of 100 years, back to *1275 BC (–11 = *1264 accession

[6] Such as sundry cranks naively cited by Finkelstein 2002, 110. These are refuted by me (Kitchen 2003b, 121–125), with further references.

of Ramesses II, his Year 5 (Qadesh) = *1260, and treaty of Year 21 = *1244 BC).

§12.

Thus a minimal end for the 18th Dynasty would be *1275 BC, on this rock-bottom basis. For Haremhab at 28 years, Ay at 4 years, Tutankhamun at 9 years, Smenkhkare at 1 year and Akhenaten at 16 years, we would have:

Amenophis III	38 years	*1371–1333
Akhenaten	16 years	*1333–1317
Smenkhkare	1 year, solo	*1317–1316
Tutankhamun	9 years	*1316–1307
Ay	4 years	*1307–1303
Haremhab	28 years	*1303–1275

These figures would then put the accession of Tuthmosis III some 90 years earlier at c. *1461, and of the 18th Dynasty (with Tuthmosis II at 3 years) some 61 years before this, at c. *1522 BC for accession of Ahmose I. But these ultra-low, theoretically rock-bottom figures may not allow (i) for essential synchronisms abroad, and also for (ii) more realistic reign-lengths where our total data are insufficiently explicit in terms of full regnal years.

§13.

To those synchronisms, we now turn. Burnaburiash II (1360–1333, or else 1350–1323 BC) had correspondence with Amenophis III, Akhenaten (Amenophis IV) and (by-passing Smenkhkare) with Tutankhamun. On this basis, within his 27 years, the death of Amenophis III must fall no earlier than either 1359/1349 BC, and the accession of Tutankhamun no later than c. 1334/1324 BC; in either case, the intervening 25 years is ample for the 16+1 years of Akhenaten and Smenkhkare, with 8 years leeway. Now in §12 above, we arrived at extreme low dates for these pharaohs. These set the accession of Tutankhamun in *1316 BC, which is 18/8 years too late to match the Mesopotamian data. So, such dates in Egypt would need to be raised accordingly. Tutankhamun would have to accede not later than 1334 or 1324 BC, and reign (9 years) c. 1334–1325 or else 1324–1315 BC. Ay would be 1325–1321 or 1315–1311 BC (4 years); and Haremhab's 28 years in c. 1321–1293 or else 1311–1283 BC. In turn, Ramesses I would come in 1293–1292 BC, or else 1283–1282 BC. Sethos I at 10/15 years would come within 1292–1282/1277 BC, or (low) 1282–1272/1267 BC. This would place Ramesses II's accession at within 1282/77 or 1272/67 BC, respectively. Amusingly, the high date is around our previous 1279 BC (actually, a mean value), the low one barely a decade below it (with 1269 likewise a mean value). Then, the Battle of Qadesh (Year 5) would be within c. 1278/73 (or 1268/63), and the treaty of Year 21 be within c. 1262/57 BC or (low) 1252/47 BC (mean values, at 1275, 1259 BC, *i.e.*, dated as formerly; or (low) 1265, 1249).

§14.

At this point, we turn again to Babylon and also the Hittites. At one crisis of hostility between Hatti and Egypt, when Hattusil III was minded to wage war on Egypt, the Babylonian king Kadashman-Turgu offered his military support (*KBo* I, 1, 10; Oppenheim 1967, 139–146, especially 143–144). At some point *before* the Hittite Treaty, this action makes sense; *after* it, it does not. So, it is fair to suggest that Kadashman-Turgu's help was offered after Hattusil III's accession, and at least a year or two before Year 21 of Ramesses II and the treaty then. Between the Battle of Qadesh in Year 5 and the Treaty in Year 21, we have 16 years which include: (i) death of Muwatallis; (ii) 7 year-reign of Mursil III/Urhi-Tesup; (iii) seizure of power in Hatti by Hattusil III; (iv) Urhi-Tesup's escape to Egypt; (v) Ramesses II's refusal to give him back to Hattusil III; and (vi) change of attitude by both kings, and negotiations to the treaty (about 2 years?). It is possible that Muwatallis did not long survive Qadesh;, dying in Ramesses II's Year 6 or so at earliest. Then Years 7 to 13 at earliest would see out the 7-year reign of Mursil III, and from Year 14 onward, Hattusil III would take over, leaving a maximum of 7 years more for quarrels and then peace-negotiations up to Year 21. On mean dates suggested here, Kadashman-Turgu's 18 years would be c. 1282–1264, or (low) 1272–1254 BC. So, Years 14–18 would be a fitting context for Kadashman-Turgu's offer of military aid, within c. 1282/64 or 1272/54 BC (mean dating). In his Year 18, Ramesses II set up a very bellicose (if vague) stela at Beth-Shan in northern Canaan (translated and annotated, Kitchen 1996a, 27–29, and 1999, 60–63). It is possible that, expecting a Hittite attack, he went forth, but no attack came and he therefore contented himself with this generalized triumphal monument. If the Babylonian offer of aid occurred within Years 14–18 not long before that king's death, say c. 1265 or (low) 1255 BC, then Year 1 of Ramesses II would fall within c. 1282/1278 BC (high) or c. 1272/68 BC (low). The first option compares with 1282/77 and the second with (low) 1272/67 BC of §11 above, in each case very closely.

Any very early dates in the reign of Kadashman-Turgu would progressively go against both the higher and lower dates for Ramesses II and Egypt. So relatively late in the Babylonian's reign is likeliest for his offer to Hatti.

§15.

At this point, we may pull together the results so far. The high and low options of 49 and 59 years of combined reigns for Ninurta-apil-ekur and Ashur-dan I of Assyria lead to matching high and low options just 10 years apart for all the Assyrian and Kassite Babylonian kings that precede him in time, and in turn (through synchronisms) for Egypt's l8th and 19th Dynasties. Before Tutankhamun (acc. 1334/1324), the basic known reigns of the 18th Dynasty would take us back to c. 1351/41 BC for accession of Akhenaten, to c. 1389/79 BC for that of Amenophis III, and to c. 1477/67 BC for the accession of Tuthmosis III. In turn, with 13 or 3 years for Tuthmosis II, the 18th Dynasty and Ahmose I would begin at c. 1548/38 (Th II, 13), or c. 1538/28 (Th. II, 3 years).

§16.

The frank truth of the matter, therefore, is that with 1477 we are so near the old 1479 date that we should keep it for convenience (giving us a tidy 1550/40 for Ahmose I), with the consequential dates that follow on from it, all the way down to c. 1070 BC, as a "high date" series, to avoid massive confusion adopted merely for the sake of needless novelty. As a current alternative, a corresponding "low date" series can be set up, just 10 years lower, from c. 1540/30 BC (Ahmose I), c. 1469 BC (Tuthmosis III), c. 1381 BC (Amenophis III), c. 1326 BC (Tutankhamun), and so on to 1269–1203 BC for Ramesses II, and on down to c. 1070 BC for the 21st Dynasty as usual; deleting any slack would come from eliminating Amenmesses' sole reign (into that of Sethos II) at 3 years, the 4 years added to Ramesses X, the 2 extra years for R. VIII, and the odd extra year for R. IX. On that low-date basis, Merenptah = 1203–1193; Sethos II (incl. Amenmesses), 1193–1187; Siptah-Tewosret, 1187–1179; Setnakht, 1179–1177; Ramesses III, 1177–1146 (Year 8, c. 1170); R IV, V, 1146–40, and 1140–36; R. VI, 1136–1129; and R VII–XI at 59 years (8+1+18+3+29), c. 1129–1070 BC. Thereby, we can offer two alternative sets of dates, until the anomaly over two Assyrian kings (49/59 years) can finally be ironed out.

§17.

But let us go back to Ramesses II and Kadashman-Turgu of Babylon for a moment. If Year 18 of Ramesses II's Beth-Shan stela was indeed a mark of crisis, a monument to an aborted Hittite advance (without Babylonian help, which had been refused), to which Ramesses II had initially responded by marching forces up to Beth-Shan, then we may date the Babylonian offer of help at about Year 17 at earliest. On our high dates (1282/78) and previous equivalent (1279), Year 17 would fall within c. 1266/62 BC, or previous equivalent, in 1263 BC. This all falls very close indeed to the supposed 1264 date for the death of Kadashman-Turgu, in fact straddling it. The same applies to the low-date scheme, 10 years later, with Year 17 within 1256/51 (from accession within 1272/67) or at 1252 (if accession in 1269) and Kadashman-Turgu in 1272–1254. As any date during 1264–62 (high) or 1254–52 (low) would be 2+ years too low for this synchronism, one may enact one or other of three options.

Option (i) is to raise the date of Year 17 of Ramesses II to at least 1265/1255 BC (= 1281/1271 for accession), if we keep the link with Beth-Shan. Option (ii) is to lower the dates of Kadashman-Turgu by, say, 3 years, to c. 1279–1261/1269–1251 BC. Then, Kassite dates before him would be lowered accordingly, setting Burnaburiash II, in c. 1357–1330 BC, dying just a couple of years before Assur-uballit I, whose avenging campaign would itself then come just that 2 years before his decease, and may even have hastened it, through its stress for an ageing ruler. Option (iii) is to sever any direct link between the dateline of the purely rhetorical Beth-Shan stela and the timing of Kadashman-Turgu's offer of anti-Egyptian help to Hattusil III. As pointed out above, Hattusil III could have succeeded Mursil III/Urhi-Tesup at as little as 8 years' interval after the Battle of Qadesh, by Year 14 or 15 of Ramesses II onwards. This could date the help-offer (on high dates) to within (1282/78 – 13/14) = 1269/65–64; or else (on low dates) within (1272/68 – 13/14) = 1259/55–54 BC. On 1279/1269 accession-dates for Ramesses II, this would be in 1266/65 or else 1256/55 BC, in each case safely within the high or low dates for Kadashman-Turgu (1282–1264/1272/54 BC). In such a case, the Year 18 rhetorical stela would have been erected in Beth-Shan for reasons still unknown to us (and likely to remain so, just as with the Year 56 stela from near Damascus).

On balance, it seems (to me at least) wisest to go for option (iii), dating the accession of Hattusil III to Year 14 or 15 of Ramesses II (acceding at c. 1279/1269), and thus keep the link between the Hittite king and his Babylonian ally within the mean figures for the Kassite Dynasty, and before Assur-ubal-

lit I got any too old for even vengeful campaigning. Bluntly, there is no point in going for purely theoretical, currently unverifiable and needless minor novelty just for the fun of it. Far better to await the discovery of some *real* and fresh data, either to confirm or amend these present dates, seeing that they still fit the case overall.

§18.

That being the case, and with either 1550/40 or else 1540/30 BC for the beginning of Egypt's 18th Dynasty *nach wie vor,* and without any scientific aids invoked so far, we need in closing to tackle the preceding era from the 11th and 12th Dynasties through the Second Intermediate Period. Now, long since (1987 and 1996/2000), I pointed out very clearly that one can make a very good case for estimating the length of the Second Intermediate Period (13th to 17th Dynasties) by establishing the basic succession of kings who had actually ruled over Thebes during that period. Using existing, known reign-lengths, plus analogous lengths of reign for kings of similar status, it was possible to offer a figure of some 240/250 years for the period between the 12th Dynasty and the accession of Ahmose I and the 18th Dynasty in Thebes. Here, the 240 is a decidedly minimal figure, and 250 may prove to be closer to the truth, eventually. Therefore, we may (on high datings) offer 1550/40 + 240/250 = either 1800/1790 or 1790/1780 BC for the end of the 12th Dynasty – the mean would be 1790 BC, and that Dynasty's 187 years would go back to a mean date of c. 1977 BC, with top limit at c. *1987 BC, and bottom limit of c. *1967 BC. This would give a mean of c. 2120 BC for the start of the 11th Dynasty (between upper and lower extremes of *2130 and *2110 BC). On the lower datings, 1540/30 + 240/250 would correspondingly give 1790/1780 or 1780/1770, end of 12th Dynasty, with a mean at 1780; then 187 years back to its start as *1967, between extremes of *1977 and *1957 BC. Then, for the 11th Dynasty (143 years), a mean starting point would be *2110 BC, with extremes at *2120 and *2100. Using these purely textual/archaeological data, it is piquant to note how little their results vary from the former dates of c. 2116–1973 BC for the 11th Dynasty, then c. 1973–1795 BC for the 12th Dynasty, and 1795–1550/40 and 1540/30, for the Second Intermediate Period, when Sothic and lunar dates were still in fashion. The most those could ever do was to provide "fine tuning", but clearly we can well survive without this, even though it might be a desirable aim if really reliable means were established to permit it. But not yet!

Finally, the Archaic Period, Old Kingdom and First Intermediate period. Frankly, with almost the same general dating-horizon as previously – there is no significant difference, between *2130/10, *2116, and *2110/00! – there is simply no point in suggesting any theoretical, overall changes in the dates already suggested a decade ago, and printed only three years ago (Kitchen, in: Bietak 2000, 48–51). So here, we may fitly conclude.

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An Egyptian Chronology for Dynasties XIII to XXV

Rolf Krauss[*]

Methodological remarks

Dead reckoning, checked by synchronisms, and by lunar and Sirius dates, is the standard procedure in Egyptian chronology. Egyptologists like Parker and his precursor Borchardt analysed pharaonic lunar and Sirius dates, utilizing the astronomical tables of P.V. Neugebauer. Both more often missed the historically correct chronology than hit it, since, for example, they did not realize how difficult it is to interpret recorded Sirius dates. Nowadays, experts do not agree about whether a pharaonic Sirius date refers to an actually observed heliacal rising or a schematically reckoned one. Another contentious issue is the geographical reference for the rising of the star: Memphis, Thebes or Aswan. The variability of these two factors alone can result in a difference of about 30 years for a Middle or New Kingdom Sirius date.

Specialists are also at odds concerning the value for the arcus visionis, the negative height of the sun at the moment when Sirius is at the horizon. On the basis of observations in Egypt Neugebauer arrived at an arcus visionis of 9°.[1] Decades later, in 1993, Pachner was able to correct the value slightly to 8.85° ± 0.15°, on the basis of new observations in Middle Europe.[2] Recently Schaefer, disregarding the observational values of 1929/1993, reckoned with an arcus visionis of 11° in accordance with his theory of visibility of stars during twilight.[3] This mandates a shift of a pharaonic Sirius date by +8 years over against those using the values of Neugebauer/Pachner. The possibility of an arcus visionis as low as 8.4°,[4] which would shift a pharaonic Sirius date by –4 years, was derived by Aubourg from the 1926/1993 observations. To avoid the difficulties inherent in the recent and earlier discussion of Sirius dates, I intend here to establish an Egyptian chronology based on lunar dates, i.e. without reference to traditional Sirius dates.[5]

According to Parker, the Egyptians reckoned the lunar month from the first calendar day of the moon's invisibility, i.e. the day after old or last crescent;[6] the calendar day began at dawn before sun rise.[7] A complete Egyptian lunar date is a double date consisting of a civil calendar date and a day in the lunar month. For example, the date of the Battle of Megiddo in the reign of Thutmose III is recorded as "Regnal year 23, first [month of the] Shemu [season], calendar day 21, day of pesedjentyw [1st day of the lunar month], exactly."[8]

Spalinger notes that "there are myriad challenges involved in sighting the moon";[9] doubtless his remarks are not based on myriad lunar observations. Any observer using the naked eye alone could recognize the first day of invisibility with certainty only when he had been able to see the crescent on the previous morning. Mistaken observations can occur on either day. Thus Egyptian lunar dates exhibit random qualities and therefore analysis of them mandates consideration of probabilities. I am aware that others have erred by neglecting to pay "close attention to the wording of all reasonings on questions of probability,"[10] yet I dare to analyse recorded Egyptian lunar dates in a probabilistic manner below. Parker asserted that "an Egyptian lunar date as given in the civil calendar and as calculated by modern tables may lack agreement by a day ... due to faulty observation or any other reason".[11] Did Parker mean that there is a 50% chance that a lunar date is correct or that it is more probable that a lunar date is correct than wrong? Until recently the accuracy of Egyptian lunar observation was thought to be the same as

[*] Museum für Vor- und Frühgeschichte, Berlin.

[1] Neugebauer 1926, 445f; Neugebauer 1929, 59.

[2] Pachner 1998, 133.

[3] Schaefer 2000a, 151.

[4] Aubourg 2000, 41f.

[5] For the time being see Krauss 2003a, 182–190, and Excursus 1 below.

[6] Parker 1950, 9–13.

[7] Krauss 2003a, 193–195; Krauss 2004, in press.

[8] The tag '*r-mtr* (exactly)' is documented only in this date.

[9] Spalinger 2002, 381.

[10] Herschel 1866, 496.

[11] Parker 1958, 211f.

lunar observation in Babylonia. In both cultures observations were made under similar meteorological conditions and by experienced observers.[12] The accuracy of Babylonian lunar observation was investigated by Schoch and Huber who concluded that at least 85% of the recorded lunar dates represent correct new crescent observations.[13] Fatoohi *et alii* have surmised that Schoch might have used not only observed crescents, but predicted ones as well;[14] Huber's data consist of observed crescents only.[15] Both relied on the same slightly outdated astronomical parameters and on Neugebauer's crescent visibility values, which ought to have resulted in some incorrect determinations of new crescents in both lists. Thus the 85% probability that a recorded Babylonian lunar date is correct, and by implication an Egyptian lunar date as well, is not really sound.

No modern series of observations provides a basis for extrapolating the quality of professional ancient Egyptian old crescent observations. Minimal criteria include a) consecutive old crescent observations over a period of at least one year; b) the observers ought to be the same experienced persons to simulate the situation of professional ancient observers; c) observations should be made at the same latitude and/or at least under comparable meteorological conditions as at Thebes and Illahun (the sites furnishing practically all pharaonic lunar dates). Recently Doggett and Schaefer organised 'moon watches' in North America to establish the Lunar Date Line for specific months.[16] Such moon watches do not constitute a model for old or new crescent observations by professional observers in ancient Egypt or Babylonia. In particular, the quotas for positive and negative errors are inapplicable. "A positive error occurs when an observer mistakenly claims to see the crescent; a negative error is when an observer mistakenly does not see the Moon."[17] The moon watches yielded one example of a positive error: 20 observers had no possibility of seeing the new crescent, because they were outside the zone of visibility, yet 3 apparently inexperienced observers reported sighting the moon.[18] To account for this error, Doggett and Schaefer proposed that "there are many objects in the sky (e.g., whisps of cloud or aircraft illuminated by the Sun) that can be taken for a crescent."[19] My own observations lead me to reject this explanation. Nor have I ever heard of an experienced observer believing he saw old crescent (sic) when it was not visible. Admittedly "honest observers may make honest mistakes," but it seems more likely to me that the three moon watchers were not honest. Regardless, the moon watches did not yield a rate of positive errors made by *experienced* observers. The ratio of negative errors among the moon watches is also not binding, as the commentary of Doggett and Schaefer implies. They report error rates of 1% and 2%, but "suspect the rate of negative error is greater (and probably much greater) than 1%."[20]

In the absence of adequate observational data I have attempted to establish the rate of error in observing old lunar crescent and first day of lunar invisibility from a theoretical model. By definition, an ancient lunar date is 'correct' if it is confirmed by modern astronomical computation.[21] Since the early 20th century old crescent visibility, as the lunar phase which is used today to determine such 'correct' lunar dates, has been calculated within the azimuth/altitude diagram introduced by Fotheringham:[22] For the moon to be considered visible it must have a minimal altitude (without parallax) which is dependent on the distance in azimuth of the sun and moon at the moment when the sun is in the mathematical horizon.[23] Especially Neugebauer improved upon the minimal altitudes of Fotheringham. Astronomers, Egyptologists and specialists in other disciplines have used Neugebauer's visibility criteria since the publication of his *Astronomische Chronologie* in 1929.[24]

In the late 1980's Schaefer developed a model for reckoning old/new lunar crescents which took the following factors into consideration: the physiology of the human eye, the brightness of the twilight sky, the surface brightness of the moon, the extinction in the atmosphere and the local observing conditions. From this model the minimal old/new crescent altitude h, relating to the azimuth-difference between sun and

[12] For Egypt see Fissolo 2001, 15ff.
[13] Krauss 1986, 26.
[14] Fatoohi 1999, 64.
[15] Huber 1982, 25ff.
[16] Doggett and Schaefer 1994, 402; Schaefer 1996, 759f.
[17] Doggett and Schaefer 1994, 397.
[18] Doggett and Schaefer 1994, 397, 402.
[19] Doggett and Schaefer 1994, 398.
[20] Doggett and Schaefer 1994, 397.
[21] All astronomical calculations for this article were made with UraniaStar 1.1; for the program's reliability, see Firneis 2003, 48.
[22] Fotheringham 1910, 530 f; cf. Krauss 2003b, 53f.
[23] For a topocentric model see <http://www.saao.ac.za/~wgssa/as5/caldwell.html>.
[24] Neugebauer 1929, 79–82.

moon, can be determined, together with its mean deviation σ.[25] In Schaefer's model, the term h ± σ defines a zone of uncertainty. If the minimal altitude h of any computed old/new crescent falls within h ± σ, then visibility or invisibility of the crescent depends on extinction at the time of observation, a factor that cannot be predicted exactly.

According to Schaefer, there is a general shift from clear skies in winter to hazy skies in summer – in other words, in the northern hemisphere the minimal altitude is lower in winter and higher in summer. This rule does not seem to apply for all regions, but it is valid for Egypt.[26] Schaefer's and Neugebauer's minimal altitude values coincide in March and September, but they diverge for the other periods. According to Schaefer, low old crescents from October to February which Neugebauer thought might not be visible, might have been visible; conversely, low crescents from April to August which Neugebauer thought could be visible, might not be, according to Schaefer. Especially March through June and, to a lesser extent, in October it must be remembered that a sandstorm (*khamsin*) can affect extinction dramatically and thus the evaluation of recorded lunar dates.

According to a test made by Doggett and Schaefer, the latter's method is optimal: "The altitude/azimuth criteria by Fotheringham, Maunder, Ilyas, and Yallop proved to be reasonably accurate, with the particular implementation by Yallop being better than the other three. The model by Schaefer yielded significantly better predictions than any other algorithm."[27] Down to the present this claim has not been challenged. However, the possibility to check Schaefer's algorithm is limited since his software program LunarCal is no longer accessible.[28] Regardless Schaefer's model represents an improvement over Fotheringham-Neugebauer and so I have adopted his values h ± σ, despite some reservation, for determining whether recorded Egyptian lunar dates are correct or not. I reckon with three possible sources of error in Egyptian old crescent observation: a) the observer himself; b) observational difficulties when the crescent is in the zone of uncertainty; c) observational difficulties resulting from unfavorable weather conditions, i.e. haziness or overcast skies due to clouds or sandstorm.

A professional observer can err as far as recording the date is concerned. The tendency seems to be to give the date of the previous day, rather than that of the actual observation, which results in a negatively incorrect lunar date for old crescent observations.[29] Schaefer cites 7 such errors among 201 dated lunar observations,[30] but *none* for the day following the date of observation. For the present, I shall reckon with a 3% quota of negatively incorrect Egyptian lunar dates on this basis.

I cannot quantify how often a professional observer succumbs to the temptation to falsify data. Even when an observer conscientiously does his job, the observation can be hindered or rendered impossible. If extinction is high or the sky is overcast, the observation of old crescent may be impossible. If the sky is overcast, the invisibility of the moon on the day after old crescent cannot be determined. It is possible to quantify what conjectures an observer who stays in bed might make. If the sky is overcast on one day (e.g. on the day of old crescent), then it is likely that the sky will also be overcast on the following day.[31] I am unaware of any detailed statistics for cloudy days in the Nile valley; there are only statistics available for the average monthly level of cloudiness.[32] For the time being, the following commentary must suffice. If the yearly mean cloudiness is p %, then the sky is completely overcast at most on p % of 365 days. The maximum mean cloudiness in Upper and Middle Egypt occurs in December; during the summer cloudiness is minimal. At the entrance to the Fayum, and thus in Illahun, the yearly mean cloudiness is about 20.5%, or at most 75 days of totally overcast sky. Ideally, there could be 75 successive overcast days in winter, resulting in the impossibility of observing 2.5 old crescents.

If, towards the end of the lunar month on an overcast day, the observer presumes that the moon is visible or not, then there are in fact six possible situtations. If it is astronomically the 29th day of the lunar month, then any assumption concerning the presence

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[25] I owe specific numerical values for h ± σ and the permission to use them in publications to a generous personal communication from Schaefer in November 1999; see also Krauss 2003a, 190–192.

[26] Shaltout 2001, 631–634; and personal communication from January 17, 2004.

[27] Doggett and Schaefer 1994, 402.

[28] See the slightly unfavorable review of Mosley 1990, 228.

[29] The result is a positively incorrect lunar date in the case of new crescent observation.

[30] Schaefer 1988, 512f.

[31] Schaefer 1992, S37.

[32] Griffiths 1972, 84–85, 126–128.

of the crescent is (a) correct, the opposite assumption (b) negatively incorrect. If it is a 30th day of the lunar month, then the assumption concerning the presence of the crescent is (a) correct, and the opposite assumption (b) negatively incorrect. If it is not the 30th but rather already the 1st day of the next lunar month, then the assumption that the crescent is present is (c) positively incorrect and the opposite assumption (d) correct. Consequently there are 6 possibilities: three correct, two negatively incorrect, and a single positively incorrect. If, as presumed, poor weather obtains in 20.5% of all cases, then the observer who stays in bed on these days will guess altogether 10.25% correct dates, 6.8% negatively incorrect and 3.4% positively incorrect dates.

To quantify errors resulting from variations in extinction, I calculated about 150 old crescents for the years between +2001 and +2013 at the latitude of Illahun. About 84% of them ought to have been visible without difficulty, whereas about 16% would have been situated within the zone of uncertainty. Because high and low extinction are equally possible, I expect that half of the uncertain cases or 8% will be correct sightings, and 8% negative errors; positive errors do not arise. In 290 days (i.e. 365 minus 75 days) or about 79.5% of all cases with good weather there would be 73.1% = 0.795 × (84% + 8%) correctly observable old crescents and thus correct first lunar month days. The result for negative sightings or minus one day falsely begun lunar months is about 6.4% = 0.795 × 8%. If I have made no gross error, then there would be in all 80.35% (= 73.1% + 10.25% – 3%) correct lunar dates and furthermore 16.2% (= 3% + 6.8% + 6.4%) negatively incorrect and 3.4% positively incorrect dates. Thus I suggest replacing Parker's general statement about an Egyptian lunar date being possibly wrong by one day, with the assertion that the probability p of an Egyptian lunar date being correct is p = 0.81; the probability that a lunar date is negatively incorrect is about 0.16 and that it is positively incorrect about 0.03. These values are to be understood as mean values which lie between those for Upper and Lower Egypt. With the aid of these probabilities I intend to determine which equivalents in absolute chronological terms are astronomically and historically correct for recorded lunar dates. There would be many different possible dates, if presumption of Egyptologists that a particular lunar day corresponds to the very same day in the civil calendar every 25 Egyptian years were correct.[33] This notion is erroneous; rather, for astronomical reasons there is only a 70% probability that an Egyptian lunar date repeats after a single 25 year shift (see Excursus 2).

An example are five lunar dates preserved from Dynasties 18 and 19: the Megiddo and Karnak dates, Piramesses date, and two feast-of-the-valley dates from 7 Tewosre and 7 Ramesses III. For these dates there are two possible correlations with the first years of the reign of Thutmose III and Ramesses II (see Table 10). If the first regnal years coincide with 1479/1279 BC, then 4 of the lunar dates are correct and only one is a day early (represented here by cccc-). If the first years coincide with 1504/1304 BC, then two of the five dates are correct while two fall a day too early and one a day too late (cc– –+). The comparative probabilities of cccc– and cc– –+ can be calculated assuming that about 3700 first lunar days occurred between the Megiddo date and the feast-of-the-valley date of 7 Ramesses III ; of these 3700 lunar dates 2997 (i.e. 81%) may be considered correct, 592 (i.e. 16%) negatively incorrect and 111 (i.e. 3%) positively incorrect. The probability P is defined as the ratio "number of favorable chances" ÷ "whole number of chances". In the example, the whole number of chances is the combination of 3700 lunar dates choose 5, i.e. C (3700,5) = (3700! ÷ 5!) × 3695! = 5.763 ×10^{15}. The number of favorable chances in the form cccc– is the product C(2997,4) × C(592,1) = 1.986 × 10^{15}, yielding $P_{cccc–}$ = (1.986 × 10^{15}) ÷ (5.763 × 10^{15}) = 0.344, i.e. the probability that of five lunar dates exactly four are correct and exactly one is negatively incorrect. The number of favorable chances in the form cc––+ is the product C(2997,2) × C(592,2) × C(111,1) = 6.681 × 10^{13}, yielding $P_{cc––+}$ = (6.681 × 10^{13}) ÷ (5.763 × 10^{13}) = 0.0151, i.e. the probability that of five lunar dates exactly two are correct, two negatively incorrect and one positively incorrect. Accordingly, cccc– is approximately 23 times more probable than cc––+. Obviously, cccc– is much more likely to be historically correct than cc– –+. An alternative would be the use of the trinomial formula:[34]

$$P_{x,y,z} = (n! \div x! \times y! \times z!) \times 0.81^x \times 0.16^y \times 0.03^z$$

x is the number of correct lunar dates, y those nega-

[33] E.g. Kitchen 1991, 204: "These moon-risings occur in the ancient calendar every twenty-five years."

[34] For this suggestion I am indebted to Sylvie Roelly, Lehrstuhl für Wahrscheinlichkeitstheorie, Institut für Mathematik, Universität Potsdam, who read and commented on a preliminary draft of this ms.

tively incorrect and z the positively incorrect ones, with n = x + y + z. Accordingly, the probability of cccc– works out to be:

$P_{4,1,0}$= (5! ÷ 4! × 1! × 0!) × 0.81^4 × 0.16^1 × 0.03^0 = 0.344

Dynasties XXII to XXV

The reign of Psammetichus I began in February 664 BC and the reign of Taharqo in 690 BC. According to the Tang-i Var inscription, Shebitqo ruled from at least 707/706 BC.[35] The highest attested date for Shabaqo is year 15. Thus 1 Shabaqo corresponds to 722/721 BC at the latest. Shabaqo defeated Bocchoris as ruler of Memphis.

According to Vercoutter, a graffito in the burial chamber of the Apis that was buried in 6 Bocchoris mentions year 2 of Shabaqo. He states "the underground vaults of the Serapeum were opened only for the 70 days during which the body of Apis was being embalmed and for the actual burial."[36] The corresponding equation 6 Bocchoris = 2 Shabaqo has become the *communis opinio*.[37] In fact, Vercoutter paraphrased Boreux who wrote: "Pausanias nous apprend que l'intérieur du Sérapeum n'était ouvert que pendant les soixante-dix jours qui s'écoulent entre la mort d'un Hapi et son ensevelissement."[38] Pausanias however did not mention a 70-day period; rather, he noted that "neither foreigners nor even priests can enter there [Serapeum] before they bury Apis."[39] Whatever Pausanias reported in the 2nd century AD concerning contemporaneous practices at the Serapeum, is not pertinent for the 8th century BC. Furthermore Vercoutter's information about the Shabaqo graffito is incorrect. Actually Mariette wrote: "Une petite stèle grossièrement écrite à l'encre noire nous apprend qu'un Apis est mort l'an 2 de ce dernier prince [Shabaqo]. J'ai copié dans la chambre où la stèle précédente a été trouvée la fin d'une légende royale dont ce fragment de cartouche <///*kꜣw*> était lisible. Je n'ai pas osé, sur un document si incomplet, attribuer un Apis au règne de l'Éthiopien Schabatoka [*Djedkꜣwre*], successeur de Sabacon".[40] The chamber where Mariette found the stela mentioning a second year of Shabaqo is apparently not identical with that housing the burial of the Bocchoris-Apis: "Il est à noter d'ailleurs que l'Apis mort l'an 37 de Scheschonk IV [V]… et l'apis mort l'an 6 de Bocchoris, … furent ensevelis dans la même chambre, et que l'étude de la tombe prouve que ces deux Apis occupèrent successivement et sans intermédiare l'étable sacrée de Memphis."[41] Subsequently Mariette speaks of "... la porte même de la chambre sur les parois de laquelle les légendes de Bocchoris et de Scheschonk IV [V] ont été tracées."[42] These remarks cannot be reconciled with Mariette's plan of the Serapeum which Vercoutter published, which shows the Bocchoris-Apis (XXXIV) and that buried in 2 Shabaqo (XXXV) together in chamber S, while the Apis of 37 Sheshonq V (XXXIII) is in chamber R.[43] It would seem that Vercoutter overlooked this contradiction. Clearly, his reconstruction of the succession of the Apis bulls must be reconsidered, but for the time being it is preferable to date 6 Bocchoris earlier than 2 Shabaqo = 723/722 BC.

The Bocchoris Apis was the successor of the Apis that died in 37 Shoshenq V. According to the data concerning the three Apis bulls buried between 28 Shoshenq III and 37 Shoshenq V, and presuming that Pami's reign ended in a year 7,[44] 95 years lie between 1 Shoshenq III and 37 Shoshenq V (inclusive). If the Bocchoris-Apis was born very soon after the death of its predecessor[45] and had a life span of 26 years at most (the maximum life span attested),[46] the highest possible date for 1 Shoshenq III is 723/722 BC + 26 + 95 = 844/843 BC. This limit would need to be adjusted upwards if Shebitqo's reign began before 707/706 and/or Shabaqo occupied the throne longer than 15 years.

When reckoning the lower limit, it must be taken into consideration that Shepsesre Tefnakhte may or may not have ruled in Memphis as predecessor of Bocchoris for at least 7 full years.[47]

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[35] Frame 1999, 52–54.

[36] Vercoutter 1960, 66 n. 28.

[37] Vercoutter 1960, 65f; Kitchen 1973, 141 n. 247; Leclant 1984, 505 n. 9; Depuydt 1994, 23; Beckerath 1997, 91.

[38] Boreux 1932, 171.

[39] Pausanias I 18.4; see Levi 1971, 50.

[40] Mariette 1857, 26; Mariette 1904, 228. The present location of the stela is not known.

[41] Mariette 1857, 24; 1904, 215.

[42] Mariette 1857, 24; 1904, 223.

[43] Vercoutter 1960, fig. 1.

[44] Bickel 1998, 40.

[45] Vercoutter 1960, 64; Vercoutter 1958, 339–341.

[46] Vercoutter 1958, 340–342.

[47] Kitchen 1973, 141f; Beckerath 1997, 93, does not take a stand on this issue.

Tefnakhte's initial take-over of Memphis occurred at the earliest in the course of 38 Shoshenq V.[48] In his 20th (?) year Piye drove a non-royal Tefnakhte out of Memphis;[49] subsequently Shepsesre Tefnakhte and Bocchoris may have ruled for at least full 12 years (7+5) until the death of the Bocchoris Apis. Accordingly, the lower limit for Shoshenq III would be ca. 722 BC + 12 + 2 (?) + 95 = 831 BC or 722 BC + 5 + 2 (?) +95 = 824 BC, if Shepsesre Tefnakhte did not rule as king in Memphis.

Aston's conclusion that the rival kings Takelot II and Pedubaste I ruled Thebes when Shoshenq III reigned in Lower Egypt[50] seems self evident and has been widely accepted.[51] The following synchronisms are deducible or attested: 5 Takelot II = 1 Shoshenq III[52] and 5 Pedubaste I = 12 [Shoshenq III].[53] The resulting upper and lower limits are: 1 Shoshenq III : 844/824 BC, 1 Takelot II : 848/828 BC, 1 Pedubaste I : 833/813 BC. More specific dates can be derived by using the information provided by lunar feast dates. The five-day *Tepi Shemu* feast began at Karnak on lunar day 1. It is documented from the MK to the Saite Period; Parker analyzed an occurrence dated to 14 Psammetichus I.[54] According to Vernus and Kruchten the inductions of priests took place during the *Tepi Shemu* feast. Bubastide examples of the *Tepi Shemu feast[55] and/or of inductions are as follows:[56]

*(A)	11 Takelot II:	I Shemu 11	= lunar day 1 to 5
(B)	7 Pedubaste I:	I Shemu [1]	= ditto
(C)	8 Pedubaste I:	I Shemu 19	= ditto
*(D)	39 Shoshenq III:	I Shemu 26	= ditto

Assuming that 11 Takelot II fell between ca. 838 and 818 BC, table 1 contains the lunar days which correspond to feast date A within that interval. Here and below, lunar days are counted forward as positive from lunar day 1 to 15; starting with the last lunar day, whether day 30 or 29, the lunar days are counted backward as negative down to lunar day 16. Since a lunar date can occasionally be wrong by minus one day and less often by plus one day, the lunar *Tepi Shemu* feast may have been celebrated in a given year on lunar days –1, 1, 2, 3 and 4 or on lunar days 2, 3, 4, 5 and 6, instead of the intended lunar days 1, 2, 3, 4 and 5. In general, we do not expect a lunar date to be off by more than a single day; accordingly no alternative with feast days earlier than lunar day –1 and later than lunar day 6 is acceptable. Between 838 and 818 BC these premises yield six possibilities for feast date A corresponding to a lunar day (LD) –1 to 6 (underlined).

11 Tak II	*LD*	*11 Tak II*	*LD*
838	–3	827	–5
837	8	826	7
836	–12	825	–11
<u>835</u>	<u>–1</u>	<u>824</u>	<u>–1</u>
834	11	823	10
833	–9	822	21
<u>832</u>	<u>3</u>	<u>821</u>	<u>2</u>
831	14	820	12
830	–6	819	22
<u>829</u>	<u>5</u>	<u>818</u>	<u>4</u>
828	15		

Table 1

Table 2 contains the acceptable and the nearest unacceptable possibilities which result when the lunar day equivalents of the feast dates of Shoshenq III and Pedubaste I are also computed and correlated with those of Takelot II within the limits for their first regnal years.[57]

	lunar day equivalents for dates			
1 Tak II	*A*	*B*	*C*	*D*
845	–1	5	4	1
842	3	7	7/8	3
839	5	11	10	6
834	–1	5	4	–1
831	2	7	6	3
828	4	9	9	5

Table 2

There are only two acceptable alternatives. Either 1 Takelot II corresponds to 845 or to 834 BC. In 834 BC two of four lunar dates would have been incorrect by minus one day (cc—), but in 845 BC there is only one error of this kind (ccc–); Table 3 lists the corresponding probabilities.

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[48] For year 38, see Beckerath 1995, 95.

[49] Piye ruled at least until a year 24. A year 30 (or 40) is by no means certain, see Kitchen 1973, 152 n. 292.

[50] Aston 1975, 139 ff.

[51] Except by Kitchen 1995, XXIII–XXV; 1996, 2.

[52] Beckerath 1995, 9f.

[53] Beckerath 1966, 51; Beckerath 1995, 9f.

[54] Parker 1962, 7f.

[55] Vernus 1975, 24; Kruchten 1989, 244 n. 3.

[56] Legrain 1913, 130: A; Kruchten 1989, 239f: B–D.

[57] There is a single case in which it cannot be decided whether a given date corresponds to a lunar day 7 or 8.

combination	*probability*
cccc	0.430
ccc–	0.340
cc– –	0.100
c– – –	0.013
– – – –	0.0006

Table 3

It is more probable that only exactly 1, instead of exactly 2, out of 4 lunar dates is incorrect by minus one day. Therefore 1 Takelot II is more probably 845 BC, than 834 BC and 1 Shoshenq III = 841 BC. Fragment 5b of the Karnak Priestly Annals allows an extension of the argument, for if I Shemu [1] of 14 Osorkon II is interpreted as a date of the *Tepi Shemu* feast,[58] then 1 Osorkon II = 872 BC[59] and *31 Osorkon II = 842 BC.[60] Current conventional estimates are slightly different. For example, Beckerath equates 1 Shoshenq III to ca. 837 BC.[61]

Dynasty XXI

According to Vernus, two of the priestly inductions known from Dynasty XXI occurred during the lunar *Tepi Shemu* feast:[62] (a) 2 Akheperre setepenre (Osochor/Osorkon) : I Shemu 20, induction of a father; (b) 17 Siamun : I Shemu [1], induction of a son. Assuming that the two inductions would have been separated by 20 to 30 years, i.e. a generation,[63] at least 21 years separate the dates, provided that both correspond to lunar days 1 to 5; other possibilities are 22, 24 and 32 years. The distance of 21 years is methodologically preferable, because it results in 6 regnal years for Akheperre Osochor as recorded in the Manethonian tradition. Upper and lower limits for the first years of Akheperre Osochor and Siamun, can be established by dead reckoning:

0 y Shoshenq II (predecessor of Osorkon II)
14 y Takelot I
33 y Osorkon I
21 y Shoshenq I
*19 y Psusennes II
17[64] (*19) y Siamun
6 y Akheperre Osochor
sum = 112 (110) years

It is likely that year 19 of Psusennes [II] is mentioned in the text of the Dakhleh stela from 5 Shoshenq [I]. The stela was originally attributed to Shoshenq I,[65] but susequently to Shoshenq III on the basis of the association of the title Pharao combined with a royal name (Psusennes or Shoshenq respectively) which is a feature of historically later texts.[66] But this is invalid, since '*Pharaoh Siamun*' is attested.[67] If Shoshenq III ruled Lower Egypt when Takelot II was in charge of Middle and Upper Egypt, Takelot II, rather than Shoshenq III, would have been the ruler who sent his representative from the 7th Upper Egyptian nome to Dakhleh. The text records a judgment on the ownership of a well in Dakhleh. According to Gardiner's understanding (sic), the mother of the claimant was mentioned as the owner in a document dated to year 19 of a king Psusennes.[68] Because at least 80 years separate 5 Shoshenq I and 19 Psusennes I, it is unlikely that the document was written in 19 Psusennes I, instead of Psusennes II.

If 112 (110) years are added to 1 Osorkon II = 872 BC, then year 2 of Akheperre Osochor corresponds to 984 (982) BC at the latest. Table 4 lists the acceptable and nearest unacceptable lunar day (LD) equivalents for the *Tepi Shemu* feast dates of Akheperre (a) and Siamun (b), together with the probabilities that exactly two of two lunar dates or only exactly one of two are correct and one is negatively incorrect; the table allows for an uncertainty of ca. 15 years.

2 Akheperre	*LD a*	*17 Sia*	*LD b*	*combin.*	*prob.*
990	5	970	2	cc	0.65
993	2	973	–1	c–	0.26
1001	5	981	3	cc	0.65

Table 4

Clearly, it is more probable that year 2 of Akheperre Osochor corresponds to either 990 or 1001 BC, rather than to 993 BC. Which alternative is preferable depends upon the results of the analysis of earlier New Kingdom chronology.

Early Dynasty XXI

At present there are two Egyptological models for the history and chronology of Dynasty XXI prior to

[58] Kruchten 1989, 52.
[59] I Shemu [1] in 14 Osorkon III = November 21, 859 BC = lunar day 1 or 2.
[60] For years 29 and *30 see Beckerath 1997, 95.
[61] Beckerath 1997, 98.
[62] Kruchten 1989, 47f.
[63] Young 1963, 100f.
[64] For 19 years of Siamun see, for example, Beckerath 1997, 101.
[65] Gardiner 1933, 23; Kitchen 1973, 289.
[66] Jacquet-Gordon 1979, 180ff.
[67] Kruchten 1989, 47f.
[68] Gardiner 1933, 28.

Akheperre Osochor. Inscriptions with anonymous regnal years of early Dynasty XXI are found in Upper Egypt only. Traditionally, these regnal years have been interpreted as years of the kings who ruled in Lower Egypt. But Jansen-Winkeln proposes that they refer instead to the High Priests of Thebes in Upper Egypt who assumed royal prerogatives.[69] The traditional Manetho-based chronology reckons with 86 years between Smendes and Amenemope, or with 82 years, if Nepherkheres is considered a coregent: Amenemope 10 years + (Nepherkheres 4 y +) Psusennes I 46 y + Smendes 26 y = 86 years.

In Jansen-Winkeln's model, the attested series of anonymous regnal years should be attributed as follows: High Priest Menkheperre 49 y + HP Pinudjem I 26 y + HP Herihor 6 + x years = 81+x years. The possible synchronism [year x of king] Amenemope = year 49 [of the High Priest Menkheperre][70] would provide the link between the two chronologies. If Amenemope ruled only for 10 years, as is generally assumed, the overlap with Menkheperre amounts to between 1 and 9 years or a mean of 4.5 years. The difference of ca. 5 years between the two models is too small to affect the analysis of lunar dates. If Herihor counted at most 8 regnal years, the years of the High Priests add up to 87.5 or 88 years. Table 5 shows the resulting possibilities for the first year of Dynasty XXI:

2 Akheperre	*duration of early Dynasty XXI*	*resulting year 1 of Dynasty XXI*
991/990	88 y	1079/76
994/993	ditto	1082/79
1002/01	ditto	1090/87

Table 5

Dynasties XX and XIX

The work of Beckerath, Ohlhafer and others appears to have securely established the chronology of Dynasty XX from the reign of Ramesses III onwards.[71] Debates continue about the transition from Tewosret to Sethnakhte and the position of Amenmesses at the end of Dynasty XIX. Both problems can be resolved by using lunar dates. Besides the explicit lunar date from 52 Ramesses II (Piramesses date),[72] there are several graffiti from Deir el Bahri (DB) referring to the lunar feast-of-the-valley.[73] According to the Medinet Habu calendar (and the DB graffiti), the feast began on a lunar day 1 in the second (or third) month of Shemu. On that day a cult statue of Amun crossed the Nile to spend the night in the funerary temple of the ruling king, returning to Karnak on lunar day 2. According to DB 3 and 10, Amun spent the night in the funerary temple of Tewosret and Ramesses III, respectively, thus furnishing the following lunar dates:

DB 3: year 7 of Tewosret, II Shemu 28 = lunar day 2 or 1
DB 10: year 7 of Ramesses III, III Shemu 9 = lunar day 2 or 1

The position of Amenmesses is deducible from the interval between the Piramesses date and DB 3. If Amenmesses was a usurper in control of Nubia and Upper Egypt between the middle of the first and the fifth regnal year of Sety II, then the interval between the two lunar dates amounts to 36 years +121 days = 13261 days = 449 mean lunar months + 2 days. This figure corresponds to the proper interval between a first lunar day and a lunar day 1–2 as a feast-of-the-valley date, either of which might be wrong by one day. Thus the reign of Amenmesses was contained within that of Sety II. If, by contrast, the interval between the two lunar dates is lengthened by a chronologically independent reign of Amenmesses of 4 or 5 years, then the feast-of-the-valley date DB 3 would fall far outside its proper time.[74]

Queen Tewosret is attested for about 412±15 days between DB 3 and year 8, IV [Shemu, day x] of oCG 25293.[75] Sethnakhte ruled into a year 3, so that his reign amounted to between 731 and 1095 days. 2234 days lie between Ramesses III's accession and DB 10. 3377 to 3741 days are accounted for between DB 3 and DB 10 which allows for their proper lunar minimal distance, i.e. 10 years + 11 days = 3661 days = 124 mean lunar months. There is no workable alternative, because *at least* 14 more years would be required for the next coincidence of the lunar dates of DB 3 and 10. Considering the wealth of dated material between the end of Dynasty XIX and the beginning of Dynasty XX, a gap of 14 unattested years is out of the question. Provided that the chronology of Dynasty XX between Ramesses III and XI is securely established, the interval between III Shemu 27 as the accession day of Ramesses II

[69] Jansen-Winkeln 1992, 34–37.
[70] Kitchen 1973, 32; cf. Jansen-Winkeln 1992, 37.
[71] Beckerath 1997, 103–108.
[72] Beckerath 1997, 51.
[73] Marciniak 1974.
[74] For details, see Krauss 1997a, 175–177.
[75] Beckerath 1994, 74–76.

and the last attested date of Ramesses XI (I Shemu 25, year 10 of the era *wehem mesut* = regnal year 28) inclusive amounts to 199 years + 304 days ~ 200 years. By adding the latter figure to the alternatives for year 1 of Dynasty XXI, we arrive at three possibilities for 1 Ramesses II:

year 1 of Dynasty XXI	*interval between Ramesses II and XI*	*year 1 of Ramesses II*
1079/76	200 y	1279/76
1082/79	ditto	1282/79
1090/87	ditto	1290/87

Table 6

Table 7 lists the differences between recorded lunar dates and computed dates and the respective probabilities for the Piramesses-date in conjunction with the lunar dates of DB 3 and 10. The conventional alternative 1304 BC is included for the sake of traditionalists.

1 R II	*Pir difference rec.*	*DB3 minus com.*	*DB10 date*	*comb.*	*prob.*
1304	–1	0	–1	c– –	0.062
1290	+1	+1	0	c++	0.002
1282	–3	–1	–2	–	–
1279	–1	0	0	cc–	0.314

Table 7

1282 BC is not an acceptable alternative, because the indiviudal errors exceed 1 day. 1304 would be preferable to 1290 BC, but 1304 BC is incompatible with the chronology established for Dynasties XXI–XXII and it is far less probable than 1 Ramesses II = 1279 BC.

Mid- and Late Dynasty XVIII

The next step is to subject the lunar dates from the reign of Thutmose III to scrutiny: (a) year 23, I Shemu 21: lunar day 1; Battle of Megiddo; (b) year 24, III Peret 1: lunar day 1; foundation of the Akhmenu at Karnak. The possible correspondances for these dates depend on the time elapsed between the Megiddo date (alternatively the Karnak date) and the Piramesses lunar date which amounts to:

51 years + 215 days[76]
11y Sety I[77]
1y + ?m Ramesses I
27 [28] y Haremhab
4y + 1m Aya[78]
9y [10 y][79] Tutankhamun
?y Ankhetkheprure[80]
2y + ? m Smenkhkare[81]
16y + 9m Akhenaten
37y + 6(?)m Amenhotep III
7y + ?m or 9 y + 8 m Thutmose IV
25y + 10m[82] Amenhotep II
31y + 194 d[83] Thutmose III
sum = 227 years

Table 8

Thus a minimum interval of about 227 regnal years separates the Piramesses and the Megiddo date, corresponding to a minimum of 197 years between 1 Ramesses II and 1 Thutmose III. There are some indications that this minimum is smaller than the historically correct interval. To cite two examples, Manetho recorded 9y + 8m ~ 10 regnal years for Thutmose IV and Kitchen reckons with a year 28 of Haremhab which is not directly attested, although it may be deduced from extant sources.[84] The historically determined minimal distance between 1 Ramesses II and 1 Thutmose III amounts to 197+x years, corresponding to an interval of a) 261 years between the Piramesses and Megiddo lunar dates and b) 226 years between the DB 3 and Karnak lunar dates. Only the distances of 261+3 and 226+3 years, respectively, make up proper lunar distances, corresponding to an interval of 200 years between 1 Thutmose III and 1 Ramesses II. Table 9 contains the differences of the Megiddo and Karnak lunar dates as recorded first lunar days minus computed dates in relation to a generous range of possibilities for 1 Thutmose III.

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[76] Interval between day 1 of Ramesses II and the Piramesses date.

[77] Against the attribution of 15 years to Sety I, see Jansen-Winkeln 1993. Kitchen 1996,5, who attributes 15 years to Sety I, uses the debated coregencies of Akhenaten/Smenkhkare and Thutmose III/Amenhotep II to eliminate the 4 extra years.

[78] Krauss 1994, 74–78.

[79] A year 10 is attested by a wine jar from Tutankhamun's tomb which might belong to an earlier reign; cf. Tallet, 1986, 369–383.

[80] After some hesitation, scholars seem to have accepted the existence of this ruling queen, cf. Beckerath 1997, 112.

[81] Krauss 1997b, 225–250.

[82] Current arguments in favor of a coregency between Thutmose III and Amenhotep II are invalid, see Krauss 1995, 241–242.

[83] Distance between the end of the reign and the Megiddo date.

[84] Kitchen 1996, 5.

1 Th III	*Megiddo difference rec.*	*Karnak minus com.date*	*comb.*	*prob.*
1454	0	+1	c+	0.048
1465	+1	+2	–	–
1468	0	–1	c–	0.259
1476	+2	+2	–	–
1479	0	0	cc	0.656
1482	–2	–1	–	–
1490	+1	+2?	–	–
1493	–1	–1	– –	0.025
1504	+1	0	+c	0.048

Table 9

The years 1465, 1476, 1482 and 1490 BC are not acceptable as 1 Thutmose III, because they imply errors of more than 1 day. 1468 is more probable than 1454 and 1504, while 1493 BC is less probable than 1468, 1454 and 1504 BC. In sum, 1479 BC is by far the most probable alternative for 1 Thutmose III. The acceptable and some unacceptable possibilities for 1 Thutmose III and 1 Ramesses II are combined in Table 10 as five interrelated lunar dates.

1 Th III/R II	*M*	*K*	*Pir*	*DB 3*	*DB10*	*comb.*	*prob.*
	difference rec			*minus com.date*			
1504/1304	+1	0	–1	0	–1	cc– –+	0.015
1493/1293	–1	–1	–3	–1	–2	–	–
1479/1279	0	0	–1	0	0	cccc–	0.344

Table 10

Evidently 1 Thutmose III = 1479 is far more probable than 1504 BC. There is an additional historical argument in favor of 1 Thutmose III = 1479 BC. In the report concerning date M, it is explicitely stated that the battle occurred precisely (Egyptian: *r-mtr*) on the first lunar day, which suits only 1 Thutmose III = 1479, not, however, 1504 BC.[85] Thus 1 Thutmose III = 1479 and 1 Ramesses II = 1279 BC.

Consequences for the chronology of Dynasties XX–XXII

Since 1 Ramesses II = 1279 BC, 2 to 3 unattested years must be added to bridge the gap between 1077/76 BC, as the calculated year 1 of Dynasty XXI and February 1079 BC, as the last attested date of Ramesses XI. Adding these years to the reign of Ramesses XI would suit Wente's dating of a Late Ramesside Letter to year *XII of the era *wehem mesut* = year *30 of Ramesses XI.[86] Then the earliest possible equivalents for the lunar *Tepi Shemu* feast dates in 2 Akheperre Osochor and 17 Siamun would be January 990 and December 970 BC. Reckoning from 17 Siamun, 1 Shoshenq I would fall at the earliest in 948.5 BC = 969 BC – 2 – 18.5; alternatively, reckoning from 1 Osorkon II, the first year of Sheshonq I would correspond at the latest to 938.5 BC = 872 BC + 13.5 + 32.5 + 20.5; the mean lies at 943.5 BC. A likely lunar date corresponding to 1 Shoshenq I = 943.5 BC is that of the *weresh* procession on IV peret 25 in 5 Shoshenq [I], mentioned in the text of the Dakhleh stela.[87] If the procession occurred on a lunar day 1, then the reign of Shoshenq I began in 943 BC.[88] On this assumption there would be 5 unattested years between 6 Osochor and 1 Shoshenq I which implies a total of 134 years for Dynasty XXI. The unattested years may be distributed between Siamun and Psusennes II, or all attributed to Psusennes II.[89] If the 21 years and x month reign of Shoshenq I ended in 922 BC, then there are 49 full years before 1 Osorkon II = 872 BC. Of these 49 years, 46 are accounted for as mean values: 32.5 for Osorkon I + 13.5 for Takelot I. Thus about 3 unattested years remain to be distributed among Osorkon I, Takelot I, and Shoshenq II.

Early Dynasty XVIII

Manetho ascribes 13 years to Chebron < Akheperenre (Thutmose II). As Hornung noted, a 13-year reign does not suit the archaeological record for Thutmose II, whereas a 3 year rule would.[90] Kitchen concedes 3 years to Thutmose II and 12 years to Thutmose I, arriving at 1494 BC for the end of the 20y + 7m reign of Amenhotep I.[91] A comparison of the archaeological remains of Thutmose I/II and Hatshepsut supports the attribution of (4.1 + 10.2) years = 14.3 years to Thutmose II and I, resulting in 1 Thutmose II = 1483 BC and 1 Thutmose I = (March) 1493 BC. If then the reign of Amenhotep I began in III Shemu/July 1514 BC, the Manethonian 25y + 4m reign of Ahmose began in March 1539 BC.[92] Year 9 of

[85] According to computation, old crescent occurred on –1481/5/14 = I Shemu 19, if 1 Thutmose III = 1504 BC; old crescent should have been visible on I Shemu 20 = May 15, if I Shemu 21 were a first lunar day.

[86] Wente 1967, 17.

[87] Cf. Krauss 1985, 165f.

[88] IV Peret 25, 943 BC = December 5th .

[89] The 14 regnal years ascribed to Psusennes II by Manetho-Africanus are conceivably a scribal error for *24.

[90] Hornung 1964, 32f.

[91] Kitchen 1996, 12.

[92] The highest contemporaneous date is year 22; the Manethonian figure of 25 y + 4 m is quite possible, but should not be taken for granted.

Amenhotep I corresponds to 1507 (according to Kitchen) or to 1506 BC. In 1506, a lunar day 1 coincided with III Shemu 9 as the Ebers calendar, according to Parker, indicates.[93] In this context, I shall not use this lunar date, but I encourage my colleagues to study the Ebers calendar under the assumption that 9 Amenhotep I corresponds to a particular year BC – in all likelihood 1506 BC.[94]

Dynasties XVI/XVII and XIII

Ryholt has shown that there are grounds for separating the Theban kings between the end of Dynasty XIII and the accession of Ahmose into two Dynasties (XVI and XVII).[95] But because the dividing line between them is unclear, I shall use the overall designation "Theban Dynasty" here. Presumably the Theban Dynasty began with the king whose name and regnal figure are lost in Turin Canon X 31. The names of nine kings are preserved in TC XI 1–9; about eight more are attested epigraphically, whereas the names of five kings in TC XI 10–14 are lost. In order to minimize the number of Theban kings, I presume that five of the epigraphically attested kings in the following list are those whose names are lost in TC XI 10–14; the sequence is tentative.

TC X 31: name lost, ? years
XI 1: Sekhemre///, 3 y
XI 2: Sekhemre-seusertaui, 16 y
XI 3: Sekhemre-se///, 1 y
XI 4: Se///re, 1 y
XI 5: Sewadjenre Nebiriau I, 26 y
XI 6: Nebiriau II, ? y
XI 7: Semenenre, ? y
XI 8: Seuserenre Bebiankh, 12 y
XI 9: Sekhemre-shed<waset>taui/Sebekemsaef I, ? y
[XI 10]: Sekhemre-wahkhau Rahotep, ? y
[XI 11]: Sekhemre-wadj-khau Sebekemsaef II, 7 y
[XI 12]: Sekhemre-up-maat Antef, ? y
[XI 13]: Nebukheperre Antef, 3 y
[XI 14]: Sekhemre-herhermaat Antef, ? y
Senakhtenre
Seqenienre
Kamose, 3 y

The figures attested for the reigns of nine kings in the Theban Dynasty add up to ca. 73.5 years; for at least 9 other Theban kings the regnal years are unknown.[96] It follows that the king of TC X 31 would have begun to rule *before* autumn 1613 BC = spring 1539 BC + 73.5 years. Ryholt argues that the Turin Canon originally listed at least 57 kings for Dynasty XIII.[97] Therefore the Manethonian figure of 60 kings might well be historically correct and will be used here as an upper limit. For 21 of these kings, a total of about 105 regnal years are attested: 83.5 years in TC and 21.5 years in contemporaneous sources. The 4.5 regnal years attested in pBoulaq 18 should be added, since this document belongs to one of the kings in TC VII 21–23 whose regnal figures are otherwise not known.[98] Thus about 109.5 regnal years for 22 Dynasty XIII kings are accounted for. If these 109.5 years are added to (autumn) 1613 BC as the lowest possible date for the last year of Dynasty XIII, we arrive at (spring of) 1722 BC as lowest possible date for the first year.

The regnal figures for at most 60–22 = 38 kings of Dynasty XIII are not preserved. It is methodologically sound to deal with the chronological uncertainties of Dynasty XIII by reference to an earlier fixed point which should yield a figure for the regnal total of the kings whose years are unattested. I do not intend to use the Sirius date from the Illahun archive to establish such a fixed point,[99] but shall rely instead solely on the 21 lunar dates from the same source as published by Luft.[100] These lunar dates span a period of 42 calendar years; the earliest, in 9 Sesostris III, the latest in 32 Amenemhet III. From an historical perspective the astronomical equivalents 1 Sesostris III = 1862/61, 1837/36 and 1812/11 BC are possible, but only 1 Sesostris III = 1837/36 BC is astronomically correct. The astronomically possible equivalents for the Illahun lunar dates can be expressed in relation to the last year of Dynasty XII, for example as 1 Sesostris III minus 77 years. The alternatives for the last year of Dynasty XII prior to (spring of) 1722 BC are 1735/34, 1760/59, 1785/84 BC and so forth, at intervals of 25 years. Of them, only 1760 BC, corresponding to 1 Sesostris III = 1837/36 BC is astronomically correct (see Excursus 2).

If Sobeknofru indeed ruled 2 m + 14 d of her fifth calendar year,[101] then Dynasty XII ended in mid-January, 1759 BC. It follows that the last year of Dynasty

[93] Parker 1950, 42.
[94] For the present, see Krauss 2003a, 187–190.
[95] Ryholt 1997, 151.
[96] Ryholt 1997, 202, 204.
[97] Ryholt 1997, 72.
[98] Ryholt 1997, 193–194.
[99] See nevertheless Krauss 2003a, 186–187.
[100] Luft 1992; see the comments of Krauss 2003a, 175–178.
[101] Beckerath 1964, 218.

XIII would have been later than summer 1650 BC = January 1759 BC minus 109.5 years. The gap of 37.25 years between the summer of 1650 BC, as minimum last year of Dynasty XIII, and the autumn of 1613 BC, as minimum first year of the Theban Dynasty, is due to the loss of the regnal figures for a maximum of 38 + 15 = 53 kings and for a minimum of 35 + 9 = 44 kings of the Thirteenth and Theban Dynasties. It is methodologically sound to break up the 37.25 years proportionately into mean regnal lengths of the 44 to 53 SIP kings. The resulting interval for the end of Dynasty XIII/beginning of the Theban Dynasty is (the spring of) 1623 to 1620 BC.

A further correction is possible on the basis of the Sirius date from Gebel Tjauti:[102] "Regnal year 11 [of an unnamed king], II Shemu 20, observing *prt Spdt*". As Darnell realized, the date apparently refers to an actual observation of *prt Spdt* (heliacal rise of Sothis/ Sirius) on the territory of a king of the Theban Dynasty, so that II Shemu 20 should correspond to the Julian date of the heliacal rise of Sirius at Gebel Tjauti around 1600 BC. By contrast to other Sirius dates, the site where the observation was made is not at issue nor does the question arise whether the date could have been arrived at schematically. Darnell postulated a local arcus visionis of ß = 8.4° for 1600 BC and inferred that Sirius rose on II Shemu 20 = July 11 in the years 1593 to 1590 BC. There are two problems with this correlation: a) ß = 8.4° is smaller than any actually observed arcus visionis, although it may be just possible; b) on July 11 in 1592–1591–1590 BC the local arcus visionis would have reached only 8.24°, 8.02° and 7.81°. These values seem to be unworkably small, making the corresponding years very unlikely alternatives. Around 1600 BC a workable local arcus visionis of ß = 8.4° is computable for July 12 = II Shemu 20 in 1594–1595–1596–1597 BC. In all likelihood the Gebel Tjauti date refers to the years 1597 to 1594 BC and just possibly to 1593 BC. If Schaefer's theory about heliacal risings is applied, then the rising could not have occurred before July 14/13 which would shift the quadrennium to at least 1601/1598 BC.[103]

If only full years are reckoned, year 11 of Nebiriau I corresponds minimally to 1591/1588 BC = 1623/1620 BC – 32 years, whereas 11 Sekhemreseusertaui corresponds minimally to 1609/1606 BC = 1623/1620 BC – 14 years. Thus the Gebel Tjauti date can only be ascribed to Nebiriau I. If 11 Nebiriau I includes July of 1597/1593 BC, the minimal date for his first year must be raised to include July of 1607/1603 BC. The correction mandates raising the beginning of the Theban Dynasty from 1623/1620 to 1628/1624 BC or the end of Dynasty XIII to 1626 BC as the mean year.

The 'stèle juridique' provides a possibility to check this result:[104] In 1 Merhetepre Ini of Dynasty XIII the visier Aya conferred the office of Hatj-a of El Kab on his son, the priest Aya. The latter's brother Iymeru inherited the office and passed it on to his own son Kebsi who sold it in 1 Nebiriau I. In minimal chronology, 1 Merhetepre Ini equates to 1662 BC = 1759 BC – 97 years, and there would be a difference of 55/59 years between 1 Merheptepre and 1 Nebiriau I = 1607/(1603) BC. In a second step one may correct 1 Merhetepre = 1662 BC, arguing that he had about 13 predecessors and at the most 25 successors within Dynasty XIII who ruled together for 34 to 38 years (1662 BC minus 1628/24 BC). If 34 to 38 years are apportioned equally to 38 kings, then about 11.5 to 13 years must be inserted between 1759 BC, as the first year of Dynasty XIII, and 1 Merhetepre Ini, which shifts 1 Merhetepre to about 1651/1650 BC. The resulting interval of 48 to 43 years between 1 Merhetepre Ini and 1 Nebiriau I is compatible with the time span of two generations when two brothers and the son of one of them held the office of Hatj-a in El Kab.

Excursus 1

Due to adverse atmospheric conditions, I was not able to observe the consecutive risings of Sirius in Abu Simbel, Aswan and Luxor in July 2003;[105] however, I did see old crescent on July 28 in Abu Simbel and new crescent in Luxor on July 30. I succeeded in observing the heliacal rising of Sirius (and, coincidentally old crescent) in the vicinity of Berlin at Caputh, (12° 59.3' E, 52° 20.44' N, 65 m above sea level) on August 26, in the company of Dr. Bernhard Gramsch, who suggested the observation site. We had no difficulty in seeing Sirius between 4h 21m and 4h 51m MET; the computed arcus visionis was 9.04°. This observation and those of 1926/1993 contradict Schaefer's theory insofar as it should not allow the equivalent of an arcus visionis of about 9°. Clearly, the theory must be corrected in view of the data obtained by observation.

[102] Darnell 2002, 49–52.
[103] See note (3) above.
[104] Beckerath 1964, 182f.

At which minimum arcus visionis Sirius becomes observable at heliacal rising is a different problem altogether. Aubourg suggested that the arcus visionis of 9° from a modern urban area like Münster in Westfalia[106] implies a lower value for rural ancient Egypt with less air pollution. One may compare the following observed and *computed values for the arcus visionis within a 4 year cycle:[107] (Münster) year *1992: arcus visionis ß = 9.22°; 1993: 9.03°; *1994: 8.83°; *1995: 8.64° or 9.41°. (Minya)[108] *1924: 9.15°; *1925: 8.92; 1926: –8.7°, *1927: 8.47° or 9.37°.

Aubourg could argue that ß=9° in urban Münster implies the possibility of ß=8.5° in rural Minya. The problem is whether the rising in Minya is indeed observable under ß=8.5° or whether it is necessary to wait one more day to allow ß to increase to about 9.4°. The question can be resolved by making observations in the respective cycle years to come. Aubourg also presumes that the greater distance in azimuth between the sun and Sirius at heliacal rising in antiquity implies a smaller arcus visionis then now. On the basis of the 1926/1993 observations, Pachner could not confirm the expected relationship between the arcus visionis and the difference in azimuth.[109]

Excursus 2: Repetition of lunar dates

Figure 1 presents all astronomically possible sets of equations between the Illahun lunar dates and the years 2300 to 1200 BC, a total of 36 sets of 21 lunar dates each. All 756 cases of old crescent have been calculated; the solid lines in Fig. 1 indicate the percentage of those cases in which old crescent would have been observable without reasonable doubt; dashes signify the cases in which the calculated height of the crescent would have been within the zone of uncertainty. Thus Fig. 1 illustrates how the set of 21 Illahun lunar dates (actually the respective old crescent dates) repeats every 25 Egyptian years. This occurs because every 25 Egyptian years, the sun and the moon reach the approximate same distance from each other once again. The return of the moon to the same star occurs within the siderial month of

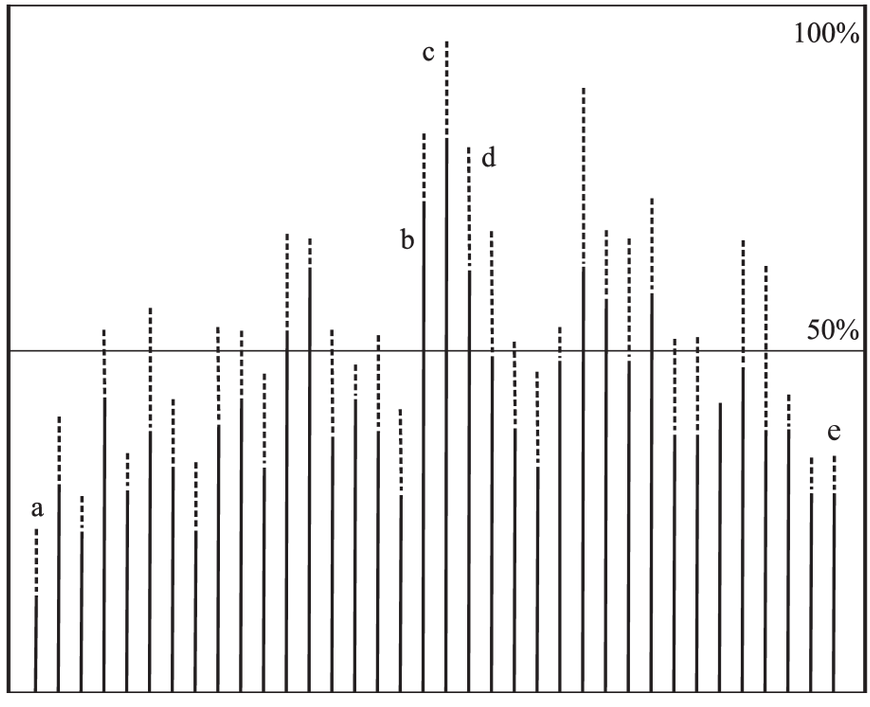


Fig. 1 Percentages of correct and uncertain Illahun lunar dates in 25-year shifts with 1 Sesostris III corresponding to a = 2286, b = 1862, c = 1837, d = 1812 and e = 1387 BC[110]

[105] Residents told me that the haze prohibiting observation was extraordinary.
[106] Pachner 1998, 127.
[107] The figures refer to Sirius on the horizon, without refraction.
[108] Borchardt and Neugebauer 1927, 444.
[109] Pachner 1998, 134.
[110] In 2286 BC I Akhet 1 corresponds to March 18, and so forth.

27.32158 days. Because the sun also travels during the siderial month, it takes the moon additional time to reach the same distance to the sun again; the additional time is the difference between the synodic month of 29.53059 d and the siderial month. Within the siderial and synodic months the moon travels at a mean velocity of 13.176° per day. Within 25 Egyptian years = 24.982 siderial years, the sun travels in the mean 24 × 360° + 353.683°, whereas the moon travels 333 × 360° + 354.272°. In other words, in 25 Egyptian years the positions of sun and moon have decreased by about 6.317° and 5.728° respectively, whereas their original distance has decreased only by about 0.52°. The latter difference increases every 25-year shift resulting in a gradual decrease in repetitions of lunar dates.

The mean siderial and synodic movement also comprises (a) the anomalistic and (b) draconitic movement of the moon, which do not share a common period of 9125 days:

9125 days =
(a) 331 × 27.55455 d + 4.44 d =
(b) 335 × 27.21222 d + 8.91 d =
25 Egyptian years

Thus the mean anomalistic velocity is not the same after 25 years; the moon reaches the same velocity earlier or later. Furthermore, in ±25 years the mean draconitic movement results in a different latitude of the moon. The combination of these factors mean that Egyptian lunar dates do not repeat uniformly every 25 years.[111]

By computing 75 consecutive old crescents and their single ±25 year shift equivalents, I established that on the average only about 70% of a set of lunar dates repeat on the same day.[112] Those old crescents which do not repeat on the same civil calendar day shift by +1 or –1 day. Under these premises any large set of Egyptian lunar dates tends to have only one astronomically correct solution whereas its ± 25 years shifts are not correct. This is exemplified by the computational correlations for the Illahun dates of 1862, 1837 and 1812 BC, as 1 Sesostris III. As Table 11a shows, the three alternatives differ in the percentages of correct, doubtful and wrong dates. If half the doubtful dates are correct and half negatively incorrect, then the probabilities for each set in Table 11b can be calculated. The set '1 Sesostris III = 1837 BC' is about 25 times more probably correct than '1862' and about 6 times more probably correct than '1812'. These two alternatives can be interpreted as ±25 year shifts. They contain fewer correct dates, and display about the same rate of negative and positive errors. Presuming a shifting rate of 70% it is not possible to explain the alternative 1837 BC as the result of a 25-year-shift of either one of the two alternatives.

1 Ses. III	*correct*	*doubtful*	*wrong*	*neg.*	*pos.*
1862 BC	71.5%	9.5%	19.0%	1	3
1837 BC	81.0%	14.2%	4.7%	1	0
1812 BC	62.0%	19.0%	19.0%	2	2

1 Ses. III	*correct*	*neg.*	*pos.*	*prob.*
1862 BC	16	2	3	0.0048
1837 BC	18 (19)	3 (2)	0	0.122 (0.098)
1812 BC	15	4	2	0.0203

Table 11a.b

Shifts of 2 × 25, 3 × 25, 4 ×25 and 5 × 25 years, result in decreasing percentages of correct repetitions as can be inferred from Figure 1. There is an increase in the number of repeated lunar dates within 6 × 25 = 150 Egyptian years; lesser increases occur within (2, 3, 4 ...) × 150 Egyptian years. Apparently the increase is linked to the fact that (a) the mean siderial, (b) synodic, (c) anomalistic and (d) draconitic months have an approximate common period of 150 Egyptian years:

54750 days =
(a) 2004 × 27.32158 d – 2.44d =
(b) 1854 × 29.53059 d + 0.28 d =
(c) 1987 × 27.55455 d – 0.89 d =
(d) 2011 × 27.21222 d – 0.99 d =
150 Egyptian years

Thus the decisive components for a particular lunar phase recur within approximately 150 Egyptian years, resulting in a relatively high percentage of repeated lunar dates. A yet higher percentage is precluded by the fact that within 150 Egyptian years the mean distance between sun and moon at old crescent decreases by 5.53°.

[111] For a specific example see Krauss 2003a, 190–192.

[112] Krauss 1985, 27; similarly Parker 1950, 25. The figures in Krauss 1985, 27, were based on Neugebauer's tables and his lunar visibility criteria. A recomputation with Uraniastar and Schaefer's visibility criteria resulted in the same figure of ca. 70%. I am aware that the 70% lies between the 3s–limits of about 54% and 82%.

Chronological results (dates with an * are liable to slight modifications)

Dynasty XXV	
Taharqo	690–664
Shebitqo	*706–691
Shabaqo	*722–707
Dynasty XXIV	
Bocchoris	*728–723
Tefnakhte	*736–729
Dynasty XXII	
Shoshenq V	783–746
Pami	790–784
Shoshenq III	841–(789)
Takelot II	845–821
Osorkon II	872–842
Shoshenq II	*873
Takelot I	*887–874
Osorkon I	*922–888
Shoshenq I	943–923
Dynasty XXI	
Psusennes II	*967–944
Siamun	986–968
Osochor	992–987
Amenemope	*1002–993
Amenemnisut	*1005–1002
Psusennes I	*1051–1006
Smendes	*1076–1052
Dynasty XX	
Ramesses XI	1106–1078
Ramesses X	1110–1107
Ramesses IX	1129–1111
Ramesses VIII	1130
Ramesses VII	1138–1131
Ramesses VI	1145–1139
Ramesses V	1149–1146
Ramesses IV	1156–1150
Ramesses II	1187–1157
Sethnakhte	1190–1188
Dynasty XIX	
Tewosret	1192–1191
Siptah	1197–1193
Sety II	1202–1198
Amenmesses	1202–1200
Merneptah	1213–1203
Ramesses II	1279–1213
Sety I	1290–1279
Ramesses I	1292–1291
Dynasty XVIII	
Haremhab	1319–1292
Aya	1323–1320
Tutankhamun	*1333–1324
Ankhetkheprure	1334/1333
Semenkhkare	1336–1334
Akhenaten	1353–1336
Amenhotep III	1390–1353
Thutmose IV	1400–1390
Amenhotep II	1425–1400
Thutmose III	1479–1426
Hatshepsut	1479–1458
Thutmose II	1482–1479
Thutmose I	1493–1483
Amenhotep I	1514–1494
Ahmose	1539–1515
Dynasty XVI/XVII	
Kamose	*1542–1540
Bebiankh	*1578–1566
Nebiriau	*1606 –1580
Sobekhotep VIII	*1622–1608
Dynasty XIII	
Merhetepre Ini	*1651–1650
Merneferre Aya	*1675–1651
Ibiau	*1686–1675
Khahotepre	*1691–1686
Sobekhotep VII	*1701–1692
Neferhotep I	*1712–1702
Khendjer	*1728–1724
Dynasty XII	
Sobeknofru	1763–1759
Amenemhet IV	1773–1764
Amenemhet III	1818–1772
Sesostris III	1837–1819
Sesostris II	1845–1837
Amenemhet II	1878–1843
Sesostris I	1920–1875
Amenemhet I	1939–1910

Addendum

At the SCIEM2000 Workshop "Egypt&Time", Vienna 30 June–1 July 2005, Kim Ryholt demonstrated that John C. Darnell's reading of Gebel Tjauti Rock Inscription no. 11 as referring to an observation of Sothis in a regnal year 11 is not acceptable. Therfore my deductions in "Dynasties XVI/XVII and XIII" which are based on Darnell's reading should be disregarded.

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Some Ornamental Scenes on the Wall Paintings from Tell el-Dab^{c}a: Iconography and Context[1]

Katherina Aslanidou

„Minoan art, in contrast to Greek and Egyptian art, shows a constant shifting back and forth between abstract and representational motifs and a sophisticated play with formal possibilities.“[2]

The large assemblages of Minoan painted plaster fragments, which were unearthed in the area of palatial complex G (area H/III),[3] include, among others, a significant group of fragments that belong to ornamental scenes. These fragments were mostly discovered in the vicinity of the *in situ* fresco material and according to the preliminary reports of the site they may originate from the nearby wall of the palatial doorway (Fig. 1).[4]

The first ornamental scene represents a frieze of running double spirals (Pl. I). The 19 fragments with spiral or border motifs from this frieze were found mostly southwest and some northeast of the enclosure wall of the entrance of palace G (Pl. II). The restoration of the material revealed that the biggest plaster fragment of this scene was joined by smaller ones, that were found both on the west and the east of the entrance of the palatial complex G (Pl. II: a).

The proposed reconstruction shows a frieze with a minimum length of 17.94 m and a height of 0.43 m. The preserved fragments occupy 12.2 % of the reconstructed surface. The running spirals are orientated to the right. Multiple zones in blue, white and black colour frame the upper part of the frieze. The lower part of the frieze may have been framed by the same border, as it can be observed in many occasions of spiral zones, like the one from the ‘Monkey’ fresco at Akrotiri[5] and the one from the Domestic Quarter[6] at Knossos. However, without excluding this possibility, on the basis of the preserved material there are no secure evidence to support a reconstruction with a second border.

Before the spirals were painted black, they had been incised precisely by using a pointed tool or more probable with a compass on the wet lime plaster surface.[7] The earliest use of a compass in the Minoan art until now has been detected on the offering table from Amnisos.[8] Five impressed string lines were used for the definition of the ornamental spiral zone. First and foremost, the string line in the middle of the spiral zone has been proved very important for the placement of the plaster fragments in the reconstruction.

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[1] I would like to thank Professor M. Bietak for generously giving me the opportunity to participate in the 2nd international SCIEM 2000 Conference. I am also indebted to him for allowing me to study a part of the fresco material from Tell el-Dab^{c}a/^{c}Ezbet Helmi, depicting ornaments, that has been a research topic of my PhD Thesis.

[2] G. Walberg, *Tradition and Innovation. Essays in Minoan art*, Mainz am Rhein 1986, 38.

[3] M. Bietak, Connections between Egypt and the Minoan Word – New Results from Tell el-Dab^{c}a, in: W.V. Davies and L. Schofield (eds.), *Egypt, the Aegean and the Levant: Interconnections in the Second Millennium BC*, London 1995, 19, fig. 2; M. Bietak, Rich beyond the dreams of Avaris: Tell el Dab^{c}a and the Aegean World – A Guide for the perplexed: A Response to Eric H. Cline, *BSA* 95 (2000), 192, figs. 2–3; M. Bietak and N. Marinatos, The Minoan Wall Paintings from Avaris, *E&L* 5 (1995), 49, fig. 1; M. Bietak, J. Dorner and P. Jánosi, Ausgrabungen in dem Palast von Auaris. Vorbericht Tell el-Dab^{c}a/^{c}Ezbet Helmi 1993–2000 mit einem Beitrag von Angela von Driesch und Joris Peters, *E&L* 11 (2001), 38f., fig. 7.

[4] M. Bietak, *BSA* 95 (2000), 192, fig. 5; M. Bietak, J. Dorner and P. Jánosi, *E&L* 11 (2001), 42, 87.

[5] Ch. Doumas, *The Wall Paintings of Thera*, Athen 1992, fig. 85.

[6] *PoM* III, 343f., 381f., fig. 229, 253–255, 259; S. Immerwahr, *Aegean Painting in the Bronze Age*, Philadelphia 1990, 179, No. 13; M. S. Hood, Cretan Fresco Dates, in: S. Sherratt (ed.), *The Wall Paintings of Thera*. Proceedings of the 1st International Symposium (Petros M. Nomikos Conference Centre, Thera, Hellas, 30 August–4 September 1997, Athens 2000, 205: nr. 32.

[7] The same opinion was expressed by M.A. Cameron concerning a fragment of a similar spiral frieze from Knossos, M.A. Cameron, *A General Study on Minoan Frescoes with Particular Reference to Unpublished Wall Paintings from Knossos*, Ph. D. Thesis, Newcastle-upon-Tyne 1975, pl. 155b; R.E. Jones, Analysis of Frescoes: technical Studies and Replication Experiments, in: D. Evely (ed.), *Fresco: A Passport into the Minoan Past: Minoan Crete through the Eyes of Mark Cameron*, Athens 1999, 152.

[8] M.A. Cameron, *Op. cit.*, 413; Idem, Theoretical Interrelations among Theran, Cretan and Mainland Frescoes, in: Ch. Doumas and H.C Pulchelt (eds.), *Thera and the Ancient World I*: Papers presented at the 2nd International Scientific Congress, Santorini, London 1978, 583, pl. 2A.

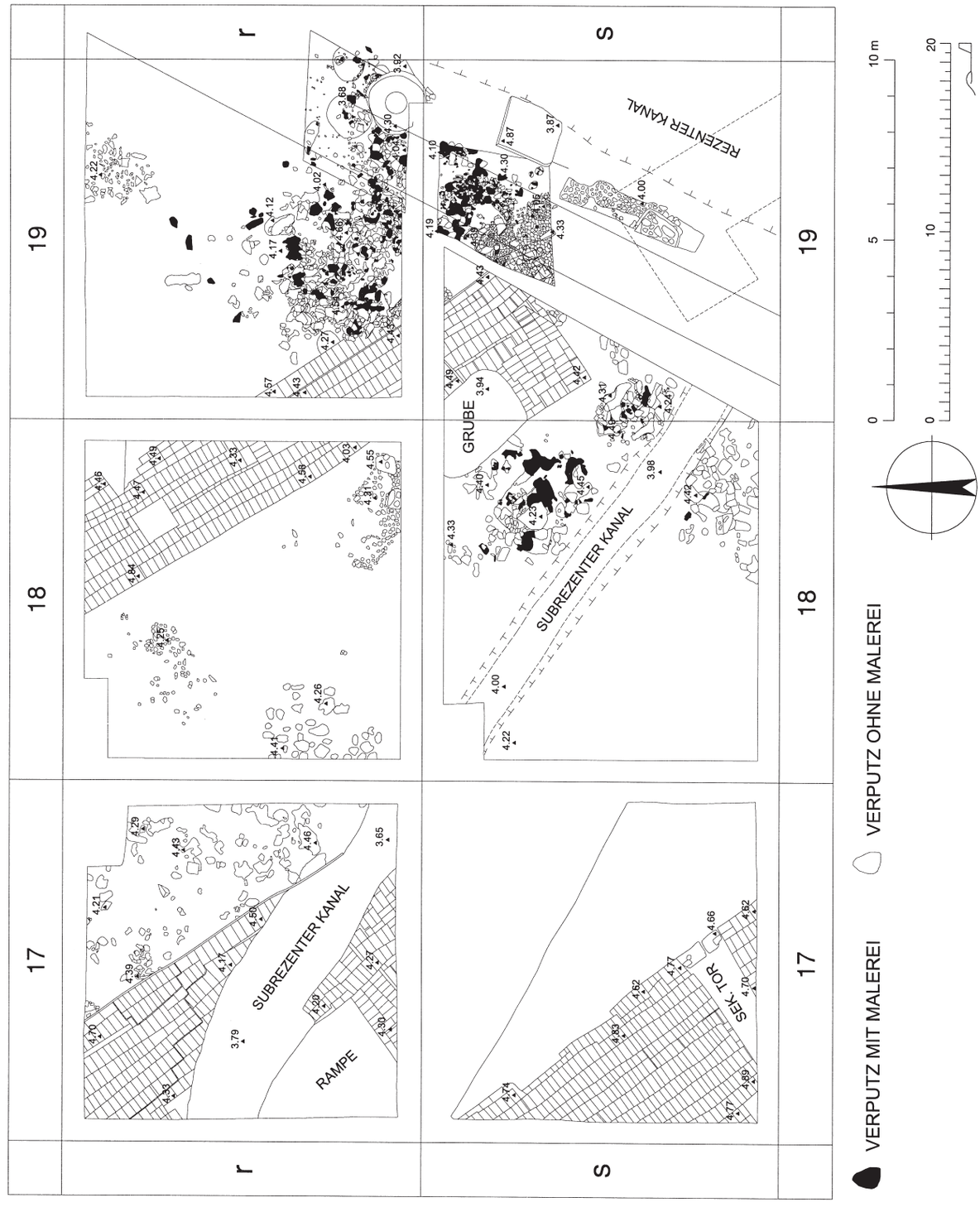


Fig. 1 The distribution of the Minoan plaster fragments around the portal of palace G (after M. Bietak, J. Dorner & P. Jánosi, *ÉdeL* 11 (2001), fig. 7)

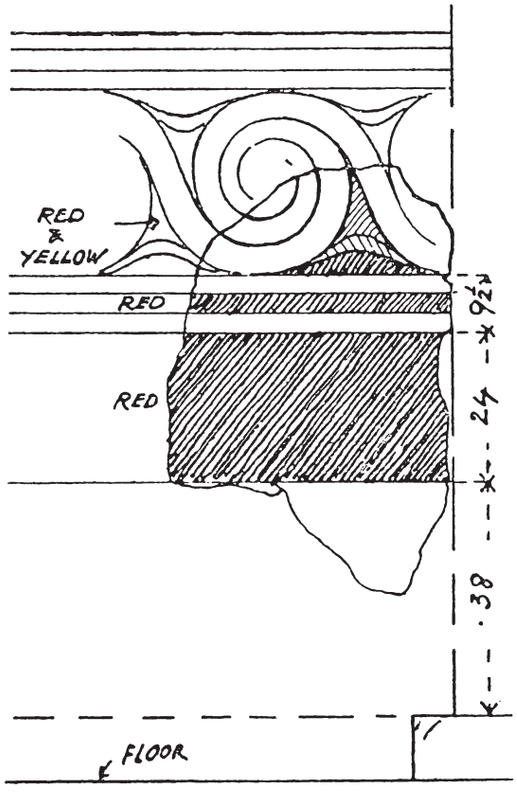


Fig. 2 Spiral frieze from Knossos
(Corridor of the Painted Pithos) (after *PoM* III, 388, fig. 259)

The motif of the retorted spirals is very common in the Minoan palatial art and primarily in the wall paintings. The plaster fragment from a house on the slope of Phaistos belongs to a frieze, which has been dated by the excavator in the Old Palace period.[9] However, later studies suggest on the basis of stylistic comparisons and the chronological sequence of the building activities at the palace of Phaistos that this fragment can be dated to MM III.[10] A distinction between this frieze and the one from Tell el-Dabᶜa, is that the spiral hooks from Phaistos were painted with red colour. Moreover, a MM IIIA plaster fragment from Phaistos shows a black painted spiral with a white and blue coloured border that is similar to the spiral motif from Tell el-Dabᶜa.[11] We can also observe the same combination of colours on the MM IIIB/LM IA plaster fragment from Kasteli-Chania.[12] However, in this case the spirals are painted less meticulously and they are arranged with a round red mottle in their centre.

Further wall paintings representing friezes of running double spirals originate from the palace of Knossos, and particularly from the Corridor of the Painted Pithos (Fig. 2).[13] The frieze was dated by A. Evans in the LM IA period, but according to S. Hood and M. Cameron it belongs to the final decoration of the Domestic quarter, namely to LM II.[14] Finally, a fragment with two spiral hooks on a further plaster fragment from Knossos, precisely from the Royal Road/South, that has been dated in MM IIIA.[15] This example is identical to one of the fragments of the frieze from Tell el-Dabᶜa (Pl. II: b).

A very important equivalent frieze was discovered in Akrotiri, in Room 2 of Xeste 3 (Fig. 3).[16] Two colours were engaged for the spirals of this ornamental scene, red and black. Numerous plaster fragments were also found in Sector Gamma of Akrotiri and belong to an additional frieze of double spirals.[17] Although this ornamental zone was created on the grounds of the same spiral type, it is rendered more elaborately with the addition of red dots and rosettes.

As far as the distribution of the motif in the pottery is concerned, there is a extensive range of parallels from the Old and New Palace period. The frieze of the retorted spirals is a very frequent pattern for the pottery decoration and its development can be mostly detected during the MM IIIB/LM IA transitional phase and in LM IA.[18] An early appearance of the motif can be seen on a pottery vessel of the Classical Kamares Ware.[19] A further example comes from the habitation deposit of Area N in Kea, where we have a fragment of a spouted jar of the Minoan light on dark pottery (MM IIB/ MM III).[20] The same motif

[9] L. Pernier and L. Banti, *Il Palazzo Minoico di Festòs. Il secondo palazzo*, Rome 1951, pl. XL: 1; S. Immerwahr, *Op. cit.* (note 6), 22, fig. 6a; P. Militello, Gli Affreschi minoici di Festòs, *Studi Archeologia Cretese* II (2001), 219, pl. D: 1.

[10] M.A. Cameron, *Op. cit.* (note 7), 590; P. Militello, *Op. cit.*, 217.

[11] P. Militello, *Op. cit.*, 219, pl. D.

[12] A. Andreadaki-Blazaki, Υπόγειο Άδυτο ή "Δεξαμενή Καθαρμών" στα Χανιά, *AAA* XXI (1988), 66, pl. B.

[13] *PoM* III, 388, fig. 259; S. Immerwahr, *Op. cit.* (note 6), 179: No. 13; S. Hood, *Op. cit*, 205, no. 23A.

[14] M.A. Cameron, *Op. cit.*; S. Hood, *Op. cit. TWP*, 206.

[15] M.A. Cameron, *Op. cit.*, 383f., 725f., pl. 138b.

[16] Ch. Doumas, *Op. cit.*, fig. 93–94.

[17] *Thera* III, 39–40, fig. 24, Color Plate B: 2, pl. 59: 1; *ΠΑΕ* 1969, 173, pl. 205a, 238a; Ch. Doumas, *Op. cit.*, 184f., fig. 145.

[18] P. Warren and V. Hankey, *Aegean Bronze Age Chronology*, Bristol 1989, 63, 74f.

[19] G. Walberg, *Kamares. A Study of the Character of Palatial Middle Minoan Pottery*, Boreas 8, Uppsala 1976, fig. 37: 5: i: 3.

[20] J.L. Caskey, Investigations in Kea Part II, a Conspectus of the pottery, *Hesperia* 41 (1972), 387, pl. 90: F 3; pl. 92: G 18.



Fig. 3 Frieze of running double spirals from Akrotiri (Xeste 3)
(after C. G. Doumas, *The Wall Paintungs of Thera*, Athen 1992, fig. 94)



Fig. 4 Eyed ewer with spiral decoration from Akrotiri
(after *Thera* IV, pl. 69a)



Fig. 5 Jug with spriral frieze from Poros (Herakleion)
(after A. Lembesi, *PAE* 67, 1967, pl. 179γ)



Fig. 6 Incense burner from Akrotiri (after C. G. Doumas, *Αλς* 1 (2003), fig. 31)

is shown on the light-on- dark ware of the second palace phase of Phaistos.[21] In Akrotiri and specifically in Room D3 of the Delta Complex, an eyed ewer demonstrates running spirals, identical to the spiral frieze from Tell el-Dabᶜa. (Fig. 4).[22] A jar from a chamber tomb from Poros, belonging according to the excavator to the MM IIIA burial phase, shows exactly the same motif (Fig. 5).[23]

The retorted spirals are also very common in the decoration of bronze vessels, as it can be observed on four examples from the shaft graves in Mycenae.[24] The same motif occurs as well on the bronze cup that has a very uncertain manufacture date (EM II–LM IB) and it was found in Grave VII at Mochlos.[25] However, the jar and the bronze incense burner from Akrotiri display another comparable pattern (Fig. 6).[26] An Egyptian amphora, found in a tomb in Giza, bears a similar spiral frieze, only in this case the centers of the spirals are filled with blue dots, demonstrating a strong influence probably from Minoan bronze vessels that according to C.A. Hope can be dated from the time of Amenophis II to the time of



Fig. 7 The rhyton of the peak sanctuary from Zakros (after N. Platon, Ζάκρος. Το νέον μινωϊκον ανάκτορον, Athens 1971, fig. 76)

[21] L. Pernier and L. Banti, *Op. cit.* (note 9), fig. 219.

[22] *Thera* IV, pl. 69a.

[23] A. Lembesi, Ανασκαφή σπηλαιώδους τάφου εις Πῴρον Ηρακλείου, *ΠΑΕ* 67, 1967, 201, pl. 179γ. The burials of the tomb are dated from MM IIIA till LM IB.

[24] R. Higgins, *Minoan and Mycenean Art*, London 1994, 147, fig. 184; H. Matthäus, *Die Bronzegefäße der kretisch-mykenischen Kultur, Prähistorische Bronzefunde, Abteilung II. Band 1*, München 1980, 179f., 315, pl. 32: 259–260, pl. 58: 514; pl. 74: 1.

[25] H. Matthäus, *Op. cit.*, 237; 260, pl. 44: 369.

[26] C. Doumas, Ανασκαφή Ακρωτηρίου Θήρας, *ΠΑΕ* 149 (1994), 161f., pl. 83α, pl. 93α; Idem, Bringing to Life a Dead City at Akrotiri on Island of Thera, *Αλς* 1 (2003), 54, fig. 31.

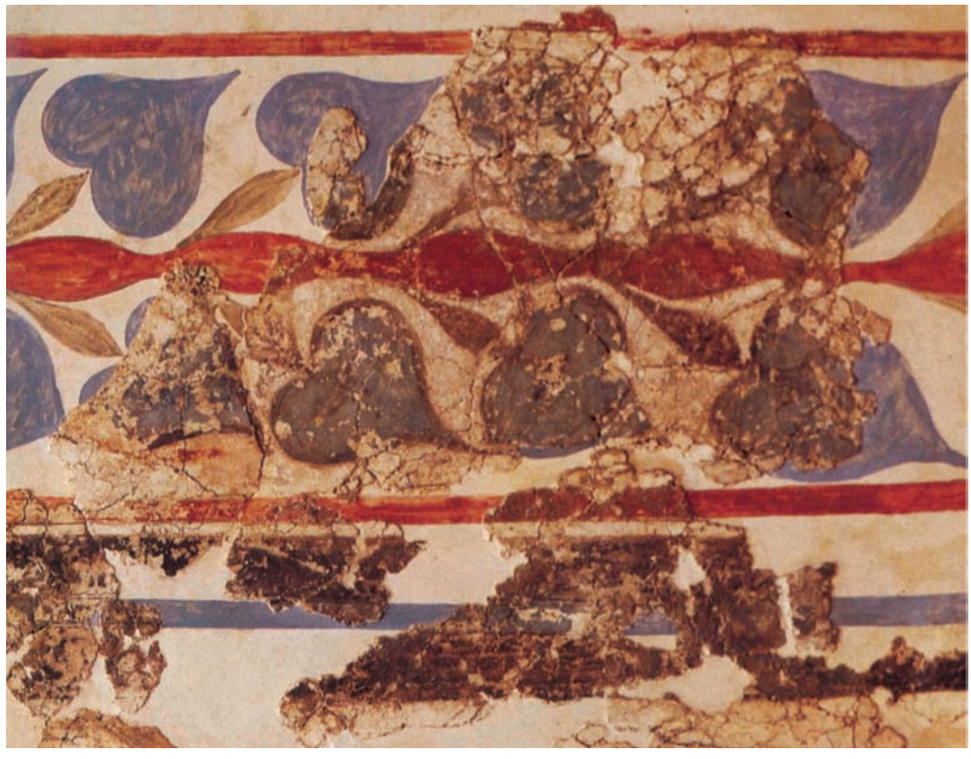


Fig. 8 The ivy frieze from Akrotiri (Beta House) (after Ch. Doumas, *The Wall Paintings of Thera*, Athen 1992, 78)

Tuthmosis IV.[27] Similarly decorated metal vessels are being carried by the Keftius, as depicted on the walls of the Theban tombs of the 18th Dynasty.[28]

The distribution of the retorted spirals in seals and seal impressions is very sparse and chronologically uncertain. This motif can be attested on a button seal from Knossos[29] that has been stylistically assigned to the Old Palatial period and another seal of unknown provenance.[30] A LM II miniature pyxis from Tholos B at Platanos is decorated with the same type of running double spirals.[31] Finally, on the rhyton of the Peak sanctuary from Zakros, found in the Hall of Ceremonies of the Palace some vertical and horizontal friezes of an equivalent spiral design adorn the central wing of the tripartite building (Fig. 7).[32] The panel of the gate is covered with a complex of spirals and framed by a frieze of running spirals.

The second ornamental zone illustrates an ivy frieze (Pl. III). One of the plaster fragments bearing this motif has been published in preliminary reports.[33] Only two fragments were found from this frieze and they lay next to each other and near the entrance of the enclosure wall of the palatial complex G.[34] The frieze is 44 cm. high and 48 cm long. The fragments

[27] F. Schachermeyr, *Ägäis und Orient. Die überseeischen Kulturbeziehungen von Kreta und Mykenai mit Ägypten, der Levante und Kleinasien unter besonderen Berücksichtigung des 2. Jahrtausends v. Chr.*, Vienna 1967, 42, pl. XXVIII: 110; C.A. Hope, Some Memphite Blue painted pottery of the mid-18th Dynasty, in: J. Phillips, L. Bell, Bruce B. William, J. Hoch and Ronald J. Leprohon (eds.), *Ancient Egypt, the Aegean and the Near East. Studies in honor of Martha Rhoads Bell*, 254, 258–259, figs. 20–21. I warmly thank D. Aston and B. Bader for the latter reference.

[28] L. Vercoutter, *L' Égypte et le monde Égéen Préhellénique*, Cairo 1956, fig. 33; W. Helck, *Die Beziehungen Ägyptens und Vorderasiens zur Ägäis bis ins 7. Jahrhundert v. Chr.*, Darmstadt 1995, 54f.; *PoM* II, 736f., fig. 470; R. Higgins, *Op. cit.*, 150, fig. 187.

[29] S. Xanthoudides, Εκ Κρήτης, *ArchEph* (1907), 150, pl. 6: No. 7; P. Yule, *Cretan Seals: A Study on Chronology*, Mainz am Rhein 1980, 158, pl. 24: 43: 2; *CMS* II 1, Ïï. 456.

[30] *CMS* XII, Nï. 5

[31] S. Xanthoudides, *The Vaulted Tombs of Mesara*, London 1924, 102, pl. XI; P. Warren, *Minoan Stone Vases*, Cambridge 1969, 81, 264: D. 247, pl. P 454.

[32] N. Platon, Ανασκαφαί Ζάκρου, *ΠΑΕ*, 63 (1963), 185, pl. 152b, 152, 154a; N. Platon, Ζάκρος. Το νέον μινωϊκον ανάκτορον, Athens 1971, 132, fig. 76.

[33] M. Bietak and N. Marinatos, *E&L* 5 (1995), 57, fig. 13; M. Bietak, J. Dorner and P. Jánosi, *E&L* 11 (2001), 86, fig. 42a.

[34] M. Bietak, *E&L* 11 (2001), fig. 7.

cover a 39.30% of the reconstructed area. The red ivy leaves are painted on dark yellow ground, whereas the ornamental zone is framed by a multicoloured border, painted with alternative red and white stripes. The combination of these two colours is very common in the old and new palatial art. As explained in the case of the spiral frieze, the stripes were initially defined with impressed sting lines and the form of the ivy leaves was incised with a pointed tool.

The ivy frieze from Akrotiri illustrates an alternative version of this ornamental motif (Fig. 8).[35] The heart-shaped ivy leaves are painted blue and have a black outline. On the other hand, the ivy of Tell el-Dabᶜa consists of two antithetical J-spirals, which are joined with a C-spiral. The shape of the leaves is sometimes crudely rendered. However, in both cases we have the combination of different geometric forms in order to represent a naturalistic motif, expressing at the same time a very essential artistic tendency of the Minoan art, the *pictorialism*.[36] This form differentiates totally from the purely naturalistic ivy plants that are depicted, either independently, or integrated in a natural surrounding.[37] A plaster fragment from Knossos presents a textile pattern of a dark coloured ivy zone against yellow ochre background.[38] The form of these ivy leaves demonstrates a style closer to the example from Akrotiri.

The ornamental function of the ivy or sacral ivy can be observed on many objects of the Minoan art since the EM III period.[39] A sealing from the 'Temple Repositories' (MM IIIB) shows the same abstract variation, consisting of two antithetical J-spirals.[40] But since the first appearance of the ornamental ivy, we can distinguish typologically two main categories. A simple, abstract and mainly stylized version and a second elaborate representational form, which however retains its geometrical character.[41] Obviously, the frieze from Tell el-Dabᶜa can be classified belonging to the first category.

A possible early parallel of the ivy from Tell el-Dabᶜa could be represented with a MM III–LM IB bronze cup from tomb XII at Mochlos.[42] Although the excavator of the site was certain that the context of the tomb can be assigned only to the MM III period, on the basis of the ceramic material, the dating of the necropolis of Mochlos in general has been seriously questioned.[43] The stone lamp from the Southeast House at Knossos[44] shows an ivy motif, which is also identical to the one from the bronze cup from Mochlos.[45] Its context is equally unsafe, since A. Evans dated it according to stylistic standards and

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[35] *Thera* IV, pl. 117; Ch. Doumas, *Op. cit.* (note 5), pls. 78–79, 83.

[36] A. Furumark, *The Mycenean Pottery. Analysis and Classification*, Stockholm 1941, 133; S. Alexiou, *Μινωικά και Ελληνικά*, Athens 2002, 50f.

[37] *PoM* I, fig. 391; *PoM* II, fig. 201; M. Möbius, Pflanzenbilder der minoischen Kunst in botanischer Betrachtung, *JdI* 48 (1933), 32, fig. 22b–c; W. S. Smith, *Interconnections in the Ancient Near East*, New Haven 1965, fig. 110; S. Hood, *The Arts of Prehistoric Greece*, Hamondsworth 1978, 67, fig. 50a; D. Evely, (ed.), *Fresco: A Passport into the Past. Minoan Crete through the Eyes of Marc Cameron*, Athen 1999, 212; M. Bietak and N. Marinatos, *E&L* 5 (1995), figs. 12, 14.

[38] M.A.S. Cameron and S. Hood, *Sir Arthur Evans` Knossos Fresco Atlas*, London 1967, 23, pl. E: fig. 1.

[39] G. Walberg, *Tradition and Innovation. Essays in Minoan Art*, Mainz am Rhein 1986, 81–82, fig. 98.; Idem, Minoan Floral Iconography, in: R. Laffineur and J. Crowley (eds.), *EIKON. Aegean Bronze Age Iconography*. Proceedings of the 4th International Conference, Australia, 6–9 April 1992, Aegeaum 8 (1992), 245.

[40] P. Yule, *Op. cit.*, 160, pl. 25: 45: 12.

[41] For the second elaborate type of the ivy ornament cf. G. Walberg, *Op. cit.*(note 20), fig. 48: 25: 1: 11; *Tradition and Innovation*, 82, fig. 99; L. Pernier and L. Banti, *Op. cit.* (note 9), pl. XVIII: a; W.-D., Niemeier, Das Problem der Palaststilkeramik von Knossos, in: *Πεπραγμένα του ΔΔιεθνους Κρητολογικού Συνεδρίου (Ηράκλειο, 29 Αυγούστου – 3 Σεπτεμβρίου 1976) Τόμος Α΄. Προϊστορικοί και Αρχαίοι Χρόνοι*, Αθήνα 1980, 372, pl. 102β; Idem, *Die Palaststilkeramik von Knossos*, Berlin 1985, 66, fig. 22 (1): Nr. 5; *Thera* II, pl. E: 2; pl. 10: 1; *Thera* VI, pl. 12c–d; pl. 73b.

[42] R.B. Seager, *Explorations in the island of Mochlos*, Boston 1912, 62, fig. 31: XIIf; *PoM* II: 2, fig. 288c; R. Higgins, *Op. cit.*, 144, fig. 177; H. Matthäus, *Op. cit.*, 238f., pl. 42: 357. It has been suggested that the destruction of the settlement (LM IB) may be a *terminus ante quem* for the dating of the cup.

[43] A re-examination of the pottery from certain tombs at Mochlos is being made by V. Wartous. V. Wartous, Cretan International Relations during the MM IA Period, in: R. Laffineur (ed.), *Emporia. Aegeans in Central and Eastern Mediterranean*, 10th International Aegean Conference, Italian School of Archaeology, 14-18 April 2004 (forthcoming).

[44] *PoM* I, 344f., fig. 249; *PoM* II: 2, fig. 287g, 288a; Ch. Zervos, *L'Art de la Crète Néolithique et Minoenne*, Paris 1956, pl. 468 ; P. Warren, *Minoan Stone Vases*, Cambridge 1969, 57: Herakleion Museum Nr. 66; S. Hood, *Op. cit.*, 151, fig. 143.

[45] *PoM* II: 2, fig. 288c. The same form of heart-shaped ivies is represented with the glass beads from the Mycenean Cemetery of Ahdonion. The context of the beads is also uncertain. K. Demakopoulou, *Ο Θησαυρός των Αηδονιών. Σφραγίδες και Κοσμήματα της Υστερης Εποχής του Χαλκού στο Αιγαίο*, Athens 1998, 65, pl. 54. Cf. gold relief beads in shape of stylized ivy-leaves in R. Higgins, *Op. cit.*, 173, fig. 205.

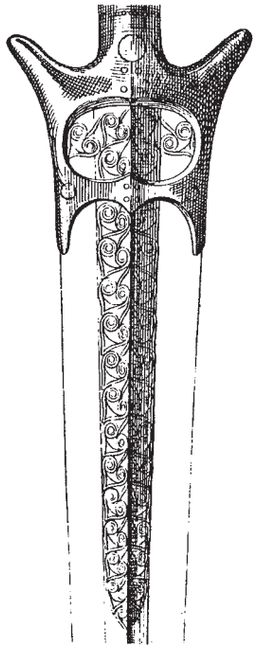


Fig. 9 Ivy decoration on a sword blade from shaft grave IV at Mycenae (after S. Hood, *The Arts in Prehistoric Greece*, Harmondsworth 1978, fig. 172)

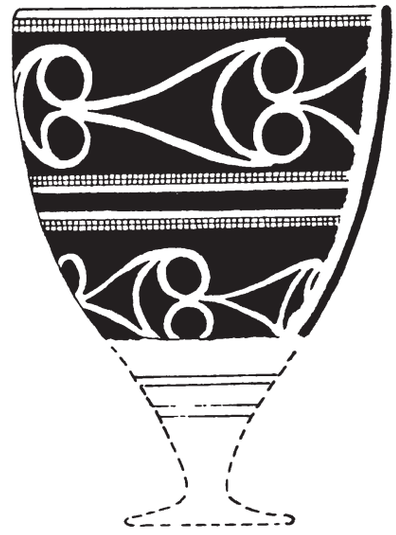


Fig. 10 Ivy zones on a "fine buff ware" cup from Knossos (after J.A. Macgillivray, *Knossos: Pottery groups of the Old Palace Period*, British School at Athens Studies, London 1998, pl. 22: 651)

mostly in relation to the above mentioned bronze cup from Mochlos in MM III. However, a closer examination of the pottery material by M. R. Popham has shown that the House was used till the LM IIIA–B period.[46] Similar heart-shaped ivies are found on the golden necklace from SH IIA-Dendra[47] and on the golden leaves from a Mycenaean tholos tomb at Volos-Kapakli.[48]

In contrast to the already mentioned unsafe parallels, the ivy on the sword blade from shaft grave IV of Grave Circle A at Mycenae represents a more reliable example for the stylistic classification of the frieze from Tell el-Dab^{c}a (Fig. 9).[49] The same opinion can be suggested for a much earlier parallel on the 'fine buff' ware cup from Knossos, which has been dated in the transitional phase between the Old and New palace period (MM IIA) (Fig. 10).[50]

The appearance of the abstract ivy becomes more frequent from LM IB to LM IIIA period.[51] On the bronze incense burner, which was found at the workshop unit of the South wing of the Palace of Zakros, there is an additional example demonstrating the abstract and geometrical version of the ivy (Fig. 11).[52] The same affinities can be observed on two seal impressions from Zakros[53] and Phaistos[54] (Fig. 12). A

[46] M.R. Popham, *The Destruction at the Palace at Knossos. Pottery of the LM IIIA Period*, SIMA 12, (1970) 32, pl. 1h, pl. 10c, pl. 22a–g; W-D. Niemeier, *Op. cit.* (note 39), Berlin 1985, 67, 163f.

[47] A. W. Persson, *The Royal Tombs at Dendra near Midea*, Lund 1931, 38, pl. XVIII: 2; K. Demakopoulou, *Op. cit.*, 106f., pl. 41.

[48] K. Korouniotis, Ανασκαφή Θολωτού τάφου εν Βόλω, *ArchEph* (1906), 235, fig. 12; R. Avila, Das Kuppelgrab von Volos-Kapakli, *PZ* 58 (1983), 42f., fig. 5:3; K. Demakopoulou, *Op. cit.*, 112, pl. 51.

[49] G. Karo, *Die Schachtgräber von Mykenai*, Munich 1930, 99, no. 402; *PoM* II, 481, fig. 288b; S. Hood, *Op. cit.* (note 37), 175, fig. 172.

[50] J. A. Macgillivray, *Knossos: Pottery groups of the Old Palace Period*, British School at Athens Studies, London 1998, 67, 153: 651, pl. 22: 651, 108: 651, 100: 651.

[51] P.P. Betancourt, *Η Ιστορία της Μινωικής Κεραμεικής*, Athens 1985, fig. 105; W.-D. Niemeier, *Op. cit.*, 74, fig. 23: 4, 6; S.W. Manning, From process to people: longue durée to history, in: E.H. Cline and D. Harris-Cline (eds.), *The Aegean and the Orient in the Second Millenium*: Proceedings of the 50th Anniversary Symposium, University of Cincinnati, 18–20 April 1997, Aegeum 18 (1998), 318f.; Idem, *A Test of Time*, Oxford und Oakville 1999, 104–105, fig. 25.

[52] N. Platon, *Op. cit.*, 219; H. Matthäus, *Op. cit.*, 304, pl. 53: 466.

[53] *PoM* I, fig. 523: a–b; I. Pini, Towards a Standardization of Terminology. Problems of Description and Identification, in: R. Laffineur and J.L. Crowley (eds.), *Aegean Bronze Age Iconography: Shaping a Methodology*. Proceedings of the 4th International Aegean Conference, 6–9 April 1992, Aegeaum 8, EIKON, 14, pl. IVc.

[54] P. Yule, *Op. cit.*, 159, pl. 25: 45: 11; *CMS* II 5, no. 173, nos. 180–181; *CMS* II 1, no. 296.



Fig. 11 Incense burner from Zakros
(after H. Matthäus, *Op. cit.*, 304, pl. 53: 466)



Fig: 12 Seal impression with ivy motif from Zakros
(after I. Pini , Aegeaum 8, (1992), pl. IVc)

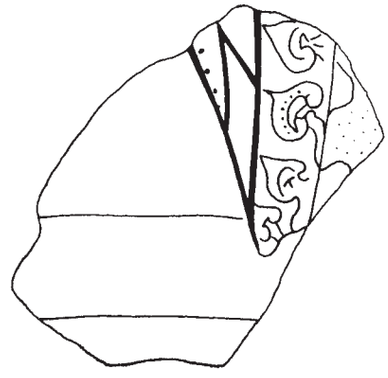


Fig. 13 Schematic drawing of a plaster fragment with stylized ivies from Knossos (after E. Sapouna-Sakelaraki, *Μινωικό Ζώμα*, Athens 1971, fig. 76:δ)

sealing from Knossos, unfortunately of unknown provenance, shows the same abstract ivy form.[55] Further parallels are demonstrated by a silver spoon, a golden and a silver cup from Midea[56] and several metal vessels from Dendra.[57] The kilt of a life-size processional male figure from Knossos is adorned with a network of stylized pendant ivies.[58] Finally, a special resemblance to the ivy of Tell el-Dab^{c}a can be detected on a plaster fragment probably of LM I age from Knossos (Fig. 13).[59]

The third ornamental zone from area H/III represents a foliate band (Pl. IV). The six preserved fragments of the foliate frieze were revealed east and northeast of the entrance (Pl. V). One of the fragments has been published in a preliminary report.[60] The vertical zone is 2.25 m high and 40 cm wide. The fragments cover 18.9 % of the reconstructed surface. The border of the ornamental zone consists of alternative blue and whitish squares. The position of the two ornaments was initially defined by a string line. Between the elegantly and naturalistically rendered leaves are placed red triangular filling ornaments, which bear a fine black outline, like the whitish squares of the border. The majority of the fragments

[55] *CMS* II 8, nos. 138–139.

[56] A. W. Persson, *New Tombs at Dendra Near Midea*, London 1942, 89, fig. 101; fig. 88, pl. IV: Nr. 1–2; E. Davis, *The Vapheio cups and Aegean gold and silver ware*, New York University Ph. D. 1973, 240–242, figs. 203, 205.

[57] S. Hood, *Op. cit.*(note 37), 169, fig. 165; H. Matthäus, *Op. cit.*, 130, pl. 17: 153; pp. 189f., pl. 34: 285; p. 190, pl. 34: 286; p. 264f., pl. 46: 391

[58] *PoM* III, 729, fig. 456c, suppl. pl. XXVII; E. Sapouna-Sakelaraki, *Μινωικό Ζώμα*, Athens 1971, 61, fig. 72γ, pl. B:α, pl. 41:β Cat. no. 138γ; M. Shaw, Anatomy and Execution of Complex Minoan textile patterns in the Procession Fresco from Knossos, in: A. Karetsou (ed.), *ΚΡΗΤΗ–ΑΙΓΥΠΤΟΣ. Πολιτισμικοί δεσμοί τριών χιλιετιών*, Athens 2000, 56, fig. 1 C, pl. 2 C. *cf.* pendant ivies from jewelry craft K. Demakopoulou, *Op. cit.*, 76, pl. 10

[59] E. Sapouna-Sakelaraki, *Op. cit.*, 181, fig. 76:δ, pl. E:α, Cat. No. 139.

[60] M. Bietak, J. Dorner and P. Jánosi, *E&L* 11 (2001), 86, fig. 42b.



Fig. 14 Kamares ware jug with foliate bands (after D. Levi, *Festòs e la civiltà minoica I*, Rome 1976, pl. XXXVIIb)

originate from a wall corner. Therefore, it is safe to assume that the foliate band decorated a wall opening, such as an entrance or a niche.

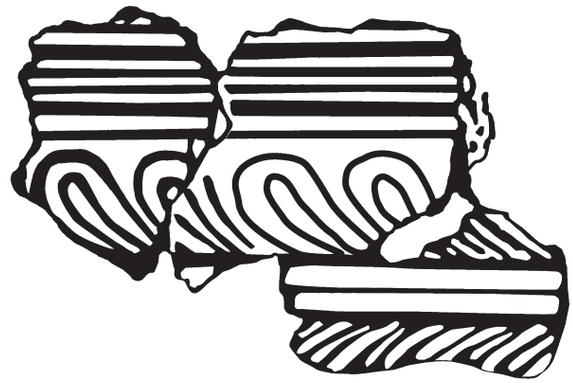


Fig. 15 Plaster fragment with foliate band from Phaistos (after S. Immerwahr, *Aegean Painting in the Bronze Age*, Philadelphia 1990, fig. 6b)

A very significant feature of the foliate band from Tell el-Dabᶜa is the use of red triangles as filling ornaments. The depiction of red triangles and red filling ornaments in general characterize many wall paintings of the site. The spiral frieze with the red diamonds[61] and the griffins with the red filling ornaments between the spiral hooks of the wing,[62] reveal only some of the examples.

The motif of the foliate band appears already in the Old Palace period and belongs to the decorative patterns of the Kamares Ware (Fig. 14).[63] The ivory seal from the Tholos Γ in Platanos[64] and several others[65] from the Old Palace period represent a similar foliate band. From the Aegean wall paintings there is a parallel belonging to MM III (Fig. 15).[66] However, both the colours and the style of this painting differentiate totally from the ornamental scene from Tell el-Dabᶜa. The foliate band of Phaistos is completely stylized in comparison to the more naturalistic leaves of Tell el-Dabᶜa. Foliate bands are also very popular in the LM IA and LH I Pottery.[67] The appearance of

[61] K. Aslanidou, Der minoische Spiralfries aus dem Grabungsareal H/IV in Tell el-Dabᶜa. Malvorgang und Rekonstruktion, *E&L* 12 (2002), 13f., pl. VI.

[62] M. Bietak, Avaris, Capital of the Hyksos kingdom: new results of excavations, in: E. D. Oren (ed.), *The Hyksos: new historical and archaeological perspectives*, 117, Philadelphia 1997, 117, Abb. 4. 31.; Idem, Science versus Archaeology: Problems and Consequences of high Aegean Chronology, in: M. Bietak (ed.), *The Synchronization in of Civilizations in the Eastern Mediterranean in the Second Millenium B. C. II*, Vienna 2003, 29, fig. 2.

[63] G. Walberg, *Op. cit.* (note 20), 61, fig. 45: 18: 16; D. Levi, *Festos e la civiltà minoica I*, Rome 1976, pl. XXXVIIb.

[64] *CMS* II 1, no. 333:b. The burials of the Tholos are dated from EM II till MM II.

[65] *CMS* IV, no. 101; *CMS* II 1, no. 222a, no. 333b.

[66] L. Pernier and L. Banti, *Op. cit.* (note 9), 161ff., pl. XL: 1; S. Immerwahr, *Op. cit.* (note 6), 183: no. 1, fig. 6b; M. Cameron, *Op. cit.* (note 7), 590; P. Militello, *Op. cit.* (note 9), 219, pl. D: 5.

[67] O.T.P.K. Dickinson, The Definition of the Late Minoan I Pottery, *BSA* 69 (1974), 109–110, fig. 1; P.P. Betancourt *Op. cit.* (note 48), 183, fig. 98: Z–Θ; p. 186, fig. 100: B, Δ, E; W.-D. Niemeier, 91–95, fig. 37: 1, 5, 6, 8–10, 13, 16; Idem, Die Katastrophe von Thera und die spätminoische Chronologie, *JdI* 95 (1980), 37, fig. 18: 7

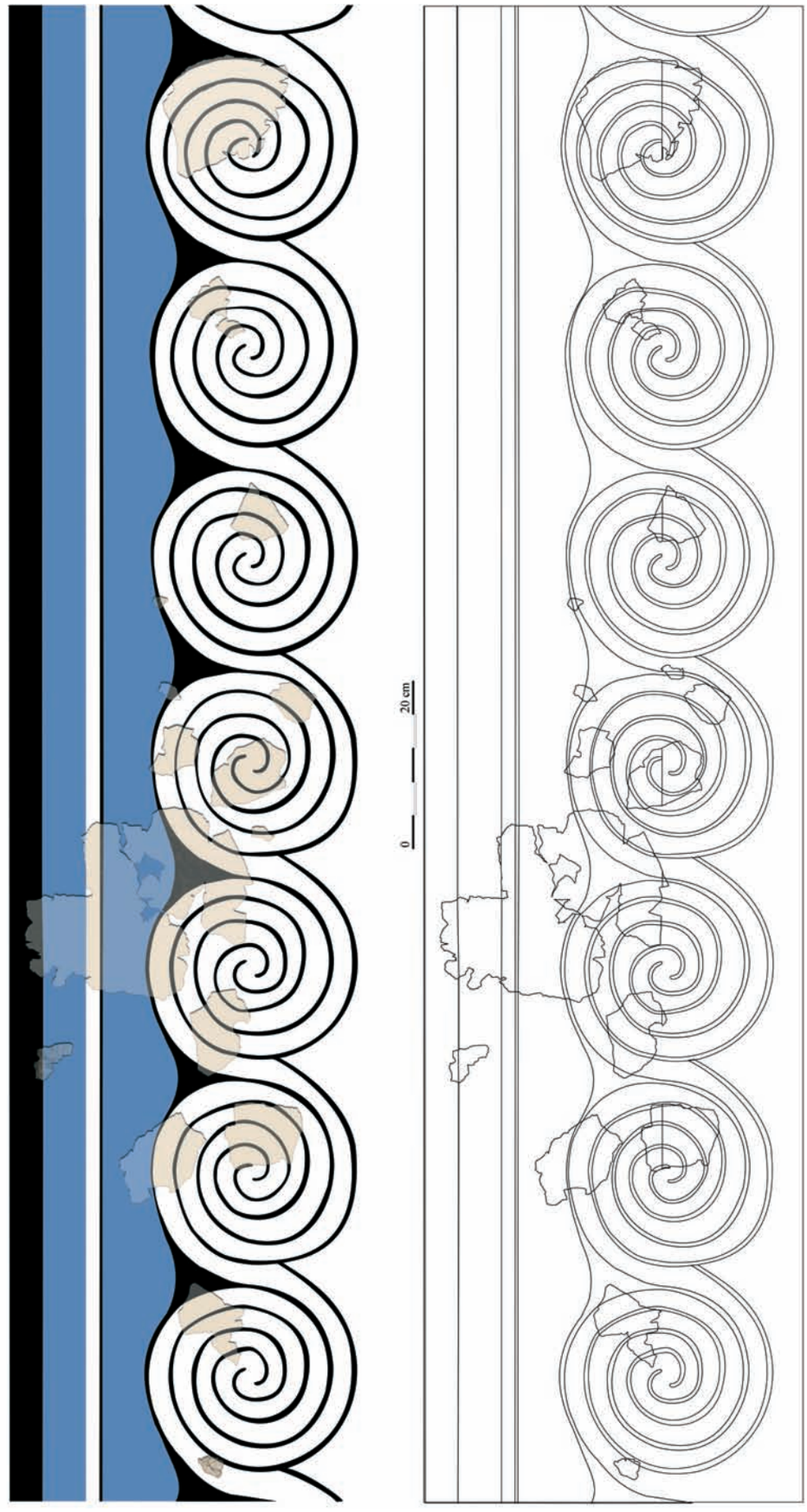
the motif continues into the LM IB period.[68] Nevertheless, the foliate bands of that period depict mostly single or multiple rows of leaves and less antithetical ones like in the previous periods.

In this constant back and forth of the Minoan art, as described by G. Walberg, we can isolate some instances that reveal a very close connection with the ornamental scenes of palace G and at the same time originate from secure contexts. The above analysis connects the running double spirals, the ornamental ivy and the foliate band with well dated material from Kea, Phaistos, Akrotiri, Poros, the LH I art from the shaft graves in Mycenae and Zakros. The Aegean painters from Tell el-Dabᶜa originate definitely from the new palatial tradition and especially the LM I art schools. A particular association of the ornamental friezes to the parallels from Akrotiri and the LH I art of the shaft graves might indicate a stylistic classification in the pre-eruption art of the LM IA period. Nevertheless, the dating of the Minoan wall paintings from Tell el-Dabᶜa can only be determined by the pottery classification and the stratigraphy of the site that connects the Minoan frescoes with str. d/C3, meaning the time of Tuthmosis III.[69]

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[68] P.P. Betancourt *Op. cit.*, 193, fig. 103: A; p. 105, fig. 105: Γ, I.

[69] M. Bietak, Science versus Archaeology: Problems and Consequences of high Aegean Chronology, in: M. Bietak (ed.), *The Synchronization in of Civilizations in the Eastern Mediterranean in the Second Millenium B.C. II*, Vienna 2003, 29, fig. 1; M. Bietak and I. Fostner-Müller, Ausgrabungen vom Palastbezirk von Avaris: Vorbericht Tell el-Dabᶜa/ᶜEzbet Helmi, *E&L*, 13 (2003), 44, fig. 5; I. Hein, Erste Beobachtungen zur Keramik aus ᶜEzbet Helmi, *E&L* 4 (1994), 42; Idem, Untersuchungen und vorläufige Bilanz zur Keramik an ᶜEzbet Helmi, speziel Areal H/V, *E&L* 11 (2001), 140; P. Fuscaldo, Preliminary Report on the 18th Dynasty Pottery von ᶜEzbet Helmi Area H/III-u/17 (The Bathroom), *E&L* 11 (2001), 149f., Abb. 5b; Idem, The Base-Ring Wares from the Palace Complex at Tell el-Dabᶜa (ᶜEzbet Helmi, Areas H/III and H/VI), *E&L* 13 (2003), 72; D. Aston, The Pottery from H/VI Süd Strata a and b: Preliminary Report, *E&L* 11 (2001), 167f.



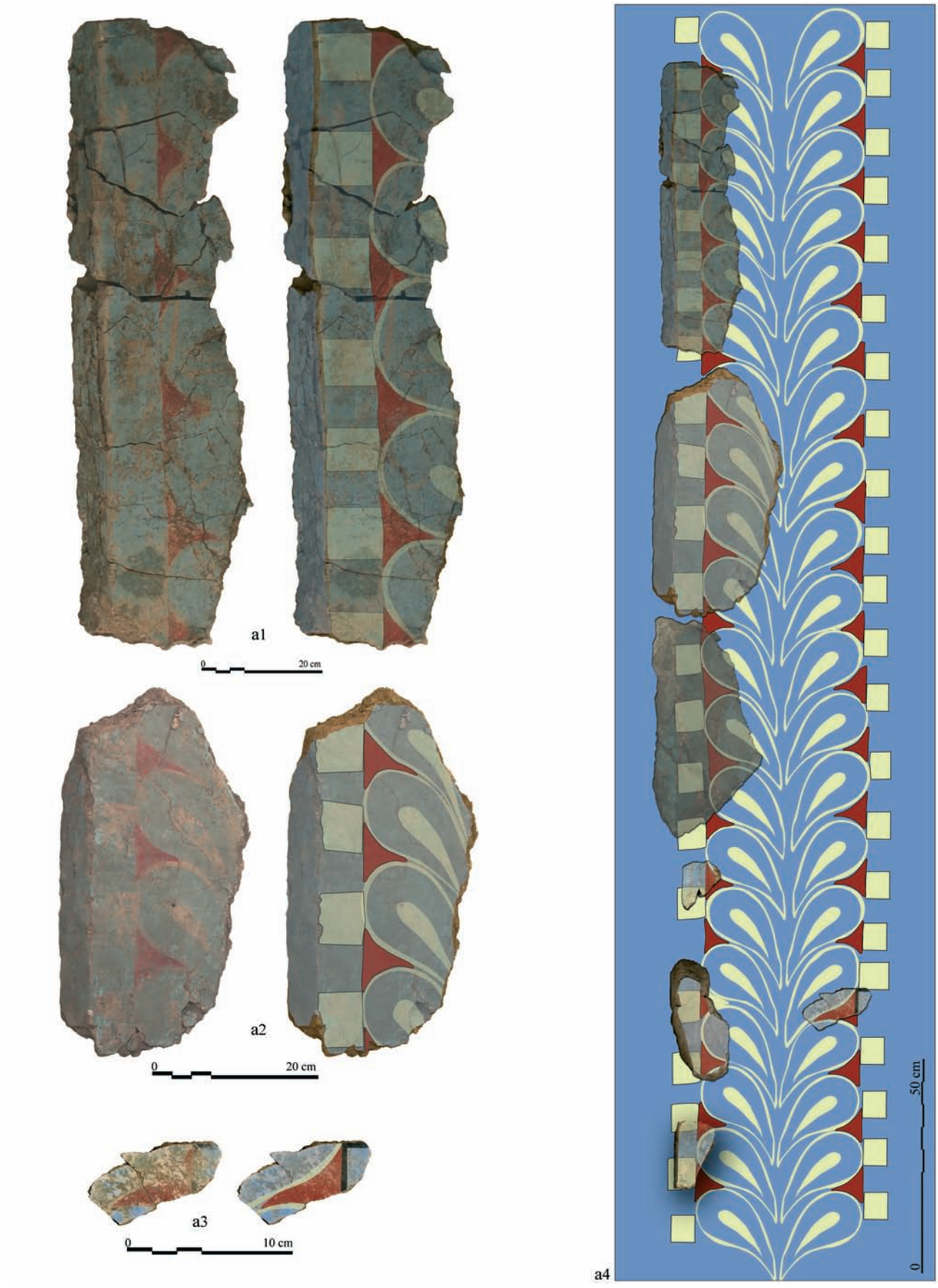
Pl. I The frieze of the running double spirals (reconstruction by K. Aslanidou)



Pl. II Some plaster fragments from the running double spiral frieze:
a) Inv. no. B13; b) Inv. no. B 13a; c) Inv. no. B15a; d) Inv. no. B15d; f) Inv. no. B15



Pl. III The ivy frieze (reconstruction by K. Aslanidou)



Pl. IVa1–a3 Plaster fragments with foliate bands (Inv. nos. B20, B14, B19)
a4: the vertical zone of the foliate band (reconstruction by K. Aslanidou)

Kom Rabi^{c}a, Ezbet Helmi, and Saqqara NK 3507
A Study in Cross-Dating

David A. Aston

Students of Egyptian ceramics have long known that New Kingdom pottery can be conventionally divided into four typological phases, and that the first of these is probably contemporary with the reigns of Ahmose to Tuthmosis II. Unfortunately this is also the worst known of the four phases, not least because our scant knowledge of this period comes almost entirely from tombs.[1] What a relief then, that excavations at Deir el-Ballas between 1983 and 1986, at Kom Rabi^{c}a, Memphis, between 1984 and 1990, and at Ezbet Helmi, Avaris, between 1989 and 2003 provided large quantities of settlement material to balance the ceramic corpus known from the cemeteries. Although only preliminary reports have appeared from these places, it is, however, clear, that a schism is developing between the (preliminary) dating of these sites, with consistently earlier dates being applied to the Memphite pottery than to the Ezbet Helmi material. The work at Deir el-Ballas unfortunately ended before any worthwhile input could be contributed to the debate. It is thus the purpose of this article to find correlations, if any, between Kom Rabi^{c}a and Ezbet Helmi, and to critically examine the dates so-far given to the levels concerned.

A) The New Kingdom Burial associated with Mastaba 3507 at Saqqara

In the earliest preliminary reports concerning the New Kingdom pottery from Kom Rabi^{c}a, much was made of the similarity of the pottery from contexts RAT 305 and RAT 365 to that found with an intact burial situated on or near Mastaba 3507 within the Archaic Necropolis at Saqqara, and it is as well to begin this paper with that tomb group. Although the number 3507 should really be applied only to the archaic mastaba itself, I will, for the remainder of this discussion, refer to the otherwise unnumbered New Kingdom Burial as Saqqara N(ew) K(ingdom) 3507 (cf. Figs. 1–2). Saqqara NK 3507 has no internal dating evidence, but was dated firstly by Merrillees no later than his Dynasty XVIIIA (Ahmose–Tuthmosis II), with the possibility that it could belong to the second half of the Second Intermediate Period,[2] and later, perhaps more dogmatically, to around 1525 BC.[3] In between it was dated by Bourriau to the reign of Amenophis I, originally with some reservation,[4] but then more firmly by a comparison with the material from Kom Rabi^{c}a, particularly contexts 305 and 365, dated to the reigns of Ahmose–Amenophis I, which fits well with a scarab of Tuthmosis I, which was deemed in a preliminary report to be in a near-contemporary position,[5] that was found in a higher context.[6] A dating into New Kingdom ceramic Phase I (Ahmose–Tuthmosis II), is also suggested by the kohl pot, since kohl pots tended to be replaced sometime during the reign of Tuthmosis III by kohl tubes, so, provided this was not an antique, this would give tomb NK 3507 a *terminus ante quem* before the later part of the reign of Tuthmosis III. Moreover the kohl pot is an early type, since, as pointed out by Bourriau, the interior is carved out whereas the latest ones tended to have a simple cylindrical tubular hole.[7]

At first glance, then, a dating into the reign of Amenophis I seems entirely acceptable, but an examination of some of the other objects found in this tomb might suggest pushing this group to the very end of this ceramic phase.

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[1] Cf. Bourriau, 1987, and for differences between cultic and burial pottery at this time see Seiler 1994.

[2] Merrillees, 1968, 28.

[3] Merillees, 2001, 26. 'A date of around 1525 B.C. for the deposit cannot be too far wrong.'

[4] Bourriau, 1981b, 31–32;

[5] Jeffreys, Smith and Malek, 1987, 16.

[6] Bourriau, 1991a, 140; 1997, 165.

[7] Bourriau, 1981b, 40–41, Aston B.G. 1994, 146–149. An example with hollowed out interior, from Qustol, tomb R46, comes from a tomb group which has been dated to the reigns of Amenophis II/Tuthmosis IV, though I see no reason for dating this tomb so late. Cf. Williams, 1992, 17, with the grave goods shown on pp. 221–222. In my opinion this group should be similar in date to Saqqara NK 3507.

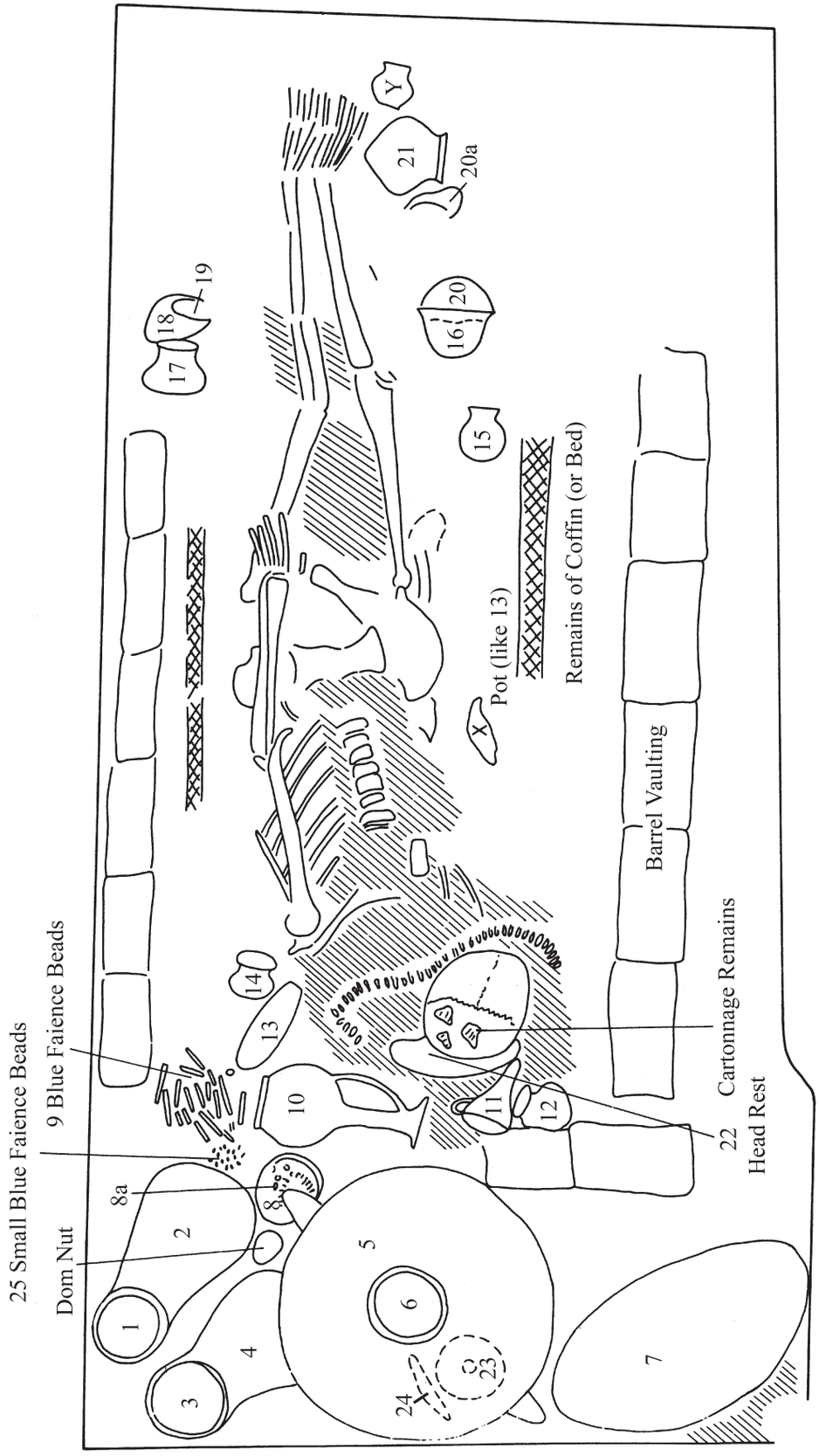


Fig. 1 Plan of Burial NK 3507 (after Bourriau 1991a, 137)

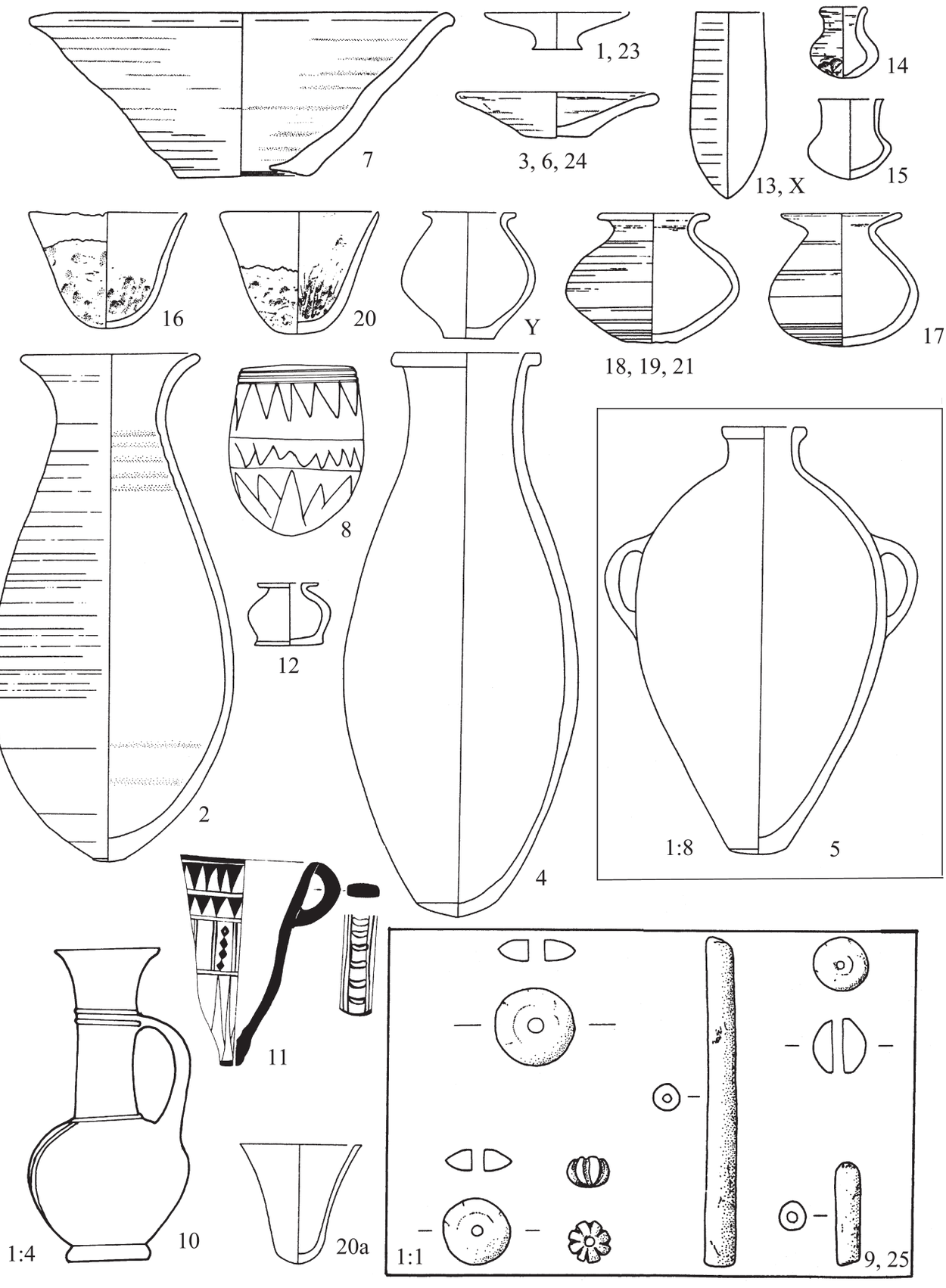


Fig. 2 Tomb Group NK 3507 (after Bourriau 1991a, 138, with additions and renumbering to parallel Fig. 1)

The Kerma beakers can be attributed to Gratien's Kerma Récente,[8] which, succeeding the Kerma Classique phase, must be contemporary with the Eighteenth Dynasty, although since no major deposits of the Kerma Récente have yet been published, a more exact date for this phase cannot be given. However, despite the vivid descriptions of the sack of Kerma given in the bibliography of Ahmose, son of Ebana, there is no archaeological evidence for the common assumption that the campaign of Tuthmosis I put an end to the Kerman state.[9] It is possible, and by analogy with Avaris, even probable, that the potters of Kerma continued to produce the same pottery as before although for new masters, and the cups *per se* need not necessarily predate the campaign of Tuthmosis I.

The blue faience beaker, was, on account of its rilled rim, thought by Bourriau to be from Kerma, and, indeed many ribbed bowls were found there, but these are of a different type, being wider, shallower, and entirely ribbed over the entire surface.[10] The faience vessel from this tomb is perhaps more reminiscent of a copy of an early 18th Dynasty pottery vessel than a Kerma beaker,[11] and in terms of size, – height 12.7 cm. and rim diameter 4.8 cm. – it is probably closest to a Theban pottery vessel found in a context dated to the reign of Tuthmosis III.[12]

Of the faience copy of a Kerma tulip beaker, nothing can be said since it appears to be unique and its present location is unknown, however a similar vessel comes from a grave at Dra Abu el-Naga, which dates to the reign of Tuthmosis III.[13]

When considering the faience rhyton it is noticeable that, with the exception of this example, all the otherwise extant faience copies of Aegean rhyta found in Egypt cluster around the middle of the 18th Dynasty.[14] Note, too, that the earliest representations of such in Egyptian tomb paintings are those from the tombs of Amenweser TT 131, Rekhmire TT 100 and Menkheperreseneb, TT 86, all of which date to the reign of Tuthmosis III, though how much reliance should be placed on this is a moot point since few painted tombs of the earlier Eighteenth Dynasty are extant. However it may be telling that conical rhyta only occur in the latest tombs to show scenes of Aegean tribute.[15]

There can be no doubt that the Egyptian pottery from this tomb is best placed somewhere in the early to mid Eighteenth Dynasty,whilst the flower pot, being wide and shallow is probably earlier rather than later in the phase, since the earlier examples tend to be wider and shallower.[16] At this point it is worth recalling Williams' pertinent observation that flower pots are not found in tombs of the earliest part of the Eighteenth Dynasty, becoming common only during the reigns of Hatshepsut and Tuthmosis III, and dying out by the reign of Amenophis III[17] which is undoubtedly of interest when considering the date of NK 3507. This observation is confirmed through a correspondence analysis of the material found by the Scandinavian Joint Expedition to Nubia, which convincingly demonstrates that the vast majority of flower pots date no earlier than the reign of Hatshepsut, with only 11 of 394 examples being found in the 97 earlier graves.[18]

The Base Ring jug was recently re-examined and fully published by Merrillees. It is of Aström's type VID1dβ1 with three vertical ridges down the front of the body. Unfortunately this type is not closely datable, though the fabric and finish suggested to Merillees that it was an early vessel, which he attributed to LC IA2.[19] The dating of Base Ring ware found in Egypt has long been disputed. On one side stands

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[8] Bourriau, 1991, 139 n.89 referring to Gratien, 1978, 224.

[9] As indeed pointed out by P.L. Shinie, 1996, 81, and cf. below page 218.

[10] G. Reisner, 1923, 155, fig 182; A.J. Millward, 1982, 142.

[11] Cf. the photo with the pot, Smith, Jeffreys, Malek, 1987, p. 17 fig 3 said to part of the installations dug into the sand at Kom Rabia, RAT 394 level V drawing no. 152; cf. also Petrie, 1897, 8 and pl. vii.20 from a tomb beneath the mortuary temple of Tuthmosis IV; and Williams, 1992 type CJ-1d, from tomb R58 dated to the reigns of Amenophis II and Tuthmosis IV.

[12] Seiler, 1992, 128–129, Abb. 11. Cf. also Sowada, 1999, 81, pl. 47 no. TNE94:38, unstratified.

[13] Anne Seiler, personal communication.

[14] R.B. Koehl, 2000, 96. The reference there to earlier examples at Ezbet Helmi can be dismissed since the Ezbet Helmi strata have since been redated. (cf. below page 214–217).

[15] Cf. Wachsmann, 1987, 69–70.

[16] Williams, 1992, 34–35. Note, however, that his shallow type example comes from tomb R40 in which were also found late style kohl pots and a *mechak* razor, and thus R40 presumably dates to around the beginning of the reign of Tuthmosis III or a little earlier.

[17] Williams, 1992, 34–35.

[18] Säve-Söderbergh/Troy, 1991, 44, 225–239, 241–244. Altogether 680 burials were analysed, thus 11 flower pots were found in the earliest 97, and 383 in the remaining 583 burials. In terms of numbers of the flower pots, 268 are dated to the reigns of Hatshepsut/Tuthmosis IV, and 115 to the reigns of Amenophis III–Late Eighteenth Dynasty.

[19] Merrillees, 2001, 27.

Merrillees, who believes that Base Ring I was imported into Egypt during the Second Intermediate Period,[20] a position which finds some support from Bergoffen who suggests that the earliest Base Ring found in Canaan arrived during the MB IIC.[21] If this is the truth an attribution of this vessel to the reign of Amenophis I causes no problems. On the other side of the fence are Oren and Eriksson who are strongly of the opinion that Base Ring I ware was not imported into Egypt, or elsewhere in the Levant, before the reign of Tuthmosis III (Oren) or just before (Eriksson). Of Merrillees' supposed 85 early Egyptian tomb groups, i.e. those dated before the reign of Tuthmosis III, which contain Base Ring Ware, only two are associated with inscribed objects datable to a given pharaoh. One was found in the tomb of Minmontu, with a stone vessel inscribed with the name of Ahmose,[22] but stone vessels were often placed in tombs long after they were originally made, whilst the other comes from a tomb group which contained scarabs of both Tuthmosis I and II,[23] so can be no earlier than the reign of the latter. Oren, one year after Merrillees published his dates, had, also, on the basis of the Cypriote pottery, disputed the dating, considering all these groups to be unreliable,[24] and would move them into Merrillees' Phase XVIIIB at the earliest. Eriksson has recently re-emphasized this belief, though, on the basis of the Minmontu group, allowed for a possible earlier introduction of the ware.[25] Since, however, this is an isolated example, and, as far as I am concerned, unreliable context, this may be dismissed from the equation.

Thus two possibilities exist for the dating of this tomb group; with Bourriau and Merrillees a dating in the reign of Amenophis I, or with Oren and Eriksson an implied date nearer the reign of Tuthmosis III, to which would also perhaps better fit the faience rhyton. Against such a late date would be the kohl pot and, perhaps, the remaining Egyptian pottery, though, of course, it should not be assumed that the ceramic phases began and ended with the beginning and end of a king's reign. Phase I may well have extended into the first years of Tuthmosis III's reign, and thus Saqqara NK 3507 could well be that late. The inexact dating of a single tomb group is, in the wider scheme of Second Millennium chronology, of little substance. Unfortunately this tomb group has achieved some notoriety since the unsupported Amenophis I date has been used, not only to date various contexts at Kom Rabiᶜa, but also to date Egyptian contacts with Cyprus and Crete.

B) New Kingdom Levels at Kom Rabiᶜa

The excavations at Kom Rabiᶜa took place over six seasons, initially in an area of 500 square metres which revealed settlement remains of the Third Intermediate Period, Ramesside and Late Eighteenth Dynasty occupation levels (Levels I–III).[26] The excavation area was then drastically reduced to two 5 metre wide strips along the northern and western sides of the original 20 × 25 m. square, which led to the discovery of early Eighteenth Dynasty and pre- New Kingdom levels (levels IV–VIII). The three earliest levels, VIII–VI are said to be of Late Middle Kingdom date and were eventually buried beneath a mass of collapsed mud brick debris. This debris was then covered by a layer of sand (Level V), over one metre high in places, which effectively sealed off the Late Middle Kingdom structures from the New Kingdom levels, Levels IV–II. Level IV comprises a series of buildings laid out on a different alignment to that under the sand, and this new pattern was seemingly respected throughout the remainder of the New Kingdom. Unfortunately only a small segment of the Level IV settlement was revealed in the northern five metre strip. Levels III and II were excavated over a larger area. Since Level II is clearly Ramesside, it will play no further part in this paper.

Little New Kingdom pottery has so far been published from Kom Rabiᶜa,[27] but the deposits which have been partially made available to scholars are all dated by reference to Saqqara NK 3507 and the unpublished context RAT 782 supposedly similar to Dahshur complex 7, but as it is not published the veracity of this statement cannot be checked. These are RAT 289, 305, 365, 452, 530, 531, and 536. By analogy with Saqqara

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[20] Merrillees, 1968, 147–168.

[21] Bergoffen, 2001, 48.

[22] Tomb of Minmontu. Cf. Legrain, 1908, 55 no. 12; Merrillees 1968, 120.

[23] Carnarvon and Carter, 1912, 64–88, pl. lxviii.1; Merrillees, 1968, 120.

[24] Oren, 1969, 148

[25] Eriksson, 2001a, 58.

[26] For the excavations see Jeffreys, Malek and Smith, 1987, 15–18; Jeffreys and Malek, 1988, 17–20, Jeffreys and Giddy, 1989, 1–8; Giddy and Jeffreys, 1991, 1–4; Giddy, 1999, 1–4.

[27] The principle publications, which include pottery drawings are Bourriau, 1990b, 1991a, 1997, and Bourriau and Eriksson, 1997. Other references to the Kom Rabiʿa material are to be found in Bourriau, 1992; 2002, Bourriau and Nicholson, 1992, Bourriau, Nicholson and Smith, 2000, Bourriau, Smith and Serpico, 2001.

NK 3507, RAT 305 and 365 are dated to the reigns of Ahmose–Amenophis I. As well as the illustrated pottery (Fig. 3), these contexts are also said to contain body sherds of a Cypriote Base Ring I juglet, Cypriote Red Lustrous Wheel-Made Ware (in addition to the Cypriote Black Lustrous jug Fig. 3 no. 10) and a Canaanite jar. The question of the dating of Base Ring ware has already been referred to, and one sits either in one camp or the other. This also affects the dating of the Red Lustrous Wheel-Made Ware. The earliest presumed contexts in which (published) Red Lustrous Wheel-Made Ware appear are Gurob 27, Aniba SA29, RAT 349 and RAT 308/327/334 at Kom Rabiᶜa. Gurob 27, which has no internal dating evidence but also contained Base Ring ware, was dated to the reign of Amenophis I by the excavators, a date challenged by Oren who dated it to the reign of Tuthmosis III, whilst Eriksson, slightly more conservatively dated this tomb to between the reigns of Amenophis I and Tuthmosis III. Aniba SA29, although dated to the reign of Amenophis I through a scarab of that king found in it, was clearly in use to at least the reign of Tuthmosis III, since it also contained an Egyptian pottery jug of a type not known before then. Leaving aside the RAT deposits, all the other Red Lustrous Wheel-Made Ware contexts examined by Eriksson are assigned to the reigns of Hatshepsut/Tuthmosis III. RAT 289 is also dated to the time of Ahmose–Amenophis I, though only one amphora handle from this deposit has been published sofar.[28] RAT 349 was apparently sealed by RAT 308/327/334 in which was found the scarab of Tuthmosis I. Since neither of these contexts appears on the part of the site matrix which has been published one can assume that these contexts are somewhat higher than 305 and 365, hence the logical sequence which would place 305 and 365 contemporary with Ahmose–Amenophis I, 349, by implication to Amenophis I–Tuthmosis I, and 308/327/334 to Tuthmosis I. From the published matrix RAT 530 is contemporary with 305 and 365, all three being above the sand layer which divides *New Kingdom pottery styles* from *Middle Kingdom pottery styles*,[29] which, of course, should not to be confused with a division between the New and Middle Kingdoms. RAT 530 is only dated, *on ceramic evidence*, between the reigns of Ahmose and Hatshepsut,[30] though Bourriau at first dated the context to the reign of Amenophis I,[31] and subsequently, less dogmatically, to anywhere between Year 22 of Ahmose and the end of the reign of Tuthmosis I.[32] Reasons for defining a closer date are based on the unsubstantiated assumption that the Kerma cooking pottery found in RAT 452, "stratigraphically just a little later" was brought there by Nubian mercenaries before the sack of Kerma early in the reign of Tuthmosis I.[33] RAT 530 is particularly important since it also included a sherd, RAT 530.1301, of an Aegean vessel. Although the sherd is not very large, current opinion now favours its identification as a fragment of a Minoan LM IB bridge spouted jar.[34] As with the Cypriote Base Ring Ware, there are two schools of thought relating to the dating of LM IB. On the low chronology such as that followed by Warren and Hankey,[35] and Eriksson,[36] a correlation of LM IB with the reign of Ahmose (perhaps earlier, if the sherd were residual) would be impossible, whilst a dating to the very end of the reign of Tuthmosis I would be more acceptable since the Warren/Hankey/Eriksson start of LM IB, which they argue is co-eval with the beginning of the reign of Tuthmosis III, would need only a slight adjustment. However, not long before this sherd was discovered, Kemp and Merrillees had argued that Late Minoan IB appears to overlap with the later part of the Hyksos period and the first part of the Eighteenth Dynasty,[37] and this view has been championed in recent years by Manning.[38] On such a high chronology, Bourriau's dating of RAT 530.1301 would be perfectly acceptable. Also sealing the sand layer is RAT 293, from which two P31 and one P33 Canaanite jar fragments have been published.[39]

As can be seen from contexts 305 and 365, there occur bowls with black rim bands, which we are told elsewhere first appear in context RAT 740, which is in the sand. Within RAT 707, dated to the late Second Intermediate Period, and lying immediately below RAT 740, a fragment of a Marl D amphora was also found,[40] whilst bowls with ring (pattern) bur-

[28] Bourriau, 1990, 22*–23*

[29] Bourriau, 1997, 163 (my italics).

[30] Bourriau and Eriksson, 1997, 108.

[31] Bourriau, 1991b, 18.

[32] Bourriau and Eriksson, 1997, 108.

[33] Bourriau in Bourriau and Eriksson, 1997, 108.

[34] Warren and Hankey, 1989, 139; Bourriau and Eriksson, 1997; Manning, 1999, 202–204. Note, however that Wiener, 2003, 371, still prefers to see it as LM IA, though this is perhaps influenced by the supposed date of the context.

[35] Conveniently, Warren and Hankey, 1989, 138–144; Warren, 1995, 13; Idem, 1998, 326

[36] Eriksson, in Bourriau and Eriksson, 1997, 98.

[37] Kemp and Merrillees, 1980, 254.

[38] Conveniently, Manning, 1999, 202–208.

[39] Bourriau, 1990, 23*–25*

[40] Bourriau, Smith and Nicholson, 2000, 18.

nishing first appear in RAT 758, which is itself immediately below RAT 740.

To sum up the following matrix can be produced:

Level III

308/327/334	Red Lustrous Wheel Made ware	Tuthmosis I scarab
349	Red Lustrous Wheel Made ware	

Level IV

305–365	Black Lustrous; Base Ring I; Red Lustrous Wheel Made ware	Ahmose–Amenophis I	for some of the pottery see Fig. 3
452	Kerma ware	pre sack of Kerma under Tuthmosis I	for some of the pottery see Figs. 5 and 7
530	LM IB	Ahmose–Tuthmosis I	for some of the pottery see Figs. 9 and 11
536	Contemporary with, or earlier than, 530		for some of the pottery see Figs. 13 and 15

Level V
Top of Sand Layer

289		Ahmose–Amenophis I	
531 [contemporary with 289]		Ahmose–Amenophis I	for some of the pottery see Fig. 17
740	black rims		
707	Marl D amphora	Second Intermediate Period	
758	ring burnishing		

Bottom of Sand Layer

782		Dahshur Complex 7	

In essence this is very neat and undoubtedly acceptable to proponents of a high chronology.

C) New Kingdom Levels at Ezbet Helmi

Excavations carried out since 1989 in six locations at Ezbet Helmi, areas H/I–H/VI, have brought to light settlement layers covering ten distinct strata, from relative strata a (= b/2) to relative strata h (a = b/2, b = b/3, c, d, e/1.1, e/1.2, e/2, f–h).[41] Relative strata a and b, (b/2–b/3) which can probably be dated to the reigns of Horemheb and Amenophis III respectively,[42] are separated from the lower strata, c–h by a hiatus of unknown duration. Although more pottery from Ezbet Helmi than that from Kom Rabi^{c}a has so far been published, very little, with the notable exception of those articles written by Hein,[43] has been accompanied with reasoned chronological argument; most of what has been published consists of catalogues in which the pieces remain undated or are appended dates which are not defended.[44] Nevertheless, from the published sources and the pottery illustrated in Figs. 4, 6, 8, 10, 12, 14, 16, 18–21,[45] it is clear that the pottery from stratum c parallels most closely that from Kom Rabi^{c}a Level IV, whilst stratum d (cf. Figs. 22–24) probably parallels Level V. Moreover as an aid to dating the levels at Ezbet Helmi, much is made of the Cypriot imports, since they appear in a well-stratified sequence as shown in the following chart. Also in comparison with Kom Rabi^{c}a, I also list the first occurrences of black rims, in stratum e/1.1, ring burnishing in stratum d, and Marl D also in stratum d:

c	White Slip I, Base Ring I, White Painted VI, Black Lustrous, Base Ring II, Red on Black, Bichrome, White Lustrous, White Slip II, Marl D, Black Rims, Ring Burnishing
d	Base Ring I, White Slip I, Red Lustrous Wheel Made Ware, Black Rims, Ring Burnishing, Marl D
e/1.1	Bichrome, Proto White Slip, Black rims, Base Ring I (?)
e/1.2	Proto White Slip, Bichrome
e/2	Proto White Slip
f	Red on Red
g–h	White Painted V

The first definite appearance of Base Ring I, and Red Lustrous Wheel Made Ware in stratum d would suggest that Kom Rabi^{c}a contexts 305 and 365 within Level IV are no earlier than stratum d, although the suggested parallels and the Black Lustrous ware are more reminiscent of stratum c. Similarly the first

[41] Cf. Bietak, Dorner, Hein and Jánosi, 1994, 20–38; Bietak, 1996a, 63–83; Idem, 1996b, 11–44; Idem, 2000, 185–205; Bietak, Dorner, Jánosi, 2001, 27–119; Bietak and Forstner-Müller, 2003, 39–50. Note in the first preliminary report the relative strata are called stratum IV and V. These can be correlated with d and e/1 respectively. Relative strata a and b have also been renumbered b/2 and b/3 respectively

[42] Aston, 2001, 168–196.

[43] For publications with discussions of the date of the material, and its consequences see I. Hein, 1994, 39–43; Eadem, 1998, 547–553; Eadem, 2001a, 121–147; Eadem in: Bietak and Hein, 2001, 174–181.

[44] Fuscaldo, 1997, 59–69; Eadem, 2000; Eadem, 2001, 149–166; Eadem, 2002, 167–186; Eadem, 2003, 69–82; Bietak, 2001, 175–201.

[45] Note some of the Ezbet helmi pottery illustrated in this report comes from contexts to be published by Perla Fuscaldo.

black rims in stratum e/1.1 would suggest that Kom Rabi^{c}a context 740 in Level V, is no earlier than this level. To all intents and purposes, however, the Egyptian pottery from Ezbet Helmi strata c and d, cf. Figs. 4, 6, 8, 10, 12, 14, 16, 18–24, is basically identical, and must belong to the same ceramic phase, and, as such, Kom Rabi^{c}a levels III and IV could also be equated with stratum d. For the sake of argument, however, I shall proceed with the assumption that Kom Rabi^{c}a levels III and IV may be equivalent to either Ezbet Helmi stratum c or stratum d.

It is the finds from relative stratum d that have caused the major interest in archaeological circles, since it is with this stratum that the fragments of Minoan wall paintings are associated. These clearly once adorned the walls of Palace F but had fallen off before expansion of the palace in stratum c. At first stratum d was dated to the Late Hyksos Period since scarabs with the names of Ahmose and Amenophis I were found in a domestic compound which abutted (and is thus later than) the palace.[46] Subsequently it was noticed that the palatial compound itself cuts, and is differently aligned to, a Hyksos enclosure wall, thus the palace must be later in time. Consequently it was then dated into the very early Eighteenth Dynasty.[47] As such this brings Ezbet Helmi stratum d and Kom Rabi^{c}a strata III–IV into the same absolute time. This dating is supported by Fuscaldo's analysis of the Kerman pottery, from areas H/III and H/VI, which is found in levels b/3–d. She suggests that all the pieces found in stratum b/3 are from rubbish deposits or derive from stratum c walls,[48] but the sherds from c and d are deemed by Fuscaldo to provide "more archaeological evidence for the presence of Kermans in Egypt during the beginning of the 18th Dynasty that must predate the destruction of Kerma".[49] Thus if the Kerman pottery found in relative strata c and d was brought to Ezbet Helmi before Kerma was sacked by Tuthmosis I, then these two strata must predate the end of the reign of Tuthmosis I. Moreover, since she points out that all these pieces belong to the Classic Kerma Phase, rather than the later Kerma Récente, one presumes, though it is not stated, that Fuscaldo would argue that stratum c should be no later than the reign of Ahmose, since, conventionally, this is the latest secure date for Classic Kerma pottery in Egypt.[50] In so doing she clearly sides with Bourriau's dating of Kom Rabi^{c}a, and contra Bietak's current position, would place the Minoan frescoes in the late Hyksos Period. Since, however, she also ascribes Locus 66 which comes from level e/2 to the late Hyksos Period,[51] it follows that, for her, strata e/1.2, e/1.1, and d must all be very short, since e/1.2 must date from Ahmose's Year 22 (sack of Avaris) at the earliest. Then for Kermans to be in Ezbet Helmi before the sack of Kerma, based on the presence of Kerman sherds in strata d and c, it follows that the sack of Kerma took place sometime within the duration of stratum c. Therefore strata e/1.2, e/1.1, d and the beginning of c must be fitted between Ahmose's Year 22 and the sack of Kerma during the early reign of Tuthmosis I.[52]

That strata d and c could be dated as early as Ahmose or Tuthmosis I, thus favouring the dating suggested by Bourriau, gains support from a preliminary analysis of Carbon 14 samples. At a conference in Vienna in January 2004, Walter Kutschera, indicated that although the Carbon 14 dates from samples taken from the Middle Kingdom levels at Tell el-Dab^{c}a are roughly equivalent to the dates suggested by Bietak on archaeological grounds, samples from later levels are consistently earlier. Thus if stratum D/2 (= Ezbet Helmi stratum e/2) is up to 100 years earlier in (real) time than one supposes, then there would be no problem, for example, to date strata e/1.2 to the late Hyksos Period, which would thus indicate that at Ezbet Helmi, there exists a later Hyksos level, not present on the main tell, or that D/2 is longer than previously thought, as already suggested by Manning.[53] This chain of thought results in two differing scenarios depending on whether the Kerman sherds are as late as the reign of Tuthmosis I, or whether the fact that these sherds being only Classic Kerma, and not Kerma Récente, are no later than the reign of

[46] Bietak, Dorner, Hein and Jánosi, 1994, 20–38.

[47] Bietak and Marinatos, 1995, 62; Jánosi, 1995, 63–71. Cf. also, Bietak, 2000, 194–195.

[48] Fuscaldo, 2002, 174–175, nos. 6–8, 10, 11.

[49] Fuscaldo, 2002, 172, and reaffirmed by personal communication, Spring 2004.

[50] Bourriau, 1991a, 136.

[51] Fuscaldo, 2000, passim. That Locus 66 belongs in stratum e/2 is clear from the report of Bietak, Dorner and Jánosi, 2001, 33, although this is never stated in Fuscaldo's publication of the pottery from this locus.

[52] It is thus somewhat perplexing that in her publication of a catalogue of Base Ring sherds from areas H/III and H/VI strata d to the Ramesside Period, Fuscaldo, 2003, 70, she mentions Oren's and Eriksson's dates for this material (no earlier than, or just before Tuthmosis III), rather than those of Merillees (late Second Intermediate Period onwards).

[53] Manning, 1999, 325–329.

Ahmose. Then, with the reign of Tuthmosis I falling in stratum c, stratum e/1.1 could then date to Ahmose, d to Amenophis I and c to Tuthmosis I, in exact parallel to the dates proposed for these levels at Kom Rabi[c]a. Base Ring I would then appear first at Ezbet Helmi during the reign of Amenophis I, Merrillees' dating of Base Ring Ware would be proved correct, the Minoan frescoes would date to Amenophis I, and the hiatus between strata c and b/3 would be co-eval with the reigns of Tuthmosis III–Tuthmosis IV. If the reign of Ahmose falls in stratum c (Classic Kerma sherds), then e/1.2, e/1.1 and d must all fall into the Hyksos Period, Base Ring must occur within the Second Intermediate Period, as Merrillees supposed, the Minoan frescoes would be late Hyksos, and the hiatus between c and b/3, would cover the reigns of Tuthmosis I–IV.

By contrast, however, Hein has consistently argued for a different scenario. Ceramic studies from Ezbet Helmi areas H/I, H/II, and H/V are used by Hein to date stratum d to the reign of Hatshepsut – early Tuthmosis III, with the possibility that it could start as early as Tuthmosis I, and to equate stratum c with the reign of Tuthmosis III extending into the reign of Amenophis II.[54] Hein has also attempted to date the Kerman sherds found in areas H/I, H/IV and H/V from the Egyptian pottery in the contexts with which it was found, with the result that, contra Fuscaldo, she would date the levels with Kerman sherds from the early Eighteenth Dynasty on into the reign of Tuthmosis III.[55]

From the publications of the Cypriote material from Ezbet Helmi,[56] it is obvious that Bietak and Hein favour the dating proposed by Oren and Eriksson. It is also clear, however, that the Cypriote pottery can be used in support of either a high chronology (Merrillees/Manning) or a low one (Oren/Eriksson), thus, in itself, it cannot date the levels neither at Kom Rabi[c]a nor at Ezbet Helmi, and one must be wary of falling into a circular argument. An examination of the Egyptian ceramic material from Kom Rabi[c]a levels III–V and Ezbet Helmi, in particular area H/VI, strata e/1.1 to c, do, in fact, provide a number of hints indicating, to my mind, that a date in the reigns of Hatshepsut–Tuthmosis III is perhaps more likely than an earlier one. Firstly the presence of a Marl D amphora in RAT 707 in Level V, and the first appearance of such at Ezbet Helmi in stratum d is telling. Unfortunately little can be said about Egyptian amphorae before the reign of Tuthmosis I, since very few early New Kingdom amphorae have been published with enough fabric details to know whether the vessels concerned are genuine Egyptian vessels, or whether they are Canaanite imports. Egyptian made amphorae are certainly known in late Hyksos levels at Tell el-Dab[c]a, where they were produced from Nile clay, but the earliest unequivocally dated Egyptian amphorae of the New Kingdom must be those with the stamped cartouches of Tuthmosis I known from Serra East,[57] Deir el-Medineh[58] and East Karnak (Naga al-Tawil).[59] For those from Serra East and Deir el-Medineh, no indication of the fabric is given, though that from Naga al Tawil is definitely of Marl D.[60] An amphora found in cemetery 185, tomb 196 at Fadrus is dated to the Eighteenth Dynasty before the reign of Hatshepsut by correspondence analysis,[61] but, from the published fabric description it is not clear whether the vessel is of Egyptian or Canaanite origin. Many evidently Egyptian amphorae, since they bear stamped cartouches on the shoulder, are known from the reigns of Hatshepsut and Tuthmosis III, though again no indication of fabric is given. Numerous examples of such amphorae, are known from the nécropole de l'Est at Deir el-Medineh,[62] whilst the next datable, indubitably Marl D, amphorae are those from Theban Tomb 87, which are assumed to belong with the first use of the tomb, i.e. to the time of the original owner, Nakhtmin, who was in high office from the reign of Hatshepsut to at least Year 34 of Tuthmosis III,[63] and those from the tomb of the three foreign wives of Tuthmosis III, of which the pottery assemblage found therein is dated to the end of the reign of Hatshepsut or to the beginning of Tuthmosis' sole reign.[64] From the reign of Tuthmosis III onwards Marl D amphorae become extremely common, and many have been found in well dated con-

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[54] Hein, 1995, 554, 2001a, 140; 2001c, 242.
[55] Hein, 2001b, 199–212.
[56] Bietak and Hein, 2001; Hein, 2001c.
[57] Hughes, 1963, 129; Knudstadt, 1966, 172.
[58] Nagel, 1938, 129 no. 10.
[59] Aston, forthcoming
[60] Aston, forthcoming
[61] For the vessel see Holthoer, 1977, pl. 22 type AO1, and for the date, Troy, 1991, 223, 264.
[62] Bruyère, 1937, 93, with an illustration of a complete vessel on p.95, fig, 48.1, though this one does not appear to be stamped.
[63] Guksch, 1995, 82, Abb 36a–d. The vessels were shown to the present writer by Heike Guksch, and it is clear that these are of the coarse (Marl G) variety.
[64] Lilyquist, 2003, 73, 76, 101–103, figs. 75a–e, 76a–b, 77a–b.

texts, or bear dockets referring to year dates of given kings. From the above it is clear that, when RAT 707 is left out of the equation, the earliest datable Marl D amphorae can be assigned to the reign of Tuthmosis I, yet they do not become common until the reign of Tuthmosis III. It may be argued, of course, that the lack of examples before the reign of Tuthmosis I is simply due to chance, and that they were already being made as early as the Second Intermediate Period, the date assigned to the handle found in RAT 707, but this would be an argument *ex silentio*, and is better avoided. However, not only does one have to explain the gap between that found in RAT 707 and the otherwise first datable examples in the reign of Tuthmosis I,[65] one also has to explain why such Egyptian amphorae were suddenly produced during the Second Intermediate Period. Canaanite Jars, which the earliest Marl D amphorae copied, had been imported into Egypt since at least the Late Twelfth Dynasty,[66] and the most logical reason why the Egyptians would need 'home-made' amphorae is that they must have begun producing their own wine in sufficient quantities to need wine jars in which to keep it.[67] Thus the question has to be asked how and why this should have come about. Although wine was clearly a favourite of the Hyksos, being both imported – witness the large number of Canaanite jars found at Tell el-Dab^{c}a,[68] – and home grown, since Kamose specifically refers to the vineyards of Avaris,[69] – surprisingly few wine jars (that is Canaanite jars) or fragments thereof, have been found outside of the Delta in early Eighteenth Dynasty contexts. Those that have been found were probably appreciated more for their practicality than their contents, since even those found in royal tombs had been reused for holding beer.[70] Thus it may be supposed that the conquest of the Eastern Delta, and subsequent raids into Palestine by the first pharaohs of the Eighteenth Dynasty brought the Eighteenth Dynasty elite not only into contact with wine as a drink, but also, and more importantly, into acquaintance with vintners able to produce it, the latter clearly being persuaded to ply their trades for their new Egyptian masters.[71] At this time it is probable that new vineyards were planted 'on the western river', i.e. somewhere in the Western Delta, since this is where most of the wine came from that supplied Malkata, Amarna, and the tomb of Tutankhamun. Thus if a large native wine industry developed as a consequence of the conquest of the Eastern Delta, then the need for amphorae in the early Eighteenth Dynasty would be explained. Indeed Bourriau is of the same opinion supposing that 'royal encouragement of viticulture in the Delta at the beginning of the Eighteenth Dynasty suddenly required transport amphorae.'[72] It is hard to see, therefore, why Marl D amphorae should have been produced before the conquest of Avaris, and before the royal encouragement of viticulture by the early Eighteenth Dynasty pharaohs. The presence of a Marl D amphora fragment in RAT 707, therefore, suggests the fact that RAT 707 postdates the beginning of the Eighteenth Dynasty.

The stratum d fragment 8912X, Fig. 24b, is clearly the upper part of a figure vase. It is made of a red slipped and burnished marl with additional decoration in black, and, although fragmentary, clearly belongs with a small, but well-known group of female pottery figurines. These can be conveniently divided into two kinds – kneeling figures representing wet nurses, and standing women representing servant figures or musicians.[73] Although not enough is preserved to determine whether 8912X should be considered a

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[65] Note that at Dra^{c} Abu el-Naga, a number of amphora sherds are found in Seventeenth and early Eighteenth Dynasty tombs, but significantly none are of Marl D – Anne Seiler, personal communication. From Seiler's description of the fabric of these sherds, it is possible that they may derive from the oases where similar sherds, Hope's fabric B23, have also been found in Seventeenth and early Eighteenth Dynasty tombs. Cf. Hope, 2002, 99.

[66] Arnold Do., Arnold F., and Allen S., 1995; McGovern, 2000, 31–40; Aston, 2002, 44–45; Schiestl, 2002, 346–348.

[67] Whilst such jars could have also been used for transporting other contents, it is noticeable that analysis of the fabrics found in the earlier levels at Memphis seem to indicate that those found in the earliest New Kingdom levels were used for wine (fabrics, P31, P33), and only with the fabrics found in later levels were Canaanite jars used for transporting pistachio resins and honey (P11, P30) or oil (P40, P46). Cf. Serpico and White, 1998; Serpico, 2000, 458–9; Eadem, 2003; Serpico, Bourriau, Smith, Goren, Stern and Heron, 2003, 373.

[68] McGovern, 2000, 31–40.

[69] Smith and Smith, 1976, 60, 74.

[70] Note that an analysis of the residue remaining in the Canaanite jar found in the tomb of Meryetamun, revealed the contents to have been beer, Winlock, 1932, 32. According to Emery's records, the Canaanite jar found in NK 3507 contained milk: Bourriau, 1991a, 139.

[71] Cf. the clear reference to the vintner Khor, 'the Syrian', Hayes, 1951, 102.

[72] Bourriau, forthcoming.

[73] Murray, 1911, 40–46; Desroches-Noblecourt, 1952, 49–67; Brunner-Traut, 1970a, 35–48; Eadem 1970b, 145–64, Rand, 1970, 209–212; Bourriau, 1981a, 34–36 nos. 47–50; Eadem, 1987, 90–93 sections 7.3–11; Dorman, 2002, 21–24.

wet nurse or a servant figure; in terms of dating this is of little consequence, since all the examples discussed by Bourriau can be dated between the reigns of Tuthmosis III and Amenophis III.

In Ezbet Helmi stratum d, a small ring-based carinated bowl, 8963Q, Fig. 22g, with a red rim and 'splash decoration' on the interior comes from Locus 4901. Pots with such decoration are not uncommon. A number of open forms with similar decoration were found in tombs 195, 316, 420, 507, 511, 515, 517 and 598 at Fadrus.[74] The Fadrus cemetery was subjected to a correspondence analysis by Lana Troy, who was thus able to separate the tombs into three main chronological phases, divided into seven subphases, which, in real terms, were equated with (1) the early Eighteenth Dynasty pre-Hatshepsut (Fadrus Ia–b); (2) Hatshepsut/Tuthmosis III–Tuthmosis IV (Fadrus IIa–c) and (3) Amenophis III–Late 18th Dynasty (Fadrus IIIa–b).[75] As a result of this analysis, tomb 195 was placed in phase Ib; 420 and 598 in phase IIa; 507, 511, and 517 in phase IIb; and 316 in phase IIc, whilst 515 could only be assigned a broad date range encompassing phases IIb–IIIb. Thus with the exception of the bowl from tomb 195, all these vessels can be placed in the period co-eval with the reigns of Hatshepsut/Tuthmosis III–Tuthmosis IV. Yet another flat-based bowl with red rim on the interior and exterior with red splashes on the interior, was found at Qustol in cemetery R tomb 43,[76] which was dated by Williams to the reign of Amenophis III or IV, with the possibility that it could be a little earlier.[77] Large deep flat based bowls with 'splash decoration' were found within a disturbed foundation deposit of Tuthmosis III at Deir el-Bahri.[78] Bowls with incurved rims and flat or raised bases, with 'splash decoration' on the interior are found in the valley of the Three Pits.[79] The tombs found in the Valley of the Three Pits were dated by their excavators to the reign of Tuthmosis III, through an analysis of the coffin fragments, ceramic forms, and faience balls found in some of them. Such a date is confirmed by the discovery, in tomb B, of a Menkheperre sealing, which cannot have been made before the reign of Tuthmosis III.[80] The recent publication of the tomb of the three foreign wives of Tuthmosis III reveals that yet more of these bowls were associated with that tomb.[81] On the basis of a number of parallels, the pottery assemblage found in this tomb was dated to the end of the reign of Hatshepsut or the beginning of Tuthmosis III's sole reign.[82] Thus the example from Ezbet Helmi stratum d seems to fit best somewhere within the reign of Tuthmosis III, rather than earlier in the Dynasty. In this respect it should be noted that one of the vessels of type 7 found in the RAT 530 random sample is described as having irregular splashes of red slip on the inside.[83] This suggests that this bowl is yet another example of the bowls with splash decoration just discussed, with, if true, the subsequent implication that RAT 530 ought also to date within the reign of Tuthmosis III. Of course it is possible that this type of vessel with splash decoration may have a long lifespan, and examples could have been produced much earlier with the painted vessels found in Tuthmosis III contexts all being late examples. This, however is also an argument *ex silentio*, which would create a gap between the otherwise undated Ezbet Helmi and Kom Rabi^{c}a vessels and the datable examples listed above. I would suggest that it is more logical to assign the undated Ezbet Helmi and Kom Rabi^{c}a pots to the reign of Tuthmosis III thus bringing them into line with the other datable vessels. In stratum d at Ezbet Helmi, there is also a pottery rhyton.[84] As mentioned in the discussion of the tomb group Saqqara NK 3507, all the otherwise extant faience copies of Aegean rhyta found in Egypt cluster around the middle of the 18th Dynasty, in particular the reign of Tuthmosis III.[85] Whilst the Ezbet Helmi example is made of pottery, not faience, its presence with vessels that seem to belong in the reign of Tuthmosis III is probably significant.

From what has been written above, therefore, I would suggest that stratum d should be dated to the reigns of Hatshepsut–Tuthmosis III and stratum c to the later reign of Tuthmosis III, which is thus in close agreement with the work of Hein.

A dating of stratum d into the reign of Tuthmosis III has consequences on the Nubian pottery found in these levels and on the date of black rim bands, both

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[74] Holthoer, 1977, 85, pl. 18, FP2/IR/3R/d–f; 113, pl. 24, CC6/IIR/3D/f–g; 116, pl. 25 CU1/IR/3R/d–e; 118, pl. 25, CU3/IR/3R/c–e; 119, pl. 26, CU4/IR/3R/d–e; 124, pl. 27, PL3/IR/3R/d–e

[75] Troy, 1991, 223.

[76] Williams, 1992, 213 fig. 56d.

[77] Williams, 1992, 17.

[78] Szafranski, 1997, 63 nos. 5, 7.

[79] Loyrette, 1997, 186, fig. 6f, 6i.

[80] Loyrette, 1997, 180–184.

[81] Lilyquist, 2003, 94, figs. 59e–g, 60d, 97 fig. 64i.

[82] Lilyquist, 2003, 76.

[83] Bourriau and Eriksson, 1997, 114, fig. 6 no. 7.

[84] Vessel 7545, Hein, 1998, 550.

[85] Koehl, 2000, 96.

of which may now be examined in turn. Since, conventionally the Classic Kerman sherds could not have reached Ezbet Helmi any later than the reign of Ahmose, nor the Kerman cooking pot sherds after the middle of the reign of Tuthmosis I, unless they are residual, how then can this be explained ? It may, of course, be argued that the presence of Kerman cooking pot sherds are against such a chronology, but Bouriau's argument, followed by Fuscaldo, that such sherds predate the campaign of Tuthmosis I collapses when, in a later work, Bourriau indicates that those found at Memphis were made locally, from a special fabric specifically manufactured only for the production of Nubian style vessels.[86] Since similar sherds are also found at Ezbet Helmi in contexts presumably datable to the reign of Tuthmosis III, this would perhaps suggest that the presence of Nubian cooking pots has a culinary rather than political reason, in much the same way as modern European cooks use woks to prepare traditional Chinese food. Alternatively one might argue that after the sack of Kerma by Tuthmosis I, Kerma was not entirely destroyed, as obviously Avaris was not completely razed when the Hyksos were expelled, and that the survivors continued to trade, or bring with them, such cooking pots northwards. As such, Kermans and their cooking pots could have continued to emigrate to Avaris during the reigns of Tuthmosis I–III. Indeed the fact that Tuthmosis II, Hatshepst and Tuthmosis III all led campaigns into Nubia indicate that the Kerman state was certainly not wiped out as a result of Tuthmosis I's sack of its principal city. However, since the Kerman (style) cooking pots found at Memphis are now considered to be locally made, I tend to favour the first of these two hypotheses.

The presence of black rims on pottery as a decorative style is traditionally assumed to have developed at the end of the Second Intermediate Period,[87] though there is no real evidence for such a belief. It can be traced back to the bowl, which is very similar to that from Ezbet Helmi, stratum c, (8909K, fig. 12c) found with the intact burial discovered by Petrie at Qurneh, which, on the basis of a rishi coffin, was dated by him to the Seventeenth Dynasty since Seventeenth Dynasty royal coffins are of similar style.[88] This Seventeenth Dynasty date has been defended in the most recent discussion of this group,[89] on the grounds that the style of mummification is consistent with Seventeenth Dynasty mummification techniques. However the factors quoted by Eremin, Goring, Manley and Cartwright, viz the poor tissue preservation, the presence of some brain material and the unbroken nasal bones, indicative of little emphasis being placed on the actual preservation of the body, and the non-removal of the brain is also still current during the early Eighteenth Dynasty.[90] Secondly they suggest that this burial is that of a Seventeenth Dynasty queen simply on the location of the burial, in roughly the same general area as other known Seventeenth Dynasty royal burials. They also point out, however, that the gold earrings associated with this burial would appear to be the earliest noted example of an open-ended form which subsequently became popular in the New Kingdom, thus they, themselves, also leave the way open for a dating into the early New Kingdom. Later excavations at the Nécropole de l'Est at Deir el-Medineh,[91] brought to light more rishi coffins in combination with amphorae, stamped with the cartouches of Hatshepsut and Tuthmosis III, and it is now known that such coffins extend from the Seventeenth Dynasty well into the reign of Tuthmosis III.[92] In her examination of Petrie's Qurneh burial, Bourriau has drawn attention to another rishi coffin, that of Rennefer, wife of Neferkhewet, keeper of palace documents under Hatshepsut. Providing Rennefer did not long predecease her husband that coffin can thus be dated to the reign of Hatshepsut. In its style of decoration it bears two transversal bands as part of the design. These transversal bands are also found on the coffin of Rai, wet nurse to Ahmose-Nofretari, but are not found on any of the Seventeenth Dynasty royal coffins. Since these bands are not found on the Qurneh burial coffin, Bourriau concludes, with Petrie, that the Qurneh burial should also be dated to the Seventeenth Dynasty. However, the rishi coffin found in tomb 1389 at Deir el-Medineh, is also of the type without transversal bands.[93] The man buried in this coffin was accompanied by various grave goods including a double kohl tube (not pot) of wood and a *mechak* razor, both of which are indicative of a date no earlier than the reign of Tuthmosis III,[94]

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[86] Bourriau, Smith and Nicholson, 2000, 18, 32.

[87] Bourriau, 1981, 74; Aston 2003, 142, but the discussion in this paper leads me to reject the Seventeenth Dynasty date for black rims.

[88] Petrie, 1909, 6–10.

[89] Eremin, Goring, Manley and Cartwright, 2000.

[90] Cf. Dawson and Elliot Smith, 1924, 89–90.

[91] Bruyère, 1937.

[92] Taylor, 1989, 30.

[93] For the coffin, Bruyère 1937, 26 fig. 10; for the tomb group as a whole, Bruyère, 1937, 197–201.

[94] For kohl tubes as opposed to kohl pots, see above xxx, and for the razors see Eriksson 2001b. Note if the razor shown in Bruyère, 1937, 81 fig. 40 is from tomb 1389, then the

and, what is more telling, some of his accompanying boxes were sealed with seals of Tuthmosis III, hence Bourriau's argument for the date of the rishi coffin found by Petrie falls, a point already mentioned by Eriksson.[95] That the Qurneh burial could be later than Petrie supposed, however, is also indicated by the bronze cutter (linen cutting knife) found with it, which finds ready parallels within, *inter alia*, Abydos Garstang tombs E.10, E.176, E.268 and E.294.[96] That from tomb E.10 was found with a scarab of Tuthmosis III. The pottery from tombs E.10 and E.176 is so similar that E.176 must also be of the same date,[97] whilst E.258 and E.294, an intact burial in which the cutting knife was found in association with a *mechak* razor, a double kohl tube, Base Ring ware, Red Lustrous Wheel Made Ware, both contain pottery traditionally dated to the reign of Tuthmosis III. Although Petrie dated the knife found in the Qurneh burial to the Seventeenth Dynasty, he seems to have subsequently changed his mind since in a later publication he dated such knives to the reign of Tuthmosis III, pointing out that they are different to one found in a context dated to the reign of Tuthmosis I.[98] Moreover it should be noted that the pottery found with Petrie's Qurneh burial is indisputably later in style than that associated with the Seventeenth Dynasty tomb of Nubkeperre Intef located in Dra Abu el-Naga.[99] At this point it is probably significant to recall that among the relatively intact Theban tomb groups collected by Tyson Smith, none of those dated before the reign of Tuthmosis III, with the exception of Petrie's Qurneh burial, which he accepted as of Seventeenth Dynasty date, contained black rimmed pottery.[100] With the removal of the Qurneh burial, it is hard to pin down the earliest undoubted evidence for black rim bowls. Many can be found in tomb contexts which clearly belong to, or were last used in, the reign of Tuthmosis III, but anything which is unequivocally earlier is hard to find. A bowl with a black rim was found in Gurob tomb 26 together with a scarab of Amenophis I, which thus acts as a *terminus post quem*. Black rimmed bowls are found in the filling material associated with tomb K93.11 at Dra Abu el-Naga,[101] which along with tomb K93.12, is currently thought to be the original tomb of Amenophis I and Ahmose-Nofretari,[102] thus if the ceramic material can be associated with the first use of this double tomb, this would be a further indication that black rims developed during the reign of Amenophis I. At best, therefore, it would seem that black rims developed sometime between the reigns of Amenophis I and Tuthmosis III. This is in keeping with the strata at Ezbet Helmi since if stratum d should be dated to the reign of Tuthmosis III, it follows that e/1.1, where the first black rims occur, should be a little earlier. At Kom Rabiᶜa the first black rims appear in RAT 740, just above RAT 707 with the Marl D amphora. Thus if RAT 707 should be reassigned to the early Eighteenth Dynasty (see above), then the black rims in RAT 740 would fit perfectly in sequence, since RAT 740, being above 707, should also date to the Eighteenth Dynasty – within the reign of Amenophis I at the earliest. However if we date the Marl D amphora no earlier than the previously earliest known examples (Tuthmosis I), and recall that the first ring burnishing, which at Ezbet Helmi is first found in stratum d, occurs below the first black rim, then Stratum V could indeed be considerably later.

Whilst the above arguments are perhaps only superficial, it seems to me more logical than the previously published dates for the ceramic sequence at Kom Rabiᶜa. Paradoxically the recent publication of the New Kingdom and Post-New Kingdom objects from Kom Rabiᶜa by Lisa Giddy not only strengthens the case for a re-dating of levels III–V but would seem to support the dating suggested by the material from Ezbet Helmi. She records that throughout Level III there are, in addition to that of Tuthmosis I (deposit RAT 308),[103] also scarabs of Amenophis II (RAT 423),[104] Tuthmosis IV (RAT 534),[105] and two of Amenophis III (RAT 565 and 738)[106] along with two finger rings of Tutankhamun (RAT 432 and 1103).[107] Since no matrix has yet been published for this level,

razor is of Eriksson's type 2. Eriksson had noted that the type from tomb 1389 is unknown. Bruyère, 1937, 81 fig. 40 shows a double kohl tube, the razor and an ointment spoon, but the accompanying text refers only to the double kohl tube which is stated to come definitely from tomb 1389.

95 Eriksson, in Bourriau and Eriksson, 1997, 100.

96 Garstang, 1901, 12, pl. xvi.

97 I fail to understand why Garstang dates E.176 to the reign of Amenophis III – Garstang, 1901, pls. xvi, xxvii. The tomb group has been extensively discussed by Merrillees, 1968, 100, and Eriksson, 1993, 89–91.

98 Petrie, 1917, 50–51, pl. lxii:17–20.

99 Cf. Seiler in Polz and Seiler, 2003, 49–72

100 Tyson Smith, 1999, 193–231.

101 Seiler, 2003, 338–343, and forthcoming.

102 Polz, 1995, 217.

103 Giddy, 1999, 62–3, no. 990.

104 Giddy, 1999, 68, no. 1403.

105 Giddy, 1999, 70 no. 1776.

106 Giddy, 1999, 64, nos. 1779 and 2015.

107 Giddy, 1999, 105 no. 1420, 108 no. 2694.

it is possible that 432 and 1103 are higher than 565 and 738, which are higher than 534, which is itself higher than 423, which is higher than 308, thus level III could extend throughout the Eighteenth Dynasty from the reigns of Tuthmosis I to Tutankhamun. However RAT 565, with the scarab of Amenophis III is ascribed to level IIIb, thus the latest phases of level IIIb are already no earlier than the reign of Amenophis III, and RAT 423 with the scarab of Amenophis II comes from the higher Level IIIa, thus the scarabs are not in sequence. Moreover Giddy also publishes a plaque from a Level IV wall (context RAT 332) inscribed with the cartouche of Tuthmosis III (Men-Kheper-Re).[108] Thus if level IV continued to the reign of Tuthmosis III, and level IIIb to the reign of Amenophis III, it is clear that the Tuthmosis I scarab found in RAT 308 was an old one at the time of deposition, and can thus play no part in the dating of the pottery found with or beneath it. In addition an incomplete kohl pot with a simple cylindrical tubular hole, rather than having the interior hollowed out, was found in RAT 289 within Level V.[109] Such vessels with a tubular hole apparently do not occur before early Dynasty 18.[110] It thus seems obvious that the previously proposed Kom Rabi^{c}a dates should be brought down later in time.

The similarities between Ezbet Helmi strata d and c and Kom Rabi^{c}a Levels IV and V cannot be ignored. From the above it is apparent that Helmi (H/VI) strata c–d are probably contemporary with RAT 308/327/334 down to 740. Since the arguments outlined above indicate that stratum d should be contemporary with the reign of Tuthmosis III, then the dating of the Ezbet Helmi strata should fall within either one or the other of the two following schemes

A) Scheme one

c ≃ Amenophis II/Tuthmosis IV
d ≃ Tuthmosis III late
e/1.1 ≃ Tuthmosis III early (Hatshepsut)
e/1.2 ≃ Tuthmosis I/II
e/2 ≃ Ahmose–Amenophis I

B) Scheme two

c ≃ Tuthmosis III late
d ≃ Tuthmosis III early (Hatshepsut)
e/1.1 ≃ Tuthmosis I/II
e/1.2 ≃ Ahmose–Amenophis I
e/2 ≃ Late Hyksos

In either case stratum d, and hence, incidentally, the date of the Minoan frescoes, would fall within the reign of Tuthmosis III.[111] Of the two schemes, the second is perhaps preferable since this allows time for a hiatus between c and b/3. The consequences of dating stratum d to early Tuthmosis III are immediately apparent in relation not only to the date of the levels at Kom Rabi^{c}a, but also to the dating of Saqqara NK 3507. From what has gone before it seems obligatory to date Memphis Level III to the late Eighteenth Dynasty, Level IV to the reign of Tuthmosis III, and Level V to the Eighteenth Dynasty no earlier than the reign of Amenophis I, and no later than the reign of Tuthmosis III, and, moreover, the sand layer at Memphis would have to be dated entirely within the Eighteenth Dynasty, as Eriksson has already presumed.[112] If RAT 305, 365 and 530 should date to the reign of Tuthmosis III, it follows that Saqqara NK 3507 must also come later in time, and together this has an impact on the Cretan and Cypriote pottery sequence found in Egypt.

