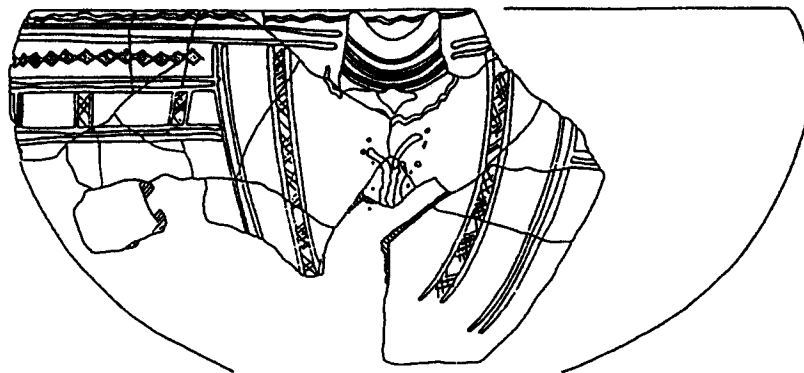


THE WHITE SLIP WARE OF LATE BRONZE AGE CYPRUS



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by

VASSOS KARAGEORGHIS

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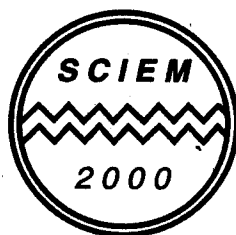


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THE WHITE SLIP I OF TELL EL-DAB^a AND THERA: CRITICAL CHALLENGE FOR THE AEGEAN LONG CHRONOLOGY¹

Malcolm H. Wiener

In recent years the Aegean Long Chronology, which places the eruption of Thera during a mature stage of LM IA at 1628 BC, has gained many adherents. Advocated by BETANCOURT (1987), NIEMEIER (1990) and MANNING (1995), the Long Chronology has been accepted as a matter of fact in review articles in the *American Journal of Archaeology* (DAVIS 1992, 736–37; *idem* 1995, 733 [“dendrochronology and reinterpretation of precision radiocarbon dates have now virtually resolved the dispute...”]); RUTTER 1993, 756; REHAK and YOUNGER 1998, 97–100); in an article entitled “Revising Bronze Age Chronology” in the magazine *Archaeology* that refers to “the 1628 BC eruption of Thera” (ROSE 1995, 20), and recently in a review of the current state of Aegean archaeology in the *Journal of Archaeological Research* (BENNET and GALATY 1997, 83–84). Others, including in particular P. WARREN (e.g., WARREN and HANKEY 1989; WARREN 1998), have steadfastly upheld the Aegean Short Chronology which would place the eruption of Thera between 1530 and 1500 BC.

Advocates of the Long Chronology rely principally on two factors: radiocarbon dates and the indications (and absence of indications) in tree rings of climate-forcing volcanic eruptions. As to the former, Long Chronology adherents contend that radiocarbon dates indicate early dates for the beginning of the Aegean Late Bronze Age and the Theran eruption (BETANCOURT 1998). The radiocarbon evidence, however, remains inconclusive for a number of reasons, particularly the notorious ‘back-bending’ or ‘oscillating’ calibration curve for the period *ca.* 1670–1535 BC, during which the amount of ¹⁴C trees

absorbed from the atmosphere was greater than the amount of decay resulting from the half-life of the ¹⁴C atom (MANNING 1995). Unusual sunspot activity has been suggested as a possible major cause for this phenomenon, along with varying rates of release of ¹⁴C by oceanic, atmospheric and stratospheric reservoirs. The oceans contain about forty times the amount of radiocarbon as the atmosphere (WIENER forthcoming b).

The second main factor cited in support of the Aegean Long Chronology is evidence of a major climate-forcing event affecting tree-ring formation in 1628 BC, with the eruption of Thera suggested as the likely event in question. The 1628 BC event is reflected in trees in higher elevations in California and in trees in northern Europe. A splendid data set of sixty-two logs from a postern gate at the Hittite fortress at Porsuk above the Cilician Gates, 840 km. to the east of Thera, shows average growth of 240% of normal in 1628 BC, with individual logs showing growth of as much as 740% of normal (KUNIHOLM *et al.* 1996 and pers. comm.).² There is, however, no direct evidence to link the 1628 BC tree-ring event with the eruption of Thera rather than another volcano or some non-volcanic climatic disturbance.

The most cogent argument for the Aegean Long Chronology lies in the fact that there is no indication in the tree-ring sequences of long-lived trees – the bristlecone pines of southern California, foxtail pines of the California-Nevada border, or oaks of Ireland, England or Germany – of a severe climatic event between 1550 and 1450 BC, the dates within which the massive Theran eruption that buried LBA

¹ I am deeply indebted to Manfred Bietak and Irmgard Hein, Kathryn Eriksson, Robert Merrillees and Eliezer Oren for making available to me their contributions to this conference and allowing me to incorporate their research and conclusions with my own; to James Allen, Susan Allen and Daphna Ben-Tor for permitting me to see their review prior to publication; to James Allen, Susan Allen, Dorothea Arnold, Celia Bergoffen, Manfred Bietak, Mary Jaye Bruce, Kathryn Eriksson, Vassos Karageorghis, Peter Kuniholm, Sturt Manning, Eliezer Oren, Jeremy Rutter, Peter Warren

and James Weinstein for much beneficial commentary and advice; to Hector Catling for an extensive commentary distilling four decades of experience with the material and the sites; to Mervyn Popham for a detailed and most helpful line-by-line critique; and to Jayne Warner and Bragan Thomas for editorial assistance.

² Studies of the direction of the fall of Theran pumice from the Bronze Age eruption show a strong dispersion to the east (NINKOVITCH and HEEZEN 1965; STANLEY and SHENG 1986; SULLIVAN 1988; KELLER *et al.* 1990; GUICHARD *et al.* 1993).

Akrotiri would fall on the Aegean Short Chronology. Work is ongoing as to why a massive eruption might not be recorded in a distant tree-ring record. Possible reasons include: (1) an eruption of low sulfur content; (2) an eruption outside the spring to early fall growth season for trees and hence less likely to impact tree growth significantly, and whose ejecta are dissipated or offset by other factors prior to the following growth season; and (3) an eruption whose effect on the trees in question is diminished or offset by conditions of cloud cover, wind velocity and direction, temperature and/or soil moisture (WIENER forthcoming b).³

Unfortunately we do not as yet have a sufficiently comprehensive and sensitive nearby Aegean or Anatolian dendrochronological sequence for the years 1550 to 1450 BC. The Porsuk sequence noted *supra* effectively ends in 1551 BC, the year in which all but five of the sixty-two trees were felled. None of the five exceptions, the latest felled in 1527 BC, shows any indication of a major climate event, but the small size of the sample, diminishing from five to one, inhibits any inference being drawn. Eight logs from Gordion show no major climate-induced response during the years 1550–1450 BC, but these logs show no great response in 1628 BC either, perhaps because at the time of that volcanic eruption (whatever the source) the trees from Gordion had already received adequate moisture, in which case massive rains would not necessarily trigger a growth spurt, or because of high local variability in weather effects of volcanic eruptions.⁴

The proposed Aegean Long Chronology is severely challenged by links between Egyptian historical chronology and Minoan/Cypriote interconnections with Egypt. The last fifteen years have seen a dramatic change with respect to New Kingdom chronology, for we are no longer dependent as before on disputed astronomical calculations. Largely as a result

of the work of K. KITCHEN and also M. BIERBRIER on the Third Intermediate Period, there now exists a continuous sequence of textual sources allowing New Kingdom dates in all likelihood to be fixed within about a dozen years (BIERBRIER 1975; KITCHEN 1987, 1991a, 1991b, 1992, 1996a and 1996b; WIENER 1998a). The most likely date now for the accession of Tuthmosis III, in whose reign interconnections with the Aegean and Cyprus become marked, is Kitchen's preferred Egyptian Middle Chronology date of 1479 BC, although 1490, the next available lunar date, remains a possibility (KITCHEN 1987, 40–41).

The chronology of the preceding Second Intermediate Period in Egypt is not securely based on a succession of interlocking texts and inscriptions, but rather rests in the first instance on the Turin Canon, a papyrus which on the verso of a Ramesside tax record of about 1200 BC lists Hyksos rulers whose reigns span 108 years.⁵ While it is possible that the compiler of the Turin Canon underestimated the interval somewhat, or that he wished, perhaps subconsciously, to minimize the period of foreign rule, and that the scarab of a Hyksos ruler called Shenshek found at Tell el-Dabca between strata D/2 and D/3 is not that of an earlier Hyksos ruler previously known by a somewhat similar name (BIETAK 1989a, 96) or a minor ruler not a part of the official list, but rather the scarab of a previously unknown ruler of some duration to be added to the list, any major chronological extension of the Hyksos period is limited by a well-supported 12th Dynasty astronomical date (LUFT 1986, 1989 and 1992; KRAUSS 1981 and 1985) and generally by Near Eastern interconnections independently dated by the Babylonian/Assyrian Chronology (GASCHE, ARMSTRONG, COLE and GURZADYAN 1998), supported by recent dendrochronological evidence which favours the Low, or at the most a Lower/Middle, Babylonian Chronology (KUNIHOLM *et al.* 1996, 782).

³ Many trees exhibit little response to climatic events. The more marginal the environment, the greater the effect of a major weather event on tree growth; e.g., trees in semi-arid environments (such as the Porsuk trees) will grow dramatically as a result of a superabundance of rain in the growth season, whereas trees above the dew line (such as the California bristlecone pines) or in cold climates (such as the Irish oaks) will experience significantly less growth as a result of increased coldness. I am most grateful to P. KUNIHOLM for discussing these matters with me.

⁴ POST 1977, 1–26; P. KUNIHOLM, pers. comm., for which I am most grateful. The chronological implications drawn, however, are solely those of the author.

⁵ The Heliopolitan priest Manetho, writing in the 3rd century BC, gives a much longer period, including, for example, a reign of Apophis, the penultimate Hyksos ruler, of sixty-one years rather than the forty years listed in the Turin papyrus, which is generally accepted as the preferable account (VON BECKERATH 1964; RYHOLT 1997; ALLEN, ALLEN and BENTOR 1999). For the problematic history/chronology of the Second Intermediate Period in general, see in addition SAVV-SÖDERBERGH 1951; HELCK 1956; *idem* 1971, 89–106; VAN SETERS 1966; BIETAK 1980, 1984, 1991b, 1994, 1996 and 1997; Redford 1970; *idem* 1992, 82–122; *idem* 1997; KEMPINSKI 1985, 1990, 1992 and 1997; WARD in TUFNELL 1984, II, pt. 1, 162–79; O'CONNOR 1997; OREN 1997; SCHNEIDER 1998.

Given the New Kingdom dates, the Aegean Long Chronology faces serious obstacles on the Minoan front. In order to fill the period between the proposed 1628 BC Long Chronology date for the Theran eruption and the *ca.* 1479 BC accession of Tuthmosis III, in whose reign Minoan objects similar in appearance to those found in LM IB destruction deposits on Crete are depicted on Theban tombs, it is necessary either to extend the LM IA period for several decades after the eruption and to stretch LM IB to fill at least three generations, or to extend LM IB to five generations. It is possible to reduce the gap by a generation by postulating that the objects of LM IB aspect which Minoans are shown carrying were heirlooms or removed from earlier deposits, or had been circulating in the eastern Mediterranean, or were still being manufactured in LM II (WIENER 1998a). Any major increase in the chronological span between the Theran eruption late in LM IA and the destructions at the end of LM IB strikes many Minoan specialists as unlikely, however, given the lack of any site with successive LM IB strata providing indications of stylistic evolution of the pottery. In addition, the Aegean Long Chronology requires that a series of Aegean objects in Egyptian and Near Eastern contexts must have been deposited later than their floruit in the Aegean, and a scarab bearing the cartouche of Amenophis III from Sellopoulo Tomb 4 at Knossos must have arrived and been deposited very soon after his accession in *ca.* 1390 BC, or have been deposited after the period of production of the LM/LH IIIA1 pottery in the tomb (MANNING 1995, 226). None of these propositions is attractive singly, and the problem is compounded when they are considered collectively.

The most serious challenge to the Aegean Long Chronology, however, comes from the evidence gathered in the current excavations at Tell el-Dab^a, ancient Avaris. Material from New Kingdom stratum C there includes Minoan or Minoanizing wall paintings that in a number of respects closely resemble those of Thera, Theran waterborne pumice chemically determined to be from the Bronze Age eruption, and above all, White Slip I pottery (BIETAK 1996, 76, 78; *idem* 1998; see also BIETAK, this volume).

While the presence of both the wall paintings and pumice in New Kingdom contexts supports the Aegean Short Chronology, neither is conclusive. As regards the wall paintings, it has been argued that the paucity of LM IA comparanda from Crete leaves open the possibility that the Dab^a paintings may be of a later date than those of Thera. The existence of Minoan or Minoanizing paintings at the Hyksos site of Tell Kabri and at Alalakh, together with the use of yellow as a skin colour at Tell el-Dab^a, not thus far attested in Crete, has led to the suggestion that the Dab^a wall paintings were the work of Minoan-trained artists or families of artists who had lived abroad for a number of years, if not generations (SHAW 1995, 110).⁶ It is also possible that pattern books existed and remained in use, and even that the Dab^a wall paintings represent two time periods, similarities between the two deposits of wall painting fragments notwithstanding. The forthcoming publication of new reconstructions of the Dab^a paintings based on the discovery and cleaning of additional fragments and the use of computer imagery, however, should provide support for the view that some of these paintings are among the finest of Minoan works, executed at least in part by a Knossian artist, and that at least some of the depictions, particularly that of the griffin, are very close to their Theran counterparts.⁷

Similarly, the first appearance of Theran pumice at Tell el-Dab^a in early New Kingdom stratum C is certainly a significant argument in favour of the Aegean Short Chronology, but the fact that the pumice was waterborne and could have been picked up at any time, or even deliberately imported as an abrasive, means that this evidence also is inconclusive. Theran eruption pumice is very often found in the Aegean in much later contexts (WIENER 1998b, 26).

It is the Cypriote pottery from Tell el-Dab^a, and particularly the White Slip I and Proto-White Slip, which is critical to the debate. The finds there, to date, include ten examples each of PWS and WS I plus another four of WS II and eight examples which are undiagnostic (BIETAK and HEIN, this volume). Six of the PWS examples come from the final Hyksos stratum D/2, including one complete bowl clearly

⁶ M. BIETAK has noted (*pers. comm.*) that one of the Xeste III boys from Thera has a yellow colouration not always apparent in reproductions. M. POPHAM has kindly called my attention to the fact that analysis of the pigments employed at Thera disclosed the use of the colour yellow (FILIPPAKIS 1978).