The removal of a pre-Tuthmosis I context for the Late Minoan IB bridge spouted jug from RAT 530, and a re-dating of that context into the reign of Tuthmosis III places that LMIB sherd into the (generally) accepted position that no Late Minoan IB pottery is found in Egypt before the reign of Tuthmosis III.[113] It should also be pointed out that the Memphis sherd, *as long as it was dated prior to the end of the reign of Tuthmosis I* was the only secure Egyptian evidence for dating the beginning of LM IB before the reign of Tuthmosis III, since all other examples of LM IB sherds found in Egypt do not come from undeniably early contexts. In fact there is so little LM IB pottery found in Egypt, that Egypt should probably play no role in

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[108] Giddy, 1999, 73–74 no. 1111. Note that Giddy suggests that the plaque could have been made in the Nineteenth Dynasty, but as she says, this would contradict the stratigraphic position of the piece. For our purpose, what is important is that the piece can obviously be no earlier than the reign of Tuthmosis III.

[109] Giddy 1999, 262, pl. 57, stone vessel no. 1827.

[110] Bourriau, 1981, 40–41; B.G. Aston, 1994, 146–149.

[111] Hence the current position, Bietak, Dorner, Jánosi, 2001, 44–45, following pottery analysis by Hein. Since Tuthmosis II only reigned three years, it is probably unlikely that we will ever be able to differentiate pottery of the reign of Tuthmosis II from that of Tuthmosis III, thus a dating of the frescoes into the reign of Tuthmosis II cannot be entirely excluded

[112] Eriksson, 2001a, 56.

[113] Warren and Hankey, 1989, 138–142; Eriksson, in Bourriau and Eriksson, 1997, 100–101.

determining the dating of the LM IB, but nevertheless it is as well to review what little evidence there is.[114] An incomplete LM IB baggy alabastron, was found in Sedment tomb 137,[115] although unfortunately some confusion exists as to what was, and what was not, found in that tomb, since some of the objects (reputedly) from that tomb, now in Chicago, do not tally with the objects as recorded on Petrie's original tomb cards.[116] The tomb group itself cannot be dated. Petrie was of the belief that this part of Sedment was not used before the reign of Tuthmosis III, and indeed a scarab of this king was found in a neighbouring tomb, 279. In their discussion of the tomb contents Kemp and Merrillees conclude that the contents of tomb 131 seem to belong to the first part of the Eighteenth Dynasty, and in any case cannot be earlier than the beginning of this Dynasty.[117] A second sherd comes from Abydos tomb 328,[118] but once again this tomb group is not independently datable. Moreover there is a possibility of mixing of the contents with tomb 301, since access to 328 may have been gained from 301 by means of a robbers' tunnel. Nevertheless two Egyptian pots, one from 328 and one from 301 – no others are listed on Garstang's notes – are well known types which are found both during the Late Second Intermediate Period, and during the early Eighteenth Dynasty, up to and including the reign of Tuthmosis III. A date in the Hyksos Period with Kemp and Merrillees[119] is thus possible but not proven. No other definite LM IB pieces have been found in Egypt, though there are three other famous pieces, which, as Kemp and Merrillees rightly point out, can be considered as LM IB or Late Helladic IIA depending on the academic bias of the beholder, and are thus best left out of the discussion.[120]

The dating of stratum d at Ezbet Helmi, and its consequent effect on the Kom Rabi^{c}a Levels III and IV, however, does play a significant role in the dating of the Late Cypriot pottery sequence as it is found in Egypt. The sequence shown above (page 000), shows, that at Ezbet Helmi, Proto White Slip is first found in stratum e/2 and continues into e/1. Base Ring I may occur in stratum e/1.1,[121] though it is not sure that the sherd in question is really Base Ring ware, whilst White Slip I, definite Base Ring I, and Red Lustrous Wheel Made Ware first appear in stratum d, continuing into stratum c and beyond whilst Base Ring II and White Slip II first appear in stratum c. It should here be noted that the dates used by Louise Maguire in her 1995 paper which correlates the Late Cypriot wares with the Late Hyksos levels at Ezbet Helmi[122] is based on the first preliminary dating of these levels, and must be abandoned. The presence of a possible Base Ring I in an e/1.1 context, and unquestionable Base Ring ware not before stratum d tends to favour the hypothesis of Oren and Eriksson that Base Ring Ware is not found in Egypt, until, or shortly before, the reign of Tuthmosis III (cf. above p. 000). Contra such a position, however, is the Base Ring jug found below the sand level at Kom Rabi^{c}a and the report of a Base Ring sherd from Tell el-Dab^{c}a area A/II stratum D/2.[123] The Kom Rabi^{c}a Base Ring jug has become famous although neither the piece in question nor its context has been published.[124] Since it comes from below the sand layer it has always been dated to the Second Intermediate Period, however if the sand, as argued here, was not deposited until some time in the Eighteenth Dynasty, the vessel could be of Eighteenth Dynasty date and still lie below the sand. Moreover, Merrillees points out that the vessel comes from a rubbish dump,[125] which thus may have been tipped there just before the sand was dumped in order to make a clean foundation for the new buildings of Level IV. Moreover in a discussion of this context with Janine Bourriau and Dorothea Arnold in Vienna in 1996, the profile appears to show that this vessel comes from tip lines behind a wall which indeed were sealed by the sand, but if the tip lines were cut by the sand, then, as Dorothea pointed out, the vessel could have travelled down to its current position from a higher level. In any case until both the pot and the

[114] Cf. Kemp and Merrillees, 1980, 226–249; Warren and Hankey, 1989, 138–144; Manning, 1999, 202–208.

[115] Petrie and Brunton, 1924, pls. xlviii–xlix; Kemp and Merrillees, 1980, 228–231.

[116] Kemp and Merrillees, 1980, 229–231. Personally I suspect that the wooden Sons of Horus supposedly found in this tomb actually came from tomb 131 cf. Aston, 1996, 40.

[117] Kemp and Merrillees, 1980, 231, 253.

[118] This tomb group was not published in the report of the excavator, Garstang, but see Kemp and Merrillees 1980, 232–240.

[119] Kemp and Merrillees, 1980, 240, 253.

[120] Kemp and Merrillees, 1980, 240–245; Warren and Hankey, 1989, 144.

[121] Bietak, 2003, 26.

[122] Maguire, 1995, 63.

[123] Maguire, 1992, 117 fig. 2.

[124] The jug is mentioned, *inter alia* by, Merrillees, 1992, 50–51; Idem. 2001, 27; Eriksson, 1992, 169, Eadem, 2001a, 56; Bietak, 1986, 86 n. 85; Manning, 1999, 120 and notes 552–557; Idem, 2001, 76, 86; Bergoffen, 2001, 46; Bietak and Hein, 2001, 172 n.5.

[125] Merrillees 1992, 51.

archaeological context are fully published, it seems archaeological nonsense to build chronological theories on this one pot, and it should not be considered in any discussions of Base Ring Ware found in Egypt. As for the reputed stratum D/2 Tell el-Dab^{c}a piece, TD 5054, it comes from A/II-p/22 planum 1, in a context, K2581, which is mixed with New Kingdom sherds, and thus the date of the Base Ring fragment cannot be determined. In any case stratum D/2 on the tell lay immediately below the surface, and is not a good stratum in terms of well-dated contexts. Indeed D/2 deposits which come from the tell are probably best avoided, no matter what kind of pottery is under discussion, but in terms of Base Ring ware, it is perhaps significant that in area A/V where the D/2 strata were better preserved no Base Ring sherds were found in that stratum.[126] Thus the Ezbet Helmi evidence is strongly suggestive of the fact that Proto White Slip was already present in Egypt during the Hyksos Period, that Base Ring I Ware appeared shortly before the reign of Tuthmosis III; and that White Slip I probably did not arrive before the reign of Tuthmosis III, whence it arrived along with Red Lustrous Wheel Made Ware; and that Base Ring II and White Slip II first appeared late in the same reign. However should Eriksson be proved right in that Base Ring II is not found in Egypt until after the reign of Tuthmosis III,[127] then stratum c must extend later in time, or Scheme One of the two proposed chronological schemes outlined on page 000 would have to be adopted. This, whilst moving the earliest Base Ring found at Ezbet Helmi into the reign of Tuthmosis III, would, however, move the Minoan frescoes well on into the same reign, something which our Aegeanist colleagues would perhaps find too hard to swallow. The hiatus between strata b and c would also come down to practically nothing, thus it seems on the whole better to assume that Base Ring II was indeed beginning to appear late in the reign of Tuthmosis III,[128] even though it may not have become common until the reign of Amenophis III. Since in overall terms the amount of Cypriote sherds found at Ezbet Helmi are numerically small, and because they are broken and therefore sherdage, it may be argued that this does not mean much.[129] However, the fact that they occur in the correct ceramic sequence is suggestive and surely cannot be mere chance. Since the same sequence for Cypriot imports can be traced in Palestine, it is probable that this also reflects both their floruit and their date of production on Cyprus itself.

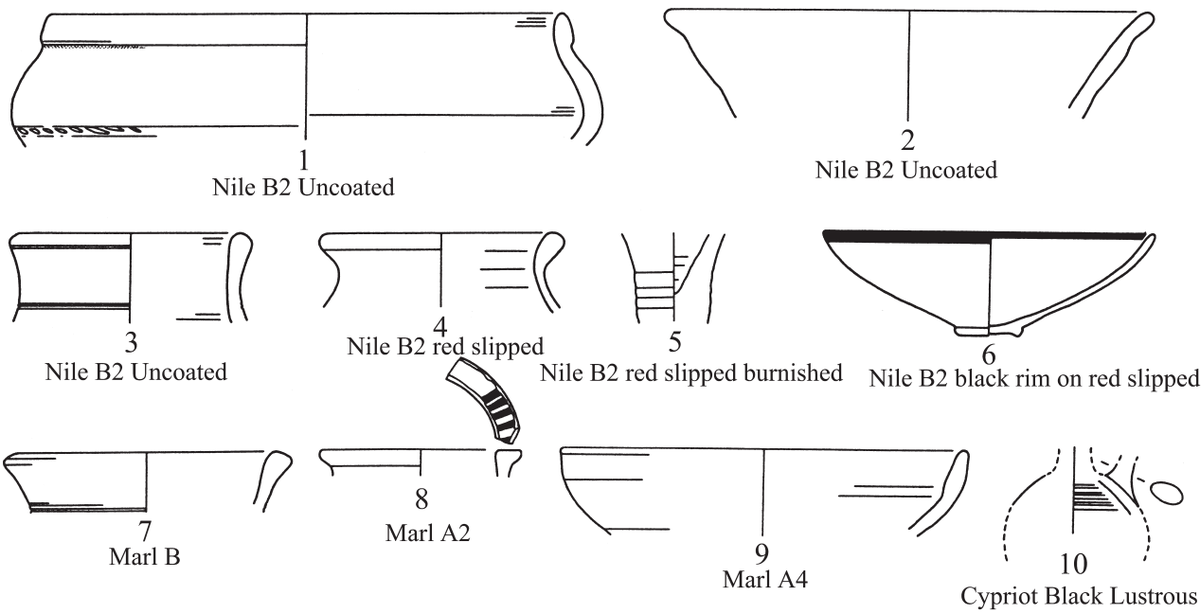


Fig. 3 Kom Rabi^{c}a RAT 305–365, 1:4
(after Bourriau 1991a, 135)

[126] Maguire, 1992, 117, figs. 2–3.
[127] Eriksson, 2001a, 65.
[128] Cf. also Bergoffen, 2003, 405.
[129] Cf. Merrillees, 2001, 28.

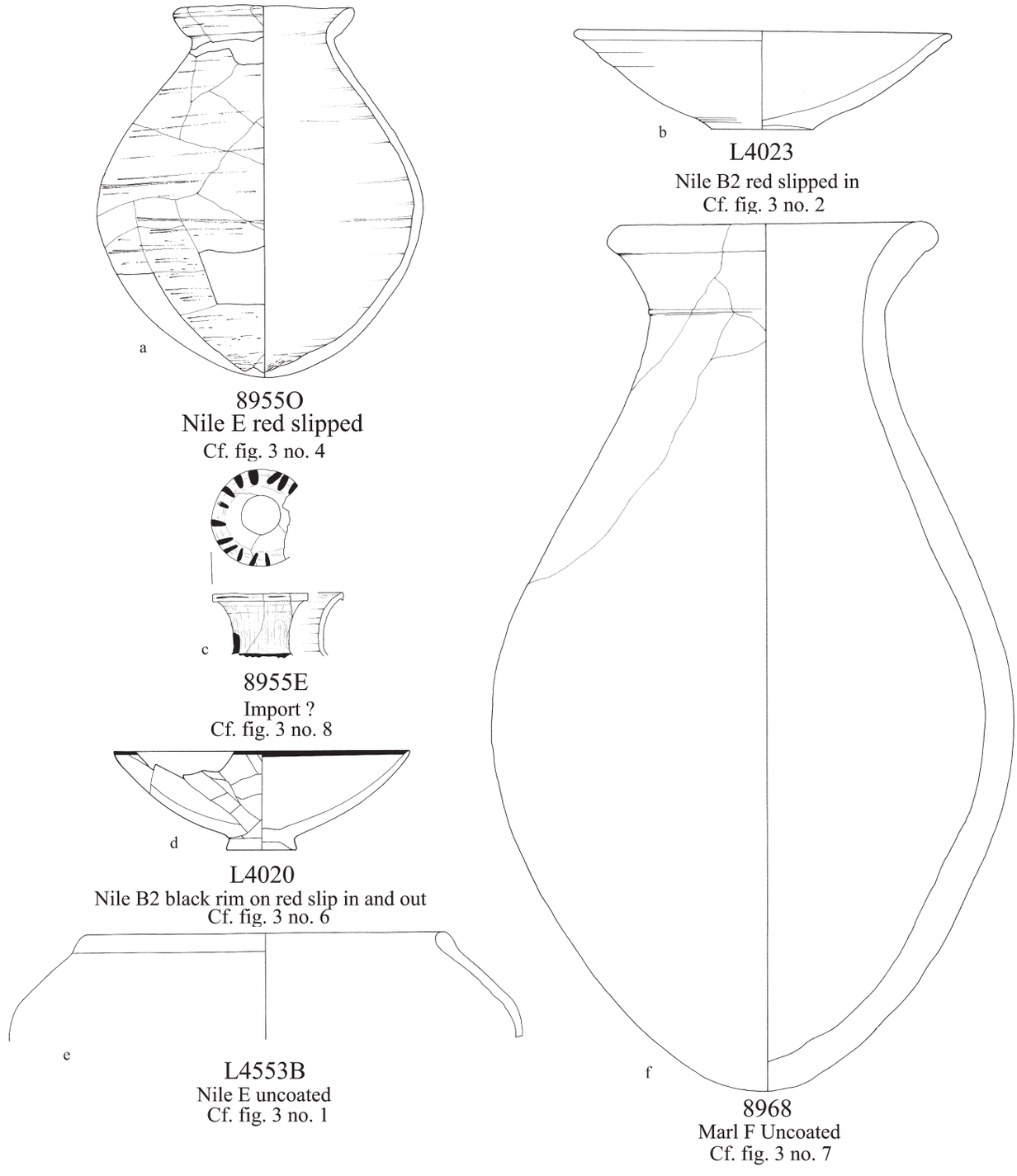


Fig. 4 Ezbet Helmi H/VI, stratum c – various contexts 1:4

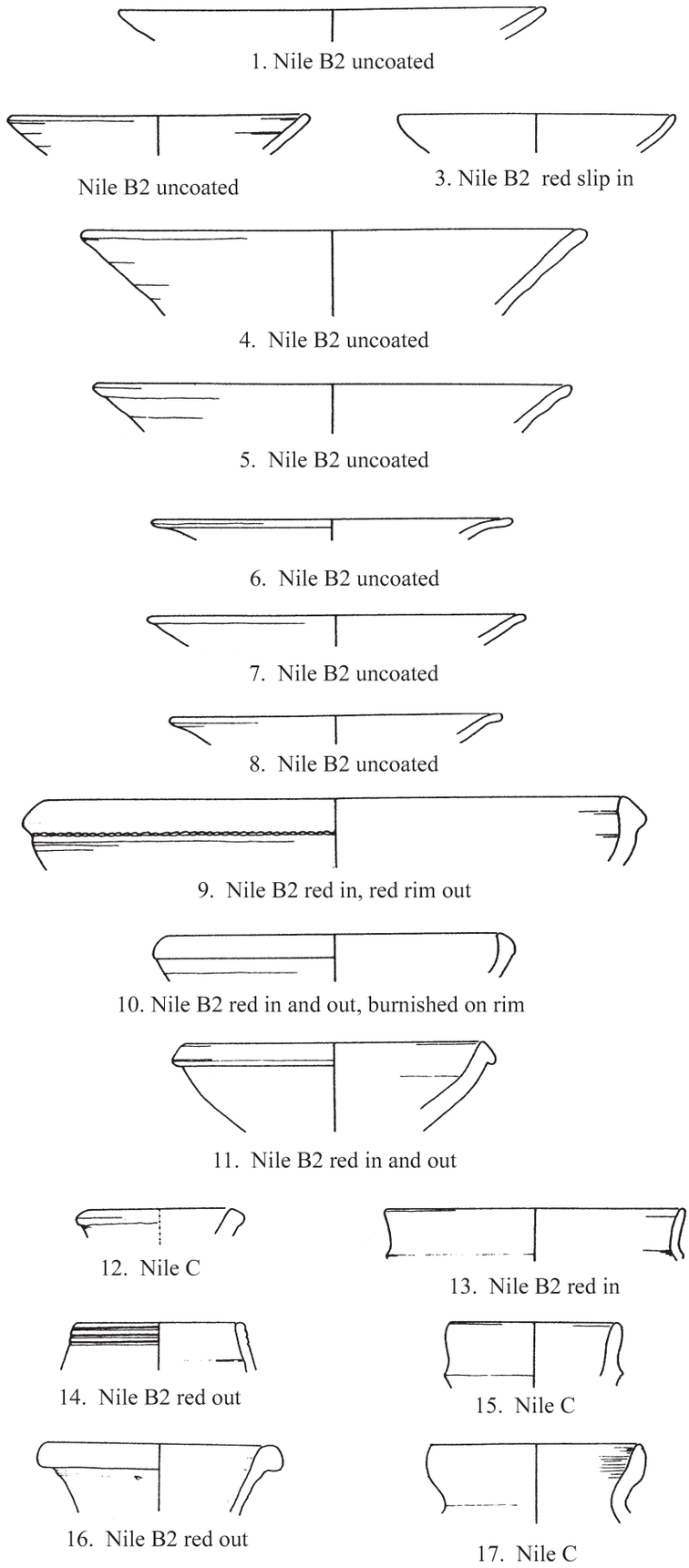


Fig. 5 Tomb Group NK 3507 1:4 (after Bourriau/Eriksson 1997, 118)

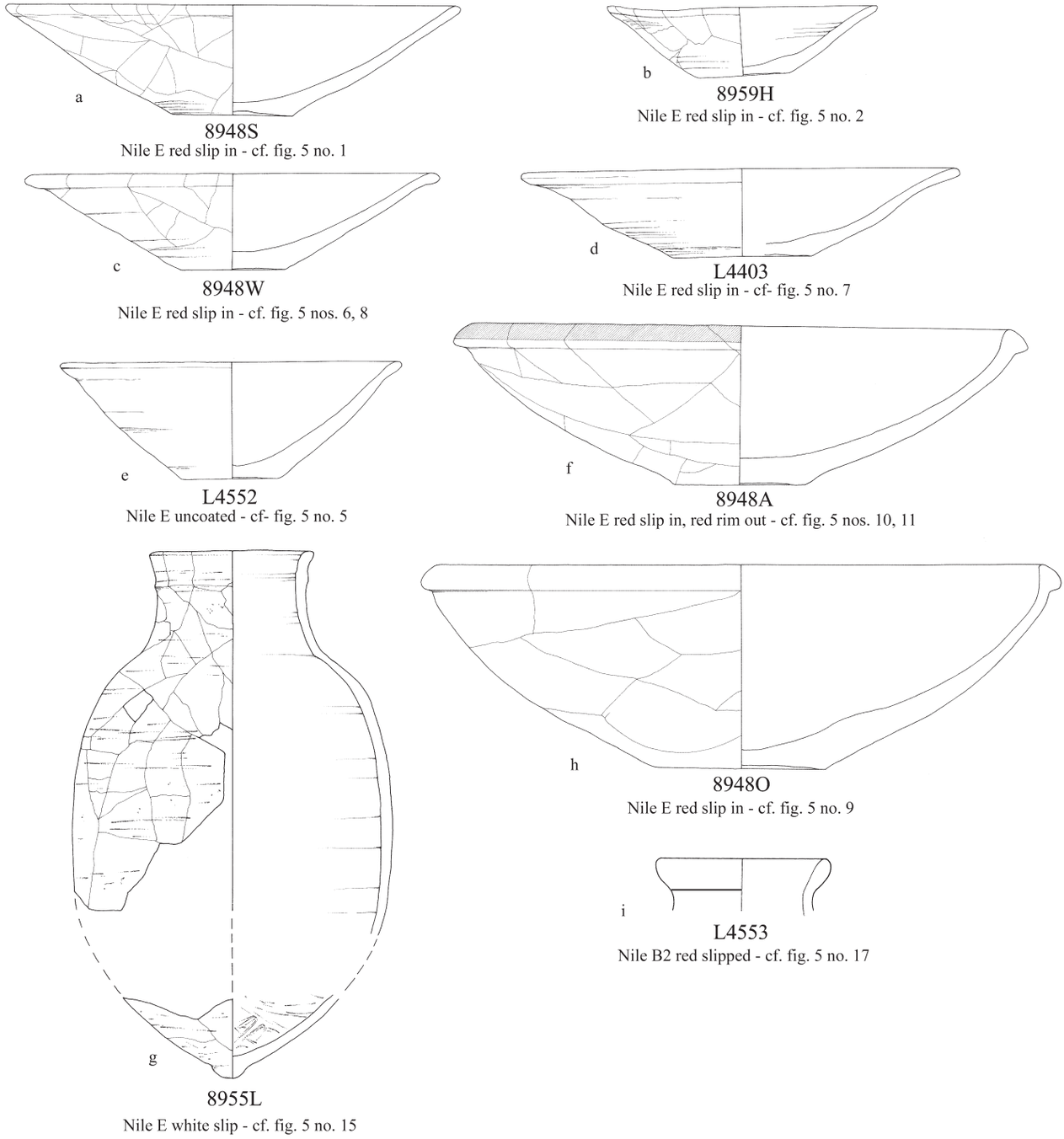
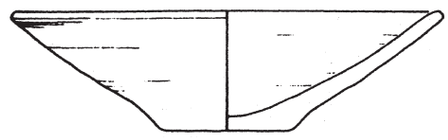
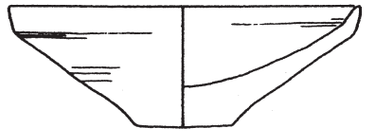


Fig. 6 Ezbet Helmi H/VI, stratum c – various contexts 1:4



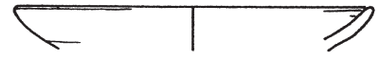
1. Nile B2 uncoated



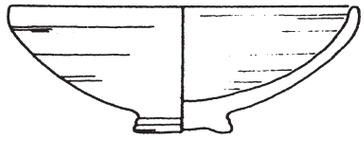
2. Nile B2 red slip in and out, ring burnished in



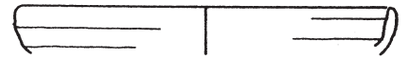
3. Nile B2 black rim on red slip in and out



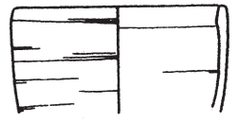
4. Marl B uncoated



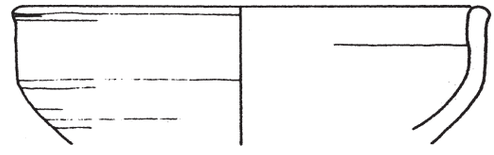
5. Nile B2 red slip in and out



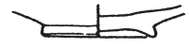
6. Nile B2 red slip in and out, burnished in



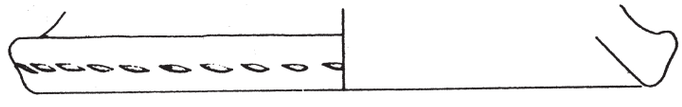
9. Nile B2 uncoated



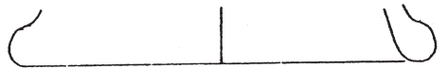
7. Nile B2 red rim on uncoated



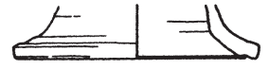
8. Nile B2 uncoated - base to 7



10. Nile C uncoated

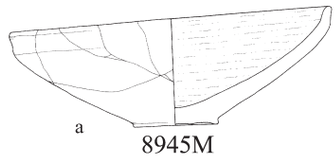


11. Nile C uncoated



12. Nile B2 red slip out

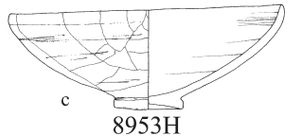
Fig.7 Kom Rabi^{c}a RAT 452 Purposive sample 1:4 (after Bourriau/Eriksson 1997, 119)



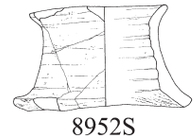
a
8945M
Nile B2 red slip, ring burnished in
cf- fig. 7 no. 6



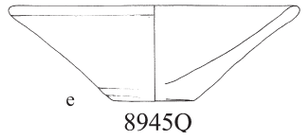
b
8951
Nile B2 red slip in and out
- cf. fig. 7 no. 2



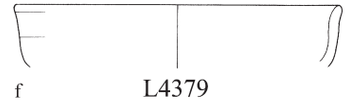
c
8953H
Nile E red slip in and out
cf. fig. 7 no. 5



d
8952S
Nile E uncoated - cf. fig. 7 no. 12



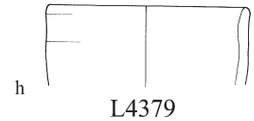
e
8945Q
Nile E uncoated - cf. fig. 7 no. 1



f
L4379
Nile E red slip in and out
- cf. fig. 7 no. 7

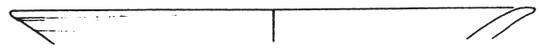


g
L4023
Nile E black rim on red slip in and ou
- cf. fig. 7 no. 3



h
L4379
Nile E uncoated - cf. fig. 7 no. 9

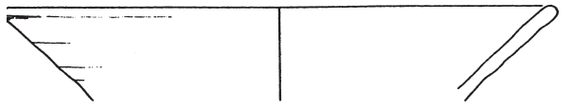
Fig. 8 Ezbet Helmi H/VI, stratum c – various contexts 1:4



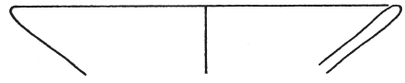
1. Nile B2 uncoated



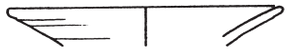
2. Nile B2 uncoated



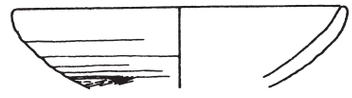
3. Nile B2 uncoated



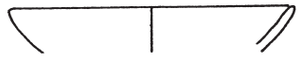
4. Nile B2 uncoated



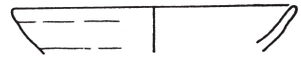
5. Nile B2 red slip in and out



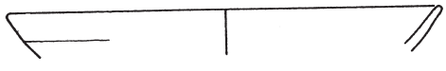
6. Nile B2 red rim on red slip in and out



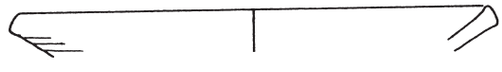
7. Nile B2 red slip in and out, ring burnished



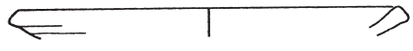
8. Nile B2 red slip in and out, ring burnished in



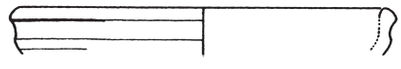
9. Nile B2 uncoated



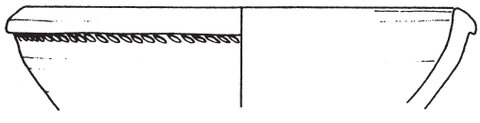
10. Nile B2 uncoated



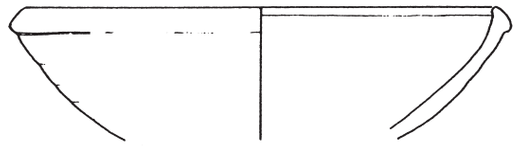
11. Nile B2 red rim on uncoated



12. Nile B2 uncoated



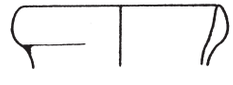
13. Nile B2 red slip in, red rim out



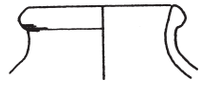
14. Nile B2 red slip in and out, burnished in



15. Nile B2 uncoated



16. Nile C uncoated



17. Nile B2 uncoated



18. Nile B2 red slip in and out

Fig. 9 Kom Rabi^{c}a RAT 530 Random sample 1:4 (after Bourriau/Eriksson 1997, 114)

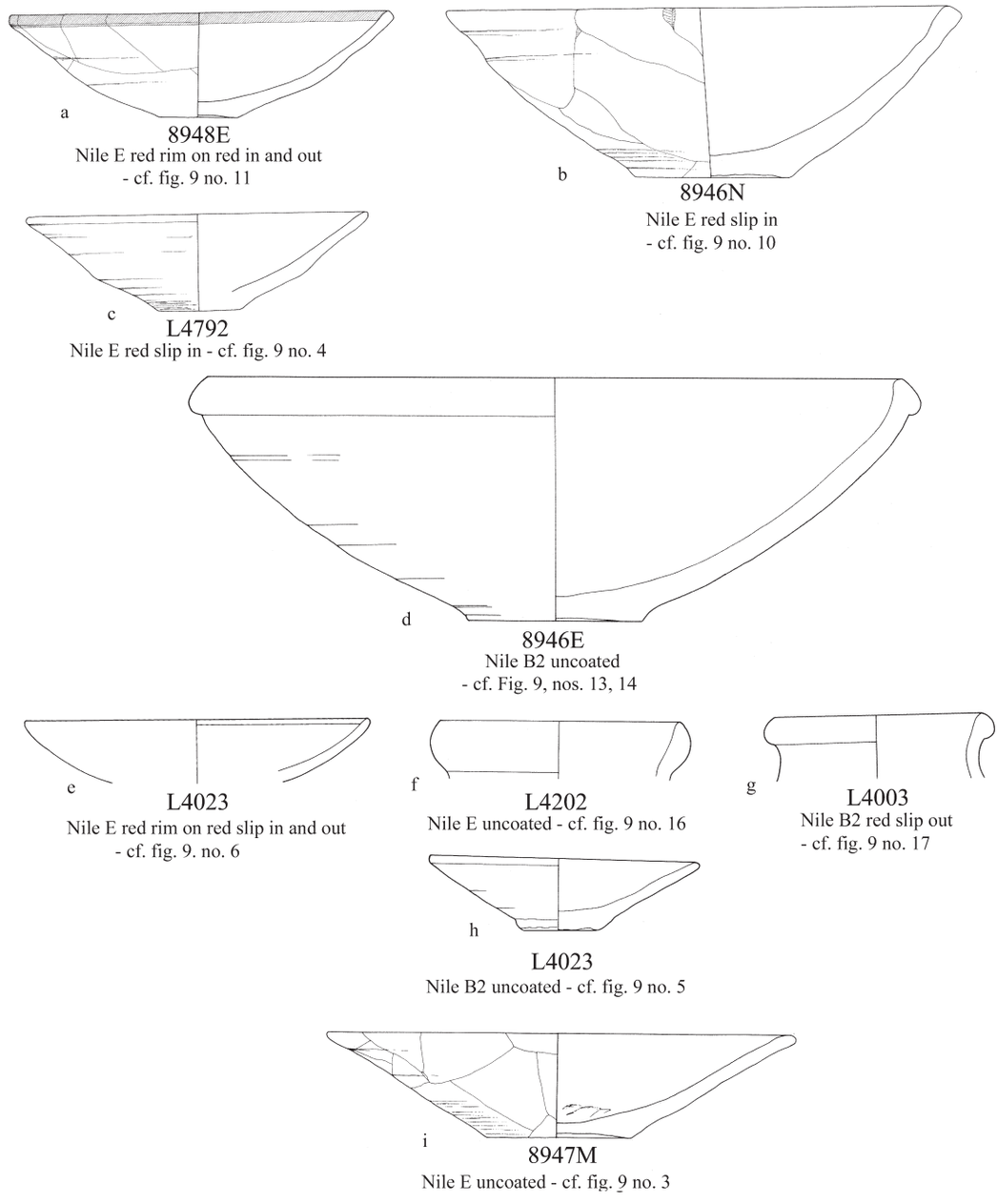
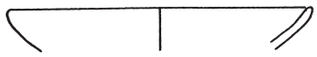
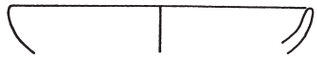


Fig. 10 Ezbet Helmi H/VI, stratum c – various contexts 1:4

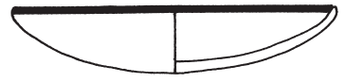




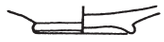
2. Nile B2 black rim on red slip in and out



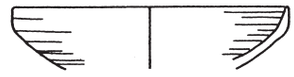
3. Marl B uncoated



5. Nile B2 black rim on red slip in and out



4. Marl B uncoated



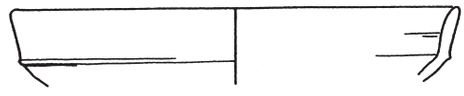
7. Nile B2 red slip, ring burnished in



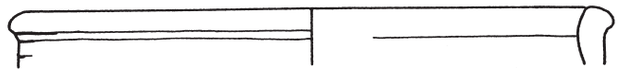
6. Nile B2 black rim on red slip in and out



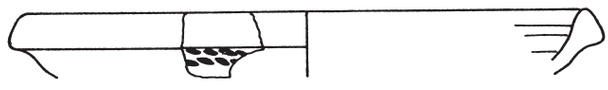
8. Nile B2 black rim on red slip in and out



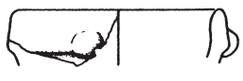
9. Nile B2 uncoated



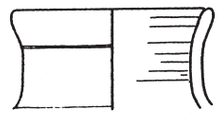
10. Nile B2 red rim on red slip in, ring burnished



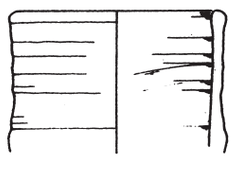
11. Nile B2 red rim on red slip in and out



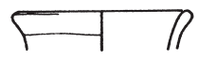
12. Marl A3



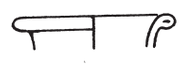
13. Marl B uncoated



14. Marl B uncoated



15. Nile B2 uncoated

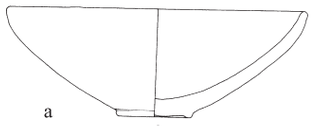


16. Marl A4 uncoated



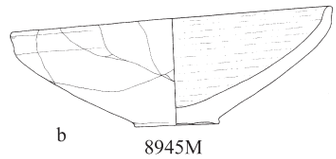
17. Marl A2, black decoration

Fig. 11 Kom Rabi^{c}a RAT 530 Purposive sample "New Kingom Style" 1:4 (after Bourriau/Eriksson 1997, 116)



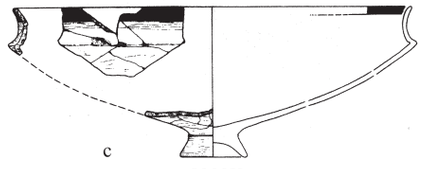
a

8945V
Nile E red slip in and out
cf. fig. 11 no. 1



b

8945M
Nile E red slip in
- cf. fig. 11 no. 7



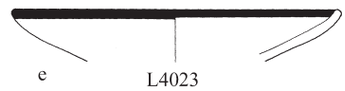
c

8909K
Nile E black rim on red slip in and out, burnished
cf. fig. 11 no. 8



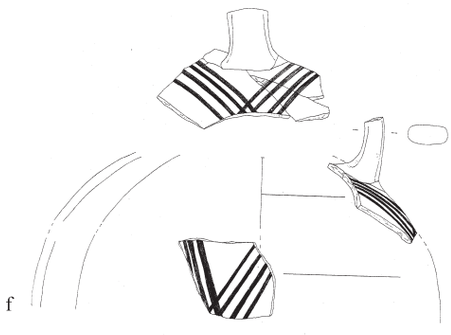
d

L4552
Nile B2 black rim on red slip in and out
- cf. fig. 11 no. 2



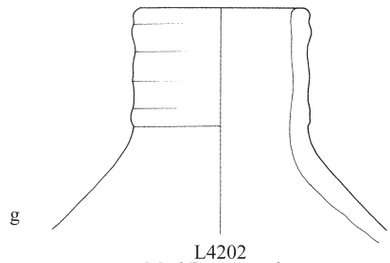
e

L4023
Nile E black rim on red slip in and out
- cf. fig. 11 no. 5



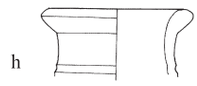
f

8947E
Marl, black decoration
- cf fig. 11 no. 17



g

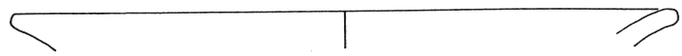
L4202
Marl B uncoated
- cf. fig. 11 no. 14



h

L4379
Marl A4 uncoated
- cf. fig. 11 no. 16

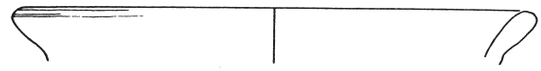
Fig. 12 Ezbet Helmi H/VI, stratum c – various contexts 1:4



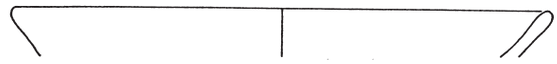
1. Nile B2 uncoated



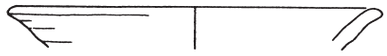
2. Nile B2 uncoated



3. Nile B2 uncoated



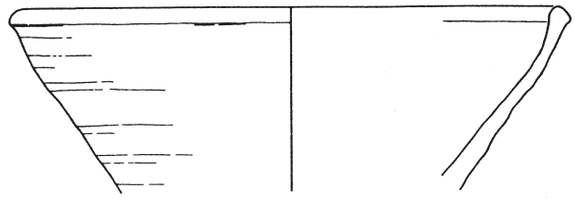
4. Nile B2 red slip in and out



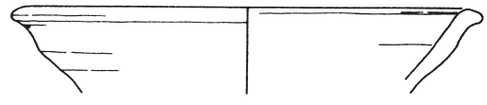
5. Nile B2 uncoated



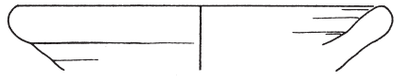
6. Nile B2 uncoated



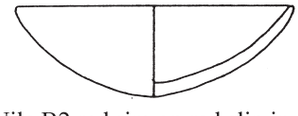
7. Nile B2 red rim on uncoated



8. Nile B2 uncoated



9. Nile B2 uncoated



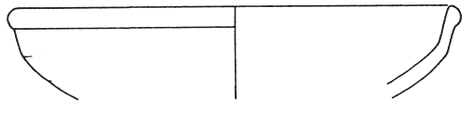
10. Nile B2 red rim on red slip in and out



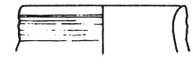
11. Nile B2 black rim on red slip in and out



12. Nile B2 black rim on red slip in and out

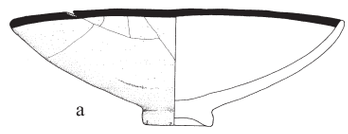


13. Nile B2 red slip in and out, burnished in

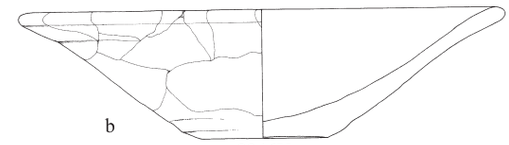


14. Nile B2 red slip out

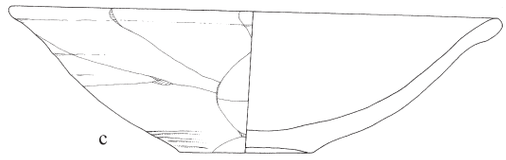
Fig. 13 Kom Rabi^{c}a RAT 530 Random sample 1:4 (after Bourriau/Eriksson 1997, 112)



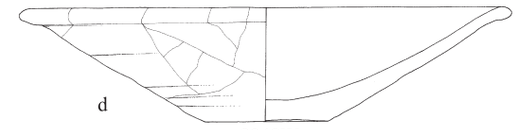
a
8908R
Nile E black rim on red slip in and out
- cf. fig. 13 no. 12



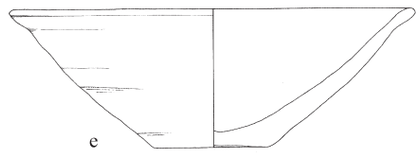
b
8946U
Nile E uncoated - cf. fig. 13 nos. 1-2



c
8946P
Nile E red slip in - cf. fig. 13 no. 3



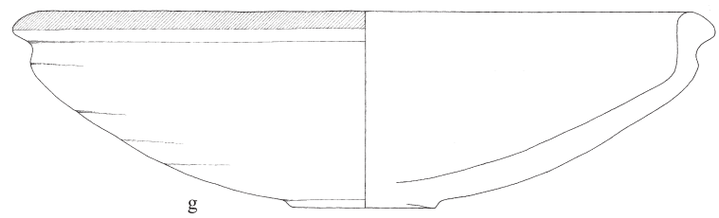
d
8948W
Nile E red slip in - cf. fig. 13 no. 6



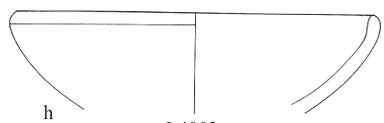
e
L4552
Nile E uncoated - cf. fig. 13 no. 5



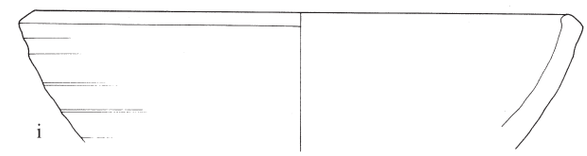
f
L4020
Nile B2 red rim on uncoated out, red slip in
- cf. fig. 13 no. 8



g
L4829
Nile E red rim on uncoated out, red slip in
- cf. fig. 13 no. 13

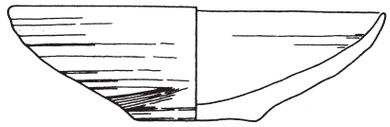


h
L4003
Nile E red rim on uncoated out, red slip in
- cf. fig. 13 no. 10

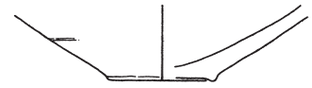


i
L4403
Nile E red slip in - cf. fig. 13 no. 7

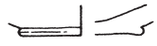
Fig. 14 Ezbet Helmi H/VI, stratum c – various contexts 1:4



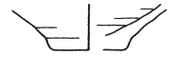
1. Nile B2 red rim on red slip in and out



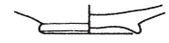
2. Nile B2 red in, ring burnished



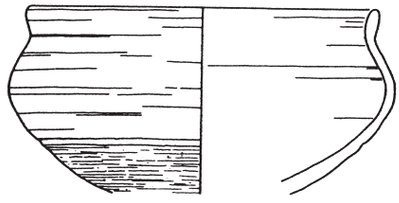
3. Marl B uncoated



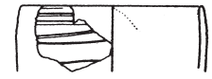
4. Nile B2 uncoated



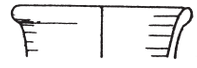
5. Nile B2 red slip burnished



6. Nile B2 red slip out



7. Nile B2 red rim on red slip out



8. Marl A4 uncoated



9. Marl A2 dark red decoration



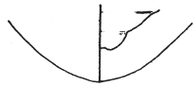
10. Nile B2 red slip



11. Marl A2 black decoration



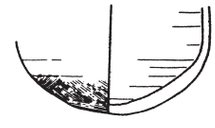
12. Marl A4 black decoration



13. Nile B2 uncoated



14. Nile B2 uncoated



15. Nile B2 uncoated

Fig. 15 Kom Rabi^{c}a RAT 536 Purposive sample 1:4 (after Bourriau/Eriksson 1997, 113)

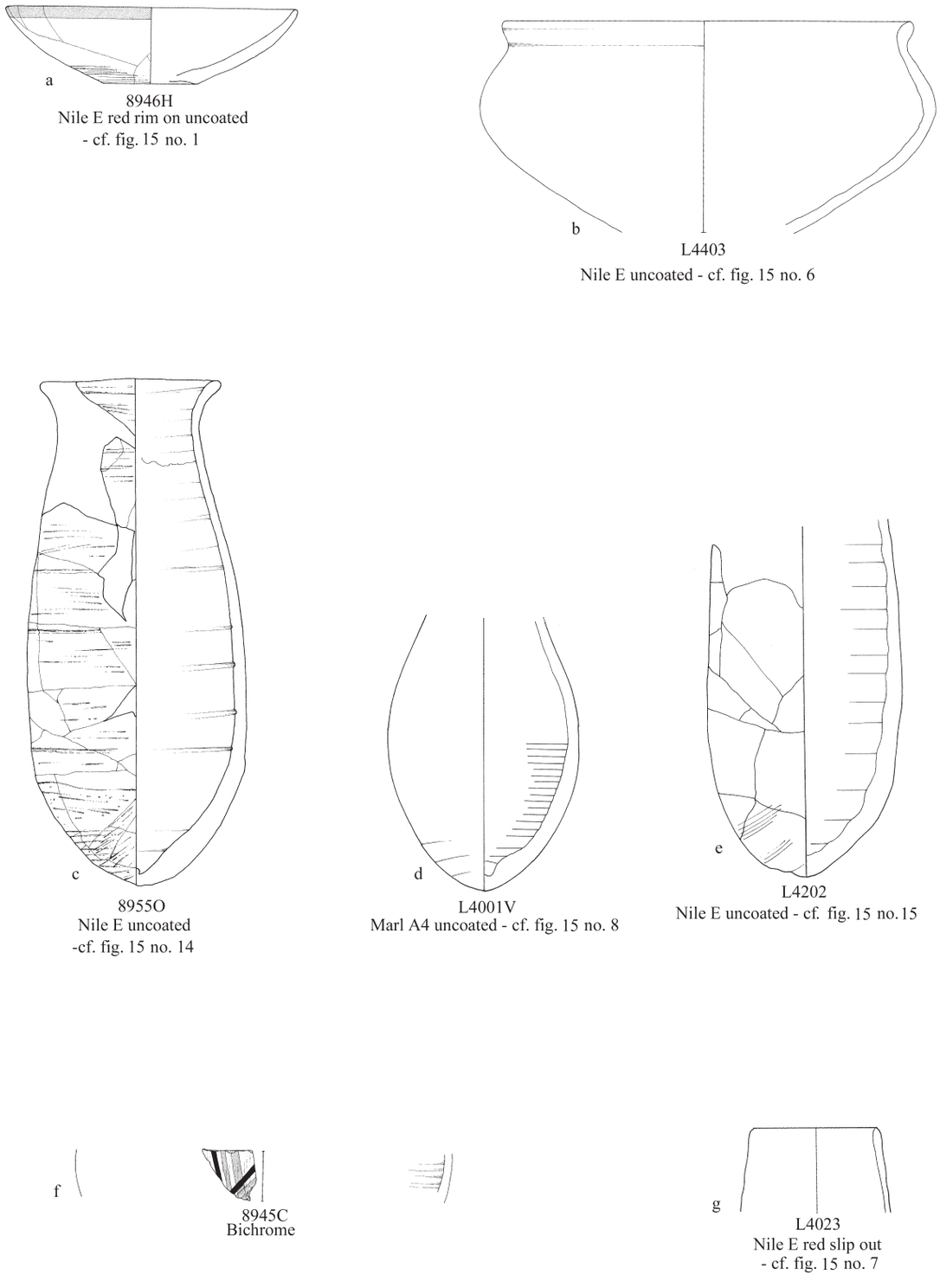
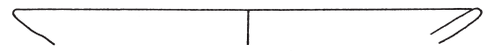
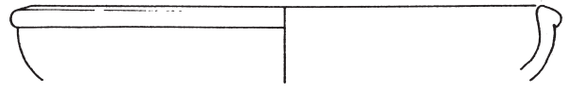


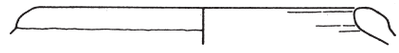
Fig. 16 Ezbet Helmi H/VI, stratum c – various contexts 1:4



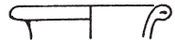
1. Nile B2 red slip in



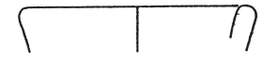
2. Marl B uncoated



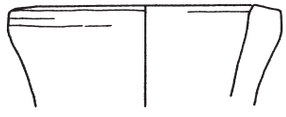
3. Nile E uncoated



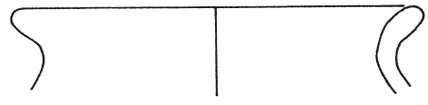
4. Marl A4 uncoated



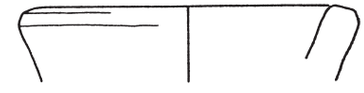
5. Marl C uncoated



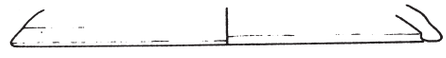
6. Bread mould



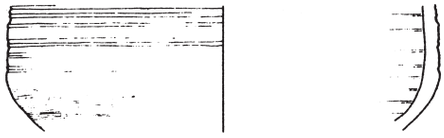
8. Nile B2 white slip out



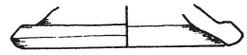
7. Bread Mould



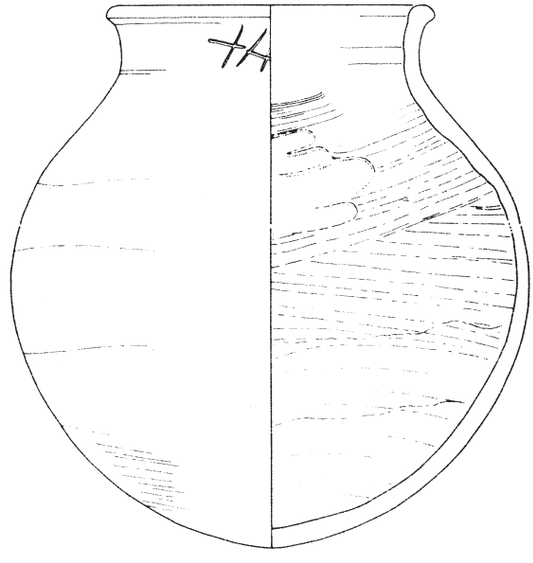
9. Nile B2 uncoated



10. Nile B2 red slip in and out

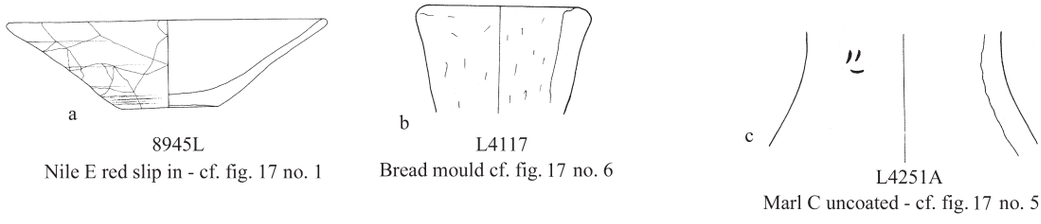


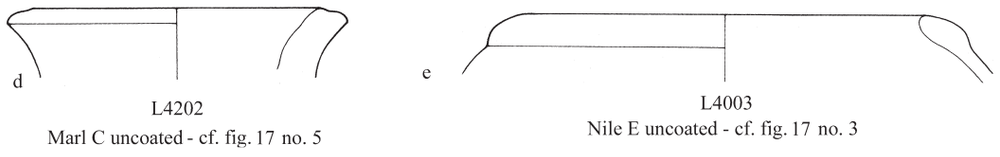
11. Nile B2 red slip out

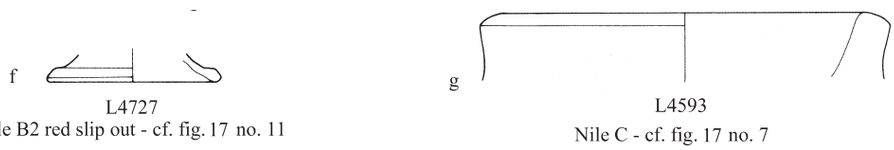


12. Marl C uncoated

Fig. 17 Kom Rabi^{c}a RAT 531, Random and purposive samples 1:4 (after Bourriau/Eriksson 1997, 110–111)







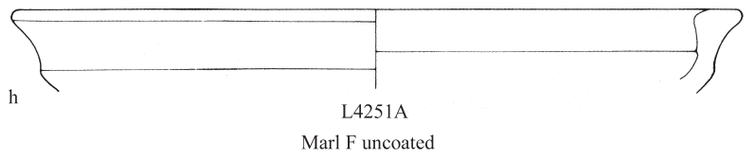


Fig. 18 Ezbet Helmi H/VI, stratum c – various contexts 1:4

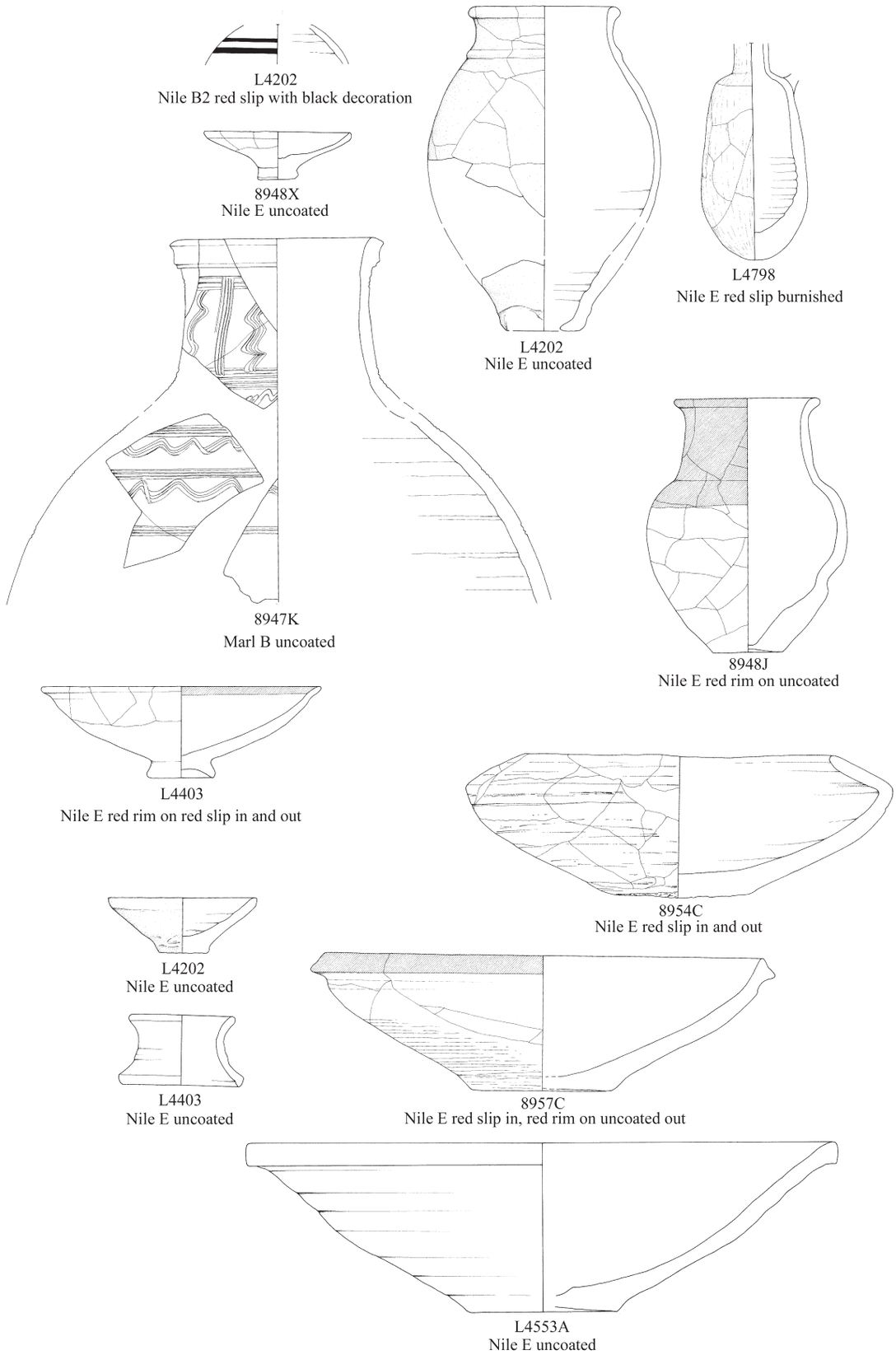
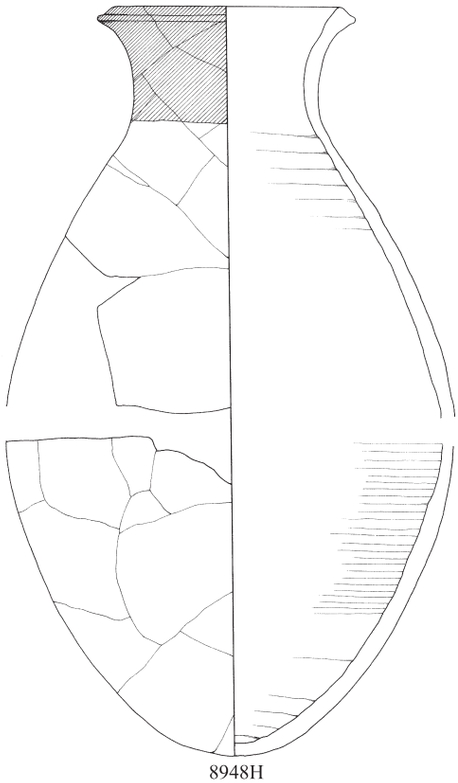
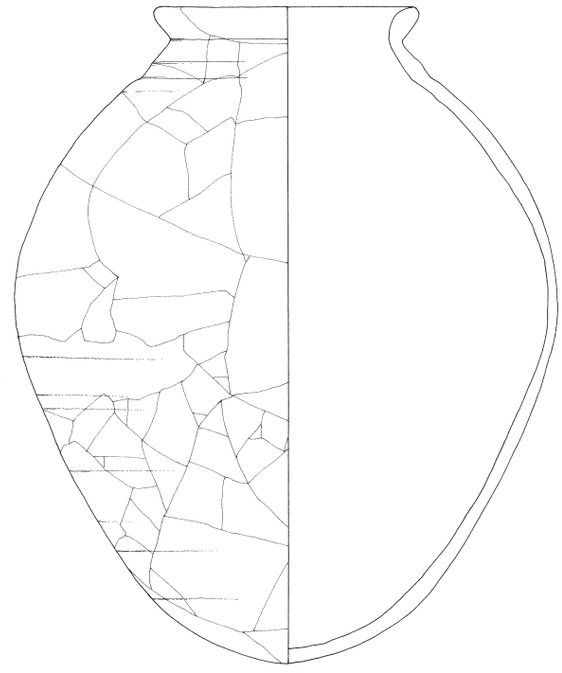


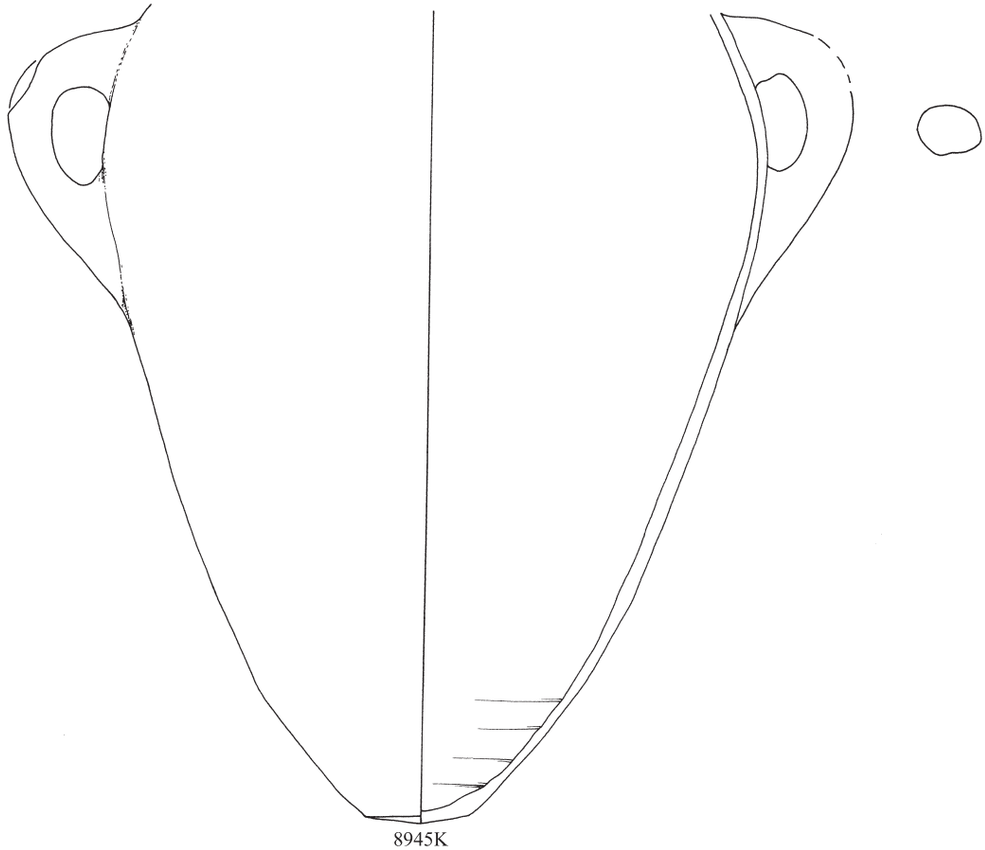
Fig. 19 Ezbet Helmi H/VI, stratum c – more typical vessels, various contexts 1:4



8948H
Nile E red rim on uncoated



8946K
Marl F uncoated



8945K
Marl D cream coated

Fig. 20 Ezbet Helmi H/VI, stratum c – more typical vessels, various contexts 1:4

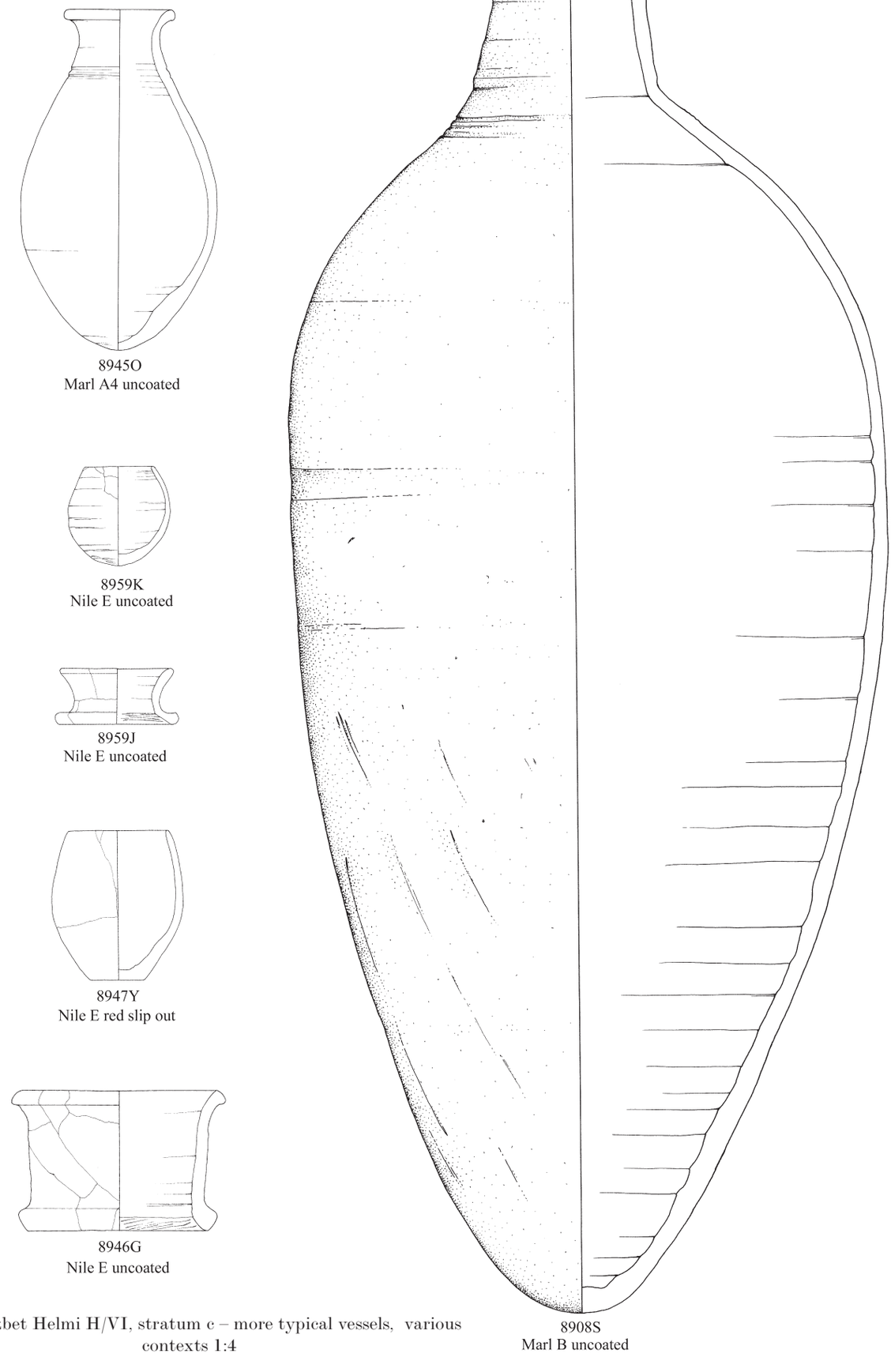


Fig. 21 Ezbet Helmi H/VI, stratum c – more typical vessels, various contexts 1:4

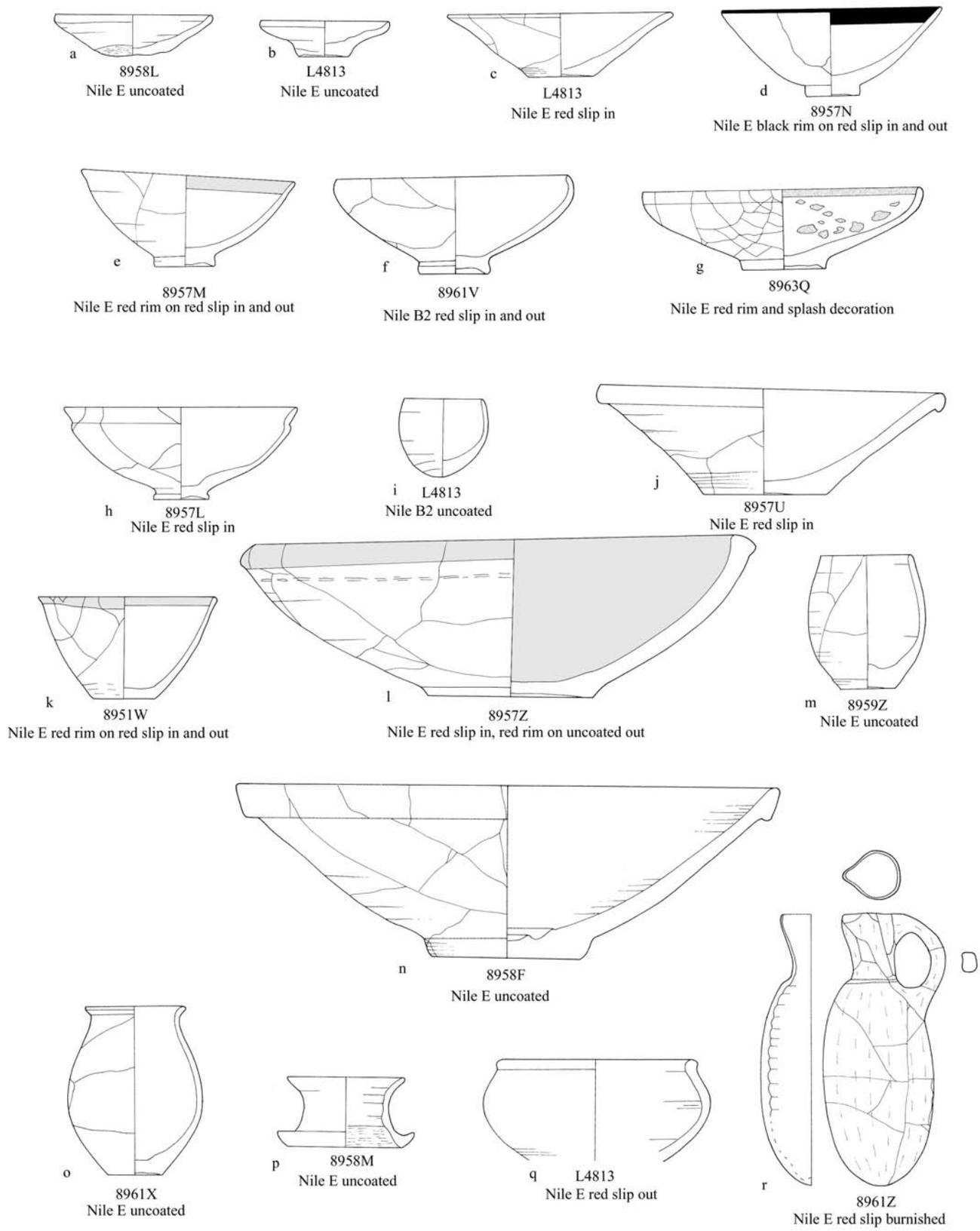


Fig. 22 Ezbet Helmi H/VI, stratum d – typical vessels, 1:4

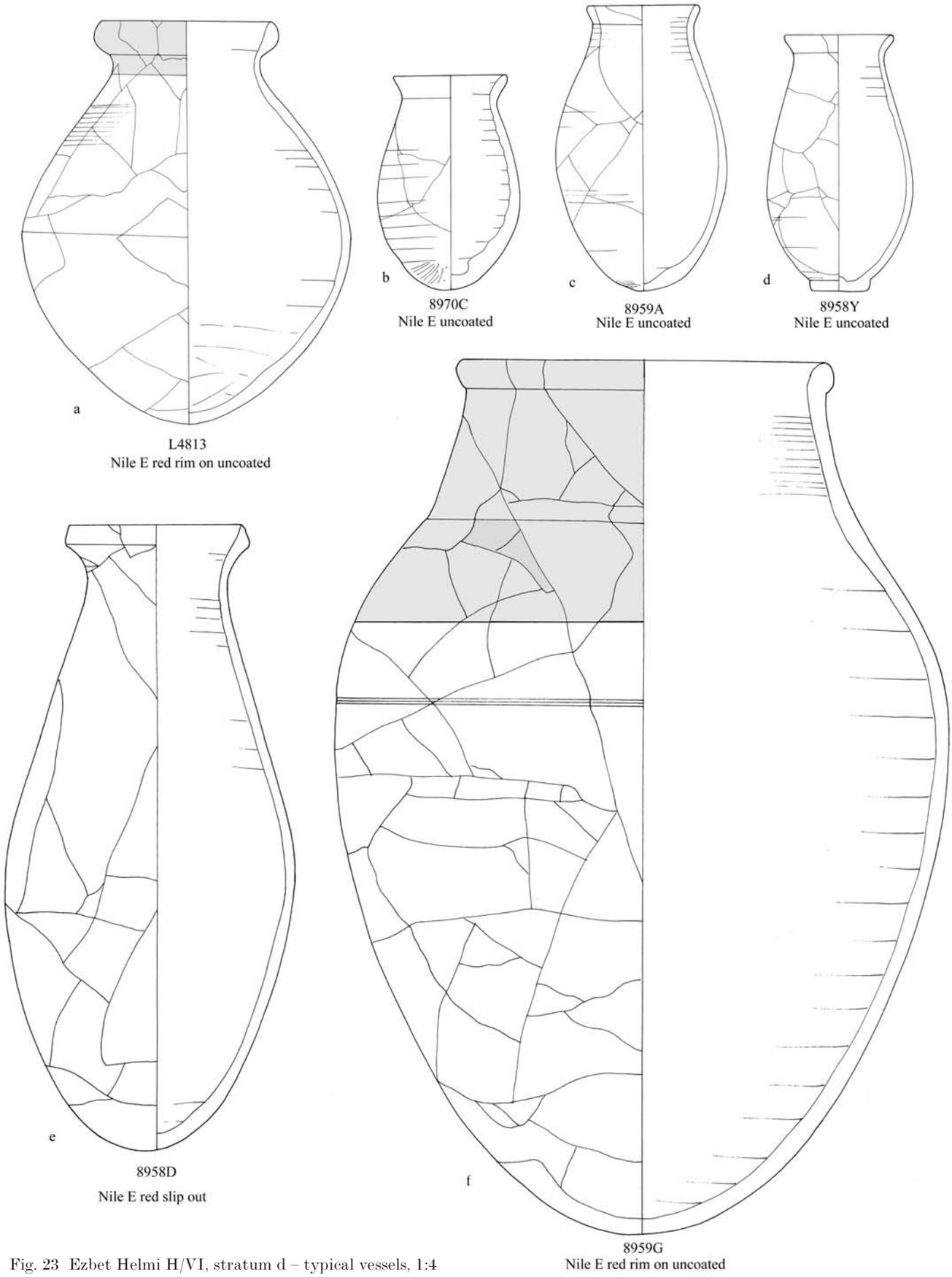


Fig. 23 Ezbet Helmi H/VI, stratum d – typical vessels, 1:4

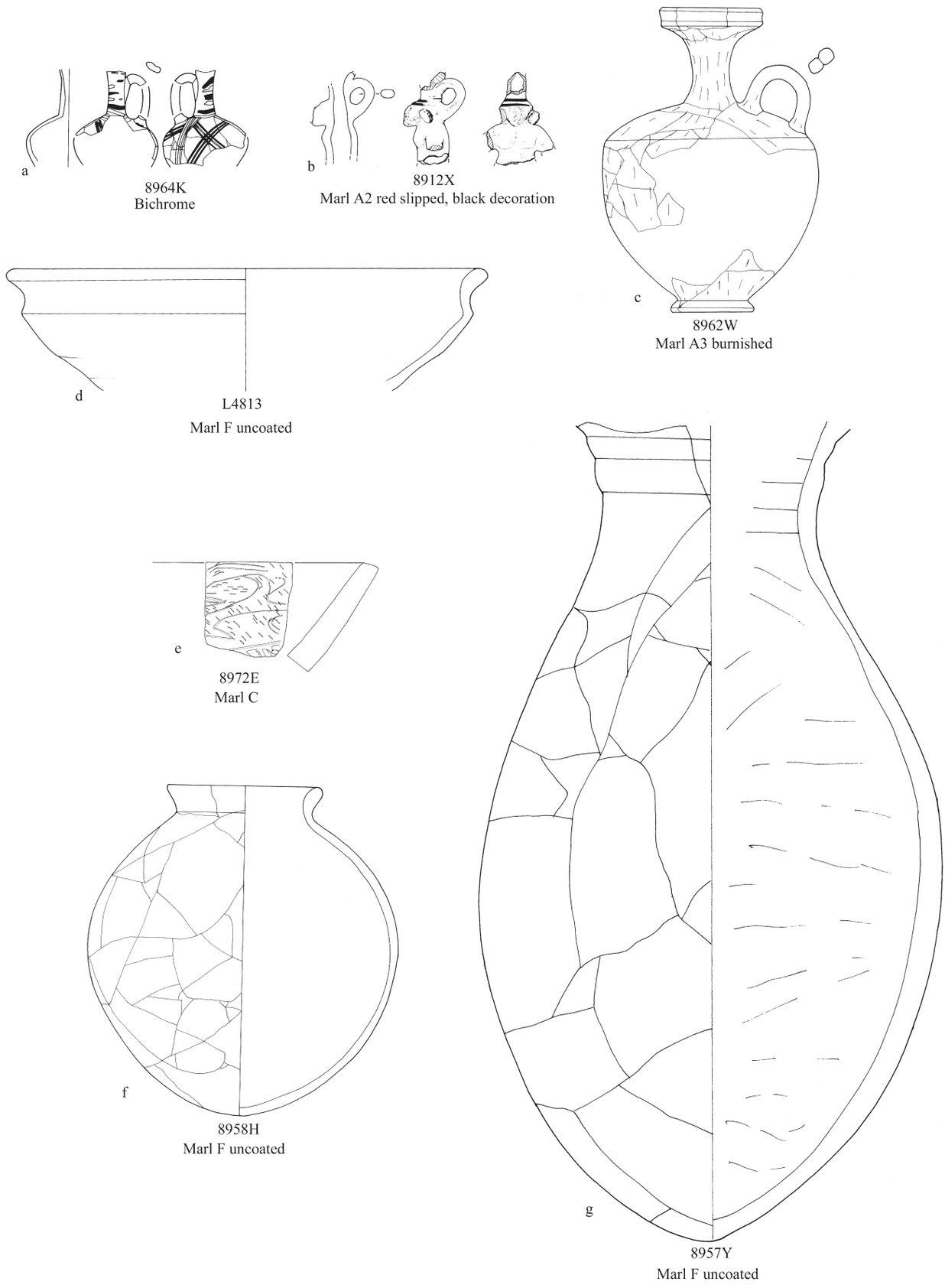


Fig. 24 Ezbet Helmi H/VI, stratum d – typical vessels, 1:4

Postscript

In writing this article I ignored, perhaps too cavalierly, the fact that pottery styles changed at different times in different parts of the country so that, for example, new pottery fashions at Kom Rabᶜia could have evolved earlier than they did at Ezbet Helmi. There is surely no doubt among ceramicists that pottery of the Tuthmosid era developed from a southern (Upper Egyptian) tradition, and thus, from a purely diffusionist point of view this new style ought to have reached Kom Rabᶜia before it reached Ezbet Helmi. If this new (New Kingdom style) pottery arrived at Kom Rabᶜia, say one generation before it was adopted at Ezbet Helmi, then one could accept an earlier date for the Kom Rabᶜia levels and a later date for those levels with the same material at Ezbet Helmi, thus justifying the preliminary dating given to both sites. However, whilst this might satisfactorily explain the similarities between the native Egyptian ceramics, it does not work when one considers the imported pottery, since one would have to postulate that the Cypriot pottery types found, both at Kom Rabᶜia and Ezbet Helmi, also reached Kom Rabᶜia one generation before they reached Ezbet Helmi. For this reason I believe that the Kom Rabᶜia and Ezbet Helmi levels are contemporary with one another whether, in real time, that is the high chronology of Kom Rabᶜia or the low chronology of Ezbet Helmi.

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A Tale of Two Cities:[1] First Results of a Comparison Between Avaris and Memphis

Bettina Bader

As part of the synchronisation project SCIEM 2000 this venture attempts to tie the stratigraphies of the Delta site of Tell el-Dabᶜa, also known as Avaris, and the site of Kom Rabiᶜa lying on the soil of the old Egyptian capital Memphis. These two sites are situated about 130 km apart from each other.[2] Although this distance is not great in terms of kilometres it turned out to be more difficult to compare the material of those two sites than first anticipated, because of different developments over time of even the most common so-called type fossils, especially the hemispherical cups[3] and the beer/wine jars,[4]

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[1] I am very grateful to Charles Dickens and Irene Kaplan for suggesting this title. Further thanks are due to D. Aston and J. Bourriau for reading versions of this text and giving valuable suggestions.

[2] Cf. J. Baines, J. Malek, *Atlas of Ancient Egypt*, Oxford 1980, 41, fig. on left hand side.

[3] The parallels given here are only covering the period of the late Middle Kingdom and Second Intermediate Period. For earlier examples see Do. Arnold, The Pottery, in: Di. Arnold, *The Pyramid of Senwosret I, The South Cemeteries of Lisht*, vol. 1., New York, 1988, 140. Tell el-Dabᶜa: M. Bietak, *Tell el-Dabᶜa V*, passim. Tell Ibrahim Awad: van den Brink, Preliminary Report on the Excavations at Tell Ibrahim Awad, Seasons 1988–1990, in: E. van den Brink (ed.), *The Nile Delta in Transition 4th–3rd Millennium B.C.*, Tel Aviv 1992, fig. 2.4. Serabit el-Khadim: J. Bourriau, Observations on the Pottery from Serabit el-Khadim (Zone Sud), *CRIPEL* 18 (1996), fig. 4.1. Dahshur: Do. Arnold, Keramikbearbeitung in Dahschur 1976–1981, *MDAIK* 38, passim. Lisht: Do. Arnold, *The Pottery*, 128, fig. 65. Qasr el-Sagha: Di. and Do. Arnold, *Der Tempel von Qasr el-Sagha*, fig. 22.5–7. Lahun/Kahun: G. Brunton, *Lahun I*, London 1920, pl. XIX.46–48. W.M.F. Petrie, *Kahun, Gurob and Hawara*, London 1890, pl. XII.2. Hawara: W.M.F. Petrie, *The Labyrinth, Gerzeh and Mazghuneh*, London 1912, pl. XXX, top left, pl. XXXIII.9–11. Harageh, Riqqeh: R. Engelbach, B. Gunn, *Harageh*, London 1923, pl. XXXIV.7J2, K, M, N2, O. Gurob: G. Brunton, R. Engelbach, *Gurob*, London 1927, pl. XI.2,3,5. Rifeh: W.M.F. Petrie, *Gizeh and Rifeh*, London 1907, pl. XIIIc.102, 104, 117, 118, 124, 126; XIIId. 182, 184; XXV.27–40; XXVI.74–80. Beni Hassan: J. Garstang, *The Burial Customs of Ancient Egypt*, London 1907, pl. XV.57. Qau: G. Brunton, *Qau and Badari III*, London 1930, pl. XIII. 13L, 14F, 15J, F, H, L, 18F, K, M. Mostagedda: G. Brunton, *Mostagedda*, London 1937, pl. LXXIII. 18G. Abydos: J. Wegner, V. Smith, S. Rossell, The Organization of The Temple *Nfr-K3* of Senwosret III. in Abydos, *ÄeL* 10 (2000), fig. 8.1–2; fig. 16.1–3. Hu: W.M.F. Petrie, *Diospolis Parva, The Cemetries of Abadieyeh and Hu*, London 1901, pl. XXXV.122, 123. Denderah: R.A. Slater, *The Archaeology of Dendereh in the First Intermediate Period*, Univ. of Pennsylvania 1974, fig. 22.5–7. Thebes: W.M.F. Petrie, *Qurneh*, London 1909, pl. XIII. 8, 9, 11. Do. Arnold, Weiteres zur Keramik von el-Tarif, *MDAIK* 28 (1972), fig. 2.4. A. Seiler, Ein Keramikensemble aus dem Mittleren Reich, in: Bericht über die 6., 7. und 8. Grabungskampagne in der Nekropole von Draᶜ Abu el Naga/Theben West, *MDAIK* 33 (1999), fig. 15.a, b. Armant: R. Mond, O.H. Myers, *The Cemeteries of Armant I*, London 1937, pl. XXXII 8G, G1, K, L, L1, M, M1. Esna: D. Downes, *Excavations at Esna, 1905–1906*, Warminster 1974, 31, type 24A. El-Kab: J. Quibell, *El Kab*, London 1898, pl. XV.18. Kubaniyeh: H. Junker, *Bericht über die Grabungen der Akad. d. Wiss. in Wien auf den Friedhöfen von El-Kubanieh-Süd*, Wien 1919, 179, fig. 80, top. Elephantine: C. v. Pilgrim, *Elephantine XVIII*, fig. 143.a, e, f, 145.c, 146.h, 147.f, g, 148.k, n, 150.j–l, 152.d–f, l, m, 153.a, b, 159.f–j. Nubia: J. Vercoutter, et al., *Mirgissa I.*, Paris 1970, z.B., 218, fig. 26.b. D. Dunham, *Second Cataract Forts II, Uronarti, Shalfak and Mirgissa*, Cambridge, Mass. 1967, 179, nrs. 28/1/29, 32/1/18, 32/1/107. W. Emery, H.S. Smith, A. Millard, *The Fortress of Buhen. The Archaeological Report, EES* London 1979, pl. 64.69. G. Steindorff, *Aniba II*, Glückstadt, Hamburg, New York 1937, pl. 69, right. S. Tyson Smith, *Askut in Nubia*, London, New York 1995, fig. 3.7.A. Oases: F. Colin, D. Laisney, S. Marchand, Qaret el-Toub: Un fort romain et une nécropole pharaonique, *BIFAO* 100 (2000), 184, Nr.1. S. Marchand, *La céramique datée de la fin de la XIIIe Dynastie (Deuxième Période Intermédiaire) decouverte en contexte artisanal à Ayn Asîl*, fig. 1.a–c, in Press. P. Ballet, La céramique du Kom I, *BIFAO* 90 (1990), 21, nr.6; 27, Nrs. 20–22, 24, 25.

[4] Hawara: W.M.F. Petrie, *The Labyrinth*, pl. XXXV. 100–103. Harageh, Riqqeh: R. Engelbach, B. Gunn, *Harageh*, pl. XXXVII. 41B, C, D, D, F2, G2, H, I, J, K, L, M, O, P, V, X. Rifeh: W.M.F. Petrie, *Gizeh and Rifeh*, pl. XIIID.177. Beni Hassan: J. Garstang, *Burial Customs*, pl. XIV.45. Abydos: J. Wegner, V. Smith, S. Rossell, *ÄeL* 10, fig. 9.28, 9.31, 17.38, 17.39. Hu: W.M.F. Petrie, *Diospolis Parva*, pl. XXXIII.22. J. Bourriau, *Umm el-Gaᶜab*, 70, cat.nr. 133. Thebes: W.M.F. Petrie, *Qurneh*, pl. XIII.25–28. A. Seiler, Nub-Cheper-Re, Abb. 16, 19, 21–25. Asasif: Do. Arnold, *MDAIK* 28, fig. 4.9. Esna: D. Downes, *Excavations at Esna*, 40, type 89, 89A; 41, type 89B. El-Kab: J. Quibell, *El Kab*, pl. XVI.60. Kubaniyeh: H. Junker, *Kubanieh-Süd*, 178, fig. 78. Elephantine: C. v. Pilgrim, *Elephantine XVIII*, fig. 150.g. Nubia: S. Tyson

which are known from all over Egypt, Nubia and even the Oases.

The core of the comparison of these two sites are their settlement layers. At Tell el-Dabᶜa they were unearthed by the Austrian Archaeological Institute under the direction of Manfred Bietak.[5] The site of Kom Rabiᶜa was excavated by David Jeffreys and Lisa Giddy on behalf of the Egypt Exploration Society in the years 1984 to 1990.[6]

The levels or phases, respectively, which are part of this study are Phases G/4 to D/2 at Tell el-Dabᶜa[7] and Levels VIII to V at Memphis. The stratigraphy of the excavation of Memphis/RAT was worked out by Lisa Giddy and the final excavation report is currently in the process of being finished.[8]

It should briefly be mentioned that Level VIII was only excavated in a few contexts as the water table was reached and made further excavation impossible. Level VII was divided into Level VII East and VII West by the nature of the site and Level VI is subdivided into 5 major units Level VIa to VIe.[9] Level V is a layer of sand and so far it is not entirely clear how it came into existence. It is thought by Janine Bourriau to mark the introduction of a New Kingdom pottery style and historically to correspond to the final phase of the Second Intermediate Period.[10]

I would like to present here the first result of the comparison of two type fossils of ceramic vessels, which are considered to be the backbone of fine dating in the field of ceramic studies in the Egyptian Middle Kingdom and Second Intermediate Period. These are the round-based hemispherical cups made of Nile B1 and B2 clays[11] and the large Nile C beer/wine bottles, or to be more precise the rims of such vessels.[12] The clay materials are classified according to the Vienna System.[13]

It should be mentioned, that the work on both sites, was undertaken with the help of a random sampling strategy developed by Nick Fieller, Head of the Department of Probability and Statistics, University of Sheffield, U.K., and Janine Bourriau, Research Fellow of the MacDonald Institute of Archaeological Research, University of Cambridge, U.K., in order to provide a firm methodological basis for applying statistical analyses.[14] Thus we will be able to obtain a precise answer to questions like "which quantity of type x, y, z or fabric a, b, c does occur in level 1 in contrast to level 2." This random sampling system will not be discussed in this paper,[15] because in the treat-

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Smith, *Askut*, fig. 3.6.C, 3.7.F. Oases: a fragment resembling beer bottle type 5: F. Colin, D. Laisney, S. Marchand, *BIFAO* 100 (2000), 184, nr. 6.

5 I would like to thank Manfred Bietak and Irmgard Hein, who placed unpublished ceramic material at my disposal. For a comprehensive overview of the site see M. Bietak, *Avaris – The Capital of the Hyksos*, BM Press, London 1996. On areas A/IV and A/V see I. Hein, *Two Excavation Areas from Tell el-Dab'a, Atti di Sesto Congresso Internazionale di Egittologia*, Turin 1992, Vol. 1, 249–253.

6 For the use of unpublished material thanks are due to J. Bourriau, L. Giddy, D.G. Jeffreys and above all the Egypt Exploration Society, London. For preliminary reports see H. S. Smith, D.G. Jeffreys, J. Malek, The Survey of Memphis, 1981, *JEA* 69 (1983), 30–42. L.L. Giddy, D.G. Jeffreys, Memphis 1990, *JEA* 77 (1991), 1–6. L.L. Giddy, D.G. Jeffreys, Memphis 1992, *JEA* 78 (1992), 11–14. D.G. Jeffreys, L.L. Giddy, Memphis 1988, *JEA* 75 (1989), 1–12. D.G. Jeffreys, L.L. Giddy, Looking for Memphis, *Egyptian Archaeology* 1 (1993), 5–8. D.G. Jeffreys, J. Malek, Memphis 1986, 1987, *JEA* 74 (1988), 15–25. D.G. Jeffreys, J. Malek, H.S. Smith, Memphis 1985, *JEA* 73 (1987), 11–20.

7 Most current stratigraphic chart in M. Bietak, J. Dorner, P. Jánosi, Ausgrabungen in dem Palastbezirk von Avaris. Vorbericht Tell el-Dabᶜa/ᶜEzbet Helmi 1993–2000, *Ä&L* 11 (2001), fig. 2.

8 L.L. Giddy, *Kom Rabiᶜa: The Late Middle Kingdom Phases (Levels VI–VIII)*, in preparation.

9 J. Bourriau, personal communication.

10 J. Bourriau, Beyond Avaris: The Second Intermediate Period in Egypt Outside the Delta, in: E. Oren (ed.), *The Hyksos, New Historical and Archaeological Perspectives*, University Museum Monograph 96, Philadelphia 1997, 159–182. J. Bourriau, K.O. Eriksson, A Late Minoan Sherd from an Early 18th Dynasty Context at Kom Rabiᶜa, Memphis, in: J. Phillips (ed.), *Ancient Egypt, The Aegean and the Near East: Studies in Honour of Martha Rhoads Bell*, San Antonio, 1998, 95–120.

11 Do. Arnold, Keramikbearbeitung in Dahschur 1976–1981, *MDAIK* 38 (1982), 60–65. M. Bietak, Problems of Middle Bronze Age Chronology: New Evidence from Egypt, *AJA* 88 (1984), 480–482. Do. Arnold, The Pottery, 140–141.

12 Z. E. Szafranski, Seriation and Aperture Index 2 of the Beer Bottles from Tell el-Dabᶜa, *Ä&L* 7 (1998), 95–119.

13 H.Å. Nordström, J. Bourriau, *Ceramic Technology: Clays and Fabrics*, in: Arnold, Do. Bourriau, J. (eds.), *An Introduction to Ancient Egyptian Pottery*, SDAIK 17, Mainz 1993, Fascicle 2, 168–182.

14 J. Bourriau, The Memphis Pottery Project, *CAJ* 1/2 (1991), 263–268.

15 A description and overview of random sampling strategies as well as a larger study on the procedures with actual sherd collections from Tell el-Dabᶜa will be provided by the author in the course of her Ph.D work. In general see C. Orton, *Sampling in Archaeology*, Cambridge Manuals in Archaeology, Cambridge 2000. S. Shennan, *Quantifying Archaeology*², University of Iowa Press, Iowa City 1997.

ment of the two vessel categories undertaken here all available information had to be used, both that from the random and that from the purposive sample, in order to get the fullest picture possible.

1. Hemispherical Cups

a) Vessel index

At the beginning of this treatise stands the very well known hemispherical cup seriation from Tell el-Dab^{c}a,[16] which very nicely shows, that the form of the cups changes in the Delta from wide and shallow to closed and deep (Fig. 1). This development has been made obvious with the aid of the vessel index, that is achieved by the division of the maximum width through the height of the vessel.[17] As time passes with the strata the vessel index gradually becomes smaller.

Most of the complete Nile B1 hemispherical cups from the excavations at Kom Rabi^{c}a/Memphis were found in Level VII East. The vessel indices of these some 11 cups range between about 120 and 150, the average of this Level being 138, 9 (Fig. 2).[18] A cross check with the Tell el-Dab^{c}a chart (fig. 1) the closest numbers with most examples are to be found in Phase G/3–1. For the remaining 5 complete examples from Memphis it is hard to say anything definite (Fig. 2). The average of the vessel indices from Level VIe still resembles that of Phase G/3–1 very much. Thus, this vessel type seems to provide evidence for at least one sound synchronism in the time period considered here.[19]

A different character altogether is expressed by the complete examples of Nile B2 hemispherical cups from the excavation of RAT in Memphis. Before Level VId no complete Nile B2 cup exists. The sherd material (i.e. the random sample) has shown though, that Nile B2 cups occur from Level VIII onwards,

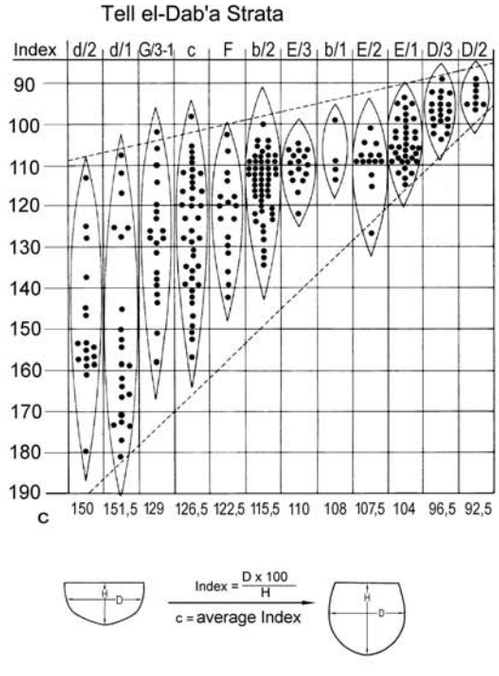


Fig. 1 Seriation of hemispherical cups in Nile B1 and B2 from Tell el-Dab^{c}a, (after M. Bietak, *AJA* 88, fig. 2)

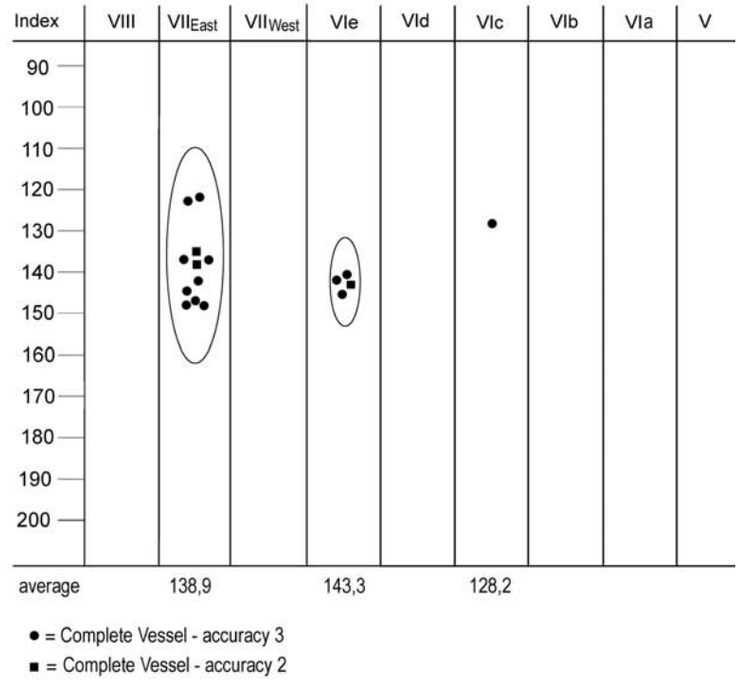


Fig. 2 Vessel indices of hemispherical cups made from Nile B1 from Kom Rabi^{c}a/Memphis

[16] M. Bietak, Problems of Middle Bronze Age Chronology: New Evidence from Egypt, *AJA* 88 (1984), 480–482.

[17] Do. Arnold, Keramikbearbeitung in Dahschur 1976–1981, *MDAIK* 38 (1982), 60–65. Eadem, *The Pottery*, 135–136.

[18] Note that „accuracy 3" means that a vessel/sherd was actually drawn, while „accuracy 2" means that a vessel/sherd in the same state of preservation was typed to an existing and identical drawing.

[19] Further research by the author by means of the random sample has shown, however, that cups from Nile B1 cease to exist at Tell el-Dab^{c}a from Phase E/1 onwards, while they continue happily at Memphis until Level V.

but only in small quantities. From Level VIe they become more frequent. The vessel index of the earliest three complete cups of Levels VId and VIc lies between 125 and 140 (Fig. 3). In comparison with the Tell el-Dab^{c}a chart: this is possible in almost all of the older phases from Phase H to E/3 and therefore not of much help in finding connecting points (cf. Fig. 1).

In the following Level VIb the difference in the development of the hemispherical cups from Kom Rabi^{c}a/Memphis becomes apparent, because the vessel index increases instead of diminishing as at Tell el-Dab^{c}a (cf. Fig.1). Eight complete Nile B2 cups were found within Level VIb, whose vessel indices range from about 140 to 180, the average being 159,6 (fig. 3). This figure disagrees radically with any average figure known from the younger Phases of Tell el-Dab^{c}a (ca. 110 (E/3)–92,5 (D/2), cf. Fig. 1). The same holds true for Level VIa, from which four complete Nile B2 cups were recovered. Their vessel indices vary from ca. 150–180, as well with an average of 166,3. Level V, however, also contained one complete cup with a vessel index of 166,7 (Fig. 3).

The depiction of the vessel indices of hemispherical cups of Nile B1 and B2 combined (in order to be exactly comparable with the Tell el-Dab^{c}a chart, where there are both Nile B1 and B2 cups in one diagram) shows that the clusters move rather to the bottom right corner of the chart (Fig. 4) than to the upper right corner (cf. Fig. 1).

This fact seems to be connected with the advent of a new type of hemispherical cup made from Nile B2 at Kom Rabi^{c}a, which in contrast to Tell el-Dab^{c}a is not closed but a remake of the old open-shaped hemispherical cup. It can be distinguished by thick vessel walls and by its very thick and rather carelessly made base with plenty of excess clay,[20] that would have been removed in previous times (Fig. 5).[21] But this does not mean that the other, more delicate type would disappear completely.

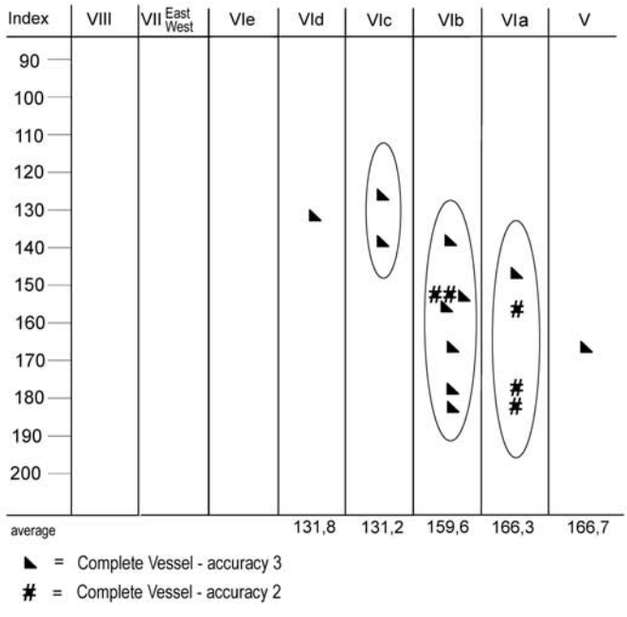


Fig. 3 Vessel indices of hemispherical cups from Nile B2 from Kom Rabi^{c}a/Memphis

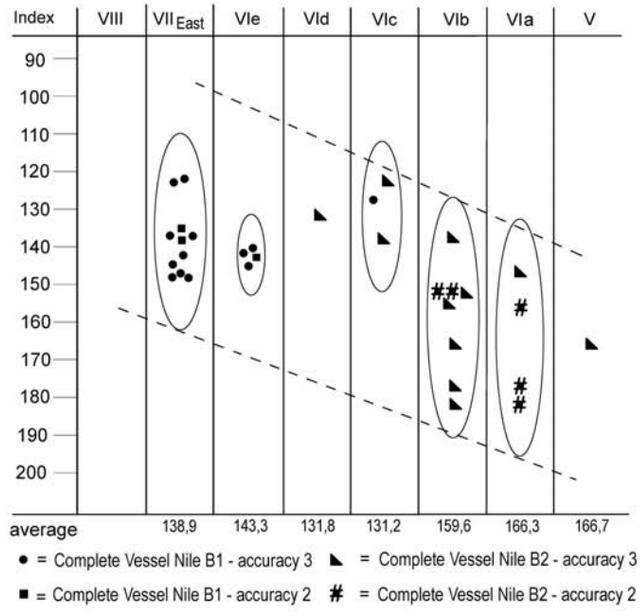


Fig. 4 Vessel indices of hemispherical cups made from Nile B1 and B2 combined from Kom Rabi^{c}a/Memphis

[20] A similar cup type occurs in Elephantine, Bauschicht 11 with a vessel index of 157, 1. Cf. C. v. Pilgrim, *Elephantine XVIII*, Abb. 146.h.

[21] For typical cups of Nile B2 with „thin" base, see M. Bietak, *Tell el-Dab^{c}a V*, 55, Abb. 27.13, 14 – Phase F; 68, Abb. 35.28–Phase F; 94, Abb. 52. 1, 2 – Phase E/3; 99, Abb. 56. 14–18, 20–21 – Phase E/3; 141, Abb. 98. 12–19 – Phase E/2; 185, Abb. 141.15 – Phase E/1; 195, Abb. 149.1 – Phase E/1; 211, Abb. 177.2 – Phase E/1; 217, Abb. 184.22–28 – Phase E/1; 247, Abb. 213.1–2 – Phase D/3; 249, Abb. 215.4–6 – Phase D/3; 265, Abb. 235.6, 10 – Phase D/3.

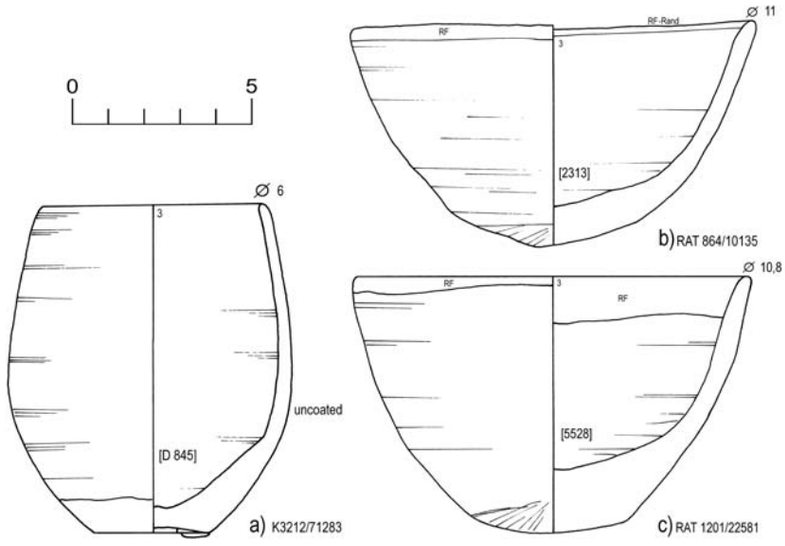


Fig. 5 New types of Nile B2 cups: a) with flat base at Tell el-Dab^{c}a; b) and c) with round base at Kom Rabi^{c}a. Scale 1:2

Another feature, so common at Tell el-Dab^{c}a, are cups with flat bases, whereas at Kom Rabi^{c}a the only known complete – and therefore securely identifiable – example of this type appears in Level VIb. The comparison of the relative date of the arrival of a new cup type at both sites, may provide another synchronism. At Tell el-Dab^{c}a cups with flat bases first occur in Phase E/1,[22] whereas at Memphis the new round based hemispherical cup type with high vessel index first appears in Level VIc. The quantity of both types increases in the next Phase or Level, respectively. Thus, Phase D/3 and Level VIb could be linked to one another. The fact, that the one and only complete flat-based cup made of Nile B2 also occurs in Level VIb at Memphis lends further corroboration to the argument.

b) Rim diameters of hemispherical cups

Closely connected with the general change of the shape of hemispherical cups as observed at Tell el-Dab^{c}a from wide and shallow to closed and deep is the systematic decrease of the rim diameter.[23] Amongst the randomly selected sherd material at Tell el-Dab^{c}a this change was clearly represented in the average rim diameters from hemispherical cup sherds in both Nile B1 and Nile B2 (see Table 2).[24] At Memphis, on the other hand, the beginning of the sequence is approximately in accord with Tell el-Dab^{c}a, but in the later Levels the average diameter of cups stays about the same, according to the shape development of hemispherical cups at Memphis (see Table 1). The comparison of the average rim diameters of the hemispherical cups also provides a few synchronisms, especially between the lowermost Levels of the two sites.

The average rim diameter of hemispherical cup rims, made of Nile B1, in the lowermost level at Memphis – Level VIII – is 11,7 cm and fits fairly well with that of Phase G/4 at Tell el-Dab^{c}a, which is 11,5 cm. There is also a good correlation between the next two levels – VII East with 11,4 cm and Level VII West with 11,1 cm and Phase G/3–1 at Tell el-Dab^{c}a with 11,0 cm. But analogous to the different developments of the cups' shape at the two sites as noticed in the complete cups (see above) in the next two levels the average rim diameters begin to diverge. While the

[22] M. Bietak, I. Forstner-Müller, Ch. Mlinar, The Beginning of the Hyksos Period at Tell el-Dab^{c}a: A Subtle Change in Material Culture, in: P. Fischer (ed.), *Contributions to the Archaeology and History of the Bronze and Iron Ages in the Eastern Mediterranean*, Festschrift für P. Aström, Sonderschriften des ÖAI 39, Wien 2001, fig. 3. For examples see M. Bietak, *Tell el-Dab^{c}a V*, 250, Abb. 217.5; 259, Abb. 226.2; 296, Abb. 276.2–4; 305, Abb. 282. 5–7 (Depot A), Abb. 282.5–7 (Depot B); 307, Abb. 283.3 (Depot D), Abb. 283.2 (Depot F). D. Aston, *Tell el-Dab^{c}a XII/2*, group 220, Cat.Nr. 838–844, pl. 212–213.

[23] As it becomes obvious in the explanatory drawing for the vessel index in Fig. 1.

[24] For all collected data see the Ph.D. dissertation of the author, chapter 6. Typologische Serien.

Kom Rabi^{c}a/RAT (all available information)[25]

RAT/Levels	VIII	VII (E)	VII(W)	VIe	VId	VIc	VIb	728+851	VIa	V
Nile B1	11,7**	11,4	11,1	11,3	10,9	11,0	11,1	11,3**	11,3	11,1
Nile B2	11*	11,3	10,3	10,8	10,2	10,4	10,6	9,4	10,3	10,5**

*only 1 rim ZBRAT; ** less than 10 rims

Table 1 Average rim diameters of hemispherical cups made from Nile B1 and B2 from Memphis/RAT (BRAT + ZBRAT)

Tell el Dab^{c}a (all information from randomly selected contexts)[26]

TD/Phases	G/4	G/3–1	F	E/3	E/2	E/1	D/3	D/2
Nile B1	11,5 cm	11,0 cm	9,8 cm	9,5 cm	8,7 cm	–	–	–
Nile B2	11,4 cm	10,9 cm	9,8 cm	9,4 cm	8,5 cm	8,0 cm*	7,0 cm**	7,6 cm

*only 1 rim TDPUR; ** less than 10 rims

Table 2 Average rim diameters of hemispherical cups made from Nile B1 and B2 from Tell el-Dab^{c}a (TDRAN + TDPUR)

cups show an average rim diameter of 11,3 cm in Level VIe at Kom Rabi^{c}a/Memphis, it is in Phase F at Tell el-Dab^{c}a 9,8 cm and therefore no longer similar. The difference becomes greater in the following Levels or Phases, respectively.

In the tables it also becomes clear that at Tell el-Dab^{c}a hemispherical cups made from Nile B1 disappear after Phase E/2 while they continue at Kom Rabi^{c}a.

The same development of diminishing average rim diameters can be observed in the group of Nile B2 hemispherical cups. While Level VIII is only represented by one single cup sherd with a rim diameter of 11,0 cm, that for this reason cannot build a firm basis for any conclusion, Level VII East and West show a more similar average rim diameter to Phase G/3–1 (10,9 cm in G/3–1 compared with 11,3 cm and 10,3 cm in Level VII East and West, respectively). After this point the difference between the averages becomes greater the more time passes by.

c) Differences in the shape of hemispherical cups

The next feature of the cups to consider is their general shape. As there are not many cups with rim and base preserved I will concentrate in the following quantitative analysis on rim fragments of the cups regardless of the shape of the base.[27] This analysis is based on randomly selected sherds from both sites under consideration. The shape of the cups has been defined according to the following characteristics: open, straight-walled and closed (Fig. 6).

The random selection of sherds of both sites was used here in order to quantitatively show the development of the shape of the cups at Tell el-Dab^{c}a as well as at Kom Rabi^{c}a.[28] As measurement for the quantification the preserved sectors of the rims, that

[25] In the Kom Rabi^{c}a/Memphis nomenclature the short cut BRAT stands for the randomly selected sherds from each context. The short cut ZBRAT represents those sherds, that were purposively selected, because they were interesting. For this table all information from both these sherd groups were collected.

[26] Analogous to Kom Rabi^{c}a/Memphis I chose for the two groups of sherds from Tell el-Dab^{c}a TDRAN for the randlomly selected sherds from each context and TDPUR for the purposively selected sherds, that were chosen because of special interest in the type or similar reasons.

[27] This means that also rim fragments most probably belonging to the type of straight-based cups („S-Näpfe“) are included within this group.

[28] All contexts with pottery at Kom Rabi^{c}a were analysed by using the random sampling procedure. These sherds are referred to as „all BRAT“. Out of those contexts were randomly chosen another set of contexts with their respective sherds. These are named „randomly selected sherds from randomly selected contexts“ = „BRAT, RS“. At Tell el-Dab^{c}a the contexts used for this study are equivalent to the „randomly selected sherds from randomly selected contexts“. Please note, that in this paper I used all randomly selected sherds (all BRAT), not just the BRAT, RS as at Tell el-Dab^{c}a. This decision was taken in order to provide a data set as large as possible in order to achieve a safe basis for our general assumptions. For procedure and theory see chapter 4. Methodologie of a PhD prepared by the author.

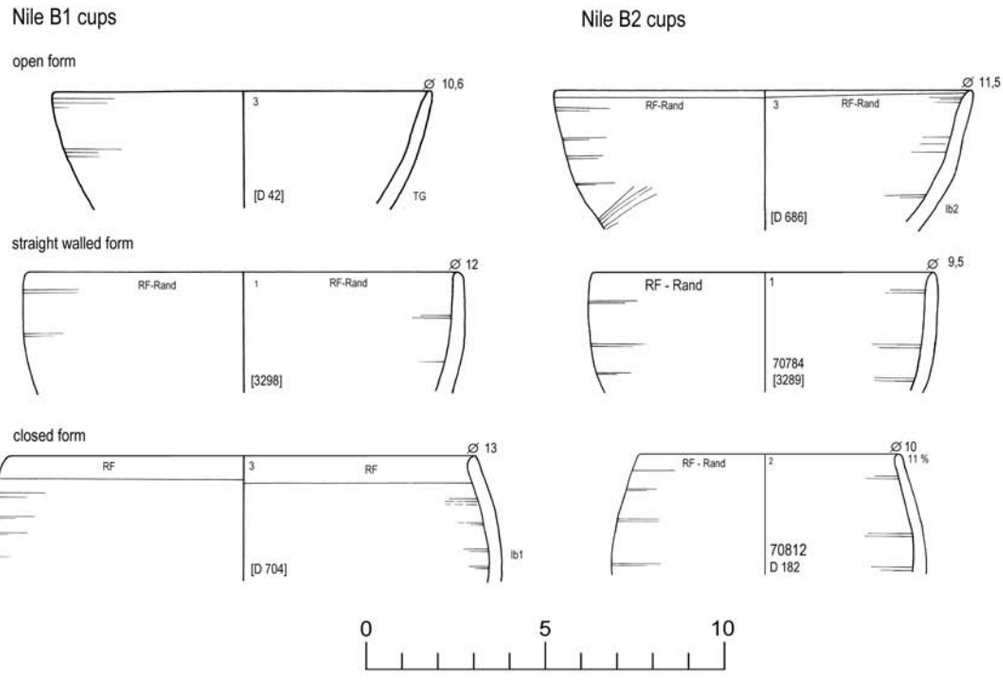


Fig. 6 Shape types of hemispherical cups from Nile B1 and B2 based on sherds from Tell el-Dab^{c}a. Scale 1:2

were measured while recording the sherds, were used. The term applied to this measurement is following C. Orton's "*estimated vessel equivalent*" (*eve*), which is given here as a percentage.[29] Because I feel readers should have the immediate possibility of comparing the raw data between levels and sub groups as well as with other types of vessels the raw data has not been turned into a frequency table, in which the proportion of all subgroups always results in 100 %.[30] This is the reason why the percentages of the single type groups when added in this paper, will mostly result in more than 100 %.[31]

Fig. 7 shows that the proportion of open cups made of Nile B1 is rather small compared to the other shape groups, and moreover they disappear before the beginning of Phase E/2. The closed cups make their appearance already in Phase G/3–1, but rather rarely while they become more frequent in the younger phases with a peak in Phase E/2. Straight-walled cups occur in all Phases from G/4 to E/2, as well as being the dominating shape group in some of them. The quantity of this sub group seems to be the most stable one.

Looking at the chart derived from Kom Rabi^{c}a (Fig. 8) it becomes immediately apparent that there is a distinctly smaller quantity of closed Nile B1 cups. They are not entirely missing at Kom Rabi^{c}a, but occur much less frequently.

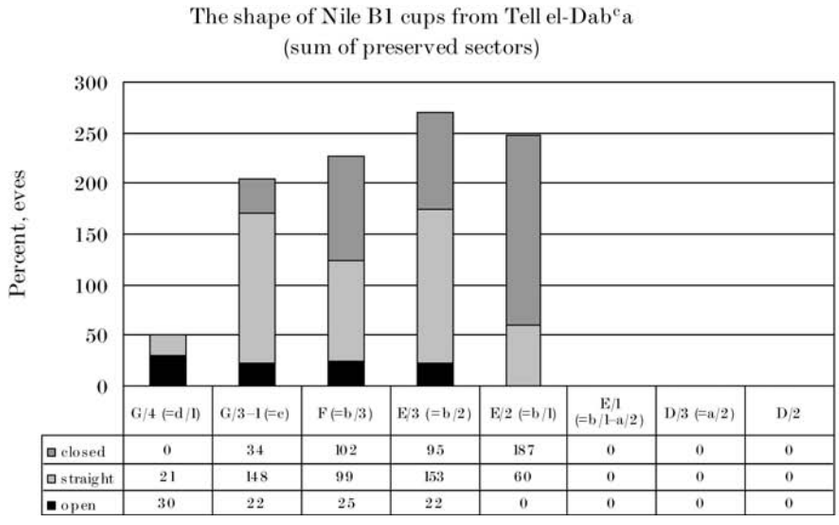
At Kom Rabi^{c}a the bulk of the Nile B1 cups are either of open or straight form. The open cups are only dominant in Level VII West[32], while open and straight cups are almost equally frequent in Level VId, VIb, VIa and V. The straight cups on the other hand are more numerous in Level VII East, VIe and VIc. Whereas Nile B1 cups are no longer found at Tell el-Dab^{c}a after Phase E/2, at Kom Rabi^{c}a they still persist in considerable quantities till Level VIc and in smaller numbers even later. Common to both sites is the dominance of the straight-walled cups most of the time.

The different distribution of the quantities and also the different preferences of open, straight-walled and closed cups made of Nile B1 in the stratigraphies

[29] C. Orton, P. Tyers, A. Vince, *Pottery in Archaeology*, Cambridge Manuals in Archaeology, Cambridge 1993, 21, 168–171. C.R. Orton, Quantitative Pottery Studies: Some Progress, Problems and Prospects, *Science and Archaeology* 16 (1975), 30–35. A decimal number would also be possible, but percentages facilitate the use of computers.

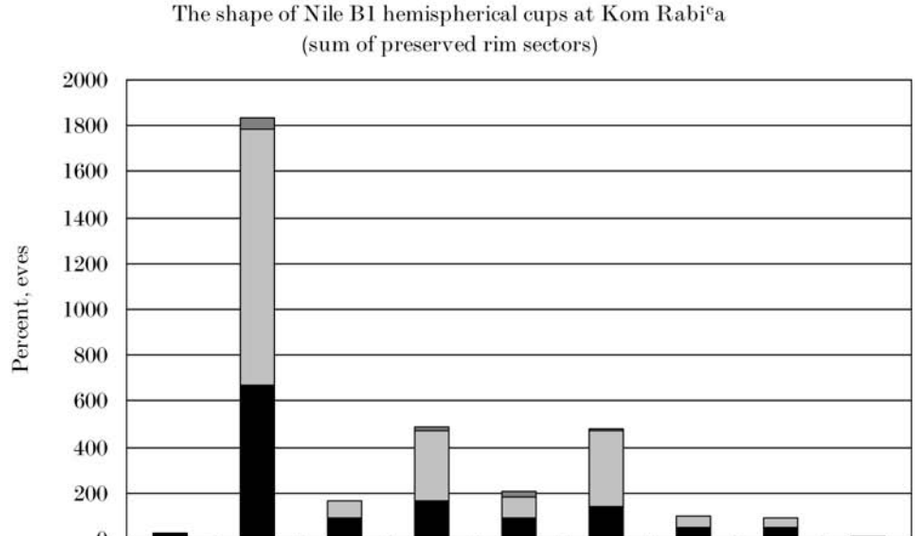
[30] M. Fletcher, G.R. Lock, *Digging Numbers. Elementary Statistics for the Archaeologist*, Oxford University Committee for Archaeology Monograph 33, Oxford 1994^{2}, 14–16.

[31] 100% rim *eves* are equivalent to one completely preserved rim of a vessel = 1/1. For relative proportions see the author's PhD, chapters 5 and 6, 100–265.



	G/4 (=d/1)	G/3–1 (=c)	F (=b/3)	E/3 (=b/2)	E/2 (=b/1)	E/1 (=b/1–a/2)	D/3 (=a/2)	D/2
closed	0	34	102	95	187	0	0	0
straight	21	148	99	153	60	0	0	0
open	30	22	25	22	0	0	0	0

Fig. 7 The shape of hemispherical Nile B1-cups in the stratigraphy of Tell el-Dab^{c}a (randomly selected sherds TDRAN) – raw data



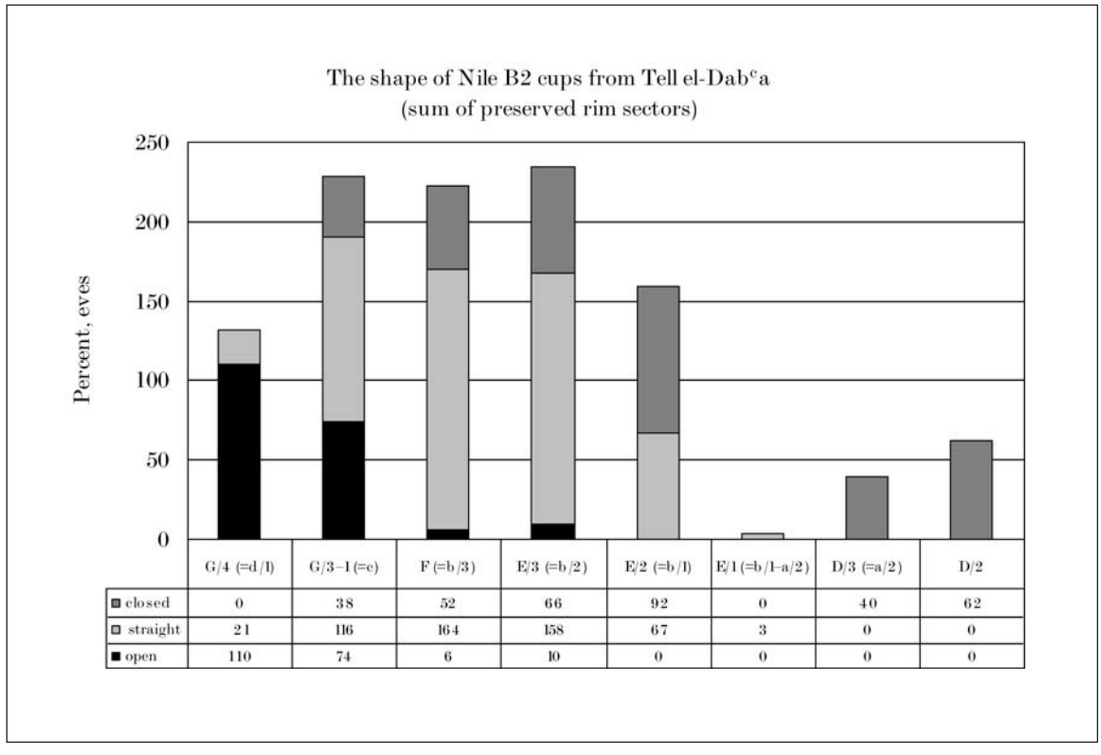
	Level VIII	Level VII East	Level VII West	Level VIe	Level VId	Level VIc	Level VIb	Level VIa	Level V
closed	0	48	0	17	22	1	0	5	0
straight	5	1133	73	309	86	343	48	41	6
open	21	671	93	162	81	148	50	48	5

Fig. 8 The shape of the hemispherical Nile B1-cups in the stratigraphy of Kom Rabi^{c}a (all randomly selected sherds BRAT) – raw data

of Tell el-Dab^{c}a and Kom Rabi^{c}a shows once more that the beginning of the sequence coincides fairly well (Phase G/3–1 with 22% eves open, 148% straight-walled, 34% closed and Level VII East with 671% open, 1133% straight-walled and 48 % closed). As time goes by the dissimilarities prevail and no longer is there any bridge between those two distribution patterns to be found.

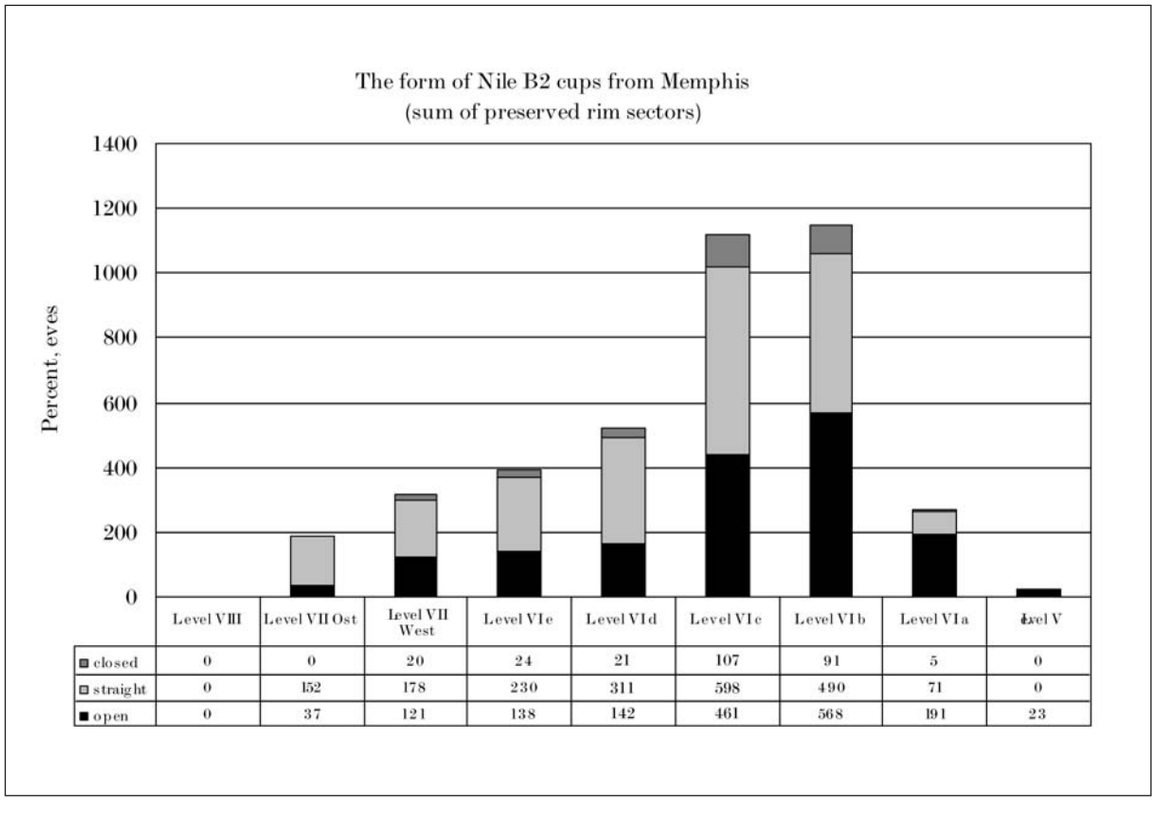
In the earliest Phase of Tell el-Dab^{c}a considered in this study, that is to say Phase G/4, the presence of open hemispherical cups made of Nile B2 is at its highest (Fig. 9). In the succeeding Phases the proportion of this shape steadily decreases. This shape variant has completely disappeared by Phase E/2. Straight-walled cups also occur from Phase G/4 onwards, in small numbers at first, steadily increasing until they dominate in Phases F and E/3 and decline from Phase E/2 to E/1 until they vanish in Phase D/3.

The closed variety of Nile B2 cups, however, is first noticeable in Phase G/3–1 in a relatively low pro-



	G/4 (=d/l)	G/3–1(=c)	F (=b/3)	E/3 (=b/2)	E/2 (=b/l)	E/1(=b/l–a/2)	D/3 (=a/2)	D/2
closed	0	38	52	66	92	0	40	62
straight	21	116	164	158	67	3	0	0
open	110	74	6	10	0	0	0	0

Fig. 9 The shape of hemispherical Nile B2-cups in the stratigraphy of Tell el-Dab^{c}a (randomly selected sherds, TDRAN) – raw data



	Level VIII	Level VII Ost	Level VII West	Level VIe	Level VId	Level VIc	Level VIb	Level VIa	Level V
closed	0	0	20	24	21	107	91	5	0
straight	0	152	178	230	311	598	490	71	0
open	0	37	121	138	142	461	568	191	23

Fig. 10 The shape of the hemispherical Nile B2-cups in the stratigraphy of Kom Rabi^{c}a (all randomly selected sherds BRAT) – raw data

Kom Rabi^{c}a Levels	VIII	VII	VIe	VId	VIc	VIb	VIa	V
Tell el-Dab^{c}a Phases	G/4?	G/3-1			E/1?	D/3		

Table 3 Synchronisms between the relative chronologies of Tell el-Dab^{c}a and Kom Rabi^{c}a

portion, which interestingly does not vary much over time, the biggest change in quantity occurring between Phases E/3 and E/2, and E/2 to E/1. From Phase D/3 onwards this is the only type found.

The quantitative situation of the same vessel type follows completely different rules at Kom Rabi^{c}a. While the Nile B1 cups show an all-time high in Level VII East, the Nile B2 cups only gradually become more frequent and reach their main *floruit* in Levels VIc and VIb. After that the proportion falls rapidly. Among the Nile B2 cups from Memphis the closed shape is a little more frequent than among the Nile B1 cups, but nowhere near as prominent as at Tell el-Dab^{c}a in the Hyksos-period and later from Phase E/1 onwards. In contrast to Tell el-Dab^{c}a the open cups become more frequent in the later Levels of Memphis. This fact is in accordance with the result of the study of the vessel indices (see above). The straight-walled cups are common on both sites. The general pattern of hemispherical cups made of Nile B2 clay at Kom Rabi^{c}a shows a gentle increase in quantity for all shape varieties up to Level VIc and VIb respectively (for open cups) and are afterwards proportionally speaking distinctly less common. By Level V this vessel type is practically no longer found.

Thus it can be shown in this instance, that the quantitative (raw) data of hemispherical cups made of Nile B1 and B2 when collected using controlled sampling methods, differ noticeably from each other from a certain point in time onwards, namely in the relative stratigraphies from Phase F and E/3 and VIe and VId, respectively.[33]

Nevertheless it does seem possible to find some synchronisms – especially in the beginning of the two sequences – for this pottery type:

The vessel indices of Nile B1 cups indicate a connection between Phase G/3–1 at Tell el-Dab^{c}a and Level VII East at Kom Rabi^{c}a. The advent of new cup types at the two sites may associate Phase E/1 and VIc as well as Phase D/3 with Level VIb. The study of the average rim diameters suggests a synchronism between Phase G/4 and Level VIII. Table 3 may help to visualise the connections.

2. Beer bottles

a) Typology

This is another vessel type which has a wide distribution throughout Egypt.[34] The large Egyptian beer or wine bottles, made from a rough Nile C clay have already been the focus of attention of several scholars. First Do. Arnold studied the chronological alteration of the relation of the openness of the rim to the height of the necks for beer bottles from Lisht and found a chronologically sensitive index for the neck.[35] Z. Szafranski invented another Index, which sets in relation to each other the openness of the neck and the height of the outer bulge of the beer bottle rim fragments.[36] He studied the beer bottles, which preserved a complete profile, from Tell el-Dab^{c}a. A typology of complete beer bottles at Tell el-Dab^{c}a has been compiled by D. Aston.[37]

Relevant to the period under consideration are bottles from the late 12th dynasty onwards to the 13th dynasty.[38] The definition of most of the bottle types from the 13th dynasty is based on their entire shape traits, which were not present in the sherd material with which this study is concerned. It should be said, that it was not possible to reconstruct a single complete profile, neither at Memphis nor amongst the randomly selected settlement contexts from Tell el-Dab^{c}a, which I used for this project.

[32] Please note, that Level VIII could not be entirely excavated because of the reaching of the water table and is thus not representative. It is given here for completeness' sake. Cf. L.L. Giddy, D.G. Jeffreys, Memphis 1992, *JEA* 78 (1992), 11–14.

[33] This is the case for the raw data as well as for the relative proportion thereof. There is, however, the – hypothetical – possibility that the actual time span covered by the two stratigraphical sequences is not the same, but at this stage of the study of the material this seems extremely unlikely, because for this to be true on the other hand there are too many connections.

[34] See supra n. 4

[35] Do. Arnold, The Pottery, 141–143, the so-called Index 1.

[36] Z. Szafranski, *Ä&L* 7, 101, so-called Index 2.

[37] D. Aston, *Tell el-Dab^{c}a XII*, fig. 12.

[38] Cf. D. Aston, *Tell el-Dab^{c}a XII*, fig. 12, types 5 to 11.

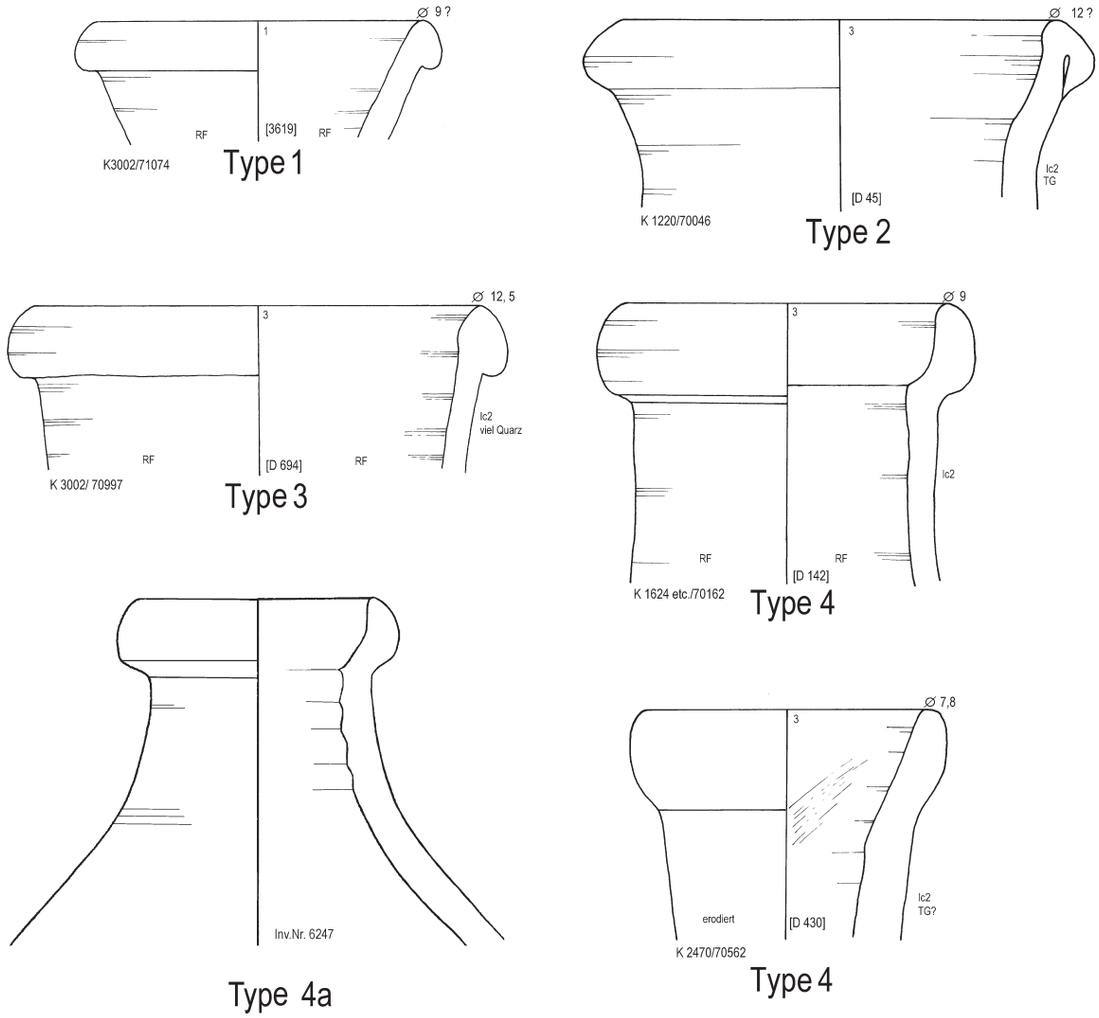


Fig. 11 Rim typology of beer bottles based on sherds from Tell el-Dab^{c}a, scale 1:2

While this is not surprising, as the nature of settlement contexts usually yields only sherd material, the typology used here is almost entirely[39] assembled from rim fragments (Fig. 11). The same holds true for the typology of beer bottles at Kom Rabi^{c}a (Fig. 12).

At Tell el-Dab^{c}a, there is the great advantage of being able to use an existing seriation prepared by Z. Szafranski,[40] based on complete beer bottles deriving mainly from closed contexts, such as tombs and offering pits. This seriation reveals the development of the shape and the slight alterations this type undergoes through time. It will be tested here as well, if the difference in the nature of the contexts plays a role in the distribution of the index.

Mostly these two typologies (Figs. 11 and 12) are in accord with each other. A short description with the most characteristic traits is given below:

- Rims/necks of type 1 show the shape of a funnel with a simple external lip.
- Rims/necks of type 2 are also funnel-shaped, but have an indent or dimple on the inside.
- Rims/necks of type 3 display a straight neck with a more or less pronounced external lip.
- Rims/necks of type 4 have a straight neck as well, but are also equipped with an indent/dimple on the inside of the rim forming the so-called "kettle"-shaped mouth. This type sometimes looks less pro-

[39] Only type 4a was taken from a complete beer bottle of *Ä&L* 7, as it was not represented amongst the randomly selected sherds from Tell el-Dab^{c}a.

[40] See supra n. 13.

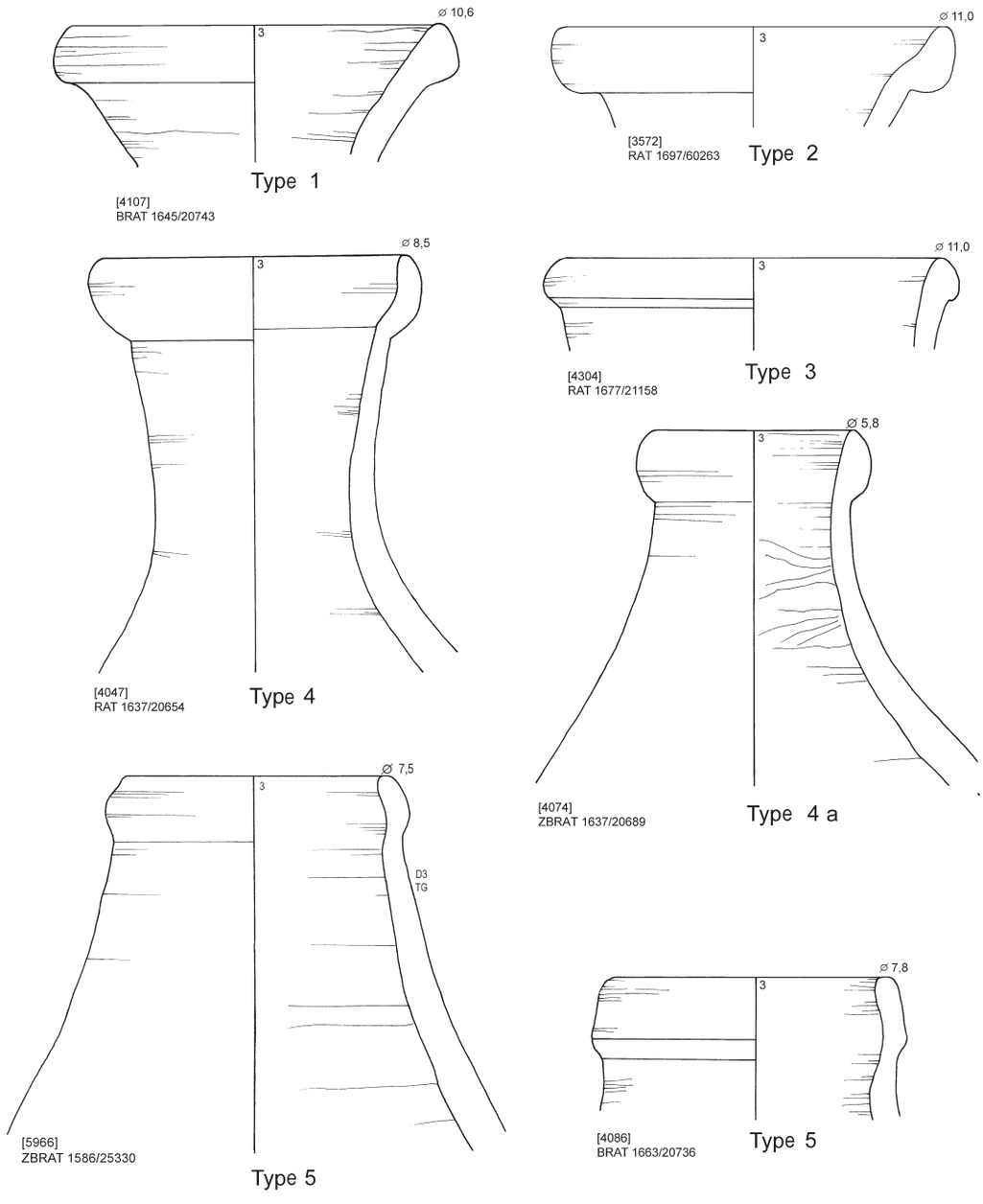


Fig. 12 Rim typology of beer bottles based on sherds from Kom Rabi^{c}a, scale 1:2

nounced, like the second example of type 4 from Tell el-Dab^{c}a (Fig. 11).

- Rims/necks of type 4a are provided with a relatively short, broad neck. They occur with "kettle"-shaped rim (like the Tell el-Dab^{c}a example[41]) as well as with external lip (like the Kom Rabi^{c}a example), but they are generally so rare, that this type has not been further subdivided.
- Rims/necks of type 5 show a relatively thin-walled rim turned inwards and hollowed out on the inside. The neck is short and becoming broader, but some examples seem to have a straighter neck. No com-

[41] Note that this rim is taken from a complete bottle from Szafranski's article, *Ä&L* 7, pl. 4. 16, nr. 6247. Among the randomly selected group of sherds this type statistically plays no important role, because numerically it is extremely rare. As a type it provides a further connection between Tell el-Dab^{c}a and Kom Rabi^{c}a. See below.

plete example of this beer bottle type has been found. However, nothing similar exists at Tell el-Dab^{c}a.

b) Aperture Index 2

As already mentioned, there are two indices describing certain shape details and their change over time. The first one (Index 1), introduced by Do. Arnold, is concerned with the height of the neck of the bottles.[42] The second one (Index 2), developed by Z. Szafranski, detects the relation of the maximum diameter of the rim itself, to the height of its external lip.[43]

As the required state of preservation of the beer bottle neck is less demanding for Index 2, I will present here Index 2 for beer bottles.[44] For the demonstration of the behaviour of Index 2 only beer bottle rims of sufficient preservation were used.[45] Szafranski's chart (Fig. 13, left) demonstrates, that the index is higher in the earlier phases and lower in the later phases. The reason for this pattern is that in the course of time the rim diameters become smaller, while the external lips become longer. This development is paralleled largely by the randomly selected settlement material from Tell el-Dab^{c}a (fig. 13, right). It can be observed, though, that the ranges are more extensive in settlement layers than in the tomb contexts and offering pits Szafranski used for his study as is to be expected in "open" contexts.

Even a first glance at the charts (Fig. 14) shows, that the development of index 2 of beer bottle rims at the two sites under consideration is quite different.

At Tell el-Dab^{c}a in Phase G/3–1 beer bottle types 1, 2, 3 and 4 are to be found. Amongst these – types 2 and 4 with "kettle"-shaped rim are most frequent. In the following Phase F the "kettle"-shaped rim of type 4 becomes even more dominant and after that the only rim type existing. After Phase E/2 in the randomly selected contexts no more beer bottle rims exist. Singular rim fragments may be found up to phase E/1 in other contexts at the site.[46]

At Memphis/Kom Rabi^{c}a Level VII East provides most clues in regard to the synchronisation of the two sites, because the same assemblage of types as mentioned above occurs there: 1, 2, 3 and 4. In addition to that types 4a and 5 appear, but they are rare. If we compare the beer bottles of type 4 in Phase G/3–1 and Level VII East, we see similar indices ranging between 300 and 450 at Tell el-Dab^{c}a and between 300 and 500 at Kom Rabi^{c}a. The other types also fall into several clusters, which are partly paralleled at Tell el-Dab^{c}a, but show a wider range in indices and therefore a higher average.

The next phase, F, at Tell el-Dab^{c}a only contains types 2 and 4, both of which show "kettle"-shaped rims, with indents on their insides. At Memphis the *floruit* of the new Type 5, which does not appear at Tell el-Dab^{c}a at all, can be seen in Level VIe. This level also contains some examples of the other types, but they are nowhere near as numerous. This tendency is still noticeable in Level VId, where more type 5 rims are present than other rim types. After Level VIe the quantity of beer bottles diminishes altogether, while no clear dominance of one particular type makes itself obvious after Level VId.

The disappearance of beer bottles at the two sites cannot be correlated in a meaningful way, because in contrast to Tell el-Dab^{c}a they do not vanish completely at Kom Rabi^{c}a. However, they become noticeably rarer from Level VIb onwards (cf. Figs. 15 and 16). Perhaps this allows at least a very rough and tentative synchronism between Level VIb and Phases E/1–D/3. Finally there is a minor synchronism to be found with type 4a, since it appears very rarely at both sites. At Memphis it is found in Levels VII East and VIe, while at Tell el-Dab^{c}a it occurs in Phase G/3–1 and F.[47]

c) Quantitative distribution

The comparison of the quantity of beer bottles, regardless of their types for the time being, gives another clue for linking the sequences. Again the dia-

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[42] See note 36.

[43] See supra note 12 and 36.

[44] However, the study of the index 1 in the course of the author's PhD work resulted in yet another difference of development. While at Tell el-Dab^{c}a the necks of the bottles in the younger Phases become longer, sometimes even grotesquely, (cf. Szafranski, *Ä&L* 7, 98–101 and fig. 2), the necks of the bottles at Kom Rabi^{c}a become shorter in the younger Levels. While the clusters in Do. Arnold's seriation figure (Do. Arnold, The Pottery, fig. 76) seem to wander from the bottom right corner to the top left corner, the clusters derived from the index 1 applied to the later Kom Rabi^{c}a material moves from top left corner to bottom right corner. Resulting from this is a curving movement of neck heights in the Memphis/Fayum region. Cf. Bader, *op.cit.*, 199–205.

[45] The charts contain only rims with accuracy 3 (actually drawn) and 2 (typed to one given drawing in the same state of preservation).

[46] Z. Szafranski, *Ä&L* 7, 105, fig. 6.1; 110, Cat.nr. 43. I. Forstner-Müller, *Die Gräber des Areals A/II*, vol. 2, 195, Inv.Nr. 8692 A – Phase D/3, body without rim. M. Bietak, *Tell el-Dab^{c}a V*, 238, fig. 202.47.

[47] Z. Szafranski, *Ä&L* 7, 107. K. Kopetzky, pers. comm.

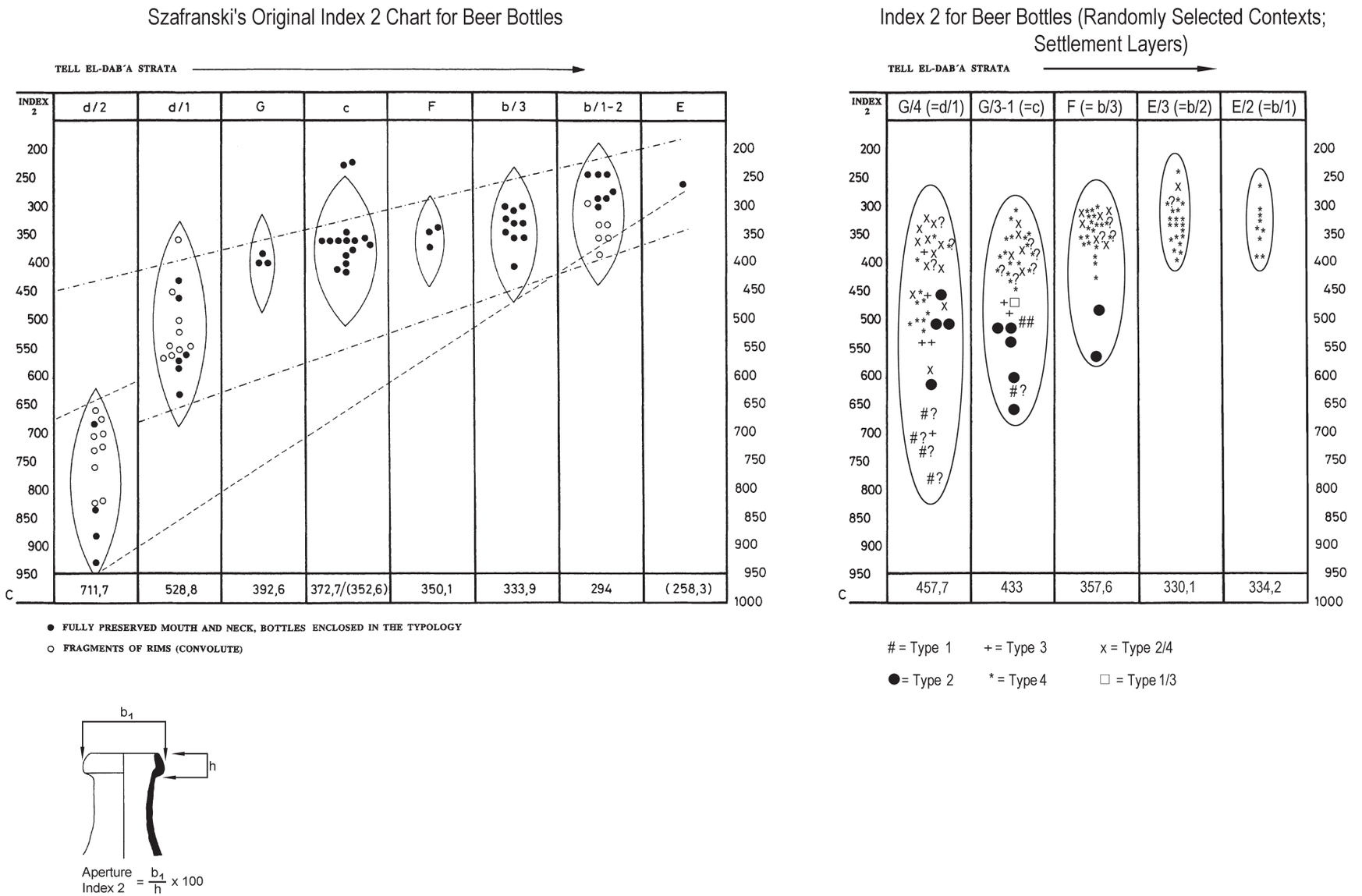


Fig. 13 Aperture index 2 of beer bottles and distribution in the stratigraphy of Tell el-Dab^{c}a (after Z. Szanfranski) opposite the randomly selected contexts from Tell el-Dab^{c}a

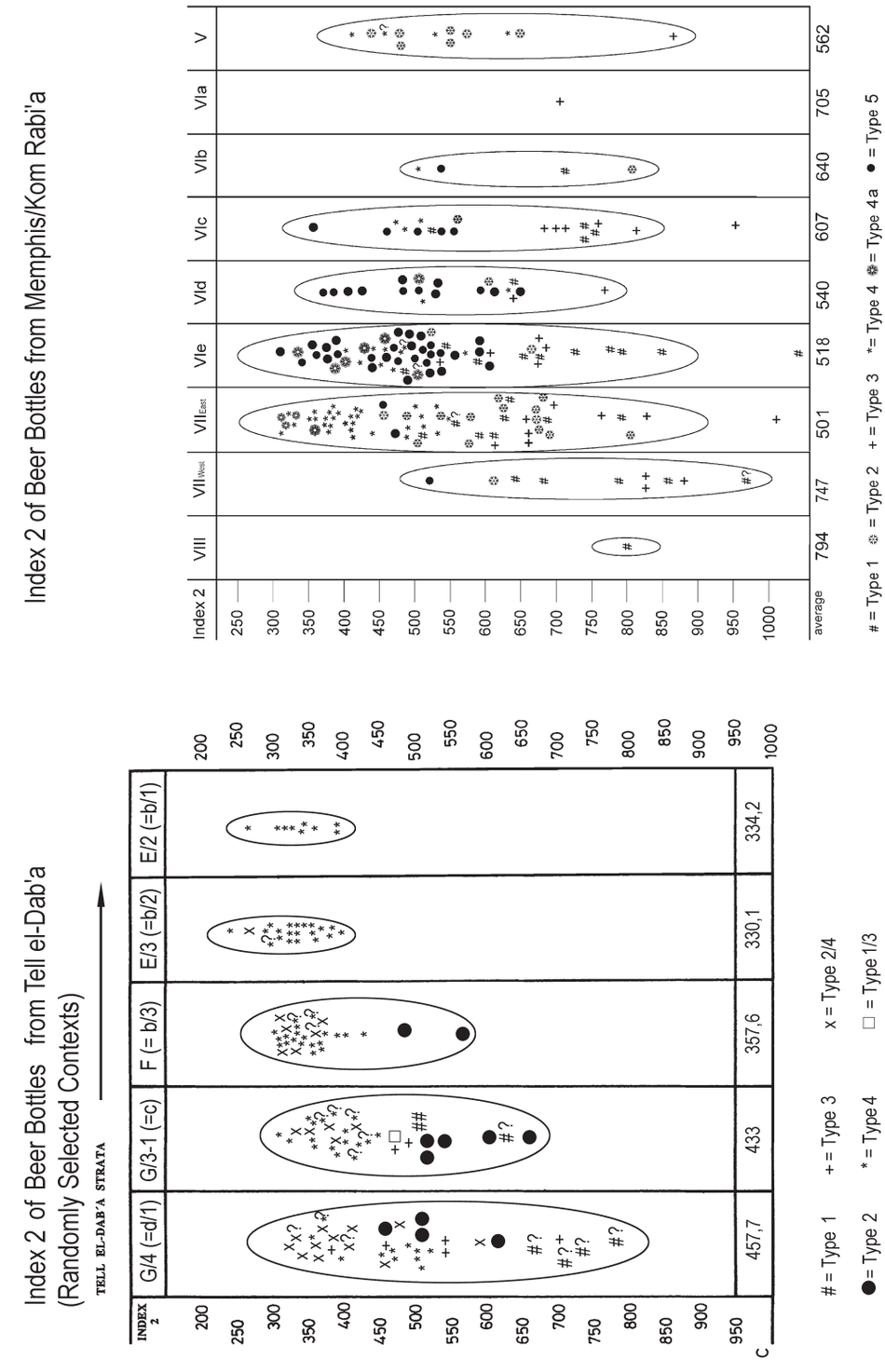


Fig. 14 Aperture Index 2 of beer bottles and distribution in the stratigraphies of Tell el-Dab'a (randomly selected contexts) and Kom Rabi'a (all available information)

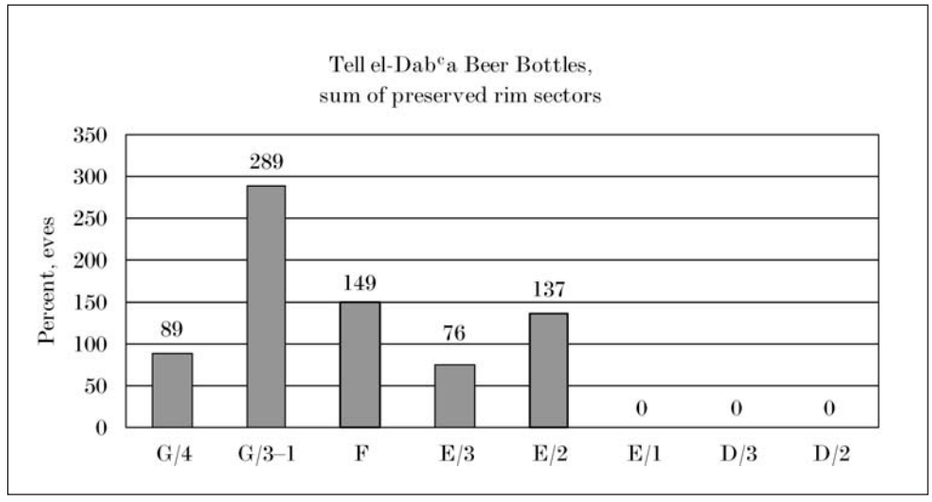


Fig. 15 The quantity of beer bottles from Tell el-Dab^{c}a in the stratigraphy (randomly selected sherds TDRAN) – raw data

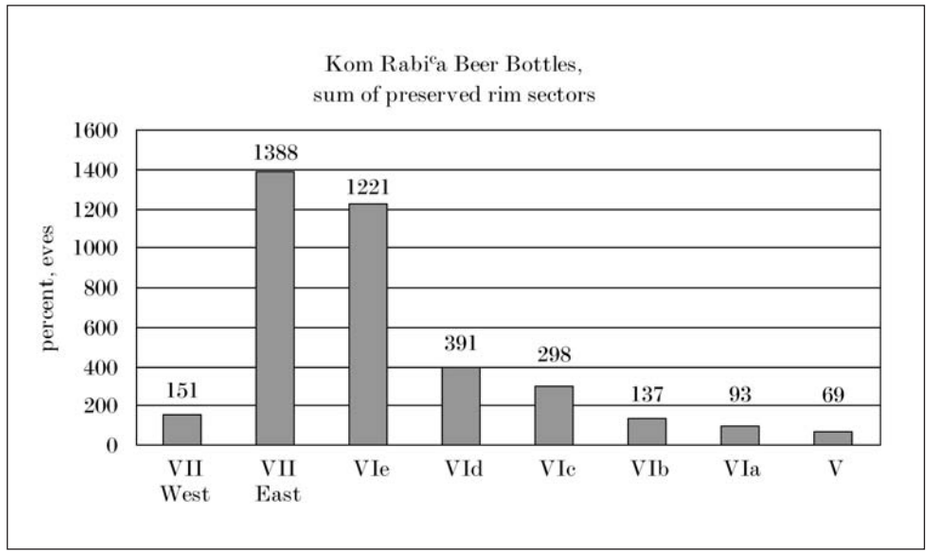


Fig. 16 The quantity of beer bottles from Kom Rabi^{c}a in the stratigraphy (all randomly selected sherds BRAT) – raw data

grams (Figs. 15 and 16) represent the amount of rim *eves* (= estimated vessel equivalents, see above) of beer bottles in each Level or Phase, respectively, which were taken from the random selection of sherds.[48]

The highest frequency of beer bottles can be observed in Phase G/3–1 and Level VII East and VIe, respectively. At both sites the quantity decreases afterwards. But while at Tell el-Dab^{c}a this vessel type comes to an end after Phase E/2, it continues to be found at Kom Rabi^{c}a albeit in low quantities to Level V. There is always the possibility of residuality to be considered when working with settlement material. But why on one site there should be such a phenomenon, which can not be traced in this form on the other remains a problem, that can not easily be explained away. It seems safer at the moment and in

[48] Cf. the tabulation of relative proportions in Bader, *op.cit.*, 255–256, 260–262.

Kom Rabi^{c}a Levels	VII	VIe	VId	VIc	VIb	VIa	V
Tell el-Dab^{c}a Phases	G/3–1	F			E/1–D/3		

Table 4 Synchronisms between the relative chronologies of Tell el-Dab^{c}a and Kom Rabi^{c}a gained from beer bottles

Kom Rabi^{c}a Levels	VIII	VII	VIe	VId	VIc	VIb	VIa	V
Tell el-Dab^{c}a Phases	G/4?	G/3–1	F		E/1?	E/1–D/3		

Table 5 Synchronisms between the relative chronologies of Tell el-Dab^{c}a and Kom Rabi^{c}a gained from hemispherical cups and beer bottles

this case to note the differences between the two sites and see if more hints can be added to characterize these regional differences and pin them down in greater detail.

The analysis of beer bottles provides the following synchronisms: Level VII is best paralleled by Phase G/3–1, Level VIe by phase F and Level VIb perhaps by E/1–D/3 (Table 4).

This result is mostly congruent with connections gained by the analysis of hemispherical cups. Table 5 sums up what can be said to date with some certainty.

Conclusions

This paper clearly demonstrates that two widely spread and frequently occurring vessel types – hemispherical bowls and rims/necks of beer bottles – from Tell el-Dab^{c}a and Kom Rabi^{c}a undergo a different development of certain shape details from a distinct point in time onwards (in the Tell el-Dab^{c}a stratigraphy from Phase F–E/3, in the Kom Rabi^{c}a stratigraphy from Level VIe–VId),[49] while the beginnings of the relative sequences could be more or less well synchronized. It seems that this divergence at the current state of knowledge is best explained by regional differences in ceramic production, although the two sites are, in real terms, not very distant from each other. This further implies that some sort of "cultural" or "political" border began to exist between these two regions already before the actual Hyksos-Period in the Delta started in full bloom (Phases E/2–D/2). Thus two different regional developments/styles, the existence of which has been suggested in previous studies,[50] are clearly attested and with the aid of quantitative analysis shown in detail here for the first time.

Nevertheless it should be borne in mind, that the results presented here are but the beginning, and there is a lot more data to be processed, before a final conclusion can be reached, if there is, indeed, one hidden among the overwhelming amount of data.

[49] There are more vessel types, which are discussed in the author's PhD. thesis, showing the same phenomenon.

[50] Do. Arnold and J. Bourriau always maintained the opinion that different ceramic traditions are to be found in Upper and Lower Egypt especially in the First and Second Intermediate Periods, as well as at the change of the 11th Dynasty to the 12th. Cf. Do. Arnold, The Pottery, 144–146. J. Bourriau, *Beyond Avaris*, 159.

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Bronze Age Paintings in the Levant: Chronological and Cultural Considerations[1]

Manfred Bietak

I. Chronology

In a series of sites in the Near East wall paintings have come to light, some of them applied on damp lime plaster (Fig. 1).[2] Most of them are considered Aegean or at least carried out by artists inspired or even trained in Aegean workshops.[3] In the meantime, more material of this kind has been unearthed. Therefore, it seems appropriate to reconsider the evidence, keeping a focus on chronology and the specific cultural setting.

Barbara and Wolf-Dietrich Niemeier, who have dealt repeatedly over the last 15 years with the overall phenomenon of those paintings, preferred to place them all into the same timeframe, into a late phase of the Middle Bronze Age II, albeit not in its final stage.[4] The paintings of Tell el-Dab^{c}a seemed, at the time of their discovery, to fit into this scheme.[5] Therefore the paintings altogether were seen as the result of a kind of "Versailles effect"[6] i.e., to have been disseminated throughout the ancient Near East in a manner similar to that of the spread of French palatial architecture and interior design throughout Europe. According to Niemeier's theory, it was supposed to be considered a kind of prestige among kings and princes in the eastern Mediterranean to have Minoan paintings in the representation rooms of their palaces. Artists from Minoan courts or centres were supposed to have been sent to embellish palaces in the Levant and Egypt with paintings of Minoan style.[7]

Niemeier cites, as a proof for this workshop dissemination, an Ugaritic text mentioning that the goddess Anat sent for the god of handicrafts Kothar wa Khasis sitting on his throne at *Kptr* (Kaphtor) to construct the palace for Baal.[8] This text is, however, from the 14th century BC, and it is entirely unclear whether this association in fact reflects a mythic feature that predates the text itself by several hundred



Fig. 1 Map of the Near East with the sites with wall paintings discussed in this paper (showing the Orontes and the Litani river and the Euphrates and the sites of Alalakh, Ebla, Qatna, Kabri, Tell Sakka 20 km east of Damascus, and Tell el-Dab^{c}a

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[1] The original version of this paper was to appear among the proceedings of a conference on the Late Bronze Age. As it was reworked according to current research results, the manuscript failed to meet the deadline, and it was integrated among the proceedings of the SCIEM Euroconference in the same year, where this subject was also on the agenda. For the English editing of this paper I am indebted to Eitan Grossmann, Jerusalem, and for bibliographical assistance to Irene Kaplan, Vienna. I would also like to thank Lyvia Morgan and Peter Warren for their kindness in reading this paper and giving valuable advice.

[2] Surveyed mainly by Niemeier 1991; Niemeier & Niemeier 1998; 2000; 2002a: 282–285; 2002b: 86*–131*.

[3] Shaw 1995: 110.

[4] Cf. n.2.

[5] Bietak 1992; Jánosi 1994.

[6] This term was introduced by Malcolm Wiener (Wiener 1984:17–26).

[7] Niemeier 1991.

[8] Last: Niemeier & Niemeier 2002a: 285.

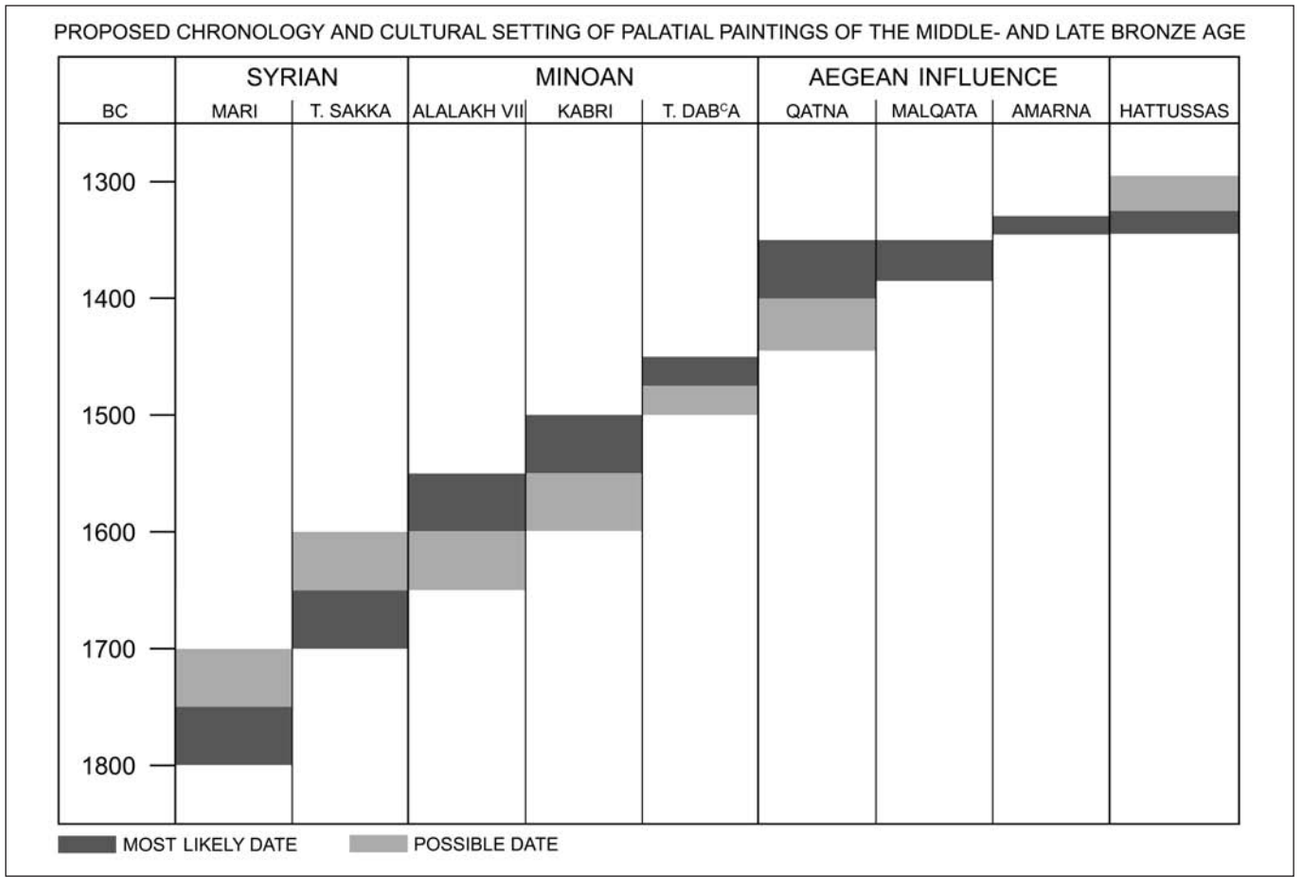


Fig. 2 Chronological table showing the date of the wall paintings of the Bronze Age in the Levant at Ebla, Mari, Tell Sakka, Alalakh, Kabri, Tell el-Dabᶜa and Qatna

years. Moreover, this god is Canaanite and not Minoan, and finally, *Kptr* is not mentioned as the sole abode of this god. The Egyptian Memphis is mentioned in the same passage as the land of his heritage.[9] This text is neither chronologically nor culturally suitable to be used as a historical source for this discussion. We shall see in the following that this vision of the dissemination of mural art is highly simplified and should be reconsidered on a more correct chronological footing. Also, the specific cultural setting should be scrutinised. This should include palatial paintings, which are of local origin but show inspirations from abroad.

As a result, the proposed chronology, which has been assessed by B. & W.-D. Niemeier around a high supposed date of the Thera eruption (around 1628 BC[10]), does not work out, neither in the relative chronology, nor in combination with the historical/archaeological absolute chronology of the Levant and Egypt, despite a numerically stupendous amount of publications by Sturt Manning claiming the contrary.[11] The paintings in question are not all

[9] New translations in Del Olmo 1981: 192; M. Smith in: Parker 1997: 119. For those references, I am grateful to my colleague Michael Jursa, Univ. Vienna. For Kothar wa Khasis in Memphis, see Leibovitch 1948: 435, pl. I.; Stadelmann 1967: 123–124.

[10] For a long time the high Aegean chronology was backed by the claim that an eruption such as that in Thera should be visible as a distinct climatic signal in the tree ring chronology. Indeed, in northern Europe and in North America, the only significant signal in the expected time range can be found at 1628 BC. See last Manning 1999: 268–281. Of course, the link between Thera and this climatic signal is not cogent. In the meantime, the eruption date has been raised by scientists to 1645±7 BC (see Hammer *et al.* 2003; Manning *et al.* 2001; Manning *et al.* 2002). New radio carbon dates seem to cluster again around 1620 BC, see Bietak, Höflmayer, Introduction, this volume.

[11] See the long list of his literature cited by Manning in this volume.

contemporary, nor were they produced in the same way and by the same kind of workshop (Fig. 2).

The paintings of Alalakh VII date before the fall of the town in the second year of Hattushili I, according to the Middle Chronology before c. 1628 BC, according to the Low Chronology c. 1575/1564 BC,[12] and before 1532 BC according to the even lower New Chronology of Gasche et al.[13] This would be about 60 to 150 years before the paintings of Tell el-Dabᶜa/ᶜEzbet Helmy, depending on how long before the destruction of this site they had been produced.[14] There is also evidence of paintings from Alalakh IV, a level that yielded wares of the Red Lustrous Wheel Made-, White Painted VI-, White Slip I, II and Base Ring I and II Wares[15] and is, according to Celia Bergoffen, most probably contemporaneous with the late Tuthmoside Period in Egypt.[16]

A date between the Alalakh VII and Tell el-Dabᶜa (phase C/3)-paintings can be assessed for the paintings of Tel Kabri in northern Galilee, c. 8 km from the seashore.[17] They were found in secondary position used as filling material of the door to the main hall of the palace with floor paintings still *in situ*. The claim of a late 17th century date by W.-D. Niemeier[18] would lead to a synchronisation with Alalakh VII (Middle Chronology)[19] and the high date of the eruption of Thera at 1628 BC.[20] The relative chronology of the palace was fixed by the late A. Kempinski and W.-D. Niemeier to a phase in the late Middle Bronze Age II before the introduction of the Bichrome Ware.[21] The destruction date of the palace was given, without further explanation, as 1600 BC. It was presented as an archaeological proof of the high date of the Thera eruption.[22] How the absolute date was obtained is not explained. One may only guess that because of similarities of the Kabri paintings to those of Thera and the conviction that the eruption of Thera could be fixed at 1628 BC, such an early date of the frescoes was transferred to Kabri. To reverse this procedure and use Kabri as an inde-

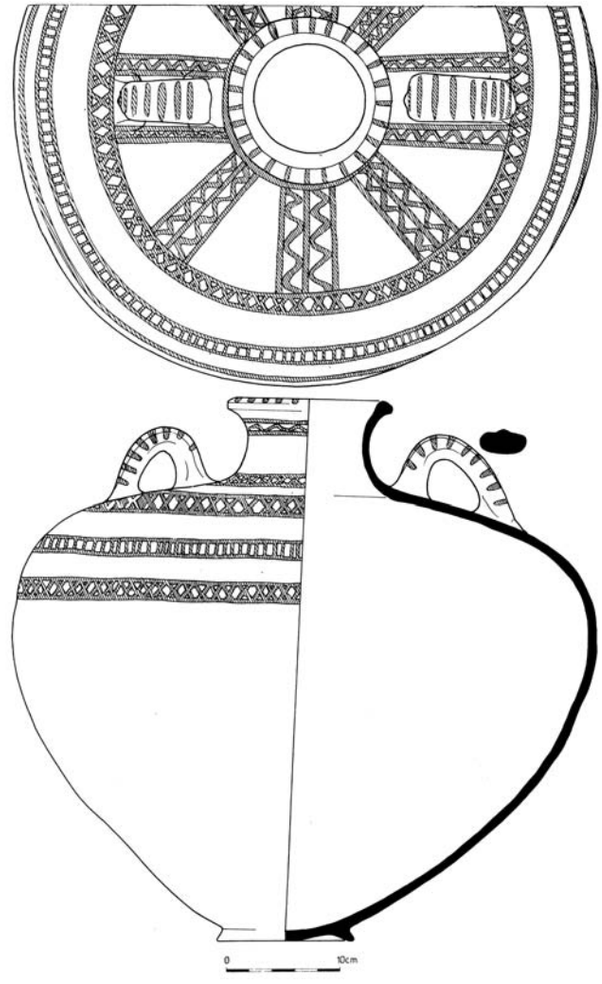


Fig. 3 The Chocolate on White Ware amphora imitation (after N. Sheftelowitz in Kempinski *et al.* 2002:174, fig. 5.62)

[12] Gates 1987: 75–70, and Heinz 1992: 207 estimate 1575 BC as the end of Alalakh VII.

[13] Gasche 1998; Gasche 2003; Gasche *et al.* 1998.

[14] For the chronology of the Tell el Dabᶜa paintings, see Bietak, Dorner, Jánosi 2001: 38–45.

[15] Woolley 1955: 174–180, 363; Bergoffen 2003; Bergoffen 2005: 24–26.

[16] Bergoffen 2003: 397–402; Bergoffen 2005: 64–65.

[17] Niemeier & Niemeier 1992; Niemeier & Niemeier 2002a. For the site in general, see Kempinski 2002.

[18] Niemeier 1990.

[19] Smith 1940.

[20] See n. 10.

[21] Last: Niemeier & Niemeier 2002a: 270, who adopted this assumption from Kempinski 1983: 223 that the beginning of the Bichrome Ware in Egypt can be dated to the reign of Apophis and, more likely, to the beginning of his reign, if not even to the late reign of his predecessor, and that the beginning of Apophis' reign would date between 1620/10 and 1590/80. Nobody would support such a view today. There is no proof of a connection between the beginning of Apophis' reign and the introduction of Bichrome Ware in Egypt. All what we can say is that this ware appears at the end of the latest stratum of the Hyksos Period D/2, which comprises approximately a third of the time-span of 108 years before the conquest of Avaris. This would fall into the reign of Apophis, but hardly into the beginning of his long reign of 40 years or even into the time of his predecessor. The Tell el Ajjul Bichrome Wheel-made Ware (BIWM) appears, however, not before the New Kingdom (study group I. Hein).

[22] Niemeier 1990.

pendent archaeological proof for the high date of Thera would mean using what one wants to prove as a proof – a classic circular argument.[23]

There is, however, no independent chronology for the Palestinian Middle Bronze Age, and the ceramic assemblage clearly contradicts the relative dating within the Palestinian chronology. The presence of Chocolate on White Ware (CoW) in the main room of the palace (Fig. 3) shows that we are at the end of the Middle Bronze Age if not at the beginning of LB I.[24] A date for this magnificent and representative large amphora to a time after the destruction of the palace (Stage 3d) is neither claimed nor documented in the publication and would also not fit into this poor post-palatial phase. Besides that, Bichrome Wheelmade Ware (BIWM) which had not been recognised by the excavators was found on the floor of the same room.[25] A potential explanation of this piece as being post palatial does not get the support of the documentation of the palace and is equally unlikely, given the fact that it is a luxury ware. Therefore, Kabri did not end before the Bichrome phase as claimed by the excavators.

Another indicator for a transitional date to Late Bronze Age I is tomb 902 (Fig. 4a, b) with a CoW bowl, Black Lustrous Wheelmade (BLWM) jugs, red polished dipper juglets with carinated shoulders,[26] and two White Painted VI jugs. To separate this rich tomb from the palace period and to date it to the poor post palace phase would not make any sense.[27] This tomb should therefore be associated with the late phase of the Palace in its proper function. The contents of the tomb, especially the presence of the Black Lustrous Wheel Made Ware[28] would date already to the time of the early 18th Dynasty but still before the phase of the White Slip I/Base Ring I Ware. WP VI and BIWM Ware both appear nowhere before the Late Hyksos Period and are still in use in the Tuthmoside Period. For example, WP VI Ware is still present in the so-called Fosse Temple at Lachish (Tell el-Duweir), where the oldest temple cannot be dated to a period before the time of Tuthmosis III,[29] and it is still present in Alalakh IV,[30] which can be dated according to the other Late Cypriot Wares such as White Slip II and Base Ring II even to the late Tuthmoside Period.[31]

All the evidence taken together would mean that the Kabri frescoes were at least still on the walls of the palace in the time of the early 18th Dynasty, which means shortly before 1500 BC according to the Egyptian historical chronology. This would be approximately 100 years later than the excavators had dated the assemblage. One may argue, of course, that the paintings were produced earlier and stayed for a long time on the walls till the palace was destroyed. According to the stratigraphic evidence, however, the paintings belonged to the third and last phase of the palace in its function as a palace (Stage 3c).[32] How long this phase lasted is difficult to say, but the pottery, which can be associated with Stage 3c, belongs nevertheless to the final phase of the Middle Bronze Age. The evidence shows that the Kabri paintings seem to have been at least one or two generations longer on the walls than those at Alalakh and were also most probably produced later. Whether they can be used as evidence of a "Hyksos" koiné[33] with respect to the distribution of wall paintings in the eastern Mediterranean world is therefore highly doubtful.

At other sites in the Levant such as Ebla, Qatna, Tell Sakka, and the most remote one, Mari, wall paintings in palatial contexts have also been found. They represent, however, different techniques and periods. The paintings in Qatna were, according to the excavators Mirko Novák and Peter Pfälzner, still

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[23] Unfortunately, such argumentation is adopted uncritically by others, e.g. Manning 1999: 29–30.

[24] For the chronology of the CoW Ware see Fischer 1999.

[25] Na'ama Scheftelowitz in: Kempinski 2002: 166, 168, fig. 5.57/8. Inventory number No. 2114 from locus 611. It is a rim fragment of a crater found on the floor of the representation hall 611. It was not recognised as the surface has largely flaked away. But at an examination of the Kabri material in the magazines of the Israel Antiquity Authorities, kindly offered 2004 by Na'ama Sheftelowitz to the author and his colleague Karin Kopetzky (SCIEM 2000 Research Programme), the BIWM Ware fragment was identified and the bichrome bands on the neck were still verifiable. A later petrographic analysis by Yuval Goren of Tel Aviv University verified the Cypriot origin of the fragment. A re-publication of the fragment by Na'ama Scheftelowitz is planned for the next issue of *E&L*.

[26] Fig. 4a, last row. For the date of carinated dipper juglets see Kopetzky 2002: 234–237, fig. 6.

[27] A. Kempinski, N. Sheftelowitz, L. Gershuny, in: A. Kempinski 2002: 117–119, fig. 5.61/8–12.

[28] The Black Lustrous Wheel Made Ware appears nowhere in Egypt in Second Intermediate Period contexts, but only within contexts of the early 18th Dynasty, according to a current M.A. study carried out by Jürgen Hörburger, Univ. Vienna.

[29] Tufnell *et al.* 1940: 84–85.

[30] Bergoffen 2005: 37.

[31] *Ibidem*: 64–65.

[32] Ronit Oren in: Kempinski 2002: 63–65.

[33] Niemeier & Niemeier 2002a: 270–275.

on the walls when the palace was destroyed c. 1340 BC.[34] It is hardly conceivable that the paintings remained intact from the time of the Middle Bronze Age, when, according to deposits in dated pits, wall paintings were also produced.[35] The walls were of dried mud brick, which do not provide a stable setting for hard plaster for centuries. They are probably the latest of the known palatial wall paintings in the Levant and were perhaps contemporary with wall paintings of Temple 9 at Boghazköy, which date before the reign of Muwatalli, a time when the temple was abandoned.[36]

At Ebla, some highly polished wall plaster fragments of lime with paint have been found from Middle Bronze Age contexts, and it would not be surprising if many more were to appear one day. In addition, in an Early Bronze IVA public building, plaster fragments with paintings have been found.[37] They are hitherto the oldest produced by an excavation in the Levant and may date as early as c. 2300 BC.[38]

Paintings in palatial contexts have also been unearthed in a palace of the Middle Bronze Age II at Tell Sakka, c. 20 km east of Damascus, by an excavation of the Syrian Antiquities Authorities under Ahmad Ferzat Taraqji.[39] Relying most probably on the Middle Chronology, the excavator dated the context to the 18th and the early 17th century.[40] According to parallels in the glyptic art and according to local types of Tell el-Yahudiya Ware found in the palace,[41] the paintings should be dated into the 17th century in keeping with the present historical chronology developed for Egypt.[42] They may be from the same period or slightly later than the paintings of Mari.[43]

The date of the paintings at Tell el-Dabᶜa has been established by ceramic evaluation[44] beyond a shadow of a doubt to the early Tuthmoside Period (c. 1500–1450 BC) in connection with a palace precinct of phase C/3.[45] The fragments of wall paintings have been found in dumps at the foot of the rampart leading up to Palace F. Others were found fallen from the walls and dumped at a gate leading to the main Palace G.[46] There is no room for manoeuvring for this date, as not a single fragment has been found in a Hyksos layer, nor was there at that time a palatial building in the immediate vicinity of the find spots of the paintings. The original Hyksos period date has been proposed after the first season on inadequate evidence.[47] It is not understandable why till 2004 some colleagues from Aegean archaeology stick to the old date, presumably because this would fit better to their original conception. The correction of the date had been made already in 1996 and only experts in Egyptian archaeology (especially in Egyptian pottery) would be in a position to challenge this correction.[48]

Within the early Tuthmoside Period the most likely date seems to be the time of Hatshepsut and young Tuthmosis III (c. 1479–1457 BC), when the first representations of Keftiu delegations and the first distinctive Minoan influence in art appear simultaneously in Theban tombs.[49] The earliest representations of Keftiu are known from the tombs of Senenmut (TT 71),[50] Useramun (TT 131)[51] and most proba-

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[34] Novák & Pfälzner 2001: 183–185.

[35] Information kindly provided by Constance von Rüden, in charge of the painting workshop of the expedition of Tübingen. The paintings in Qatna, which became known to us thanks to the recent excavations, are from the Late Bronze Age, possibly as late as the 14th century BC.

[36] Neve 1992: 30, fig. 75; Müller-Karpe 2003: 392f.

[37] Kindly shown to me on a visit to Ebla by Paolo Matthiae shortly after their discovery .

[38] Information kindly provided by Paolo Matthiae. Preliminary publication expected in 2007 by Matthiae (in print).

[39] Taraqji 1999: 35–41.

[40] *Ibidem*: 40

[41] *Ibidem*: 41, fig. 12.

[42] Bietak 2002, Bietak 2003. See also Ben-Tor 2004.

[43] As the transition from MB IIA to MB IIB can be dated according to research in Tell el Dabᶜa and Ashkelon to the decades around 1700 BC (Bietak 2002, Stager 2002), which was the time when larger Hazor was established, the Mari correspondence dealing with Hazor can only be dated to the 17th century BC. This is the conclusion of Amnon Ben-Tor, the present excavator of Hazor. See Ben-Tor 2004 and the contribution of van Koppen in this volume.

[44] Hein 1994a: 42, 1994b: 244–247, 258, 259, 261; 1998; 2001a; 2001b: 242–243; Fuscaldo 2000; Fuscaldo 2001.

[45] Bietak 2000: 187–195; Bietak, Dorner, Jánosi 2001: 38–45.

[46] Bietak 2000: figs. 4 and 5; Bietak, Dorner, Jánosi 2001: figs. 4 and 7.

[47] Originally: Bietak, Dorner, Hein, Jánosi 1994: 31f., 44; Jánosi 1995; see the explanation of the correction above in n. 45. See also the ceramic evaluation in n. 44.

[48] Bietak 1996a: 67–83; Bietak 1996b: 11–44. Bietak 1997: 117 (still in the early 18th Dynasty) and detailed with documentation in Bietak 2000, 187–200; Bietak, Dorner, Jánosi 2001: 38–45.

[49] Vercoutter 1956; Wachsmann 1987; Matthäus 1995.

[50] Recently Dorman 1991: pl. 21d.

[51] Davies 1926: figs. 1, 6; Wachsmann 1987: 31f, with a reconstruction of the scenes with Keftiu on pls. XXVI–XXXII after details rendered by Vercoutter 1956 on detailed illustrations. Dziobek 1994: 91f., pls. 20, 21, 23, 92, 93. On

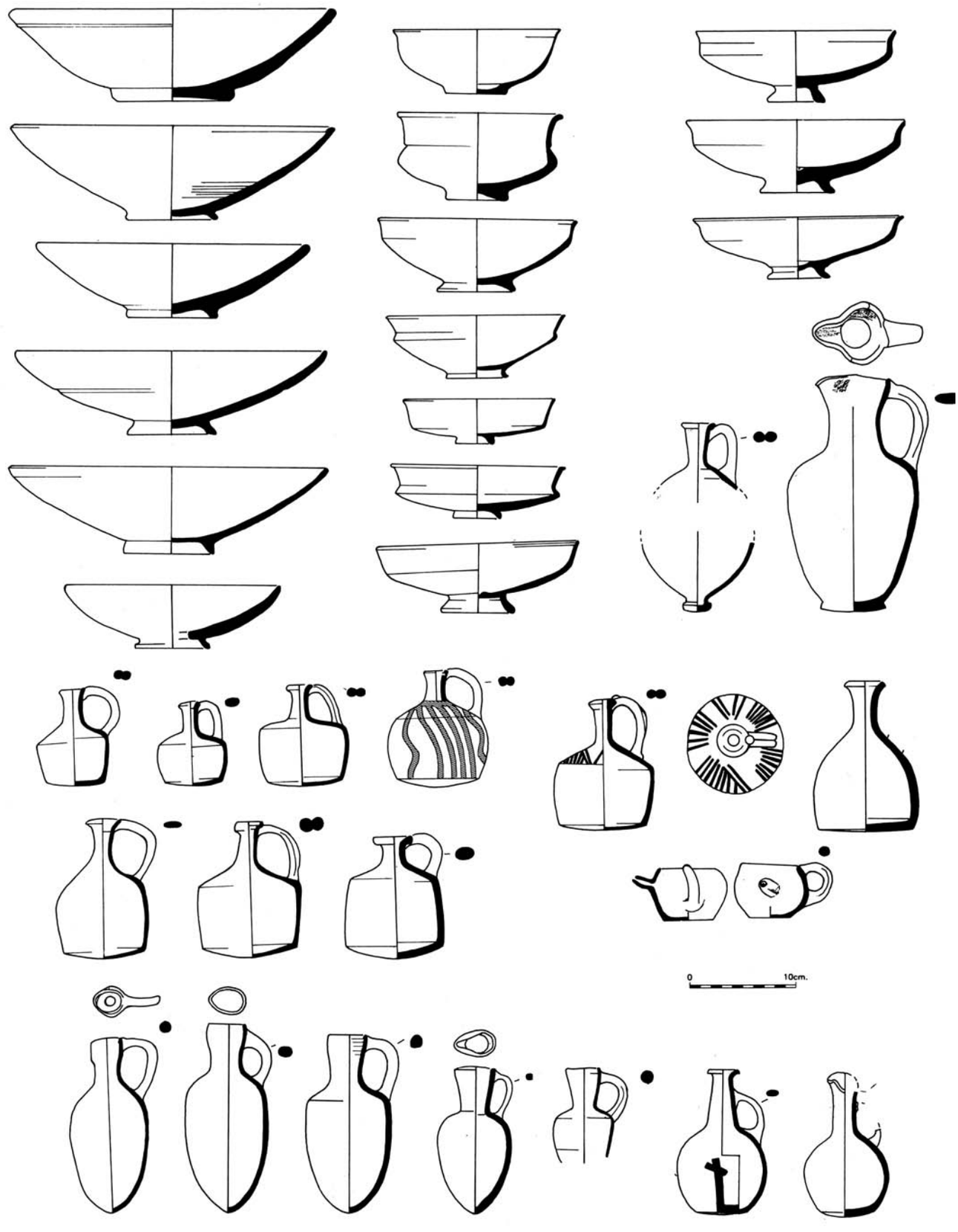


Fig. 4a Pottery from Tomb 902 from Tel Kabri, dating to the terminal Middle Bronze Age IIC showing BLWM jugs and dipper juglets with carinated shoulders (N. Sheftelowitz in: Kempinski 2002:172–173, figs. 5.60 and 5.61)

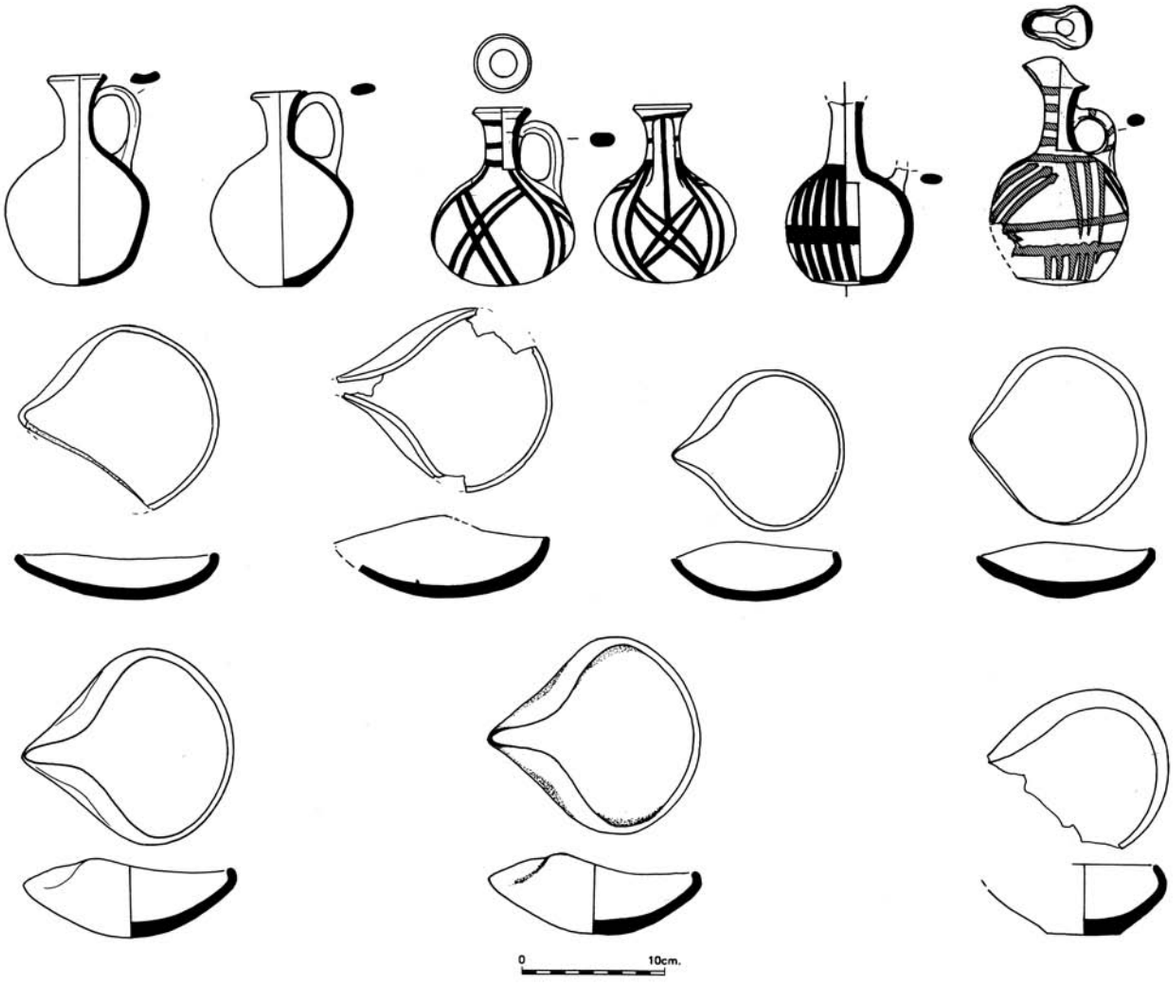


Fig. 4b Pottery from Tomb 902 from Tel Kabri, dating to the terminal Middle Bronze Age IIC showing BLWM juglets and WP V and VI jugs (N. Sheftelowitz in: Kempinski 2002:172–173, figs. 5.60 and 5.61)

bly of Intef (TT 155).[52] They all date to the time of the joint reign of Hatshepsut and Tuthmosis III. The two tombs of Useramun were completed before the year 28 of Tuthmosis III.[53] It can hardly be a coincidence that at the same time we have also distinctive evidence of features of Minoan art such as the first hesitant rendering of the flying gallop with the inner legs in front and at the rear and the *en-face* killer[54] (the hunting dog) in the tomb of User (TT 21).[55] Typical Minoan patterns can be found in the decoration of ceilings in the tomb of Senenmut.[56]

Summing up, Qatna seems to provide the latest paintings, possibly from the time of the late 15th or even 14th century BC, while those from Tell el-Dab^{c}a may be up to a century earlier; Kabri may be a generation or two earlier than Tell el-Dab^{c}a and Alalakh VII a generation before Kabri if not more, but both would date from the 16th century if we apply the low

the chronology of the tomb of Useramun (T131) in relationship to the tombs of Senenmut (TT71) and Rechmire^{c} (TT100) see recently Duhoux 2003: 182–187, 226–228.