⁷ I am most grateful to Professor MANFRED BIETAK for showing and discussing with me the wall painting fragments and reconstructions at Tell el-Dab^a in the week following this conference.

stratified in a child's tomb. Five of the WS I fragments come from New Kingdom stratum C. The remaining examples all come from unstratified contexts. As to whether WS I might already have been present in Hyksos stratum D/2, I. HEIN concludes (this volume) that there is "only a slight possibility but it is very unlikely".⁸

To date the site has yielded a total of 600 fragments of Middle and early Late Cypriote pottery, a large proportion of which came from a particular stratum, although usually not from a fixed context such as an intact tomb or destruction level. Pottery seriation plus scarabs (and in one case a statue) of known rulers help define the successive strata (BIETAK and HEIN, this volume; BIETAK 1996, 1997 and 1998). The stratified Cypriote pottery follows the chronological pottery progression established for Cyprus by the Swedish Cyprus Expedition, in particular by P. ÅSTRÖM and M. POPHAM (ÅSTRÖM 1972b), and is supported by Cypriote pottery sequences in the eastern Mediterranean (OREN, this volume). At Tell el-Dab^a stratified PWS in the D/2 final Hyksos stratum and WS I in the earliest 18th Dynasty stratum occur just where they would be expected in relation to other Cypriote wares and to the Aegean Short Chronology. Of course, any particular object from abroad may arrive and reach its final resting place with a long delay, but it is not credible that such a process could affect so many items uniformly over centuries and in different regions.

If the eruption of Thera occurred in 1628 BC as proposed by the Aegean Long Chronology, how is it possible to explain the discovery by the French excavation in 1870 of sherds of a WS I bowl below the tephra of the eruption, a century or so earlier than the first appearance of WS I at Tell el-Dab^a and elsewhere according to Egyptian historical chronology (*supra*)? The date of the New Kingdom conquest of Avaris, the Hyksos capital at Tell el-Dab^a, by Ahmose, and hence the beginning of New Kingdom stratum C in which WS I pottery first appears, cannot have been prior to 1540 BC, and was probably no earlier than 1530 BC (KITCHEN 1992, 327).

First, is there any possibility that the WS I bowl from which the sherds came was deposited on Thera after the eruption? The 1870 French expedition left no information as to the exact find-spot of the sherds, and no subsequent WS sherds, so easily rec-

ognizable, have been identified among the millions of vessels and sherds uncovered by the current ongoing excavations. As a result of the meticulous research of R. MERRILLEES concerning the history of the French excavations and the sherds in question (this volume), there can now be no doubt that the French excavators and BURNOUF, then the Director of the French School, believed that the WS I sherds from Thera belonged with the other eighty-one pieces catalogued by RENAUDIN (1922) and that all of them came from beneath the tephra of the eruption. The question remains whether the WS I bowl from which the sherds came could have been deposited or deliberately buried, perhaps as a cult offering, by a post-eruption visitor to Thera, for example at the deepest point in the ravine which runs along the site of Akrotiri, one of the places where GORCEIX and MAMET dug? S. MARINATOS reported that when he began his excavations the tephra and ash cover was only 70 cm. thick in some places as a result of rainwater washing through the ravine, and that along the beach the topsoil had been exposed by erosion (1968, 4-8). Moreover, S. MARINATOS specifically mentioned encountering Volcanic Destruction Level (VDL) remains a metre below the surface in an area near where he believed MAMET had dug (1968, 8). Could violent storms, in the year following the eruption or subsequently, have sent water rushing down the ravine, removing some of the tephra cover and allowing the bowl to be placed into a cavity, perhaps deliberately dug, in proximity with the VDL material? Could the stratigraphy have been disturbed by subsequent earthquakes and eruptions of Thera, in such a manner that a post-eruption offering became difficult to distinguish from VDL material? None of these hypotheses seems at all likely, and accordingly the possibility of post-eruption deposition of the WS I bowl is remote.

The contemporaneity of LM IA and WS I is further supported by the finds at *Toumba tou Skourou* in Cyprus (ERIKSSON, this volume).

If the VDL context of the WS I sherds from Thera appears secure, and is reinforced by other LM IA-LC I links, what possibilities remain to support a 1628 BC date for the eruption of Thera, a century or so earlier than the first appearance of WS I at Tell el-Dab^a in the form of five sherds in New Kingdom stratum C (*supra*)? The problem is com-

⁸ Two of the WS I sherds come from the later phase of stratum C which extends into the reign of Thutmose III. WS I-Thutmose III links exist at various sites including Tell el-Ajjul (OREN, this volume) and *Toumba tou Skourou* (ERIKSSON, this volume).

pounded by the fact that no example of WS I has appeared in a stratified MB context at any site in Cyprus, the eastern Mediterranean or the Aegean (OREN, this volume; ERIKSSON, this volume; cf. BERGOFFEN, this volume, discussed *infra*). The problem is even more acute if it is accepted that PWS precedes WS I in the main, since six examples of PWS, including a whole bowl in a stratified tomb, appear in the final Hyksos stratum D/2 at Tell el-Dab^a, which must also begin considerably after 1628 BC.

Proposed explanations for the gap of a century between Thera and Tell el-Dab^a created by the Aegean Long Chronology combine (1) infrequency of export of open shapes in general; (2) regionalism in Cyprus, with Cypriote pottery exports in MB limited almost entirely to wares from SE Cyprus whereas PWS and WS I were produced in the west; and (3) the lack of clearly stratified successive building levels at sites in Cyprus and particularly in the NW to rebut the possibility that PWS and WS I were in use in NW Cyprus for half a century before reaching sites on the south and east coasts in any number, where they would be available for export to Egypt.⁹

Certainly Cypriote open shapes are rarely found abroad during LC I. PWS and WS I hardly appear at such extensively excavated sites as Ugarit, Ashdod, Megiddo, Hazor or Lachish (OREN, this volume). Such pottery typically would not have been the subject of state exchange, nor is it likely to have been much involved in merchants' or captains' trade, since filling any space available aboard ship between precious metals, luxury objects and copper or tin ingots or other state-exchange goods with small juglets containing perfume, oil, honey or opium (and any combinations thereof) would likely have been more profitable. The Middle Cypriote pottery found at Tell el-Dab^a and other sites abroad consists almost exclusively of such juglets used as containers (MAGUIRE 1995, 54). The one exception found thus far at Tell el-Dab^a is a Red-on-Black bowl handle from stratum E/l. PWS and WS I open milk bowls may largely have represented sailors' trade, and their arrival at a site may have depended on the point of departure and place of origin of the ship plus an element of chance. Moreover, during the period of strata D/3 and D/2, Hyksos Egyptian acculturation was underway, and Egyptian tablewares may have been pre-

ferred at Tell el-Dab^a. Given the fact that PWS/WS I bowls were distinctive in appearance, pleasant to handle, easy to clean, impervious to liquids and easily stackable aboard ship, the scarcity of exports of these wares, readily recognizable when they appear in excavation sherd trays, underscores the emphasis given to the contents of closed containers as exchange goods in the MB and beginning of the LBA. With the great increase in seaborne trade in the 14th century BC, transport of open shapes increases. Over 1,000 examples of WS II have been found in Palestine to date (OREN, this volume).

There is, however, one clear and one possible significant exception to the general absence of Cypriote open shapes from sites abroad in the Middle and early Late Bronze Age. At Tell Atchana (Alalakh), both the palace and individual houses contained numerous WS bowls, particularly in Level IV where they are the most common drinking vessel (WOOLLEY 1953 and 1955). Although WOOLLEY referred to many of these as WS I, subsequent examination by E. OREN of the sherds from Alalakh in the collection of University College, London identified only about twenty as WS I, and the remainder as WS II. The Alalakh collections in the Ashmolean Museum and the British Museum contain almost no WS I.¹⁰ Level IV probably begins in the reign of Thutmosis III (GATES 1987; MCCLELLAN 1989, 188–89). Some WS I bowls appear in earlier contexts, but none clearly prior in date to the beginning of the New Kingdom, although uncertainties as to the stratigraphy have led to a variety of views regarding absolute dates (MCCLELLAN 1989, 182–86 and references cited therein). No examples of PWS have been identified at Alalakh.

The clear illustration of major WS I export is found at Tell el-Ajjul, the Hyksos stronghold and port near Gaza, perhaps the ancient Sharuhen. Here PETRIE uncovered about 1,100 examples of Cypriote imports, of which 200 were various MB wares, 25 PWS, and 200 WS I (OREN, this volume; BERGOFFEN 1989, this volume). Over 50% of the MB examples were Red-on-Red or Red-on-Black open shapes, and the prevalence of open shapes increases at the beginning of LB. The majority of the WS I milk bowls were found in the area of the Tell el-Ajjul palace, but some were found in the town as well (OREN, this volume). The open shapes at Tell el-Ajjul

⁹ S. MANNING, letters of 26 September and 1 October 1996, for which I am most grateful.

¹⁰ Pers. comm., for which I am most grateful. C. BERGOFFEN's upcoming re-examination of the Alalakh material in Antakya may shed light on this question.

may represent a special elite drinking practice (OREN, this volume), the consumption of a particular food-stuff such as yogurt, or simply a familiarity with Cypriote pottery arising from trade connections, which in the case of PWS and WS I milk bowls may reflect both the attractions of the pottery and the opening of new direct links to western Cyprus with its metal sources in the Troodos.

Some of the WS I examples from Ajjul have close parallels at *Toumba tou Skourou* in NW Cyprus. E. OREN (this volume) has noted that Tell el-Ajjul also shows particularly close connections (in architecture, burials, jewellery and pottery imports, Egyptian as well as Cypriote) to Tell el-Dab^{ca}. It seems likely that Tell el-Ajjul was a key node in a network distributing copper and perhaps other goods from NW Cyprus and that by LC IB at the latest (ERIKSSON, this volume) the network extended in the other direction to the Aegean (WIENER 1990). WS I appears in the Aegean in LB I in a few instances, in addition to the bowl from Thera (CATLING 1991), but in no other case in a clear LM IA context.

The Cypriote pottery from Tell el-Ajjul is of chronological significance, inasmuch as a re-examination of the stratigraphy (OREN, this volume; BERGOFFEN 1989, this volume) reveals that the progression of Cypriote wares including PWS and WS set forth by P. ÅSTRÖM and M. POPHAM in *The Swedish Cyprus Expedition* in 1972, and observed in the successive strata at Tell el-Dab^{ca}, applies at Tell el-Ajjul as well. Moreover, WS I at the city site first appears stratified in contexts later than the beginning of the New Kingdom in Egypt.¹¹ The site of Megiddo displays the same WS sequence (although with far fewer Cypriote imports): PWS appears in the final MB stratum X and WS I in LBA stratum VIII, along with Egyptian New Kingdom material (OREN, this volume). At the large and well-excavated sites of Tel Batash and Tel Sippori, WS I and other Late Cypriote wares do not appear before the beginning of the New Kingdom (OREN, this volume), but the number of stratified Cypriote sherds is small (BERGOFFEN, pers. comm.).

Regionalism in Cypriote pottery production has been suggested as a second factor (in addition to

restricted export of open shapes) to explain the putative export of a WS I bowl from western Cyprus to Thera a century before WS I pottery can be documented in the eastern Mediterranean or the Nile Delta.¹² The Cypriote juglets and jugs which appear in eastern Mediterranean and Nile Delta sites are almost exclusively SE Cypriote wares. Ideas travel overseas as well, again with SE Cyprus as the interlocutor. White Painted juglets produced in Syria are closely similar to Cypriote White Painted Eyelet Style juglets (KARAGEORGHIS 1995a, 74). Other SE Cypriote and Syrian wares display reciprocal influences, to the point of constituting a single hybrid style (MAGUIRE 1995, 65). Western Cypriote versions of White Painted IV, V and VI with geometric decoration are thus far represented at Tell el-Dab^{ca} only by a few sherds of White Painted V. MB White Painted III/IV wares produced in the north are almost totally absent on the SE coast (MAGUIRE 1992, 118, following ÅSTRÖM 1972a). Moreover, the great majority of Tell el-Yahudiyyeh juglets travelling in the reverse direction are found in the SE region of Cyprus. Internal regionalism clearly played a role at times in limiting the movement of pottery from western Cyprus to the eastern Mediterranean and Egypt. During MC III-LC IA, insecurity in Cyprus (evidenced by the appearance, *inter alia*, of fortifications and new weapons) may have limited traffic across a NW-SE divide (MERRILLEES 1971; MAGUIRE 1995, 54).

The internal division is not absolute, however, as shown for example by the parallel development of White Painted V and VI in east and west at the end of the Middle Cypriote period. H. CATLING¹³ has noted the contrast between on the one hand the distribution of wares clearly localized, such as MC Red-on-Black, White Shaved and Palestinian Wheelmade Bichrome, and on the other hand PWS and WS I, which appear, at least at some points or points in time, in reasonable quantities in central Cyprus (e.g., Akhera, Dhenia, Politiko, Akaki, *Ayia Paraskevi* and *Ayios Sozomenos*); at Enkomi and nearby Trachonas; at Kalopsidha; and at sites on Larnaca Bay (Arpera Chiftlik, Klavdhia, Hala Sultan Tekke and *Pyla-Verghu*). Further west on the south coast, Kourion

¹¹ The contexts of the WS found in the palace area at Ajjul are confused by pits resulting from rebuilding; WS I, local MB and Islamic pottery are sometimes found mixed (OREN, pers. comm.). BERGOFFEN (this volume) suggests that some of the WS I may belong with the Hyksos palace. The restudy now underway of the contexts of all of the mater-

ial from the palace area will provide further information on this question.

¹² The closest counterpart to the Thera bowl comes from Tomb 104 at Palaepaphos-*Teratsoudhia* on the SW coast of Cyprus (MERRILLEES, this volume).

¹³ Letter of 22 July 1999 for which I am most grateful.

(*Bamboula*) receives significant quantities of WS I (BENSON 1961, 1969 and 1970). The internal barrier hypothesis would require that WS I did not appear in the south or east of Cyprus in significant quantity until LB IB. Some Tell el-Yahudiyeh juglets made of Nile clay, along with a larger number of Cypriote imitations, appear in the NW of Cyprus, at least by LC I (BIETAK and HEIN, this volume; MERRILLEES 1974a, 43–79). Of course PWS reaches sites in the eastern Mediterranean and the Nile Delta by the end of MB and WS I at the beginning of LB, as described above.