[52] Wachsmann 1987: 31, pl. XXVI/A.

[53] Davies & Gardiner 1915: 1–2, 70–72, l. 11–12; Vercoutter 1956: 207, n. 1; Wachsmann 1987: 31, n. 23; Dziobek 1994: 100.

[54] Typical for predators in Aegean art in the moment of killing, see on this subject Morgan 1995b. *En face* hunting dogs continue in Egyptian art from this time onwards.

[55] Davies 1913: pl. XXII; Morgan 2004: 293, fig. 15, 295; User according to *PM* I^{2}: 35–37, served under Tuthmosis I but his tomb may have been completed as late as Tuthmosis III.

[56] Dorman 1991: pl. 27b–c.

or the even lower New Chronology of Gasche *et al.*[57] or the historical chronology based on Egypt.[58] This is the only way one could bring the time of the paintings as closely together as possible. Applying the Middle Chronology would result in the dates of the paintings at the different sites drifting even further apart.

At Alalakh VII and Kabri, the paintings date to a time before the first appearance of Red Lustrous Wheelmade, White Slip I and Base Ring I Wares.[59] At Tell el-Dab^{c}a phase C/3 and Alalakh V–IV, the paintings are from a period when those wares were already disseminated.[60] This is not a speculative conclusion, as the spread of the WS I Ware in the Levant is also attested in find combinations from Cyprus, which shows even synchronisation with LM IA.[61] Nevertheless, the appearance of WS I in Thera could theoretically be earlier than its advent in the Levant, although the claim of a northern Cypriot origin with an earlier production phase is an unproven hypothesis.[62] Opinions about the relative date within the WS I series are divided and good parallels of the Thera bowl are known from southern Cyprus.[63] White Slip Ware as a whole does not seem to be originally a northern Cypriot product.[64]

Mari and Tell Sakka – both technically and stylistically different from the other mentioned paintings – might date from the time of the 18th and the17th century. The paintings from the Middle Bronze Age II at Ebla are not yet clear in their precise chronological position and comprise, at the present, a few solitary fragments without actual motifs. The paintings from the Early Bronze IVA, however, are as early as the late 3rd millennium, and are an indication that there existed also a local Syrian tradition of palatial paintings.

It becomes clear from this survey that the paintings are not from the same period, covering a range of possibly 600 years or more. If we take the time span of the paintings with Minoan style (Alalakh, Kabri, Tell el-Dab^{c}a), we still face a period of 150 years, if not more. A common origin of all the paintings seems unlikely. Therefore, we have to try to differentiate the paintings in their specific cultural setting.

II. Cultural Setting

In Syria, long before Minoan influence can be found in palatial wall paintings, there appears to have been a local tradition of wall painting on lime plaster and even plaster relief for embellishing reception rooms of palaces. Little is known about those paintings and the origin of this tradition, but wall paintings have recently been found at Ebla that date to the Early Bronze Age Period IVA, the time of Sargon of Akkad (c. 2300 BC). They originate from an important room belonging to Building FF2, which is to be seen in connection with the famous Palace G of the Early Bronze IVA Period.[65] It is not yet clear if it is a secular palatial building or a kind of temple. All of the plaster fragments seem to belong to a decorated niche, which probably indicated the position of a throne.[66] The technique still has to be analysed, but the paintings were applied on lime plaster varying in thickness from a thick whitewash on mud coating to a thick lime layer. The surface was unpolished. The niche was encased with a moulded ledge decorated with lozenges. The pattern was executed according to a grid in thin red painted lines. Among the motifs were rosettes, concentric lozenges, and a maze pattern consisting of angular spiral motifs. The only two colours used were red and black. Those paintings, much older than the Middle and Late Bronze Age mural art, show that there was a local Syrian tradition in wall painting for public buildings.

Also in the Middle Bronze Age II Period, a local Syrian tradition of mural art can be found in the

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[57] Gasche 1998; Gasche 2003; Gasche *et al.* 1998.

[58] The beginning of the New Kingdom differs according to the current results of experts only within little more than a decade. There is, however a difference in opinion, how far the astrochronological data such as the Sothis date from the papyrus Ebers and lunar dates can be considered as reliable. For the New Kingdom, however, calculations (dead reckoning) by regnal and genealogical dates from the 26th Dynasty backwards lead within margins of error, which could be estimated at present within ±20 years, to ± 1550–1540 BC, cf. Helck 1987; Hornung 1964; Kitchen 1987; Kitchen 1996; Kitchen 2000; Krauss 1985; Luft 1992; Von Beckerath 1994; Von Beckerath 1997.

[59] Bergoffen 2005: 17–21, 41–54, table VII, 62.

[60] Bietak & Hein 2001. Hein 2001b. Bietak 2003: fig. 1.

[61] Vermeule & Wolsky 1990: 329–239, T I/ 324,495; *ibidem*: 219–220, T I/34A, 34 B, 340, 485, 494–500.

[62] Manning 1999: 150–158, 170–192, 367–419. – For criticism see Bietak 2003: 23–27; Bietak 2004: 211–214.

[63] Merrillees 2001: 93, considers the Thera bowl as belonging to southern Cypriot production.

[64] Joanna S. Smith, Columbia Universityin charge of the northern Cypriot Phlamoudhi Project, personal information. See also Gomez *et al.* 1995.

[65] Information kindly provided by Paolo Matthiae during a visit to Ebla (4/10/2003). I also would like to thank Paolo Matthiae for all the information given to me before the publication of this important discovery and for his kind permission to report it.

[66] Matthiae in print.

royal palace of Mari.[67] There are paintings at several places within the palace. The most important ones can be found on the walls of the nearly square central courtyard 106 on the front wall of a throne room, with the throne looking into the courtyard. They are arranged in front of a long room sanctuary. Among the motifs are processional scenes of attendants with bulls. Of particular importance is the so-called "investiture painting of Zimrilim", which was, at the time of excavations, still *in situ* on the western half of this front wall (see below).[68] The technique is tempera painted on wall coating of mud, which was partly whitewashed and on lime plaster.[69] Paintings were also found in the throne room,[70] in the so-called audience room no. 132[71] and in the "chamber of the queen",[72] the "chamber of the king",[73] and other so-called private apartments (Fig. 9).[74]

The paintings show motifs which are originally Mesopotamian. Their style and technique can be identified, however, as local. There is a rigid register composition and a tendency for ornamental division zones and vertical separations. Besides the aspective view,[75] at home in Mesopotamian as well as in Egyptian art, the bold, even crude, outlining of figures is typical of the local style. Features such as the iconography, the dress, the mythological scenery, and the emblems are all Mesopotamian. The female colour code for male and female is not in accord with either the Minoan nor with the Egyptian Middle Kingdom codes, as no distinction of gender is shown.

There may, however, be some Minoan influence. The Niemeiers have claimed,[76] correctly I believe, that the veined rendering of imitations of floor slabs and dados[77] (Fig. 10) have their earliest examples in Crete at Phaistos[78] and Mallia,[79] dating to the early 19th century BC. This would be somewhat earlier than Mari, especially if the low Mesopotamian chronology is applied. The Niemeiers also pointed out that ashlar stone is not the building material typical of Mesopotamia.[80] It is well known, however, in Syria, although mainly volcanic stones were used there. Also of Minoan influence seem to be the borders of running spirals along the edges of the top of the podium enclosing the above-mentioned veined slab imitation.[81] There is also a border of running spirals along the outer edge of the "investiture painting" in the courtyard 106.[82] This painting, which most probably renders the investiture of a king by Ishtar in warrior outfit, is considered to be the copy of a wall carpet.[83] The upper and lower ends of the pannel clearly show tassels and the rendering of a fibre seam.[84] Is it possible that the element of running spirals became a typical border for wall tapestry, but was perhaps originally imported as an element of ornamented textiles from Crete? The latter seems to be the more feasible interpretation, as the multicoloured border with thin stripes of red-white-black-white-red-black or red-white-black-white-black-red framing the core of the investiture painting[85] reminds one of the borders of Minoan paintings as well.[86] The emblematic griffins at both sides of the investiture scenes are rendered, however, in Near Eastern fashion.

Contacts between Mari and Crete are known from the Mari correspondence,[87] and it was surely not the only Near Eastern town visited by Minoan merchants at that time. There was a mutual borrowing in art as well, which sometimes makes it difficult to decide which motif originated from where, e.g., the rendering of the bull leaping and grappling scenes in

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[67] Parrot 1958.
[68] *Ibid.*: 53–66, figs. 47, 48, 50, pls. VII–XIV, col. pl. A.
[69] *Ibid.*: 83–85. Other paintings were applied on 1mm thick gypsum(?) plaster (*ibid.*: 53). The definition "Fresko auf Gipsputz" = fresco on gypsum plaster, is wrong (e.g. Kohlmayer & Stromenger 1982: 112).
[70] *Ibidem*: 67–69, fig. 54.
[71] *Ibidem*: pls. XVI–XX, col. pl. E
[72] *Ibidem*: pl. I.
[73] *Ibidem*: pl. III.
[74] *Ibidem*: pl. II.
[75] Brunner-Traut 1965: 309–330.
[76] Niemeier & Niemeier 2002a: 262–263.
[77] Parrot 1958: pls. I, XV.
[78] Levi 1957/58: 332–333; Hirsch 1977: 17, no. C43.
[79] Daux 1965: 1000–1001, figs. 1–2; Hirsch 1977: 13, nos. C22–23.
[80] Niemeier & Niemeier 2002a: 262–263.
[81] Parrot 1958: pl. XV/2.
[82] *Ibidem*: pl. XI, col. pl. A.
[83] Moortgat 1952.
[84] *Ibidem*: 92.
[85] Parrot 1958: col. pl. A.
[86] Following references for multiple striped bands were kindly provided to me by Lyvia Morgan: From Knossos: Cameron in: Popham 1985: pls. 46, 48; Cameron & Hood 1967: pl. B, fig. 1A–B, ii, iia; Evans 1921: fig. 188 a–b; Evans 1930: 46 ff.; Evely 1999: 207, 209, 247; Immerwahr 1990: 23, fig. 6, Karetsou & Andreadaki-Vlazaki 2000: 276f., 298f.; for Kea, mainly unpublished, see Immerwahr 1990: 34; for Thera: Doumas 1992: pls. 1–8, 18f., 22f., 49–56, 85 f., 90, 116, 122, 129, 131, 140 f., 151.
[87] Dossin 1939: 111–112.

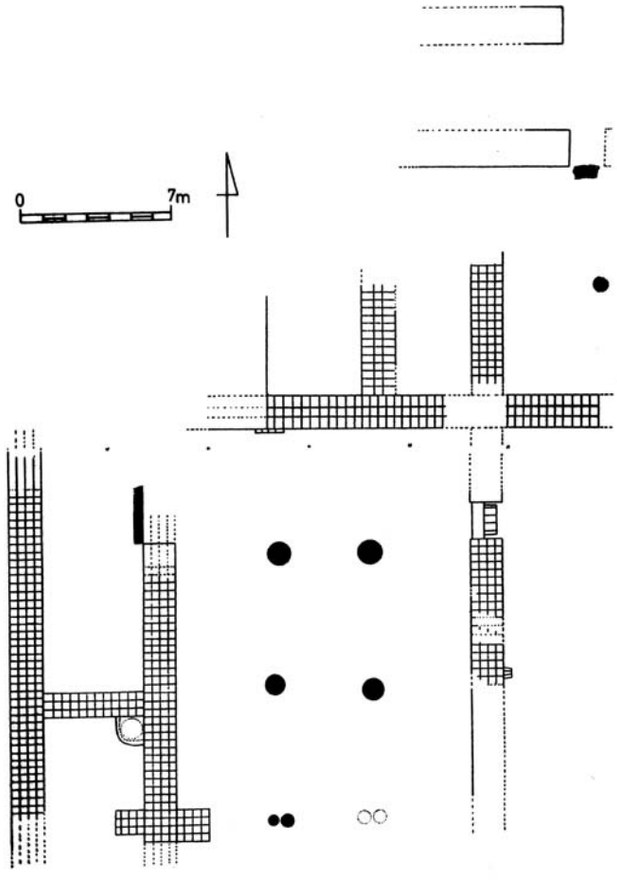


Fig. 5 Palatial building at Tell Sakka (after Taraqji 1999:35, fig. 7)

the Syrian glyptic art of the 18th or 17th century BC, depending on whether the middle or the low chronologies are applied.[88] Surely, the Minoans borrowed griffins as heraldic elements, but they domesticated them with a specific Minoan iconography. We may also assume that the divine marker, the multiple horns – a frequent motif in the Minoan glyptic art – came from the Near East.[89]

Perhaps not much later than in Mari, in MB II, one can find Syrian mural paintings in the west of the country at Tell Sakka, c. 20 km east of Damascus.[90] They originate from a palatial structure with a major reception room of four columns (22.2 × 14.5 m) and a pair of double columns at its entrance. It is possibly a throne room (Fig. 5).

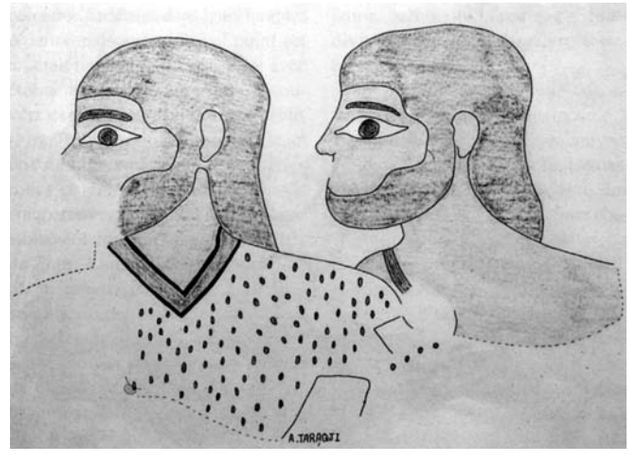


Fig. 6 Wall paintings from a palace at Tell Sakka (after Taraqji 1999: 37, fig. 9)

The paintings on whitewashed mud coating, like those found in Mari, can only have been applied in tempera technique. They covered the walls of this columned hall, the adjacent room 5, and some other side rooms in the west. Most of the paintings were found in the rubble on the floor, tumbled from the walls. What could be recognized from larger pieces of wall coating is the following: whereas the zone along the base seems to have been covered in simple patterns of red dots – probably a dado imitation – the walls carried figural motifs. Among those published were representations of bearded Near-Easterners with long hair, and, it seems, with a headband (Fig. 6).[91] They are shown, as in Egyptian art, with heads in side view with big expressive eyes, rendered in frontal aspect. Their shoulders are depicted in front view. They are dressed in white dotted robes with an ornate neck seam. Another male figure is dressed in a robe, which covered only one shoulder and also shows an ornate seam. It is not clear to which kind of scenes those figures belong. Some of the scenes undoubtedly depict warfare or hunting, as one fragment depicts a hand with a bow and an arrow poised at some unknown target.[92] Legs of animals may belong to this scene.

Most amazing is the representation of a god with the Atef-crown and ram horns, painted within the representation of a niche (Fig. 7).[93] The excavator

[88] Collon 1994 with all the references. She believes in the Syrian origin of the bull acrobatics.
[89] Marinatos 1993: 143.
[90] Taraqji 1999.
[91] *Ibidem*: 37, fig. 9.
[92] *Ibidem*: 40, fig. 11.
[93] *Ibidem*: 38, fig. 10.



Fig. 7 Image of a god on a wall painting at Tell Sakka with Egyptian Atef crown (after Taraqji 1999: 38, fig. 10)



Fig. 8 Bone inlays at Ebla with Egyptian gods (after Matthiae, Pinnock, Scandone-Matthiae 1995: 458–460, nos. 372–374)

thinks of an Egyptian influence, or even artists from Egypt. The style and the Egyptian influence can, however, be best explained together with the strong Egyptian influence on minor objects of art in this period, such as the glyptic art which frequently shows Egyptian religious emblems like the winged disk, the ᶜAnkh sign, and Egyptian gods and goddesses wearing the Atef crown, Hathor horns and other iconographic attributes (Fig. 8).[94] According to Dominique Collon, this influence should be dated to the 18th century BC in keeping with the Middle,[95] or to the 17th century in keeping with the Low Chronology.[96] According to the types of Tell el-Yahudiya Ware found in the palace,[97] this is precisely the date of the paintings.[98]

In summary, the paintings, although displaying the typical Egyptian influence and motifs of that time, can be identified as local Syrian art. They show equally Mesopotamian motifs such as a heraldic composition of goats touching a tree with their front legs.[99] Also, stylistic features such as the hand grasping the bow and arrow[100] remind us, with its heavy outlines, of the paintings of Mari.

The other paintings in Levantine palaces show distinctive Minoan features or are Minoan. The sites of Alalakh and Qatna are both situated in the

[94] Eder 1995; Teissier 1996; Bietak 1998. See also the bone inlays found at Ebla in: Matthiae, P., F. Pinnock, G. Scandone Matthiae 1995: 458–460, nos. 372–376.

[95] Collon 1994: 81

[96] Bietak 1998.

[97] Taraqji 1999: 41, fig. 12. They belong to the Palestinian Tell el-Yahudiya Ware. They do not show features of late MB I (18th cent. BC). They already have the rolled rims, but are still distant from the late types. The Piriform 3 type still has many stripes, which dates it to the early MB II Period (17th cent. BC). The Biconical jug in the foreground with oblique stripes also belongs to this time.

[98] For the typology of the Tell el-Yahudiya Ware and its chronology, see Bietak & Aston forthcoming.

[99] Taraqji 1999: 39.

[100] *Ibidem*: 40, Fig.11.

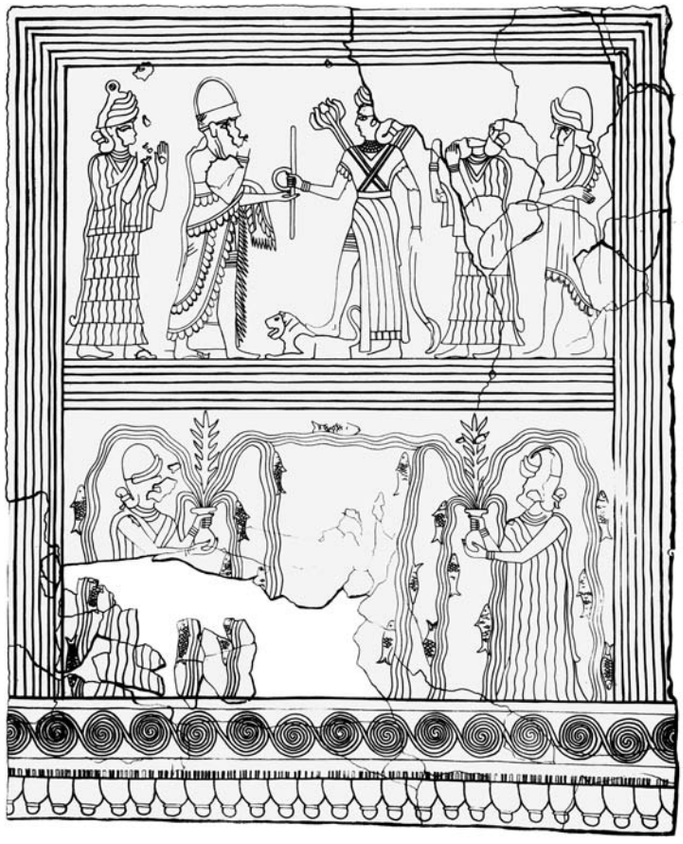


Fig. 9 Representation of wall paintings at the palace of Mari with the imitation of a wall carpet, showing a zone with running spirals (after Parrot 1958: pl. XI, 1)

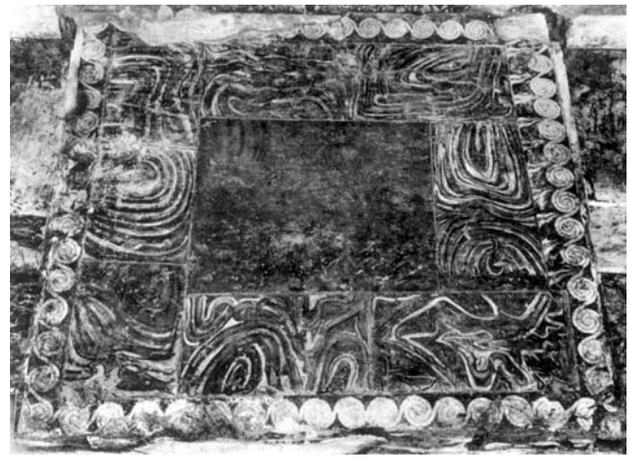


Fig. 10 Plaster imitation of veined ashlar construction (dado) and running spiral border, from Mari (after Parrot 1958: pl. XV,2)

Orontes valley, the former at its lower reaches, the latter at its middle reaches. Both sites were powerful city-states and had palatial cultures. At both sites there is evidence of wall paintings in the palaces in the time of the Middle and Late Bronze Ages. This suggests an established tradition from the 17th/16th–15th/14th century BC.

The paintings at Qatna, dating most likely from the Late Bronze Age, probably even from the 14th century BC,[101] have technique and features which look Minoan. Among the motifs are undulating horizons and friezes with running spirals and palmettos in between (Figs. 12–13). A palm tree with bushels of dates and brown grass also look very Minoan. Arranging the spiral zones in alternative directions, is not unusual in the Minoan world. The representation of turtles, however, has no parallels there. As a part of a sea landscape, but this could be taken as an inspiration from Aegean sea landscapes. The style, however, looks different from that typically found in Minoan paintings.

The paintings are executed on lime plaster, compressed by floats. It is unclear if the colours were applied to the damp surface or if tempera technique was used. String impressions were generally not used for the planning of the decorative zones, which would be unusual in the Minoan world but usual in Mycenaean paintings. The lines of the zones were drawn with free hand in colour. This is all the more astonishing, as a single cord impression was observed.

The paintings come from a small room (N) of a huge palace (Fig. 11). We may expect, however, that the major throne room had also been embellished with paintings displaying emblems of power, such as griffins.

In general, the paintings of Qatna give the impression of being inspired by Minoan art. However, the style and technique do not follow Minoan art so strictly that the involvement of Minoan artists might be claimed cogently. There seems to be another tradition involved, which could be explained as a local one. If the paintings are indeed from the 14th century, the distance from Minoan art could also be explained by the time factor. One might think of a Mycenaean contact. A comparison could also be drawn to the paintings of the Malqata palace, which

[101] Information on Qatna kindly provided by the excavator Peter Pfälzner and the analyst of the paintings Constance von Rüden, For the paintings see: Novák & Pfälzner 2001: 183–85; 2002: 226–231.

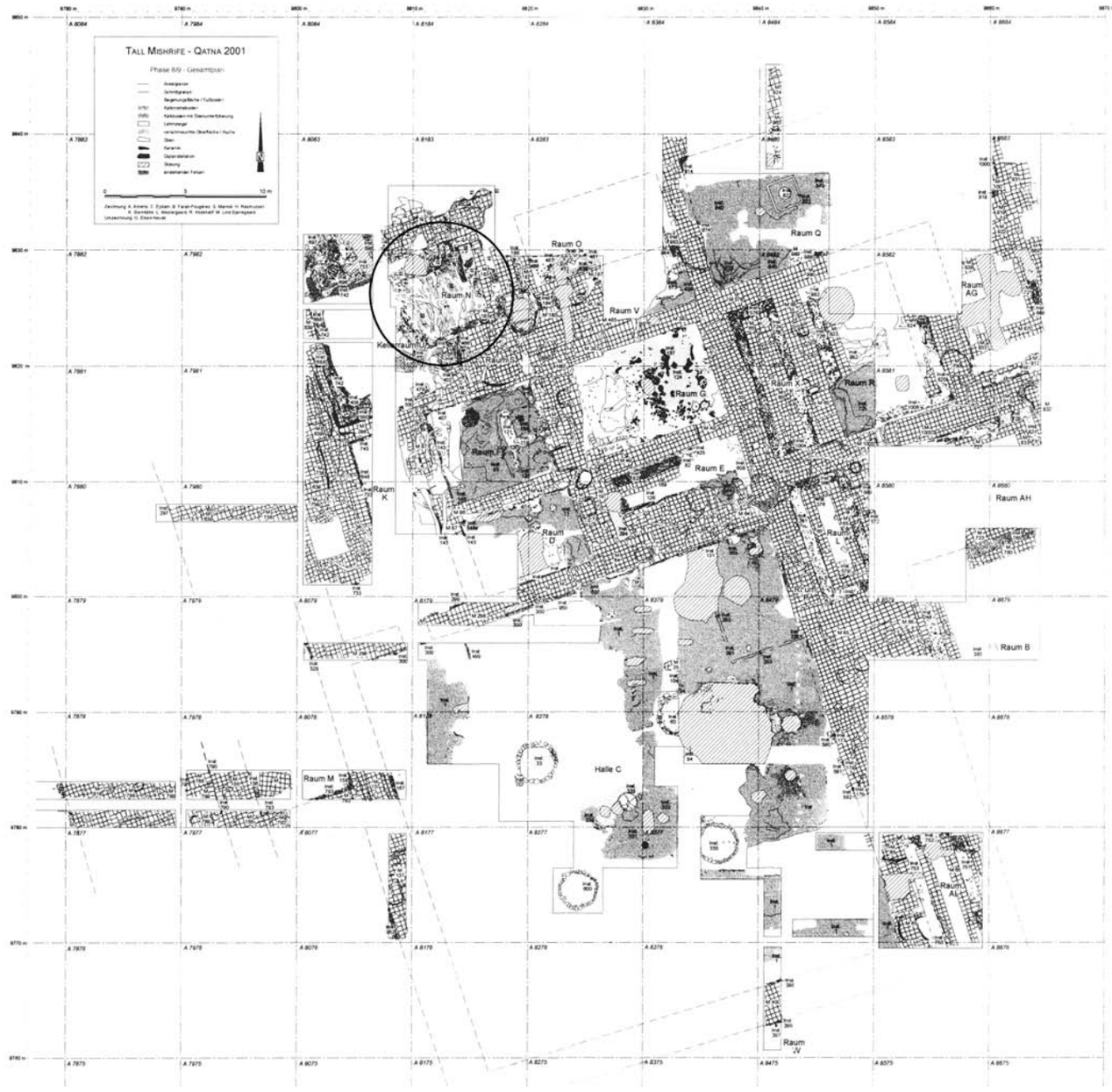


Fig. 11 Plan of the palace of Qatna showing the position of wall paintings (after Novák & Pfälzner 2002, Planbeilage)

show inspiration from Aegean art and probably a similar distance in time to the Minoan palatial art as the paintings in Qatna. Only the local substratum was different. At present the relationship to the paintings of Temple 9 at Boghazköy is unclear,[102] They have been found in tiny fragments, are not sufficiently studied and date from the 14th century BC. A fragment with an identified feature is a flying bird which has been compared with Egyptian paintings of the contemporary Amarna palatial art.[103] Another fragment seems to originate from the representation of a rosette. Future studies may show in which way those paintings should be assessed.

At Ebla, which lies in between those sites (but not

[102] See n. 36.

[103] Müller-Karpe 2003: 392, fig. 10.

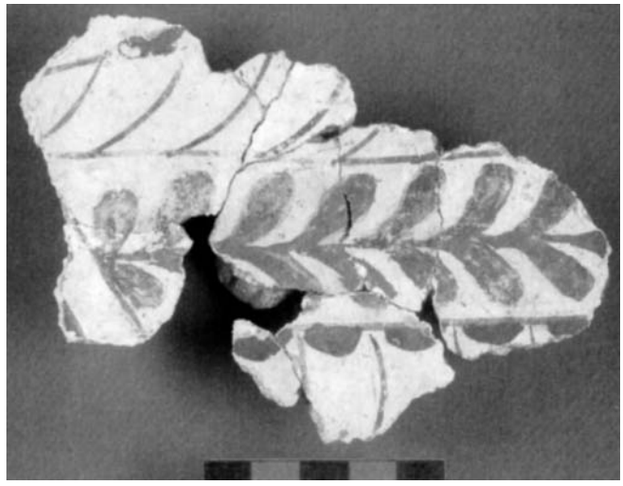


Fig. 12 Sample of wall paintings from Qatna with floral motif (after Novák & Pfälzner 2002)

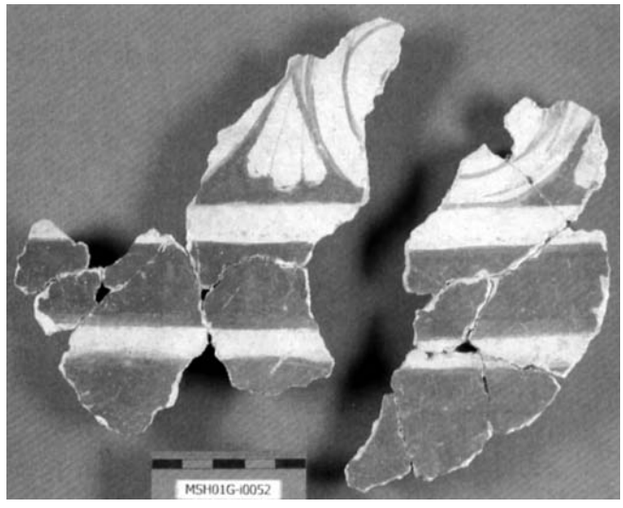


Fig. 13 Sample of wall paintings from Qatna with spiral decoration and palmettos (after Novák & Pfälzner 2002:)

at the Orontes river), some highly polished wall plaster fragments of lime with remains of linear painting have also turned up in Middle Bronze Age II contexts from the Southern Palace.

In Alalakh, the older paintings originate from the palace of the ruler Yarim Lim (Fig. 15).[104] They embellished the main hall of the palace ("The chamber of audience," according to Woolley), which could possibly be identified as the throne room. Leonard Woolley identified the technique as Minoan.[105]

Typical features there were ashlar dado imitations in the throne room.[106] From the same room probably came a representation of a big emblematic griffin (Fig. 16), of which only some notched plume pattern fragments together with parts of the body, have been identified by W.-D. and B. Niemeier, as the result of a re-examination of the paintings in the Ashmolean Museum.[107] It seems that the fantastic animal rests on the ground, which is rendered as a schematized rocky zone in a fashion similar to the ground of the taureador frieze at Knossos. Inverted landscape has also been identified in this scene. It could be hypothesized that there was originally another griffin of the same size, and both were arranged on either side of a throne as emblems of royalty.

The Niemeiers also identified horns, possibly belonging to a frieze of bucrania with a double axe between the horns (Fig. 16). The motif may originate from Knossos.[108] It seems that the Alalakh paintings were inspired by the art of the palace of Knossos, although the preserved comparanda from there could date later than Alalakh. In this case, one may expect that they had predecessors in Knossos of the same kind.

The frescoes of Kabri were also found in a palatial context of the late Middle Bronze Age (MB IIC).[109] They were found in what had been most probably the main reception room 611 (Fig. 17). Unfortunately, the building was not completely excavated. The room was nearly square and originally measured c. 8.80 × 9.30m. It had three entrances within niches through the middle of three walls. The fourth wall probably served as the back wall for a throne. On the final floor of compressed lime plaster, the painted imitation of a marble pavement was still *in situ* (Fig. 18). The pattern of slabs seems to have been planned by cord impressions in the still-wet surface.[110] The slabs were outlined with bold red lines, imitating the red mortar used in Aegean stone masonry and pavement. The real stone slabs of the entrance from room 607 were also covered with lime stucco and the continuation of the slab pattern. The colours were applied when the surface was still damp, which had caused the veined stone pattern to become slightly blurred. In some

[104] Woolley 1955: 230.
[105] Woolley 1955: 231.
[106] Woolley 1955: 230.
[107] Niemeier & Niemeier 1998: 84–85, pl. VIf.; Niemeier & Niemeier 2000: 784–789, fig. 22; Niemeier & Niemeier 2002a: 275, 282–283.
[108] Evans 1930: 208, fig. 142.
[109] For the chronological assessment, see above 271–272.
[110] Niemeier & Niemeier 2002a: 257, pl. VIII.

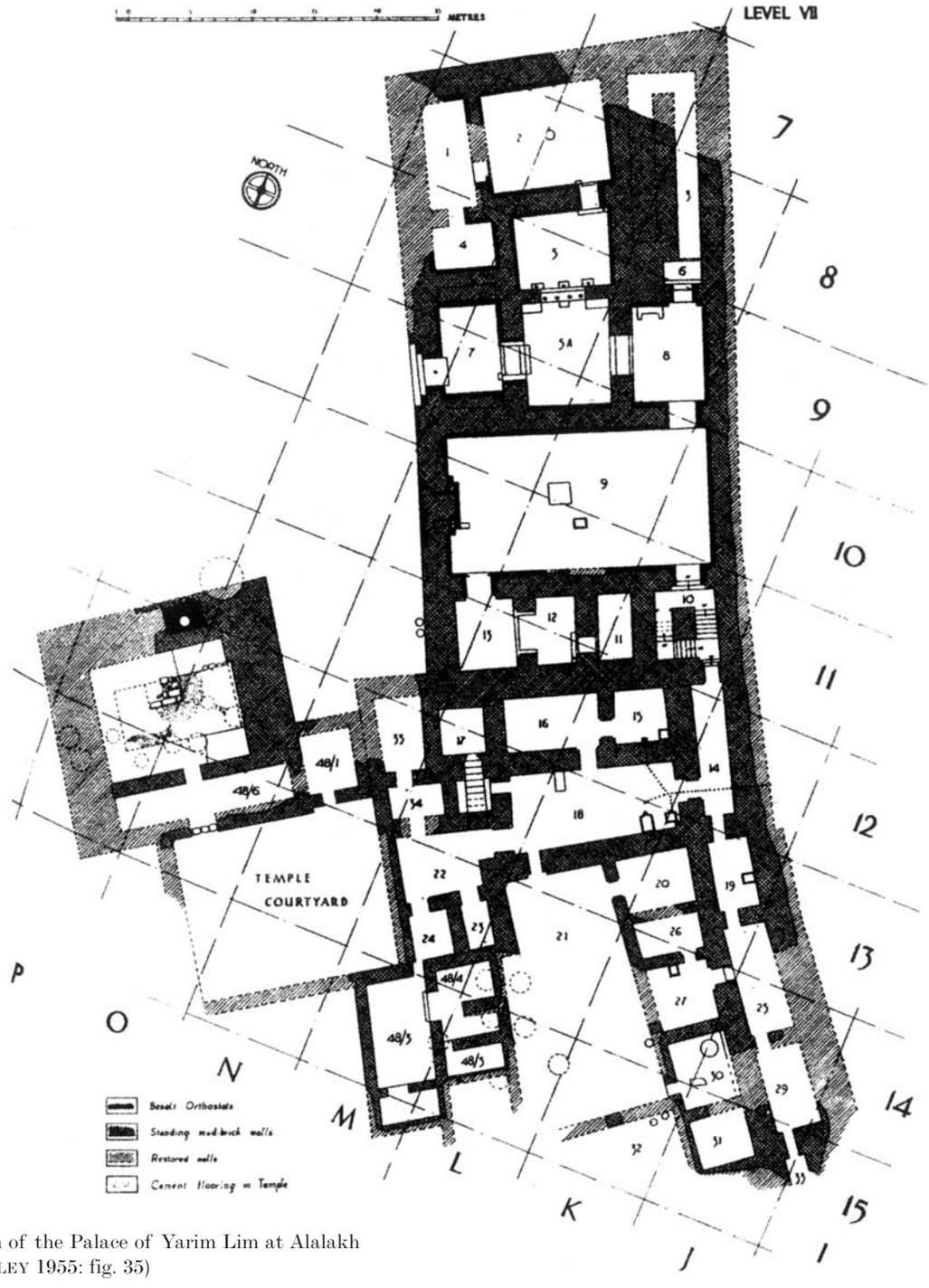


Fig. 14 Plan of the Palace of Yarim Lim at Alalakh (after Woolley 1955: fig. 35)

areas, the veined stone pattern gives way to red or blue flower decorations, which run either with the orientation of the floor pattern or in a diagonal direction to it. Sometimes, the blossoms seem to be scattered on the floor like flowers strewn on the ground for festive occasions. The flowers were identified as iris blossoms and, in one case, there was also a representation of an object that looked like a red pomegranate.[111]

[111] *Ibidem*: pl. XVIII.

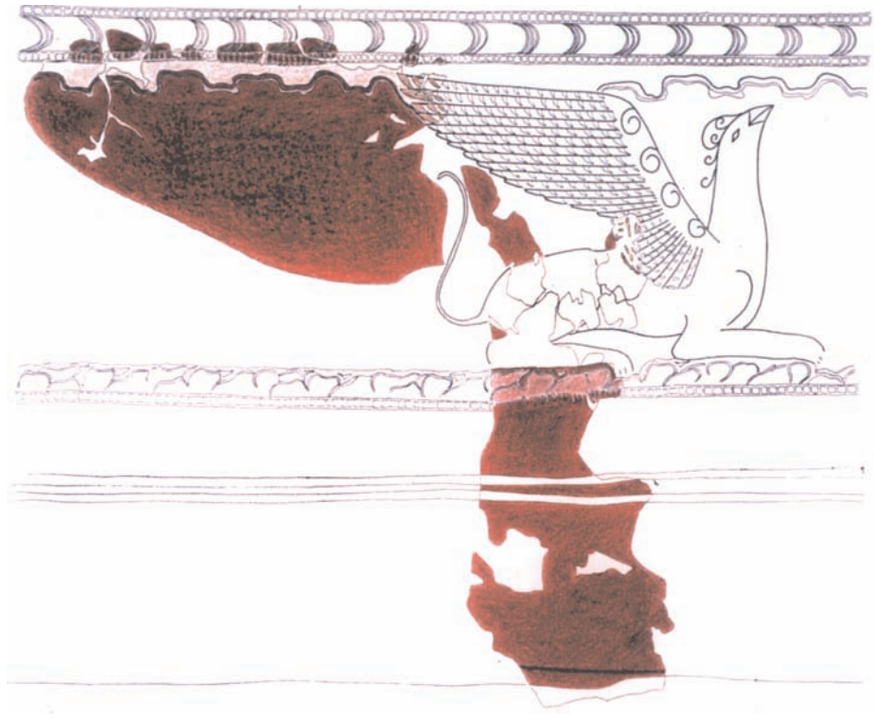


Fig. 15 Reconstruction of an emblematic griffin from the Palace of Yarim Lim at Alalakh (after Niemeier & Niemeier 2000: fig. 22)

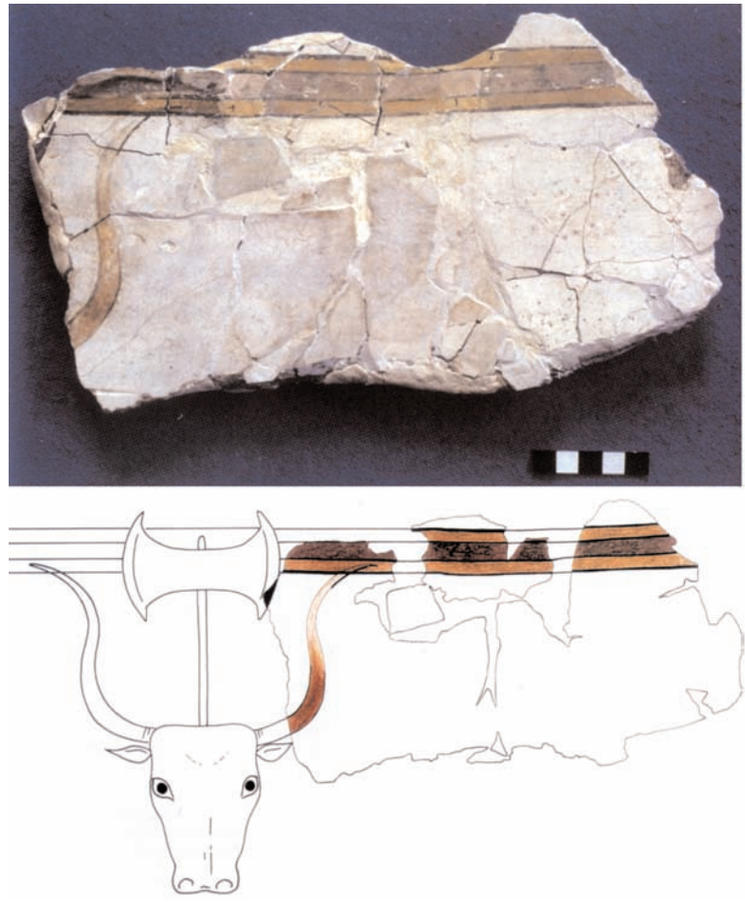


Fig. 16 Reconstruction of a bull bucrania frieze from Alalakh VII (after Niemeier & Niemeier 2000: fig. 14, 15)

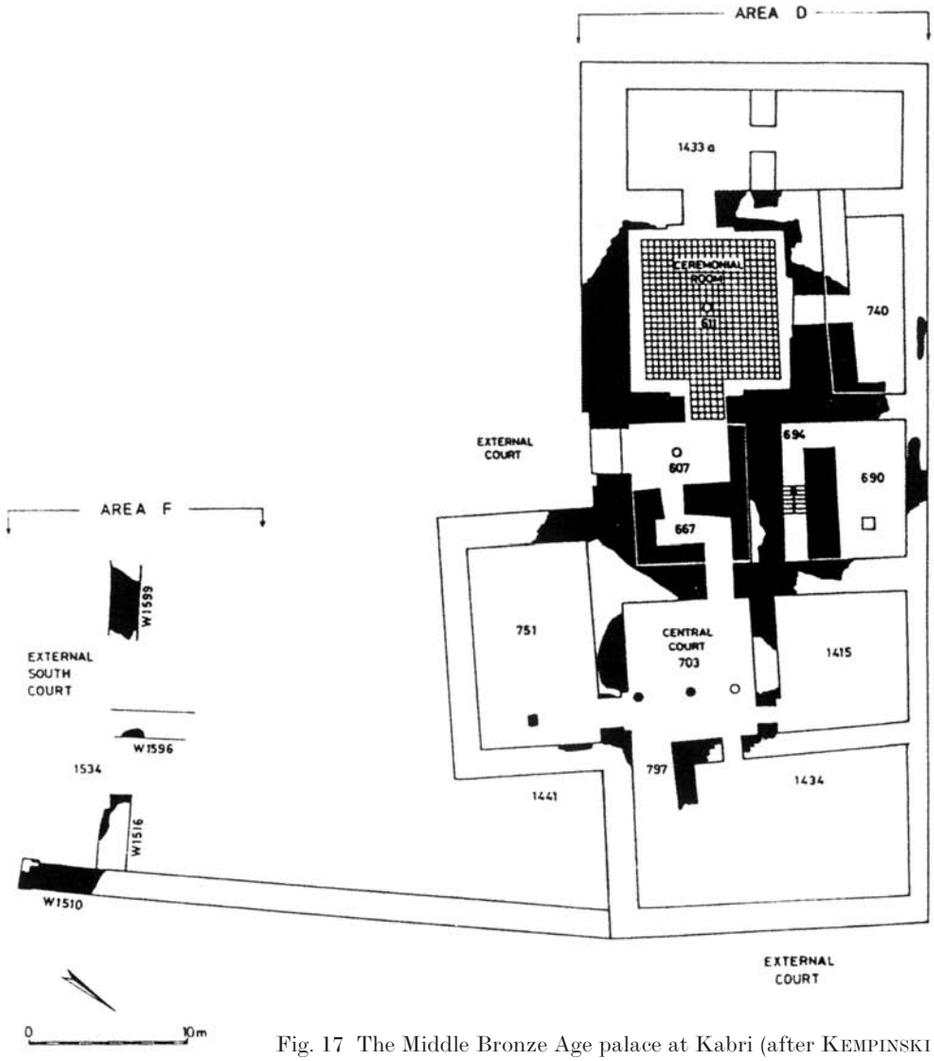


Fig. 17 The Middle Bronze Age palace at Kabri (after Kempinski 2002: fig. 4.73)

There is also evidence of miniature wall paintings from this room. They were found in a secondary context on numerous tiny plaster fragments as filling of threshold 698 between the rooms 611 and 740. Some were also found in debris of wall plaster fallen from the walls. The paintings must have covered the walls above a base zone of orthostats, which had been removed by stone robbers. The lime plaster was applied against a wall with a mud coating with small notches to improve the adhesion to the wall.[112] The paintings were probably arranged in friezes above windows and niches, similar to arrangements typical of Minoan mural art.[113]

The combination of compressed and stone polished plaster, the fresco technique and the application of cord impressions for planning of patterns are purely Minoan, as the excavators pointed out. Despite the small size of the fragments, Barbara and Wolf-Dietrich Niemeier were able to identify features known in the same style in Minoan mural and vase paintings, such as landscape elements consisting of rocky protuberances and outlets (Fig. 19), known from representations of seashores.[114] The paintings from Room 5 of the West House in Thera offer especially good parallels for the abovementioned landscape features of the Kabri paintings.[115] This frieze also shows other features we encounter in Kabri, such as representation of ashlar façades and the round

[112] *Ibidem*: pl. XX.
[113] *Ibidem*: pl. 266 f.
[114] Niemeier & Niemeyer 2000: 777, fig. 9.
[115] *Ibidem*: 266, pls. XXII, XXV, XXXIV.

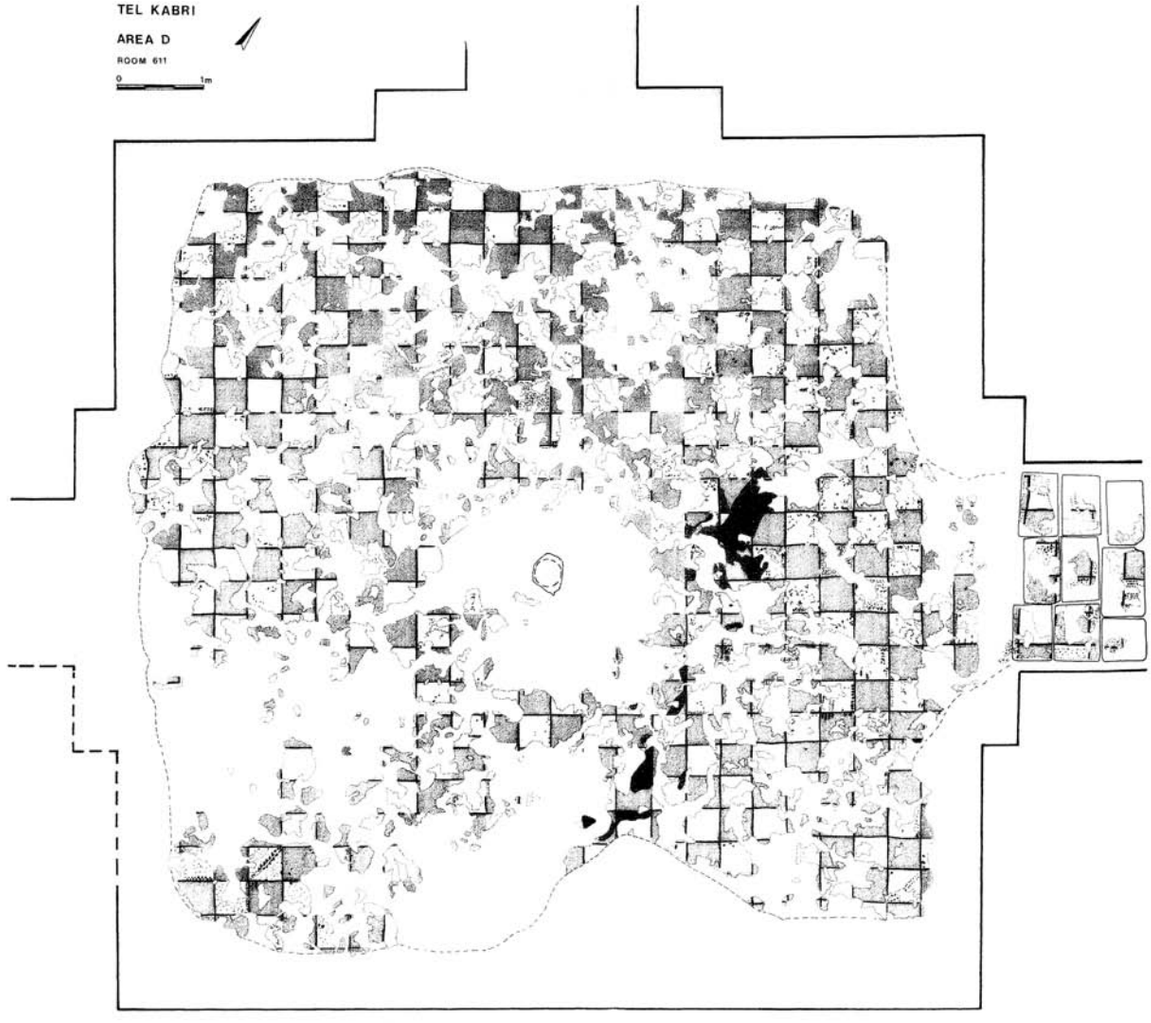


Fig. 18 The plan of the floor paintings, imitating stone slabs from room 611 at Kabri (after Niemeier & Niemeier 2000: fig. 2)



Fig. 19 Reconstruction of Aegean seashore rocks with outlets (after Niemeier & Niemeier 2000: fig. 9)

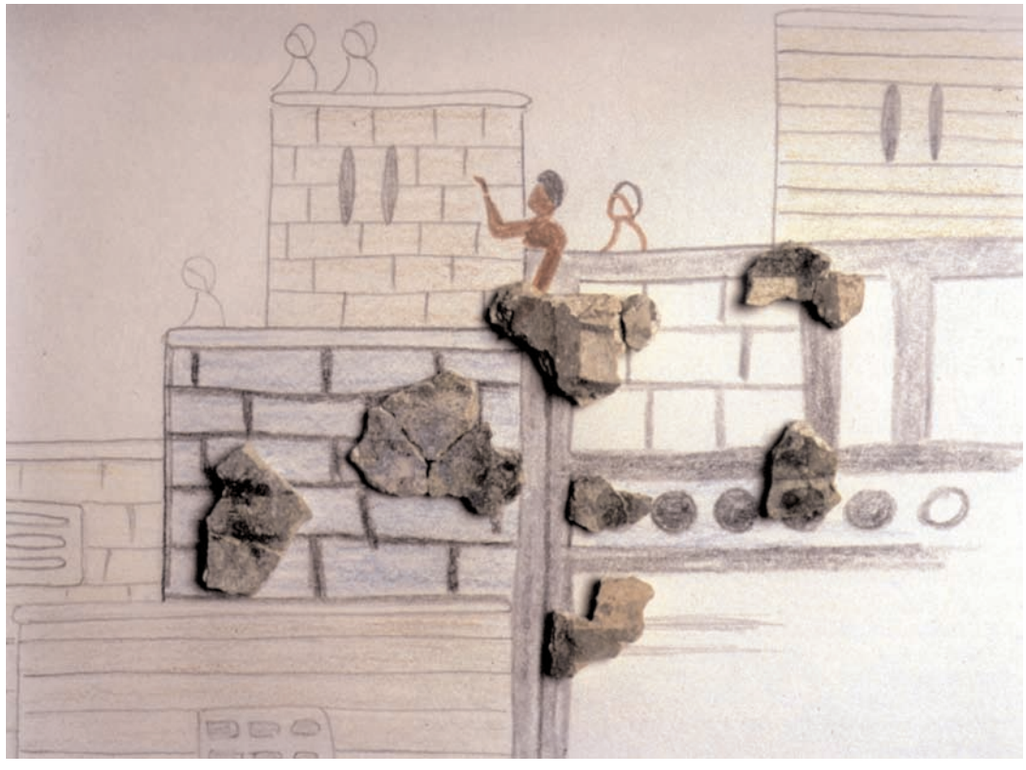


Fig. 20 Reconstruction of ashlar masonry in the wall paintings from Kabri (after Niemeier & Niemeier 2000: fig. 11)



Fig. 21 Fragment of wall painting from Kabri with a representation of a swallow (after Niemeier & Niemeier 2000: fig. 12)



Fig. 22 Fragments of notched wings of a griffin from Kabri (after Niemeier & Niemeier 2000: fig. 13)

beam ends visible in the zones of ceilings (Fig. 20).[116] Finally, very convincing features in comparison with the art of Thera are the representation of a swallow (Fig. 21)[117] and the notches of the wings of an Aegean griffin (Fig. 22) showing the same pattern as the chasing griffin in the West House frieze.[118]

In summary, in Kabri we have a reception room within a Near Eastern princely palace – probably the

[116] *Ibidem*: pls. XXIX, XXXI.
[117] *Ibidem*: pl. XXXIII. Cf. Doumas 1992:146, fig. 146.
[118] *Ibidem*: pl. XXXV. Cf. Evans 1921: 548–551; Morgan 1988: 50–54; Doumas 1992: 65, fig. 32.

throne room, which may, according to the ceramic finds, also have served ritual functions. It was embellished with Minoan paintings. We have a situation similar to that found in Tell el-Dab^{c}a/^{c}Ezbet Helmy. We do not find, however, the typical Knossian emblems and motifs as in Tell el-Dab^{c}a, such as bull acrobatics, big emblematic griffins, and the half-rosette frieze.

In Tell el Dab^{c}a, the setting of the mural art is quite different. This site, with its complex stratigraphy, was the capital of the Hyksos, called Avaris, before the time of the New Kingdom. After its conquest by the founder of the New Kingdom, Ahmose, c. 1530 BC, the site was abandoned but immediately resettled by the Thebans along the east bank of the Pelusiac branch of the Nile. Georges Daressy[119] and Labib Habachi[120] have suggested that Peru-nefer, the major naval and military stronghold of the 18th Dynasty should be located at the site of former Avaris. Until now, no 18th Dynasty installations were attested. This has changed now. Excavations in the last 10 years have revealed overwhelming evidence of a major military base with stockpiles of grain storage, of camps and cemeteries of Nubian archers, and of palatial quarters in royal dimensions.[121] What would also speak in favour of an identification with the base of Peru-nefer is that the palace precinct dates from the time of Tuthmosis III and Amenophis II (together c. 1479–1401 BC).[122] This would be the prime time of Peru-nefer as documented from texts.[123] Moreover, the function as a naval base as attested for Peru-nefer can also be found in Avaris. According to the Kamose stela it was a harbour in the Hyksos Period,[124] and served the same purpose still during the time of the 19th and 20th Dynasty, as the harbour of Piramesse the Delta residence of the Ramessides.[125] Therefore, it is feasible that it also may have been a harbour in the 18th Dynasty, the period of Peru-nefer. Such an identification could be backed by the presence of Canaanite cults as documented in the Papyrus Eremitage 1116A.[126] Such cults and the employment of Canaanite ship carpenters there[127] could be explained as survivals from Hyksos Avaris. Indeed, the temple of Sutekh in Avaris shows continual occupation beyond the time of the conquest by Ahmose.[128] Also, the ceramic production at Tell el-Dab^{c}a in the early New Kingdom shows a continuum from the Hyksos Period, which proves that at least a part of the Canaanite population – the former carriers of the Hyksos rule – remained at the site.[129] The identification with Peru-nefer may not yet be considered as conclusively settled, but its localisation at Avaris instead of Memphis is surely a strong option.

The setting from which the paintings came is the palatial district from the Tuthmoside Period (Fig. 23).[130] It consists of three palaces, a huge one (G), a medium-sized one (F), and a small one (J). They were all constructions elevated on platforms of c. 7 m height. Most of the fragments with paintings originated from Palace F, which can be considered as a purely ceremonial building, as it lacked the usual private section in the rear.[131] Many more wall plaster fragments with paintings were found on the floor of a doorway of an enclosure wall and on a connected porch leading to the major Palace G. Some fragments were even found *in situ* on the wall.[132]

The paintings can be identified as Minoan[133] because of the following properties:

1. Technique (highly polished compact lime plaster mixed with murex shells,[134] the application of fresco, impressed cords for planning, plaster relief[135]).
2. Colours and colour conventions (white for women, reddish brown for men,[136] blue for grey,

[119] Daressy 1928–29; Daressy 1929–31.
[120] Habachi 2001: 106–107.
[121] Bietak, Dorner, Hein and Jánosi 1994; Bietak 1996; Bietak 1996a; Bietak 1997; Bietak, Dorner, Jánosi 2001; Bietak & Forstner-Müller 2003; Bietak 2005a, Bietak 2005b; Bietak 2006.
[122] Kitchen 2000.
[123] Spiegelberg 1927; Glanville 1931; Glanville 1932; Daressy 1928–29; Daressy 1929–31; Säve-Söderbergh 1946: 37; Badawy 1943: 43; Badawy 1948: 137–139; Helck 1939: 49–50; Helck 1971: 446–73; Der Manuelian 1987: 174–175; Bryan 1991: 49–50; Habachi 2001: 106–107.
[124] Habachi 1972: 36; Bietak 1975: 192.
[125] Bruyère 1930: 22; Yoyotte 1971–72: 172; Bietak 1975: 30, n. 38, 187, 205–206.
[126] Stadelmann 1967: 32–34.
[127] Golenischeff 1913: 24.
[128] Bietak 1985.
[129] Bietak 2005; Bietak forthcoming; Goldwasser forthcoming.
[130] Bietak 2002; Bietak, Dorner, Jánosi 2001, Bietak & Forstner-Müller 2003; Bietak 2005; Bietak 2006.
[131] For the reconstruction see Bietak 2005b: fig. 15.
[132] Bietak 2000: 196–197, fig. 5, no. 5.
[133] Bietak 1994; Bietak 2000, Bietak, Marinatos, Palyvou forthcoming; Marinatos 1998; Morgan 1995a; Morgan 2000: 939–940; Niemeier & Niemeier 1998: 90–93.
[134] Seeber 2000; Brysbaert 2002: 99, no.11 and pl. 4.
[135] Bietak 1994: 45.
[136] Morgan 2000: 939–940.

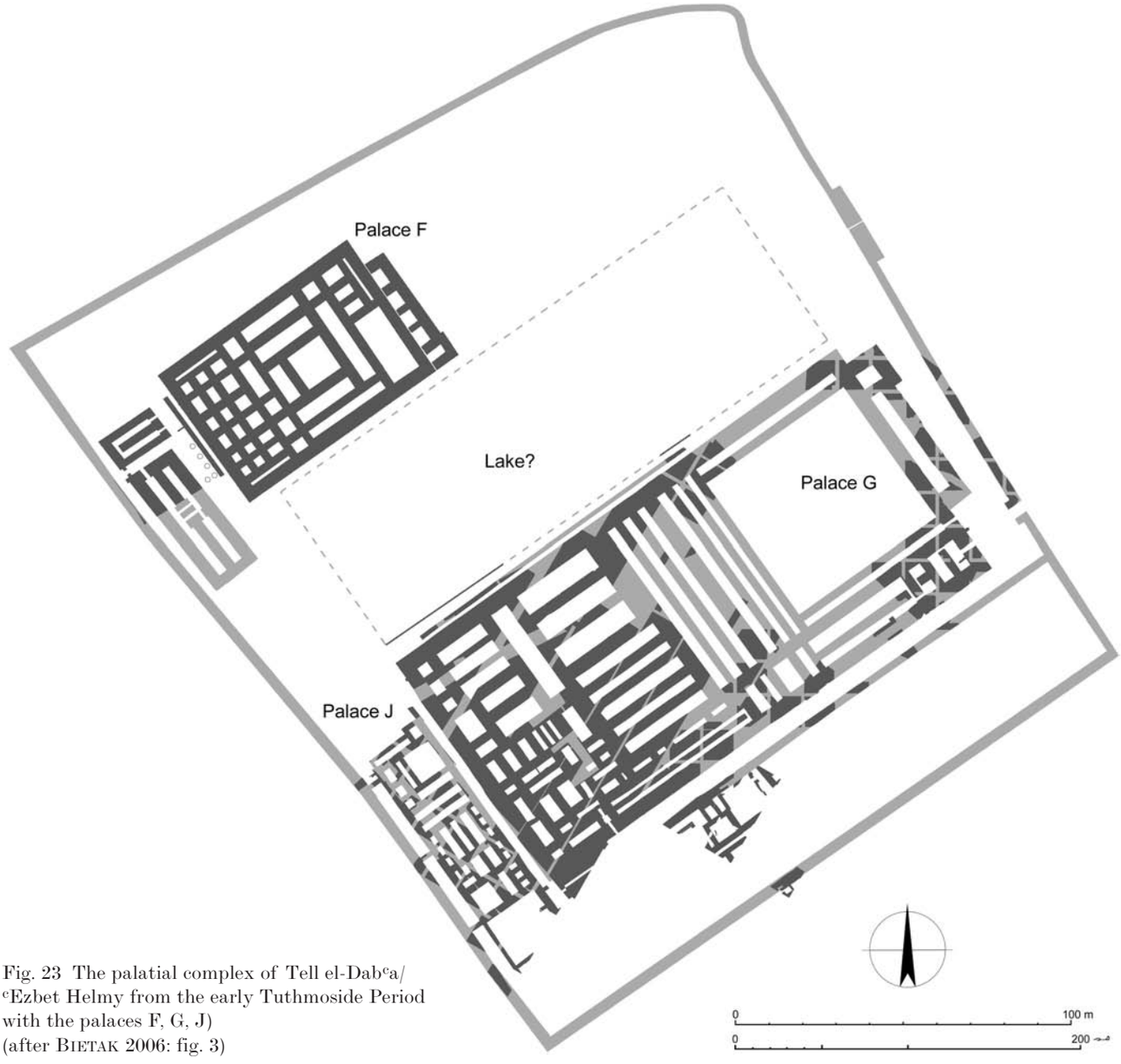


Fig. 23 The palatial complex of Tell el-Dab^{c}a/ ^{c}Ezbet Helmy from the early Tuthmoside Period with the palaces F, G, J) (after Bietak 2006: fig. 3)

such as the scalp[137] or hoofs, horns and grey speckles on bull hides, blue for green for rendering vegetation).

3. Motifs (bull acrobatics, floor acrobatics such as somersaulting, hunting, lions chasing ungulates,[138] rocky landscape, architectural ashlar façade, ivy, wadj = waz-lilies, maze pattern).
4. Style (Minoan style dress for men and women, coiffure,[139] jewellery, cushion shaped seal,[140] ornaments[141] such as running spirals with specific attributes, loop pattern, ivy pattern, flying gallop, etc.).
5. Composition (foreground ending in an undulating horizon, frequently red background, men and ani-

[137] Doumas 1985: 29–34.
[138] A frequent theme in glyptic art, see Marinatos 1998: 84–86.
[139] Marinatos 1998: 88.
[140] Bietak 2000: 200 and pl. C(c).
[141] Aslanidou 2002; Aslanidou 2004.

mals arranged in two registers, one at the base line, one near the horizon, palmtree as divider).[142]

6. Emblems such as the half rosette frieze, large griffins, the symbolic maze pattern, and the motif of bull acrobatics show a direct connection to the major Minoan palatial centre of Knossos.

Until the time of the Mycenean palaces, paintings of bull acrobatics did not turn up in any palace other than Knossos.[143] This makes the paintings at Tell el-Dabᶜa/ᶜEzbet Helmy so unique. As far as evidence goes, it is the first time that such a subject is shown outside Knossos. The maze pattern, together with the motif of bull acrobatics, have to be seen as a strong indication of their originating from the palace of Knossos.[144] In my opinion, this evidence shows a special relationship between Tell el-Dabᶜa and Knossos, which goes beyond every other site in the Levant with comparable mural art (see below).

Although most of the paintings were collected in dumps outside the ramp and landing of Palace F (Fig. 24), it is not overly daring to reconstruct the large emblematic griffin with hanging spiral decoration on the wings, most probably with an antithetical companion, on the rear wall of the four columned throne room, in an arrangement similar to that of the griffins in the throne room in Knossos (Fig. 25).[145] It is also highly likely that the floor painting with the maze pattern (Fig. 26)[146] was originally situated in the throne room. The two friezes with the bull acrobats (Fig. 27), the felines chasing ungulates (Fig. 28),[147] and the hunting friezes may have been applied above doors of the same hall or its vestibule. According to our knowledge from Knossos, plaster reliefs were positioned in daylight under the protection of roofed column galleries (Fig. 29).[148] Their place may have been in the galleries of the central court and in the porch of the entrance courtyard.

The architecture is Egyptian, but the paintings and their arrangement within the given space is Minoan. One may compare the situation with Kabri and with Alalakh, where the main rooms of palaces of Near Eastern architecture were embellished with Minoan paintings. The explanation of Wolf-Dietrich Niemeier,[149] viz., that the paintings resulted from a demand in the courts of the time to have the representative rooms of their palaces embellished with Minoan paintings within the trend of a network of gift exchanges with artists sent as a favour by the Minoans to their princely partners all over the Eastern Mediterranean is not, in my opinion, feasible for several reasons. First, the paintings of those three sites are not contemporary. Second, the three sites do not, as far as evidence goes, show the same pattern of rep-

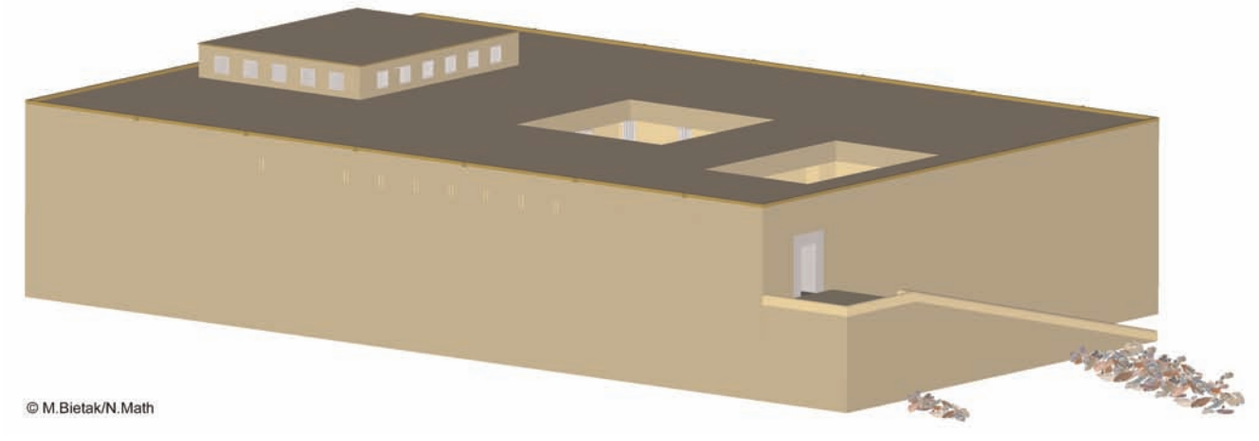


Fig. 24 Reconstructed plan of Palace F, showing also the dumps of wall plaster (reconstruction M. Bietak & N.C. Math)

[142] Bietak, Marinatos & Palyvou 2000.
[143] While painted representation of bull leaping was confined to Knossos, as far as evidence goes, representations of this theme on Minoan seals had a wide distribution.
[144] For the maze pattern in Knossos see Evans 1921: 356–357, fig. 256.
[145] For the reconstruction see Fig. 25.
[146] Bietak 1994: 46, pl. 14B–C.
[147] Marinatos 1998; Marinatos & Morgan forthcoming.
[148] Kaiser 1976.
[149] Supra n. 2.

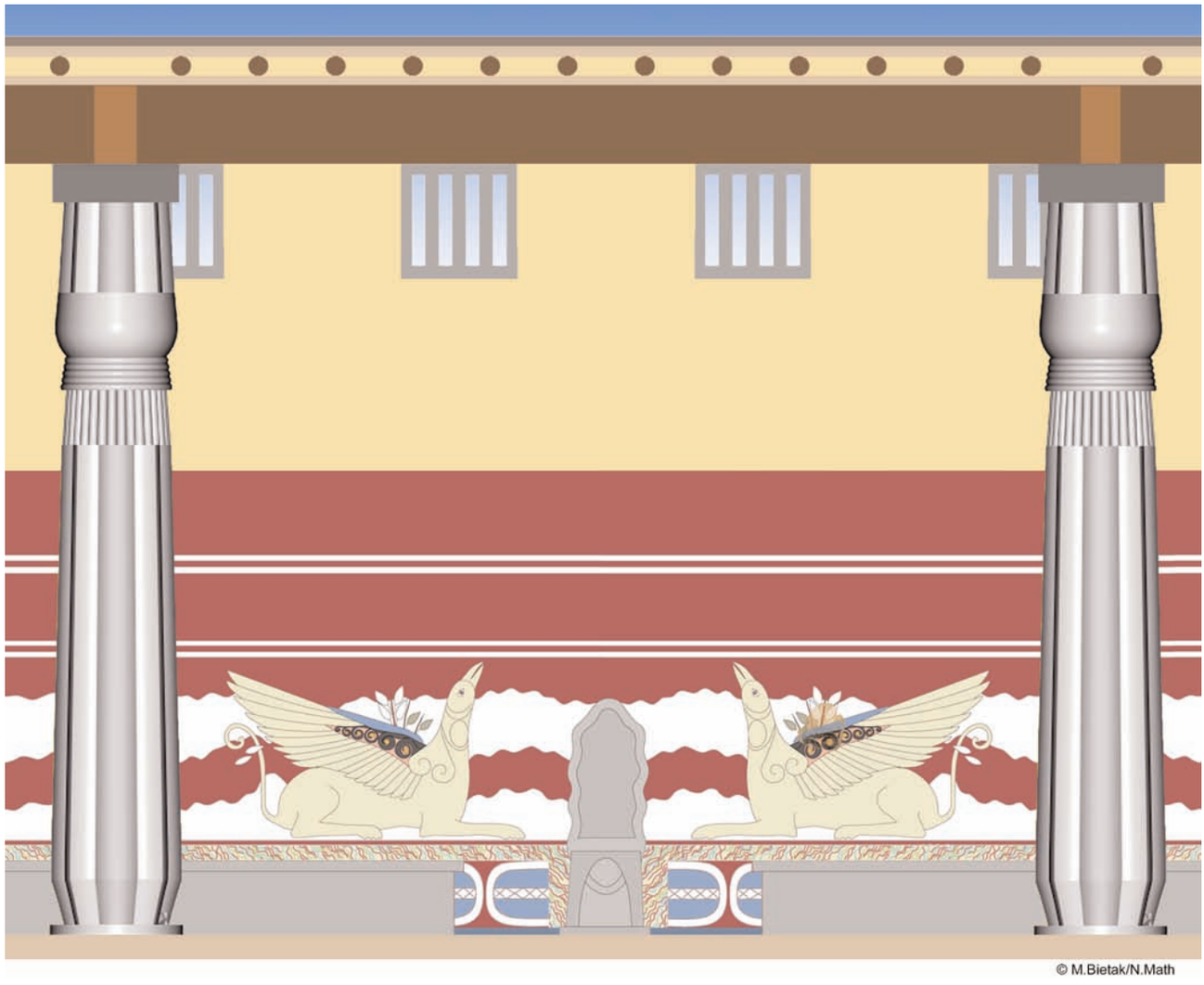


Fig. 25 Reconstruction of the throne room with the heraldic griffin on the back wall (reconstruction M. Bietak & N.C. Math)

resentations in the paintings, what should be expected if they are an expression of an export of Minoan ideology and style of embellishment of palaces in the Levant. The iris and crocus flowers of Kabri, the birds and the ships there did not show up among the numerous representations at Tell el-Dabᶜa.[150] On the other hand this place has numerous motives not shared by the other two sites such as bull grappling and leaping, floor acrobatics, felines chasing ungulates, hunting scenes with dogs, life-size figures, and plaster relief of human and animal representations. The representations of the lions and leopards should also not be understood as an adaptation to the Nilotic environment. Both are well incorporated into Minoan art, including wall paintings, glyptic art and other carriers of Minoan art.[151] Third, even if the Egyptian crown commissioned the embellishment of their palaces by Minoan artists, it is hardly believable that they would not have required them to paint Egyptian emblems of royal power or the names of the king of the time. Nothing of this kind was found. Emblems of palatial power, such as the half-rosette frieze and the griffins, are purely Minoan. The maze pattern is to be taken as a symbol of the palace of Knossos in particular. It is due to the exclusivity of painted scenes of bull acrobatics in the palace of Knossos that I proposed for Tell el-Dabᶜa a different scenario of takeover than at the sites of Kabri and Alalakh.

[150] Nicolakaki-Kentrou 1998.

[151] Marinatos & Morgan forthcoming.



Fig. 26 Fragments of a floor painting with a maze pattern (graphic work L. Zelenková)



Fig. 27 Reconstruction of the Bull & Maze Frieze (after Bietak, Marinatos & Palyvou: 2007, fig. 59)



Fig. 28 Leopard from Tell el-Dab^{c}a (after Morgan 2004: fig.4)



Fig. 29 Plaster relief from the Palace F at Tell el-Dab^{c}a (after Bietak & Marinatos 1995: fig. 5)

The Minoan palatial symbols of power should be read as evidence of direct contact from court to court on the highest level. It should be understood as a political expression. The paintings, with all their symbolism and meaning, were made for a Minoan of the highest position. A political marriage – so important and frequent in the diplomacy of the Near East and Egypt, and so well attested during the time of Tuthmosis III[152] – would be a highly feasible theory.[153] A political meeting of two leading figures of the Egyptian and the Minoan world would be another theory. In the second case, the paintings would have been applied for this event, which may have lasted, perhaps, just a few days. This would be entirely possible in the Ancient World.

The possible identification of this site with Peru-nefer and the presence of Minoan paintings create an association with entries on a papyrus in the British Museum (BM 10056), which is a document of the royal dockyards at Peru-nefer.[154] There, so-called Keftiu, i.e., Cretan, ships are mentioned. Till now, this specification was taken as a type of a ship or as

[152] Winlock 1948; Bietak 2000: 204–205; Lilyquist 2003.

[153] It was never presented by me as anything other than a theory. Therefore, the criticism offered by Cline 1998: 202, 209, 210, 211, 217 and Niemeier & Niemeier 2002a: 285 and others is completely unnecessary, and shows a lack of understanding of Near Eastern politics and history.

[154] Glanville 1931; Glanville 1932. See, however, Warren 2000: 25.



Fig. 30 Keftiu delegation from the tomb of Senenmut at Thebes (after Dorman 1991: pl. 21d)

its destination, not as a place of origin.[155] Given the strong evidence from the paintings, however, this may be an entirely feasible interpretation. The contact with the Keftiu in the Tuthmoside Period is reflected in the representation of Keftiu delegations (Fig. 30)[156] and the appearance of Minoan influence in Theban tomb paintings[157] from the time of co-regency of young Tuthmosis III and Hatshepsut onwards (see above). This date seems to coincide precisely with the archaeological date of the Tell el-Dabᶜa-paintings.[158]

III. Conclusions

The wall and floor paintings found in different palatial contexts in the Levant from the Early to the Late Bronze Age cover a considerable time span and display a differentiated cultural origin. Therefore, they should be explained in an appropriately differentiated fashion, considering the local setting and the individual cultural background. There can not be a doubt that there was a local Syrian tradition of palatial paintings, which goes back to the Early Bronze IV A-Period in the late 3rd millennium BC as in Ebla and can be studied in individual creations of the Middle Bronze Age at Mari and Tell Sakka, the latter showing Egyptian influence. They share the technique of tempera painting (secco) on whitewashed mud coating and plaster coating. In the Middle Bronze Age they display influence of Egyptian iconography (Tell Sakka) and Minoan decorative art (Mari). We know nothing of figural palatial paintings in Egypt during the Old and the Middle Kingdoms. We have remains of linear patterns and hieroglyphic inscriptions from the Hyksos Period on mud coating from a palace district at ᶜEzbet Helmy.[159] In the Southern Palace of Deir el-Ballas of the 17th Dynasty, even figural paintings have been found, which resemble provincial tomb paintings.[160] We may expect therefore palatial mural art in Egypt already in the time of the Middle Kingdom if not earlier.

The paintings of Alalakh VII, Kabri and Tell el-Dabᶜa/ᶜEzbet Helmy can be considered as Minoan. Those sites are nearest to the Mediterranean, while Mari and Tell Sakka, which display local Syrian art,

[155] Säve-Söderbergh 1946: 49.
[156] Vercoutter 1956; Wachsmann 1987; Matthäus 1995; Duhoux 2003.
[157] Morgan 2004: 293–295.
[158] Bietak, Marinatos, Palyvou 2007: 26–40; Bietak, Dorner, Jánosi 2001: 44.
[159] Bietak, Dorner, Jánosi 2001: 40.
[160] Smith 1958:159, fig. 53.

are inland and can even be considered remote from the Mediterranean system. The paintings of Qatna show a high degree of Minoan influence and technique but also show, in my opinion, a blending with the local tradition. They may be explained perhaps as post-Minoan, given the late destruction of the palace in the 14th century. Even if we stick to the palatial paintings, which can be considered as purely Minoan (Alalakh VII, Kabri and Tell el-Dab^{c}a), they cover altogether a time span of 150 to 200 years, depending on whether the Low or the Middle Chronology is applied.[161] A common explanation for all of them seems to me, therefore, unfeasible. Alalakh VII and Kabri may be closer to each other in time, if the low chronology is applied, and could perhaps be explained as a result of a Versailles effect that had not reached Egypt at that time. The chronological table on Fig. 2 may show the expansion of the sphere of influence of the Minoan Thalassocracy in geographical and chronological terms. Tell el-Dab^{c}a, however, stands out in its setting in a ceremonial palace with its Minoan palace emblems, the maze pattern and the scenes of bull acrobatics – a combination known only from Knossos. A political *rencontre* on the highest level offers itself as an explanation.

Addition: J.-C. Magueron 2004 in a recent study has raised the date of the Mari paintigs, particularly the "investiture" painting. For the argumentation in this article this would not change anything. The newly discovered paintings at Tell Bourak in the Lebanon have not yet been considered in this article.

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[161] See above 270–272, 276

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Tell el-Dab^{c}a: Some Remarks on the Pottery from ^{c}Ezbet Helmi (Areas H/III and H/VI, Strata e/1 and d)

Perla Fuscaldo[*]

The ceramic material from the Tell el-Dab^{c}a/Avaris palace complex at ^{c}Ezbet Helmi (areas H/III and H/VI) presented in this paper belongs to the first two strata of the 18th Dynasty. Stratum e/1 corresponds to the earliest phase of the New Kingdom (=Phase D/1), an archaeological horizon characterised by the construction of large storage buildings, offering pits and soldier tombs. Stratum d (= Phase C/3) is the first phase of construction of the main palace, the palace G.[1]

I. Stratum e/1 = Phase D/1 (Fig. 1)

I.1. The fabrics

The most remarkable feature is the use of sandy Nile clay fabrics (especially Nile B_2 that is the most common fabric and, to a smaller extent, Nile C_1 for making large sized vessels). Nile B_2 sandy (= Nile E_4) is used for modelling stratum e/2 shapes, that continue to be used in stratum e/1, as for the new ones. Nevertheless, the typical Nile B_2 fabric from the earlier period is still used, but on no more than 10 to 20% of the vessels. Marl F, a local marl from the eastern Delta which had appeared in stratum e/2 (= Phase D/2) for shaping open and closed vessels, is still used without the variations in its fabric that will appear later. Very few imports from Upper Egypt, in Marl A_2 and Marl A_4, are found in this stratum. There are not yet Marl C zirs from the Memphis-Fayum region.

I.2. The techniques

Some changes appear in the pottery techniques, such as:

a) the use of a tall-stemmed wheel slightly improved (W_2), which leaves ridges on the exterior and interior surface of the vessel. An example of a pot shaped with this technique is the flat-bottomed cup 8732S, in Nile E_4 fabric (*No. 1*, Fig. 1);

b) the shaping of open and closed vessels in two, three or four parts, which are then joined together, as can be seen in the change of direction of the ridges and the smoothing marks or the fingerprints made during the joining. The drop jar 8711O (*No. 2*, Fig. 1) was modelled in Nile E_4 fabric with this technique in a tall-stemmed wheel slightly improved. This technique is more marked on the marl clay vessels.

I.3. The wares

There is neither *Black Rim ware*, typical from the 18th Dynasty, nor *Egyptian fine ware*, but the last one could be related to the function of the different buildings from this period. Besides the imported amphorae used as containers, very few fine imports can be mentioned from this stratum. The *Proto White slip ware* appears in two styles, a plain-white milk bowl (*No. 3*, 9070W_{1-5}, Fig. 1) and a rope lattice decorated milk bowl (*No. 4*, 9070X, Fig. 1). The plain-white milk bowl (*No. 3*) reconstructed from sherds, 16 cm in rim diameter, has a low-lustre white slip burnished with a pebble inside and out. It is the only example of this style found in Tell el-Dab^{c}a. *No. 4* is the body sherd of a milk bowl decorated with a rope lattice pattern with bichrome effect in reddish brown and reddish yellow on a pinkish white slip. Like the other milk bowl, it has the surface pebble burnished with low lustre.[2] The Cypriote *wheel-made Bichrome ware* appears in this stratum.[3]

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[1] The *stratum e/2* (= Phase D/2) corresponds to the last Hyksos period, and *e/1* (=*Phase D/1*) from the beginning of the Ahmose occupation of Avaris. The *stratum d* is the *Phase C/3* and *c* the *Phase C/2*, both phases of the Tuthmoside palace complex at ^{c}Ezbet Helmi On the stratigraphy of ^{c}Ezbet Helmi see: Bietak, Dorner und Jánosi 2001 and Bietak, in Fuscaldo forthc. a.

[2] On the *WS ware* see the different articles in Karageorghis 2001, Bergoffen 1998. On the published material from Tell el-Dab^{c}a: Bietak and Hein 2002; on the WS ware from ^{c}Ezbet Helmi, Area H/VI: Fuscaldo forthcoming d.

[3] On the *BI ware* from different areas of Tell el-Dab^{c}a: Bietak 2001, and Hein 2001.

I.4. The shapes

There are few new shapes. Most of the *flat-bottomed cups* show a slightly angular transition between the diameter at the major point and the base, made with the finger while the vessel is thrown on the wheel; the vessel index is between 90 and 93. *No. 5* (8895U, Fig. 1) and *No. 6* (8711B, Fig. 1) are made of Nile E_4 with an uncoated surface.

The model or votive pottery includes *deep model bowls* with flat bases and everted direct rims (*No. 8*, 8903Z, Fig. 1). This shape is not found in stratum d. Some *drop jars* have *flat bases*, flattened with the thumb from a round one, or cut off from the wheel with a string to make a small flat base (*No. 7*, 8895P, Fig. 1). Besides the *flat-bottomed bowls* with straight- or flaring-side walls and rounded rim, there are bowls with trimmed rims, such as *No. 9* (K-6681 [016], Fig. 1). The same typological feature is found in the shallow *model bowls* with flat, raised or wobbly bases (*No. 10*, Fig. 1; K-6564 [037]).

II. Stratum d = Phase C/3 (Figs. 2–6)

II.1. Fabrics

The fabrics, techniques and shapes that have already appeared in stratum e/1 are fully developed in stratum d. Nile E_4 is the fabric most frequently used for shaping the table and votive wares. Fine Nile B_2 and Nile E_4 fabrics are used for fine wares. Besides the common Marl F, a finer version of this fabric (the so-called "New Kingdom Marl F", with small quartz grains) and a Marl F with crushed limestone particles included in the fabric to make it appear stronger. Open and closed vessels in Marl A_2 and Marl A_4, and large water jars –"zirs"– in Marl C_2 are found and developed throughout this period. There are few jars made of Marl B.

II.2. Techniques

Besides this, a new feature in the shaping technique – the finishing of round bases on the wheel – and new shapes appear. The round-bottomed jars made of Nile and marl clay fabrics present ridges on the base, a clear indication that the base was finished on the wheel (*No. 11*, Fig. 2). This feature is found also on the pointed bases of cups (*No. 13*, Fig. 2) and small jars.

II.3. The shapes

a) Cups

Beside the flat-bottomed cups, the most frequent shape of the beakers, cups with round or pointed bases are found. *No. 12* (8962E_{1-2}, Fig. 2) is a *round-bottomed cup* made of Nile E_2 fine,[4] with the base cut off the wheel with a knife and then smoothed, with an index of 96. This fabric is quite uncommon in this stratum. *No. 13* (K-9072 [070], Fig. 2) is a red-slipped *cup with pointed base* made of Nile B_2/E_4, with the base finished on the wheel. Some of these cups present a rim band –red or black–[5] and convex sided walls (*No. 14*, K-9046 [154], and *No. 15*, K-9046 [246], Fig. 2).

b) Fine ware

There are examples of Egyptian fine ware, such as footed carinated bowls, ring-based bowls, flat-bottomed bowls, small bowls with interior lip and small jars or pots. The walls of all these shapes are very thin, with a red-slipped surface, well smoothed or polished. The *footed carinated bowl* 8909K (*No. 16*, Fig. 2), a reconstructed shape, is made of Nile E_2 fine fabric with a red polished slip and a black rim band. It has a sharp carination and very thin walls.
The ring-based bowl is a common type, but *No. 17* (K-9002 [1], Fig. 2), as an example of fine ware, is made of Nile B_2, with a red-slipped polished surface and is small in size, with 16 cm of rim diameter. There are other types of fine bowls with a flat base and a red-slipped polished surface, such as *No. 18* (K-9002 [7], Fig. 2) in a fine Nile E_4 fabric, or with interior lip, such as *No. 19* (K-9002 [9], Fig. 2), made of Nile E_4 with the red-slipped surface well smoothed. *No. 20* (K-9046 [169], Fig. 2) is a pot with an everted, direct rim and red-slipped polished surface made also of Nile E_4.

c) Black Rim ware

The Black Rim ware is specially used for decorating ring-based bowls (*No. 23*, 8961W, Fig. 3), but it is also found on cups (*No. 22*, K-9074 [150], Fig. 3), fine bowls[6] and small jars.

d) Small bowls

A finer version of the flat-bottomed bowls are *small bowls*, 14–15 cm of rim diameter with convex or

[4] A Nile clay fabric tempered with sand. A coarser fabric is used for cooking pots. A finer version is the common fabric during the Ramesside period.

[5] There are few cups with a black rim band.

[6] See above.

straight sided walls, and the exterior and interior surfaces with a red slip *horizontally burnished* in Nile E_4 (*No. 24*, K-9072 [17], and *No. 25*, K-6576 [75], Fig. 3). These shapes are not very common in stratum d, but became a characteristic feature of the stratum c pottery typology.

e) Jars

A new shape of the household ware includes *jars with everted rims*, direct, thickened or compound, and flat, rounded or pointed bases. This shape is the commonest one of the jars from stratum d. A *small size jar* (*No. 21*, 8960A, Fig. 3) presents a flat base made on the wheel and an everted direct rim. A red-slipped band –or a black one on others– decorates the rim.

The *slender jar*, short or tall necked, slender or medium-broad type, small, medium or large size, with a round, flattened or slightly pointed base, is a very common shape in stratum d. *No. 26* (8960B, Fig. 3) is a medium-sized short necked jar with an everted direct rim, made of Nile B_2. *No. 27* (9060, Fig. 3) is a large water jar ("zir") in Nile E_4, of short necked slender type with a flattened base,. This type of zir is also made of Marl F. A special type of jar is the *white washed* (WF-Vase) with nipple base and thin walls, made of Nile B_2/E_1 or Nile E_4 fabrics. The firing produces a white exterior surface or a white self-slip. This type of jar is found from stratum e/2 onwards. The shape of the rim presents two variants, trimmed (*No. 28*, Fig. 3) or rounded (*No. 29*, Fig. 3), the first one being the most common.

f) Egyptian imitations of imported wares

A specific feature of the stratum d is the *Egyptian imitations of imported wares*, most of them of Levantine MB II B–C origin. That they are not older ceramic material is clearly shown by the use of a sandy fabric and/or by the shaping technique (W_2), which are typical of the New Kingdom.

There are burnished jugs, with a funnel-shaped mouth, double-strap handle, pronounced shoulder and ring base, in a very fine Nile clay, with fine quartz and few organic inclusions. The fabric analysis made by Maciej Pawlikowski indicates that there is Nile clay from the Delta, or from the valley, with ash in the matrix. *No. 30* (8962W_{1-2}, Fig. 4) is a reconstructed vessel from this type. The surface is vertically burnished, horizontally on the rim and shoulder, with medium lustre The change in the surface colour of the same jug, which is a feature common to all of them, in this example from red (2.5YR 5/6) to pink (5YR 8/3), was probably caused by a poorly controlled firing process. This surface treatment is a precedent of the polished Marl D jars from the Ramesside period. In nearly all the contexts from stratum d some fragmentary vessels or body sherds of this ware appear, but from stratum c onwards it is not found again.

A *medium sized jar* and a *large storage jar* is reminiscent of the shape of fine pots made in Avaris during the Canaanite period (Phases E/1–D/2) from MB II B–C origin. Examples of this shape are a jar (*No. 31*, Fig. 4) and a large storage jar (*No. 32*, Fig. 4).[7] The first one, 8960E, is made of Nile B_2 and W_2 technique; it has a raised base of 3 cm, a rim of 13 cm, a maximum diameter of 19 cm, and a height of 23 cm. The storage jar is made of Nile E_4, with a rim diameter of 33.5 cm, a diameter at the major point of 56 cm, and a height of 83 cm. Other storage vessels of the same shape as No. 32 are 8959G (from L4813) and 8973B (from L2100). Until now, these are the only three examples of this shape of storage jar among the 18th Dynasty registered material from ^{c}Ezbet Helmi, areas H/III and H/VI. This shape continues in stratum c, but in a smaller and finer version with a red slipped polished surface. A complete shape of this type is 8953E (from L4576), in Nile E_4, with the base raised in the middle (R_2-base), 6.2 cm of rim diameter, 9 cm wide, and 9.2 cm high. The surface has a red-polished (10R 4/8) slip with high lustre.[8]

A *bowl* in Nile E_4 with *rounded carination*, horizontally burnished above the carination and radially below it, on the inside preserving the fine ridges of the shaping, and a soft burnishing on the exterior surface (*No. 33*, K-6576 [72], Fig. 4), is an Egyptian imitation of the carinated bowls from the MB II B–C period, made of a fine Nile B_2 fabric in W_2 shaping technique.

Two *large bowls burnished inside*, one in Nile E_4 and the other in the New Kingdom Marl F, are other examples of Egyptian imitation shapes from MB II B–C origin. One is a cross-burnished bowl (*No. 34*, Fig. 4), 36 cm of rim, made in Nile E_4 with the uncoated exterior and the red-slipped interior, horizontally burnished below the rim and vertically on the body making a cross inside, and smoothed out-

[7] The contexts where these two vessels and that mentioned in n.12 are from will be published by David Aston.

[8] 8953E and the rim sherds from other two jars (K-9072 [7] and [84]) will be published in Fuscaldo forthcoming b.

side. The other is a bowl (*No. 35*, Fig. 4) in a very fine Marl F fabric with a radial burnished decoration inside.

A very common shape from the same origin during the Hyksos dynasty (Phases E/1–D/2) is the red *burnished dipper juglet*. The stratum d shape presents two variants, both of ovoid type: one with a short body (*No. 36*, Fig. 4) and the other with a long body (*No. 37*, Fig. 4; 8961Z). The first one is made of Nile E_4 with a burnished slip between pale red and red (10R 4/3–4/6); the second one, in Nile B_2, was made in three parts; the base trimmed with a tool on the wheel, then smoothed, then vertically burnished all over the body (10R 5/8 red slip). The rim is flaring sided, the neck funnel-shaped, and the handle a strap handle from the rim to the rounded shoulder. The second variant is most frequently found in stratum c.[9]

The *Egyptian imitations of Cypriote pottery* that appear in stratum d onwards include the *Cypriote wheel-made Bichrome*, in Nile and marl clays (usually Marl A_2 and A_4),[10] *White Painted V* and *Proto White Slip* wares. An example of the Egyptian *BI* ware is jug *No. 38* (8962$X_{1–3}$, Fig. 5), a carinated jug with a horizontally painted band, black-red-black, at the major diameter, and oblique patterns of black cross-lines framed by a black-red-black band, from the neck-base to the maximum diameter of the vessel, made of Nile B_1 in the W_2 shaping technique, with a high lustre polishing. No handle was preserved. Egyptian *BI* ware is more abundant in the contexts from stratum c.[11]

There is a sherd from an Egyptian juglet in the *WP style*, made of Marl A_3 (*No. 39*, K-9301, L4813), and a globular bowl with everted rim in Nile B_2 with a black decoration of a wavy line framed by a straight band below the rim and an oblique ladder pattern on the body as an imitation of a *PWS ware*[12] (*No. 40*, 9021A, L4813, Fig. 5).

In stratum d, and continuing in stratum c, many Kerma sherds were found.[13] There is an *Egyptian imitation* (*No. 42*, 8930C, Fig. 5) *of an incised Kerma cooking pot* (*No. 41*, Fig. 5). The Egyptian cooking pot is made of Nile E_2, the fabric tempered with sand and used for this kind of vessel, in the W_2 shaping technique, with an obliquely incised-line pattern made on the smoothed surface.

g) The imports. There are *imported Levantine amphorae* in stratum e/1 and d, more abundant in the number of vessels in later strata. Beside these containers, *fine wares* were imported *from Cyprus* and the *Levant*. An example of *Cypriote wheel-made BI ware* is a juglet, *No. 43* (8963$A_{1–5}$, Fig. 5), of globular type with a flat base, decorated in a style which combines a vertical pattern of three painted bands and a pattern of four cross-lines, on the body, a horizontal band (black-red) on the neck and inside on the rim, on a very pale brown slip polished surface.

There is a body sherd from a *WS I milk bowl* (*No. 44*, 8967B, Fig. 5), decorated with a lozenge pattern and two parallel lines, painted in brown and reddish-yellow colours making a bichrome effect, on a pinkish-white slip pebble burnished.

There are three interesting pieces imported from the Levant which are examples of imitation of foreign wares. Two of them imitate *Cypriote PWS milk bowls*. The first one (*No. 45*, 9071D, Fig. 5) is a red burnished bowl, 16 cm in rim diameter, made of IV.22[14] fine fabric with W_2 technique, a rope-lattice style-decoration, and with a horizontal handle with a nearly round section. In the preserved fragment, the decoration shows a vertical rope lattice pattern below the handle, on the body, a horizontal wavy line framed by a band below the rim and some decoration on the handle not well preserved, in reddish black and reddish grey colours with a bichrome effect, on a light red surface burnished with a pebble. The second one (No. 46, 9071J, Fig. 6) is a sherd of an everted rim bowl, in IV.21 fine fabric, decorated on both surfaces with two horizontal parallel bands and an oblique cross-line pattern below, outside, and with two horizontal wavy lines framed by a horizontal band, below the rim inside; polished after painting.[15]

The third one is the pointed base of a *burnished dipper juglet* (*No. 47*, K-6531 [61], Fig. 6), in a fine IV.13 fabric, with the same surface treatment as jug No. 30 mentioned above. The surface has a colour that changes from light brown (7.5YR 6/4) to pink

[9] On the typology of the dipper juglets from Tell el-Dab^{c}a: Kopetzky 2001.

[10] On the *Egyptian Bichrome ware* from ^{c}Ezbet Helmi see Bietak 2001, 175–201.

[11] David Aston personal communication.

[12] These two sherds and the contexts where they were found will be published by David Aston.

[13] On the Kerma pottery see below.

[14] The fabric classification of Levantine (= IV) pottery was made by I. Hein.

[15] There are not only *Levantine imitations* of *WS ware* but also of *BR ware*, as the Canaanite jug imitating a *BR II* jug from Tell Far^{c}ah South mentioned in Bergoffen 1998, plate 106.

(7.5YR 7/3). Another interesting piece, but from a later stratum (b = Phase C/1) is a Levantine pilgrim flask, a lentoid-shaped vessel made of IV.21 fabric, with the same surface treatment, in a colour from reddish yellow (5YR 7/6) to very pale brown (10YR 8/2).[16] As this surface treatment is not found in the Levantine ceramic material, these two vessels, the dipper juglet and the pilgrim flask found in ᶜEzbet Helmi, could have been made by Egyptians in the Levant with local clays and then carried with them to Tell el-Dabᶜa.

All the *Cypriote Base-Ring*[17] material from ᶜEzbet Helmi, H/III and H/VI,[18] stratum d[19] onwards belongs to the BR I ware.[20] Most of them are closed shapes.[21] *No. 48* (8963L, Fig. 6) is the body sherd of a BR I black-matte slipped juglet, from an offering pit. It is a sherd near the base because some fingerprints can be seen inside.[22]

A large amount of *"Classic" Kerma pottery* was found in H/III and H/VI.[23] From stratum d, as examples, I include here a black-topped beaker and two cooking pots. *No. 49* (8943M, Fig. 6) is the rim sherd of a *black-topped beaker* of tulip type, polished with high lustre inside and out and made of Nile B_2. One of the cooking pots (*No. 50*, 8929M, Fig. 6), a rim sherd of a convex sided vessel in Nile E_3 (a coarse fabric made of Nile clay tempered with sand and chaff), has an *incised decoration* with an obliquely incised-line pattern below a horizontally polished rim band framed by an incised line. The other is a body sherd (*No. 51*, 8942F, Fig. 6) in the same fabric with *impressed decoration*, a diagonally impressed pattern made with a toothed wheel on the smoothed surface.

III. Conclusions

The fabrics, techniques and shapes of the Egyptian pottery and the imports from the Eastern Mediterranean and the Levant presented in this paper characterize the ceramics from the first two strata of Tell el-Dabᶜa at ᶜEzbet Helmi, areas H/III and H/VI, at the beginning of the 18th Dynasty.

The stratum e/1 (= Phase D/1) belongs to the period when Avaris was an army camp used in the campaigns against the Canaanites in the area between Avaris and Sharuhen.[24] The earlier part of this stratum, e/1.2 (= Phase D/1.2), has a large complex of storage buildings with round silos, stables for transport animals and paved courts, no doubt for measuring and handling grain, surrounded by an enclosure wall, used to supply the Egyptian army during the military campaigns. Evidently this complex was used for a long time as the silos had been reinstated at least four times.[25] A foundation deposit (L1058) with votive vessels related to the storage complex,[26] and an execration pit (L1053) with three male human skulls and three right hands, two of them with Negroid features, in H/III, and an offering pit L4329 in H/VI, with broken pottery and animal bones –some of them burnt– as part of a festivity meal, belong to this phase. The later part of this stratum, e/1.1 (= Phase D/1.1) has offering pits (L1017, L1021, L1029), an execration pit (L1016) and soldiers tombs, all of them in H/III, and two other offering pits in H/VI (L4919 and L4932, the last one with a large number of ceramics).[27] There is no difference in the pottery typology from both parts of stratum e/1.[28]

The first appearance of imports in stratum e/1

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[16] 8894E. Two samples were found in Qantir (David Aston personal communication).

[17] On the *BR ware* see the articles published in Åström 2001, especially Vaughan 123 and figs. 1–4; and the classic publication by Åström 1972.

[18] Fuscaldo 2003.

[19] In the last campaign in H/III, the rim of another *BR I* (Brushed Brown Burnished Slipped carinated bowl, 9071E, L2033, stratum d) appeared.

[20] Only a handle belongs to the *BR II ware* (8205Q, in the article mentioned above).

[21] The closed *BR* vessels were used as containers for opium and special oil. On this subject: Bergoffen 1991, especially 72–73.

[22] A *BR I* jar (8943D, L1008, stratum d–c, from the Palace G) was published in fuscaldo 2001. On the material from other areas of Tell el-Dabᶜa: Hein 2001.

[23] There are sherds from around sixty beakers, bowls and cooking pots. On the Kerma pottery from these areas: Fuscaldo 2002a and Fuscaldo 2004. Beside this material, other Kerma cooking pots appeared among the pottery recorded in the spring campaign 2003. The Kerma pottery from other areas of ᶜEzbet Helmi was published by Hein 2001a.

[24] There are some references to the Egyptian campaigns in this area in the "Inscription of Ahmose, son of Abana", in his tomb at El-Kab (*Urk*. IV, 1–11).

[25] Bietak, in Fuscaldo forthc. a.

[26] The objects, made of Nile silt clay, include: a fine ring-based bowl, model pottery (a model basket, model sieves, a model basket with a sieve and a model mortar with a pestle), symbolic offerings (a model piece of bread and a model piece of meat), an agriculture tool (a model "*mr*" hoe) and a lump of mud. Fuscaldo 2002b.

[27] On the offering pits from some areas of Tell el-Dabᶜa: Müller 2002.

[28] On the pottery from this stratum see Fuscaldo forthcoming a and Fuscaldo forthcoming c.

used as a datum line in chronology, worth mentioning is the Cypriote *wheel-made BI ware*. *PWS* ware appears earlier, in stratum e/2 (= Phase D/2, the end of the late Hyksos period in Tell el-Dabᶜa) and continues to be imported in stratum e/1 (= Phase D/1). The *hand-made BI* ware is not found again. Bietak dated the Phase D/1.2–D/1.1 from the late reign of Ahmose after the fall of Avaris onwards (c.1530 B.C.) until the end of the reign of Thutmosis I (c.1482 B.C.), assigning c. 25 years for each of the two phases (c.1530–1482).[29]

According to Bietak, the palace complex dates into the Thutmosid period (15th Century B.C.). The stratum d (= Phase C/3) corresponds to the first phase of construction of the main palace of the complex (the palace G), and the stratum c (= Phase C/2) to the second phase of construction, which Bietak attributes to the time of Thutmosis III (1479–1425 B.C.) to Amenophis II (c.1479–1401), but also thinks that the first phase C/3 (stratum d) may even belong to the early reign of Thutmo-sis III.[30] In Phase C/3 (stratum d) *WS I* and *BR I* wares appear for the first time in Tell el-Dabᶜa and continue being imported into the next phase (C/2, stratum c). No *Red Lustrous Wheel-made* ware is found in Phase C/3, but in C/3–C/2.[31]

If we accept the opinion of Oren[32] that *BR I* ware does not appear in quantity before the time of Thutmosis III, and of Eriksson[33] that *BR I* and *RLWM* wares can be dated closely to the reign of Thutmosis III in Canaan, and that the bulk of these imports is likely to be from the reign of this pharaoh, the dating of Phase C/3 to the early reign of Thutmosis III will be confirmed.

VI. The Catalogue

VI.I. The pottery from stratum e/1 = Phase D/1 (Fig. 1)

No. 1: Flat-bottomed cup, 8732S, Fig. 1 *(H/III-s/16, L1016*[34]*)*

RF	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Rd: 8.0 cm; Md: 9.0; Bd: 3.0–3.3; Wd: 0.5; H_1: 8.8–9.0; H_2: 4.3. 10R 5/6 red slip. Break: 2.5YR 5/1 grey core; 2.5YR4/8–5/6 red oxidation zones. Base cut off the wheel and then smoothed; smoothed.

No. 2: Drop jar with round base, 8711O, Fig. 1 *(H/III-s/16, L1029)*

TG	I-b-2	mid	W_2	Bd. gesp.	ox	2

Rd: 9.0 cm; Md: 13.5; Wd: 0.6; H_1: 35.0; H_2: 10.5. 2.5YR 6/6 light red–7.5YR 6/4 light brown. Break: grey interior, reddish brown exterior. Ridges inside; smoothed.

No. 3: PWS plain-white milk bowl, 9070W_{1-5}, Fig. 1 *(H/VI-x/19, L4926)*

P	VI	mid	Ha_2	——	mid	3

Rd: 16.0 cm; Wd: 0.25–0.30; H_1: 5.5 + x. 10YR 8/1 white slip. Break: 5YR 6/1 grey. Quartz (>0.25 mm, >0.50: abundant). Pebble burnished, low lustre.

No. 4: PWS milk bowl, **BI effect,** 9070X, Fig. 1 *(H/VI-x/19, L4973)*

PBI	VI	mid	Ha_1	——	ox	3

Wd: 0.35 cm. 7.5YR 8/2 pinkish white slip. Break: 5YR 5/4 reddish brown. Quartz (>0.25 mm, >0.50: abundant; >2: few), crushed limestone particles (>0.25: few). A rope lattice pattern? 2.5YR 4/4 reddish brown and 7.5YR 6/6 reddish yellow. Pebble burnished.

No. 5: Flat-bottomed cup, 8895U, Fig. 1 *(H/III-s/16, L——)*

TG	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Rd: 6.8 cm; Md: 9.5; Bd: 6.0; Wd: 0.6; H_1: 10.5; H_2: 5.5. 2.5YR 5/8 red. Break: 7.5YR 4/2 brown core; 2.5YR 5/8 red oxidation zones. Smoothed.

No. 6: Fat-bottomed cup, 8711B, Fig. 1 *(H/III-s/16, L1021)*

TG	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Rd: 8.0 cm; Md: 9.0; Bd: 3.6; Wd: 0.4; H_1: 9.7; H_2: 5.2. 10R 6/6 light red. Break: 2.5YR 5/6 red. Smoothed.

No. 7: Drop jar with flattened base, 8895P, Fig. 1 *(H/III-s/16, L1016)*

RF	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Md: 13.0 cm; Bd: 3.5; Wd: 0.7; H_1: 12.5 + x; H_2: 10.5. 10R 5/6 red slip. Break: 2.5YR 5/1 reddish grey; 2.5YR 4/6 red oxidation zones. Base cut off with a string, then smoothed and flattened with the finger. Smoothed.

No. 8: Model deep bowl, 8903Z, Fig. 1 *(Intrusive material from str. e/1 in H/III-u/17, L1290, str. e–f=Phase D/2, an ash fill under a compact mud-filled layer with mud-brick debris, cut by a sandy pit and by the deepest part of the foundation trench for the wall M1011)*

TG	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Rd: 8.0 cm; Bd: 3.5–4.0; Wd: 0.7; H_1: 4.4. 7.5YR 6/4 light brown. Break: 10R 5/8 red core; 5YR 6/4 light reddish brown oxidation zones. Smoothed.

No. 9: Bowl with trimmed rim, K-6681 [16], Fig. 1 *(H/III-u/16, L1149)*

TG	I-e-4		W_2	——		

[29] On the dating of these strata, see note 1.
[30] On the pottery from stratum d: Fuscaldo forthcoming c.
[31] In "the bathroom" of the Palace G, L1008. See n. 22.
[32] Oren 1969.
[33] Eriksson 2001, especially 51–53.
[34] Fuscaldo forthcoming a.

Rd: 20.0 cm; Wd: 0.7; H_1: 2.0 + x. 2.5YR 6/6–6/8 light red. Break: 2.5YR 5/2 weak red core; 5/6 red and 7.5YR 5/6 strong brown oxidation zones. Smoothed.

No. 10: Model bowl with trimmed rim, K-6564 [37], Fig. 1
(H/III-s/16, L1028)

TG	I-e-4		W_2	—		

Rd: 10.0 cm; Wd: 0.35, H_1: 2.0 + x. 7.5YR 6/4 light brown. Break: 5YR 6/4 light reddish brown grey core; yellowish red oxidation zones. Smoothed.

VI.II. The pottery from stratum d = Phase C/3
(Figs. 2–5)

No. 11: Jar with round base finished on the wheel, 8962H, Fig. 2
(H/VI-x-/19, L4932)

RF	I-e-5	mid	W_2	Bd. W	ox	2

Rd: 11.5–12.0 cm; Md: ca. 21.5; Wd: 0.7; H_1: 26.0; H_2: 12.0. 5YR 6/4 light reddish brown; 10R 5/4 weak red slip. Break: 5YR 6/4 light reddish brown. Fingerprints on the base inside; string marks at the major point. Smoothed.

No. 12: Cup with round base, 8962E_{1-2}, Fig. 2
(H/VI-x-/19, L4932)

RF	I-e-2	mid	W_2	Bd. gesp. g.	ox	2

Rd: 5.0–5.6 cm; Md: ca. 6.5; Wd: 0.35; H_1: 6.8; H_2: 4.0. 7.5YR 6/4 light brown; 10R 6/6 light red slip. Break: 7.5YR 5/1 grey core; 2.5YR 5/8 red and 7.5YR 6/4 light brown oxidation zones. Smoothed.

No. 13: Cup with pointed base, K-9072 [70], Fig. 2
(H/III-w/16–17, L4602)

TG	I-b-2/e-4	mid	W_2	Bd. W		

Md: 7.8 cm; Wd: 0.45; H_1: 5.4 + x; H_2: 3.5. 2.5YR 5/6 red; 10R 5/6 red slip. Break: grey core; red oxidation zones. Base finished on the wheel; smoothed.

No. 14: Cup, concave sided, K-9046 [154], Fig. 2
(H/VI-t/13–u/12, L4107)

RF	I-b-2	mid	W_2	—		

Rd: 5.0 cm; Md: 7.8; Wd: 0.3; H_1: 6.2 + x. 10R 5/6 red slip. Break: reddish brown. [Pointed base finished on the wheel]; a weak red (10R 5/4) rim band. Smoothed.

No. 15: Cup, concave sided, K-9046 [245], Fig. 2 *(H/VI-t/13–u/12, L4107)*.

RF	I-b-2	mid	W_2	—	ox	2

Rd: 5.0 cm; Md: 7.3; Wd: 0.24; H_1: 5.8 + x. 10R 5/6 red slip. Break: reddish brown. A weak red (10R 5/4) rim band. Smoothed.

No. 16: Carinated bowl with black rim band, 8909K, Fig. 2
(H/VI-h/17, L3005)

RP	I-e-2	f–mid	W_2	Bd. W	ox	2–3

Rd: 22.0 cm; Fd: 4.0; Wd: 0.25; H_1: 8.5; H_4: 1.2. 10R 5/6 red slip. Break: light brown. A black rim band inside and outside. Polished, medium lustre.

No. 17: Ring-based bowl, K-9002 [1], Fig. 2
(H/VI-h/17, L3005)

RP	I-b-2	f	W_2	—	ox	2–3

Rd: 16.0 cm; Wd: 0.25; H_1: 2.7 + x. 10R 4/8 red slip; rim band: 10R 4/4 weak red. Break: brown. A rim band on the exterior and interior. Polished, high lustre.

No. 18: Small bowl, convex sided, K-9002 [7], Fig. 2
(H/VI-h/17, L3005)

RP	I-e-4	f	W_2	Bd. W	ox	2–3

Bd: 2.4 cm; Md: 7.5 + x; Wd: 0.4; H_1: 2.1 + x. 10R 5/8 red slip. Break: brown. Polished.

No. 19: Small bowl with interior lip, K-9002 [9], Fig. 2
(H/VI-h/17, L3005)

RF	I-e-4	mid	W_2	—	ox	2

Rd: 13.0 cm; Wd: 0.3; H_1: 1.2 + x. 10R 5/6 red slip. Break: brown. Smoothed.

No. 20: Pot with everted direct rim, K-9046 [169], Fig. 2
(H/VI-t/13–u/12, L4107)

RP	I-e-4	mid	W_2	—	ox	2–3

Rd: 9.0 cm; Wd: 0.5; H_1: 3.1 + x. 10R 4/6 red slip. Break: reddish brown core; brown oxidation zones. Ridges outside; the slip continues inside, forming a rim band. Polished, medium lustre.

No. 21: Small jar with everted direct rim, flat base, 8960A, Fig. 3 *(H/VI-x-/19, L4932)*

RF	I-e-4	mid	W_2	Bd. abg. g.	ox	2

Rd: 6.5–7.2 cm; Md: 9.0–9.5; Bd: 4.0; Wd: 0.5; H_1: 11.5; H_2: 5.5. 2.5YR 6/8 light red (ext.); 10R 5/8 red slip (int.). Break: light red. Made in parts; a weak red (10R 5/4) rim band; smoothed.

No. 22: Cup, **concave sided**, **black rim band**, K-9074 [150], Fig. 3
(H/VI- w/16, L4568)

RF	I-e-4	mid	W_2	—	ox	2

Rd: 5.5 cm; Wd: 0.3; H_1: 2.6 + x. 10R 5/6 red slip. Break: red. A 10R 4/3 weak red band. Smoothed.

No. 23: Ring-based bowl with black rim, 8961W, Fig. 3
(H/VI-x-/19, L4932)

RF	I-e-4	mid	W_2	Bd. W. gef.	ox	2

Rd: 18.5 cm; Bd: 4.5–4.6; Wd: 0.5; H_1: 6.5–7.0. 5YR 6/6 reddish yellow; 10R 6/6 light red slip. Break: 5YR 5/1 grey core; 6/4 light reddish brown oxidation zones. A 10R 4/1 dark reddish grey – 4/2 weak red rim band. Smoothed.

No. 24: Red-slipped burnished bowl K-9072 [17], Fig. 3
(H/III-w/16–17, L4602)

RP	I-e-4	mid	W_2	—	ox	2–3

Rd: 14.0 cm; Wd: 0.4; H_1: 1.7 + x. 10R 5/6 red burnished slip. Break: 2.5YR 5/6 red core; 7.5YR 6/4 light brown oxidation zones. Convex sided; horizontally burnished outside and inside.

No. 25: Red-slipped burnished bowl, K-6576 [75], Fig. 3 *(H/III-t/15, L1058)*

RP	I-e-4	f–mid	W_2	——	ox	2–3

Rd: 15.0 cm; Wd: 0.45; H_1: 1.8 + x. 10R 6/6 light red slip. Break: 5YR 6/4 light reddish brown. Straight sided; horizontally burnished outside and inside.

No. 26: Jar with everted direct rim, 8960B, Fig. 3 *(H/VI-x-/19, L4932)*

RF	I-b-2	mid	W_2	Bd. gesp. g.	ox	2

Rd: 8.2 cm; Md: 11.0–11.4; Wd: 0.6; H_1: 19.3; H_2: 7.0. 2.5YR 6/4 light reddish brown–5/4 reddish brown (exterior); 10R 5/6 red slip (interior). Break: 2.5YR 6/4 light reddish brown. Smoothed.

No. 27: Large jar with everted direct rim, 9060, Fig. 3 *(H/VI-x-/19, L4932)*

TG	I-e-4	mid	W_2	Bd. gesp. g.	ox	2

Rd: 18.0 cm; Md: ca. 25.0; Wd: 1.0; H_1: 49.5; H_2: 18.0–19.0. 5YR 6/4 light reddish brown. Break: 5YR 4/1 dark grey core; 5/6 yellowish red oxidation zones. Smoothed.

No. 28: White-washed jar with trimmed rim, K-9055 [1], Fig. 3 *(H/VI-u/12, L4107A)*

TG	I-e-4	mid	W_2	——	ox	2

Rd: 10.0 cm; Wd: 0.35–0.4; H_1: 3.6+x. 10YR 8/2–7/3 very pale brown. Break: 2.5YR 4/6 red. Smoothed.

No. 29: White-washed jar with rounded rim, 8955L_{1-2}, Fig. 3 *(H/VI-w/15, L4576, str. c)*[35]

TG	I-e-4	mid	W_2	Bd. Ha	ox	2

Rd: 10.8–11.8 cm; Md: c. 22.0; Wd: 0.4; H_1: 24.5 + 6.0 + x; H_3: 6.5. 5YR 7/6 reddish yellow; 5YR 8/2 pinkish white wash self slip. Break: 5YR 5/1 grey core; 5YR 6/4 light reddish brown. Fingerprints on the base inside. Smoothed.

No. 30: Jug, 8962W_{1-2}, Fig. 4 *(H/VI-x-/19)*

RP	Nilton	f–mid	W_2	Bd. W.	ox	2–3

RP Nilton f.–mid. W_2 Bd. W. ox 2–3
Rd: 7.4–7.5 cm; Nd: 2.8; Md: 16.0; Bd: 6.0; Wd: 0.4; H_1: 22.0. 2.5YR 5/6 red to 5YR 8/3 pink slip. Break: 5YR 5/6 yellowish red. Fine quartz (abundant); straw (very few). Double strap shoulder handle. Horizontally burnished on the shoulder, vertically on the body; medium lustre.

No. 31: Jar, 8960E, Fig. 4. *(H/VI-x-/19, L4932)*

RF	I-b-2	mid	W_2	Bd. abg. g.	ox	2

Rd: 12.5–13.0 cm; Md: 19.0; Bd: 3.0; Wd: 0.5; H_1: 21.0–23.0; H_2: 10.8–12.0. 2.5YR 6/8 light red; 10R 5/6 red slip. Break: light red. Fingerprints inside on the base; smoothed.

No. 32: Storage jar, 8966, Fig. 4 *(H/VI-w/16, L4673)*

No. 33: Carinated bowl, K-6576 [72], Fig. 4 *(H/III-t/15, L1058)*

P	I-b-2	f	W_2	——	ox	2–3

Md: 14.5 + x cm; Wd: 0.3; H_1: 3.0 + x. 2.5YR 6/6 light red. Break: 5YR 6/6 reddish yellow. Vertically burnished outside; horizontally above the carination and vertically below it on the interior.

No. 34: Large bowl, K-9032 [5], Fig. 4 *(H/VI-h/18, L3004V)*

TGRP	I-e-4		W_2	——		

Rd: 36.0 cm; Wd: 1.0; H_1: 3.5 + x. 10R 4/4 light red slip inside. Horizontally burnished on the rim inside, and vertically on the body making a cross. Smoothed outside, burnished inside.

No. 35: Radial burnished bowl, K-6531 [60], Fig. 4 *(H/III-s/16, L999A+B)*

TGP	II-f	f	W_2	Bd. W	ox	3

Bd: 5.0 cm; Wd: 0.85; H_1: 3.0 + x. 2.5Y 7/3 pale yellow out.; 5Y 7/2 light grey ins. Break: 10YR 7/2 light grey–6/2 light brownish grey. Radial burnished inside, smoothed outside.

No. 36: Dipper juglet, 8951Q, Fig. 4 *(H/VI-w/17, L4602)*

RP	I-e-4	mid	W_2	Bd. W	ox	2–3

Md: 8.0 cm; Wd: 0.5; H_1: 13.0 + x; H_2: 6.5; Hd: 1.8 x 1.0. 5YR 6/4 light reddish brown; slip: 10R 4/3 light red–4/6 red. Break: 5YR 6/4 light reddish brown. Burnished.

No. 37: Dipper juglet, 8961Z, Fig. 4 *(H/VI-x-/19, L4932)*

RP	I-b-2	mid	W_2	Bd. gesp. g.	ox	2–3

Rd: 4.0–4.3 cm; Md: ca. 8.2; Wd: 0.55; H_1: 20.2; H_2: 8.0–8.5; Hd: 1.45 x 1.2. 5YR 6/4 light reddish brown; 10R 5/8 red slip. Break: 5YR 5/2 grey inside; 2.5YR 5/6 red outside. Burnished.

No. 38: Egyptian Bichrome jug, 8962X_{1-3}, Fig. 5 *(H/VI-x-/19, L4932)*

PBI	I-b-1	f–mid	W_2	——	ox	2–3

Md: ca. 19.0 cm; Wd: 0.5; H_1: 12.5 + x. 5YR 6/6 reddish yellow. Break: 5YR 6/6 reddish yellow. A horizontally painted band black-red-black (5YR 5/1 grey and 10R 5/6 red) at the major diameter; oblique patterns of black cross-lines framed by a black-red-black band from the neck-base to the maximum diameter of the jug. Polished, low lustre.

No. 39, Egyptian imitation of a WP juglet, K-9301

No. 40, Egyptian imitation of a PWS milk bowl, 9021A, Fig. 5

No. 41: Classic Kerma cooking pot, 8964J_{1-2}, Fig. 5 *((1) H/VI-w-x/18, L5018, str. e/2 (= Phase D/2), (2) H/VI-x/18, L4944, str. e/1)*

TGSP	I-e-1	mid–r	Ha_1	Bd. Ha	re	2

[35] This jar was included here because it has a complete shape, but only the rim is represented. Rims, bases and body sherds from other jars of the same shape come from stratum d contexts.

Rd: 22.0 cm; Md: 23.0; Wd: 0.9; H_1: 13.7; H_2: 9.6. 5YR 5/4 reddish brown–7.5YR 6/3 light brown out.; 7.5YR 2.5/1 black inside. Break: 7.5YR 2.5/1 black. Quartz (>0.25 mm: abundant) and mica (few); sand and straw tempered. Striations marks inside. Incised oblique lines, crossing in the base. Polished on the rim outside; horizontally polished inside.

No. 42: Egyptian imitation of a Kerma cooking pot, 8930C, Fig. 5 *(H/VI-h/18, L3004V)*

TG	I-e-2	mid	W_2	——	re	2

Md: 16.0 + x cm.; Wd: 0.5; H_1: 4.8 + x. 2.5R 4/1 dark grey. Break: 2.5Y 3/1 very dark grey core; 2.5Y 4/1 dark grey exterior zones. Quartz (>0.25 mm,>0.50 and >2: abundant), feldspar and mica; sand tempered. Obliquely incised-line pattern; smoothed.

No. 43: Cypriote w-m-BI juglet, 8963A$_{1-5}$, Fig. 5 *(H/VI-x-/19, L4932)*

BPBI	VI	f	W_2	Bd. W	ox	3

Rd: 7.5 cm; Md: 8.5; Bd: 3.0; Wd: 0.35; H_1: 8.4 + x; H_2: 5.0. 10YR 7/2 light grey–6/2 light brownish grey; 10YR 7/3 very pale brown slip. Break: 10YR 6/3 pale brown. Quartz (>0.25 mm, >0.50: abundant; <2: few), red grit. A vertical pattern of three painted bands and a pattern of four cross-lines, on the body, and a horizontal band on the neck and on the rim band (black-red) (5YR 3/1 very dark grey and 10R 5/4 light red). Polished, low lustre.

No. 44: WS I milk bowl, BI effect, 8967B, Fig. 5 *(H/VI-x/19, L4840)*

PBI	VI	mid	Ha_1	——	mi	3

Wd: 0.25 cm. 7.5YR 8/2 pinkish white slip. Break: 7.5R 5/3 brown. Quartz (>0.25 mm, >0.50: abundant; >2: few), crushed limestone particles (>0.25: medium). A lozenge pattern and two parallel lines, 7.5YR 5/3 brown and 6/6 reddish yellow. Pebble burnished, low lustre.

No. 45: Levantine imitation of a PWS milk bowl, 9071D, Fig. 5 *(H/III-o/13, L2033)*

PMO	~VI.22	f–mid	W_2	——	ox	3

Rd: 16.0 cm; Wd: 0.4; H_1: 5.0 + x; Hd: 1.0 x 0.9. 2.5YR 6/6 light red. Break: 2.5YR 5/6 red. Horizontal handle, nearly round, single strap; a vertical rope lattice pattern below the handle, on the body; a horizontal wavy line framed by a band below the rim; some decoration on the handle (2.5YR 2.5/1 reddish black and 5/1 reddish grey). Pebble burnished, low lustre.

No. 46: Levantine imitation of a PWS milk bowl, 9071J, Fig.6 *(H/III-o/13, L2034)*

PMO	VI.24	f–mid	W	——	ox	3

Rd: 21.0 cm; Wd: 0.6; H_1: 3.1 + x. 5YR 7/4 pink–7/6 reddish yellow. Break: 5YR 5/3 reddish brown.
Painted (10R 4/6 red); outside: two horizontal parallel bands and an oblique cross-line pattern below; inside two horizontal wavy lines framed by a horizontal band below the rim.

No. 47: Levantine dipper juglet, K-6531 [61], Fig. 6 *(H/III-s/16, L999A+B)*

P	IV.13	f	W_2	Bd. W	re	3

Md: 4.5 + x cm; Wd: 0.5; H_1: 2.5 + x. 7.5YR 6/4 light brown to 7/3 pink. Break: 10YR 5/1 grey interior; 7.5YR 5/3 brown exterior. Vertically burnished, high lustre.

No. 48: BR I black-matte slipped juglet, 8963L, Fig. 6 *(H/VI-x/19, L4932)*

SP	VI	f	Ha_1	——	re	3

Wd: 0.2 cm; H_1: 2.5 + x. 5YR 6/6 yellowish red outside; 10YR 6/1 grey inside; 10YR 4/1 dark grey slip. Break: 10YR 5/1 grey. Quartz (>0.25mm, >0.50: abundant; >2: few), limestone particles (>0.25 mm: few). Fingerprints inside, near the base; burnished, matte lustre.

No. 49: Classic Kerma beaker, "black-topped tulip type, 8943M, Fig. 6 *(H/VI-t/13, L4107)*

[RP]SP	I-b-2	f	Ha_1	——	re	2–3

Rd: 16.0 cm; Wd: 0.30; H_1: 3.5 + x. 5YR 2.5/1 black. Break: 5YR 2.5/1 black. Quartz (>0.25 mm: very few) and mica (few); straw (very few) tempered. Polished, high lustre.

No. 50: Classic Kerma cooking pot with incised decoration, 8929M, Fig. 6 *(H/VI-h/18, L3003V)*

TGSP	I-e-3	mid–r	Ha_1	——	re	2–3

Rd: 16.0 cm; Wd: 0.55; H_1: 1.9 + x. 10YR 4/1 dark grey. Break: 10YR 3/1 very dark grey. Quartz (>0.25 mm,>0.50: abundant;<2: few), mica (abundant), small crushed limestone particles and rocks; sand and chaff tempered. A horizontally polished rim band (10YR 3/1 very dark grey) framed by a horizontally incised line; an obliquely incised-line pattern on the shoulder. Polished rim band.

No. 51: Classic Kerma cooking pot with impressed decoration, 8942F, Fig. 6 *(H/VI-u/12, L4107A)*

TG	I-e-3	mid–r	Ha_1	——	re	2

Wd: 0.65 cm; H_1: 5.0 + x. 7.5YR 4/6 light brown. Break: 5YR 4/6 yellowish red outside and 7.5YR 3/1 very dark grey inside. Quartz (>0.25 mm: medium;>2: few), mica (abundant) and crushed limestone (few); sand and coarse straw (few) tempered. A diagonally impressed pattern made with a toothed wheel.

Abbreviations

Bd: base diameter. *Bd.abg.g.:* base cut off the wheel with a string or a knife to form a flat base and then smoothed. *Bd.gesp.:* base cut off the wheel and scraped smooth. *Bd.Ha:* hand-made base. *Bd.W.gef.:* base finished on the wheel. *Bd.W.:* base made on the wheel. *BPBI:* brown polished bichrome. *f–mid:* fine to medium quality of the matrix. *f:* fine matrix. *H_1:* total height of the vessel. *H_2:* height from the base to the major point. *H_3:* height of the neck. *Ha_1:* hand-made vessel. *I-b-1:* Nile B_1. *I-b-2:* Nile B_2. *I-e-1:* Nile E_1. *I-e-2:* Nile E_2. *I-e-3:* Nile E with chaff. *I-e-4:* Nile B_2 sandy. *I-e-5:* Nile C_1 sandy. *II-F:* marl F. *IV:* Levantine fabric. *Md:* diameter at the major point. *mid.:* fired in a medium atmosphere. *mid:* medium quality of the matrix. *mid–r:* medium to coarse quality of the matrix. *Nd:* neck diameter. *ox:* fired in an oxidizing atmosphere *P:* polished. *PBI:* polished bichrome. *PMO:* polished monochrome. *Rd:* rim diameter. *re:* fired in controlled firing conditions. *RF:* red slipped surface. *RP:* red-slipped polished

or burnished. *RPSP:* red-slipped polished exterior, black polished interior. *SP:* black polished. *TG:* uncoated surface. *TGRP:* uncoated exterior, red-slipped polished interior. *TGSP:* uncoated exterior, black polished interior. *VI:* Cypriote fabric. W_2*:* tall-stemmed wheel slightly improved. *Wd:* wall thickness. *WF:* white-washed surface.

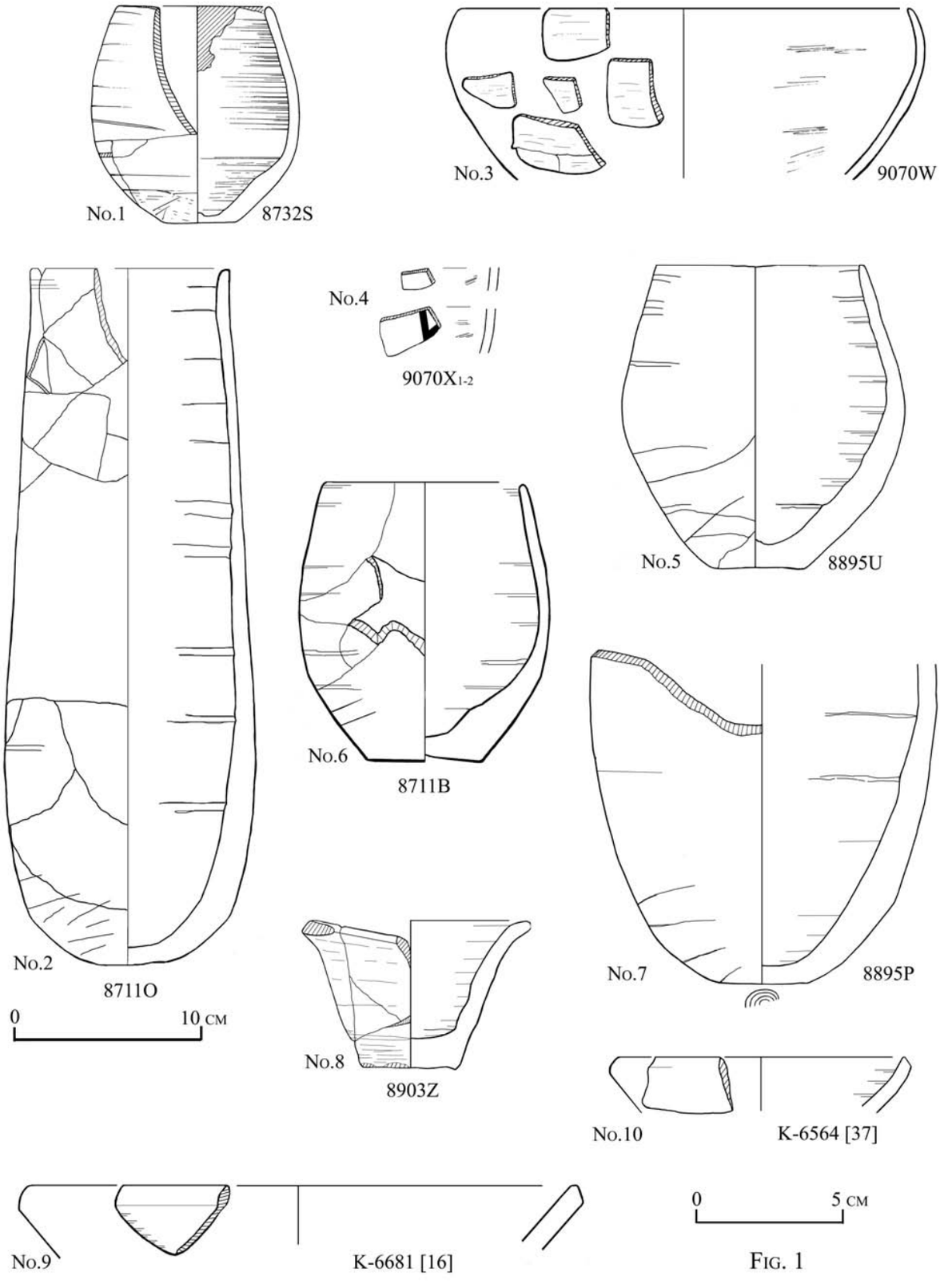


FIG. 1

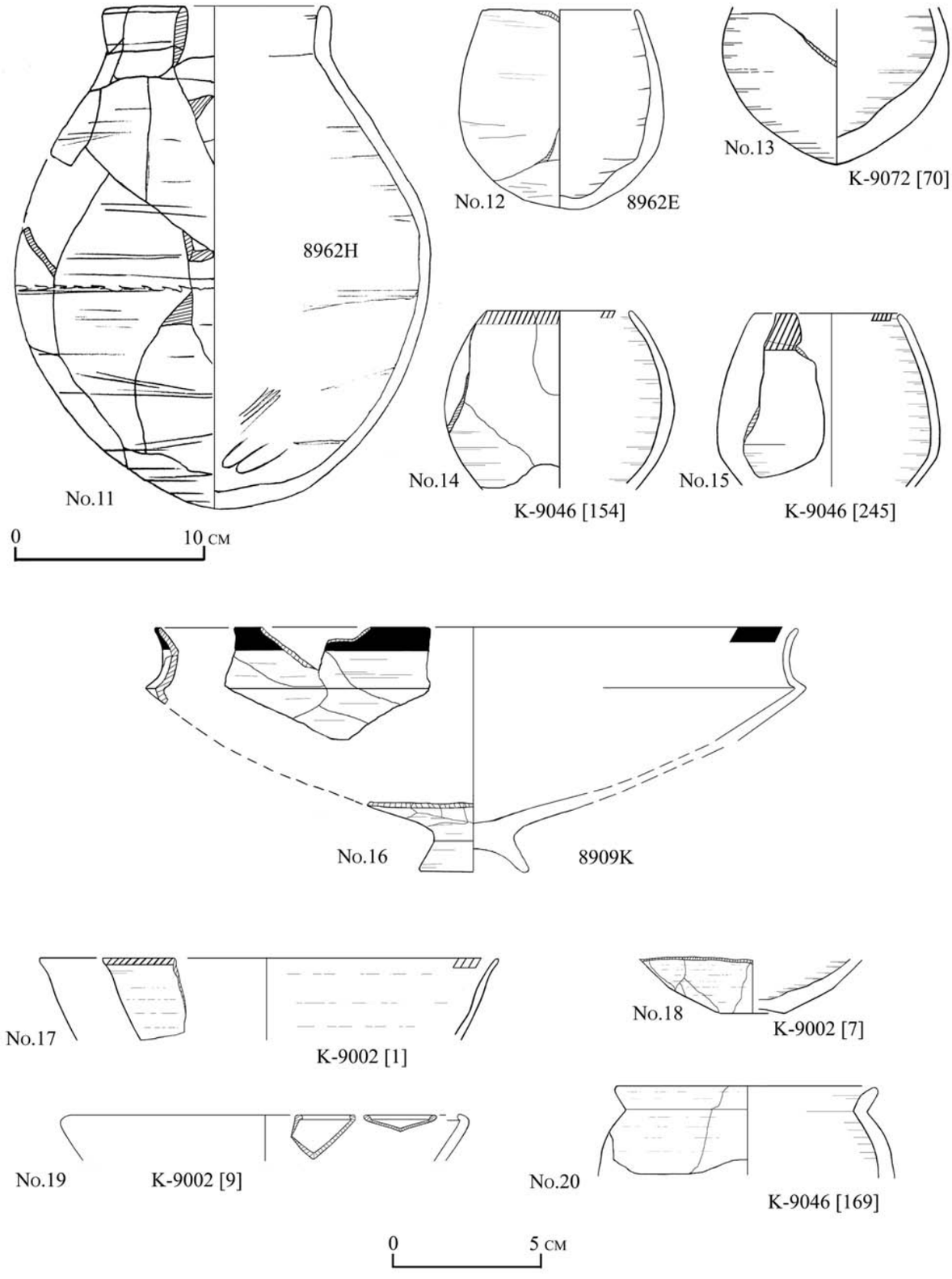

No.11
8962H
No.12
8962E
No.13
K-9072 [70]
No.14
K-9046 [154]
No.15
K-9046 [245]
0
10 см
No.16
8909K
No.17
K-9002 [1]
No.18
K-9002 [7]
No.19
K-9002 [9]
No.20
K-9046 [169]
0
5 см

Fig. 2

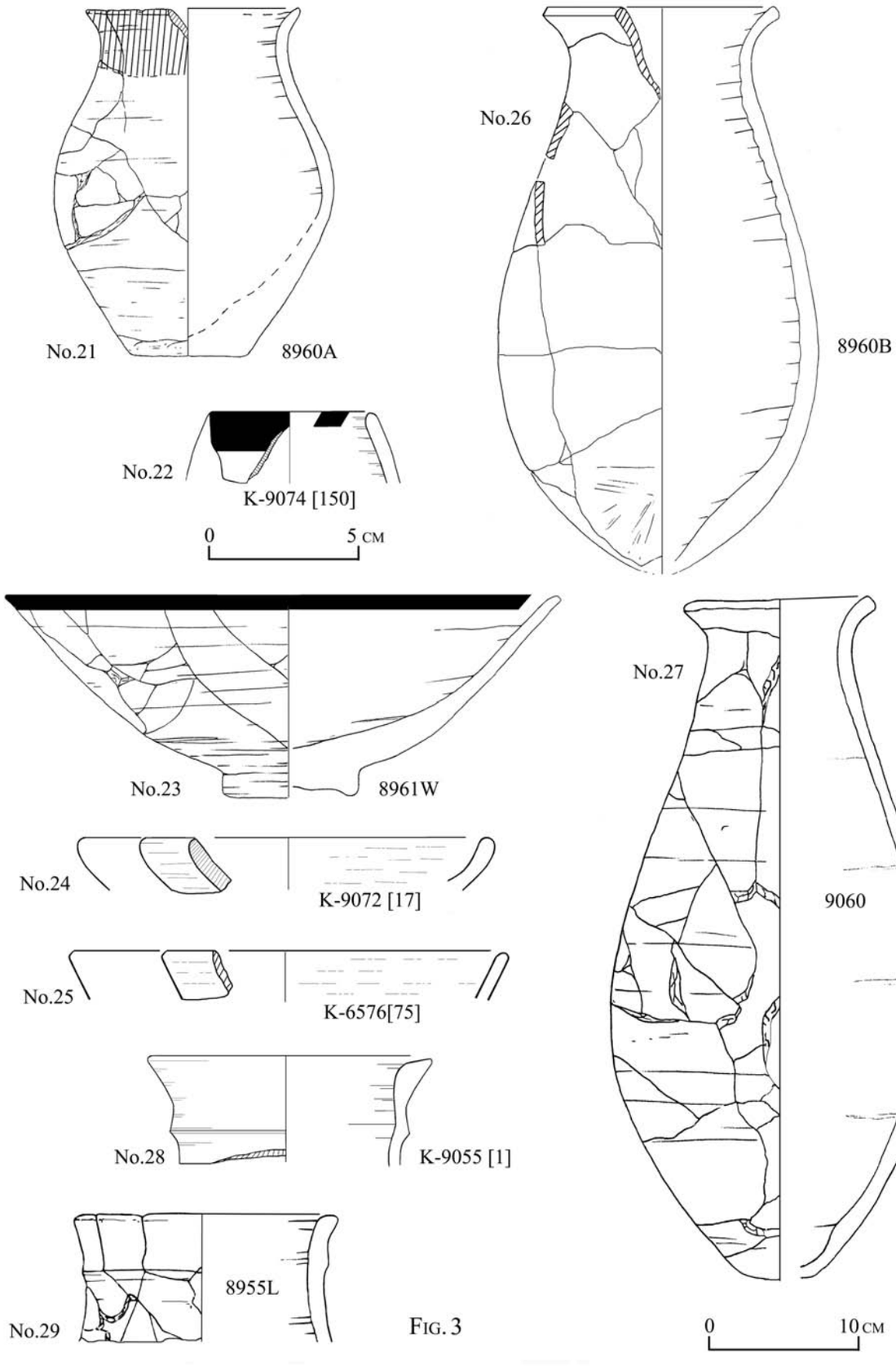

No.21
8960A
No.22
K-9074 [150]
0
5 CM
No.26
8960B
No.23
8961W
No.24
K-9072 [17]
No.25
K-6576[75]
No.28
K-9055 [1]
No.29
8955L
No.27
9060
0
10 CM

FIG. 3

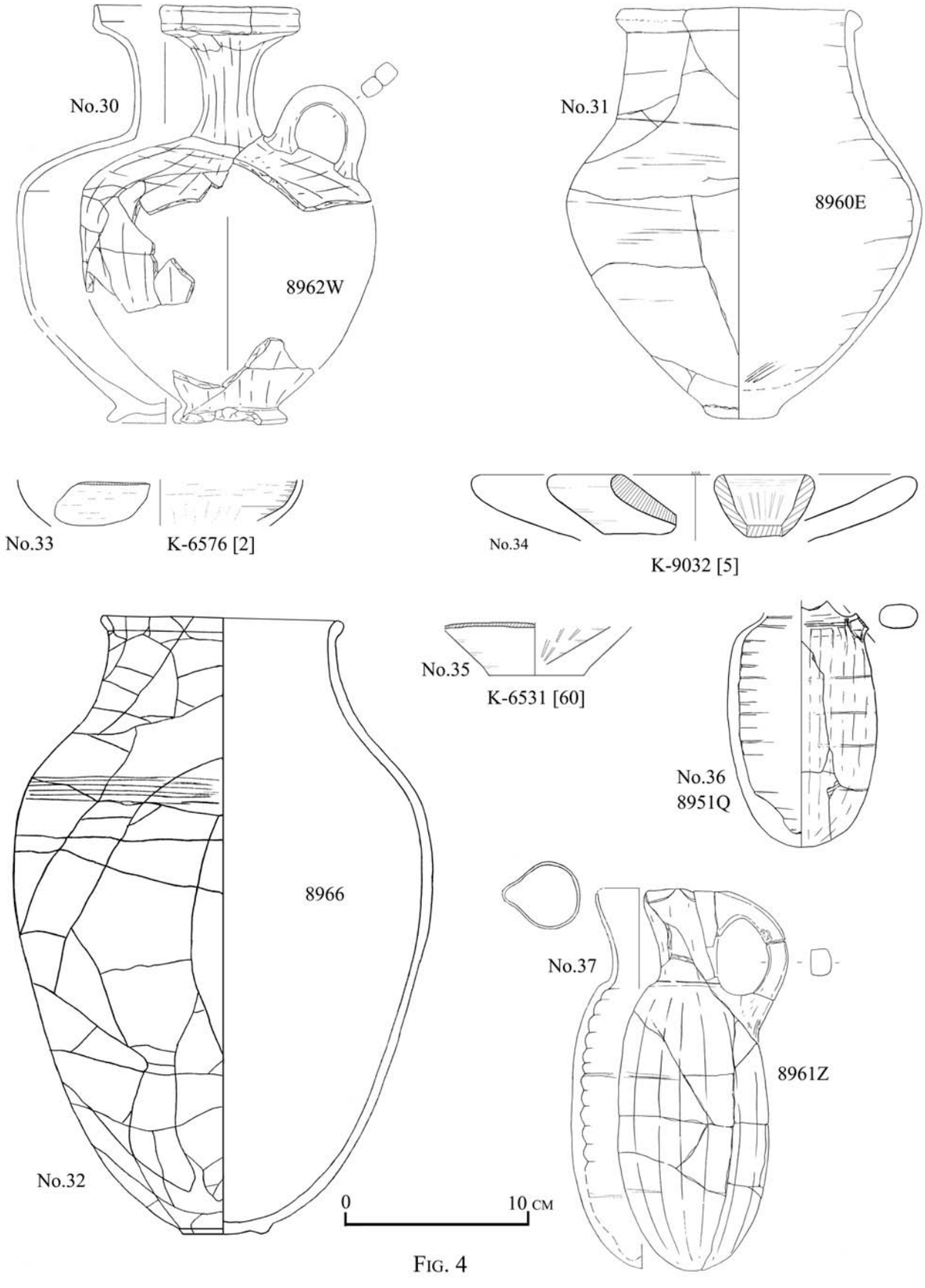

No.30
8962W
No.31
8960E
No.33
K-6576 [2]
No.34
K-9032 [5]
No.35
K-6531 [60]
No.36
8951Q
8966
No.37
8961Z
No.32
0
10 CM

FIG. 4

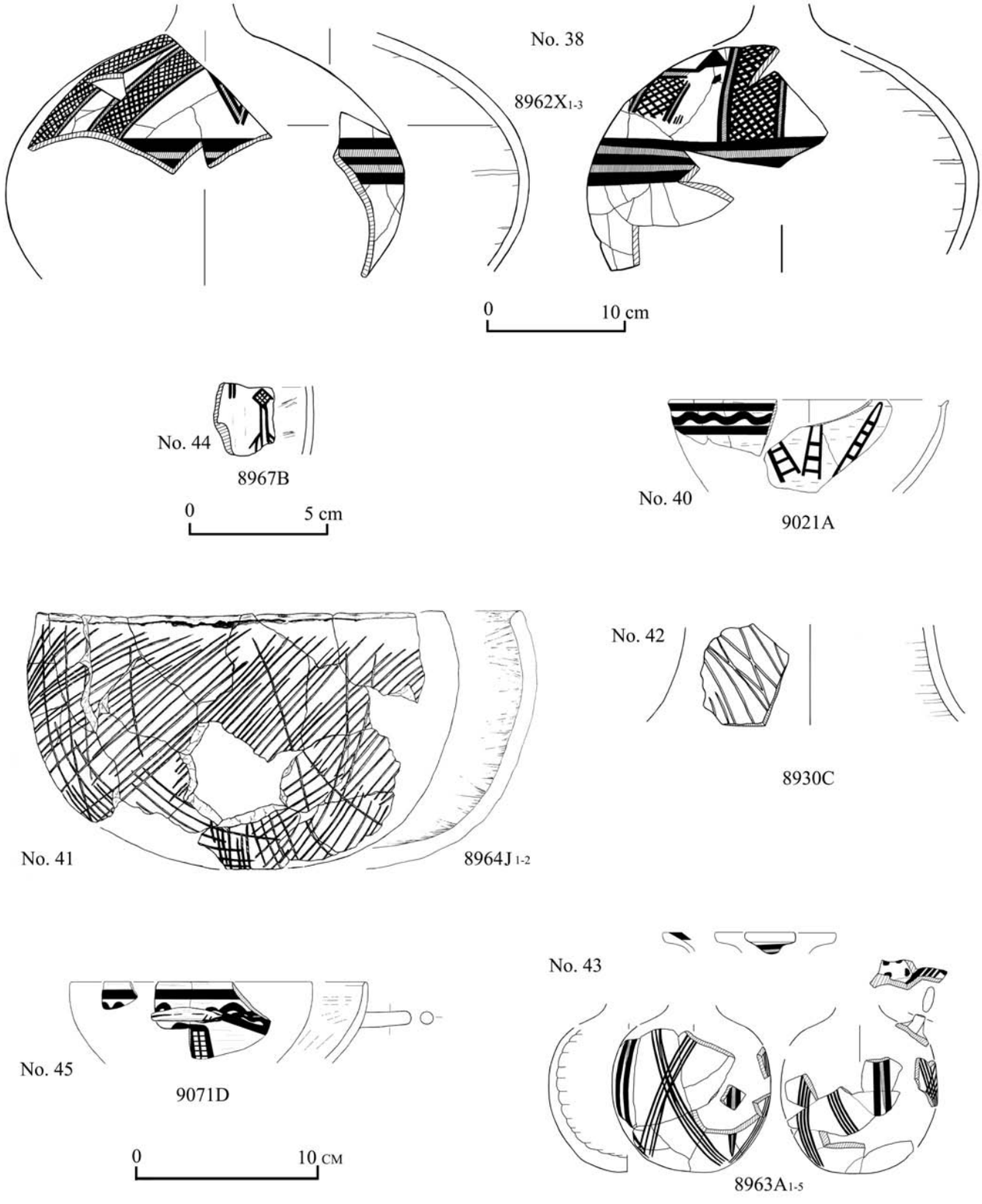

No. 38
8962X1-3
0
10 cm
No. 44
8967B
0
5 cm
No. 40
9021A
No. 42
8930C
No. 41
8964J1-2
No. 45
9071D
0
10 cm
No. 43
8963A1-5

FIG. 5

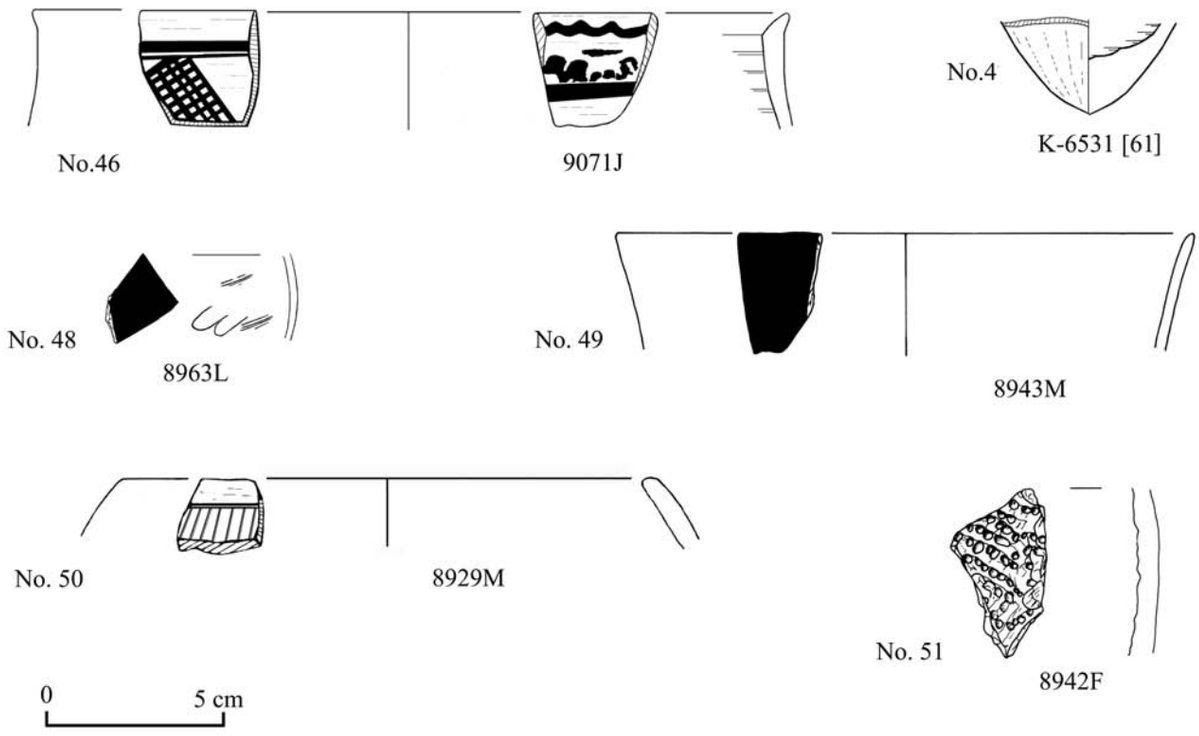


FIG. 6

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A Habitation Site at Karnak North Prior to the New Kingdom

Helen Jacquet-Gordon

The imposing remains of the temples situated at Karnak in Upper Egypt, vast and complex as they are, do not include any buildings still standing *in situ* which can be dated earlier than the reign of Thutmosis I at the beginning of the XVIIIth dynasty. That such earlier temples did however exist is of course copiously attested by the presence of architectural elements from their dismantled walls reused in later structures. There can be no doubt that an important temple complex existed here from at least as early as the beginning of the Middle Kingdom. But where, we may ask, are the remains of the town in whose midst it stood? Where did the people of that remote time whose lives were passed in the shadow of these imposing monuments live and work? We know the tombs of the more prosperous among them but we know practically nothing about their dwellings. Has the great city of Thebes which was to become the capital of all Egypt, and in which was situated this important religious center, irretrievably disappeared under the hotels and souvenir bazaars of the modern town of Luxor?

An unexpected light was thrown on this problem when the scattered remains of a habitation site covering an extensive area to the north of Karnak temple was brought to light during the excavations carried out at Karnak-North on behalf of the IFAO from 1968 until 1992. The development of the site could be followed from the early Middle Kingdom through the 2nd Intermediate Period to the beginning of the XVIIIth dynasty.

Karnak-North designates the area outside and to the north of the main Amon temple complex where the ruins of the so-called temple of Montu,[1] built by Amenhotep III, stand within a spacious temenos wall dating to a much later period. Our predecessors on the site, C. Robichon and his associates, who excavated there for the IFAO until 1956, were mainly interested in the Montu temple itself and in the reused blocks of earlier buildings with which it was in great part constructed.[2] However, the remains of a stone gateway attributed to Tuthmosis I[3] situated just outside and to the west of the Montu enclosure wall but at a somewhat lower level, led them to clear the area behind the gateway in order to ascertain to what it gave access. During one season's work, excavation in the axis of the gateway and north along the outer face of the Montu enclosure disclosed a densely constructed area the continuation of which could be traced eastwards undeneath and inside the northwest angle of the temenos wall. The discovery of a stela dating to the year 16 of Hatshepsut and Tuthmosis III erected in a small chapel just north of the gateway led the excavators to identifiy this agglomeration as being of New Kingdom date.[4]

It was recognized at the time that earlier remains lay below these New Kingdom structures, but no further investigation of them was undertaken.[5] It is difficult at present to distinguish between the earlier and the later building periods represented on the plans included in the publication of the excavations[6] (Fig. 1) as no levels are indicated on them. Moreover, the pottery from these different levels was not recorded, with the significant exception of one Tell el-Yahudieh juglet still preserved in the magazines at Karnak North. Nevertheless, one thing is clear. Differences of orientation visible in photographs of the excavated area support the hypothesis that at least two distinct periods are to be distinguished in the maze of walls there visible: an early XVIIIth dynasty level contemporaneous with the gateway and an earlier underlying one[7] (Fig. 2).

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[1] It has recently been shown that this temple was originally dedicated to Amon. It was only during the XXVth dynasty that it was rededicated to Montu (Gabolde and Rondot, *BSFE* 136 (1996) 27–41). For convenience sake we will continue to call it the Montu temple in order to differentiate it from the great Amon temple.

[2] P. Barguet et J. Leclant, *Karnak-Nord IV*, FIFAO 21, 1954.

[3] Recent investigation has shown it to be in fact the work of Hatshepsut. Cf. C. van Siclen, *GM* 80 (1984) 83.

[4] L. Christophe, *Karnak-Nord III*, FIFAO 23 (1951), 71–91.

[5] *Ibidem*, 81–82.

[6] *Ibidem*, p1. VI.

[7] *Ibidem*, pls. VII and VIII.

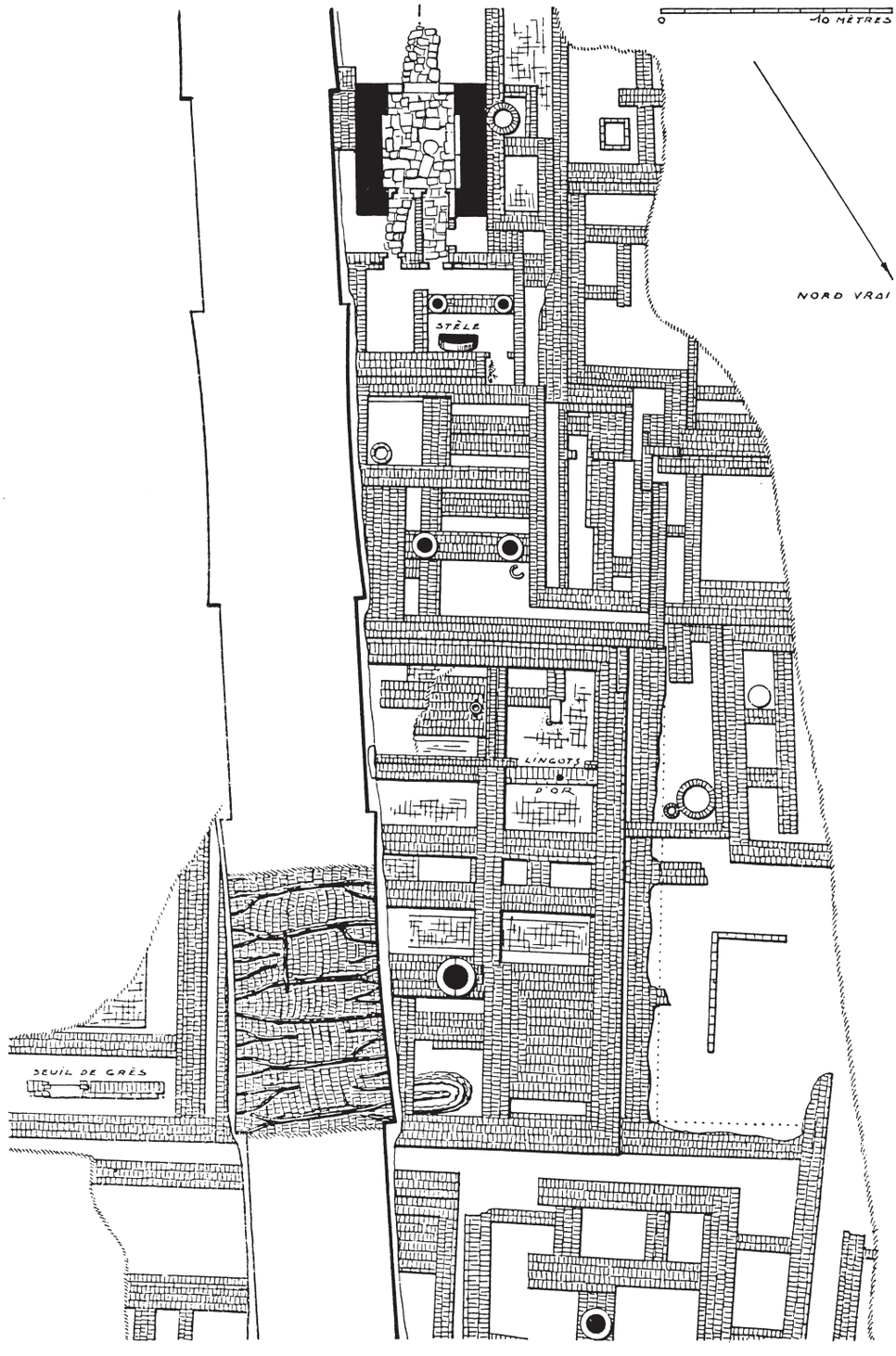


Fig. 1 Plan of the excavations north of the gateway of Hatshepsut.
Reproduction of pl. VI from: L. Christophe, *Karnak-Nord III*, FIFAO 23, IFAO, Cairo, 1951

That these earlier strata extended eastwards under the Montu temple itself was demonstrated in their subsequent work. The colonnades which had been added in front of the temple of Montu, first by Taharka and later in the Ptolemaic period, were no longer standing but their foundations, constructed entirely of reused blocks from earlier buildings, remained *in situ*. These blocks were almost all decorated and inscribed. In order to be able to document them in detail, the excavators completely dismantled



Fig. 2 Photograph of the excavations north of the gateway of Hatshepsut. Reproduction of pl. VII from: L. Christophe, *Karnak- Nord III*, FIFAO 23, IFAO, Cairo, 1951

the foundations. The removal of these structures disclosed the fact that the strata at the base of the foundation trenches contained the remains of walls, silos and small column bases, the eastward continuation of the earlier strata uncovered outside as well as inside the north-west angle of the temenos wall.

When excavation was resumed at Karnak-North for the IFAO by Jean Jacquet in 1968, a first objective was the recognition of the original ground level surrounding the Montu temple at the time of its construction and if possible the identification of its original enclosure wall. Two season's work made it possible to recognize the former, characterized by its white-washed surface, adjoining the temple on the east and to identify the latter parallel to the eastern side of the temple as far as its south-east corner. It was ascertained that this wall, like the temple itself, rested on earlier strata belonging to the inhabited area already recognized elsewhere.

It was then decided to investigate the traces of a rectanglular form, apparently indicating the emplacement of a building whose outlines, though invisible at ground level, could be traced on the surface in aerial photographs. It lay to the east of the present-day Montu enclosure, in the outer angle formed by the junction of the enclosure walls of the Amun and Montu complexes, but differed in orientation from both. By comparison with the orientations observed in the above mentioned excavations west of the Montu enclosure and underneath the colonnades of the temple we surmised that the building here adumbrated probably dated, like the upper stratum exposed there, to a period preceding the construction of Amenhotep III's temple. Subsequent excavation confirmed this hypothesis. The building, eventually identified as a treasury of Thutmosis I, was indeed older than the Montu temple of Amenhotep III.[8]

Apart from the intrinsic interest of Thutmosis I's treasury building, unique of it's kind, its excavation helped us to a clearer interpretation of the earlier strata that lay around and underneath it and enabled us to attach their evolution to that of the early strata identified around and below the Montu complex. It became evident that the leveled area which had been prepared for the construction of the Treasury[9] had taken the form of a cut made into the northern face of the then existing mound of Karnak whose surface descended gradually towards the north. As a result of this configuration, the southern part of the ground level on which the Treasury was built lay more than a metre below the surface of the older strata visible in the section exposed on its eastern side. This means that more than a metre of the archaeological strata over an area of at least 50 metres square had been removed in order to prepare a flat surface for the building, thus destroying the early XVIIIth dynasty strata that had certainly existed here. Investigation of the subsoil lying directly below the Treasury floor at points where that floor had been destroyed in ancient times confirmed the fact that the pottery found in the strata directly below the Treasury was of Second Intermediate Period date.

[8] J. Jacquet, *Karnak-Nord V*, FIFAO 23 (1983) 3–4.

[9] Or possibly for an even earlier brick building whose remains could be identified just below the floor of the Treasury.

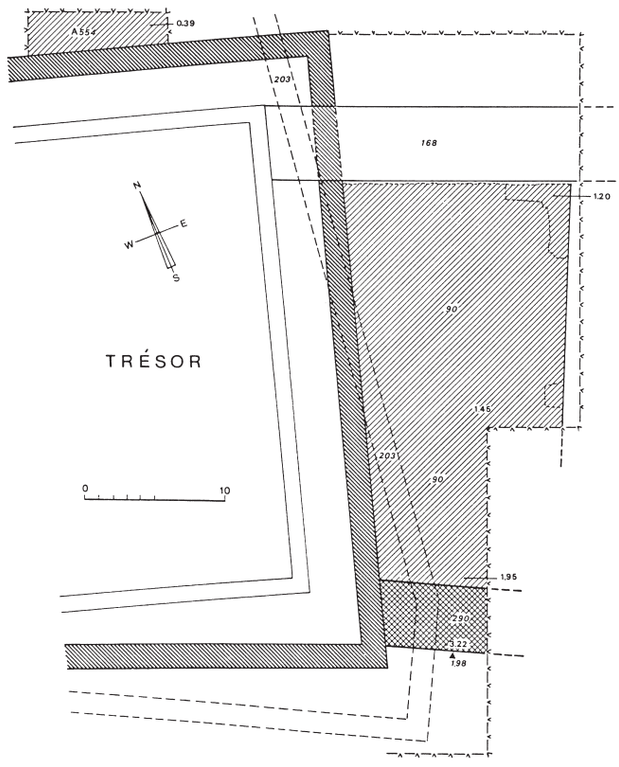


Fig. 3 Hatshepsut's brick terrace (nº 90) east of the temenos wall of the treasury of Thutmosis I. Reproduction of fig. 35 from: J. Jacquet, *Karnak-Nord IX*, FIFAO 44, IFAO, Cairo, 2001

When the excavation of the Treasury was completed, a new excavation was planned directly to the east of the Treasury enclosure wall in order to investigate what remained of these earlier strata where they were still preserved above ground and where they extended towards the west under the temenos wall and under the Treasury itself. The temenos wall had been built by Hatshepsut[10] after her father's early death and, approximately at the same time that the wall was built, a mud brick floor was laid down covering the entire area (about 15 metres in width) which remained between the wall and the cutting in the eastern face of the mound (Fig. 3). This floor effectively sealed off the earlier strata beneath and created a sort of terrace around the Treasury.

In order to reach the older strata it was necessary to remove Hatshepsut's floor. This was done at first by removing alternate squares of 5 metres square over the whole area. In the glimpses thus provided of the underlying stratum a new world was revealed in which the nature of the terrain had changed radically. Whereas in the strata above the floor the pottery had been typically XVIIIth dynasty or later, here we came upon an area where Second Intermediate Period pottery was omnipresent in a context of small brick enclosures, granaries and large storage jars imbedded



Fig. 4 Second Intermediate Period strata revealed under the terrace of Hatshepsut

[10] A foundation stone in her name was found in association with the north-east angle of the wall as well as a scarab ring in the name of Thutmosis III which had been inserted into one of the bricks.



Fig. 5 Handle of a Camares bowl (K.N. P.2491) from the excavations east of the treasury of Thutmosis I

in the ground, a work space of some sort located on the periphery of a settled area (Fig. 4). This level could be attached to the lower part of the early stratum visible in the face of the cutting. The upper part, destroyed by the leveling operations preliminary to the building of the Treasury, was here occupied by Hatshepsut's floor. It was in this upper level of the Second Intermediate Period stratum still visible in the cut just beyond the eastern limit of Hatshepsut's brick terrace that an unexpected potsherd came to light. It was the twisted handle of a Minoan Camares ware bowl, black polished with white splashes (Fig. 5). A second Minoan fragment was found during the excavation, but unfortunately in a later mixed context which can provide no indication of it's date.

Continuing to descend below Hatshepsut's floor, always in the same area between the temenos wall of the Treasury and the cutting on the east, an entirely new feature appeared. This was a wall about 1.5m wide the south-east angle of which was located slightly outside the south-east angle of the Treasury temenos wall but at a lower level. It could be followed in a northerly direction for more than fifty metres passing under the temenos of the Treasury and continuing beyond the limit of the excavation (Fig. 6). On the south side also it was traced westwards for at least forty metres without attaining its south-west corner. It was possible to date this installation to the thirteenth dynasty by the pottery discovered in a dump heap deposited outside the wall (Fig. 7). The contents of this dump consisted mainly of about sixty large Nile silt storage jars which must originally have contained some commodity such as grain (Fig. 8). These jars, stoppered and



Fig. 6 Area east of the treasury of Thutmosis I looking north. On the left: the outer stone wall of the treasury; in the center the brick temenos wall of the treasury; on the right: the XIIIth dynasty brick wall (n° 203) as it passes north under the temenos wall of the treasury

sealed, had apparently been emptied by breaking the necks of the jars which were then thrown away. On the other hand, the seal impressions on their stoppers had been carefully gathered together and placed in the bottom of one of the broken vessels before it was abandoned on the heap. Among other seal impressions included in the lot are several apparently in the form of a rectangular, round-topped stela containing the inscription *sḫm n pr-ḥḍ* "seal of the treasury", similar to specimens found in the Middle Kingdom Nubian fortresses.[11] It is probable that the inhabitants of the village enclosed by this wall were closely connected in one capacity or another with the temple administra-

[11] Cf. Geoffrey Martin, *Egyptian Administrative and Private-Name Seals*, Oxford 1971, pls. 43, 44.

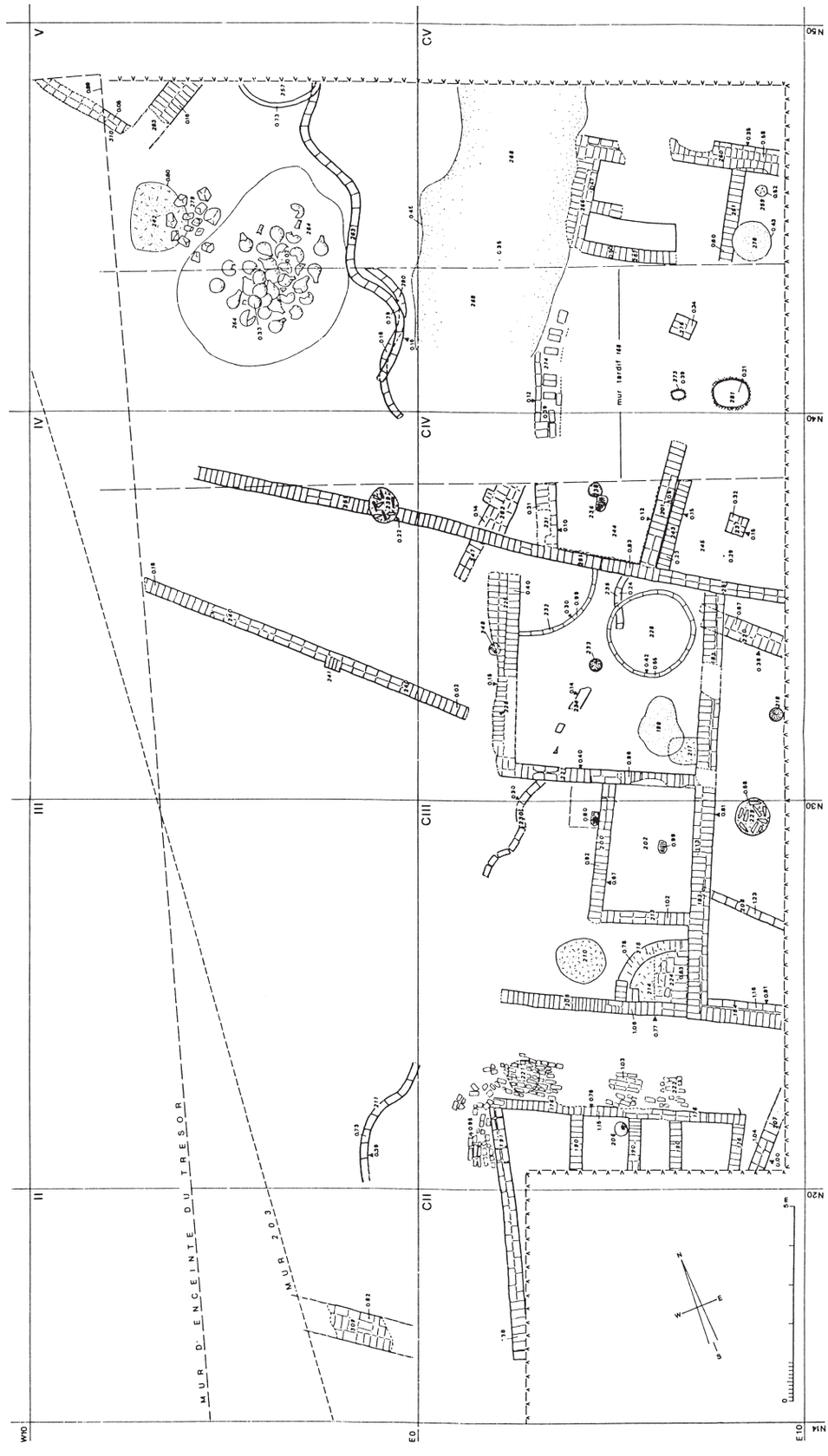


Fig. 7 In the upper left-hand corner: position of the pottery dump n° 264 east of the XIIIth dynasty brick wall (n° 203). Reproduction of pl. 3 from: J. Jacquet, *Karnak-Nord IX*, FIFAO 44, IFAO, Cairo, 2001



Fig. 8 Pottery dump nº 264

tion: servants, lay priests, artisans, etc. It is significant perhaps of their favored status that a considerable amount of imported Palestinian pottery, mainly amphorae, can be identified intermingled with the indigenous pottery found in these strata.

A sounding of about 4.5 meters square made on the east side of these installations and reaching down to the level of the present-day underground water table indicated that in this area no further inhabited strata were to be found below the XIIIth dynasty level. The contents of this sounding was almost wholly composed of mud-brick rubble, sand and ashes mixed with a surprising number of nodules of red ochre but with very few potsherds and gave the impression of being an intentional fill of a low-lying perhaps humid area.

Proof that still older strata did nevertheless exist elsewhere on the site was found farther to the west under the southern wing of Hatshepsut's temenos wall. Her wall, on this side, had been founded on the destroyed XIIIth dynasty wall mentioned above and on a second still older wall running parallel to it. When the southern wing of her wall in it's turn was demolished in Ramesside times, the remains of the older walls were again exposed to view. In the space of about two meters which separated these two walls there were to be seen the remains of a room belonging to a preceding level of construction. It had been cut through and partly demolished when the two walls were constructed. This room about 4m long, was found to be entirely filled with pottery apparently stored there intentionally. Although very much broken, it was possible to restore entirely or partially more than sixty vessels forming a homogeneous ensemble which could be dated to the early XIIth dynasty. The majority of these pieces was composed of large, thin walled, marl clay storage jars whose round bases were formed by hand in a mould to which the upper parts, turned on the wheel, were attached. They were of excellent workmanship and many were elaborately decorated with a variety of combed motives as well as with flattened beads and small cup-like objects attached around the neck. They had been standing along the wall of the room on heavy Nile silt cylindrical supports. A particularly attractive jar decorated in this style was further provided with breasts, these being embraced by small arms descending from the neck. The breasts are hollow so that when the jar was filled with milk the breasts also were filled. A large spout is attached between the arms. I know of only one other similar vessel but it lacks the spout.[12] None of the vessels in

[12] D. Downes, *The Excavations at Esna 1905–1906*, 1974, 48, Liverpool inventory number E6393.

this room seems to have been used as the interiors were clean without any trace of a possible content. Several smaller, very highly polished white jars with flat rims appear to imitate alabaster vessels.

Fine table wares in Nile silt: plates, hemispherical bowls and deeper bowls with slightly pointed bases as well as carinated bowls with an annular base, were also present. Several large vats provided with spouts apparently belong to the apparatus used in the preparation of beer.

A small number of objects was found together with the pottery in this room: a tall alabaster (calcite) bottle, and a small limestone unguent jar; a limestone plumb-bob; a small, round, black granite grinding stone with it's pebble grinder; a faience amulet and several seal impressions. To what kind of establishment did all these objects belong?

To sum up: the evidence here presented although very fragmentary, seems sufficient to lead to the conclusion that from the early Middle Kingdom a habitation site was situated to the north of Karnak temple. It appears to have evolved without drastic interruption until the end of the 2nd Intermediate period. With the growing importance of Thebes and the expansion of the temple of Amon at the beginning of the XVIIIth dynasty, the area of the village was transformed into a royal enclave including perhaps a building erected by Ahmose of which a door-jamb and other fragments were found during the excavation. It may also have included the palace of Thutmosis I known to have been situated north of the temple as well as his treasury, and the chapel of Hatshepsut in which the stela of the year 16 was found. The whole appears to have been enclosed in a temenos wall built by the Queen, since the southern wing of the Treasury enclosure wall, which we found to have continued westward beyond the limits of the dig, may possibly have stretched as far as the stone gateway recently identified as being the work of Hatshepsut. It is an intriguing hypothesis which needs investigation in the future.

Some Remarks on the Egyptian *kernoi*

Teodozja Rzeuska[*]

Introduction

A group of Egyptian vessels is presented in this article, which includes both open forms, such as bowls or plates, and closed forms such as jars. The characteristic distinguishing these vessels as a separate group is their decoration. All are decorated with slight plastic elements, which could be reminiscent of a closed or open flower blossom.[1] Due to this decoration, these vessels are sometimes termed *kernoi*.[2] To date, several dozen published fragments or whole vessels have been identified.[3] They originate from various contexts, settlements, necropolei and temples, which are dated to the period from the end of the Ist Intermediate Period to the late New Kingdom.

Material description

The small number of identified Egyptian *kernoi* could suggest that these are infrequently occurring vessels. It is more probable that they are not easily identified among ceramic materials, since affixed decorations most often break off from the vessel and are found separately. The limited amount of available material impedes investigation, particularly since a large portion comes from early 20th century excavations.[4] It is also not easy to apply homogenous criteria to ceramics from such varied contexts. Rather, one should take into account differences stemming from the specifics of the material such as settlement and necropolis.[5] All of the material published to date is from sites in Upper and Middle Egypt, which does not necessarily mean that it is characteristic of those areas only.[6] In the case of the jars we are unable to perform a wider analysis as we only have two vessels available. A fragment of a jar with miniatures applied under the rim was identified among the Middle Kingdom pottery of Elephantine in 2004. Among the open forms, however, three groups can be discerned, which roughly approximate a chronological sequence: Ist Intermediate Period, Middle Kingdom, 2nd Intermediate Period–Late New Kingdom. The material from each group is presented in order of sites, beginning with the northernmost ones.

Closed forms

1. Globular jar with four flower miniatures applied under the rim. (Fig. 1A)

Thrown on the wheel, made of Nile silt C, red washed (10 R 4/6), burnished. Height: 21,3 cm, max. body diameter: 16,9 cm. Band decoration, etched prior to firing, composed of horizontal rows of lines and half-circles.

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[1] So as not to force an interpretation of the function of these vessels, henceforth the decorations will be termed „miniatures".

[2] These vessels are sometimes referred to as ring vase cf. Bourriau 1981, 60. According to this definition *kernos* is a "vase de terrre cuite avec de petits réciptiens, qui ne communiquent pas etre eux, qui n'ont pas de fonction utilitaire apparente autre que de contenir des offrandes" cf.: Yon 1976, 174; Yon 1981, 133. Though all of the vessels presented in this text fulfil the first condition, we cannot be certain as to the second.

[3] This group should not include vessels with decoration in the form of small, buttons, "pockets" or filled tubes affixed to the vessel, such as those from El-Kab cf.: Hope 1987, 34, fig. 34; Qau and Badari, cf.: Brunton 1930, pl. XII, no. 9p, tomb 600. It is difficult to specify, whether the double vase from Dendera cf.: Petrie 1900, pl. XVIII, no. 155, is a *kernos*, which is likely a fragment of a greater whole. The situation is similar for vessels from Harageh, cf. Engelbach and Gunn 1923, pl. XL, nos.70 G and 70 G3. Both vessels lack "miniatures" decorations, but do possess several washes, which is highlighted in the picture. The author excluded the vessel found at Diospolis Parva, cf.: Petrie 1901 pl. XXXVI, no. 184. Though its shape is reminiscent of a *ring-kernos*, this vessel is in all probability an import. No descriptions were given.

[4] Not all vessels which are thought of as being Egyptian *kernoi* are published. In Thebes, the Metropolitan Museum discovered vessels described as „flower vase" in at least four contexts. Data from J. Bourriau cited in Kemp and Merrilles 1980, 218.

[5] Clear differences between material from the necropolis and settlement were shown in material from Deir el-Ballas; cf.: Bourriau 1986, 52.

[6] Vessels of this type have been identified in material from Karnak North, cf.: Jacquet-Gordon 1979, 29–30, seminar by H. Jacquet-Gordon at the 2nd EuroConferenc of "SCIEM 2000" in Vienna, 28 May to 1 June 2003, as well as in ceramics from Tell el-Daᶜaba, data from D. Aston. Vessels of this type were not found in Memphis, data from J. Bourriau. I am very grateful for both pieces of information.

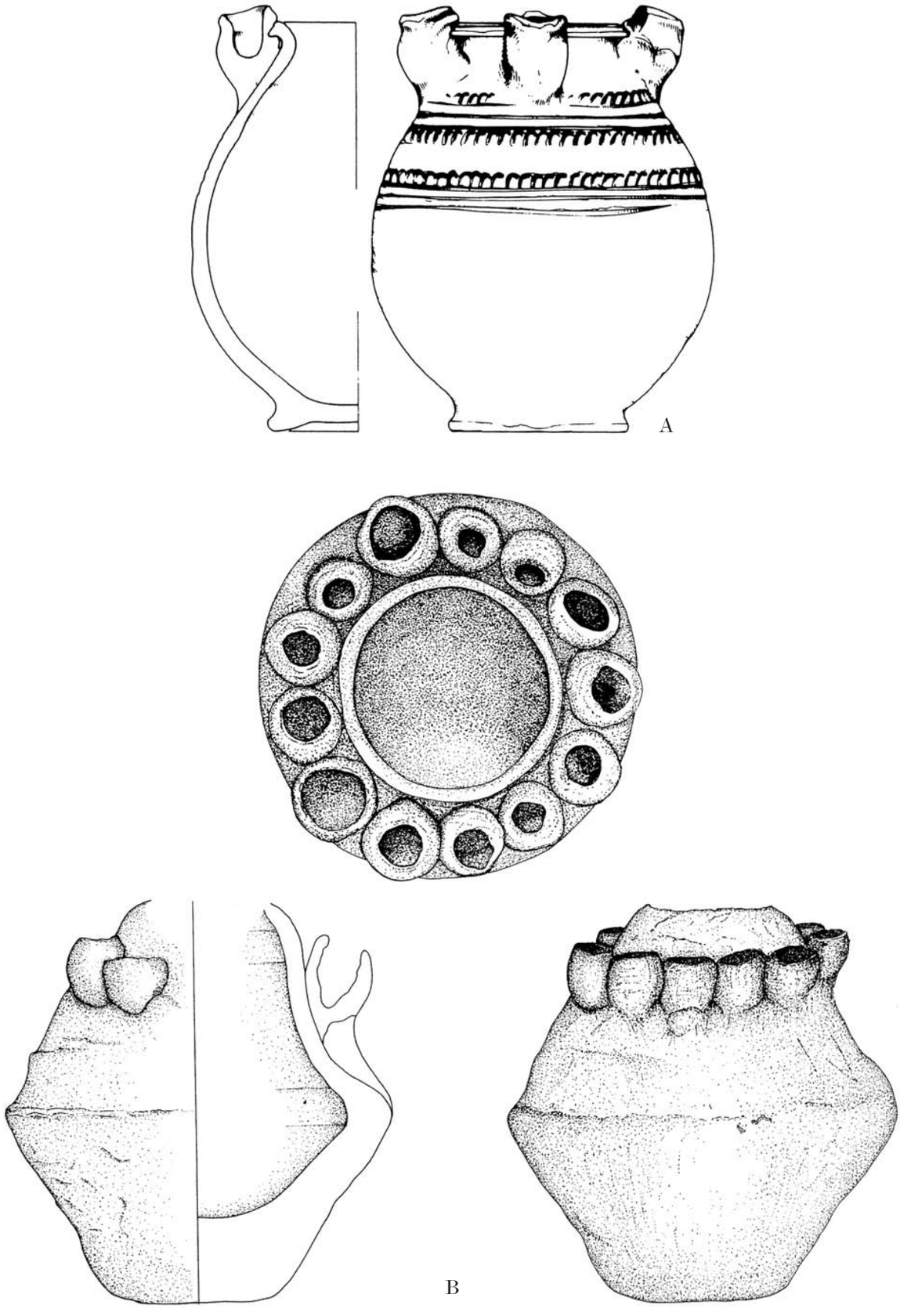


Fig. 1 Closed forms of *kernoi:* A) Hu or Beni Hassan; B) Edfu. (not to scale)

Provenance: indiscernible, likely from Hu[7] or Beni Hassan.[8] Stored at the Fitzwilliam Museum, Cambridge (no. E.28.1903).
Dating: 11th–12thDynasty
Literature: Eggebrecht 1975, 355, no. 346c; Bourriau 1981, 67, no.120; Bourriau 1988, 134.

2. Globular jar with several[9] miniatures applied to the middle of the body. (Fig. 1B)

Thrown on the wheel, made of Nile silt B2, surface greatly eroded, Height: 14 cm, max. diameter: 14 cm.
Provenance: Edfu, an exact location is not given. Stored at the National Museum in Warsaw, (no. MN 138615).
Dating: probably late Middle Kingdom.
Literature: Bruyère and Michalowski 1937, 123, no. 173 (KS 6), pl. XXXVIII, 4. Estimated as coptic (?).

Open forms

First group: Basin or platter with closed miniatures

3. Basin with four miniatures applied to the rim. Traces visible on the vessel's rim indicate that originally there were six. (Fig. 2A)

The publication lacks a description of the ware, technique or size.
Provenance: Sediment, necropolis, tomb no. 1546.
Dating: according to the excavators 9th–11th Dynasty. The Ist Intermediate Period dating is confirmed by the so-called Sedment-type bottles found along with the *kernos*. The former are characteristic of that period.[10]
Literature: Petrie and Brunton 1924, pl. XXX, 40.

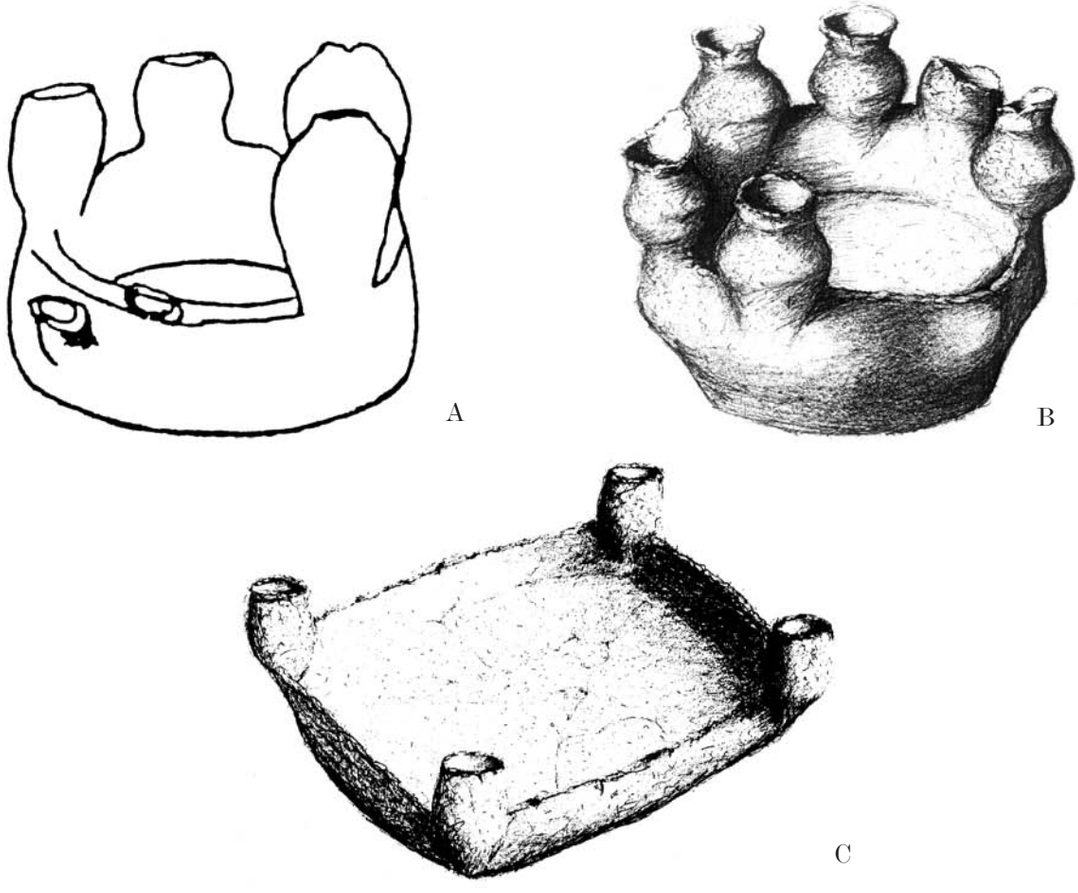


Fig. 2 *Kernoi* from the Ist Intermediate Period: A) Sedment; B) Beni Hasan; C) Medamud. (not to scale)

[7] Bourriau 1981, 67.
[8] Eggebrecht 1975, 355.
[9] 4 miniatures remain presently, but the clay residues around the vessel body suggest that there were many more.
[10] Seidelmayer 1990, 274– 284.

4. Basin with six miniatures applied to the rim, originally there were eight. (Fig. 2B)[11]

Thrown on the wheel, Nile silt C, red slipped (2.5 YR 5/8). Height: 13,8 cm, maxi. body diameter: 22,8 cm.
Provenance: Beni Hassan, tomb no. 178 (not published), stored at the City of Birmingham Museum and Art. Gallery (no. W 13423).
Dating: Ist Intermediate Period[12]
Literature: Garstang 1907, fig. 212, 195, Bourriau 1981, 60–61, no. 105.

5. Rectangular platter with four miniatures applied on each corner. (Fig. 2C)[13]

Measurements: 25 × 25 cm, made of brown clay, apparently Nile silt, Height of miniature: 6 cm.
Provenance: Medamud, the temple of Montu.
Dating: Ist Intermediate Period.
Literature: Robichon and Varille 1940, str. 5 and 10, fig. 10.

Second group: Bowls with closed miniatures

6. Bowl with several miniatures applied under the rim (Fig. 3A)

In the drawing, 9 miniatures are visible, and the trace of one more, but originally there were most likely around twenty. The publication lacks a description of the ware, the technique and vessel dimensions.
Provenance: Rifeh, an exact location is not given.
Dating: Middle Kingdom.
Literature: Petrie 1907, 226, pl. XIII, no. 168.

7. One miniature (Fig. 3B)

No ware description, Height ca: 6 cm. White painted.
Provenance: Heracleopolis Magna, necropolis.
Dating: late Ist Intermediate Period.[14]
Literature: Padró 1999, fig. 43, 68.

8. Bowl with several miniatures applied to the rim (Fig. 3C)

Made of Upper Egyptian Marl clay, probably Marl A3,[15] incised decoration in the form of horizontal and wavy lines; Height: 29,5 cm.
Provenance: Thebes, the valley between Shekh Abd el-Qurna and Qurnet Marai. From the debris covering the unfinished royal monument.
Dating: 11th–12th Dynasty.
Literature: Arnold 1991, 9–10, fig. 9.

9. Bowl with four miniatures applied on the rim (Fig. 3D)

Thrown on the wheel, made of Nile silt C, red-slipped, incised decoration in the form of horizontal and wavy lines. Height: 33 cm, diameter: 44 cm; miniatures Height: 6 cm.
Dating: Middle Kingdom
Provenance: Thoth Hill at Thebes, temple.
Literature: Vörös 1998, Pudleiner 2001, 241, fig. 2, pl. 38b.

10. Bowl with probably sixteen miniatures applied to the rim[16] (Fig. 3E)

Thrown on the wheel, made of Nile silt C, red slipped, incised decoration in the form of horizontal and wavy lines. Height: 14 cm, diameter: 30 cm; miniatures Height: 2,6 cm.
Dating: Middle Kingdom
Provenance: Thoth Hill at Thebes, temple.
Literature: Pudleiner 2001, 242, fig.3.
There were at least five other fragmentary *kernoi* found at the temple on Thoth Hill. [17]

11. Bowl with 19 miniatures applied to the rim (Fig. 3F)

Thrown on the wheel, made of medium fine clay (7,5 YR 6.4), most probably Nile silt B2, red slipped (10 R 5/6), incised decoration in the form of horizontal and wavy lines. Height: 21, 6 cm, diameter: 38, 2 cm; miniatures height: 2,6 cm.
Dating: Middle Kingdom.
Provenance: Qubbet el-Hawa, tomb no. 88. Sherds were found in the burial chamber as well in the shaft filling.
Literature: Edel 1980, 181, no. 305, Abb. 63.

12. Bowl with six miniatures applied to the rim (Fig. 3G)

Thrown on the wheel, made of Nile silt B2, red

[11] Drawing by K. Orzechowska on the basis of a photograph in Garstang 1907, fig. 212.
[12] Though the tomb is not published, this vessel is strongly reminiscent of Sedment vessels, which would indicate that the one from Beni Hasan is also dated to the Ist Intermediate Period.
[13] Drawing by K. Orzechowska on the basis of a photograph in Robichon and Varille 1940, fig. 10.
[14] Padró 1999, 44–49. Phase II, destruction of the necropolis.
[15] Arnold 1991, 9.
[16] Pudleiner 2001, 242. The real number of the miniatures is not known because the vessel was not preserved in its entirety.
[17] Pudleiner 2001, 242.