The third factor cited in support of a putative early development of WS I is the scarcity of well-stratified building levels for MB and early LB Cyprus, a situation which is said to open the way to proposals that wares commonly accepted as chronologically distinct, for example Proto Base-ring/Base-ring and PWS/WS I, are in fact contemporary regional variants (EAMES 1994). K. ERIKSSON's detailed analysis (this volume) presents a convincing case for a chronological progression in the burials at the *Toumba tou Skourou* cemetery in NW Cyprus, and generally for the progression from PWS to WS I, with an overlap only between the production of the third and final phase of PWS with WS I.¹⁴ The LC IA period of PWS and Proto Base-ring production preceding the introduction of WS and Base-ring in LC IB proposed by ERIKSSON is however relatively brief. The chronological progression from PWS in Hyksos strata to WS I in New Kingdom strata has been observed in settlement contexts at Tell el-Dab^a, Tell el-Ajjul and Megiddo, as noted above.

In the main, the chronological progression of Cypriote MB to LB I wares seems secure, with recent work in Cyprus and abroad confirming the Swedish Cyprus Expedition sequence set forth in 1972. The Tell el-Dab^a Cypriote catalogue of 600 fragments, a large percentage of which can be assigned to strata, is important not only for what is *not* present – stratified PWS before stratum D/2, stratified WS I before stratum C – but also for what *is* present, namely a coherent Cypriote sequence including White Painted III–IV in strata G to E/1, White Painted V in D/3 with a floruit in D/2, and White Painted VI (togeth-

er with Bichrome ware) in D/2 continuing into stratum C of the 18th Dynasty (BIETAK and HEIN, this volume).

Accordingly, in order to rescue the Aegean Long Chronology based on a putative overlap of WS I production with earlier wares, it is necessary to imagine the creation around 1650 BC of PWS and shortly thereafter WS I, presumably somewhere in the foothills of the Troodos judging from the clay used, and a period of local use of both PWS and WS I (except for the only known exception, a WS I bowl which arrives in Thera prior to the putative 1628 BC eruption), then a period of widening use encompassing first sites in the SW, west and NW of Cyprus, and finally the south coast of Cyprus followed eventually by export of WS I to Tell el-Ajjul, Tell el-Dab^a and other points abroad first documented after ca. 1530 BC. Such a scenario seems highly unlikely.

It is worth noting that other Cypriote wares, including Proto Base-ring/Base-ring, Bichrome and in particular, Red Lustrous Wheelmade (ERIKSSON 1992, 178–81), magnify the problem posed by Cypriote pottery for the Aegean Long Chronology, for they appear with PWS or WS I and not earlier in Cyprus, and maintain the same relationship in the stratified sites abroad. Finally, we should observe that the Aegean Long Chronology would require a period of production of White Slip I of 175 years and a Late Cypriote I period of 200 years.

To what extent would the prospects for the Aegean Long Chronology be improved if the D/2 stratum at Dab^a encompassed sixty years rather than the thirty years proposed by the excavator, and the massive fortification wall of D/2 became the work not of Apophis, the penultimate Hyksos ruler, but perhaps of Khyan, as proposed by S. MANNING (pers. comm.)? On the basis of transformations in architecture, burial practices, weapons and other finds including royal scarabs, the Hyksos period at Tell el-Dab^a is believed to include strata D/3, E/1 and perhaps part of E/2 as well as D/2 (BIETAK 1996, 6 fig. 3, 64). A sixty-year D/2 stratum beginning around 1600–1590 BC would account for more than half of the total Hyksos period stated in the Turin

¹⁴ The initial version of this paper, presented at the conference, referred to “early WS I”. In the general discussion, M. POPHAM observed that it would be preferable to speak of “WS I normal”, reserving “WS I late” for examples which are transitional to mass-produced WS II. References to “early WS I” have accordingly been deleted. Cf. BERGOF-

FEN (this volume) who argues on stylistic grounds that the WS bowl from Thera appears to be earlier than the WS fragments from New Kingdom levels at Tell el-Dab^a. Clear stratigraphic evidence for a temporal distinction in WS decoration is currently lacking, however.

Canon.¹⁵ In order to accommodate a 1628 BC date for the WS I bowl in the Volcanic Destruction Level at Thera, the Aegean Long Chronology would still require: (1) the deposit of the stratified PWS bowl from a Dab^a D/2 tomb (together with the other nine examples of PWS from that stratum) near the beginning of the time period encompassed by stratum D/2; (2) the production of the PWS bowl fifty years prior to the date of its deposition in Egypt, together with all of the PWS and WS I fragments found in D/2 and C respectively (unless they are survivals from earlier strata in which no such examples, but large numbers of MB Cypriote wares, were found); (3) the arrival of one of the earliest pieces of WS I at Thera not long before the eruption (notwithstanding some evidence that the bowl in question was repaired in antiquity, as noted by MERRILLEES, this volume); and (4) the existence of significant chronological overlap between PWS/WS I and White Painted V at least, if not the Pendent Line and Cross

Line Styles of White Painted III/IV as well, notwithstanding the fact that such an overlap is not observable at any site in Cyprus, the eastern Mediterranean or the Nile Delta, and goes against the evidence at *Toumba tou Skourou*, Tell el-Ajjul and Tell el-Dab^a (ERIKSSON, this volume; OREN, this volume; BERGOFFEN, this volume; BIETAK and HEIN, this volume).

Each of these four propositions is individually unlikely, and the chance of all of them obtaining is slim indeed. The White Slip pottery from Tell el-Dab^a and Thera accordingly presents a most critical challenge to the proposed 1628 BC date for the eruption of Thera and to the Aegean Long Chronology.

DISCUSSION

There was no discussion following this paper.

¹⁵ All dates would be raised by eleven years if the accession date for Thutmose III is 1490 BC rather than 1479 BC (*supra*). The absolute date of the end of stratum D/2 will also reflect when in the decade after the eleventh year of Ahmose the conquest of Avaris (Tell el-Dab^a) occurred. (The longer the duration of stratum D/2, the less of an obstacle, although formidable in any event, the clearly

stratified PWS bowl from a child's tomb in stratum D/2 and the other nine pieces of PWS from the same stratum at Tell el-Dab^a pose for the Aegean Long Chronology; however, the longer the duration of D/2, the less likely it becomes that the absence thus far of WS I in stratum D/2 is fortuitous.)

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GENERAL DISCUSSION

Karageorghis: Now that we have come to the general discussion, so that we do not create confusion, I suggest that we discuss each aspect of the symposium separately. Let us start with the techniques and manufacture of White Slip, the first three papers of the meeting.

Todd: As one of the participants in the first three papers, now that you have had the chance to see the fire-bars from Sanidha in the museum and also to handle a few here, does anyone have any suggestions or any new information on similar artifacts which they know of which they did not mention before? Please, help!

Dothan: As to your bars from the kiln, when we looked at them here in the office, I mentioned that they reminded me very much of what I found in kilns in Deir el-Balah of the 13th century in which we think that the coffins were fired. I will check it when I return to Jerusalem. On the upper part of a large kiln which we found, there were bricks in this shape which we thought could have been the upper structure of this kiln. Maybe your examples at Sanidha will help us out.

Todd: I would love to find some anthropoid sarcophagi at Sanidha!