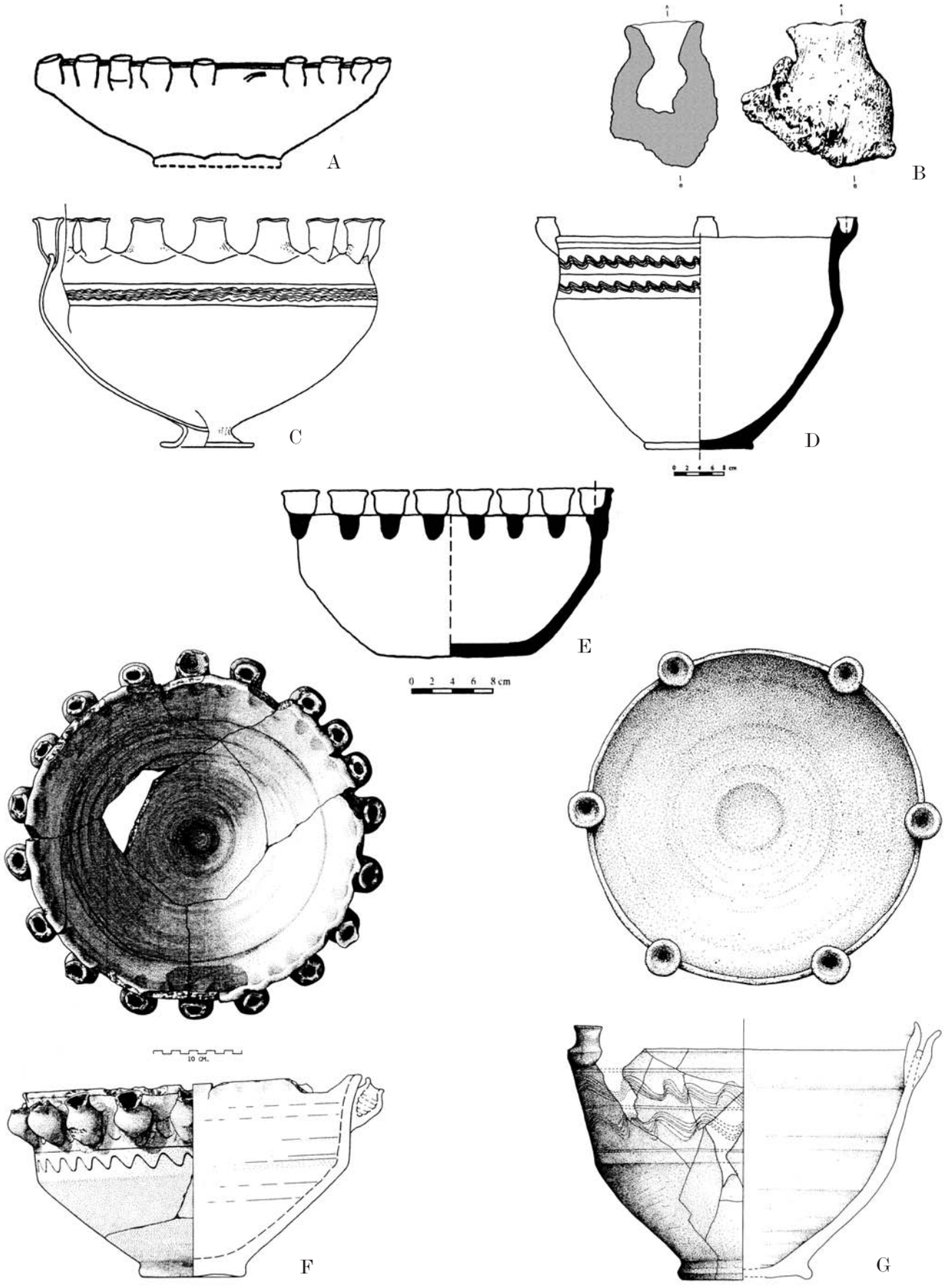


Fig. 3 *Kernoi* from the Middle Kingdom: A) Rifeh; B) Heracleopolis Magna; C) Thebes; D–E) Thebes – Thot Hill; F) Qubbet el-Hawa; G) Elephantine (not to scale)

slipped (2.5 YR 5/4 reddish brown), incised decoration in the form of horizontal and wavy lines. The miniatures bear traces of white paint. Height: 24, 2 cm, diameter: 31 cm; miniatures height: 5 cm. Restored from the sherds.
Provenance: settlement on the Elephantine Island, inventory number. MR 142 (archaeological context: 25601 O/d, from the house 87c[18]).
Dating: Middle Kingdom–middle of the 12th Dynasty.
Literature: not yet published.
Several parts of such vessels have been found among the pottery from the Middle Kingdom settlement on the Elephantine.[19]

Third group: Bowls with open miniatures

13. Part of bowls with several miniatures applied to the rim. (Fig. 4A)

Provenance: Diosppolis Parva
Dating: 12th(?) Dynasty.
Literature: Petrie 1901, pl. XXVI, no. 183.

14. Ring vase with four miniatures bottles, two miniatures and figurines of cat and cow (Fig. 4B).

Several miniature pots from the edges of larger pots. No description of the ware, or vessel dimensions.[20]
Provenance: Deir el-Bahari.
Dating: 18th Dynasty.
Literature: Hall 1913, 25, pl. XXXII, nos. 16, 24, pl. XXIII, nos.2 and 5; Pinch 1993, 317– 321, fig.1, nos.16, 24.

15. Fragments of several bowls with miniatures applied to the rim (Fig. 4C)

It is unknown whether they really were bowls on legs, as Nagel reconstructed them.[21] The miniatures are from 5 to 7 cm in diameter, and are painted: black and red outside, and red, black and blue inside.
Provenance: kom of Deir el-Medina.
Dating: New Kingdom, reigns Ramesses III/IV.[22]
Literature: Nagel 1938, 211– 212, pl. XVIII, Aston 1999, 108–109, figs. 6–7.

16. Upper part of bowl with at least 10 miniatures applied to the rim (Fig. 4D)

Diameter ca. 30 cm, red slipped.
Provenance: Esna, grave 222.
Dating: early 18th Dynasty.
Literature: Downes 1974, 28, 48, no. 167A.

17. Upper part of bowl with several miniatures applied to the rim

Thrown on the wheel, made of Nile silt C, red slipped.
Provenance: Edfu, necropolis, including tomb no. IV. Presently in the National Museum in Warsaw (no. MN 141143).
Dating: probably 2nd Intermediate Period–early New Kingdom
Literature: Michałowski and de Linage 1938, 126, no. 598, pl. XL, 2; Michałowski and Desroches 1950, 266, no. 770, pl. XLI, nos. 6 and 9.

At the Kôm in Edfu fragments of several bowls with miniatures were found. Here, the reconstruction of the best preserved vessel is presented.

Discussion

In the material presented above, one can see that each of the groups is characterised by a series of features particular to it. The oldest group of vessels is dated to the Ist Intermediate Period. A bowl of similar decoration dated to the late IV Dynasty was found in Giza. The author thanks Anna Hodzinska, working o the pottery from Giza (GPMG) for this information. The miniatures glued to the rim of those vessels are low and straight and have closed forms and are tall, relative to the height of the vessel. This is particularly visible in both bowls where they are the dominante feature of the object. If the miniatures are painted, they are red.

The second group are vessels of the Middle Kingdom. They are shaped like carinated bowls, one of the most characteristic and popular vessels of the period. The miniatures have not changed shape, they are still closed forms, but here they do not dominate the vessel with their size. They are glued either on the edge of the vessel, or just underneath it. The youngest members of this group seem to be the vessels from Heracleopolis Magna (Fig. 3b), the larger bowl from Thot Hill (Fig. 3d) and the bowl from Elephantine (Fig. 3g). Though from the first vessel we only have one of the miniatures, its shape and size is reminiscent of the miniatures from the other vessels. It is painted white,

[18] von Pilgrim 1996, 118.
[19] A publication of Middle Kingdom ceramics is in preparation by this author, who would like thank Dr. Cornelius von Pilgrim for entrusting with the publication of the pottery.
[20] Since the publication described miniatures with only very small fragments of the vessels' rims a graphic reconstruction was not possible.
[21] Nagel 1938, 210. It is freely admitted that there was no basis for such a reconstruction.
[22] Aston 1999, 108–109, figs. 6–7.

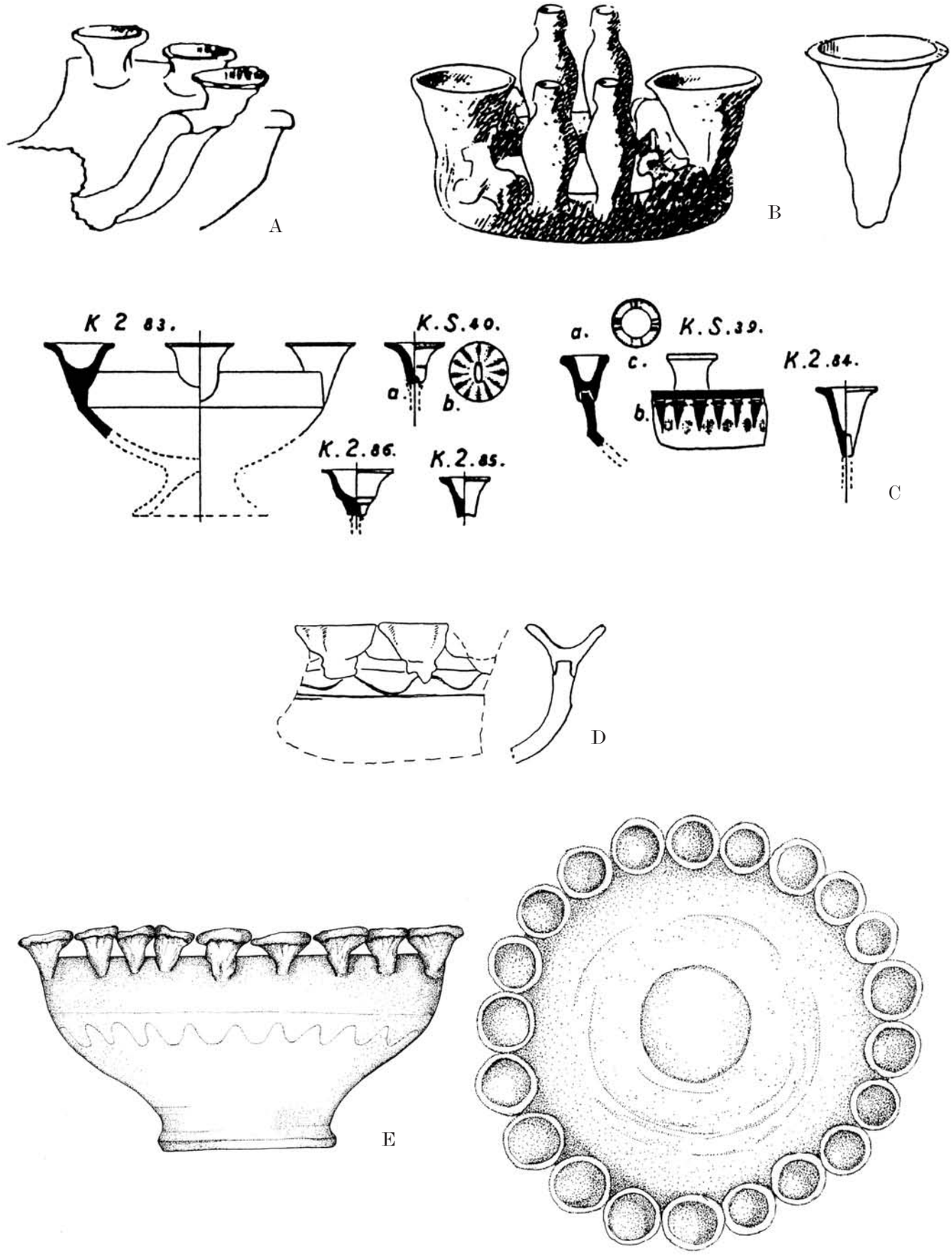


Fig. 4 *Kernoi* from the 2nd Intermediate Period to New Kingdom: A) Diospolis Parva; B) ring vase from Deir el-Bahari; C) Deir el-Medina; D) Esna; E) Edfu (not to scale)

like the miniatures from Elephantine. The similarity of vessels from Thot Hill and Elephantine is particularly striking. Both have decorations characteristic of the first half–middle of the 12th Dynasty, composed of horizontal lines separating numerous wavy lines. The latter were made with a tool resembling a comb. Both vessels have six miniatures each. The decoration on vessels from Qubbet el-Hawa (Fig. 3F) is somewhat different. It is composed of horizontal lines and a single wavy one. This form is characteristic of the Aswan region during the late Middle Kingdom, starting with the 18th Dynasty.[23] The evidence suggests that this bowl should be dated to this period. The *kernos* from Rifeh (Fig. 3A) is also from this period. Though the drawing in the publication is very schematic, it displays a characteristic enough shape to show that there really is no doubt that we are dealing with the late Middle Kingdom.[24] Concerning the small bowl from Thot Hill (Fig. 3E), this vessel lacks sufficient characteristic features which would allow the dating to be more precise than the Middle Kingdom. One could perhaps presume, since 12th Dynasty vessels have several miniatures, and those from the 18th Dynasty were decorated with many, that the small one with numerous miniatures on the rim also comes from the 18th dynasty period. For the sake of accuracy, however, it behoves to find a more convincing argument.

It is much more difficult to classify the vessel from the unfinished royal monument in Thebes West (Fig. 3C). It is the only pot from the entire city made of marl clay. The large number of miniatures would suggest a late Middle Kingdom dating. But both the material it is made of, likely Marl clay A3, the etched decoration on the body, a gentle carination placed relatively high up in relation to the body and a very characteristic ring base date this vessel to the early 12th Dynasty.

To sum up the *kernoi* from the Middle Kingdom, the vessels from its early phase have several miniatures, the preserved forms usually have six, and they are usually painted white.[25] They are glued to the edge of the vessel. During the later phase the number of miniatures increases. They are painted red and may be glued underneath the vessel rim.

The last group are the vessels from the 2nd Intermediate Period to the New Kingdom. During this time the miniatures took open forms, their shape reminiscent of an open flower blossom. It is not known how many were applied to a vessel, because no bowls were preserved in their entirety. Nevertheless, on the basis of the preserved fragments one ma surmise that during the 2nd Intermediate Period and early 18th Dynasty, a tradition of placing many red-painted miniatures on vessel rims was alive and well. The vessels from Deir el-Bahari (no drawing) are of this period, as are those from Esna (Fig. 4D), Edfu (Fig. 4E) and most likely Diospolis Parva (Fig. 4A). Other *kernoi* from Deir el-Medina are dated to the XIX dynasty. The actual appearance of the vessels is unknown. Nagel's proposal of a reconstruction, where the vessels had tall legs, is unfounded. The miniatures of this period are still open and decorated according to the "blue painted" convention of ceramic decoration, prevalent at the time. On the outside the lines are black and red, and inside red, black and blue.[26]

To complete the discussion of Egyptian *kernoi*, one should touch on the problem of the differences between Egyptian *kernoi*, as presented above, and the votive ring vase, such as the vessel from Deir el-Bahari (Fig. 4B). The differences between these types of vessels are immediately visible. All *kernoi* have miniatures of one size and shape. Votive ring vases are decorated not only with two types of miniatures, which suggest we are dealing with vessels of various functions, but also with animal figurines e.g. a cow or cat. Vessels of the votive ring vase type should arguably be classified as a separate sub-group of *kernoi*, not only due to their shape but more importantly their function. They are typical Egyptian votive vessels, also likely used for cult practices.[27]

The question of the exact nature of the miniatures decorating the edges and walls of the vessels is very interesting. In the literature, one can find several interpretations of the vessels. The decorative elements were interpreted as an offering bowl, in which fruit was offered, including first fruits and various liquids for the gods, as was done in ancient Greece.[28] The interpretations also include miniature vessels, which contained flowers during holidays,[29] or *nmst*

[23] Vessels with such decoration do not occur in XII dynasty ceramics from Elephantine. They only appear during the XIII dynasty.

[24] Carinated bowls of such proportions are rather typical of the late Middle Kingdom. They do not occur in XII dynasty materials from Elephantine.

[25] It is not excluded, that the miniatures of the large vessel from Thot Hill (Fig. 3.D) were also white painted. If sacrifices were burnt on them, then the white paint was likely already invisible.

[26] Nagel 1938, 211.

[27] Pinch 1993, 317–318.

[28] Garstang 1907, 195.

[29] Eggebrecht 1975, 355; Bourriau 1981, 67; Bourriau 1988, 134.

vessels which held a sacrifical water.[30] The first interpretation should be discarded. Though the name of the vessels has been borrowed from the Greek, the extrapolation of the function of Greek *kernoi* onto the similarly shaped Egyptian vessel cannot be convincingly established.

It seems unlikely that the miniatures could be a container for flowers, even short stemmed ones because the vessels are simply too shallow although it cannot be entirely excluded.

As for *nmst* vessels, this question is difficult to answer definitively. These vessels, although used in everyday life, are known primarily as an object used for ritual purification with water, such as during mummification, in the ritual of the opening of the mouth, or to make water sacrifices to the gods and the deceased.[31] They are somewhat reminiscent in shape of canopic jars. These are jugs with wide, almost vertical arms, a slightly narrowing body and a flat bottom. Sometimes *nmst* vessels were joined into groups, but in fours. Such quadruple vessels were used to make sacrifices of water to the four cardinal directions. In the case of *kernoi* it seems that we are not dealing with *nmst* vessels. None of the miniatures is reminiscent of that type of jug. They are not connected together, as would be done with quadruple *nmst*, but attached to the bowl's edge. Even if the *kernoi* were used in water sacrifice rituals, which cannot be excluded, there is no trace of such use on the vessels.

A different explanation must exist for the existence of the miniatures. Both Hall and Nagel pointed out that the vessels decorated with the miniatures are reminiscent of similar metal vessels decorated with flowers. Such vessels are known from representations in tombs at Thebes.[32] They thought that the clay *kernoi* are an imitation of the more expensive metal ones. The miniatures, however, would not be anything other than flowers which decorated the metal wares. It is worthwhile to consider whether the decoration was not inspired by bowls decorated with living flowers during various celebrations. If so, the proof must be sought among representations much older than the New Kingdom. The oldest *kernoi* originate from the Ist Intermediate Period (as mentioned before a similar fragment is dated to the IVth dynasty), so such inspirations should be sought in representations from the Old Kingdom period. In many tombs one finds representations showing bowls and jars decorated with flowers of various kinds (Fig. 5). Therefore it seems likely that bowls decorated in this fashion inspired potters to render the decorations in clay.

What could the Egyptian *kernoi* have been used for? It is very difficult to answer this question, as the vessels come from three culturally different contexts: necropolis, a settlement and a temple. It is very difficult to define them as vessels typical for any ot the contexts. One should take into consideration that they may have served a different purpose. Nagel made an astute observation that the vessels from the necropolis do not belong to the burial goods entombed with the deceased, because all the fragments were found in the kom.[33] The situation is similar in the case of the vessels from Edfu, which were found not in a tomb, but in the kom. The fragments of the vessel from Qubbet el-Hawa were dispersed. Having been found in several places, it is difficult to say whether it was actually a part of these settings. It is possible that such vessels were used in the necropolis in rites of the cult of the deceased.[34]

As for the *kernoi* from the temples, they bear clear signs of secondary burning and traces of ashes, such as those from Thoth Hill[35] and Deir el-Bahari[36] Such traces suggest whole-burnt offerings. The most difficult function to interpret is that of the *kernoi* from settlements. Due to their similarity to vessels from temples and necropolis they should be viewed as cult vessels. This matter, though, is not so clear-cut. We still have too little settlement material to even begin a discussion on the subject. Furthermore, in the Middle Kingdom settlement at Elephantine there are no clear architectural clues such as niches, that would suggest a private cult.[37] Our deliberations about the function of Egyptian *kernoi* will end for now with the hope that in the future we will find additional materials which will facilitate the continuation of our research and an

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[30] Bourriau 1981, 60.

[31] *LÄ* III, 315–316; Balcz 1933, 219–227.

[32] Hall 1913, 15; Nagel 1938, 212.

[33] Nagel 938, 212.

[34] Offerings were probably burnt in the vessel from Heracleopolis Magna, as the miniature bears signs of fire, cf.: Padró 1999, 68. The information presented is not precise. It is unknown whether the miniature became misshapen during kiln firing or whether they are secondary and therefore signs of something being burnt in it.

[35] Pudleiner 2001, 241.

[36] Hall 1913, 25.

[37] Data from Dr. C. von Pilgrim, for which the author is extremely grateful.

analysis in depth. It is also highly probable that it will modify the classification of the vessels presented above.

In the preceding article we present a sparse, but interesting group of Egyptian *kernoi*. This short treatise is merely the preliminary presentation of the material. Many problems were not touched on, such as the striking resemblance of Egyptian *kernoi* to vessels characteristic of other Mediterranean regions: Cyprus, Greece and the Middle East.[38] This is outside of the scope of this article, but also the author lacks the strength to undertake this topic.

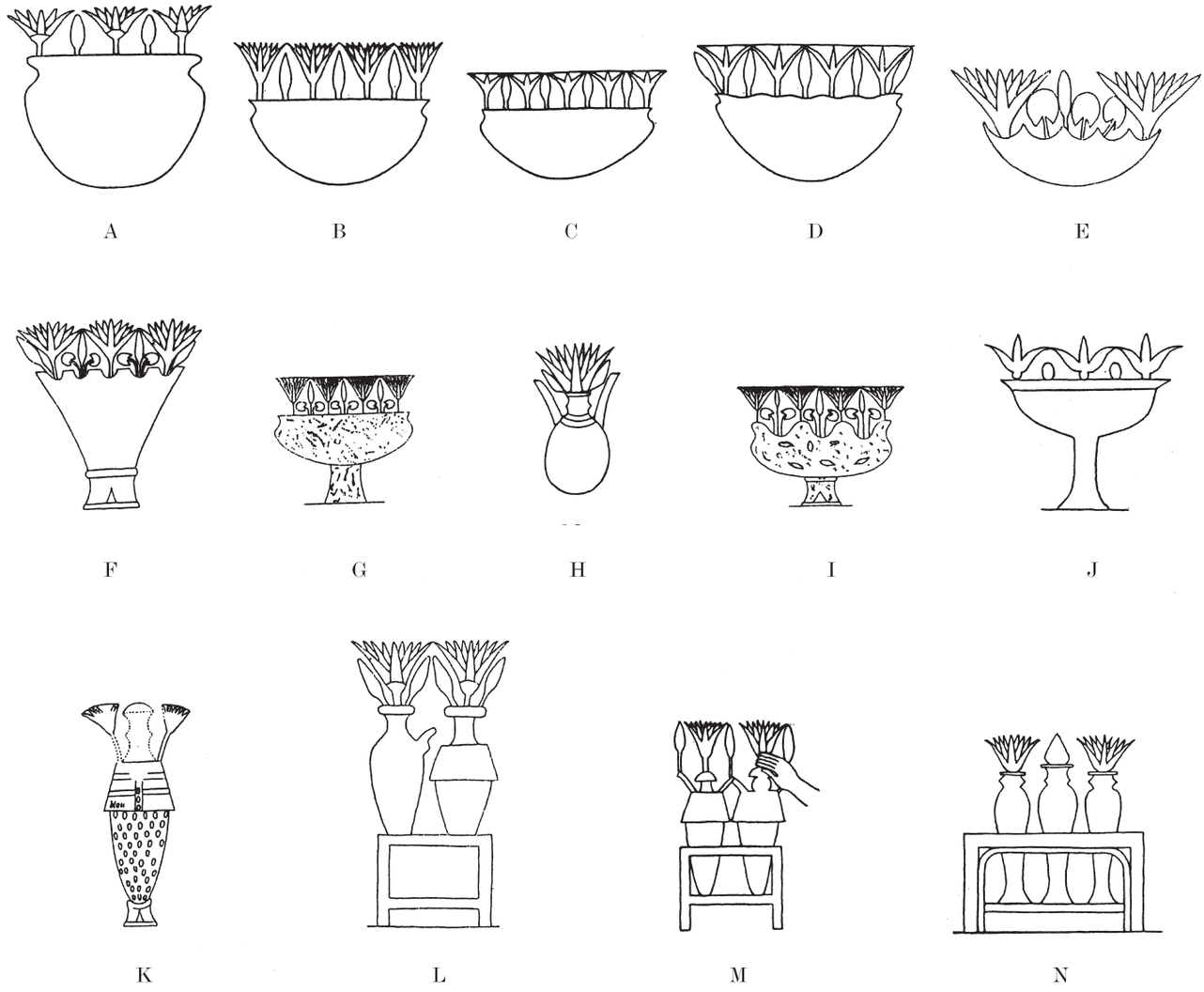


Fig. 5 Vessels decorated with flower from the Old Kingdom tombs: A) Saqqara (Kagemuni); B) Saqqara; C) Saqqara; D) Saqqara; E) Saqqara (Ptahhotep); F) Saqqara (Weser-neter); G) Saqqara; H) Saqqara (Kagemuni); I) Saqqara; J) Saqqara; K) Deir el-Gebrawi; L) Saqqara (Kagemuni); M) Saqqara (Kagemuni); N) Saqqara[39]

[38] Yon 1976, 174–176; Garstang 1907, 15; Amiran 1969, 303, photo no. 350 on the page 306; Yeivin 1976, 113.

[39] For the bowls A, H, L, M see: von Bissing 1905, pl. XXVI, pl. XXIX, pl. XXII, pl. XXIV; for the bowls B, C, D see Capart 1907, pl. XLVIII and LII; for the bowl E see: Paget, Pirie and Griffith 1898, pl. XXXV; for the bowl F see: Murray 1905, pl. XXIII; for the bowls G and I see: Firth and Gunn 1926, pl. 5A and 4, for the bowl J: LD Erg. pl. XLII; for the bowl K see: Davies 1902, pl. XVIII; for the bowl N see: LD, II, 68.

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Intra moenia Middle Bronze Age Burials at Tell es-Sultan: A Chronological Perspective

Sandra Antonetti[*]

Tell es-Sultan, ancient Jericho, has always been considered a key site for the study of Middle Bronze Age in Palestine, especially because of its necropolis located north and north-west of the tell, which is one of the best known cemetery for that period, while the stratigraphy of the Middle Bronze Age town[1] is less known due to the severe erosion which afflicted the mound.

1. History of archaeological excavations

Four archaeological expeditions excavated at Tell es-Sultan and each of them discovered some *intra moenia* burials, which do not seem to have had a significant role within the town layout, but which can provide new knowledge about the urban history of ancient Jericho.

The first expedition[2] was directed by Ernst Sellin on behalf of the *Deutsche Orient Gesellschaft* between 1907 and 1909 and started the systematic exploration of the mound, digging several important areas: the northern part of the town and a large sector of the so called Spring Hill, where some burials were found.[3] The publication of the results was as accurate as fast,[4] but the preliminary state of knowledge at the time did not allow right dating, so that the Middle Bronze Age remains were called "israelitisch".[5]

The Marston-Melchett expedition, directed by John Garstang from 1930 to 1936,[6] continued the excavation of the tell, mainly digging on the Spring Hill, where some jar burials were found: he also discovered the existence of an *extra moenia* necropolis, located west of the mound. This excavation was undertaken with a biblical perspective, which concurred in assigning sometimes a wrong dating of the archaeological remains.[7]

In 1952, Kathleen Kenyon, on behalf of the British School of Archaeology in Jerusalem, began her famous excavations at Tell es-Sultan, which lasted until 1958, digging several tombs in the northern part of the *extra moenia* cemetery, cutting three main trenches and uncovering other important areas on the mound, where some graves and a tomb were excavated. The stratigraphic method of excavations adopted by the archaeologist and the subdivision of the tombs into five groups based on their pottery sequence made her final reports a fundamental tool for the study of Middle Bronze Age pottery.

The joint Expedition of Rome University "La Sapienza" and the Palestinian Department of Antiquities resumed the excavation of the site in 1997. The excavation concentrated on eight areas in various parts of the tell, dating from the Early Bronze Age to the Byzantine Period: in Area G,[8] where the main archaeological features date from the Early Bronze Age, a Middle Bonze Age grave was excavated in 1999. Moreover other areas yielded Middle Bronze Age remains, which clarified the urban history of Tell es-Sultan during this period, especially as far as stratigraphy is concerned.[9]

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[*] Rome University "La Sapienza"

[1] The recent re-evaluation of Middle Bronze Age remains on the Spring Hill conducted by N. Marchetti (Marchetti 2003b: 304–316) has permitted a stratigraphic phasing of this and other periods.

[2] C. Warren dug the first shafts on the tell in 1868, concluding that there was nothing interesting to dig (Moorey 1991: 20).

[3] Sellin and Watzinger 1913: pl. 1.

[4] Sellin and Watzinger 1913.

[5] After some years, Carl Watzinger corrected this dating, thanks to the new results obtained from the excavations of other Palestinian sites (Watzinger 1926: 133–134; 136).

[6] The results were only preliminarily published by Garstang (1932a; 1932b; 1933; 1934; 1935; 1936) and were summarized in a book written with his son (Garstang and Garstang 1940). K. M. Kenyon (1951) later revised the results obtained for the 2nd millennium BC before she started her own excavations.

[7] Moorey 1991: 64.

[8] Marchetti and Nigro 2000b.

[9] In Area A (Marchetti and Nigro 1998: 117–140; Marchetti and Nigro 2000a: 193–281) a military building and a domestic quarter dating to MB II were uncovered; in Area E (Marchetti and Nigro 2000a: 181–192) and in Area C (Marchetti and Nigro 1998: 103–115) were excavated parts of the first rampart; in Area D (Marchetti and Nigro 2000a: 167–179), on the east side of the mound, a massive wall near Garstang's "East Tower" was excavated.

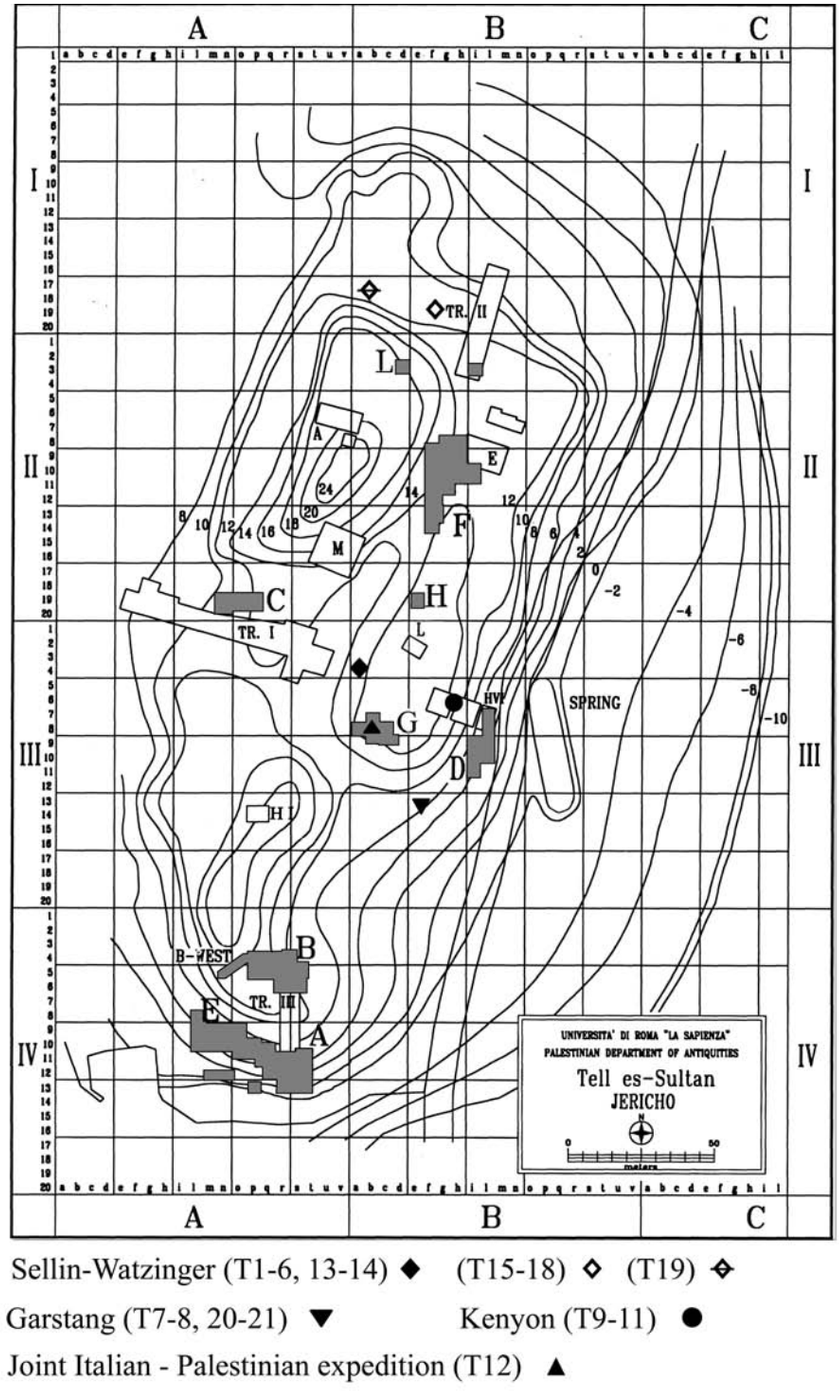


Fig. 1 Plan of Tell es-Sultan with indicated areas excavated by the joint Italian-Palestinian Expedition (1997–2000) and the Middle Bronze Age *intra moenia* burials

2. The *intra moenia* burials of Tell es-Sultan

A somewhat interesting feature of Middle Bronze Age Jericho is the presence of a small number of burials on the tell itself, notwithstanding the huge *extra moenia* necropolis attested to since many centuries.[10] The examination of these burials allows to add new data on the urban plan of Tell es-Sultan during the period considered and about the use of the necropolis (see §§ 2.3. and 2.5.).

Before starting the analysis of the burial remains, it is important to remark that Middle Bronze Age vestiges on the tell have been brought to light in several areas: in the region in front of Elisha's Spring;[11] in the northern part of the mound[12] and in the Lower Town, south of the tell (Area A excavated by the joint Italian-Palestinian Expedition). The central zone of the town is not preserved due to erosion and other activities of later times.

Intra moenia burials were retrieved in two areas:[13] in the northern part of the tell, in particular in a battered domestic quarter excavated by Sellin,[14] and on the Spring Hill: near Garstang's "East Tower",[15] in Kenyon's Squares HII–HIII–HVI[16] and in Area G, excavated by the Italian-Palestinian Expedition[17] (Fig. 1).

Further, in comparison with the Middle Bronze burial tradition of other Palestinian sites,[18] it is possible to divide these inhumations in three main types based on the burial method, that is: jar burials for infants; graves simply dug in the ground, and built-up tombs. These three main types were adopted in different proportions depending on their chronology and were employed according to the age or the number of buried people (see § 2.4.).

2.1. MB I burials

Digging the so called Spring Hill, the German expedition retrieved a group of eight burials, located west and east of a wall,[19] which could be part of the domestic structure to which Sellin said the burials were related.[20] The archaeologists described them as graves for adults, except a jar burial, older than the nearby domestic quarter. It is possible to establish the date of these burials thanks to the analysis of their funerary assemblages, which are very scanty, but characterised with diagnostic MB I[21] types, such as red burnished carinated bowls and black or red burnished piriform juglets, typical of the MB I period,[22] which represent a sort of fixed set, including at least a carinated bowl and a piriform juglet, while other objects were almost absent, except for two shells in T1[23] and T6, which contained some bird's bones too, and a limestone grindstone in T2.

The funerary equipment of T1 was composed of a shell, a fragmentary black burnished piriform juglet with double handle and a fragment of a red burnished vessel, that can be dated to MB I, because of the rounded body of the piriform juglet and for its flat base.[24]

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[10] Nigro 1999: 8.

[11] Kenyon 1993: 679.

[12] Sellin and Watzinger 1913: 62–66, pl. III. The German expedition excavated a domestic quarter, in which MB pottery was retrieved (Sellin and Watzinger 1913: 122, figs. 113–116).

[13] The burial of a woman aged 25–30 without funerary set was retrieved by Kenyon in Trench III site N (Kenyon 1981: 217), in the corner of a room built outside the city wall, and dated to the Middle Bronze Age because of its stratigraphic position. This feature could hint at the presence of other burials in the domestic quarter outside the defence, as noted in the report on some human bones in a secondary context in Area A (Marchetti and Nigro 2000a: 200; 319–327).

[14] Sellin and Watzinger 1913: 63–65.

[15] Garstang 1934: 119, Sellin and Watzinger 1913: 70–71.

[16] Kenyon 1981: 348–350, 356.

[17] Marchetti and Nigro 2000b.

[18] E.g.: Tell el-Mutesellim (Loud 1948), Tell el-Qadi (Ilan 1996), Tell el-Fâr^{c}ah north (Mallet 1973).

[19] Sellin and Watzinger 1913: 70.

[20] Sellin and Watzinger 1913: 71. Even after the analysis of all the evidence of the period it is not easy to attribute this wall to one or another phase (Marchetti 2003b: 309, note 35).

[21] Throughout the present paper the terminology MB I, MB II and MB III is adopted, instead of MB IIA, MB IIB and MB IIC.

[22] These vessels, imitating metallic prototypes in bronze (red burnished vessels) or in silver (black burnished pottery), both in shape and in treatment of the outer surface, are mainly found in funerary equipments (Nigro 2000: 1193–1194, Nigro 2002: 310).

[23] See the catalogue for the original excavation number of the burials.

[24] The same juglet, however, is said to be part of the funerary assemblage of T15 (Sellin and Watzinger 1913: 63, pl. 30:D,4), which could be dated to MB II, for the presence of two jars, typical of this period. It has to be noted that often the same vessel is shown for different burials, just to illustrate the typology, so the presence of a red burnished fragment of a not better identified vessel should hint at a MB I dating.

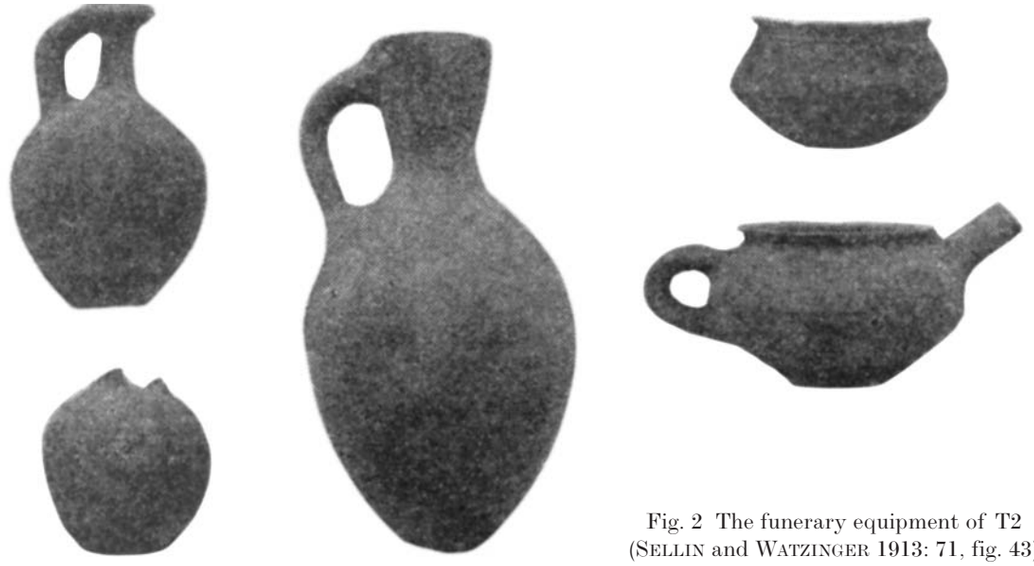


Fig. 2 The funerary equipment of T2 (Sellin and Watzinger 1913: 71, fig. 43)

T2 contained five vessels (Fig. 2): a red burnished carinated bowl, a "teapot", two black burnished piriform juglets and a dipper, the not yet elongated shape of which and the little flat base are comparable whith similar juglets from Ras el-ᶜAin/Aphek Palace phase;[25] in addition to the vessels there was a grindstone. The vessels are firmly placed within the MB I pottery tradition, in fact one of the two black burnished piriform juglets has a stepped rim and a ring base, typical of MB I, which is comparable with examples from Tomb 1181 at Hazor,[26] with a juglet from tomb 427 of the Post Palace phase at Ras el-ᶜAin,[27] which however is red burnished and has a triple handle, and in particular with a black burnished fragmentary juglet from Tomb K 3[28] of the *extra moenia* necropolis of Jericho, which is the only tomb in the cemetery dating to MB I.[29] The "teapot" has a direct comparison with two specimens found in tombs A 34[30] and B 51 of the necropolis datable to MB II,[31] but the example found by Sellin has on its surface a brown burnish,[32] which is not present on the other two vessels; another example of this rare vessel comes from Tomb 5234 at Tell el-Mutesellim,[33] associated to a trefoil-mouthed jug, a piriform and a cylindrical red burnished juglet which however seem datable to MB II. The carinated red burnished bowl has many comparisons in the Palace phase at Ras el-ᶜAin/Aphek.[34]

T3 had as single offering: a jar without handles (Fig. 3) comparable with several findings from Tell el-



Fig. 3 The jar from T3 (Sellin and Watzinger 1913: pl. 28)

[25] Beck 1975: 61, fig. 7:1.

[26] Maeir 1997: 310, fig. IV.5:1. It has to be remarked that the pottery assemblage of this tomb is dated to MB I and MB II.

[27] Beck 1975: 71, fig. 11:4.

[28] Kenyon 1965: 205, fig. 93:16–17.

[29] Kenyon 1965: 204.

[30] According to J. M. Weinstein (1975: 7), this tomb should be partly dated to MB I.

[31] Kenyon 1960: 357, fig. 140:31; Kenyon 1965: 340, fig. 166:8. It has to be stressed that the contents of the two tombs span in more than one phase, the oldest of which were characterised by vessels dating back to Group I or older, as stated by Kenyon (1960: 355; Kenyon 1965: 333).

[32] Sellin and Watzinger 1913: 125. It is probably a burnished self slip, as the colour of the ware and the colour of the slip are the same.

[33] Loud 1948: pl. 34:5.

[34] For example Beck 1975: 65, fig. 9:6 from Tomb 468.

Mutesellim (although many of them were painted, while the jar from T3 is not decorated), as, for example, a jar from Tomb 5181[35] and another from the pre-Palace II phase at Ras el-ᶜAin,[36] but again a jar from Tomb K 3[37] seems to be the best comparison, especially because these two jars are characterised by a larger maximum diameter at the centre of the body, which makes the shape a little bit squat.

T4, besides the already mentioned red burnished carinated bowl and a dipper juglet, contained also a black burnished juglet with ring-base and a double handle, which, although not illustrated,[38] should be a piriform juglet. In this case too, the funerary equipment is datable to MB I.

T5 has a scanty funerary set composed of two piriform juglets: one red slipped and with double handle, and the other black burnished, both with button base. The shape of the two juglets, still oval, with the largest diameter at the centre of the bodies, is typical of MB I. The third vessel of the funerary assemblage is an S-shaped bowl, which might have had a yellowish slip;[39] this bowl has a parallel in T7 retrieved by Garstang in the same part of the tell (see below).

The last burial, T6, had several vessels: two red burnished carinated bowls, similar to the other already described, another carinated bowl, but with a black burnish (Fig. 4) and a black burnished dipper; in addition there was a big jug with an ovoid shape and a small flat base (Fig. 5), the upper part of which was fragmentary, but can be compared with examples from Tomb K 3.[40]

Garstang dug three infant burials on the Spring Hill, just west of his "Tower", in his squares K.6 and I.6. In his report, the archaeologist described these funerary remains simply as child burials (one of them, not precisely identified, seems to have been of twins);[41] from the associated pottery, it is clear that T7 and T8 must be two graves, because they were not characterized by the presence of a jar or another large vessel, in which inserting the corpse.

The funerary equipment was very scanty (Fig. 6), as it is usual in child burials: in fact in T7 it consisted



Fig. 4 Bowl from T6 (Sellin and Watzinger 1913: pl. 30)



Fig. 5 Jug from T6 (Sellin and Watzinger 1913: pl. 28)

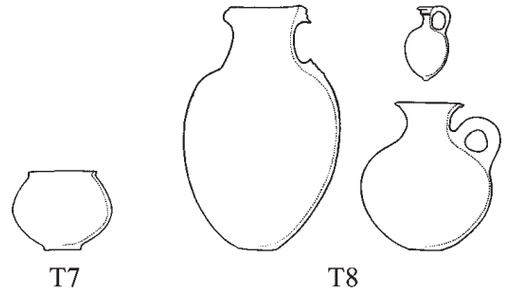


Fig. 6 Pottery vessels from T7 and T8 dug by J. Garstang (Garstang 1934: 119, pl. XVII:13, 15–17)

[35] Loud 1948: pl. 13:1.
[36] Beck 1985: 191, fig. 5:2.
[37] Kenyon 1965: 205, fig. 93:15.
[38] For the description of the vessel see Sellin and Watzinger 1913: 127.
[39] As Watzinger describes not the single vessel, but the type in general, is not possible to say it with certainty (see Sellin and Watzinger 1913: 126).
[40] Kenyon 1965: 205, fig. 93:11, 13.



Fig. 7 T9 in square HIII (Kenyon 1981: pl. 192b)

of only a yellow burnished S-profiled bowl with a disk base, which is associated in the illustration with an EB IV bottle clearly coming from an earlier stratum. Bowls of this type were found in MB I strata at Ras el-ᶜAin/Aphek Palace phase,[42] while they were not present in the necropolis of Tell es-Sultan except for Tomb K 3.[43]

T8 was equipped with two jugs and one juglet: the first jug has a good parallel again in a vessel from Tomb K 3 and in two tombs excavated on the tell by Kenyon,[44] and a similar kind of jug was retrieved in Cave 11 at Efrata,[45] which yielded materials dating to MB I and to the transition between MB I and II. The black burnished piriform juglet with profiled rim, too, is comparable with two specimens from Tomb K 3 (see note 28 above) or with the already mentioned specimen from Tomb 427 at Ras el-ᶜAin/Aphek (see above), while the shoulder handled jug is less well represented, due to its wide mouth, which is unusual for this shape.

These two burials, found in square K.6, are therefore dated to MB I thanks to the comparisons established with materials from other sites, while as far as the stratigraphy is concerned, Garstang[46] says that they were probably in relation with the floors found in the nearby, but in this case, too, the strong erosion prevented to understand their exact stratigraphic position.

North-east of Garstang's excavations on the Spring Hill, Kenyon opened three squares: HII, HIII and HVI, in which she uncovered four graves. Two of them, one of which was a child burial, were simple pits dug near the city wall, about which unfortunately Kenyon does not say much, only noting that the pits were cut in the occupational levels.[47]

Another child burial, T9, datable to MB I, was excavated by Kenyon in square HIII. The small corpse was lying on its right side, apparently in a flexed position (Fig. 7), equipped with an open bowl

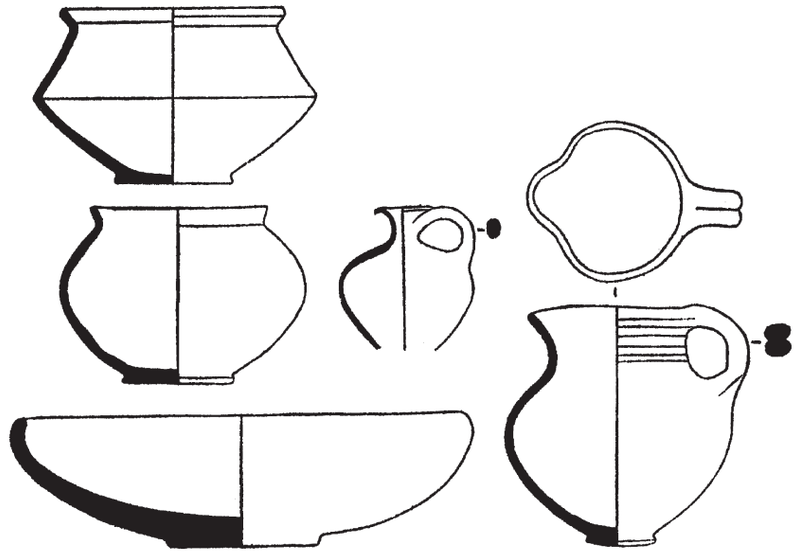


Fig. 8 Burial equipment of T9 (Kenyon 1981: 389, fig. 13:10–14)

[41] Garstang 1932a: 17.
[42] Beck 1975: 59, fig. 6:5.
[43] Kenyon 1965: 205, fig. 93:7–8.
[44] Kenyon 1965: 205, fig. 93:14; Kenyon 1981: 389, fig. 13:4, 8.
[45] Gonen 2001: 71, fig. 48:3.
[46] Garstang 1934: 119.
[47] Kenyon 1981: 348.

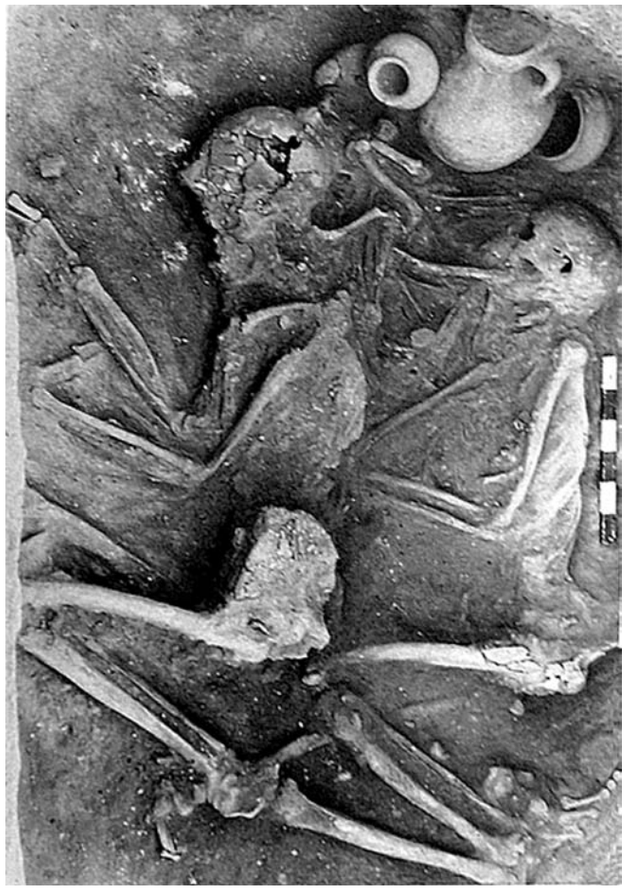


Fig. 9 T10 in square HIII
(Kenyon 1981: pl. 187b)

comparable with specimens coming from phase 2 at Ras el-ᶜAin/Aphek[48] or with a similar specimen found in T12/D.641, excavated by the Italian-Palestinian Expedition, an S-profiled bowl with traces of burnish, a red burnished carinated bowl, a trefoil-mouthed jug and a red burnished piriform juglet (Fig. 8). The trefoil-mouthed jug is a feature which appears late in the MB I, the oldest specimen at Tell el-Mutesellim being a vessel from Tomb 3109,[49] while a similar jug comes from T11/HAR, situated near the burial under discussion.

In the western corner of square HIII, another burial was excavated, T10: two adults were lying in a flexed position with heads to south (Fig. 9). The burial was lined on the east side with some mudbricks, but no traces of roofing were documented. The dead had a provision of meat between their heads, represented by parts of an ovine, and four vessels in the same position: three burnished S-profiled bowls of different size and an ovoid jug, with slightly convex base and moulded rim (Fig. 10). The jug is similar to that retrieved in T8 excavated by Garstang and is again comparable with vessels from Tomb K 3 (see note 40 above), from Cave 11 at Efrata and has a precise replica in T11 (see below). The position of the bodies resembles that of the skeletons in T12, although the orientation is different, but in both depositions the bodies were laid one after the other and the funerary equipment was set near the heads of the dead together with some meat provisions.

In square HII Kenyon excavated T11,[50] which has the peculiarity of being a mudbrick built tomb, with a vaulted ceiling. It had an opening on the southern short wall, blocked with a stone and approached by a stepped entrance (Fig. 11). Unfortunately, it was not possible to understand to which surface the tomb was related.[51] Several bones were found piled along the walls of the tomb, in particular at the rear of the chamber, as it will be usual in the *extra moenia* shaft tombs of the succeeding period, while six skeletons, nearly intact, were placed in the central part of the chamber with heads to south or to north.

The funerary equipment was composed of only five vessels, all datable to MB I: an S-profiled bowl and a carinated bowl, both with traces of slip, a dipper, a black-burnished trefoil-mouthed jug and a jug with moulded rim similar to those found in the already described burials (Fig. 12). The scanty number of vessels is unusual in respect of that of the buried corpses and of the several scattered bones: however, it is probable that the tomb was cleared from older funerary gifts in order to make room for the last inhumations, as it is normal in the later tombs of the necropolis.

It has to be remarked that the architecture of the tomb finds comparison only in tombs excavated at Tell el-Dabᶜa.[52] Although the poor conditions of preservation of T11 do not allow precise parallels, the rectangular shape of the chamber and the possibly vaulted roofing, in addition to the use of mudbricks, nonetheless make this tomb unique at Tell es-Sultan and in Middle Bronze Palestine in general: it

[48] Kochavi and Yadin 2002: 198.
[49] Gerstenblith 1983: 82.
[50] Kenyon 1981: pl. 328a.
[51] Kenyon 1981: 349.
[52] van den Brink 1982: 21.

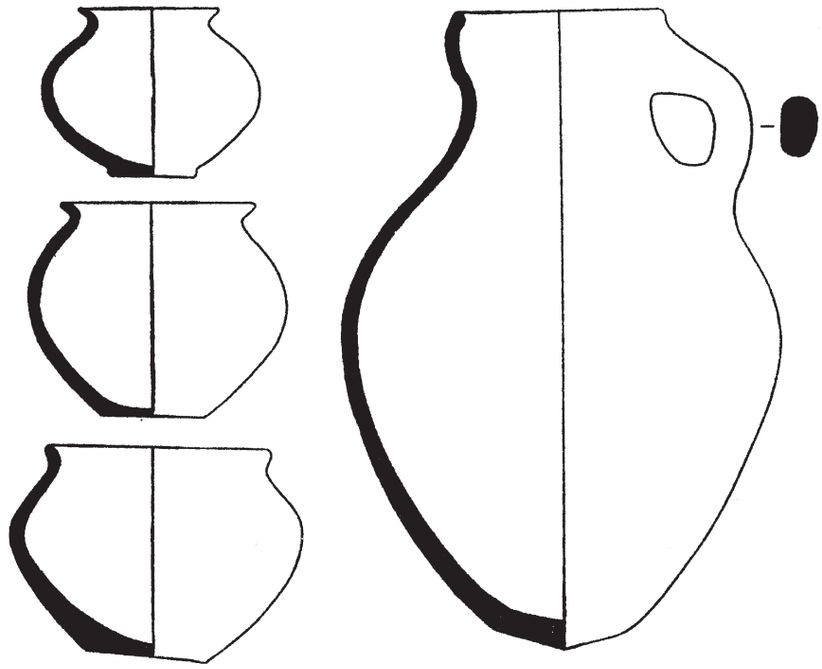


Fig. 10 Burial equipment of T10 (Kenyon 1981: 389, fig. 13:1–4)

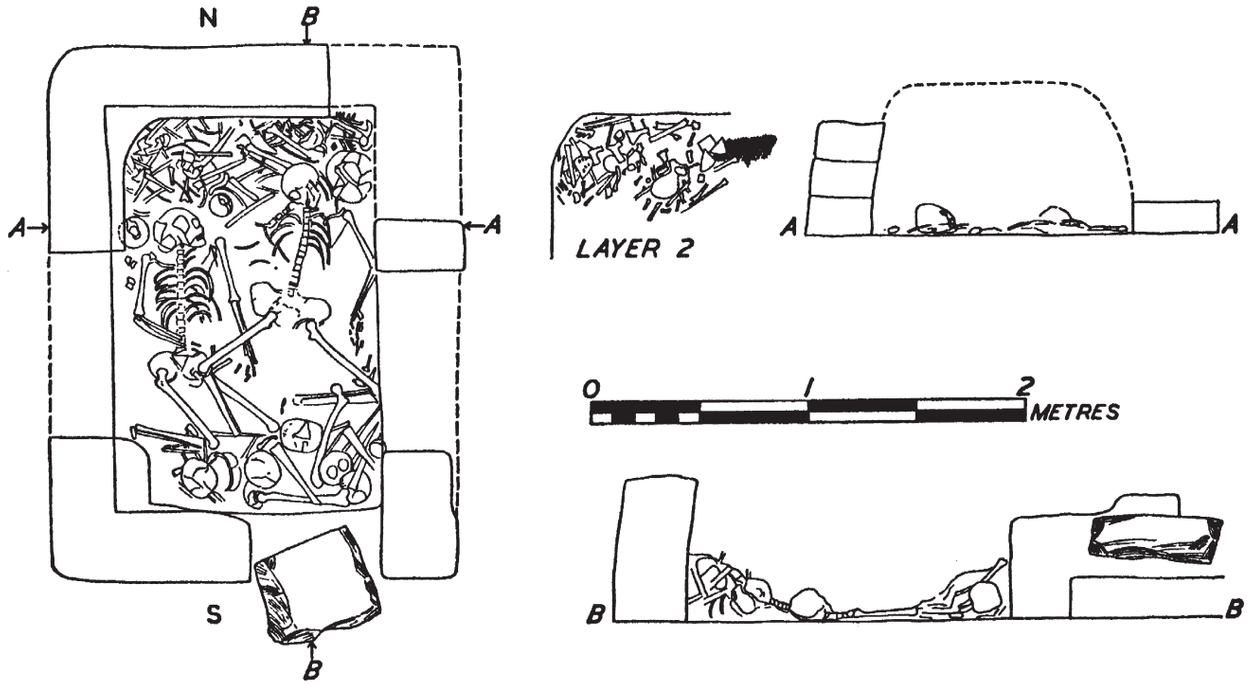


Fig. 11 Plan and section of T11 (Kenyon 1981: 349, fig. 5)

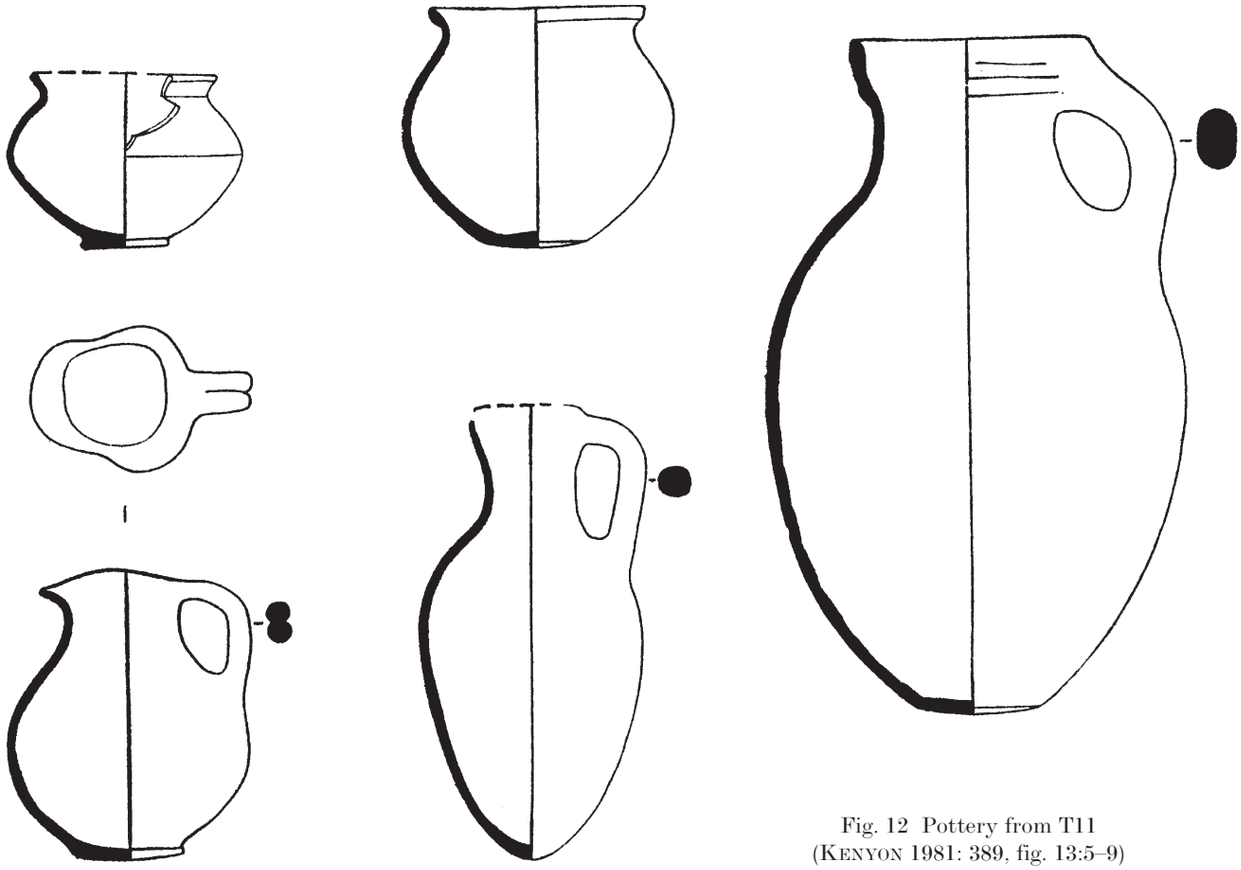


Fig. 12 Pottery from T11
(Kenyon 1981: 389, fig. 13:5–9)

can be only compared to stone chambers found in northern Palestine,[53] as regards the general plan.

The joint Italian-Palestinian Expedition excavated a burial, T12/D.641, in Area G, located on the Spring Hill south-west of Kenyon's squares HII–HIII–HVI. The tomb, square in shape, built of mudbricks and with a mudbrick covering too, contained two bodies: one of an adult and the other of a young girl, apparently twelve-fourteen years old,[54] lying on the left side, with heads to the east (Fig. 13), in a position similar to that noted in burial T10 in Square HIII, which was lined with mudbricks too. The building technique of this tomb is unusual and reminds of the tombs retrieved in Tell el-Dabᶜa[55] because of the mudbrick lining and roofing, even if in T 12 it was rather a covering.

The burial set was richer than those described until now, for it had some personal ornaments such as: bronze earrings, a bronze ring, two steatite scarabs,[56] a bronze pin, near the left shoulder of the young girl, and many stone and frit beads, in addition to meat offerings and vessels. The pottery assemblage includes fine pieces such as a red slipped bowl with three-loop feet, a shoulder handled jug with a ring base,[57] a black burnished piriform juglet, an open bowl with a painted red band on the rim, which is comparable with specimens from Palace II phase at Ras el-ᶜAin/Aphek,[58] and two S-profiled bowls of different sizes, the largest one pattern burnished, and the smaller red slipped (Fig. 14).

T12 is different from the other MB I burials of Tell es-Sultan for the richness of its funerary equip-

[53] The most famous specimens come from Tell el-Mutesellim (Loud 1948) and Tell el-Qadi (Ilan 1996).

[54] Marchetti 2003a: 9.

[55] van den Brink 1982: 20.

[56] For an analysis of the scarabs from Tomb T12/D.641 see Marchetti 2003a.

[57] This element usually denotes a late stage in MB I, since it is not well attested among similar jugs from Ras el-ᶜAin (Kochavi and Yadin 2002: 210), while a shallow ring base is more widespread at Tell el-Mutesellim (Loud 1948: pls. 10:12; 19:21, 23:2).

[58] Kochavi and Yadin 2002: 210.



Fig. 13 T12/D.641 seen from the west



Fig. 14 Pottery from T12/D.641



Fig. 15 Pottery from T15 in House A (Sellin and Watzinger 1913: pl. 28)

ment, characterised – besides meat provisions in open bowls – also by personal ornaments and by the first appearance of steatite scarabs, well attested in the later tombs of the necropolis. These features make this burial comparable with the *extra moenia* tombs, in particular with Tomb B 35, which yields a similar tree loop footed bowl[59] and piriform black burnished juglets[60] or with Tomb D 9 with similar shoulder handled jugs.[61]

The considerations about the funerary equipment, and the parallels established with the necropolis suggest a date for T12 late in MB I, probably in MB IB, Period IVa2 of the site.[62]

2.2. MB II–III burials

The number of burials on the tell dating to MB II and MB III is smaller, although remains from those periods were better preserved.[63] This circumstance seems to be connected to the use of the *extra moenia* necropolis and is comparable with the general situation in Palestine: moreover, all the MB II–III *intra moenia* graves were jar burials for infants, with a funerary set usually composed of a single juglet put inside or nearby the jar used as a container, broken at the neck or at the bottom for facilitating the insertion of the corpse.

As regards the eight burials discovered by Sellin on the Spring Hill, only two can be assigned to MB II: while T14 with only a dipper as grave good, is difficult to date, it is T13 which offers the best evidence. It is a jar burial of an infant, with the typical funerary equipment composed of a dipper, two brown burnished juglets, one with double handle (which can be compared with specimens from the necropolis),[64] and an S-profiled bowl.

In the northern part of the mound, the German expedition excavated a domestic quarter, identifying a group of five houses,[65] under the floors of which were found some child burials. Under the floor of House A, a jar burial, T15, with an infant inside, was retrieved, accompanied by an ovoid handleless jar and a piriform juglet (Fig. 15), a pottery assemblage which fits the MB II period. Under the floor of House B, west of the previous one, another jar burial, T16,

[59] Kenyon 1960: 372, fig. 148:7.
[60] Kenyon 1960: 377, fig. 150:22–23.
[61] Kenyon 1965: 284, fig. 136:1–2.
[62] Marchetti 2003b: 306.
[63] About the domestic remains on the Spring Hill see Marchetti 2003b: 310–316 with bibliography.
[64] The funerary set is not illustrated, but the information given by Sellin and Watzinger (Sellin and Watzinger 1913: 70) points out to vessels similar to, for example, Kenyon 1960: 377, fig. 150:18.
[65] Sellin and Watzinger 1913: 62, pl. III.



Fig. 16 Juglet from T16 in House B (Sellin and Watzinger 1913: pl. 22)

was found, with a painted "Chocolate-on-White" piriform juglet nearby,[66] which is datable towards the end of MB III[67] (Fig. 16).

West of the preceding domestic quarter, in Square C 6,[68] Sellin found other domestic structures heavily eroded away,[69] under the floors of which two more child burials were excavated. The first burial, T17, yielded a small double handled jar with yellowish slip, a carinated bowl with ring base and flaring rim and a black burnished juglet with ring base, which is comparable to materials found in the Tell es-Sultan necropolis[70] (Fig. 17); the second one, T18, excavated near the city wall, has a funerary set constituted by a jug with ring base and pinched mouth, similar to the jug retrieved by Kenyon in Tomb H 11[71] or to a specimen found in room 40 of Garstang's "Palace storerooms",[72] and a cylindrical juglet with double handle with button (Fig. 18), comparable with specimens from the *extra moenia* necropolis,[73] besides a copper ring. The comparisons mentioned for these vessels suggest a date for these burials late in MB III.

In square C 5 another jar burial, T19, belonging to an eroded domestic building, was found, with a black burnished cylindrical juglet nearby (Fig. 19), similar to those retrieved in tombs of the necropolis,[74] and which is therefore datable to MB II. T20 was excavated by Garstang in Square I.6 and it is datable to MB II, too. It was a jar burial, with a funerary equipment composed of a piriform juglet and a cylindrical Tell el-Yahudiyeh juglet (Fig. 20). Garstang hints at another child burial, T21, of which he publishes only a jar:[75] it seems significant that it comes from the "Palace storerooms", which were, indeed, a domestic quarter.[76] The jar, broken at the bottom, as customary, was buried under the floor of room 31, so that it is possible to date this vessel to late MB II or MB III for its stratigraphic position and for the comparison with jars from the *extra moenia* tombs.[77]

2.3. Comparison with the new excavations and the Necropolis

As seen in the preceding paragraphs, many of the *intra moenia* burials of Tell es-Sultan belong to MB I, a period not well represented in the renewed joint excavations of Rome University "La Sapienza" and the Palestinian Department of Antiquities, which, on the other hand, retrieved useful materials for comparisons with burials dating to MB II and III, especially because of the possible parallels with the *extra moenia* necropolis, unbound of course to any stratigraphic evidence.[78] The excavations conducted

[66] Sellin and Watzinger 1913: 63.
[67] Amiran 1969: 159.
[68] Sellin and Watzinger 1913: pl. III.
[69] Sellin and Watzinger 1913: 64–65.
[70] See, for example, Kenyon 1960: 399, fig. 161:3.
[71] Kenyon 1960: 474, fig. 205:6.
[72] Garstang 1934: pl. XXII:16. For a recent re-evaluation of Garstang's "Palace storerooms" see Marchetti 2003b: 312–316.
[73] Kenyon 1965: 443, fig. 231:3.
[74] For example Kenyon 1960: 336, fig. 131:15.
[75] Garstang 1934: pl. XXI:14.
[76] Bienkowski 1986: 126; Marchetti 2003b: 312.
[77] For example Kenyon 1960: 508, fig. 220:1–3.
[78] Moreover the great amount of piled objects inside the shaft tombs makes difficult the right chronological distinction of the vessels, which in fact has been based on pottery features (Kenyon 1960: 264, 268.)

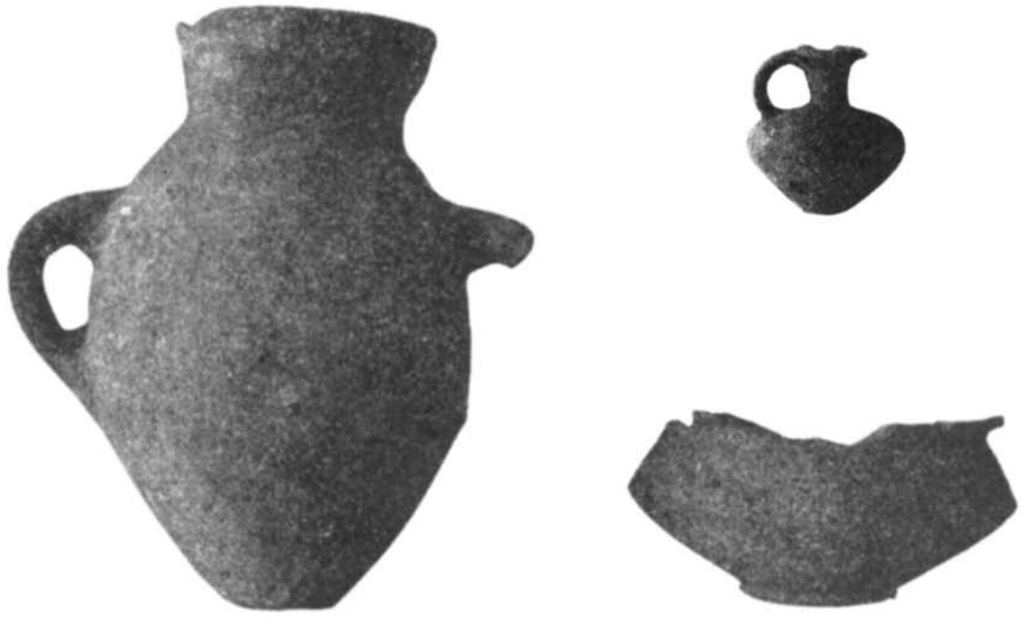


Fig. 17 Funerary set of T17 in Square C 6 (Sellin and Watzinger 1913: 64, fig. 36)



Fig. 18 Funerary set of T18 the city wall (Sellin and Watzinger 1913: 64, fig. 37)

by the joint Italian-Palestinian Expedition at Tell es-Sultan, besides the discovery of Tomb T12/D.641, investigated in Area A a tower along a fortification wall and a residential quarter, located in the Lower Town, which yielded good stratified materials dating to MB II and comparable in some cases with the pottery from the burials described above.

In the houses of Area A many sherds of jars and simple ware vessels were found. In particular a two handled jar,[79] nearly intact, is a good comparison for vessels used for infant burials during MB II, while

[79] Marchetti and Nigro 2000a: 261, fig. 5:48.2.



Fig. 19 A cylindrical juglet from T19 (Sellin and Watzinger 1913: pl. 29)

some rims of jars[80] retrieved in Building A1[81] resemble that of the handleless jar found as burial furnishing by Sellin in T15. Even a fragmentary carinated bowl[82] found in the same building is useful to establish a correct chronology for T17, as it seems the case for the small double handled jar of the same burial, the rim of which is similar to a fragmentary rim retrieved again in Building A1.[83] In Building A2, a domestic unit in the southern Lower Town,[84] the base of a cylindrical black burnished juglet[85] was retrieved, which is a useful comparison for the juglet retrieved in T19.

Comparison of the *intra moenia* burials with the outer Necropolis shows that they were not contemporary, but the former preceded the latter, as it is the case also in other Palestinian sites, as at Tell el-Fâr^{c}ah north,[86] where the *intra moenia* tombs were frequent during MB I, and diminished in the following period, when the *extra moenia* cemetery was more used. Moreover, the presence of Tomb K 3 in the necropolis indicated that the latter was not unknown during MB I, so it seems to have been a conscious decision not to reuse the *extra moenia* cemetery, but to bury the dead on the tell. This fact was noted by

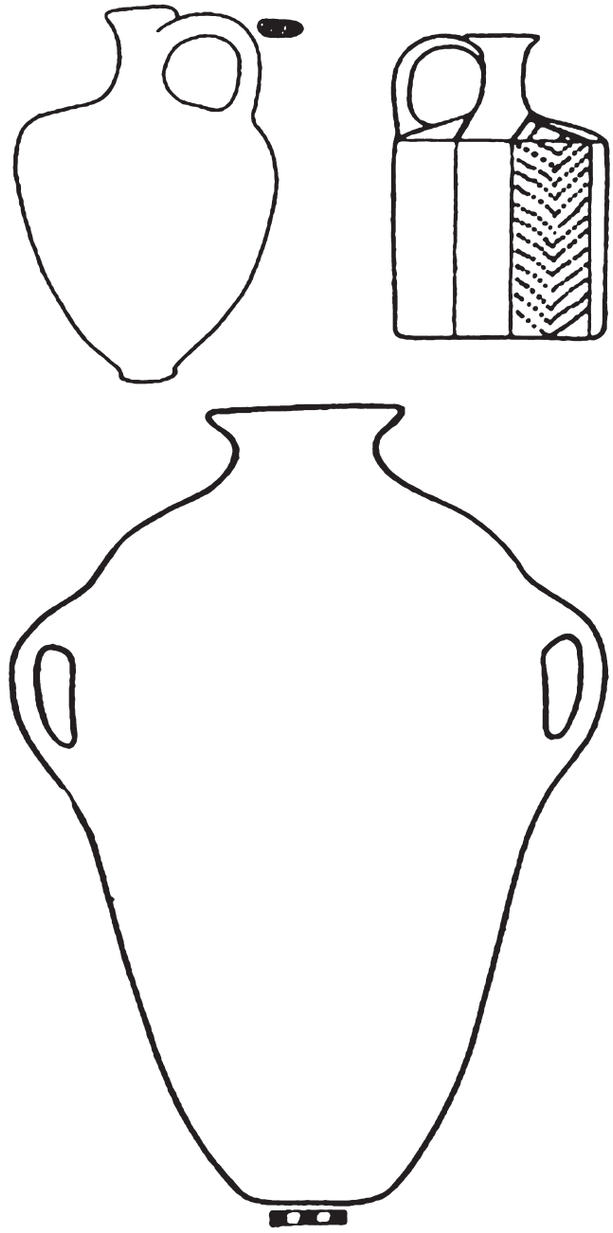


Fig. 20 Pottery from T20 in Square I.6 (Garstang 1934: 119, pl. XVII:13, 18–20)

Sellin and by Kenyon,[87] who attributed it to the fear of desecration of burials by enemies. In this respect it is important to note that MB I *intra moenia* burials

[80] Marchetti and Nigro 2000a: 257, fig. 5:46.4–5.

[81] For the description of this building see Marchetti and Nigro 1998: 124–135; Marchetti and Nigro 2000a: 199–207 and recently Marchetti 2003b: 310–311.

[82] Marchetti and Nigro 1998: 179: 4:25.1.

[83] Marchetti and Nigro 1998: 179, fig. 4:25.6.

[84] About this building see Marchetti and Nigro 2000a: 207–214.

[85] Marchetti and Nigro 2000a: 265, fig. 5:50.7.

[86] Mallet 1973: 97–98.

[87] Kenyon 1993: 679 and Kenyon 1971: 17, where the archaeologist suggests that during MB I the tell was not crowded with houses and there was enough space to build *intra moenia* burials.

were not restricted to children, as it is usually the case during MB II and III, but they were used also for adults, which were usually buried in simple pits or in tombs built or lined with mudbricks.

2.4. Tomb typologies and burial equipment

The analysis of the *intra moenia* burials shows differences in burial customs during the Middle Bronze Age, affecting tomb typologies and burial equipments.

During MB I, in fact, two kinds of burials were present on the tell: simple graves, for children and for adults, and built-up tombs. In the latter category, all the burials having parts built of mudbricks are comprised, but each of them differs from the other in its building technique. The first is T10, the burial of two adults found by Kenyon in square HIII, which was lined with mudbrick just on one side; the second is T12/D.641, which was lined with mudbricks on its four sides and covered with mudbricks, too, and the third is T11/HAR, completely built of mudbricks and with a vaulted ceiling. T11 is comparable with some tombs excavated at Tell el-Dabᶜa, concerning the appearance and the building technique, but it is different for the burial equipment and for the number of skeletons inside.[88] The other two tombs are comparable for the position of the bodies, but they differ very much as the grave goods are concerned. In fact, while T11 and T10 yielded similar grave goods, composed of S-profiled bowls and jugs with moulded rim, T12 had a completely different set of pottery and displayed also personal ornaments. Despite the differences in building technique and burial equipment, these tombs are comparable because occupied by adults and by more than one body, antedating the burial custom of the following period.

The other MB I single graves, both for children and for adults, had a homogeneous burial equipment, made of one or two S-profiled or carinated bowls, a piriform juglet and sometimes a jug. Despite this homogeneity, it has to be stressed the complete absence of jars in adult burials on the tell:[89] since these vessels are the main characteristic throughout the entire Middle Bronze Age for infant burials, they seem to have had a more elaborate significance than a simple funerary gift, as it seems instead the case in the funerary equipments retrieved in the *extra moenia* tombs. In fact jars, since they constitute the shelter of the infant after the death, may allude to the womb,[90] shelter of the infant before the birth, an hypothesis which may shed some light on the funerary belief in Middle Bronze Age Palestine.

During MB II there seems to take place a major change, because adult burials are no more found on the tell, while jar burials continued to be realised under the floors of the dwellings. Also the funerary equipment for jar burials becomes slightly different, as it is usually composed of one or two juglets put inside the jar or near it, without any other vessel or object; only rarely it is found also a second jar, in this case perhaps intended as gift.

Apart from the findings of two shells and a grindstone in the burials investigated by Sellin near the Spring Hill, there were no objects in *intra moenia* burials, the only tomb which had some objects being T12. This situation, compared to the tombs of the necropolis, seems very unusual and calls for an explanation: it seems possible that we are dealing with poorer inhabitants of Tell es-Sultan, the dead buried in T12 representing perhaps a different class.

We cannot say much about the position of the skeletons, but the comparison between T9 (Fig. 7), T10 (Fig. 9) and T12 (Fig. 13), shows that the flexed position on a side (whether left or right it seems to have been unimportant) was adopted, while the orientation of the bodies does not seem to have been fixed, as it is the case in the tombs of the necropolis and as customary in Palestine during the Middle Bronze Age in general.

	MB I		**MB II**	**MB III**
	Child burials	Adult burials	Child burials	Child burials
Sellin-Watzinger	/	6 (T1–6)	4 (T13–15, 19)	3 (T16–18)
Garstang	2 (T7–8)	/	2 (T20–21)	/
Kenyon	1 (T9)	2 (T10–11)	/	/
University of Rome "La Sapienza", Dept. of Antiquities of Palestine	/	1 (T12)	/	/
Total	3	9	6	3

[88] Bietak 1991.

[89] Only the burial of an adult, T3, contained as single gift a handless small jar, which is, anyway, very different from the types associated with infant burials.

[90] Ilan 1995: 135.

2.5. *Intra moenia* burials and urban plan

The presence of *intra moenia* burials on Tell es-Sultan allows some reflections about the development of the town during the Middle Bronze Age, which unfortunately is very difficult to reconstruct due to the poorly preservation of the structures: notwithstanding this situation, it is possible to obtain some useful information by studying the location of the burials.
As regards the distribution of MB I burials, it seems that they were concentrated only on the Spring Hill, in an area isolated from other buildings, but both Sellin[91] and Garstang[92] referred to the burials that they excavated, as dug under the floors of some badly eroded houses. This hypothesis seems reasonable because of the vicinity of the spring, which makes this area the most suitable for a residential quarter,[93] as it is demonstrated by the attribution to the MB I of a little domestic structure excavated by Kenyon in the same area of the graves[94] and the successive densely built domestic quarter dating from MB II and MB III. The situation of Tell es-Sultan in this period is similar to other Palestinian sites, for example at Tell el-Fârᶜah north, where houses, later destroyed by the successive building of the rampart,[95] had graves either for adults or for children under the floors or in their nearby.[96]

The analysis of the pottery inventory shows that most of the *intra moenia* burials at Tell es-Sultan date to a final stage of MB I, as shown by comparable materials from Ras el-ᶜAin, dating to phase 3 or 4,[97] from Tell el-Mutesellim, from Efrata and from part of the assemblage of Tomb 1181 at Hazor, so that it is possible to ascribe the renaissance of urban life at Tell es-Sultan in this period or slightly before, when the settlement was probably confined to a restricted area and the necropolis was not yet in use, except for Tomb K 3.

In the following period, MB II, the area on the Spring Hill becomes densely built and the new domestic quarter follows in part the older building plans,[98] but funerary customs changed: on the site only infants were buried, the *extra moenia* necropolis became again the burial place for adults. Moreover, the settled area expanded to the northern part of the tell, too, where some houses were found by Sellin, with jar burials sunk under the floors.

In MB III,[99] characterized by the introduction of new wares, such as Chocolate-on-White (see for example the piriform juglet retrieved by Sellin in T16), by a refurbishing of the fortification and by a rebuilding of the domestic quarter on the Spring Hill, the burial customs remained unchanged: jar burials for infants on the tell and *extra moenia* shaft tombs for adults.

Unfortunately, the strong erosion of Middle Bronze Age remains and the lack of some data on the burials in older publications prevent us from more precise conclusions about this subject: only more excavations will give new elements on *intra moenia* burials at ancient Jericho.

2.6. Unpublished MB I–III burials

Since this paper was written, the author had the chance to examine the original archives from Garstang and Kenyon's excavations,[100] discovering the existence of further *intra moenia* burials unearthed on the Spring Hill and finding out more details about the yet described inhumations. Regarding the latter, more information was collected about the position of the skeletons excavated by Kenyon near the city wall in square H VI[101] and dated to MB I. Both of them were crouched, the child on the right side and the other dead[102] on the left side, with hands bent near the chin, while the orientation was different: the child with head to east and the second burial with head to north. This information confirms the preference for the crouched position noted in other MB I burials at Tell es-Sultan, T9–10 and 12, and the carelessness for the orientation of the burial.

An analysis of the original documents from

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[91] Sellin and Watzinger 1913: 71.
[92] Garstang 1934: 119.
[93] The very long stratigraphic sequence detected on the Spring Hill seems to confirm this opinion (see Marchetti: 2003b: 295).
[94] Marchetti 2003b: 305, fig. 7; see also the eroded wall east of T12/D.641 in Area G.
[95] Mallet 1973: 93.
[96] Mallet 1973: 97.
[97] Kochavi and Yadin 2002: 210–218.
[98] Marchetti 2003b: 310.
[99] For the distinction between MB II and MB III, see Dever 1992.
[100] I would like to thank R. Chapman and F. Cobbing of the Palestine Exploration Fund for the help they gave me with Garstang's archive and R. Boast, curator of World Archaeology of the Museum of Archaeology and Anthropology of Cambridge, and A. Taylor for letting me examine Kenyon's original documents.
[101] See § 2.1.
[102] This skeleton is said to be in very bad condition, therefore its age is not obtainable.

Garstang's excavation led to recover four more jar burials located on the Spring Hill. Two of them, both containing a piriform juglet,[103] were dug in square K.6 and are dated to MB II, while the remaining two, one with a piriform[104] and the other with a cylindrical juglet, were sunk under the floors of the "Palace storerooms" (room 18 and 46) and are therefore datable to MB III. The existence of these unpublished jar burials adds another piece of evidence to the diffusion of this funerary custom in MB II and III Jericho, strengthening the evidence that these were the only *intra moenia* burials allowed on the tell at that time.

Catalogue

T1
Excavation register: "Grab 1"
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T2
Excavation register: "Grab 2"
*Type:*Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T3
Excavation register: "Grab 4"
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T4
Excavation register: "Grab 6"
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T5
Excavation register: "Grab 7"
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 71

T6
Excavation register: "Grab 8"
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 71

T7
Excavation register: Burial 1
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Garstang's square K.6 (i.e. BfIII14, Marchetti 2003b:fig. 8)
Bibliography: Garstang 1934:119, pl. XVII:13–14

T8
Excavation register: Burial 2
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Garstang's square K.6 (i.e. BfIII14, Marchetti 2003b:fig. 8)
Bibliography: Garstang 1934: 119, pl. XVII:15–17

T9
Excavation register: /
Type: Simple grave
Dating: Late MB I
Localization: Spring Hill, Kenyon's square HIII (i.e. BhIII7, Marchetti 2003b:fig. 8)
Bibliography: Kenyon 1981: 356

T10
Excavation register: /
Type: Built-up tomb
Dating: Late MB I
Localization: Spring Hill, Kenyon's square HIII (i.e. BgIII5, Marchetti 2003b:fig. 8)
Bibliography: Kenyon 1981: 349

T11
Excavation register: Tomb HAR
Type: Built-up tomb
Dating: Late MB I
Localization: Spring Hill, Kenyon's square HII (i.e. BfIII6, Marchetti 2003b:fig. 8)
Bibliography: Kenyon 1981: 350, pl. 328a

T12
Excavation register: Tomb D.641
Type: Built-up tomb
Dating: Late MB I
Localization: Spring Hill, Area G (i.e. BbIII8, Marchetti 2003b:fig. 8)
Bibliography: Marchetti 2003a: 9

[103] Garstang 1934: pl. XVII: 6, 10. The vessels were published as coming from the Tower area, without more specific details.

[104] Garstang 1934: pl. XIX: 17.

T13
Excavation register: "Grab 5"
Type: Jar burial
Dating: MB II
Localization: Spring Hill, square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T14
Excavation register: "Grab 3"
Type: Simple grave
Dating: undatable
Localization: Spring Hill, Sellin's square G 5 (i.e. BaIII4, Marchetti 2003b:fig. 8)
Bibliography: Sellin and Watzinger 1913: 70

T15
Excavation register: /
Type: Jar burial
Dating: MB II
Localization: Northern part of the tell, House A
Bibliography: Sellin and Watzinger 1913: 63

T16
Excavation register: /
Type: Jar burial
Dating: Late MB III
Localization: Northern part of the tell
Bibliography: Sellin and Watzinger 1913: 63

T17
Excavation register: /
Type: Child burial
Dating: Late MB III
Localization: Northern part of the tell, Sellin's square C 6
Bibliography: Sellin and Watzinger 1913: 64

T18
Excavation register: /
Type: Child burial
Dating: Late MB III
Localization: Northern part of the tell, Sellin's square C 6
Bibliography: Sellin and Watzinger 1913: 64

T19
Excavation register: /
Type: Jar burial
Dating: MB II
Localization: Northern part of the tell, Sellin's square C 5
Bibliography: Sellin and Watzinger 1913: 64

T20
Excavation register: Burial 3
Type: Jar burial
Dating: MB II
Localization: Spring Hill, Garstang's square I.6 (i.e. BgIII12, Marchetti 2003b:fig. 10)
Bibliography: Garstang 1934: 119, pl. XVII:18–20

T21
Excavation register: /
Type: Jar burial
Dating: MB II
Localization: Spring Hill, "Palace storerooms", room 31 (i.e. BeIII9, Marchetti 2003b:fig. 11)
Bibliography: Garstang 1934: pl. XXI,14

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Tell Abu Hawam: News from the Late Bronze Age

Michal Artzy[*]

General Information and History of the Site

Tell Abu Hawam, a site on the estuary of the Qishon River north of the Carmel Ridge in the confines of Haifa, modern Israel, has been a constant companion to archaeologists in the quest of contacts and trade, especially between the Aegean and the Levant (Fig. 1). Its location underneath the Carmel Ridge, which guards it from the prevalent southwesterly winds, enhances its importance as an anchorage/harbor where the Maritime and the terrestrial routes meet. The favorable position of the site as an anchorage in antiquity was matched by its auspicious location in the modern industrial development of the area, already during the British mandate and later the state of Israel. There are however drawbacks which caused the area to be utilized as an anchorage/harbor only intermittently. These include its position on an active geological fault line, frequent swamps caused by the Qishon River and the proximity and the nature of the Carmel Ridge, which hampers sustaining a terrestrial route towards the east or the south.

The tell is now located ca. 1.5 km from the coast due to geomorphological and tectonic changes, silting caused by the Qishon River and industrial and urban development. The modern history of this important early harbor has not been good. Already in the early part of the 20th century, industrial development was planned for the area. The British Mandate Department of Antiquities carried out several salvage excavations on and around the site. In 1922, P.L.O. Guy and G.M. Fitzgerald excavated the Mt. Carmel necropolis and in 1929, L.A. Mayer and N. Makhouly excavated on the tell as did N. Makhouly in 1930 followed by D.C. Baramki and A. Vilensky and eventually in 1932–33 R.W. Hamilton and L. Sorial carried out a major excavation. Hamilton proceeded to publish the results of his, at times, harried project, which became the base of reference to the site (Hamilton 1934; 1935). The Israel Department of Antiquities and Museums continued salvage projects among which was a cemetery excavation carried out in 1952 by E. Anati and M. Prausnitz (Anati 1959) and in 1963 on the edge of the site by E. Anati and Y. Olami (Anati 1962).

J. Balensi, following her methodological study of material from the previous excavations by Hamilton held a tenacious belief that parts of the site remained and could be found. In 1985–86, an archaeological project on the tell was carried out by the Centre de Recherche Français de Jérusalem (J. Balensi and G. Finkielsztejn) in cooperation with the Casa de Santiago de Jerusalem (M. D. Herrera) and with the Center for Maritime Civilizations of Haifa University (M. Artzy). Work was supported by the Ecole Biblique et Archéologique Française de



Fig. 1 General Map

[*] University of Haifa

Jérusalem and the Society for Preservation of Nature in Haifa. In coordination with the archaeological research, the then Center for Maritime Studies of Haifa University undertook geomorphological probes (1985–6, A. Raban, and I. Galanti). It was during this period that the surroundings of the site were formally re-declared as an antiquity site. Any construction or damage had to be reported to the Israel Antiquities Authority. Already in 1990, a small salvage excavations of the Department of Antiquities under the direction of S. Yankelevich took place in the eastern side of the site. Hamilton divided the layers to 5 *strata* from I to V:

Stratum I	Surface Material	
Stratum II	Graeco–Persian	Late 6th to early 4th cent. BCE
Stratum III	Iron I	c. 1100–925 (?) BCE
Stratum IVb	Iron I	1194–1100
Stratum IVa	Late Bronze	1230–1195
Stratum V	Late Bronze	1400–1230

Following the 1985–86 excavations, Balensi and Herrera reworked the stratigaraphic relationships and proposed a new division of the layers (Balensi *et al*. 1993):

Stratum I	Medieval, Byzantine, Roman, Hellenistic	Surface mixed debris	
Stratum IIB	Late Persian – Early Hellenistic?	Fortified city	
Stratum II A	Persian	Regional granary	5th–4th cent. BCE
	Assyrian, Babylonian	Gap in occupation	7th–6th cent. BCE
Stratum III B	Iron Age II (A–), B–C	Renewed planning	Late 10th–8th cent.
Stratum III A	Iron Age II A	Fortified city	10th cent.
Stratum IV B	Iron Age I / II A	Public Buildings	11th–10th cent.
Stratum IV A	Iron Age I B	Three-room houses	11th cent.
Stratum V C	Late Bronze II B / Iron Age I	New settlement	13th–12th cent.
Stratum V B	Late Bronze (I B /) II A–B	Cyclopean Fortifications	Mid 15th cent.
Stratum V A	Late Bronze IB	Quarrying and terracing	15th cent.
Stratum VI A	Middle Bronze II, Late Bronze I	Maritime Trade	16th–15th cent.

This table followed another one which Balensi and Herrera published a few years earlier in which they noted a possible abandonment of the site in what seems to be their Level V C (Herrera and Balensi 1985). This possible gap had been previously noted by others who have, in the past, tried to understand the transition of the Late Bronze and the early Iron Age at the site, B. Mazar (Maisler 1951), Anati (1962) and Van Beek (1951).

The date of the initial settlement has been addressed by several archaeologists and historians as well (Fig. 2). Balensi dates the original settlement of the site to as early as the 16th century BCE, in a not completely identifiable Level VI (Balensi 1985), Gershuni seems uncertain, although she favors an early settlement (Gershuni 1981). Hamilton and Anati placed it at the end of the 15th, or the very first years of the 14th century BCE and Maisler (B. Mazar) places the original habitation to as late as ca. 1300 BCE as an Egyptian Naval Base (Maisler), which was in turn negated by Weinstein (1980).

The 2001 Project

In 2001, with renewed public works in the area of Tell Abu Hawam, a salvage project centering on the northeastern outskirts of the site was carried out by the Recanati Institute for Maritime Studies at the University of Haifa and the Israel Antiquities

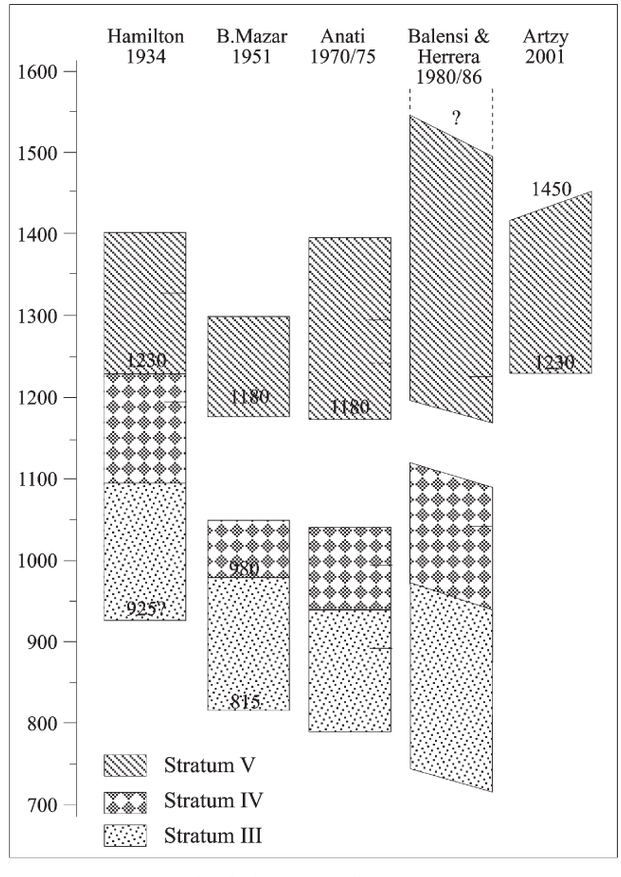


Fig. 2 Chronological Chart

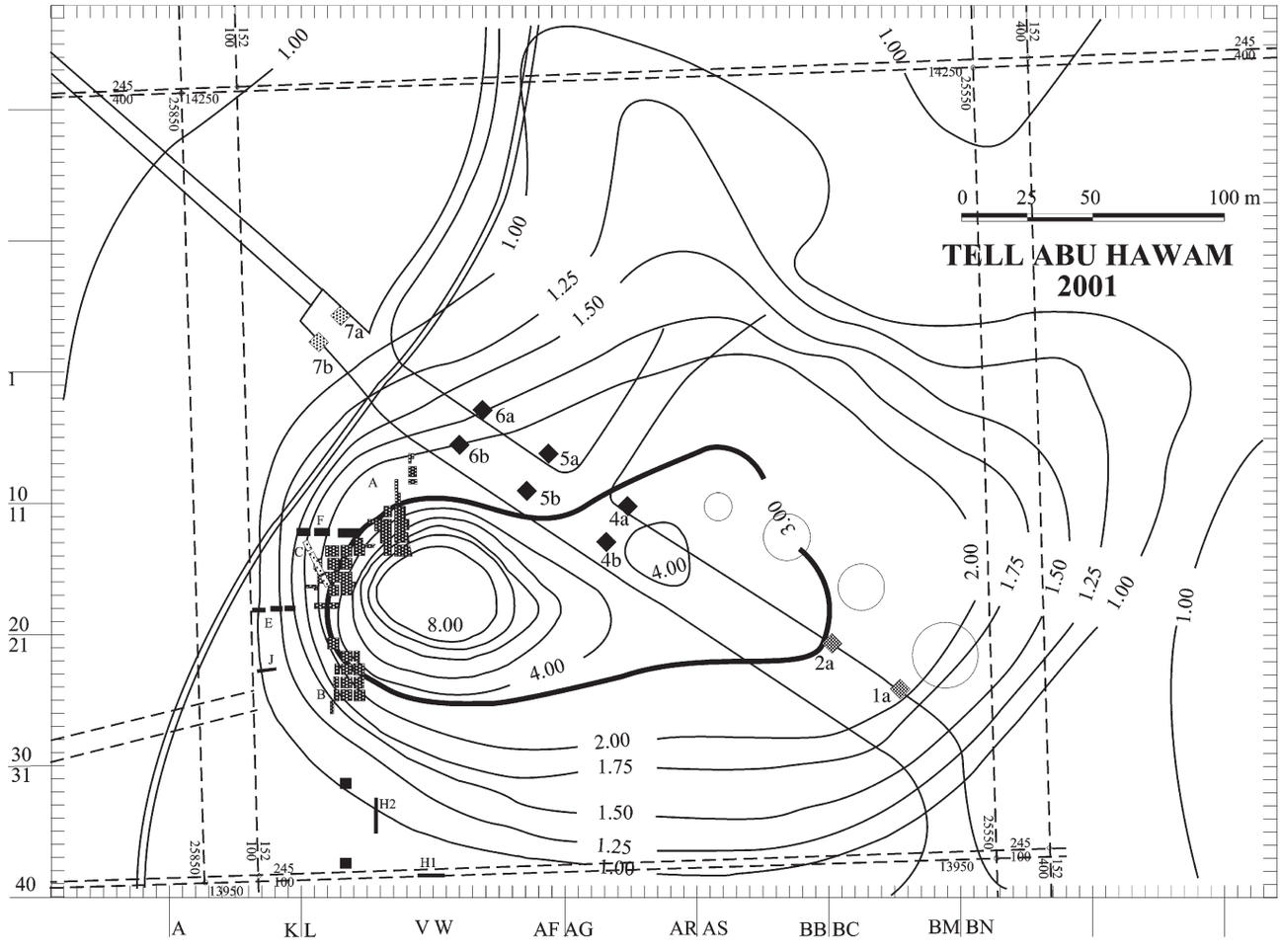


Fig. 3 Map of Excavated areas (Yossi Salmon)

Authority under the direction of M. Artzy with the participation of S. Yankelelvich. U. Ad and A. Abu Hamid. Yossi Salmon surveyed and prepared the maps in agreement with the 1985–6 grids prepared for the Balensi excavations (Fig. 3). The project included a geomorphological study by E. Reinhardt and B. Goodman. This area was not excavated in any of the previous archaeological ventures and thus could contribute greatly to the further understanding of the geographical setting of the site. Balensi's concerted efforts to gain all the data available about the site, located a map prepared in the 1920's by Treidel. She compiled the data with later maps and her own ones and envisioned the area as being the 'lower city'. Her conclusions were based on the 1920 contours, which seemed to indicate such a possibility.

The proximity of the area excavated to the Qishon River and the Sea and a major geological fault necessitated an interdisciplinary project. In order to facilitate the archaeological task a few metal caissons were constructed to form 5 × 5 meter squares (Fig. 4). The caissons were placed, when possible, to the depth of ca. –3.00 below sea level. This made it possible to excavate the squares properly well below modern sea level. Since there was no way to have sections where the metal sheets were, a bulk was kept in the middle of the square. Thus, each square was divided into two rectangles. Not all of the squares contained stratified archaeological material, although they provided information about the ancient ecology of the site. Two squares (1A and 2A) showed Ottoman interference, probably related to works associated with the Qishon River. In others, 7A and 7B, no archaeological finds were noted. In square 4B, little or no archaeological material was found. It might note the small hill (+4) noted in Treidel's map. Square 4A posed a problem. It is situated in close proximity to modern construction and past sewage installations made it hard to place the caisson and thus to excavate properly. This was unfortunate since there were clear remains of ancient constructions, which seemed to be of the Late Bronze Age although their height above



Fig. 4 Metal Caisson in Sq. B5 (Shalom Yankelevich)

sea level is problematic. We reserve judgement until further study, especially ecological, of the area. Thus squares 5A, 5B, 6A and 6B supplied the archaeological, especially ceramic, information. The strata, which could be discerned belonged to two periods, Persian, Stratum II with two different levels of which very little undisturbed architecture was noted (square 6A) and Late Bronze II, Stratum V, in which the architectural remains were very limited. In square 5B architectural remains were found at the height of –0.50m. The stones, 60 × 60 × 45cm, were covered by mollusks, especially oysters which indicate that, at some time in the past, the area was covered by water, a body of seawater at ca –0.50 to –1.50



Fig. 5 Clay mask (Michal Artzy)

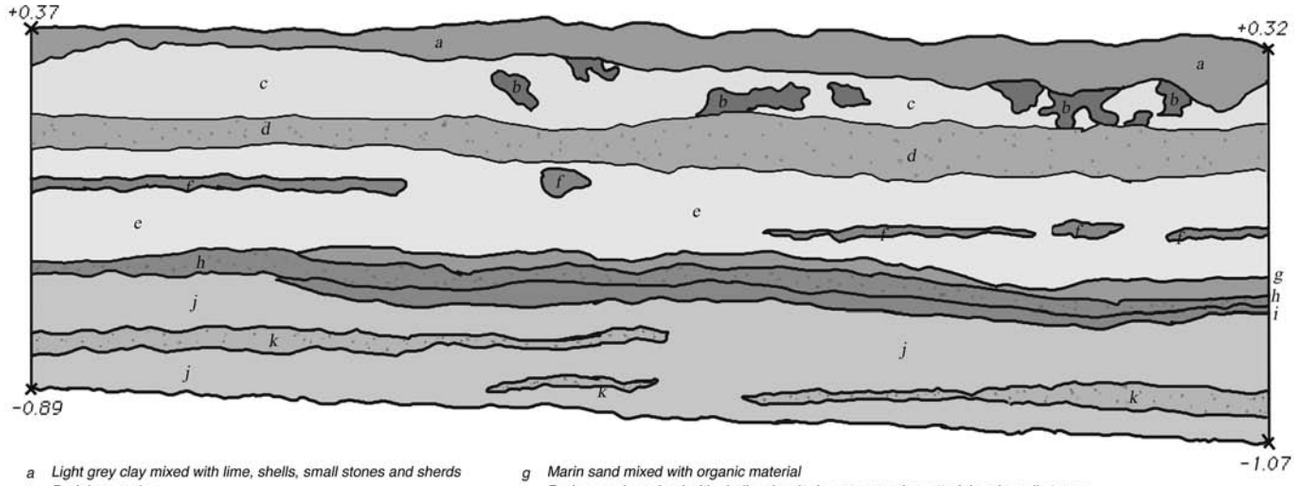


Fig. 6 Section of Sq. A5 (Amani Abu Hamid)



Fig. 7 Base Ring Shards



Fig. 8 White Slip Shards

meters. It is likely that this architectural remain was part of an anchorage installation. The archaeological layers are very rich and the material goods include oil lamps, pieces of tuyeres and a bellow as well as worn and corroded pieces of bronze and a mask the like of which was found in Hazor Area A (Fig. 5).

The distinction of the layers within the squares was based on the changes in soil substances attributable to river flooding, sand silting and human intervention (Fig. 6). One way to stabilize the area closest to the shore was by piling stones and heaving industrial waste such as murex shells from the purple industry (Baruch *et al.* 2006). Yet, despite the lack of clear architectural remains, there were unusual quantities of ceramic assemblages, as well as the faunal remains, all attributable by type to Hamilton's general Level V in 4 squares, namely 5A, 5B, 6A and to a lesser extent 6B.

A large percentage of the ceramics are imports although provenance studies have yet to be carried out. It is likely that a good percentage of the pieces were refuse, damaged wares, thrown from the boats during or following the arrival. When the boats were anchored and the goods were unloaded, damaged ceramics from either the journey or the treatment at the port were then thrown out and ended up in shallow water. It should be emphasized that very few shards showed any signs of water wear, probably because they ended in the silt soon after being disposed of.

Not surprisingly, although it should be emphasized, Cypriote imports are by far the majority of the foreign wares. They include a few samples of Base Ring I and numerous examples of Base Ring II (Fig. 7), White Slip II, mainly bowls, although a few shards of jugs were also noted (Fig. 8), Monochrome ware, White Shaved juglets and Red and White Lustrous pieces. There are also wall brackets, all likely from Cyprus. In addition there were numerous diagnostic shards of *pithoi* as well and although their provenience is still being tested, some are likely of Cypriote origin and possibly from other sites along

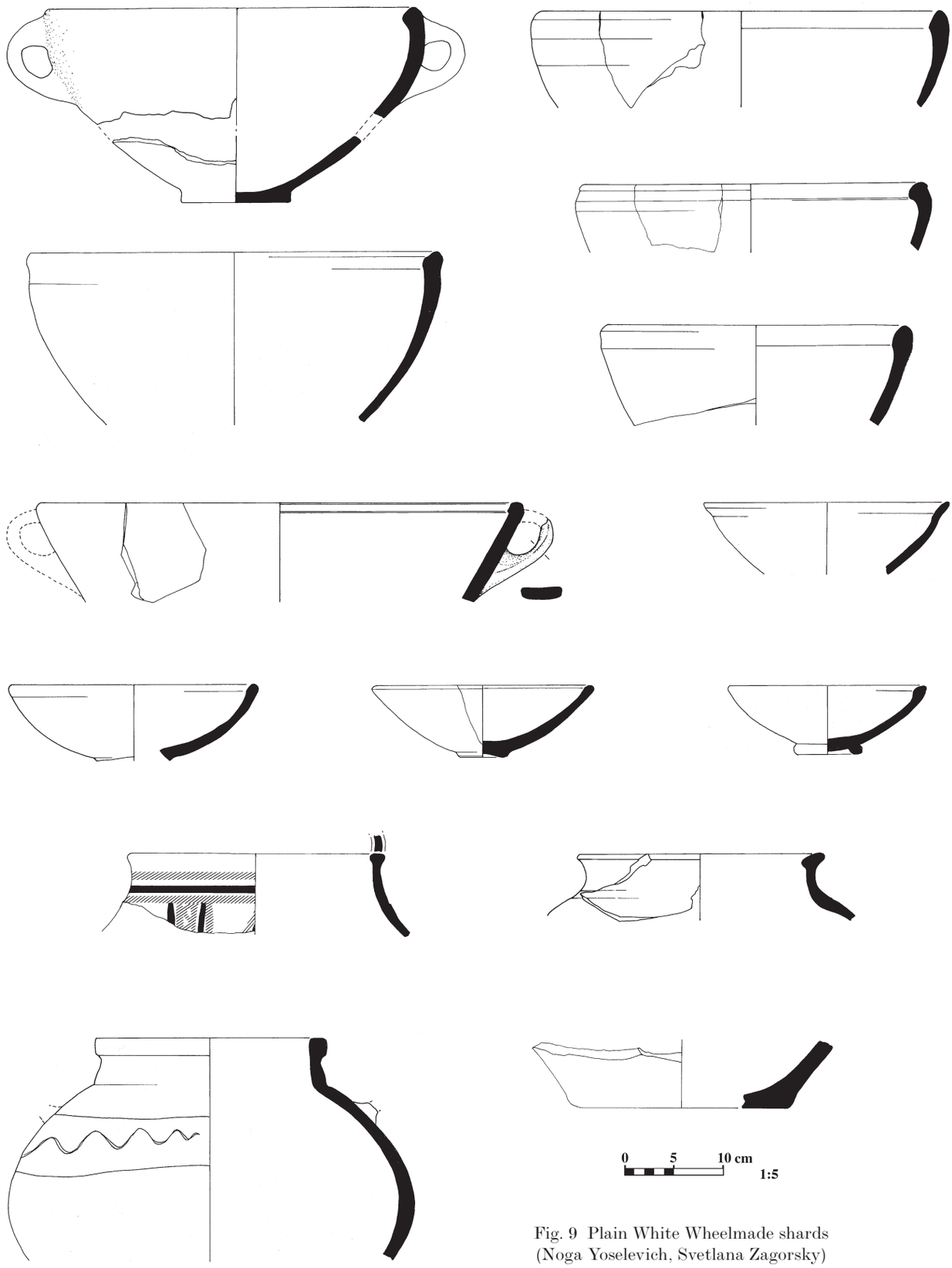


Fig. 9 Plain White Wheelmade shards (Noga Yoselevich, Svetlana Zagorsky)

the eastern Mediterranean. We have noted in the past (Artzy 1994) that *pithoi* were part of the usual ships' load as those on the Ulu Burun (Pulak 1997) and the Cape Iria shipwrecks (Lolos 1999). Other ceramic types are, by sight, Late Bronze Cypriote Plain White Wheelmade Ware (Fig. 9), although, the conclusions must await an extensive provenance study.

Another group of imports, by far a smaller one is the Mycenaean Ware. While no comprehensive statistical study has been carried out by the time of the presentation, a guess might place the ratio between the two groups (Cypriote and Mycenaean) at 40:1, or even higher. The majority of the Mycenaean shards compare well with those from Hamilton's excavations, which have been analyzed in the past at the Lawrence Berkeley Laboratory (Asaro and Perlman 1973; French *et al*. 1993). They are of the Mycenaean IIIA2/B type and said to have originated in the Greek mainland, mainly the northeastern Peloponnese. There are some Minoan stirrup jars and parts of Oatmeal ware, which could be compared to sites in Crete.

Among the imports is a very small group of Egyptian wares, including, however, a storage jar handle bearing a cartouche of Ramses II (Fig. 10). Several pieces of Anatolian Grey Ware have also been noted, among which is an almost complete krater (Fig. 11). There is a group of mostly handmade, black-grey and reddish shards belonging mostly to small craters or even cook ware, whose origin, at this juncture, can not be established by style only. The variety of cook ware is staggering and might indicate the varied origin of the ships frequenting the international harbor of Tell Abu Hawam.

Faunal remains include mollusks, among which are numerous crushed Murex shells as well as imported shells from Egypt (Baruch *et al*. 2006). Numerous fish and animal bones, as well as turtle shells, as befitting the marshy environment were found.

Our dating is necessarily based on the ceramics, which were locked by the geological layers. The excavation reached depths in which no signs of human activity were noted. The earliest ceramics should be dated at the earliest to the end of the 15th century BCE, likely post Tuthmosis III, or even later. The majority agrees well with the 14th century and the first half of the 13th century BCE (Fig. 2). Thus, for whatever reason, geo-political or geo-morphological, the anchorage ceased to exist. According to Hamilton's division, this is Level V and there are no remains, which could be attributed to his Stratum IVb. In this area there are no signs of Balensi's Stratum VI, Middle Bronze IIb and LB Ia. There are no White Painted Cypriote imports and what might be attributed to the Bichrome ware family is more likely of the Cypriote Wheel Made White Painted III Ware of the Late Bronze age. The end of the activity was sometime in the 13th century BCE and the subsequent human activity can only be dated to Hamilton's and Balensi's Level II, the Persian period.

Conclusions

The finds from the 2001 excavation could not substantiate the assumptions that a lower city existed in the northwestern extant of the site during the Late Bronze Age and thus the map drawn by Treidel in the 1920's was done following Ottoman works in the



Fig. 10 Cartouche of Ramses II (Michal Artzy)

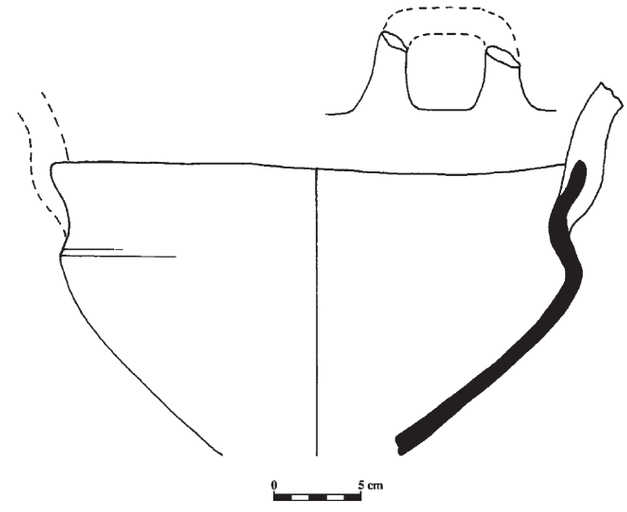


Fig. 11 Drawing of Anatolian Grey Ware Krater (Noga Yoselevich)

area and the lay of the land was not necessarily that of the more ancient past. Most of the area was at times covered by water in a part of the period and the changes in the relationship of coast and sea were most pronounced. From time to time sand covered the area followed by inundations of the river. The likely explanation as for the usage of the area in antiquity, especially that of the Late Bronze Age, is that of an anchorage or even harbor which served the inhabitants during a specific period, now assumed to have been the heyday of the site. There have been changes in the route of the Qishon River over the centuries. Avnimelech (1959) for instance, suggested that the river flowed northwest of the site. Balensi addressed the possibility that the site was an island. Renewed geomorphological studies would hopefully establish the ancient ecology.

The new data from the 2001 salvage excavation can be related to only a segment of the site's chronology. These two include the major part of Stratum V, which is assigned to the Late Bronze Age and a part of Level II, two levels which are assigned to the Persian period. The lack of any remains dated to either Level IV and Level III (Hamilton's, Balensi and Herrera), is of interest. Obviously, the human activity assigned to Stratum II overlapped that of Level V with a complete absence of the intermediate periods, namely the last of the Late Bronze Age IIb, Iron I and Iron II. This left the Iron age site to have been limited in size, although its extent to the east is still not established. The ceramics agree well with those from the site excavated by Hamilton. As to the earliest habitation, the finds do not include any Bronze Age White Painted Ware from Cyprus and no Cypriote Bichrome Ware, although one painted piece could, to the untrained eye, seem like a member of the family, but is more likely of the Cypriote Late Bronze Wheelmade White Painted III family. No other styles which one might have assumed to belong to the end of the Middle Bronze II and Late Bronze I were noted. The latest pieces, chronologically, could well be attributed to the middle of the 13th century BCE and not much later. Again, certain transitional ceramics of the 13th–12th centuries BCE, which are to be attributed to that period seem, at this juncture, to be completely absent from the 2001 excavation.

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Syrian Trade Routes of the Mari Age and MB II Hazor

Frans van Koppen[*]

The celebrated appearance of the city of Hazor and its king Ibni-Adad in the Mari archive has stimulated the idea that the kingdom was an important political power at that time. This assumption has been applied to the interpretation of archaeological data.

In the early days of the excavations at Hazor, it was used for the purpose of dating, as the Mari evidence was accepted as a synchronism between Mesopotamian chronology and a particular phase in the history of that site. For the excavator of Hazor, Y. Yadin, this could only have been the age when the city had become the most prominent power in the region, in his view the phase when also the lower town was occupied (*e.g.* Yadin 1972: 2–6). According to this interpretation, the Mari evidence offers a *terminus ante quem* for the establishment of the greater, fortified city of Hazor (upper city stratum XVII, lower city stratum 4). Yadin remarked on the implications of this position for the correlation between the archaeology-based chronology of the MB II Levant and the astronomy-based chronology of Babylon (Yadin 1972: 107–108), and the implications of his view were elaborated by A. Malamat (1992).

Many archaeologists have been reluctant to apply this premise for chronological purposes. The reasons for this are the persistent lack of consensus about absolute dates for Mesopotamian chronology of the second millennium BC (*e.g.* Dever 1992: 10–11; Ilan 1996: 244) and, more importantly, the fact that the correlation of the Mari references with the large stratum 4–3 city of Hazor has never been proven (*e.g.* Ilan 1996: 244; Maeir 1997: 321).

Recently the assumption of the political importance of Hazor on the basis of the textual evidence from Mari was used once again for archaeological interpretation, this time to argue that a regional pattern of material culture allows to recognize the area under political control of Hazor (Maeir 1997, 2000).

It is the purpose of this paper to re-evaluate the basic assumption underlying these interpretations – that the Mari sources are evidence for Hazor's being an important political power at that time – by an examination of the political relations and historical events in the greater Syrian area of the Mari age. In the course of this discussion, an alternative interpretation for the observed regional culture will be proposed.

The Mari palace archive

The kingdom of Mari was situated at the crossroads between Syria, northern Mesopotamia and the southern alluvial plain and maintained diplomatic contact with partners from all these regions. This makes the palace archive an important source for the history of much of the ancient Near East in the early second millennium BC, even though it allows for little historical depth: the available texts represent those parts of the personal archive of king Zimri-Lim that were considered not relevant enough to be transported to Babylon when Hammurabi had conquered the city. The archive covers the thirteen years of the reign of Zimri-Lim and includes also a selection of records from the time of his predecessors, Samsi-Addu of Ekallatum and his son Yasmah-Addu, the viceroy of Mari, mostly dating to the last years of their rule. In this way the archive offers dense, but incomplete documentation for a wide geographical horizon during a period of about twenty years (c. 1718–1698 BC).[1]

The royal archive of Mari is less helpful in providing information about the polities of the Levant, for this was the area of influence of the powerful kingdoms of Aleppo and Qatna, where Mariote embassies as a rule were not established, while the potentially rel-

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[1] Absolute dates in this paper are given according to the Low Chronology; for a justification see below. The date of events that cannot be placed with precision in the relative Mesopotamian chronology (the beginning and end of the reign of Yahdun-Lim; the date of conquest of Mari by Samsi-Addu) is qualified accordingly; with the exception of the beginning of the reign of Yahdun-Lim, the chronological leeway is only a few years.

evant letters of the kings of Aleppo and Qatna have not survived (Durand 1999: 153–154). The information about Hazor in the archive is therefore limited to references to the traffic of gifts and messengers; no letters sent from Hazor or devoted to events pertaining to that city can be expected in the archive, or have for that matter been found. Apart from a single attestation from the time of Samsi-Addu, Hazor appears only in records from the time of Zimri-Lim, but as the relevant text categories are nonexistent for the preceding reign, it is not clear whether this fact is at all meaningful. The Mari archive thus shows that Hazor partook in the long-distance diplomatic exchange, but the available sources are inadequate to understand the relations of this kingdom with other powers in the region or to appreciate historical developments.

The reigns of Yahdun-Lim (c. 1744–c. 1730 BC) and Samsi-Addu (c. 1728–1711 BC)

The fragment of an Old Babylonian letter discovered in 1996 in an unstratified context at the upper city of Hazor (Horowitz & Wasserman 2000) most fortunately complements this picture. The names of the sender and addressee of this letter are lost, but it is clear that one person of superior status is addressing several subordinates. The largest part of the letter is taken up by a long list of commodities in large quantities: mostly textiles, but also objects made of precious metals and weapons. In a short narrative passage before the break at the end of the text, the sender announces that he will proceed to Ekallatum once he has arrived in Mari and orders the addressees to fulfil his commands. W. Horowitz and N. Wasserman presented their historical interpretation of this document at the Rencontre Assyriologique of 2000, where they persuasively argued that the episode referred to in this letter is most likely to be identified with the formal creation of an alliance between the royal houses of Qatna and Ekallatum through a marriage of Yasmah-Addu with a daughter of Išhi-Addu of Qatna, an event that is known to have occurred in the eponym year of Ikkupiya (1717/1716 BC), seven years before the end of the reign of Samsi-Addu (Horowitz & Wasserman 2004; Lerouxel 2002: 461). They identify the sender as Išhi-Addu and the multiple addressees as the king of Hazor together with his vassals; however, it is equally possible to understand the second person plural as the joint vassals of the king of Qatna, which would make the Hazor document just one specimen of a letter that was sent in multiple copies to all vassals of Išhi-Addu, instructing them to contribute to the compilation of his huge dowry and to safeguard its transport to Mari.

However the lost beginning of the letter is reconstructed,[2] the presence of the tablet at Hazor shows beyond reasonable doubt that this kingdom was at that time counted among the vassals of the king of Qatna. This observation raises a number of questions, such as whether Hazor – situated in the northern Jordan valley – formed the southern limit of Qatna's sphere of influence, and whether its recognition in this area was an ephemeral phenomenon or perhaps of a more permanent nature, but these questions cannot yet be answered for lack of relevant sources, especially of an earlier date. What seems clear, however, is that the Hazor letter shows the kingdom of Qatna at the peak of its power, because the twenty-year period of dense documentation from Mari which starts around the same time allows, to some extent, to observe the decline of Qatna's influence in the South over the following years. At that time Hazor first appears in the Mari sources, and it will be our concern to discuss whether these two facts may be connected.

The history of western Syria in the Old Babylonian period is determined by the continuous rivalry between the kingdoms of Qatna and Aleppo, the latter gradually expanding its influence at the cost of the former (Klengel 2000; van Koppen in print). The kingdom of Yamhad, with Aleppo as its capital, encompassed the wide northern plains with fertile soils, sufficient rainfall and open routes of communication. Qatna also held sizeable, though less extensive, arable domains and controlled a key junction of north-south and east-west routes, where the main overland trade route, connecting the southern Levant with the North,

[2] Given the fragmentary condition of the Hazor letter, other reconstructions of the lost address formula and different interpretations of its historical background are possible. Another view has been put forward by Charpin and Ziegler 2004, who argue that it was a letter of Samsi-Addu, written at the time of his conquest of Mari with the object of intimidating the western kingdoms and extracting a huge payoff to prevent his advance to the shores of the Mediterranean Sea. I do not follow this interpretation, because the exhortations to prevent harm and perform properly (lines 21' and 26') are consistent with a set relationship between a superior and his subordinates, rather than a message between an aggressor and his opponents. The detailed list of required goods also argues for an established tributary relationship between the correspondents. Perhaps *qātum kašādum* with Mari as the object (line 22') does in this case not imply the nuance of conquest, since *qātum* (and *rittum*) as the subject of the verb can express the execution of control in a wider range of meanings (see the dictionaries s.v.). This manner of speaking does not surprise in a high-toned public statement of a superior ruler informing his followers about his foreign exploits.

and a direct access way to the Mediterranean coast met (Astour 1995; Joannès 1996). Mountains and deserts block east-west circulation almost everywhere in the Near East, but a route along the edge of the desert, passing through the oasis of Palmyra, allowed direct passage from Qatna to the middle Euphrates (Joannès 1997). The one other major route for the flow of goods between the Mediterranean Sea and the Mesopotamian plain led from Ugarit on the northern coast to the port of Emar on the Euphrates, passing all the way through territories under the control of Aleppo.

Mari and Qatna had a shared interest in the flow of Levantine-Mesopotamian trade by way of the Palmyra route, and these commercial interests help to explain why Mari was routinely allied with Qatna and from time to time in conflict with Aleppo. This political alignment was already in place in the time of Yahdun-Lim, the first powerful king of Mari and a predecessor of Samsi-Addu: a new source treats a joint operation of Yahdun-Lim, Amud-pi-El, the first known king of Qatna, and a third ruler against a city under the control of Aleppo (Heimpel 2003: 312). The famous campaign of Yahdun-Lim to the Mediterranean Sea and the cedar mountains (Frayne 1990: 605–606), an event for which so far no political motive could be discerned, can now be understood as a military campaign undertaken with, and on behalf of his ally, the king of Qatna, who seems to have exercised control over the rulers of the coastal region afterwards (see below). Yahdun-Lim's inscription recounts this feat as a solo action, in line with genre conventions and comparable with a later inscription of Samsi-Addu, who relates his aid to Qatna in words similar to those of Yahdun-Lim (Grayson 1987: 50). The analogy with the Syrian policy of Samsi-Addu may even suggest that the common enemy of Yahdun-Lim and Qatna during this campaign was no other that the king of Aleppo; fact is that, according to Yahdun-Lim's inscription, this king responded by instigating a rebellion of Mari's own vassals. Subsequently the king of Mari erected the fortress of Dur-Yahdun-Lim at, or near, Deir ez-Zor in order to protect the strategic point where the Palmyra route met the Euphrates valley (Durand 1997: 119–120).

The dynastic change following the removal of Yahdun-Lim's family from the throne of Mari did not affect the established alliance with Qatna, for Samsi-Addu of Ekallatum, the new ruler over Mari, and Išhi-Addu, the king of Qatna who had, presumably, succeeded Amud-pi-El, concluded their pact in the eponym year of Ikuppiya, shortly after Samsi-Addu's son Yasmah-Addu had taken up residence in the palace of Mari. It is well possible that the single reference to Hazor from the time prior to the reign of Zimri-Lim can be dated to this year, or thereabouts. It is a short letter in which Samsi-Addu instructs his son to entrust the messengers of the king of Hazor and of "four kings of Amurrum" to a messenger from Qatna, who will escort them back to Išhi-Addu (Bonechi 1992: 10). This letter was sent together with a delegation that left the court of Samsi-Addu to return home, and it is probable that the rulers mentioned in it considered Išhi-Addu as their superior. That the king of Hazor himself did so is revealed by the Hazor letter, but the political status of the "kings of Amurrum" is otherwise unknown. Amurrum was an important entity in Syrian geography, as is clear from a Mari letter where the trio of "the land of Yamhad, the land of Qatna and the land of Amurrum" stands for all of western Syria; Amurrum referred in later times to the area along the central Levantine coast, approximately between Byblos and Ugarit, including the Jebel Ansariyah in the East (Singer 1991); it is almost certain that this geographic delimitation is also valid for the time of Samsi-Addu. The Mari archive is almost silent about this region, but its position, directly west of Qatna, and the fact that it appears together with Hazor in the aforementioned letter may imply that its kings were counted among the vassals of Qatna as well. If so, then the visit should be seen in the context of the blossoming relationship between Išhi-Addu and Samsi-Addu, with the king of Qatna honouring his ally by arranging a delegation of faraway nations to appear at his court.[3]

In the course of the following years, troops of Samsi-Addu were called upon according to the terms of the mutual pact in order to help Išhi-Addu fight an uprising of cities in the South of his kingdom. The letters of Samsi-Addu's generals to their lord offer a unique view on southern Syria in the early second millennium BC, but remain largely unpublished; we rely on a recent historical synthesis that incorporates their evidence (Charpin & Ziegler 2003: 101–102, 124–125; see also Charpin 1999). These troops were based at the city of Qadeš and sent against various cities in the Beqaᶜa valley and, some years later, against cities in the land of Apum, the region around the oasis of Damascus. It would be premature to speculate about the background of this conflict with most of the evidence inaccessible, but it is worthy of note that Charpin and

[3] A possible ideological motive for this visit was Samsi-Addu's ambition to find recognition at the "four corners" of the known world, imitating a paradigm of Sargonic kingship (Eidem & Højlund 1997).

Ziegler 2003: 101 think that the revolt of the southern cities was originally supported by Aleppo. After a number of years Samsi-Addu's troops returned home. This happened after an episode of intensive diplomatic traffic between the courts of Samsi-Addu, Qatna and Aleppo (Charpin and Ziegler 2003: 124), which may suggest that a diplomatic reconciliation between Samsi-Addu and Yarim-Lim, the new king of Aleppo, had now taken place that brought Mari's military support to Qatna to an end. Whether this implies that permanent control over the southern Syrian cities ultimately surpassed Qatna's capacities cannot be substantiated without adequate sources of a later date.

The reign of Zimri-Lim (1710–1698 BC)

At the time of Samsi-Addu's successor Zimri-Lim, Syria was experiencing a period of tranquillity. The age-old hostility between Qatna and Aleppo had been decided in favour of the latter, and Amud-pi-El II, the new king of Qatna, was one among many to acknowledge the leadership of Yarim-Lim of Aleppo, a kingdom that was now celebrated as the most powerful of the Amorite states. In the words of a Mari official: "There is no king who is strong enough by himself: ten or fifteen kings follow Hammurabi, the ruler of Babylon; just as many Rim-Sin, the ruler of Larsa; just as many Ibal-pi-El, the ruler of Ešnunna; just as many Amud-pi-El, the ruler of Qatna; (but) twenty kings follow Yarim-Lim, the ruler of Yamhad". The Amorite world was, according to this text, carved up in six zones of influence,[4] five of which were headed by kings of more or less equal power, but with a sixth, the kingdom of Aleppo, in a class of its own. This is surely an idealized view, because there were in fact only two truly decisive powers at that time: the eastern kings acknowledged the supremacy of the mighty king of Elam in southwestern Iran (Charpin and Durand 1991), whereas the western kings felt much the same towards the king of Aleppo.

The rise of Aleppo had deeply transformed the balance of power in Syria. Zimri-Lim owed his throne to the support of Aleppo and took care to act in its interests, but also maintained diplomatic contacts with Qatna, as this kingdom was now reconciled with Aleppo. Qatna accepted, at least in theory, the superiority of Aleppo, but its behaviour at the time of the next major crisis, when Elam invaded Mesopotamia in Zimri-Lim's tenth year (Charpin and Ziegler 2003: 222), suggests that its aspirations to power had not yet subsided.

One consequence of the political changes in Syria was that the overland trade routes running from the southern Levant and the Mediterranean coast to Qatna were now conveniently linked with Emar, Aleppo's port-of-trade on the Euphrates; traders' caravans from Hazor were now coming all the way up to Aleppo (Durand 1990: 63–64). One effect of this was that the Palmyra route, which had until then connected Qatna through the desert with Mari, lost its strategic importance; another result was that all the major trade routes between the Mediterranean Sea and the Mesopotamian plain fell under the direct control of Aleppo. This rearrangement of power over the Syrian trade routes secured the wealth and strength of the kingdom of Aleppo, explains Qatna's gradual decline of power and was certainly one of the factors why Mari, a kingdom that in the days of Zimri-Lim had already lost much of its former strategic and commercial importance (Durand 1990: 84–89), never recovered from its Babylonian defeat.

The opening up of a relatively safe and undemanding trade route between the southern Levant and Emar coincides with the proliferation of text types in the Mari palace archive in which references to Hazor can be found: accounts of gifts and expenses for visitors and letters announcing the arrival of messengers. Zimri-Lim first entered into diplomatic contact with Hazor in his third year, when a delegation from that city, together with many other foreign representatives, was received at his court for the celebration of the Ištar festival (Charpin & Ziegler 2003: 196). The contact between the two courts was rekindled in his seventh year, and from then on gifts were exchanged on a regular basis, with a noticeable intensification in the tenth year, when Zimri-Lim stayed with his court and army in Aleppo and Ugarit. The gift traffic was conducted by messenger caravans, but others travelled between the kingdoms as well, like a group of "Amorite singers" from Hazor who stayed for some time at the court of Mari.[5] Mari was not the only Mesopotamian kingdom to maintain contacts with the southern Levant; the mention of diplomatic traffic between Babylon and Hazor in the

[4] The writer omits his own lord, Zimri-Lim of Mari, from the list (Lafont 2001: 222 note 34).

[5] Or, perhaps, the other way round; their arrival in Mari is reported in Ozan 1997: 296–297 no. 143; the departure of, possibly, the same singers is the subject of an unpublished letter (Bonechi 1992: 16 note 21). It is not clear whether "Amorite singers" are performers of a certain type, or musicians from a specific place of origin (Ziegler 1999: 118 and 216).

letters indicates that a similar exchange of persons and goods took place between these kingdoms.

Hazor and the long-distance gift exchange during the reign of Zimri-Lim

The most important fact which can be learned about Hazor from the Mari archive is that its king, Ibni-Adad, exchanged messengers and gifts with Zimri-Lim. The donation of gifts from one royal court to another was an essential component of international communication; the relations between rulers were expressed in terms based on the family model, and the gift exchange between them followed the reciprocity pattern of household-level interactions. The continuing giving and receiving of presents fulfilled various functions: it expressed the respect of the participants for each other, confirmed their political solidarity and was a means of non-verbal communication, all of which was governed by an intricate, but largely elusive, code of behaviour (Lafont 2001: 306–312). In addition, long-distance gift exchange gave the contributors access to foreign luxury wares that marked their social distinction. Throughout the ancient Near East, exquisite foreign products were highly appreciated (for Mari see *e.g.* Guichard 1999), and the possession of such objects and the ability to maintain relations with far-away partners carried high prestige (Liverani 1990: 227). This allowed ruling elites to interact with their remote fellows to their mutual advantage, as the reception of foreign ambassadors, the employ of foreign specialists and the display and distribution of exotic luxury products increased their prestige in their communities and also abroad, when these products were passed on to recipients even further away from their original source.

Zimri-Lim exchanged gifts with kings of equal political status and his vassal kings, but also with kings who acknowledged others as their political superior: the important trade cities of northern Syria, for example, were vassals of Aleppo but maintained lively diplomatic and commercial connections with the palace of Mari. It would thus seem that political federations were no exclusive groups, and that diplomatic contact with a subordinate state was possible if the partner was on friendly terms with the superior ruler. This allowed Zimri-Lim to engage in diplomatic contacts with all parts of Syria and bring the received goods into circulation among his Mesopotamian partners, while in return products from Mesopotamia and eastern regions found their way to the Syrian courts through the hands of the king of Mari.

The evidence for gift exchange between Mari and Hazor is found in administrative texts that record the receipt and expenditure of commodities in the royal treasury (Bonechi 1992). Two groups of attestations can be distinguished. The first occur in a number of texts that all refer to the same event: an exchange of metal objects between the two royal houses, when Zimri-Lim, on his trip through the kingdom of Aleppo, sent tin to the king of Hazor and received in turn objects of silver and gold. One of the objects received at that occasion was some years later handed out again as a gift to a king of Karana. The other group consists of two tablets that summarize the annual gifts of non-metal items (textiles in particular) that were sent to, and received from Hazor over a period of several years (ZL 7–12); one text gives details of the transactions of the year when Zimri-Lim travelled to Aleppo and the following year (ARMT 23 541), the other deals with the earlier and later years of contact between the two royal houses (Bonechi 1992: 11–13). Both texts bring together expenditures and receipts on a single tablet, and it has been observed that this feature has no parallel elsewhere in the corpus about royal gifts (Durand 1999: 159; Lerouxel 2002: 445–446). While they resemble bureaucratic balanced accounts in this way, there is no indication for their purpose: the suggestion that they represent an act of accounting after the death of Ibni-Adad remains unproven (Lerouxel 2002: 446).

These records reveal that diplomatic communication between Mari and Hazor was of low intensity, and that Zimri-Lim always took the initiative by sending out his gift first, which was then reciprocated by an shipment of comparable goods. All incidents of diplomatic exchange can be situated in specific ceremonial contexts: the yearly gift of textiles is most likely to be understood in the setting of the Ištar festival, an important dynastic celebration for which the attendance of fellow rulers or their representatives was mandatory (Lerouxel 2002: 437); the preceding gift of Zimri-Lim may well have been part of the invitation. The singular instance of an exchange of gifts outside of this annual pattern is the donation of tin and the reciprocal gift of silver and gold from the time of Zimri-Lim's stay in the kingdom of Aleppo (Villard 1986). This motive for this trip remains rather opaque in the light of the available sources, but it may well have been part of some larger diplomatic and military gathering of the allies and vassals of Aleppo (Charpin and Ziegler 2003: 214–215); it is quite likely, but at this time not confirmed by other evidence, that a delegation from Hazor participated in this event as well.

It is generally assumed that the choice of gifts carried an inherent meaning, but these connotations largely escape us. Textiles and objects of precious metals were customary gifts, but a shipment of tin – the base material for bronze – may have carried particular

significance in the context of a military gathering; Zimri-Lim had received a large supply of this strategic commodity in previous years from Elam as a payoff for military aid, and distributed most of it again to his overlord, the king of Aleppo, and Aleppo's allies, among them the king of Hazor, during his trips through this kingdom (Joannès 1991). Worthy of note are also the chariot fittings which Zimri-Lim sent instead of the usual textiles as his annual gift for the king of Hazor in his eleventh year (Bonechi 1992: 13); the description of the gift received in return is broken, and the significance of these items remains unknown.

Conclusions

The Mari evidence shows Syria in the time of Zimri-Lim as a region without border restrictions, as allegiance to the all-powerful kingdom of Aleppo facilitated and regulated exchange between the local rulers. Hazor's participation in this gift exchange is hence no argument for its sovereignty.

In fact, the Mari sources do not allow a qualified judgment about the political or economic importance of this entity at that time; instead it reveals, to some degree, the views held by a Mesopotamian king about this distant place. Its remoteness lent particular prestige to the messengers and gifts coming from Hazor, and Zimri-Lim no doubt appreciated and put effort in their participation in the yearly Ištar festival in his palace at Mari. Aside from these occasions, it seems that he encountered an embassy from Hazor only at one other occasion, during an international event hosted by the mighty king of Aleppo. If this suggestion can be substantiated, then it would confirm that Hazor in the days of Zimri-Lim acknowledged the superiority of Aleppo; if so, then the attendance of its ambassadors at the Ištar festival at Mari may likewise have been brought about by Zimri-Lim's sponsor, the king of Aleppo. But whatever the status of Ibni-Adad of Hazor at that time may have been – autonomous ruler or follower of the kings of Qatna and Aleppo – it is clear that he entertained good relations with these kingdoms, as this was a prerequisite for all communications via the land route passing through their territories.

Diplomatic contacts with this faraway partner were also a means to acquire much-appreciated exotic luxury products. Hazor was in this respect a particularly attractive partner, as it was situated on the northbound trade route for merchandise from the Arabian peninsula and especially Egypt. The annual gift for the king of Mari, for example, consisted of linen textiles "from Byblos" (*gublayû*), which were in all probability of Egyptian origin (Durand 1999: 159; Maeir 2000: 45). The role of Hazor as a trade emporium between Syria and the southern Levant is well established (Ilan 1995: 306–308; Maeir 2000), and this feature may have been a decisive factor why this city attracted the interest of Mari and Babylon.

Access to this far-away port-of-trade was always entirely dependent on the cooperation of the powerful kingdoms of Syria. It cannot be excluded that direct contact between Mesopotamian rulers and the king of Hazor was already taking place before the reign of Zimri-Lim, when the king of Hazor is known to be a vassal of Qatna, but the Mari archive does not contain the relevant text categories from that time that would allow to prove or disprove this possibility. Yet it can be expected that this was not the case, as the king of Qatna may well have preferred to employ the southern imports through Hazor for his own interests. Substantial textual evidence about long-distance gift exchange only becomes available for the following period, and then Hazor appears as an established member of the system. It is quite possible that its membership was of recent date and was connected with an expansion of diplomatic contacts and trade between Mesopotamia and the southern Levant at that time, encouraged by the new political order in Syria and the opening up of convenient trade routes. Mesopotamian rulers – Zimri-Lim of Mari and Hammurabi of Babylon – now entered into direct diplomatic contact and exchanged without intermediaries with Hazor; if this indeed was an innovation, then it can perhaps be attributed to the decline of Qatna's influence in the South, but textual evidence to prove this assumption is so far not available.

Archaeological implications

We will now return to the two applications of Mari evidence about Hazor to archaeological interpretation, as mentioned in the beginning of this paper. The first concerns the date of the expansion of Hazor: at first, only the upper town of this city was settled, but in the course of the MB II phase a sizeable and fortified lower town came into existence. There is no scholarly consensus about the date of the lower town's first settlement, or of the construction of the first fortifications: both happened, according to the traditional view, in the MB IIB phase (Yadin 1972: 201–206); according to another view, at least part of the lower town was already settled and fortified during the MB IIA phase (Herzog 1997: 120; Maeir 1997: 321). The Mari evidence is obviously irrelevant to this problem, as settlement size cannot be considered a factor for the participation in long-distance diplomatic exchange.

However, the Mari evidence is certainly relevant for the interpretation of a pottery assemblage from a large tomb in Hazor of the transitional phase MB IIA–B

(Maeir 1997). This assemblage is considered a prime example of a regional type of material culture, "which is virtually identical to that of south-eastern Syria (*i.e.* the Damascene basin)" (Maeir 2000: 38). A.M. Maeir has argued that this is the manifestation of a contemporary political sphere of influence, which he identifies as the domain of the kingdom of Hazor. He refers to the Mari evidence to support the theory that, during the MB IIA–B phase, the political control of Hazor extended well into southern Syria, perhaps as far north as Yabrud (Maeir 1997: 322–323; 2000).

The interpretation of the Mari evidence presented above excludes this view, as the kingdom of Qatna is known to have been in control of this region at the beginning of the two decades documented in the Mari sources; Qatna may have been in power already before this time, and perhaps afterwards as well, but the sources are silent in this respect. While Maeir may be right in his observation that the archaeological material from the MB II A–B period at Hazor, as well as nearby Tell Dan (Ilan 1996: 244) is characterized by long-lasting Syrian styles, more work is necessary for a definition of a regional style and the identification of its chronology and geography; as he remarked himself (Maeir 1997: 322), the archaeology of southern Syria remains poorly known. If a political explanation for this alleged regional pattern of material culture is permissible at all, then historical considerations favour its identification as the effect of a Syrian political centre in regions as far south as the northern Jordan valley, rather than the other way around: the date for the transitional phase MB II A–B on the basis of the Tell ed-Daba'a excavations (Bietak 1991) does not discourage the hypothesis that it may in fact be linked with the southern expansion of Qatna's realm of influence discussed in this paper, but new historical and archaeological evidence is necessary to substantiate this idea.

Appendix: a note on chronology

Mesopotamian history of the late third and early second millennium BC possesses a secure relative chronology (for the synchronisms of the Mari age see the table in Charpin & Ziegler 2003: 262), but the absolute chronology has been much discussed in recent years; many support significantly lower dates than the conventionally used Middle Chronology (Gasche *et al.* 1998), but arguments against this lower dating have also been put forward. These discussions have made it clear that the astronomical foundation of early Mesopotamian chronology is best left aside, and that no alternative tools are yet available to propose absolute dates. Absolute dates in this paper follow the Low Chronology (for the Middle Chronology, deduct 64 years), which seems closest to the known historical facts (but no margin of error can yet be given); they are to be seen as convenient tools for the benefit of interdisciplinary discussion only.

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A Collection of Egyptian and Egyptian-style Pottery at Beth Shean

Mario A.S. Martin

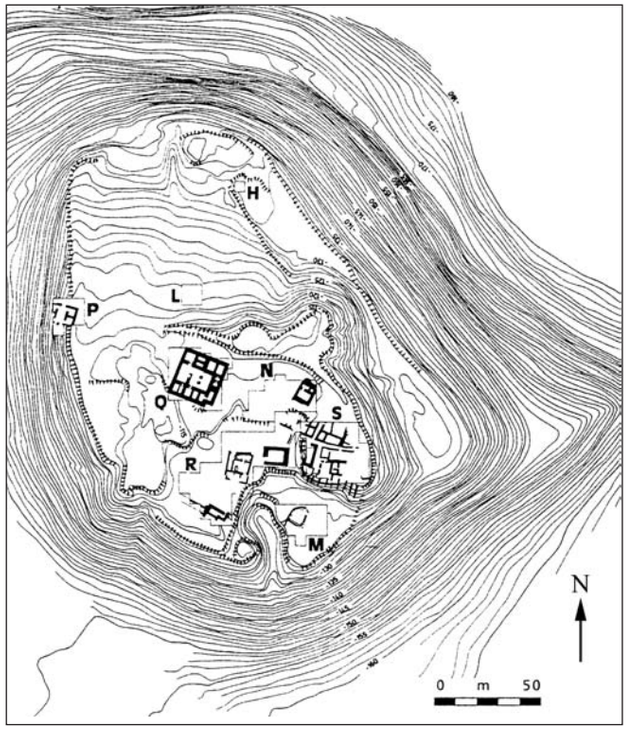


Fig. 1 Tel Beth Shean, the mound

1. Introduction

In the renewed excavations at Beth Shean, which were carried out by the Hebrew University of Jerusalem in co-operation with the Beth Shean Tourism Administration under the direction of Amihai Mazar between 1989–1995, Areas R, NA, NB, Q, and S produced well stratified Late Bronze Age assemblages (Fig. 1). In the here presented paper the author concentrates on the Egyptian material from Strata S-3 to S-5 in Area S, Strata N-3 and N-4 in Areas NA and NB, Strata Q-1 to Q-3 in Area Q and Levels VI (Lower), VII and VIII of the past excavations of the Pennsylvania Expedition (hereafter UME ["University Museum Expedition"]), all datable to a period between the Thirteenth (or late Fourteenth) to Twelfth centuries BCE (Fig. 2).[1] All these areas produced considerable amounts of locally produced Egyptian-style pottery beside the occasional Egyptian imports. The Egyptian and Egyptian-style pottery from Areas Q, S and NB is prepared for publication by the author (Martin forthcoming a–c).[2]

2. Typology

The Egyptian assemblage at Beth Shean is represented by a variety of types. Apart from handled cups and a two-handled storage jar (hereafter: "amphora")[3] all forms are exclusively made of local clays. The locally produced assemblage of Egyptian forms consists mainly of open shapes, namely simple bowls with straight or rounded sidewalls and a plain, everted or, occasionally, flanged rim (Fig. 3:1–5).[4] Bases are mostly flat and, like in Egypt, often string-cut. While in New Kingdom Egypt in the Nineteenth and Twentieth Dynasties round or rounded bases prevail over the flat ones, the prevalence of flat bases at Beth Shean is paralleled at other Canaanite sites with Egyptian-style assemblages such as Tel Sera[c] (Martin forthcoming d) and Tel Mor (Martin and Barako forthcoming). Like their Egyptian counterparts these bowls are uncoated, decorated with a red rim or red slipped on one or both sides (for the various decoration styles see, *e.g.*, Aston 1998: 75–81). Apart from simple bowls the assemblage of Egyptian forms includes large open bowls, which generally exhibit horizontal rows of rope impressions (Fig. 3:6). These

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[1] For preliminary reports of the renewed excavations, see, e.g., Mazar 1993a–b; 1997. For the results of the Pennsylvania expedition see mainly FitzGerald 1930; Rowe 1930; 1940; James 1966; James and McGovern 1993. For the previous excavations of the Hebrew University conducted by Yadin and Geva see Yadin and Geva 1986.

[2] I would like to thank Prof. Amihai Mazar, who invited me to publish his material and allowed me to include illustrations in this paper, and to Dr. David Aston, who patiently provided me with the tools to study Egyptian pottery. Finally, I am deeply indebted to Prof. Manfred Bietak, who always supported me on every step of my study.

[3] How these jars are termed in the Egyptological literature; e.g. Hope 1989: 86–117.

[4] For parallels to the various types in Egypt and at Egyptianized "garrison"-sites in Canaan see, *e.g.*, Martin forthcoming a–c; Martin and Barako forthcoming.

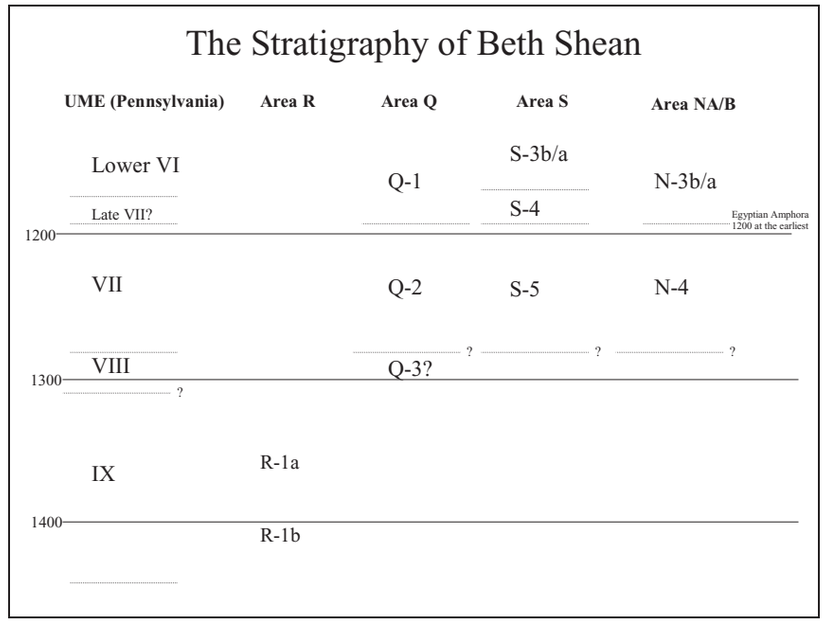


Fig. 2 Stratigraphy of the various excavation areas at Beth Shean

impressions also frequently appear on large bowls in Egypt. The ropes, which caused the impressions in the finished products, were intended to hold larger vessels together during the drying process (Arnold 1993: 91; Aston 1998: 110). Vessels with an industrial function in the textile manufacture are spinning bowls with two interior handles (Fig. 3:7). To the closed vessels belong beer jars – small tall jars with short necks (Fig. 3:8). The examples from Beth Shean show the same crude manufacture as their parallels in Egypt – ribbed sidewalls, string-cut bases with superfluous lumps of clay and, generally, more or less deep fingerprints in the area around the base, which are remains of lifting the vessel from the wheel without further attempt of surface treatment (Bourriau and Aston 1985: 34–35). Like in Egypt these jars are occasionally perforated at their bottom. Further closed forms include small ovoid to drop-shaped jars (Fig. 3:9a–b), funnel-necked jars (Fig. 3:10) and neck-less slender jars with rolled rim (Fig. 3:11). Apart from the beer jars, which are always undecorated, all these handleless jars are red slipped. While the forms mentioned up to now are always locally produced, small handled cups are exclusively imported from Egypt (Fig. 3:12). The analysed examples from Beth Shean appear either in a Marl D or, rather exceptionally for this type, in a Nile B.02 fabric (cf. "Vienna system": Nordström 1986: 629–634; "Tell el-Dab^{c}a system": Bietak 1991: 324ff.).[5] These cups are absent from room loci of the last Egyptianized level represented by Stratum S-3 in Area S, four rims from street loci possibly originate from the previous Stratum S-4. The trade connection to Egypt might have been cut off in the second half of the Twelfth century. Another imported type is a single example of an Egyptian amphora of Marl D (Fig. 3:13). The almost complete profile originates from Stratum N-4 in Area NA (Killebrew 1998: 162, Ill. III.23:2; for a chronological discussion see below).

3. Ware fabrics

As already noted above, the Egyptian assemblage from Beth Shean comprises mostly locally produced forms. These forms reproduce the characteristic Egyptian Nile silt types, namely, everyday household wares, while typical Egyptian marl clay forms, i.e. handled cups and the amphora, are imported from Egypt and not imitated locally. The same tendency is borne out at other sites, such as Tel Aphek (Martin, Gadot and Goren forthcoming), Tel Mor (Martin and Barako forthcoming) and Tel Sera^{c} (Martin forthcoming d). The following discussion of ware fabrics of the Egyptian forms at Beth Shean is largely based on petrographic analyses conducted by Cohen-

[5] One intact example from Stratum S-4 in Area S (Figure 3:12) could not be checked by the author but was identified as "*Nile B or E*" in a petrographic analysis (Cohen-Weinberger 1998: 409).

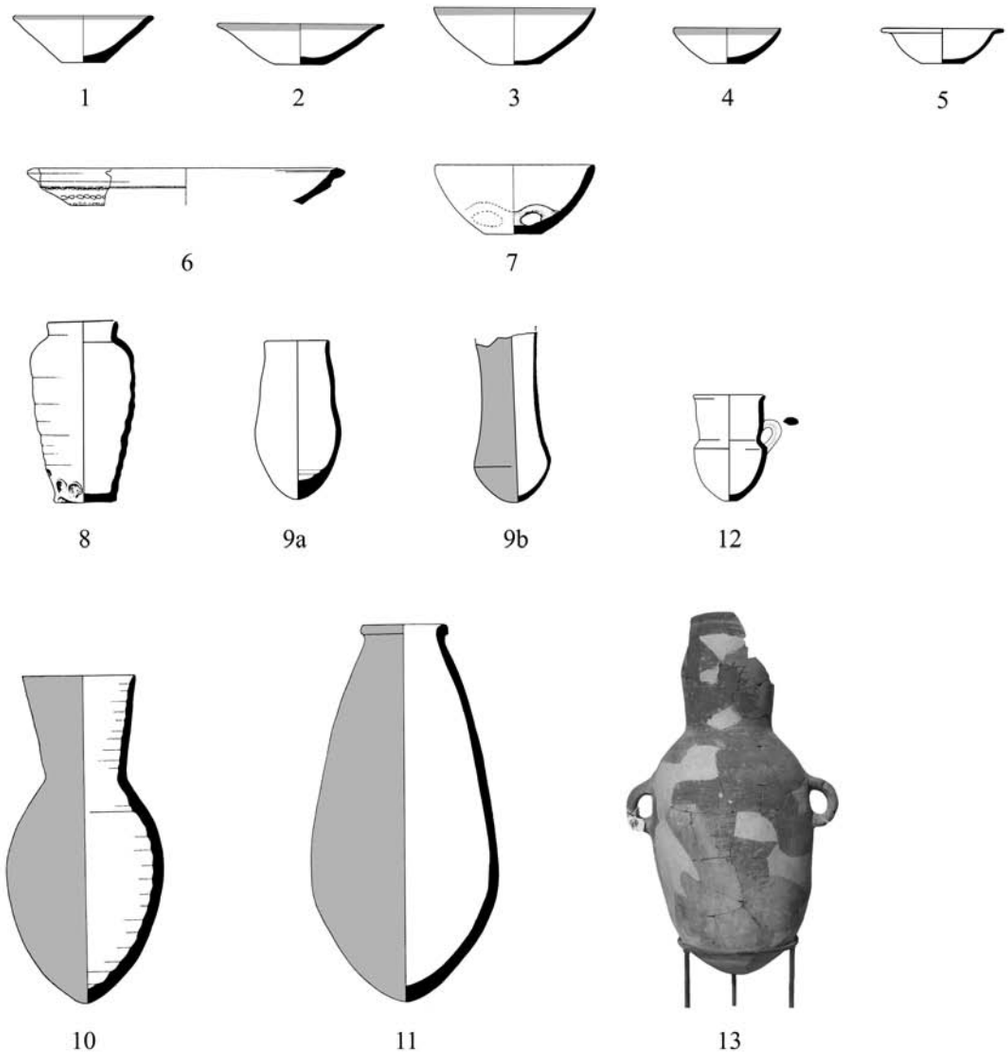


Fig. 3 Egyptian forms at Beth Shean: Simple bowls with straight walls and plain rim (1), everted rim (2), rounded walls and plain rim (3–4), and flanged (5) rim; large open bowls (6); spinning bowls (7); beer jars (8); small ovoid to drop-shaped jars (9a–b); funnel-necked jars (10); neck-less slender jars with rolled rim (11); imported handled cups (12); imported amphora (13); scale 1:10

Weinberger (1998) with additional remarks by the author. Most of the locally produced Egyptian forms at Beth Shean belong to a single fabric group termed the "Travertine family" by Cohen-Weinberger (1998: 409). Her results indicate that more than 95% of the Egyptian-style pottery can be attributed to this family. Of the local Canaanite pottery, only 75% belong to the "Travertine family" and the remaining 25% to various other petrographic classes. A preference for a single clay source is therefore attested for Egyptian forms. According to A. Cohen-Weinberger's petrographic analyses, the clay of this fabric is carbonatic and contains some silty quartz particles (1–2%). Non-plastic components consist of poorly sorted travertine particles of silt-to-sand grain size, some with a pisolithic appearance. The admixture of large amounts of chopped straw into the paste is common among Egyptian forms, mainly in red-rimmed Egyptian-style simple bowls, large open bowls, and beer jars, while the rarer Egyptian-style storage jars and red-slipped simple bowls generally contain smaller amounts of organic temper.[6] Considerable quantities of straw temper are also common in Canaanite bowls and kraters (but generally in small-

[6] The varying amounts of added temper attested for different types show no direct correspondence to the related Egyptian Nile silt types.

er amounts than in the Egyptian-style bowls and beer jars) but not in Canaanite closed forms. Straw temper is generally visible to the naked eye as elongated, burnt-out voids in the section and on the surface or, if not burnt out, as whitish-yellowish, rod-shaped fibres rather than the voids. Burnt-out organic inclusions result in a quite porous matrix (= "coarse ware"). The clay is often soft and somewhat brittle. The more thick-walled vessels, namely large bowls and beer jars, commonly exhibit a grey to black core of varying thickness, an indication that not all the organic temper was fully oxidized.

From a functional point of view, the addition of straw raises the plasticity of the clay to make it more easily workable on the wheel, allows for a faster and more even drying process, helps to reduce shrinkage during the drying process, and saves on raw materials. The porosity provided by the straw temper enables faster and shorter firing, thereby saving fuel. Temper allows for the freer penetration of hot gases through the vessel wall, leading to a better-fired product, and for the escape of steam from the vessel to prevent bursting (Arnold 1993: 105).

While at Beth Shean the admixture of organic temper can be traced back to at least the Fifteenth century (Stratum R-2 in Area R; however, still in very small amounts), when Egyptian-style pottery is still absent, it can be observed that this temper appears in gradually increasing amounts and in more vessels towards the Twelfth century, coinciding with the increase in Egyptian influence and an increased share of Egyptian forms.[7] The admixture of chopped straw and animal dung, especially in large amounts, is a characteristic property of Egyptian Nile clays (e.g., the Nile B, C, and E classes). Straw temper in large quantities is also common in Egyptian-style assemblages at other "Egyptianized" sites (e.g., Deir el-Balah [Gould forthcoming], Tel Sera^{c} [Martin forthcoming d], Tel Mor [Martin and Barako forthcoming], and Tel Aphek [Martin, Gadot and Goren forthcoming]). At Tel Aphek, a large Egyptian-style assemblage was found in the "Governor's residency" of Stratum X-12, roughly dated to the Thirteenth century. Massive amounts of straw temper appear in the Egyptian-style bowls, in contemporaneous Canaanite vessels straw temper is virtually absent. At Tel Mor straw temper is almost absent in strata preceding an increased Egyptian influence (XII–X) and appears in massive amounts in Egyptian-style vessels in Strata IX–V dating from the late Fourteenth to the Twelfth centuries. Corresponding to Beth Shean large amounts of straw temper are also attested in several Canaanite bowls and, especially, in kraters of Strata VI/V (late Thirteenth to Twelfth centuries, in which the Egyptian influence is most strongly attested), a phenomenon, which most probably can be explained as Egyptian influence in the local pottery manufacture of Beth Shean and Tel Mor respectively.

Furthermore, James and McGovern observe that pottery artefacts, including Egyptian and Canaanite pottery forms, from the combined assemblages of Levels VIII–VII were fired at a lower temperature (500–700°C) than wares from the previous Stratum IX[8] (1993: 245; see also Cohen-Weinberger 1998: 409). This decrease of firing temperature was also explained as general Egyptian influence on the pottery tradition at Beth Shean (James and McGovern 1993: 245). In Egypt, Nile silt pottery was also fired at a low temperature (600–800°C). The finer Egyptian marl pottery, on the other hand, was fired at temperatures of 800–1050°C and for a longer time (Aston 1998: 37). It was noted above that in Canaan it is namely the characteristic (lower fired) Nile silt forms, which are locally reproduced. The author conducted no refiring studies. It can, however, be noted that at Tel Mor, Tel Aphek, as well as at Beth Shean in Strata S-5 to S-3 in many vessels large amounts of straw rods are not burnt out, suggesting a very low firing temperature for these vessels. Referring to Mackenzie (1957), Nordström and Bourriau note that in an oxidizing atmosphere the combustion of organic matter takes place at temperatures between 380–600°C (1993: 155).

Concluding it can be argued that above-described methods of clay preparation in conjunction with Egyptian forms and manufacturing techniques suggest a cultural background that goes beyond the purely functional, and can tentatively be interpreted as an imitation of Egyptian Nile clays, supporting the assumption that the potters at Beth Shean were Egyptians or, at the very least, under close Egyptian guid-

[7] Cf. also James and McGovern 1993: 245, who note an increased amount of organic inclusions in the Level VIII/VII vessels, explaining it as Egyptian influence on the local pottery tradition.

[8] The material of Stratum IX was only preliminarily analysed by James and McGovern. As to firing temperatures it was, however, compared to the Late Bronze Age I material of the Beqah Valley [700–850°C] (McGovern 1986).

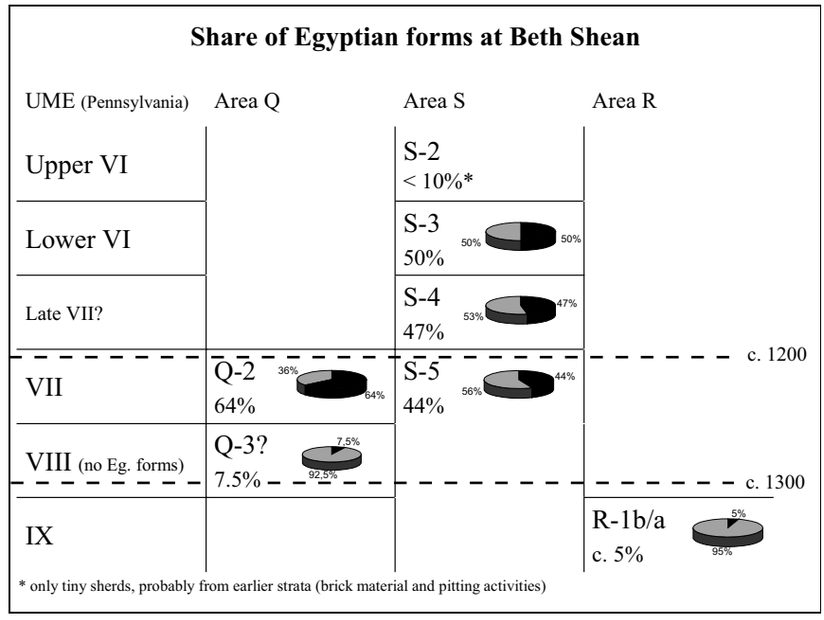


Fig. 4 Share of Egyptian and Egyptian-style vessels in Areas R, Q and S

ance (James and McGovern 1993: 244–245; Cohen-Weinberger 1998: 411; Killebrew 1998: 275).

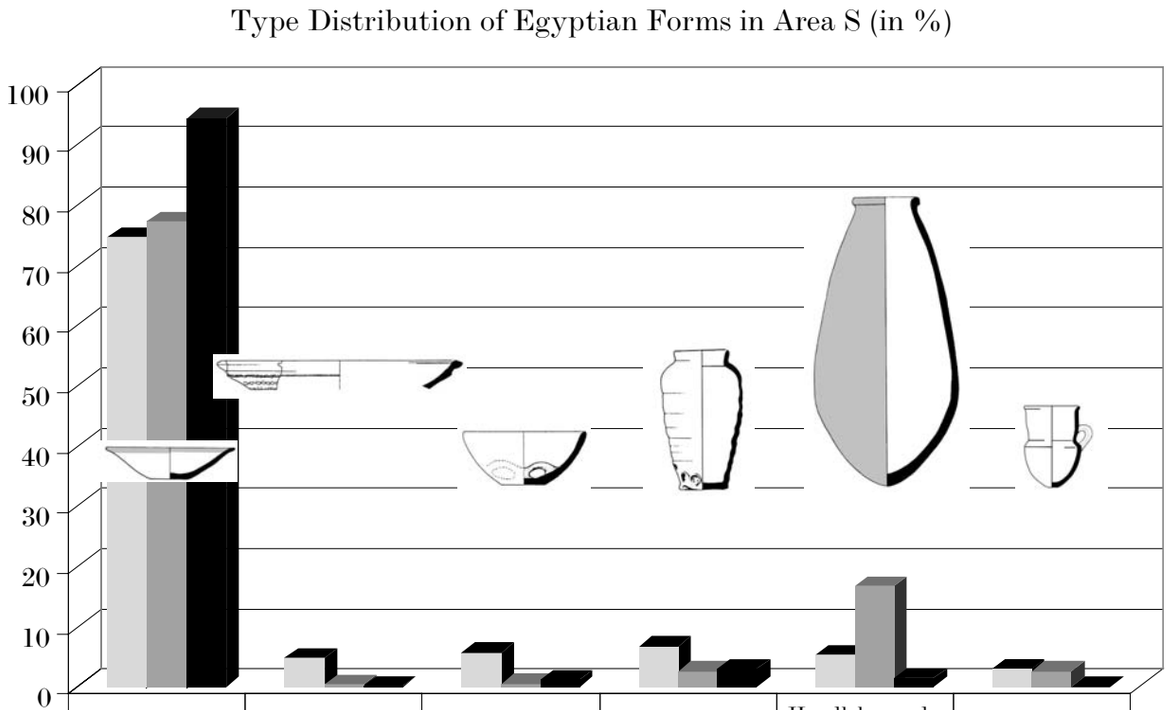
4. Statistics

A statistical analysis (Fig. 4) of the collections of Areas R (courtesy of B. Mullins), S and Q mainly reveals the strong increase of Egyptian presence in the Nineteenth and Twentieth Dynasties (Strata S-5 to S-3 in Area S and Stratum Q-2 in Area Q) as compared to the Eighteenth Dynasty (Strata R-1b and R-1a in Area R combined with the evidence of UME's excavations). The notable rarity of Egyptian forms in Stratum Q-3 in Area Q might already correlate this stratum to UME's Level VIII, from which Egyptian forms also seem to be almost absent (see mainly James and McGovern's Level VIII plates [1993: figs. 15–19, 31–32, 35, 53–57]; James and McGovern also note explicitly that Egyptian-style bowls were not found in this Level [1993: 79]; for possible chronological implications see below). The share of Egyptian forms does not decrease in the Twentieth Dynasty (represented by Strata S-4 and S-3 in Area S), which reflects Egypt's – mainly Ramses' III – final attempt to maintain its rule in Canaan. Note in this connection that in the first half of the Twelfth century the share of Egyptian and Egyptian-style pottery is higher than ever before at Tel Mor (Barako and Martin forthcoming) and Tel Sera^{c} (Martin forthcoming d). In Stratum S-2 in Area S at Beth Shean, the stratum after the end of the Egyptian domination in Canaan, Egyptian-style pottery almost disappears, the few remaining sherds being small and probably residual.

Fig. 5 shows the quantitative distribution among the various Egyptian types in Strata S-5 to S-3 in Area S. As already noted above, simple bowls form the vast majority of the Egyptian forms throughout all strata. Furthermore, the development of decoration styles of these simple bowls might be important for the correlation between the areas (presented below): Red slipped simple bowls are extremely common in Stratum S-5, while rare to absent in S-4 and S-3 (Fig. 6).

5. Character of the assemblages and ethnical implications

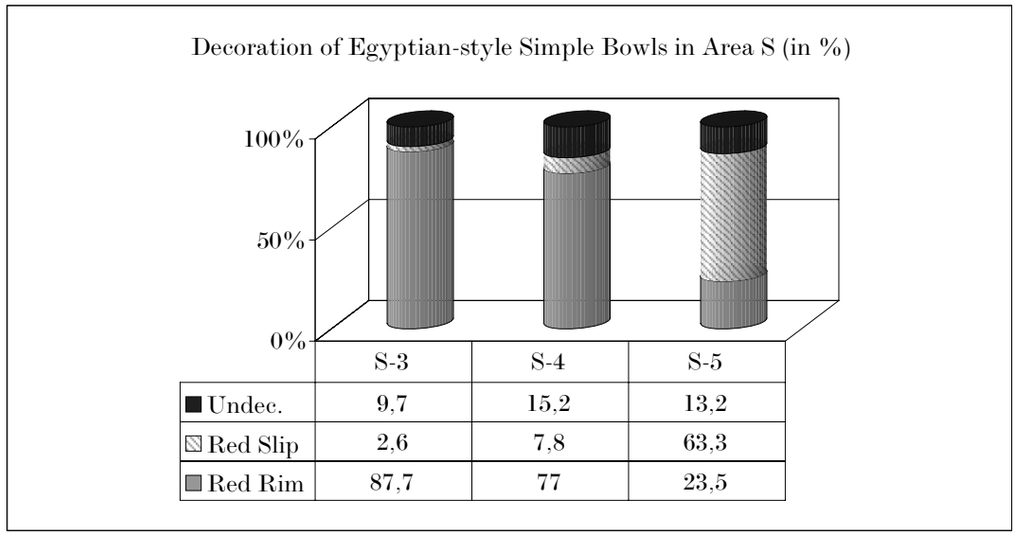
The here presented Egyptian collection as well as other evidence of Egyptian material culture leaves no doubt that there was a strong physical Egyptian presence at Beth Shean in the Thirteenth and Twelfth centuries BCE. The potters producing the Egyptian forms were most probably Egyptians or, at the least, under close Egyptian guidance, as already noted elsewhere by James and McGovern (1993: 245), Cohen-Weinberger (1998: 411) and Killebrew (1998: 275). This is indicated by distinct fabric properties, Egyptian technological characteristics, the fact that namely the coarse household wares are locally mass-produced, which are unlikely to be emulated by the local Canaanite pottery tradition, and the fact that apart from spinning bowls Egyptian-style pottery has vir-



	Simple Bowls	Large Bowls	Spinning Bowls	Beer Jars	Handleless red-slipped jars*	Handled Cups
Stratum S-5	74,6	4,7	5,7	6,7	5,3	3
Stratum S-4	77,2	0,4	0,5	2,6	16,7	2,6
Stratum S-3	94,4	0,1	1,1	3	1,4	0

* including small ovoid to drop-shaped jars, funnel-necked jars and neck-less slender storage jars with rolled rim(a specimen of the latter is illustrated exemplary)

Fig. 5 Type distribution within the corpus of Egyptian forms in Area S (in percent; number of included vessel profiles and rim sherds respectively: 1689)



	S-3	S-4	S-5
Undec.	9,7	15,2	13,2
Red Slip	2,6	7,8	63,3
Red Rim	87,7	77	23,5

Fig. 6 Decoration of Egyptian-style simple bowls in Area S at Beth Shean (in percent; number of included vessel profiles and rim sherds respectively: 1532)

tually disappeared by the later part of the Twelfth century (Stratum S-2 in Area S; cf. also the evidence at other Egyptianized sites, such as Tel Mor [Martin and Barako forthcoming] and Aphek [Martin, Gadot and Goren forthcoming]) coinciding with the end of the "Egyptian Empire" in Canaan. If Egyptian shapes would have been imitated or "emulated" by local Canaanite potters for Canaanites, one would not expect the production of these forms to cease so abruptly after the Egyptian retreat.

For a reconstruction of a cultural scenario it has to be noted primarily that Egyptian forms appear alongside Canaanite ones in virtually all loci. We therefore have to come to the conclusion that resident Egyptians beside their Egyptian forms also used Canaanite ones and, the other way around, that local inhabitants also used Egyptian forms. Note that Egyptian(ized) cooking pots at Egyptianized sites in Canaan in general are striking by their rare occurrence. The characteristic Canaanite cooking pots are *the* cooking vessel at all the examined sites (cf. also Martin and Barako forthcoming). It is interesting to compare this situation to the evidence at early Philistine sites, where "Aegean-style" cooking jugs largely supplant Canaanite cooking pots (Barako 2000: 523). Based on the argument that food preparation was primarily the domain of women in the ancient world (e.g. King and Stager 2001: 64–65; Holaubek 1992: 44; Watterson 1991: 128–134), it can be argued that, while women were part of the Sea Peoples' migration and settlement (Barako 2003), it must have been mainly men, who were sent to serve in an Egyptian garrison in Canaan. The theory that Egyptian soldiers and male administrators were living under the same roof in marriage with Canaanite women – hence the sole presence of Canaanite cooking pots – might be another appealing theory.

6. Correlation of the areas with the aid of the assemblage of Egyptian forms – Absolute Chronology

The correlation between the various areas is somewhat hampered by the fact that not all phases produced satisfying quantities of datable pottery. However, there is still enough evidence to provide the tools to elaborate such a correlation, namely royal scarabs, imported pottery, Egyptian-style pottery and several local pottery forms (Fig. 7; the correlation presented in the following was elaborated by A. Mazar with additional remarks by the author regarding the Egyptian forms): Late Mycenaean IIIB (being heirlooms, Myc. IIIA wares are of no relevance here) and Cypriote imports are attested in UME's Level VII (James and McGovern 1993: 103–124), Strata Q-2 in Area Q and N-4 in Area NA. Stratum Q-1 in Area Q equals building 1500 of the UME excavations (James 1966: 8–11 and fig. 77) and only represents the cleaning of this building. A published Mycenaean IIIC stirrup jar marks the first appearance of this ware in this stratum (James 1966: fig. 49:4). A considerable collection of Mycenaean IIIC sherds comes from Strata S-4 and S-3 in Area S (Myc. IIIB and Cypriote imports are no longer attested). Another single sherd of Mycenaean IIIC from an unreliable context might originate from Stratum N-3a in Area NA.[9] Based on the last appearance of Mycenaean IIIB and Cypriote imports a correlation between UME's Level VII, Stratum Q-2 in Area Q and N-4 in Area NA can therfore be suggested. This is corroborated by the fact that all these strata are still mainly LB IIB in character, datable to the Thirteenth and, probably, very early Twelfth centuries BCE (see below). According to the first appearance of Mycenaean IIIC pottery, Stratum Q-1 in Area Q has to be correlated to Strata S-4 and S-3 in Area S (note that Area S-strata are domestic [likely more short-lived], while the building in Area Q is monumental in character). From UME's Level Late VII, Stratum N-4 in Area NB and Stratum S-5 in Area S imports (be it Mycenaean IIIB and Cypriote or Mycenaean IIIC) are absent. As this absence may also be explained by the limited exposure of these strata, imports seem of no help for their correlation. We can confidently also attribute Stratum N-4 in Area NB to the last LB IIB horizon (UME's Level VII and its correlates), a correlation strengthened by local Canaanite pottery (a carinated bowl [a type absent in S-4 and S-3 of Area S] and a red slipped biconical krater with white painted registers clearly point to UME's Level VII). As to S-5 in Area S, based on imported wares one can only argue that this stratum cannot be earlier than Q-2, N-4 and UME's Level VII, as S-4 is already marked by the first appearance of Mycenaean IIIC pottery, which is absent from Q-2, N-4 and UME's Level VII (no gap is attested between S-5 and S-4).

For the correlation of S-5 and UME's Level Late VII respectively the development of decoration

[9] According to P. Mountjoy all Mycenaean IIIC wares at Beth Shean belong to Mycenaean IIIC:early 1 and 2 (personal communication; Second Euro-Conference SCIEM 2000, Vienna May/June 2003).

styles of Egyptian-style simple bowls might be of help. In Area S, the largest area with the by far largest ceramic assemblage, red-slipped Egyptian-style simple bowls (with plain or flaring rim) are very popular in Stratum S-5 (63% as opposed to 23% red-rimmed bowls; see above Fig. 6). In the following Strata S-4 (8% red-slipped, 77% red-rimmed) and S-3 (3% red-slipped, 88% red-rimmed) red-slipped bowls almost disappear (small sherds probably mostly originate from an earlier stratum), the red rim being the preferred decoration. In Stratum Q-2 in Area Q red-slipped bowls prevail over the red-rimmed ones (54% *versus* 46%; Martin forthcoming a). Red-slipped bowls further appear in considerable amounts in Stratum N-4 in Areas NA and NB. Note, however, that they also appear in Stratum N-3 of both areas. Due to the limited exposure and flimsy nature of this stratum in both areas this evidence is, however, inconclusive (as a statistical analysis as to the relative quantity of red-slipped bowls is of no help). In UME's Level VII red-slipped bowls are well known (James and McGovern 1993: 79; e.g. figs. 12:9, 12:12, 36:3, 41:2; in the published material appear 9 red-slipped versus 6 red-rimmed bowls), while they are absent from the few Level Late VII loci, in which red-rimmed bowls are very popular (James and McGovern 1993: figs. 48–51; of 42 Egyptian-style bowls 36 are red-rimmed). Also in UME's Level VI (Lower) red-rimmed bowls appear to be popular and red-slipped bowls are entirely or almost entirely absent (James 1966: 27; pls. 49–58).[10] In Yadin and Geva's Stratum 4 (which correlates to S-3 in Area S) among the few published Egyptian-style bowls only red-rimmed variants appear (1986: fig. 22; of 9 examples 5 are red-rimmed and the others undecorated).

The popularity of red-slipped Egyptian-style bowls in Q-2, N-4 and UME's Level VII and their rarity or absence in UME's Late VII, S-4/3, UME's Level (Lower) VI and Yadin and Geva's Stratum 4 favour a correlation of S-5 with UME's Level VII and its correlates (or a later part of this horizon respectively) and of UME's Level Late VII with S-4 in Area S (or its first half respectively). The fact that red-slipped bowls still occasionally appear in N-3 in Areas NA and NB somewhat hampers this correlation to be straight-forward. Assuming that the distribution of red-slipped bowls has no relative chronological value, the possibility remains that S-5 post-dates UME's Level VII and correlates to UME's Level Late VII. From both strata imported Mycenaean (be it IIIB or IIIC) and Cypriote wares are absent. However, the author clearly favours the correlation presented first, as otherwise three strata in Area S (S-5 to S-3) would postdate UME's Level VII and its correlates. While this is theoretically possible due to the domestic character of Area S, it seems rather unlikely. Both, Strata S-4 and S-3 are substantial phases, which must have endured a considerable amount of time. The accumulation of Stratum S-4 averaged half a meter in depth with minor rebuilds and floor raisings attested. Also the subsequent Stratum S-3 must have endured a considerable amount of time, as two substantial subphases (S-3b and S-3a) were encountered in many places. We know from the evidence of an Egyptian imported amphora that UME's Level VII and its correlates ended c. 1200 at the earliest but probably even later (see below). Considering the last royal name appearing at Beth Shean (Ramses IV [1153–1147] – James and McGovern 1993: fig. 165:8) as approximate date for the end of the Egyptian presence at Beth Shean, we would be left with only 50 years at the most for three strata. While it is without any doubt very well possible that the Egyptian garrison at Beth Shean outlived Ramses IV (see below), based on archaeological data it does not seem reasonable to push its end too far.

Another evidence that might argue for a contemporaneousness of Q-2 in Area Q and S-5 in Area S is finally the appearance of Egyptian-style simple bowls in a distinct ware fabric (WF 78 in the Beth Shean system; cf. Martin forthcoming a) in these strata and their absence in this fabric in the large assemblages of Strata S-4 and S-3.

[10] In the published plates of Level VI of the UME appear 12 Egyptian-style bowls, of which 6 are decorated with a red rim and 2 with a red slip. Note, however, that both red-slipped examples might come from unreliable contexts; one comes from Locus 1342, in which also scarabs of Thutmosis III and Ramses II were found (James 1966: fig. 57:4) and one from Locus 1343 at the edge of the tell (fig. 58:5). While the evidence of the plates is rather flimsy and not satisfactory in nature (James notes that only a fraction of the ceramic material was published in her report [1966: 2–3]), it might be significant (for an argument that red-rimmed bowls largely prevail), what James notes about the Level VI pottery (1966: 27): "*Perhaps the most interesting aspect of Level VI pottery is the large number of Egyptian forms. The vast majority of these Egyptianizing forms fall in the small bowl category.*" These bowls are "*likely to have a band of red wash around the rim, inside or out, or both*" (emphasis mine).

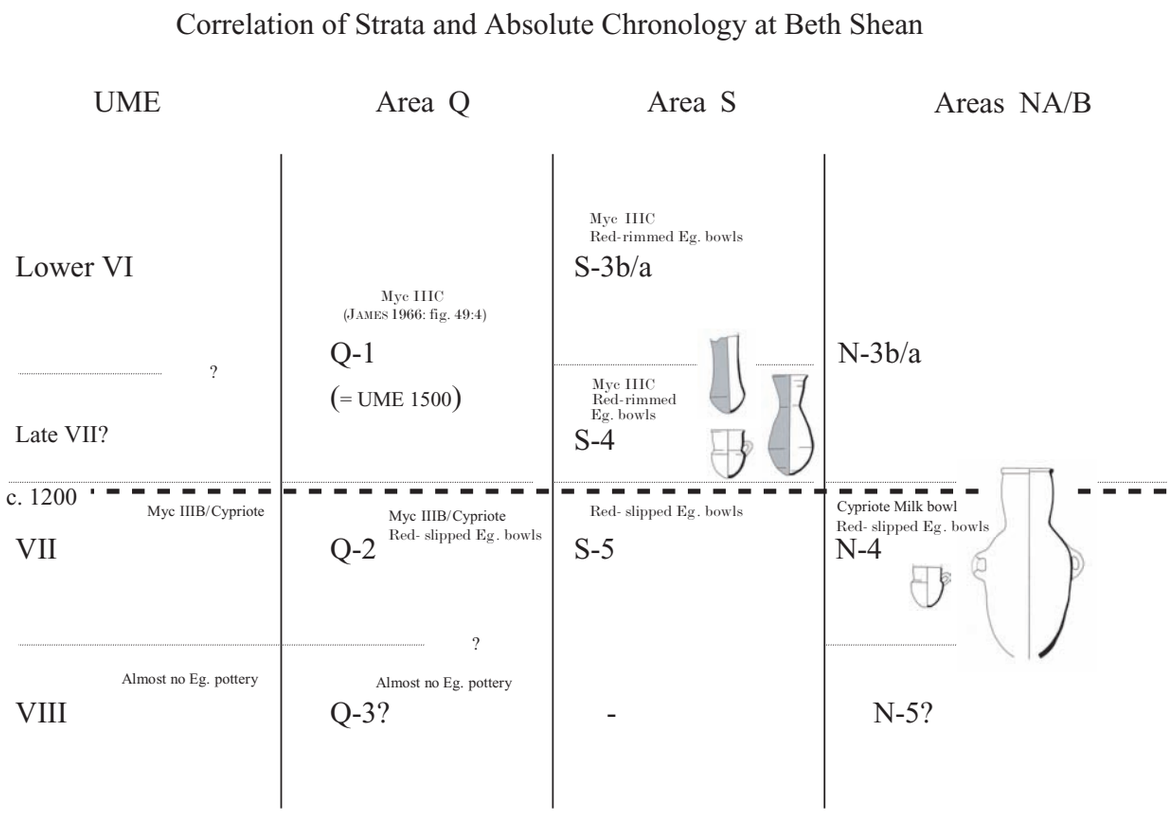


Fig. 7 Correlation between the various areas and absolute chronology at Beth Shean
(based on the evidence of material culture in general and on Egyptian and Egyptian-style vessels in particular)

To conclude, the correlation of Stratum Q-3 in Area Q remains. Basically, Q-3 might correlate with an earlier part of UME's Level VII or, more probably, already with UME's Level VIII. In Stratum Q-3 Egyptian forms are rare (7.5%), while they are very popular in Q-2 (64%; Martin forthcoming a). A similar development might be borne out in the assemblage of UME's excavations, where Egyptian forms appear to be almost absent in Level VIII (see above section 4), while they are common in Level VII. While the low percentage of Egyptian forms in Stratum Q-3 might be explained as spatial variation in the distribution of Egyptian and local Canaanite forms, the flimsy evidence from Stratum N-5 in Areas NA and NB, where sherds of Egyptian-style vessels also seem to be conspicuously rare, speaks against this assumption.

Bearing in mind the correlation elaborated above, following are the main chronological anchors from sides of the Egyptian pottery (Fig. 7): A *wide-bodied Egyptian amphora* of Marl D from Stratum N-4 in Area NA (Room 10429/98417, sealed by destruction debris),[11] as imported marl vessel a finer tool for dating than the Nile silt types, which are the ones that are locally reproduced at Beth Shean (Fig. 3:13). Apart from fragments of a collared rim jar and other vessels, this amphora was found in context with a very late Cypriote milk bowl. Although part of the base is missing, it is clear that it belongs to a round-based type of Egyptian amphorae typical for phase 4 of the Egyptian pottery sequence, which evolves between 1200 and 1185 (Aston and Pusch 1999: 41; for the division in phases cf. Bourriau 1981: 72–72; 1990: 19*). This type of jar appears with carinated base in the Nineteenth Dynasty. In the time between Sethos II (1200–1194)[12] and Tausret (1188–1186)[13] it evolves

[11] This vessel is illustrated in Killebrew 1998: 162, Ill. III.23:2.

[12] Chronology after Kitchen 2000: 49.

[13] The change definitely takes place after Merenptah and has fully evolved by Ramses III.

into its round-based variant (Aston and Pusch 1999: 41, 45).[14] Examples of this later type were found at Qantir,[15] Tell el-Yahudieh,[16] at Thebes in the Valley of the Kings[17] and as far as Hala Sultan Tekke[18] in a Late Cypriote IIIA1 context dated to 1190–1175 BCE. Based on the evidence of this Egyptian amphora, the end of Level N-4 and its correlates – UME's Level VII, Stratum N-4 in Area NB, Q-2 in Area Q and, probably, S-5 in Area S – therefore dates to 1200 at the earliest, but possibly 10–20 years later. Also an *Egyptian handled cup* from the same Stratum N-4 in Area NA (Killebrew 1998: Ill. III.21:2) seems to corroborate an end of this horizon only in the Twelfth century. Although not preserved completely – the rim was cut away, the vessel exhibits several features that would well fit into the Twentieth Dynasty: a slender body and an handle attached very low at the neck, resulting in a ring-like section; most important is the vessel's fabric, Egyptian Nile B2. While handled cups are a classical marl type, their occasional production in Nile silts until now does not seem to evolve before the Twelfth century (pottery phase 4). Note that in the probably contemporaneous Stratum S-5 two fragmentary handled cups are made of a Marl D fabric, the N-4 cup therefore being the first Nile silt example. Apart from the amphora and the handled cup the general chronological horizon for the end of the last LB IIB phase at Beth Shean is mainly indicated by a faience plaque of Merenptah (1213–1203) found "*near or north of the steps*" of the Level VII temple (L1068), which was regarded as *terminus post quem* for (or being contemporaneous with) the end of this level and its correlates in the other areas (James and McGovern 1993: 6–9; fig. 165:6; Killebrew 1998: 73). Note that the plaque was found in context with or in immediate vicinity to four other plaques, which bear the name of a Ramses – Ramses I according to Rowe (1936: 157, pl. 17) and Ramses I and/or II according to Weinstein (1993: 221). Porter excludes the attribution to Ramses I and, on the basis of a brief survey, suggests Ramses IV as the most likely candidate, with Ramses III a slight possibility (1994–5: 65). Consequently he resumes Rowe's interpretation (1930: 24; 1940: 9) of the various plaques as belonging to foundation deposits of the above-lying Level VI temple (Porter 1994–5: 66), which would render the Merenptah-plaque irrelevant for the dating of the end of UME's Level VII. However, based on several lines of evidence, B. Brandl evaluates Porter's interpretation as highly unlikely (personal communication).

The following phase, which is represented by Stratum S-4 in Area S, is characterized by a new urban plan, and has to fall within the reign of Ramses III (1184–1153). The Egyptian presence at Beth Shean during the time of Ramses III is well attested by inscriptive and monumental evidence, such as the basalt statue of this pharaoh retrieved from Lower Level V (L1024, northern temple; Rowe 1930: 36–38, pl. 51; James 1966: 35 fig. 81:3; see, recently, Higginbotham for the theory of an usurpation of a divine statue of the Nineteenth Dynasty [1999]), and the Level VI door lintel of the official Ramses-weser-khepesh kneeling in adoration before this king (James 1966: 4–8; figs 92:1, 93:1; Ward 1966: 161–163, 167–169). That Ramses' III last attempt to secure holdings in Canaan, to push back the Sea Peoples' attacks, and, evidently, also to exploit the natural resources of the country, is much more than a *swan song*, is suggested by Papyrus Harris I (pHarris I, 9:1–3),[19] where an inauguration of a temple in Gaza under this king (see most recently Wimmer 1990: 1086–1089) is mentioned, by the harvest tax as evidenced by hieratic Ostraca, such as at Tel Sera^{c} and Lachish (*e.g.* Goldwasser 1984), and by the bronze plaque bearing the cartouche of Ramses III in the gate area at Lachish (Ussishkin 1983: 168–170; 1985). In Stratum S-4, Mycenaean IIIB and Cypriote imports are absent and Mycenaean IIIC wares make their first appearance. One can assume that the relatively long-lived Stratum S-4 covers a large part of the reign of Ramses III. Egyptian and Egyptian-style vessels of S-4 confirm that we are dealing with a clear Twentieth Dynasty horizon: A *funnel-necked jar* finds its best comparanda in the Twentieth Dynasty (Fig. 8:1). The closest parallels were found at Qantir Area Q-I, in Stratum B1, dated to the Twentieth to Twenty First Dynasties (Fig. 8:2; Aston 1998: no. 2457), at Tell el-Yahudiyeh in a context dated between Ramses III and Ramses VI

[14] Some examples were stated to appear in Nineteenth Dynasty contexts, but none of these contexts are closed. Cf. Aston 1996a: 66; Aston 1997: 43–66; Aston and Bader 1998; Aston and Pusch 1999: 45.

[15] Aston and Pusch 1999: no. 49; Aston 1998: nos. 2498, 2511, 2513.

[16] Griffith 1890: pl. XIV:5.

[17] Aston, Aston and Brock 1998: nos. 335, 383–384, 392–399.

[18] Eriksson 1995: 201.

[19] For a hieroglyphic edition of Papyurs Harris see Erichsen 1933, for a translation of the relevant passage Pritchard 1955: 260–261.

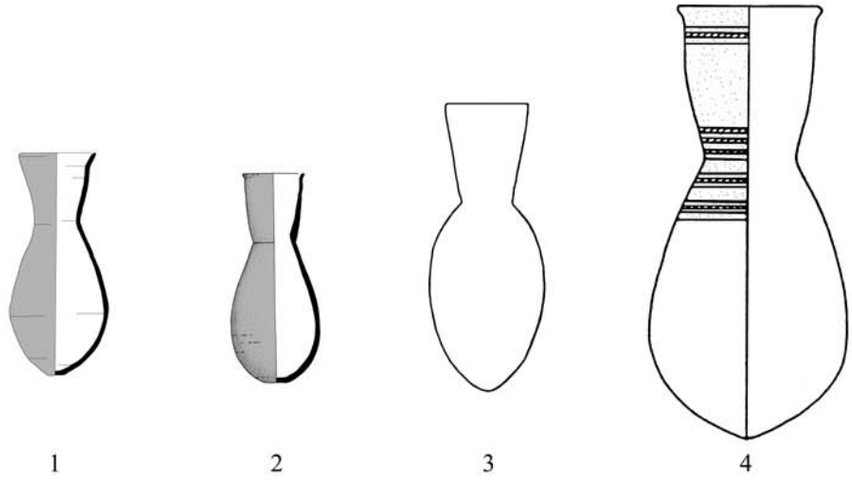


Fig. 8 Funnel-necked jars from the Twentieth Dynasty from Beth Shean Stratum S-4 in Area S (1), Qantir (2), Tell el-Yahudiyeh (3), and Tanis (4); scale 1:10

(Fig. 8:3; Griffith 1890: pl. XIV:6) and, though larger and with a less slender body, at Tanis (Fig. 8:4; Brissaud 1987: 99 no. 273) in a context dated to the Twentieth Dynasty. The Twentieth Dynasty date of the Beth Shean example and its Egyptian counterparts is mainly indicated by the very tall neck in relation to the vessel's height (Figs. 8:1–4), but also by the slender body (Figs. 8:1–3), the slight flare of the rim (Figs. 8:1–2, 4), as well as by the slightly flattened base (Figs. 8:1–2), all common features on these jars in the Twentieth Dynasty. Small *drop-shaped jars* of Stratum S-4 and the following Stratum S-3 mostly have a very low maximum body diameter, a common trait in the Twentieth Dynasty in Egypt (Fig. 3:9b). Also the slight carination in the profile seems to be characteristic for this period (cf. hereto an earlier example of Level VII with higher maximum body diameter and more rounded profile; Fig. 3:9a). Another chronological anchor of Stratum S-4 is a *handled cup of "Nile B or E"* (according to a petrographic analysis conducted by Cohen-Weinberger [1998: 409]). The Twelfth century date of Nile silt cups was already discussed above.

The subsequent Stratum S-3 in many places was subdivided into two substantial subphases (S-3b and S-3a). One might date the destruction of S-3a somewhere in the later part of the Twelfth century. As already mentioned above, the last royal name attested at Beth Shean is the one of Ramses IV (1153–1147), which appears on a scarab (James and McGovern 1993: fig. 165:8). The piece was found in an unreliable Level VII locus near the edge of the tell (L1253) and is likely intrusive from Level VI above. As the scarab provides only a *terminus post quem*, the end of the Egyptian garrison at Beth Shean might also be pushed more towards the last third of the Twelfth century, maybe even to its end. However, the later the date one considers, the less likely it will be, as the problem of the lack of royal names after Ramses IV (*e.g.* Ramses VI) becomes more and more immanent. Favouring a later date, one could, however, argue that the absence of royal names later than Ramses IV can be explained by the hypothesis that Beth Shean was possibly cut off from Egypt in this time, a theory based on the absence of Egyptian imports in the last Egyptianized stratum (S-3 in Area S; Martin forthcoming b). Anyhow, relying on the present evidence, it is most reasonable to reconstruct an end of the Egyptian garrison at Beth Shean in the time of Ramses IV or shortly afterwards, maybe in the time of Ramses VI (1143–1136).

Finally, a word should be said about the date of UME's Level VIII: James and McGovern tentatively date this level into the reign of Sethos I (1294–1279), possibly beginning with Ramses I (1295–1294) (1993: 5 and 235–236). Aston, on the other hand, argues for a dating in the late Eighteenth Dynasty (and consequently re-assigns the two Sethos I-stelae from Beth Shean to Level VII; 1996b: 226–228). He bases his argumentation mainly on following lines of evidence: (1) James and McGovern's dating is mainly based on the two monumental stelae of Sethos I, both of which come from later contexts and therefore could have originated also in Level VII (1993: 236). (2) In his treatment of the scarabs, plaques, seals, and rings of Levels VIII and VII at Beth Shean Weinstein notes that the Level VIII material does not offer direct support for its dating specifically to the early Nineteenth Dynasty and that one could reasonably assume that

Level VIII belongs primarily to the Fourteenth rather than the early Thirteenth century BCE. Five royal name pieces retrieved from Level VIII mention pharaohs not later than Amenophis III (Thutmosis III, Hatschepsut, Amenophis III; 1993: 224). (3) In her treatment of the Mycenaean pottery Hankey shows that the majority of the material of Level VIII dates to Late Mycenaean III:A2 and early Late Mycenaean III:B (the bulk of the material belonging to III:A2; 1993: 103–110). Late Mycenaean III:A2 fully belongs into the Eighteenth Dynasty (c. Amenophis III – Haremhab). Mycenaean IIIB already starts before the end of the Eighteenth Dynasty (Hankey 1993: 109). Dating Level VIII in the early Nineteenth Dynasty one would expect more Mycenaean IIIB pottery and not early within this phase. This discrepancy can only be explained by defining the vast majority of Mycenaean pots as heirlooms. (4) Finally Aston notes that, while the local Canaanite pottery is clearly LB II in character, it is not exclusively LB IIB, which is what it should be if Level VIII is no earlier than the Nineteenth Dynasty (1996b: 228). LB IIA forms also occur, which would also point to a Fourteenth rather than Thirteenth century BCE date.

Assigning Level VIII to the late Eighteenth Dynasty and the stelae of Sethos I to Level VII would well fit to the evidence of the Egyptian-style pottery. One could then reasonably explain the scarcity of Egyptian forms in Level VIII and Q-3 in Area Q (see above section 4) by associating it with a period towards the end of the Eighteenth Dynasty, in which Egypt has regional difficulties in Canaan (Weinstein 1981: 15–17). The marked increase of Egyptian forms in UME's Level VII and its correlates in the various areas of the renewed excavations (see above Fig. 4) could then be connected to the activities of Sethos I and his successor, Ramses II. The historical aspect of this scenario might be borne out in the Sethos-stelae from Beth Shean themselves. In the first Sethos-stela, which is dated to the first year of this king, we hear about an enemy, who is in the town of Hamath and seizes the town of Beth Shean (*KRI* I, 12:7–14). The king dispatches three army units to oppose this threat. The entire operation is told to have been accomplished within the course of one day. The second Sethos-stela, which does not provide us with a date formula, reports upheavals near Beth Shean caused by ^{c}Apiru tribes (*KRI* I, 16:91–4). Again, Sethos I sends his troops, who finish their mission in two days time. Both stelae clearly reflect local unrest in and near Beth Shean, which Sethos I claims to have restored. We might therefore associate Level VIII at Beth Shean with a period of upheaval, which endured an undefined time, before Sethos I took initiative. The scarcity of Egyptian forms in Level VIII could reflect this period, in which the Egyptians temporarily might have only had a loose grip on Beth Shean. The marked increase of Egyptian-style pottery in Level VII could then be result of a reinforcement of locally stationed Egyptian troops in the aftermath of Sethos' I successful military activities.

List of Illustrations

Fig. 1 Tel Beth Shean, the mound: Mazar 1997: 64 (plan)

Fig. 2 Stratigraphy of the various excavation areas at Beth Shean

Fig. 3 1) Vessel no. 108031/11, Locus 98852 Stratum S-4 (Area S), unpublished, courtesy of A. Mazar; 2) James and McGovern 1993: fig. 50:2; 3) James and McGovern 1993: fig. 48:5; 4) James and McGovern 1993: fig. 48:6; 5) James and McGovern 1993: fig. 51:2; 6) Vessel no. 887043/15, Locus 78740 Stratum S-3 (Area S), unpublished, courtesy of A. Mazar; 7) Vessel no. 288053, Locus 28811 Stratum S-3 (Area S), unpublished, courtesy of A. Mazar; 8) Cohen-Weinberger 1998: fig. 2:6; 9a) James and McGovern 1993: fig. 13:14; 9b) Vessel no. 108043/2, Locus 10809 Stratum S-4 (Area S), unpublished, courtesy of A. Mazar; 10) Cohen-Weinberger 1998: fig. 2:7; 11) Cohen-Weinberger 1998: fig. 2:8; 12) Vessel no. 988135/1, Locus 88866 Stratum S-4 (Area S), courtesy of A. Mazar; 13) Vessel no. 184038/3, Locus 10429 Stratum N-4 (Area NA); the illustrated vessel is not yet finally published (for a drawing see: Killebrew 1998: Ill. III.23:2); the author got the permission to include a photo; courtesy of A. Mazar

Fig. 4 Share of Egyptian and Egyptian-style vessels in Areas R, Q and S

Fig. 5 Type distribution within the corpus of Egyptian forms in Area S (in percent) – included are vessels and fragments with a preserved rim taking the size of the preserved part of the rim into consideration (for this method cf. Martin forthcoming b)

Fig. 6 Decoration of simple bowls in Area S (in percent) – included are vessels and fragments with a preserved rim taking the size of the preserved part of the rim into consideration (for this method cf. Martin forthcoming b)

Fig. 7 Correlation between the various areas and absolute chronology at Beth Shean (based on the evidence of material culture in general and on Egyptian and Egyptian-style vessels in particular); the handled cup from Area NA (no. 104289 in Locus 98417 Stratum N-4; for a drawing see: Killebrew 1998: III.21:2) like the amphora (see above Fig. 3:13) is not finally published; the author got the permission to include a small drawing of both vessels; courtesy of A. Mazar

Fig. 8 1) Beth Shean: Vessel no. 888320/1, Locus 88866 Stratum S-4 (Area S); courtesy of A. Mazar; 2) Qantir: Area Q-I Stratum B/1; Aston 1998: no. 2457; 3) Tell el-Yahudiyeh: Griffith 1890: pl. XIV:6; 4) Tanis: Brissaud 1987: 99 no. 273

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forthc. b The Egyptian and Egyptianized Pottery Assemblage from Area S, in: A. Mazar, *Excavations at Tel Beth Shean*, Volume IV, Qedem, Jerusalem.

forthc. c The Egyptianized Pottery Assemblage from Area NB at Beth Shean, in: A. Mazar, *Excavations at Tel Beth Shean*, Volume IV, Qedem, Jerusalem.

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Mittani Empire and the Question of Absolute Chronology: Some Archaeological Considerations

Mirko Novák[*]

1. Introduction[**]

When the Hittite king Ḫattušili I started his forays to Northern Syria, a certain "King of the Hurrians" appeared as one of his main opponents. Nowadays it is widely accepted that this person must have been one of the first rulers of the political entity later known as "Mittani" (Fig. 1).[1] Therefore, the formation of this powerful kingdom must have taken place during the latest phase of the Old Babylonian Period and predated the sack of Babylon by the Hittites under Ḫattušili's grandson Muršili I by at least two generations (Fig. 2).[2] From an archaeological point of view there must be a significant overlap of what is called "Old Babylonian" and "Mittani" Periods in Northern Mesopotamia, although they appear in nearly all chronological charts as succeeding one the other with a distinctive break in between.

Still, until today archaeology has failed in establishing a stratigraphical and chronological sequence of late Old Babylonian and early Mittanian layers on sites in the core area of the kingdom, the so-called

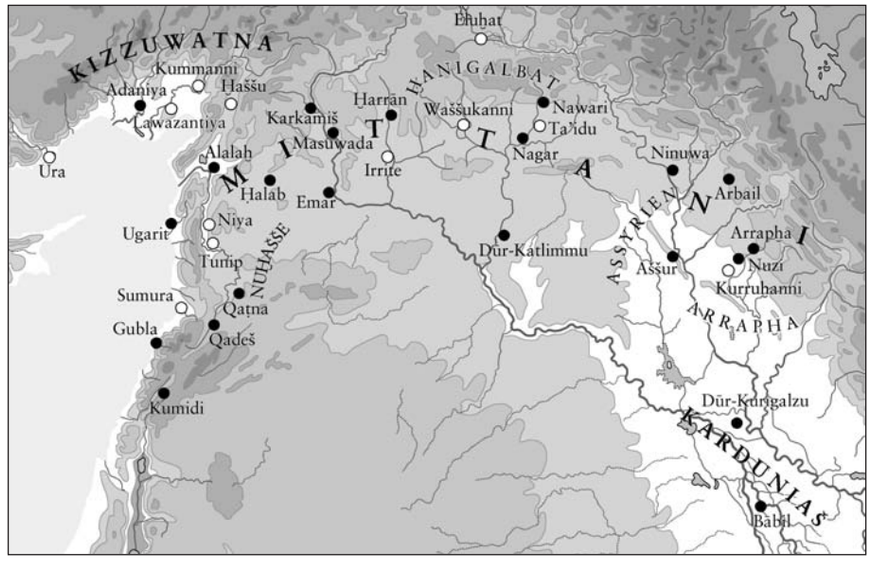


Fig. 1 Map of the Near East with the sites mentioned in the text

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[1] Klinger 1988; Kühne 1999: 208; van Koppen 2004: 19ff. On the history of the Mittani Empire and its very beginnings, cf. Wilhelm 1982 and Kühne 1999. The kingdom of "Mittani" is characterised by two significant features: Frist, the dominating linguistic group of this entity are the Hurrians, and second, the rulers bear exclusively non-Hurrians, in many cases definitivly Indo-Arian throne-names. This second point distinguishes Mittani from all the other Hurrian units, either earlier or later. If van Koppen's (2004: 23) proposal is right, that the kingdom was a result of the acquisition of civil power by leaders of mercenaries, who derived from deportees, than the Indo-Arian influence may date back to the time in which these groups settled somewhere in or beyond the mountain ranges of the Zagros

[2] van Koppen (2004: 23) has argued on the base of Old Babylonian slave trade records, that the Hurrian kingdom of "Ḫanigalbat", which later became known as "Mittani" may have constituted itself "at least 50 years before the end of the Old Babylonian period". Side by side to this entity a first Kassite principality may have been established in Northern Mesopotamia (Podany 2002: 50f.; van Koppen 2004: 22), although coming from the Zagros mountains (Sassmannshausen 1999).

"Ḫābūr-triangle".[3] One reason for that may be that none of the major urban capitals of the Mittani Empire has been excavated or investigated in a serious degree. Even the locations of its political centres Waššukanni,[4] Taʿidu[5] and Irride[6] are still uncertain. The only site in this region, which has revealed a spotlight on the transition phase of Old Babylonian and Mittani Period, is Tell Brāk, the ancient Nagar.

Nevertheless, two outposts at the periphery of the empire provide the best archaeological evidence on Mittanian chronology: Nuzi (Yorġan Tepe) in eastern Iraq and Alalaḫ (Tell Atchana) in the Hatay, both excavated in the 1920s to 40s!

It was just in the recent years that several sites have revealed new archaeological material dat-ing to the Mittani period. In Umm al-Marra between Aleppo (Ḥalab) and Emar layers were ex-plored, which contained sherds of Nuzi-Ware and a cuneiform tablet dated to the reign of Šuttarna II with the impression of the seal of Sauštattar.[7] Another impression of the same seal was discovered in Tall Bazi at the Middle Euphrates.[8] Mittani layers were furthermore excavated at Ekalte (Tall Munbaqa),[9] Emar[10] and Terqa[11] alongside the Euphrates. The data of these sites do not yet bring us forward in the question of absolute chronology[12] but may do so in the future.

Thus, the contribution of Mittani to the discussion about absolute chronology[13] seems to be quite limited although it is one of its keys. Some recent re-evaluations of the material culture of both sites can help to get some indications for the length of the Mittani Period.

2. Nuzi

The middle size town of Nuzi (Fig. 3) belonged to the kingdom of Arrapḫa (modern Kirkūk), a vassal to the Mittani Empire in the area east of the Middle Tigris close to the Zagros ranges. The excavations concentrated on the Upper Town, the so-called *kerḫu*.[14]

Here, a palace, a temple, a storehouse and three quarters with private dwellings were explored. Stratigraphical sequences were counted separately in

	Mittani	Ḫatti	Aššur	Egypt
~ *1540 BC*	"King of the Hurrians"	Ḫattušili I		
~ *1500 BC*	Kirta (?)	Muršili I		
	Šuttarna I (?)	Ḫantili		Thutmosis I
	Parrattarna	Zidanta		
	Parsatatar			Thutmosis III
~ *1420 BC*	Sauštatar	Ḫattušili II		Amenophis II
	(Parrattarna II ?)			
~ *1400 BC*	Artatama I			Thutmosis IV
	Šuttarna II	Tutḫalia II		
	Artaššumara	Arnuwanda I		Amenophis III
~ *1360 BC*	Tušratta (daneben: Artatama II und Šuttarna III)	Šuppiluliuma I	Aššur-uballiṭ I	Amenophis IV (= Akhetaten)
	Šattiwaza			Tutankhamen
~ *1290 BC*	Šattuara I	Muršili II	Adad-nērārī I	
	Wasašatta			
~ *1240 BC*	Šattuara II	Ḫattušili III	Shalmaneser I	Ramses II
~ *1200 BC*	Atal-Teššub	Tutḫaliya III (IV)	Tukultī-Ninurta I	

Fig. 2 List of known Mittani rulers and synchronisms with Ḫatti, Aššur and Egypt (after: Wilhelm 1982 and Kühne 1999)

[3] There are just a few sites on which a sequence is attested at all (an overall comparative stratigraphy is given by Pfälzner 1995: 259, Abb. 162).

[4] Most likely to be identified with Tall Faḫḫarīya; cf. Kühne 1995: 208, Cancik-Kirschbaum 1996: 33 (see map fig. 7 on p. 34) and Gorem *et al.* 2004: 44.

[5] Probably to be identified with Tall al-Ḥamīdīya (cf. Haas and Wäfler 1985; doubted by Röllig 1997: 282).

[6] To be localised most likely somewhere at the upper Balīḫ.

[7] Schwartz *et al.* 2003: 349ff., fig. 34.

[8] Kind information by Dr. Adelheid Otto, Munich.

[9] Mayer 2002.

[10] Finkbeiner / Sakal 2003.

[11] Podany 2002; Rouault 2004.

[12] Pruzsinsky 2004.

[13] On the recent discussion about absolute chronology cf. Gasche *et al.* 1998 and a number of articles published in *Akkadica* 119–120 (2000) and Hunger / Pruzsinsky 2004. See furthermore Reade 2001. Most of the scholars prefere either the low or the ultra-low chronology.

[14] Starr 1939; Novák 1999; Wilhelm and Stein 1998–2001.

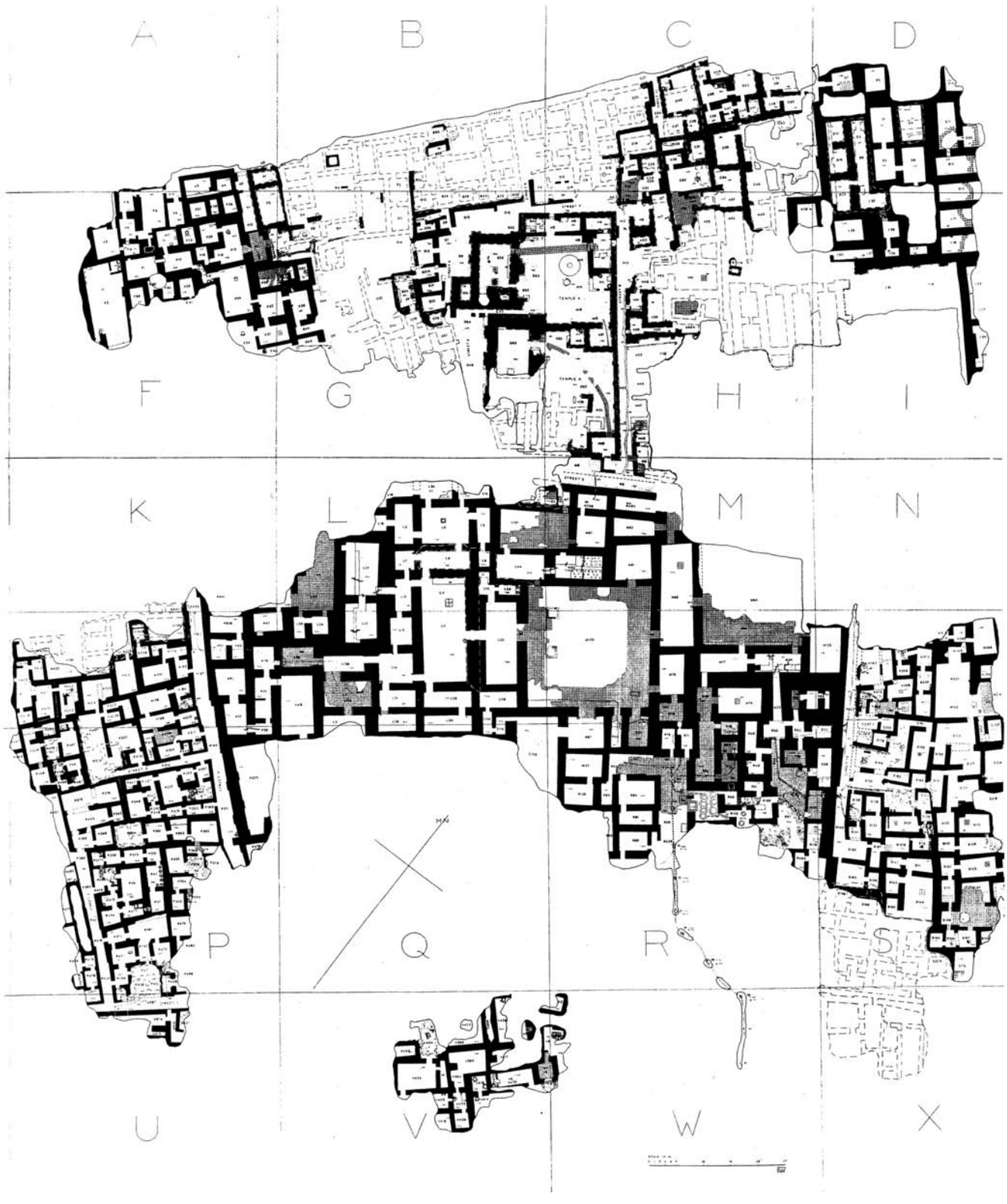


Fig. 3 Plan of the inner town of Nuzi (from: Starr 1939, Plan 13).

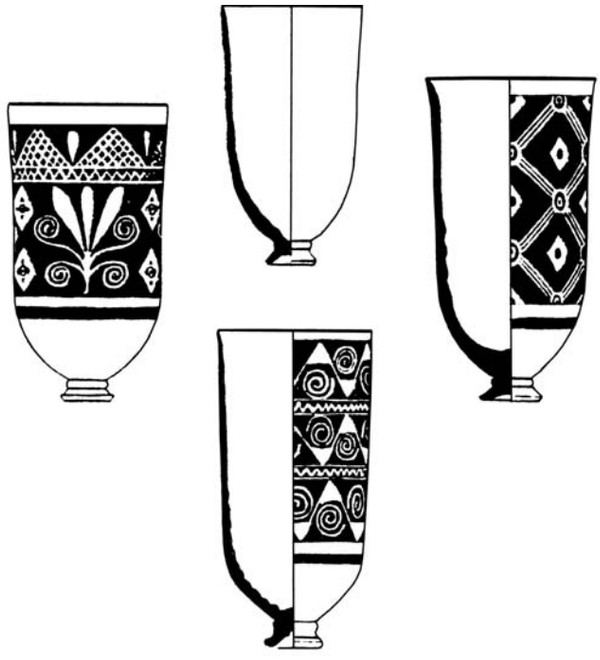


Fig. 4 So-called "Nuzi-Ware" found in Nuzi, Stratum II (from: Stein 1984: 49, 10–13)

each area. The best-investigated phase was Stratum II of the town with the contemporary Temple A. The characteristic pottery was the proper "Nuzi-Ware" with its white-on-black painted decoration (Fig. 4).[15] Although thousands of cuneiform tablets were discovered, only one direct historical connection could be established so far: It is the seal of the Mittani king Sauštatar that provides us with a *terminus post quem*. However, the duration of Stratum II can be assigned to four or five generations, a period of about 100 years.[16] An evaluation of the chronology of Stratum II by Diana Stein has shown that it was destroyed around 1340 BC, so that its beginnings can be dated approximately around 1440 BC.[17]

The material culture of Stratum III, lying directly below Stratum II, shows a close relation to the material of the following layer. The most significant exception is the "Nuzi-Ware", which is not attested in Stratum III at all.[18] This could mean, that this ware probably was not developed before the middle of the 15th century. The beginning of Stratum III must be dated to the second half, probably to the last quarter, of the 16th century BC, if its duration was equivalent to that of the succeeding Stratum II.

The underlying Stratum IV can most probably be linked with Temple F.[19] Due to the objects found in Temple F, this phase is dated to the Old Babylonian Period. This is confirmed by the close relationship of the pottery found in Stratum IV and Stratum VII, the latter definitively belonging to the latest phase of the Neo-Sumerian or the earliest phase of the Old Babylonian Period.[20] There is, however, also some tradition in the material culture from Stratum IV to III, thus indicating a general continuity of occupation.

The transition of Old Babylonian to Mittani Period in Nuzi is characterised by the massive infiltration of Hurrian speaking persons on the one hand and the re-naming of the town from *Gasur* to *Nuzi* on the other hand. Nevertheless, architecture indicates an unbroken development:[21] The palace, for example, connects elements of Old Babylonian palace architecture with those of the Middle and Neo Assyrian type. The layout of the Temple shows no evident change from 3rd millennium on to its very end. And the houses follow some old-fashioned layout patterns.

Period	Dwelling Area	Temple Area	Pit L 4	Material
Middle Assyrian	Stratum I			
Mittani (~1440–1340)	Stratum II ↔ Temple A ↔ Pavement I			Nuzi-Ware, Archives
Early Mittani	Stratum III	Temple B–E		no Nuzi-Ware but related Material
Old Babylonian	Stratum IV	Temple F	↑	↑
Neo-Sumerian	Stratum V–VII	Temple G	Pavement IIA	**Old Babylonian Tablet**
	Stratum VIII		Pavement IIB	**Ur III-Tablet**
Akkadian			Pavement III	Old Akkadian Tablets

Fig. 5 Stratigraphical sequence in Nuzi with proposed absolute dating

[15] On Nuzi-Ware cf. Stein 1984; Hrouda 1989; Pfälzner 1995: 238ff.; Postgate *et al.* 1997: 54f.; Oates *et al.* 1997: 67f.; and the references cited there.

[15] Wilhelm and Stein 1998–2001: 636f.

[17] Stein 1989.

[18] Stein 1984.

[19] Wilhelm and Stein 1998–2001: 641; Opificius 1961: 18f.

[20] Starr 1939: 203; Wilhelm and Stein 1998–2001: 641.

[21] Novák 1999.

Therefore, a *hiatus* can be excluded and a short chronological distance from Old Babylonian to Mittani Period can be taken for sure. This leads us to the following stratigraphical and chronological scheme (Fig. 5).

Even if there is no significant overlap of the Old Babylonian Period (in Babylonia) and the beginning of the Mittani Period (in Nuzi and other places in the North), as it should be taken in account, the transition between both phases should be dated not earlier than the second half of the 16th century.

3. Nagar

The site of Tell Brāk is situated at the lower Ǧaġǧaġ in the so-called "Ḫābūr-triangle", the heartland of the Mittani kingdom, also known as "Hanigalbat".[22] It can be identified with the ancient city of Nagar,[23] flourishing in the late 3rd millennium, but still of some importance during the 2nd millennium as well.[24]

Tell Brāk has been the subject of archaeological investigations during the 30s and from the 80s until today. A sequence of nearly uninterrupted occupation is attested from the 3rd or even the late 4th until the second half of the 2nd millennium BC. However, the excavations could not provide a profitable contribution to the discussion of absolute chronology of early Mittani.

In Area HH at the northern edge of the mound, a stratigraphical sequence was identified, that distinguishes ten building levels from the Old Babylonian to

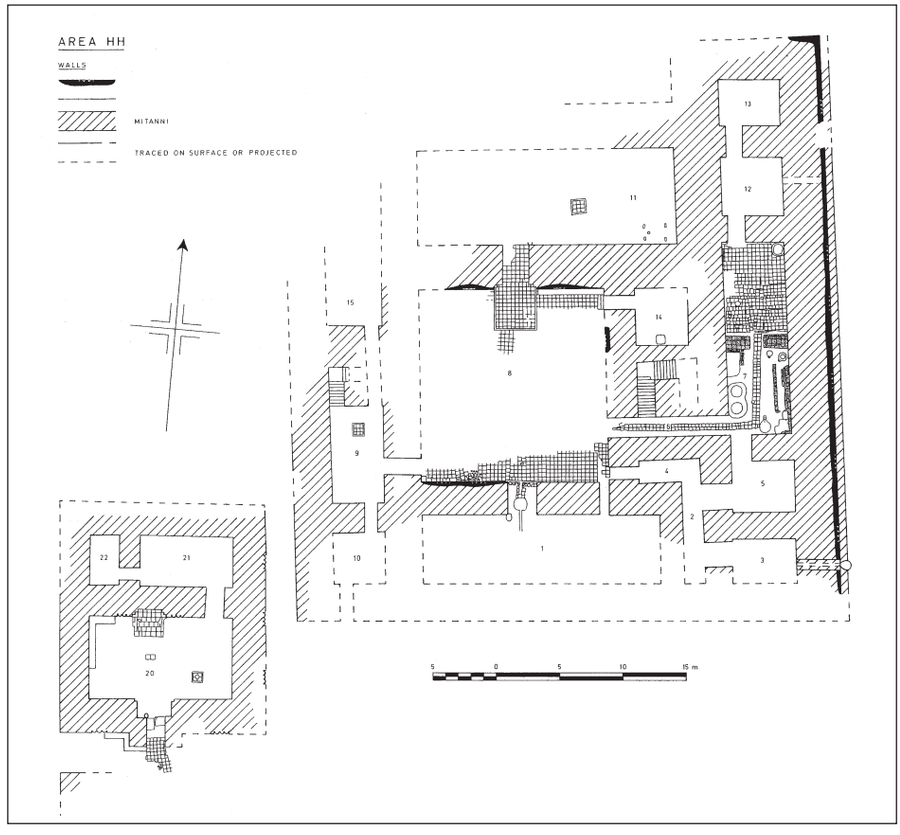


Fig. 6 Mittanian palace and temple in Nagar (Tell Brāk), Area HH (from: Oates et al. 1997: 4, fig. 12)

[22] On the distinction of Ḫanigalbat and Mittani cf. Kühne 1999.

[23] Matthews and Eidem 1993; Eidem, Finkel and Bonechi 2001.

[24] It should not be mixed up (as done by Steinkeller 1998, 95) with another, close-by town, which was named Nawar in the 3rd and 2nd millennium BC and Nabula in the Neo-Assyrian period (Kessler 1978–79 and 1999) and is to be localised at Girnavaz close to the modern city of Nusaybin (Erkanal 1988; Donbaz 1988; Röllig 1998). This is most likely the Nawar, to which the titulatury of the Hurrian king Atal-Šēn, ruler of Urkeš and Nawar in the late 3rd millennium, refers (Wilhelm 1982: 12ff.; Salvini 1998, 108ff.). It was an important worship centre of the Storm God. On the localisation of Urkeš at modern Tall Mozān cf. Buccellati 1998, Buccellati and Kelly-Buccellati 1999.

the Middle Assyrian period.[25] Besides that, some domestic structures, a palace and an adjacent temple were examined (Fig. 6). All of them were built in Level 6, in which the "Nuzi-Ware" appears for the first time and is associated with "Ḫābūr-ware".[26] Both types of pottery are attested together in the following Level 5 as well, while in the pre-dating Levels 8 and 7 just pure Middle Bronze Age material was found, represented e.g. by distinct bronze pins or by comb incised decoration on ceramic jars and pots. Therefore, Level 7 and probably also Level 8 may represent the transition or overlapping period between what is called "Old Babylonian" and "Mittani" in Northern Mesopotamia.[27] Level 10 is ascribed to the time of Assyrian king Šamšī-Adad I based on the objects.

The excavators date the foundation of the palace to the late 16th or early 15th century. But in contrast to this opinion and judging from the first appearance of Nuzi-beakers in Nuzi itself (see above), the beakers found in the construction phase point to a date not before the middle of the 15th century BC. Level 6 should then be correlated more or less to Stratum II at Nuzi.

Several cuneiform tablets were discovered within



Fig. 7 Plan of Alalaḫ (from: Woolley 1955)

[25] cf. Oates, Oates and McDonald 1997: 35, Table 1 and 2.

[26] cf. Oates, Oates and McDonald 1997: 68.

[27] This is confirmed by the fact, that the ceramic material both from Tell Rimaḫ and Tell Brāk does not allow to distinguish between painted wares of "Late Old Babylonian" and "Early Mittani" levels (cf. Oates, Oates and McDonald 1997: 64).

[28] cf. Eidem in: Oates, Oates and McDonald 1997: 39–46, documents 4 and 5.

[29] Wilhelm 1982: 40f.; Kühne 1999: 218.

[30] Colbow 2000: 119f.

the palace. Two legal documents are dated precisely to the reigns of the Mittani kings Artaššumara and Tušratta,[28] both sons of king Šuttarna II, ruling in the first half of the 14th century BC.[29] The glyptic associated with the tablets is of pure Mittani style.[30]

4. Alalaḫ

Alalaḫ was the capital of the minor kingdom of Mukiš at the lower Orontes River (Fig. 7). During the Old Babylonian Period it belonged to the powerful kingdom of Yamḫad with its capital Aleppo and was ruled by a *secundogenitur* of the royal house of Yamḫad. After the sack of Aleppo by the Hittites under Muršili I the city of Alalaḫ, just like all the other territories of former Yamḫad, got under the control of the newly established Mittani empire. The events during this period of changing political constellations are known from the so-called "autobiography" of king Idrimi of Alalaḫ, written on his famous statue.[31] Idrimi was the youngest son of the last independent king of Aleppo who lost his throne after a certain *mašiktu* "event".

In the stratigraphical sequence of Alalaḫ two layers are well dated through archives: Level VII of the late Old Syrian Period (equivalent to Old Babylonian Period in Mesopotamia), and Level IV of the developed Mittani period. Both can be associated with rulers or events attested in other sources as well: Level VII was founded by the kings of Yamḫad after the time of the Mari-archives and destroyed most probably by Ḫattušili I during his first campaign to Syria, one or two generations before the fall of Babylon. Level IV was established by king Niqmepa, son of the mentioned Idrimi.[32]

Since the Levels VI and V are "sandwiched" by these two levels it is of high interest to estimate their duration. One problem is that none of them provided us with cuneiform texts. In addition, architecture is preserved in a very bad and fragmentary way. Is this just bad luck of the excavations or does this mean, that these two levels were just short-living interfaces? A possible answer was given by five very thorough studies: by Marie-Henriette Gates (1982), Marlies Heinz (1992), Wilfred van Soldt (2000),

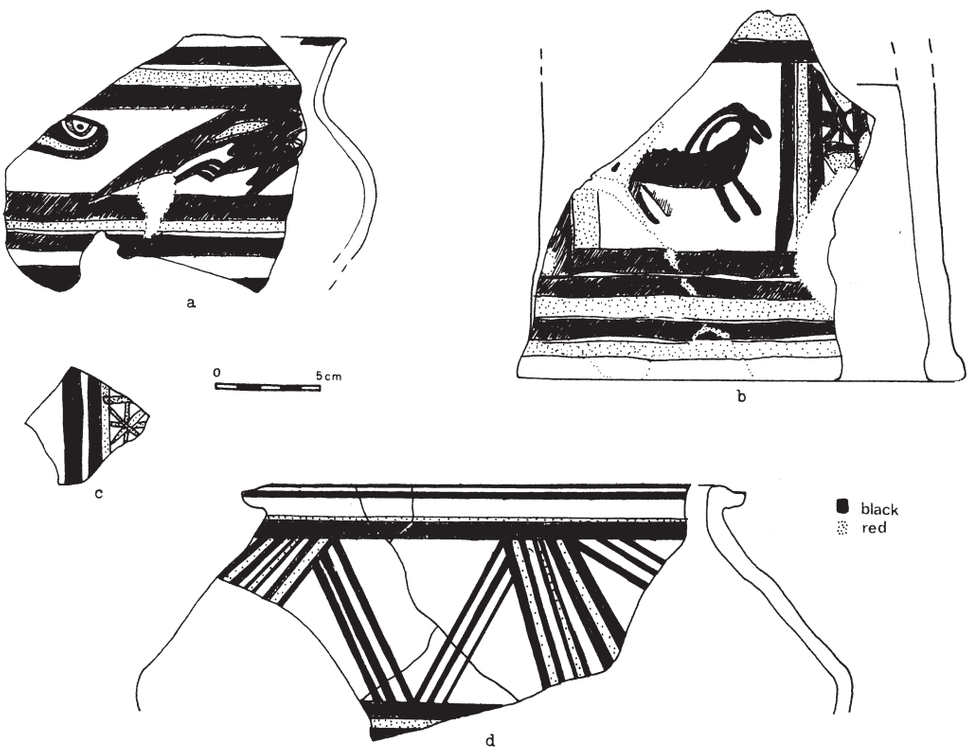


Fig. 8 "Bichrome Ware" found in Levels VI and V at Alalaḫ (from: Gates 1981: 20, Ill. 5)

[31] Cf. Dietrich and Loretz 1981; Klengel 1981; Mayer-Opificius 1981.

[32] And not, as often suggested (Zeeb 2004: 87), by Idrimi (cf. Bergoffen 2005).

Level	Ceramic	Kings	Events	Proposed Date
VII	Middle Bronze Age Material		Destruction by Ḫattušili I (?)	1645–1540 BC
VI	Cypriote Bichrome-Ware and Local (Syrian) Ware in Level VII-tradition		End: *mašiktu* in Ḥalab (Muršili I ?)	1540–1499 BC
Va		Idrimi ↓ Niqmepa	Interface and Reign of Idrimi	1499–1450 BC
Vb			End: campaign of Thutmosis III (?)	
IV	Cypriote White-Slip-II-Ware, Nuzi-Ware		Destruction by Šuppiluliuma	1450–1340 BC

Fig. 9 Stratigraphical sequence in Alalaḫ with proposed absolute dating (dating based on van Soldt 2000)

Frank Zeeb (2001), and most recently by Celia Bergoffen (2005).

M.-H. Gates examined the archaeological evidence and stressed that only in Levels VI and V the so-called Cypriote "Bichrome-Ware" (Fig. 8) is to be found.[33] In Level IV it is replaced by "White-Slip-II-Ware" and "Nuzi-Ware", both formerly not attested. The common ware is in close connection to Middle Bronze Age pottery. As a result of her analysis Gates concluded that the material clearly points to a lifespan of both levels of little more than a century. Thus she argued for a short absolute chronology. She was followed by M. Heinz's analysis of the ceramic found in Level VII.[34] Heinz pointed to the close relations of this material to such one found in very early Mittani levels on sites like Hadidi. This should be taken as a clear indication for a close chronological connection. C. Bergoffen re-examined once again the Cypriot pottery from Alalaḫ in connection to all available archaeological and philological data.[35] Also her results confirm the low chronology in most aspects.

Since these three studies base mostly on archaeological data, W. van Soldt and F. Zeeb paid most of their attention to philological and epigraphic data.[36] Independently of each other they both even reduced the proposed lifespan of Levels VI and V and concluded that the ultra-low absolute chronology of H. Gasche[37] should be preferred. In contrast to Zeeb, van Soldt argues that the so-called *mašiktu* in Ḥalab should be identified with the destruction of the city by Muršili I. Therefore the beginning of the reign of Idrimi in Alalaḫ should be connected rather with Level Va than Level Vb (as preferred by H. Gates before).

Taking all these studies in account, we can, in my point of view, come to the following and most convincing chronological correlation and interpretation of the Alalaḫ sequence (Fig. 9):[38] Level VII was destroyed by Ḫattušili I[39] and directly followed by Level VI. The end of Level VI was marked possibly by the siege or threat of the town by Muršili I, grandson of Ḫattušili, about 30 or 40 years later.[40] This event could have been the same one, which was mentioned as *mašiktu* in the inscription of Idrimi.[41] In Level V two phases can be distinguished: Va and Vb. The change of the architecture between both phases may have been the result of building activities of Idrimi during his long reign. The reconstruction of the city under Idrimi's son Niqmepa, which mark the beginning of Level IV, may has happened more or less in the time of the first forays of Thutmose III in Syria around 1450 BC.[42]

This chronological framework with the dating of Idrimi as one of the immediate successors of the Old

[33] Gates 1982. In a later publication, the author follows the ultra-low chronology of Gasche et al. 1998; cf. Gates 2000: 78.

[34] Heinz 1992.

[35] Bergoffen 2005.

[36] van Soldt 2000; Zeeb 2001 and 2004.

[37] Gasche *et al.* 1998.

[38] For a different, but in my eyes not convincing, reconstruction of the chronology of Alalaḫ cf. Eder 2003.

[39] Zeeb 2004: 86f.

[40] The timespan between the sack of Alalaḫ under Ḫattušili I and the siege of Ḫalab under Muršili I is difficult to estimate (van Soldt 2000: 108f.; Zeeb 2004: 86).

[41] One problem connected with this suggestion is the chronology of Kizzuwatna: Idrimi is attested as contemporary of Pilliya of Kizzuwatna (AlT 3) who has made a treaty with Hittite king Zidanta (KUB XXXVI 108), most probably the second bearer of this name (di Martino 2004: 36f.). There ruled approximately eight (!) kings between Muršili I and Zidanta II (cf. Wilhelm 2004) of uncertain duration. It is not impossible but quite unlikely that Idrimi's reign could overspan this time.

[42] On the possible synchronism of Idrimi, Parratarna and Thutmosis III cf. Redford 2003: 229ff. and Helck 1971: 117f. That the Egyptians reached the territory of Alalakh at this time is proven by the mentioned place names (Astour 1963). Most likely Thutmoe has invaded the territory of Alalakh during his 33rd year in his 8th campaign (Redford 2003: 220ff.). I thank Alexander Ahrens for this information.



Fig. 10 Seal of king Idrimi of Alalaḫ, found in Level IV (from: Collon 1975: 99, fig. 189) Scale 2:1

Syrian kings of Ḥalab would explain, e.g., why his seal is of pure Old Syrian style in the tradition of the glyptic from Level VII and shows no Mittani influence at all (Fig. 10).[43]

5. Comparative analysis of the stratigraphy and chronology of Nuzi, Nagar and Alalaḫ

Let us now take a look at the stratigraphical and chronological correlation of Nuzi, Nagar and Alalaḫ. This must primarily base on the analysis of first appearance of equivalent features like seals and pottery.

A thorough study by Diana Stein has helped to establish the detailed relation of Stratum II in Nuzi and Level IV in Alalaḫ:[44] At least the beginning of both levels should be more or less contemporary, since both are characterised by the first appearance of "Nuzi-Ware". The construction of the Mittani palace at Nagar (Level 6) dates to the same chronological horizon.

Levels VI and V in Alalaḫ should be more or less contemporary with Stratum III at Nuzi. Both predate the appearance of "Nuzi-Ware" and "White-Slip-II-Ware" but belong to the same material horizon as their following layers. That means, that both can be labelled "Middle Syrian" or "Mittani" in a pure chronological sense of these terms. The same could be taken in account for Nagar Level 7, although too few material was discovered there to judge.

Historical considerations lead to the result, that the Mittani Empire was already established and developed in the time of Nuzi Stratum III and Alalaḫ Level V or even Level VI. Its formation must have taken place in Northern Mesopotamia simultaneous to the final phases of Nuzi Stratum IV, Alalaḫ Level VII and therefore also Nagar Level 8, all of them clearly to be labelled "Old Babylonian" due to their material culture.

If we now try to calculate the lifespan of Nuzi Stratum III and Alalaḫ Levels VI and V, they both cannot exceed over more than 100 years because of several reasons. That is, roughly speaking, the chronological distance between Ḫattušili I and Sauštatar of Mittani, the latter attested both in Alalaḫ Level IV and Nuzi Stratum II. This indicates that there are good reasons to prefer a short or even an ultra-short chronology (Fig. 11).

5. Qaṭna

Some observations made in the recent re-excavation of the Bronze Age palace at Qaṭna can possibly support a low chronology.[45] The city is situated close to the Orontes River near the modern city of Ḥoms. It was the capital of a major kingdom in the Old Syrian Period and became a vassal to the Mittani Empire during the Middle Syrian Period.[46]

The re-examination of the chronology of the royal palace of Qaṭna shows that it was established in the middle of the Old Syrian Period (early MBA II), that is to say, more or less, during the time of the Mari archives, and was destroyed in the time of the

Nuzi	Nagar	Alalaḫ	Ceramic	Mesopotamian Chronology	Proposed Date
II	6	IV	Nuzi-Ware, White-Slip II	*Mittani*	~ 1450/40–1340 BC
III	7	VI–V	Bichrome (Al.), Ḫābūr (Ng.)	Late Old Babylonian / Early Mittani	~ 1540–1450/40 BC
IV	8	VII	MBA II	Late Old Babylonian	~ 1650–1540 BC

Fig. 11 Comparative stratigraphy of Nuzi, Nagar and Alalaḫ with proposed approximate dating

[43] Collon 1975: 99, Cat. No. 189.

[44] Stein 1989.

[45] On the excavations in Qaṭna cf. Du Mesnil du Buisson 1935, al-Maqdissi 2001, al-Maqdissi *et al.* 2002, Morandi Bonacossi *et al.* 2003. On the German excavations of the palace in particular cf. Novák and Pfälzner 2000, 2001, 2002 and 2003.

[46] On the general history of the city cf. Klengel 2000.

Hittite king Šuppiluliuma I in the 14th century.[47] The architecture of the palace in many aspects followed Old Babylonian patterns. As far as we know at present stage of investigations, even the floors of the building were in use during its complete lifespan. Even if we take in account that they consisted of very hard and long-living lime-mortar, the duration of their use could not have been too long.

The material found in the layers of the destruction phase reminds strongly of that of Alalaḫ Level IV, because here it is represented two "Nuzi"-beakers and three "White-Slip-II"-vessels.[48] Nevertheless, many groups of objects show a continuous development during the time of existence of the palace. This is, e. g., the case with the ceramics and is most of all obvious with the well represented glyptic.[49]

Most of the sealings certainly date to the Old Syrian Period because of stylistic and iconographic reasons. But it is difficult to distinguish between older and younger examples since even the sealings found on the cuneiform tablets dating to the 14th century nearly show no elements different to those of the Old Syrian glyptic style.[50]

Hence the material culture of Qaṭna also seems to indicate a relatively short chronological distance from the time of the Mari archive to that one of the Amarna archive.

7. Summary

This very brief archaeological evaluation of the comparative stratigraphy and chronology may help to estimate the duration of the Mittani Empire. Historical records show that it must have been founded one or two generations before the sack of Babylon and thus before the transition from Middle to Late Bronze Age. Its end as independent realm can be dated to the time of Hittite king Šuppiluliuma I in the middle of the 14th century BC.

Only few sites situated in the area of the Mittani Empire provide stratigraphical sequences covering the formation period and the complete lifespan of the Mittani empire: Nuzi, Nagar, Alalaḫ and Qaṭna. Nevertheless, they reveal an important key to the answer of absolute chronology. The examination of their material culture leads to the conclusion that there is clear evidence on a short chronology system. We can eliminate both the Middle and the High Chronology and must therefore choose only between the Low and the Ultra-Low Chronology. This would help us to erase one of our fictional "Dark Ages".[51]

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[47] Cf. Novák 2004 and Richter 2002 and 2003.

[48] Novák 2004: 308f., figs. 9 and 10.

[49] Elsen-Novák 2002 and Elsen-Novák *apud* Novák and Pfälzner 2003.

[50] Elsen-Novák in: Novák and Pfälzner 2003: 152ff.

[51] One of the few strong opponents to the ultra-low chronology in recent years is Hittitology (cf. Beckman 2000). Its argumentation bases only on the estimation of the average duration of generations of Hittite kings. In its view the ultra-low chronology provides too little time for each generation. But how weak these arguments are, is demonstrated by the generation chart published by Beckman himself: In several cases it is not at all clear, how the relation between predecessor and successor was (see e.g. Beckman 2000: 26, Chart 1, No. 5, 9, 12 etc.). If we erase doubtful candidates for own generations like e.g. Generations V (Zidanta I), VIII (Alluwamna) and X (Zidanta II) we can count 16 instead of 19 generations in total and therefore the whole argumentation is not striking any more. It must be stressed that the Hittite evidence is of no significance to the question of absolute chronology (cf. now Wilhelm 2004)!

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Late Old Syrian Fortifications and Middle Syrian Re-Occupation on the Western Rampart at Tell Mardikh-Ebla

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Introduction

Over the last eight seasons of excavation at Tell Mardikh-Ebla, the renewed investigations of the Middle Bronze defensive system (Matthiae 1997, 10–12; 1998, 572–588; 2000a, 580–600; 2000b, 1032–1035; 2001; 2002, 547–558; Peyronel 2000; in press) have identified the presence of forts and fortresses spaced at short intervals (250/300 m) on the top of the 3,5 km long earthen rampart. Some of them must have been connected to city-gates, and others were devoted to the control of sections of the city-wall.

Excavations of large tracts especially of the Western, Northern and Eastern rampart brought to light several buildings: the Western Fort, the Northern Fort, the large bastion flanking the as yet unexcavated 'Aleppo' Gate, the North-East city Gate (the so-called 'Steppe Gate'), the Fortress East-North-East (Area EE). Moreover the Fortress East-South-East in Area M (discovered in 1971) was partially cleaned and re-examined; finally the restoration of the well-known South-West city-gate has involved the excavations of some rooms in the inner part of its southern bastion (Fig. 1).

The Western Fort (Fig. 2) runs along a major South-North axis for a length of 70 m and includes several units arranged around an open upper courtyard which connects the main units by means of an intricate system of staircases and ramps (Peyronel 2000, 1354–1357, fig. 1; Matthiae 2001, 44–46, fig. 8). The architectural organization of the Northern Fort seems to be quite different, with the absence of a large open courtyard with a connecting ramp: several building phases have been identified, dating from the MB IB–II, with major changes in the arrangement of the inner space during MB II, when some rooms were deliberately filled with mud bricks. Nevertheless, it is possible to point to a number of features common to the two complexes: the position of the Fortress at the north-western edge; the presence of rows of chambers along the major north axis; the existence of terraces at different levels (Matthiae 2001, 46–48; 2002, 547–552).

The architectural and spatial analysis of the defensive buildings on the Eastern and Western rampart has shown that only isolated bastions (Fortress East-North-East and East-South-East) are apparently located on the eastern side of the urban Eblaic fortification (Peyronel in press). They were massive rectangular buildings used as arsenals/towers and they were built on the middle/upper part of the rampart inner slope, but not on the top and partially projecting outside as in the opposite western rampart. On the contrary it is now ascertained that further intricate complexes (Western and Northern Forts) in which bastions are only one of the inner architectural units were planned on the western rampart. The main city-gates (SW and NW) are defended by huge circular or square towers with chambers on the inner or outer sides; on the contrary, the evidence from the NE and SE gates (the so-called 'Euphrates Gate' and 'Steppe Gate') suggests a simpler organization without massive bastions (Matthiae 1989, 141–147; 2001, 34–40). In spite of these differences, forts and fortresses are certainly part of a planned urban project of the Old Syrian town, they were under the control of high officials, and therefore they were a direct expression of royal power (Pinnock 2001; Peyronel in press).

This contribution deals with the evidence concerning the last phase of the Western Fort (MB IIB) and that relating to the re-occupation of the area immediately after the siege and the destruction of the Old Syrian town, namely during the Late Bronze I(A), trying to define the chronology of the area in this period through the stratified ceramic materials and other diagnostic finds. The Middle and Late Bronze sequence of Area V spans from the construction of the massive earthenwork rampart at the beginning of Middle Bronze I (Phase 1, beginning of the II Mill. BC, *c.* 2000/1950 BC), till the re-occupation of the Fort area with private dwellings created

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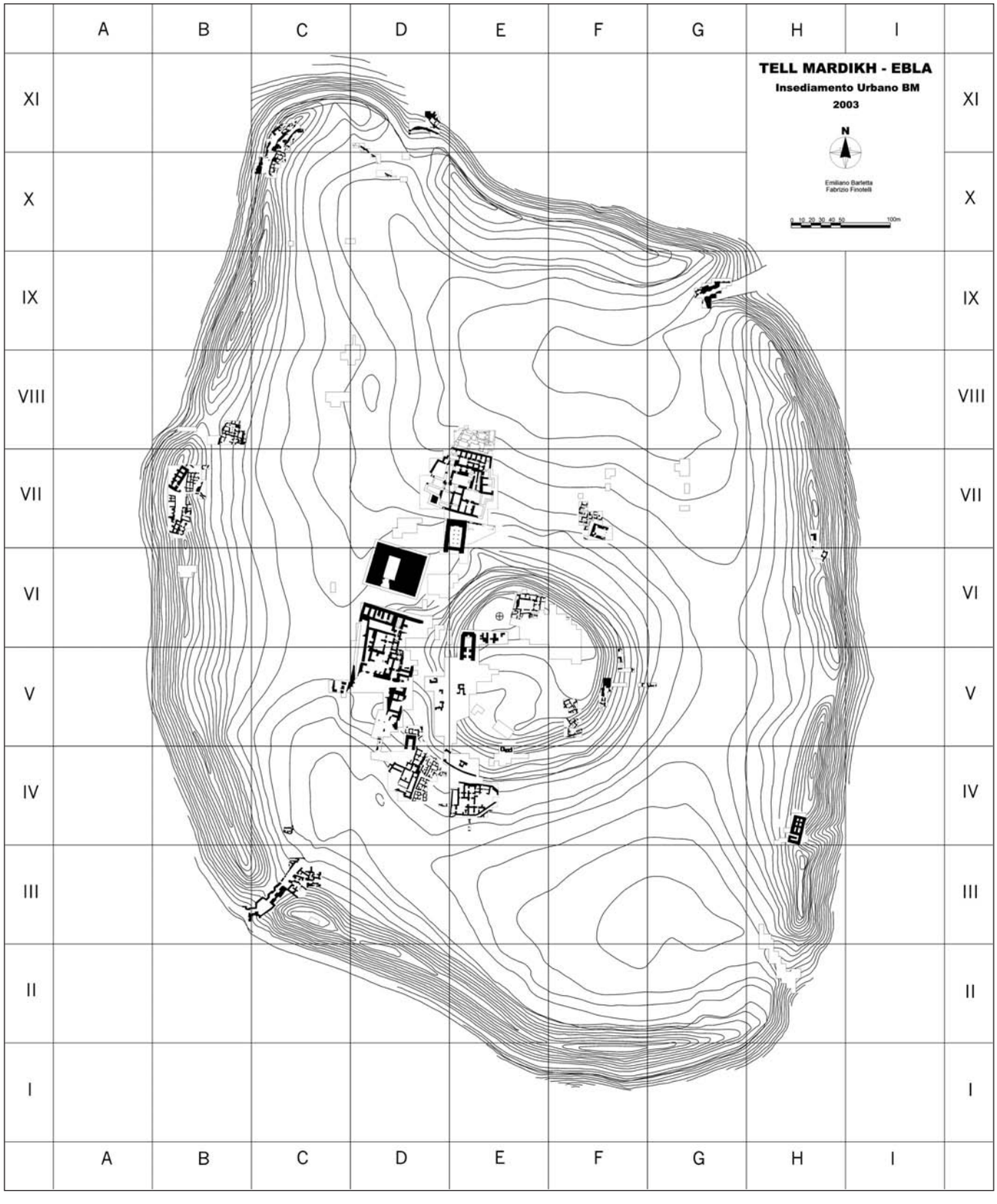


Fig. 1 Topographic plan of Tell Mardikh-Ebla



Fig. 2 Western Fort. Aerial view

partially dismantling the eastern retaining wall, and by re-using some rooms of the upper western wing (Phase 4, *c.* 1550–1400 BC).[1]

The Middle Bronze II – The Western Fort

As referred to above, the Western Fort is an intricate and functionally diversified complex with several quarters or units organized around the upper central courtyard (L.6315+L.6621) (Fig. 3). These units are not always on the same axis, because they follow the curve of the rampart, which was planned in order to control the foot of the fortification. The western structures of the Fort collapsed outside and the plan cannot always be integrated, especially as regards the external system of retaining walls. The South wing was divisible into two different units: the South-East is connected with the court through a staircase and pivots upon a room (L.6427), devoted to primary activities related with food processing and preparation, as revealed by several grinding facilities found *in situ* on the floors together with simple and preservation wares (Fig. 4); the South-West unit, located on the outer slope of the rampart, is characterized by a large rectangular room, with the entrance from the court flanked by orthostatic limestone slabs, and could be considered a kind of treasury. A third south-eastern sector is characterized by two parallel rectangular courts paved with flat basalt stones, reached from the central court through a small square vestibule with a staircase on one side. In all the units mentioned, the floors were sealed by the destruction layers which yielded large amounts of pottery dating to the last phase of the MB II (Mardikh IIIB2). The central courtyard is flanked on the western side by two separate groups of chambers with the typical architectural elements of the 'palatial' Old Syrian architecture, which seem to indicate residential functions for the western wing. It is important to underline that, in some of these rooms, we have identified an architectural phase dating from the LB I, which testifies to the re-occupation of parts of the fort after its destruction. The East is lower-lying and includes a ramp flanked by two wings with rows of small rooms, probably a block for soldiers and storing rooms with two peculiar chambers at the corners (Figs. 5–6).[2] The ramp

[1] For the complete sequence of Area V see Peyronel 2000, 1354. The rampart was quite probably built at the very beginning of the MB IA (beginning of the II Mill. BC), between the end of the EB IVB settlement and its use as burial area during the MB I (see Matthiae 1989, 140–143; 2001, 30–34). Recently published by F. Baffi Guardata (2000), the burials of the Western rampart show funerary pottery assemblages well-dated to the late MB IA and MB IB horizons of North Inner Syria. Among the most peculiar vessel types are the so-called Gublite bowl and the proto-carinated bowl for the MB IA; the bowl with high carination and out folded rim and the so-called collared bowl for the MB IB (see also Nigro 2000; 2002b). The Fort was probably built around the end of the XIX or at the beginning of the XVIII cent. BC, and the complex was used, with smaller architectural changes, until the end of the Middle Bronze Age (Mardikh IIIB; Area V Phase 3A–B, *c.* 1800–1600 BC), when heavy destruction brought the building to a sudden end. For historic and cultural considerations on the destruction of the Old Syrian town at the end of the XVII cent. BC, see Matthiae 1989, 55–56; 2002, 572–574; in press b; see also Klengel 1992, 80–83.

[2] Cf. *infra* note 20 for an hypothesis on the function of these rooms.



Fig. 3 Western Fort. The upper courtyard and related units with the modern village of Tell Mardikh in the foreground



Fig. 4 Western Fort. South-East unit with grinding facilities *in situ*

connects the upper court to an irregular narrow court parallel to the eastern external wall of the Fort which is a huge structure more than 3 m large, with the main entrance to the fortified complex from the Lower Town (Fig. 7). This wall intersects the northern retaining wall, very badly preserved but still recognizable on the inner slope of the rampart. Finally, the north-western corner is occupied by the massive rectangular bastion named Fortress V (Fig. 8), opening to the East into an irregular triangular court (Peyronel 2000, figs. 2–3). The Fortress is composed of a wide vestibule (L.6522) and of 6 non-communicating chambers 3 by 4 m large. From the vestibule the upper floor is accesible by a staircase, the understairs and the room L.6516. All of the rooms were filled with thick layers of ash and burnt bricks from the destruction of the Fort, as a consequence of the fire which spread throughout the



Fig. 5 Western Fort. L.7400

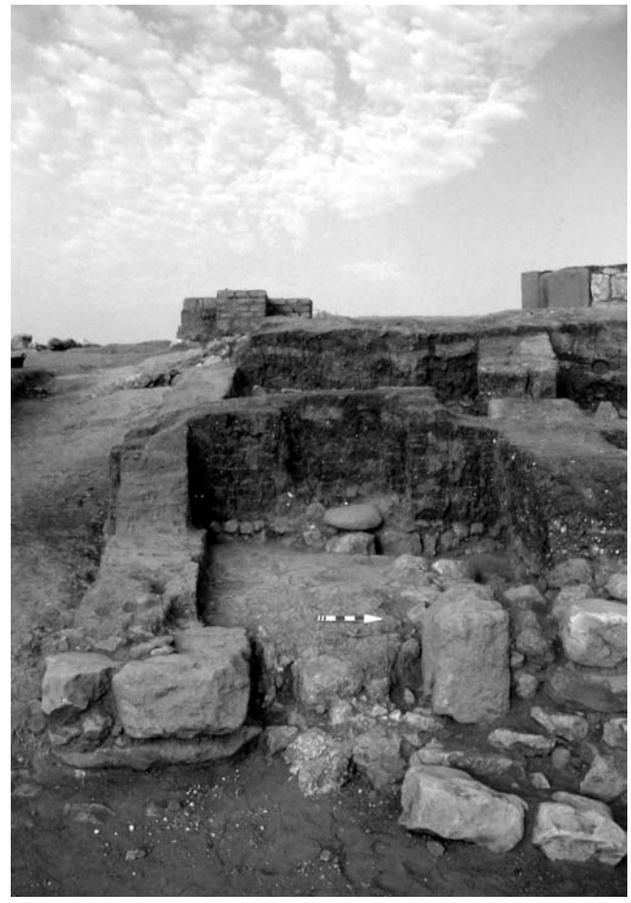


Fig. 6 Western Fort. L.7113



Fig. 7 Western Fort. The eastern external wall with the entrance



Fig. 8 Western Fort. The Fortress V



Fig. 9 Western Fort.
Burnt skeletons in front of the Fortress entrance

structure. A large amount of pottery and small finds was found in the vestibule and adjacent room. All of the materials must be attributable to the last phase of MB II (Peyronel 2000, 1359–1364). Also in the triangular court we have found thick layers of ash and burnt bricks and just in front of the entrance two completely burnt male skeletons were discovered lying directly on the paving, positive proof of the battle on the fortification during the siege of the Old Syrian town (Fig. 9).

A test-pit in one room of the Fortress has revealed that the construction of the building can be dated to the beginning of the MB IIA or slightly before, because of the presence of distinctive pottery types such as inturned-rim bowls related to the first floor. The same pottery horizon was recognized in the lowest level of the central courtyard of the Fort, where another probe was dug in order to test the chronological sequence.

The undisturbed archaeological deposits dating from the end of MB IIB which covered the majority of the structures, were produced by a single event, namely the destruction of the Fort during the siege of the city: due to the sudden nature of the event a large amount of pottery and small finds could be uncovered *in situ*. This enlarged the ceramic corpus of Mardikh IIIB2 (dating from the second half of the 18th century) most importantly with the addition of complete objects, as found in the destruction layers of the



Fig. 10 MB IIB pottery assemblage and grinding tools from the Fortress floor level

Western and Northern Palace, the Western Residence, the private houses in Area B and now by the Southern Palace (see Matthiae 1989, 216–220; Nigro 1998, 282 and tab. 2; 2002a, 321–327; 2002b, 111–112; Pinnock 2005).[3]

The quantitative evaluation of thousands of body fragments and several hundreds of rims and bases collected from undisturbed layers attributed to floors or to the debris collapsed over them is actually still in progress but it is possible to present here some general observation on the ceramic assemblage linked to the last phase of the Western Fort.

The pottery shows several diagnostic shapes of the Simple, Painted Simple, Kitchen, and Preservation Wares, frequently documented by complete specimens (Fig. 10). The most diagnostic shapes in Simple Ware are represented by small bowls of different sizes with thin flaring rim thinner at the edge, pronounced shoulders and ring-base and by the deep bowls with large flat base, gentle carination and swollen rim: they are known only during the MB IIB phase in conjunction with hemispherical bowls with an outwardly grooved rim; The same attributes are also attested in the Fort together with some examples of fine and thin-walled "Palatial" Ware or White-Slip Ware, an extremely fine metal-like production of a few basic shapes.[4]

The most peculiar type of large open vessel is the deep bowl with a gentle carination and out-turned expanded rim and flat base, usually in a whitish or yellow-whitish fabric, well-represented in the pottery assemblage of the Fort with several sub-types, whereas the in-turned rim bowl, which is the hallmark of the MB IIA period, continue being used, albeit their percentage in the Fort is very low (20% of the open shapes). Particularly worthy of mention among the closed Simple Ware shapes are the medium-size jars with combed decoration on the shoulder and double or everted rim (Fig. 11 left), and the ovoid jars or jugs again with combed decoration, with thickened band rim and slightly convex base, which can be rated the final evolution of types appearing during the MB IIA (Fig. 11 right).

Two interesting vessels were found in the Fortress room L.6516: the first is a fragmentary dipper juglet in greenish clay with pointed bottom, which is a very rare shape at Ebla; it shows striking analogies with juglets very common in Palestine at the end of the MB Period (Peyronel 2000, fig. 10b). The second is a pear-shaped juglet with button base and vertical black burnishing (Fig. 12; Peyronel 2000, fig. 10a). Black-Burnished-Ware is well known at Ebla during the MB IB and MB IIA, with inner stepped-rim bottles and juglets coming exclusively from burials, whereas open

[3] For a revision of the pottery assemblage chronology of Hama H which belongs to the same ceramic area of Ebla during the Middle Bronze Age, see Thuesen, Ribbe 2000 and especially Nigro 2002b.

[4] For this specialized production see Nigro 2003, 359–360 (Syrian White Slip Ware).



Fig. 11 MB IIB Simple Ware jars and cooking pot



Fig. 12 Black-Burnished piriform juglet from Fortress V (TM.96.V.4183)

shapes such as bar-handle bowls have been retrieved also in cultic or religious contexts (i.e. the *favissae* in Area P South) (Nigro 2003, 351–353). This very peculiar metallic production almost completely disappears during MB IIB, and the specimen from the Fortress could be linked to a southern tradition since piriform juglets are widely attested in Palestine and in southern and coastal Syria during MB II–III.

Kitchen Ware is widely represented in the pottery of the Fort, with several complete specimens found on the floors, sometimes near andirons or *tannur* (Fig. 13): the open shapes include large dishes with circular burnishings and thickened horizontal rim with an outer groove, typical of the later phase of MB II, inturned rim bowls and large bowls/dishes with carination and outwarded rim, all of them with burnishing inside and out. Cooking pots are attested by round types with everted square or simple rims (Fig. 11 middle)(Peyronel, Spreafico in press).

Several large and deep pottery vats and basins, decorated with ridges or combed grooves, were found scattered around several rooms of the building; they represent the peculiar equipment for keeping fresh water. Preservation jars are represented mainly by the types of *pithoi* with swollen rim and ridge decoration on the shoulder, usually at the junction between the wheel-made neck and the spiral-coiled body, although no definite storerooms have been identified, like the ones of the Northern Palace in the Lower Town (Dolce 1990, 126–127).

Clay figurines fit very well into the general Eblaic typology of MB IIB elaborated by N. Marchetti, who recently published the corpus of the Middle Bronze materials from the 1964–1980 campaigns (Marchetti 2002; see also Marchetti 2000 and Marchetti in press). Out of more than 300 specimens from the Area V excavation, one half comes from the destruction layers of the MB IIB. Generally speaking, the assemblage from the Western Fort is characterized by mass-produced specimens with a lesser use of details in the case of the MB I and MB IIA choroplastic: female figurines (*c.* 50 specimens) are always characterized by the basic type with hands clutching the breast (Marchetti 2000, fig. 21); males (*c.* 50



Fig. 13 Andiron and cooking pots in the vestibule of Fortress V



Fig. 14 Sealings from L.6516: a) TM.96.V.223; b) TM.96.V.214



Fig. 15 Sealing TM.96.V.180 from L.6516

specimens) show the predominance of riders and charioteers associated (Marchetti 2000, figs. 34, 36, 39) with chariots and chariot wheels (*c.* 20 specimens); Animals (*c.* 15 specimens) presenting mostly ram, equids and cattle.[5]

Among more than five hundreds small finds coming from the destruction layers of the Fort, those which have the most important chronological bearing are certainly the seals and sealings: four cylinder seals, one Hyksos scarab and three sealings with impressions of two different cylinder seals were found in the Western Fort.

The sealings come from the Fortress room L.6516. Two brown-reddish clay sealings, which must be door-

[5] Cf. for the MB IIB materials Marchetti 2000, 50–59 (females), 75–82 (males), 105–114 (quadrupeds), 126–128 (chariots and wheels); for distribution of types during the different MB phases, see 138–142.

sealings, showing the impression of a peg/knob with cord latching, bear single (TM.96.V.223; Fig. 14 left) and multiple (TM.96.V.214; Fig. 14 right) albeit incomplete impressions of the same cylinder (only the lower half visible) at the back (Peyronel 2000, 1362, fig. 14).[6] However, the original scene can be reconstructed: the god Hadad/Baᶜal holding the mace and the reins with the bull squatting at his foot and facing right is probably followed by the goddess Khepat/ᶜAnat (almost completely missing) wearing a long cloak, and a human figure, with one hand raised in prayer, is standing in front of the two deities.[7] A sealing from the destruction level of Sanctuary B2 (Baffi Guardata 1979) with three impressions of the same cylinder showing two worshippers, a goddess and possibly a god, points to the presence of a distinctive style at Ebla characterized by a seal-cutting technique with extensive use of drill and cutting-wheel (in the seal, evident in the rein held by the god), identified for the first time by B. Buchanan (1970) and thought to be peculiar to the late Old Babylonian glyptic. In Syria this 'drilled' style is also attested at Alalakh VII (Collon 1975, nn. 132–135), Ugarit (Schaeffer-Forrer 1983, 41-R.S.10.029, 45-R.S.20.53), Emar (Beyer 1990; 2001, 165–172 esp. D18–22) and Terqa (Gualandi 1997, 150–151).[8]

The second seal is rolled three times on a completely burnt sealing (Fig. 15).[9] It shows three figures and a very damaged cuneiform inscription in which it is still possible to read without doubt *dub-sar* 'scribe', and possibly the name *Te-ir-she*.[10] On the left a goddess with a large cylindrical tiara dressing a *kaunakes* skirt receives two human figures: a child and a male character, both with hands raised in prayer. The style is engraved in a modelled late Old Syrian style, and it is arguable that the seal's owner was a high-ranking official of the fort.[11] It is difficult to trace a close parallel for the impression, although a cursory seal of unknown provenance now in the Aleppo Museum (Hammade 1987, n. 151) shows two figures facing each other, the left one with a welcoming gesture and the other with one hand raised in greeting; between



Fig. 16 Faience scarab TM.98.V.520



Fig. 17 Cylinder seal TM.97.V.160 (modern impression)



Fig. 18 Cylinder seal TM.97.V.240 (modern impression)

[6] TM.96.V.223: 3,3 × 3,5 cm, thick: 1,8 cm; TM.96.V.214: 2,6 × 2,4 cm, thick: 1,5. Both were found in L.6516 near the door.

[7] The iconographies are peculiar to the Classic or Mature Old Syrian glyptic. The two gods are known at Ebla from the famous cylinder seal impression on jars, which probably belonged to the Crown Prince, son of King Imdilingur (Matthiae 1969; 1980, 114–117, fig. 14; 2000, 607–608, figs. 31–32; Matthiae *et al.* 1995, n. 242). For the shift to a Hadad predominance in the iconographic repertoire at Ebla during the MB II, see Matthiae 2003.

[8] The use of a tubular drill in Syria seems to precede its introduction in Southern Mesopotamia and therefore cannot rule out the possibility of a Syrian influence on the late Old Babylonian production: see Beyer 2001, 171 and Baffi Guardata in press.

[9] TM.96.V.180; 3,2 × 2,9 cm, thick: 1,2 cm. The sealing was found on the floor near a group of small bronze and stone objects together with wooden burnt remains; therefore it was probably used to seal a wooden box, since the back shows also wooden impressions.

[10] The read of the inscription was done by M.G. Biga whom I sincerely thank.

[11] If the read of the name Teirshe is correct, we could have an important clue to the presence of an official involved in the administration not only of the Western Fort, but of the defensive system in general, since a bronze spear-head inscribed with the same name and title was found in the Fortress East-South-East (Area M) on the eastern rampart: see de Maigret 1976, 34; Peyronel in press, fig. 4.



Fig. 19 Cylinder seal TM.98.V.70 (modern impression)

the characters, a third small figure is depicted facing right, and with one arm lifted (differing from this impression, where the child is turned towards the goddess and raises one hand in the same gesture of the worshipper).[12]

One Hyksos scarab in faience with whitish glaze dating from the Second Intermediate Period (Fig. 16) was found in one room (L.7405) of the eastern units south of the ramp.[13] The back is of a simple type with lunate head and no lines dividing the elytra. It shows a fish-bone design, attested in a group scarabs from Palestine.[14] It is probably attributable to the XIII–XV dynasties, although it is difficult to use it for a more precise chronological correlation.[15]

A steatite cylinder seal coming from the same room (Fig. 17; Baffi Guardata in press, n. 3)[16] can be easily ascribed to the Syro-Anatolian 'common style': it shows two figures facing left, a ball-and-staff symbol and a caprid. Several seals from Mardikh III belong to this glyptic style: they were previously dated by S. Mazzoni (1975; 1979; 1980) to the MB I (Mardikh IIIA, *c.* 2000–1800 BC),[17] but recent findings from secure Mardikh IIIB contexts (such as the case of our specimen) seem to point to a long-time span of use, until the end of the Old Syrian period (cf. Baffi Guardata in press).

Another steatite seal from L.7108, a room of the eastern unit north of the ramp (Fig. 18; Baffi Guardata in press, n. 6)[18] is engraved in a cursory style with a banquet scene, which resembles the Syro-Anatolian 'common glyptic': a seated and a standing figure are placed on both sides of a schematic table bearing loaves and a globe; on the left, a double register divided by a simple line with a squatting sphinx and a goat below is seen. Symposia scenes are often represented especially in the archaic Old Syrian period and the double register frieze with animals and fantastic creatures (mainly sphinx or griffin) combined together frequently appears, usually during the mature and late periods (*c.* 1850–1600 BC).[19]

From L.7400, a small square room joined at the south-eastern corner of the eastern unit south of the ramp, comes a dark green steatite cylinder seal (Fig. 19).[20] It is a well-executed Old Syrian seal with two figures between a large *ankh* symbol and a crescent above.[21] The left figure raises one hand in wor-

[12] We must bear in mind that the insertion of a small figure between two main figures is a scheme well known in a group of Old Syrian seals: see i.e. Teissier 1994, nn. 526–527, 529–530, 533, 540, 543.

[13] TM.98.V.520; 1,7 × 1,3 cm, thick: 0.8 cm.

[14] See i.e. the scarabs from the Jericho and Far^{c}ah South tombs: Tufnell 1984, pl. I: 1007–1011; cf. Keel 1995, 164.

[15] On the chronology of the 'Hyksos' scarabs, see O'Connor 1985. For other MB scarabs found at Ebla, see Matthiae 1995, 678; Scandone Matthiae 1976; in press; for a general outline of the interrelations between Ebla and Egypt, see Scandone Matthiae 1997.

[16] TM.97.V.160; h. 1,9 cm, d.: 0,8 cm.

[17] Some seals were also published by H. Hammade (1994, nn. 378–387) and wrongly dated to 1700–1600 BC.

[18] TM.97.V.240; h. 2,5 cm, d.: 0,9 cm.

[19] Cf. i.e. Buchanan 1981, nn. 1222 (sphinx? and ibex), 1227 (griffin and antelope); Teissier 1984, nn. 447 (sphinx and ibex), 456 (sphinx and antelope); the frieze is usually divided by a scroll, but simple lines are also attested to.

[20] TM.98.V.70; h. 1,6 cm, d.: 0,6 cm. This room (see Fig. 5) has no entrance and was filled by an ash layer with crushed MB IIB pottery and small objects. It is a later addition as the specular room at the opposite north-eastern side of the ramp (L.7113). The latter room (Fig. 6) is characterized by a niche on the western wall with a round basalt stone at the bottom and could have acted as a shrine or chapel of the fort (cf. Matthiae 2001, 46). If this is the case we can postulate that L. 7400 is related to the chapel as a place where offerings were performed or remains of some ritual acts involving burning activities were kept.

[21] For the presence of *ankh* in Syrian glyptic, see Teissier 1996, 158; Otto 2000, 264–265.



Fig. 20 Cylinder seal TM.96.V.276 (modern impression)

ship and the right one holds a cup. A pair of scorpions with curved tails and segmented bodies flanking the human figures is depicted in a tête-bêche composition with a fork-like motif above.[22] This seal belongs to the same mature Old Syrian group of the sealing TM.96.V.160 and should be dated to the XVII cent. BC; the scene with two standing men, one holding a cup and the second in a gesture of devotion, can be compared with a seal from Cyprus dating to MB IIA–B (*c.* 1850–1750 BC), where two characters and an *ankh* are depicted (Schaeffer-Forrer 1983, 64 Cyprus A13), and with a MB specimen found at Hattusa with a standing man and a seated figure holding a cup with a *ankh* below (Otto 2000, n. 407). The scorpions, which – when the seal was rolled – flank the figures on both sides, and the large ankh symbol in the centre of the composition are clear reference to the fertility and 'life' themes and point to an interpretation of the scene as an homage to the king.

The most remarkable seal from the Fort is certainly the hematite small cylinder (Matthiae 1997, 11–12; Pinnock 2000, 1400, fig. 3b) from the central quarter of the Fort from an MB level of bricks collapsed over the courtyard L.6621 (Fig. 20).[23] It is intact, with some abrasions and small chippings, and shows a scene with three characters and a vertical frieze divided by simple lines and by a rope motif into three parts. The main scene is represented by three figures: the first on the left, wearing a long robe, is standing with one leg uncovered and streched out whilst raising the left hand towards the second seated character, facing right, holding a small globular jar with neck and outturned horizontally expanded rim, dressed with a *kaunakes* skirt; the third figure, nude or with a short girdle, stands behind the seated man raising an unidentified vertical object. The scene is interpretable as an homage (by the first figure on the left) to a (deified?) king (the seated figure) with an attendant or servant to the back. Behind the presumed worshipper is a standard with schematic double human heads fastened to a pole (see Otto 2000, 266–267), considered by H. Seryig (1960) to the forerunner of the *semeion* described by Lucian of Samosata in *De Dea Syria* and now strongly linked to the North Inner Syrian milieu and Ebla, as P. Matthiae has recently pointed out (Matthiae 1994 and in press a).[24] The vertical frieze is divided into three superimposed parts, the upper with two unidentified symbols or objects, the middle with a figure of a crouching lion facing right and the lower with two human heads. The seal is dated to the late MB IB period or to the beginning of MB IIA, around the end of the XIX–beginning of the XVIII cent. BC. and it was probably kept in the Fort for its high symbolic value.[25]

Summing up, the glyptic evidence from the Western Fort can be dated to the XVIII–XVII century BC and show different contemporary styles, from the Syro-Anatolian and schematic seals, to the drilled style specimen on the sealings, to the mature Old Syrian examples. This variety reflects the co-existence of different workshops at Ebla during the MB II, probably linked to a difference in the social position of the owners, as well as some possible chronological staggering. The production originated during the MB I and continued being used in the following phase side-by-side with new glyptic styles which can be rated peculiar to the last Old Syrian sphragistic (such as the very distinctive drilled style). However, the still limited group of seals from Mardikh IIIB prevent a

[22] Pairs of scorpions frequently appear in Old Syrian glyptic (see i.e. Collon 1975, nn. 97–101 for Alalakh; Otto 2000, 256, nn. 207 (British Museum), 241 (Hatay Museum), 283, 285 (Marcopoli Coll.), albeit never in the specular representation with curved tail.

[23] TM.96.V.276; h. 1,7 cm. d.: 0,9 cm.

[24] See also Marchetti 2003 for a tablet from Sippar dating to the time of Hammurabi, bearing an Old Syrian cylinder seal impression showing the two-headed standard (fig. 1) and for a meaningful seal from Hammam et-Turkman showing the king with peaked cap in front of a god with the pole with human heads at the back worshipped by a male figure (fig. 6). The ruler with the peaked cap (see Teissier 1993) was convincingly interpreted by P. Matthiae (1994) as the representation of the Eblaite king during the Old Syrian Period.

[25] The other seal with double head's standard was found in *favissa* F.5238, located on the Cisterns Square of the Ishtar's Sacred Area: Matthiae 1993, 659–660, fig. 25; Marchetti, Nigro 1997, 32, fig. 19; Matthiae *et al.* 1995, n. 275.

definitive typological classification of the Eblaic material, with the exception of the Syro-Anatolian common style group. Finally, the presence of sealings in the Fortress points at the administrative control of the building, also indicated by several balancing weights found scattered over different rooms of the Fort (Ascalone, Peyronel 2000).

The Late Bronze I – The dwellings inside the ruins of the Western Fort

As previously remarked, after the destruction of the Fort, the Late Bronze I period (Mardikh IVA) is represented by two different kinds of stratigraphic and architectural evidence.

In the lower north-eastern part of the Fort, along the massive external wall, a dwelling unit was built by dismantling and re-using the stones of this wall. A room of this house (L.6352) was partially excavated and inside it, a fair amount of pottery and some objects were found *in situ* (Fig. 21). A large pit to the North and the surface to the East prevented the conservation of the structures; however, two doors opening to the South and to the West lead us to reconstruct the existence of other two rooms.

An andiron full of ash with three cooking pots near it was found in the South-Western corner (T.6353); other vessels were located along the Southern wall and a complete jar was found to the North (Fig. 22). It is very probable that the fill with a layer of red clay with limestone crumbs, which level up the area immediately to the West of the external wall, might be ascribed to the same period, when the entrance of the Fort was also blocked. Conversely, no Late Bronze evidence comes from the ramp where the last floor is firmly dated to the MB IIB.

The second context of this phase was located on the western top of the Fort. Here the northern rooms of the Western unit were re-used and partially re-adapted for a dwelling. Four rooms were obtained with partition walls and the large room L.6318 was probably re-used as a court. The stratigraphy shows that no MB destruction layers are present in these rooms (Fig. 23). The presence of two different architectural phases of this LB occupation seems to be attested by the final block of the doors which may indicate a progressive abandonment of part of the house.

The pottery collected at the LB levels is all local

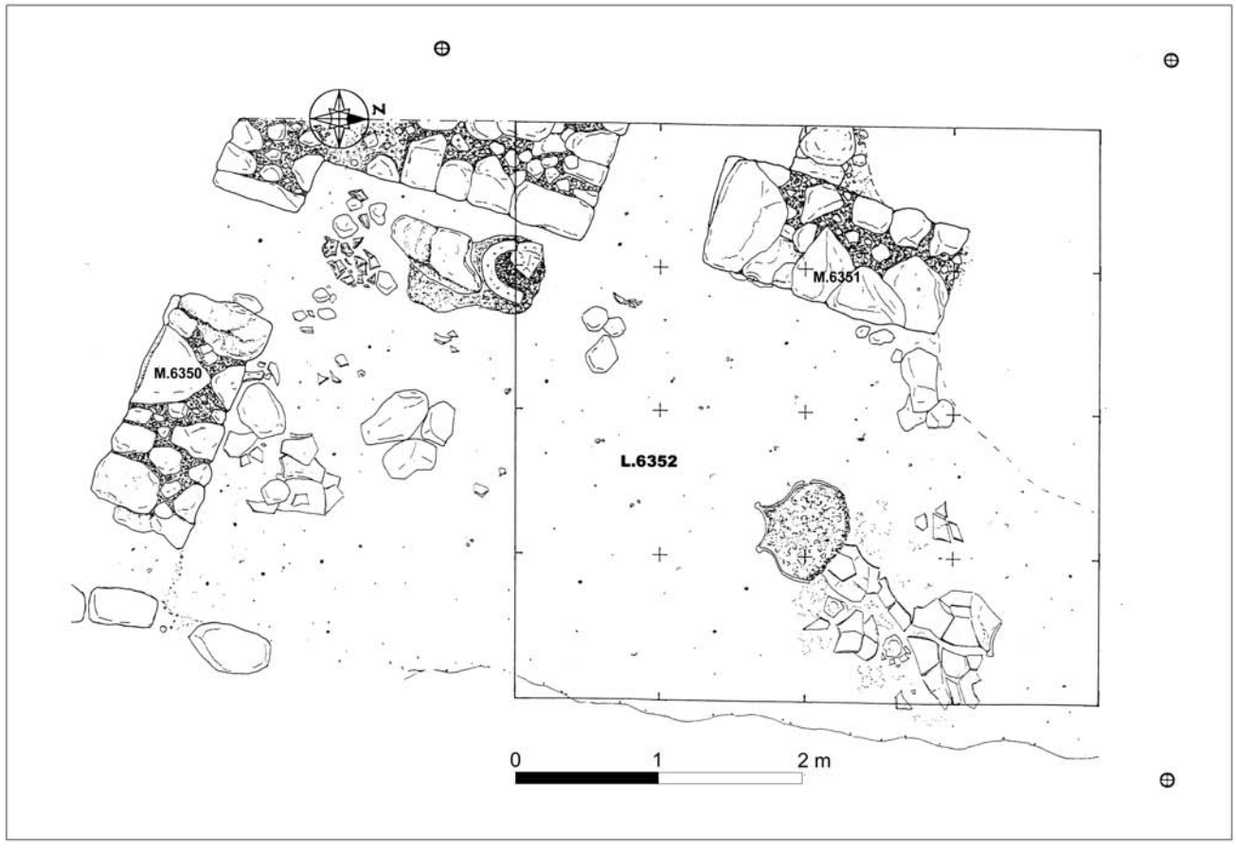


Fig. 21 Plan of the LB I dwelling from Area V (L.6352)



Fig. 22 L.6352 floor level with materials *in situ*



Fig. 23 Northern rooms of the Western unit re-used for a LB I dwelling

manufacture with a predominance of pale grey/whitish or pale brown/pinkish fabrics with mineral inclusions. The materials seem to link to an early LB horizon and permit to underline the following general observations, which demonstrate that MB/LB cultural changes were a gradual and complex phenomenon in North Inner Syria (Figs. 24–26).[26] First of all, there is a clear persistence of shapes and types attested during the last phase of the MB IIB, such as the inturned rim bowls, the hemispherical bowl, the deep krater and bowl with horizontally expanded and grooved rim (Fig. 24:4–5), the medium-sized jars and jug with banded rim. The trend towards a widespread diffusion of the ring-base, which can be observed since the MB IIB, is also sign of a continuity of ceramic traditions. However, this general typological continuity corresponds to a change in the clays and fabrics. Secondly, new shapes make their appearance, like the dish/bowl with triangular or rounded rim and straight sides (Fig. 24:6–8) and the beaker with carination and simple out-folded or outward rim, which can be considered as the final transformation of the classic late MB IIB bowl with globular body and flaring rim (Fig. 24:1–2).[27] Among open shapes a distinguished large krater of pinkish clay with high carination and outturned rim is attested by a complete specimen from the upper dwelling (Fig. 25:1). Finally, a drastic reduction of the MB tradition of kitchen ware can be observed. The horizontal burnishing almost completely disappears. Cooking pots now usually appear with short neck, more or less evident shoulder and outturned rim or everted stepped rim (Fig. 25:3–5). Preservation ware, here illustrated by the complete medium-size storage jar

[26] This still little-known pottery horizon is likely to be contemporary with Hama G3 and Grave XIII (Fugmann 1958, 117–134, figs. 143, 153.), Alalakh VI–V (Woolley 1955, 316–317; Gates 1981), Tell Afis VIA - Lev. 14 in Area E1 (Mazzoni 1998) and is comparable with Hadidi LB IA on the Euphrates (Dornemann 1981). For a recent evaluation of the Late Bronze I pottery productions of North Inner Syria, see Mazzoni 2002; note, however, that she considers Hama G3–1 as a whole and correlates it with Alalakh IV and Hadidi LB IB.

[27] This kind of beaker was also in widespread use during the LB II, is attested only by few pieces in the LB phase in Area V, whereas it is now well-defined by several specimens from the *favissa* P.5213 in the Cisterns Square (Nigro, Marchetti in press) and from large LB I refuse pits in Area B East.

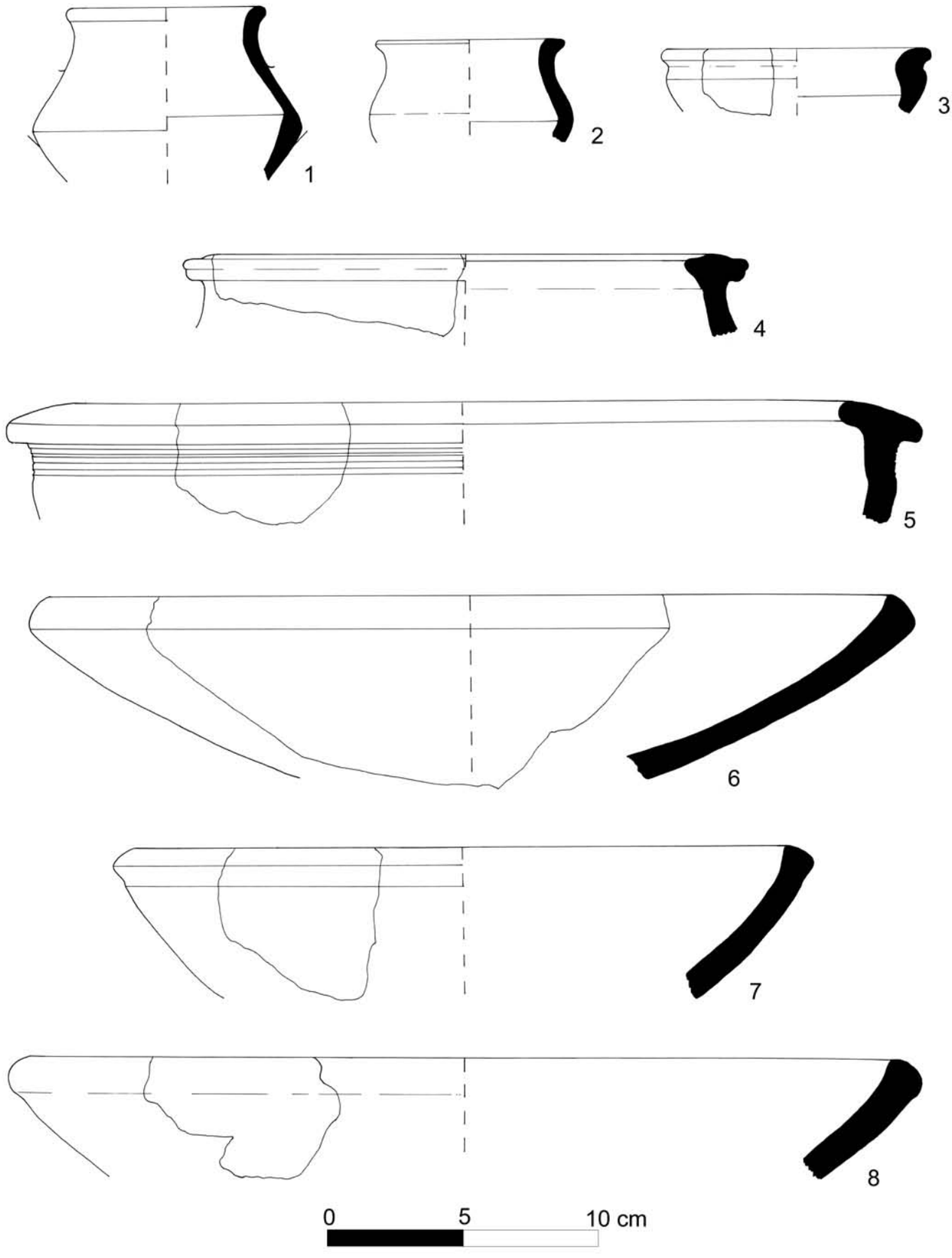


Fig. 24 LB I pottery from Area V: 1) TM.95.V.404/1; 2) TM.95.V.419/6; 3) TM.95.V.419/5; 4) TM.95.V.419/4; 5) TM.95.V.419/3; 6) TM.95.V.418/1; 7) TM.95.V.418/2; 8) TM.95.V.419/1

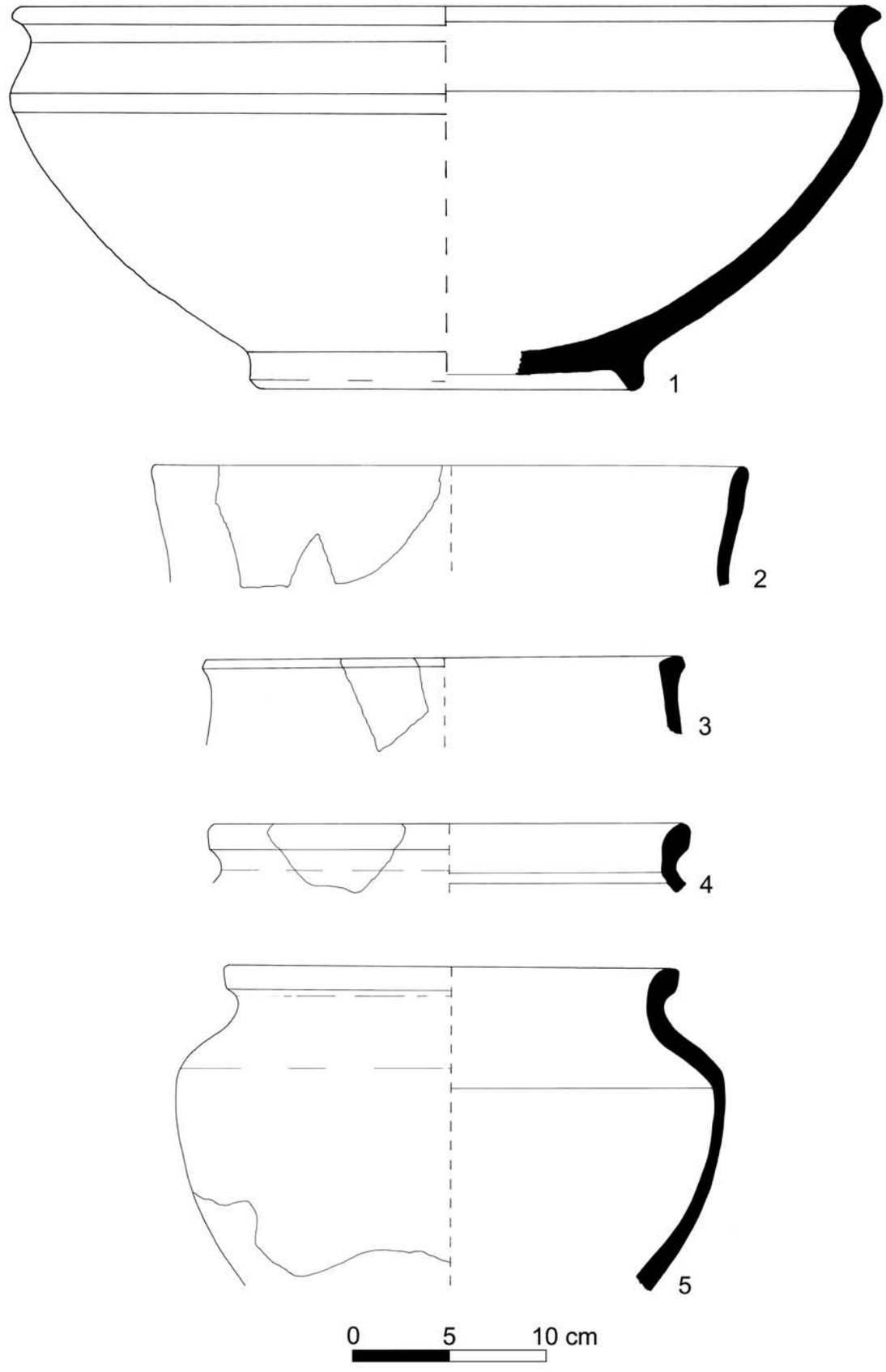


Fig. 25 LB I pottery from Area V: 1) TM.95.V.419/11; 2) TM.95.V.418/4; 3) TM.95.V.418/3; 4) TM.95.V.410/1; 5) TM.95.V.419/9

found in the south-eastern house (Fig. 26), also demonstrates the link with the MB II tradition with the jar with oblique shoulders, ovoid body marked by rope-ridges and expanded square rim.[28]

The Late Bronze I at Ebla – the missing link?

This preliminary analysis of the final MB II and LB I periods in Area V, leads us to point to the actual evidence of Late Bronze I at Tell Mardikh-Ebla. Thanks to the recent discoveries in the Western rampart and in Areas P South, B East and FF, an increasing volume of documentation of this ephemeral period at Tell Mardikh-Ebla is now available. After the destruction of the Western and Northern Forts private houses were built testifying to an occupation of the rampart probably during the XVI and XV century BC, revealing a similar situation recorded at the Acropolis, where an LB building was found above the ruins of Royal Palace E at the beginning of the excavations at Tell Mardikh. Moreover, a continuity of use in the dynastic temple of Ishtar in Area D and a rearrangement of the nearby shrine G3, show a survival of the cult of the Eblaic goddess shortly after the destruction of Mardikh IIIB2.

In the Square of the Cisterns of the Sacred Area of Ishtar where two *favissae* were cogent signs of evidence of the cultic and ritual activities at Ebla during MB I and II (Marchetti, Nigro 1997; 2000), a third votive pit (cistern P.5213) has revealed a level filled with offerings and materials dating from an early LB period (Marchetti, Nigro in press). This important item of evidence attests without doubt the continuity of the Old Syrian religious tradition. Moreover, the presence at the bottom of the cistern of a level with materials from the MB IIB demonstrates that the *favissa* was re-used shortly after the destruction of the town. Finally, the presence of large refuse pits full of pottery sherds in Areas B East and FF, over large private houses of MB IIB and over part of the Southern Palace, seem to indicate that an important building or a concentration of houses of LB I could be located in the nearby area.

In conclusion, what at the beginning of the archaeological exploration of Tell Mardikh had originally been considered as an ephemeral occupation of LB I–II, linked to few dwellings on the Acropolis which spanned four centuries (Matthiae 1989, 56), can now be regarded as an occupation, certainly dispersed at the Tell, but probably linked to a re-occupation of the site during the XVI and XV century and which is deserving of closer analysis, in order to verify and investigate not only the transition of the material culture at the midpoint of the Millennium, but also the more general cultural and socio-economic aspects in the crucial historical passage between the Old Syrian and Middle Syrian periods in North Inner Syria.

Acknowledgments

I wish to express my deepest thanks to Prof. P. Matthiae who gave me the opportunity to study the Western Fort and encouraged me throughout; I also sincerely thank F. Pinnock, L. Nigro, N. Marchetti and E. Ascalone for their invaluable advice and discussions on several topics of this article. Pottery drawings are by A. Angelini, photographs and plans by the archive of the Italian Archaeological Mission at Ebla (©MAIS).

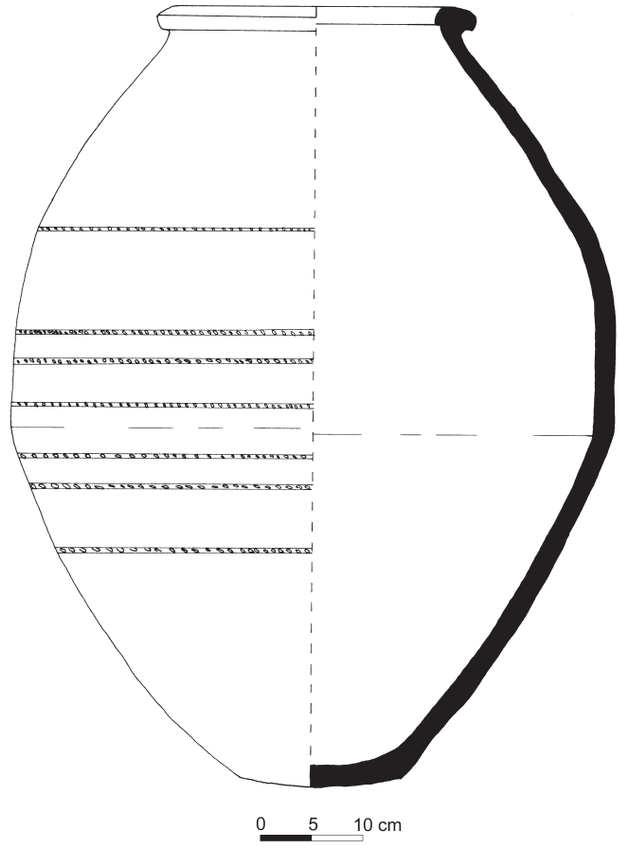


Fig. 26 LB I storage jar from L.6352 (TM.95.V.419/10)

[28] This preservation jar finds good parallels at Hama G (Fugmann 1958, fig. 143 R1) and Mishrifé – Palace phase G7 (Novak, Pfälzner 2002, pl. 27).

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New Research on Middle Bronze Age Chronology of Western Syria

Uwe Sievertsen[*]

In my paper I would like to sketch an ongoing research project at the University of Tübingen. The main aim of this project is to refine the framework of the relative chronology of the Middle Bronze Age in Western Syria on the basis of an analysis of stratified pottery assemblages. In the following, I define the term „Western Syria“ as the area to the west of the Euphrates, to the east of the Mediterranean Sea, to the north of the Lebanon mountains and to the south of modern Turkey (Fig. 1).

The beginnings of archaeological exploration of the Middle Bronze Age in Western Syria go back to the time immediately after the First World War, when Syria and Lebanon had become the mandate of France. This led to intense archaeological fieldwork by French archaeologists throughout the region. Excavations were undertaken at several important archaeological sites, for instance Tell Nebi Mend, the ancient Qadesch,[1] Qatna[2] and Ugarit.[3]

By and by archaeologists from other European countries started fieldwork on Bronze Age sites in Western Syria. A Danish mission undertook excavations in Hama,[4] an English expedition dug in Alalakh[5] and in 1964 an Italian team under the direction of Paolo Matthiae began exploring the ruins of Ebla.[6] Further relevant sites are Tell Kazel in the vicinity of Tartous,[7] Tell Arqa, located on the Lebanese side of the Akkar plain,[8] as well as Tell Qarqur[9] and Tell Ascharneh,[10] both situated on the Orontes river. Finally, since 1999 new excavations

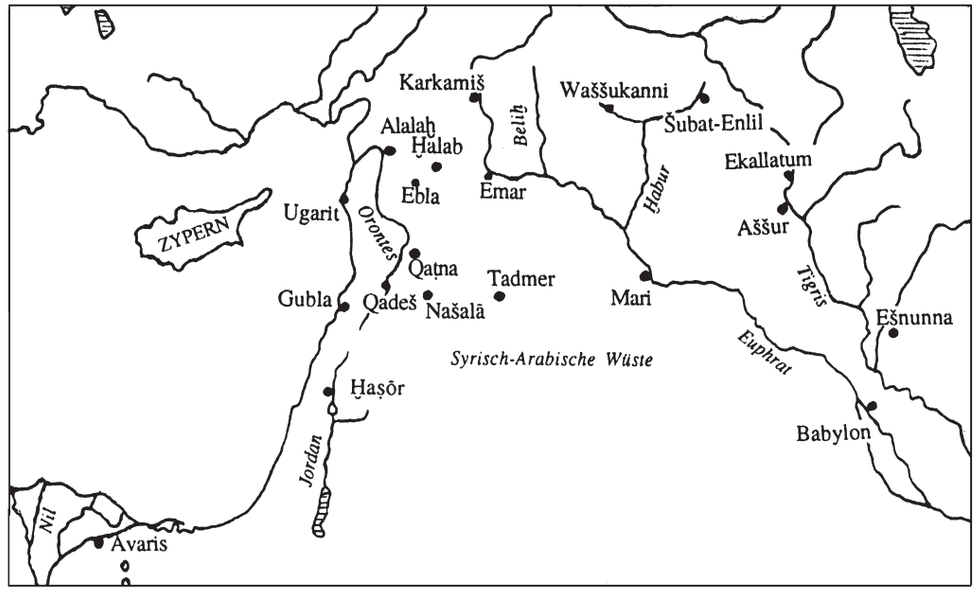


Fig. 1 Map of the Ancient Near East (after Klengel 2000, fig. 1)

[*] University of Tübingen.

[1] Pézard 1931.

[2] Du Mesnil du Buisson 1926, 1927, 1928, 1930, 1935.

[3] Yon 1997.

[4] Fugmann 1958.

[5] Woolley 1955.

[6] Matthiae *et al.* 1995.

[7] Badre and Gubel 1999–2000.

[8] Thalmann 2000.

[9] Dornemann 1997.

[10] Fortin 1999.

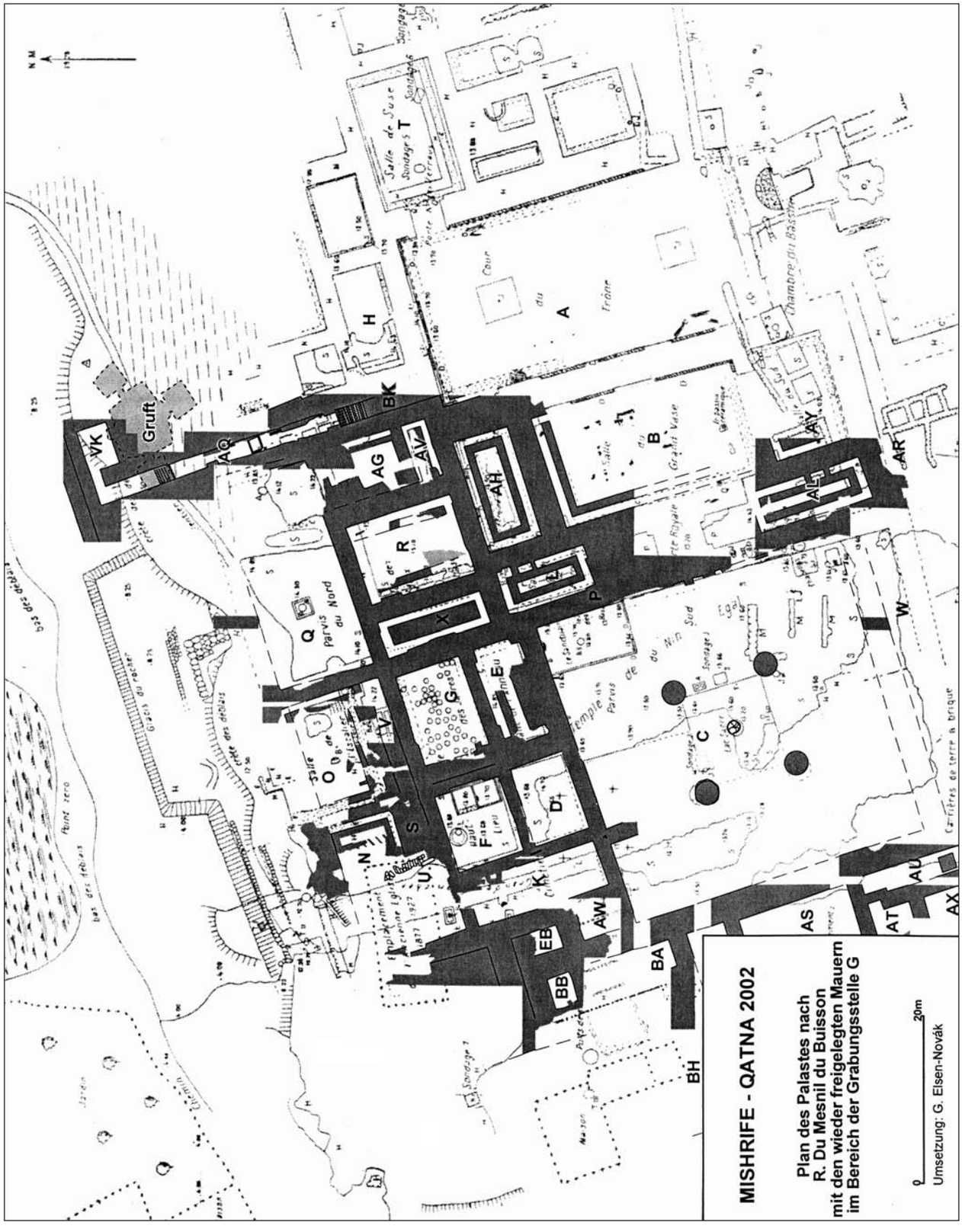


Fig. 2 Qatna, Bronze Age palace

take place in Qatna within the scope of a cooperation of the Syrian Antiquities Service, the University of Udine and the University of Tübingen. The operations of the German team concentrate on the area of the huge Bronze Age palace in the Upper town of Qatna (Fig. 2).[11] Here during the field campaigns of 2001, 2002 and 2003 Peter Pfälzner kindly gave me the opportunity to work on the newly excavated pottery material.

Besides excavations at present also surveys are carried out on a rather large scale in Western Syria, for example in the Akkar plain,[12] in the plain of Homs[13] and in regions further to the north. In autumn 2003 the German Archaeological Institute started surveying the region of Hama between Shaizar and Rastan.

To sum up, one may say that archaeological fieldwork in Western Syria is witnessing a boom. It is on this background that it seemed desirable to try and further improve the system of the relative chronology of the Western Syrian Middle Bronze Age, doubtlessly one of the most crucial periods in the history of the region.[14] Indeed, as to date there exist only a few reliable pottery sequences for the whole area, namely in Ebla and Tell Arqa, the importance of a submission of new material from stratified contexts such as those which have come to light in Qatna has always been stressed in the past.

I intend to carry out my research project in three steps: Firstly on a local level confined to the site of Qatna, secondly on a regional level restricted to Western Syria and finally on a supraregional level. On this last level the refined chronological framework of Western Syria will have to be discussed considering the pottery traditions of adjacent regions such as Southern Syria,[15] the Lebanon and Northern Palestine and with regard to systems of absolute chronology in the Ancient Near East.

Initially I would like to make a few remarks concerning the work on the local level. Here the ceramics from the current excavations of the University of Tübingen in the royal palace of Qatna shall serve as a starting point for a fine-typological pottery sequence of the Middle Bronze Age at that site.

The palace of Qatna was explored for a first time almost 80 years ago by a french expedition under the direction of Comte du Mesnil du Buisson. But diverse shortcomings of these old investigations with regard to excavation technique and documentation work left many questions unanswered, particularly concerning the chronology of the building.[16] Here indeed substantial progress has been achieved within the last five years, for a new stratigraphic sequence could be established in the palace by the Italian and German teams, working in excavation areas H and G respectively. Among the newly defined architectural phases, the phases covering the Bronze Age history of the palace in both excavation areas G and H have been termed 7, 8 and 9.[17] In the following, however, I will concentrate on the results in area G in the western part of the palace (Fig. 3).

There G 9 corresponds to the phase during which the palace was built and is characterized by fillings coming from construction floors and foundation trenches. G 8 represents the main use, while G 7 covers the latest occupation phase of the palace.

Of course, excavation work in Qatna has not yet come to an end, and at the moment only preliminary datings can be put forward. But according to the cumulative evidence of the pottery, the glyptic,

Phase	Structures	Period	Date
G 1	Modern surface, house ruins, debris	↑	AD 1982
G 2	Later houses of the modern village of al-Mišrife		AD 1929
G 3	Excavation activities of Du Mesnil du Buisson		AD 1924-29
G 4	Earlier houses of the modern village of al-Mišrife	Modern	AD 1850
Hiatus			
G 5	Later structures of re-occupation of the palace area	Neo Syrian Period	
G 6	Earlier structures of re-occupation of the palace area		1000 BC
Hiatus (?)			1340 BC
G 7	Later occupation of the palace	Middle Syrian Period	1500 BC
G 8	Main use of the palace with lime mortar floors	Old Syrian Period	
G 9	Construction horizon of the palace		
Hiatus (?)			2000 BC
G 10	Abandonment surface above occupation levels	Early Syrian Period	
G 11	Later structures of pre-palace occupation		
G 12	Earlier structures of pre-palace occupation		2200 BC

Fig. 3 Chronological chart of the new excavations in the Bronze Age palace of Qatna (after M. Novák and P. Pfälzner)

[11] Novák and Pfälzner 2000, 2001 and 2002.
[12] Matsumoto and Wada 2001; Bartl and Chaaya 2002.
[13] Philip *et al.* 2002.
[14] For the present state of research cf. Einwag 1998; Nigro 2002.
[15] Cf. now Braemer and Al-Maqdissi 2002.
[16] Novák and Pfälzner 2000, 260f.
[17] Novák and Pfälzner 2001, 161–165 fig. 4.

the architecture and the cuneiform sources, we place the construction of the palace in phase G 9 at present near the begining of MB II.[18] Phase G 8 also can be dated to the Middle Bronze Age, whereas the inventories of G 7 show that this phase already belongs to the early Late Bronze Age. So, the palace was definitely occupied throughout the later part of the Middle Bronze Age, which is still not known very well in Western Syria. The end of G 7 actually can be fixed even more closely around the middle the of 14th century BC, since in 2002 an archive of cuneiform texts from the destruction level of the palace has been unearthed which makes it almost certain that the palace has been destroyed during the reign of the Hittite king Suppiluliuma I.

To define the Middle Bronze Age pottery from Qatna as precisely as possible in its wares, formal types and chronological range, it is, however, necessary to separate it neatly both from the Early Bronze material of the preceding phases and from the Late Bronze material of the last occupation of the building.

Of great importance in this context will be the accurate assessment of the chronological date of the construction phase of the palace. Here the pottery from the foundation trenches and fillings of the construction phase G 9 will have to be examined in detail in order to identify the latest material which can give a terminus post quem for the building of the palace.

Another task will be to arrive at a dating of the pottery assemblages of the palace's main occupation phase G 8. Here, quite a lot of pottery derives from the two rooms N and U in a sector which has been left largely untouched by the early excavations of the Comte. Room N originally belonged to the ground floor level of the palace, but at a certain point in time it collapsed into room U together with its walls and inventory.

Throughout the field campaigns of 1999, 2000 and 2001 the last occupation phase of the palace G 7, which already dates to the Late Bronze Age, had been come across unambiguously only in room G, the so-called ‚Salle des Jarres'. This room had been discovered first by the Comte, but its inventory which consisted mainly of great storage jars was left in place by him at that time. Thus it could be rediscovered during the new excavations.[19]

Very important discoveries concerning phase G 7,

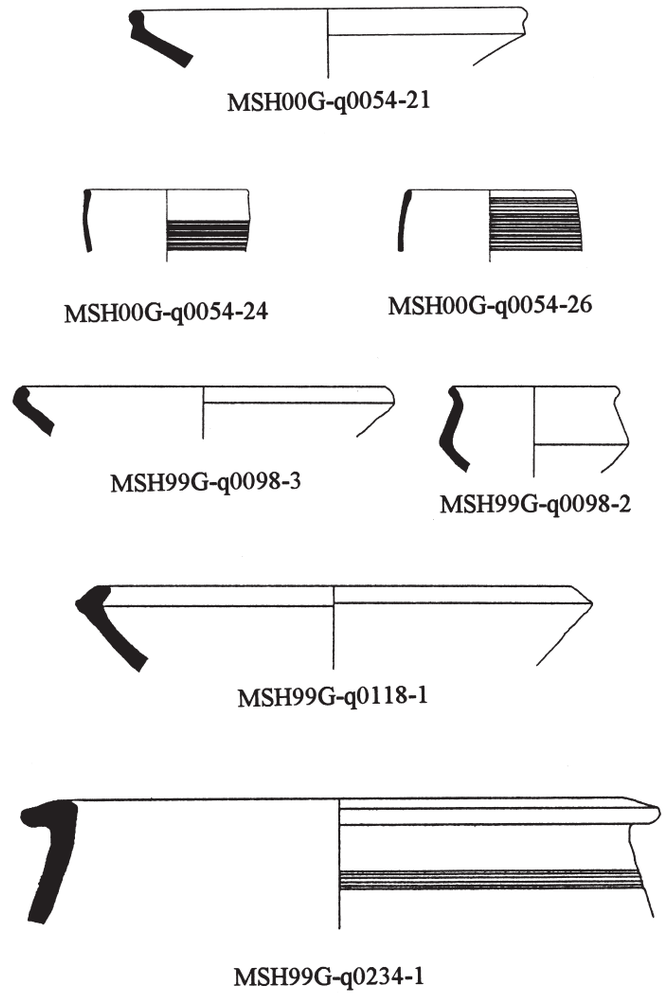


Fig. 4 Qatna, pottery of phase G 9, Scale 1:4

however, were made in the course of the campaign of 2002 in the northern sector of the building. Here the debris of the final destruction of the palace which had fallen from the ground floor into the subterranean corridor AQ leading to the royal tombs brought about a bulk of original inventories including a large number of pottery belonging to the last occupation phase of the building.

The pottery from the royal tombs too mainly should date to the Late Bronze Age. Still, part of it may be older, as it seems that the tombs have been cut into the bedrock simultaneously with the construction of the palace in the course of the 18th century BC. Other than in the destruction debris, the

[18] Novák and Pfälzner 2002, 244.

[19] Novák and Pfälzner 2000, 267. 270f. 283f. fig. 20.

ceramics from the tomb in the majority were still found complete.[20]

This short overview may show, that in spite of the fact that the palace of Qatna has been excavated once before, quite a lot of stratified contexts and even complete inventories have been unearthed in the course of the new excavations and are to be expected for the future as well. In the following I will give some examples of pottery coming from the palace of Qatna and also name a few comparisons from other sites.

Typical pottery from the fillings of phase G 9 are painted ‚Hama-Beakers' (Fig. 4, MSH00G-q0054-24 and MSH00G-q0054-26). These may originally come from a small-scale occupation of the Early Bronze Age IV as it is evidenced in phases G 11 and 12 underneath the palace.[21] Good parallels can be found in Hama J.[22]

At the same time one can find in phase G 9 already quite a lot of carinated vessels, above all carinated bowls (Fig. 4, MSH00G-q0054-21). Similar carinated bowls still exist in phase G 8 (Fig. 5, MSH01G-q0558-9 and MSH01G-q0614-2). To these carinated bowls one can show up a great number of parallels in MB contexts of Alalakh, Hama and especially Ebla.[23]

Other, apparently later bowls from G 9 show rims thickened on the inside and on the outside (Fig. 4, MSH99G-q0118-1) as well as retracted rims (Fig. 4, MSH99G-q0098-3). Characteristic for phase G 9 are also carinated beakers (Fig. 4, MSH99G-q0098-2). They have parallels in MB contexts from Hama, Alalakh, Halawa and again Ebla.[24] A complete carinated beaker of phase G 9 comes from a grave of a young woman.[25]

Pots of phase G 9 with combed decoration and rim turned outside (Fig. 4, MSH99G-q0234-1) again can be compared to vessels from Ebla.[26]

The pottery from rooms N and U on the whole may serve to give an impression of the plain ware forms which were in use in the palace during the main occupation phase G 8.[27] Indeed, already on brief inspection one can notice numerous similarities in the material of G 8 and G 9.

Thus bowls with rims thickened inside and outside (Fig. 5, MSH01G-q0160-12, MSH01G-q0160-15 and MSH00G-q0683-1) already occurred in phase G 9 (Fig. 4, MSH99G-q0118-1). Similar bowls as from Qatna are attested at Ebla and Alalakh VIII.[28]

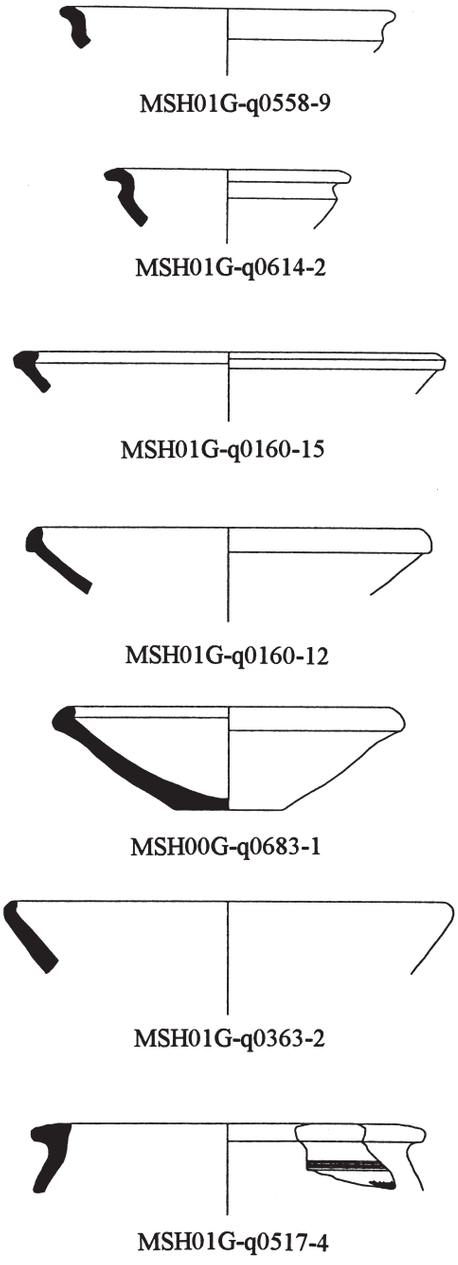


Fig. 5 Qatna, pottery of phase G 8, Scale 1:4

[20] Cf. AW 2/2003, 200 fig. 2.
[21] Novák and Pfälzner 2002, 210f.
[22] Fugmann 1958, fig. 93.3 F 583 (Hama J 3).
[23] Marchetti and Nigro 1997, fig. 7.4–14 (Favissa F.5328/i–iii, MB IB–MB IIA).
[24] Marchetti and Nigro 1997, fig. 7.1–3 (Favissa F.5328/i, MB IB).
[25] Novák and Pfälzner 2001, fig. 23, MSH00G-i0154.
[26] Matthiae 1979, fig. N:5 (‚Tomb of the Lord of the Goats', MB IIA).
[27] Still part of the material from this context is definitely later.
[28] Heinz 1992, pl. 18.11.

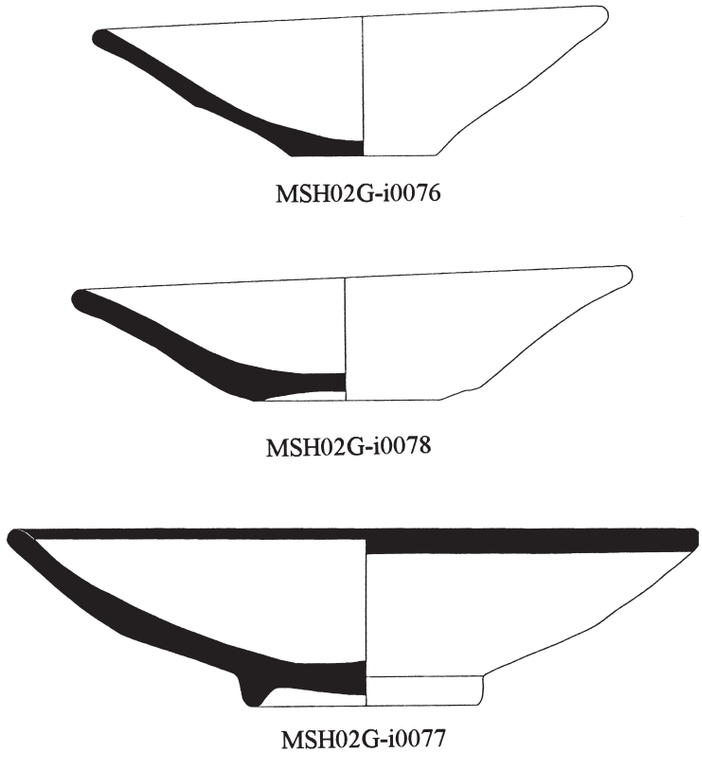


Fig. 6 Qatna, pottery of phase G 7, Scale 1:4

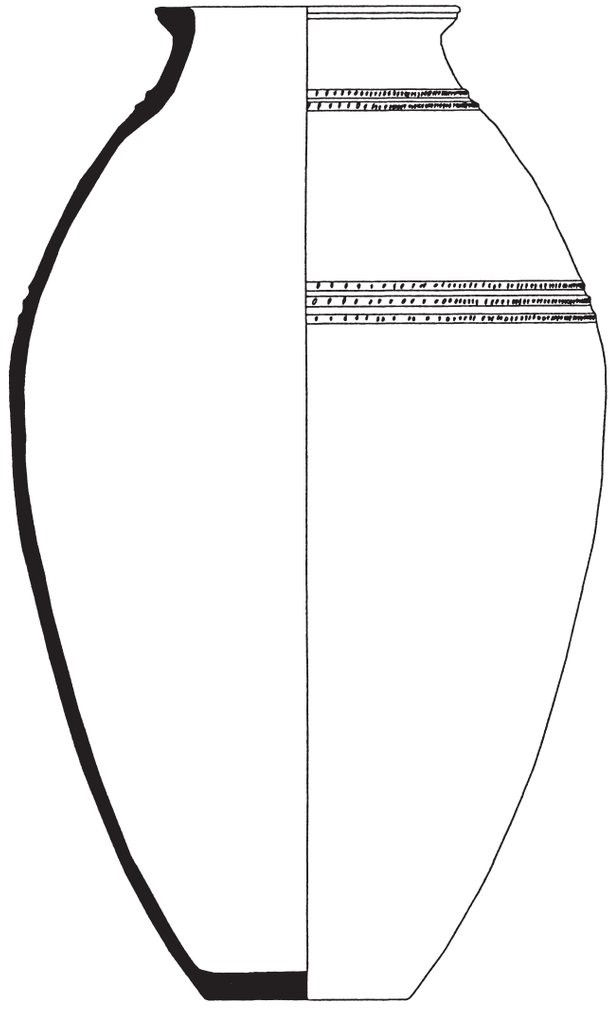


Fig. 7 Qatna, storage vessel of phase G 7, Scale 1:8

Also the bowl-type with retracted rim is found both in phase G 8 (Fig. 5, MSH01G-q0363-2) and phase G 9 (Fig. 4, MSH99G-q0098-3). Comparable bowls are known, for instance from Alalakh.[29]

The pots with combed decoration and rim turned outside too are characteristic not only for phase G 9 but also for phase G 8 (Fig. 5, MSH01G-q0517-4). Besides the aforementioned comparisons from Ebla further parallels come from sites as Halawa and Alalakh and with somewhat more pronounced grooves on the rim from Tell Nebi Mend.[30]

The most characteristic vessel type of phase G 7 and the LB occupation of the palace are flat, straight-sided and sometimes also rounded bowls with unthickened rounded lips. These bowls occur with or without base-rings. Many of them could be recovered complete or nearly complete from the destruction debris of the subterranean corridor as well as from the tomb and its antechamber (Fig. 6, MSH02G-i0076, MSH02G-i0077 and MSH02G-i0078). Some specimens show a band of red colour along the inner and outer side of the rim. Further flat bowls with rounded rim of phase G 7 are attested in room G.[31]

Comparable bowls are known from many Late Bronze Age contexts in the Gezirah, on the Euphrates, in Western Syria and in the Lebanon. One may quote examples from Ugarit[32] and from Kamid el-Loz.[33] However, to a great part the pottery from room G consists of large storage vessels (Fig. 7,

[29] Heinz 1992, pl. 18.12 (Alalakh VIII).
[30] Bourke 1993, fig. 17.3 (Phase G, MB IIC) and fig. 17.1, 2, 4, 5 (Phases H–F, MB IIB–LB I).
[31] Novák and Pfälzner 2000, fig. 23, MSH99G-q0144-5 and 14.
[32] Courtois and Courtois 1978, 263 fig. 22:8.
[33] Hachmann 1980, pl. 30:2–6.

MSH00G-i0137). Good comparisons come from MB and LB levels in Hama.[34]

In summary it may be said that although quite a lot of the MB and LB pottery types from the palace of Qatna reflect general changes in the pottery of Syria and the Levant, the closest parallels can be found at sites like Hama, Ebla, Alalakh and Tell Nebi Mend, which means in the Orontes valley and neighbouring zones.[35]

This already leads to the overriding objective of the research project, namely to put the stratified pottery sequences of the Middle Bronze Age in Western Syria into a supraregional context. Here special attention has to be paid to imports of widely distributed pottery wares which open up the opportunity to establish reliable datum lines of first appearances across a broad geographical area. For the moment, though, in the palace of Qatna the situation in this respect is a little bit more favourable with regard to the Late Bronze occupation, where several sherds and vessels of White-Slip-II-Ware as well as two vessels of ‚Nuzi-' or ‚Acana-Ware' were unearthed 2002 in the destruction debris of phase G 7 and in the royal tombs.[36]

During a later stage of the project, the information gained from the pottery comparisons will be related to systems of absolute chronology. Western Syria as a zone of intense and permanent contact among civilizations from the North, West, East and South has always been viewed as particularly apt for a discussion of the different chronological systems of the Ancient Near East, because here, more than in other regions, the mutual compatibility or incompatibility in the datings of archaeological contexts and historical events in different regions can be tested.[37] It is to be hoped that the new discoveries in Qatna can give a fresh impetus to the efforts in this field of research.

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[34] E.g. Fugmann 1958, fig. 143.S.No. (R.1) from Hama G.

[35] Sievertsen in Novák and Pfälzner 2002, 243.

[36] Examples are MSH02G-i0433 and MSH02G-i0977.

[37] See Gates 1987, 60; Matthiae 2000, 136f.; Zeeb 2001, 70.

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A Seldom Used Parameter in Pottery Studies: the Capacity of Pottery Vessels

Jean-Paul Thalmann[*]

The state of the art in the field of pottery studies has nowadays reached an unprecedented degree of sophistication. Not only can the slightiest typological/chronological variation be tracked down through a host of statistical procedures, but most details of manufacture, provenience and distribution come under close scrutiny through physical, chemical or petrographical analysis. It seems that the modern archaeologist is now able to acquire and publish any kind of information about her or his pots – except, in most cases, the quantity of goods, whatever they may have been, which these pots were intended to process, store or carry in Antiquity.

This parameter – the capacity of a pottery vessel – is however of paramount importance to understand its function, as much as peculiarities in shape (flat or rounded bottom, wide or restricted opening, presence or absence of handles etc.). For individual items, it will in most cases allow to decide between collective or individual use, short or long-term storage, the possibility of easily moving or carrying them when full for commercial purposes. In the case of full sets or assemblages of pottery, it is probably the safest test for specialization of pottery production and procedures of control over storage or long-distance trade.

Such observations do not reach very far beyond common sense and many scholars have also stressed the potential of the capacity of pottery vessels as a measure of the social and economic characteristics and even of demographic trends in the populations which produce and use them, as was conveniently summarized in a recent article by R.T. Schaub (1996, with relevant references), which awakened an old interest of mine for this topic. More generally, considering the wealth of informations which they are liable to provide, it is most surprising that functional typologies are so seldom used (Schaub 1996: 231–235).

Calculating the capacity of pottery vessels

I have found in recent literature very few studies concerned with such problems. In addition to the pottery studies from Bab edh-Dhra (Schaub 1987: 249; 1996), the most systematic one is probably the study by M. Roaf (1989) of the Ubaid "burnt house" at Tell Madhhur in the Hamrin. A full domestic assemblage was retrieved, and all vessels have been plotted on a plan of the house according to type and capacity (Roaf 1989: 121); but, surprisingly enough, few conclusions seem to have been derived therefrom. S. Mazzoni (1994) has measured drinking vessels from Ebla and A. Maeir has plotted the capacities of juglets, jugs and bowls from tomb 1181 at Hazor on graphs which show some trends towards standardization of the first two types, but none for the latter (Maeir 1997: 315–317 and note 80 for references to some other studies of palestinian material).

The reasons for this general lack of interest are easy to understand:

– Although not a rarity, whole or wholly reconstructed vessels, and especially large size jars, are far less common than sherds, and one must have a fair quantity of them in order to statistically assess *e.g.* trends towards standardization.

– Calculating the capacity of a pot from a section drawing is an operation simple in its principle, but which requires tedious measurements and calculations.

Different methods have been suggested (fig. 1) by approximating the general shape by means of elementary volumes such as spheres, cones, cylinders

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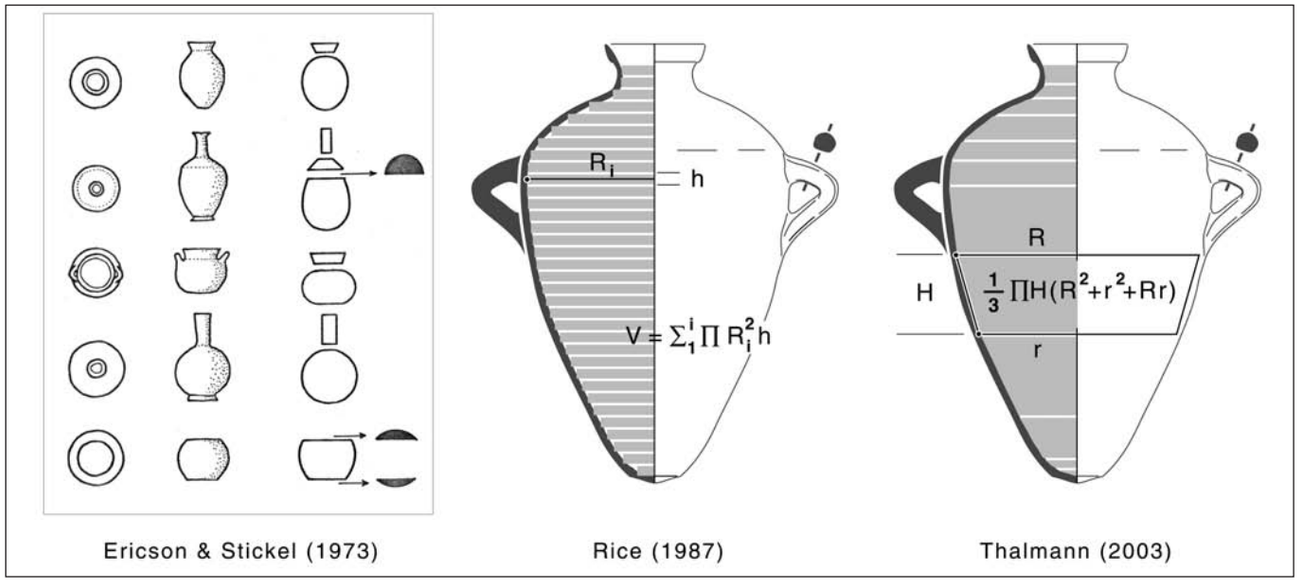


Fig. 1 Different methods for calculating the capacity of pottery vessels

etc. (Ericson & Stickel 1973) or of an array of small cylinders (Rice 1987: 220–222). But the former method is applicable only to very simple shapes. Moreover, it should be stressed that general approximations are not enough: volumes are a cubic function of size so that slight variations in diameter or the "flattening" of what would be taken for a more or less spherical body may result in wide differences of capacity. If we consider an "ideal" pot with a perfectly spherical body of, say 40 cm in diameter, it has a capacity of 33,5 litres; the same pot with a diameter of 50 cm will hold 65,5 litres, just twice as much! It is obvious that estimating capacity classes on the basis of similar sizes or proportions may result in wide-ranging errors: accurate measurements are absolutely needed.

The approximation of the shape by means of small cylinders is probably accurate enough only with a very large number of them, which anyway require too many measurements. It is much easier, quicker and more accurate to sub-divide the volume of the pot in a series of truncated cones (Fig. 1). The calculations for the volume of the truncated cones are not so straightforward, but, in the age of computers, this should not be a problem. I used at first an Excel chart, then designed a small standalone computer utility which requires only a few clicks to get the result (Fig. 2); it is freely available to all colleagues, for Mac and PC computers, on request at my e-mail address jp.thalmann@wanadoo.fr.

Even when computerized, the procedure is not so quick, and investigating the capacities and possible standardization of *e.g.* MB and LB commercial jars from available publications will take time. Moreover, most published drawings of large vessels are at a much reduced scale, and usually provided with a much too small *graphic scale*: it is common for large jars to be published with a scale of 10 cm only, when the graphic scale on a plate of pottery should be at least the size of the largest pot represented, in order to allow for direct measurements. Unavoidable approximations in re-scaling such improperly published drawings may result in the kind of errors outlined above. For these reasons, I shall present here only a few preliminary results (*cf.* also Thalmann 2003) obtained using excellent drawings from Tell Arqa (Lebanon) and Tell ed-

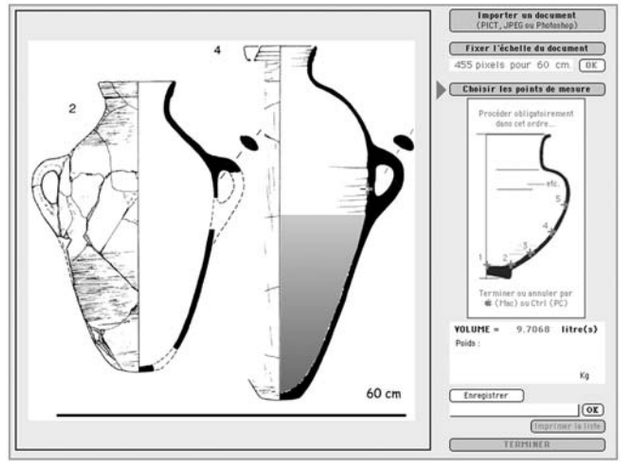


Fig 2 Measuring jars from Tell ed-Dabᶜa on the computer

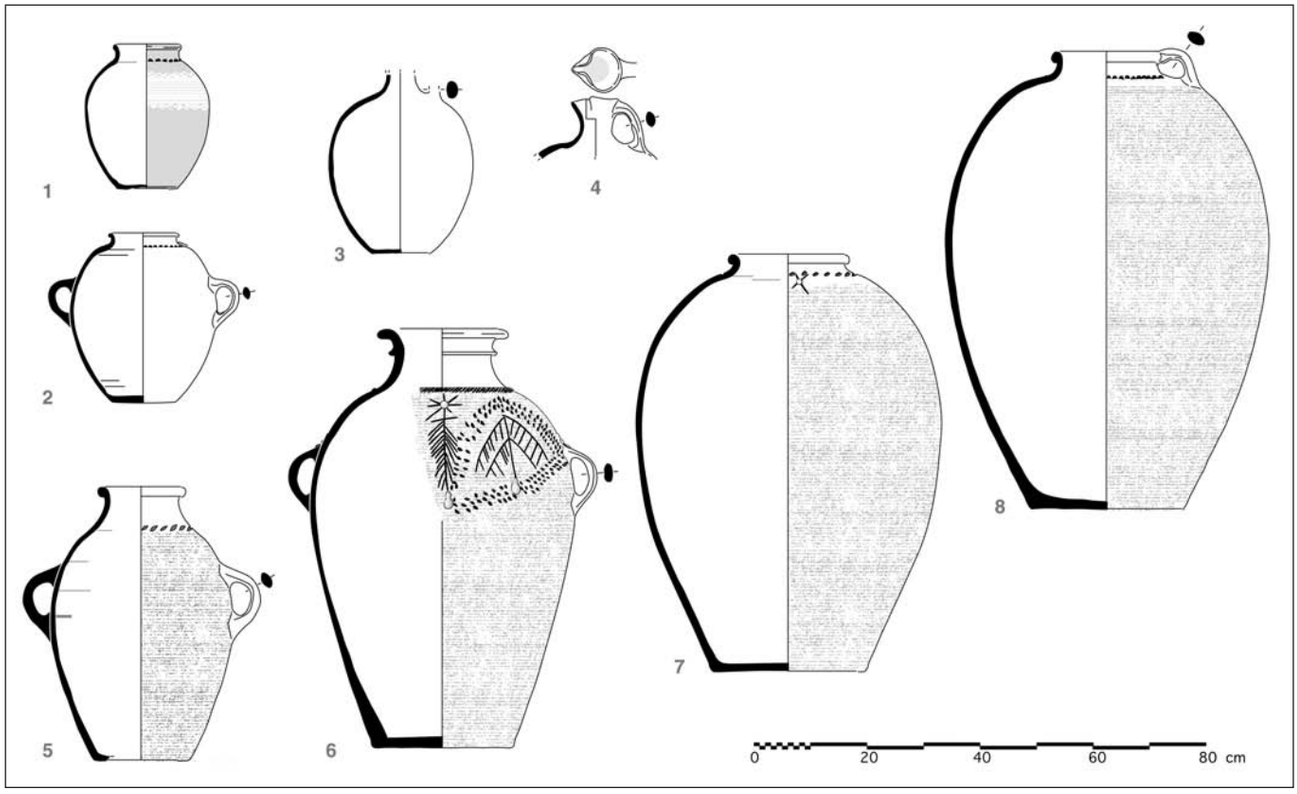


Fig. 3 Jars from Arqa Phase P (EB IV)

Dabᶜa, in order to illustrate the potential of vessel capacity in pottery studies.

Some observations on storage and commercial containers

The excavation of Tell Arqa (North Lebanon), under the direction of the author (Thalmann 2000; 2002; forthcoming), has produced a large number of complete or wholly reconstructible pots, mainly from late EB IV (Phase P) to MB I (Phase N) *i.e.* from ca. 2200 to 1800 BC, which provide a sound preliminary basis for further capacity studies. They range from small-sized "domestic" jars to medium-sized jars probably intended for short-term storage and transport (most types have a version without and a version with handles), and finally to very large storage vessels (Fig. 3, 5). When sorted out by capacity, they fall into groups which correspond only loosely with the main types designed on the basis of general shape and proportions, but can be interpreted in functional terms and show an evolution in the production and use of such vessels from one period to the next.

Jars from Arqa Phase P

The capacity diagram (Fig. 4) shows five distinct groups, probably related mainly to functional specialization. The distribution of jars with (black and grey dots) and without handles (white dots) is especially remarkable.

Group 3 is comprised of jars with handles only, and capacities ranging from 20 to 25, exceptionally 30 litres (Fig. 3: 5). Such jars weighing about 30 kilos when full would probably not be difficult to carry, although the position of the handles or the large flat bases are not well suited for this use. The vertical handles are attached low on the body, *i.e.* close to the center of gravity of the vessel, as is ususal in the Levant during the EB (Amiran 1969: 59, 63, 66, 67) and still at the beginning of the MB period (Amiran 1969: 104; Aston 2002: figs. 1–4). This makes it easy to move the jar or to tilt it for pouring when it rests on the ground, but does not insure verticality when carrying it by hand. Such jars would therefore be better considered as short-term storage vessels, which for this reason had to be frequently displaced within the dwelling area, rather than as transport or "commercial" containers.

In a significant manner, during MB and LB, handles will be attached higher and higher on the shoulder, well above the center of gravity, and pointed or stump bases will replace the flat ones. Capacities are then frequently more important; the jars can be carried in a vertical position by two men or handled for pouring, using one handle and the stump base, by

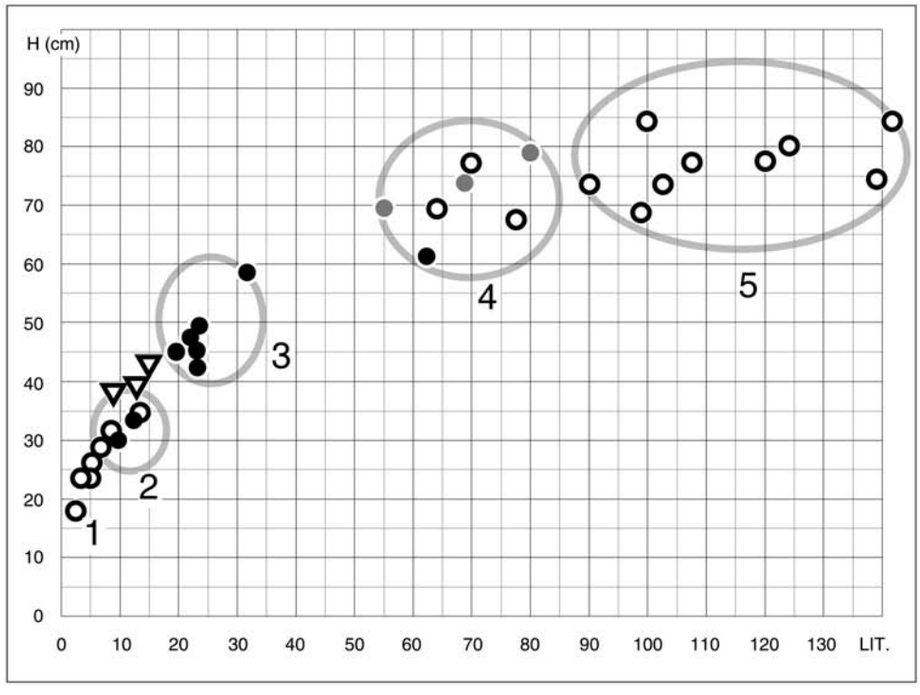


Fig. 4 Capacity diagram for Phase P

one. One of the earliest examples of this morphological adaptation of jar handles to the logics of transport, and especially of maritime trade, is to be found in the group of jars from the Royal Tombs at Byblos (Tufnell 1969: fig. 6); their average capacity is 40 to 45 litres, nearly twice as much as the jars of Arqa group 3.

Groups 4 and 5, with capacities ranging respectively from 55 to 75 and 90 to 120 litres, are clearly distinct, although the sheer consideration of general size and proportions, as noted above, would not allow to set them apart. Because of their weight, all such jars (Fig. 3: 6–8) must be non-movable storage vessels, and indeed most of them were found filled with cereals in both destructions layers of Phase P. The two groups may correspond to the storage of different kinds of products (liquid/solid) or to different conditions (middle-range or long-term storage).

Surprisingly, handles are occasionally found on jars from these groups. A few jars of group 5, all above 100 litres, have a small loop handle from the top of the shoulder to the rim (Fig. 3: 8): while unpractical to tilt such large vessels for pouring, the handle would be well suited to attach with a rope a wooden stopper for instance, if the jars had to be frequently opened and closed. For this reason, they could be interpreted as water containers – the type is rare, and one or two such jars only were necessary in each house.

Some jars of group 4 (grey dots) have vertical handles, similar in position and shape to those of group 3 jars (Fig. 3: 6): for reasons given above, they were certainly very ill-suited for carrying the jars when full. The group is very homogeneous, characterized by the profiling of the rim, but above all by the incised and impressed decoration on the upper part of the body (Fig. 3: 6). They are found at Arqa from level 16B to level 15A, a period of about two centuries. With the exception of a few fragments from sites close to Arqa and a unique fragment from Byblos (*BYBLOS II*: 16572), I know of no parallels to this type of decoration and consider it most probably the work of one or two families of local potters over a few generations.

Apart from the possible symbolic connotations of a part of the decoration ("suns" or "stars" and stylized vegetal elements), the overall pattern seems to be derived from a practical device of ropes or basketry, comparable to our modern *dames-jeannes*. The wide "arches" of impressed dots are "attached" to a row of similar impressions on the maximum diameter of the body, to the handles and to small lugs which are otherwise part of the applied decorative elements. It is impossible to know whether such a system, which would permit to carry easily the large jars and may also have been used on their smaller counterparts of group 3, was actually used in the Levant by the end of the IIIrd millenium. It is however such a simple device that the probability is high; it would have been

later abandoned with the above-mentioned evolution of pottery containers specifically adapted to trade. Representations of such devices, probably on handleless jars only, exist on cylinder-seals at the end of the Uruk period (e.g. Le Brun 1978: fig. 8: 5; Le Brun and Vallat 1978: fig. 6: 4, 9, fig. 7: 12), but I know of no later ones.

Finally, some puzzling questions arise from the capacity diagram. None of the smaller jars of group 1 (5 to 13 litres, Fig. 3: 1, 2), either because they are handleless or have too wide and short necks, appear to be adapted to the carrying of water for daily use. Everywhere in the Middle East, and especially at Arqa, where the river flows in a deep gorge at the foot of the tell some 40 to 50 m below the settlement, this was a painstaking but important task, for which one would expect to find specially designed containers. In the whole assemblage of Phase P, only very large jugs (9 to 15 litres, triangles on the diagram, Fig. 3: 3, 4), with restricted neck and trefoil mouth, meet the necessary requirements. They are too large for pouring water for individual use into the small cups and goblets which are the standard drinking vessels of the period. But they could be easily carried on the shoulder or on the head, while the restricted neck prevented the spilling out of water. This type is frequent, as can be expected for vessels with a high probability of breakage, and very many of them were probably necessary in every single house.

Jars from Arqa Phase N

The capacity diagram for Phase N shows a very different picture (Fig. 6). It is obvious that pottery production is much more specialized and standardized: only three groups. Jars with (black dots) and without handles (white dots) are represented in the first (12 to 17 litres) and the second one (20 to 30 litres), while all larger jars in group 3 (40 to 60 litres) are handleless; the very large containers with capacities of 90 litres and more seem to have disappeared. This is probably in part due to the fact that the Phase N assemblage is derived from the potters' quarter and workshop (Thalmann 2000: 47–50; 2002: 368–369), not from an ordinary dwelling quarter. It is however noteworthy that, if large jars of about 100 litres were in use elsewhere in the settlement during Phase N, they were manufactured in a different location and by different potters; this was certainly not the case in the preceding period, when all groups of jars exhibited a strong technical homogeneity (Thalmann 2000: 44).

The jars with handles of group 2 (Fig. 5: 3) differ in shape from their counterparts of Phase P, group 3, and with the same range of capacities may have served the same purposes for short-term storage. But

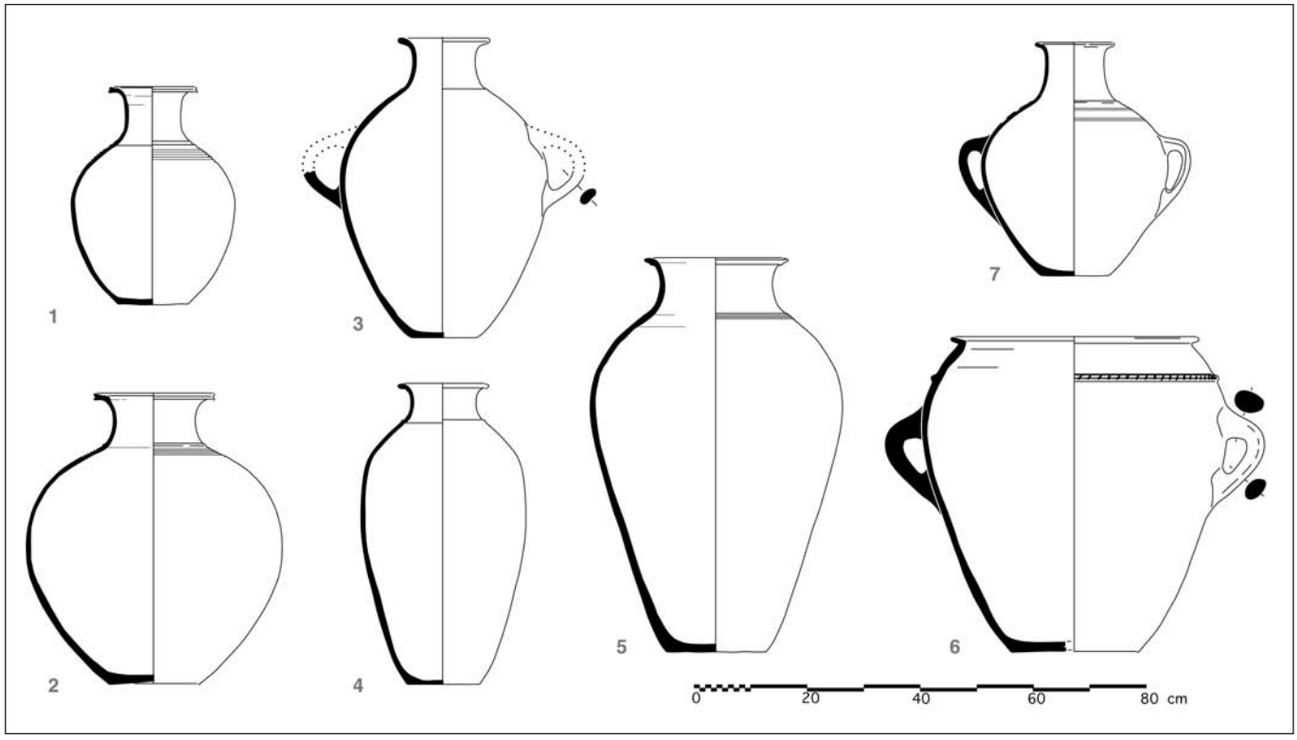


Fig. 5 Jars from Arqa Phase N (MB I)

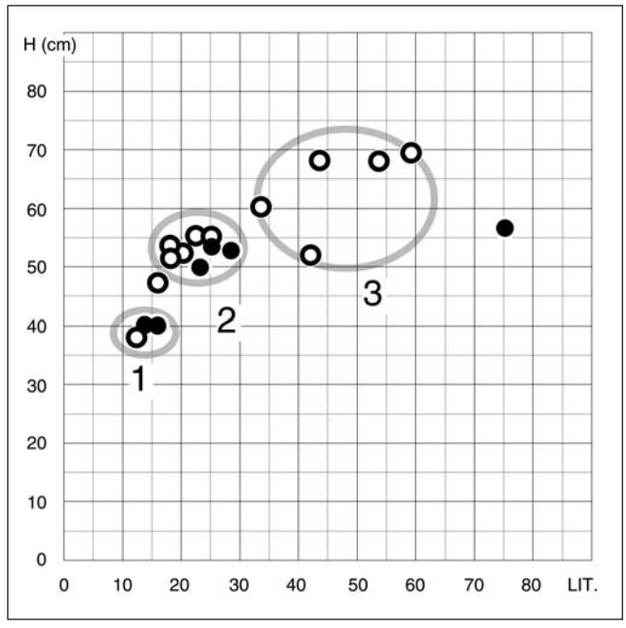


Fig. 6 Capacity diagram for Phase N

they are also probably more specifically designed for transport, because there now exists a number of handleless jars in the same group, which are equally well suited for short-term domestic storage, but of course not for transport. A further indication is that we have from Phase N a number of non-local jar sherds, probably originating from the Byblos area or southern Lebanon, what can be inferred from their limestone tempering *vs*. the strong basaltic component in the temper of all local wares.

Handleless storage jars are more or less evenly distributed (which can also be checked from fragments) between groups 2 and 3, and two morphological types only: a plump one with rounded body (Fig. 5: 1, 2) and a tall slender one (Fig. 5: 4, 5). Probably as for the jars of Phase P, this corresponds to different products and conditions of conservation, but the repertoire of Phase N and the capacity groups show a much higher degree of standardization and specialization.

Jars with handles in group 1 (ca. 15/16 litres, Fig. 5: 7) are equally interesting, as they probably are the counterparts of the large jugs of phase P for carrying water. Jug types in the assemblage of Phase N are numerous (Thalmann 2000: figs. 44, 46b; 2002: fig. 8), but they are all of small size and belong to what may properly be termed "tableware". On the contrary, the jars of group 1, with their moderate capacity and their shape (rounded body, "low" handles and tall restricted neck) meet all the requirements for easy carrying on the head or shoulder; the handles attached at center of gravity level are especially well suited for pouring when holding the vessel with both hands.

Only one large vessel with a capacity of 75 litres coud be reconstructed (Fig. 5: 6), but fragments of rims of a similar shape are numerous. The wide opening and the two strong handles do not match the usual types of contemporary storage jars, handleless and with a restricted neck, easy to seal with a clay stopper. This could well be also a specialized shape, new to Phase N, for storing domestic water: the wide opening allows for drawing up water with pots of all shapes and sizes, and the vessel can be held by the handles and tilted down for pouring when it is half or nearly empty.

Evolution of the repertoire in later periods

For later periods, the number of complete shapes available from Arqa is too low for significant capacity calculations. But new trends in the production and use of medium and large size containers are nevertheless apparent.

Very large storage vessels or *pithoi* (150 litres and more) appear only with Phase M (MB II) and different types of similar capacity are also produced during Phase L (early LB). Medium-sized jars are numerous and, much more than during the preceding phase N, some of them are of non-local origin.

Most of these jars, so far as can be inferred from fragments, probably fall within a capacity range between *ca*. 30 and 40 litres. Is this possibly a more or less standard capacity for many MB II jars, as noted above in the case of the Byblos jars? It should be necessary to accurately measure a wide sample from many Levantine sites to get the beginning of an answer. One of the few complete jars from Arqa, very similar in shape to the Byblos specimens and ascribed to the very beginning of phase M, holds 33 litres. On the other hand, it is probably not by chance that very few rims or large body fragments can be ascribed to the "intermediate" capacity group of 60/80 litres, which was well represented, although in somewhat different ways, in both preceeding phases P and N.

It seems therefore that the specialized production of different types of storage vessels and the standardization so apparent in the Phase N repertoire was not continued during phase M. This may be related to the widespread circulation along the Levantine coast, especially from the beginning of MB II, of the true "commercial" jars, well adapted as noted above to the constraints of maritime trade, but which could

also be re-used when empty for all kinds of local storage. At least for the manufacture of medium and large size containers, a trend in the de-specialization of local pottery manufacture, *vs.* the higher specialization of fewer workshops which produced the "commercial" jars, probably began at Arqa during MB II with the wider availability of imported vessels; it becomes more visible in later periods, in all classes of pottery including tableware.

Canaanite jars from Tell ed-Dabᶜa

Capacities were calculated for some 20 jars from Tell ed-Dabᶜa, all Canaanite "commercial" jars dating to MB IIA probably originating from southern Palestine (Aston 2002 : figs. 1–4 ; Schiestl 2002 : fig. 12), and plotted on the graph Fig. 7. Most surprisingly, it shows that there is no preferred or standard capacity, all intermediate values between ca. 14 and 25 litres being represented. It is obvious that these jars, at first glance rather standardized because they are all very similar in shape, size and proportions, did not however correspond to any standard capacity or system of measures as containers.

Although they have elongated bodies and restricted, slightly convex bases, most of them still retain archaic characteristics such as the « low » position of the handles placed on the body rather than on the shoulder. It is possible that a capacity of ca. 25 litres or a weight of about 30 kilos when full was, as in the case of group 3 jars from Arqa Phase P, a practical limit posed by the possibility of easily handling and carrying them – or by the mode of stacking in the ships used for transport. Whether it may be considered as an early stage in the technologies related to maritime trade, in comparison with the possibility to transport more important quantities of goods in containers of higher capacity (but of only slightly larger size !), such as the above-mentioned jars from Byblos which are chronologically hardly earlier than the Jars from Dabᶜa – this should be investigated on the basis of a large database of jar capacities from all sites in the Levant, and well into MB II and LB.

Concluding remarks

No definite conclusions can be derived from such a limited sample, but it illustrates the wide range of problems which may be tackled through systematic investigation of vessel capacities. The case of the storage/transport jars at Arqa is perhaps especially clear because the earlier of the periods considered (Phases P and N) is there characterized, as everywhere else in the Northern Levant, by a general trend in handicraft specialization ; and because there is a strong contrast between Phases P and N, when no or limited interaction occured between local productions and imported vessels, and Phase M or later, when such interactions became more frequent. The proposed model could probably be easily tested on sites where local wares are readily distinguishable from imported ones ; but the excavation of a workshop specialized in the production of specifically "commercial" jars is still lacking.

Calculations of capacities made only in a cursory

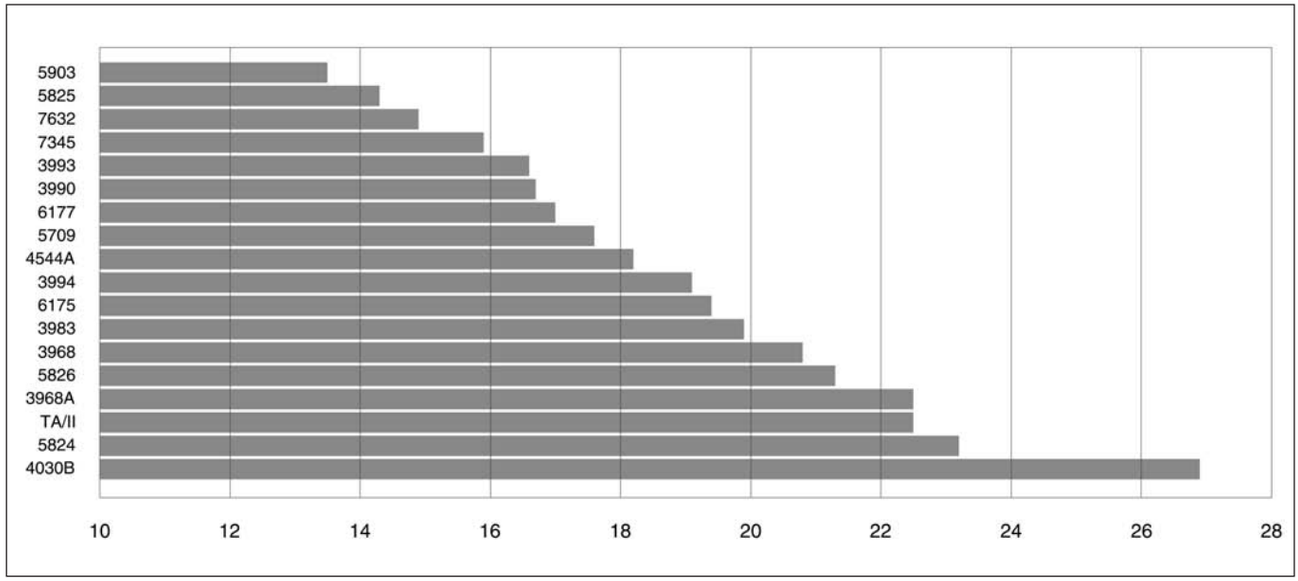


Fig. 7 Capacities of some MB IIA jars from Tell ed-Dabᶜa

manner on publications of MB II and LB levantine jars lead to the supposition that different classes of capacity did exist – for different products? different systems of "weights and measures"? short or long-distance trade? The test on the jars from Tell ed-Dabᶜa shows that it was not always and everywhere the same : different chronological "stages" in the development of maritime trade may be an explanation, but many others are of course possible.

Before even preliminary results can be obtained, it is clear that the painstaking compilation of a large database is necessary, taking into account accurate measurements only : no too small or approximately scaled drawings should be used. It is however hoped that increased interest in capacity calculations and wider reliance on easy-to-use computer utilities such as the one proposed above will give significant resuts in a not too far future.

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The Foundation of Enkomi: A New Analysis of the Stratigraphic Sequence and Regional Ceramic Connections

Lindy Crewe

Introduction

The beginning of the Late Bronze Age on Cyprus sees a range of dramatic changes in the settlement patterns and material culture of the island. These include the move from small inland to larger nucleated coastal settlements; the construction of a series of inland and coastal fortifications (Fig. 1); an increase in social stratification attested in the mortuary record; the first evidence for literacy; and Cyprus beginning to take an active role in the exchange networks of the eastern Mediterranean. Central to interpretations of the early Late Cypriot (LC) is the coastal settlement of Enkomi, both for the wealth of finds recovered and, importantly, the greater extent of excavation compared to other sites of the period. Due to incomplete publication and limited excavation of the earliest occupation levels, debate has centred on the date of foundation of the site (MCIII or LCI), the extent of control Enkomi may have exercised over external trade relations and the role of the settlement in the development of polities on Cyprus. LCI Enkomi has been interpreted as either the first state-like entity on the island (Peltenburg 1996; Webb 1999) and identified with Alashiya (*cf.* Muhly 1989; Knapp 1996) referred to in contemporary external textual sources, or as one of a series of independent regional polities (Keswani 1993, 1996; Manning and De Mita 1997). Problematically, the majority of evidence for the Late Cypriot period – including that from Enkomi – derives from LCIIC (c. 1340–1200 BCE). Architectural remains underlying LCIIC occupation are poorly preserved and we have little understanding of the processes of change which culminated in what may be broadly called urban centres during LCIIC (Negbi 1986). The focus of this

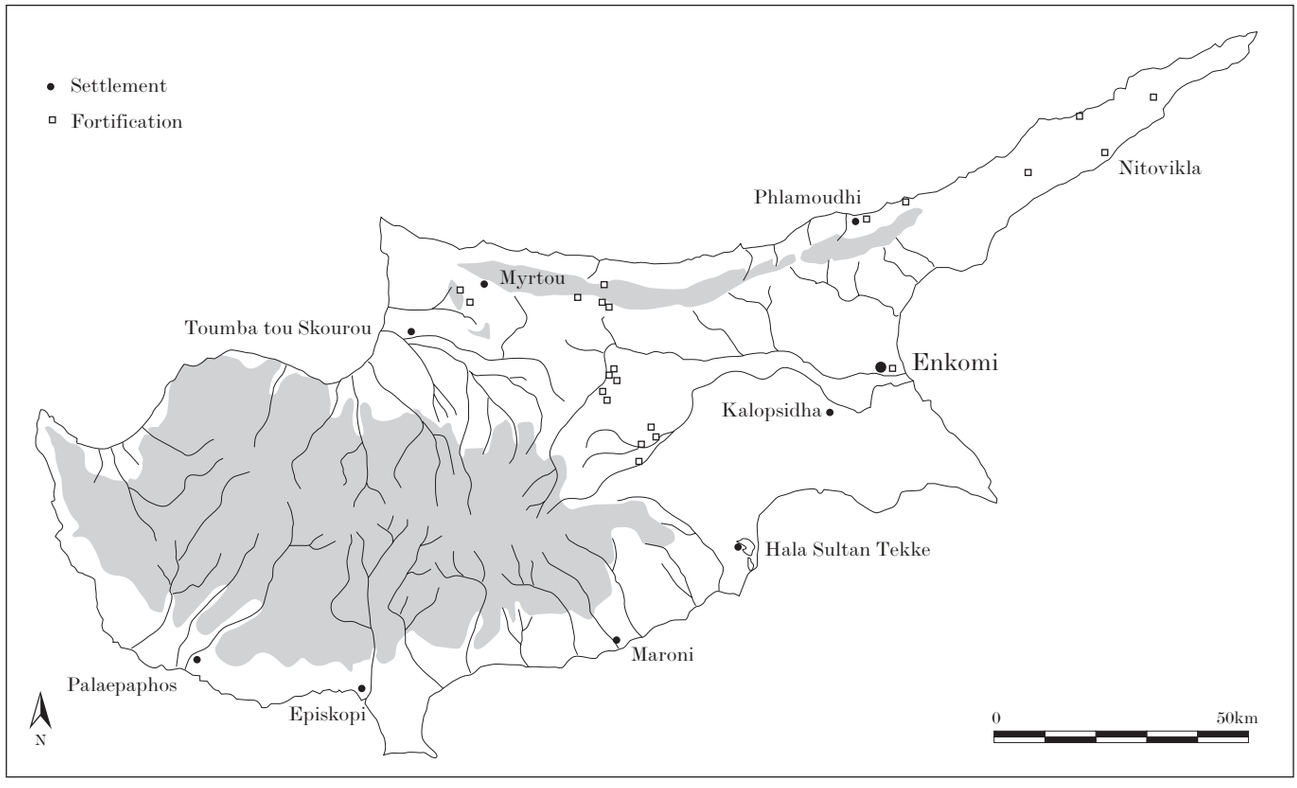


Fig. 1 Plan of Cyprus showing Late Cypriot I sites mentioned in the text

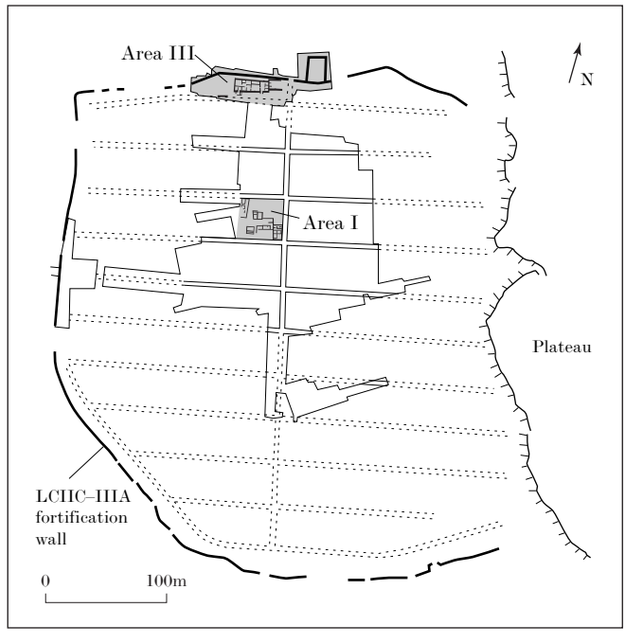


Fig. 2 Plan of Enkomi showing main excavation areas (after Courtois *et al.* 1986: 3, fig. 1; Dikaios 1969–71:pls. 243, 267)

paper is on the period from which the evidence for interregional contacts increases dramatically (MCIII, commencing c. 1750 BCE) to the period immediately prior to the urban phase (LCIIA–B).

Although it is widely recognised that LCIA ceramics display pronounced regionalism, manifested in the appearance of new wares and developments from the preceding MC traditions, this regionalism is held to disappear by LCIB, when Cyprus is understood to have developed an island-wide material culture (*cf.* Merrillees 1971; Manning 2001). Central to this thesis is the widespread belief that ceramic wares developed in the northwestern and central regions became common at eastern Cypriot sites, including Enkomi, by this phase. The wares that ceramically define the beginning of the LC period, or LC1A1 (PWS, PBR and also Proto-Monochrome), developing into the mature versions of WS I, BR I and Monochrome by LCIA2 are all central and northwestern innovations and this has led to considerable ambiguity and confusion in the application of relative chronological phases to eastern Cypriot assemblages (Merrillees 1971; Manning 2001). Certain eastern wares, such as Bichrome Wheelmade and some of the later White Painted Handmade varieties, were initially dated to MCIII on the basis of their lack of association with the LC marker wares (*cf.* Dikaios 1969–71). It is now agreed that Bichrome dates to LCIA (*cf.* papers in Åström 2001), and that White Painted V (WP V), White Painted Pendant Line Style (WP PLS) and White Painted Cross Line Style (WP CLS) were manufactured in the east of Cyprus in both MCIII and LCI (Åström 1966; Merrillees 2002). The eastern Cypriot ceramic developments are no less striking (for example the Painted Wheelmade wares) but the majority of innovations are attested in the Plain wares (Plain White and Red/Black Slip), which have received little attention in the literature to date. My research suggests that the degree of cultural homogeneity on Cyprus during LCIB has been overestimated, primarily due to an early archaeological focus on mortuary rather than settlement material. LCIB is best viewed as a time of increasing intra-island contacts, with the different areas largely retaining their regional traits until the greater uniformity of the LCII period. This will be addressed below in relation to the Base Ring (BR) and White Slip (WS) wares in the stratified deposits at Enkomi. These two wares have been considered of paramount importance to researchers investigating cross-cultural interactions in the eastern Mediterranean, and have formed the focus of recent conferences and publications in association with the SCIEM project (Åström 2001a; Karageorghis 2001).

The basis for this paper is research undertaken for my PhD dissertation (Crewe 2004), the aim of which was primarily to investigate the introduction of wheelmade pottery on Cyprus within the context of the broader social and technological changes occurring during the early LC. Although a full stylistic analysis of the entire ceramic repertoire was beyond the scope of my study, one of the goals of my research was to provide a full publication of the ceramics at the site as this has not been undertaken to date. Before addressing the ceramics, I wish to briefly comment on the deposits relating to the earliest occupation at Enkomi and the sequences of occupation at the site.

Enkomi – Dikaios' excavation areas

My study is concerned with the component of the site excavated by the Cypriot Department of Antiquities from 1948–58, under the direction of Porphyrios Dikaios (Dikaios 1969–71). Dikaios' excavation was concentrated in two areas, Area I and Area III (see Fig. 2), and was part of a joint excavation with a French team under the directorship of Claude Schaeffer (Schaeffer 1936, 1952, 1971). The French Mission provided funding and dictated the extent of the excavation to be undertaken by Dikaios in order to facilitate their own research aims. Dikaios (1969–71:5) states that these limitations had the

Level	Relative chronological phase (Dikaios 1969–71)	Relative chronological phase (Crewe 2004)
A	MCIII	LCIA1 in Area I, LCIA in Area III
IA	LCIA	LCIA1 in Area I, LCIA2 in Area III
IB	LCIB	LCIA2/LCIB extending to mid LCIB in Area I, LCIB extending into early LCIIA in Area III
IIA	LCIIA–B	LCIIA:2–IIB
IIB	LCIIC	LCIIC

Table 1 Relative chronological phases of occupation Levels at Enkomi

advantage of allowing him to excavate a small area of the site in greater detail but certain research goals – such as extending trenches to define the extent of buildings and solving certain stratigraphic problems – could not be pursued. The French Mission exposed earlier remains only in soundings and the excavation remains incompletely published and the material dispersed. It is thanks to the highly professional and extremely thorough excavation and recording undertaken by Dikaios, and the later care taken by the Cypriot Department of Antiquities to ensure preservation of the material, that this study has been possible. All the sherdage, including non-diagnostic pieces, has been retained in the Larnaca and Nicosia Museums and detailed information concerning provenance is recorded within the trays, enabling a detailed reanalysis of the stratified deposits, greatly illuminating the sequences of construction, occupation and abandonment at the site.

The two areas, Area III and Area I, are not linked stratigraphically and my discussion below follows Dikaios' strategy in treating them as separate entities, dividing the description into phasing by the 'Levels' assigned in each Area (see Table 1). 'Level' is the term used by Dikaios to signify an occupation phase and is based upon his interpretation of the sequences of construction, occupation and abandonment/destruction identified during excavation. 'Level' is also a designation that applies site-wide, as Dikaios (1969–71: 500–509) considered the broader occupation phases of the two areas to be contemporaneous. During the Levels relevant to my analysis (Levels A–IIA), each Area contained what Dikaios defined as a single 'building', comprising a series of rooms and courtyards forming a coherent architectural unit, surrounded by unoccupied space into which contemporary chamber tombs were dug. In Area I, Levels A–IB are linked stratigraphically and the Level IIA building was located in a previously unoccupied area of the trench. In Area III, the Level A building is not stratigraphically linked to the Level IA–IIA building. Both Areas were completely built over by LCIIC–IIIA architecture, which extends beyond the limits of the trenches and is associated with the gridded town plan and fortification wall seen in Fig. 2.

The integrity of the Enkomi deposits

Fine-scale phasing of Dikaios' Levels was delineated by 'floors', both within the defined architectural boundaries of rooms and also the external spaces that formed part of the activity areas of the site. Therefore, a Level may comprise one or more episodes of floor construction, rebuilding and subsequent deposition of layers of cultural material. During Level A to Level IIA, built floors were rare and the term 'floor' usually signifies a surface on which either occupation was evidenced (in the form of built structures such as hearths or negative features such as postholes) or a levelling and compacting of the underlying deposit (not necessarily exhibiting evidence of occupation). On occasion a built floor was present (usually a layer of crushed limestone or mud plaster of up to 10cm thick) and these were isolated and excavated as discrete units, where recognised (although this often seems only to have occurred during later removal of baulks). Given the excavation technique of removing floor deposits as part of the underlying fill, it is important to remain aware that any analysis should be considered only to be of broad-scale chronological resolution. During Levels A and IA only a single 'floor' is assigned to each Level in each area. During Level IB, the Area III building exhibits a complex series of refurbishing and rebuilding episodes of up to six floors, whereas the majority of the Area I rooms contain only a single floor construction episode. Level IIA in Area I is more complex than Area III, where only single occupation floors are evidenced and a large proportion of the Level I building was in use as a central courtyard (Dikaios 1969–71:35–43).

Prior to undertaking the analysis of the ceramics, it was necessary to examine the integrity of the published deposits in order to be able to slot the unpublished material into the occupation sequence. The only contexts to be published by Dikaios were those relating to select sherds or small finds. By isolating the deposits relating to intentional floor construction episodes from those relating to the later deposition of material due to the collapse of the mudbrick superstructure within each level I have constructed a fine-scale phasing of the site which has helped to illuminate the internal sequences to a greater extent. This has also resulted in the exclusion of some of the deposits due to irresolveable conflicts between the published data and the material stored in the museums. Therefore, it should be stressed that although my study does not include all the material from Dikaios' excavation areas, it does include all the material deemed to be from reliable contexts. The majority of material which was not included originated from areas extensively disturbed by pit digging (by earlier excavations, tomb looters or by the occupants of the site) or from external areas which were not fully investigated by Dikaios due to the imposed excavation limits discussed above.

The date of the foundation of Enkomi (Level A)

The Level A architectural remains in Area III consist of a single wall remnant, immediately underlying, and extensively disturbed by, LCIIC architecture. There is no stratigraphic relationship attested between the Level A and the Level IA buildings and the ceramic repertoire, including the presence of WP PLS, WP CLS, WP VI, Canaanite jar sherds and local wheelmade wares argues for an early LCI date for construction. The Level A material in Area I consisted of two contexts, one the fill of a foundation trench (Inv. 2191) dug for the Level IA building, and the other below a floor remnant preserved beneath the foundation trench cutting (Inv. 2192). As the foundation trench dates to construction of the Level I building it should be dated to LCIA. This deposit also contains WP VI and a Canaanite jar sherd. This unfortunately leaves only 19 poorly preserved sherds dating to Level A in Area I, none of which may be considered diagnostic of MCIII or LCIA occupation (including Red and Black Slip Handmade, White Painted, Composite Ware, Plain White Handmade and Red on Black). It should be noted however, that no wheelmade wares occur in this deposit. Whether MCIII occupation may be present in other areas of the site remains unknown but it would appear that the evidence from Dikaios' excavation areas indicates that the site was not founded until early LCI.

Level I–IIA

Although space constraints here do not allow full exposition of my thesis, it is my contention that the Area I building was constructed earlier (LCIA1) than the Area III building (LCIA2) and that the occupation sequences differ in the two areas, with probable continuous occupation in Area III from LCIA2 but a mid LCIB abandonment in Area I (see Crewe 2004 for full discussion). Contrary to the situation indicated by Dikaios' publication of the ceramics, the initial Area I construction deposits contain only PWS, no WS I, a few sherds of Bichrome and no White Painted Wheelmade wares or Monochrome. There is very little wheelmade pottery in Area I but Area III has substantial amounts from the earliest deposits, along with WS I, BR I, Monochrome and White Painted Wheelmade II. Both Areas show substantial amounts of Canaanite jars from the earliest Level I construction, suggesting that trade links were established from the very beginning of the site's occupation. The extent of damage to the buildings at the end of Level IA supports Dikaios' interpretation of destruction at this time (1969–71:20, 155–156), but the evidence for the end of Level IB is more indicative of gradual disuse and abandonment, prior to rebuilding during Level IIA. At this time, the Area III building was built over and extended the limits of the earlier building and the Area I building was erected in a previously unoccupied part of the site. All Area I, Level IIA material utilised in my study originates from a single room (Room 142) as the other Level IIA room (Room 102) was extensively disturbed by later occupants of the site. The Room 142 deposit appears to be of a special nature, containing far higher numbers of WS, BR and imported LH IIIA:2 ceramics than other areas of the site (primarily pouring and serving vessels, suggesting an elite assemblage), inflating the percentages of these wares.

The Enkomi ceramics

Although detailed publication of the architecture and discussion of the stratigraphy was undertaken by Dikaios, the publication of the ceramics has remained incomplete. At the time during which Dikaios published the Enkomi volumes, the comparative material available was extremely limited and almost entirely based upon ceramics excavated from tombs, (Gjerstad 1926; Sjöqvist 1940; Aström 1957). Essentially, he dated the earliest deposits to MCIII based upon the *absence* of certain wares: the

	Area III						Area I					
	Level IA		Level IB		Level IIA		Level IA		Level IB		Level IIA	
BR I	***0.3***	1.0	***1.0***	1.8	***0.7***	2.4	***0.1***	1.2	***0.6***	2.3	***0.6***	7.0
BR II	–	–	***0.1***	–	***1.0***	7.0	–	–	***0.1***	1.9	***4.6***	8.2
PWS	***<0.1***	–	***<0.1***	–	***<0.1***	–	***0.3***	–	***0.2***	–	–	–
WS I	***0.8***	3.0	***4.0***	7.0	***2.0***	5.4	–	4.9	***0.8***	3.7	***0.1***	0.6
WS II	–	–	***0.4***	–	***2.7***	17.0	–	–	***0.6***	3.6	***22.5***	17.4

Table 2 Base Ring and White Slip as a percentage of the Enkomi ceramic assemblage by Level showing the significant differences apparent when plain wares are included in the statistics. Percentages calculated for the present study shown in the first columns in bold italic script and those by Dikaios (1969–71:442, 444, 447) in the second columns in Roman type

White Slip and Base Ring and also Monochrome, which as discussed in the introduction, are now understood to be rare in eastern Cyprus at the beginning of the LC. Dikaios published statistics of the percentages of the fine and decorated wares within the assemblage, focussing on the imported wares, especially the Mycenaean pottery, in an effort to tie in the sequences of occupation at Enkomi with known historical events (Dikaios 1969–71:442, 444, 447). Statistics for the plain wares were not published, resulting in the ommission of over 50% of the assemblage and creating a vastly inflated picture of the importance of certain wares at the site, including the Base Ring and White Slip wares (see Table 2). Generally, the percentages published by Dikaios are at least double those calculated for my study. The exception is the WS II in Level IIA of Area I, where my statistics exceed Dikaios' due to the special nature of the Room 142 assemblage.

Base Ring Ware (BR)

The ware is subdivided into Proto Base Ring (PBR), BR I and BR II on the basis of fabric, shape, decoration and surface treatment, with chronological relationship between the divisions implied (Åström 1972a:126–198, 1972b:700–701). PBR is thought to have evolved from a number of different MCIII regional ceramic traditions. These include the south coast region around the area of Episkopi-*Phaneromeni* (from Drab Polished Ware) and in the northwestern region around *Toumba tou Skourou* and the Myrtou group of sites (from Black Slip ware) (Herscher 2001). A comprehensive program of petrographic and macroscopic characterisation of the fabrics undertaken by Vaughan (1987), in order to define the ware subsets and provide provenience and chronological data, identified four fabric groups, three of which occur in both BR I and BR II, and established that superficial appearance or fabric type is not a reliable indicator of chronology (Vaughan 1994:89). Therefore, although I have retained the traditional terminology of BR I and BR II, in order to facilitate comparison with the statistics calculated by Dikaios, the divisions are based upon the superficial appearance of the material (as defined by Åström 1972a) and should not be considered to have chronological significance.

Figure 3 shows BR I and BR II amalgamated and illustrates the ware's occurrence as a percentage of the assemblage by fine phasing from Levels A–IIA. BR occurs only in small quantities in the stratified deposits at Enkomi but may have increased in popularity as a funerary offering during LCIB (from tomb contents listed in Dikaios 1969–71:348–418). Although a detailed stylistic analysis of the BR ware was beyond the scope of my research, a few comments on the assemblage are relevant. No flat bases or features diagnostic of PBR were found but as the majority of the sherds are very small and mainly non-diagnostic, identification remains unlikely. One example of PBR is known from French Tomb 5 at Enkomi (Åström 1972a:131). Shapes represented most commonly at Enkomi are small bowls, with very few sherds from larger closed vessels or juglets. A horn from a BR II bull rhyton occurs in a Level IIA end deposit from Area III (within Inv. 2798 in Room 5).

Table 2 retains the traditional typological divisions and shows the occurrences by coarse phasing, alongside the statistics calculated by Dikaios (1969–71:444, 447). It is obvious (as discussed in the preceding section) that Dikaios' statistics greatly exceed those calculated for my study. Fine-scale phasing further illuminates the occurrences of the ware (see Fig. 3). BR does not occur in Area I, Level IA or Level IB construction deposits and only a single sherd is present in

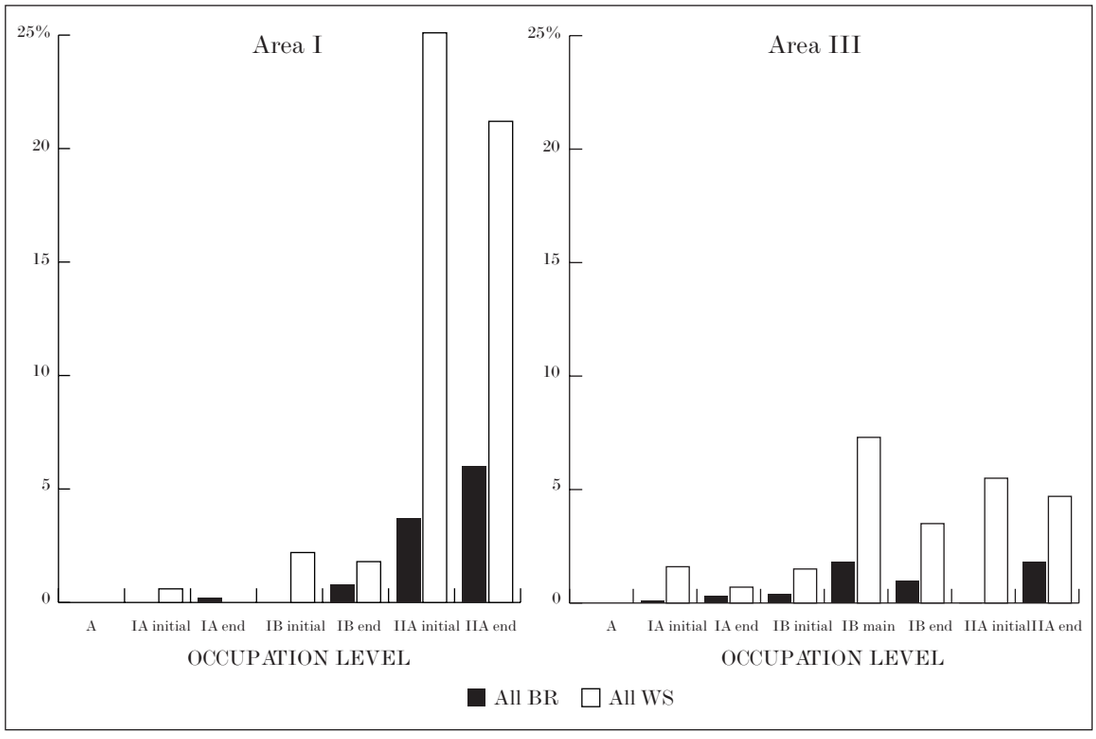


Fig. 3 Base Ring and White Slip as a percentage of the Enkomi assemblage by occupation Levels

Level IA end deposits. In contrast, BR (albeit only a single sherd) is present in a Level IA construction context from Area III (contained in Inv. 2300 from Room 101) and figure rises to 0.3% of the assemblage (sixteen sherds) in Level IA end deposits. Figure 3 illustrates that there is a slight increase in the amount of BR during Level IB in Area III, decreasing during Level IB end, most probably due to the deposition of material relating to collapse of the mudbrick superstructure of the building, erected prior to the greater occurrence of BR at the site. This pattern is mirrored for the WS (discussed below).

Material with the appearance of BR II first occurs during Level IB late in Area III (two sherds which have been included in the Level IB main category) and in Level IB end deposits in Area I. Distribution of BR sherds in the Level I deposits in both areas appears to be random and it is not until the appearance of BR II at the site that the ware appears in larger numbers, and then only in Area I. Again, it should be noted that the higher numbers in Area I during Level IIA are from a single room (Room 142), and may reflect the special nature of this deposit.

White Slip Ware (WS)

The ware was codified by Popham (1972a, 1972b) who divided it into three main variants, PWS, WS I and WS II, based upon fabric, slip and decorative techniques, forming a chronological sequence. The ware has figured extensively in chronological debates due to its widespread distribution throughout the Aegean and eastern Mediterranean (*cf.* Manning 1999, papers in: Karageorghis 2001). PWS first appears in LCIA1 and WS I in LCIA2 (Åström 2001b:50). WS II first appears at the end of LCIB or the beginning of LCIIA (Åström 1972b: Table pp. 700–701). Although considered partially to have developed out of northwestern Cypriot WP traditions (with *Toumba tou Skourou* considered to be integral to this development), the technological innovations present in WS manufacture have also been linked with pyrological advances in the copper industry and exposure to Syro-Palestinian slipping techniques (Eriksson 2001:52–53). The distinctive clays used for the manufacture of WS are different from all other Cypriot fabrics and comprise several compositional subgroups (Artzy *et. al* 1981:45). Changes in fabric, slip and pigment useage have been shown to correspond with the typological and chronological divisions of the ware (Aloupi *et al.* 2001).

The majority of sherdage in Area III (99.4%) comes from small bowls. Only two sherds are from large bowls and a single large closed sherd is present. In Area I, all sherds are from small bowls. Although,

Level	A	IA initial	IA end	IB initial	IB main	IB end	IIA initial	IIA end
Area I	(19)	(335)	(419)	(179)	–	(1851)	(271)	(448)
PWS	0	0.6 (2)	0	0.6 (1)	–	0.2 (4)	0	0
WS I	0	0	0	1.7 (3)	–	0.8 (14)	0.4 (1)	0
WS II	0	0	0	0	–	0.7 (13)	24.7 (67)	21 (94)
WS Gen.	0	0	0	0	–	0.1 (2)	0	0.2 (1)
Area III	(261)	(695)	(4732)	(1683)	(2662)	(3078)	(90)	(2506)
PWS	0	0.1 (1)	<0.1 (2)	0	<0.1 (1)	<0.1(1)	0	<0.1 (1)
WS I	0	1.3 (9)	0.7 (33)	1.5 (25)	7.2 (192)	2.7 (82)	2.2 (2)	2 (51)
WS II	0	0	0	0	<0.1 (1)	0.8 (26)	3.3 (3)	2.6 (66)
WS Gen.	0	0.1 (1)	0	<0.1 (1)	<0.1 (1)	0	0	<0.1 (1)

Table 3 White Slip wares as a percentage of the total Enkomi assemblage by fine phasing. Numbers in brackets indicate sherd count

a detailed stylistic analysis was again beyond the scope of my research, this would be a useful avenue for future investigation, in order to identify connections with other LC sites and to delineate where in the WS I and II sequences the material from the different levels is placed. As PWS was not recorded separately by Dikaios (1969–71:444, 447), I have assumed that PWS and WS I have been lumped together in his statistics.

In Area I, two sherds of PWS occur in Level IA construction deposits (both from Room 121) and no PWS or WS I is present in Level IA end deposits. Due to the nature of Dikaios' publication of the site, previous researchers have been under the impression that PWS and WS I appeared simultaneously at Enkomi but the separation of the Level IA construction and end deposits would appear to indicate that there is a chronological horizon between the appearance of the wares at the site. As discussed in the preceeding section, it is my contention that the Area I building was constructed prior to the Area III building. The only WS I sherd included in Dikaios' catalogue from Level IA of Area I was from Room 112 (Inv. 5830/1, Dikaios 1969–71:553, pl. 56.14). Room 112 has not been included in my analysis due to severe disturbance by the Level IIB occupants at the site and the presence of LCIIC sherds in the supposedly early contexts. The only other relevant material listed in the catalogue of Level I pottery (Dikaios 1969–71:543–553) and not included in my study, is an additional PWS sherd from a Level IA context in Room 117 (Inv. 2288/1, Dikaios 1969–71:546, pl. 56.10, not located in the museum stores). The dramatic discrepancy between Dikaios' Level IA figures and my study is best explained by his inclusion of Room 112 and deposits relating to poorly defined external areas.

In Area III, six sherds of PWS occur, in contexts ranging from Level IA to Level IIA end, and it first appears alongside WS I in a Level IA construction deposit (within Inv. 2296 and Inv. 2297–8 both equaling Floor X-bedrock in Room 101). WS I is not present in significant numbers until mid Level IB in Area III. During this phase 82% of all WS I sherds (157 of a total of 192 sherds) are found in Room 103 and WS I as a percentage of the Room 103 Level IB main assemblage is 11.7% (drastically raising the average amount for this level to 7.2%). The remainder of the WS I sherdage for Level IB main is dispersed evenly throughout the building. This should not be considered of overmuch much significance as Room 103 material comprises 50% of the sherdage for this Level but there does seem to be some concentration of the ware. Interestingly, Room 103 is the room that exhibits the most intensive copper working evidence during this phase and is also where the fragmentary Cypro-Minoan tablet was found (Dikaios 1969–71:22–23, pl. 248).

In Area III, one sherd of WS II is present in a Level IB late context (from Inv. 3789 in Room 108 and included in the Level IB main category) and by Level IB end deposits is increasing in quantity (to 26 sherds). During Level IB end in Area I, both WS I and WS II are almost equally represented (see Table 1). In Area III during Level IIA, WS II (3.3%) outpaces WS I (2.2%) but the most dramatic contrast is seen in the Level IIA, Area I deposits where WS II is second only to Plain White ware during this phase.

These figures must be taken within the context of the single room to which they belong but it is a striking difference with both the earlier phases of Area I and the Level IIA building in Area III. As noted above, the presence of LH IIIA:2 ceramics require a date of LCIIA2 at the earliest for the Level IIA building in Area I.

White Slip, Base Ring and LC ceramic regionalism

The only other settlement with LCI material published to an extent where statistical comparisons can be drawn is the south coast site of Episkopi-*Bamboula*, located within the BR and WS production zone. Deposits dated to LCIA2 or LCIB show around 18% BR and 26% WS, and LCIB deposits show up to 25% BR and up to 14% WS (Benson 1970:42–73; Åström 1972b:675–686). Other sites located in the eastern or Karpas regions exhibit only insignificant amounts of the wares. Trench 9 at Kalopsidha has total WS at around 0.5% of the assemblage and BR at 1.8% (Åström 1966:49). Kalopsidha Trench 9 is considered here to date primarily to LCI, with limited use extending into LCIIA (see Crewe 2004:104–109 for further discussion of the relative dating of deposits at Kalopsidha). The other two eastern LCI–IIA sites with published ceramic statistics, Phlamoudhi-*Vounari* (Al Radi 1983) and *Nitovikla* (Hult 1992), show BR at a maximum of 0.8% and WS at a maximum of 1.7% for the former site and BR at 0.4%, WS at 0.3% for the latter. Although material from the majority of the coastal centre excavations has been incompletely published, the overwhelming impression from the ceramic repertoire of all early LC sites is the strong regional identity and extremely infrequent deposition of ceramics from areas outside the immediate hinterland of the coastal settlements. The exceptions to this may be the south coastal centres at Hala Sultan Tekke and Maroni, which, located at the junction between the regional zones, appear to display a mixture of eastern and northwestern ceramic types (Manning *et al.* 2002).

Perceptions that an island-wide material culture was adopted by LCIB are largely coloured by the greater extent of cemetery excavations and incomplete publication of settlements. This issue has been explored by Merrillees (1971:60–62, 72), who concluded that for whatever reasons of ritual or cultural preference, there were very few MC-style vessels buried with the inhabitants of the early LC tombs at Myrtou-*Stephania* (Hennessy 1963) contrasted with the assemblage at the nearby settlement at Myrtou-*Pigadhes* (Catling 1957) and that ceramic innovations in mortuary assemblages do not accurately reflect contemporary settlement use of these wares. Further investigation into the discrepancies between the deposition of mortuary and settlement ceramic types is required. For example, juglets are an important component of mortuary assemblages at LC sites (Keswani 1989:576; Maguire 1995), including Enkomi, but are poorly represented in settlement strata. WP is the only ware occurring in the occupation deposits at Enkomi from which high proportions of sherds originating from juglets and small closed vessels occur (Crewe 2004:231) and I believe that this may be viewed as a by-product of trade in these vessels passing through the site.

Merrillees (1971) suggested a disruption in Enkomi's control over external trading contacts at the end of LCIA due to the effects of the expulsion of the Hyksos. A combination of the occupation and ceramic evidence indicates that the disruptions caused to Enkomi's trading links may be more severe than previously supposed. Close links between Hyksos-ruled Egypt and eastern Cyprus are attested in the amount of eastern Cypriot ceramics at Tell el-Dᶜaba (Maguire 1990, 1995) and, although the specific mechanisms are unknown, a breakdown of the regular trading channels may have occurred, causing some disruption to Enkomi's fortunes during LCIB. Given the small amounts of WS and BR, particularly closed vessels or juglets, in the Enkomi deposits, it seems difficult to argue that Enkomi played a role in the redistribution of these wares abroad. Based upon the dramatic decline in imports of Canaanite jars in Level IB deposits (Crewe 2004:275–278), along with the increased activity relating to metallurgical production occurring in the Area III building during Level IB, it seems that the occupants of Enkomi may have sought to restructure their role in external trade and that during this crucial phase, regions which were already producing the BR and WS wares exploited their own trade networks (Manning and De Mita 1997), supplanting the trade in goods contained in ceramic commodity containers reflected in the transition from WP to BR juglets for export during this period. A hiatus in eastern exports, whilst the western exports increased may also have seen a change in fashions on the mainland and the earlier WP-style ceramics may no longer have been in demand. Although it remains a possibility that Enkomi was serving as a transshipment point for WS and BR wares without adopting them for their own consumption, it would appear more likely that sites where these wares were common at this time served as the trading partners of the merchants from the

mainland. Although WS I begins to occur in significant proportions during Level IB main (7.2% of the assemblage) at no time does BR become common before Level IIA (remaining at less than 1% of the assemblage).