Herscher: I would like to comment on the paper by Aloupi and Lekka. I think we paid a lot of attention to the details which they talked about which were very interesting, but to me what was really striking about the paper was the way in which they demonstrated what a really phenomenal technological advance the invention of White Slip ware was, what a technological feat especially in terms of the pyrotechnology, and to create the bichrome effect considering the unsuitability of the slip to the fabric etc. We should put this in the context of the culture of the time, perhaps related to advances in metallurgy as well, also a pyrotechnological development. I think that really helped to put that transition, in which I am interested, into a very important larger picture.

Aloupi: What I tried to show was that you, as archaeologists, must try to convince the scientists with whom you collaborate to analyse the slip and the decoration apart from the body, to try to establish a

chemical sequence or a technological sequence for your pottery, apart from trying to solve only provenance questions. As far as we may have established some of the characteristics of other sequences apart from the Kouklia sequence, I think that we will be able to solve, or you will be able to solve some typological and chronological aspects or problems.

Eriksson: In relation to your paper, I noted the concentration on the analysis of the sherds from the Kouklia area, and then you also analysed material at random that was available in the Cyprus Museum in Nicosia. There have been a lot of analyses done of White Slip; Peter (Fischer) mentioned some that he has done, Manchester has also undertaken some analyses. It would be really great if we could bring these analyses together and look at aspects of regional production and work along the lines that Sarah Vaughan developed for the Base-ring series.

Aloupi: Our results and those sherds will be available to other groups, and it will be very useful to give our results for petrographic analysis and also Neutron Activation analysis, and I hope that these sherds will be included in data bases in future. The problem is that the results that we published up to now are results that refer to different techniques. For example, we have results from Neutron Activation analyses from a certain area or certain type of White Slip. We have results concerning petrographic analyses, and we have our results that refer to the chemistry of the slip, the decoration and the chemistry of the body and technological aspects such as firing temperature and so on. But they are still scattered. In future we can form some groups and work together with archaeologists and be very careful in sampling. These are all common problems when we apply these analytical techniques.

Artzy: I would just like to add that there are, I think, hundreds of samples which have been analysed at the Lawrence Berkeley Laboratory. The laboratory is no longer active, in fact it has not been active for many years, and the samples have been lying around ever since. We have analysed the wares of WS I and WS II from quite a few sites; this is material from the Swedish Cyprus Expedition, material which was sent

by Gjerstad many years ago to the Lawrence Berkeley Laboratory, and some I should add sent by our host. After I arrived there we started running the tests on the slip rather than the ware; the ware seems to be similar, but the slips change from WS I to WS II. All this material is there; the numbers are in Berkeley but I have all of the information in Israel, and if people are interested you are very welcome to it.

Oren: There are more and more studies on physical aspects of the White Slip, the paste, slip, paints and so on done by different laboratories and scholars, and I think that one of the happy outcomes of this conference should be an appeal for better coordination. I do not think that we have enough funding to duplicate the tests, and we should coordinate these past efforts which involved great expense.

Cadogan: I just have a simpleton's question. With petrography, which has been useful in other parts of the world including Crete in sorting out various problems of provenance, is there enough distinction among the deposits around the Troodos that it would be a helpful tool or not?

Aloupi: I am afraid that I am not the person to give you an answer to that question since I am not familiar with Cypriote geology. But many analyses have been done in the past by Sarah Vaughan. There are some people who collaborated with Ellen Herscher; I am thinking of Basil Gomez, and you may be able to answer this.

Herscher: I really do not know very much about it. To make a comment from the archaeologist's point of view, we have collaborated and helped a number of people in the past, but we have had a great deal of trouble in ever obtaining any results from them. I have a small note in the next *RDAC* on this very matter, that many archaeologists have been willing to cooperate and provide samples and participate in these kinds of studies, and I think we are all very eager to obtain results, but we never receive any results from those who are doing the study. In one case something was published which had never been submitted to the excavators and it was totally wrong in terms of the archaeological information. I think we all need to work together more closely.

Manning: To answer Gerald Cadogan's question, John Williams, who has been working with Eliezer Oren, has managed to identify the source of samples derived from the different geological regions of Cyprus quite successfully. I would also like to introduce Helen Hatcher who is just beginning a Ph.D. studentship at the University of Reading which has facilities for doing thin sections and this type of

work, and she will be undertaking analyses of White Slip and other Cypriote wares. I support Eliezer (Oren's) plea for coordination; we do not want duplication. Perhaps a small working group could be established to facilitate this.

Karageorghis: The local person who could undertake such liaison is Lena Kassianidou of the Archaeological Research Unit. She is qualified for this and she resides in Cyprus.

Popham: It seems to me rather fantastic that after so many years we only know of one White Slip furnace production centre. There must be others and they must be fairly easily recognizable. If we are looking, as I believe we are, at the pillow lavas of Troodos I wonder whether we should bring in the Forestry Department who walk over a lot of this land and probably know nothing about archaeology.

Karageorghis: We move now to the second unit of our symposium which is styles of PWS, WS I and WS II.

Popham: As chairman of the last session I was getting very worried about the constant use of the term "White Slip I early". I wish I knew what "White Slip I early" was!

Wiener: Perhaps foolishly I would like to attempt a brief response to Mervyn Popham's question. I picked up the term from Robert Merrillees' paper and Sturt Manning has also been using the term. I would have thought that the typical example was what Mervyn (Popham) himself described as the lozenge and ladder style of WS I at *Toumba tou Skourou*. I think that is really what people mean by it and suppose that that was early in the WS I sequence. But there are others far more competent than I am to comment on this.

Merrillees: When I used the term "early" I used it in the context of describing a WS I bowl as an early WS I bowl. But when you put the adjective at the end, "White Slip I early", then you are creating a new category which I certainly did not intend to do on the basis of one bowl. My purpose was to indicate that I thought it came early in the series. But it does enable me to make one point that has become increasingly clear to me in using these categories, which are very useful rules of thumb for classifying all of the White Slip pottery, and that is that if you wish to try to attribute a particular White Slip sherd, bowl or whatever it is to a particular chronological horizon, you cannot rely exclusively on calling it WS I. If you call it WS I, you give it a possible chronological range of LC IA:2 right down to the end of LC IB and possibly into LC IIA. If you want to be

more precise than that, then you have to compare the piece with others that are well dated, and that is exactly what I tried to do in my own paper, to find exact or the closest possible parallels for the Thera bowl in the corpus of White Slip wares that we have available to us for comparative purposes.

Popham: Thank you for the clarification. I think that until we are a little more sure of where we are going it might be better to keep to "White Slip I normal" and "White Slip I late". We know what might be late stylistically; I am not at all convinced that the bichrome is an early stage, I would have thought that it might be a much later stage. That is why I say I think we ought not to jump the gun too much. But what it does provide for us is almost the equivalent of Black and Red Figure ware. This painter stands out, and I do not think there can be more than one or two painting like that. And I noticed another feature of that particular painter or, if I am going too far, factory, that unlike any other normal White Slip bowl I know, the base has been flattened. Flat bases are quite normal with spouted bowls and they are quite normal with the small lug bowl, but I know of no normal, ordinary WS I bowl with a flattened base. I have just been looking through Eriksson's paper on WS I and I see that she has some sherds that look as though they are bichrome of this type, and sure enough she has marked it "flat base". It is not a real base. My other point on style is that I am delighted that it now looks pretty definite that PWS is, in fact, a stage, and an earlier stage on the whole than WS I, although inevitably there may be some overlap. The centre of manufacture looks as though it is in the Morphou-Nicosia foothills. I am surprised to find so much on other sites that were not known before such as Maroni, Kouklia and so on.

Manning: As somebody who has been partly blamed for introducing the term, I should try to explain the basis for it. I think Mervyn (Popham's) term "White Slip I normal", as opposed to "White Slip I late", is the same as "White Slip I early". We are talking about the same category and it is a question of the name applied to it. But the point that should be emphasized is that it is quite possible to distinguish a later or subsequent or mature phase of WS I which has two parallel lines with lozenges inside which equals a variety of things which we have seen from Tell el-Dab'a in particular, which is all mature or later WS I, versus an earlier or normal or initial or whatever you might like to call it phase represented at *Toumba tou Skourou* and elsewhere on the island. This is the distinction that is effectively being drawn, and the notable feature is that this earlier or initial or

normal style of WS I is not found outside Cyprus in the Levant or in Egypt with the sole exception of Tell el-Ajjul. By contrast it is found on Thera, and that is the notable fact about the Thera bowl. The Phylakopi piece was called early in a publication some years ago, and today it is being called a variety of things. I phrased the railway track motif, which is the two slightly thicker parallel lines with the very much fainter little nicked ones in between, mature WS I or even later. This is arguable, but it is by no means early. It counts in the classic phase, and the piece from Trianda, which surprisingly has not been illustrated at all during the conference, is a classic example of the two parallel lines with lozenge in the middle, and it comes from a context with Late Minoan IB and nothing earlier. It is an absolutely definite LM IB context. That is the later, mature phase as found in most of the contexts across the east Mediterranean. It is the early phase that should, in some senses, be of most interest to us and which is the most enigmatic because it is not, in fact, found in nice contexts abroad. This does not mean that either the high or low chronology is correct; the point is that we cannot tell because we are lacking this material at Tell el-Dab'a; it is also lacking at Tell Atchana, and I have looked in the Antakya museum amongst other places, it just is not there. The only place where it is found is Tell el-Ajjul, and as Eliezer (Oren) will undoubtedly tell me if I try to claim anything, the stratigraphy there is somewhat debatable. I hope that his forthcoming study may elucidate it, but I fear that the answer will be negative.

Karageorghis: With this we come to the problem of diffusion.

Popham: I am not going to talk about diffusion since my knowledge of its export abroad is very much out of date. It is almost a circular argument to say that early WS I is not found outside the island. You have already defined what early WS I is. The fact that we get the ladder pattern as you were describing it, and I am sure that the Thera bowl is of that type, I don't think it has anything to do with chronology in that particular context.

Merrillees: I would just like to take up the question of confusion. In the discussion on diffusion reference has, on several occasions, been made to things called Middle Cypriote fabrics. I have had occasion before to question this particular usage of the term, because there are no such things as Middle Cypriote fabrics, if it means that fabrics that are called Middle Cypriote cannot go into Late Cypriote. In fact nearly all of the fabrics of MC III continue throughout LC IA. All of the White Painted Cross Line style, Pendent Line

style, White Painted V, Red-on-Black, Black Slip (Reserved Slip) wares, indeed all of the characteristic and diagnostic ceramic features of MC III, continue almost unceasingly throughout the whole of LC IA and only begin to die out in LC IB. If you use the term Middle Cypriote fabrics you must be absolutely specific what you mean by this. If you then try to say that Middle Cypriote fabrics never occur with PWS, that is, of course, in terms of the Cypriot relative chronology, a nonsense, because PWS in Cyprus occurs very irregularly in many contexts in LC IA and the overwhelming masses of pottery in both stratigraphic and funerary contexts are all what is called Middle Cypriote – that is fabrics that belong to those particular wares that I have just described. All of this is set out in full detail in *SCE* IV, 1B where you will never find anyone referring to Middle Cypriote fabrics.

Karageorghis: We are going to discuss chronology later, so let us stick to the subject of diffusion now.

Oren: As far as diffusion is concerned, we have focused on the early styles of White Slip – PWS, WS I and not WS II which is totally different, and I think that what emerges so far is that, unless there are some real surprises in the field, we are dealing with the diffusion of a trickle of PWS and WS I outside Cyprus. This is probably not going to change in general terms, and it is interesting that the major concentration of this is Tell el-Ajjul and to a lesser extent at Tell el-Dab'a in the Delta. If we would like to refer to this as a phenomenon, it is certainly a phenomenon which is in total contrast to what we know about WS II. I think this is one aspect that has emerged out of this meeting.

Herschler: I was very glad to hear Malcolm (Wiener) remind us that most of the trade to the Levant probably came from SE Cyprus, because the theme of the conference being White Slip, there have been many references to NW Cyprus where, of course, *Toumba tou Skourou* and the other sites have yielded so much fine PWS and WS I ware. But the vast majority of the wares found in the Levant do come from eastern Cyprus as Malcolm (Wiener) correctly pointed out. This is where I think Maroni is going to be extremely important. For the first time at Maroni, both with the settlement deposits, particularly in the southern part of *Vournes*, and with the seabed deposit which we only briefly referred to (and I have just completed the report on the ceramics of that deposit, and I think they fit in very well), we see the Cypriote side of the trade that we have already seen in the Levant, the same sort of assortment of wares, the Red-on-Black, Black Slip and so on with White Painted also,

including these other kinds of wares that are perhaps not so attractive and that we have not picked out so much, but these are the wares that arrive in the Levant. I would also like to add an update: Hala Sultan Tekke is very much a part of this trade. There is PWS at Hala Sultan Tekke. Very recently I looked again at the sherds in Larnaca Museum which I excavated in a trench there in 1972, and I am pleased to say that I have learnt something more in the last quarter century about LC I pottery. In Trench 15 at Hala Sultan Tekke there is well stratified PWS at the bottom, then there is a layer of brick and some sort of disturbance, and above that WS I. I would also like to add that in that lowest level at Hala Sultan Tekke and in other somewhat disturbed LC IA contexts, there is a fairly substantial amount of Canaanite jars. From the limited amount of work undertaken on the site in those early levels, I think we can see that it is very much the same pattern as at Maroni.

Karageorghis: Of course our knowledge of centres of production is not that complete in order to determine which region was exporting where.

Artzy: Let us not forget Enkomi.

Karageorghis: We have quite enough material from Enkomi, but there are other sites all along the Larnaca district coast which have not yet been excavated, so the picture may change radically.

Manning: To follow up on what Ellen (Herschler) was saying, I would like to propose two ideas or hypotheses to engage discussion. The first is: if you look in Egypt until very recently, and basically that means Robert Merrillees' book, two WS I bowls have been found in the whole of Egypt. In other words WS I was clearly not very popular in most of Egypt that had been explored until the end of the 1960s. The obvious place that had not been explored was the Delta. I think this is interesting. Then if you look in Cyprus there is this quite sharp differentiation between SE/south coastal Cyprus and the NW/western part of the island at the end of the Middle Cypriote and beginning of the Late Cypriote. This is not an original observation. Åström and Merrillees have debated this since the early 1960s onwards. The south coast/SE region, whichever sites are involved, is the area exporting to the Levant and particularly to Tell el-Dab'a at the end of the Middle Bronze Age. These sites perhaps, in their so-called PWS phase, are going to last a little bit longer in this phase than those in the north, where they have a slightly different tradition and which may well have been producing what we are going to describe as initial WS I at the same time as

sites in the south were continuing to produce PWS. There is no clear evidence of that but it is possible. But the question has to be, at Tell el-Dab^a for example, why are we finding PWS and WS I there and not elsewhere in Egypt? When it comes to diffusion, the topic of the present part of the discussion, why were those wares popular there and not elsewhere in Egypt? Bowls are a drinking type of vessel used in certain rituals, they are not trading containers. This would seem to be an interesting problem for future research to explain this differential pattern.