I also argue against any 'resistance' by the eastern Cypriots to central and western innovations in ceramics during LCIA or the abrupt adoption of wares of these styles in LCIB (*cf.* Merrillees 1971:70; Manning 2001). As outlined above, comparison of mortuary and settlement data indicates that different processes were operating in relation to the usage of the wares. It would appear far more likely that the reason BR and WS wares were not an important feature of settlement assemblages is that the eastern region developed their own set of innovations, more relevant to their specific cultural responses to increased external contacts. These combinations of tradition and innovation are manifested in wares such as the Bichrome, Plain White and White Painted Wheelmade wares. The close association of WS with the copper producing regions and pyro-technological advances (Jones and Catling 1986:526; Knapp and Cherry 1994:160 with references) suggests that this ware was more likely to accompany copper exports than eastern wares. My thesis regarding the later date for construction of the Area III building effectively removes Muhly's (1986:200) dilemma that the copper industry was apparently already established at the time of the initial foundation of the site. Increase in WS during Level IB in Area III, particularly in Room 103, the room most closely associated with copper working evidence, may indicate that the ware was being transported to Enkomi along with primary smelting debris.

Despite the fact that residual sherdage from ealier levels has undoubtedly caused the incidence of the non-local wares to be underestimated, the presentation of the above data from Enkomi, and the limited information available from other excavated sites of the period must call into question the widely held assumption that regionalism in ceramics ceased at the beginning of LCIB. It is my contention that LC regionalism persisted until LCIIA–B, when other features, such as the widespread appearance of imported LHIIIA:2 pottery (Sherratt 1999) and the introduction of an island-wide complex of ritual paraphernalia, such as BR bull rhyta and Astarte-type figurines appear (Webb 1999; Nys 2001). It is not until this time that the remainder of the ceramic wares which had their roots in MC stylistic traditions, such as the previously ubiquitous Red and Black Slip wares ceased to be produced and the adoption of Plain White ware became island-wide (Crewe 2004:273–275). It is possible to see the incorporation of the WS and BR wares into the ceramic repertoire at Enkomi during LCIIA as a combination of economic factors, related to the wares' evident popularity as an export, and a greater sense of island-wide identity.

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Early and Middle Bronze Age Stratigraphy and Pottery from Aegina Kolonna

Walter Gauss[*] *and Rudolfine Smetana*[**]

This paper tries to demonstrate the development of pottery found at Aegina Kolonna from the Early Helladic III period to the beginning of the Middle Helladic period.[1] It will also attempt to compare the stratigraphic excavation results from the two main excavation areas with each other, namely the fortification wall (Fig 1 A), and the so called inner city to the west (Fig. 1 B). The excavations of the fortification wall in the late 60's and 70's were directed by the late Hans Walter and Florens Felten and formed the basis of the stratigraphic exploration of Aegina Kolonna.[2] Important additional evidence for the stratigraphic sequence was obtained during the 1993 to 2002 excavations directed by F. Felten and Stefan Hiller in the so called prehistoric inner city, particularly in area 19[3] (Fig. 1). Based within the SCIEM 2000 project a new excavation was initiated in 2002 with a primary focus on the Middle Bronze Age and the transition to Late Bronze Age.[4]

At Aegina Kolonna three phases of occupation



Fig. 1 Aegina Kolonna, schematic plan

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[1] For discussion and comments we would like to thank F. Felten, J.B. Rutter and D. Scahill, who read and corrected the manuscript. As our studies are not yet finished, the paper presents state of our research.

[2] Walter and Felten 1981; Gauss and Smetana 2003; Gauss and Smetana 2004. For the labelling system used to catalogue material from the old excavations see: Gauss and Smetana 2003, 472 n. 16; Gauss and Smetana 2004, 1158 n. 56.

[3] Felten and Hiller 1996; Gauss and Smetana 2004.

[4] Felten *et. al.* 2003, 54–63; Felten *et. al.* 2004, 114–126.

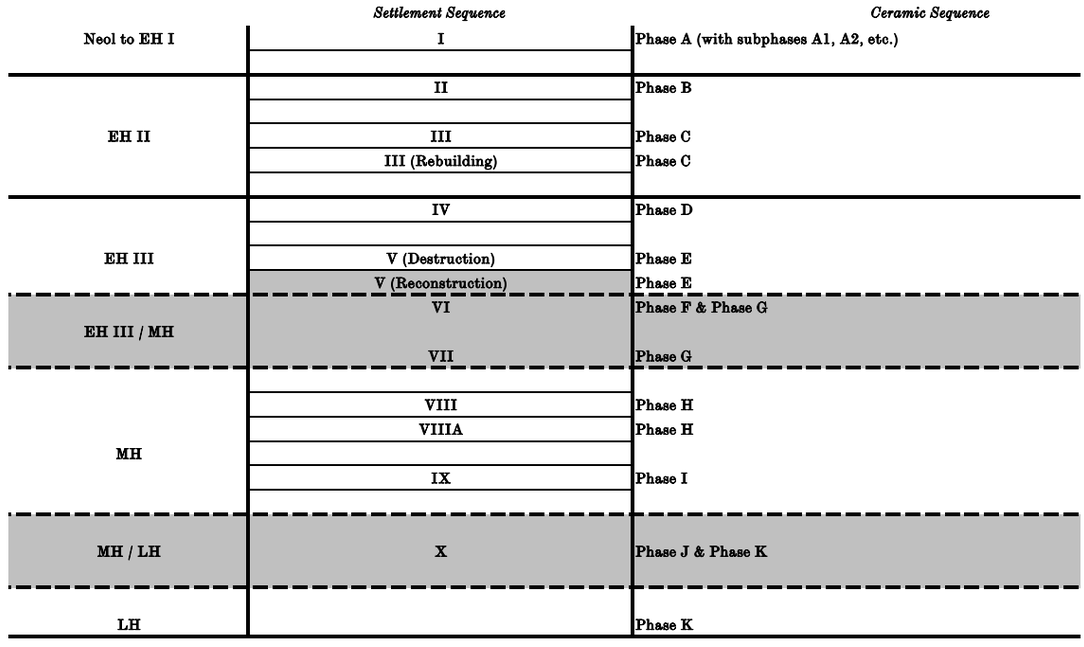


Fig. 2 Aegina Kolonna, Early and Middle Bronze age settlement and ceramic sequence

have been distinguished for the Early Helladic III period (Kolonna IV, V and VI), whereas four settlement phases (Kolonna VII to X) have been differentiated for the Middle Helladic period (Fig. 2). Until now the same terminology has been used for describing the sequence of ceramic phases, which in our opinion is liable to be misunderstood. We therefore suggest that a clear distinction should be made between the stratigraphic sequence of these settlement phases and the sequence of ceramic phases[5] (Fig. 2). The settlement phases Kolonna VI and VII are marked on Figure 2 by a grey background, as the correlation of settlement and ceramic phases is most difficult.[6]

The stratigraphic sequence of the settlement phases Kolonna IV to Kolonna VI can be best demonstrated with the help of the excavations in the area of the fortification wall (Fig. 3). Above bedrock a fill layer was found (Fig. 3, 1), containing mainly Early Helladic II sherds and a few Early Helladic III sherds. On top of that a floor was identified, covered with fill (Fig. 3, 2; 4, 11–14). The walls and the floor are attributed to the settlement phase Kolonna IV.[7] The few characteristic sherds can be assigned to an early stage of the Early Helladic III period and define our ceramic phase D. On top of that a floor with an impressive destruction layer was identified. This destruction horizon characterises the end of settlement phase Kolonna V[8] (Fig. 3, 3; 4, 8–10). In the heavily burnt debris approximately 120 complete or almost complete vessels have been found.[9] This pottery defines our ceramic phase E.[10] After the destruction the occupation ends in this area of the settlement. The ruins were filled up with stones and formed the basis for the newly erected fortification wall (Fig. 3, 4; 4, 1–7).[11] The construction of the fortification wall defines the settlement phase Kolonna VI.[12] It is important to note that no architectural remains of the Kolonna VI settlement other than the fortification wall were found in this excavation area. Thus ceramic deposits characterising the pottery of the Kolonna VI settlement are more or less absent.[13]

[5] Gauss and Smetana 2004, 1158–1159.

[6] The terms used in the chart are preliminary, as neither the work of Lydia Berger, who studied the Early Helladic II stratigraphy and pottery, nor our own research are finished yet. Our final goal would be the creation of a common pottery phasing system for the entire assemblage of Bronze Age pottery found at Aegina Kolonna.

[7] Walter and Felten 1981, 23–28 (Kolonna IV settlement). See also: Gauss and Smetana 2002, 12–13; Taf. 1; Gauss and Smetana 2003, 472–474.

[8] Walter and Felten 1981, 28–42.

[9] Walter and Felten 1981, 108–117; Gauss and Smetana 2003, 474–478.

[10] Gauss and Smetana 2003, 373–478. Gauss and Smetana 2004, 1160–1163.

[11] Walter and Felten 1981.

[12] Walter and Felten 1981, 43–50 (Kolonna VI settlement).

[13] Walter and Felten 1981, 140. The pottery from Fundgruppen XXIII, XXXIV and XXV was found in the Kolonna VI fortification wall. Fundgruppe XXII was associated by

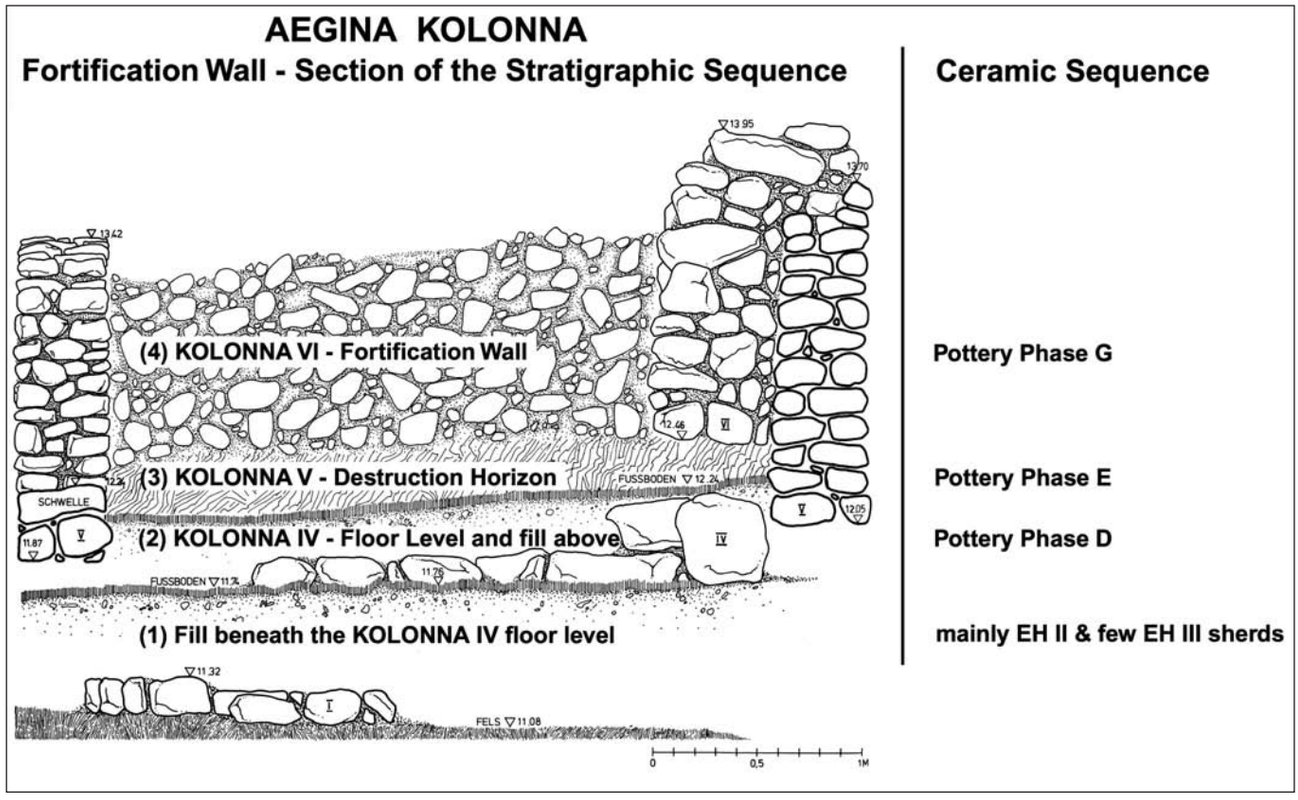


Fig. 3 Schematic sequence in the area of the fortification wall, based on Walter – Felten 1981, 44 fig. 36

Therefore most important are the results of the newly studied sherds from the fill of the Kolonna VI fortification wall (Fundgruppe XXIV) (Fig. 4, 1–7), as the dating of the sherd material provides a terminus post quem for the erection of the fortification wall.[14] The number of sherds found in and beneath the wall is not large. Most fragments show ordinary Early Helladic III features, not much different from phase E pottery. However, one sherd (XXIV-13) shows features not common until the transition to the Middle Bronze Age (Fig. 4, 6). Moreover the rim fragment joins with a vessel already published by H. Walter and F. Felten[15] (Fig. 5). The best parallels for this kind of a solidly painted and burnished vessel with horizontal grooves on the exterior of rim and shoulder and on the interior of the rim are found at the so called Transitional Phase at Lerna.[16] At Kolonna, these horizontal grooves make their first appearance in pottery phase G (see below page). At the moment, and stressing the fact that the final conclusions have not yet been drawn, we feel the fortification wall of Kolonna VI was erected at the transition to the Middle Bronze Age. We therefore have to raise the question whether or not the Kolonna VI settlement phase should still be called purely Early Helladic III rather than Early Helladic III to Middle Helladic. As house remains and well stratified ceramic deposits of the Kolonna VI settlement have not been found until recently, we do not know if the Kolonna VI settlement lasted for some time. Neither architectural nor cermic subphases have been distinguished yet for the Kolonna VI settlement.

In the next phase of occupation, Kolonna VII, which is not shown on Figure 3, the fortification walls were strengthened and new entrances built.

stylistic criteria with the Kolonna V settlement by H. Walter and F. Felten (1981, 145). This association has been criticized by Rutter 1983, 107; Rutter 1995, 643. Fundgruppe XXII was found on top of the destruction debris of the Kolonna V settlement. It was deposited when the fortification wall of the Kolonna VIII settlement was erected.

[14] Walter and Felten 1981, 145 (Fundgruppe XXIV) The sherd material was studied in summer 2003 and in spring 2004.

[15] Walter and Felten 1981, 175 Katnr. 442; Taf. 121 (XXXVII-1).

A different stratigraphic situation has been found in the western part of the settlement, in the so-called inner city. The following explanations focus mainly on the excavation results of area 19. Unfortunately no traces of the Kolonna IV settlement have been discovered. In general this occupation level is lacking in the inner settlement apart from a few exceptions.[17] Traces of a heavily burnt floor horizon (layer 19/38) are situated directly above what remains of the Early Helladic II occupation levels[18] (Fig. 6). The vessels found on the floor are, rather, similar to the pottery found in the destruction debris of the Kolonna V settlement.[19] Therefore the pottery from context 19/38 may be attributed to our ceramic phase E and the floor horizon is to be correlated with the Kolonna V settlement phase.

In contrast to the excavation area of the fortification wall, the occupation and habitation at area 19 does not cease with the destruction. Especially interesting is the fact that immediately above the burnt floor another floor horizon was determined (layer 19/36). The fill above the floor contained few sherd of little significance (Fig. 7).[20] At the moment it is not clear to which settlement phase this floor horizon may be attributed and whether the pottery in the fill above the floor is to be linked with our ceramic phase E or phase F. Perhaps the floor is part of the rebuilding phase immediately after the destruction of the Kolonna V settlement.[21]

Above this floor horizon and fill another floor layer was distinguished (layer 19/28). On this floor, which was partly covered with an ashy layer, at least 12 complete or nearly complete vessels were found (Fig. 8–12). The characteristic elements of this context may be summarised as follows: The large proportion of pattern painted-pottery (Fig. 8; 11, 2–5; 12, 2–4) is remarkable and differs from the sparsely pattern-painted vessels of our ceramic phase E. New vessels appear, with shapes, decorative motifs and technological features[22] unknown in ceramic phase E (Fig. 11, 1. 4. 5; 12, 1, 3. 4). Nevertheless, syntax and motifs of the pattern painted pottery can be placed within the corpus of Early Helladic III pottery. Finally, characteristic elements of the Middle Bronze Age pottery, such as "real matt painted" or so called lustrous decorated pottery, are not present. We therefore use context 19/28 to define our ceramic phase F. According to our present knowledge, ceramic phase F may belong to a very late/final stage of the Early Helladic III period. Most important are the following four vessels (Fig. 8; 11, 1. 4. 5; 12, 1, 3. 4):

The large narrow necked jar (Fig. 8) is exceptional with respect to its size and the quality of its pattern painted decoration. Two alternating motifs, multiple triangles with a cross-hatched centre and cross-hatched rectangles, decorate the shoulder.[23] The combination of these two motives seems to be a late and perhaps also typical Aeginetan feature[24] (Fig. 13, 1). Another exceptional find is the large ovoid narrow-necked jar (Fig. 11, 1; 12, 1); it is handmade, solidly painted with an orange paint and highly burnished. According to macroscopic and petrographic analysis it is of Aeginetan origin. A completely preserved neck and rim of the same shape was found in another context that is also attributed to pottery phase F (Fig. 13, 2).

The beaked jug (Fig. 11, 4; 12, 3) is pattern painted with a Dark-on-Light decoration consisting of vertical bands with hatching and, below the handle, a multiple triangle with cross-hatching. Fragments with a similar pattern painted decoration from other excavation areas may belong to the same vessel

[16] Zerner 1978, 191, characterising the Transitional pottery: "Cups and bowls have rounded bodies which swell at the shoulder. Shoulder zones and interiors of rims were incised with horizontal lines. […]." We are thankful to J.B. Rutter and C. Zerner for the comments on vessel XXXVII-1 and this class of pottery in general. See also Rutter 1995, 626: "But not until Phase 3 does such decoration become widely used in a variety of classes and positions and on a fairly wide spectrum of shapes. The combed version of such ornament, which is typical of the Solidly Painted and Burnished class, above all on handmade kantharoi and Bass bowls, surely imitates the equivalent decoration on contemporary vessels of precisely the same shapes, but wheelmade, in the Fine Burnished Gray Class."

[17] Gauss and Smetana 2004, 1160 mit Schlußfolgerungen.

[18] The Early Helladic II remains belong to a late phase (phase E according to L. Berger).

[19] Walter and Felten 1981; Gauss and Smetana 2003, 472–474; fig. 2–6.

[20] Number 1 is a solidly painted and un-burnished closed vessel, number 2 a tiny fragment of a Light-on-Dark pattern painted vessel of non-Aeginetan origin.

[21] Gauss and Smetana 2003, 472; Gauss and Smetana 2004, 1159. 1161.

[22] Such as the handle attachment of the kantharos (shape Rutter XI; 44 Ill. C-5). Compare with Rutter 1995, 209–210 catnr. 1053; fig. 84, 1053.

[23] Multiple triangles with a cross-hatched center are not found within the corpus of pattern painted Early Helladic III pottery at Lerna.

[24] At Lerna in the Argolid both motifs seem to be unknown.

shape, including a larger fragment of spout and neck that was found during the pre-WW II excavations under the direction of Gabriel Welter (Fig. 13, 4–7). The pattern painted bowl with the slightly incurving rim and with a rim handle is another new vessel shape (Fig. 11, 5; 12, 4). The way how its paint is applied may remind of matt painted pottery of the Middle Helladic period. However, the motif of the cross-hatched rectangles is an Early Helladic III motif, known for instance from the large amphora (Fig. 8; 13, 1) but also from other bowls with an incurving rim found during new as well as the pre-WW II excavations (Fig. 13, 8–10). Until now the cross motive and the row of dots have been unknown in the repertoire of the Early Helladic III painted pottery at Aegina Kolonna.

It is also most important to note that most vessels are according to macroscopic and petrographic inspection (Fig. 11, 1; 12, 1) of local Aeginetan origin. However, a Cycladic influence or inspiration is perceptible. Having in mind the opposing opinions on the synchronisation of the Cycladic and Mainland chronologies in the Early Bronze Age throughout the last two decades, Aegina Kolonna and especially context 19/28 are of fundamental importance for solving these problems.[25] A Cycladic influence is obvious for the beaked jug (Fig. 11, 4; 12, 3), which is made of local clay and decorated in Early Helladic III manner but imitating a common Cycladic shape. Close comparisons have been found at Phylakopi on the island of Melos.[26]

A Cycladic or perhaps even further eastern inspiration has been perceived for the solidly painted and burnished narrow necked jar (Fig. 11, 1; 12, 1; 13, 2), but no exact parallels have been found so far. The best comparison is a Cycladic import found in a slightly later context at Kolonna itself Fig. 14). Roughly similar vessels with a somehow less accentuated narrow neck and rim handle (solidly or pattern-painted) are known for example from Kolonna,[27] Phylakopi[28] or Samos (Fig. 13, 3).

When does the Early Bronze Age end at Aegina and when does the Middle Bronze Age begin? And what does its pottery look like?[29] We assume that the stratigraphic sequence of excavation area 19 at Kolonna also provides information regarding what the context dating for the transition of Early to Middle Bronze Age might look like. Above the floor layer 19/28, a fill consisting of two strata was perceived, which contained mainly small sherds with few matching pieces. Nevertheless, the pottery shows characteristics not present in the previously described ceramic phase F. We therefore use it to define our ceramic phase G.

In contrast to our ceramic phases E and F the amount of fine grey pottery increases in phase G. This is true for imported pottery but also for that which is locally manufactured (Fig. 15, 1–4; 16, 1–4). For the first time so-called potters' marks are found on local Aeginetan pottery (Fig. 15, 4; 16, 4). Dark burnished so-called Bass Bowls (Fig. 15, 5–6; 16, 5–6) are a characteristic shape for the previous ceramic phases. They are still common, but now the rim may have a more angular profile and for the first time horizontal grooves or incised lines are found on the exterior and interior of the rim and shoulder.

For the first time unpainted and burnished or smoothed bowls with an incurving rim appear, all made of light brown to buff clay (Fig. 15, 7–9; 16, 7–9). The increasing amount of vessels made of light brown to buff clay is to be distinguished from the pottery of phases E and F. Matt painted pottery of Aeginetan origin is absent and this is also true for Minoan imports.[30] But for the first time few fragments of imported lustrous decorated pottery (Fig. 15, 10–11; 16, 10–11) and imported Light-on-Dark matt-painted pottery can be found (Fig. 15, 13). The fragment of an incised closed vessel (Fig. 15, 12; 16, 12) is much worn; originally it was solidly painted and burnished and might belong to a large duck vase.

The medium to coarse pottery (Figs. 17–18), also shows changes in shape and surface treatment. The surface of the entire vessel is generally speaking smoothed. This stands in real contrast to the pottery

[25] See in particular Gauss and Smetana 2004, 1165–1166 with references.

[26] Gauss and Smetana 2004, 1164 n. 92; Gauss and Smetana (in print). For similar vessels said to come from Melos see: Zervos 1957, 120 fig. 130–132; 1125 fig. 144.

[27] The fragments of Fig. 13, 3 were published by Kilian-Dirlmeier 1997, 131 catnr. 63; fig. 74, 63; pl. 9, 63; see also 133 catnr. 72; fig. 75, 72; pl. 10, 72.

[28] Atkinson et. al 1904, pl. 8, 7 and 152 fig. 134 (with small spout on the side of the body).

[29] See also Gauss and Smetana 2002, 15–16 n. 33–36. For the division between Early Helladic III and the Middle Bronze Age see in particular: Zerner 1978, 4. 6–12. 22–25. 191–192 and Rutter 1986.

[30] For the first appearance of Minoan imports at Aegina see: Gauss 2006.

from ceramic phases E and F where traces of coarse burnishing on the shoulder and especially on the interior of the rim were rather common. At the same time the transition from shoulder to rim becomes more angular.[31]

Hopefully further research will provide us with more distinguishing features.

Summary

To avoid misunderstandings we would like to differentiate between settlement phases and ceramic phases. For the latter, a preliminary numerical order is used, starting with phase D. Our final goal would be to establish a common pottery phasing system for the entire assemblage of Bronze Age pottery at Aegina Kolonna.

Occupation levels belonging to the settlement phase Kolonna IV are sparse. The associated pottery shows characteristic features, which define our ceramic phase D.

Traces of the Kolonna V settlement have been found throughout the entire excavation area west of the fortification wall. The Kolonna V settlement ends in a massive destruction horizon. The pottery found in this horizon is uniform and defines our ceramic phase E.

In at least two different places a floor level was found immediately above the destruction horizon. The pottery associated with the floor and the fill above is not very significant. At the moment it is not clear to which settlement and ceramic phase this horizon may be attributed.

A large complex of pottery – deposit 19/28 – defines our ceramic phase F. To our present knowledge it may be dated to a final stage of the Early Helladic III period. New vessel shapes and motives appear and a Cycladic influence is perceptible. A correlation of ceramic phase F and Phylakopi phase I–ii is likely.

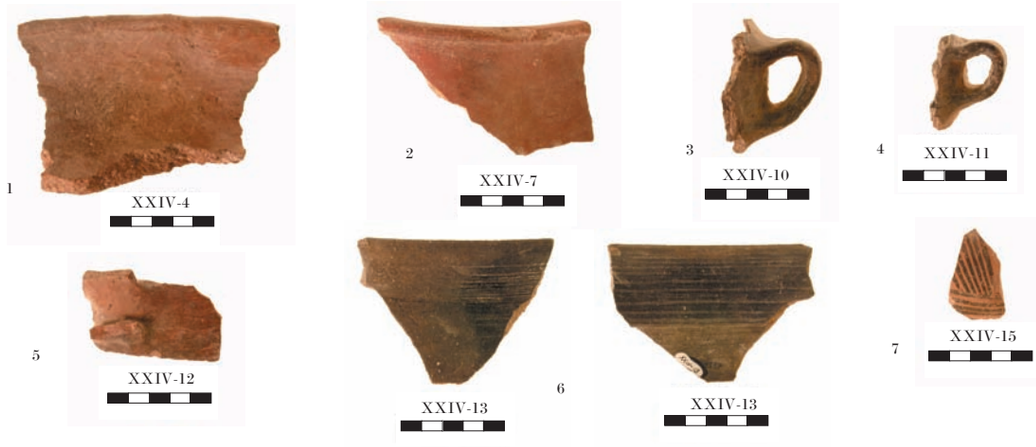
The layer found on top of ceramic phase F contained numerous small sherds with few joining pieces. The pottery shows features not present in the previous ceramic phase F and we therefore use it to define our ceramic phase G.

The fortification wall of the Kolonna VI settlement was erected at the transition to the Middle Bronze Age. The latest pottery found in the fill of the fortification wall belongs most likely to our ceramic phase G.

The features of the phase G pottery assemblage prove that there was an increase in imported as well as locally produced grey pottery. For the first time so-called potters' marks appear on the locally produced pottery. Furthermore it is important to note the increase of open vessels made of light brown and buff clay. Unpainted bowls with an incurving rim, so-called lustrous decorated pottery and matt-painted pottery appear for the very first time. Both of the latter are imported and found only in small quantities, whereas bowls with an incurving rim are frequently found. Aeginetan matt-painted pottery as well as Minoan imports are absent. We therefore assume that the pottery of phase G shows the transition from the Early Helladic to the Middle Helladic period.

[31] See also Maran 1992, 187 with similar observations and criticism on Dietz 1991, 33.

Pottery from the fill of the Kolonna VI fortification wall (ceramic phase G)



Pottery from the destruction horizon of the Kolonna V settlement (ceramic phase D)



Pottery from the floor and fill above the Kolonna IV settlement (ceramic phase D)

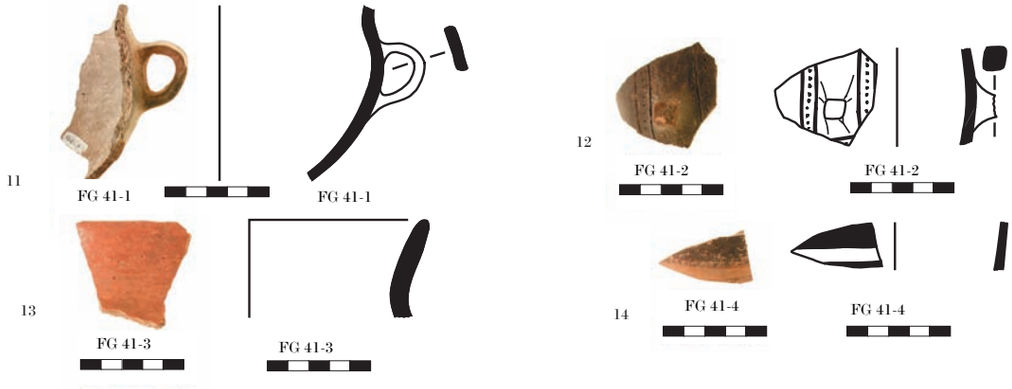
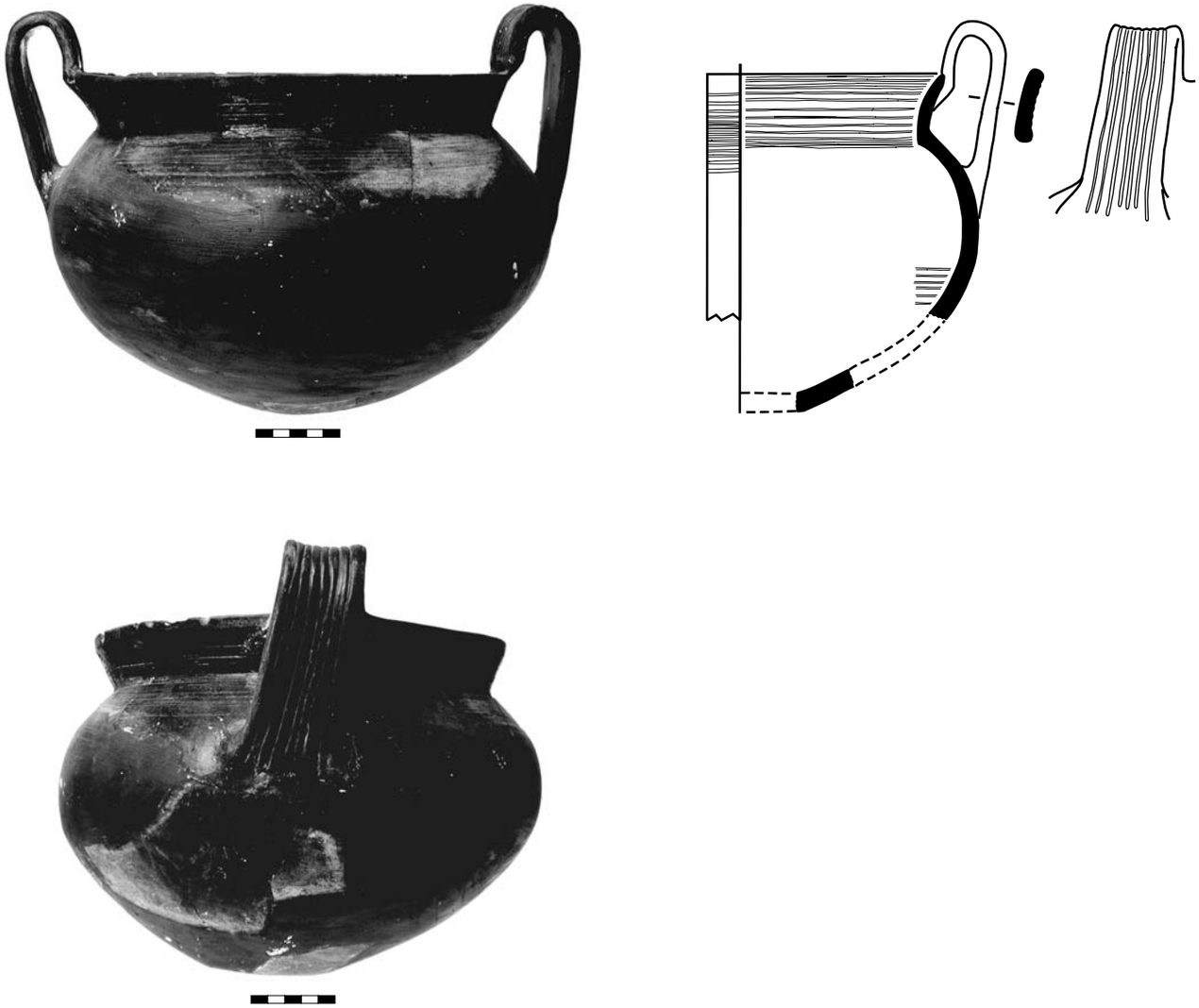


Fig. 4 Pottery from the sequence of the fortification wall (photos): 1) XXIV-4; 2) XXIV-7; 3) XXIV-10; 4) XXIV-11; 5) XXIV-12; 6) XXIV-13; 7) XXIV-15; 8) XVIIIg-1; 9) XVIIIg-4; 10) XVIIIg-03; 11) FG 41-1; 12) FG 41-2; 13) FG 41-3; 14) FG 41-4



XXXVII-1

Fig. 5 Kantharos XXXVII-01 photo and drawing (first published by Walter – Felten 1981, 175 cat. 442; pl. 121, 442)

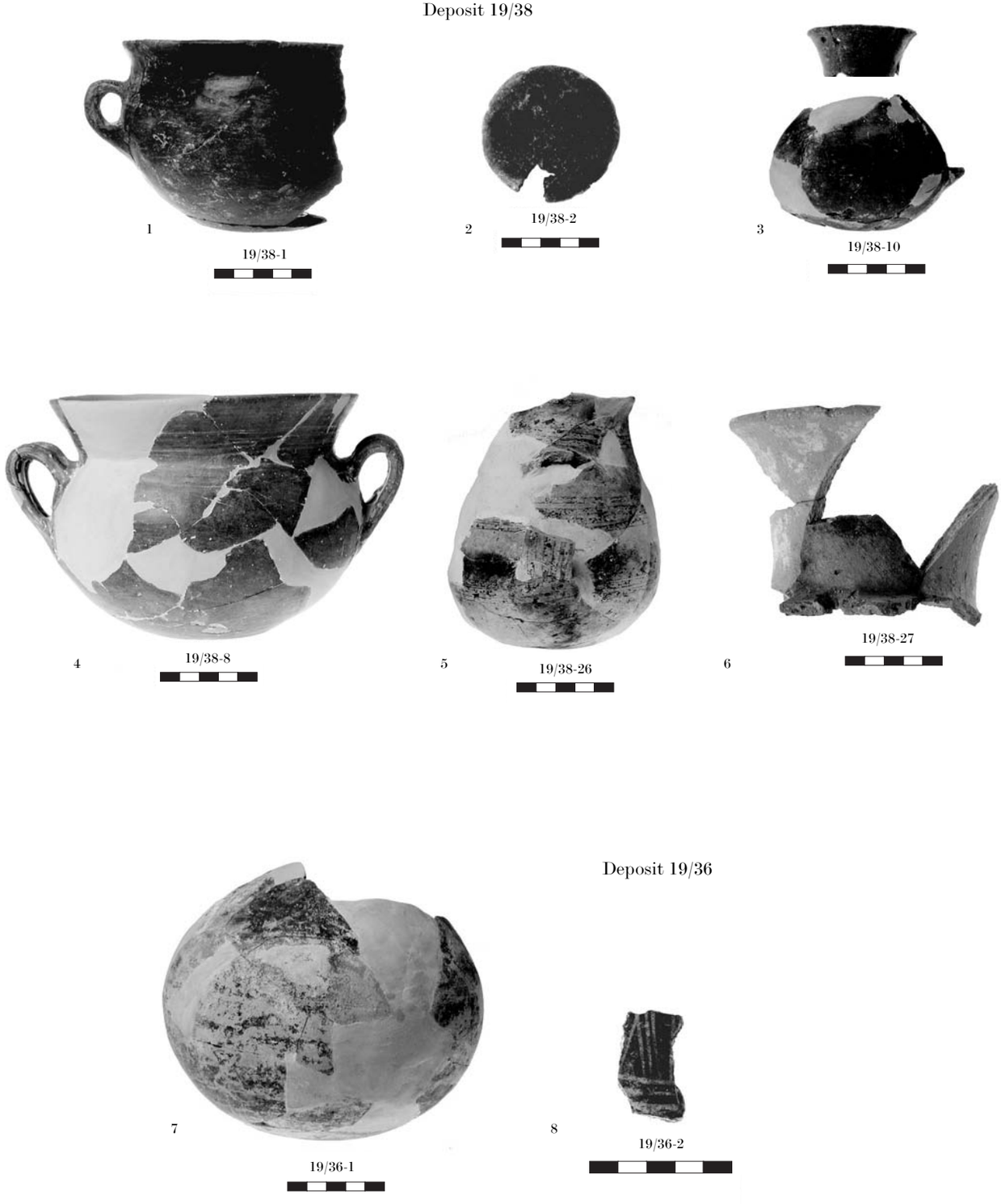


Fig. 6 Pottery from contexts 19/38 and 19/36 (photos): 1) 19/38-01; 2) 19/38-02; 3) 19/38-10; 4) 19/38-06; 5) 19/38-26; 6) 19/38-27; 7) 19/36-01; 8) 19/36-02

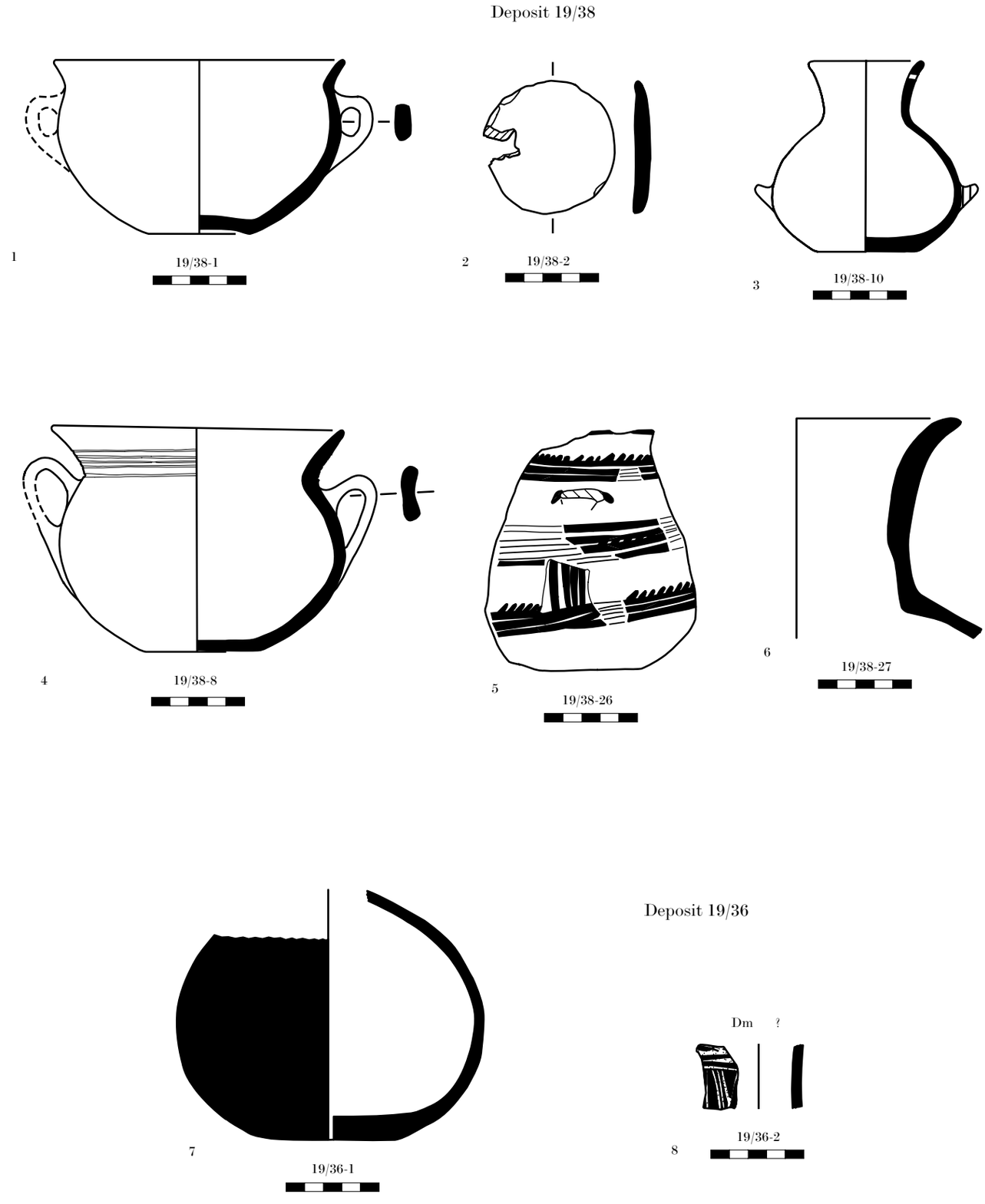


Fig. 7 Pottery from contexts 19/38 and 19/36 (drawings): 1) 19/38-01; 2) 19/38-02; 3) 19/38-10; 4) 19/38-06; 5) 19/38-26; 6) 19/38-27; 7) 19/36-01; 8) 19/36-02

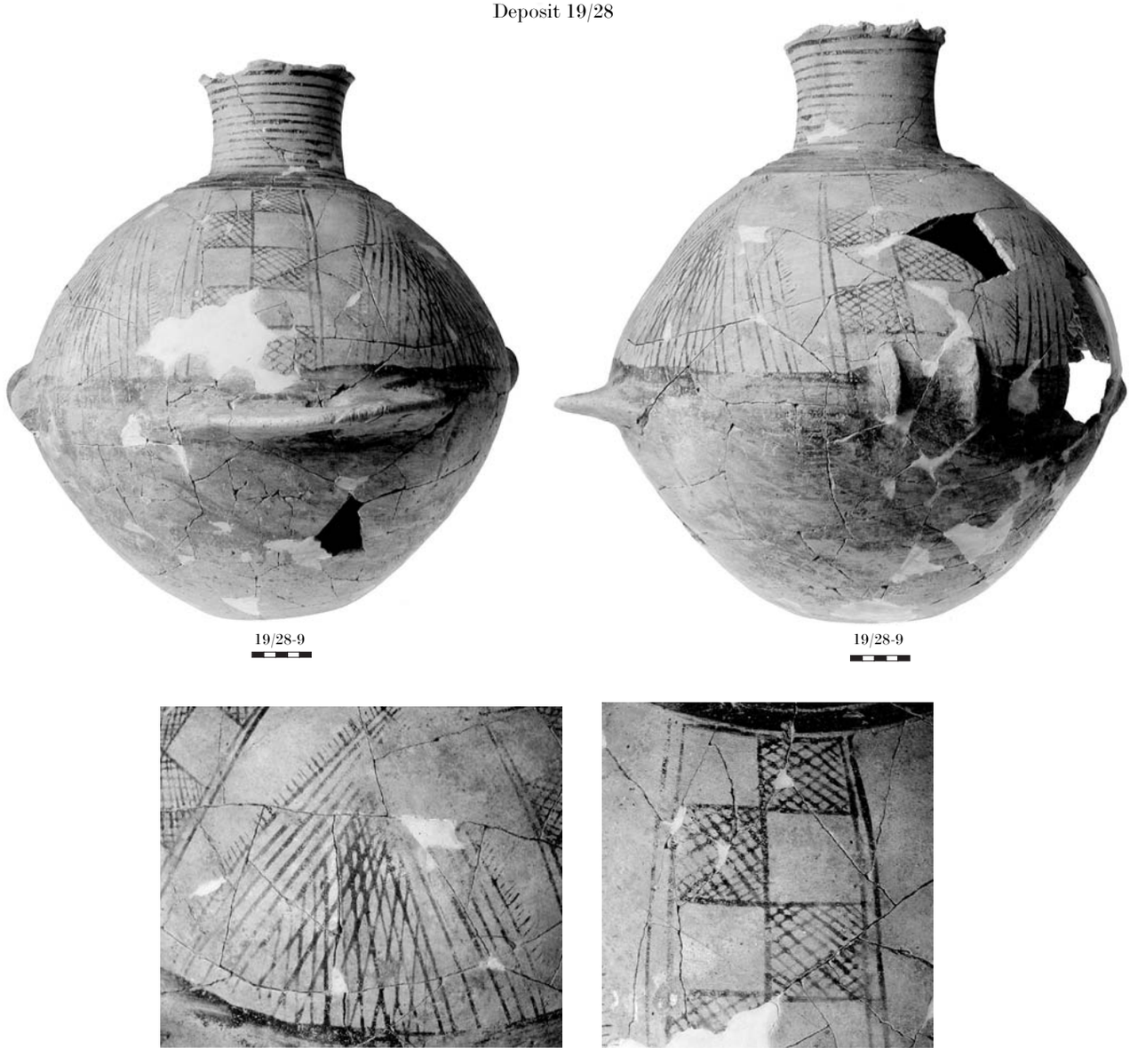


Fig. 8 Pottery from context 19/28 (19/28-9)



Fig. 9 Pottery from context 19/28 (photos): 1) 19/28-8; 2) 19/28-10; 3) 19/28-44; 4) 19/28-14; 5) 19/28-18; 6) 19/28-35; 7) 19/28-15

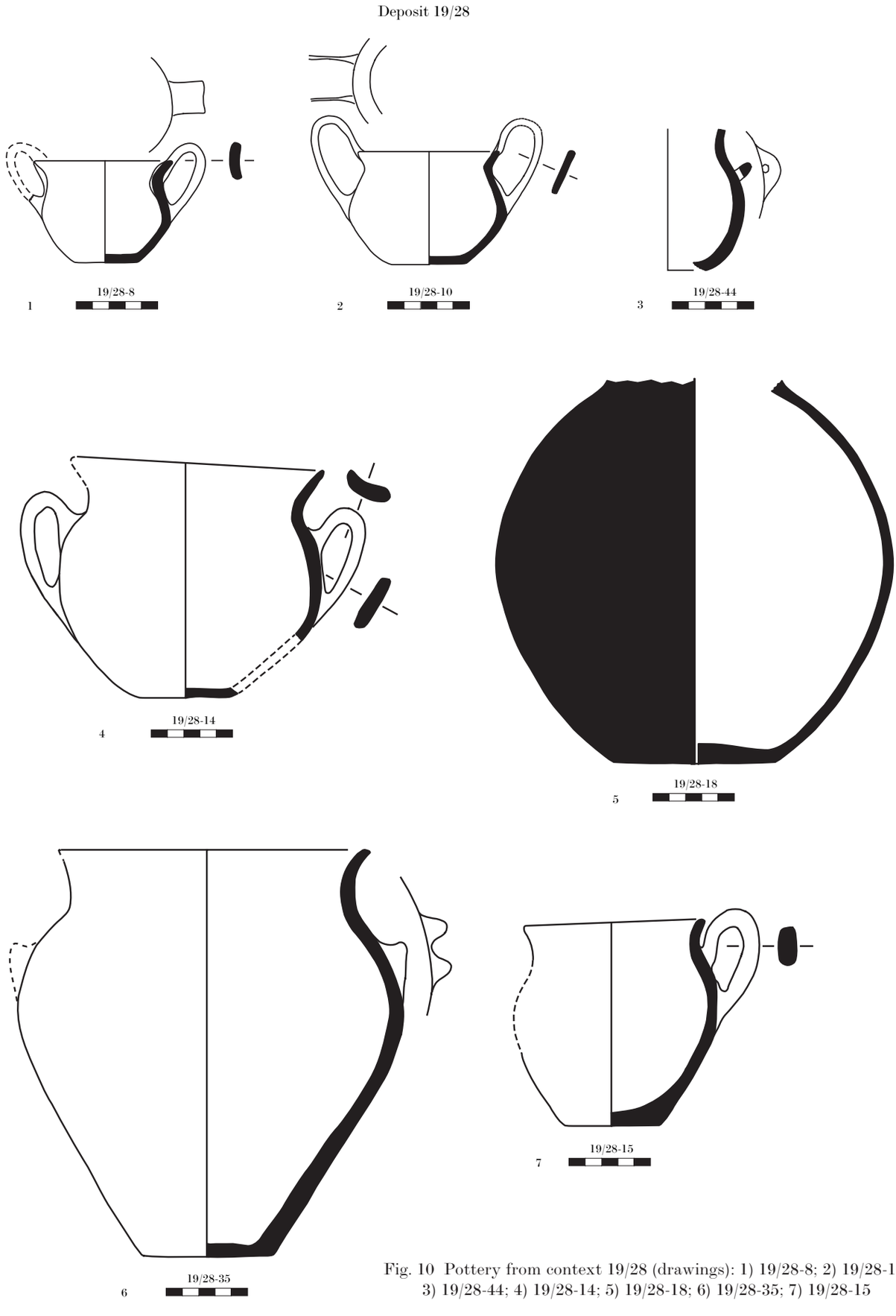


Fig. 10 Pottery from context 19/28 (drawings): 1) 19/28-8; 2) 19/28-10; 3) 19/28-44; 4) 19/28-14; 5) 19/28-18; 6) 19/28-35; 7) 19/28-15

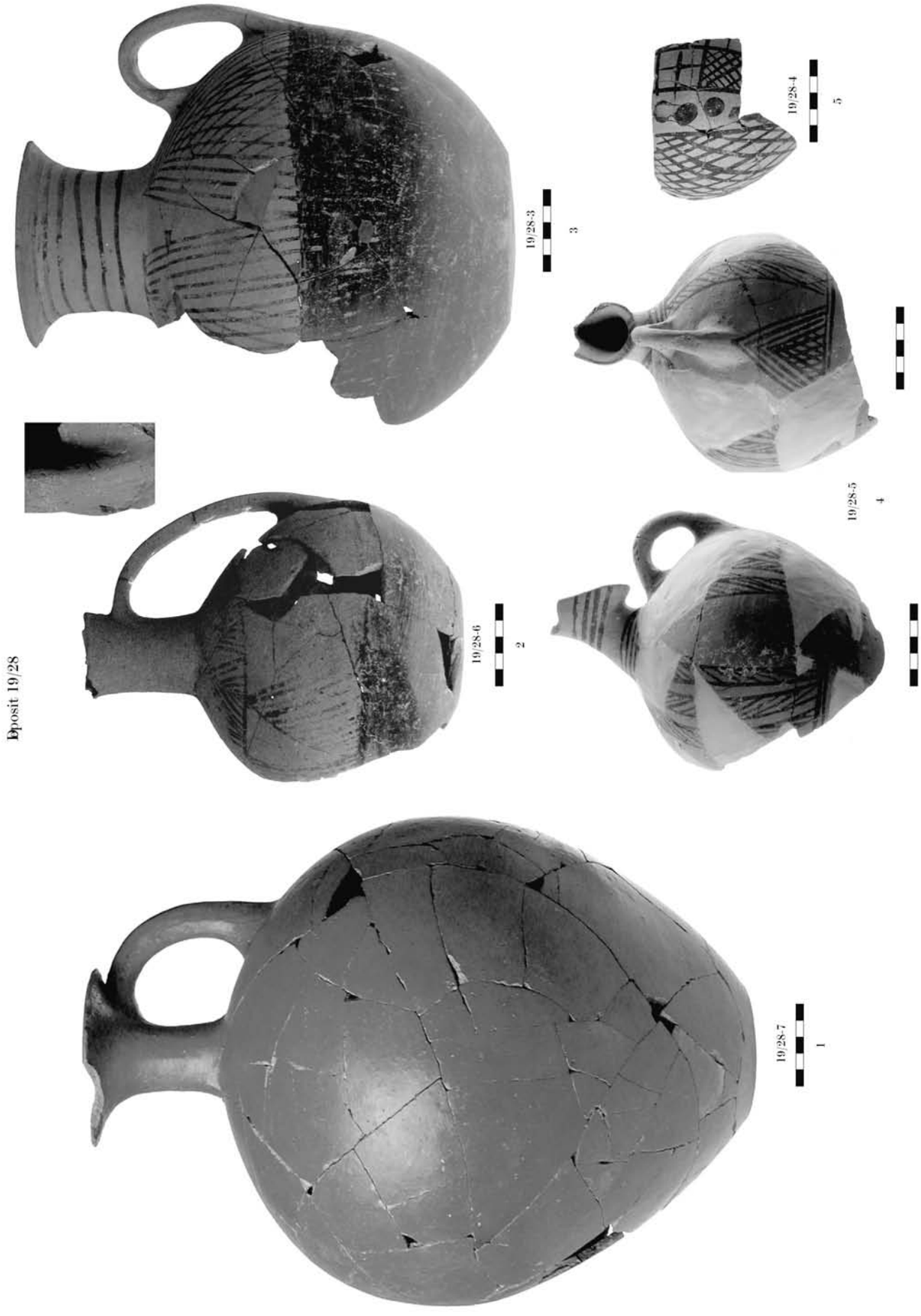


Fig. 11 Pottery from context 19/28 (photos): 1) 19/28-07 ; 2) 19/28-06; 3) 19/28-03; 4) 19/28-05; 5) 19/28-04

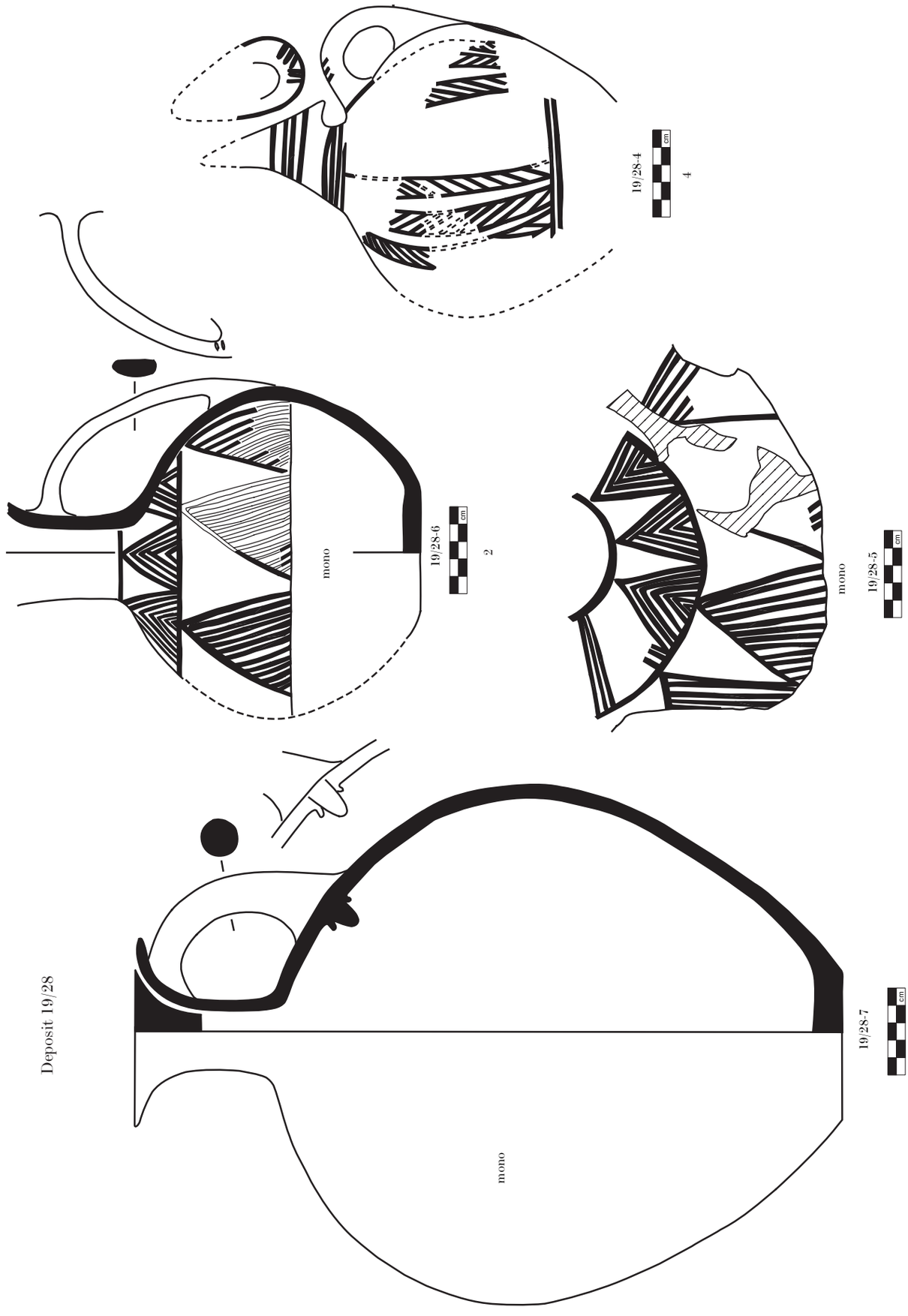


Fig. 12 Pottery from context 19/28 (drawings): 1) 19/28-07 ; 2) 19/28-06; 3) 19/28-05; 4) 19/28-04

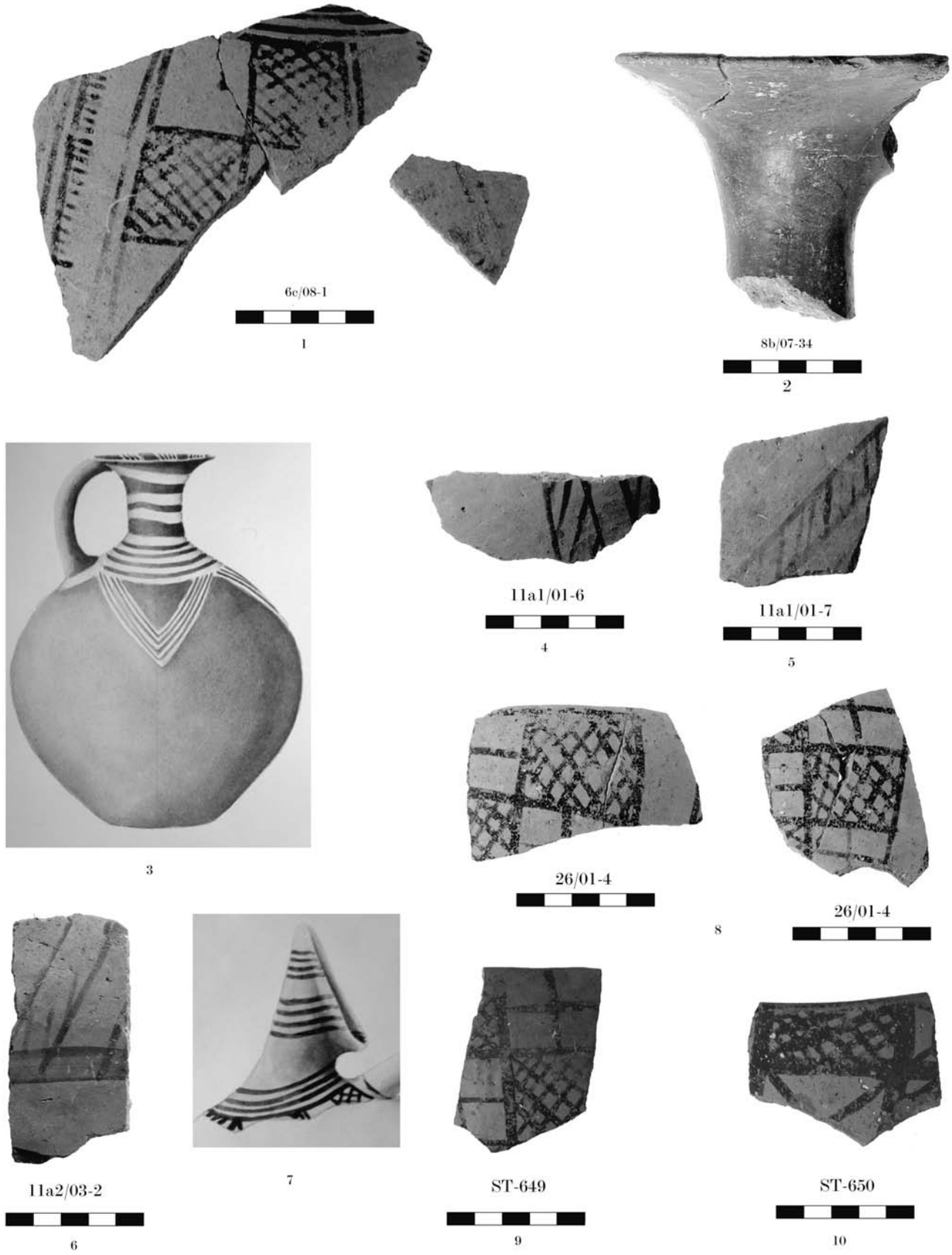


Fig. 13 Comparisons to context 19/28: 1) 6c/08-1; 2) 8b/07-34; 3) Alte Grabung (Photo DAI Athen, Neg. Ägina 504, Archiv Institut Klassische Archäologie Univ. Salzburg); 4) 11a1/01-06; 5) 11a1/01-07; 6) 11a2/03-2; 7) Alte Grabung (Photo DAI Athen, Neg. Ägina 483, Archiv Institut Klassische Archäologie Univ. Salzburg); 8) 26/01-04; 9) ST-649; 10) ST-650

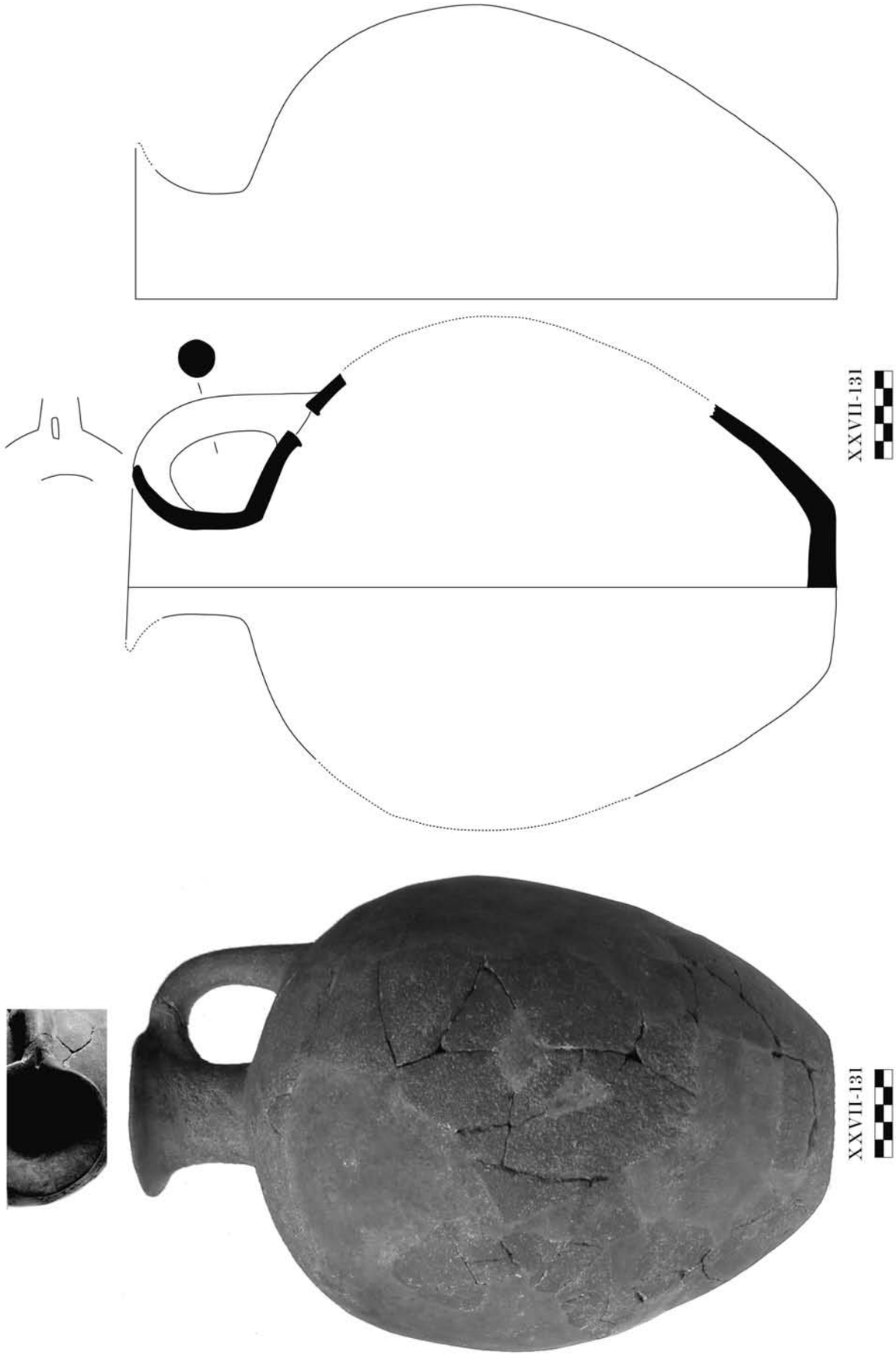


Fig. 14 XXVII-131 (photo and drawing)

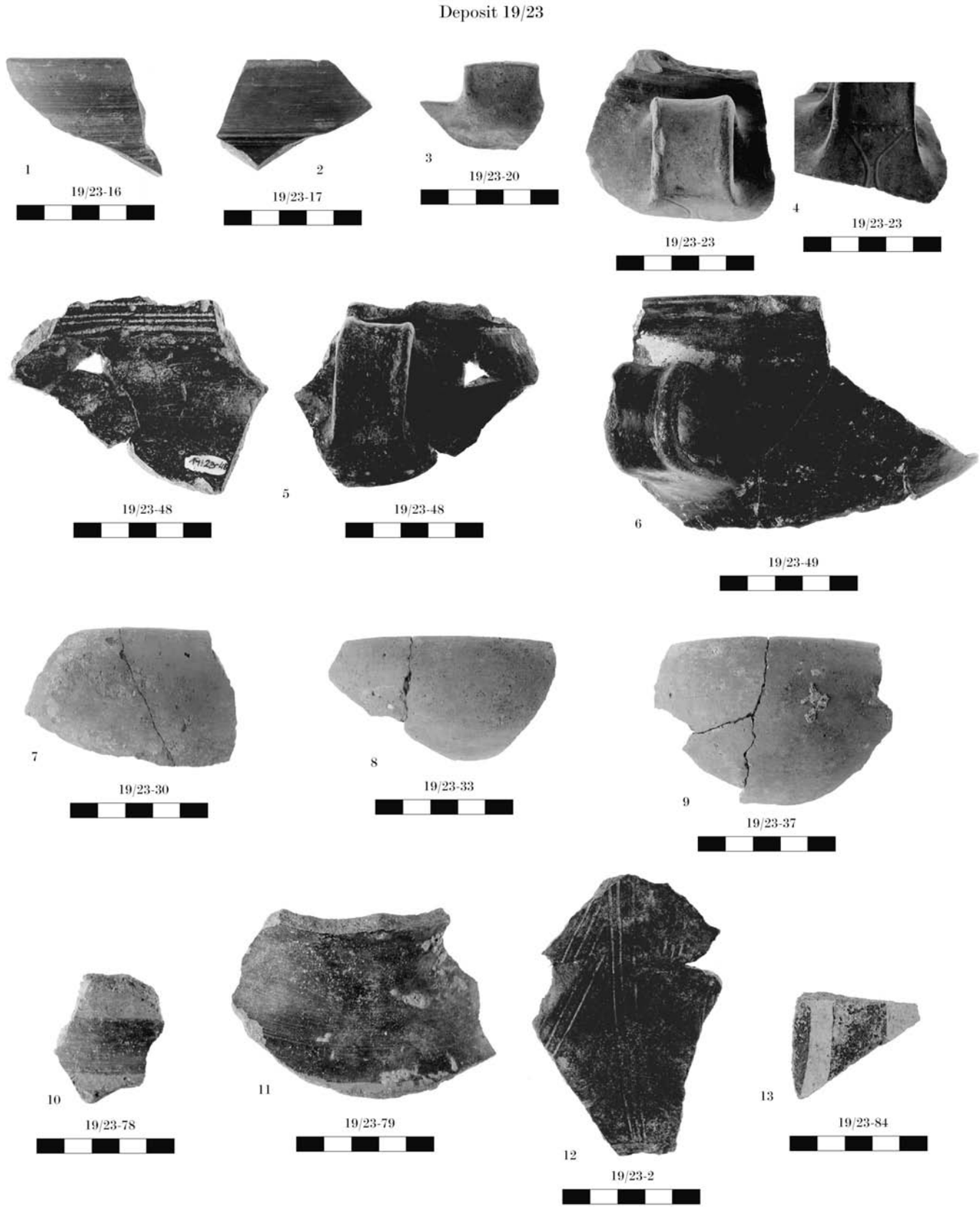


Fig. 15 Pottery from context 19/23 (photos): 1) 19/23-16; 2) 19/23-17; 3) 19/23-20; 4) 19/23-23; 5) 19/23-48; 6) 19/23-49; 7) 19/23-30; 8) 19/23-33; 9) 19/23-37; 10) 19/23-78; 11) 19/23-79; 12) 19/23-03; 13) 19/23-84

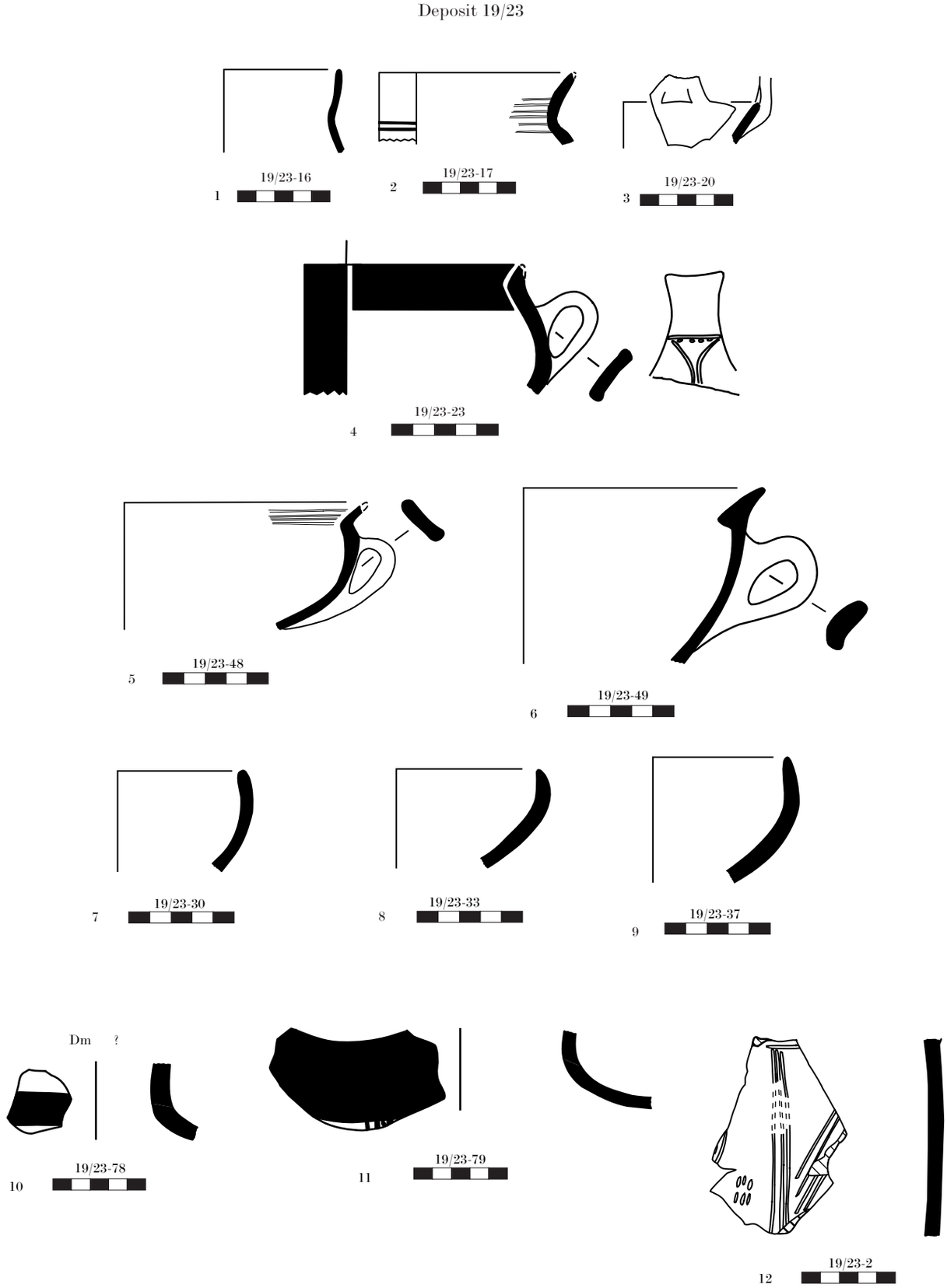


Fig. 16 Pottery from context 19/23 (drawings): 1) 19/23-16; 2) 19/23-17; 3) 19/23-20; 4) 19/23-23; 5) 19/23-48; 6) 19/23-49; 7) 19/23-30; 8) 19/23-33; 9) 19/23-37; 10) 19/23-78; 11) 19/23-79; 12) 19/23-03; 13) 19/23-84

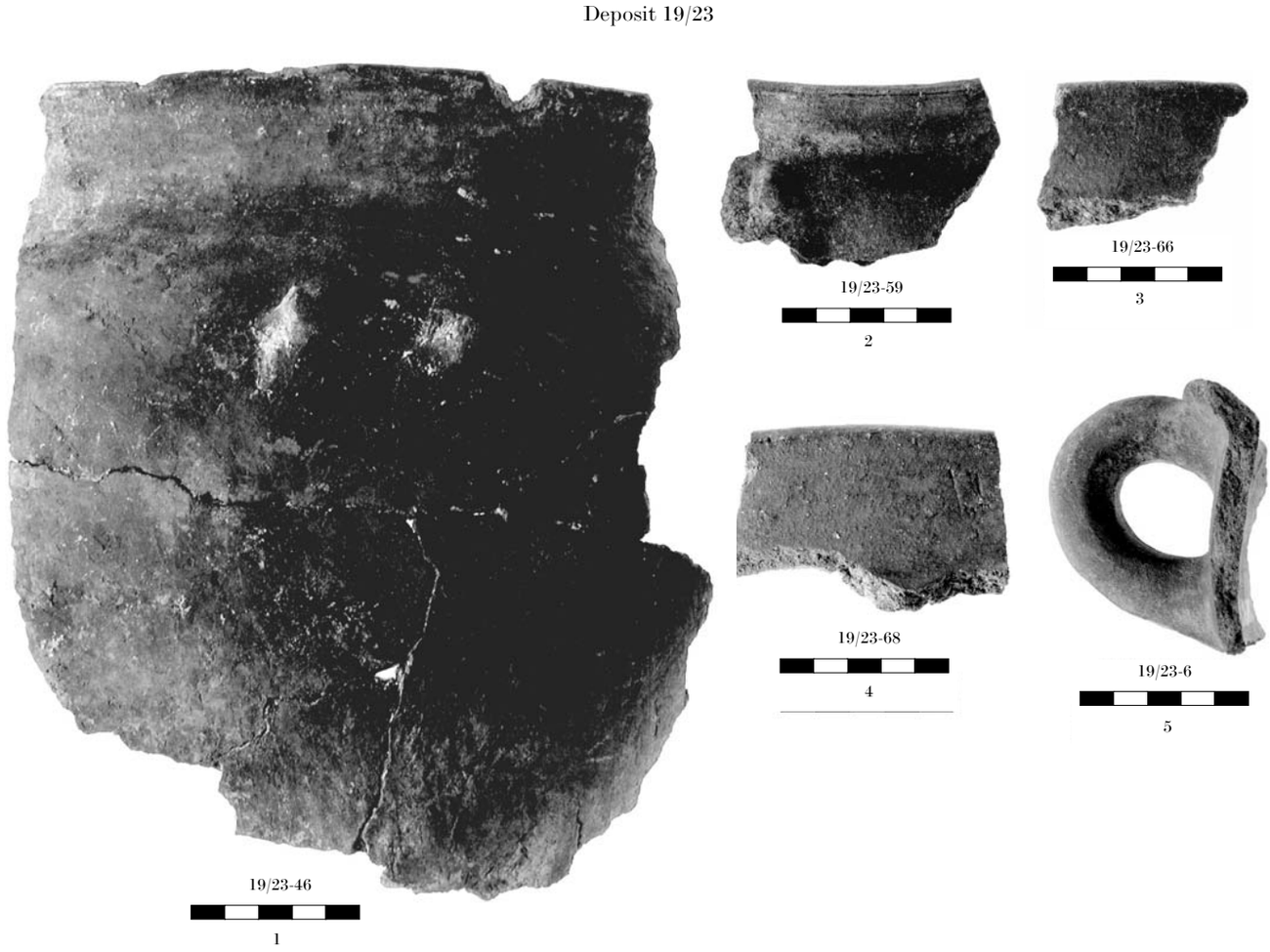


Fig. 17 Pottery from context 19/23 (photos): 1) 19/23-46; 2) 19/23-59; 3) 19/23-66; 4) 19/23-68; 5) 19/23-06

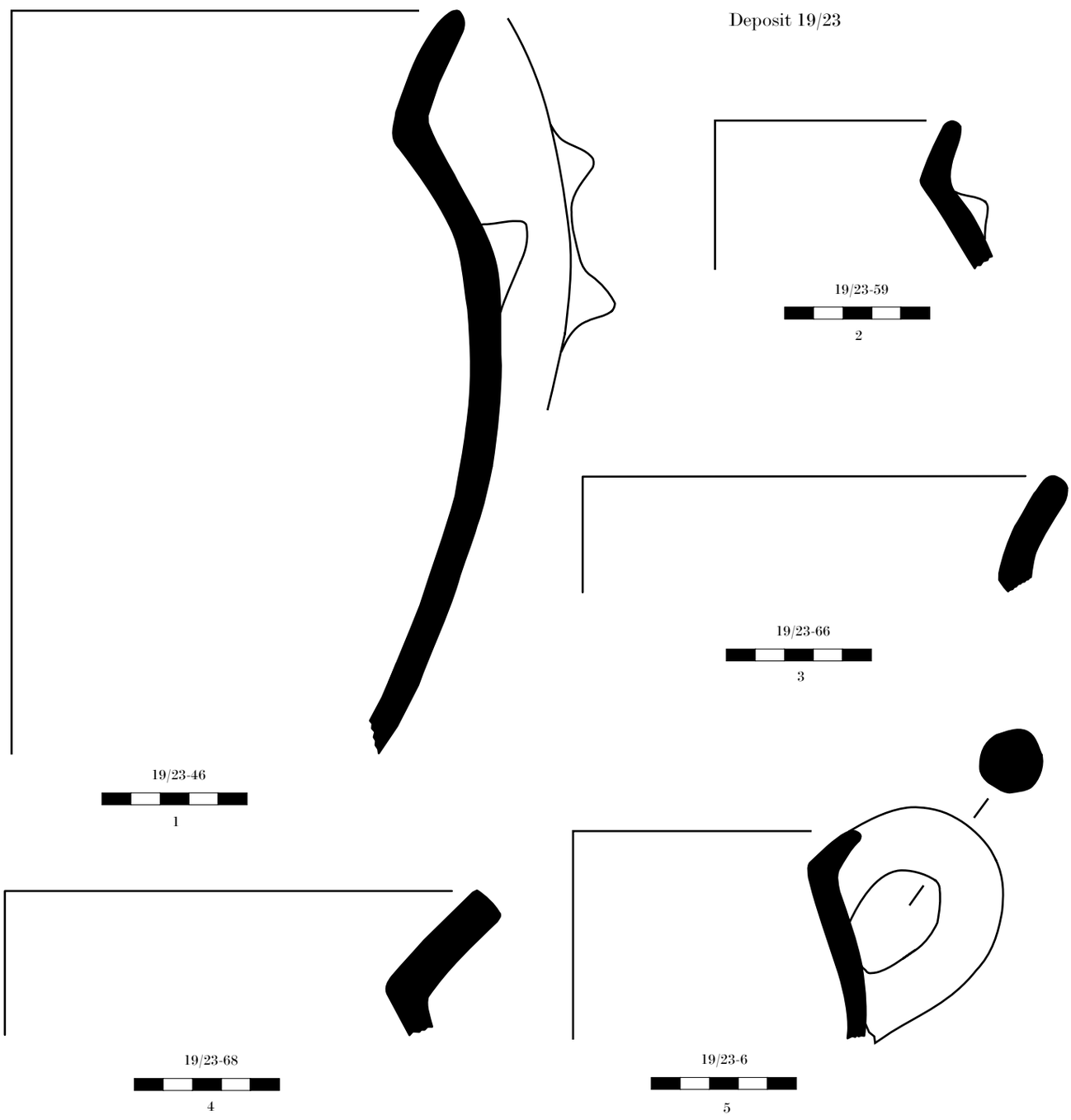


Fig. 18 Pottery from context 19/23 (drawings): 1) 19/23-46; 2) 19/23-59; 3) 19/23-66; 4) 19/23-68; 5) 19/23-06

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New Perspectives on Troia VI Chronology

Peter Pavúk[*]

Over the past 100 years Troia has become a key prehistoric reference site. However, whereas there has always been a steady interest in all aspects of the Early Bronze Age of the site, the material from the second millennium has remained largely unstudied. Research has concentrated only on the Mycenaean painted pottery and 95 % of the other material evidence has largely been omitted. Therefore, the focus of this article will be on Middle and Late Bronze Age Trojan chronology and its wider implications. The new phasing of Early and Middle Troia VI will first be described after which the overall dating of Troia VI will be discussed.

Since the new results are still somewhat preliminary, only a shorter version of the paper delivered at the SCIEM 2000 Conference is presented here. More in-depth discussion of the related topics and the new evidence will be included in the final publication series on the Troia excavations.[1]

The new phasing of Early and Middle Troia VI

Blegen divided Troia VI into 8 phases labelled by the lower case letters a–h and grouped them somewhat loosely into 3 sub-periods: VI-Early, Middle and Late.[2] His subdivisions are mainly architectural so I often wondered whether they could also be defined and distinguished in ceramic terms. The recent excavations have made such an investigation possible and some of the preliminary results are presented here.

Four ceramic phases of Troia VI have been distinguished. The hallmark of the first ceramic phase is the initial appearance of the so-called Anatolian Grey Ware, a.k.a. Grey Minyan. There is now increasing agreement among specialists that the Greek and Anatolian grey wares should be kept separate and treated individually.[3] However, despite the proclaimed chronological and typological differences between the two, almost all the new shapes in which Anatolian Grey Ware first occurs at Troia come from the Aegean. They comprise the Lianokladhi goblet, Pteleon Goblet, semiglobular cup and kantharos.[4] It should be noted, though, that this applies only to Troia and to some other West Anatolian coastal sites; further inland there are no Aegean shapes at all.[5] The only local Grey Ware shape at Troia is the so-called bead-rim bowl. The rest of the shapes and wares continue in some way from Troia V; indeed, this phase could almost be called the final Troia V phase. However the hallmarks of Troia V, Red Cross Bowls and volute handles, are already absent. This is Blegen's architectural phase VIa and it can be described as a transitional phase between Troia V and VI.

The second ceramic phase witnesses the introduction of a whole new set of shapes. The Aegean goblet-like shapes are gone, kantharoi become more popular and occur in two subtypes and there are new types of carinated cups. This is also the beginning of the era of the typical Northwest Anatolian ribbed two-handled bowls. Proportions of Anatolian Grey Ware now reach ca. 30% and for the first time closed shapes (jugs) are also produced in this ware. A different kind of semiglobular cups, as well

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[1] The sudden death of the director of the Troia excavations, Prof. Dr. Manfred O. Korfmann, has shattered all members of the Troia Project, as well as many other colleagues. I would like to acknowledge my deep gratitude for all his help and encouragement over the years. His omnipresence will be sorely missed.
Relevant summaries of the new results from the ongoing excavations at Troia VI and VIIa are presented in Korfmann 1996; 2001 and in Becks 2002. The article has been completed with the aid of the Slovak Scientific Grant Agency VEGA, grants 1/9092/02 and 1/1211/04.

[2] Blegen *et al.* 1953, 11–20.

[3] Pavúk 2002a, Note 4. For a good overview of the problem see Schachner's contribution in Bayne 1963 (2000), 300–301, 305–306. The first one to use the expression Anatolian Grey ware seems to be Karagheorgis (quoted in Allen 1994, Note 4), but the main proponent of the name has more recently been S. H. Allen, who has suggested the even more restricted name North-West Anatolian Grey Ware (Allen 1990, 1994).

[4] For a more in depth discussion of these shapes see Pavúk, 2002a; 2002b.

[5] Survey material from North Western Anatolia was presented in French 1967 and 1969.

as some krater-like shapes, appear in Burnished Plain Ware and are often decorated with zig-zag patterns in added white paint. From what we know until now, they seem to be imported from Samothrace.[6] There is no Mycenaean pottery in this phase, but there are some Mattpainted sherds from Blegen's excavations.[7] This second phase spans Blegen's architectural phases VIb and VIc.

The third ceramic phase covers the whole of Blegen's VI-Middle and the beginning of his VI-Late. The shapes of the drinking cups change once again. Especially popular become now carinated cups and show a wide variety of sub-types. The ribbed bowls probably continue but a number of further types of two-handled carinated bowls come into existence. Tan Ware, a light-faced companion to Anatolian Grey Ware, appears for the first time; at first quite modestly with just two or three shapes, such as plates, small round handleless bowls and jugs. Much later, during Troia VIIa, it becomes the dominant Trojan ware.[8] The third ceramic phase corresponds to Blegen's architectural phases VId, VIe and VIf and from now on Mycenaean pottery occurs. There is also imported mattpainted pottery of an unknown origin in this phase; for the time being we can only say that it is different from all the variants present on the Greek mainland.

Dating Troia IV and V

To date the beginning of Troia VI, one has to go back and establish absolute and relative dates for Troia IV and V. The majority of previous chronological schemes have still placed Troia V within the 3rd millennium, although the evidence speaks clearly for a date at the beginning of the 2nd millennium.[9] There is, for example, EH III pattern painted pottery from Blegen's excavations in Troia IV and an imported winged jar of a Trojan type datable to Troia III–IV was found in Lerna IV, which also dates to EH III.[10] Moreover, S. Manning has shown, that the end of Troia IV probably overlaps with MM Ia indicating a date in the 2nd millennium.[11] Therefore, Troia V cannot be older than 2000 BC, but apart from a few C^{14} dates and an imported Minoan jug, there is not much evidence from Troia for its date. The C^{14} dates come from possible wooden coffins in two Troia V graves from the Lower Town (Area D20); their combined range is ca. 1850–1770 B.C.[12] The context and the dating of the Minoan jug is unfortunately less clear, than has been claimed in the preliminary reports. The jug comes from a cist grave, which dates either to the end of Troia V or to the beginning of Troia VI (and not to Middle Troia V). It belongs to the so called Creamy-bordered Style and in Cretan terms dates to early MM IIIA according to J.A. McGilliveray.[13] Some scholars would prefer to date the jug to MM IIB,[14] but in any case the date is somewhere around 1750 B.C. on high Aegean chronology. This means that Troia VI should start not much earlier.

Dating the first Troia VI phase

The majority of the new shapes in Anatolian Grey Ware in the first ceramic phase of Troia VI can be dated to MH III, but some point to MH II and yet others to LH I. The Lianokladhi goblet is a good yard stick, because its typological development on the Greek mainland is known; the Trojan examples are similar to its later variants.[15] The Pteleon goblet is not so well understood in Greece, but the Trojan semiglobular cup has LH I parallels on the Greek mainland.[16] There are some ceramic imports in this phase but no firm date has been established for them yet.

There are also some C^{14} dates from burnt grain and charcoal found within an oval house in area K8, but their combined range around 1900 B.C. is too early[17] The oven has been cut into a burned Troy IV layer and since the dates are so high, it is possible that the grains were re-deposited and are in fact also Troy IV. However, in 2001 two adult graves belonging clearly to the end of the first Troia VI ceramic phase

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[6] Pavúk 2005, 271–272, pl. LXVa, with further references. A good example has also been published by D. Easton (1993, 65 fig. 25). Easton suggested Keos as a place of origin for this type of pottery, but new excavations showed this unlikely.

[7] Blegen *et al.* 1953, fig. 382: 1–3 (Mattpainted VI b/c). However, the majority of the mattpainted sherds at Troia are from the next ceramic phase.

[8] Tan Ware is the topic of a dissertation in progress by W. Rigter.

[9] Korfmann, Krommer 1993, Abb. 1.

[10] Blegen *et al.* 1951, fig. 170:10, 185: EH-704, Rutter 1995, 434, 463.

[11] Manning 1995, 103 See also Maran 1998, 416–426.

[12] Korfmann, Krommer, Jablonka 2003 fig. 5.

[13] Korfmann 1997, 32–38, Abb. 31.

[14] Personal communication from Sinclair Hood, Gisela Walberg and Aleydis van de Moortel.

[15] Pavúk 2002a, 47–48, fig. 9.

[16] Pavúk 2002a, 48–51, fig. 10: 38 (Pteleon goblet), fig. 15: 52–53 (Semiglobular cup).

[17] Korfmann, Krommer 1993, 160.

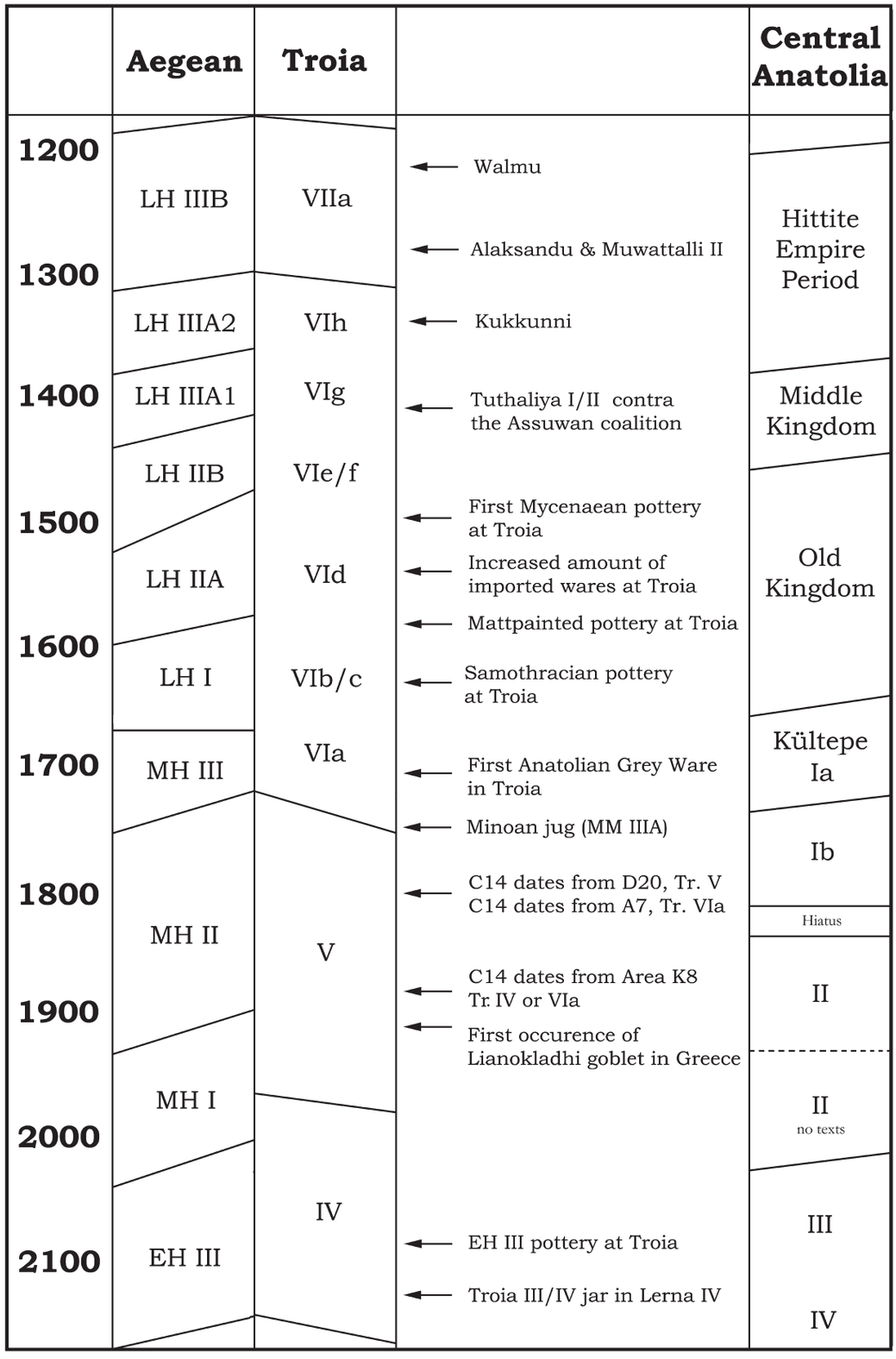


Fig. 1 Chronological table. For the Aegean the High chronology is used (Rutter 1993, Tab. 2; Shelmerdine 1997, Tab. 1). For Troia the table relies on the results of Maran (1998), Mountjoy (1997; 1999) and Pavúk (this volume). For Central Anatolia the Middle chronology is used. The Hittite dates are based on McMahon (1989, 64) and the Assyrian dates on Veenhof (2000)

were found in area A7.[18] The C^{14} dates (this time on the human bones) again turned out to be too early for the current chronological scheme based on pottery. Material for more C^{14} dates was collected in summer 2003 and the results will be presented in a future volume of *Studia Troica*.

On the basis only of the Grey Ware one can at most speculate that Troia VI starts in MH II. It cannot be earlier, because of the presence of the Lianokladhi goblet, a shape which does not occur in MH I on the Greek mainland. A starting date for Troia VI in the 19th century would for example fit the MH II dating of similar oval houses in Liman Tepe near Izmir.[19] However, there still remains the mid-18th century Minoan jug, which should be either late Troia V or earliest Troia VI and the 19th century Troia V graves in D20. Moreover, if a 19th century date for the beginning of Troia VI is accepted, there would remain barely a 100 years for Troia V, which itself has 3–4 architectural phases! Despite these uncertainties, it still seems most likely that Troia VI started somewhere on the turn of MH II and MH III periods on the Greek mainland, that is somewhere around 1750 B.C. on the high Aegean chronology.

Dating of the other Troia VI phases

The second ceramic phase is less contradictory but also has fewer fixed points for dating it. The Anatolian ribbed bowls mentioned above show a wide variety of subtypes, but since Troia is the only well stratified and published site in NW Anatolia, we cannot date them via analogies from other sites. The later variant of the semiglobular cups, the shape of which recalls similar LH I–LH IIA cups on the Greek mainland, is more helpful. At Samothrace, they postdate the deposits with Minoan roundels of a possible MM III date.[20] Mycenaean pottery itself did not appear at Troia yet. As regards imports, Blegen found one sherd with creamy white paint, which he identified as Kamares Ware and dated by a somewhat circular argument to be pre-MM III.[21] Unfortunately, the published photograph does not help much in this respect and the context is also somewhat mixed.

Blegen's finds of Mattpainted pottery are likewise not very useful. They start in the second ceramic phase and continue into the third and possibly later. The majority come from the third ceramic phase, that is VI-Middle; some pieces are polychrome.[22] Despite Blegen's optimism they offer little help in dating, because nobody has inspected them since the 1950s and the published photographs are not very clear. R. Buck, in his seminal work on Mattpainted pottery, refers to them only in passing and describes them as Middle Helladic.[23] This is hard to believe, because the third ceramic phase at Troia most probably dates to LH IIA and LH IIB, a date derived from the associated Mycenaean pottery.[24] Moreover, during the past 10–15 years it has become clear that there is such a wide variety of Mattpainted wares on the Greek mainland that a new inspection of the Trojan examples is absolutely necessary in order to identify and date them.

The fourth ceramic phase corresponds largely to Blegens VI-Late, more specifically to his phases VIg and VIh. Their dating depends likewise largely on the dates for the associated Mycenaean pottery. The Blegen finds were recently re-assessed by P. Mountjoy and the results are as follows: Troia VIg = LH IIIA1 and Troia VIh = LH IIIA2.[25]

A massive program of C14 dating has also been started at Troia, but the second half of the second millennium has not yet been evaluated properly. The results so far suggest that the dates for Late Troia VI and for Troia VIIa and VIIb do not contradict the generally proposed absolute dates derived from the Mycenaean pottery.[26] Unfortunately they do not refine the Mycenaean dates, nor can they implement a decision in favour of the high or the traditional Aegean chronology, because the samples are mostly charcoal and, at a site with such a complicated stratigraphy as Troia, the risk that some of them may be out of original context cannot be ruled out.

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[18] Korfmann 2002, 18 Abb. 16. For further discussion see also Pavúk 2007.

[19] Erkanal, Günel 1995, 275 Şek. 4 (Liman tepe). For the Trojan VIa oval house in the Area K8 cf. Easton, Weninger 1993, fig. 15 and passim.

[20] The large bibliography on the site of Mikro Vouni and the latest results are summarized in Matsas 2004. I would like to thank Dr. Matsas for showing me his finds and discussing them with me on several occasions.

[21] Blegen *et al.* 1953, fig. 360:11. The identification is not certain; it is not completely excluded, that the sherd is not Minoan at all but simply one of the coarser Samothracian imports.

[22] Blegen *et al.* 1953, 38, fig. 382. The Ware is discussed also in Pavúk 2005, 272, pl. LXVb.

[23] Buck 1964, 238.

[24] Mountjoy 1997, 275–277, 286–287.

[25] Mountjoy 1997, 292; 1999, 256–258.

[26] Korfmann, Krommer, Jablonka 2003, 50, fig. 6.

Correlating the Trojan sequence with central Anatolia is less easy as there are very few datable material connections. The only way this can be achieved is to turn to absolute dates. High Aegean chronology has been used in this article but the decision between the use of "high" and "traditional or low" chronology is an arbitrary one in this case. One should, however, bear in mind, that when correlating absolute dates between the Aegean and Central Anatolia, the low Aegean chronology should be compared with the low Anatolian chronology and the high Aegean chronology with the middle Anatolian chronology.[27]

Conclusions

To conclude, Troia IV is contemporary with EH III and overlaps with MMIa. Troia V covers most of the Aegean MBA, that is MH I and MH II. The first ceramic phase of Troia VI (Blegen Phase VIa) corresponds roughly to MH III, the second phase (Blegen VI b/c) to LH I and the third phase (Blegen's Troia VId, VIe and VIf) to LH IIA and LH IIB. Finally, the fourth ceramic phase of Troia VI (Blegen's VIg and VIh) dates to LH IIIA1 and LH IIIA2.

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[27] For the basics of the low Aegean Chronology see Warren and Hankey 1989 with further references. For recent overview of both low and middle Anatolian chronology see McMahon 1989, table on page 64. The middle Anatolian Chronology is also favoured by the recent dendrochronological investigations, which in turn correlate nicely with the high Aegean chronology based on C14 dating (Manning *et al.* 2001). I would like to thank Mirko Novák for a useful discussion on this issue.

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The Amenhotep III 'Plaques' from Mycenae: Comparison, Contrast and a Question of Chronology

Jacke Phillips[*]

Abstract

In 1886 and 1890, Ch. Tsountas found several faience 'plaque' fragments inscribed with the names and titles of Amenhotep III in the citadel of Mycenae. Further fragments subsequently were recovered by G.E. Mylonas and W.D. Taylour in the 1960s. Another recently has come to light in the excavation storerooms at Mycenae. Direct comparanda for these 'plaques' are not forthcoming, even in Egypt, and they have been the source of many theories regarding the relationship between Egypt and Mycenae, especially between Amenhotep III and those who ruled Mycenae during his reign. But the 'plaques' themselves have not been directly and intimately studied until now. The author's study indicates several key features of the 'plaques,' not recognised or considered before, that allow their place in the complex relationship between Mycenae and Egypt to be more specifically understood and for previous contentions to be discarded.

In 1886 and 1890, Ch. Tsountas found several glazed faience 'plaque' fragments inscribed with the names and titles of Amenhotep III within the citadel walls at Mycenae, at two separate locations. The one fragment from 1886 was recovered, it seems, in what is now known as 'Tsountas' House' in the Cult Centre, whilst the six fragments from 1890 were generally recovered north-east of the Lion Gate, in the area of the North Quarter that includes House N (Fig. 1).[1] Two of the 1890 fragments joined, and together provided three edges of a single 'plaque.' Tsountas (1891, pl. 3.3–4) published drawings of one face of only two 1890 fragments, one being the joined pair. The others preserved little of any inscription, one (Tsountas 5[2]) being entirely without text on either face. Little further information was made available, but Sewell (1904, figs. 1–2) published photographs of the same side of both published 'plaques.' Fimmen (1921, fig. 171) published these photographs again, together with *both* faces of the 1886 fragment for the first time.

Direct comparanda for the 'plaques' have never been forthcoming, even in Egypt, and they have been the ultimate basis for numerous theories concerning the relationship between Egypt and Mycenae, especially between Amenhotep III and those who ruled Mycenae during his reign *c.* 1388–1350 BC. (LH IIIA1 and early 2 in Mycenaean terms). Sewell (1904) called them 'tiles' and suggested that the "Greek king actually ornamented his own residence with a quantity of Egyptian tile-work," before Tsountas informed him that he believed "they have no connection with the palace at all." Fimmen (1921, 175) called them "*Platten*" and for the first time distinguished not only the two separate find areas and museum numbers, but also their different thicknesses and the point that the 1886 fragment inscription need not necessarily be of Amenhotep III – which is true, as only two Tsountas fragments (Tsountas 2 and 3) undoubtedly identify Amenhotep III himself – before suggesting Smenkhare, Akhenaten's immediate successor, was the pharaoh named on that piece instead.

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[1] No exact record exists of where he excavated, although certain areas are known to have been excavated in particular years, and some objects were recorded by find location and year of excavation. Perusal of the original National Museum register in Athens also is helpful in this respect. Nothing is certain, however. Fig. 1 is a very general indication of Tsountas' excavation areas in 1886 and 1890, together with the more precisely recorded findspots of 'plaque' fragments recovered by later excavators. Iakovides 2003a, 118 fig. 1 indicates the full extent of Tsountas' activities within the citadel. My thanks to Drs. Elizabeth French and Kim Shelton for their help in placing the 'plaques' in their respective contexts as much as possible.

[2] The 'plaque' fragments are assigned specific identifications for the purposes of this article, consisting of the excavator's name and a number. These are identified in the caption to Fig. 3. Note that, for the museum catalogue numbers given in parentheses, 'NMA' refers to the accession number of objects held in the National Archaeological Museum in Athens, and 'MycM BE' refers to the accession number of objects held at the new Mycenae Museum, whilst 'MM' refers to the *display* number of the objects exhibited in the latter's galleries.

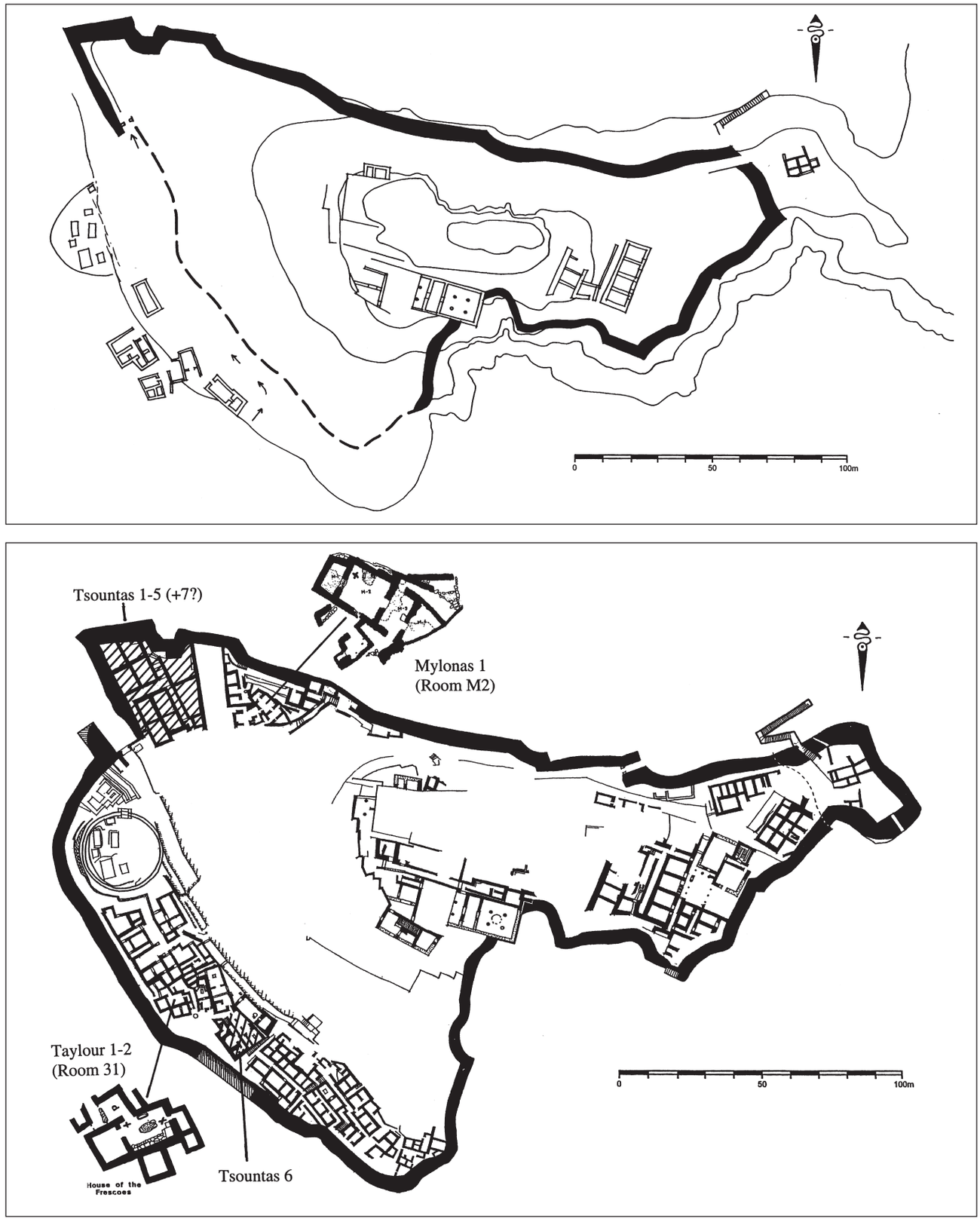


Fig. 1 Maps of the Mycenae citadel, showing findspots and find areas of 'plaques:' a) Structures of LH IIIA2-B1, during which the 'plaques' most likely arrived at Mycenae; b) Structures of 'palatial' Mycenae, LH IIIB2-B/C, within which the 'plaques' were (or, for the Tsountas material, most likely were) recovered. Plans courtesy of L. French, with additions by the author from original computerfiles for French 2002, figs. 16 and 19

This undoubtedly is the source of Porter and Moss's (1952, 402) identification of Smenkhare as the king named on the 'plaques' in Vol. VII of their *Topographical Biography*. They did not speculate about the circumstances behind any of their listed objects abroad, but these fragments and their discovery at Mycenae continued to be the source of much further speculation and fanciful reconstruction. Helck (1979, 97) envisioned them as having been set into a door of an 'Egyptian Room' in the palace at Mycenae. Cline (1990, 211) suggested they may have been "sent from Egypt ….as part of a formal contract, perhaps even via an official Egyptian delegation" that may have been recorded on the Kom el-Heitan list of Aegean place names. Hankey (1981, 45–46) thought they were foundation deposits for a temple or shrine. The last actually is the most plausible suggestion, but none have any archaeological basis whatsoever. Although they have been displayed (and thus accessible) for many years in the National Museum in Athens, good photographs and a general description of the findspots were published for the first time only in 2000.[3]

Further pieces were recovered in the 1960s, basically in the same two areas of the site. Two barely joining fragments were recovered in Building M by G.E. Mylonas in 1962 (Mylonas 1963b, pl. 76.β; Iakovides 1983:52) and W.D. Taylour found two separate pieces in 1968 in the 'Room with the Fresco' of the 'Cult Centre' (Taylour 1969:95–96) (Fig. 1). Another, from Mylonas's subsequent excavation and cleaning of the 'Cult Centre' area in the early 1970s, recently has come to light in the excavation storerooms at Mycenae. I shall not be discussing this last piece, which has just been published by Eric Cline and myself,[3a] except to say it exhibits some unique features that will underline some of my comments here. These recent finds have enabled less tentative reconstructions of the 'plaques' to be made, since the larger of Taylour's fragments preserved three edges and was nearly square, whilst the two Building M pieces could be reconstructed with the same text as Taylour's larger piece.

Helck (1979, 96 fig.) presented a reconstruction incorporating all Tsountas's fragments, somewhat embellished by a fragment that is not a 'plaque'

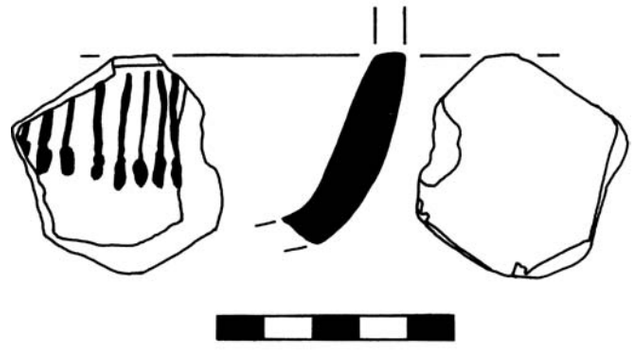


Fig. 2 Drawing of 'tray' NMA 2719

(Fig. 2),[4] as an inscription of Amenhotep III's *prenomen* and *nomen* titulary with epithets, the names each within a cartouche.[5] The basics of this reconstruction have been generally accepted, although the extra embellishment has been removed. The 'complete' text as reconstructed and generally accepted today is as follows:

nṯr nfr (*Nb-mꜣꜥt-Rꜥ*) *sꜣ Rꜥ* (*Jmn-ḥtp ḥkꜣ wꜣst*) *di ꜥnḫ*

'the good god, (Neb-maᶜat-Raᶜ), son of Raᶜ, (Amenhotep, ruler of Thebes), given life.'

The fragments were closely studied but not published by P. Kourachanes before his untimely death in 1988. The interpretation and reconstruction now displayed in the National Museum in Athens is the result of his work, and shows an amalgamation of the preserved texts.[6] Shortly afterwards, the Taylour finds were evaluated by Cline (1987; 1990), with reference to the Tsountas material.

None of the fragments was found in a context remotely contemporary with Amenhotep III's reign, a point that already has long been recognised. The vast majority simply were recovered in later fill levels – if any of the Tsountas fragments had a decent context, it is lost to us now – and only two in a 'good' context. The large Taylour fragment was recovered in a crumpled lead alabastron on the LH IIIB (middle) floor of the 'Room with the Fresco.'[7] Mylonas's 'Building M' excavations are not yet published in detail, but we are informed that the two barely joining fragments (Mylonas 1) were recovered in an LH IIIB (late)

[3] By L. Papazoglou-Manioudaki, in Karetsou *et al.* 2000, 252–254 #249.

[3a] Phillips and Cline 2005.

[4] NMA 2719; see Fig. 2. Its purpose remains obscure.

[5] See Gardiner 1957, 71–76, also 294 §378 for a detailed description of the full royal titulary.

[6] Papazoglou-Manioudaki 2000, 251 figs. lower.

[7] The smaller Taylour fragment (Taylour 2) was recovered in a fill context of the next level above, also dated within LH IIIB (middle). The lead alabastron is discussed in Tzedakis and Martlew 1999, 203 #202.

deposit next to an internal structure against the north wall of Room M2.[8] Both dates are over a century later than Amenhotep III's death *c.* 1350 BC. Any scenario regarding Amenhotep III's relationship with LH IIIA1/early 2 Mycenae and its ruler(s) is no more than hypothetical. The pieces in fact could have arrived at any time during or after his reign.[9]

The 'plaques' themselves have never been directly and intimately studied together before.[10] Let us evaluate some physical features that need to be considered in attempting to shine further light on these objects. New drawings by the author, taken directly from the original fragments, are here shown as Fig. 3. References to individual fragments are as indicated in its caption. The obverse face of each 'plaque' (as distinguished here) is illustrated on the left, and its reverse face on the right.

Quantity

The first point to be noted is that, with the exception of the two barely joining Mylonas fragments and the two joining Tsountas pieces, *no* individual fragment can be associated with any other. All are individual pieces, representing a minimum of 10 'plaques' (excluding the recent Mycenae Museum storeroom find, an 11th). All possible pairings can be rejected on one or more technical criteria. These are:

Fabric

Some fabrics are grittier than others, and visual inclusions appear to differ. Fabric colour, however, is immaterial, as is the colour and texture of their surface glaze, and more generally their present condition, all of which can be affected by their use, their context conditions, and their modern cleaning and handling.

Text overlap

Certain fragments duplicate the same portion(s) of text so, unless the titulary is repetitive (a unique circumstance if so), such fragments must be from different 'plaques.' Tsountas 1, 4, 6, and Mylonas 1 all duplicate portions of Taylour 1. Tsountas 2 duplicates a large portion of Tsountas 3. Tsountas 4 has the *nb* sign (V 30)[11] of Taylour 1 on one face, and the *di* sign (X 8) of Tsountas 3 on the other.

Hand (Palaeography)

Several different scribes have painted the texts on these fragments, although the writing itself is all good New Kingdom Egyptian orthography and palaeography.[12] The most obvious is the distinctly individual and very heavy hand seen *only* on Mylonas 1. Different hands seem to have painted the obverse and reverse text on several fragments, whilst the different shape and actual line application of several cartouches also seems to indicate different hands at work. Compare the sun disc (N 5) on Tsountas 1 with those on Mylonas 1 and Taylour 1, and those on the obverse with reverse of Tsountas 6, the duck (G 39) on either face of Tsountas 1 (especially the legs and feet), the flattened lower curves on most cartouches with that on Tsountas 3 obverse, and the painted line application on Taylour 1 and Tsountas 2 obverse. The upper cartouche curves of Tsountas 6 obverse and reverse are particularly distinct, as are the round and flattened sun signs (N 5) on this fragment. These are palaeographic differences more likely indicating different scribes at work, rather than variations of the same hand.

Orientation

The obverse text of all 'plaques' faces right. The reverse text of three fragments (Tsountas 2–3, Mylonas 1) also faces right, whilst four reverse texts (Tsountas 1, 4, 6, Taylour 1) unmistakably face left. The remainder are indeterminate as preserved. Also, most fragments show the individual signs positioned higher on the obverse than the reverse, whilst

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[8] Not Room M3, as stated by Cline 1987, 9 and repeated in later references. Mylonas's description of the material from Building M is provided here as an Appendix. My thanks to Elizabeth French for her translation of Mylonas 1963b, 101.

[9] But see below for further discussion.

[10] I would like to thank Dr. Lena Papazoglou-Manioudaki (Ephor, Prehistoric Antiquities) and her staff at the National Museum in Athens, Dr. Alexandras Mantis (Ephor, 5th Ephoreia) and all the staff at the Mycenae Museum, and Dr. Elizabeth B. French and Prof. Spyridon Iakovides for permissions to see and study the 'plaques.' I am especially grateful to Eleni Palaiologou (widow of Dr. Kourachanes) of the 5th Ephoreia, for taking the time to discuss with me her late husband's research.

[11] All code references to hieroglyphic signs correspond to the sign list in Gardiner 1957, 438–548.

[12] Note that the right border line of Tsountas 3 obverse and the flag on Tsountas 6 reverse are no longer visible, but could be seen shortly after the fragment was discovered; see Tsountas 1891: pl. 3.4 and Fimmen 1921:176 fig. 171, lower right and upper left. As for Tsountas 5, with no decoration visible on either face, either all the paint has worn off or it was never inscribed.

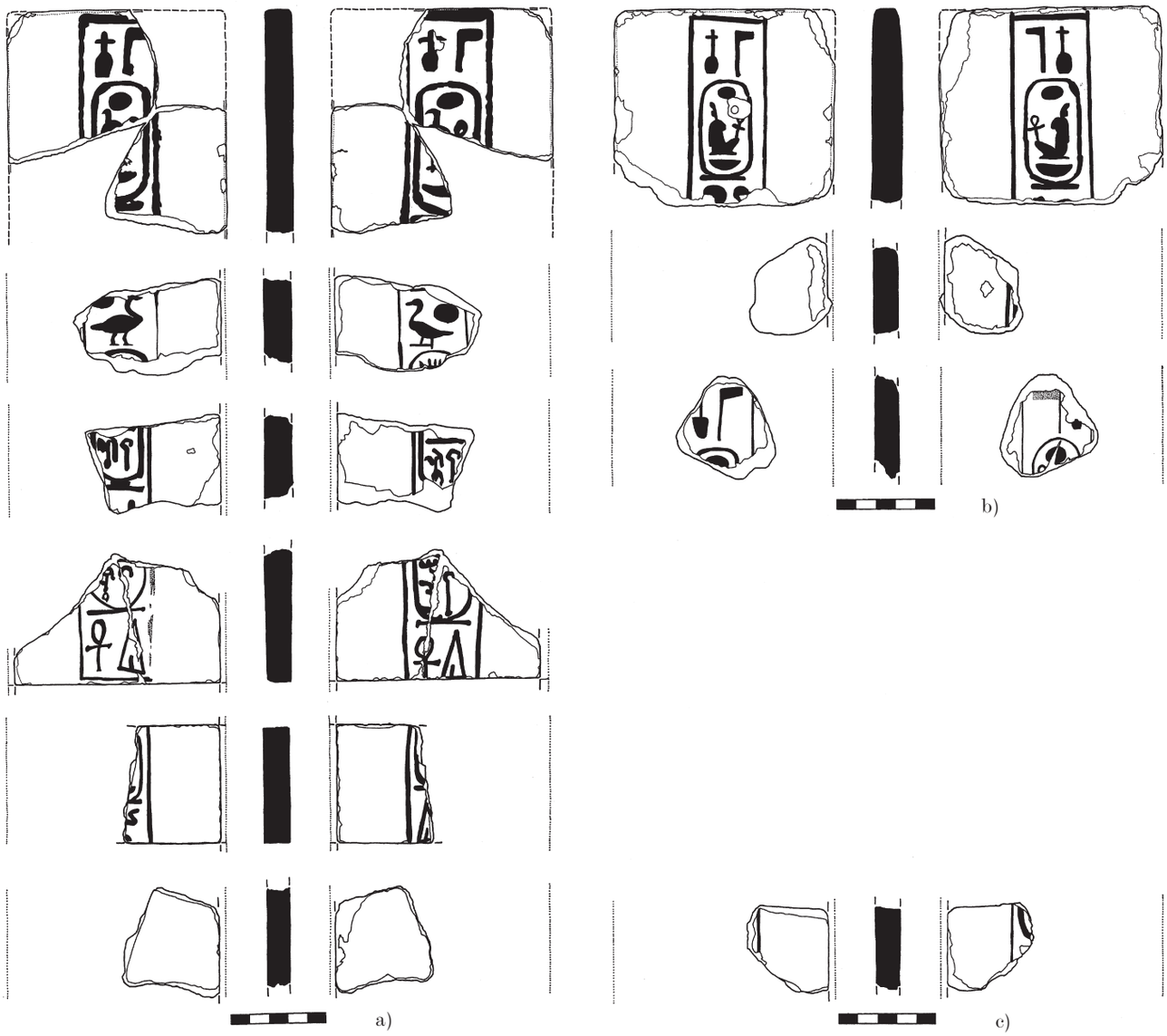


Fig. 3 Drawings of all plaques, with obverse shown at left and reverse at right. In order, top to bottom: **a)** From the area north-east of the Lion Gate: Mylonas 1 (MycM BE 13887+13888 = MM 1499+1498); Tsountas 1 (NMA 2566.1); Tsountas 2 (NMA 2566.5); Tsountas 3 (NMA 2566.2); Tsountas 4 (NMA 2566.4); Tsountas 5 (NMA 2566.3); **b)** From the 'Cult Centre' area: Taylour 1 (MycM BE 18340 = MM 1501); Taylour 2 (MycM BE 18532); Tsountas 6 (NMA 2718); **c)** General position uncertain, most likely north-east of Lion Gate: Tsountas 7 (NMA 12582)

Tsountas 4 and 6 have the obverse text lower instead, thus limiting options for compatibility even within text orientation. The 'long' Cline reconstruction (1990, 208 fig. 4) is impossible using the fragments we have, as is the Helck reconstruction (1979, 96 fig.) as illustrated, despite their generally accepted text reconstruction. Whilst it is possible that some indeterminate fragments might be associated with another on this criterion, they are rejected on other grounds.

Thickness

The 'plaques' range between 12.6 and 17.1 mm in thickness, and some simply cannot be associated with another on the basis of their incompatible thicknesses.[13] This includes Taylour 1 and 2, previously linked

[13] The distinctly different thickness of NMA 2719 (Fig. 2), between 9.2 and 11.0 mm, confirms it is unrelated to the 'plaques.'

by Cline (1990, 206, pl. 3). Their thicknesses are too disparate to be the same 'plaque.' Taylour 1 is 15.1–17.1 mm thick and Taylour 2 is 13.3 mm thick. Large individual fragments are somewhat thicker at the centre than at the preserved edges, but the thinning is gradual and relatively even. Not all differences can be explained in this manner. Whilst glaze thickness, preservation and conservation also must be taken into account for this criterion, the 1.8 mm difference at the same location near the preserved edge, where glaze preservation is similar, still remains too great to associate Taylour 1 and 2.

Compatibility

If the border line(s) on the obverse of any two fragments are positioned, those on their reverse are incompatible. I have attempted all possible associations, using both drawings and, when possible, actual fragments. Line thickness is immaterial, as this changes quite abruptly on at least one fragment (Tsountas 1), and in any case often is directly related to the surface condition of the 'plaques' themselves.

Findspot

The final criterion is findspot, since the fragments were recovered in two distinctly separate general areas of the citadel, either NE of the Lion Gate (Tsountas 1–5, Mylonas 1, likely also Tsountas 7) or the 'Cult Centre' area (Tsountas 6, Taylour 1–2). This criterion remains questionably valid, as we do not know how any of the fragments arrived in their final fill contexts, and both find areas contained considerable downwash from further uphill that arguably had originated from the palace. However, all these downwash layers are at higher levels, overlying the known contexts of all the Mylonas and Taylour fragments which are not downwash contexts. Thus, we cannot demonstrate these 'plaques' originated from a single uphill (and thus likely palatial) source, although it remains *theoretically* possible. Whether any of the Tsountas fragments were recovered in downwash layers is, of course, unknown but we must consider that Tsountas himself declared they "had nothing to do with the palace at all" (Sewell 1904, 259).

Present Condition

This constitutes the second point to be considered. The accidentally achieved wear-and-tear condition of the 'plaques' is immaterial here,[14] only their *deliberately* and anciently altered condition that has never been considered in previous discussion. Only two of the fragments (Mylonas 1, Taylour 1) retain any original edges.[15] This is indicated by the faience glaze extending over the entire 'plaque' surface, including along the surface of the edges. The edges on all other fragments are *not* glazed. It is manifestly apparent that they have been deliberately sawn to reduce (at least) their width and in some cases also their height. All sawn edges, as the original glazed edges, essentially are at right angles to the reverse and obverse faces.

Width

Both fragments (Mylonas 1, Taylour 1) with fully preserved side edges are 11.3 cm in width, with their texts generally centred on both obverse and reverse.[16] The one fragment (Tsountas 3) with both surviving 'reduced'[17] side edges is only 10.7 cm wide, with the text generally centred on both obverse and reverse. This suggests that Tsountas 3 also had the same 11.3 cm width originally, and 3 mm must have been removed from *both* side edges, in order to retain the centrality of the text. When this same 'reduced' width of 10.7 cm also is applied to fragments having only one surviving 'reduced' side edge (Tsountas 1–2, 4–5, 7, Taylour 2), the preserved text also generally is centred. The original width of 11.3 cm is indicated by the dotted lines on either side of all fragments in Fig. 3. The one fragment lacking both

[14] Mylonas 1 retains a good strong blue-green colour in its glaze, whilst the glaze of all other 'plaques' either is virtually leached of colour or it has been lost altogether. This is assumed here to be the result of the different conditions in the various specific contexts where the individual fragments were recovered. In general, colour rarely survives on glazed faience objects recovered in Aegean contexts, whereas those in Egyptian contexts normally retain their colour; see Ben Tor, this volume. Thus the actual physical situation of the context of Mylonas 1 must be most unusual for the Aegean, and its description will make interesting reading when it is published in detail. A preliminary account in English is Iakovides 1983, 52, but see also Appendix and Mylonas 1963a–b.

[15] As does the 'rim' of NMA 2719 (Fig. 2).

[16] This figure corresponds almost exactly to the ancient Egyptian measurement of 6 digits, as kindly pointed out to me by Rolf Kraus; my thanks also to Kate Spence for further details. An Egyptian cubit corresponds to 52.5 cm. There are 7 palms to a cubit, and 4 digits to a palm, therefore 28 digits to a cubit. One digit thus is 1.875 cm, and 6 digits therefore 11.25 cm. See Arnold 1991, 10.

[17] This term is employed here for the edges that have been sawn (in antiquity).

side edges (Tsountas 6) also can be generally positioned by similarly centring its text. Thus, most likely, all 'plaques' originally had this same width of 11.3 cm before their side edges were sawn. The 'plaques' therefore likely all were the same shape and size originally or, at minimum, had a uniform width.

This point seems always to have been assumed, but had never actually been demonstrated. The 'reduced' side edges all are absolutely flat and smooth to the touch, with glaze chipped off along the face edges (most clearly seen on Tsountas 3–4). That these edges were in fact *sawn* rather than, for example, shaved or worn down, is indicated by one fragment (Taylour 2) that clearly has been 'snapped' at the last minute rather than entirely sawn off, leaving part of the original edge standing proud of the 'reduced' edge and somewhat rough to the touch. Furthermore, none of the 'reduced' edges shows any indication of further treatment. Their surface, or rather lack of it, was unimportant for their altered function, but the centred text *was*, since it is a tricky, time-consuming and otherwise unnecessary process to remove so narrow a portion of the edge.

The uniform width of all but the two still with edges intact (Mylonas 1, Taylour 1) also strongly suggests that the rest all would have been deliberately altered at the same time to this uniform width, for a single unknown purpose or perhaps several related purposes. Also, whenever they actually arrived at Mycenae – and we have over a century to play with – they most likely would have been kept or stored together before this alteration was done. This suggests either storage under controlled conditions, possibly in the palace uphill, or alteration immediately upon arrival, before any hypothetical distribution either within or beyond the palace but still within the citadel occurred. They appear to have been valued not for their text or any association with the Egyptian pharaoh named, but rather for their visual and physical attributes as thick flat colourful (blue-green) decorated objects. Nonetheless, the text clearly remained visually important (and clearly was intended to be *seen* on completion), as both edges were reduced by 3 mm to retain its central position, rather than just one side having 0.6 mm removed. The characteristic Mycenaean love of decorative symmetry is quite apparent in this alteration.

The two 'plaques' separately treated, or rather not treated, in this manner, perhaps arrived at a different time, or simply were used for a different purpose or purposes at Mycenae. They are, after all, the largest and the least altered fragments.

Height

So, what shape were the 'plaques' intended to have *after* their alteration? The answer is: 'different' shapes. Despite their fragmentary condition as recovered and their uniform (reduced) width, their final, deliberately reduced, vertical dimensions demonstrably differ. One fragment (Tsountas 4) has a 'reduced' edge at both top and bottom and at one side, thus three of the four edges, and is now horizontally rectangular and 6.2 cm in its intentionally 'reduced' height. It is possible, although not demonstrable, that the majority of other fragments would also have been reduced to this shape or even shorter, as they do not exceed this height, except the three large fragments. Tsountas 3, Mylonas 1 and Taylour 1 all have different heights. Tsountas 3 and Mylonas 1 both were longer than they now appear, as they have a broken edge either at the top (Mylonas 1) or bottom (Tsountas 3), whilst Taylour 1 may or may not be is as it was intended. The bottom edge of Taylour 1 is further discussed below.

The sawn bottom edge of two fragments (Tsountas 3–4) also may have had 3 mm removed, as on the vertical edges. It requires only the bottom painted border line to complete the text on the reverse of Tsountas 3 (as is seen on its obverse, with border line), as the reverse text is slightly lower on the 'plaque' than the obverse text. Adding 3 mm to the bottom would supply this needed extra space for the border line on the reverse. Tsountas 4 is somewhat problematic (I shall return to this point below), but that text also probably had only this same 3 mm of original edge removed, onto which its border line would have fit on both faces.

The other, smaller, fragments as preserved may have been cut down to a horizontal rectangle like Tsountas 4, with the inscribed text retained and deliberately centred, instead of (for example) cutting it only on one edge instead of both or cutting it lengthwise to avoid the text altogether or as much as possible – thus reducing the labour involved. Some dedicated effort seems to have been made to incorporate the text on at least one 'plaque' face as part of the new reduced form: Tsountas 4 is cut at the top just above the cartouche on the obverse, both it (on the reverse) and Tsountas 3 reverse strive to retain the base line of the *di* sign (X 8) whilst removing the bottom border line, and Taylour 1 has just the very top of the *s3R^c* (N 5, G 39) on the reverse. All this also suggests that their new purpose(s) may have required

only one visible side, which still needed to be balanced visually even if the text itself could not be read. If Mylonas 1 was reduced at the bottom, this would have done again at least below the cartouche, and possibly at a point similar to Taylour 1 reverse. The broken height of Tsountas 3 (6.8 cm) clearly is taller than the deliberately 'reduced' height of Tsountas 4 (6.2 cm), but we can only guess whether it was as tall as Mylonas 1 or Taylour 1, or neither. If we presume that visual asthetics played some role in deciding its reduced height, we may assume that Tsountas 3 likely was sawn just above the top of the cartouche, estimated to be at about 9.5 cm above its reverse 'reduced' bottom edge. Therefore, it likely also was horizontally rectangular, not quite square, and almost the same preserved height as Taylour 1.

Taylour 1 usually is seen as a square and has become the model for reconstructing other less complete fragments as square, as was suggested as one possible reconstruction by Cline (1990, 207 fig. 3).[18] In fact, it actually is much shorter than it is wide – a full 14 mm shorter, at 9.9 cm – but the original text clearly continued below the bottom as preserved, as it still preserves the upper portions of the *s3 Rᶜ* text. The bottom edge of this 'plaque,' our most complete fragment, is the only edge that is neither original nor sawn. The edge looks rather smooth and is surprisingly straight, so much so that it usually is taken as a straight edge. However, it is not glazed, yet is comparatively much too rough and uneven to the touch to have been sawn like other 'reduced' edges. It clearly differs from them, but *how* this was achieved is problematic. The nearest one might suggest is that it might have been chiselled in a single blow, except that this is not apparent at the break.

Mylonas 1 is 11.8 cm tall as preserved, when the two fragments are correctly positioned (as on Fig. 3), and therefore it is now 5 mm longer than its width. This 'plaque' too was not square but must have been vertically rectangular even after it had been sawn, *if* indeed it had been sawn. Therefore, the projected original and reduced shape both must also have been even taller than it is now, since the bottom edge is not preserved. Whether this bottom edge was reduced, or remained glazed as it was made originally, we will never know unless further joining fragments are recovered. There is no evidence for any secondary alteration on this 'plaque,' and it may not have been altered at all.

In other words, we have two 'plaques' unaltered on three sides, of two different preserved lengths, one vertically and the other horizontally rectangular. If both originally displayed the same text *in full*, Mylonas 1 must either have been considerably taller than Taylour 1, or its lower (missing) text was squashed into a much smaller space. The same text on Mylonas 1 extends much farther than that on Taylour 1.[19] These are not a pair, nor (perhaps) were they intended for the same purpose at Mycenae. The other fragments likely were used for another purpose, or at least reduced to several different heights, and probably all were horizontally rectangular (possibly excepting Tsountas 3).

Altered Appearance and Function(s)

What might their altered appearance and function(s) have been? Mylonas 1 and Taylour 1, despite their differing heights, appear to share several features in common. The first and most obvious commonality is their original edges. Another is their findspots, apparently related to cultic deposits: that of Taylour 1 is clear, whilst Mylonas 1 is not so clear due to its lack of specific context information (see Appendix), but the associated features (one of a variety of small finds near an internal structure [platform or bench?] in the large central room, of buildings having a similar megaron-like plan) of its context are reasonably alike insofar as is published.[20] Both might then have served the similar purpose of having been offerings to the deity or deities to whom the buildings or rooms were dedicated, presuming Building M also was a cult building.

What of the others, with deliberately and carefully 'reduced' edges indicating that specifically inten-

[18] Kourachanes' reconstruction (see n. 6, above) actually is rectangular, including the epithet following the royal *prenomen (Nb-m3ᶜt-Rᶜ)* in cartouche. An epithet is not included in Cline's reconstruction of the *prenomen*, as it does not appear – at least in full – on Taylour 1, and he has squashed the remaining *nomen* text onto a similarly square reconstruction (Cline 1990, 207 fig. 3 lower. As Kourachanes' reconstruction has only been recently published, Cline's reconstructions usually are cited in the literature.

[19] The texts are identical, although Taylour 1 preserves the top of the *s3 Rᶜ* text on both surfaces, that is lost on both Mylonas 1 texts as preserved. The shorter (obverse) Mylonas 1 text is 10.3 cm from the top edge, the reverse text being at least 11.0 long, to the bottom of the cartouche. The Taylour 1 text is 8.1 cm on the obverse and 9.2 cm on the reverse, from the top edge to the bottom of the cartouche.

[20] See n. 14, above.

sive and carefully achieved labour was required for their altered appearance and function(s)? One (Taylour 2) was recovered in the same cultic space as Taylour 1, but in fill of a later level. We have no contextual record for the others. Their final deliberately 'reduced' flat shapes are not all the same. One suggestion for their altered function is perhaps as boxes or perhaps box-lids, as only one flat surface and centring the text appear to have been of consideration. Yet nothing now indicates any attempt at inset along the edge, nor any other means to prevent it from shifting, although inset Mycenaean lids are known in ivory and other materials. Nor is there any indication of gluing or otherwise joining the pieces, which are not all of uniform size in any event, to make a box-container.[21] They might instead have been employed as insets on a larger object that has not survived, much like the famous Knossos 'Town Mosaic' inlay pieces had been; this would explain the different sizes, as well as the need for considering only one face and lack of concern for the raw 'reduced' edges, but would not explain the need to reduce the edges at all. Might they have been edged with gold, like some stone and faience seals? This would not elucidate their purpose, but would hide the raw edges, although it would also obscure the top and bottom edges of the text and still would not explain the need to reduce the edges so minimally. This factor is the key to understanding their specific altered function. Their use as cultic deposit objects may, I think, be secondary (tertiary?) to their deliberately altered initial purpose or purposes at Mycenae.

Original Appearance and Function

What shape were the pieces with 'reduced' edges originally? Their apparently similar width and text strongly suggest they also would have been of similar shape to Mylonas 1. Cline's reconstruction of an originally long rectangular 'plaque' therefore would have to be correct (Cline 1990, 208, fig. 4), at least in principle. His alternative reconstruction of smaller square 'plaques' (Cline 1990, 207, fig. 3) must be rejected, on several grounds – not the least of which is that these "would not satisfy the Egyptian's love of balance and completeness," as Lilyquist (1999, 303 n. 3) has recently pointed out. Also, the 'epithet' *s3 Rˁ* after the *prenomen* cartouche is not found in Egyptian texts without being followed by the *nomen* cartouche – it is in fact the formulaic text separating *prenomen* and *nomen*, as can be seen on Tsountas 1 – so the *nomen* cartouche must be restored on Taylour 1, a long rectangle. The 'plaques' generally are thought to have been 'foundation deposit' bricks, but such bricks almost always are inscribed only on one face, and indeed – as has been repeatedly stated – there are no direct parallels in Egypt for these 'plaques,' nor anywhere else for that matter. Nonetheless, the most closely similar objects to these 'plaques' remain Egyptian 'foundation deposit' plaques and bricks, and those dated to late Dynasty XVIII (Amenhotep III and later) are most closely similar in size.[22]

So, what were they originally, and where did they come from? And what were they converted into, presumably but not necessarily at Mycenae? Lilyquist, quite rightly, cautions against our wholesale acceptance of these 'plaques' as *Egyptian* objects, despite their good orthography and palaeography. They equally could have been Canaanite products, produced under an outwardly overwhelming Egyptian cultural influence that began in the reign of Thutmose III in the mid-15th c. Whether either Egyptian or 'egyptianising,' we should also distinguish between those with text facing the same direction on both sides, and those that do not. As Lilyquist also points out, left-facing text is a *deliberate* choice, and a reason needs to be sought for it. Right-facing text, as seen on the obverse of all 'plaques,' is the norm. When the norm is not employed, it is done for a reason, such as counterbalance on the right side of a doorway, when both texts need to face inwards.[23] Knowing this reason (or reasons) is key to under-

[21] Similar perhaps to that found in Shaft Grave V, see Hood 1978, 114, fig. 100. Several inset lids are known as early as the Shaft Grave period.

[22] Weinstein 1973, 214–215 #72 includes one from the temple at Abydos inscribed with the name of Amenhotep III (one face only, facing right), as 19.7 × 10.5 × 1.2 cm in size. More generally, see his discussion on Dynasty XVIII and Dynasty XIX–XX plaques and bricks, *ibid.*, 126–133, 239–247. Foundation 'bricks' of Ramesses II (Dynasty XIX) are inscribed on both sides and have both texts facing right; these are much larger in size; see *ibid.*, 235 #88.B–D.

[23] As was projected by Helck 1979, 96 fig. Fischer 1977, 9 gives three basic reasons: confrontation, symmetry and concordance, with further discussion and examples; see especially *ibid.*, 17–20 §11. Left facing text on the reverse side of portable objects is quite rare throughout the dynastic period; Fischer lists but 10 examples, only three of which are of Dynasty XVIII date and the latest in the reign of Tutankhamun.

standing the original purpose(s) of the 'plaques' – however, knowing their original purpose(s) also is key to explaining the left-facing text. We cannot separate the 'plaque' orientations by their text, as the same text is indicated in different directions on the obverse of different 'plaques.' Mylonas 1 has the same text as Taylour 1 and Tsountas 6 (all *prenomen* cartouche), as do Tsountas 1 (beginning of *nomen* cartouche) and Tsountas 2–3 (end of the same *nomen* cartouche).

Nonetheless, they obviously *were* separately distinguished by text orientation when they were initially produced. They are in fact two different 'things,' possibly even for different although possibly related functions, originally. At least one group, those with opposite orientations, certainly does *not* constitute 'foundation bricks,' as there is no reason to differentiate orientation on the two faces, even *if* (as is not the case in any event) foundation bricks were inscribed on both faces. Nonetheless, it is highly unlikely that the same-orientation-text 'plaques' are foundation bricks either, as they are otherwise too similar to the first group. The two groups must be related, although distinguished by text orientation. All we have done is eliminate the only apparent possible interpretation of their function. No comparison for alternative functions can be suggested at present.

We might note, however, that two 'plaques' have unique features. Unlike on all other fragments, the texts of Tsountas 4 are not the same on both surfaces. The reverse preserves the cartouche but no interior text, and enough of the *di* sign (X 8) that we may be confident in restoring the missing text as the *nomen* and epithet *(Jmn-ḥtp ḥkꜣ wꜣst) di ꜥnḫ* ('Amonhotep, ruler of Thebes), given life') as on Tsountas 3 (and 2). The obverse of Tsountas 4 preserves again the cartouche but also a portion of the interior text that can only be the *nb* sign (V 30), and thus should be restored as the *prenomen (Nb-mꜣꜥt-Rꜥ)*, followed by an epithet that is difficult to interpret from the surviving text. The usual epithet following *(Nb-mꜣꜥt-Rꜥ)* is *sꜣ Rꜥ* ('son of Re;' compare with Mylonas 1), but personal detailed observation of the fragment allows me to state that the visible remains of the text cannot be *sꜣ Rꜥ*. This also had been the conclusion of Kourachanes, who considered it to be the snake (I 10) and bread loaf (X 1), reading *ḏt* ('forever, eternally'), and thus the end of the inscribed royal titulary. This indeed seems to be the best interpretation of the text as preserved, and one accepted here for lack of a better. It clearly is not *di ꜥnḫ* ('given life'), but nor is it *ꜥnḫ ḏt* ('living forever') since the *ꜥnḫ* sign (S 34) would precede the snake. The epithet tells us the obverse text faces right, whilst the *di* tells us the reverse (unlike Tsountas 3) faces left.

The other unique 'plaque' is Tsountas 5, with no indication of any text on either face. Based on the proposed 'reduced' width of 10.7 cm for all sawn fragments, this piece should have at least the vertical border line on either face (*if* the edge is in fact a vertical edge). Either its text too has faded completely, like some lines on Tsountas 3 and 6,[24] or it had never been inscribed. If the latter, then this would allow us to reconsider the rejection of *all* 'plaques' original identification with 'foundation bricks' in this case, as most 'bricks' have large undecorated areas on one face and no decoration on the other.

Would they be contemporary with the reign of Amenhotep III, or could they have been produced at a later date? Either, generally speaking, is possible. Ancient Egyptian monarchs often continued to be venerated long after their death, and many were the foci of posthumous cults. Various objects of amuletic significance (scarabs in particular) are well known to have been manufactured and inscribed with the name of a long-dead king as protective devices imbued with the power of the king named. There is, however, little evidence for posthumous veneration of a deified Amenhotep III,[25] and so the likelihood of objects of later date being inscribed with his name is extremely limited. The odds are overwhelmingly that these 'plaques' were manufactured during his lifetime or only shortly thereafter, almost certainly not later than the reign of Tutankhamun at the most – if that, and hence possibly later in, but not the latest years of, Dynasty XVIII and the 14th c. BC. Nonetheless, it is not entirely impossible that they are posthumous, especially since they are unique.

Origin

Setting aside the generally unanswerable question of whether the original artefacts were Egyptian or 'egyptianising' from Palestine (or elsewhere?), cer-

[24] No original photograph or early drawing of Tsountas 5 was published, so we cannot make comparison with its present state.

[25] Even his mortuary temple had been demolished by the time of Ramesses II.

tain evidence has been cited suggesting that these 'plaques' were in fact created in the Aegean, perhaps even at Mycenae. Crucial for this argument are the results of Brill's lead isotope analysis of the glaze covering the Taylour 1 'plaque' that indicated a source for the lead trace constituent at Laurion in the Attic peninsula.[26] Combined with their good orthography, this has led to the suggestions (amongst other scenaria) that one or more Egyptian artisans were resident at Mycenae, or that a Mycenaean artisan was familiar enough with Egyptian hieroglyphs and technology that he could both produce these 'plaques' at Mycenae and inscribe the text properly.[27] The implications of such interpretations of events surrounding the 'plaques' are far-reaching and, I think, should await far stronger evidence than we presently have. Nothing else recovered at Mycenae even tentatively suggests either possibility.

What was known from its initial publication but not *considered* a strong factor in concluding an Aegean origin is that this particular 'plaque' had been found actually within a Mycenaean vessel made of lead, and thus had been directly exposed to *its* lead isotopes for the more than 3,000 years between LH IIIB (middle) and AD 1968 when Taylour excavated it.[27a] The results of this analysis are inherently flawed, and I think so too any scenaria based upon it. A more useful analysis might be conducted on Mylonas 1, the only fragments to have retained their original glaze colour. The glaze on all other 'plaques' have been leached out to become virtually whitish opaque or colourless, the result of their context conditions.[28] Clearly, the context conditions under which the two Mylonas 1 fragments survived must have differed considerably from the other contexts, even those excavated by Tsountas somewhere nearby.

Alternatively, importation of Laurion lead (or silver) for production in Egypt has been postulated. The apparent use of the Egyptian measuring system in their manufacture, being 6 digits in width, would suggest an Egyptian origin for the 'plaques' as the most likely option or, at minimum, manufacture in an area or situation where Egyptian units of measurement were employed.

Arrival

So, when would the 'plaques' have arrived at Mycenae? This cannot of course be said with any degree of certainty, presumably all at the same time but this could have been at any point between Amenhotep III's reign in the first half of the 14th c. (= LH IIIA1/early 2), and LH IIIB (middle), the earliest actual context date, over a century later. What else arrived at Mycenae during this period, and can we reduce the margin at all? The first question to ask is: Is there any period within this more than a century that anything could *not* have arrived? Answer, no. No Egyptian import found at Mycenae post-dates LH IIIB (middle) in Egyptian terms (latter half of the 13th c. BC, later reign of Ramesses II), and anything dateable between this point back to the reign of Amenhotep III could have arrived at any point within this period up to the date of its depositional context.

Is there a limited period of *manufacture* in Egyptian terms for the imports that arrived within this period. Actually, yes, very much so. No imported Egyptian object recovered at Mycenae need be any later in date than Dynasty XVIII, with the exception of two Dynasty XIX scarabs in CT 526, which therefore can only have *arrived* at Mycenae no earlier than LH IIIB.[29] The other tombs at Mycenae having Egyptian imports, all with Mycenaean contents indicating long use within LH II–IIIB or even later, contain only Egyptian artefacts not demonstrably later than Dynasty XVIII in manufacture. These chiefly are stone vessel types that do not continue into Dynasty XIX.[30] Even though many Egyptian objects are recovered in contexts of LH (and LM) IIIB date, very little Egyptian material recovered in the Aegean can actually be dated to Dynasty XIX in Egyptian terms. Exceptions are the two small Ramesses II cartouches and eight scarabs from Perati ('end of LH IIIB through

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[26] See Cline 1990, 209–210.

[27] See, for example, Lilyquist 1999, 306.

[27a] Brill's caveat to this effect, cited by Cline (*ibid.*, 210 n. 44), seems to have been largely ignored. Two samples were taken beside breaks, one from each face; the glaze is still quite green-stained at one of these points, either remains of the original glaze colour, or from contact with the lead vessel.

[28] They do not appear to have been so faintly coloured originally, as faint traces of blue-green colouration can be still discerned on some fragments upon close examination. See also n. 14, above.

[29] Wace 1932:93 #1–2, 198, pl. IX.1–2; French 2002, pl. 8.

[30] A detailed analysis of Egyptian and 'egyptianising' objects and their contexts at Mycenae and elsewhere on the Greek Mainland is in progress by the author. This follows a similar analysis of material on Crete (Phillips forthcoming).

LH IIIC') on the Mainland,[31] and one scarab each at Zaphor Papoura Tomb 99 (very late LM IIIA2 or fairly early LM IIIB)[32] and in an LM IIIB house at Poros,[33] both on Crete. Three of the five scarabs recovered in the Cape Geledonia shipwreck are Dynasty XIX in date,[34] and the wreck itself is roughly dated to *c.* 1200. Other scarabs found both on Crete and the Mainland are no earlier than Dynasty XIX in date, but are surface finds or otherwise without provenance. They could not have arrived in the Aegean earlier than LH/LM IIIB because of their Egyptian date, but could have arrived at any point thereafter. Thus some types of goods, specifically small amulets and scarabs, clearly continued to arrive in the Aegean in Dynasty XIX. We cannot therefore discount any period within this century or more that the 'plaques' – or even fragments of 'plaques' – arrived at Mycenae. There is nothing to argue against their having arrived in less than perfect condition. Nonetheless, the odds are more strongly in favour of their having arrived at Mycenae earlier rather than later, in the 14th (later Dynasty XVIII) rather than 13th (Dynasty XIX) century BC, and so had been around for some time before the circumstances under which Taylour 1 and its container were last seen (or used?) by a Mycenaean in the 'Room with the Fresco.'

I don't have a snappy ending, nor a full set of conclusions. We can now say that they originally were a group, or rather two probably related groups, of like objects. That there seem to have been a considerable number of them. That two of them (Taylour 1 and Mylonas 1) seem to have been of some cultic importance, possibly offerings to one or two deities, at Mycenae. That the others were deliberately altered with great care into something quite particular, not necessarily at Mycenae but certainly something different from the two already mentioned. We must accept that the reversed text on some of them is key to understanding their original purpose(s), and their uniformly 'reduced' edges are key to understanding the intended (initial?) purpose of the majority of them at Mycenae. And that, whilst we must remain uncertain of their actual Egyptian origin, the apparent 'fact' of an Aegean origin cannot be taken at face value.

But the 'plaques' remain problematic. What *were* they originally? *Why* were they reduced in size? *Where* were they reduced in size? *What* were they intended to be? *When* were they brought here, and by *whom*? And, perhaps most importantly, *why* can't we find anything like them anywhere else? They are still a mystery.

More than half of the pieces have an apparently deliberate gouge at or near the centre of one face (Mylonas 1, Taylour 1, Mylonas 2) or elsewhere on the face (Tsountas 2, Taylour 2), whilst Tsountas 3 may have been broken by such use (centrally radiating break). This all seems too coincidental for accidental damage, especially as Taylour 1 apparently is the result of a small circular drill still distinguishable within the damaged area. They most likely are the result of the *raised prongs* used to support the 'plaque' during firing, as all surfaces were glazed and so such prongs would have been used during firing. The positions strongly suggest that three such *prongs* were used, one centred near one end, and the other two near the corners of the other end. If so, and since the two centrally positioned 'gouges' are at the 'top' *prenomen* end (Mylonas 1, Taylour 1), and the off-centre 'gouge' is at the 'bottom' *nomen* end (Tsountas 2), this would suggest that the cartouche on Taylour 2 encased the *nomen*. This in turn would provide further evidence for a 'long' rectangular shape for the 'plaques' in their original state.

Appendix

"In almost the middle of the NE wall of room M-2 were found the remains of an elliptical structure, of

[31] Iakovides 1969–1970, III, pls. 48.β.Δ11, 113.δ.Δ140 (cartouches), pls. 30.α.Δ128, β.Δ 129, 37.α.Δ196, 58.β.Δ132, 85.α (scarabs); most if not all the scarabs are of Ramesside date. It may be that some of the cornflower beads and amulets also found in this cemetery (*IBID.*, III: pls. 31.α.Λ172, 46. α.Λ13, 61.α.top row, 69. α.Λ269 (cornflower beads), 91.β.Δ69, Δ75 [Bes figures] , Δ70, Δ74 [*T3-wrt* figures], Δ71–Δ73 [crocodile figures]) also are Dynasty XIX–XX, but they cannot be dated so precisely. The 'end of LH IIIB through IIIC' dating of the Perati graves as a whole also allows for the possibility of Dynasty XX manufacture for this material (see Iakovides 2003, 128–130). The quantity of imports at Perati is the exception that proves the rule at this time.

[32] Karetsou *et al.* 2000, 322 #332. This scarab has a virtually identical text to one from Mycenae CT 526. See Phillips forthcoming, #482.

[33] Karetsou *et al.* 2000, 324 #336. Note that the inscription on this scarab is *not* the name of Queen Ankhesenamun, but rather an amuletic formula, and the scarab itself is of Dynasty XIX date. See Phillips forthcoming, #265.

[34] Brandl 2003. The scarabs and other seals were the only Egyptian objects found at the wreck site, suggesting that little else Egyptian was being transported (although perishable goods may well have been) at this time.

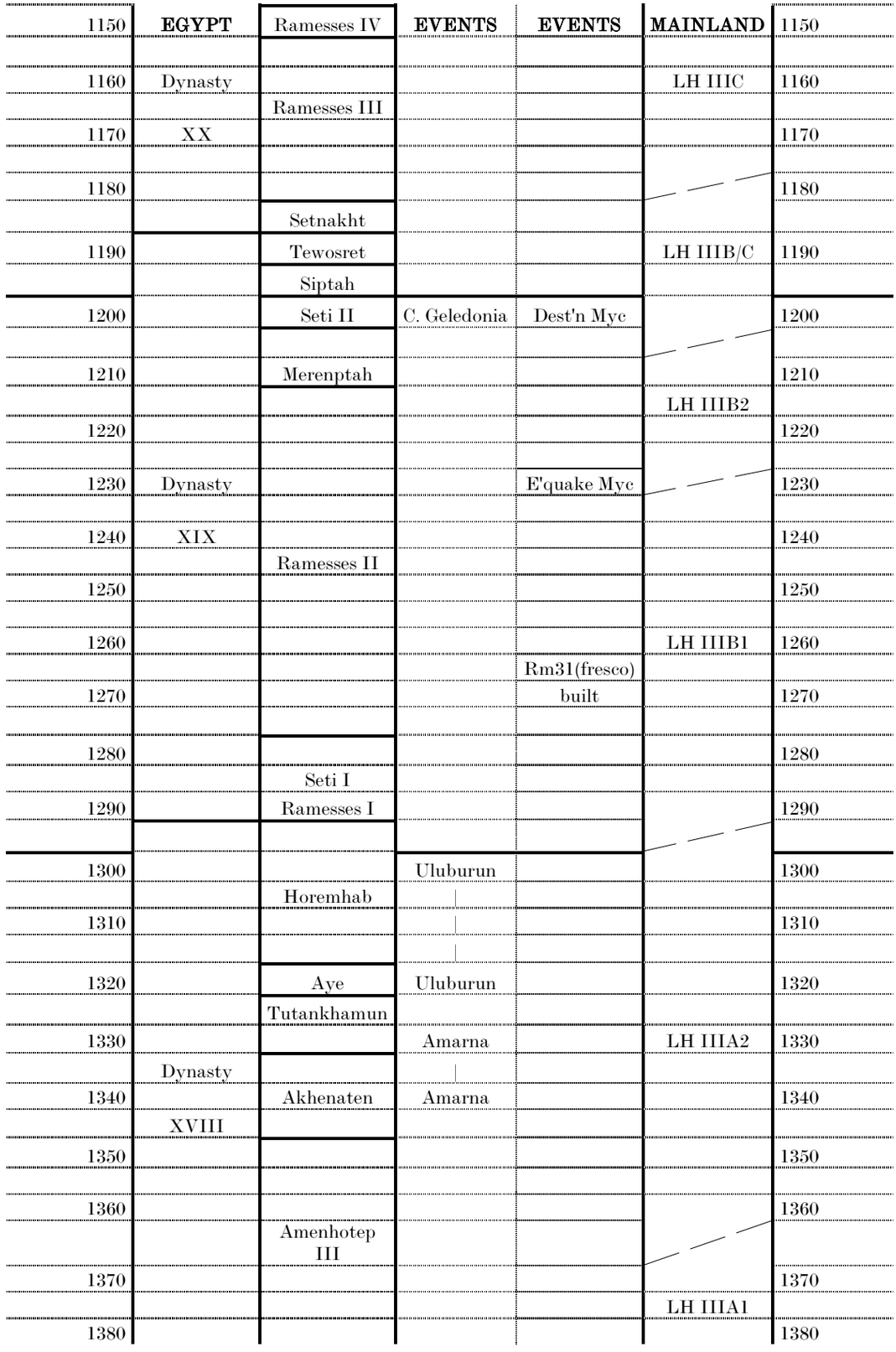


Fig. 4 Chronological chart

which the actual use remains uncertain because the structure for the most part had been disturbed in the original excavation of the area (pL. A). The floor, which survived under this structure, as well as the wall behind it were coated with a layer of limeplaster. The deposit between this structure and the NW corner of the room to a depth of about 0.70m. along the length of the wall, was found undisturbed and in it were found beads of glass paste, sherds dating to the last years of LH IIIB, fragments of figurines of the well known Mycenaean types Φ and Ψ and animals as well as one complete figurine of Φ type the surface of which was covered with sinuous lines in reddish paint (pl. 77a). Immediately above this figurine were found two small fragments of an Egyptian faience plaque which bears the same inscription on both sides (pl. 76b). The inscription, according to Mr. N. Boufidis of the National Archaeological Museum, includes the cartouche of Amenophis III of the 18th dynasty (about 1405–1370 B.C.). In the same deposit was found a bronze ring the circular bezel of which had been of silver and originally carried further decoration or a carved seal stone, which has not been preserved. Among the other finds from the building I note a fragment of a small rectangular plaque of glass paste on which we have the depiction of a lion-shaped daemon, of the type known from the finds of Tsountas in the Tholos Tomb of the Genii, and many fragments of lead vessels. One example, almost complete but heavily crushed, of a lead vessel was found *in situ* (pl. 78a) in room M-3. In the undisturbed deposit of the triangular area Φ was found a figurine of ivory, carved in the round, only 0.035m. in height, of a woman holding her naked breasts in her hands (pl. 77b) and a carved seal stone with the representation of a lion and a bull." (Mylonas 1963b, 101; translation by Elizabeth French).

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A New Pumice Analysis from Knossos and the End of Late Minoan I A

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The volcanoes of the Aegean Hellenic Arc made pumice available for human use at all times. In Crete, for example, at Neolithic Knossos, in the words of the excavator J.D.Evans, "many small pieces were found throughout all the strata".[1] Two pieces were used in the Early Bronze Age settlement at Myrtos, Room 57,[2] c. 2200 BC, and a piece was found in the Early Minoan Tomb II at Lebena.[3] An analyzed sample from a Middle Minoan II level at Knossos probably came from the Cape Riva eruption of Thera c. 21,000 BP.[4] When pieces of pumice or volcanic ash occur in Late Minoan I in contexts in Crete, c. 1580–1425 BC, the finds are of particular interest because they have the potential to correlate the Minoan levels with the great LB 1 eruption of Thera, which put an end to the brilliant settlement at Akrotiri. The town was destroyed in Late Cycladic I, with hundreds of ceramic vessels exported from or locally made but based on Late Minoan IA Cretan workshops. As far as Crete itself is concerned definitive proof that the eruption occurred in Late Minoan IA, well before the major destruction of Minoan civilization in Late Minoan IB c. 1440/1430 BC, came in 1989 with the discovery of Thera eruption ash in the settlement at Mochlos, accompanied by LM IA vases and stratified below the LM IB destruction level.[5] This crucial discovery was supported by the evidence of pieces of pumice from LM IA contexts at nearby Pseira[6] and from LM IB Knossos,[7] samples at the latter site showing that the eruption has already occurred by the time of the Minoan destruction. The evidence from all three sites was presented at the Thera and the Aegean World III conference in 1989, published in 1990.[8]

There remained the question: at what point in LM IA in Crete did the eruption occur? The event is usually taken to mark the end of LM IA, but there has remained room for precision on this matter. Was there, for example, any period of LM IA time after the eruption but before the LM IB ceramic period?

In 1997 excavation on the Stratigraphical Museum site at Knossos, directed by the writer for the British School at Athens, revealed a quite substantial deposit of about 140 vases datable to LM IA. This material was presented in 1999 in *Meletemata*, the volume (actually three large volumes) in honour of Malcolm Wiener.[9] A brief summary is all that is needed here, but incorporating and synthesizing new information. The deposit was found in a very small room or walled space; it formed a layer 0.50 m thick from the paved floor upwards. Together with vase SEX/97/P2423 in the deposit was a piece of pumice. Given the context it was hoped that analysis would show the piece to be a product of the Theran Minoan eruption. It turns out, however, to be probably from the Theran Lower Pumice Series (Unterer Bimsstein), c. 200,000 BP and so somewhat less than helpful for our present chronological purposes.[10] It is simply one of those pre-Minoan eruption pumice pieces picked up and used in Crete, like the Neolithic and Early Minoan pieces referred to above.

Above the 0.50 m thick LM IA vase deposit came a fill or accumulated soil 0.63 m thick, with nine more catalogued vases, again LM IA. At the top of this level was a probable floor marked by two stone slabs, with more LM IA vases. Among these were fragments, identified in 2001, of a Vapheio cup with small running spirals (Fig. 1A, Pl. 1A). This is a very distinctive composition; other examples are known at Knossos, including at least one from another LM IA context (Gypsadhes Well) and all probably products of a single Knossian workshop or even painter.[11] A related Vapheio cup, with a single band of relatively

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[1] Evans 1968, 271

[2] Warren 1972, 52, 240

[3] Alexiou and Warren 2004, 138, 140.

[4] Warren and Puchelt 1990, 78–79 (sample C). Bichler, Huber and Warren 2006.

[5] Soles and Davaras 1990.

[6] Betancourt, Goldberg, Hope Simpson and Vitaliano 1990.

[7] Warren and Puchelt 1990, especially 79–80 (sample J).

[8] Hardy and Renfrew (eds.) 1990.

[9] Warren 1999.

[10] Bichler, Huber and Warren 2007.

[11] Warren 1999, 899–900 and notes 61–63 for a list.

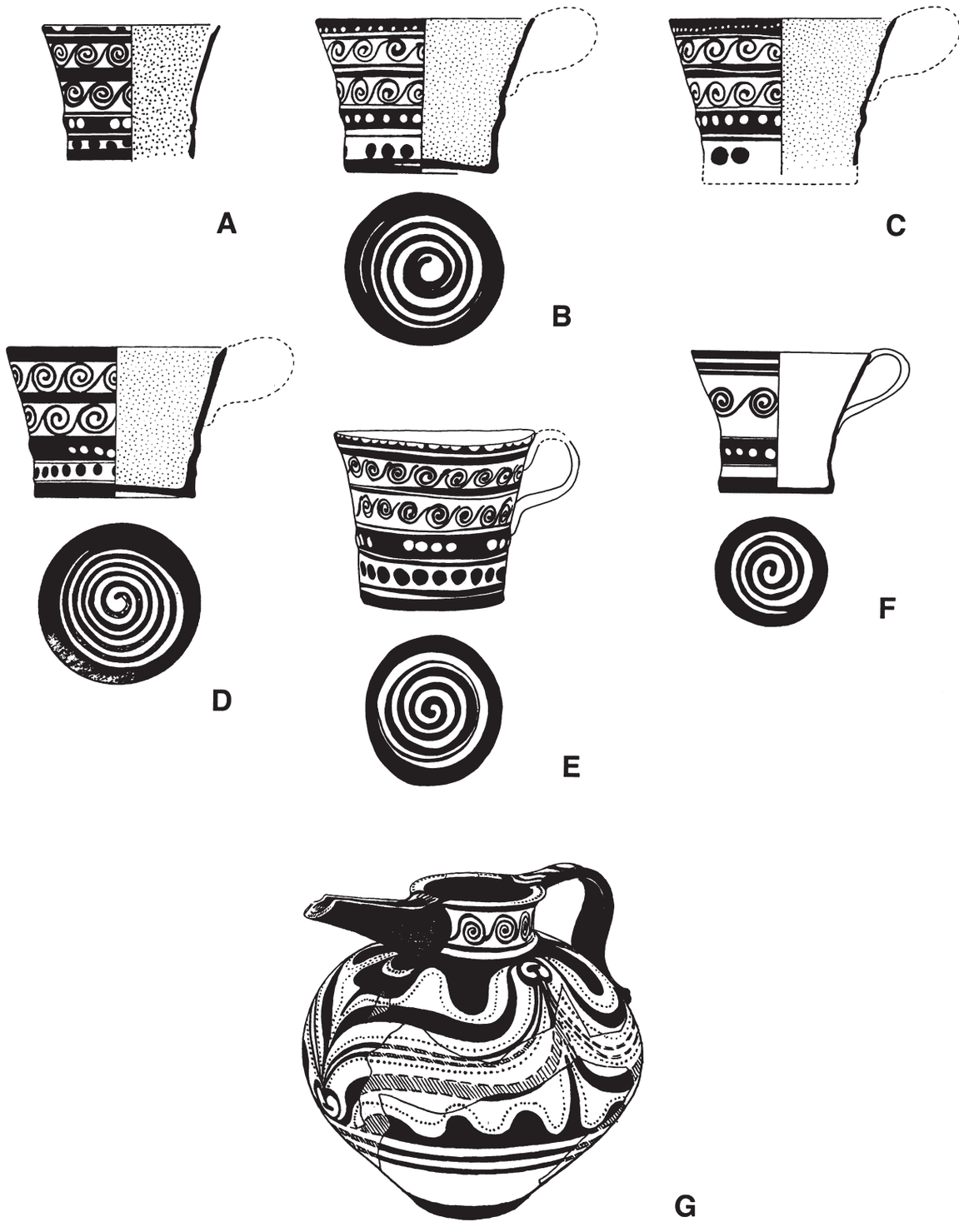


Fig. 1 Late Minoan I A Knossos Small Spirals Workshop Vapheio cups (A–F). A–D: Knossos Stratigraphical Museum Site P2579, P442, P442a, P443. E: Akrotiri West House, Thera Museum 3191 (drawing after photo Marthari 1993 pl. 40). F: Ayia Irini: Paleokastro (Cyprus) Quilici Tomb vase no. 228 (drawing derived from combination of Quilici 1990, figs. 220–221, 316a no. 228, not checked against original vase). G: Late Helladic II A bridge-spouted jug, Palaikastro (Crete) Block B Room 10 lower level (LM I A) (after Bosanquet 1902–1903, 285 fig. 5), all scale 1:3



Pl. 1 A Late Minoan I A Knossos Small Spirals Workshop Vapheio cup. Knossos Stratigraphical Museum Site P2579. B: Late Helladic I/II A border bridge-spouted jug, Akrotiri West House, Thera Museum 5115 (after MARTHARI 1990, fig. 8)

small spirals, can be classed as Minoan and probably from the same Knossian workshop; it comes from the tomb excavated by L. Quilici at Ayia Irini: Paleokastro, stratum V (the second earliest, including also Proto White Slip and White Slip I) (Fig. 1F).[12]

At exactly the same level as the slabs which probably indicate a floor level, only four metres away to the south-east, was a very clear plaster floor, over which were again fragmentary LM IA vases. In addition to a plant style bowl and a bowl with ripple decoration there were three more Vapheio cups from the Small Spirals Workshop, an excellent link to the piece near the slabs at the same level (Fig. 1B–D).[13]

In terms of the Minoan sequence we thus have here a mature LM IA vase deposit, followed by later LM IA material, followed by even later LM IA vases with the slabs and the plaster floor. What is of special interest is that another Vapheio cup from the Small Spirals Workshop comes from Akrotiri, from the destruction debris of the West House, published by Marisa Marthari (Fig. 1E).[14] On the basis of the Knossian evidence just outlined it is reasonable to conclude (a) that the Small Spirals Workshop producing Vapheio cups is a distinct feature of very late LM IA, (b) that the Akrotiri destruction, which briefly preceded the eruption (as new work at Akrotiri is making clear), can be closely correlated with this very late LM IA in Crete, confirming the views of Betancourt and Soles.[15] It may in turn be speculated that the Stratigraphical Museum deposit and other deposits with Small Spirals Vapheio cups were the result of the same earthquake as that which destroyed Akrotiri shortly before the eruption.

I say very late LM IA because Greek Mainland evidence suggests there might have been a final phase of LM IA after the eruption. This evidence is the combination of two factors, (a) the presence of a Mainland bridge-spouted jug in the Theran destruction, again from the West House, datable to late LH I or the LH I/LH IIA border (Pl. 1B),[16] and (b) the presence of an LH II A import in an LM IA context at Palaikastro in Crete (Fig. 1G)[17]. With the Theran destruction, in Mainland terms, no later than the LH I/LH IIA border, the clearly LH IIA jug at LM IA Palaikastro suggests a little more time for that period, at least in east Crete.

This paper attempts to link more precisely the relative chronology of Minoan Crete to the Thera eruption. For this the new pumice evidence is neutral, the ceramic evidence positive. As for absolute dates, we can state that the LH I/IIA border, and so too the Thera eruption, very late LM IA and Late Cypriote IA:2 (the White Slip I bowl from Akrotiri[18] and the association at Ayia Irini:Paleokastro Quilici tomb stratum V – see above), date to after 1540 BC; this is because there is an Egyptian XVIIIth Dynasty (began 1540 BC) stone vase from Mycenae Shaft Grave IV, LH I (contemporary with Late Cycladic I Thera and largely contemporary with LM IA), and another from Akrotiri itself.[19] The date of the LH I/IIA border, the eruption etc may be set at 1530 BC or very soon afterwards (about the middle of the reign of Ahmose). For the slightly later point of earliest LH IIA and final LM IA (see above) the date is set at about 1520/1510 BC.[20] LM IB thus begins at this point. In full sequential accord with this absolute dating is the fact that both LM IB and LH IIA have links with the early XVIIIth Dynasty, down through the first half of the reign of Tuthmosis III (LH IIA) and through most of the reign (LM IB).[21]

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[12] Quilici 1990, 86–87 no. 228, figs. 220–221, 316a no. 228. Eriksson 2003, 425–426. I am grateful to Kathryn Eriksson for discussion of this vase and for photocopies from Quilici's volume.

[13] Warren 1999, 899–900 and pl. CCVII P442, P442a, P443.

[14] Marthari 1990, 62 and fig. 4b. Marthari 1993, pl. 40 no. 3191. I thank Dr Mariza Marthari for her kind permission to make a drawing, inked by Susan Grice, from her photograph (*ibid.*) and to copy her photograph for Pl. 1B.

[15] Betancourt 1985, 133. Soles, Taylor and Vitaliano 1995, 391.

[16] Marthari 1990, 64 and fig. 8 (Thera Museum 5115). Warren 1999, 894. The short spout occurs on LH I and LH IIA bridge-spouted jugs (Mountjoy 1999, 82 and fig. 10 Argolid 11 (LH I), 90 and fig. 13 Argolid 38 and 40 (LH IIA)), suggesting that the canonical longer spout of LH II A bridge-spouted jars may not yet have evolved. The particular form of dotted cross motif on the Thera jug is paralleled in LH I (Mountjoy 1999, 893 and fig. 363 Melos 1).

[17] Bosanquet 1902–3, 284–287 and fig. 5 (House B Room 10, lower (LM I A) level). Close parallels Blegen 1937, 401–402 and pl. V no. 375 (Prosymna Tomb XXV); Mountjoy 1999, 872–873 and fig. 350 Kea 25 (Kea VIIb). Compare also Keramopoullos 1910, 209 and pl. 9, 2 and Mountjoy 1999, 651 and fig. 247 Boeotia 13.

[18] Merrillees 2001. Wiener 2001.

[19] Warren 2006, 303–319.

[20] See n. 19.

[21] See n. 19.

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Section "Mycenaeans and Philistines in the Levant" Introduction

Sigrid Deger-Jalkotzy[*]

The Research Program "Synchronisation of Civilisations in the Eastern Mediterranean in the Second Millennium B.C." (SCIEM 2000) of the Austrian Academy of Sciences also includes a project named "End of the Mycenaean Culture". It concentrates on the chronology and synchronisms of the period which in the Aegean followed after the collapse of the Mycenaean palaces. In archaeological terms this period of the 12th and early 11th centuries B.C. is called LH III C. The schedule of the "End of the Mycenaean Culture" project also comprises several international workshops on the chronology of LH III C, as well as on synchronisms of the cultural developments and the history of the various regions of the Aegean.

For the year of 2003 a workshop had been planned on the character, origin and chronological setting of the Aegean impact on the cultures of Cyprus and the Levant in the 12th century B.C. Like the Greek mainland and the Aegean islands these regions had been afflicted by destruction and abandonment of sites around the turn of the 13th to the 12th century B.C. In Cyprus the subsequent cultural phase of Late Cypriote (LC) III A was marked by various novelties which exhibited a distinct Aegean character. This is particularly true of LC III A pottery styles. Civilisations of the 12th century B.C. in the Levant and the Philistine culture in particular also seem to have been influenced by elements that were of Mycenaean or Aegean origin. Whether or not population movements from the Aegean had been responsible for the introduction of these novel cultural features in Cyprus and the Levant has been the subject of a longstanding and controversial scholarly discussion.

It soon became clear to Manfred Bietak and myself that many scholars figuring on the invitation list for the 2003 "End of the Mycenaean Culture" workshop on cultural and chronological correlations between the Aegean, Cyprus and the Levant 12th century B.C. had also been asked to participate at the 2003 EuroConference of SCIEM 2000. We therefore agreed that the two events be combined. In place of the workshop planned for the "End of the Mycenaean Culture" project a separate section of the EuroConference was dedicated to the subject of "Mycenaeans and Philistines in the Levant".[1] This section brought together 21 participants and may thus be regarded as a workshop in its own right.[2]

The *final session* of the "Mycenaean and Philistines" section was dedicated to a lengthy discussion of questions which had been brought up during the conference and appeared to require a closer introspection. Particular attention was paid to the following issues:

The nature of the Philistine culture

It was generally agreed that the Philistine civilisation integrated Cypriot, Cretan and Mycenaean elements, as well as the heritage of indigenous cultures. It was also marked by Egyptian influence. However, the participants of the "Mycenaean and Philistines" section found it difficult to characterise the multifarious nature of the Philistine culture by means of a fitting terminology. Descriptions such as "hybrid", "eclectic" or "cultural agglomeration" were put forward. None, however, appeared as satisfactory.

1. The origins of the Philistine culture

Most scholars agree that the Philistine settlement and Philistine culture were the result of a migration process (or migration processes). However, a vivid discussion on the nature, origin and chronology of the Philistine migration(s) still continues. According to some scholars the immigration took place by sea, others think in terms of immigration by land, e.g.

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[1] I would like to express my thanks to Professor Bietak for accepting this idea, and to Dr. Angela Schwab for her substantial help with the organisation of the "Mycenaean and Philistines" section.

[2] Sincere thanks are due Dr. Ernst Czerny for taking care of the edition of the proceedings.

from Anatolia. The discussion at Vienna did not yield any new aspects on this point. It was, however, agreed that the Philistine migration should be viewed in terms of immigration(s) rather than of a large-scale invasion. The immigrants need not necessarily have come in great numbers.

On the controversy relating to the chronology of Philistine immigration in the southern coastal plain of Canaan see below, 4.3.

2. Pottery styles and pottery chronology

2.1.

E. B. French's analysis of the most recent stratigraphic evidence from Mycenae and the Greek mainland, and P. A. Mountjoy's assessment of the correlation of this evidence to the pottery chronology of Cyprus have made it clear that the Aegean impact on the pottery of Cyprus and the Levant has to be dated to LH III C Early. The term "LH III C 1: b" therefore should be abandoned in connection with Cypriote and Philistine pottery, particularly since it has also become obsolete with the classification of LH III C Aegean pottery.

2.2.

According to P. A. Mountjoy the Mycenaean impact on the pottery styles of the Levant not only emanated from the Greek mainland. The Dodecanese (Rhodes!), too, may have contributed to the pottery styles of the 12th century B.C. in the Eastern Mediterranean. This aspect may add a further facet to the question of the origin of "Aegeanising" pottery painting in Cyprus and the Levant.

2.3.

Several participants expressed the opinion that the term of "monochrome" or "Philistine monochrome" was inadequate for designating a class of pottery derived from Aegean prototypes but locally produced in the Levant. The term had once been created in order to differentiate the brown-on-light patterned decoration of this pottery class from the bi-chrome (red-and-dark) decorative scheme of the "classical" Philistine pottery. However, specialists of Aegean and Cypriote pottery refer the word "monochrome" to surfaces which are completely coated with paint. At the Vienna meeting it therefore was suggested that "Philistine monochrome" be replaced by a term that takes into account the Aegean background of this pottery rather than its colour scheme. Descriptions such as "locally produced Mycenaean III C-derived" or "Mycenaean III C 1: b locally made" were rejected since the origins of this pottery cannot be exclusively related to the Greek mainland, and in any case the term of "Mycenaean III C 1: b" has become obsolete (see above). However, alternative suggestions such as "Local Aegean III C", "Local Aegeanising III C" or "Aegeanising pottery" did not prevail either. The discussion therefore will continue.

2.4. Reports on new sites such as Tell Kazel in Syria have drawn attention to the fact that Philistine pottery may not have been the only Levantine style that had its origins in the Aegean. The pottery of Tell Kazel, showing no similarities with Cypriot pottery, seems to have been directly influenced by LH III C Early pottery of the Greek mainland.

2.5. It was pointed out that local elements, too, contributed to the many components that made up the Philistine pottery style. Apart from Canaanite traditions, particular mention was made of the so-called "Simple Style", a local derivation of 13th century Mycenaean pottery.

3. Regionalism

Several speakers felt a need for explaining the fact that "monochrome" Philistine pottery (cf. above, 2.3.) has been found at Tel Miqne/Ekron, Ashdod and Ashkelon but not at Lakhish and Tel Sera^{c}. It was only during the subsequent phase that the "classical" bi-chrome Philistine pottery style found wider distribution within and slightly beyond the territory of the Pentapolis. According to A. Mazar the limited geographical distribution of the Aegeanising "monochrome" pottery was a manifestation of Philistine immigration, demonstrating the initially isolated situation of the newcomers within an environment marked by Canaanite civilisation and Egyptian influence. Referring to the instance of Tell Kazel (cf. above 2.4.), some speakers furthermore promoted the view that the limited distribution of the early phenomena of Philistine culture could have resulted from a co-existence of various population groups in the Levant who may well have come from different parts of the Aegean or the Eastern Mediterranean. However, it is admittedly difficult to visualize a marked cultural regionalism prevailing in a territory as limited as the Philistia (cf. also further below, 4.3.).

4. Chronology

4.1.

Mycenaean/Aegean chronology. As has been already mentioned (cf. above, 2.1.), the Aegeanising styles of Cyprus and the Levant were inspired by Aegean pottery of the phase LH III C Early. This phase therefore represents a *terminus ad quem vel post quem* for the emergence of Late Cypriot III A and "Aegeanis-

ing" pottery styles in the Levant such as "monochrome" Philistine pottery.

4.2.

Evidence of written sources also has to be considered. According to a letter written by Beya, vizier of the pharaoh Siptah and Queen Tausret, to Hammurapi, last king of Ugarit, this town still existed between the years of 1194 and 1188, if not 1186. Thus the chronology of the destruction of Ugarit was closer to the 8th year of the reign of Ramesses III than scholars had assumed in earlier days.

4.3.

The chronology of Philistine settlement in Canaan

The controversy on the "low" chronology advocated by I. Finkelstein and D. Ussishkin against T. Dothan and A. Mazar inevitably also affected the discussions at the Vienna conference. Starting from the limited geographical distribution of Philistine "monochrome" pottery (cf. above, 2.3), Ussishkin and Finkelstein have come to the conclusion that this pottery class did not appear before the emergence of the bi-chrome Philistine style, and that both pottery classes were introduced together at the time when the Philistines settled in Canaan. Moreover, it is claimed this event did not take place during the reign of Ramesses III but around 1140/30 B.C., after the withdrawal of Egyptian administration from southern Palestine. In contrast, T. Dothan and P.A. Mountjoy maintained that at Tel Miqne/Ekron great quantities of "monochrome" pottery were found in Stratum VII without any admixture of bi-chrome Philistine pottery, and that the latter only appeared during a subsequent of the settlement. Moreover, D. Ussishkin's attempt to synchronise Strata VII and VI of Tel Miqne was refuted by the excavators of the site. As might have been expected, the issue was discussed fiercely, while no rapprochement was reached between the contradicting parties.

Sinda and the Absolute Chronology of Late Cypriote IIIA

Paul Åström

The absolute chronology for the end of Mycenaean IIIB – 1230 B.C. – proposed by Furumark in 1941 was based on the circumstance that pottery of that date had not been found in a context later than the reign of Ramses II.[1] This date must now be lowered, since Mycenaean IIIB pottery was found at Deir Allah associated with a faience vase inscribed with the name of Queen Tausret /Tewosret who reigned 1188–1186.[2]

Another confirmation of a low date is the fact that Pharaoh Siptah (1194–1188) is mentioned among the documents from Ras Shamra.[3] "There is evidence of destruction and fire throughout the city".[4] Furumark emphasized to me more than once that he considered the latest pottery found at Ugarit to be not later than Mycenaean IIIB; hence that style lasted down to the reign of that pharaoh.

A lowering of Furumark's date for Mycenaean IIIB has also been made by others, e.g. Iakovides[5] and Hankey and Warren. The latter suggest 1185/1180 using evidence from Kition.[6] We are then very close to the eighth year of Ramses III who reigned 1184–1153 according to Kitchen.[7]

It follows that the absolute dates assigned by Furumark to the different periods at Sinda must be lowered. Period I, in which Mycenaean IIIB pottery was found, should thus end *c.* 1190/1180 B.C. It is impossible to know exactly how long Period II (= Late Cypriote IIIA1), characterized by Furumark's Mycenaean IIIC:1a and III C:1b (1), lasted. Furumark assumed 40 years (1230–1150). In the new scheme the duration of the period may have been from 1190/1180 to 1150/1140.

If we trust – and some do not[8] – that the Sea Peoples after having destroyed Hattusha, Kode, Carchemish, Arzawa and Alasia were defeated by Ramses III in his eighth year we may have traces of these events in Alasia assuming that Cyprus is meant with that name. Should we attribute one of the destruction layers at Sinda to the Sea Peoples and in that case which one? Furumark assumed that the second catastrophe was due to an attack from the Sea Peoples. Should we hypothesize that the first destruction, which we now date to 1190/1180 B.C., was caused by the Sea Peoples? In that case who destroyed Sinda the second time?

I would like to emphasize that these suggestions are hypothetical and it also involves the equation of Alasia with Cyprus. Personally I believe that pirates and adventurers consisting of Mycenaeans and groups from other areas are responsible for the destructions. We may also have to reckon with wars between the Cypriotes themselves, as the Cypro-Minoan signs on the lead sling bullets from Hala Sultan Tekke suggest.

The duration of Sinda Period III (= Late Cypriote

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[1] A. Furumark, *The chronology of Mycenaean pottery*, Stockholm 1941, 115 and 117.

[2] H.J. Franken, The excavations at Deir ᶜAlla in Jordan, *Vetus Testamentum* 11, 1961, 362–372; *idem*, *Excavations at Tell Deir ᶜAlla. The Late Bronze Age sanctuary*, Louvain 1992, 31; V. Hankey, Mycenaean pottery in the Middle East: notes on finds since 1951, *BSA* 62, 1967, 131–134 (Furumark accepted in a letter to me the Mycenaean IIIB date for *fig. 5* and classified the vases on *fig. 6* as "Mercenary" style); K.A. Kitchen, The basics of Egyptian chronology in relation to the Bronze Age, in: P. Åström (ed.), *High, Middle or Low?*, SIMA PB 56, Gothenburg 1987, 52.

[3] M. Yon, The end of the Kingdom of Ugarit, in: *Crisis years*, 120; K.A. Kitchen, The basics of Egyptian chronology in relation to the Bronze Age, in: P. Åström (ed.), *High, Middle or Low?*, SIMA PB 56, Gothenburg 1987, 52. There are now very few transitional IIIB/IIIC1 sherds from Ras Shamra suggesting that Mycenaean IIIC1 started somewhat before the destruction of the city at about 1185 B,C. according to M. Yon in: Yon *et al.*, *Céramiques mycéniennes* (Ras Shamra–Ougarit XIII), Paris 2000, 18.

[4] Yon, *op.cit.*, 117.

[5] S. Iakovides, The chronology of LH IIIC, *AJA* 83, 1979, 454–462 (1190/85–1170/65 for IIIC:1a).

[6] P. Warren and V. Hankey, *Aegean Bronze Age chronology*, Bristol 1989, 159–162.

[7] Kitchen, *op.cit.*, 52.

[8] E.g. L.H. Lesko, Egypt in the 12th century B.C., in: *Crisis years*, 151–154.

IIIA2) is difficult to assess. If we equate the Mycenaean IIIC:1b (2) ware, which is characteristic of Sinda Period III, with Mycenaean IIIC Middle, we may obtain some dates from Greece. Hankey and Warren suggest two generations or about 50 years for IIIC Middle: 1150/1140–1100/1090 B.C.[9] Furumark suggested 40 years for Sinda Period III. This would now mean 1150/1140–1110/1100.

The dates are applicable on other sites in Cyprus which show developments similar to that of Sinda, e.g. Enkomi, Sinda and Maa, *Palaeokastro*.

The Proto White Painted Ware appears after Sinda Period III. This is in agreement with Maria Iacovou's opinion that the ware is derivative of LH IIIC Middle and postdates it "making it largely contemporary with LH IIIC Late".[10]

These new considerations of absolute dates for Late Cypriote III will give fuel for the discussion of the radical low chronology for Palestine proposed by Finkelstein[11] and Ussishkin.[12] There are links between the pottery from Sinda Period III and the Philistine pottery[13] and the new, low dates proposed here for Sinda effect Palestinian dates. It may be assumed that some of the inhabitants fled to Palestine, where the Philistine pottery betrays influence from Sinda.[14]

If we adjust Furumark's absolute dates in the light of new discoveries which make a revision necessary, keeping his approximate, relative dates for the Sinda Periods, the following Table 1 emerges.

It should also be mentioned that Sinda was abandoned in the Late Cypriote IIIB period, when Proto White Painted ware appears.

Period	LC	Absolute dates	Pottery styles (Furumark)	Greece
Period I	LC IIC2	c. 1220–1190	Mycenaean IIIB	IIIB2
Period II	LC IIIA1	c. 1190–1150	Mycenaean IIIC:1a	IIIC Early
			Mycenaean IIIC:1b (1)	
Period III	LCIIIA2	c. 1150–1110	Mycenaean IIIC:1b (2)	IIIC Middle

Table 1

[9] Warren and Hankey, *op.cit.* 168.

[10] *Mediterranean peoples*, 337.

[11] E.g. I. Finkelstein, Philistine chronology: High, Middle or Low?, in: *Mediterranean peoples*, 140–147.

[12] D. Ussishkin, The destruction of Megiddo at the end of the Late Bronze Age and its historical significance, in: *Mediterranean peoples*, 197–219.

[13] T. Dothan, *The Philistines and their material culture*, New Haven, London, Jerusalem 1982, e.g. 109, 115, 154, 217, 310.

[14] See the preceding note. Charles Adelman remarks that the later Close Style pottery at Sinda is so different from the earlier and so similar in many ways (but for the Bichrome) to the Philistine that it is possible that influences for it came from the East as well. I have pointed out some imports and influences from the Phoenician area to Cyprus, even suggesting movement of people from that area to Cyprus, in: *Mediterranean peoples*, 82 and 84.

[15] Down to (?) the beginning of Mycenaean IIIC Early or transitional IIIB/IIIC Early.

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Coexistence and Impermeability: Egyptians and Philistines in Southern Canaan During the Twelfth Century BCE

Tristan J. Barako[*]

I. Introduction[1]

In the absence of chronological anchors for the twelfth century BCE in southern Canaan, scholars have often relied on two interrelated, geopolitical events that affected the region at this time: first, the migration and settlement of the Philistines; and, second, the withdrawal of the Egyptian hegemonic presence. According to the traditional paradigm, the Philistines wrested control of southern coastal Canaan from Egypt ca. 1175 BCE. Recent revisionist theories, however, hold that the Philistines arrived after the Egyptian withdrawal from the region at about 1130 BCE. At the heart of the matter lie the following questions: Did the Philistines and Egyptians coexist and, if they did, were their material culture boundaries impermeable? An examination of sites that possess either Philistine or Egyptianized material culture may provide an answer. An analysis of the excavations at Tel Mor, a small Egyptian outpost located close to the Philistine city of Ashdod, is an especially important case study in this regard.

The date of the Philistine settlement is determined largely by Egyptian texts. According to the notice at Medinet Habu, Ramesses III repelled the Sea Peoples (including the Philistines) in the eighth year of his reign, which corresponds to 1174 BCE following the Low Egyptian Chronology (Wente and van Siclen 1977).[2] Papyrus Harris I reports that they were then settled in strongholds (76.7–8), generally assumed to be in southern coastal Canaan, which was the location of the Philistine Pentapolis according to the Hebrew Bible (e.g., Josh 13:3; I Sam 6:3–4, 17). Excavations in this region produced an intrusive material culture with affinities to the Philistines' presumed area of origin – namely, the Mycenaean cultural ambit. More importantly, this material culture appeared suddenly in strata dated to the early twelfth century BCE. Thus, the traditional paradigm for the chronology of the Philistine settlement was formed.

In recent years, Israel Finkelstein and David Ussishkin have argued that the date of the Philistine settlement should be lowered by roughly fifty years – that is, to about 1130 BCE.[3] Briefly described, their argument is as follows: Because certain sites located near the Philistine Pentapolis contain strata that are clearly datable to the reign of Ramesses III, but have not yielded Philistine pottery, this type of pottery found at sites elsewhere must have been produced for the first time after the destruction of these strata. The two key sites mentioned in this regard are Lachish Stratum VI and Tel Sera[c] Stratum IX. In both these strata, Egyptian inscriptions dating to Ramesses III were found (Gilula 1976; Goldwasser 1982; 1984) and Philistine pottery was absent. On the other hand, neighboring sites thought to be contemporaneous, particularly Tel Haror Strata B3–2, did produce both Philistine Monochrome and Bichrome pottery (Oren 1993:582–583).

The underlying assumption of this argument is that cultural boundaries must be permeable for all types of material culture. A corollary holds that two neighboring sites that do not possess the same full range of material culture cannot be contemporaneous. Amihai Mazar, however, has adduced examples from the archaeology of Syria-Palestine to demonstrate that different material cultures have coexisted side by side with little or no interaction (1997:158; see also Ben-Tor and Ben-Ami 1998:31). Furthermore, as pointed out by S. Bunimovitz and A. Faust (2001), even intensive interaction between cultures

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[1] I would like to thank Manfred Bietak for inviting me to participate in the second EuroConference of SCIEM 2000 and Sigrid Deger-Jalkotzy for organizing the Mycenaean and Sea Peoples section.

[2] Cf. Kitchen, however, who assigns the dates 1173–1142 BCE for Ramesses III's reign (2000:table 3).

[3] See especially Finkelstein 1995 and, most recently, 2000. Ussishkin (1985) was the first to propose the "Low Chronology" based on his excavations at Lachish; however, Finkelstein has been, by far, the more vocal advocate for chronological revision.

does not necessarily lead to material culture exchange. Indeed, in order to maintain group identity and strengthen solidarity during times of conflict with a neighboring group, people tend not to acquire items emblematic of their rivals. A comparison of the material culture assemblages belonging to the Philistine city at Ashdod and the nearby Egyptian garrison at Tel Mor during the twelfth century BCE further undermines the proposed Low Chronology for the Philistine settlement.

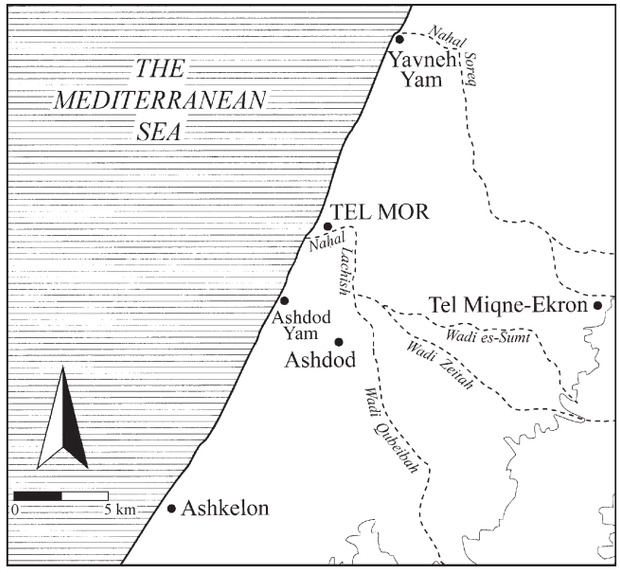


Fig. 1 Map of the southern Levantine coastal region (adapted from T. Dothan 2000:fig. 7.1)

II. Case Study: Philistine Ashdod and Egyptian Tel Mor

A. Ashdod

Tel Ashdod is a large mound located a few kilometers inland along the southern Levantine coast (Fig. 1). According to the excavator Moshe Dothan, the large fortified building excavated in Area G, which was in use throughout the Late Bronze Age (= Strata XVI–XIV), served as an Egyptian-style "Governor's Residence" or palace (Dothan and Porath 1993:10, 39–49, plan 7). With this "Residence" the excavators associated an inscribed stone fragment, possibly from a doorjamb, which was found in a later fill context (= Stratum XIIB) from the same area.[4] The inscription reads "Fanbearer on the Kings' Right Hand" (*t3i hw* [*hr*] *wnmy n nsw*), an honorific accorded only to high officials in the pharaonic court (Kitchen 1993). Egyptianized bowls (Dothan and Porath 1993:45, figs. 10.1, 11.1–6, 8–12), a beer jar (fig. 11.24), and an alabaster vessel (fig. 12.15) were also found in Area G, Stratum XIV.[5] Mycenaean and Cypriote imports still arrived at Ashdod before the Stratum XIV Canaanite city was largely destroyed at the end of the thirteenth century BCE (e.g., Dothan and Porath 1993:48–49, fig. 12.2–4).

More than anything else, the initial appearance of locally-produced, Mycenaean IIIC:1b (or Philistine Monochrome) in large quantity characterizes the rebuilt settlement in Stratum XIII (e.g., Dothan and Porath 1993:fig. 14). In this same stratum, Mycenaean and Cypriote imports and Egyptianized pottery are absent. Dothan correlated Stratum XIII and Philistine Monochrome pottery with an early wave of Philistines prior to Year 8 of Ramesses III's reign. He associated Philistine Bichrome pottery, which first appears in abundance in Stratum XII, with a second wave after Year 8. The gradual shift from Monochrome to Bichrome pottery in tight stratigraphic sequence at two other Philistine Pentapolis sites, Tel Miqne-Ekron and Ashkelon, however, indicates otherwise – namely, stylistic change over time within a single population group (Mazar 1985:102–107; Singer 1985:112; Stager 1985:62). Therefore, the appearance of Monochrome marks the arrival of the Philistines during the reign of Ramesses III, and Bichrome corresponds to the second generation of Philistine ceramic production. To sum up, the stratigraphy and associated material culture changes at Ashdod may be broadly tabulated as follows.

Stratum	*Philistine pottery*	*Mycenaean and Cypriote Imports*	*Egyptianized pottery*
XIV	absent	present	present in small amount
XIII	present in large amount (Monochrome)	absent	absent
XII	present in large amount (Bichrome)	absent	absent

Table 1

[4] Note also that a surface survey in the area between Ashdod and Tel Mor produced a fragment of a monumental statue of a queen of Ramesses II (Schulman 1993).

[5] At Late Bronze Age Ashdod, Egyptianized bowls were found also in Areas A (M. Dothan 1971:fig. 1.1), B (Dothan and Freedman 1967:fig. 22.1–4), and H (M. Dothan 1971:fig. 81.3). Another beer jar was found in Area H (fig. 81.14).

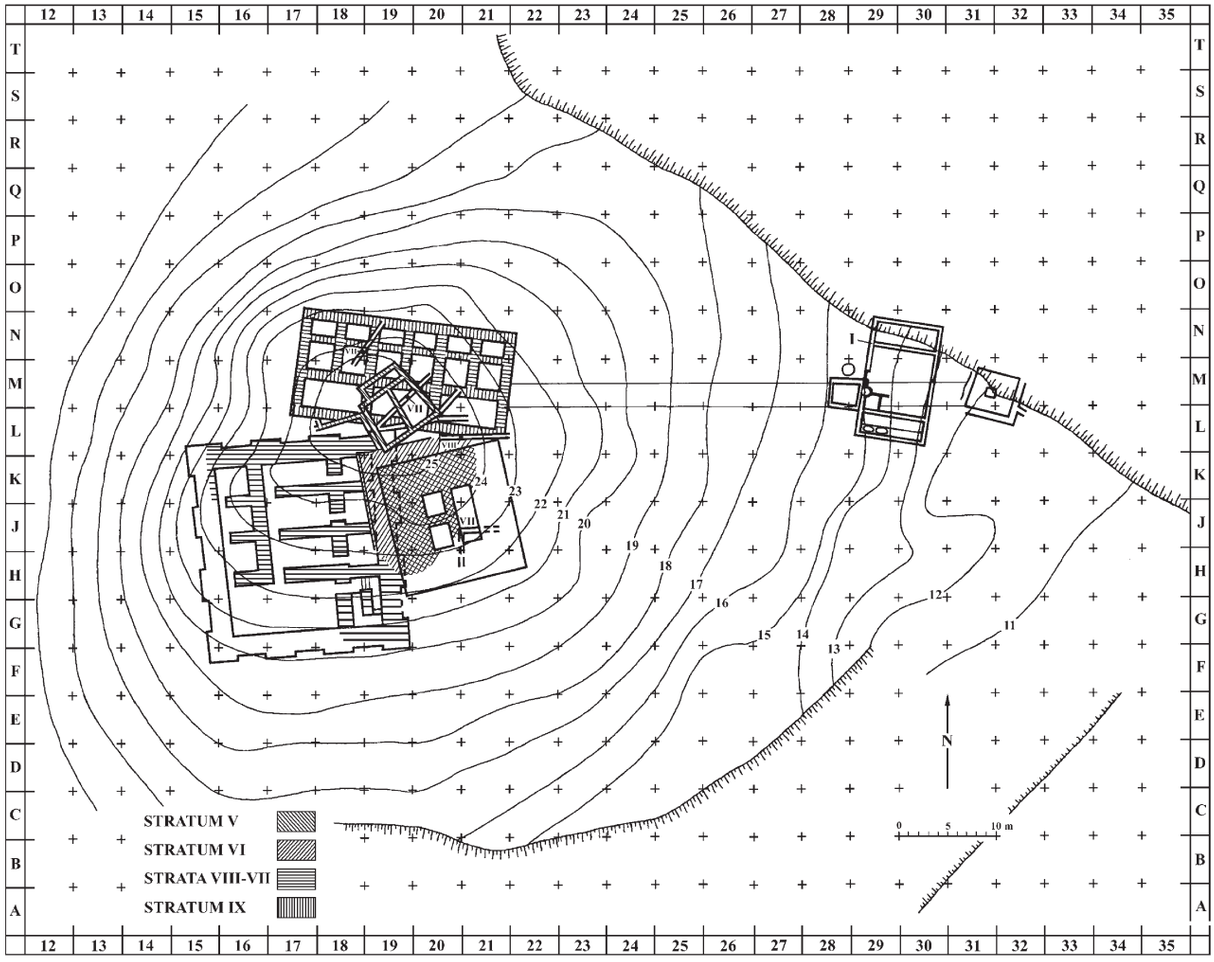


Fig. 2 Topographic plan of Tel Mor (drawing by I. Dunayevsky)

B. Tel Mor

Moshe Dothan also directed excavations at Tel Mor, a small site located only six km northwest of Tel Ashdod (Fig. 1).[6] By virtue of its location along the Naḥal Lachish approximately one km from the Mediterranean Sea, Tel Mor offered a rare protected anchorage along the southern Levantine coast. The base of the mound covers an area of six dunams, whereas the summit, which has been reduced considerably by erosion, encompasses only one dunam (Fig. 2). During the thirteenth century BCE (= Strata VIII–VII), a large Egyptian-style building dominated the summit (for an isometric reconstruction, see M. Dothan 1993:1073). Egyptianized pottery, mostly shallow bowls and beer jars, accounted for nearly 10% of the registered sherds from Stratum VII (Martin and Barako in press). Toward the end of the thirteenth century BCE, Stratum VII ended in a violent and thorough destruction. A layer of ash and burnt mud brick, in places a meter and a half thick, covered the entire Egyptian-style building. A thin layer of windblown sand over this destruction indicates a subsequent period of abandonment.

In Stratum VI a much smaller *migdol*-type fortress (= Building F) was constructed above and

[6] Results of these excavations appear only in preliminary reports and encyclopedia articles (e.g., M. Dothan 1960; 1993). Through generous funding from the Shelby White - Leon Levy Program for Archaeological Publications, the present author will publish the excavations at Tel Mor in a forthcoming volume of the Israel Antiquities Authority Monograph Series.

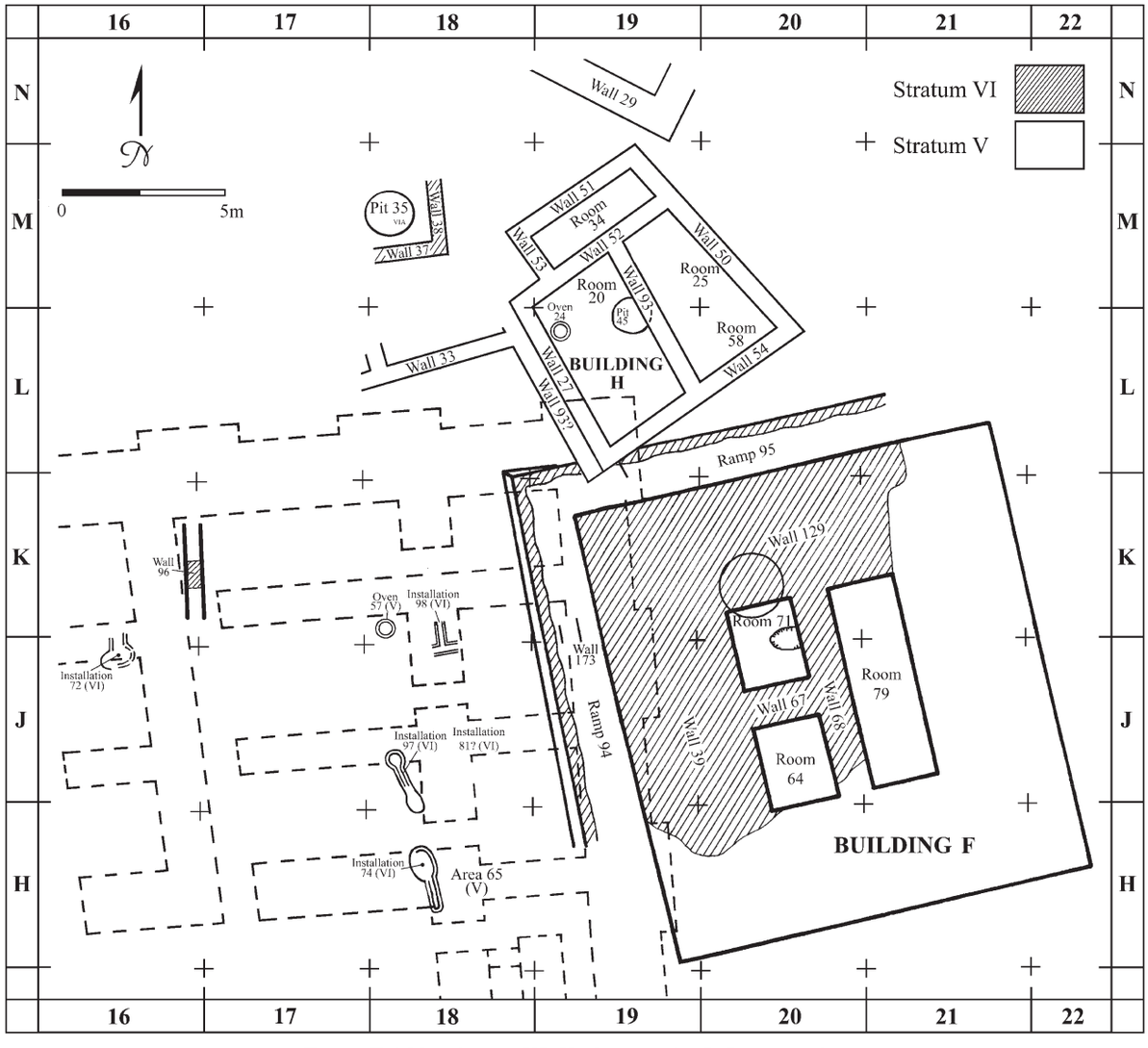


Fig. 3 Tel Mor, Strata VI–V (drawing by I. Dunayevsky)

partially over the larger building of Stratum VII (Fig. 3). In terms of pottery types present, there are no major differences between the assemblages of Strata VI and VII. The amount of Egyptianized pottery in Stratum VI, however, nearly doubled from 9 to 15% of the total number of registered sherds. Imported Cypriote pottery continues to appear in this stratum.[7] Notable in this regard are a couple late White Slip II bowls (= Type IDß) found in Room 71 of Building F (Fig. 4). Sloppily drawn, parallel line decoration characterizes the type, which belongs to the end of the White Slip ware sequence (Popham 1972:466, figs. 57.3–5, LXXXVI:2–3; Gittlen 1977:473–474, Pl. 18). These bowls appears in Canaan primarily in the Late Bronze Age IIB at sites such as Aphek in Stratum X-12 (Beck and Kochavi 1985:36, fig. 2.6) and Ashdod in Strata XV (Dothan and Freedman 1967:80, fig. 21.8) and XIV (82–83, fig. 23.14, Pl. XIII:2).

Stratum VI ended in destruction. Numerous whole or almost whole vessels lay smashed on the floors of Building F, particularly in Room 71. On top of these vessels were fallen mud bricks and then more broken pots, which, taken altogether, indicates a sec-

[7] The author is grateful to Celia Bergoffen, who, based on an examination of drawings and photographs, made many valuable suggestions concerning the White Slip and Base Ring ware found at Tel Mor.

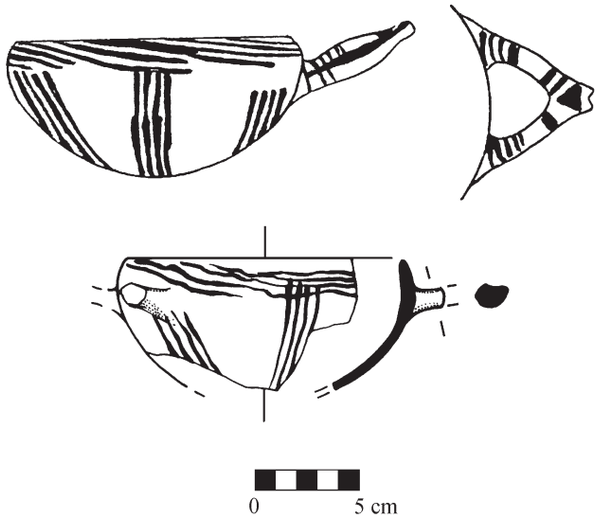


Fig. 4 Late White Slip II bowls from Tel Mor, Stratum VI (drawing by J. Rudman)

ond story collapse. In Stratum V, Building F was rebuilt, and a smaller building (H) was constructed to the north of it (Fig. 3). Egyptianized pottery comprised about 12% of the registered sherds from this stratum. Noteworthy is an intact globular cooking jar found in Room 34 of Building H (Fig. 5). It is probably a Nile B Egyptian import that dates to the period of the Twentieth Dynasty (David Aston, pers. comm.). Egyptian imports either made of Nile clay or used for cooking are rarely found at sites in Canaan.[8]

With the exception of small, probably residual sherds, neither Cypriote nor Mycenaean pottery was found in Stratum V. It appears that Stratum V, including Building F, simply fell out of use. In the succeeding four strata, the character of the site changed considerably. A single massive structure no longer dominated the tell. Instead, the settlement became more open with relatively little architecture. Stratum IV, for example, consists only of a poorly preserved building, the walls of which roughly follow the outline of Building H from the preceding stratum. Virtually no Egyptianized pottery was found in Stratum IV or any subsequent strata. For the first time, however, small amounts of Philistine Bichrome pottery appeared in Stratum IV. The following Stratum III, which is comprised mostly of pits, contained even more Bichrome. Altogether about 70 sherds and whole vessels that may be described as "Philistine" were found at Tel Mor. Most of the Philistine pottery



Fig. 5 Egyptian globular cooking jar from Tel Mor, Stratum V (photograph courtesy of the Israel Antiquities Authority)

[8] The author wishes to thank Mario Martin, who provided much of the information regarding the Egyptianized and imported Egyptian pottery at Tel Mor.

Stratum	*Philistine pottery*	*Mycenaean and Cypriote Imports*	*Egyptianized pottery*
VII	absent	present in small amount	9% of assemblage
VI	absent	present in small amount	15% of assemblage
V	absent	absent	12% of assemblage
IV	present in small amount (Bichrome)	absent	absent
III	present in small amount (Bichrome)	absent	absent

Table 2

is the familiar Bichrome with smaller amounts of "Aegean-style" cooking jugs and "Ashdod Ware" present (Barako forthcoming). Surprisingly, no Philistine Monochrome pottery was found in this or any other stratum at Tel Mor. The stratigraphy and associated material culture changes at Tel Mor may be tabulated, then, as in Table 2.

The critical strata at Tel Mor are V and IV, wherein, respectively, Egyptianized pottery last appeared and Philistine pottery first appeared. Based on the initial appearance of Philistine Bichrome, the stratigraphic sequences of Tel Mor and Ashdod match up as in follows:

	Ashdod	*Tel Mor*
Stratum	XIII	V
Pottery	Philistine Monochrome	Egyptianized
Strata	XII–XI	IV–III
Pottery	Philistine Bichrome	Philistine Bichrome

Essential to the question of coexistence and impermeability are Ashdod XIII and Tel Mor V, both dated by Moshe Dothan to the first half of the twelfth century BCE. Ashdod XIII produced significant amounts of Philistine Monochrome but no Egyptianized pottery; whereas, conversely, the ceramic assemblage of Tel Mor Stratum V was comprised of approximately 12% Egyptianized pottery but no Philistine Monochrome.

III. Synthesis

For some Syro-Palestinian archaeologists, this marked patterning of Egyptianized and Philistine material culture is best explained by chronological revision.[9] The evidence from Tel Mor shows, however, that two propinquitous sites can, indeed, be both contemporaneous *and* possess different material culture assemblages. A more convincing explanation holds that in response to the Philistines' carving out their homeland from the southern coastal plain, the Egyptian administrative and military system in Canaan adopted a policy of containment. Or, as M. Bietak (1993) and L. Stager (1995:342–344) have argued, a *cordon sanitaire* was created, whereby Egypt strengthened its outposts on the periphery of Philistia. As part of this policy, the Egyptians refortified Tel Mor to counter the establishment of the nearby Pentapolis site of Ashdod along the northern boundary of Philistia.

Tel Mor was of added strategic importance to the Egyptians on account of its protected anchorage along a mostly harbor-less coast. Although Tel Mor was undoubtedly also a point of entry to the interior, it was probably bypassed by many of those traveling overland along the Via Maris, which stretched a few kilometers to the east, beyond the sand dunes. In any event, the garrison (Egyptian *iw'yt*) at Tel Mor could have provisioned troops travelling by land or sea along the coast, similar to how the harbors in Lebanon supplied Thutmose III's forces during his seventh Asiatic campaign (ARE II §472). The strength of the garrison stationed at Tel Mor need not have exceeded 50 soldiers. The Amarna tablets record requests made by Canaanite mayors to Pharaoh that a contingent of this approximate number be sent to defend their respective towns (EA 108:67, 238:11, 289:42). A petty officer probably commanded the small garrison at Tel Mor, which might have been visited periodically by a circuit official along the lines of Ḫaya mentioned in the Aphek Letter (Owen 1981; Singer 1983).

The reign of Ramesses III, the greatest pharaoh of the Twentieth Dynasty, offers the best geopolitical context for the refortification of Tel Mor in Strata VI–V. At this time a reinvigorated Egypt attempted to reassert hegemony over southern Canaan. The numerous Egyptian finds that date to

[9] Often this revision involves the insertion of "post-Mycenaean IIIB/pre-Monochrome" strata at Philistine Pentapolis sites (for Tel Miqne-Ekron, see Finkelstein 2000:164; for Ashdod, see Finkelstein and Singer-Avitz 2001:234–239).

the Twentieth Dynasty/twelfth century BCE excavated at sites in Canaan reflect this attempt. Most of these sites are located in the northern Negev, inner Shephelah, and along the interior valleys (i.e., Jordan and Jezreel). At Philistine Pentapolis sites, on the other hand, there is an almost complete absence of evidence for an Egyptian presence during the Twentieth Dynasty (Weinstein 1992:145; Bietak 1993:299–300; Stager 1995:344; Barako 2004). It is significant that this evidence is missing from strata, regardless of the absolute dates assigned to them, which both precede and correspond to the period of the Philistine settlement.

This lack of Egyptian activity during the Twentieth Dynasty in southern coastal Canaan/Philistia is no mere coincidence. The Egyptians were not in the region during this period because a people hostile to Egypt – namely, the Philistines – were present in their place. The weight of the evidence from Philistine Pentapolis sites is considerable: Several seasons of excavations in wide exposures of twelfth-century-BCE strata at Ashdod, Ashkelon, and Tel Miqne-Ekron have produced virtually no Egyptianized material culture. This pattern cannot be dismissed due to the vagaries of archaeological discovery. Instead, it reflects a historical development that offers a more reasonable explanation of the archaeological data than does chronological revision. In short, Egyptians and Philistines coexisted in southern Canaan during the twelfth century BCE and their material culture boundaries (at least for certain types of pottery) were impermeable.

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Is the Philistine Paradigm Still Viable?

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The Philistine Paradigm has dominated archaeological and historical research since the work of R.A.S. Macalister a century ago (Macalister 1914), and to an even greater degree since it was formulated in a more detailed way by W.F. Albright (1932:58) and A. Alt (1944) a few decades later. According to this model – which was based on an interpretation of ancient Near Eastern records, the biblical text and archaeological finds in Philistia – large numbers of Philistines invaded Canaan by land and sea, and Ramesses III blocked their onslaught and resettled them in Egyptian strongholds in the southern Coastal Plain. When Egypt retreated from Canaan the Philistines took over these territories and established a powerful system of five cities – the biblical "Pentapolis" (for the current presentation of the Philistine Paradigm see, e.g., Dothan and Dothan 1992; Bietak 1993; Stager 1995; Dothan 1998).

Recent studies and accumulation of new data on old excavations call for a reexamination of the world of Philistia and the Philistines in the Iron I. In this overview I wish to relate, albeit briefly, to six issues that constitute the backbone of the Philistine Paradigm.

I. Additional Data Pertaining to the Debate on the Date of the Philistine Settlement

An ever-growing amount of evidence demonstrates that the Egypto-Canaanite system of the Late Bronze Age continued to function at least until the days of Ramesses IV.

A scarab of Ramesses IV was identified by Krauss (1994) at Lachish. It was found near the tel, probably in a Level VI context. Since Lachish was not inhabited in the Iron I, there is no doubt that the scarab indeed originated from the city of Level VI. An additional scarab of Ramesses IV, found in the excavations of the 1930s, has recently been identified by Nir Lalkin, a Ph.D. student at Tel Aviv University (personal communication).

Uehlinger (1988:13–15) dated two scarabs from Cemetery 900 at Tell el-Farah (south) to the days of Ramesses IV and Goldwasser and Wimmer (1999) have recently assigned two hieratic fragments found at the site by Petrie to the days of Ramesses III. Cemetery 900 at Tell el-Farah did not produce Philistine pottery.

A reevaluation of the Ashdod finds (Finkelstein and Singer-Avitz 2001) seems to indicate that the Late Bronze city there was not destroyed before the mid-12th century BCE. This observation is based on the ceramic finds from local Stratum 6 in Area H and on the scarabs of Ramesses III and Ramesses IV that were found (in unstratified contexts) at the site (Brandl 1993:138–139; Ben-Shlomo 2003:89 respectively). It seems,, therefore, that general Stratum XIV at Ashdod (local H6) was a contemporary of Level VI at Lachish (Ben-Shlomo [2003] rejected our observations; for our reply to his rejoinder see Finkelstein and Singer-Avitz, 2004).

Stratum XIV at Gezer "was rather thoroughly destroyed" (Dever *et al.* 1974: 51). Its limited pottery assemblage is typical of the latest phase of the Late Bronze Age. A relatively large quantity of unstratified Egyptian artifacts of the late 19th and 20th Dynasties was found at the site, mainly in the course of the old Macalister excavations (for details see Singer 1986–87). They include a scarab of Ramesses IV and a cartouche of Ramesses IX. Singer (*ibid.*) rightly suggested that these finds should probably be assigned to Stratum XIV, the last Late Bronze city at Gezer.

At nearby Aphek, a Ramesses IV scarab was found in a Stratum X10 pit (Kochavi 1981:81; Keel 1997: 85, no. 17), which also yielded bichrome Philistine pottery. It is clear, therefore, that the scarab must have originated from an earlier stratum. Of the two possibilities – the residency of Stratum XII and the post-residency-pre-Philistine Stratum XI – I tend to prefer the former. But the much-debated date of the destruction of this stratum is beyond the scope of this paper.

Stratum IX at Tel Sera produced a hieratic

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inscription from the 22+x year of an Egyptian pharaoh – doubtless Ramesses III (Goldwasser 1984).

A late-Late Bronze tomb at Beth-Shemesh also produced a scarab of Ramesses IV (Grant 1932 51:40; I am grateful to Nir Lalkin for referring me to this item).

Finally, various finds from Deir el-Balah, most of which were retrieved from the antiquity market, hint that the site continued to function in the time of the 20th Dynasty, possibly until the days of Ramesses VI (Giveon 1977; Finkelstein 1995:222–223).

In the northern part of Canaan, Megiddo could not have been destroyed before the days of Ramesses III. In fact, it was probably still occupied in the days of Ramesses VI (Singer 1988–89; Ussishkin 1995). A group of finds from Beth-Shean testify that the Egyptian stronghold there still functioned in the days of Ramesses IV (Finkelstein 1996a).

The picture that emerges from all these sites – which cover the areas of Philistia, the Shephelah and the Jezreel Valley – is clear: The Egyptian administration in Canaan survived at least until the days of Ramesses IV, and possibly a bit later.

Needless to say, not a single locally made Mycenaean IIIC:1b ("Monochrome") sherd – the pottery commonly affiliated with the early phase of the Philistine settlement – has ever been found in any of these strata, even at sites located only a few kilometers from the main Philistine centers. Moreover – and no less important – not a single Egyptian 20th Dynasty vessel (to differ from stray sherds) has ever been found in any of the Monochrome strata. These phenomena (for a full discussion see Finkelstein 1995) are too widespread to be explained as coexistence between the two cultures – that is, that the Philistines took over part of Philistia while Egypt continued to control nearby sites (see recently Dothan 1998:159–160).

Bunimovitz and Faust (2001) have recently traveled far in search of ethnographic case studies from the Baringo district in Kenya, to show that two distinct cultures can coexist without mixing much of their material traits. They and other scholars (e.g., Bietak 1993; Dothan 1998) argued that the early Philistines could have lived parallel to the Egyptian 20th Dynasty strongholds, with each community maintaining its distinctive culture in total isolation from the other.

The quantity and variety of contemporary human cultures enables ethnologists to find a parallel to each and every historical phenomenon. In fact, ethnography can supply examples to conflicting situations. The task of the archaeologist is to set the rules of comparison. There is no question, to my mind, that 20th century CE African tribes *cannot* be compared to the 12th century BCE Egyptian Empire in the Levant, or to the 12th century migrants from the Aegean basin. These phenomena are simply incomparable: The level of social and political organization, the environment, the contact with neighboring cultures – all are too different from each other. Bunimovitz and Faust's comparisons are far beyond the legitimate limits of analogy (see details in Finkelstein 2002a).

In any event, Ashdod and Tel Miqne supplied evidence that the Monochrome phase postdated the Egyptian rule. In both sites the remains of the two cultures were found one on top of the other (Ashdod XIII and H-6, and Tel Miqne VII and VIIIA). Therefore, the two parties – Egyptian and Philistine – are separated stratigraphically, that is, separated chronologically and not geographically. There are no clues regarding the settlement of the Sea Peoples, Philistines included, before the 1140s or 1130s BCE.

II. The Demographic Aspect: Was there a Large-Scale Philistine Invasion of Canaan?

The issue regarding the size of the Philistine migration is strongly debated. Beliefs range from a major invasion by sea of tens of thousands of people (e.g., Stager 1995:344), to the assumption that about 10,000 people came, mainly by land (Yasur-Landau 2003a:368), through to a total rejection of such a migration (Sherratt 1998).

The only way to deal with this issue quantitatively is to compare the two successive settlement systems in Philistia – in the Late Bronze and the Iron I. The data for such a comparison, which I presented a few years ago (Finkelstein 1996b), needs to be updated and amended, mainly because I now tend to reduce the area of Iron I Ashkelon (below). The field data – from excavations and surveys alike – seem to indicate that the Late Bronze/Iron I transition featured both change and continuity. The main change was the total annihilation of the city-state of Lachish and its surrounding villages. Continuity is hinted by the reoccupation of some of the major and secondary sites and by the fact that only a few new settlements were established in the Iron I. As for numbers, the total built-up area diminished from ca. 175 hectares in the Late Bronze to ca. 135 hectares in the Iron I, with the latter number probably representing the peak of the period in its advanced stage. If we translate these numbers into population figures – and I do this only for the sake of

demonstration without claiming accuracy – we reach an estimate of about 35,000 people in the Late Bronze and 27,000 in the Iron I.

Attempting to evaluate the meaning of these numbers one should remember that: A) It is reasonable to assume that not all the Late Bronze population was annihilated; B) It seems likely that the transformation in Philistia was gradual and that the numbers for the Iron I represent a relatively later stage of this period, after the recovery from the crisis of the mid-12th century. From the demographic perspective these two points seem to balance each other. It is clear, therefore, that we are *not* dealing with a migration of more than a few thousand people.

III. Were the Iron I Philistines a Maritime Power?

Many scholars – recent among them Stager (1995) and Barako (2003) – have described the migration of the Sea Peoples as a large-scale maritime invasion. Yasur-Landau, on the other hand, has recently pointed out the immense difficulties in organizing a massive maritime migration from the Aegean world to the coast of Canaan in the late second millennium BCE, arguing that the archaeological finds from Philistia do not support a theory of a major maritime invasion. He therefore opted for a migration by land (Yasur-Landau 2003a; 2003b). Since this debate has – as far as I can judge – come to a stalemate, I wish to pose a slightly different question that *may* shed light on this issue: Were the Philistines, after their settlement, a maritime power?

The settlement pattern in Philistia seems to provide a negative answer. First, only one of the major Philistine centers – Ashkelon – is located on the coast. Ashdod and Gaza are wrongly described as coastal cities since a harbor to their west was needed in order to engage in maritime activity. This phenomenon – an inland city with a port-quarter on the coast – is not known in Philistia before the Assyrian takeover in the late-8th century BCE, when Ashdod Yam to the west of Ashdod and Blahiah to the west of Gaza were founded (Finkelstein and Singer-Avitz 2001:246–253; Humbert and Abu Hassuneh 1999 respectively).

Moreover, the settlement patterns of the southern Coastal Plain show that there were more coastal sites in the Late Bronze (11) than in the Iron I (only 6). And of the 10 settlements established in the Iron I, only 3 were built on the coast or in its immediate vicinity (Finkelstein 1996b).

Therefore, there is no clear-cut link between the Iron I settlement system in Philistia and the coast.

IV. Were the Iron I Philisitne Cities Fortified?

Most scholars agree that, apart from reuse of the old Middle Bronze ramparts, the Late Bronze city-states were unfortified. And based on the results of the excavations at Tel Miqne, Ashdod and Ashkelon, scholars have argued that the Philistines built large-scale fortifications in an early stage of their settlement (Dothan and Gitin 1993; Dothan and Porath 1993:69–72; Stager 1995:342 respectively).

Ussishkin (2005) has recently scrutinized the data from Tel Miqne. He has convincingly shown that Ekron was not fortified in the Iron I; that massive fortifications surrounded it only in the late 8th century BCE, probably after it was taken by Sargon II; and that it subsequently grew to function as one of the most important Assyrian-related centers in the west. I accept these arguments.

Singer-Avitz and I have recently reexamined the Ashdod remains (in Area G) which were interpreted by Dothan and Porath (1993:69–72) as belonging to a fortification system dated to the Iron I. To the best of our understanding, the Stratum XII remains do not belong to a fortification system. The "glacis" is nothing but a fill and the parallel walls that were described as a section of a casemate fortification probably belong to a large building that functioned in Strata XIV–XI. And as far as Singer-Avitz and I can judge, the massive city wall assigned to Stratum X was built later in the history of Iron II Ashdod, probably in the 8th century BCE (Finkelstein and Singer-Avitz 2001).

The full results of the excavations at Ashkelon have not yet been published and one needs, therefore, to turn to Stager's general description of the finds. Remains of a massive mudbrick tower were found on the crest of the huge Middle Bronze earthen rampart, on the northern edge of the site. The ancient earthwork was renovated by a "huge glacis-type rampart." Stager (1991:7) dated the construction of these fortifications to ca. 1150 BCE. A few years later he described these remains in a somewhat different way, reporting that Monochrome Philistine pottery was found in fills sealed by the earliest Philistine rampart; here Stager (1995:342) dated the construction of the rampart to the early Iron II. Since we are dealing with the crest of the rampart, it is clear that no floors were found and that the dating of the remains is indeed based on pottery from fills. This general observation and the early Iron II date given by Stager (probably meaning that early Iron II pottery was found in the fills together with the Monochrome sherds), lead to three conclusions: A) The Iron I material comes from fills; B) The towers were

built in the Iron II, possibly even in an advanced stage of the period; C) No evidence for Iron I fortifications has thus far been found at Ashkelon. It is possible, of course, that future excavations will uncover such evidence in el-Hadra – the inner, relatively small tel of Ashkelon – though the excavations of Grid 38 on its northern edge have not, for the time being, unearthed such remains.

To sum-up this issue, no evidence for Iron I fortifications has thus far been found in Philistia. The opposite is true: The long period of time between the Middle Bronze and the Iron II is characterized by unfortified cities. The great fortifications of Tel Miqne, Ashdod and Ashkelon were probably built in the 8th century BCE, in relation to the growing Assyrian involvement in Palestine.

V. Did the Philistine Cities Constitute a System of Five Peer-Polities?

In order to answer this question I must broaden the discussion to include the development in Philistia in the Iron II.

Iron I Ekron stretched across the entire mound – the upper and lower tels alike – an area of ca. 20 hectares. It seems the site was destroyed in the late 10th century BCE (Low Chronology; early 10th century BCE according to Dothan and Gitin, e.g., 1993:1056). When the city revived, it shrank to the upper mound. In the late 8th century BCE, Ekron again expanded across the entire mound, reached its peak prosperity and was surrounded by massive fortifications (Ussishkin 2005). By the 7th century it had become the most important city in Philistia (Gitin 1995).

Iron I Ashdod stretched across the upper mound only – an area of 7 hectares. In the early Iron II (the 9th century BCE), Ashdod began a gradual expansion to the lower mound. It reached its peak prosperity and maximal size – about 35 hectares – in the 8th century BCE (Finkelstein and Singer-Avitz 2001). At that time it was surrounded by massive fortifications, including the four-entry gate in the east (for its date in the 8th century BCE see Ussishkin 1990:77–82). In 711 BCE, Ashdod was conquered by Sargon II and never recovered from the shock. It is possible that a small settlement existed there in the early 7th century, but it seems that following the Assyrian conquest the main settlement was relocated to the coastal Ashdod Yam, which served the Assyrian trade policies better than the inland site (Finkelstein and Singer-Avitz 2001).

Stager (1995:342–343) suggested that Iron I Ashkelon covered the entire area of the immense Middle Bronze enclosure – about 60 hectares. This assumption is based on evidence from fills containing (Iron I) Philistine sherds found on top of the northern rampart. Yet, pottery from fills – especially pottery found in an earthwork belonging to a fortification system – does not necessarily testify to the existence of a settlement (from the time of these sherds) at that spot. In other words, when the Ashkelon rampart was renovated, probably some time in the Iron II, the builders could have used settlement refuse-material from other parts of the site. Stratified remains from the Iron I were found only in the inner mound (el-Hadra) which covers an area of 6.5 hectares (also Yasur-Landau 2003a:358). It seems that Ashkelon grew in size – to include at least parts of the Middle Bronze enclosure – in the Iron II.

We know nothing about the size of Gaza in the different phases of the Iron Age. As for Gath, the excavations – old and new – have not supplied clear data for a reliable reconstruction of the development of the Iron Age city. But in this case the written sources add valuable information. We do not know the size of Iron I Gath, but it seems that the city reached its peak prosperity in the 9th century BCE. Maeir (2001:113) has estimated that the city of that time stretched over an area of 35 or even 40 hectares – the largest city in Philistia, if not in the entire country. This city was destroyed in a conflagration in the late 9th century BCE, probably brought on by Hazael, King of Damascus (II Kings 12: 18, which is usually considered a reliable source). Gath never recovered from this blow. When Sargon II campaigned in Philistia, it was already a secondary town in the territory of Ashdod. Nor does it appear in the 7th century sources (for Gath in the historical sources see Schniedewind 1998).

Another matter that should be checked is the settlement hinterland of the Philistine cities. One can expect that a peer-polity organization would be characterized by a somewhat equal, hierarchic settlement system around the major cities. This is not the case in Iron I Philistia (see Finkelstein 1996b). Ashdod had only a sparsely settled hinterland and Ashkelon had no rural hinterland at all. Gath is located on the northern edge of its possible territory, with most of the small sites located far to its south. Only Ekron and Gaza had a reasonably intensive rural hinterland. This is not surprising, as Ekron seems to have been the major Philistine city in the Iron I.

It seems to me, then, that the answer to the question of a peer-polity system in the Iron I should be negative. First, Iron I Philistia features urban centers of different size, density of settlement in the hinterland and thus political power. Second, and more

important, it seems that the center of gravity in Philistia *shifted* in the course of the Iron Age from one place to another: In the Iron I Ekron was a central, important settlement, possibly the largest in Philistia. Its destruction in the late 10th century BCE opened the way for the rise of Gath, which reached its peak in the mid-9th century. And when Gath was destroyed by Hazael, Ashdod came to the fore. Ashdod was the largest and most prosperous city in Philistia in the 8th century BCE. When it was thoroughly destroyed in 711 BCE, Ekron rose again. In the 7th century BCE it was probably the largest and most important city in Philistia.

VI. Can the Iron I Philistines be Equated with the Biblical Philistines?

Scholars have tended to read the "early" biblical description of the Philistines on an Iron I background (e.g., Singer 1994; Noort 1994; Ehrlich 1996; Machinist 2000). This idea rested on the theory of a great Solomonic enlightenment period, with strong scribal activity that produced texts describing the realities of the time (Rost 1982 [in German 1926]). Yet, over a century of archaeological activity in Judah has not produced even a single trace of widespread scribal activity before the late 8th century BCE.

Indeed, it is quite clear that the Iron I Philistines and the biblical Philistines are two worlds apart. Other Sea Peoples, such as the *Sikila* and the *Sherdani* – are not mentioned in the Bible, and the text is unaware of any component of the Iron I Philistine material culture. On the other hand, the biblical Philistines perfectly fit the realities of the late Iron II. Following are a few examples in brief (for the full arguments see Finkelstein 2002b).

One of the best and most well-known examples is the case of Achish. The Assyrian texts and the Ekron Inscription both speak about a 7th century Ikausu, who was probably the figure around whom the story of Achish, King of Gath in the days of David, was formed (Naveh 1998).

In several cases the biblical text mentions five *seranim*, and in one place (Josh 13:3) it specifically refers to a Philistine Pentapolis. The term *seren*, which lacks Semitic etymology, has usually been connected with Greek *tyrannos*, which appears for the first time in the 7th century. Pintore (1983) compared the word *tyrannos* – probably of Lydian origin – to the Luwian *tarwanis*, a title given to neo-Hittite governors. He saw the chain of influence as *tarwanis>tyrannos>seren*, but faced a problem: The biblical *seren* was dated to the Iron I – before the first appearance of the other two terms. Dating *seren* in the 7th century resolves the problem: *tyrannos* developed in the 7th century or earlier in western Anatolia from *tarwanis* and *seren* appears about the same time in the biblical text. It was probably transferred to Philistia by Lydian, Carian and Ionian mercenaries who served in the army of Psammetichus I, and who must have been deployed, among other places, in Philistia (for details see Finkelstein 2002b). The biblical notion of a confederation of five cities may also reflect 7th century Greek and Western Asia Minor realities of leagues of cities, which first appeared in the Archaic period (e.g., Jeffery 1976).

The armor of Goliath was sometimes interpreted as reflecting the look of Early Iron Age Aegean warriors, and compared to the figures on the Warrior Vase from Mycenae (Yadin 1963:354; Stager 1991:17; Dothan and Dothan 1992:47). Yet, the warriors on the vase are protected by non-metallic armor and the weapons of the Sea Peoples in the Medinat Habu reliefs are completely different from those used by Goliath. In fact, the best comparison of Goliath's armor is to the armor of Greek hoplites of the 7th-to-5th centuries BCE. The Judahite authors probably knew Greek hoplites who served – in their own days – in the Egyptian army of Psammethicus I.

These are but a few of many examples. But they are sufficient to stress my point: The biblical Philistines are completely devoid of Iron I background. On the other hand, they are thoroughly imbued with 7th century realities.

Summary

Was there a Sea Peoples migration to the coast of the Levant? Yes. Was it a maritime migration? Possibly. Was there a massive maritime Sea Peoples invasion? Probably not. Did the Philistines settle en-mass in Philistia in the days of Ramesses III? No. Were the Iron I Philistine cities fortified? No. Were the Iron I Philistines organized in a peer-polity system? Probably not. Was there a Philistine Pentapolis system in the Iron I? No. Are the Iron I Philistines the Philistines described in the Bible? No.

If only some of the above answers are valid, they mark the end of the Philistine Paradigm, which is based – directly or indirectly – on uncritical reading of the biblical text, and which has dominated research in the last century. But what *can* we say?

In the 1140s or 1130s BCE, Egyptian domination in Canaan collapsed and many of the cities there were devastated, or partially destroyed, probably by

groups of Sea Peoples and other, local, unstable elements. Many of the main cities recovered after a while and were resettled by a mixed population of local Canaanites and Aegean immigrants. The share of the new settlers in the population of Philistia was limited – probably a few thousands. But constituting an elite group, their material culture developed to dominate the scene. The Iron I cities in Philistia did not conduct a lively maritime trade; they were not equal in size, power and prosperity and they were not fortified. Dramatic changes in the size and power of these cities can be traced from the 9th century and mainly in the 8th century BCE. Philistia reached its peak power and prosperity only with the Assyrian conquest and the transformation of its cities into agents of Assyrian economic and political interests. Then, and only then, do the Philistines of archaeology become the Philistines of the Bible.

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The Impact on Correlations to the Levant of the Recent Stratigraphic Evidence from the Argolid

Elizabeth French[*]

The problem under dicussion here results from the time lapse between research and its dissemination by publication or other means. Most of those working on correlations in the 12th cent. BC are specialists in only part of the wide spectrum of information needed to produce a meaningful result and depend on their colleagues to keep them in touch with the rest. Thus when in the 1980's two vital books appeared, both well researched and based on up-to-date information supplied by a multitude of scholars (Mountjoy 1986, Warren and Hankey 1989), these became essential tools. They are still almost universally quoted but unfortunately research has moved on in the intervening period. Though the new information has been communicated as far as possible, it has been variously ignored.

Some of this new information comes from my work at Mycenae. It had been Lord William Taylour's intention to publish himself the stratigraphic results from the 12th cent. strata of the Citadel House Area as he did both the overlying Hellenistic and underlying Bronze Age Palatial levels. But at his death in 1989 he had completed only preliminary notes and it became obvious that I should undertake to deal with the Post Palatial levels as soon as time allowed.[1]

From the very first discovery of good LH III C strata at Mycenae in 1960 the excavation team had circulated work notes; in addition some very preliminary suggestions were put forward in my note in the *AA* (French 1969) but this article has subsequently proved to be partially wrong and definitely misleading. It should no longer be quoted. We were in constant communication with Penelope Mountjoy and with Vronwy Hankey and our preliminary conclusions were used by both but they were preliminary conclusions and liable to change. Then amid the multitude of compelling finds of the Palatial levels which came to light in the final years of the excavation the minutae of the Post Palatial strata became overlooked.

I gave warning of the implications of my new study in Jerusalem in 1995 (French 1998) and presented the detailed results to the Mycenaean Seminar in London 1997 (French 1999) and subsequently in Israel two years ago (French forthcoming). The implications have been accepted by all those working intimately on similar material.

So what is this new stratigraphy and what are the implications that follow? In the excavation of the Citadel House Area at Mycenae by the British School on behalf of the Archaeological Society, the last remaining major sector within the Acropolis untouched by either Schliemann or Tsountas (Iakovides 2003, fig.1) was excavated and it is for this reason that we suggest that the stratigraphy we found *may* form a valid interpretive hypothesis for the other remains on the citadel.[2] In the levels of the Palatial period there was revealed the northern half of a Cult Centre. The construction of this sector began during the late 14th and 13th centuries, well before the West extension of the Citadel Wall. The area was adapted, enclosed by the citadel wall and then suffered a calamity probably in the form of earthquake, leaving large quantities of pottery *in situ*. Following this, some buildings were restored, others mothballed (i.e. taken out of use) and the whole area finally suffered a major destruction with burning which we place about 1200 BC.

Some walls of solid construction and, in particular, the relatively recently constructed West section of the citadel wall survived this major destruction intact. This feature conditioned the rescue strategy.[3] At Mycenae the programme of reconstruction was

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[1] On my retirment from Athens in 1994, I was awarded a Leverhulme Trust Emeritus Fellowship for this purpose. These Fellowships are aimed specifically at those who have been hampered in completing research by the duties of academic administration.

[2] *Pace* Iakovides 2003 and 2005.

[3] Those of us who have lived and worked in the earthquake-prone areas of Greece and Turkey will be well aware of the very different reactions of the survivors and how they face rebuilding – indeed the late Klaus Kilian kept an album of both the damage and the subsequent reconstruction throughout the East Mediterranean area.

ROOM xxxiv			
PAINTED	32	UNPAINTED/COOKING WARE	45
Storage: Stirrup Jar	1	Vat	1
Inscribed Stirrup Jar	1	Pithos	1
Piriform Jar	1	Amphora, small	1
Kalathos	1	Kalathos	1
Pouring: Hydria, small	1	Hydria, large	5
Jug	1	Spouted krater	1
Miniature: Bowl	1		
Cup	1		
Bird Askos	2		
Drinking: Cup	2	Cup	1
		Mug	1
		Kylix	12
		Stemmed Bowl	1
Eating: Deep Bowl	16	Deep Bowl	2
Shallow Bowls/Basin	4	Shallow Bowl	2
		Shallow Angular Bowl	1
Cooking		Tripod Cauldron	3
		CP Jar	4
		CP Jug	1
Accessory		Dipper	2
		Lamp/Brazier	4
		Platter	1
ROOM xxi (additional shapes)			
Collar-Necked Jar			
Strainer Jug			
Krater			
Kylix			
Ring Vase			

Fig. 1 Table of shapes found in Rooms xxxiv and the additional ones from Room xxi

based on two factors: first, previous experience: about a century earlier some event had caused building practice to alter and a method of solid construction based on an artifical foundation terrace, well constructed and well drained had become commonplace;[4] second, the problem caused by the very diverse levels of the surviving remains. The floor levels of the Palatial period varied not only from East to West[5] but also from North to South: those of the South House itself, standing on an exceptionally strong terrace, lay at 238 ASL, the floor of the adjacent Area 36 was at 236.46 and of the Room with the Fresco (Room 31) at 234.75.

The major reconstruction comprised the use of the surviving walls, including the Citadel Wall, to form a solid terrace on which to build two complexes. In two or three places we can trace intermediate floors or working surfaces, accompanied by pottery finds. The main reoccupation floor that can be traced over a large portion of the West side of the area, however, lies above these interim surfaces at a height ASL of 236.80. This is lower that those of the original South House where the equivalent levels of the 12th cent. were apparently removed during later terracing for building in the Hellenistic period.

By one of those amazing strokes of good luck, these complexes also seem to have been destroyed by earthquake leaving again large quantities of pottery *in situ* on the floors – some 90 vases in all from five entirely separate rooms of differing function (Fig. 1). From this material (Figs. 4–7) we can define a phase of pottery development *not previously identified*. The character of this stratified corpus of pottery is so simple and basic that were it not *in situ* in a sequence above the burnt level and the subsequent terracing and sealed by clear later levels , the individual pots would not and could not be identified stylistically.

Immediately after this destruction the complexes were reconstructed, with complete realignment of one and rebuilding of the other. There is very little restorable pottery associated with this phase – Phase X – but what there is shows clear stylistic development. Of particular interest are several pieces stratified between two subphases (Fig. 8).

The buildings of this Phase X were not destroyed but were allowed to lapse into disuse and then were in the main covered by deep layers of wash held up by the West Citadel Wall with sequential layers containing pottery of LH IIIC Middle and then LH IIIC Late. In one area however this debris had been disturbed down to a level of 238 ASL for the construction of a pyre in Submycenaean/ Protogeometric times, thus destroying much intermediate material.

Supporting evidence for all aspects of this sequence is given by the excavations of Professor Mylonas inside the Hellenistic Tower – a small area, again untouched, some 35m to the south of the Citadel House Area – here three major levels with floors, an accumulated depth of 3m, culminated in a floor on which were restorable vessels of the end of LH IIIC Middle (Iakovides 2003, 121 and forthcoming). Further confirmation comes from the excavations of 2000 to the north of the citadel of Tiryns.

[4] Current work on the Petsas' House area destroyed in LH IIIA2 is giving information on this change of practice (kind information of the field director Dr. Kim Shelton).

[5] For a section see Taylour 1981,6: Section 1.

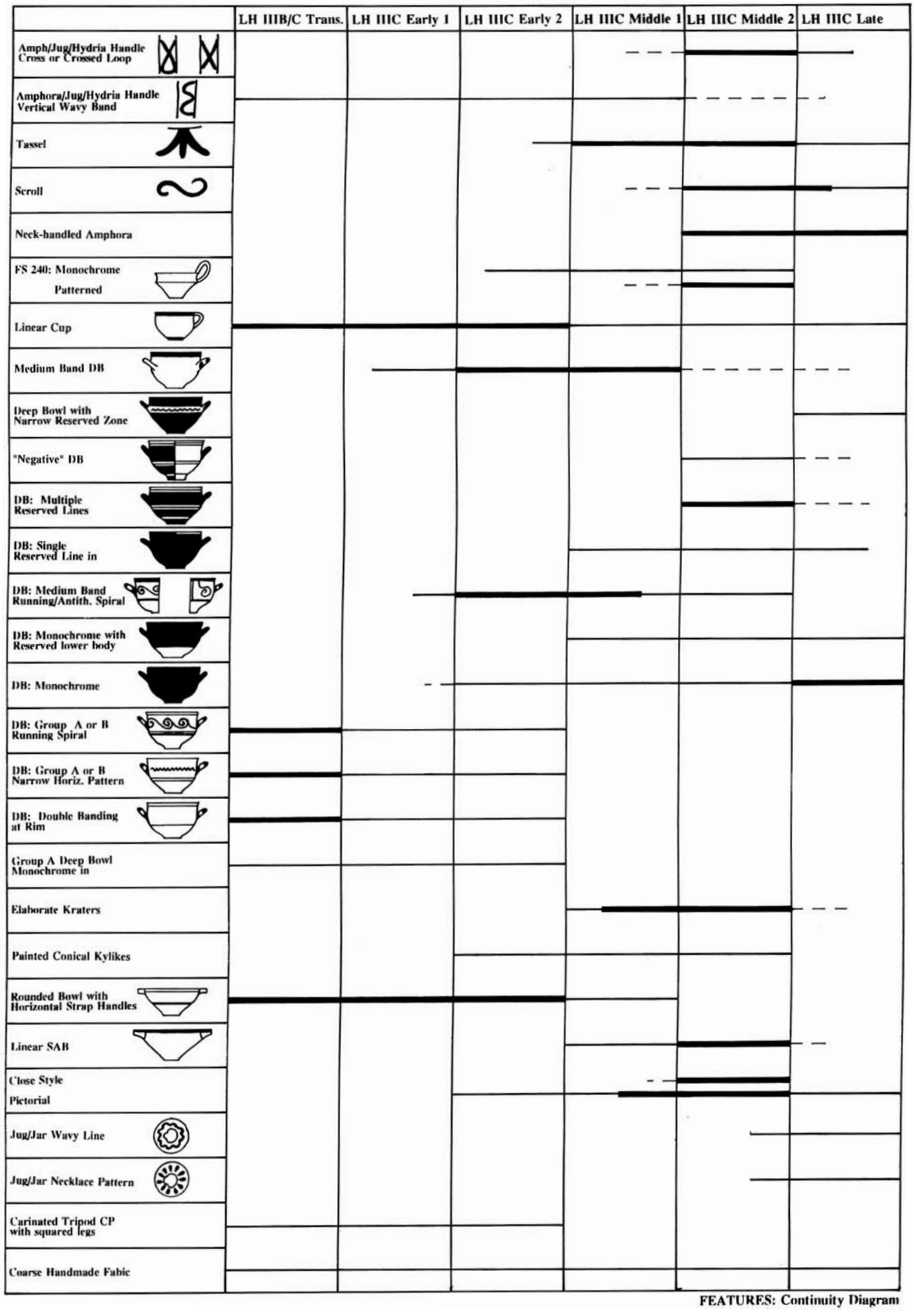


Fig. 2 Chart to illustrate the features found in the phases of LH IIIC at Mycenae

This evidence was presented by Professor Maran at the Mycenaean seminar in London (Maran 2002–03). He has kindly allowed me to mention it, sending me drawings of the material concerned. After levels which echo a stylistically developed version of Mycenae's Phase IX or the start of Phase X there is a complex – called Phase 2 – with a range of restorable pottery which can be equated with the transition between Phases X & XIA. The Tiryns evidence is especially important as Professor Maran has identified a number of Levantine imports among the material and there are thus possibilities for cross dating.

This then is the stratigraphy but what does the pottery itself indicate. Here there are two points which it is very important to bear in mind. First that restorable pottery must be used in a quite different way to sherd evidence. A group of restorable vessels is the result of an event – what has can be termed a "freeze frame"; all were in use (which of course includes storage) simultaneously though they may not have been manufactured at exactly the same time, particularly in certain types of context (e.g. shrines). Sherds are deposited in a cumulative process over varying lengths of time. A group of sherds may include residual material and they were quite certainly not all manufactured at the same time. What such a group of sherds can give are *termini post* and *ante quem* for the underlying and overlying strata. This is relevant in particular to the two groups of sherds illustrated here (Fig. 4, row 5 and Fig. 8, rows 1 and 2. These antedate the subsequent floors of Phase X but cannot be assigned definitely to the lower floors of Phase IX.

Second it does not matter what names we use to describe the pottery phases represented in the stratigraphic sequence, what is important is to relate the actual material.[6] The names are irrelevant though they can and have caused much confusion.

The copious pottery caught by the first earthquake at the end of Phase VII belongs both to the end of LH III B1 and the start of LH III B2. I have therefore often used the term LH III B mid but in absolute terms the division of the 13th century is uneven and this point is now usually placed at +/– 1230 BC. The pottery in the burnt destruction at the end of Phase VIII already begins to show features of LH IIIC and is thus now compared to Penelope Mountjoy's "LH III B/C Transitional" but there is very little of it (or of any pottery for that matter) on the floors of the actual destruction level. Pottery of this type is also found widely throughout the fill of the terraces built after the burnt destruction and on the intermediate floors mentioned above. It was studied in detail by Susan Sherratt for her PhD, forming the first of her five groups and she termed it LH III C Early. Her five-part terminology which was used in the preliminary study of the Mycenae material was shown in a diagram published by Mountjoy 1986, 133 Table II. It did not seem to apply extensively beyond Mycenae and is now largely disused in publication in favour of Mountjoy's tripartite division.

Sherratt, however, because she was not dealing with the relevant trenches, was not aware of the important deposits of whole pots that lay on the floors above the terraces (Figs. 4–7). This group must be inserted into the scheme of development given in her (unpublished) thesis.[7] The additional material forms the first part of Mountjoy's LH III C Early – which for current convenience we can call LH III Ce1. The subsequent Phase X (Fig. 8) can then be called LH III Ce2. Sherratt's chart has been adapted and appears here as Fig. 2.[8]

There follow two important conclusions in our search for correlations with the Levant. First as we now know a great deal more about the stratigraphic and stylistic sequence it can clearly be seen that the only point in the stylistic sequence of the Argolid from which the features found in the Levant could originate is between Mycenae's Phase IX and X. The new "linear" groups previously suspected and recently firmly identified can be related to ripe Phase IX, LH IIICe1, while the material more generally called in the Levant Myc IIIC 1B relates to Phase X, LH III Ce2. Second, this point in real time

[6] Thus it is by ignoring the names and looking at the material that we can link the sequence at Midea to those of Mycenae and Tiryns to make a coherent history in the Argolid.

[7] Her thesis, "The Pottery of Late Helladic IIIC and its Significance", Oxford 1981, is available for consultation in Oxford and in the British School at Athens. The Mycenae section will form part of the forthcoming Fascicule 17 of the *Well Built Mycenae* series.

[8] The correlations between the original phase names and the new ones are: Early = LH IIIB/C Transitional, Inserted phase = LH IIICe1, Tower = LH IIICe2, Developed = LH IIICm1, Advanced = LH IIICm2, Final = LH IIICl(ate) but see Addendum below.

must be well into the 12th Century given the amount of preceding deposit (Fig 3).

If we attempt to outline a background scenario at Mycenae to this development in the years after the burnt destruction of ca. 1200, we would start with a period still strongly related to the preceding but disorganized and in disarray. Eventually strong organization reasserts itself.[9] The new complexes were planned and the terraces on which they were to stand were constructed with a filling of t he pottery from the preceding period. The pottery which is found on the floors of the new complexes is of excellent quality and still has many echoes of the previous century but it seems to me to be intentionally austere and simple. I would suggest that this is a period of deliberate retrenchment, of sturdy utility both in construction and in pottery. It would not be unlikely that during this period overseas contacts which have been dormant, almost non-existent, for about half a century start to be reestablished. However this period comes once more to an abrupt end. It is not unreasonable to suggest that the impetus for more extensive overseas contact was the result of a second (if not third) earthquake within a century – ideas for "earthquake storms" in the Eastern Mediterranean are currently widely discussed[10] and would seem to have some validity. At this point the new contacts were enriched and expanded; there was perhaps some emigration, particularly of craftsmen. The contacts continued and allowed substantial stylistic development in some areas, into a full blown "pleonastic" one. The imports at Tiryns, a site which in my view always took a lead in such affairs,[11] show that the contacts were reciprocal.

The chronological chart (Fig. 3) is the result of detailed consultation with the aim of incorporating as many factors as possible. As to any potential problems with the correlations to the Levant there seem to me to be at present several considerations to be weighed.

1. It must always be borne in mind that the impetus for the new style and range may not have come solely from the mainland of Greece but also from elsewhere in the Aegean, and influenced not only the Levant but perhaps also in due course the mainland itself.

2. There may be an actual gap in occupation between the destructions at the end of the Bronze Age in the Levant and the reoccupation with material seemingly linked to the LH IIICe2 of the Aegean or perhaps an uneveness of occupation over the area of a site – exactly what caused the confusion at Mycenae.

3. Perhaps pottery of the elusive LH III B/C Transition and LH IIICe1 was present either before or after the destructions in the Levant but in such an exiguous quantity or unprepossessing quality that it has been ignored. The difficulty inherent in its identification has been highlighted above.

4. Finally we know that there is a clear fall off in trade during the *whole* of the 13th century (though perhaps particularly the latter part); many of the items and the contexts which contained material assigned stylistically to LH III B were ones, particularly ritual ones, where residual imports are frequently found.

It seems possible that one or more of these factors may in the past have caused correlations to be falsely aligned. What is clearly essential is that material from the Levant is correctly identified.

Addendum November 2006

Following the completion of the study of the Post-Palatial levels at Mycenae (which will appear as *Well Built Mycenae*, Fascicule 16/17) and the initial study of the material from Tiryns Northeast,* it has been decided to term the pottery found on the floors of the burnt destruction level of ca. 1200 BC "Late Helladic IIIB2 Late". This does not affect the identification and analysis of the pottery in any way but makes the terminological equation with both earlier Tiryns reports and the Midea excavations much clearer.

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[9] This was a suggestion some years ago by the late K. Kilian (1988, 135).

[10] The article by Nur & Cline (2000) though based on some questionable and out of date archaeological data presented an interesting hypothesis which was notably developped, with some care, by the BBC Horizon team in 2003.

[11] To be discussed by me at the Emporia conference, Athens 2004.

* J. Maran *et al.*, Forschungen im Stadtgebeit von Tiryns 1999–2002, *AA* 2006 especially 99–133 (stratigraphy) and 139–162 (pottery).

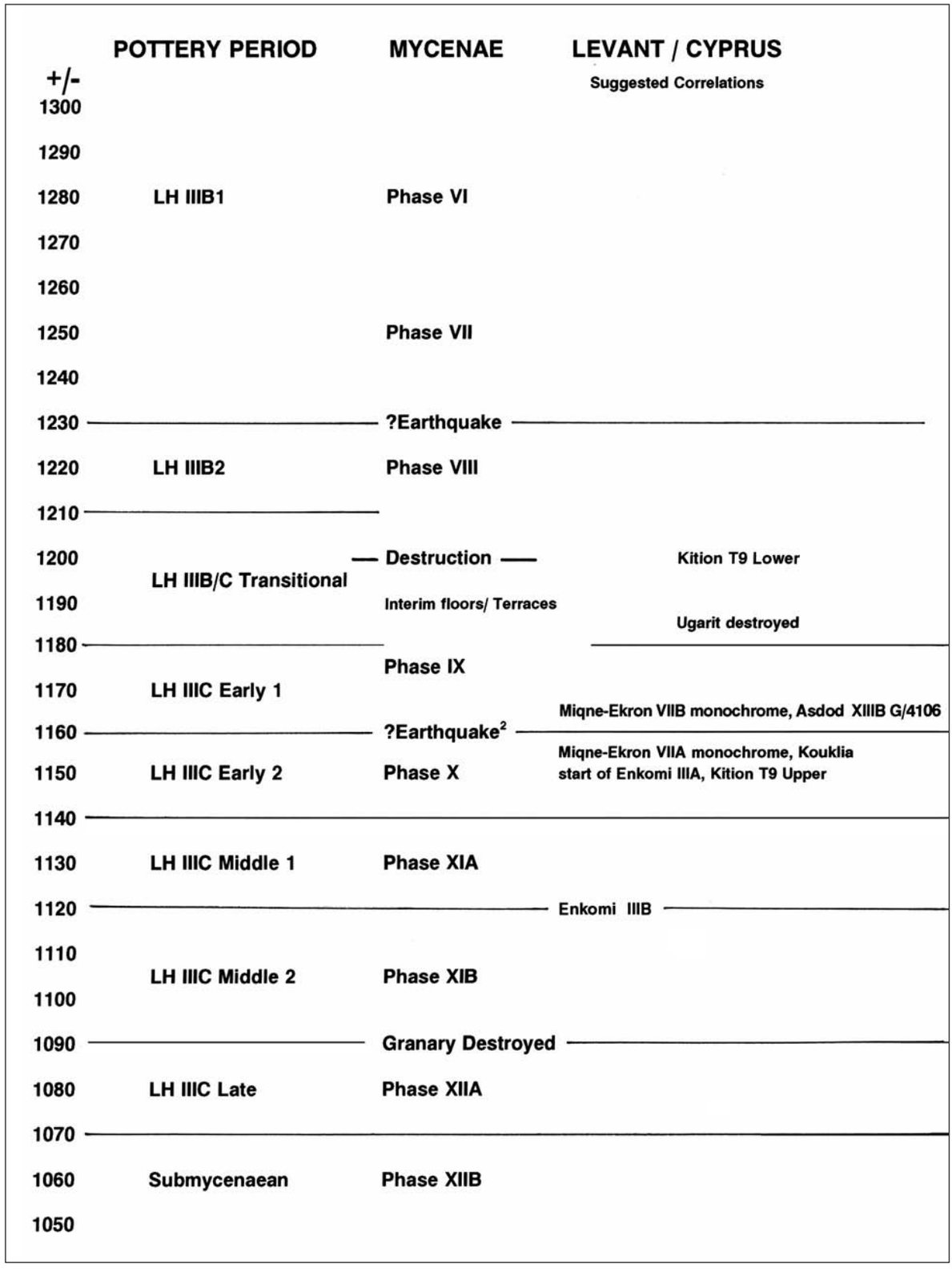


Fig. 3 Chronological chart

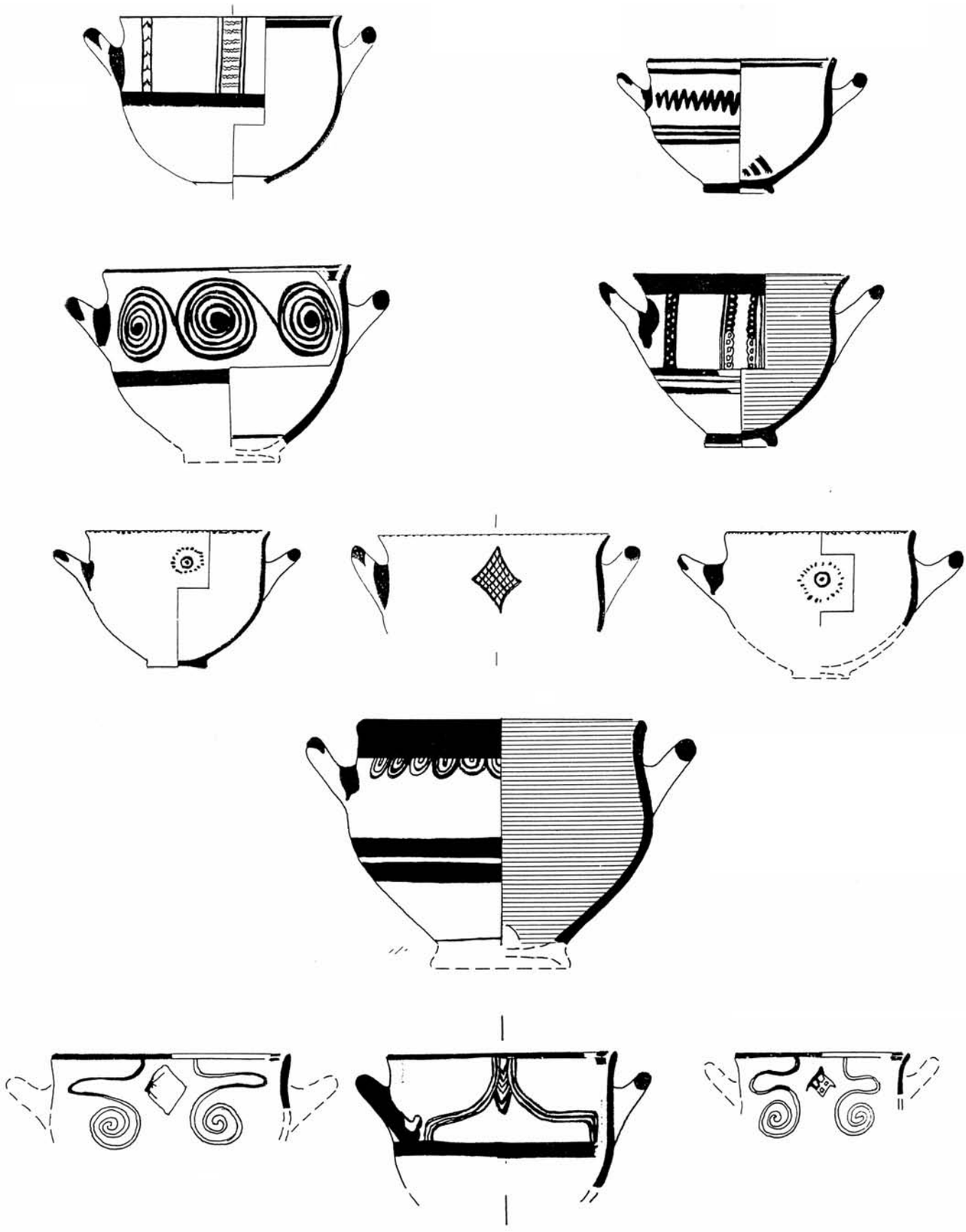


Fig. 4 *Row 1:* Phase VIII Destruction Floors, Deep Bowls of LH III B/C Transitional; *Row 2:* Phase IX Intermediate Floors, Deep Bowls of LH III B/C Transitiona: *Row 3:* Phase IX Room xxxiv Deep Bowls of LH III Ce1; *Rows 4 & 5:* Phase IX/X Deep Bowl sherds of LH IIICe 1/2

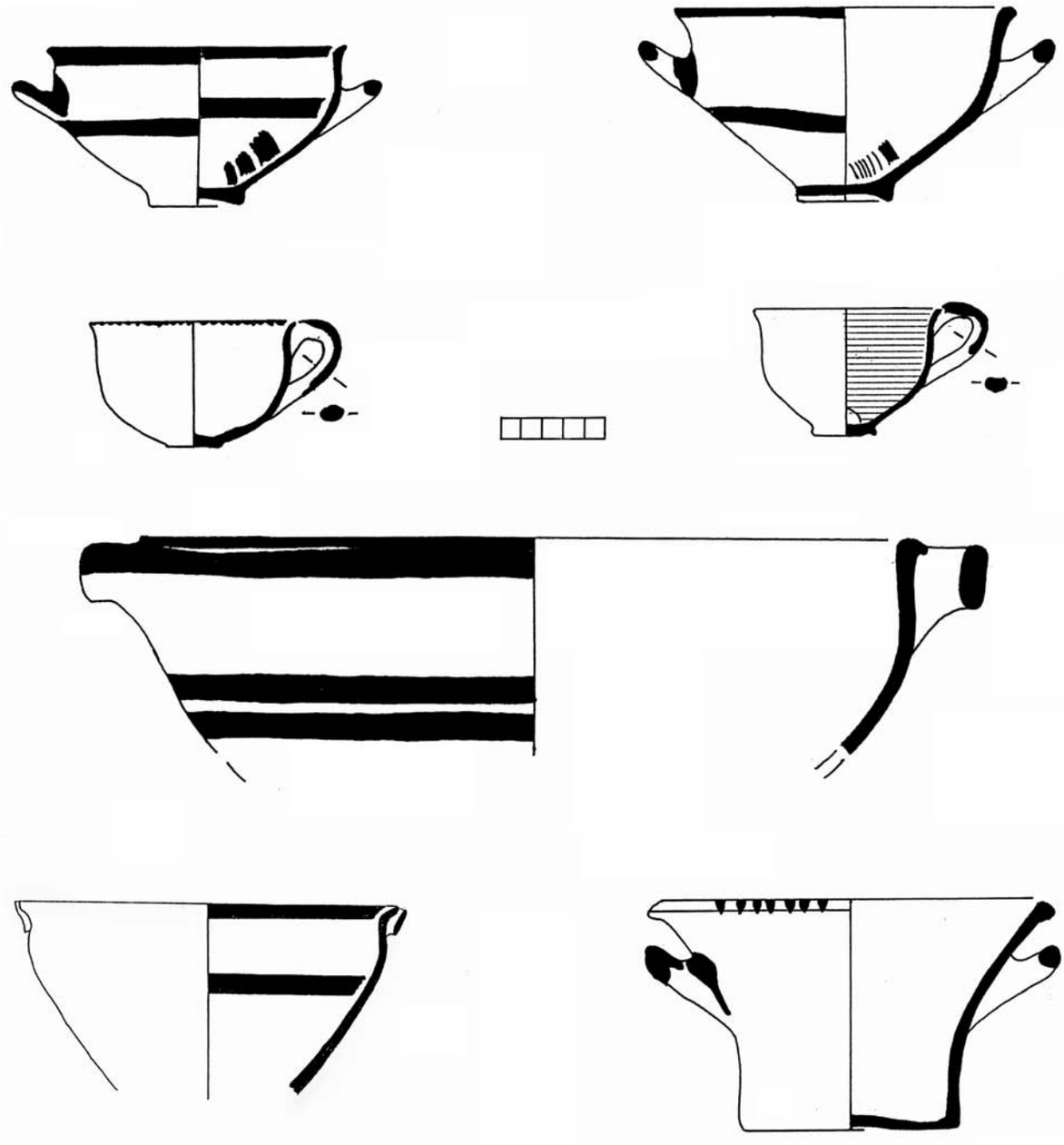


Fig. 5 Phase IX: Room xxxiv Other Open Shapes

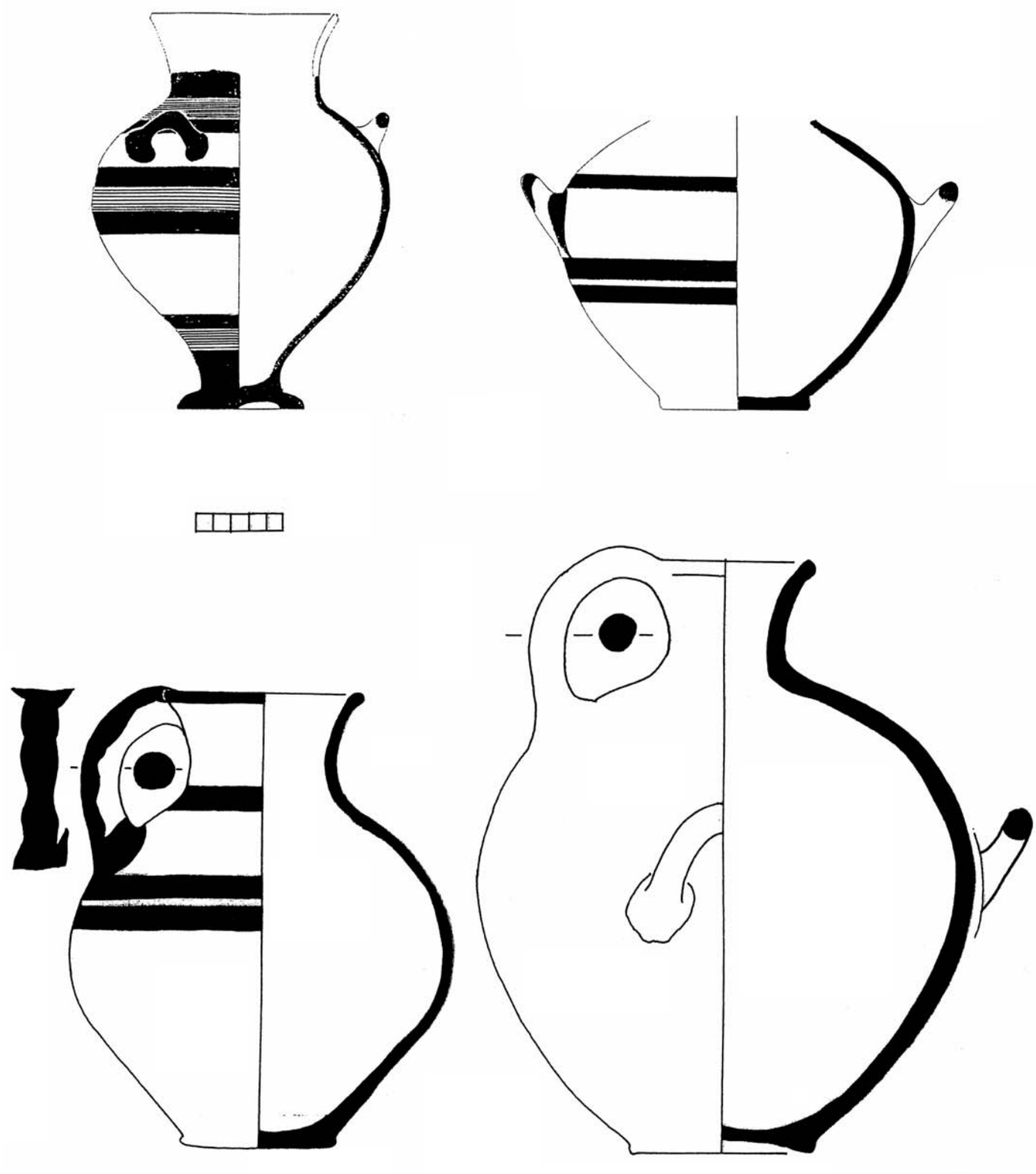


Fig. 6 Phase IX: Room xxxiv Closed Shapes

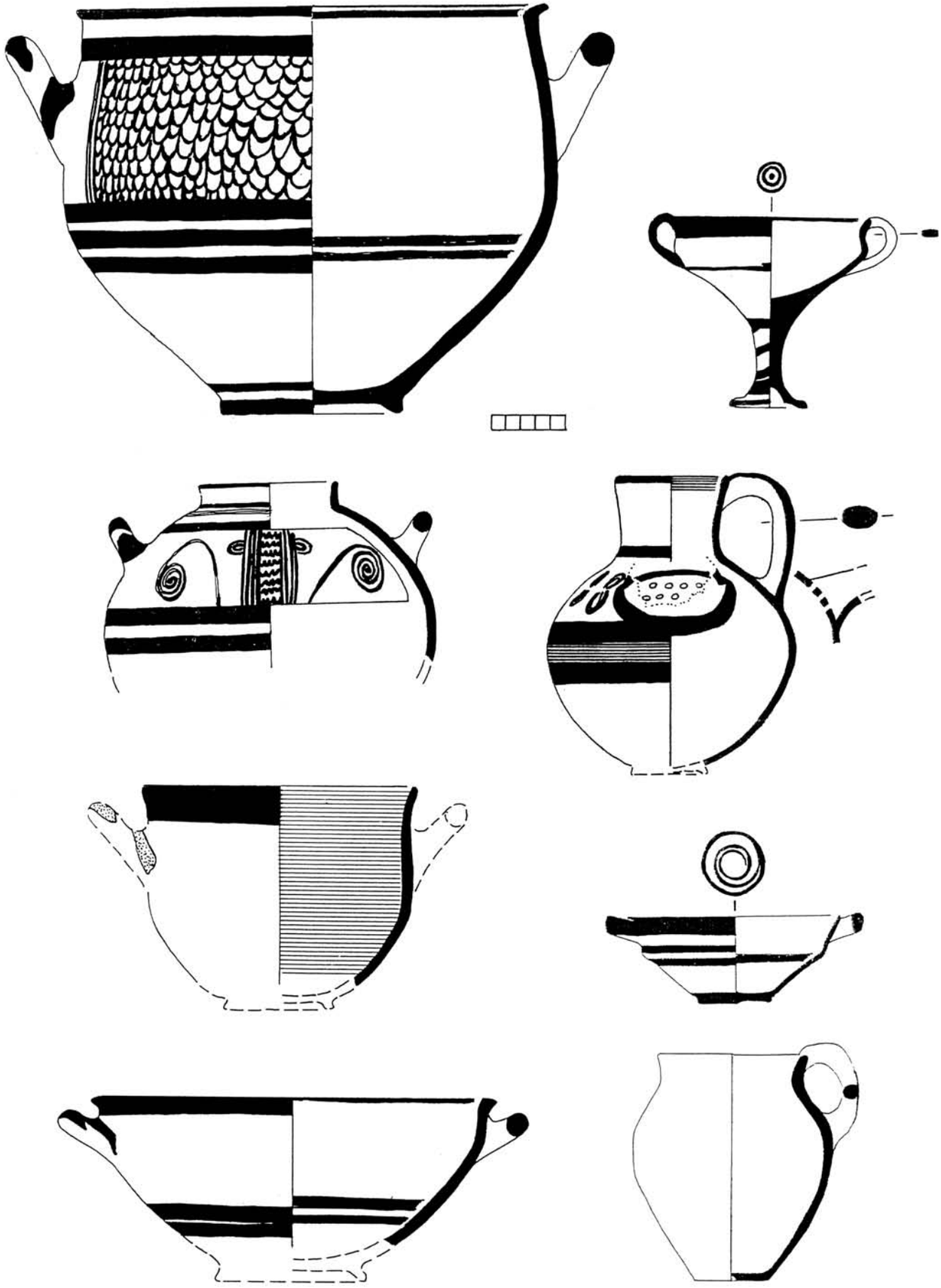


Fig. 7 Phase IX: *Rows 1–2:* Room xxi additional shapes; *Rows 3–4:* Rooms xxiv and xxxiii, duplicated set of pots

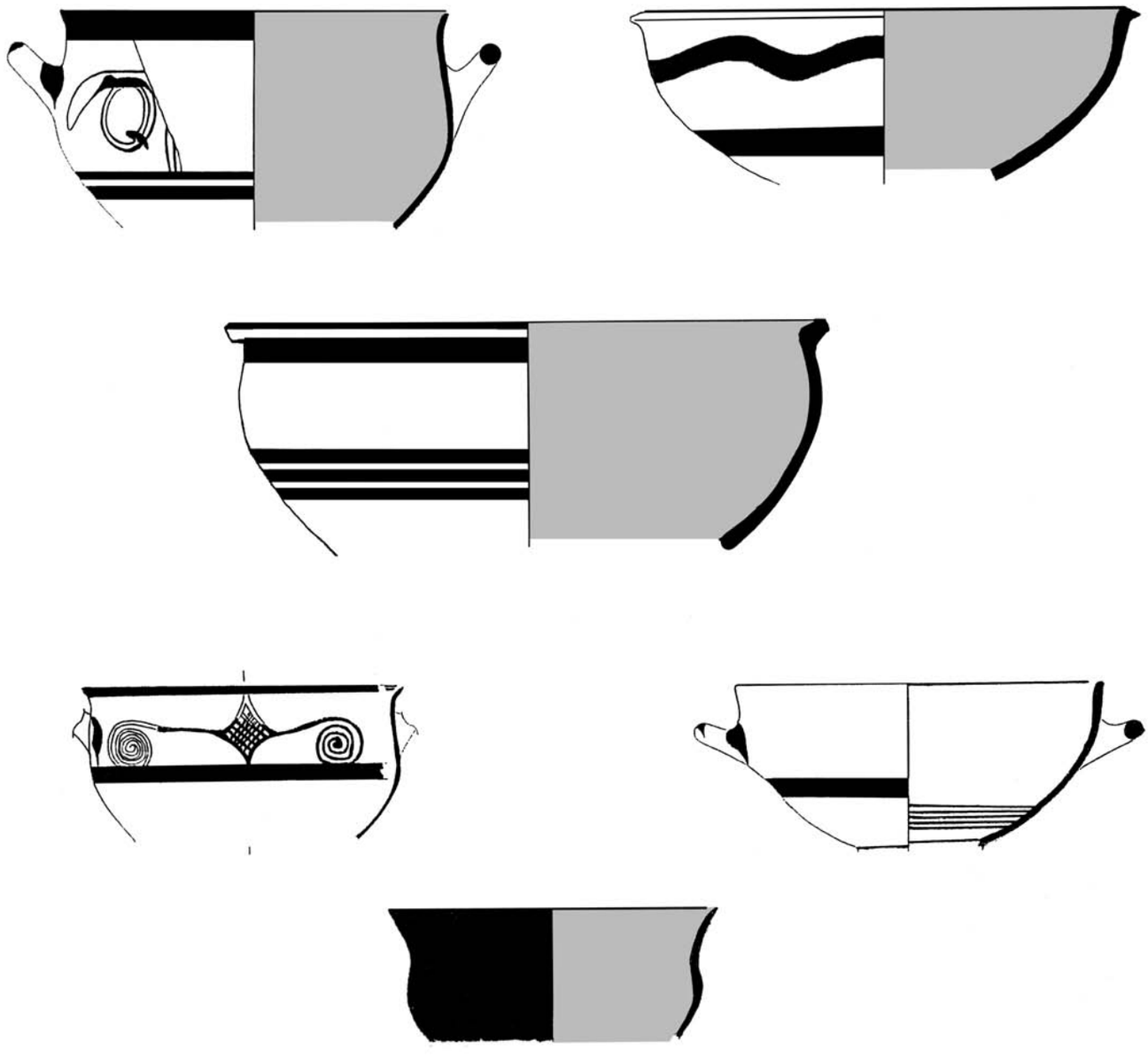


Fig. 8 Phase X: *Rows 1–2:* below the floor of Room 17; *Rows 3–4:* Room liii

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The Mycenaean Pottery from Tel Aphek: Chronology and Patterns of Trade

Marta Guzowska[*] *and Assaf Yasur-Landau*[**]

The manorial structure in Area X at Tel Aphek, sometimes referred to as the "Egyptian Residence", yielded one of the most important contexts for the understanding of the relative and absolute dates connected to the close of the Late Bronze Age in Canaan. Its contents, a rich array of local and imported pottery, as well as several inscribed clay tablets, provide excellent synchronisms between the cultures of Egypt, Palestine, Ugarit, Cyprus, and the Aegean.

The Mycenaean pottery from the residence is now for the first time fully studied and put into context. The vessels gathered by the inhabitants of the residency, dating from as early as LHIIIA2, to LHIIIB2, if not later, provide intriguing details on patterns of trade, imported pottery consumption, and new data on the possible date for the destruction of the residence and the absolute chronology of the local pottery within it.

1. Introduction

The excavations at the site of Aphek took place through most of the 1970s and were conducted by Tel Aviv University under the direction of Moshe Kochavi and the late Pirhiya Beck. Although a preliminary report has been published following the works (Beck and Kochavi 1985), and some of the Mycenaean pottery has been discussed by Warren and Hankey (1989: 156) and Leonard (1994), the entire Mycenaean pottery assemblage of Area X is only now being prepared for final publication by the present writers.[1]

The aim of our paper is to present the preliminary results of the analysis of the Mycenaean pottery in Aphek with a special emphasis put on the interpretation of its find context and spatial distribution.[2] We shall try to establish whether the evidence of Mycenaean pottery supports or refutes the chronology given by the excavators of the destruction of the Egyptian Governor's residence, a date with bearing on the question of the end of Egyptian domination in Palestine.

2. The context of the Mycenaean pottery

A. Area G, Locus 1200, the Tomb

The Late Bronze Age tomb found in Aphek was also the only context which did not raise severe questions of interpretation. Tomb 1200, located in Area G, at the edge of the summit, was a well built tomb, in which eight burials were found, belonging to some of the richer residents of Aphek (Kochavi 1989: 76–78; Beck and Kochavi 1993: 68; finds from tomb Kochavi 1990: 32–33).

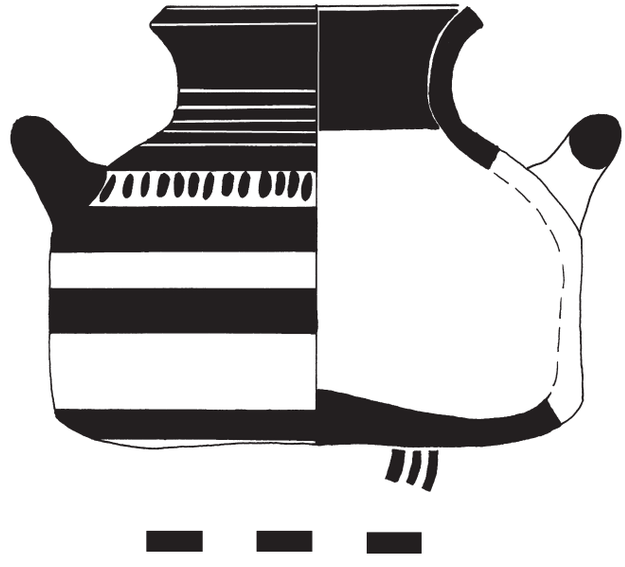


Fig. 1 A straight-sided alabastron from Tomb 1200

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[1] We are grateful to Moshe Kochavi and Yuval Gadot for allowing us to publish the Mycenaean pottery as part of the final publication of Area X in Aphek. We would also like to thank L. French and P.A. Mountjoy for very useful comments given after the paper was delivered in Vienna.

[2] So far, studies concerning the spatial distribution of Mycenaean pottery in Levantive sites have been relatively few, including Steel (1998; 2002), Leonard and Cline (1998), and van Wijngaarden (2002).

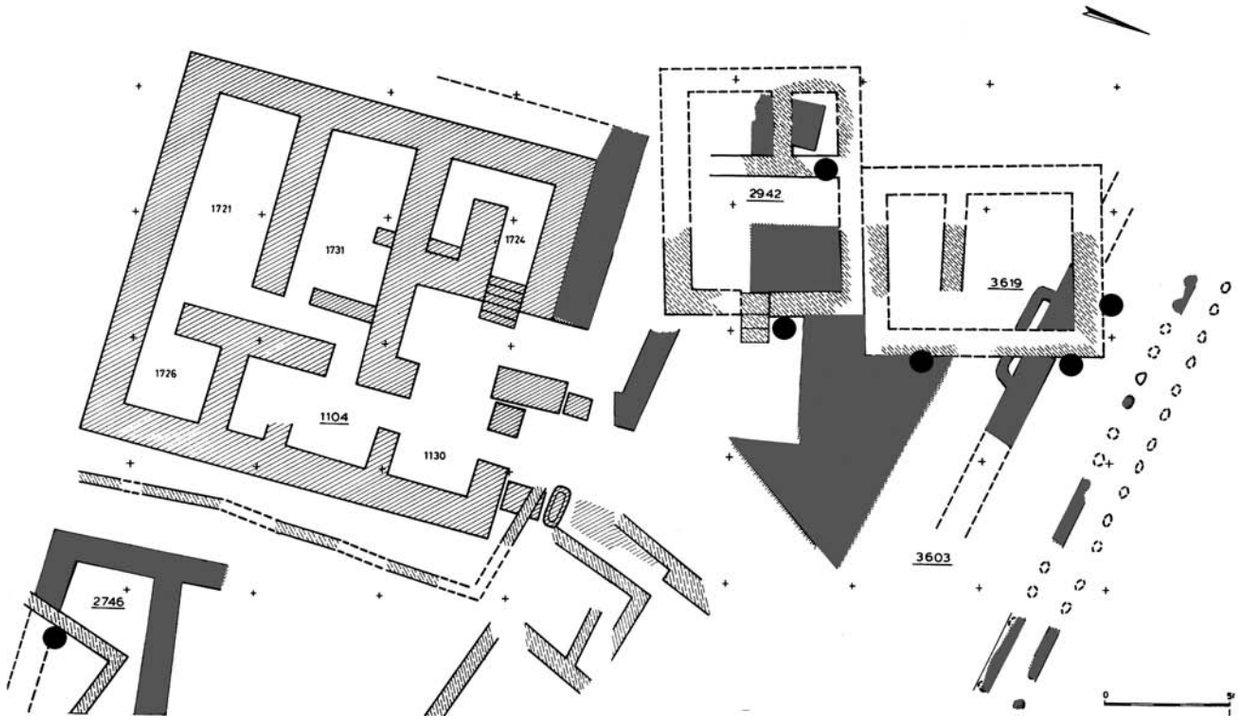


Fig. 2 Distribution of Mycenaean sherds in the stratum X13

In addition to local pottery and some Cypriot imports, it contained four almost complete Mycenaean vessels: two straight-sided alabastra (Fig. 1) and two stirrup jars as well as a fragment of a flask (Guzowska and Yasur-Landau forthcoming: cat. nos. 56–59). Unfortunately, we could not examine two of the complete vessels (a stirrup jar and an alabastron), lost in the years following the excavations, leaving behind only photographic and drawing documentation. Based on the documentation of the preserved vessels the assemblage can be dated no more precisely than to LHIIIA2–B1. Imitations of Mycenaean shapes in local clay were also deposited as burial gifts. Chronologically, this tomb may be assigned to the late 14th century into the 13th century.

B. Area X, stratum X13, "palace V" (Fig. 2)

The Mycenaean pottery in habitation strata at Aphek presented more complex questions of context and chronology. The earliest occurrence of Mycenaean pottery in habitation strata is associated with a large residential complex that was situated on the summit of the tell in the 14th century – the "Canaanite palace" or "palace V" (Kochavi 1989: 61–62; Beck and Kochavi 1993: 68). Only small parts of the structure have been uncovered and no overall plan can be recognized. It is thus hardly surprising that only few Mycenaean sherds have been found in this stratum. All of them are very small and probably moved from their primary context.

The most distinctive among them is a fragment of a LHIIIA2 jug (FS 133 or 136) decorated with an octopus (FM 21) (Fig. 3) (Guzowska and Yasur-Landau forthcoming: cat no. 24).

C. Area X, stratum X12, "palace VI" (Fig. 4)

The majority of Mycenaean sherds at Aphek have been found in stratum X12, connected with the activity of Egyptian administration (Beck and Kochavi 1985: 35–36; Kochavi 1990). In the 13th century, "palace VI", the residence of an Egyptian governor was built on the summit of the hill, on the very place of the earlier Canaanite palace. Mycenaean sherds ascribed to this stratum were found concentrated

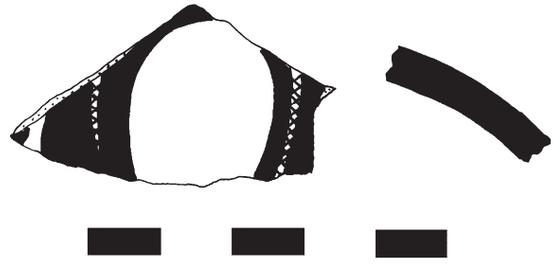


Fig. 3 A fragment of an octopus-decorated jug in stratum X13

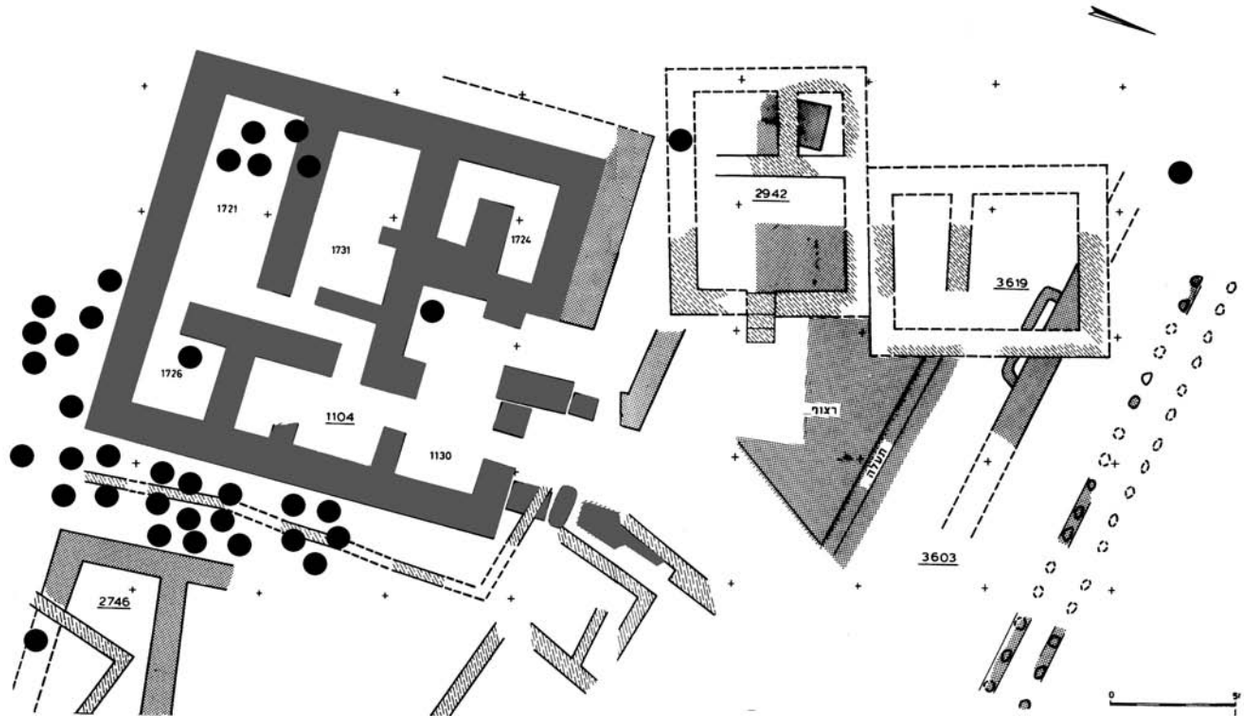


Fig. 4 Distribution of Mycenaean sherds in the stratum X12

outside the building, close to its walls, especially in the narrow alley by wall E of the residence. Other groups of sherds come from the inside of the residence. Only few sherds were found scattered around in some distance from the building.

Almost all Mycenaean material in this stratum is in the form of small, single sherds. It is possible that many fragments assigned to stratum X12 could in fact have been in circulation during the earlier stratum X13, the period of the Canaanite palace. A large part of the sherd material in question dates to LHIIIA2 –B1. Since we do not have the precise date of construction of the Governor's Residence – we only know that it took place some time during the long reign of Ramesses II, in the 13th century – it is equally likely that the Mycenaean material appeared at the site in this period or in the earlier period of the Canaanite palace. We assume that many of the small fragments could have been mixed with clay and mud used to form mudbricks and incorporated in the walls of the Governor's Residence, either during its original construction or during subsequent repairs.

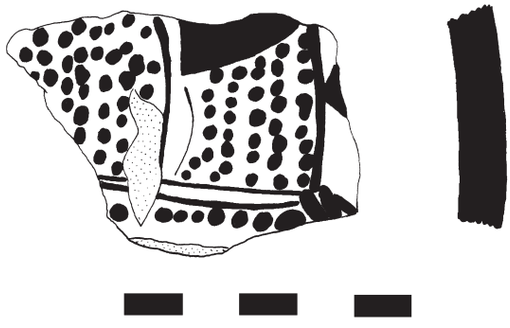


Fig. 5 A fragment of a "Chariot Krater" in stratum X12

Two small sherds from a LHIIIA2 "Chariot Krater" (Fig. 5) (Guzowska and Yasur-Landau forthcoming: cat. no. 2) may confirm this hypothesis, as they were found on and practically inside the ruined wall of the residence.

Only two restorable vessels from the stratum X12, associated with the existence of the Governor's Residence, have been recovered: a stirrup jar (Fig. 6) (Guzowska and Yasur-Landau forthcoming: cat no. 19), and a vertical flask (Fig. 7) (Guzowska and Yasur-Landau forthcoming: cat no. 25). The vessels have different dates: The flask has been dated to LHIIIA2–B1, whereas the stirrup jar comes from the later stratum of the LHIIIB. This case seems to prove that the Governor's "collection" of Mycenaean vases contained "grandmother's china" alongside with pottery acquired in the later period. This is not an isolated case in the Levant: The Amman Airport Temple (Hankey 1974) was built during the time when LHIIIB ware was in use, yet contained a very impressive array of Mycenaean imports from LHIIA to LHIIIB, meaning that some vessels were two hun-



Fig. 6 A fragmentary stirrup jar in stratum X12

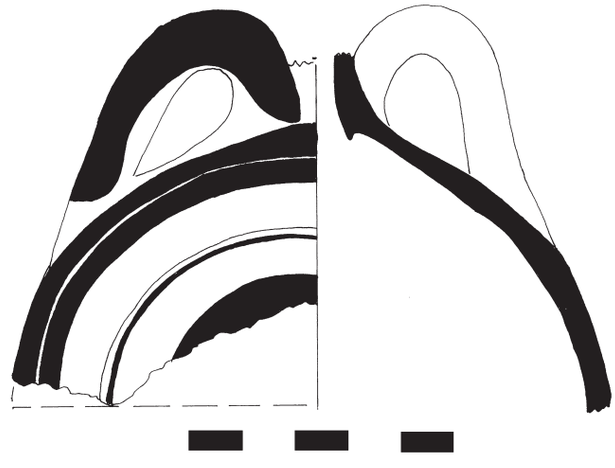


Fig. 7 A fragmentary flask in stratum X12



Fig. 8 A rim sherd from a krater in stratum X12

dred years old "antiques". In Tel Deir ᶜAlla two of the vessels, a flask (FS 192; Hankey 1967: 131, fig. 5:b) and a stirrup jar (FS 180; Hankey 1967: 131, fig. 5:a) were described as first by Hankey (1967: 132) as LHIIIA2, then by Warren and Hankey (1989: 161) as LHIIIB1, meaning that they were already in use for several decades, possibly more than half a century, before the destruction of the temple.

An almost complete cup from the residence published by Beck and Kochavi (1985: 56, fig. 2.7) was quoted by Warren and Hankey (1989: 156) as a Mycenaean cup FS 220. After close examination it has, however, turned out to be an imitation of Mycenaean shape and decoration in local clay and therefore it has been excluded from the discussion.

Other single sherds dating from LHIIIB, found below the destruction deposit, may have belonged to the vessels broken during the period of existence of the Governor's Residence and swept away before the final destruction of the building.

The possible latest sherd in stratum X12, so far unpublished, was found sealed by the destruction deposits outside the residence (Guzowska and Yasur-Landau forthcoming: cat. no. 4). It is a rim fragment of a krater (FS 282), decorated with a paneled decoration separated by a triglyph (FM 75), filled with horizontal zigzag (FM 61: 5). The main panel motif is FM 43, isolated semicircles or some form of a large lozenge (Fig. 8). The vessel can be most likely dated to LHIIIB2. Analogies to identical decoration come from Thorikos and have been assigned by Mountjoy to transitional LHIIIB2–IIIC Early (Mountjoy 1999: 561, fig. 205: 291). Similarly as in case of vessels quoted above, only a single sherd was found from the whole vessel. The krater was then very likely broken and discarded some time before the destruction of the Governor's Residence, whereas the earlier flask and stirrup jar survived, and broke only during the destruction.

D. Stratum X11 (Fig. 9)

After the destruction of Governor's Residence, the settlement in stratum X11 had small rectangular houses, built on the summit of the tell during the 12th century B.C. The finds are very meager, indicating the existence of a fishermen village on the Yarkon River (Kochavi 1989: 79–80; Beck and Kochavi 1993: 68). Very few Mycenaean sherds have been excavated in this stratum, most of them also preserved only in the form of single sherds. Taking into consideration the generally humble nature of the settlement, we assume that also in stratum X11 the Mycenaean material is in a secondary context,

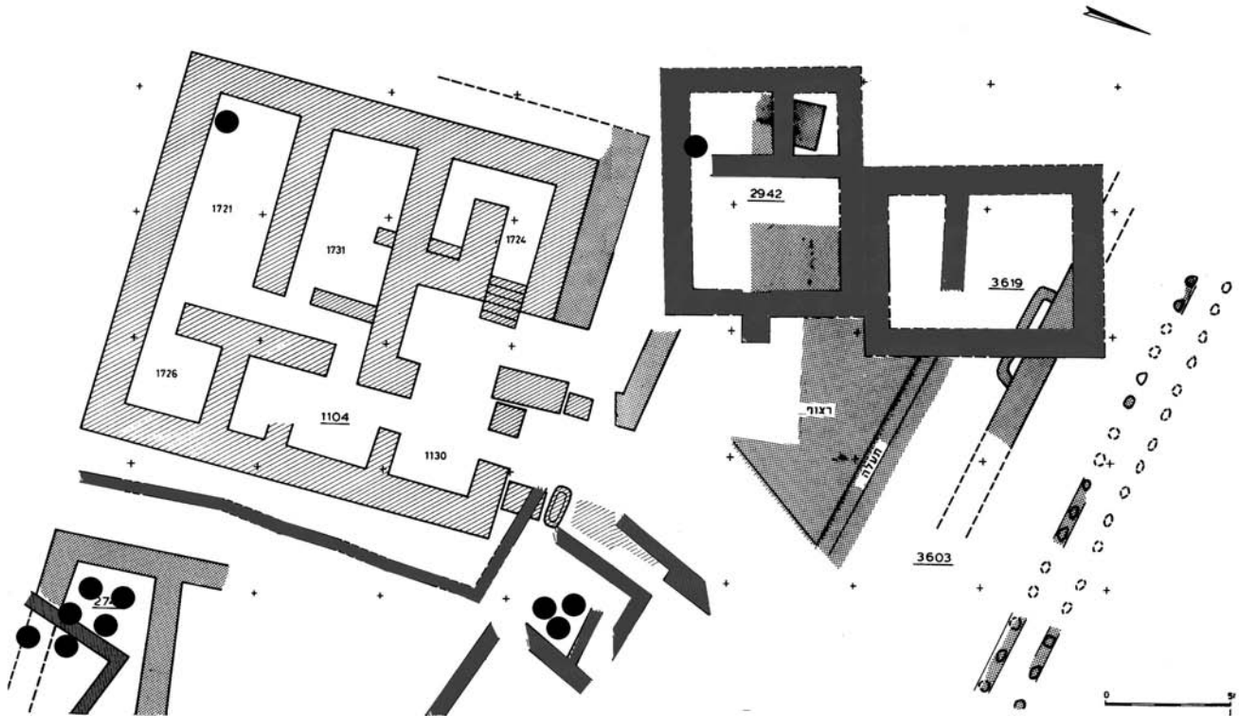


Fig. 9 Distribution of Mycenaean sherds in stratum X11

originating from the period of the Governor's Residence and maybe also transported with the mud-brick into the walls of the houses.

From this stratum came few sherds from a krater with pictorial decoration, most probably LHIIIB (Guzowska and Yasur-Landau forthcoming: cat no. 3). We assume that, like other late Mycenaean imports of the above mentioned LHIIIB stirrup jar, and the LHIIIB or early LHIIIC krater with triglyph decoration the LHIIIB krater was in use in Aphek during the existence of the Governor's Residence.

3. Aspects of consumption of Mycenaean pottery

It is possible to gain several insights on questions of consumption of Mycenaean pottery in Aphek from the entire assemblage originating in residential deposits of strata X13, X12, and X11.[3] The total of the Mycenaean pottery from the settlement strata discussed here consists of 43 closed vessels and 12 open vessels (Fig. 10), representing a ratio of ca. 8 closed vessels, i.e. containers, to ca. 2 open vessels, kraters, and small drinking ware. This conforms to ratios published from other sites in the Levant (Steel 2002: 31; Killebrew 1998: 160). According to Sherratt (in a discussion following Killebrew's paper [1998: 169]) ca. 30 % of the Mycenaean imports to the Levant were non-containers, yet most published deposits point to lower figures. In Beth Shean, an Egyptian stronghold (Hankey 1993: 103), only 15 % of the imported Mycenaean vessels of strata VIII and VII were open vessels. In Tel el-ᶜAjjul (Steel 2002: 33, fig. 3), less than 10 % of the imported Mycenaean pottery were open vessels. The ratios in the "Mycenaean" tomb from Dan (Ben-Dov 2002: 98–118), belonging to the upper elite of the site in the 14th and 13th centuries B.C. include 18 open vessels to 82 closed vessels, the closest to the Aphek ratio. The small and medium-sized closed vessels, forming the majority of imports to the Levant, were argued by Leonard (Leonard 1981: 91–100) to be containers for oils and unguents. Recent work in the Aegean on residue analysis (Tzedakis and Martlew 1999: 152, 173, 196) has demonstrated that wine was also a commodity stored inside medium-sized and large stirrup jars, and there-

[3] Because it is impossible to differentiate between Mycenaean pottery used during the existence of stratum X12 and residual pottery found within this stratum, but originating in the earlier stratum X13, it would be unsafe to draw conclusions on differences in patterns of Mycenaean imports between the 14th and 13th centuries B.C.

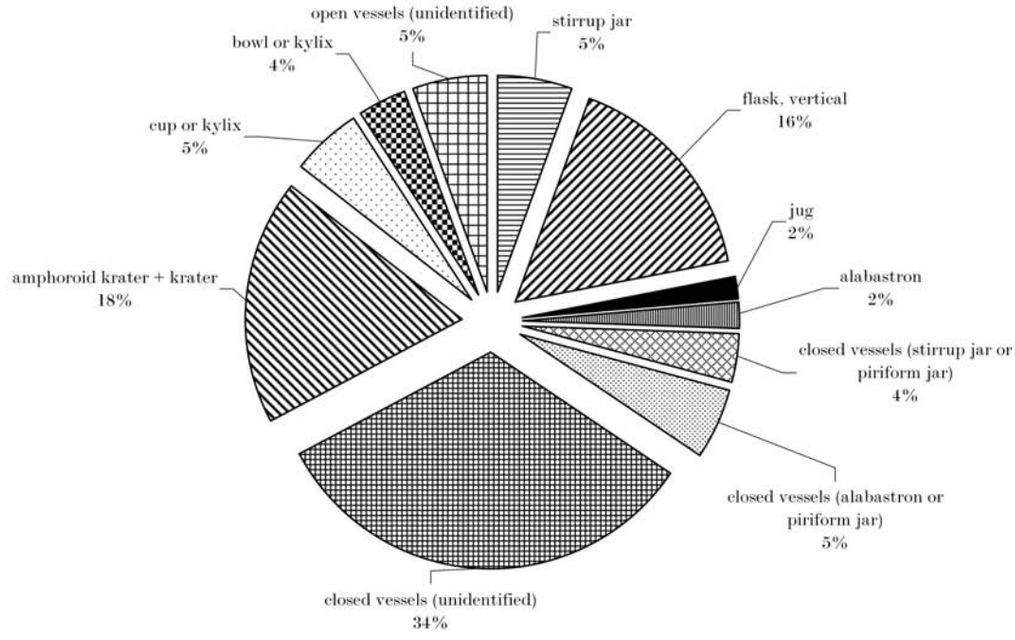


Fig. 10 Ratios of shapes of Mycenaean pottery at Aphek

fore could potentially also be shipped to the Levant.

The small number of imported tableware in the Aphek assemblage opens the question of use of Mycenaean tableware by Levantine elites. Specialized forms of Mycenaean tableware, most notable of which were the "Chariot Kraters" (Leonard 1994: 22–33; South and Todd 1997: 75; Åkerström 1987: 117; Mountjoy 1993: 73; Halstead 1994: 208; Vermeule and Karageorghis 1982: 8–9), were manufactured in mainland production centers as Berbati in order to be exported to Cyprus and the Levant (Gunneweg *et al.* 1994). The scenes on these kraters depicting chariots with Mycenaean elite deliver a complex message combining elite status with consumption of wine in Aegean tableware. The message is most explicit in the case of a krater fragment from Enkomi (Furumark 1941: fig. 75; Vermeule and Karageorghis 1982: III.21; Wright 1996: 304), in which a Mycenaean dignitary with his servant appear behind a chariot, surrounded by a complete Mycenaean drinking kit, including a rhyton, ladle or cup, goblet or krater, a jug, and a chalice. The message may be something like: "This is how a true Aegean nobleman drinks; buy the complete Mycenaean drinking kit!"

The imported kraters and smaller drinking vessels in Aphek may reflect a phenomenon similar to that in LCIIC Kalavassos/Ayios Dhimitrios in Cyprus (South and Russel 1993: 306–308), where imports of Aegean fine tableware pottery were related to high-status dwelling, while a large number of closed vessels were found in more varied contexts. This case indicates a more restricted access to high-statusimported tableware than to closed vessels. Steel (1998: 296) made a similar generalization for the patterns of use of Mycenaean pottery in Cyprus: "…on the one hand, the dinner services and pictorial style were appropriated by the elite as status symbols to define their own exclusivity. On the other hand, the small, more widely available unguent containers were adopted by those members of Cypriot society who chose to emulate the elite but did not have access to the more highly valued Red Lustrous perfume bottles". Judging from the elite residential area in which the Aphek open vessels were found, and the find of sherds of "Chariot Kraters" in what appears to be an elite area in Tel el-ᶜAjjul (Steel 2002: 36–38), it is likely that the situation in the southern Levant was very much the same.

4. Chronological implications

The destruction of "palace VI" has been given a *terminus post quem* by a letter from Ugarit, which was written between the fifth and the sixth decade of Ramses II, i.e. between 1229–1219 B.C. (Owen 1981; Singer 1983). A scarab of Ramses II found in the trough at the entrance to the building (Kochavi 1989: 67) as well as a faience plaque, possibly a foundation plate, found in a later pit with the name of Ramses II (Giveon 1978: 188–190) give further confirmation for

at least a construction date of the building and possibly its main period of use to the days of Ramses II.[4] In addition, no scarabs containing royal names of the 20th dynasty have been found in the Late Bronze Age strata of Aphek. However, a scarab in Locus 4018, stratum X-10 was read by Kitchen to include the name of Ramses IV "*Rʿ-mss mɜʿtj*" (Keel 1997: 84 no. 17). This reading was questioned by Uehlinger (1988: 21, n. 63), yet confirmed in renewed readings by Baruch Brandl and Nir Lalkin (personal communication). Theoretically, this scarab could also have originated from the Egyptian residence, thus lowering significantly its destruction date, yet there is no supporting evidence for such a hypothesis.

As for chronological evidence obtainable from the Mycenaean pottery, the only two above mentioned restorable vessels from the stratum X12 associated with the existence of the Governor's Residence were a stirrup jar and a vertical flask. They were found outside the residence, by its wall, sealed by debris from the destruction. We can assume that only those two vessels were complete at the moment of destruction of the residence. They had fallen to the street together with the remains of the upper floor and were sealed from above with debris from the remains of the roof. Warren and Hankey (1989: 156) chronologically linked the stirrup jar, identified by them to be "mature LHIIIB", to the letter from Ugarit, and thus assigned a destruction date late within the rule of Ramses II to an advanced stage of LHIIIB. However, is the deposition date of the stirrup jar necessarily close to its date of manufacture?

Two phenomena hinder any chronological conclusion obtainable from the Aphek assemblage: The first is the common practice in the Levant to "hoard" Mycenaean pottery through an extended period of time discussed above, the second is the abrupt end of the arrival of Mycenaean imports during the 13th century.

There are no Mycenaean pottery imports later than LHIIIB2 south of the Jazreel valley (Yasur-Landau 2003). Thus, for example, no LHIIIC pottery was found in Lachish, although stratum VI of the site was destroyed in the days of Ramses IV, if not later (Krauss 1994).[5] It is thus impossible to use Mycenaean vessels to date levels deposited later than the 13th century. Thus, from the point of view of Mycenaean pottery as well as Egyptian finds, it is theoretically possible that Aphek was destroyed either at the end of the 13th century or at the beginning and even middle of the 12th century, together with the end of Egyptian administration in Beth Shean and Lachish.

[4] An additional scarab from Locus 4805, stratum X5 containing the throne name of Ramses II (Keel 1997: 89, no. 30), probably also originated in stratum X12.

[5] See, however, that LHIIIC pottery, probably originating in Cyprus was found in Beth Shean (Yasur-Landau 2003) also destroyed during the days of Ramses IV, giving further indication that lack of a certain imported pottery type in a deposit may reflect also patterns of trade, rather than being necessarily a chronological indicator.

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The Public Face of the Absolute Cchronology for Cypriot Prehistory

Sophocles Hadjisavvas[*]

An invitation by the organizers of this Conference to consider the problem of the Absolute Chronology for Cypriot Prehistory gave me the opportunity to revisit some of the earliest studies on Prehistoric Cyprus. I went through almost all the lively discussions which have brought us all here.

I would like in this respect to thank Prof. Manfred Bietak and his collaborators for the invitation and the hospitality extended to me in the beautiful city of Vienna. Particular thanks are due to R. Merrillees who was consulted several times during the preparation of this paper.

Gjerstad's pioneer work "*Studies on Prehistoric Cyprus*" (1926) includes chapters on relative chronology, foreign relations and absolute chronology, a scheme to be followed by all later synthetic works of the Swedish Cyprus Expedition.

Focusing our investigations on the 2nd Millennium B.C. which is the prime interest of this series of Conferences, it is worth mentioning that although 77 years have elapsed since Gjerstad's absolute chronology, only minor modifications have been introduced either by the Swedish Cyprus Expedition or by other scholars. Nevertheless since Gjerstad's first publication a considerable mass of material related to our subject has come to light both in Cyprus and abroad.

New sophisticated scientific methods of dating have also been invented and developed over time. Along with questions related to absolute chronology new theoretical approaches for Cypriot prehistory have been proposed and new terminologies have been used. The terminology for the relative and absolute chronology introduced by the SCE which was the working instrument and a common point of reference for every student of Cypriot prehistory has been questioned.

Although welcomed from the scholarly point of view all these innovations brought about some disruptions to museum officials, not to mention visitors. A simple terminology and clear cut chronology had to be used after of course reaching a consensus, so that publications and museum displays were compatible.

The opportunity for a re-evaluation of the chronology of the earliest prehistory of Cyprus came in 2001 when the French School at Athens and the Cyprus Department of Antiquities undertook the organization of an international conference entitled "The Neolithic Period in Cyprus". On this occasion the first exhibition gallery of the Cyprus Museum was re-arranged for the first time after the 1950s in order to include the new exciting discoveries at Parekklishia-*Shillourokampos* and Kissonerga-*Mylouthkia*. Leading scholars working in this period of Cypriot prehistory were consulted to suggest a working chronology for the Cypriot Prehistory. A chronological outline was set up excluding any terms earlier suggested but not widely accepted in the archaeological literature. We knew in advance that a number of scholars had different opinions about some terms and dates but we tried to produce a chart that should serve the purpose of making the prehistory of Cyprus more intelligible to the general public.

The inclusion of the Akrotiri phase in the Early Aceramic Neolithic met with the disapproval of E. Peltenburg and I fully agree with his reservations. We could not in any way combine the hunter-gatherers of Akrotiri with the first farmers of *Mylouthkia* 1A and *Shillourokampos* Early Phase. Also terms like PPNB which mean something to archaeologists specialising in the Neolithic Levant mean little to the public and are best avoided.

The Chalcolithic/Early Bronze Age transition placed between 2500–2300 B.C. met with Peltenburg's approval on the ground "that human developments were not always straighforward", a position shared by David Frankel and Jenny Webb. Being the excavators of one of the very few settlement sites preserving remains from the Philia phase through the Middle Cypriot Bronze Age the two Australian archaeologists have had an important role to

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play in the establishment of Bronze Age chronology. They suggested the following chart:

Chalcolithic	
Late	2800–2400
Chalco / BA Transition	2500–2300
(including final Late Chalcolithic and Philia Bronze Age)	
Bronze Age	
EC I/II	2300–2100
EC III/MCI	2100–1850
MC II	1850–1750
MC III	1750–1650

This new chronological outline will be used for the re-arrangement of the second gallery of the Cyprus Museum devoted entirely to the Early Bronze Age. The new discoveries resulting from the excavations of an admittedly limited number of settlement sites of this period for the first time in Cyprus have made such a re-arrangement an absolute necessity. All exhibits in this gallery presently come from tomb groups from the north coast of Cyprus. Recent excavations, however, of sites in the south, central and west part of the island have brought in the concept of regional variation.

This new concept has challenged the earlier accepted relative and absolute chronology based on an island-wide development.

The Philia culture conference organized by the University of Cyprus last year, although it clarified some critical questions in the earliest phase of the Bronze Age, only partially touched on questions related to absolute chronology.

The differences between the supporters of a low and a high chronology for the Egyptian Middle Kingdom, however, remain and therefore affect their suggestions concerning the Middle Cypriot Bronze Age chronology. One of the supporters of the low chronology is P. Åström (1987, 62) who, by adopting in his latest proposal a higher starting point and a lower closing point, tends to extend the length of the MC period.

At the opening of the Late Bronze Age the pattern of culture and habitation was substantially transformed, as a result of the growing importance of overseas trade relations which prompted movements of towns closer to the coast and manufacture of vases with a view to the export market (Catling 1962, 142).

It was on these interconnections with the outside world, particularly Egypt, that Merrillees built his absolute chronology of the Bronze Age in Cyprus in 1977, in the absence, at that time, of any radiocarbon dates. As he pointed out, he followed the relative chronology defined by P. Åström in 1972.

The LBA represents an interesting case as it is one of the most thoroughly investigated periods in Cypriot archaeology. Although masses of material have been published, comparable studies on absolute chronology have not been undertaken. In some instances "leading" archaeologists introduced or rather modified earlier adopted chronologies without any scholarly justifications. (Bucholz and Karageorghis 1973, 138). For obvious reasons archaeologists dealing with Cypriot prehistory had the option either to adopt this chronology or simply avoid including any absolute dates in their publications.

Some scholars (e.g. B. Knapp) by adopting a new terminology such as Prehistoric Bronze Age and Protohistoric Bronze Age have tried to highlight the historicity of the Late Bronze Age horizon. Middle Cypriote III to Late Cypriot I would mark the beginning of Protohistoric BA on the island. The historicity, however, of the LBA horizon in Cyprus is closely associated with the identification of Alashiya with Bronze Age Cyprus. A recent conference on the relations between Egypt and Cyprus dealt at length with this important issue of Cypriot prehistory.

Three of the contributors, Kitchen, Goren and Grimal, quite independedly of each other, have made the identification of Alashiya with Bronze Age Cyprus more than credible. Nevertheless some reservations expressed by Robert Merrillees should be taken into consideration before any settlement of this long standing issue.

By touching on the question of historicity we have to, once again, deal with the question of the introduction of the monumental "ashlar buildings" all over Cyprus. In earlier literature this method of construction was related, at least in time with the arrival of the Achaeans in Cyprus. It was in any case much later than the Amarna letters which mark a time of considerable wealth for Alasiya.

Recent developments, however, have bridged the gap between the Amarna period and the construction of the ashlar buildings, which is a strong indication of the wealth of the Cypriots based on copper and olive oil production.

Allow me to use one example from my own excavations at Alassa-Palaiotaverna where one of the most impressive ashlar buildings has recently been excavated.

The storeroom containing a large number of pithoi is a later extension to the original building dated both conventionally and by C^{14} to the LCIIC period. The north storeroom and the south wing which preserved evidence of Aegean influence were dated in the LCIIIA period, I confess without serious

justification but mostly on "historical grounds" based on the Aegean influences.

The two most recent radiocarbon dates, however, for olive seeds from the north storerooms of Building II at Alassa lie within the period 1260–1120.

If we accept that the latest short-lived sample was deposited at the site, as it was found on floor level, just before the destruction by fire of Building II, then we have to accept that Building II was destroyed at the very end of LCIIC and only the south wing with its obvious Aegean connection was re-inhabited during the LCIIIA period, a dating confirmed by C^{14} dates.

Leaving aside the question of the historicity of the later part of Bronze Age Cyprus and returning to our request for proposals concerning the absolute chronology, all respondents pointed to Sturt Manning as the key person to be approached. It is true that Manning had initiated, with the approval and collaboration of the Department of Antiquities, a project to seek suitable samples for radiocarbon dating relevant to the LCII C period from as many sites as possible (Manning *et al.*, 2001).

At last a shortcoming pointed out by Merrillees (1977), that of the absence of any radiocarbon dates published for the Bronze Age of Cyprus, has now been rectified. A large additional number of both long lived and short lived samples are awaiting carbon 14 dating. I am very hopeful that the large number of new C^{14} dates will bridge any gaps between "historical" and absolute dates and contribute to resolving a question of primary importance, that of the duration of each period in the relative chronology.

For the sake of discussion and only, without having the intention to propose here a final chart for the Prehistoric period in Cyprus, I suggest the following chronological outline:

Pre-Neolithic	C.10000
Aceramic Neolithic	8200–5000
Ceramic Neolithic	5000–4000
Chalcolithic	
Early	4000–3200
Middle	3200–2900
Late	2800–2400
Chalcolithic / Bronze Age transition	2500–2300
Bronze Age	
EC I/II	2300–2100
EC III/MCI	2100–1850
MC II	1850–1750
MC III	1750–1650
LC IA	1650–1550
LC IB	1550–1450
LC IIA	1450–1375
LC IIB	1375–1300
LC IIC	1300–1200
LC IIIA	1200–1125
LC IIIB	1125–1050

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Tell Kazel and the Mycenaean Contacts with Amurru (Syria)

Reinhard Jung[*]

Tell Kazel is situated in coastal Syria near the modern city of Tartous and close to the present day border to Lebanon. The Tell lies in the Akkar Plain, about 3.5 km from the present-day seashore, at the river Nahr-el-Abraš. It measures 310 × 280 m and rises approximately 20–25 m above the Akkar Plain. The current excavations on the Tell have been conducted on an annual basis since 1985 by the Museum of the American University of Beirut under the direction of Leila Badre.[1]

Different ancient sources suggest that Tell Kazel should be identified with the city of Sumur/Simirra. Sumur belonged to the region, which during the Late Bronze Age was called *Amurru* and stretched between the Mediterranean Sea and the plain of Homs.[2] Whereas the Akkar Plain was divided between three city states during the Middle Bronze Age, the situation changed around the middle of the second millennium BC. Tell Kazel was then the sole urban site with monumental buildings in the plain. This is the most important argument for its identification with the city of Sumur.[3] During the 14th century BC Amurru was constituted as a kingdom under Aziru and eventually passed to Hittite overlordship, but always remained a point of contention by Egyptians and Hittites.[4] Sumur may even have been the capital of the kingdom of Amurru. At any rate it was definitely an important centre of this principality and played a part in its historical developments.[5]

From the time of Hittite domination there is a historical document, which gives us some insight into the background of our Mycenaean pottery finds at Tell Kazel. In the Šaušgamuwa treaty between the Hittite Great King Tuthaliya IV (or renumbered: Tuthaliya III) and Šaušgamuwa, the last known Bronze Age king of Amurru, a trade embargo is imposed upon Assyria by the Hittites. In this context Tuthaliya IV writes to Šaušgamuwa: "*Wie der König von Assyrien (aber?) Meiner Sonne Feind (ist), so soll er auch dir Feind sein! Ein Kaufmann von dir darf nicht ins Land Assyrien gehen, einen Kaufmann von ihm aber darfst du nicht in dein Land lassen, er darf (auch) nicht durch dein Land gehen! … Und weil ich, Meine Sonne, mit dem König von Assyrien Krieg führe: … Ke[in] Schiff des Landes Ahhijawa soll zu ihm fahren!*".[6] The natural route from the seashore

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[1] Following my first visit to the Tell in 2001, Leila Badre entrusted me with the study of the Aegean pottery from the site – an opportunity for which I am very grateful. I would also like to thank Rami Yassine, who digitalised my drawings, and Laure Salloum, who together with Leila Badre provided me with the vital stratigraphical assignments of the sherds. Thanks are also due to Emmanuelle Capet, who gave me an advance copy of her study on Area II. For useful comments (especially on the pieces Fig. 8,2 and 11) I thank Penelope Mountjoy. The German Archaeological Institute (DAI), Near East Section, provided substantial financial support, which allowed me to participate in the 2003 campaign at Tell Kazel. For this support I express my sincerest gratitude to the Institute and especially to Markus Gschwind, DAI Damascus. Last but not least I wish to thank Emily Schalk, who checked the English. Drawings are published according to the conventions in Aegean archaeology. Thus, all paint on the vessel exterior is given in black and all paint on the interior in grey, irrespective of the actual paint colour (which on Mycenaean pots can differ in places on one and the same vessel without being intentionally bichrome). Furthermore, I would like to remark that drawings and dates given in the present article represent the actual state of research. Differences in respect to previous preliminary publications of Mycenaean pottery from Tell Kazel indicate progress in research. Vague statements regarding this pottery in previous reports should not be used to argue against definite statements in the present study and vice-versa. A more extensive treatment of the Mycenaean pottery from Tell Kazel and its cultural and historical implications will appear in *Damaszener Mitteilungen* 15.

[2] Klengel 1992, 161.

[3] Thalmann 2002, 364 fig. 3; 365; see also Klengel 1984, 16–18, and Badre 1995, 88.

[4] Klengel 1992, 160–174.

[5] Klengel 1984; idem 1992, 164; Singer 1991, 158; idem 1999, 645 n. 126.

[6] Kühne and Otten 1971, 14–17 Rs. IV, lines 14–17.19.23; There is a more recent alternative reading, in which "*Kriegsschiff [von Amurru]*" instead of "*Schiff des Landes Ahhijawa*" has been proposed, see Steiner 1989; but cf. Klengel 1995, 171. To most philologists and historians, however, this new reading seems to be less plausible because of the treaty context, and because it would constitute a

towards Assyria runs through the "Homs Gap", opening between the Lebanon Mountains and the Jebel Ansariye. The heartland of Amurru, the Akkar Plain, is part of that geographical feature. Therefore, the king of Amurru is urged to block this vital land route from his seashore to Assyria.[7] There is widespread agreement today, that Ahhiyawa can be identified with the kingdom of Mycenaean Greece or with one of several Mycenaean kingdoms. Ahhiyawa repeatedly came into conflict with the Hittites during its political and military expansion to western Asia Minor in the 14th and 13th cent. BC.[8] The Šaušgamuwa treaty can be approximately dated between 1234 and 1223.[9] Thus, we may expect the Amurru-Ahhiyawa trade to have functioned at least until the beginning of the last third of the 13th century BC.

With Tell Kazel as the principal site on the Akkar Plain one should expect that the Ahhiyawa trade through the Homs Gap would have left some traces at the site. It is now obvious, I think, to assume that the significant quantities of imported Mycenaean pottery found at Tell Kazel were brought by those "ships of Ahhiyawa".[10]

During the recent excavations at the Tell Mycenaean pottery was mainly found in two large excavation areas, Areas II and IV. Area II is an extensive habitation quarter, whereas in Area IV a huge temple complex was uncovered. The excavation has reached levels of LBA II date in both areas. Leila Barde and Emmanuelle Capet established the synchronisation of the two independent areas based on the characteristics of architecture and finds as well.[11]

The repertory of imported Aegean pottery is astonishingly Helladic in character. This holds true for types and decoration and also for fabrics. Cypro-Mycenaean products, such as imitations of Cypriote handmade types, stemmed shallow bowls FT 309–310 and kraters painted in the Pastoral Style, which are all known e.g. from the neighbouring kingdom of Ugarit,[12] are almost completely absent at Tell Kazel.[13] Similarly, Late Minoan III pottery is absent at Tell Kazel, but by contrast present at Ugarit.[14] It is my impression that the majority of the Aegean imports is not only Hellado-Mycenaean in type, but even of Argive production. In the meantime, a programme of NAA analyses by Hans Mommsen in Bonn and petrographic analyses by Marie-Claude Boileau in Athens has confirmed this macroscopic fabric classification. Considering this origin, in the present study the finds from Tell Kazel are preferably evaluated in an Argive perspective.[15]

However, even though the majority of the imported Mycenaean pottery originates from the Argolid, it should be noted that the percentages of imported types at Tell Kazel reflect local, Syrian preferences and do not copy Helladic vessel sets. Amphoroid kraters FT 54–55 predominate over ring-based kraters FT 281/282 and stemmed kraters FT 8/9. Shallow cups and bowls (mainly FT 295/296[16]) are more common than kylikes FT 257–258, while deep

hapax legomenon, see Lehmann 1991, 111; Singer 1991, 171. Cf. also Mayer 1995, 212 (in favour of the older reading); Dietrich and Loretz 1998, 341 (undecided).

[7] Klengel 1992, 173.

[8] For an overview see Niemeier 1999. Even in Heinhold-Krahmer's critical view (2003, 207–210; 214) Ahhiyawa is most probably located in Mycenaean Greece.

[9] Van den Hout 1995, 114.

[10] On the other side of the sea possibly the group of Kassite cylinder seals and of pieces of lapis lazuli from the Mycenaean palace at Thebes in Boeotia can be interpreted as a royal gift by the Assyrian king Tukulti-Ninurta I to the kingdom of Ahhiyawa in the last decades of the 13th century BC, as Edith Porada proposed (Porada 1981/82, 68 f.; Mayer 1995, 212). The stratigraphic context of this treasure most probably belongs to the final destruction of the palace (Aravantinos 2001, 97 f.) in LH IIIB Final.

[11] Badre 2003; Capet, 2004, 117.

[12] J.-C. Courtois and L. Courtois 1978, 294 f. fig. 33,21; 330 f. fig. 45,1.2; Leonard 1994, 104 nos. 1591–1592; 127 nos. 1904–1906; Karageorghis in: Yon, Karageorghis and Hirschfeld 2000, 57 f. nos. 47–49; 62 no. 64; Hirschfeld ibid., 148 nos. 424–426; 157 nos. 473.474; 236 fig. 26,424–426; 241 fig. 31,473.474; 251 pl. 9,473.474.

[13] The sherd of a Pastoral Style krater from Area II is an exceptional piece.

[14] J.-C. Courtois and L. Courtois 1978, 346 f. fig. 54,4524; Hirschfeld in: Yon, Karageorghis and Hirschfeld 2000, 125 no. 280; 229 fig. 19,280; 247 pl. 5,280.

[15] An additional reason is the fact, that the Argolid still provides the most important settlement stratigraphies on which the Mycenaean pottery chronology is based. For the results of the analyses see Badre, Boileau, Jung, and Mommsen 2005.

[16] Furumark defined two different types for these shallow bowls: FT 295, "Hellado- and Rhodo-Mycenaean", and FT 296, "principally Levanto-Mycenaean" (Furumark 1941, 636). However, neither his typological subdivision nor his geographical assignment seem to be valid any more. There is no real difference in shape between some of the pieces cited and assigned by him to the two different types; compare FT 295 no. 3 (A. Wace 1921–1923, 22 fig. 6,g) and 13 (Mountjoy 1999, 129 fig. 29,214) with FT 296 no. 3 (Yon, Karageorghis and Hirschfeld 2000, 239 fig. 29,450) and 34 (SCE I, pl. 117,E.18.86). For similar comments see Leonard Jr. 1994, 123 f. The carinated bowl FT 295 should, however, be clearly separated from the rounded type FT 295/296 (cf. Mountjoy 1986, 153 f. with fig. 197,1;

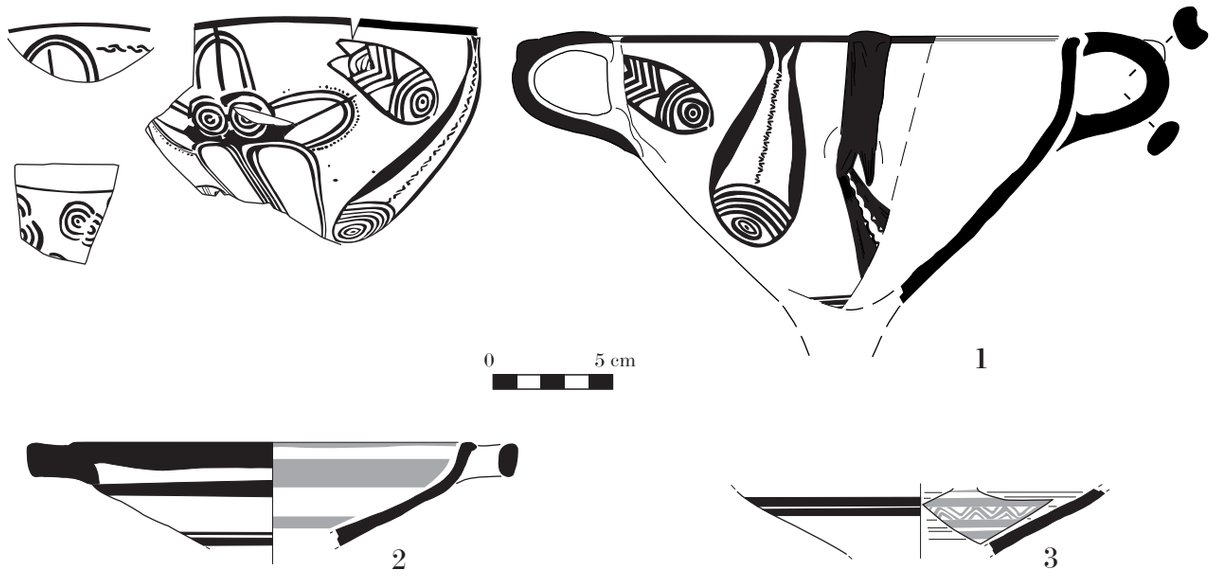


Fig. 1 Imported Mycenaean pottery from Area IV, Level 6, upper or lower floor. Scale 1:3

bowls FT 284/285 are almost totally missing. In addition, a high percentage of small closed shapes – mainly stirrup jars, but also alabastra, globular flasks and piriform jars – was noted. This overall picture contrasts strongly with settlement assemblages from the Greek mainland, but it resembles those from other Near Eastern and also Cypriote sites.[17]

The stratigraphical discussion of the finds from Tell Kazel cannot yet be based on strictly quantitative analyses, as the study of the material is still under progress. Qualitative and semi-quantitative data can, however, be presented. They are representative for the Mycenaean repertory from Tell Kazel.

Phase 1

So far the earliest phase with Mycenaean imports is mainly represented by Level 6 in Area IV and dates to Late Bronze Age II. In some places this Level can be subdivided by means of two superimposed floor levels, inside the temple cella, but mainly outside in the wide courtyard to the north of the temple.[18] Until now no significant Mycenaean material can be assigned to the lower floor with any certainty. Only a general ascription to Level 6 is possible for some pieces, which could originally have belonged to the lower floor. One of these is a large (ca. 19.5 cm rim diameter) pictorial kylix FT 257 with horned handles. It shows vertical hybrid flowers in the centre of both sides, flanked by fish at least on one side (Fig. 1,1). The production of this shape in combination with the hybrid flower motif began in LH IIIA Late and continued during LH IIIB Early,[19] whereby the fish on this shape are unique.[20] Turning to other shapes, the first shallow rounded bowls with thickened rim and strap-handles FT 295/296 belong to the same stratigraphical horizons. They either carry only

Podzuweit 1992, chapter "Knickwandschale Nr. 5"). Subdivisions of FT 295/296 should be based on rim variations. Finally, the rounded FT 295/296 is not only present in the Levant, but in Greece as well (Verdelis, E. French and D. French 1966, 147 fig. 6,3.5; 148). Moreover, many of the Cypriote and Levantine finds might turn out to be imports from the Greek mainland, once they are analysed by NAA.

[17] Cf. Leonard Jr. 1994; Steel 1998, 286–288; Hirschfeld in: Yon, Karageorghis and Hirschfeld 2000, 71 f.

[18] Badre in: Badre and Gubel 1999–2000, 137 fig. 8; 139; 142–145.

[19] Apart from the fish a good parallel can be found in a LH IIIB Early context at Thebes, 14 Oedipus Street (no secure closed context: "group B"), see Symeonoglou 1973, 23; 27 f.; 40 no. 69; pl. 57 fig. 195–196. For the production period of shape and hybrid flower motif in the Argolid see E. French 1965, 165 fig. 2,5.8; 180 fig. 7,6; 201 f.; Schönfeld 1988, 155 tab. 1 no. 7; 158; 161; 168; 180; 182. In the present article the relative phase names of the Tiryns system are used, because the Tiryns Lower Citadel excavations in the 1970s and 80s have produced a continuous stratigraphical sequence from LH IIIA Late down to Submycenaean, to which single phased contexts (e.g. from Mycenae or Tsoúngiza) can be linked, cf. Kilian 1988a, 117 f.; 120 f.; Schönfeld 1988, 163 tab. 4; Güntner 2000, 3; 362–376.

[20] The fragment listed by Güntner as "Kylix" is rather a stemmed bowl; cf. Güntner 2000, 12; 129; pl. 61,1.

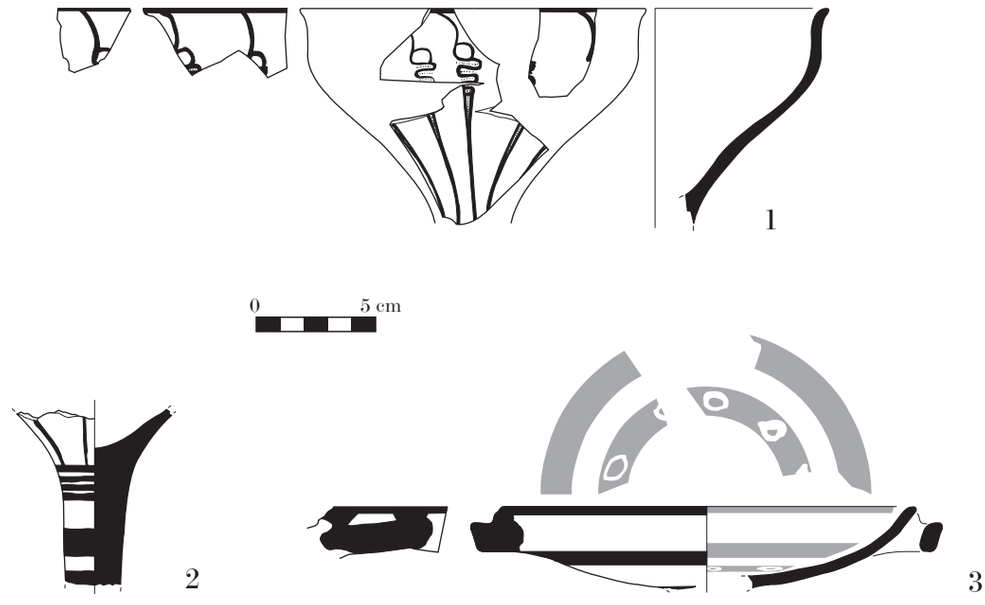


Fig. 2 Imported Mycenaean pottery from Area IV, Level 6, upper floor (1.3) and upper or lower floor (2). Scale 1:3

linear decoration (Fig. 1,2) or they show additionally different motifs on their interior (Fig. 1,3). Comparable linear bowls are attested since LH IIIB Early at Mycenae,[21] whereas those with interior motifs seem to have started with LH IIIB Middle.[22] The scarcity of shallow bowls in stratified settlement contexts in Greece, however, limits their reliability as chronological indicators abroad.

Phase 2

Some indicative pieces were found on the upper floor level of building Level 6 in Area IV. The first is a kylix FT 258B showing a continuous row of vertical whorl-shells. Both shape and motif suggest a date from LH IIIB Early to IIIB Middle (Fig. 2,1).[23] A kylix fragment preserving parts of bowl and stem is decorated with the same pattern, but cannot be assigned to a specific floor of building Level 6 (Fig. 2,2). A shallow rounded bowl with lipless rim and strap-handles FT 295/296 should probably be ascribed to the upper floor level (Fig. 2,3). On the lowest of its interior bands small circles are added in white paint. In the Argolid the earliest shallow bowls decorated in this technique are known from LH IIIA Late Mycenae, but more pieces were found in later contexts (LH IIIB Middle–Final) at Tiryns.[24]

Phase 3

From the next stratigraphical phase onwards, both Late Bronze Age excavation areas on the Tell provided architectural contexts with Mycenaean pottery. Phase 3 is represented by contexts from the lower floor of Level 5 in Area IV and from Level 6 in Area II. From Area IV, Level 5, lower floor, there is a krater with either vertical handles and stemmed foot FT 8/9 or horizontal handles and ring base FT 281/282, which carries an octopus motif (Fig. 3,1). It has a rim band decoration (type 7.1: rim band and band below the rim inside and out), which is not found at Mycenae and Tiryns in contexts before

[21] Mycenae, LH IIIB Early: E. French 1965, 177; 180 fig. 7,26; 186 f. fig. 9,5.6. The shape is also attested at Tiryns, where its innovation falls into LH IIIB Early, see Schönfeld 1988, 155 tab. 1,59; 166.

[22] Mycenae, LH IIIB Middle: E. French 1966, 223; pl. 49,e.

[23] Schönfeld 1988, 155 tab. 1,43, tab. 2,39; 158; 164; 167–170; 175 fig. 5,4; 182 f.; 186; 188; 192. This date holds for kylikes FT 258B with a continuous row of vertical whorl-shells. Kylikes FT 257 with a hybrid flower flanked by vertical whorl-shells already appeared in LH IIIA Late, see E. French 1965, 165 fig. 2,8.

[24] Mycenae, LH IIIA Late: E.B. Wace 1954, 277; pl. 48,c,10.11; cf. Schönfeld 1988, 166. Tiryns, LH IIIB Middle: Schönfeld 1988, 190; 193 fig. 9,13.16. Tiryns LH IIIB Developed–Final: Verdelis, E. French and D. French 1966, 147 fig. 6,3; 148. Tiryns, LH IIIB Final: Podzuweit 1981, 198; 199 fig. 51. In general see Podzuweit 1992, chapter "Schale Nr. 1" pl. 38,1–6.10.

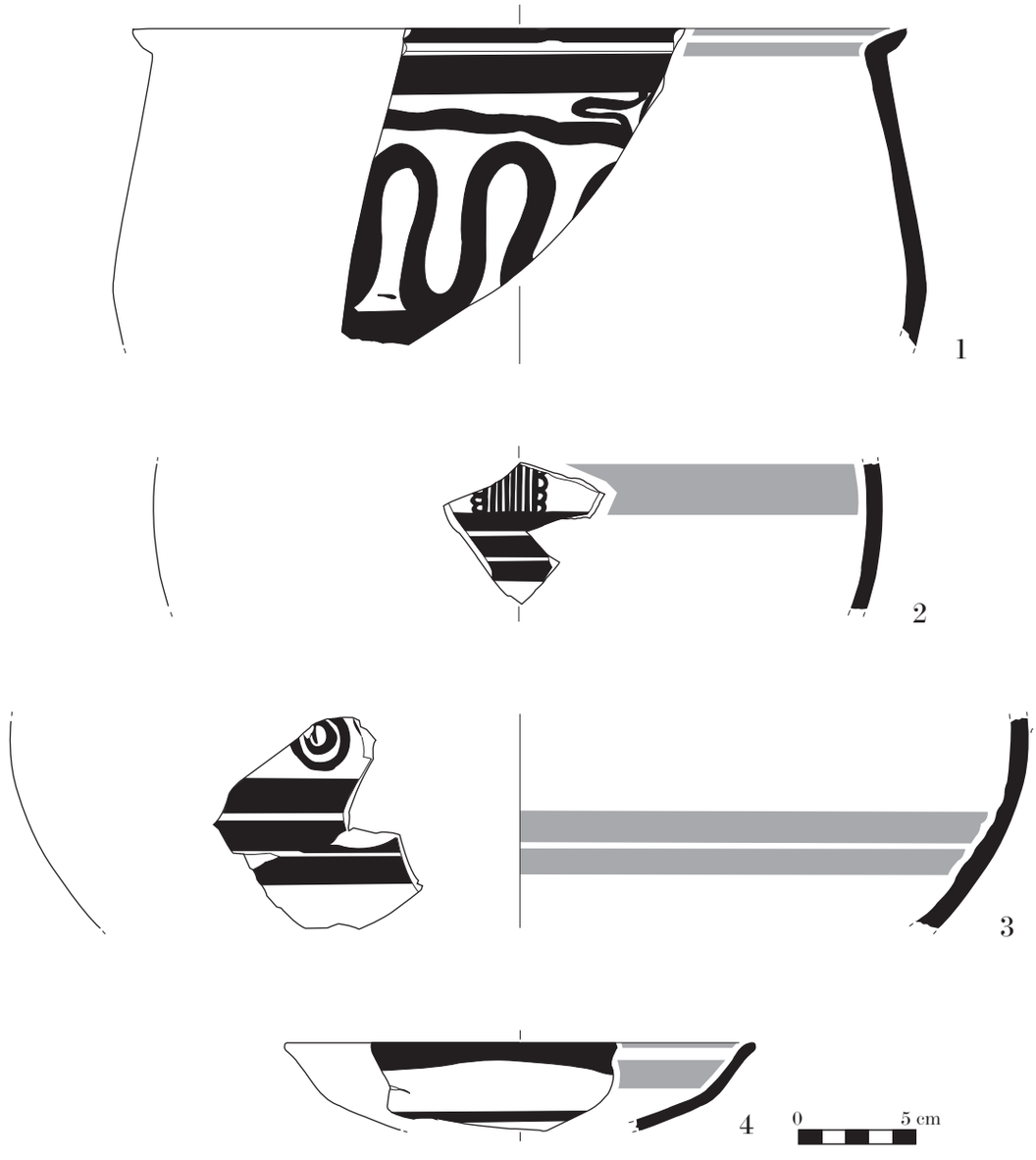


Fig. 3 Imported Mycenaean pottery from Area IV, Level 5, lower floor (1.4) and lower or upper floor (2.3). Scale 1:3

LH IIIB Middle.[25] This kind of rim decoration became very common by LH IIIB Developed and especially IIIB Late.[26] The octopus motif was not used very often for decorating kraters in the Argolid. There is, however, an example of FT 8/9 from a LH IIIB burial at Ialissós on Rhodes,[27] which is a very good stylistic match for our piece. Wolfgang Güntner regards it as intermediate between the LH IIIB Middle and Developed styles.[28] Two further krater fragments can be ascribed to the ring-based type with horizontal handles FT 281/282, because they show bands on the middle or lower part of their

[25] Mycenae, LH IIIB Middle: Mountjoy 1976, 85 fig. 4,18. Tiryns, LH IIIB Middle: Schönfeld 1988, 189; 191 fig. 8,6.8.14. For the typology of linear decorations used in the present study see Podzuweit 1992, suppl. 78; Jung 2002, 575–580; pl. I–XVII.

[26] Podzuweit 1992, chapter "Krater"; suppl. 39.

[27] Benzi 1992, 357 no. 2; pl. 90,g; Mountjoy 1999, 1021 no. 101; 1022 fig. 417,101.

[28] Cf. Güntner 2000, 319 n. 297.

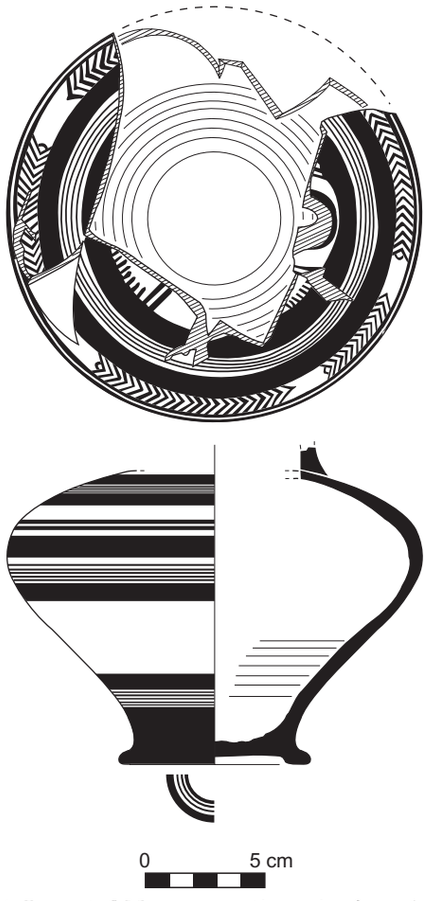


Fig. 4 Imported Mycenaean stirrup jar from Area IV, Level 5, lower floor. Scale 1:3

interior wall[29] (Fig. 3,2.3). Production of FT 281/282 in the Argolid may have commenced during LH IIIB Early, but it was increased only by LH IIIB Middle.[30] Unfortunately, both sherds from Tell Kazel cannot be securely assigned to one of the two superimposed floor levels of building Level 5 of Area IV.

Moving to the closed vessels, two stirrup jars should be noted first, one from Area IV, Level 5, lower floor, the other from Area II, Level 6 (Fig. 4; 5,1). The first stirrup jar represents FT 182 with depressed shoulder, conical lower body and torus base. This type was not produced before LH IIIB Middle,[31] as the limited evidence of well preserved examples in stratified settlement contexts in the northeast Peloponnese suggests. The second vessel (Fig. 5,1) is a medium sized globular stirrup jar FT 170, which judging by its slightly depressed shoulder and conical lower body shows a typological tendency towards the later FT 175. Its spout and false neck are joined by a single oval band (without extra circles around the bases of spout and false neck). This kind of decoration between false neck and spout was thought to be painted only from LH IIIB Developed onwards,[32] but stirrup jars from Thebes, 14 Oedipus Street,[33] and Mycenae, West House,[34] prove that there were examples in LH IIIB Early and Middle respectively. On the shoulder of our stirrup jar there is a row of alternating V and Λ motifs. Such aligned motifs arranged in a circle are typical for stirrup jars especially in LH IIIB Final.[35] Although there is no stratified exact parallel for this piece, a date in the second rather than the first half of LH IIIB seems reasonable.

From Area II, Level 6, there are two squat stirrup jars FT 178/180/181 (Fig. 5,2.3) – a shape which is attested from LH IIIB Early to Middle in the Argolid.[36] Both do not exhibit any patterns on their shoulders, a feature which is rather rare for stirrup jars used in the Aegean and much more common in the range of pottery exported to the Near East.[37] Amongst the open shapes shallow rounded bowls FT 295/296 with lipless rim and linear decoration only are still present in this phase,[38] as demonstrated by examples from both excavation areas (Fig. 3,4; 5,4).

[29] Such bands do not occur on kraters with a stemmed foot and vertical handles FT 8/9 (Mountjoy 1986, 110).

[30] Cf. Mountjoy 1986, 115.

[31] Tsoúngiza, EU 2 Pit 1, LH IIIB Middle: Thomas 1992, 45; 135 no. TS 64; 138 f. no. TS 77; 543 fig. 9,10.23.

[32] Podzuweit 1992, chapter "Kleine Bügelkanne"; suppl. 60 KN-HE no. 3.

[33] Symeonoglou 1973, 19; 23; 33 f.; 36–38 no. 29–43; 41; pl. 42–50 fig. 127–171.

[34] Verdelis 1963, 21 fig. 25 (left one and probably right one, too).

[35] Podzuweit 1992, chapter "Kleine Bügelkanne"; suppl. 61. They are also found on stirrup jars of the following phase LH IIIC Early, cf. Mountjoy 1999, 154 f. with fig. 40,310.311; 346 f. with fig. 117,100; 682 f. with fig. 261,179.

[36] Mycenae, LH IIIB Middle: E. French 1966, 218 fig. 1,4; 219; Wardle 1969, 267 fig. 2,2.6; 270. Tiryns, LH IIIB Early and Middle: Schönfeld 1988, 155 tab. 1,52; 171 fig. 3,5.6; 192.The piece in Fig. 5,3 was in fact found in a silo of Level 6 Final, where it had been deposited together with other Mycenaean and Cypriot imports. Thus, it seems to have been in secondary context and most probably had already been imported during the preceding Level 6.

[37] Leonard Jr. 1994, 59.

[38] Analogous examples from the Argolid confirm, that the production period of linear shallow bowls FT 295/296 continued in LH IIIB Middle (E. French 1966, 220 fig. 2,9; 223), IIIB Developed (Verdelis, E. French and D. French 1966, 147 fig. 6,5 [Developed–Final]), IIIB Final (E. French 1969, 84 fig. 10,6–8; 85; Wardle 1973, 318 no. 88; 319 fig. 12,88) and LH IIIC Early (Podzuweit 1992, chapter "Schale Nr. 1"; suppl. 48).

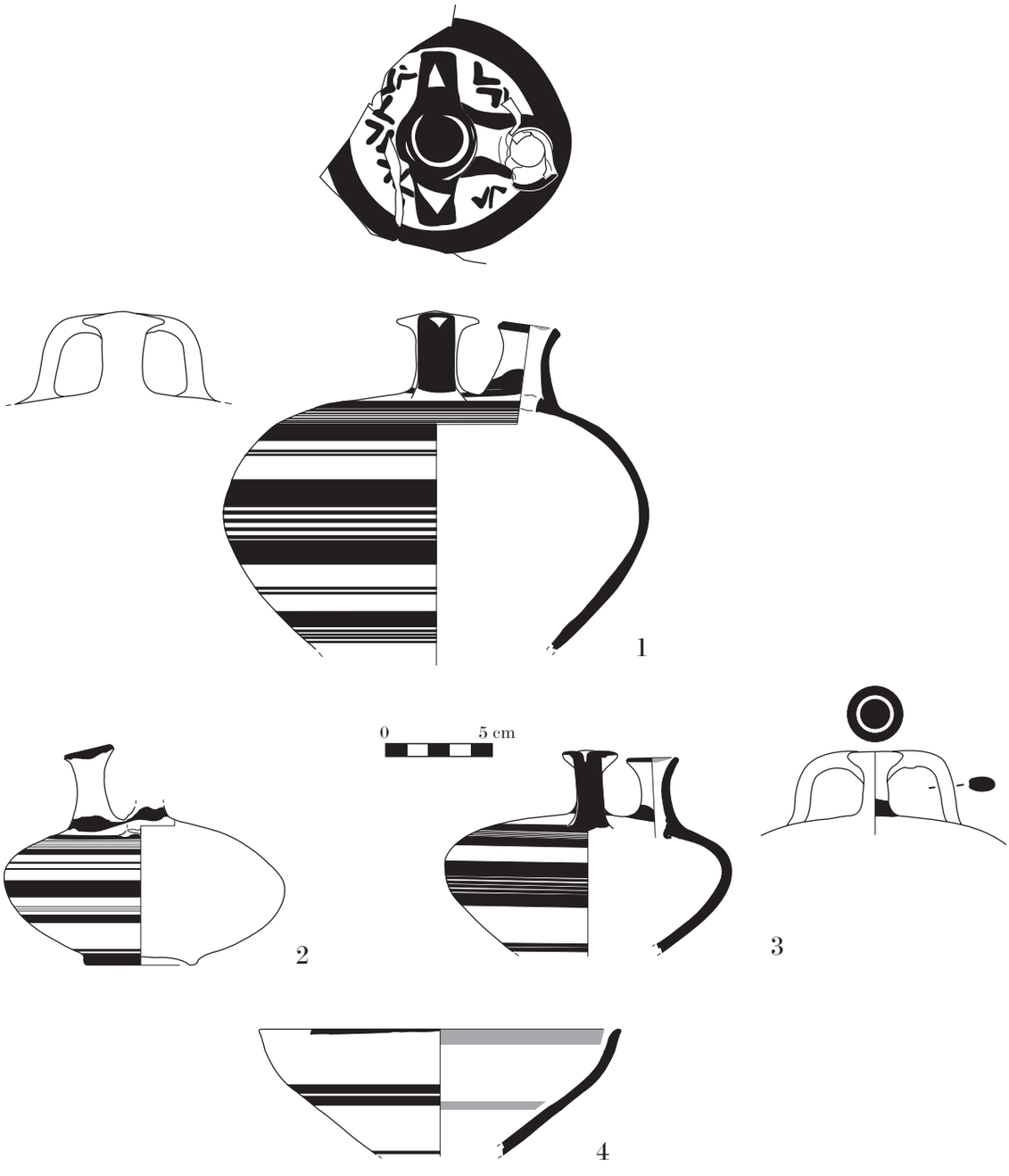


Fig. 5 Imported Mycenaean pottery from Area II, Level 6 (except for no. 3). Scale 1:3

Phase 4

The next subphase in Area IV, the upper floor of Level 5, is terminated by a severe conflagration, contemporary with the burned Level 6 Final of Area II. In Levantine chronological terms we are at the very end of Late Bronze Age II or during the transition to Iron Age I.[39] Imported Mycenaean vessels are very rare in this phase, and none of the imports from the preceding levels must necessarily be dated to LH

[39] Capet, 2004, 66, 99, 117.

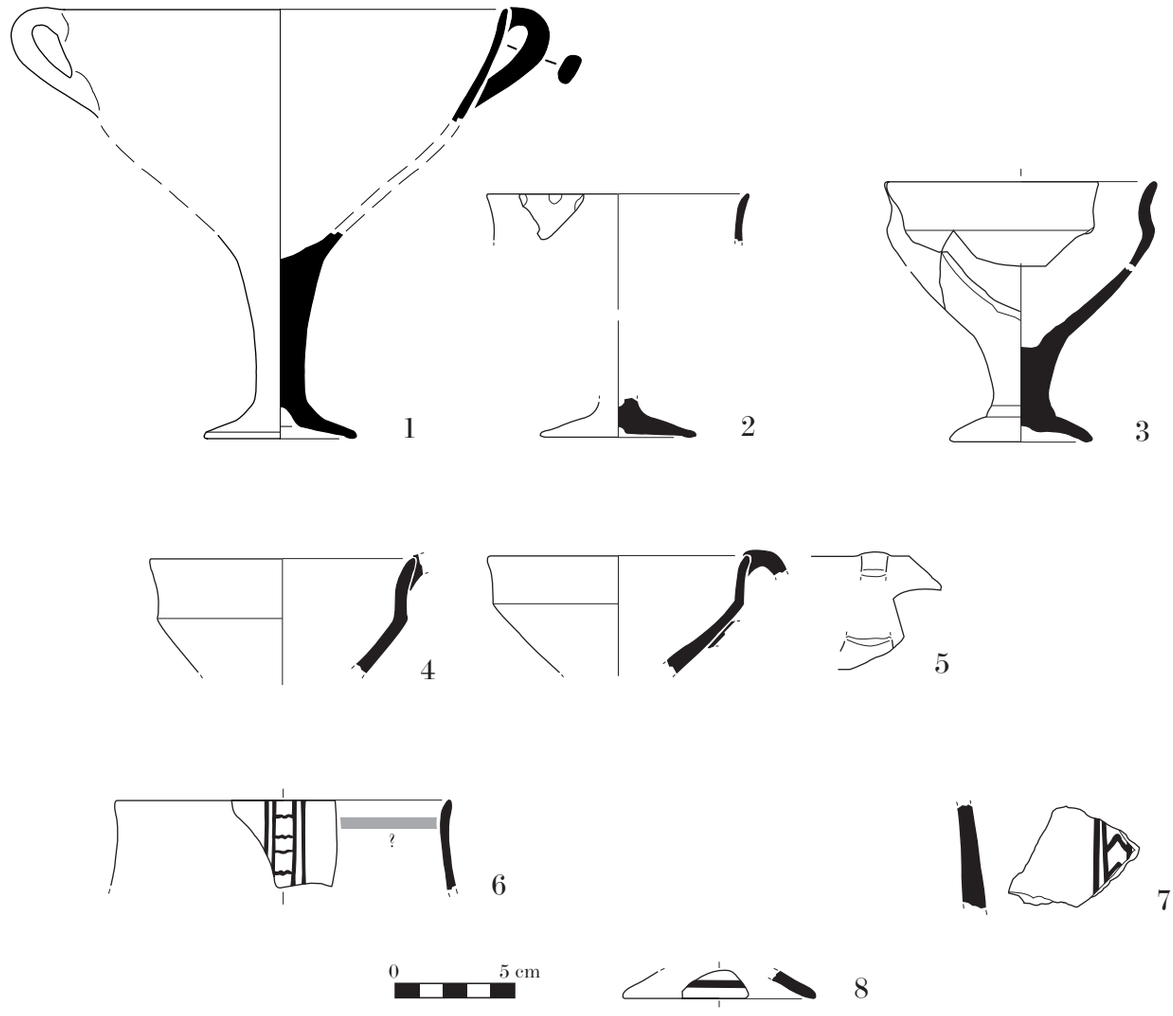


Fig. 6 Group of local Mycenaean pottery from Area II, Level 6 Final, above last floor of rooms U-V in Building II. Scale 1:3

IIIB Final. This observation may be of importance in correlation with the Hittite trade embargo referred to above. It is also in accordance with the thesis of a severe decline in Aegean maritime trade, dated to the time after LH IIIB Middle by Sigrid Deger-Jalkotzy and Wolfgang Güntner.[40]

In the levels of the Bronze/Iron Age transition locally produced vessels of Mycenaean type are found in both Areas II and IV. Their local production was verified by the chemical, petrographic and technological analysess (cf. above). We are dealing with dull painted vessels without any surface treatment; fine wheel-marks are clearly visible. This local Mycenaean pottery is very hard (having been fired at high temperatures), harder than the Syrian-type pottery. The local Mycenaean wares display medium to large quantities of mainly white inclusions, which seem numerous and of considerable size when compared to Mycenaean production series in Greece, but few and small when compared to the standard Syrian-type pottery. In addition, the quality of the red paint differs significantly between painted pots of local Mycenaean and of Syrian type. Only few basic fabric varieties exist for this local Mycenaean ware: some finer ones for small vessels and more coarse ones for kraters and large closed shapes. From a technological point of view this production can be compared with some of the Mycenaean type pottery produced locally in Cyprus during LC IIIA and IIIB.[41] The LH IIIC pottery from Cilicia also seems to be similar in tech-

[40] Deger-Jalkotzy 2002, 50–53; Güntner 2000, 369–372.

[41] Kling 1989, 1; eadem 2000, 282.

nological aspects.[42] By contrast, LH IIIC pottery in Greece (even as far north as Macedonia) often shows more or less carefully smoothed and/or slipped surfaces; a total or nearly complete lack of surface treatment is rare.[43]

We may refer to this local production of Tell Kazel as *local Mycenaean pottery*, because Mycenaean shapes, linear decorations and motifs are faithfully reproduced – without becoming a slavish copy of Mycenaean pottery from the Aegean. This kind of local Mycenaean pottery can be quite clearly distinguished by typological and technological criteria from other classes of local pottery at Tell Kazel which exhibit some links to Aegean ceramic traditions. These are, firstly, productions in which Syrian and Mycenaean features are combined, such as large amphoroid kraters and piriform jars with white slip and red paint showing Syro-Palestinian syntax of decoration and motifs,[44] secondly, unpainted Mycenaeanising pottery with non-Aegean typological and technological peculiarities,[45] and third Mycenaean shapes with bichrome decoration and non-Mycenaean motifs.[46]

An important general remark needs to be addressed here. For two reasons "local Mycenaean pottery" and "Mycenaeanising pottery" should henceforth replace Furumark's term "Mycenaean IIIC:1b", which is outdated but still frequently used in the Levant and on Cyprus. First, it was possible to refine the Furumark phasing system of LH IIIC based on results from recently excavated settlement stratigraphies at Mycenae, Tiryns and Lefkandí. Accordingly, Furumark's phase LH IIIC:1b[47] no longer exists, as it is neither the second subphase of LH IIIC, nor does it fully coincide with any of the recently defined subphases of LH IIIC. Second, this former chronological term ("LH IIIC:1b") underwent a semantic shift and became a stylistic label for any kind of seemingly "late" Mycenaean pottery (mainly deep bowls) in Cyprus and in the Levant. Thus, stylistic classification and chronological assignment were confused, which frequently led and still leads to circular argument. Therefore, the chronologically neutral, typological terms *local Mycenaean* and *Mycenaeanising pottery* suggested above should be shown preference.[48]

The repertory of local Mycenaean pottery from the destruction debris of Level 6 Final in Area II and from the upper floor of Level 5 in Area IV is fundamentally different from the imported Mycenaean repertory of the preceding phases. To be precise, the local producers of Mycenaean pottery did not try to copy the previously imported vessels in order to fill the gap of the reduced overseas exchange in goods. On the contrary, the composition of locally produced Mycenaean vessel types compares very closely to sets of Hellado-Mycenaean vessels in the Aegean. Moreover, for the first time in the sequence of Tell Kazel considerable quantities of unpainted Mycenaean

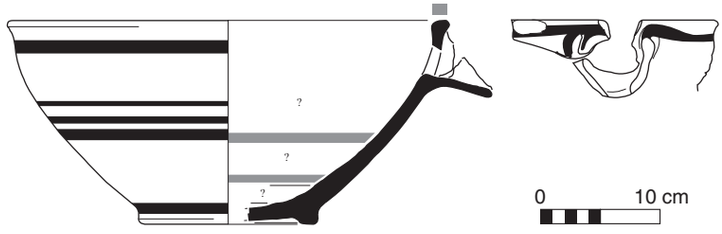


Fig. 7 Local Mycenaean spouted basin from Area II, Level 6 Final, above last floor of rooms U-V in Building II. Scale 1:6

[42] Goldman 1956, 207; fig. 330,1304; 331,1259; E. French 1975, 55.

[43] Such descriptions are, of course, rather subjective, when it comes to details and fine nuances. Therefore cf. Jung 2002, 43–47; 48; 322–410 (Macedonia); 566–573 (Argolid). The material cited in that volume was described by the present author. Thus, the classification categories (well smoothed, wet smoothed, slipped etc.) are the same as those employed for the Tell Kazel material.

[44] Badre in: Badre and Gubel 1999–2000, 173 fig. 31,j; Badre 2003, 89 fig. 5,2; 93 fig. 8,1 (drawings were not reproduced to scale by the editors); Capet, 2004, 84 fig. 21,n; 108 fig. 43,k; 109 fig. 44,a; 112.

[45] Badre, Gubel, Capet and Panayot 1994, 321 fig. 47,g; 337 fig. 59,c; Badre 2003, 93 fig. 8,2; Capet, 2004, 69 f. with fig. 7,d; 72; 75 fig. 10,b; 89 fig. 27,b; 91; 108 fig. 43,j; 112.

[46] Capet, 2004, 80, 86 fig. 23,a.

[47] For his definition and the find complexes on which he based it, see Furumark 1944, 202–209.

[48] This solution was advocated by most Aegean scholars during the debates in the "Mycenaean and Sea Peoples Section" on the 2nd SCIEM 2000 EuroConference.

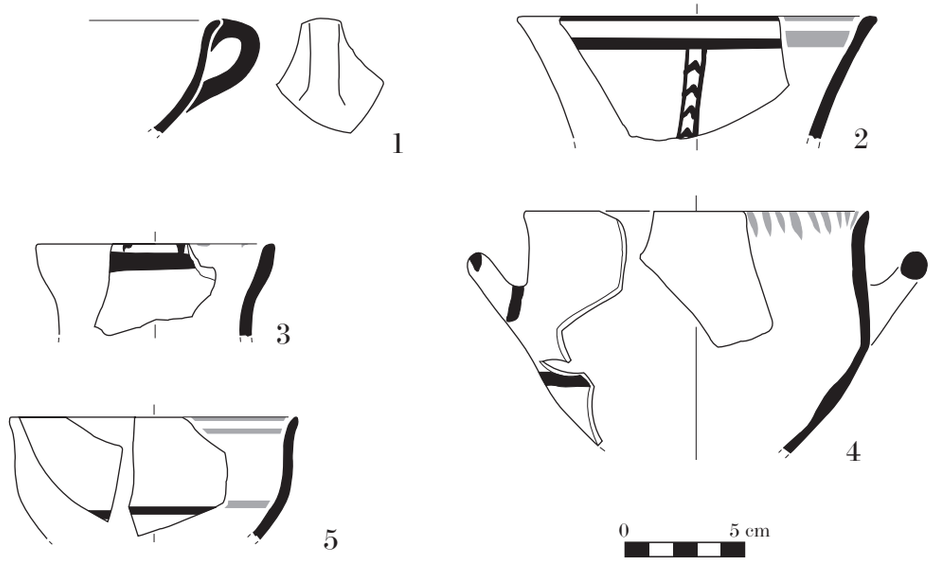


Fig. 8 Local Mycenaean pottery from Area II, Level 6 Final (1.2.4.5) and from Area IV, Level 5, upper or lower floor (3). Scale 1:3

types were in use. The following is an example of a representative assemblage: The destruction debris above the last floor of rooms U–V in Building II (Level 6 Final in Area II) contained fragments of at least four unpainted carinated kylikes FT 267 (Fig. 6,2–5), one unpainted conical kylix FT 274 (Fig. 6,1), a deep bowl FT 284/285 type A[49] with panelled pattern (Fig. 6,6), two linear spouted basins FT 302 (one is illustrated in Fig. 7), a krater wall sherd again with panelled pattern (Fig. 6,7) and a kylix base (Fig. 6,8). In other find contexts we have few additional types, such as an unpainted shallow cup FT 220 (Fig. 8,1), a mug FT 226 (Fig. 8,2) and finally some fragments of large closed vessels (such as Fig. 8,3).

It has already been pointed out, that there are technological characteristics shared by the locally produced Mycenaean pottery of the transitional Bronze–Iron Age contexts at Tell Kazel, on one hand, and the local Mycenaean pottery from LC IIIA–IIIB Cyprus and from Cilicia on the other. However, when we consider the repertory of vessel types and motives, significant differences between these regions should be emphasized. At Tell Kazel we do not have any one-handled conical bowls FT 242, known on Cyprus since LC IIIA[50] and present in considerable quantities at Tarsos.[51] Furthermore, although deep bowls FT 284/285 are present at Tell Kazel, they do not show any spiraliform motifs,[52] which are so frequent on Cyprus,[53] in the (post-Sea Peoples) reoccupation phase of Ibn Hani in Syria[54] or in the Mycenaeanising productions of Iron Age I Palestine.[55] Finally, the most important difference is the marked presence of unpainted Mycenaean pottery at Tell Kazel. Few unpainted carinated kylikes are known from Ugarit. Moreover, in contrast to the Tell Kazel pieces they show a much finer fabric with smoothed surface and

[49] In this study the Tiryns terminology is used. *Deep bowl type A* refers to any deep bowl with linear or no decoration on the interior. *Deep bowl type B* is used for all deep bowls with an entirely painted interior (monochrome interior in the Aegean terminology). *Monochrome deep bowl* refers to completely painted deep bowls, while horizontal lines and stripes can be left reserved from paint. For these definitions see Podzuweit 1981, 195; idem 1992, chapter "Skyphos A"; Benzi 1992, 148 f.

[50] Maa-Palaiokastro, floor I: Karageorghis and Demas 1988, 188 no. 574 pl. 235,574.

[51] E. French 1975, 61; 64 f. figs. 14–15.

[52] Motifs such as antithetic, running or stemmed spirals known in the Aegean throughout LH IIIB and IIIC, see e.g. Verdelis, E. French and D. French 1966, 140 fig. 1,3.4.7.8; 141 fig. 2,9.10; 142 fig. 3,2.4.6; Wardle 1969, 274 fig. 6,50; Podzuweit 1978, 475 fig. 28,8.13; 477 fig. 29,12.14; 479 fig. 30,5; 487 fig. 35,4.7.8; Schönfeld 1988, 177 fig. 6,14.15.17.19.20.

[53] Cf. Kling 1989, 95–100.

[54] Cf. Bounni, E. Lagarce, J. Lagarce and Saliby 1978, 280; 281 fig. 28,1; J. Lagarce and E. Lagarce 1988, 143; 312 fig. 5,A.B (D–G are kraters). There are a few deep bowls type A with antithetic and stemmed spirals at Ugarit, too (Yon, Karageorghis and Hirschfeld 2000, 242 fig. 32,486–

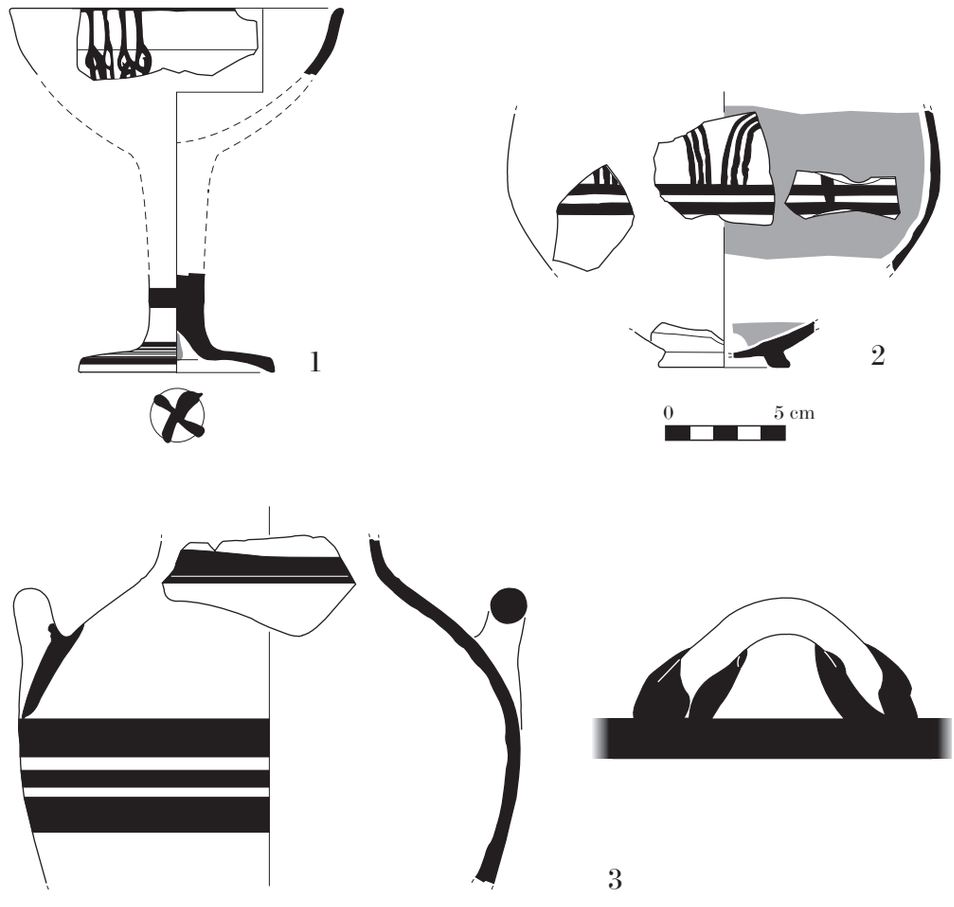


Fig. 9 Imported Mycenaean pottery from Area II, Level 6 Final (1.3) and from Area IV, Level 5, upper floor (2). Scale 1:3

thus most probably represent imports.[56] In Cyprus this unpainted Mycenaean type does not seem to ever have been very popular. There are just two examples at Maa-Palaiokastro. Compared to painted Mycenaean pottery, the unpainted variety, in general, was not en vogue in LC IIC and IIIA Cyprus.[57] This is at least the impression one gets from the sometimes selective publications. If it is confirmed in the future, this situation stands in marked contrast with the one observed at Tell Kazel, which in this respect compares much better with assemblages from the Greek mainland.[58]

Let us look now at the pottery of the Late Bronze II/Iron I transitional phase in some detail. Few imports can be ascribed to these layers with some confidence.[59] A kylix FT 258B with a group of vertical whorl-shells resembles the so-called Zygouries

488). At Tell Afis in northern Syria antithetic spirals appear on a krater (contrary to Bonatz who terms it "skyphos"; see Bonatz 1998, 217 f.; 229 fig. 5,1).

[55] Cf. Dothan 1982, 204–209 with figs. 65–67,4; Dothan and Zukerman 2004, 10 f. fig. 6,16-19; 12 f. fig. 8,1–6; 15 fig. 10–12; 37 f.

[56] Hirschfeld in: Yon, Karageorghis and Hirschfeld 2000, 145 cat. no. 399; 234 fig. 24,399. At Tell Kazel there is one unpainted conical kylix FT 274, which is surely an import.

[57] Maa-Palaeokastro, floor II: Karageorghis and Demas 1988, 122 no. 579; 124 pl. 192, Bothros 1/2.579. In general see Kling in: Karageorghis and Demas 1988, 328 f. and Podzuweit 1992, chapters "Zypern – Zusammenfassung"; "Ergebnisse und Schlußfolgerungen".

[58] There the majority of Mycenaean pottery from settlements is unpainted, from LH IIIA through LH IIIC Early, see Podzuweit 1992, suppl. 38; Jung 2002, 192 with fig. 67. However, a large fraction of the unpainted pottery is made up by coarse and medium-coarse storage and kitchen wares, which are very rare at Tell Kazel (most of the cooking pots and all of the pithoi at Tell Kazel being of Syrian and not Aegean type).

[59] Most fragments of imported Mycenaean vessels are so small, that they should be interpreted as being in secondary position. For the provenance of the better preserved ones (Fig. 9,1–3) see now Badre, Boileau, Jung, and Mommsen 2005, 33, 36 and Kerschner *ibid*. 36 f.

type (FT 258A) of LH IIIB Early–Middle,[60] but has a rim band and a banded stem (Fig. 9,1). Exact analogies are not available from the Aegean. A closed vessel with linear decoration only (Fig. 9,3) is not easy to classify and therefore cannot be dated exactly.[61] Finally, there are fragments of a deep bowl FT 284/285 type B with tricurved arch pattern (Fig. 9,2). Its decoration detail of a reserved circle in the interior base is not known before LH IIIB Developed in the Argolid.[62] It is interesting that all three of these imported vessels belong to fabrics, which are unknown in the preceding phases with Mycenaean imports and which on macroscopic examination do not seem to be from the Argolid.

Let us now turn to the locally produced pieces, and look first at the assemblage as a whole. There are no deep bowls type B (with monochrome interior), but only deep bowls type A (without monochrome interior). This contrasts with approximately contemporary settlement assemblages from the Argolid or Boeotia. However, even there in the time from LH IIIB Developed until LH IIIC Early deep bowls type A outnumbered deep bowls type B by far.[63] At Tell Kazel the deep bowls type A exhibit a variety of linear decorations, some of which can be found in the Aegean too. Some carry single bands in the middle or lower zone of the interior wall (Fig. 8,5, like the one in Fig. 10,7 from the next stratigraphical phase). This banded decoration of deep bowls type A starts at the beginning of LH IIIC Early in the Aegean.[64] The repertory of patterns on deep bowls type A is limited to panels (Fig. 6,6) and single horizontal wavy or zigzag lines.

Monochrome bowls (decoration 11.0), which appeared in the Argolid during the second half of LH IIIB,[65] but became common only in LH IIIC Early, are absent at Tell Kazel. There are also no stemmed bowls FT 304/305, which during LH IIIB Developed were still common in the Argolid, but decreased considerably during LH IIIB Final and were practically out of use by LH IIIC Early.[66] Among the unpainted material the most common type is the carinated kylix (Fig. 6,2–5). Likewise in the Aegean carinated kylikes belonged to the commonest shapes of the unpainted table ware from LH IIIA Late[67] to LH IIIC Middle.[68]

Looking at a few single examples one might reasonably start with the well preserved spouted basin (Fig. 7). In the Aegean from the second half of LH IIIB onwards some of the basins FT 294 were provided with spouts and, thus, can be classed as FT 302. However, this shape became common only by LH IIIC Early.[69] On Cyprus it was in use since LC IIIA,[70] which started sometime during LH IIIC Early. One parallel is known from a tomb at Ugarit.[71] Although without a direct analogy, the mug carries a characteristic simple panelled pattern with chevron fill (Fig. 8,2), which has parallels (mainly on deep

[60] Cf. Thomas 1992, 300 f.; 305 f.; 382–385; 592–594 figs. 58–60. Discussion of the production period in the Argolid: Mountjoy 1986, 113–115 with fig. 141,1–11; Schönfeld 1988, 155 tab. 1 nos. 42.65; 171 fig. 3,17; 173 fig. 4,19; 176; 183; 188; 191 fig. 8,9; 199 fig. 11,17; 200; 205.

[61] Because of its size (belly diameter 21.5 cm) it stands between the belly-handled amphora FT 58 and amphoriskos FT 59 or the small and large collar-necked jar (FT 64 and 63); the hydria FT 128 or 129 is also a possibility, like the one from House E (last floor level) in the city centre of Ugarit (Yon, Lombard and Renisio 1987, 97; 98 fig. 78; 99 fig. 80,81/890; 101).

[62] Podzuweit 1992, chapter "Skyphos B". Tiryns, LH IIIB Developed: Kilian 1988b, 118 fig. 13,1.3. Mycenae, LH IIIB Final: Wardle 1973, 316 fig. 11,65 (also with tricurved arch). 317 no. 65. Midea, LH IIIB Final: Demakopoulou 2003, 81f. with fig. 5,3.

[63] Tiryns and Mycenae: Podzuweit 1992, chapters "Skyphos B"; "Zusammenfassung und Ergebnisse"; suppl. 33; 44a–b. Thebes: Andrikou 1999, 80 pl. 1 and 2. When using the Tiryns' definition of type A and B, compare the rim fragments of plate 1 with plate 2.

[64] Podzuweit 1978, 487 fig. 35,4; idem 1992, chapters "Skyphos A"; Mountjoy 1997, 122 fig. 7,42.43; 126 fig. 9,51. There may be a few exceptions in LH IIIB Final (Demakopoulou 2003, 82 fig. 5,2, with a rim shape close to a stemmed bowl rim).

[65] Podzuweit 1992, chapter "Monochromer Skyphos"; suppl. 16–17; Demakopoulou 2003, 84.

[66] Podzuweit 1992, chapter "Hochfüßiger Skyphos"; suppl. 8; 20–21.

[67] Mycenae, LH IIIA Late: E. French 1965, 173. Mycenae, LH IIIB Early: E. French 1965, 182 fig. 8,16; 183.Mycenae, LH IIIB Middle: Mountjoy 1976, 98–100 with fig. 12,144–147; 111 tab. 3. Mycenae, LH IIIB Final: Wardle 1973, 322 f. with fig. 14. Thebes, LH IIIB Final: Andrikou 1999, 81; 85; 90 no. I.17; 91 no. I.19; 100 no. II.58.

[68] Phylakopí, LH IIIC Middle: Mountjoy 1985, 189 tab. 5.6; 190 fig. 5.20, 351–353; 192.

[69] In general see Podzuweit 1992, chapter "Schüsseln"; pl. 48,6; 50,4. Tiryns, Epichosis, LH IIIB Developed–Final: Podzuweit 1992, pl. 48,6. Lefkandí, phase 1b (LH IIIC Early–Developed): Popham/Milburn 1971, 336f. with fig. 2,1. Ayía Iríni (Kea), temple, stage 2 (LH IIIC Late): Caskey 1984, 245; 251 fig. 9,b.

[70] Kling 1989, 135. Examples e.g. at Maa-Palaiokastro, floor I: Karageorghis and Demas 1988, 170 no. 137; 199 pl. 125,137; 224,137; 149,South of Room 80/2.

[71] J.-C. Courtois/L. Courtois 1978, 332 no. 2; 333 fig. 46,2.

bowls type A and B) only in LH IIIB Final[72] and the beginning of LH IIIC Early.[73]

One last feature of the local Mycenaean pottery of this phase at Tell Kazel merits some comment. A number of deep bowls type A show a series of long vertical strokes on the interior rim; additionally, they may have an exterior rim band or no rim band at all (Fig. 8,4 – compare Fig. 10,4). This kind of rim decoration seems to be typical for the site. No close parallels have been published, neither from the Levant, from Cyprus, from Cilicia nor from the Aegean. Therefore, I would ascribe this feature to a local Amurru style. It seems very questionable, whether or not this can be correlated with linear deep bowls with dotted rims (decorations no. 12), which are known in the Argolid from LH IIIC Developed until LH IIIC Late.[74] It may also be difficult to relate these rims with stroke decoration to Rhodian deep bowls, kylikes and various bowl types with dotted interior rims and without further rim banding from LH IIIC Early or Middle tomb contexts at Ialissós and Pilóna, although these Rhodian vessels do show some tendency towards longer dots with pointed tips.[75] Alternatively, it is possible that this feature was adopted for the local Mycenaean deep bowls from LB II Syrian-type chalices, like the two from the lower floor of Level 6 in Area IV.[76]

To date the whole Mycenaean assemblage from the Late Bronze II/Iron I transitional phase at Tell Kazel precisely, one can first emphasise the fact that vessel or decoration types that are characteristic for LH IIIC Developed on the Greek mainland are not represented. The temporally latest features of the finds point to a date either in LH IIIB Final or in LH IIIC Early. These two phases are very difficult to separate, even in mainland Greece. LH IIIB Final is the very latest phase of LH IIIB and basically defined by the destruction deposits in the citadels of Mycenae, Tiryns,[77] Midea in the Argolid and the palace of Thebes in Boeotia as well. The beginning of LH IIIC Early is defined by the *postpalatial* reconstruction phase immediately following these destructions.[78] Therefore, a precise date to one of these two phases is of special historical interest. A number of new features of LH IIIB Final, which are first attested at the Tell during the LB II/IA I transitional phase became common in the Aegean only in LH IIIC Early. Therefore, I would date the destruction of Tell Kazel, Level 6 Final in Area II and upper floor of Level 5 in Area IV, to the beginning of LH IIIC Early and, thus, to a moment shortly after the breakdown of the Mycenaean palace system. However, a date to LH IIIB Final cannot be excluded due to the limited amount of datable pottery at Tell Kazel.

Phase 5

The destruction of the site and then a short period of abandonment were directly followed by a new building phase[79] – Level 5 in Area II and Levels 4–3 in Area IV –, that can be dated to Iron Age I in Levantine terms. This ended again in destruction, a severe conflagration, which according to Emmanuelle Capet might have been due to extensive use of wood as building material.[80] The local Mycenaean pottery repertory did not change markedly in comparison with the preceding phase. However, there are some pieces, which hint at a slightly younger date of this phase. A kylix fragment with a band deep inside its bowl (Fig. 10,9) might belong to a linear conical kylix FT 274-275. In the Argolid this type might have been in use by LH IIIB Final, but it is securely attested only from LH IIIC Early onwards.[81] The carinated

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[72] Mycenae: E. French 1969, 80 fig. 5,18; 81 fig. 6,17; Wardle 1973, 313 fig. 9,53. Thebes: Symeonoglou 1973, 19 pl. 18,5; Andrikou 1999, 96 no. II.19.

[73] Íria, Annex (no securely closed find context, but cf. Mountjoy 1999, 36): Döhl 1973, 171 no. A 13/5; pl. 68,5,A 13/5. A mug from the underground fountain on the Athens acropolis is similar to the Tell Kazel piece, but its triglyphs are slightly different (Mountjoy 1999, 555 fig. 202,261).

[74] Cf. Podzuweit 1992, chapter "Punktrandskyphos"; idem 1978, 479 fig. 30,3 is only broadly comparable to the Tell Kazel pieces, since the strokes are arranged in groups.

[75] Cf. Benzi 1992, pl. 37,p.q.s.t.u; 38,a; 60,l–o; 72,i; 170,d.e; Mountjoy 1999, 1060 fig. 434,233; Karantzali 2001, 55 no. 16784 (local Rhodian according to ICP-AES analyses); 172 fig. 40,16784.

[76] Badre in: Badre and Gubel 1999–2000, 146; 156 fig. 16,b.e; Compare also others from the LB II temple at Kamid el-Loz (Metzger and Barthel 1993, pl. 78,12; 127,3) and from Ugarit (J.-C. Courtois and L. Courtois 1978, 235 fig. 11,2; 245 fig. 15,2).

[77] Kilian 1988a, 118; 121 fig. 3; Podzuweit 1992, chapters "Die Stratigraphie von Tiryns"; "Charakterisierung der Phasen".

[78] This labelling of the time phases follows the Tiryns system, see above n. 19 (as an alternative to the phase label "LH IIIB2/IIIC Early transitional" which is used at Mycenae, see E. French and P. A. Mountjoy, this volume).

[79] Badre in: Badre and Gubel 1999–2000, 185; eadem 2003, 94.

[80] Capet, 2004, 101; 117.

[81] For the date at Tiryns see Podzuweit 1992,, chapter "Kylix"; pl. 53,14.16; 54,3.5. Thanks are due to Ursula Damm-Meinhardt and Tobias Mühlenbruch, who checked the stratigraphic assignments for me. For the unusual absence of bands on the stem cf. Koehl 1984, 211 with n. 10; 212 fig. 3,5.8; 219. There is a possibility that the type was already present in Level 6 Final of Area II (see Fig. 6,8).

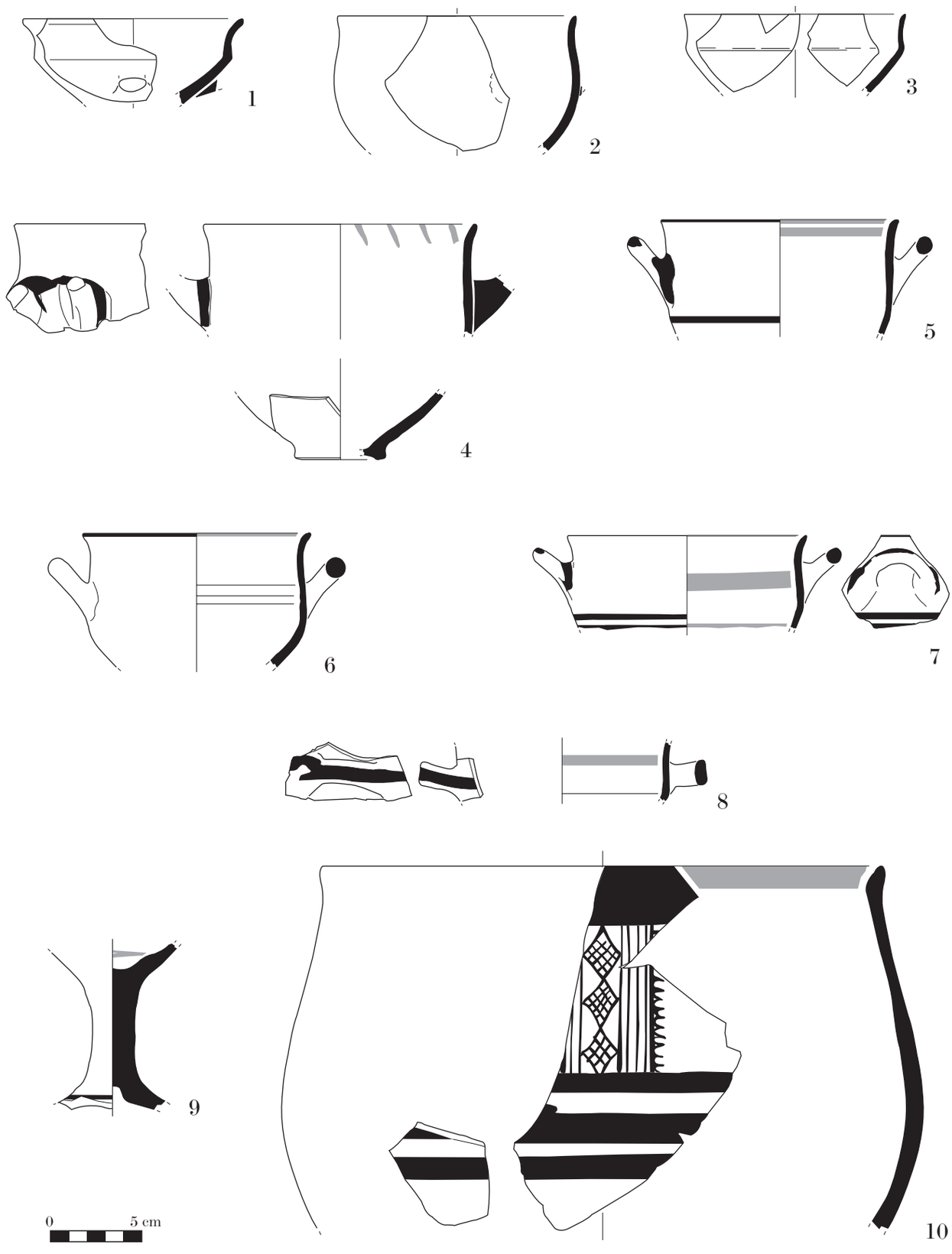


Fig. 10 Local Mycenaean pottery from Area II, Level 5 (1–7.10) and from Area IV, Level 4 (8.9). Scale 1:3

strap-handled bowl FT 295 with high upper body and linear decoration (Fig. 10,8) is more significant. Its earliest Aegean parallels may belong to LH IIIC Early, but the type became common only during LH IIIC Developed.[82] On Cyprus it is possibly attested in LC IIC, but surely in LC IIIA.[83] There are no exact parallels for this piece at Ugarit.[84] Unpainted carinated kylikes and unpainted deep bowls were still in use, and the same applies for linear and patterned deep bowls type A (Fig. 10). Since it is not easy to date the local Mycenaean style precisely, one can additionally try to establish a *terminus ante quem* for this phase, which ended in the second fiery destruction of the Tell. One indication is provided by a large amphoriskos of Proto-White Painted fabric (Fig. 11).[85] It was found in a pit that cuts into Level 5 of Area II. The local Syrian-type pottery from this pit dates to Iron Age I.[86] The best parallel for the decoration syntax and some of the complicated motifs of the amphoriskos can be found on a straight-sided alabastron from Enkomi, destruction of Level IIIB.[87] The style of these two vessels from Tell Kazel and Enkomi is close to the so-called Carpet Style, which occurred during LH IIIC Late in the Aegean.[88] The local Mycenaean pottery assemblage from the end of Level IIIB at Enkomi points to the same date.[89] There is no exact parallel on Cyprus for the shape of the Tell Kazel amphoriskos, especially for its baggy profile. One might only cite a smaller Proto-White Painted amphoriskos from tomb 17 in the Alaas necropolis,[90] while the linear decoration of rim and neck has good parallels in a number of Proto-White Painted amphoriskoi at that site.[91]

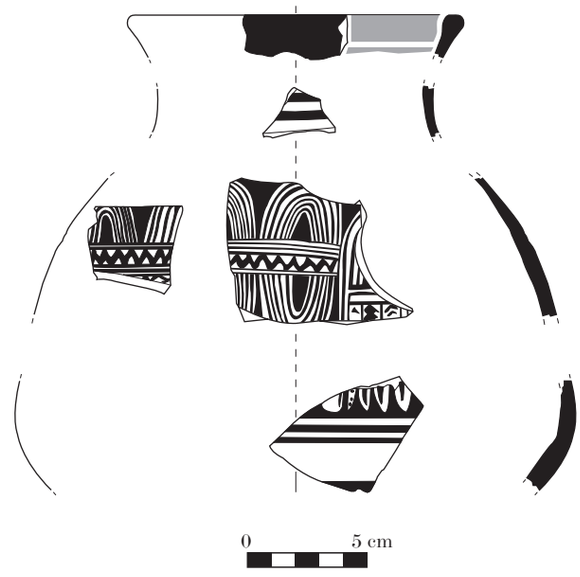


Fig. 11 Proto-White Painted amphoriskos from Area II, pit cutting into Level 5. Scale 1:3

I would like to conclude this short presentation of the Mycenaean type material from Tell Kazel with some historical considerations. In particular, I would like to comment on the chronological position of the Sea Peoples' destruction. From Egypt we have two written sources by Ramesses III, which refer to a severe destruction of the kingdom of Amurru, one

[82] Podzuweit 1992, suppl. 50; pl. 42,10.11.13.15–17; 43,4. Those on pl. 42,15; 43,4 are the earliest possible examples at Tiryns, but the latter piece may be from a disturbed context. I wish to thank Tobias Mühlenbruch and Philipp Stockhammer for the stratigraphical assignations of those finds and Stockhammer also for a discussion on the type and its production period.

[83] Enkomi, end of Level IIB (LC IIC): Dikaios 1969/71, 572 no. 5563/1; pl. 66,21. Enkomi, Level IIIA (LC IIIA): *ibid.*, 176; 318; pl. 94,21; 98,4; 123,9. Maa-Palaiokastro, floor II: Karageorghis and Demas 1988, 107 no. 292 pl. 175,292; floor I: *ibid.*, 156 f. pl. 219,Room 8/5.Room 8/6.

[84] There are similar bowls, which however do not seem to represent this type in particular (Yon in: Yon, Karageorghis and Hirschfeld 2000, 15; 25 fig. 8,d.e), because they do not show the typical concave-convex profile with high upper part above the carination (which characterises the piece from Tell Kazel). Instead their profile with short upper part (which often widens) has better parallels on Cyprus during LC IIC (cf. Dikaios 1969/71, pl. 67,22; South 1988, 226 fig. 2,K-AD 1257).

[85] Rim diameter 14 cm; Here I wish to thank Vassos Karageorghis, who confirmed my classification as Proto-White Painted and discussed this piece with me during his stay at Tell Kazel during the 2003 campaign.

[86] Information kindly provided by Laure Salloum.

[87] Dikaios 1969/71, 609 no. 4485/13; pl. 82,27.

[88] Podzuweit 1992, chapter "Krater".

[89] Cf. deep bowls type B and monochrome deep bowls with multiple horizontal wavy lines (Dikaios 1969/71, 595 no. 232; 597 nos. 732/7.734/2; 603 no. 3316/8.3316/9; 613 no. 5734/1; pl. 79,26.27.28.30; 83,10.22), cups with single wavy line (*ibid.*, 595 nos. 239.244; 608 no. 4096/7 pl. 79,19.24; 83,24), large closed vessels with wavy lines on the neck (*ibid.*, 595 nos. 229.230; pl. 80,7.8) etc.. For the date of their Aegean parallels from LH IIIC Advanced to mainly LH IIIC Late see Podzuweit 1992, chapters "Skyphos mit Wellenband"; "Tassen"; "Charakterisierung der Phasen"; Mountjoy 1999, passim.

[90] Karageorghis 1975, 19 no. 23; pl. 14,no. 23; 59,T.17/23.

[91] Karageorghis 1975, pl. 54,T.15/4; 59,T.17/18.23; with these parallels in mind one might reconstruct a slightly taller neck for the Tell Kazel piece. The drawing in Fig. 11 shows only minimum vertical distances between the various non-joining pieces. Moreover, it is possible that the lowest belly fragment should be moved upwards somewhat.

Levantine relative chronology	**Tell Kazel, Area IV: stratigraphy**	**Tell Kazel, Area II: stratigraphy**	**Events in the Aegean**	**Aegean relative chronology**
Late Bronze Age II	Level 6, lower floor	not well kown	Petsas' House destruction at Mycenae	LH IIIA Late
Late Bronze Age II	Level 6, upper floor	not well kown	first destruction of Thebes palace	LH IIIB Early
Late Bronze Age II	Level 5, lower floor	Level 6	Mycenae, Tiryns: destructions, new fortifications	LH IIIB Middle – LH IIIB Devel.
Late Bronze Age II/Iron Age I Transition	Level 5, upper floor: **destruction** (Sea Peoples)	Level 6 Final: **destruction** (Sea Peoples)	**Final destruction of palaces** at Mycenae, Tiryns, Thebes, and the citadel at Midea	(LH IIIB Devel.), LH IIIB Final – beginning of LH IIIC Early
Iron Age I	Levels 4–3: destruction	Level 5: destruction		– before LH IIIC Late

Fig. 12 Chronological chart (internal synchronisation of Tell Kazel Areas II and IV according to E. Capet, 2004, 117, and L. Badre, 2003)

from his regnal year 5 and the other from year 8, both inscribed on Ramesses' memorial temple at Medinet Habu. The latter inscription explicitly mentions the Sea Peoples as cause for this destruction: *"An encampment was [estab]lished in one place in Amor and they desolated its people and its land as though they had never come into being. ... Their confederation consisted of Pelest, Tjekker, Shekelesh, Danu<na> and Weshesh..."*.[92] Another source from Syria itself seems to fit into the picture of enemy threat before that catastrophy. In a letter to the king of Ugarit by a certain Paršu from Amurru, the king is asked to communicate all news concerning the movements of an unnamed enemy, as was the custom until then between Ugarit and Amurru. Furthermore, Paršu writes that ships will be put at the disposal of Ugarit by Amurru.[93] This strongly is reminiscent of the letters exchanged between the kings of Ugarit and Alashiya as well as an unknown king about attacking enemy ships, probably the Shikalayu, from the latest phase of the kingdom of Ugarit.[94] The latest datable written documents from Ugarit belong to the reign of Siptah (1194–1188 BC) or Tausret (1188–1186).[95] It is assumed here, that Ugarit and Ras Ibn Hani were indeed destroyed by the Sea Peoples shortly before Ramesses' III year 8.[96] Subsequently at least Ras Ibn Hani was reoccupied.[97]

From the above it seems reasonable to make the Sea Peoples responsible for a destruction of Amurru, including Sumur/Tell Kazel. The architectural findings of humble quality in Area II, Level 6 Final, together with the paucity of imports in this phase in both areas II and IV suggest a time of severely reduced international trade and economic crisis ending in violent destruction. The local pottery from that phase already allows a synchronisation with Early Iron Age sites on the Syrian coast.[98] The local Mycenaean pottery assemblage is clearly earlier in type than the local Mycenaean pottery from the

[92] Peden 1994, 29. The expression "*W3h Jhj*" for "establishing an encampment" is used in other texts for temporary (military) camps; it does not signify a permanent settlement (personal communication, gratefully received from Ernst Czerny, Vienna).

[93] RS 20.162, see Izreʿel 1991, 98–100; Klengel 1992, 174; Singer 1991, 175 f.; idem 1999, 721.

[94] Cf. Klengel 1992, 150; Noort 1994, 85–88; Singer 1999, 719–723. In the light of a re-evaluation of these letters it seems now possible, that such seaborne raids lasted for a longer period than previously thought (cf. Malbran-Labat 1999).

[95] Singer 1999, 715. The absolute dates given in the present study are based on Kitchen's chronology of the Egyptian pharaohs (Kitchen 2000).

[96] For a summary of recent discussions with bibliography see Singer 1999, 725–731.

[97] Badre 1983; J. Lagarce and E. Lagarce 1988. There seems to have been a limited reoccupation of Ugarit, too (cf. Yon in: Yon, Karageorghis and Hirschfeld 2000, 15).

[98] Capet, 2004, 64–99; 117 f.

(post-Sea Peoples) reoccupation (lower floor) at Ibn Hani.[99] These arguments make the earlier destruction of Tell Kazel during the transition from Late Bronze II to Iron Age I the better candidate for the Sea Peoples' invasion[100] and not the later conflagration during the developed Early Iron Age (Level 5 in Area II and 3 in Area IV), which in turn can be chronologically linked to the reoccupation of Ibn Hani (upper floor) by Syrian-type painted pottery.[101]

The first destruction of Tell Kazel contains Mycenaean pottery which is best dated to the beginning of LH IIIC Early, as we have seen. Consequently, Tell Kazel may provide a *terminus ante quem* for the beginning of LH IIIC Early through its destruction by the Sea Peoples, which would be 1176 BC, according to Ramesses III year 8 inscription, or alternatively 1179 BC, according to Ramesses III year 5 inscription.[102]

[99] The local Mycenaean pottery from this phase at Ibn Hani has parallels in Cyprus at Enkomi, Levels IIIA–IIIB (compare Bounni, J. Lagarce, E. Lagarce, Saliby and Badre 1979, 249 fig. 25,1–4; Badre 1983, 205 fig. 1,a–c with Dikaios 1969/71, pls. 70,23; 73,33; 74,7.18: Enkomi, Level IIIA; *ibid*. pl. 80,29.33: Level IIIB). Some features of the kraters, such as their stroke decoration on the rim, the lower bands consisting of a broad band flanked by two narrow ones, and a kind of double banded handle decoration (the so-called "long splash system"), are known in the Aegean only from the later LH IIIC phases Advanced and Late (compare Bounni, J. Lagarce, E. Lagarce, Saliby and Badre 1979, 249 fig. 25,1.7.8; Badre 1983, 205 fig. 1,a.b with Popham and Milburn 1971, 341 fig. 5,5; pl. 57,3.4; Mountjoy 1999, 186–188 with fig. 57,437.438; Podzuweit 1992, chapter "Krater"; pl. 37,5; 119). For these latest features of Ibn Hani, lower floor of the reoccupation, there are also parallels on kraters at Enkomi (J.-C. Courtois 1971, 267 fig. 104,A.D.F), which support the synchronisation of these features with the abovementioned Aegean phases.

[100] The sequence of events is unclear in Ramesses' III year 8 inscription. To me the most plausible reconstruction is, that first Amurru including Tell Kazel was destroyed, then the Sea Peoples established their base camp somewhere in the Akkar Plain (cf. above n. 92). Alternatively, they may have already established a camp in order to besiege the Tell. Indeed, local Mycenaean pottery and handmade burnished ware (Badre 2003) are already present in that destruction layer at the Tell, which most probably was caused by the Sea Peoples. This means the production and use of these classes of pottery predate the destruction of Amurru by the Sea Peoples and cannot be ascribed to these invaders.

[101] Badre in: Badre and Gubel 1999–2000, 189; 192; 193 fig. 44,b (citing Bounni, J. Lagarce, E. Lagarce, Saliby and Badre 1979, 253 fig. 27,3). See also Capet, 2004, 72 with n. 15, in comparison with J. Lagarce and E. Lagarce 1988, 153–155, in respect to the gradual introduction of bichrome decoration at Tell Kazel and Ibn Hani. However, a full publication of the finds from the reoccupation layers at Ibn Hani is necessary in order to verify this synchronisation.

[102] On the 2nd SCIEM 2000 EuroConference Kitchen expressed the view, that 1180–1160 BC are the extreme margins for Ramsses' III 8th year.

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Myc IIIC in the Land Israel: Its Distribution, Date and Significance

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Relations between the Aegean, Cyprus and the Levant in the 12–11th centuries BCE and the nature of the Philistine culture was a highly debated subject during the past generation (for recent studies and summaries see various papers in Oren [ed.] 2000; Sharon 2001; Barako 2000; 2003). This discussion largely resulted from the discovery of new data concerning Philistine settlement at a time when the theoretical aspects of archaeological interpretation went through dynamic changes. Understandably, then, some interpretations given to the Philistines and related phenomena were tied to the dominate lines of thought at the time of writing.

Some views postulated during the 1980's and later were motivated by the anti-diffusionist position of the "processual" or "New Archaeology" school or from socio-economic paradigms (Brug 1985; Bunimovitz 1990; Sherratt 1998), while other recent writers feel free from such paradigms and return to older models, though using new data and new theoretical frameworks. My own ideas on these issues have been expressed in the past (Mazar 1985a; 1985b; 1988; 1991; 1994; 2000; 2001) and I still adhere to most of them.

In this paper I will briefly explore the following subjects – all of which are related to the appearance of imported and locally-produced Myc IIIC in the Land of Israel:

1. Myc IIIC pottery from Beth Shean and other sites in northern Israel

2. Locally-made "Myc IIIC inspired" pottery from Beth Shean and other sites in northern Israel

3. The date and meaning of the locally-produced Myc IIIC of Philistia ("Philistine Monochrome")

4. The origin and nature of the Philistine settlement

1. Myc IIIC pottery from Beth Shean and other sites in northern Israel

Until recently, the small corpus of Myc IIIC or related pottery found in northern Israel was restricted to a complete or almost complete stirrup jar, a few sherds from Beth Shean (Hankey 1966; Warren and Hankey 1989: 164–165), a stirrup jar from Tel Keisan (Balensi 1981), and a small number of sherds from Akko. A few sites along the northern Levantine coast (Tyre, Sarepta, Byblos, Tell Sukas, and Ras Ibn Hani) yielded additional small quantities of such sherds, except for the latter site where a large quantity of locally(?) produced pottery of this kind was found. NAA analysis has shown that the stirrup jar from Tell Keisan originated in the Kouklia region on Cyprus (for references see Warren and Hankey 1989: 162–163; Yasur-Landau 2003b: 235–239).

In the new excavations at Tel Beth Shean from 1989–1996 we discovered an additional 27 sherds of this pottery, representing at least 10 different vessels (for the excavations see Mazar 2003; fig. 1 shows a selection of these sherds). The comments that follow are based on a study of these sherds by S. Sherratt and me (in press).

The Beth Shean sherds were found in two distinct strata (S-4 and S-3), both equivalent to Level VI of the University of Pennsylvania and clearly dated to the time of the Egyptian 20th Dynasty. The uppermost level (S-3) was destroyed in severe fire that brought an end to the Egyptian garrison at Beth Shean. Suggested dates for these two strata are as follows:

Stratum S-4: the time of Ramesses III (1184–1153 BCE according to Trigger, Camp and O'Connor 1983: 184 and Kitchen 2000: 49).

Stratum S-3: the time of Ramesses IV–VI (1163–1143 or 1153–1136 BCE).

Ten of the sherds (including the most elaborately decorated ones) came from the earlier Stratum S-4 and seventeen from Stratum S-3. Thus, Beth Shean provides the best stratigraphic/chronological anchor for dating this pottery. Stylistic analysis has shown that there is no difference between the Myc IIIC sherds of S-4 and S-3. It therefore appears that

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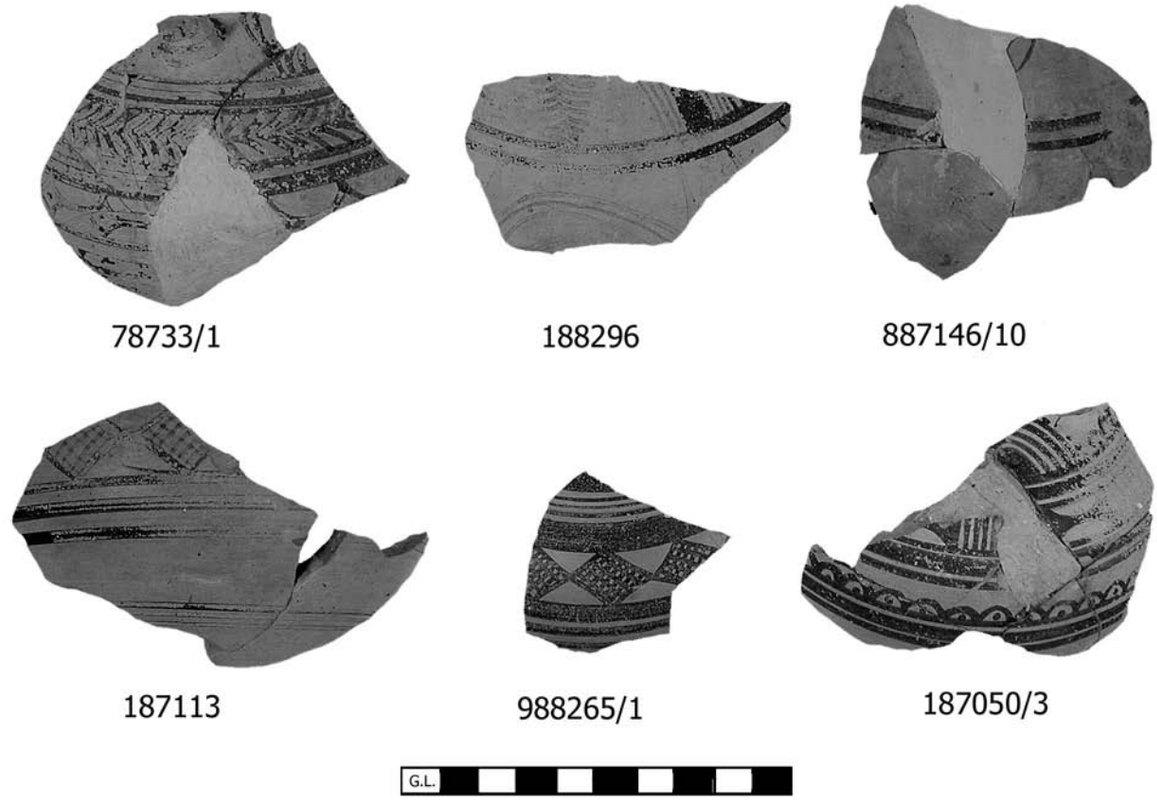


Fig. 1 Selected Myc IIIC sherds from Beth Shean Strata S4 and S3

this pottery was in use at Beth Shean throughout the lifetime of the 20th Dynasty.[1] Thirteen of the 27 sherds belong to stirrup jars, while two belonged to a jug, three to a small closed vessel,[2] four to skyphoi, and nine unidentified. The ratio between closed vessels (particularly stirrup-jars) and open vessels contrasts sharply with the assemblage of locally-made "Myc IIIC" in Philistia where stirrup jars are very rare.

Stylistic analysis of the sherds led Sherratt to conclude that the vessels arrived at Beth Shean from Late Cypriot III production centers in Cyprus and that they belong to the locally-produced Cypriot variation of Myc IIIC pottery that some call "Cypriot White Painted Wheelmade III". According to Sherratt, the stirrup jar published by Hankey and the two large fragments of stirrup jars from the new excavations at Beth Shean came from eastern Cyprus and have parallels at Enkomi in late Stratum IIIa and early Stratum IIIb, though some of the motifs are variants of motifs on LH IIIB–C stirrup jars. Two jug sherds from Beth Shean are decorated in the Sinda-style typical of this period, which appears at Kition on the earliest LC IIIA floor. All these distinct sherds came from Stratum S-4. Stylistically, none of the other fragments can be regarded as later than these well-defined sherds.

Petrographic analysis carried out by Cohen-Weinberger has shown that the vessels could have come from Cyprus, Cilicia, or the northern Syrian coast. This conclusion is now also confirmed by NAA analysis.[3]

[1] Sherratt suggests that the sherds found in the later Stratum S-3 originated from Stratum S-4.

[2] Ms. Penelope Mountjoy, who examined these three small sherds in Jerusalem, suggested that they belong to one small Hydria. The only parallel comes from Naxos (Mountjoy 1999: 948 No. 32). I thank Ms. Mountjoy for this suggestion.

[3] NAA analysis of the Beth Shean sherds was part of a wider study by A. Yasur-Landau, H. Mommsen, Y. Goren and A-L. D'Agata. The results point to Cyprus as the country of origin for these sherds. I thank A.Yasur-Landau for this information.

Warren and Hankey (1989: 164–165) defined the stirrup jar from Beth Shean as "LH IIIC Middle," and made this definition their cornerstone for dating LH IIIC Early and Middle. The generally accepted date of *ca.* 1190 BCE for the destruction of Ugarit and Tell Deir 'Alla (where the latest datable Myc IIIB pottery was found) left a time span of roughly one generation in Warren and Hankey's view for "LH IIIC Early" (i.e. from *ca.* 1185/80 BCE to *ca.* 1150 BCE). LH IIIC Middle, on the other hand, was dated by them based on Beth Shean to the short time span between the last years of Ramesses III and Ramesses V/VI (the latest date being 1136 BCE). In contrast, Sherratt's stylistic analysis of the Beth Shean group led her to conclude that:

"the use of the term 'LH IIIC Middle', when applied to the pottery recovered at Beth Shean, is essentially meaningless. Though it may well have a 'LH IIIC Middle' look about it, in the sense that some of its decoration is relatively elaborate and that it includes some motifs which, in certain parts of the Aegean, would normally be classified as 'LH IIIC Middle', the use of this label represents an ultra-normative form of classification based on general stylistic considerations alone, which of itself says nothing about the relative or absolute chronology of the Beth Shean fragments" (Sherratt in: Sherratt and Mazar in press).

This "release" of the Beth Shean vessels and sherds, as well as their Cypriot counterparts from the definition "LHIIIC Middle", fits their appearance in two separate strata dated to the time of the Egyptian 20th Dynasty. As mentioned above, the earlier level which must belong to the time of Ramesses III, yielded some of the largest and more elaborate sherds in our collection. This pottery originated from "a horizon which covers the later part of Level IIIa at Enkomi and perhaps also the early stages of Level IIIb" (Sherratt, *op. cit.*), which should be dated to the first half of the 12th century BCE (see also Yasur-Landau 2003b:237–239). This date also fits the radiometric dates of ca. 1200 BCE for the transition from LCIIC and LCIII (Manning *et. al.* 2001).

Sherratt suggests that this Cypriot "Mycenaean-related" pottery arrived through limited and casual trade between Cyprus and the Levant during the 12th century, most probably via the ports of Akko and/or Tyre. Thus, during the time of the 20th Dynasty, Beth Shean retained trade relations of some sort with the coast. This explanation is preferred by Sherratt as part of her concept of trans-Mediterranean trade developed by Cypriots and other entrepreneurs after the collapse of the centralized authorities, ca. 1200 BCE (Sherratt 1998; 2003: 44–51).

In my opinion, the imported pottery from Beth Shean is an isolated phenomenon that should be tied to specific circumstances. A possible explanation is that such pottery was brought as a luxury item by Cypriot mercenaries who served in the Egyptian garrison at Beth Shean. Such an import could have been part of a framework of local profit-making initiatives. This would explain why such pottery is so rare at Beth Shean itself and absent at major sites like Megiddo and Dor. At Beth Shean, this pottery is rare compared to the large amount of locally-produced Egyptian and Canaanite pottery.

At this time, Beth Shean was an Egyptian garrison and it is reasonable to assume that the imported Myc IIIC pottery was associated with Egyptian activity at the site. It seems that the wide scale trade system of the 13th century came to an end in the 12th century to be replaced by local, small scale and exceptional initiatives which cannot be explained as part of a larger trade system.

The lack of Myc IIIC in Stratum VIIA Megiddo was interpreted by Finkelstein (1996) as reflecting chronological differences between Megiddo and Beth Shean. In his view, both Strata S-4 and S-3 at Beth Shean postdate Megiddo VIIA. This view cannot be accepted, however, since the Myc IIIC sherds of Cypriot origin from Beth Shean were found in two distinct levels of a dwelling area contemporary with the Egyptian 20th Dynasty, a time range of about 60–70 years in the 12th century BCE. Finkelstein's view would leave no time slot for Megiddo VIIA, a city which must also be contemporary with the Egyptian 20th Dynasty. Therefore, Megiddo VIIA and Beth Shean VI (our Strata S-4 and S-3) must have been contemporary.

The lack of Myc IIIC imports at Megiddo should be understood in light of the limited import of such pottery to Beth Shean as explained above. Unlike the Egyptian garrison of Beth Shean, Megiddo VIIA was a major Canaanite city of the 12th century BCE, and belonged to a period postdating the intense trade connections with Cyprus and the Aegean (Mazar 2001). A lack of imported Myc IIIC at Megiddo should be viewed as the normal state of affairs in the period following the demise of the international trade of the 14th and 13th centuries BCE. The phenomenon of small-scale imported Cypriot "Myc IIIC" at Tel Keisan, Beth Shean, and few other sites along the Levantine coast is the

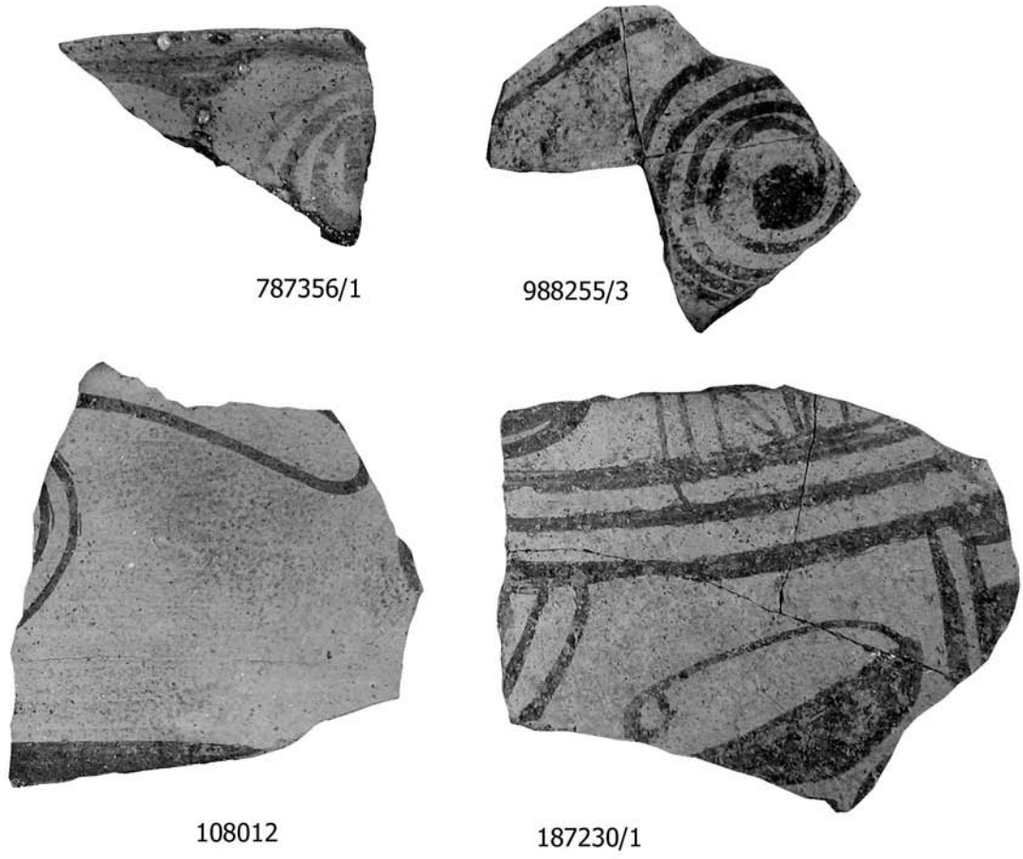


Fig. 2 Locally made "Myc IIIC inspired" sherds from Beth Shean Stratum S3

exception – not the norm.[4] Finkelstein's expectation that contemporary sites would yield similar pottery assemblages in a given geographic zone is correct in principle, yet cannot be rigidly applied when dealing with a very specific and small pottery group like the imported Myc IIIC. The limited distribution of this pottery at certain sites, and its lack in others, should not be taken as a decisive chronological factor (see also Yasur-Landau 2003b: 237–239).

2. Locally-made "Myc IIIC inspired" pottery from Beth Shean and other sites in Northern Israel

Four pottery sherds from Beth Shean Stratum S-3 (Fig. 2) belong to vessels inspired by the Myc IIIC repertoire, yet were most probably produced in the Levant (Sherratt and Mazar, in press). All four sherds are decorated in a single color – red paint.

The first is a sherd from a small skyphos painted with spirals; the second is a sherd from a large skyphos showing part of an antithetic spiral; the third is a body sherd of a krater or other closed vessel with a painted spiral and the fourth is a krater sherd painted with metopes and a bird motif which differs in details from the common Philistine bird motif.

From all appearances, these four sherds belong to a small group of "Aegean-related" pottery found at several sites in northern Israel like Akko, Dan, and Megiddo.[5] One might suggest that this group is related to "Sea People" groups that supposedly settled in northern Israel. These include the *Shkl/r* and *Shrdn* mentioned in the Onomastikon of Amenemope, or

[4] At Tel Rehov, 5 km south of Beth Shean, we found three small sherds of Myc IIIC pottery. Two (a false neck of a stirrup jar and a small body sherd) were found in Phase D-6 and one (a skyphos rim sherd) in Phase D-4 – both in Area D (Mazar 1999:10–17). Phase D-6 is contemporary with Beth Shean Level VI of or Stratum S-3 (late 20th Dynasty) while Phase D-4 is dated to the 11th century (Bruins, Mazar and Van der Plicht in this volume). The identity of these sherds was confirmed by Ms. P. Mountjoy.

[5] For examples see, Dothan (1993a): 21 and Ilan (1999: pl. 59:1). A stirrup jar from the new Megiddo excavations is not yet published.

the *Shkl/r* known from the Wen Amun tale (Stern 2000).[6] Yet, unlike the circumstances at Philistia (see below), I doubt whether such a small amount of pottery with its very limited geographic distribution justifies such an ethnic label or explanation. It may be that this pottery was produced at coastal Levantine workshops that imitated Cypriot "Mycenean-related" pottery of the LCIIC/LCIIIA periods. Thus, the few sherds found at Beth Shean may represent a limited import from such Levantine coastal production centers. The discovery of this pottery in clear stratigraphic contexts at Beth Shean prior to the end of Egyptian domination is a significant chronological anchor for dating this little known group.

3. The date and meaning of the locally-produced Myc IIIC of Philistia ("Philistine Monochrome")

The local Myc IIIC assemblage and accompanying domestic wares (e.g. cooking pots and "lakinae") of Aegean types found so far at Ashdod and Tel Miqne in the main has been the subject of vast discussion in recent years (Dothan 1998a; Dothan 1998b; 2000; Killebrew 1998; Sharon 2001; Yasur-Landau 2003b: 239–241; Dothan and Zukerman 2004 [the latter being the most comprehensive discussion of this pottery to date]). This pottery group was dubbed "Myc IIIC1b" or "Philistine Monochrome" (Dothan and Zukerman 2004). I prefer the term "local Myc IIIC" as the most fitting. Since the initial discovery of this pottery at Ashdod in 1968, the excavations at Ashdod and Tel Miqne have provided a wealth of new information, which supports in my opinion, the initial interpretation as suggested by Dothan and myself (Dothan 1982: 36–41; Mazar 1985a: 101–106). In the following paragraphs I will limit my comments to several debated issues regarding this pottery.

At Ashdod, Myc IIIC pottery was found in two excavation areas: Area G from Stratum XIIIb (Dothan and Porath 1993) and Area H from Stratum XIII (Dothan and Ben-Shlomo 2005). In both strata, the Myc IIIC appears in substantial quantities alongside local Canaanite pottery of the early 12th century BCE, before the appearance of Bichrome Philistine pottery. In the succeeding Stratum XIIIa in Area G and Stratum XII in Area H, Philistine Bichrome first appears along with a continued appearance of local Myc IIIC.[7] This later mixture of local Myc IIIC and Bichrome Philistine can be explained in three ways: 1. continued production of the local MycIIIC in a time when the Bichrome style was already produced; 2. continued use (but not production) of local MycIIIC vessels in the following period; 3. stray sherds found in later level. I prefer the last two possibilities as more plausible.[8]

A similar stratigraphic sequence emerged at Tel Miqne (Dothan 1989, 2000, 2003 and in this volume) where Stratum VII contained large quantities of local Myc IIIC pottery while Bichrome Philistine only started to appear in Stratum VI when the local Myc IIIC is still found. It seems that both Ashdod XIII and Tel Miqne VII were constructed on the terminal level of the Late Bronze Age city which contained both imported Myc IIIB and Cypriot wares.[9]

Moshe Dothan claimed that the local Myc IIIC in Philistia (called by him "Myc IIIC1") represented an early wave of Sea Peoples that preceded Year 8 of Ramesses III (Dothan 1992b: 96 and to some extent Trude Dothan 1982: 295). A while back I suggested that this pottery was produced locally by the first Philistine settlers at the time of their initial settlement following Ramesses III's 8th year (Mazar 1985a; 1988). At the same time, Ussishkin published his claim that the absence of such pottery in the Canaanite/Egyptian city of Lachish Stratum VI, securely dated to the time of Ramesses III, indicates that the local Myc IIIC pottery at Ashdod and Tel Miqne started to be produced after the end of

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[6] The transliteration of these names varies: Teukrians and Shardana (Redford 1992), Sikilis and Sherden (Stern 2000: 197).

[7] Note that at Ashdod Area G only Stratum XIIIB contained local Myc IIIC pottery while in Stratum XIIIA Bichrome Philistine pottery started to appear. It seems that Stratum XIIIB in Area G correlates with the entire Stratum XIII in Area H while Stratum XIIIA in Area G correlates with Stratum XIIB in Area H.

[8] Note that the Bichrome Philistine style also includes vessels painted in one color, either red or black (e.g. Mazar 1985: figs. 13:2; 7; 15:15–16; 16:7 etc.). Such vessels, though "mononchrome" can be easily distinguished from the earlier local Myc IIIC ("Philistine Monochrome") on the basis of fabric, production techniques and stylistic analysis. This may cause confusion, and for this reason, I prefer to avoid the latter term.

[9] The presentation by Killebrew (1998) of a level without any imported ware separating the 13th century level from the initial appearance of local Myc IIIC at Tel Miqne was based on a local feature in one excavation square. Gitin and Dothan rejected her conclusions on the basis of new data that was gathered when the same excavation area was expanded and the upper part of Field I at Tel Miqne was revealed (S. Gitin and T. Dothan, oral communication).

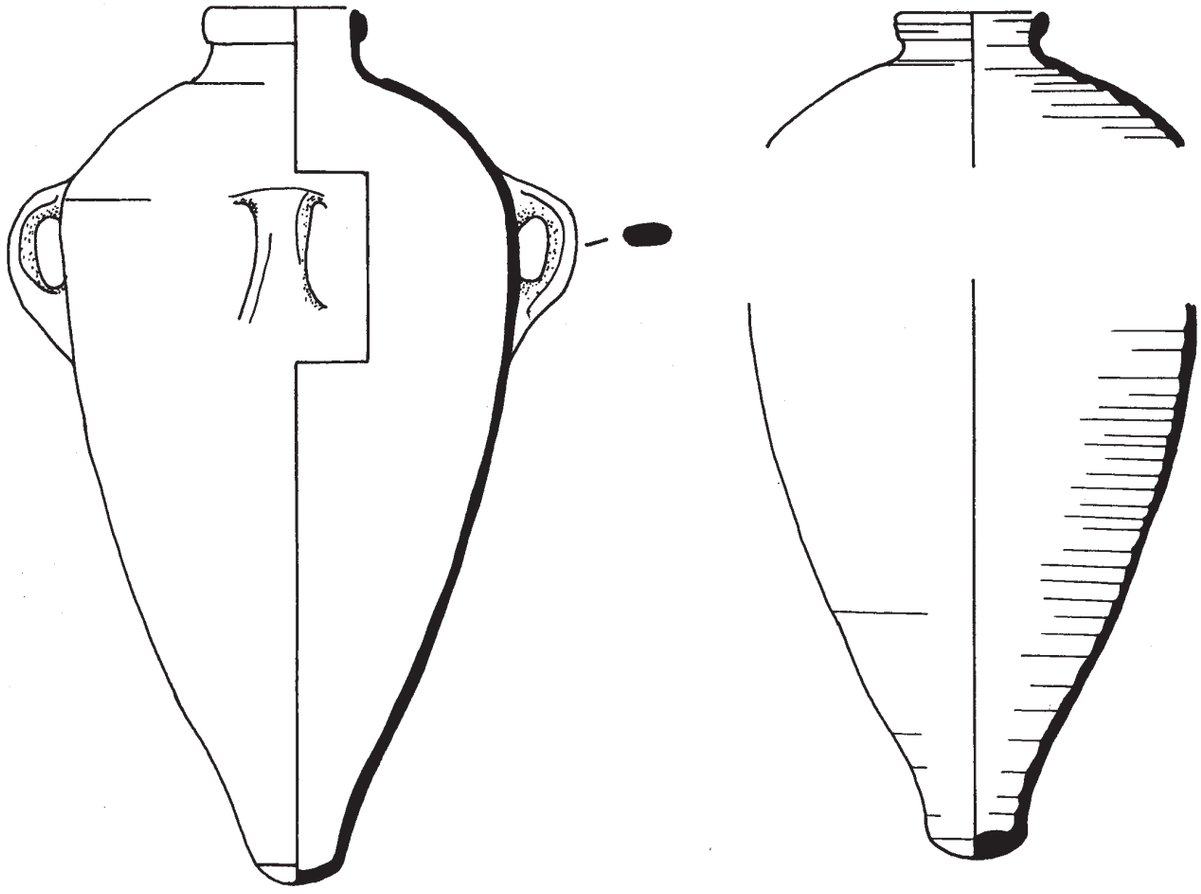


Fig. 3 Local storage jars from Ashdod Stratum XIII (right) and Lachish Stratum VI (left)

the Egyptian control over Canaan ca. 1240 BCE (Ussishkin 1985). This assertion was the starting point of Finkelstein's "Low Chronology" for the Iron Age (Finkelstein 1995; 1998; 2000:162–165; for response see Mazar 1997). Against this hypothesis I would emphasize the following:

1. The Canaanite pottery in Ashdod XIII (Dothan and Ben-Shlomo 2005:70–78) and Tel Miqne VII includes types typical to the 13th century and beginning of the 12th century BCE, and is identical to that from Lachish Stratum VI (Yannai 2004: 1051–1055; 1122–1140). While it may be claimed that this is not a sound criterion, since Canaanite pottery forms continued to survive throughout the Iron Age I, I nevertheless refer the reader to few specific forms that started to appear in the 13th century and disappeared after the end of the Egyptian presence in Canaan. Most distinctive are the jars with narrow knob base found at Lachish Stratum VI (Yannai 2004:1130 fig. 19.45:1–7; 1135 fig. 19.49) and Ashdod (Dothan and Ben-Shlomo 2005:75, fig. 3.6:6–9) (Fig. 3). Such distinct pottery type indicates that Lachish VI and Ashdod XIII must have been contemporary.

2. The local MycIIIC is very much related to and inspired by the Myc IIIC pottery from Cyprus and elsewhere in the Aegean that disappears after the mid 12th century BCE. Studies of this pottery have shown a relationship to the local Myc IIIC pottery in Philistia to that of Cyprus during the LCIIIA period and to that of the Aegean in LHIIIC Early. It is hard to believe that such pottery would have been produced in Philistia at a time when the style had disappeared on Cyprus and in the Aegean regions (Sherratt 2003: 46–47).[10]

[10] Following examination of the Philistine Monochrome (local Myc IIIC) pottery from Ashdod and Tel Miqne, Ms. Penelope Mountjoy indicated to me its early date in terms of the internal Aegean sequence. See Mountjoy's paper in this volume. Prof. J. Maran, in a lecture during a visit to Jerusalem in March 2004, also commented on the resemblance of the local Myc IIIC pottery of Philistia to Myc IIIC Early in the Aegean.

3. Ussishkin (1985) and Finkelstein (1995; 1998; 2000) contend that it is inconceivable that local Myc IIIC did not reach other sites in Philistia and the Shephelah when such pottery was produced in the major Philistine cities of Ashdod, Ashkelon and Tel Miqne. This claim was contested by myself (1997: 157–159) and by Bunimovitz and Faust (2001). Ussishkin's and Finkelstein's view would lead to the inevitable conclusion that most of the southern coastal plain and the Shephelah suffered from wide-scale occupation gaps at a time when local Myc IIIC was produced at the major Philistine cities.[11] Is it realistic to suppose that such a gap existed when the Philistines settled and produced their earliest ware – the local Myc IIIC? Such occupation gaps do not fit the evidence from sites like Gezer with a clearly defined stratigraphic continuity between LBIIB and Iron I (Dever 1986: 8–9, fig. 2 and 51–86).

The explanation for the production and distribution of the local Myc IIIC pottery only in the major Philistines cities must be cultural – not chronological. This phenomenon may teach us something about the dynamics of what appears to be a unique and unusual immigration process virtually unparalleled in the archaeology of our region.[12] The production on large scale of both fine table ware and cooking ware of Aegean forms, production techniques, and decorative style in a limited number of sites should be regarded as evidence for the manifestation of a foreign culture that has been implanted into a new cultural environment (Mazar 1985; 1988; Stager 1995).

During the earliest phase of Philistine immigration the locally-produced Aegean type pottery was not a trade good or an exchange item, but a production line intended for the newly arrived immigrants that imitates the style known to them at home. Some differences between the local Myc IIIC at Ashdod and Tel Miqne (mainly in decorative motifs) may have resulted from slightly different traditions brought by different potters or population groups that moved into the region (Yasur-Landau 2003a: 47–50).[13] This early stage might have lasted one generation, during which time the few major Philistine cities maintained their own foreign culture and kept only casual connections with the Canaanite cities at their periphery. The locally produced MycIIIC should be regarded a cultural marker and item of self identification, and thus its distribution remained limited to the communities of immigrants. Tel Lachish is located 25 km distance from each of the major Philistine cities of Ashkelon, Ashdod and Tel Miqne-Ekron. This is clearly far enough for there to be a cultural border between the zone of Philistine settlement and the Canaanite city in the inner Shephelah. Thus, the lack of local Myc IIIC pottery in Stratum VI at Lachish, Gezer, and other sites outside the Philistine heartland should not be viewed as indications of chronological separation, but as a result of the cultural isolation of the new settlers.

Supporting this view are other examples of pottery groups that were restricted to specific regions and having a clear distribution border between them. In the past, I have mentioned the case of the Khirbet Kerak ware of EBIII (Mazar 1997: 158; see now Zuckerman 2003: 181). Another parallel is the Collared Rim jar. Such jars were popular from the Yarkon basin northwards and from the border zone between the Shephelah and the Judean hills eastwards. From some reason, pithoi of this type (indeed, pithoi of any kind) were not in use in Philistia and the northern Negev and were extremely rare in the Shephelah during the Iron Age I.[14] This again indicates a cultural border rather than chronological phasing.

Finkelstein (1995; 1998; 2000) believes that Philistine settlement could not have occurred during the 20th Dynasty since Egyptian domination would not have permitted such wide-scale settlement. This issue was addressed in the past by several authors. Bietak, for example, followed by Stager, suggested

[11] Attempts to identify local Myc IIIC at other sites (Finkelstein 1995) have either failed (like at Gezer) or remain unproven (like at Tel Gerisa and Tell el-Hesi). The only site where a small amount of such pottery was probably found is Tel Haror, yet this is based on oral information provided by E.Oren.

[12] The only parallel is perhaps the Egyptian colonization of southern Palestine during the late Fourth Millennium BCE. See Van den Brink and Levy (eds.) 2003.

[13] Yasur-Landau (2003b: 240) suggested that differences between the locally-produced Myc IIIC at Ashdod, Tel Miqne and Ashkelon should be regarded as indicators of slight chronological differences between the settlement of these sites by different groups of Philistines. If such differences existed at all, they may signify somewhat differing traditions carried simultaneously by groups of immigrants, each with is own tradition. Yet it seems to me that the material available today is too limited to warrant such a conclusion.

[14] Only few sherds have been found at sites of the inner Shephelah such as Beth Shemesh and Tell Beit Mirsim. Note their lack at Gezer and Tel Batash, while they do appear at Aphek and Tell Qasile.

that a "cordon sanitaire" separated the Philistines from the Egyptians (Bietak 1993; Stager 1995). In my view, a better explanation can be found in the status of Egyptian power during the 20th Dynasty. Egyptian presence in Canaan at that time seems to have been much weaker than during the previous 19th Dynasty. A good number of Egyptian strongholds from the former period were abandoned, including Deir el-Balah (Dothan 1993: 344), Jaffa, and Aphek (Beck and Kochavi 1993: 68). The situation at Tell el-Farᶜah (South) remains unclear. Egyptian presence during the 20th Dynasty is only known from Tell Seraᶜ, Lachish (?)[15] and Beth Shean. At Beth Shean there were a number of elaborate buildings and monuments during the 20th Dynasty, such as the governor building (Building 1500) (James 1966: 8–11; Mazar 2006: 61–82). But such impressive buildings and monuments should not deceive us, since they may actually conceal an internal weakness. Egyptian dominion in Canaan during the 20th Dynasty, particularly after the 8th year of Ramesses III, appears to be on decline. Thus, the establishment of massive urban settlements during this time at several key sites in Philistia by Philistine immigrants should not be dismissed, and it may in fact be hinted at in Papyrus Harris I. In my view, Philistine settlement occurred immediately after Year 8 of Ramesses III. The production of local MycIIIC pottery marks the initial stage of their settlement. In a later stage, probably from the last decades of the 12th century BCE Philistine Bichrome pottery slowly emerged. Certain individual vessels like the skyphos bowl from Ashkelon and the jug from Tel ᶜEton (Dothan 1982: 100: fig. 3; 145: pl. 62) illustrate this gradual transition: while their decoration remains similar to Myc IIIC, they are painted in two colors, red and black.

4. The origin and nature of the Philistine settlement

Until the 80's of the last century, the notion of Achaean immigration into Cyprus during the LCIII period as suggested by Furumark, Catling, Dikaios and Karageorghis was dominant. Achaean settlers in Cyprus during LCIII were thought to have been responsible for a number of new features on the island, among them, the wide-spread use of Myc IIIC1b pottery of local manufacture as the main ware, replacing the previous hand-made Cypriot pottery groups. In contrast, the "Sea Peoples" were regarded as marauders, destroyers of cities and pirates. This latter notion can still be found today (e.g. Betancourt 2000: 300 who refers to the wars with the Sea Peoples as one possible reason for the economic decline at the end of LHIIIB on the Greek mainland).

In 1985, I suggested comparing the Achaean immigration into Cyprus with the Philistine immigration into Philistia. To support this hypothesis, I noted the similarity in phenomena taking place on Cyprus and in Philistia at the same time. This included the existence of thriving urban cultures in both regions during the 12th century and the wide-scale use of locally-produced Myc IIIC pottery. I suggested that these phenomena were probably related to one another and that Philistine immigration should be regarded as an extension of Achaean immigration into Cyprus (Mazar 1985a, 1988; also Dothan 1982: 293). The question that faces us now is to what extent this proposal is still valid today?

Over the past 20 years a number of new interpretations have been set forth with reference to Cyprus and Philistia. The notion of continuity in Cyprus from LCIIC to LCIII called some scholars for a reduction in the impact of massive immigration from the Greek mainland at the beginning of LCIII. Instead, Sherratt, Kling and others proposed that local developments rather that outside invasion lay behind the changes in Cyprus. But there are still differing assessments as to the weight of local developments vis-à-vis ideas introduced into Cyprus by newly arrived people. As Kling (2000: 291) puts it, "Clearly a balance must be achieved between those interpretations that emphasize continuity and those that focus on change". Deger-Jalkotzy (1994) and Karageorghis (2000) still opt for the idea of an influx of people from the Aegean into Cyprus during LCIII (see also Yasur-Landau 2003a: 45–47).

Concerning Philistia, Brug (1985) suggested that Philistine culture owes more to local developments than to massive immigration from the Aegean, and Sherratt (1998) has attempted to explain Philistine pottery as a manifestation of a new economic pattern in the Levant during the 12th century BCE, negating immigration of any sort. On the other hand, Dothan, Stager (1985; 1995), myself (1985), and most recently Sharon (2001), Barako (2003) and

[15] It remains unclear whether Lachish VI was an Egyptian stronghold or just a Canaanite city with some Egyptian presence and much Egyptian influence.

Yasur-Landau (2003a) maintain that the Philistine phenomenon represented immigration from somewhere in the Aegean world, even if there is no agreement as to the precise origin or route. The components of Philistine material culture as revealed in the archaeological record at Ashdod, Ashkelon, and Tel Miqne-Ekron strengthen the immigration hypothesis as the most plausible one. These components include the locally-produced Myc IIIC which appears together with domestic wares of Aegean types (cooking pots and *lakinae*), clay figurines of Aegean or Cypriote type, hearths, bathtubs, clay loom weights, ring topped knife handles, and more (Dothan 2003). Many of these features also appear in LCIII Cyprus as new features (Karageorghis 2000).

The archaeological evidence indicates that the early Philistine settlers were accustomed to an urban way of life. Tel Miqne is probably the best demonstration of the nature of the Philistine settlement: they founded a large and fortified city of ca. 18–20 hectares in a site which was previously a rather small Late Bronze town; it is an inland site, located far from the main trade routes or harbors.[16] Due to its location, Tel Miqne could in no way be part of an international trade system (contra Sherratt 1998). It therefore appears that the settlement strategy of the Philistine settlers was oriented towards a convenient location for establishing a large, well planned city with a spacious and fertile agricultural hinterland. The same may be deduced from their choice of Tel Ṣafit (Gath) as the location of another major Philistine city (Maeir and Ehrlich 2001). Of the three coastal cities of the Philistine Pentapolis (a definition based on the biblical narrative), only Ashkelon and Ashdod are known archaeologically to some extent. At Ashdod the earliest Philistine city (Stratum XIII) seems to be well planned. Ashkelon also appears to be well planned, though the earliest phases of Philistine occupation are still unknown. It should be emphasized that at both Ashdod and Ashkelon the precise size of the Philistine cities are unknown.[17]

According to at least some recent views, most notably that of Karageorghis (2000), Aegean settlers on Cyprus may have contributed to the continuation and flourishing of large urban centers along the western, southern, and eastern coasts of the island in LCIII. In this way, we can view urban Philistine culture as an extension of the contemporary phenomena in Cyprus. Moreover, both events were probably related to the circumstances of the crisis in the Aegean at the end of LHIIIB. Thus, Aegean immigration into Cyprus and Philistine immigration into Philistia may have been part of similar historical processes. We are unable at this time to be more precise regarding the origin of the Philistines and the role played by Cyprus in the process of their immigration. Thus, is spite of various trends and scholarly suggestions, it seems that the discoveries in Philistia during the last decades support my views presented some 20 years ago concerning both the date and the nature of the Philistine immigration.

[16] During the symposium at Vienna, Ussishkin suggested that the city walls found in Fields I and X at Tel Miqne should be dated to the Iron Age II and that the Iron Age I lower city at Tel Miqne was much smaller than presented by the excavators. This view stands in contrast to the data published so far and should be rejected (Dothan 2003 and her paper in this volume).

[17] In the Vienna symposium, Finkelstein suggested a shift of power between the Philistine cities based on their changing sizes. Alas, only the size of Tel Miqne can be established with any confidence. At all other Philistine sites (i.e. Ashkelon, Ashdod and Tel Safit-Gath) the size of the settled area during the 12–11th centuries remains unknown.

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The Dating of the Early LC IIIA Phase at Enkomi

Penelope A. Mountjoy[*]

After the destruction of Enkomi Level IIB at the end of LC IIC P. Dikaios suggested there was a gap of ten-twenty years before the LC IIIA Level IIIA buildings were constructed (Dikaios 1969–71, 486 table, 520–21). There is indeed a gap. In the make-up of the original floors of the Level IIIA Ashlar Building in Area I mixed with Mycenaean IIIB lustrous painted pottery there is also LH IIIC pottery. It does not belong to the beginning of LH IIIC Early, but to a later phase of this period. Also, it does not differ from the pottery found in the Level IIIA destruction deposits on the floors, suggesting the building was in use for quite a short period. Both the pottery in the floor make-up and that on the floors is equivalent to LH IIIC Early Phase 2 at Mycenae and at Tiryns, that is the second half of LH IIIC Early (See French, this volume).

The sequence in Room 14 is particularly instructive. The original floor of the Ashlar Building in this room is Floor V. A pit was subsequently dug through Floor V and sealed by a new floor on which a hearth was built. The hearth had a lower layer of mortar, a middle layer of sherds and an upper layer of concrete (Dikaios 1969–71, 176). Dikaios noted LH IIIC pottery in Floor V (Fig. 1.1–4), in the fill of the pit below the floor (Fig. 1.5–7), in the make-up

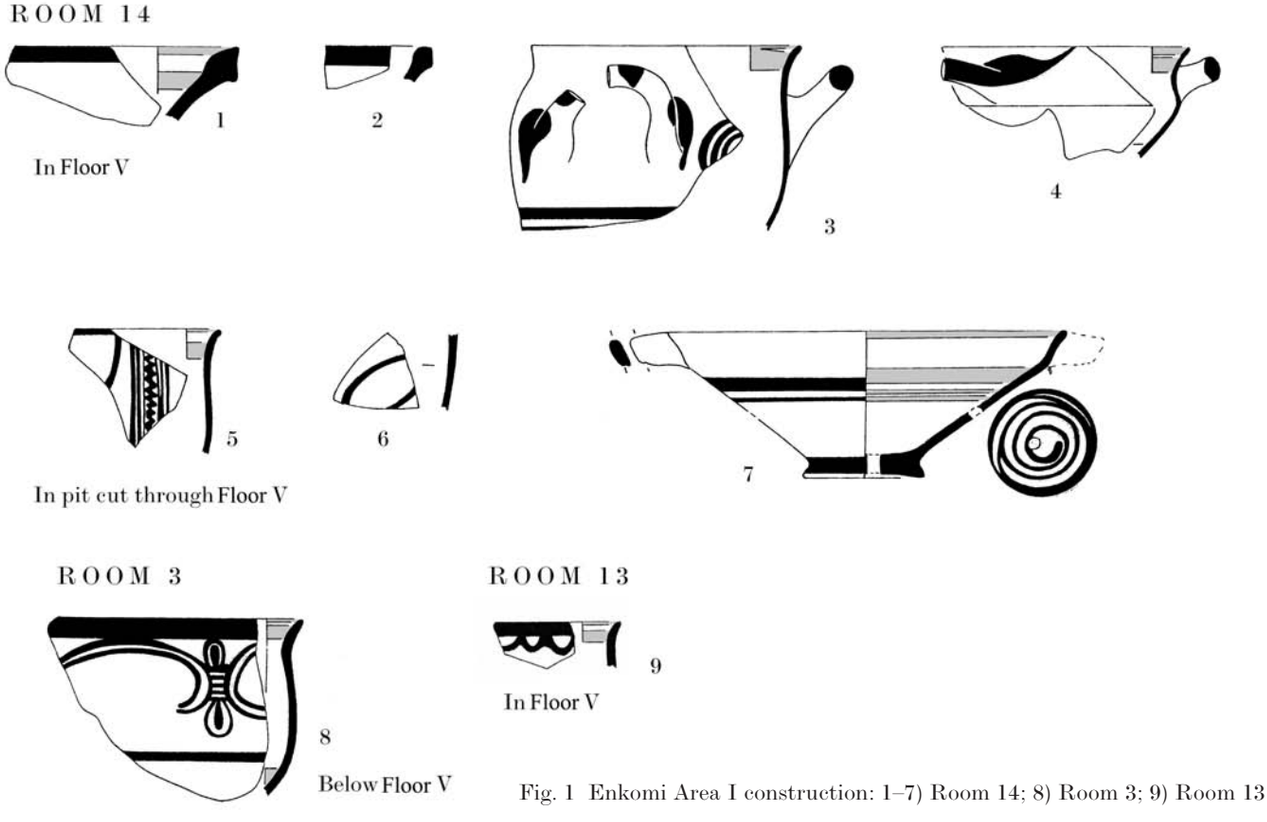


Fig. 1 Enkomi Area I construction: 1–7) Room 14; 8) Room 3; 9) Room 13

[*] British School at Athens

of the hearth (Fig. 2) and in the Level IIIA destruction on Floor V. He pointed out that the lapse of time from the construction of the Ashlar Building to its destruction must have been very short (Dikaios 1969–71, 176). A comparison of the construction and occupation deposits shows that both have the same LH IIIC pottery types. Of particular interest is the presence in both deposits of the linear shallow angular bowl FS 295 with a deep upper body (Fig. 1.4) and (Fig. 2.12). This is a true LH IIIC shallow angular bowl as opposed to the local Cypriot LH IIIB–IIIC type with short upper body, as Fig. 1.7. This type with deep upper body does not begin at Mycenae until LH IIIC Middle (Mountjoy 1986, fig. 233.1, French, this volume Features Chart). Since the Level IIIA Enkomi pottery has its best parallels with the second phase of LH IIIC Early at Mycenae, it may be that this deep type evolved in the eastern Mediterranean and went from here to the Greek Mainland.

Pottery from construction deposits in other rooms (Fig. 1.8–9, Fig. 3) also has parallels to that in the destruction deposits on the floors. The deep bowl with joining semi-circles pendent from the rim in the floor makeup of Room 45 (Fig. 3.16) and Room 13 (Fig. 1.9) has parallels in the occupation deposit in Room 14 (Fig. 2.9). This is one of the most popular motifs on deep bowls in the Enkomi Level IIIA destruction deposits. In the occupation material from Room 14 there is a complete deep bowl (Fig. 2.8) with antithetic streamers rising from the belly band for which there are parallels from the floor make-up of Room 3 (Fig. 1.8) and Room 12 (Fig. 3.1). This is a LH IIIC Middle feature on the Mainland, but LH IIIC Early on Crete (Popham and Milburn 1971, 340, Mountjoy 1999b, 513–14). The deep bowl (Fig. 2.10) has a reserved interior rim band and a reserved circle in the interior base. The reserved circle begins in Transitional LH IIIB2/LH IIIC Early on the Mainland (unpublished examples from Tiryns West Wall deposit); the narrow interior rim band begins in LH IIIC Middle (Popham and Milburn 1971, 340, 339, fig. 4.2–3,5,11), but is already present in a wider version in LM IIIC Early (Mountjoy 1999b, 512–13). There are jug sherds with tassels (Fig. 2.2–3), which are not very common at Enkomi, and a strainer jug with antithetic stemmed spirals flanking a triglyph (Fig. 2.4); the former is a LH IIIC Early Phase 2 motif at Mycenae, the latter is Phase 1 (French, this volume).

A date in Greek Mainland terms for the Level IIB destruction is suggested by the deep bowl (Fig. 3.9) with fat wavy line and lipped rim from the make-up of Floor V in Court 64. This vessel would be Transitional LH IIIB2/IIIC Early on the Greek Mainland (Mountjoy 1997, 109–37). It was found with the usual admixture of LH IIIC material (Fig. 3.8) and with the lustrous painted Mycenaean IIIB sherds (Fig. 3.10–11). Although the floor make-up of Court 64 is a mixed deposit comprising LH IIIC pottery together with LC IIC destruction material, yet the presence of the transitional bowl suggests that the LC IIC destruction at Enkomi might have taken place in the Transitional phase in Mainland terms.

The occupation material from Room 27 (Fig. 4) comes from below Floor III, from a pit dug when a rearrangement took place on the original floor of the Ashlar Building. This original floor was repaired as Floor III (Dikaios 1969–71, 177). The deep carinated bowl (Fig. 4.9–10) is present, as in the floor makeup deposits (Fig. 1.4). There is also a carinated krater (Fig. 4.3). This would be LH IIIC Middle Phase 1 (Developed) on the Greek Mainland (Mountjoy 1986, 156), but it may have evolved earlier in the eastern Mediterranean, perhaps deriving from LC IIC Rude Style kraters. Some of these vessels have carinated bodies (for example Karageorghis 1965, pls. 23.1–2, 28.5). The carination is low on the body of the Rude Style kraters, but it is possible that by LH IIIC Early Phase 2 it had moved higher up the body.

The pottery from the destruction debris of the Level IIIA Ashlar Building also has parallels to that from the occupation and construction deposits. The destruction deposit in Room 27 has deep bowls with stemmed spirals pendent from the rim (Fig. 5.6), as in the occupation pottery (Fig. 4.6); the deep shallow angular bowl (Fig. 5.8), is seen in the occupation pottery (Fig. 4.9–10) and also in the floor make-up pottery (Fig. 1.4). The antithetic spirals with lozenge instead of a central triglyph (Fig. 5.7) are a LH IIIC Early Phase 1/2 feature at Mycenae (French this volume), and the deep bowl with pendent joining semi-circles from the rim (Fig. 5.4) also has parallels in the occupation and floor make-up pottery (Fig. 2.9, Fig. 3.16). The sherds with birds (Fig. 5.1–2) belong to strainer jugs. Birds with a similar body with filled centre surrounded by dots are also found on vases from Maa Period I (Karageorghis and Demas 1988, pl. CXCII.354) and Kition (Karageorghis 1981, pl. 9.12, pl. 13). They need not be from the same workshop, but they are certainly the same date. The krater sherd from Court 64 with filled tail to the spiral (Fig. 6.5) has a very good parallel on a mug from LH IIIC Early Phase 2 at Tiryns (Fig. 6.9).



Fig. 2 Enkomi, Area I Level IIIA occupation: Room 14, hearth deposit

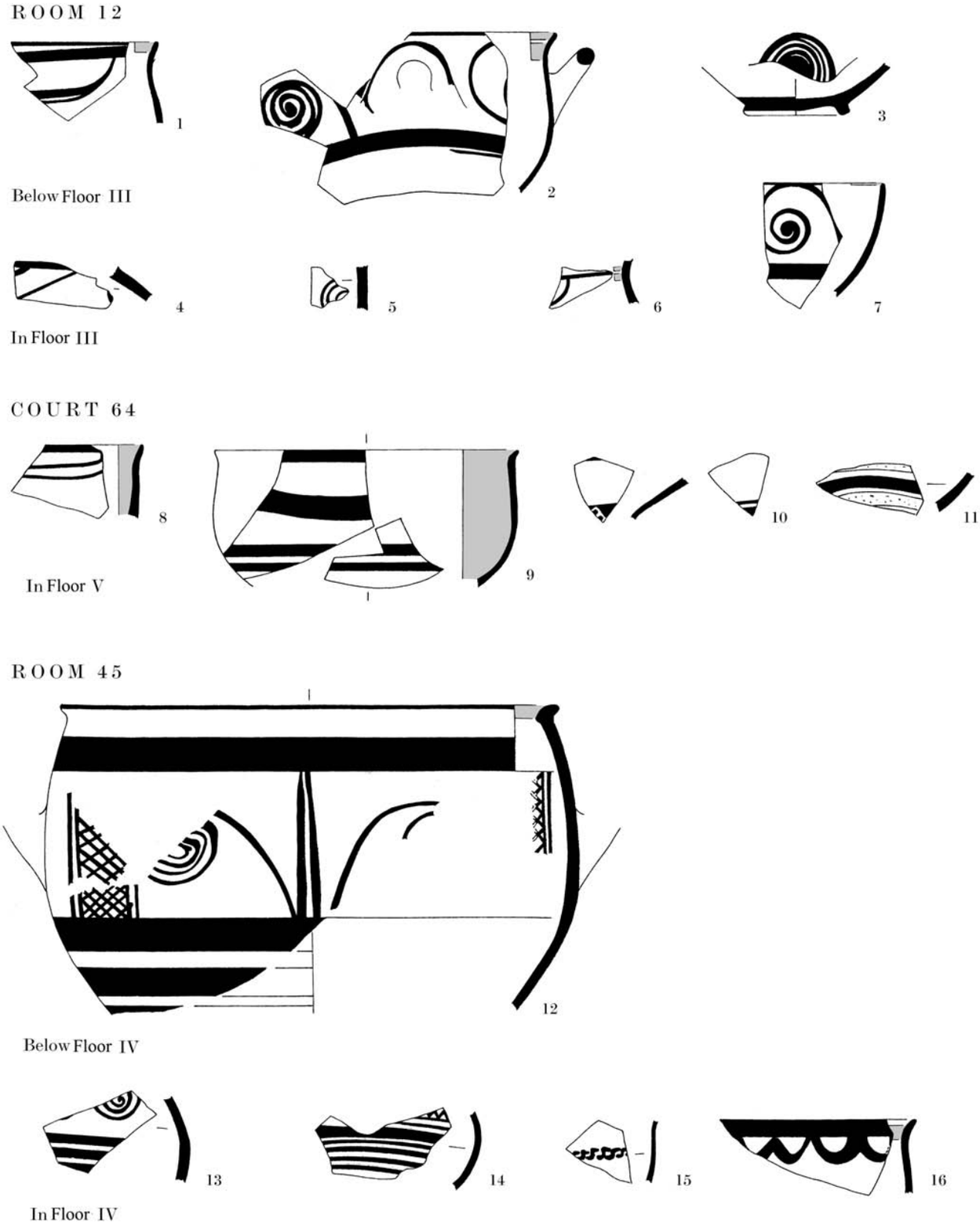


Fig. 3 Enkomi Area I construction: 1–7) Room 12; 8–11) Court 64; 12–16) Room 45

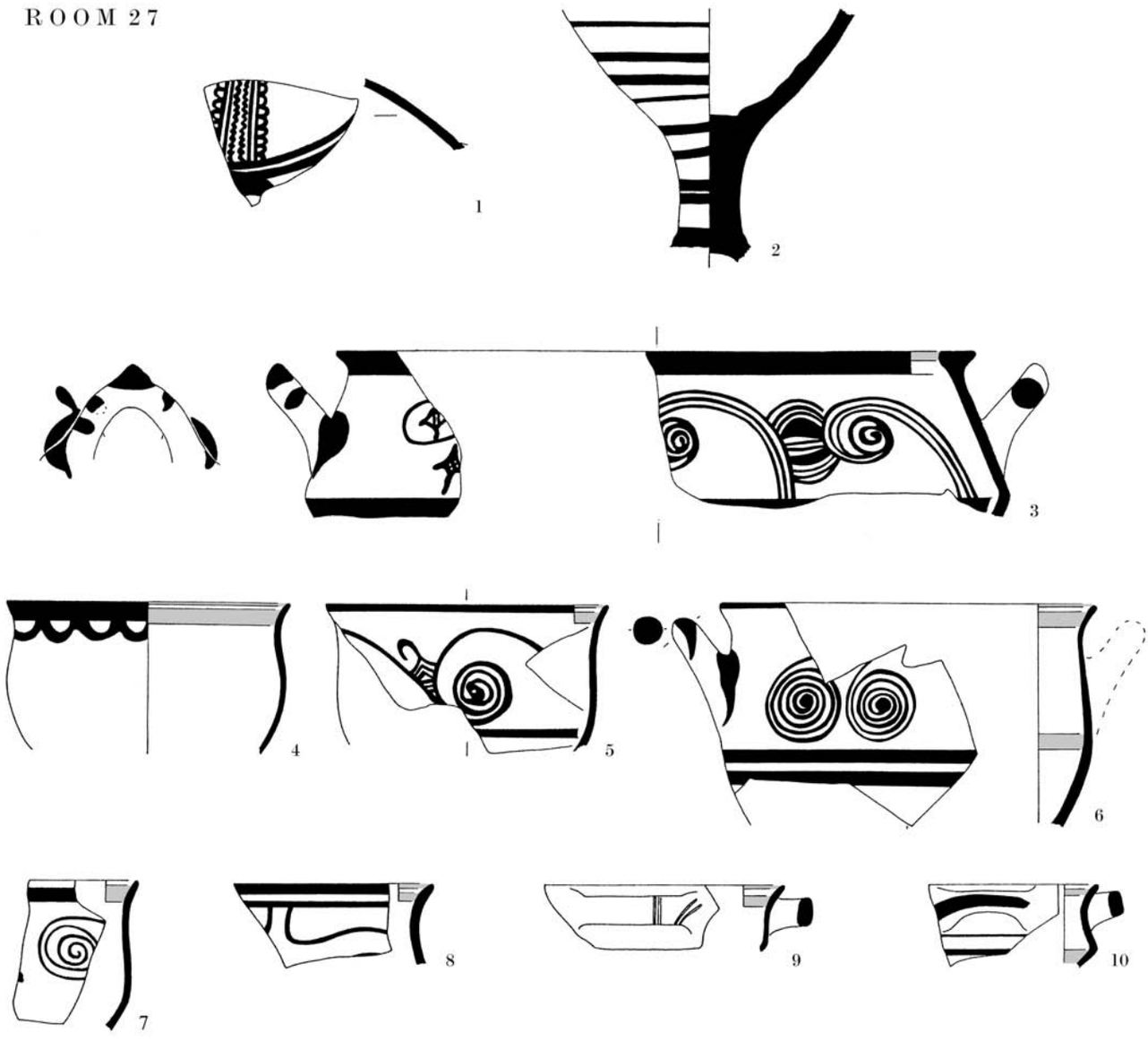


Fig. 4 Enkomi, Area I, Level IIIA occupation: Room 27

Phase 2 at Tiryns is borderline with LH IIIC Middle. The deep bowl with monochrome interior with reserved rim band (Fig. 6.8) is similar to that in Room 14 (Fig. 2.10). The bowl from Room 35 (Fig. 6.1) has antithetic streamers linked by chevrons; there is a very good LH IIIC Early Phase 1/2 parallel from Mycenae (French this volume).

This is a very brief overview of the construction, occupation and destruction deposits in Level IIIA at Enkomi. There seems to be no development between the LH IIIC pottery below the floors of the building and that on the floors in the destruction level, suggesting a short phase. The parallels to the pottery are LH IIIC Early Phase 2 at Mycenae and Tiryns going into LH IIIC Middle. In Greek Mainland terms the construction of the Level IIIA buildings took place in the second phase of LH IIIC Early and the destruction took place when LH IIIC Middle was just being produced. There is certainly a gap at Enkomi between the LC IIC destruction and the Level IIIA reoccupation, at least in the area excavated by Dikaios. However, it is quite possible that in other unexcavated areas of this very large site there was immediate re-occupation after the LC IIC destruction and no gap.

It is suggested above that the carinated krater and deep shallow angular bowl possibly developed in the eastern Mediterranean and went from there to

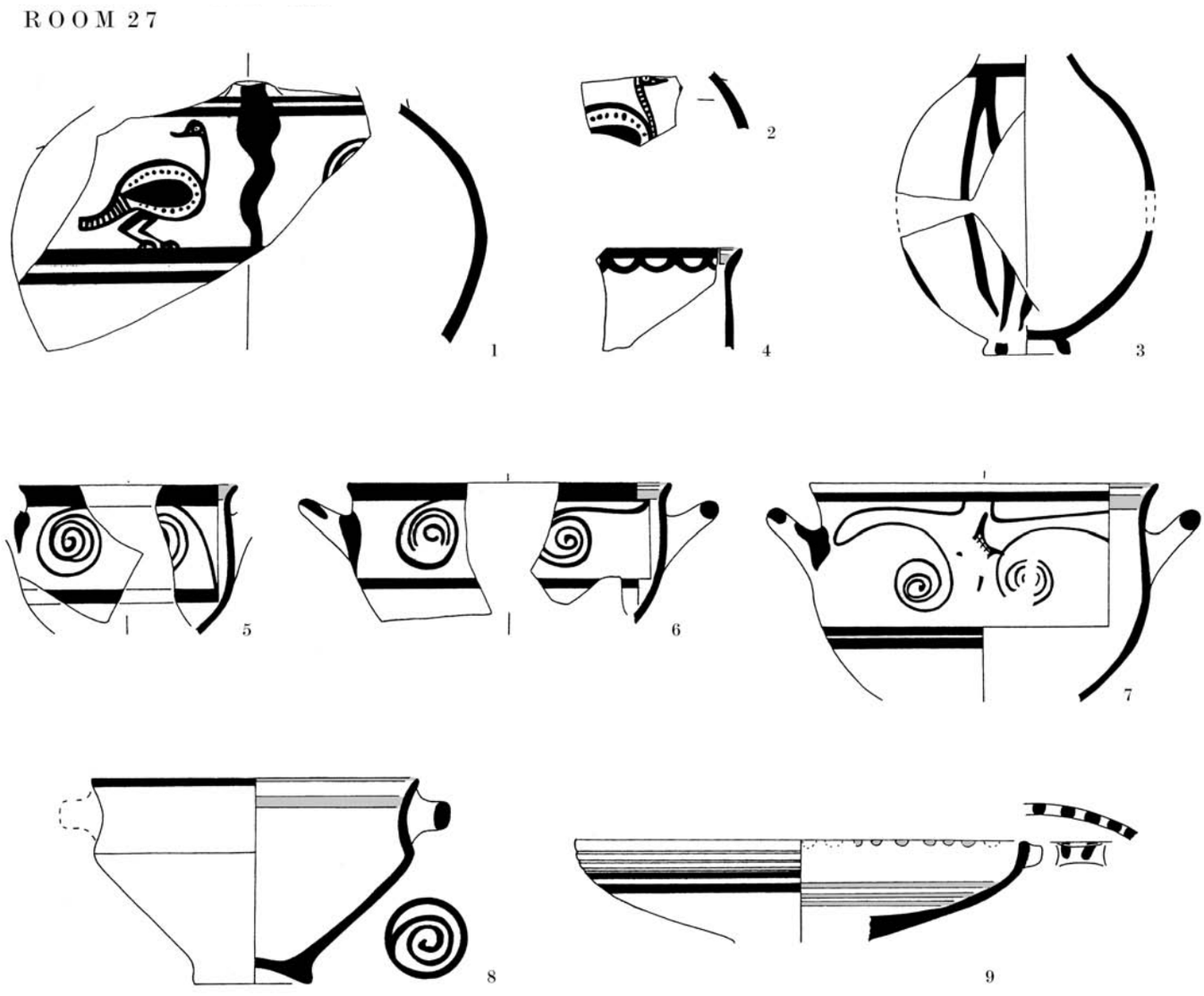


Fig. 5 Enkomi, Area I, Level IIIA destruction: Room 27 on Floor III

mainland Greece. The one-handled conical bowl FS 242 may also have this derivation. The shape appears in LH IIIC Middle on the Mainland (Mountjoy 1986, 155, 172). It is present in both Period I and II at Maa: Palaeokastro (Fig. 7.2–3), which might suggest that both phases of Maa belong to LH IIIC Middle. However, the parallels to the pottery from Maa are with the LH IIIC Early Phase 2 pottery from Enkomi. It is possible that the shape evolved in the eastern Mediterranean in LH IIIC Early and went from there to the Greek Mainland. A forerunner can perhaps be seen in a vase from Kition T.9 Upper Burial (Fig. 7.1). The shape is almost the same as that of Fig. 7.2, but the Kition vase has two strap handles instead of one round handle. The FS 242 might have evolved from this type. The distribution of FS 242 is also significant. It is present unstratified at Tarsus on the south Anatolian coast (French 1975, 64–65, figs. 14–15), in LH IIIC Late at Emporio on Chios (Mountjoy 1999a, 1151 Chios nos. 5–6 with references), in LH IIIC Middle Phase 2 (Advanced) at Aplomata and Kamini on Naxos (Mountjoy 1999a, 959 Naxos nos. 61–63), Phylakopi on Melos (Mountjoy 1999a, 928, Melos no. 201), Lefkandi on Euboea (Popham and Milburn 1971, 340, 342, 339 fig. 4. 10,11), its type site, and in Attica at Athens and Perati (Mountjoy 1999a, 598, nos. 464–465 with references). It cuts a swathe from the LH IIIC Early Phase 2 examples in the east Mediterranean westwards right across the Aegean islands to Lefkandi

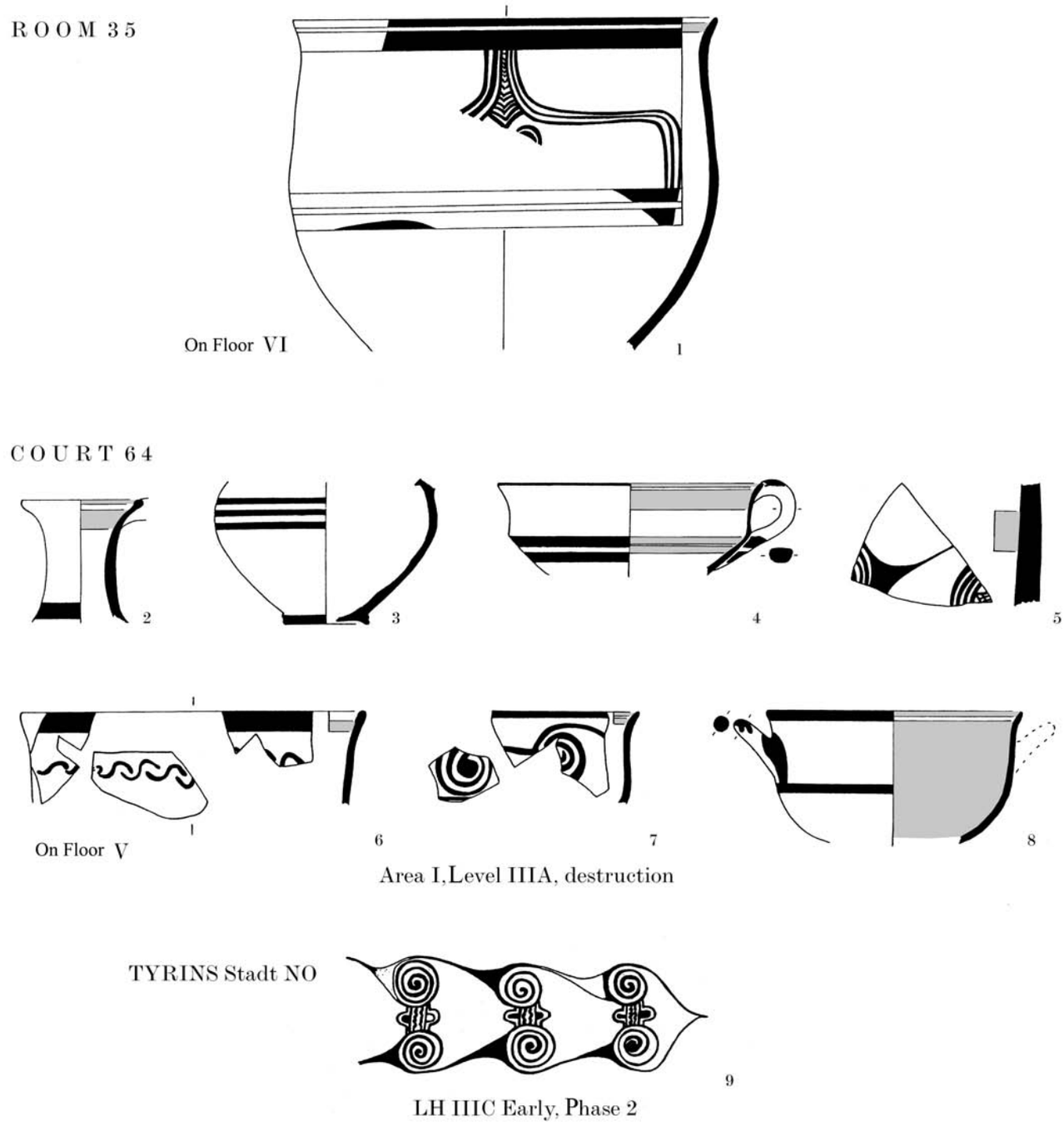


Fig. 6 Enkomi Area I destruction: 1) Room 35, 2–8) Court 64; Tiryns: 9) motif from a mug

and then to Athens; with the exception of Emporio, it appears in all these places in LH IIIC Middle. It is possible that the Tarsus examples are also LH IIIC Early Phase 2, but this cannot be proven.

There are two common motifs on LC IIIA pottery which have been borrowed from LC II White Slip II Ware. A comparison of Fig. 8.1 with the LC IIIA krater Fig. 8.2 and with the bowl Fig. 8.3 clearly demonstrates the derivation of the LC IIIA straight-sided lozenge. A comparison of the dot filled semi-circles on Fig. 8.1,5 with the LC IIIA alabastron Fig. 8.4 suggests this motif too derives from WS II Ware. This motif is present on a number of Cypriot LH IIIC vessels, for example from Pyla-

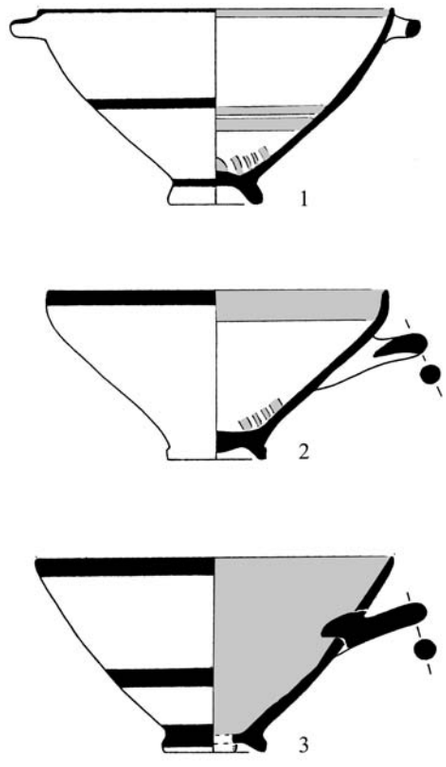


Fig. 7 1) Kition T.9 Upper Burial; 2–3) Maa: Palaeokastro

Kokkinochremos (Karageorghis 1984, pl. 35.1952.1) and from Kition (Karageorghis 1981, pl. 13). Both motifs are found on the Greek Mainland, but the dot filled semi-circles are rather rare (Mountjoy 1999a, Korinthia nos. 178,188, Attica nos. 330,356,357). It is impossible to say whether these two motifs developed independently on the Greek Mainland or whether they came from Cyprus; indeed, the dot filled semi-circle is so rare on the Mainland it might have gone there from Cyprus, but there is a LH I example from the Shaft Graves at Mycenae (Mylonas 1973, pl. 45b).

There is one other group of material I would assign to LH IIIC Early (Fig. 9). Most of it is found outside Cyprus in the Levant, but I think it is a Cypriot export. The well-known Bethshan stirrup jar (Fig. 9.1) is dated to LH IIIC Middle by Warren and Hankey (1989, 164–65). It is one of a group of stirrup jars with lozenge chain (Fig. 9). Most of these vases have lustrous paint as on Mycenaean LH IIIB pottery, but some, Fig. 9.2,4, have matt paint as on LH IIIC pottery in the Near East. The lozenge chain, if adapted from WSII pots, need not be as late as LH IIIC Middle. The vase from Kition (Fig. 8.3) provides a good LH IIIC Early Phase 1 parallel. The dot-fringed semi-circles on the Bethshan and Tarsus vases also need not be LH IIIC Middle. The motif existed much earlier, as a LH IIIA2 piriform jar from Ialysos demonstrates (Benzi 1992, pl. 90e). The triangular patch on the shoulder of the Tell Keisan vase is popular in LH IIIB2 and LH IIIC Early on the Mainland (for example Mountjoy 1986, fig. 151.3, 161.11, 165.4, 173.1, 172). The motif on the Tell Keisan vessel has slightly concave sides suggesting an early LH IIIC date. However, the main diagnostic feature of these stirrup jars is the body banding. All these vases have groups of 3–4 bands all down the body. They were possibly part of a Near Eastern group of which the Tarsus vase (Fig. 9.9), with a different shoulder decoration, provides an example. This very distinctive banding is not found on stirrup jars on the Greek Mainland or on Crete. It is present much earlier in a LH IIIA2 south Rhodian style (Mountjoy 1995, 30 fig. 9) and may have originated from the Dodecanese. These stirrup jars are not all from the same workshop. Neutron activation analysis has given Kouklia as a source for the Tell Keisan vase (Gunneweg and Perlman 1994, 559–61); it may be that they were also produced at Hala Sultan Tekke, since there are several examples from there. The stylistic parallels suggest a LH IIIC Early date for this group. The recent excavations at Bethshan have these vases stratified in Level S4, the earlier layer, which corresponds to the reign of Rameses III (see Sheratt and Mazar in press, and Mazar this volume). This level equates mostly to LH IIIC Early Phase 1, but on the low chronology of 1184–53 for this pharoah, it ends in LH IIIC Early Phase 2. The vases could date to Phase 1 or Phase 2.

To sum up. The LH IIIC Early Phase 2 pottery at Enkomi reflects a pan-Cypriote phenomenon consisting of a large corpus of shapes and motifs based on the Mainland Mycenaean repertoire which suddenly appears on Cyprus. However, in the decoration of this corpus there are elements not only adapted from White Slip Ware, but also from the Rude Style and from Minoan pottery. Space does not allow discussion of the last two. The corpus appears with all the elements fully blended with the Mycenaean elements. Since the Cypriot elements in it can only have come from the island it follows that the final blending must have taken place on the island. More LH IIIC Early Phase 1 contexts are needed to clarify this genesis.

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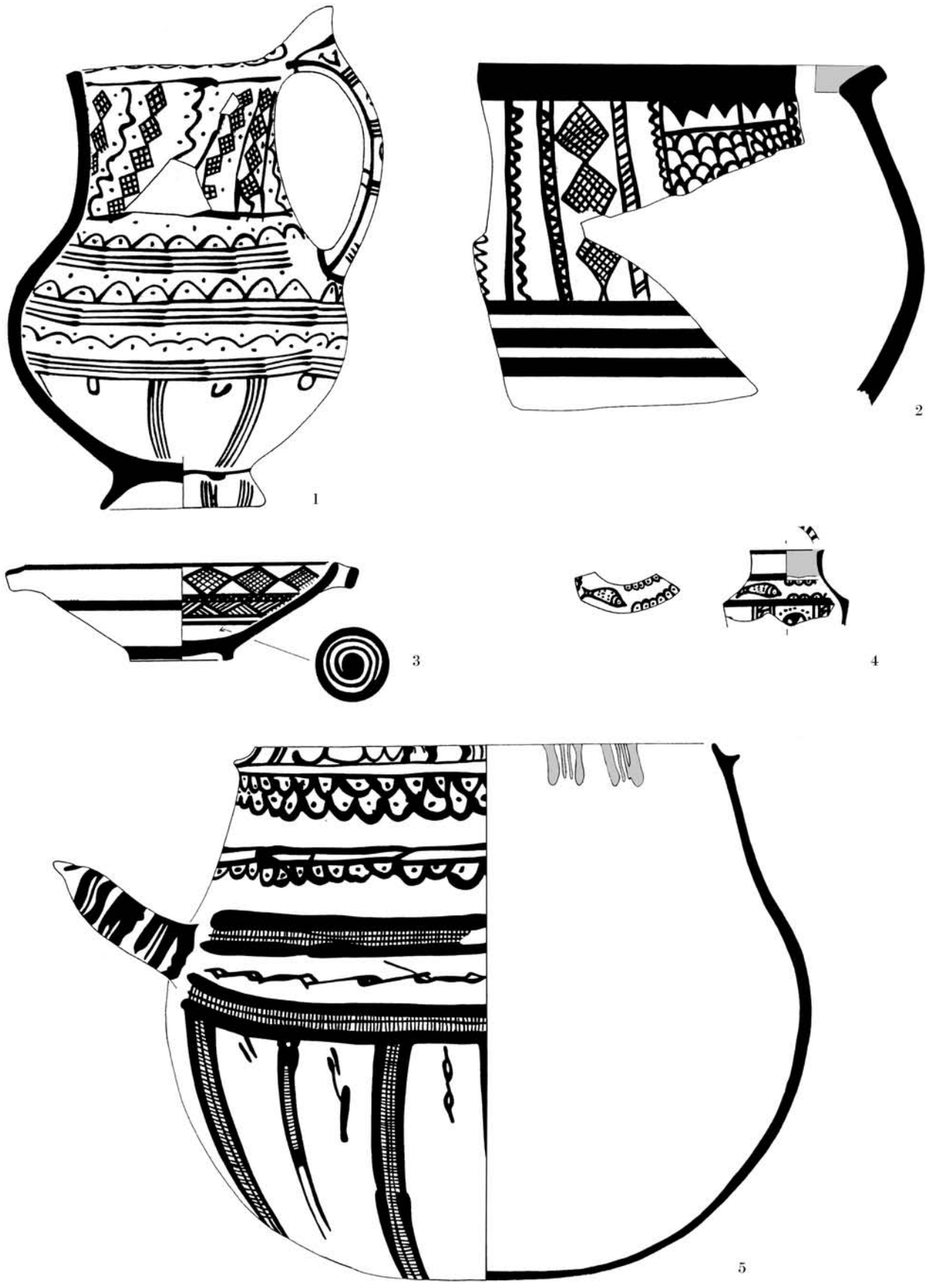


Fig. 8 1, 5) Kalavasos: Ay.Dhimitrios; 2) Enkomi Area I Room 33; 4) Enkomi; 3) Kition T.9 Upper Burial

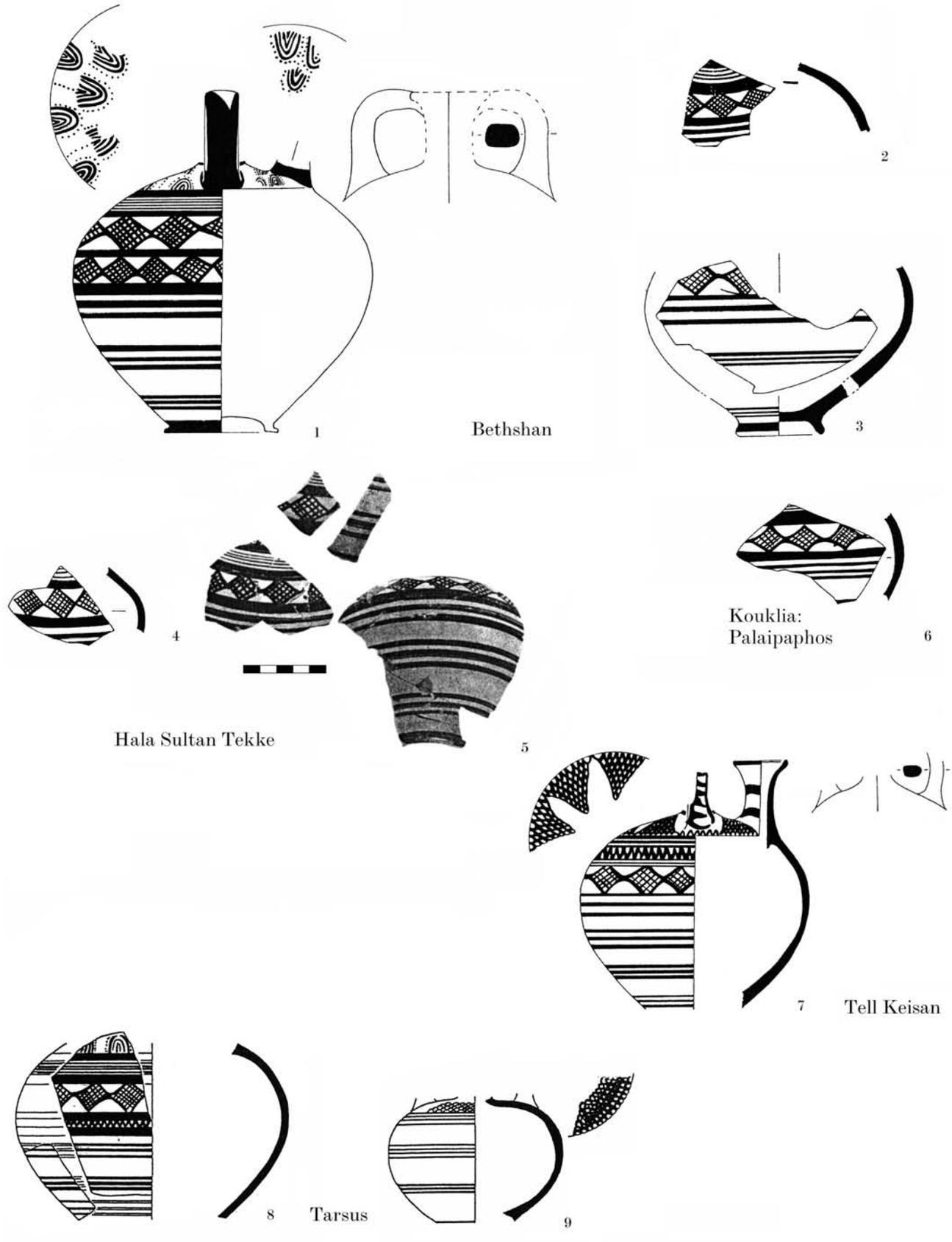


Fig. 9 Stirrup jars belonging to a Near Eastern group

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Index to illustrations

Fig. 1.1) Dikaios 1969–71, pl. 94.12; 2) *Ibid.*, pl. 94.13; 3) *Ibid.*, pl. 94.11; 4) *Ibid.*, pl. 94.10; 5) *Ibid.*, pl. 94.7; 6) *Ibid.*, pl. 94.6; 7) *Ibid.*, pl. 94.1–2; 8) *Ibid.*, pl. 70.9; 9) *Ibid.*, pl. 69.26

Fig. 2.1) Dikaios 1969–71, pl. 94.26; 2) *Ibid.*, pl. 94.22; 3) Dikaios unpublished, 758/10; 4) Dikaios 1969–71, pl. 94.29–30; 5) *Ibid.*, pl. 94.17–18; 6) *Ibid.*, pl. 94.24; 7) *Ibid.*, pl. 94.15; 8) *Ibid.*, pl. 94.28; 9) *Ibid.*, pl. 94.16; 10) *Ibid.*, pl. 94.27; 11) *Ibid.*, pl. 94.23; 12) *Ibid.*, pl. 94.21

Fig. 3.1) Dikaios 1969–71, pl. 70.26; 2) *Ibid.*, pl. 70.25; 3) *Ibid.*, pl. 70.22; 4) *Ibid.*, pl. 69.23; 5) *Ibid.*, pl. 69.30; 6) *Ibid.*, pl. 69.25; 7) *Ibid.*, pl. 71.27; 8) Dikaios unpublished, 1924/1; 9) Dikaios unpublished, 1994/2,3; 10) Dikaios 1969–71, pl. 69.39; 11) *Ibid.*, pl. 69.47; 12) *Ibid.*, pl. 100.11; 13) *Ibid.*, pl. 70.7; 14) Dikaios unpublished, 827/1; 15) Dikaios 1969–71, pl. 100.15; 16) *Ibid.*, pl. 70.4

Fig. 4.1) Dikaios 1969–71, pl. 97.7; 2) *Ibid.*, pl. 97.12; 3) *Ibid.*, pl. 97.15; 4) *Ibid.*, pl. 97.19; 5) *Ibid.*, pl. 97.18; 6) *Ibid.*, pl. 97.10; 7) *Ibid.*, pl. 97.11; 8) *Ibid.*, pl. 97.16; 9) *Ibid.*, pl. 97.13; 10) *Ibid.*, pl. 97.9

Fig. 5.1) Dikaios 1969–71, pl. 98.3; 2) *Ibid.*, pl. 98.1; 3) *Ibid.*, pl. 98.17; 4) *Ibid.*, pl. 98.7; 5) *Ibid.*, pl. 98.5,10; 6) *Ibid.*, pl. 98.9; 7) *Ibid.*, pl. 98.2; 8) *Ibid.*, pl. 98.4; 9) *Ibid.*, pl. 98.14

Fig. 6.1) Dikaios 1969–71, pl. 100.26,31; 2) Dikaios unpublished, 5890/1; 3) Dikaios 1969–71, pl. 98.33,38; 4) *Ibid.*, pl. 71.9; 5) *Ibid.*, pl. 98.32; 6) *Ibid.*, pl. 98.31,35; 7) *Ibid.*, pl. 98.34,37, pl. 99.14; 8) *Ibid.*, pl. 98.36; 9) Tiryns Stadt Nord Ost (courtesy of J.Maran)

Fig. 7.1) Karageorghis 1974, pl. CLXII.328; 2) Karageorghis and Demas 1988, pl. CCX.414; 3) *Ibid.*, pl. CCXXXV.574

Fig. 8.1) South 1980, pl. IX right; 2) Dikaios 1969–71, pl. 71.30; 3) Karageorghis 1974, pl. CLIX.174; 4) Walters 1912, C687 fig. 254; 5) South 1980, pl. IX.5

Fig. 9.1) James 1966, fig. 49.4; 2) Sherratt and Mazar in press, fig. 4.6; 3) Sherratt and Mazar in press, fig. 2.2+3; 4) Hult 1981, fig. 85; 5) Åström *et al.* 1983, fig. 487.; 6) Unpublished, TE 119; 7) Balensi 1981, pl. XI; 8) Goldman 1956, fig. 333.1282; 9) *Ibid.*, fig. 333.1280

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Exchange between Cyprus and Crete in the 'Dark Ages' ?

Constance von Rüden[*]

This paper intends to discuss different kinds of exchange between Crete and Cyprus during the so called Dark Ages, referring respectively to the Subminoan period (Crete) and to LC III B (Cyprus). In comparison to the Late Bronze Age with its interregional system of exchange throughout the Eastern Mediterranean, the Dark Ages has traditionally been seen as a period of regression and retreat, especially in regard to interregional communication. Because the archaeological data post-dating the collapse of the Late Bronze Age system is scarce and because no texts have been preserved, many studies concerning the Dark Ages have been based on the idea of regionally isolated societies in economic retreat and lacking in any standard of interregional communication. New finds, however, and changing perspectives have shed light on this picture and partly revised the idea of complete isolation. In line with this revised theory on the state of art in the Dark Ages, the following essay will investigate the question with respect to finds on Crete and Cyprus.

All material evidence indicating exchange through import and in the form of stylistic parallels has been examined against the background of the development of settlements and of changes in the material culture on both islands. In order to prevent a bias of interpretation, theories of migration have been ignored. Regarding the question of an exchange of goods, all possible imports on Crete and Cyprus, those of Cypriote and Cretan origin, as well as those of other regions in the Eastern Mediterranean have been taken into account. The phenomena of down-the-line trade und trade centres rules out the option of restricting the material base of evidence to Cretan and Cypriote finds only.

The Cretan artefacts often considered as imports have been less useful in trying to determine interregional exchange, since their context is either unknown or unreliable or cannot be exactly dated. In some other cases the poor state of preservation has prevented a stylistic or any other kind of examination as to the artefacts' place or date of origin. Under better circumstances, a transfer of ideas or materials can be proven if the stylistic or scientific characteristics of the imports and the find context are able to be dated within the examined time frame. Things are more promising in the case of Cyprus. Here there is clear evidence for an LC III B archaeological context for some of the imports. One-handled and two-handled lentoid flasks of South-Levantine origin have been found in Salamis Tomb 1[1] and in the tombs of Alaas,[2] Lapithos,[3] Kourion[4] and Palaepaphos-Kouklia.[5] A scarab from Enkomi,[6] dated stylistically to the 19th or 20th dynasty, should also be taken into account. Some exchange, especially with the Southern Levant is therefore to be assumed. CG I vessels from Israel, furthermore, support this assumption.[7] But what do these artefacts say about the range of this system of exchange? Was it restricted to Cyprus and the Southern Levant, or is it part of a far-reaching network ranging throughout the Eastern Mediterranean?

The extant data regarding imported goods on Crete is too minimal to allow an adequate assessment of the island's involvement in an Eastern Mediterranean communication system. Stylistic and thematic parallels in the local Cretan and Cypriot production do, however, hint at some kind of exchange. Quite similar to the Late Bronze Age, common stylistic and thematic elements in the different material cultures of the Eastern Mediterranean are also characteristic for the Dark Ages. Local production on Crete and Cyprus falls under this category. The most popular examples of stylistic and thematic parallels are the characteristics of

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[1] Yon 1971, 45–48, No. 93, 94, 104, 105, 106, pl. 27.

[2] Karageorghis 1973, 57, Tomb 15, No. 13, Tomb 17, No. 26.

[3] Pieridou 1965, pl. XIII, 10, Tomb P. 74, No. 79, 98.

[4] Daniel 1937, 73, 26, 66, 86, 57, 49, pl. I, IV.

[5] Karageorghis 1967, 20, figs. 8, 17.

[6] Courtois 1971, 328.

[7] Mazar 1994, 39–59.

the Proto-White Painted ware of Cyprus and the Subminoan ware of Crete.[8] But the question remains: in which cases can they be accepted as genuine proof of an exchange of style and decoration during the Dark Ages?

Such parallels are to be considered as valid indications only if they cannot otherwise be explained in the context of their local ceramic tradition. So, for the present example of Cretan and Cypriot ware, it is necessary to rule out all common features already shared by the ceramic styles of both islands in the Late Bronze Age. Most of the vessel shapes, in particular, had shown up on both islands during the flourishing Late Aegean Bronze Age, so their appearance in the Proto-White Painted and the Subminoan repertoires cannot be taken as proof of materials transfer or artistic communication. I.e., the possibility of a separate development rooted in the genuine tradition of either island cannot be ruled out.

The exclusion of the above mentioned examples is methodologically necessary, but there are other parallels between the two islands, which still have to be considered in light of the question of a system of exchange:

As Desborough already recognized, the pyxis with vertical handles, rising from the flat base, is one such example.[9] On Crete this special pyxis shape became popular from LM III C onwards[10] and is comparable with the few Proto-White Painted pyxides on Cyprus, as the examples from Salamis and Palaepaphos show.[11] A singular pyxis with a ring base from a III B stratum of Enkomi was classified by Dikaios as local LH III C and can be seen as a forerunner of the Cypriot type.[12]

Also, the painting on one Proto-White Painted example, showing a goat, a bird and a person carrying a kylix seems to have affinities to the closed style of LM III C pottery on Crete.[13] Pieridou particularly emphasizes the depiction of the goat, painted in silhouette, as a Subminoan inspiration for Cypriot animal depictions.[14] However, the LM III C ceramic from Crete shows a preference for the open style, nor could Yon find a suitable comparison in LM III B ceramic.[15] The earliest example on Crete is, in fact, a goat in silhouette on a Subminoan crater of the Giamalakis collection, and Iacovou showed clearly that this manner of representation had been known in Cyprus since the Late Bronze Age and that the Cretan goat in silhouette more likely traces back to Levantine or Cypriot prototypes.[16] On the one hand, then, we have a transfer of the shape and perhaps even some features of the painting from Crete to Cyprus, while it is assumed, on the other hand, that the goat in silhouette was passed from Cyprus or the Levant on to Crete.

Another parallel is the duck vase with a basket-handle, a simple spout instead of a head, and three small elevations like legs. This vessel form appears on both islands in the course of the Subminoan and LC III B periods.[17] Even if we accept that duck vases in general already existed on Cyprus, the variant considered in this study differs from its earlier counterparts.[18] The extreme similarities in shape and manner of the examples from both islands rule out the explanation of independent development in their respective local cultures. Therefore it must be assumed an influence between the two islands, no matter which direction– in opposition to Desborough and Pieridou, who support the idea of a one-way transfer of the duck vase from Crete to Cyprus without, however, presenting an argument for their opinion.[19]

The same results in the case of the amphoriskos with false spout. The shape is quite popular in the Proto-White Painted ware of Cyprus[20] and there are few Cretan examples.[21] The shape cannot be traced back earlier than to the Subminoan and LC III B periods. The apparent spout, which is closed and eventually became a rudimentary element, is such a characteristic feature of this vessel that its separate development on each island is hardly probable, though it must be admitted that the direction of influence cannot be determined.

The shape of the so called *kantharos* in the repertoire of Subminoan pottery is so similar to the exam-

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[8] Furumark 1944, 194–265; Desborough 1972, 112–133.
[9] Desborough 1972, 62.
[10] Seiradhaki 1960, 18, fig. 12; Sackett Popham 1965, 289; Popham 1967, 337–351, pl. 90b, c; Hall 1914, pl. 30; Kanta 1980, 282, 4–5.
[11] Yon 1971, 41, pl. 25, 79; Maier 1969, pl. III, 2; Karageorghis 1965, 73, 1–7 ; Pieridou 1973, 64.
[12] Dikaios 1969, pl. 82, 27.
[13] Desborough 1972, 62, 56, fig. A.
[14] Pieridou 1973, 64.
[15] Yon 1970, 311–17.
[16] Iacovou 1988, 81.
[17] Pieridou 1973, 68/69; Hall 1914, fig. 92, 1–2; Desborough 1972, 61.
[18] Lemos 1994, 229–37.
[19] Desborough 1972, 60–62, Pieridou 1973, 68–69.
[20] Pieridou 1973, 20, pl. 7, No. 4–8.
[21] Kanta 1980, 286.

ples in the Proto-White-Painted ceramic that it is also interpreted as evidence of a certain connection between the two islands.[22] It can be considered as a sub-group of the earlier amphoroid crater, which had been popular on both islands in the period preceding the Dark Ages. The Subminoan[23] and Proto-White Painted examples,[24] however, are smaller in size than their predecessors and have a biconical body, a broad conical foot, and the wide type of handles. The striking similarity in development of this shape, resulting in a smaller variant in both repertoires, is not explainable without assuming some kind of interregional communication.

Another parallel is the depiction of a triangle enclosing semicircles on the shoulder of a stirrup jar (FS 177). This characteristic decoration system is restricted to Cretan and Cypriot ceramic repertoires originating in the period under study,[25] so that some form of influence – regardless of the direction – is probable. Even if the decoration system plays a significant role in the Cretan context, Desborough's assumption of a Minoan origin is not necessary.

All these ceramic features appear on both islands in the course of the Dark Ages. Even if an analogous vessel form or a kind of decoration can be traced back to similarities in the respective repertoires of the Late Bronze Age, a characteristic item of the Dark Ages example enable to distinguish it from the forerunner. The idea of a one-way influence from one island to the other cannot be confirmed. The uncertainties in chronology are the greatest hindrance in determining the direction of transfer. There are a few exceptions, for which the direction of transfer seems clear, but they do not seem to be homogeneous. The example of the pyxis, for instance, suggests that a Cretan shape influenced the development of a Cypriot analogy, while the painting of a goat in silhouette suggests a transfer the other way around. Based on some aspects of the actual archaeological context, there seems to have been a situation of mutual influence.

The interpretation of an exclusive system of transfer between Crete and Cyprus in the Dark Ages seems too limited. At the very least, vessel shapes like the bottle-shaped vase, the kylix with swollen stem, and the belly-handled amphora and amphoriskos, which appeared on both islands in the course of the Dark Ages, have to be regarded in a wider context. The bottle-shaped vases from Karphi[26] and a Sub-Mycenaean example of the Kerameikos in Attica[27] stand opposite the broad spectrum of Cypriot Proto-White Painted vessels, whose prototype is assumed to be from the Levant.[28] In none of the areas is the shape documented earlier, so at least some form of exchange must have taken place among these regions during the period analysed in this study.

Also the shape of the kylix with the swollen stem,[29] as mentioned above, is represented in all three areas during this period and it can be even traced back to the LH III C ware.[30] The same applies for the belly-handled amphoras[31] and amphoriskoi.[32] Desboroughs interpretation of the Minoan belly-handled amphora as influenced by Cypriot ware[33] does not represent the whole spectrum of interpretations. The possible interpretation of an exchange is much wider, and the Greek mainland has to be considered in the context of such kind of exchange.

An interpretation based solely on imported goods does not allow the reconstruction of a system of exchange including both islands. An exchange of goods can be established to some degree for Cyprus and the Southern Levant, but there is no clear evidence for Cretan involvement.

But how can we account for the mutual assimilation of elements not able to be explained as independent developments out of similar Late Bronze Age ceramic traditions? Examples for the assimilation of stylistic elements in the Late Bronze Age are plenty, but in this period the high number of imports, which could have inspired them, explain the phenomena easily. It is not possible, however, to explain the sub-

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[22] Desborough 1972, 57.
[23] Seiradaki 1960, 1–37, fig. 14, 7, pl. 11b; Kanta 1980, 274.
[24] Pieridou 1973, 34, pl. 23, 4–9.
[25] Seiradaki 1960, 1–37, fig. 11, 1, 3.; Hall 1914, 150, fig. 89; Hood, Huxley, Sandars 1958/59, 194–262, fig. 27; Pieridou 1973, 67/68.
[26] Seiradaki 1960, pl. 11b.
[27] Kraiker, Kübler 1939, pl. 27
[28] Pieridou 1973, 69.
[29] Kling 1989, 143, fig. 9d; Pieridou 1973, 58; Kanta 1980, 264.
[30] Mountjoy 1986, 172, fig. 222.1.
[31] Mountjoy 1986, 202; Pieridou 1973, 61, pl. 19, 4–6; Hood, Huxley, Sanders, 1958–59, 241, Fig 27: VII 2
[32] Mountjoy 1986, 121, 124; fig. 150, Kanta 1980, 166, Pieridou 1973, 60, pl. 18, 6–11, Coldstream, Catling 1996, 303, fig. 73; Sackett, Popham 1965, 295, fig. 15: P 23; Seiradhaki 1960, 21, fig. 14, Boyd 1901, pl. II, 8; Hall 1914, 146, fig. 86d; Brock 1957, pl. 3, 3.
[33] Desborough 1973, 59.

sequent assimilations without assuming some kind of exchange, regardless of whether the artefacts showing these characteristics were imported or the exchange of stylistic parallels occurred otherwise.

But what kind of system of exchange can we assume for the Dark Ages? The palatial system of the Late Bronze Age disappeared or at least decreased, but this does not seem to have been the only level of exchange conducted. The texts of Ugarit hint that traders attained a certain independency from the redistributive palatial system in the course of the Late Bronze Age.[34] Furthermore, the different descriptions of traders in Ugarit are interpreted by Liverani as indications for the existence of official and private kinds of traders.[35] The private traders must have been free to regulate their administrative duties themselves and must have had enough economic freedom to carry on the transfer into the Dark Ages.

The decrease in palatial exchange also led to alterations in the character of the exchange. The palatial demand for luxury goods from far away places stopped, which resulted in the decrease of an important archaeological source of evidence.

Cyprus, in particular, contains the potential for an active participation in an interregional exchange during this period. It is true that many changes are obvious in the local material culture of Cyprus, but nonetheless it is also true that the settlement shift occurring at the end of the Late Bronze Age resulted furthermore in an orientation to the sea. Either this happened out of habit, in spite of any possible dangers, or the sea remained an indispensable factor for the maintenance of subsistence level.

In conclusion, any study into the range of a system of exchange involving Cyprus and Crete in the so called Dark Ages must widen the perspective to include the Eastern Mediterranean area and account for the development on Cyprus. That done, the stylistic and thematic parallels evident in the material cultures of both islands must be interpreted as scant archaeological remains of a much broader spectrum of exchange ranging throughout the Eastern Mediterranean. Unfortunately the types of exchange, the organizational structures, and the goods transferred, all of which triggered the sort of artistic and material assimilation described in this article, cannot be determined on the basis of the present state of art.

[34] RS 16.238: 1–11.

[35] Liverani 1962, 86.

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Lachish and the Date of the Philistine Settlement in Canaan

David Ussishkin

More than twenty years have passed since I first suggested (Ussishkin 1983:170; 1985:222–223), based on the data uncovered in the excavation of Tel Lachish, that the destruction date of Canaanite Lachish in the latter part of the 12th century BCE forms a *terminus post quem* for dating the settlement of the Philistines and the appearance of their characteristic pottery in the Coastal Plain. Several additional data in support of this conclusion have been added in the last twenty years, and it is time to discuss the issue afresh. The new information at our disposal includes the following six items:

1. Two scarabs from Lachish were identified as belonging to the reign of Ramesses IV. Hence Level VI could not have been destroyed before the reign of Ramesses IV.
2. A newly-found hieratic inscription apparently indicates that hieratic inscriptions were written in Lachish by a professional scribe.
3. The extensive study of the pottery from the renewed excavations at Lachish which has been carried out during the last decade as part of the preparations for the publication of the final excavation report (Ussishkin (ed.) 2004) did not reveal even a single Philistine sherd, either Monochrome of Bichrome.
4. The analysis of the finds from the excavations at Lachish carried out in the last decade indicated that Lachish maintained strong commercial connections with the Coastal Plain and the Mediterranean Coast during Level VI. Of particular interest are Cedar of Lebanon beams, *Glycymeris* shells, and fresh marine and Nile River fish imported to Lachish.
5. Systematic excavations have been started in Tel Zafit, the site of ancient Gath, situated 15 km. from Lachish. Both Monochrome and Bichrome Philistine pottery was discovered there.
6. The survey of the Lachish region and other surveys, and the analysis of the data, showed that all the settlements in the Lachish region were destroyed and abandoned at the end of the Late Bronze Age while the later Iron Age settlement pattern is radically different.

Below, I shall summarize all the presently available evidence, including the new data listed above, and then discuss in brief its implication for dating the Philistine settlement and the appearance of Philistine pottery in the Coastal Plain.

The Evidence from Lachish: Level VI and the Egyptian Hegemony

During the 13th and 12th centuries BCE Lachish enjoyed a period of great prosperity under the aegis of Egypt's hegemony in southern Canaan. The city-state of Lachish dominated the surrounding region, though its size and borders can only be surmised. According to Finkelstein's estimates (Finkelstein 1996; 2000:166–173) the territory of Lachish comprised 990 km.^{2} and included 25 sites which covered 30 hectares or more. Based on a coefficient of 200 persons per inhabited hectare, the territory of Lachish would have been home to some 6000 people. The city itself – which was not protected by a city-wall or any other fortifications – covered the entire area of the summit (ca. 20 acres) as well as the western and southern slopes down to the area at the base of the mound. It appears that Lachish was one of the largest and most prosperous Canaanite cities in the land during this period.

Based on the results of the renewed excavations, two strata have been discerned here: The earlier stratum, Level VII, included Fosse Temple III. This stratum was destroyed by fire, probably at the end of the 13th century BCE. The city was rebuilt in Level VI – but not the Fosse Temple which remained in ruins. The Level VI city – the last Canaanite city – came to a violent end when the settlement was totally destroyed and its population exiled and/or liquidated in ca. 1140–1130 BCE as detailed below.

The city of Level VI – which interests us here – prospered and developed under the Egyptian hegemony of southern Canaan during the time of the 20th Dynasty. The exact nature of Egypt's dominance, and the role it played in the government of the country have been variously interpreted (see Oren 1985a; Singer 1988; Higginbotham 2000). Undoubtedly, the city-state of Lachish flourished under the aegis of the Egyptian pharaoh, and some

scholars believe that it was ruled directly by the Egyptians (Singer 1988:5).

The finds from Lachish which help to elucidate the nature of the Egyptian hegemony include a number of inscriptions in hieratic script, mostly found in the constructional fills of the Judean palace-fort and apparently originating from the Level VI acropolis (see recent summary in Sweeney 2004). The most important inscription is written on a Level VI type bowl and dates to the reign of Ramesses III (Goldwasser 1982). These hieratic inscriptions, as shown by Goldwasser in conjunction with her study on the hieratic bowls from Tel Sera^{c} (Goldwasser 1984), constitute the documentation of the *šmw* (harvest tax) paid to an Egyptian religious institution probably associated with a local temple. According to Goldwasser, the recording of the harvest tax on votive bowls reflects the economic exploitation of southern Canaan by the Egyptian authorities via the religious establishment. This may mean that Canaanite Lachish, together with southern Canaan, was under Egyptian suzerainty at the time. One inscription identifies the writer as a scribe who might have had a non-Egyptian name (Goldwasser 1991), and Sweeney suggests that he may have been a Canaanite or a Syrian who received an Egyptian education and became a competent writer of hieratic. This scribe probably lived and worked at Lachish.

Another important find is the cast bronze plaque bearing a cartouche of Ramesses III found in the city-gate area in a Level VI context (Ussishkin 2004; Giveon, Sweeney and Lalkin 2004). Giveon has raised the possibility that it may have been part of the city-gate door.

Two anthropoid clay coffins were uncovered by the British expedition in Tomb 570 (Tufnell *et al.* 1958:131–132, 248–249, pls. 45–46). As generally agreed (Tufnell *et al.* 1958:131; Dothan 1982: 276–279; Yannai 2004) the pottery assemblage of Tomb 570 postdates Fosse Temple III and is contemporary with Level VI. One of the coffins bears a hieroglyphic inscription. According to M.A. Murray, A. Rowe and A. Gardiner, "the inscription is very curious, and … the writing is not that of an Egyptian scribe" (Tufnell *et al.* 1958:132). The clay of one coffin was analyzed by Neutron Activation Analysis, suggesting that the coffin was made in the Lachish area, though "final proof will demand a more thorough sampling from this area" (Perlman, Asaro and Dothan 1973:149–150). Thus, it seems that two officials probably of foreign origin and attached to the Egyptian administrative centre (see below) were buried in the coffins.

Finally, the Egyptian character of the Level VI temple, and probably that of the royal acropolis, should be emphasized. Strong Egyptian influence is seen in various structural, architectural and ornamental elements in the acropolis temple, as well as in many of the objects found within it. As the Level VI temple probably formed part of the royal acropolis, one might hypothesize that similar Egyptian influence might have been present in the royal palace complex as well. The above-discussed hieratic inscriptions may suggest the existence of an Egyptian administrative centre. (Here may have been the 'offices' of the above-mentioned scribe and the officials buried in the clay coffins.) Singer has suggested that an Egyptian "government residency" existed on the acropolis at the time (Singer 1988:5). A large brick-built silo in Egyptian style whose remains were found in the renewed excavations might have been part of the complex.

The period of Levels VII and VI is marked by trade and cultural connections with other countires as well as the cities in the Coastal Plain. These commercial connections are manifested at Lachish by pottery and other objects from Egypt, north-western Anatolia, Mycenae, Crete, Cyprus, Syria, Lebanon, north-western Arabia as well as the Coastal Plain. It was channelled primarily through the Canaanite ports along the Mediterranean coast, Ashkelon, situated ca. 30 km. from Lachish, foremost among them. We shall briefly discuss below only three aspects which portray the nature of the strong connections with the Coastal Plain during Level VI.

First, cedar of Lebanon beams were lavishly used as structural members in the Level VI acropolis temple, primarily as wooden beams spanning the roof of the main hall (Liphschitz 2004). This tree is found today in Turkey, Cyprus, Syria and Lebanon (Liphschitz and Biger 1991:167), and it may be assumed that the Lachish beams were imported from regions of Syria or Lebanon near the Mediterranean Coast. Cedar trunks may reach a height of nearly 40 m., but those brought to Lachish were probably much smaller. Nonetheless, it must have been a difficult task to transport these trunks to the Lebanese coast, to ship them by sea to the coast of Canaan, and then to haul them to inland Lachish.

Second, a great number of mollusc shells were brought from the Mediterranean beach (Bar-Yosef Mayer 2004). About 4180 shells of the common type *Glycymeris violacescens* were counted in Late Bronze loci, and about 720 of them were found in clear Level VI contexts. These shells were apparently used in construction of floors and in preparation of plaster.

Thirdly, a considerable number of fresh Mediterranean and Nile River fish were imported to Lachish and consumed by the inhabitants (Lernau and Golani 2004) during the Late Bronze period, and the discovery of fish bones in Level VI contexts indicates that this practice continued in Level VI. The marine fish included sea breams, drums, sea basses, sharks and rays and grey mullets. Of particular interest are the many bones of Nile perch from the Nile Delta imported via Ashkelon or other ports. Significantly, bones of larger Nile perch were found in the Level VI temple than were found in contemporary domestic contexts.

The Evidence from Lachish: The Destruction of Level VI

Level VI – the last Canaanite city – was razed to the ground in a violent, fiery destruction, traces of which were detected at every point in which remains of Level VI were uncovered. Significantly, no remains of battle were found, apart from a single bronze arrowhead. The destruction was complete, the population apparently liquidated or driven out, since Tel Lachish was abandoned following the catastrophe and remained desolate for a long period of time. As can be gathered from the regional survey (Dagan 2004) the Late Bronze Age settlements in the surrounding region were also destroyed and abandoned.

The date of the destruction can be fixed with much accuracy. A scarab of Ramesses III found in the constructional fills of the Iron Age palace-fort by the British expedition suggested to Tufnell that Canaanite Lachish did not come to an end before the reign of Ramesses III (Tufnell, Inge and Harding 1940:23; Tufnell 1953:51–52; Tufnell *et al.* 1958:37). Tufnell's suggestion was confirmed during the renewed excavations when a cast bronze object bearing a cartouche of Ramesses III was found, as related above, in a Level VI context in the city-gate area (Ussishkin 2004; Giveon, Sweeney and Lalkin 2004).

In 1994 Krauss discovered that a scarab uncovered in the British excavations bears the name of Ramesses IV (Krauss 1994), a reading seconded by Othmar Keel (*ibid.*:126). The scarab (Tufnell *et al.* 1958:126, pls. 39:380; 40:380) was uncovered in a locus defined as "part of a wall", with some late Late Bronze pottery and other scarabs, ca. 100 m. to the south of the mound (Tufnell 1953: pls. 108, 130; Tufnell *et al.* 1958:306). Recently, Lalkin identified another scarab uncovered in the British excavations as dating to the reign of Ramesses IV (Lalkin 2004). This scarab (Tufnell *et al.* 1958: pl. 39:372) was found near the south-western corner of the site, not far from the place where the previous scarab was uncovered. It was found near disturbed Tomb 570, which contained the above-mentioned anthropoid coffins as well as an assemblage of pottery assigned on typological grounds to the very end of Canaanite Lachish (Tufnell *et al.* 1958:67, 240–241, 248–250; Dothan 1982:276–279). It appears that the scarab in question was originally placed in this tomb and therefore it is contemporary with the coffins and the pottery assemblage.

In conclusion, we can safely assume that both scarabs stem from Level VI, especially in light of the fact that the site was abandoned for some two centuries or more following the destruction of Level VI. Ramesses IV reigned between 1151–1145 BCE (low chronology, after Wente and van Siclen 1976:218, Table I), a date which serves as a likely *terminus post quem* for the destruction of Level VI.

The evidence points to the devastation of Level VI by a strong and resolute enemy, but the silent archaeological evidence provided no direct clue as to the nature and identity of that enemy or to the immediate circumstances surrounding the city's downfall. Suggestions regarding these problems remain speculation at present. At the time I favoured the idea, first raised by Albright (Albright 1935:13–14), that Level VI was conquered and destroyed by the Israelite tribes as related in Joshua 10:31–32 (Ussishkin 1978:92). However, the suggestion that Level VI was destroyed by the Sea Peoples is clearly more attractive and has to be adopted. This idea was first considered by Tufnell (Tufnell, Inge and Harding 1940:24; Tufnell 1953:52). The razing of the Level VI city and the subsequent abandonment of the site could be compared to the fate of Ugarit and Alalakh, the final Late Bronze Age destructions of which are attributed to the invading Sea Peoples. The founding of large Philistine centres at nearby Gath and Ekron, the diffusion of Philistine pottery in the region and the complete change in the settlement pattern in Iron I (Finkelstein 1996: figs. 1–2; 2000:166–173; Dagan 2004) could then be easily explained. Thus, despite the absence of direct evidence, it seems probable that Lachish Level VI was destroyed and its population liquidated and/or exiled by the Sea Peoples who immigrated to and settled in the Coastal Plain.

Finally, it can be assumed that the Philistine conquest of the southern Coastal Plain was a prime factor in the collapse of Egyptian authority over southern Canaan (Finkelstein 1995), a loss of control that left Lachish, as well as other unfortified centres,

in particular Megiddo (Ussishkin 1995:258–265), completely vulnerable. It thus appears that the collapse of Egyptian hegemony occurred concurrenly with the destruction of Canaanite Lachish. The direct evidence from Lachish, as discussed above, indicates that Lachish still existed during the reign of Ramesses IV (1151–1145 BCE). Uehlinger has suggested, based on Egyptian finds from southern Canaan, that the Egyptians withdrew from the region during this period (Uehlinger 1988:20–25; also Na'aman 2000:4).

However, the statue base of Ramesses VI uncovered at Megiddo (Breasted in: Loud 1948:135–138), which should probably be assigned to Stratum VIIA (e.g., Mazar 1985:97; Singer 1988–1989:107; Ussishkin 1995:259–260), seems to indicate that Egyptian hegemony reached as far north as Megiddo as late as the reign of Ramesses VI (1141–1134 BCE; low chronology, after Wente and van Siclen 1976:218, table I). It is hard to believe that the Egyptians could have held Megiddo without concurrently holding southern Canaan, including the coastal highway leading to Megiddo. Hence, one may hypothesize that the end of Egyptian hegemony in Canaan and the destruction of Lachish Level VI occurred not before ca. 1130 BCE (Also Finkelstein 2000:161–162).

The Implications for the Date of the Philistine Settlement

No Philistine painted pottery – Monochrome pottery (labelled by T. Dothan and others "locally made Mycenaean IIIC:1b pottery") as well as Bichrome pottery – was found in Lachish in the large-scale excavations conducted by three successive expeditions, with the sole exception of a few sherds from the potter's workshop in Cave 4034 discovered by the British expedition: "These pieces belonged to jugs or flasks, but there were also two bowl fragments without a lime wash, painted in black and red" (Tufnell *et al.* 1958:293). The potter's workshop was in use during Level VI, but the presence of a number of red-slipped and burnished Iron Age sherds shows that the cave was not abandoned following the destruction of this level. Hence, the Philistine sherds could either have belonged to Level VI or, more likely, constitute a later intrusion, perhaps left in the cave, located on the slope of the mound not far from its main water source, by someone seeking shelter. The crucial fact is that for all practical purposes Philistine pottery is altogether missing at Tel Lachish.

This fact has far reaching implications. Tel Lachish is not too distant from the coastal region that was the distribution centre of Philistine pottery. Two major Philistine cities, Tel Zafit (Tell es-Safi), identified with ancient Gath, and Tel Miqne, identified with ancient Ekron, are situated 15 and 23 km., respectively, to the north of Lachish. Standing on the summit of Tel Lachish one can see clearly Tel Zafit, and the hillocks surrounding Tel Lachish can be seen from the summit of Tel Zafit. Large amounts of Monochrome and Bichrome Philistine pottery were uncovered at Tel Miqne, which was at that time a large Philistine settlement (e.g., Dothan 2000; Dothan and Zuckerman 2004). The recent excavations at Tel Zafit also uncovered Monochrome and Bichrome Philistine pottery (Maeir 2003:242). Bichrome pottery has been found at sites even further inland than Tel Lachish, e.g., at Tel Beth-Shemesh, Tel ʿEton and Tell Beit Mirsim.

Considering the geographical position, size, trade connections and prosperity of Lachish Level VI as detailed above, it is difficult to imagine that prosperous, central Philistine cities could have coexisted with nearby Lachish at a time when Philistine pottery was being diffused inland from the coastal region without an appreciable quantity of it reaching Lachish. Can it be imagined that supplies of fresh marine fish and large Nile River fish were regularly brought from the coast of Ashkelon to Lachish as related above but not a single Philistine vessel found its way there? The only logical conclusion is that the absence of this distinctive pottery in Level VI shows that Lachish was simply not settled at the time that Monochrome and Bichrome pottery was being produced. The evidence from Lachish is negative; however, it is so absolutely negative that it constitutes a sound argument for dating the appearance of Philistine Monochrome and Bichrome pottery anywhere in the country to after the destruction of Level VI in ca. 1130 BCE. In other words, the destruction date of Level VI serves as a *terminus post quem* for dating the appearance of Philistine pottery in the land of Canaan.

This chronological conclusion which was briefly published at the time of the excavation (Ussishkin 1983:170; 1985:222–223), has since come to form one of the cornerstones of Finkelstein's revised Iron Age chronology (Finkelstein 1995; 2000), and has gained the support of a number of other scholars (e.g., Oren 1985a; 1985b; Yannai 2004; Na'aman 2000:4).

Nonetheless, most scholars continue to hold that the city of Lachish Level VI could have coexisted with the great Philistine centres without some pottery diffusion taking place. T. Dothan has suggested

that the Philistines settled in the Coastal Plain in more than one wave, the first immigration wave formed by the users of the Monochrome pottery at the very beginning of the 12th century, and the second immigration wave formed by users of Bichrome pottery during the reign of Ramesses III (Dothan 2000:145, 156).

Most scholars, however, adopt the concept first proposed by Mazar and Singer in 1985 (Mazar 1985; 1997:157–159; Singer 1985; see also e.g., Stager 1995:334–336; Bunimovitz and Faust 2001; Maeir 2003). They believe that the process of settlement of the Philistines took place in two stages. During the initial stage, characterized by the appearance of Monochrome pottery and dated to the reign of Ramesses III, Philistine settlement and influence were limited to the Coastal Plain, to the region of the Pentapolis. Hence the diffusion of Monochrome pottery is limited to the region of the Coastal Plain. During the second stage, characterized by Bichrome pottery and dated to after the collapse of the Egyptian hegemony in southern Canaan, Philistine influence spread inland beyond the Coastal Plain. Hence Bichrome pottery is found in many places inland.

From this view it follows that the Monochrome pottery would have been contemporary with Lachish Level VI, while the Bichrome pottery would have been later in date. However, as discussed above, the negative evidence from Lachish is so absolutely negative that it constitutes a sound argument for dating the appearance of Philistine Monochrome as well as Bichrome pottery anywhere in the country to after the destruction of Level VI in ca. 1130 BCE.

Summary and Conclusions

In my view all the above data are conclusive, indicating how unlikely the commonly accepted concept of dating is. My argumentation can be summarized in three questions, asked in challenge of the "Mazar-Singer-Stager concept", defined by Bunimovitz and Faust as the "cultural segregation concept": First, how could the Egyptians have maintained their hegemony in Lachish and other parts of southern Canaan, including Megiddo and Beth-Shan, if they lost control of the Coastal Plain and the southern parts of the Via Maris which were invaded and occupied at that time by the Sea Peoples? Second, how is it possible that thriving cities such as Canaanite Lachish and Philistine Gath prospered at such geographical proximity to one another without a single piece of Philistine pottery imported to Lachish from Gath? Thirdly, how is it possible that extensive trade existed between Lachish and the Coastal Plain as well as the Mediterranean ports, including the importation of fresh marine fish, without even a single Philistine piece of pottery being imported to Lachish?

In summary, the dating of the Monochrome as well as the Bichrome Philistine pottery, and therefore the dating of the Philistine settlement in the Coastal Plain, to after ca. 1130 BCE, seems to be established on the basis of the evidence from Lachish. It appears that the settlement of the Philistines in the Coastal Plain followed the collapse of the Egyptian hegemony of the 20th Dynasty in southern Canaan, and the destruction of the Lachish Level VI Canaanite city which prospered under the aegis of Egypt's hegemony.

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Let's Do the Time Warp again: Migration Processes and the Absolute Chronology of the Philistine Settlement

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Abstract

This paper argues that establishing the absolute chronology of the Philistine settlement without first understanding the complexity of the social processes involved in it leads to simplification of historical events, which in turn blurs and distorts any chronological conclusion.

Anthropological studies, as well as historical case studies are presented as a demonstration of the complexity of migration and colonization processes, often involving several groups migrating in different rhythms, resulting in different foundation dates of the sites they build in the target country. Egyptian sources as well as archaeological evidence from sites outside and in Philistia are used to unveil the heterogenic nature of the Philistine settlement, and its possible chronological implications.

1. Forward: "42" and other irrelevant answers

In Douglas Adams' 1979 classic *The Hitchhiker's Guide to the Galaxy* some people faced a severe existentialist problem. In order to solve it, a huge computer was built, and it was asked to answer the question "of Life, the Universe, and Everything". It took the computer seven and a half million years to think about the question, and it finally gave the answer: "42". Do correct, yet incomprehensible answers to less than articulate questions occur only in science fiction books? How about a question such as: "What is the absolute date for the beginning of the settlement of the Philistines?"[1] Even if we reach a number, say after 1130 B.C. (as argued by Finkelstein 1998, 144–145; 2000, 173; Ussishkin 1998, 217) or at 1175 B.C. or slightly later (as argued by Bietak 1991, 1993, 295; Mazar 1997, 155–157, and Stager 1995, 335–336) or early in the 12th century B.C. (as argued by Dothan 1989, 8) what is the significance of this number?

In this paper, I shall argue that a major obstacle in understanding the chronology of the Philistine settlement is the view taken by most researchers, who see the Philistine settlement as a chronologically unified event, with a clear beginning and end, rather than a complex of migration processes, each with its own rhythm.

2. Temporal and cultural variability in the Aegean migration to the Levant

2.1. Migration as a complex process

Anthropological and sociological studies (e.g. Anthony 1990, 1997, 2000; Burmeister 2000) demonstrate that migration is not a sudden event of migrants pouring from their home countries to the new land. It is a prolonged process, in which information is gathered, sometimes using the services of scouts, guides or informants. Then, the initial migrants arrive, usually young men (Leonard 1992, 23), who prepare the ground for the arrival of the rest of the family or for a larger kinship group (Boyd 1989). These events may ignite different processes such as chain migration (Lee 1966, 55), in which groups follow each other, leapfrogging-migrants who move from one destination to the other (Anthony 1990, 902–903) as well as return migration (Anthony 1990, 904) of people that chose not to stay in their new country. Seeing migration as a complex process comprised of different phases, each with its own temporal aspects opens the questions of the exact definition of the beginning point of migration, crucial to the question of Philistine chronology. Would it be the time when the first scout reaches his destination, or the arrival of the first small pioneer group, or the settlement of large family groups of migrants, following the pio-

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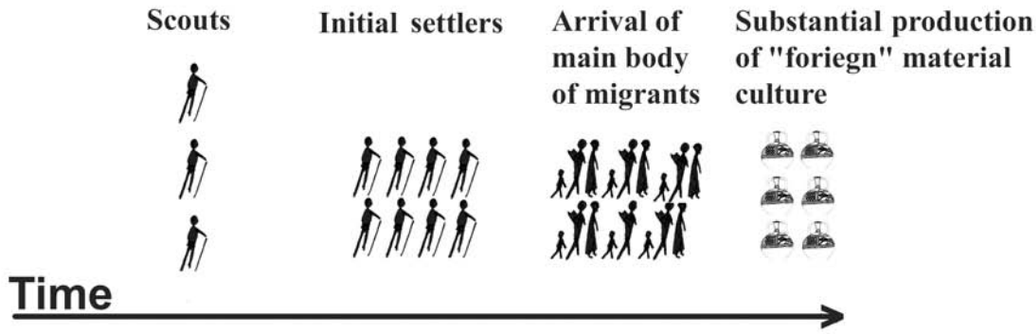


Fig. 1 Schematic pattern of migration for a single group

neer settlers? This question has serious bearing on our ability to identify the beginning of the Philistine migration: Are all these phases in migration processes archaeologically visible, or can we detect only the phase in which the settlers are already established and producing a conspicuous amount of their material culture? (Fig. 1).

Furthermore, it is implicitly assumed in the study of the Philistines that the three major excavated sites of Philistia, Ashdod, Ashkelon and Tel Miqne/Ekron, were simultaneously settled by the same groups of people. This is of course the easiest solution, but is hardly the rule. Frequently, groups of similar or different origins that settle in one geographical area, do not colonize simultaneously, but each settlement is founded on a different date. Can we, using only archaeological data, correctly solve the complex equation of migration processes that include more than one group, in more than one site? (Fig. 2).

Thus, for example, the early 17th century English settlement on the northeastern coast of America between New Haven and Portsmouth included eight settlements. The earliest in this group, Plymouth, was founded in 1620, and the latest, Newport, in 1639 (Gilbert 2003, 10; Fig. 3).

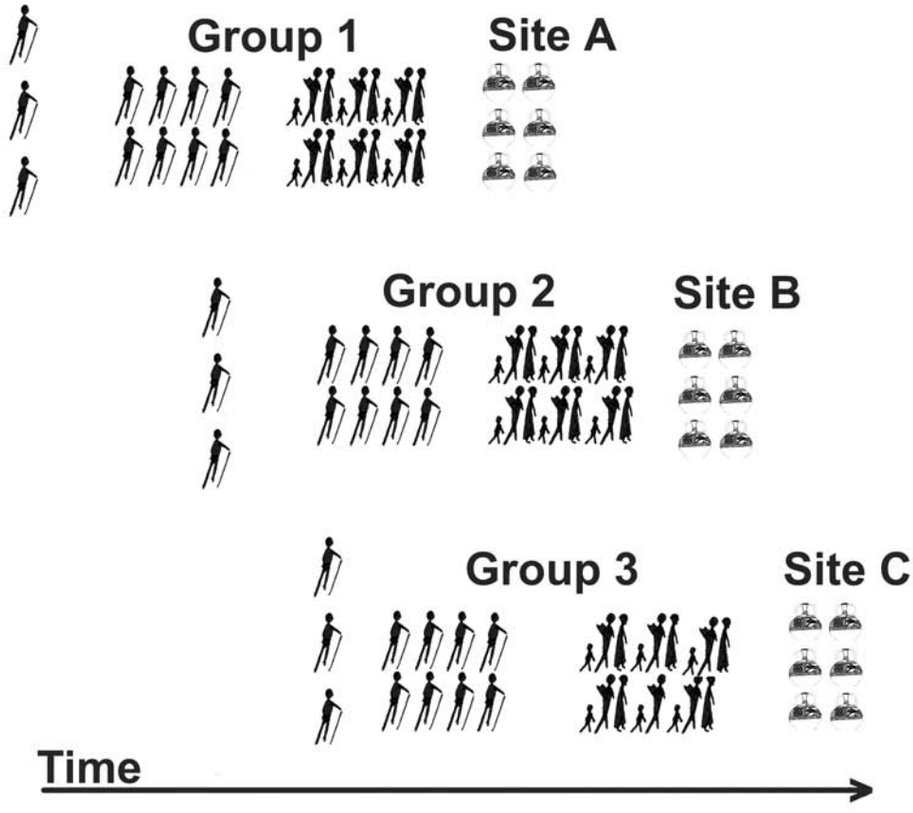


Fig. 2 Schematic pattern of several groups migrating into a single target country

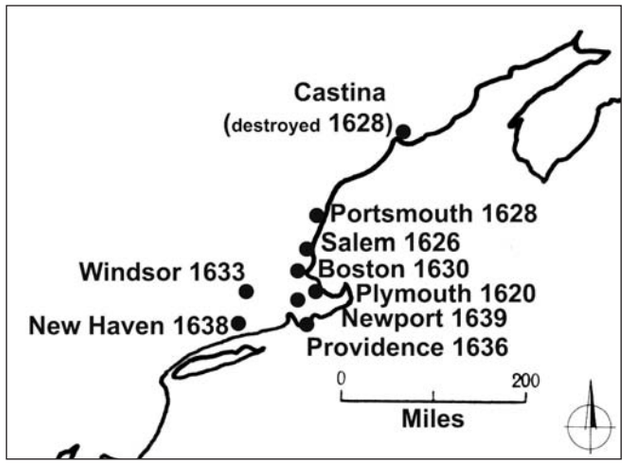


Fig. 3 British colonization in the Northeastern coast of America in the first half of the 17th century (data: Gilbert 1993, 10)

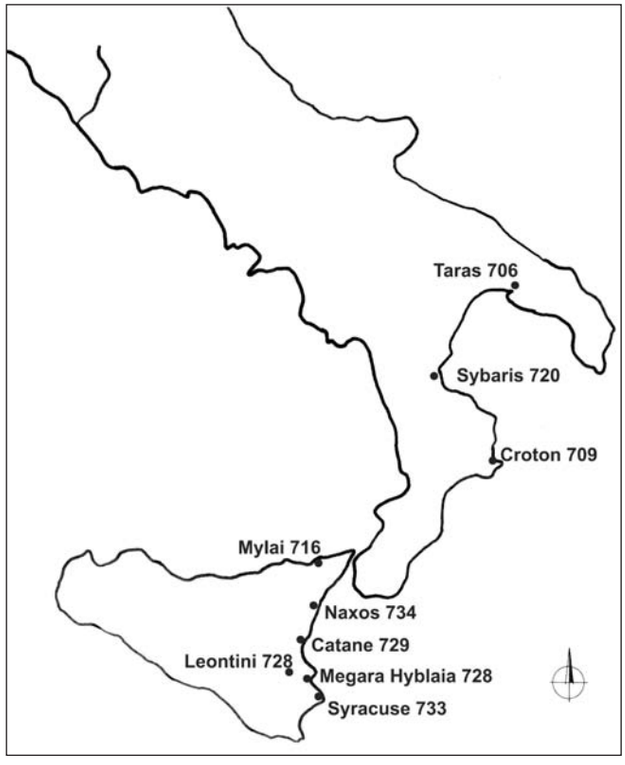


Fig. 4 Greek Colonization in South Italy and Sicily in the second half of the 8th century B.C. (data: Graham 1982, 160–162)

A most illuminating example of the problem of temporal variation in migration is the complex migration events known as the Greek colonization to southern Italy and Sicily (Fig. 4). We know from literary sources that each of the foundations at the last third of the 8th century B.C. was conducted by colonists originating at different city states, and that each settlement was established on a different date:[2] from Naxos, settled according to tradition by the Chalkidians in 734 (Graham 1982: 161; Malkin 1987, 175), to Taras, settled by Spartans in 706 B.C. (Graham 1982: 162). Would it be possible to determine such accurate data on the chronology and the origin of the migrants based on archaeological evidence? The answer is probably negative. Even in the cases when the literary date agrees with the date of the earliest Greek pottery at the site,[3] it may prove impossible to differentiate between pottery made in Greece in 734 B.C., the date for the foundation of Naxos, and that made in 716 B.C., the date of the foundation of Mylai. Furthermore, the correct identification of origin of the settlers within Greece according to pottery found at the earliest levels of the sites is also an incredibly unsafe affair. Would it be fair to assume that without the literary evidence, some scholars would interpret all foundations of the late 8th century as contemporary, mass migration, similar to that of the Philistines?

2.2. Evidence for complexity from Egyptian sources

The Medinet Habu inscriptions give us invaluable information concerning the activities of the "Sea Peoples" before their defeat in the supposedly decisive battles of year 8 of Ramses III. Its "historical background" displays a temporal depth that indicates that the arrival of the "Sea Peoples"at Egypt was not a single, sudden event of mass migration, but a prolonged process, much more reminiscent of the complexity of a migration stream with its different phases:

a. In the opening scene the "Sea Peoples" are at their home, making plans: "The foreign lands made conspiracy in their islands" (Peden 1994, 29; Redford 2000, 12). These islands are by all likelihood located in the Aegean Sea, the closest islands to

[2] Other foundations are, Syracuse, settled by Corinthians in 733 B.C., Catane in 729 B.C. by the Chalkidians, Leontini in 728 by the Chalkidians, Megara Hyblaia by the Megarians in 728, Sybaris in 720 by the Achaeans, Mylai in 716 by Zanclans, and Croton in 709 B.C. by Achaeans (Graham 1982, 160–162; Malkin 1987, 176, 179)

[3] Thus, for example, is the opposite case of Metapontum, with an Eusebian foundation date of 773/2 and an archaeological date of c. 650 or even later (Malkin 1987, 181; Carter 1993, 354–355)

Egypt. These were not Cyprus and Crete, since the latter is named elsewhere in earlier Egyptian records (Cline 1994, 34 fig. 7), and Cyprus is mentioned by the name Alashiya in the year 8 inscription.[4]

b. Then, they set on the route to the east: "No one could stand their arms, from Hatti, Kode, Carchemish, Arzawa, and Alashiya... " (Peden 1994, 29).

c. They stop and set camp in Amurru, just outside the scope of Egyptian political domination, before the encounter with the Egyptians. "An encampment was [estab]lished in one place in Amor and they desolated its people and its land as though they had never come into being" (Peden 1994, 29). It is suggested that during this stage, which could have lasted for several years, some of the migrants married local, Syrian women, and had children, all of which are shown in depictions of the land battle (Sweeney and Yasur-Landau 1999). The notion of a long stay in Amurru is supported by the proposed time gap of ten to fifteen years between the destruction date of Ugarit (1190–1185 B.C. Singer 1999, 729; Yon 1992, 119–120), probably contemporary with that of Amurru (Klengel 1992, 164–165), and the 8th year of Ramses III (ca. 1175 B.C.).[5]

Besides the clear temporal depth given by the "historical background", it is likely that several encounters with the "Sea Peoples" have been summarized into two decisive battles. Their description can be found in lines 51–59 in the inscription of year 5 (the first Libyan war), presumably connected to the events of year 8 (Peden 1994, 7): "The northern hill countries shook in their bodies, namely the *Peleset* and the *Tjek[er]*.... they were *thr*-soldiers[6] on land and another (unit) was on the sea" (Peden 1994, 17). Only the repulsion of the maritime invaders is recalled clearly in the inscription ("Those who came forward together on their sea, their seed is not". Cifola (1991, 54–55) offers a convincing explanation for the fact that there is no stated location or time given to the land or sea battles: The land battle was not a single event but rather a telescopization of a series of relatively small-scale hostile encounters between the "Sea Peoples" and Egyptian forces.

One should not rule out the possibility that the sea battle, too, was not a single event of hostile maritime activity (presumably in year 8) but the "total impression" of many similar interactions, most likely continuous maritime raids of the type that plagued Ugarit before its demise. Supportive evidence may come from the depiction of soldiers with feathered hats at the service of the Egyptians in the campaign of year 5 against the Libyans (Sandars 1978, 118 fig. 74). If this relief is not copied from earlier ones,[7] then, as argued already by Bietak (1993, 299), people wearing the feathered hats had already reached Egypt as prisoners and/or as mercenaries before year 5 of Ramses III. These could not have been prisoners of the Libyan campaigns of Merneptah, who were by that time too aged for battle.

How long did this process, so laconically described by Ramses III continued? If Ugarit and Hatti were both destroyed sometime between 1195 and 1185, it would not be inconceivable to speak of a migration stream, made of different groups of people coming from various origins, and lasting 10 to 20 years before 1175. It must be emphasized that the 1175 is only the date in which the Egyptians stopped the migrants at the Delta. But how long before this date had they been in Palestine, and how long after this date did they continue to arrive? The Egyptian sources have little to say, perhaps since Egyptian grip on Palestine severely deteriorated after Ramses III. The mention in Papyrus Harris "I slew the Denyen in their Islands"(Peden 1994, 215), if not an empty boast, may reflect an event later during the reign of Ramses III, indicating the continuation of the conflict. Much clearer evidence comes from the Onomasticon of Amenope (below), not meant for regal propaganda, which mentions the Philistines, the Sikel and the Sherden together with Gaza, Ashkelon and Ashdod, and thus gives a terminus post quem of ca. 1100 for the Aegean settlement at least in the coastal parts of Philistia. The entire duration of the events connect-

[4] Although the Egyptian term translated as "islands" may not have an absolute geographical accuracy (cf. Singer 1988, 239 note 4), Redford (2000, 12) notes that the Egyptians could and did distinguish linguistically between "maritime littoral" and "islands." According to Redford by the term "islands" the Egyptians referred to "Crete and the Aegean Archipelago." Cf. Brug 1985, 20, noting that the Egyptian term "Haunebut" (*ḫ3w-nbwt*, "islanders") was used to describe "Greeks" in Ptolemaic bilingual texts.

[5] According to von Beckerath (1997, 108, 190), Ramses III ascended the throne in 1183/1182, thus making his 8th year 1175. Kitchen (1987, 52), however, dates the beginning of the rule to 1184.

[6] Probably meaning "select or veteran soldiers" (Drews 1993, 171–152).

[7] Such as the Libyan campaign of Merneptah, where feathered-hat soldiers are shown fighting *against* the Egyptian army.

ed with the settlement of the Philistines is at least of several decades in length: between 10–20 years before 1175 for the very beginning of the movements of the first groups of migrant out of their homes, and any amount of time between the latter date and 1100 for the last groups to arrive at Philistia.

3. The Aegean/Philistine migration and the end of the Egyptian domination in Canaan

3.1. Chronology without a swift collapse

At the apparent absence of conclusive chronological data from Philistia itself,[8] most scholars made a clear connection between dates available from the Egyptian strongholds in Canaan to the process of settlement. All scholars maintained that the mass settlement of the Philistines could not have gone unnoticed by the Egyptians. As a result, two schools have formed: one, arguing that the Philistine settlement followed the end of Egyptian administration; the second arguing for a peaceful or tense coexistence at least for some time.

Would the connection between the migration of the Aegeans/ Philistines and the end of the Egyptian administration hold water if we gave up the pre-conception of mass, simultaneous migration, for more complex processes, involving different groups settling over an extended period of time? For those who favor the low chronology, the immediate uncomfortable outcome is that without a fast, well coordinated attack by the migrants, there is no possibility to reconstruct a swift end for the Egyptian administration, which can be used, via evidence in Egyptian centers in Canaan, for the absolute dating of the Philistine settlement. For those who argue for coexistence, or at least temporal overlap, between the beginning of the Philistine settlement and the end of Egyptian administration, the result is an inability to fix a clear date for the beginning of the Philistine settlement. However, as it will be demonstrated below, even if the reconstruction of a gradual, continuous migration is not accepted, it is possible to point out that the chronological evidence from the Egyptian centers in Palestine does not give any direct indication for the chronology of the Philistine settlement:

Beth Shean and Megiddo. Ussishkin argued, relating to the land movements of the "Sea Peoples", that "such hordes must have come from the north by using a cardinal land route, and therefore had to pass Megiddo and the Naḥal ᶜIron gorge before heading south (Ussishkin 1998, 216). If so, Megiddo could have held back the "Sea Peoples" to the days of Ramses VI, whose statue base, (found in a less than reliable context; Breasted 1948, 135 note 1; Weinstein 1992, 147), is presumed to date the end of stratum VII, and to give a *terminus post quem* for the Philistine incursion (Finkelstein 1996b; Ussishkin 1998; see however Mazar 1997; Yasur-Landau 2003). However, small and even large groups of migrants could also bypass Megiddo altogether by using two major alternative roads that avoid the center of the Jazreel Valley, and later cross the Carmel Ridge: a northern one ending by Yokneᶜam, and a southern one, ending by Jenin, and then going west through Emek Dothan and Naḥal Hedera (Dorsey 1991, 79 map 3; Ussishkin 1998, 215–216). A third possible route, less convenient for travel, yet entirely unguarded, was along the coast.

Beth Shean, although strategically located, is not a necessary pass for travelers coming into Palestine from the north, and therefore its role in holding back land movements of the "Sea Peoples" is minimal. Consequently, the vast body of datable *Egyptiaca* and accompanying LHIIIC (or rather Cypriot-made "Mycenaean IIIC:1b") pottery (James 1966, fig. 94: 4; Hankey 1966; Hankey 1967, 127–128; Warren and Hankey 1989, 164–165; Mazar 1993, 216; Leonard 1994, 72 no. 979; Yasur-Landau 2003) are of no direct chronological value for the settlement of the Philistines.

Lachish. The lack of locally made, "monochrome" LHIIIC pottery in Lachish, which yielded good Egyptian datable material is another cornerstone in the chronology of the beginning of the Philistine settlement presented by Ussishkin (1998, 217) and Finkelstein (1995, 230–231). The lack of such pottery is shown as *positive* evidence that the Philistine settlement did not occur in nearby Philistia before the reign of Ramses III or even Ramses IV, whose scarab (Krauss 1994) was found in the lower city of Lachish (and indeed not before Ramses VI, according to the Megiddo evidence). However, not every

[8] Important moves in the chronology of Philistia have been conducted by Dothan (1989) and Bietak (1993) with comparative stratigraphic charts which included also Egyptian finds. However, it may be argued by those who favor the lower chronology that all 20th-dynasty scarabs found in Philistia in contexts containing Aegean-style "monochrome" and "bichrome" pottery originated in fact in earlier strata.

absence of imported pottery has a chronological implication:

a. Both Lachish and Beth Shean have evidence for both Ramses III and Ramses IV names. The absence of *imported* LHIIIC pottery from Lachish (Leonard 1994, 207), which is found in Beth Shean, does not mean, of course, that Lachish was destroyed earlier than Beth Shean. This absence is only connected to the fact that LHIIIC imported pottery is found only as south as the northern valleys, probably due to trade routes. Thus, imported LHIIIC pottery cannot give a direct date for the beginning of the Aegean settlement in Philistia. Trade routes are not the only explanation for the absence of imported pottery types. Imported LHIIIC is found in Tel Keisan and in Beth Shean, yet not in Megiddo (Yasur-Landau 2003), which is on the route between them. Still, Finkelstein (1996b) sets the date of the destruction of Beth Shean stratum VI according to the statue base of Ramses VI found at Megiddo.

b. Bunimovitz and Faust (2001) argued that the small Aegean-style serving vessels and cooking wares, produced in Philistia, often of unattractive appearance and poor firing, were of no interest to the Egyptian garrisons who maintained their own foodways and cultural preferences. Their argument may be a very convincing theory for two additional reasons:

– Locally made ("monochrome") LHIIIC pottery was also not found at Beth Shemesh, less than half the distance between Tel Miqne/Ekron and Lachish. Since there seems to be a continuation of habitation at the site from the Late Bronze Age to the early Iron Age, the reason for the absence is not temporal in nature.

– Throughout the 13th century B.C., Canaanite taste for Aegean imported ware was mainly for containers such as stirrup jars and piriform jars, which contained perfumed oils, rather than to Aegean serving wares (Leonard 1981, 94–96; Steel 2002, 31; Killebrew 1998b, 160). After the fall of the Mycenaean palatial system the demand for Aegean-style *containers* continued in Egyptian garrisons, as manifested in the find of imported LHIIIC stirrup jars in Beth Shean, and the local imitations of stirrup jars in cemetery 900 at Tel el-Farᶜah (Braunstein 1998, 262).

3.2 Evidence for direct contacts between Egyptians and Aegean/Philistine migrants

It is much beyond the scope of this paper to reconstruct the entire range of interactions between the Egyptians and the Philistines/Aegeans, not to mention its chronological framework. It should be, however, noted that there is evidence for complex and continuous contacts before, during, and after the settlement in Philistia, casting heavy doubts on any reconstruction that suggest the Philistines as the cause for the end of Egyptian administration in Canaan.

– The first recorded meeting between the Egyptians and people identical or similar to the Philistines are the "feathered hat" mercenaries seen in the Libyan campaign of Merneptah (Drews 1993, 54; Cifola 1994, 3–4). These, or others involved in piracy and mercenary activities may have been the informants for the migrants that followed.

– There is *direct* evidence for the presence of Aegean migrants in Egyptian garrisons in Canaan. The "feathered hat" people are depicted on scarabs from Beth Shean (Rowe 1940, pl. XXXIX:10; James 1966, fig. 117:4; pl. 6.2:1) and Tel Farᶜa (s) (Macdonald, Starkey and Harding 1932, pl. LV:252; Noort 1994, 140–141; Keel and Uehlinger 1998, 112; Pl: 6.2:3). The depiction of what appears to be Philistine (or other "Sea Peoples") on an Egyptian object and in Egyptian posture, as well as the possible depiction of "feathered hat" on at least one of the sarcophagi in Beth Shean (Rowe 1930: 39, pls. 37–40; Wright 1959; Dothan 1982, 274 note #21), demonstrate a prolonged acculturation process of such people during their time in the Egyptian service, possibly well into the 12th century B.C.

– Contemporary with the presence of these mercenaries, started the process of movements and settlement, accompanied by clashes with the Egyptians, starting perhaps immediately after the destruction of Ugarit, certainly after the 5th or 8th year of Ramses III (above), and continuing perhaps to the end of the 12th century, when the picture given by the Onomasticon of Ameope (below) gives a picture of a Sea Peoples settled along almost the entire coast of Canaan.

– Even during the late 12th century and the 11th century the contact with Egypt continued, and evidence for Egyptian material culture in southern Palestine had led Weinstein (1998, 191) to argue that "…Philistia evidently became both a market for Egyptian trade goods and an intermediary through which Egypt maintained trade relations with other areas of Palestine". The Egyptians were well aware of the situation in Canaan after the end of their administration, as reflected in the Onomasticon of Amenope (Gardiner 1947: 190–191; Dothan 1982, 3–4; Singer 1993, 296; Stager 1995, 336). Among the traits of post-empire Egyptian influence was the adoption of an Egyptian form, the jug, into the "Philistine bichrome" repertoire (Dothan 1982, 172–185), as well as the incorporation of the Egyptian

lotus motif into the same repertoire. In addition, Egyptian pottery and other objects appear in sites in Philistia in 11th-century contexts (Mazar 1985, 56; Weinstein 1998, 19–192), indicating that the end of the Egyptian Empire in the Levant did not stop the commercial and possibly other interactions between Egypt and Philistia.

The array of interaction between Egyptians and Aegean/Philistines was probably never limited to the hostilities reflected by the Merneptah's and Ramses III's documents. Rather, it is very plausible that at any given time between the very end of the 13th to the beginning of the 11th century B.C. Aegean/Philistines were involved in piracy and served as mercenaries against the Egyptians, serving in the Egyptian garrisons in Canaan and elsewhere, settling in smaller and larger numbers in southern Palestine, as well as involved in trade with the Egyptians. Egyptian objects could have reached Philistia and other parts of Canaan regardless of the state of Egyptian domination in Canaan (with the sole exception of objects directly connected with Egyptian administration, as hieratic inscriptions). Thus, for example, it is impossible to discredit the chronological value of 20th-dynasty scarabs found within settlement contexts yielding evidence for the settlement of the Philistines, such as the Ashdod Ramses III scarab (Dothan 1989, 7–8; Bietak 1991, 43; 1993, 294 fig. 2; Brandl 1993, 138–139; yet not mentioned in Finkelstein 1995, 1998, 2000) or the recently identified Ramses IV scarab of the same site (Ben Shlomo 2003, 89–90). Furthermore, the different mechanisms of contact could well account for any 20th-dynasty object which has reached other parts of Palestine, without any connection to the end of Egyptian administration, such as the all important Ramses VI statue base from Megiddo.

4. Variability in the archaeological evidence from Philistia

Does the archaeological picture from Philistia reflect a simultaneous, violent conquest by a large mass of migrants in a short time? Dothan (1989) and Stager (1995), supporting a script very close to that written by Ramses III, used destruction levels in Ashdod, Ashkelon and Tel Miqne/Ekron to argue for a swift and violent conquest. But does the archaeological evidence from Philistia necessarily point to such reconstruction?

a. The circumstances of settlement are not identical in all the sites, and evidence for a violent conquest are inconclusive, to say the least. The Canaanite town of Tel Miqne Ekron was completely burnt (Dothan 1998, 150–151), yet evidence from field I (Killebrew 1998a, 381–382) may indicate that the Canaanite settlement of stratum VIII continued for some time after the destruction of stratum IX, *before* the appearance of Aegean material culture in stratum VII. Traces of fire do not appear in all the areas excavated in Ashdod, The only evidence for a violent destruction is a thick ash layer reported from a rather small area inside Area A (Dothan 1971, 25; 1993a, 96). Most importantly, it is not reported from area G, the location of the 13th century B.C. "Governor Residence" (Dothan and Porath 1993, 47). In Ashkelon the migrants may have found an almost empty site: The burnt layer found by Pythian-Adams (Pythian-Adams 1921; Finkelstein 1996a, 107) can also be attributed to the destruction by Merneptah. In addition, the number of migrants settling the sites varied much: While the entire 20 ha. of Tel Miqne Ekron was settled from the very beginning of the settlement process, much smaller areas have been settled in Ashdod and Ashkelon: In Ashkelon, Stager argues that the site in the 12th century consisted of 50–60 ha. (Stager 1995, 342). This argument for a radical change in the area occupied, with similarity to Tel Miqne/Ekron, has not yet been proved by showing any 12th-century occupation outside the area of the Late Bronze Age strata on the smaller mound.[9] It may well be that the size of the earliest Aegean settlement was not much larger than its Canaanite predecessor, suggested by Finkelstein (1996a) to be a smaller "E" size settlement (5.1–10 ha.). In Ashdod, the city spread outside the acropolis only in the 11th century (Dothan 1993a, 98),[10] being of similar size in the 12th century as it was at the end of the Late Bronze Age, also assigned by Finkelstein (1996a, 239) to his category "E".

b. The settlements differ in aspects of material

[9] Even if the fortification, dated to the beginning of Iron II (Stager 1993, 342), will be proved to be of a "late 12th century" date, as suggested earlier by Stager (*ibid.*, 107), the entire area within the huge circumference of walls may not have been densely inhabited.

[10] Although evidence for stratum XIII and XII pottery was found in a pit in area C1. This pit contain also later material which gives the date for their period of use (Dothan 1971, figs. 101–102).

culture. These differences, although minute, may have much significance, since they have survived powerful assimilation mechanisms, which exist within every migrant society:[11]

– Pottery iconography (Yasur-Landau 2003): The pictorial style LHIIIC pottery from Tel Miqne/Ekron and Ashdod contains fish and bird images.[12] Human figures, which were not found in Ashdod and Tel Miqne/Ekron, do appear in the pictorial repertoire of Ashkelon both in the "monochrome" and the "bichrome" style (Stager 1998, fig. on p. 157, p. 164 fig. a; Wachsmann 2000, 134 fig. 6.29).

– The settlements differ in aspects of ritual activity: Wide shouldered, long necked "Ashdoda"-style figurines are quite common in Ashdod (Dothan 1982, 234–249; Yasur-Landau 2001), and also found in Ashkelon, yet they are absent from Tel Miqne/Ekron, which however produced other types of figurines, more similar to the "mourner" types (Dothan 1998, 155; Dothan and Gitin 1993, 1053, Dothan 1995, 48, 50, fig. 3.12; Gunneweg, Dothan, Perlman and Gitin 1986: 5, fig. 1: 14). On the other hand, incised cow scapulae were found in Tel Miqne/Ekron (Dothan and Gitin 1993, 1053; Hesse 1986, 25 fig. 4), but not yet reported from Ashdod and Ashkelon.

– Additional, less secure differentiation exists in the absence of seals or sealings from all sites but Ashdod (Dothan 1982: 41, pl. 6; Dothan and Dothan 1992: 167, pl. 11; Dothan and Porath 1993: 81, fig. 36:9; Keel 1994: 22–23; 1997: 672–673 no. 27; pl. 8.4: 1, *ibid*.: 82, fig. 38:2).

Recent advances in the relative and absolute chronology of LHIIIC pottery (e.g. D'Agata 2003; Deger-Jalkotzy 2003; Gauß 2003; Iakovidis 2003; Rutter 2002; forthcoming; Yasur-Landau 2003; French in this volume; Mountjoy in this volume) give reason to optimism that once the material from Philistia will be fully accessible, it will provide additional important chronological data. With a final publication of the stratigraphic and ceramic sequences in all the excavated area at Tel Miqne/Ekron, Ashdod, and Ashkelon, it will be possible to establish the temporal relations between the appearances of earliest Aegean-style pottery in these sites, i.e. their relative "foundation dates".

However, even the preliminary observations stated above demonstrate not only a variability in the circumstances of settlement, but perhaps more importantly, a regional differentiation between the sites in Philistia in pictorial iconography and ritual costumes, which are closely connected to notions of ideology and self identity. This variability brings forth the possibility that we witness here a model of colonization similar to that seen in South Italy and Sicily: not a single event of settlement, but rather separate processes of settlement for each site. Ashdod, Ashkelon, and Tel Miqne/Ekron were settled in different circumstances, by Aegean migrants perhaps arriving from close, but different places of origin.

5. Conclusions

In this paper I have tried to demonstrate that the road for constructing a definite absolute chronology for the settlement of the Philistines may be much longer and complex than previously assumed:

1. It was suggested that the desire to reach a clear-cut date for the beginning of the Philistine settlement may stand in contrast to the actual complex nature of migration processes as known from anthropological and historical case studies. Accordingly, virtually every piece of evidence we possess, from Egyptian records to inter-site variability in material culture remains from Philistia, argues against a simplistic historical reconstruction of mass, swift invasion and settlement that would allow a single date, either "high" or "low" for the beginning of the Philistine settlement. Rather, the Philistine settlement was likely to be a series of migration processes occurring throughout most of the 12th century B.C.

2. The abandonment of the "mass invasion/migration" reconstruction pulls the plug on any attempt to connect chronologically between the demise of Egyptian domination in the northern valleys and Shephela and the beginning of Philistine settlement

[11] One such mechanism is the "principle of first effective settlement", in which new arrivals will try to assimilate to the first founders, which have already adapted to the social, economic, and environmental conditions in the new land, and have managed to consolidate their community (Anthony 2000; Cheek 1998: 153).

[12] e.g. Ashdod: Dothan and Porath 1993, fig. 15:11, fig. 16:19; fig. 17:10; other examples of pictorial pottery: Stager 1995, fig. 3:30, 31.Tel Miqne/Ekron stratum VII (phase 9a, yet note that there are none from phases 9c and 9b: Killebrew 1998a, fig. 7:15 and probably 13. Other examples of pictorial pottery: Stager 1995, fig. 3:22, 23. Dothan 2000, 154 fig. 7.7: 8.

in the southern coastal plain. Without hordes from the north, ravaging Megiddo and Beth Shean in a single sweep, it is impossible to use these sites' Egyptian objects in any relation with the Philistines. The contrary may be correct. Imported LHIIIC pottery from Beth Shean may be contemporary, or even later, than the beginning of the Philistine settlement in Philistia (Yasur-Landau 2003). Similarly, commercial and cultural reasons, rather than chronological, may account for the absence of LHIIIC pottery, either locally made or imported from Lachish.

3. An often neglected aspect with direct relation to chronology is the fact that migration is only one within the plethora of interactions between Egyptians and the Philistines and other "Sea Peoples" from the late 13th century B.C. to the early 11th. Other interactions, some occurring simultaneously with migration processes, included maritime raids on Egypt and the Levantine coast, "Sea Peoples" mercenaries fighting in the service as well as against the Egyptians, and various trade enterprises. Those left without doubt much evidence in the excavation record, in the form of Egyptian imports (as scarabs), evidence for Aegeans in Egyptian strongholds, and evidence for Egyptian influence on Philistine material culture. All this evidence has no direct connection with the date of the Philistine migration and settlement, and is therefore the source of a strong chronological "background noise", which blurs the more faint chronological clues (such as local LHIIIC pottery styles) from the sites in Philistia.

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Dating the Destruction of Canaanite Hazor *without* Mycenaean Pottery?

Sharon Zuckerman[*]

Introduction

Hazor of the Late Bronze Age is considered one of the most powerful and flourishing Canaanite kingdoms (Na^{c}aman 1997; Finkelstein 1996). The history of the site begins in the Early Bronze Age, but it gains its prominent position within the framework of Canaanite city-states only in the second millennium BC, from the Middle Bronze Age and well into the Amarna period (Yadin 1972, Maier 2000). The end of this mighty kingdom which was designated by the biblical writer as "the head of all those kingdoms" (Joshua 11: 10), is one of the major events in the history of Canaan.

Hazor, the largest Canaanite city-state in the southern Levant, occupies the upper tel and its lower city. The site was partially excavated already by Garstang in 1928. Several seasons of excavations by a team from the Hebrew University led by Yigael Yadin revealed a large and prosperous city, (Yadin 1972). Fortifications, temples, public and domestic buildings and a wealth of small finds and works of art were published in the final reports (Yadin *et al.* 1958, 1960, 1961, 1989). Early relations of the city with the Aegean world, probably through Syrian intermediaries, are indicated by the Kamares Ware sherd found in a Late Middle Bronze Age context in the Lower City (Dothan, Zuckerman and Goren 2000).

The renewed Hebrew University excavations at Hazor, led by Prof. Amnon Ben-Tor, revealed a wealth of new material from the final phase of the Late Bronze Age city (Ben-Tor 1998, Ben-Tor and Rubiato 1999). A large ceremonial complex was discovered on the acropolis of the Tel (the "Canaanite Palace" – Area A). Another monumental building which is tentatively interpreted as the administrative royal palace of Hazor (Zuckerman forthcoming) was uncovered on the northern slope of the Tel facing the Lower City (the "Podium Complex" – Area M) (Fig. 1). The end of both these Late Bronze Age monumental buildings is marked by a huge conflagration that brought an abrupt and violent end to the glory of Hazor.

This event has been a major issue in the research of the site, and the debate over its dating and interpretation can be traced back to the first excavations in the lower city directed by Garstang (Garstang 1931, Ben-Tor 1998 for a recent summary). Following Yadin's excavation seasons at Hazor in the 1950s, and the accumulation and publication of the rich finds of the period, the problem became a cornerstone in the archaeological research of Canaan (Yadin *et al.* 1960, 159–60, Yadin 1972, 46, 108). The main dating criteria used by both Garstang and Yadin were the existence (or absence) of Mycenaean pottery at the Late Bronze Age destruction level on the one hand, and the interpretation of the biblical narrative concerning the destruction of Hazor by Joshua on the other hand. In the following, I will assess our current knowledge of Mycenaean pottery at Hazor, and discuss its limited contribution to the debate over the dating and interpretation of Canaanite Hazor's destruction.

Published Mycenaean Pottery from Yadin's excavations

Mycenaean finds from Hazor, notably absent from Garstang's first excavations at the site, were naturally not included by Stubbings in his pioneering study of Mycenaean pottery in the Levant (Stubbings 1951). This absence was rectified by Hankey, who included the finds published in the three volumes of Yadin's excavations at Hazor in her study (Hankey 1967, 123). In her later reappraisal of the material, Hankey mentions less than 50 vessels found in domestic and cultic contexts at Hazor, ranging from LH IIA/LH IIB, through LH IIIA2 and ending in LH IIIB (Hankey 1993, 106). In the most comprehensive corpus of the Mycenaean pottery in the Levant, Leonard mentions 53 complete vessels or sherds from Hazor (Leonard 1994, 205–6). Of the 53 Mycenaean sherds discussed by Leonard,

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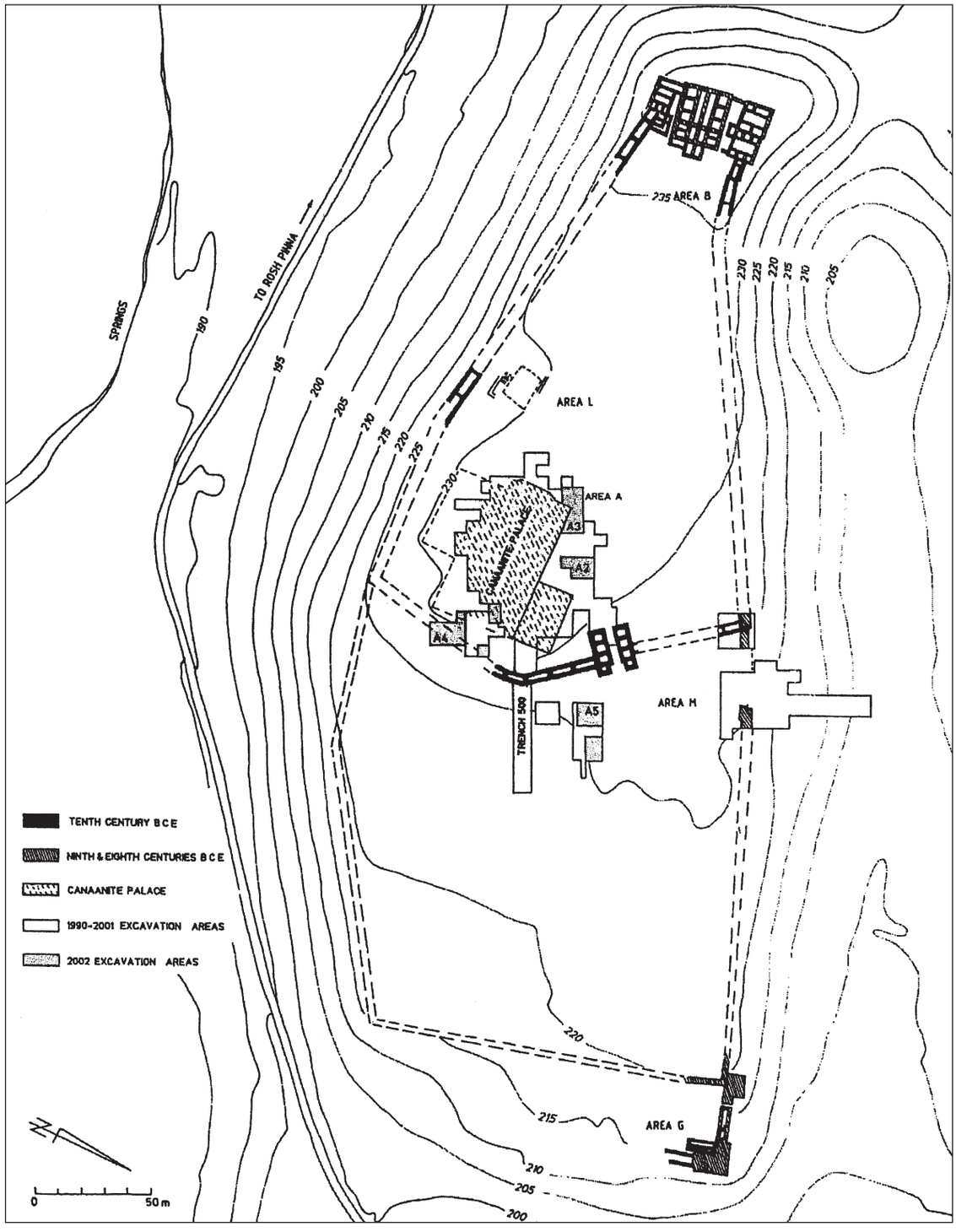


Fig. 1 A Plan of the Renewed Excavations at Hazor: Areas A and M

two are attributed to the LH IIB, 7 to the LH IIIA2, 84 are defined as LH IIIA–B and 10 to the LH IIIB (see now also Wijngaarden 2002,76–77, catalogue III). The latest sherd is attributed by Leonard, following Hankey, to the LH IIIB2 (but see below). A handle of a LH IIIB flask or stirrup jar was found in area P, which was published after Leonard's corpus appeared (Mazar 1997, 377, fig. V.4:4). Five more sherds were added to the corpus by Wijngaarden (2002). The small bull figurine from str. 1A in area H

(the "orthostats temple") was recently published as belonging to the LH IIIB in the final report (Yadin *et al.* 1989, 271). The figurine was previously dated to the LH IIIA–B (Leonard 1994, 139, no. 2236). A recent assessment of this date is suggested by Elizabeth French, who prefers to lower the date of this figurine to the last third of the 13th century BC (i.e. LH IIIB2) (French, per. Comm. July 2003).[1] This assessment should, however, be regarded as tentative until personal inspection of the figurine is possible.

The majority of the Mycenaean pottery of Hazor thus belongs to the LH IIIA2, LH IIIA–B1, and was found in all excavation areas, mostly in contexts of stratum 1B (=XIV). Sherds defined as LH IIIB were mostly found in area F. Three of these vessels belong to Tomb 8144, and probably represent the latest phase of use of the tomb (Yadin *et al.* 1960, pl. CXXXVII: 3, 9, 10). All the others were found in domestic buildings around the area, and are attributed to str. 1B or the rebuilding phase of str. 1A (*ibid.*, pl. CXLVIII: 2–7). Other LH IIIB sherds were found in phase B of the gate in area P (contemporary to str. 1B), and in the last Late Bronze Age phase of the domestic buildings on the acropolis (area BA).

One Mycenaean sherd is worth special mention. It is a very small sherd found in a cistern in area D, in connection with a burial (Yadin *et al.* 1958, pl. CXXXI: 12). The sherd was identified by Hankey and later published by Leonard, as part of a bowl FS 284 group B, belonging to the LH IIIB2 (Leonard 1994, 119, no.1772).[2] The sherd is very small, and its published drawing renders its correct identification almost impossible. The fact that acceptance of this date will make it the latest Mycenaean sherd found at Hazor to-date necessitates a very cautious approach to its dating. This is emphasised by the fact that the sherd was not found in the upper phase of the cistern but in an earlier stratum (Yadin *et al.* 1958, 138–40).

In conclusion, most of the Mycenaean sherds that were found at Hazor and attributed to strata 1B, can be safely attributed to the LH IIIA2. This period witnesses the increase in international trade in the eastern Mediterranean and marks the floruit of Mycenaean imports to Syria-Palestine (Hankey 1993). Hazor is playing a secondary role in the active system of inter and intra-regional trade in this period, and probably relied on the services of intermediaries such as the gateway sites of Tell-Abu-Hawam, Akko and Tel-Dan (Hankey 1967, Gunneweg and Michel 1999, Ben-Dov 2002). Also quite common at Hazor are Mycenaean vessels of the early LH IIIB1, dated to the end of the 14th and the beginning of the 13th centuries BC (Wiener 1998, and see below).

During the end of stratum 1B at Hazor vessels of LH IIIB1 appear at the site in small quantities, probably for the limited use of the inhabitants of the cultic precinct (?) of area F. These are the latest Mycenaean sherds that can be safely attributed to Late Bronze Age Hazor.

Mycenaean pottery from the renewed excavations (Fig. 2)

More than 150 Mycenaean sherds were reported in both excavation areas at the upper tel of Hazor during the renewed excavations. Most of these can be attributed to the final phase of the Late Bronze Age, and thus present an opportunity for a fresh discussion of the Hazor Mycenaean finds, both old and new, in the context of the last days of the Canaanite kingdom of Hazor.

The relative amount of Mycenaean sherds in the renewed excavations is very small, and they form less than 1% of the total assemblage of pottery in the destruction layer. Most of these sherds were found in later Iron Age fills and in disturbed contexts. Of the sherds found in-situ, area M is relatively rich in finds and about 60 sherds were found in the different parts of the "Podium Complex". In area A, the "Canaanite Palace" itself is almost devoid of Mycenaean sherds, and only a handfull of sherds were found inside the structure (Fig. 2:3, 18–19). Most of the sherds in area A were found scattered around the building.

A selection of the Mycenaean sherds from the renewed excavations is presented in Fig. 2. Most of the sherds belong to closed vessels, and include stirrup jars (Fig. 2:19–21), piriform jars (Fig. 2:12–16), pyxis (Fig. 2:18) and a flask (Fig. 2: 17). Also represented are shallow cups (Fig. 2:1–2), goblet (Fig 2:3) and kraters (Fig. 2:4–7).

Most of the sherds are well-levigated and highly burnished and show high technical level of workmanship. The forms and motifs are typical of the LH IIIA2, and continue to appear in the LH IIIB1. The distinction between these two families is very

[1] I would like to thank Dr. Elizabeth French for her comments on this important figurine.

[2] Leonard based his identification on earlier impressions of V. Hankey (A.L.Leonard, per.comm). It is not clear if Hankey relied on the published drawing or was shown the actual sherd by Yadin.

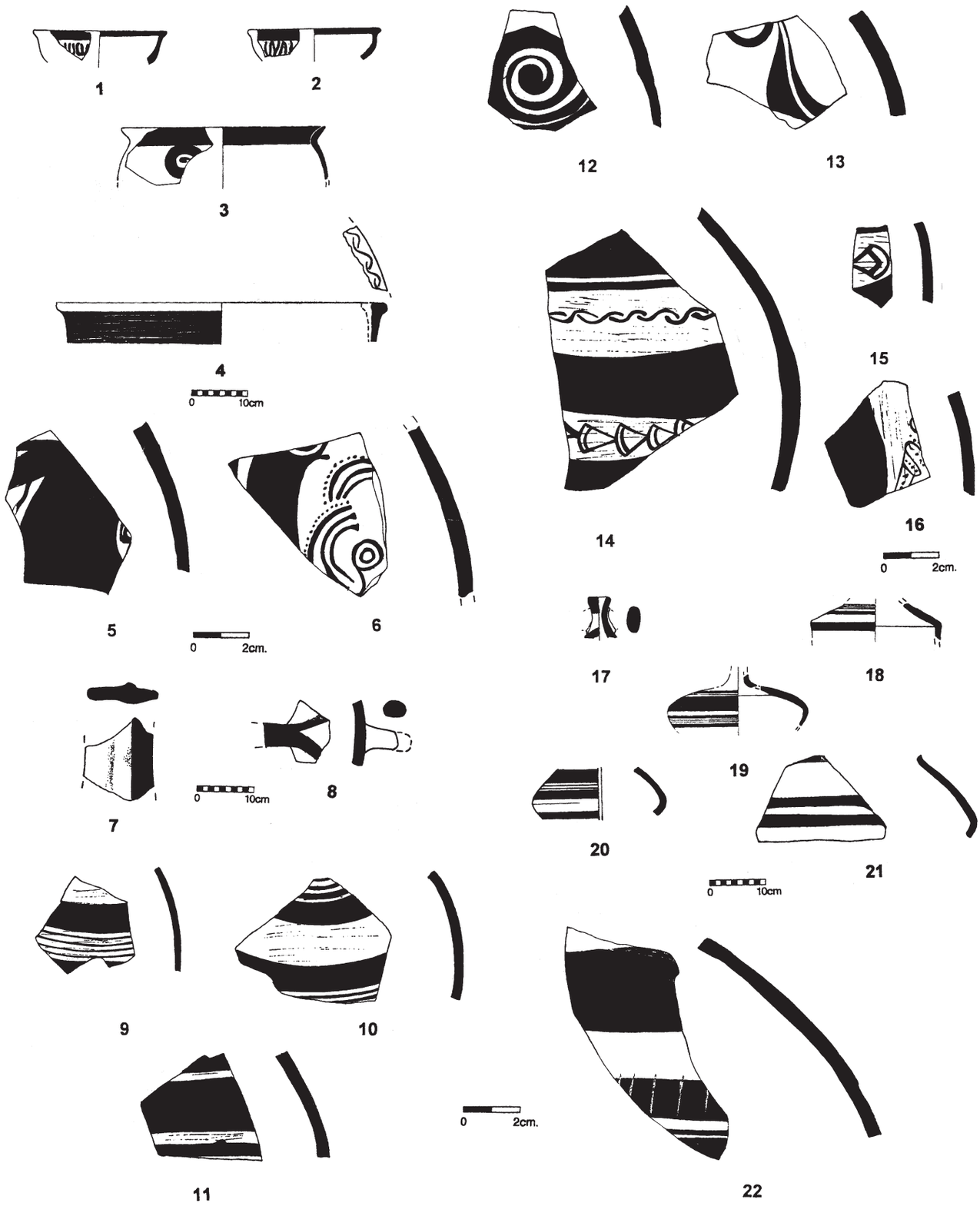


Fig. 2 Selected Mycenaean Sherds from the renewed Excavations

difficult, especially when dealing with sherds and not with complete vessels (Mountjoy 1986, 93–5). No sherd that can be safely attributed to the LH IIIB2 was identified in the renewed excavations.[3]

Discussion

Mycenaean pottery was initially the only archaeological means (together with biblical and historical considerations) for the dating of Hazor LB strata in general, and the final destruction of the city in particular. Garstang considered its absence in his excavations of the lower city as evidence for dating the Late Bronze Age destruction *before* 1400 BC (Garstang 1931, 198). Yadin dated the final destruction of Hazor, on the basis of LH IIIB sherds found in Str. 1a (=XIII) contexts, to the second third of the 13th century around 1230 BC (Yadin 1972, 46, 108–9). This dating was based on Furumark's chronological scheme of the LH IIIB (1300–1230 BC) (Furumark 1941, 113–5). The wider chronological range of LH IIIB (1320/00–1185/80 BC) accepted today necessitates reappraisal of this date and its implications (Warren and Hankey 1989, 154–162, Wiener 1998 and see below). The contribution of Mycenaean pottery to the question of dating Hazor's destruction should be evaluated in the light of better knowledge of the chronological and spatial distribution available to-date.

Chronological distribution of Mycenaean pottery of Hazor

For the purpose of dating the Late Bronze Age levels at Hazor, the important chronological peg is not the end of the appearance of LH IIIB pottery in the Levant, as suggested by Yadin (Yadin 1972, 46, 108–9). This event is safely dated to the first half of the 12th century BC, based on the last appearance of LH IIIB vessels in the temple of Deir ᶜAlla together with a Cartouche of Queen Tausert (Mountjoy 1986, 8 table I, Warren and Hankey 1989, 152–162, Franken 1992, 30, fig. 3–9: 5). There is no reason to assume that the latest Mycenaean vessels imported to Hazor are contemporary with the last imports of such pottery to the Levant. The assessment of the latest date possible for the Hazor imports should incorporate other lines of evidence in their context (see below).

Of more relevance to the dating of Hazor is the transition between the Mycenaean pottery of LH IIIA2 (characteristic of str. 1B=XIV) and LH IIIB1 (appearing in the last days of str. 1B and continuing into str. 1A). According to Warren and Hankey, the end of LH IIIA2 in the Levant should be dated to the end of the Amarna period, in the days of Tut-Ankh-Amun (1340–1330 BC). This dating is based on the discovery of LH IIIB1 in tombs from the end of the XVIIIth dynasty (Hankey and Aston 1995), and the appearance of such vessels in the temple of Kamid el-Loz (Warren and Hankey 1989, 151–154; Jung and Weninger 2004, 218–219).

The temple of Kamid el-Loz was recently published, so the latter argument can be tested (Metzger and Barthel 1993). Of the Mycenaean vessels attributed by Warren and Hankey (1989, 153) to the temple which was allegedly destroyed in the days of Akhenaten (i.e. temple T2 in its various phases), three actually belong to the later temple (i.e. T1) which is dated to the 13th century BC. Not surprisingly, most of the vessels defined as LH IIIB actually belong to the later temple (Metzger and Barthel 1993, Tafn. 103: 8, 118: 2–3, 128: 4 and 141: 4, 9, 11). Only one rhyton (designated as LH IIIB Early) can be attributed to the last phase of the earlier temple T2a (Metzger and Barthel 1993, Tafn. 130–131). Recently, Elizabeth French identified only **one** rhyton from temple T1 as LH IIIB, while all the other vessels are to be dated to LH IIIA2 (quoted by Wiener 1998, 311). These observations weaken the arguments for the early appearance of LH IIIB in Kamid el-Loz and in Syria. Wiener's conclusion is that the last appearance of LH IIIA2 and the first appearance of LH IIIB imports should be placed at 1320/1300 BC, with some preference for the later date based to the analysis of the Ulu-Burun Shipwreck (Pulak 1997, 250, Wiener 1998, 314–5). The earliest possible date for LH IIIB1 in Levantine contexts is thus the last two decades of the 14th century and the beginning of the 13th century BC. This date fits well with the dating of the end of str. 1B at Hazor, represented by the rich assemblage of Tomb 8144–8145 in the Lower City (Yadin *et al.* 1960, pls. CXXVIII–CXXXVIII).

How long into the 13th century does the import of LH IIIB vessels to Hazor continues is a different matter. It should be evaluated against the short duration of str. 1A, where most of these sherds were

[3] I wish to thank Dr. Marta Guzowska for her comments on the Hazor Mycenaean sherds, and R Jung read and commented on a draft of the article. All mistakes and ommissions are the authors responsibility.

found, their small quantity and their limited spatial and functional distribution at Hazor.

Functional and spatial distribution of Mycenaean pottery of Hazor

Most of the Mycenaean vessels found at Hazor are closed shapes, with only a few open vessels and several zoomorphic and anthropomorphic figurines (Leonard 1994; Wijngaarden 2002). This type of assemblage is typical to inland Levantine sites, and is different from that of the major coastal emporia, such as Ugarit and Tell Abu-Hawam, and Cypriot centers such as Enkomi and Kition (Leonard 1981). Based on her study of these finds, Hankey suggested a possible link between Hazor and the port at Tel Abu-Hawam (Hankey 1967, 123–125) but such a claim is yet to be tested by other means, such as chemical analyses (cf. Gunneweg and Michel 1999).

The spatial distribution of the Mycenaean pottery at Hazor shows clear preference to burial contexts over public and domestic contexts. A majority of Mycenaean vessels found in the lower city of Hazor were found in burial contexts in areas F (13 complete or almost complete vessels in tomb 8144 alone) and D. A detailed contextual study of the published Mycenaean pottery of Hazor can be found now in Wijngaarden (2002). His interpretation of the spatial distribution of Mycenaean sherds and vessels emphasizes the existence of such sherds in all excavation areas at Hazor, although their dominance in public and especially funerary contexts is admitted (Wijngaarden 2002, 94–97). Wijngaarden explains the restricted access to Mycenaean pottery in the tombs as reflecting consumption strategies of urban social groups, not necessarily belonging to the elite (*ibid*. 270–273). However, the scarcity of tombs of the LB II inside the city of Hazor, together with the architectural elaboration of tomb 8144–8145 and the rich repertoire found in it, are a hint to the high (possibly elite) status of those buried in it. The rest are small sherds found scattered throughout all the excavation areas. Noteworthy is the scarcity of Mycenaean sherds in cultic contexts, such as the temples in areas H (One Zoomorphic figurine and two sherds) and C (small sherds in the vicinity of the temple and a figurine on the surface outside the temple) (Yadin *et al.* 1958, 109, Yadin *et al.* 1960, 80, Yadin *et al.* 1989, 271). Similar distribution pattern was observed at Megiddo, where Mycenaean vessels and figurines were found mainly in tombs and in public contexts, and are absent from cultic contexts (Leonard and Cline 1998, 15–16). This type of limited distribution stands in sharp contrast to the situation in coastal sites such as Tell Abu-Hawam and Ugarit, where Mycenaean vessels were found throughout all excavation areas in a variety of contexts (Monchambert 1983, Yon *et al.* 2000, 6–10, 68–69). Concentration of imported vessels in graves and their absence in contemporary settlement contexts is a known phenomenon in Levantine and Cypriot sites throughout the 14th and 13th centuries BC (Cadogan 1993, Steel 2002, Ben-Dov 2002). A relatively large number of Mycenaean vessels, usually consisting of closed vessels such as stirrup jars, piriform jars, pixides and flasks, is characteristic of rich high-status burials. Well-known examples are the "Governor's Tomb" at Tel el-ᶜAjjul (Steel 2002, 39–44), The "Persian Garden" at Akko tombs (Ben-Arieh and Edelstein 1977), the "Mycenaean Tomb" at Tel Dan (Ben-Dov 2002) and Tomb 8144–8145 at Hazor (Yadin *et al.* 1960, pl. CXXXVII: 1–13).

This phenomenon has far-reaching implications for understanding the process of enhancing social hierarchy in Canaanite city-states, and the accumulation of wealth and economic control in the hands of élite groups (Steel 1998). The goods stored in these vessels, mainly wine, perfumed oils and unguents, were used in the Aegean area in contexts of commensal meals and feasts and incorporated within rituals of competitive consumption (Hamilakis 1996, 1999). Their appearance in local contexts in Cyprus and the Levant might hint to the emulation of such habits by the local élites, as part of their participation in the *koine* of the eastern Mediterranean in the Late Bronze Age (Sherratt and Sherratt 1991, Cline 1994, Steel 1998, Steel 2002, 42–43). In any case, the consumption of Mycenaean pottery at Hazor, a city on the edge of the maritime trade system during the Bronze Age, is uncommon and limited mainly to certain contexts of elite burials, a situation similar to that noted by Steel for Tell el-ᶜAjjul (Steel 2002, 44–46). The historical context within which such a phenomenon can be explained is the 14th century BC, and more specifically the Amarna period. This is the time of major building activities initiated at Hazor by its powerful kings, such as Abdi-Tirshi mentioned in the Amarna correspondence (Moran 1992, 288–290, 362). Most of the Mycenaean vessels probably arrived to Hazor during that period, for funerary and ceremonial use by the élite groups. In the end of the 14th and the beginning of the 13th century BC, only a small number of vessels were consumed by the same, now weakening élite groups of the deteriorating city. The exact length of time of this last phase of trade and consumption cannot be assessed with certainty, but can be esti-

mated by its meagre remains to have been of a relatively short duration.

Conclusions

Our current knowledge of Mycenaean pottery at Hazor is constantly expanding, and the renewed excavations at the site enable a fresh look at different aspects of the distribution of this imported ware at the site and its vicinity.

However, Mycenaean pottery by itself can no longer be regarded as the sole chronological tool for dating the Late Bronze Age levels of Hazor. See the recent discussion of the methodological problems inherent in the use of Mycenaean pottery for dating Near Eastern Contexts (Yasur-Landau 2004). An answer to the thorny question of dating Hazor's destruction should rely on a variety of means, such as Egyptian written sources and objects, cuneiform tablets, ^{14}C datings and, above all, the rich assemblage of local pottery found in the destruction level. Such a comprehensive discussion is beyond the scope of this paper and will be presented elsewhere. Suffice it to say that the Mycenaean pottery discussed above, taken in conjunction with other lines of inquiry, seems to support an early date for the destruction of Hazor in the first (rather than the second) half of the 13th century BC (Zuckerman 2003).

A project of NAA analyses of Mycenaean sherds from Hazor and related sites in northern Israel is planned by the author for the near future. Recent studies of Mycenaean pottery, found at sites such as Tel-Dan, Akko and Tell Abu-Hawam in Israel and Tell el-Amarna and Qantir-Piramesse in Egypt, point to an intricate international trade and intra-regional distribution system of these desirable items (Hoffmann and Robinson 1993; Gunneweg *et al.* 1992, 1999; Hankey 1997; Mountjoy and Mommsen 2001). The on-going efforts to establish the existence of pottery production centers on mainland Greece will no doubt open new horizons for the reconstruction and understanding of these trade patterns (Mommsen *et al.* 2001).

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