Oren: Also on the subject of diffusion, another site, in my opinion, of major interest and somewhat overlooked by students is that of Tell Heboua in the eastern Delta, and a smaller sister site to Tell el-Dab^a right off the Suez Canal. This site under the direction of Abd el-Maksoud is of interest simply because it yielded a vast deposit of the late Hyksos period including certainly WS I and I suspect also PWS alongside the bichrome, some White Painted and of course the array of Canaanite imports. So what is perhaps emerging is that we are dealing here with two clusters of sites, one in the eastern Delta with Tell el-Dab^a and its environs, the other at Tell el-Ajjul and neighbouring sites such as Tel Ridan. It would, of course, be exciting to try to answer the question of why specifically these regions, but I think the underlying answer is that we are dealing here with a certain economic phenomenon that has to do with the Hyksos and their economy.

Hein: Regarding Tell Heboua, I know that WS I was found there. We must remember that few settlements have been explored in Egypt. It seems that White Slip ware occurs mainly in settlement deposits, but how many excavated settlement deposits do we have in Egypt? Tell el-Dab^a is one of the few points in the eastern Delta, and we work very carefully. But how many other tells in the Delta have been excavated in that way?

Todd: I would like to draw your attention to the fact that I think our geographical coverage of the White Slip occurrences on the mainland has been somewhat incomplete. We have looked at it in Sicily, the Libyan coast and we have seen especially the Levant from Ras Shamra right down to the Egyptian Delta, but we must surely mention the Anatolian plateau. Admittedly we know of Troy. The information that is available is extremely scanty, but we must be aware that White Slip sherds, even in small quantities, are being distributed by whatever means as far north as the north central plateau. White Slip and Mycenaean sherds have been reported at Maşat

Höyük near Zile. This takes us back to what Vassos (Karageorghis) said when we started, there must be dozens, I would think, of maybe small White Slip sherds lying unrecognized amongst the excavated material from many sites. And I would add that it is not only a question of White Slip sherds, but also earlier Cypriote wares such as White Painted ware. We know of a White Painted Pendent Line style juglet at Kültepe. How much more White Slip is there still to be recognized? We have looked at the west, the south, the east, but we must also bear the north in mind.

Karageorghis: For the record I asked Wolf Niemeier to be with us and report on Anatolia, but at the last minute he was not able to come.

Wiener: I can provide one piece of information, that in the last two weeks of this season Wolf (Niemeier) found a PWS/WS I bowl but it was out of context. On the subject of Anatolia I was well aware that there was WS II in many places, but I was not aware of WS I.

Todd: I was referring to White Slip in general, not specifically to WS I.

Karageorghis: We turn now to the problem of chronology. Please be calm during this discussion! Let us start with the less controversial aspects.

Popham: My worry is not so much absolute chronology, but definition. Now that we have got rid of the expression Middle Cypriote fabric because it goes on into Late Cypriote, what defines LC I? I can only think of PWS, if PWS comes before Proto Base-ring.

Åström: We have to go back to Sjöqvist who made the following terminology: LC IA is the period when WS I begins. He did not know about PWS ware; it existed but it was not known. When I had to put it into the sequence I did not want to change the old terminology too much, but it was obvious that the PWS belonged to the Late Cypriote series, not to the Middle Cypriote. That was why I divided LC IA into two phases. The first a PWS phase, and the next (LC IA:2) the WS I phase when PWS continues. LC IA:2 in Cyprus is the period when WS I begins. The relative sequence in Cyprus is clear. The absolute date is quite a different matter and whether it occurs before or after the beginning of the New Kingdom is another matter.

Popham: I am not in any way criticizing what you did. What I am saying is that if we have two deposits, one known by God to be Middle Cypriote, the other known by God to be Late Cypriote but which did not have PWS, how would we distinguish them?

Wiener: In the earliest draft of my paper I referred to the White Slip bowl from Tell Heboua, but I was told that I should not refer to it since its stratigraphy was highly doubtful. Why is it significant? It is certainly not because there is no New Kingdom material on the site.

Hein: There is New Kingdom material at Tell Heboua, but I cannot offhand tell you in which particular place. Nomadic looters have collected fine pottery from the site and there could be White Slip bowls among their material.

Manning: On the question of chronology I would agree with Paul Åström entirely in the sense that there are two totally distinct issues here. One is building up a relative sequence in Cyprus from stratified excavations; for biased reasons I have to draw attention to Gerald Cadogan's paper which personally I think was one of the more significant papers of this conference, in that it showed a continuous sequence from initial LC IA:1 (if we are going to call it that) through to LC IIC in one stratified deposit based on actual settlement evidence, not tombs, not seriation, not guesswork, not stylistic features or anything else. LC IA can, therefore, be defined by the material in that deposit, and this entails WP V occurring with WP VI, Black Slip, PWS coming in and so on. I think Mervyn (Popham's) question can be answered by material from new excavations, especially Ellen (Herscher's) publication of the material from *Vournes*. This evidence also exists at other sites such as Hala Sultan Tekke, although it may not have been recognized as clearly at the time. The seabed deposit that we have at Maroni again provides a very nice tight horizon of the same body of material. Recognizing that and recognizing the phase of WS I, I think we are almost becoming uncontentious amongst the number of people who presented papers here. That leaves us in absolute chronology with a different debate. In Egypt in early New Kingdom levels we have mature and late WS I. I don't think this can be disputed. The only interesting question that remains is, where early phase WS I crops up, no one has ruled out that occurring in D/2 at Tell el-Dab'a or late Middle Bronze Age. That is the area of potential debate. The evidence at *Vournes* and the radiocarbon evidence that we showed is compatible with the high chronology, but also compatible with our compromise early chronology which is going to have a mid-16th century beginning for Late Minoan I, LC I etc. I regard that as an equally viable position. The only thing I would try to point out, and Malcolm (Wiener) did express his scepticism about radiocarbon dating, the one thing that science can say very comprehen-

sively is that a date after the middle of the 16th century is almost impossible, not just as a result of one analysis but all forms of scientific analysis, and I would even argue from a good look at Cypriote stratigraphy. As Robert Merrillees commented, you cannot ignore the sequence here; it has to be based on the Cypriote evidence, the sequence of White Slip and other materials that we have. At Maroni I think we can start to look at a period that runs from the late 17th century or mid-16th century through for some 200 years, and I think that should be the basis of the chronology of the Late Cypriote period from now on. I have taken Maroni as an example but there are other similar sites.

Merrillees: I have previously pointed out that the use of the term "high chronology" tends to mislead everybody. There is no agreed absolute chronology for the Late Bronze Age or, indeed, for the Middle Cypriote period in Cyprus. There is the ultra-high, the high, the middle, the low and the ultra-low. There is no way that you can classify them except by looking at the various differences. There are more people who agree with the middle chronology, which centres around the one that has been put forward by Paul Åström, than with the others. I am not certain whether Sturt (Manning) has yet put forward a date for the beginning of LC IA at 1700 or 1675 BC. He has only talked about the explosion of Thera and the implications of the bowl for chronology. So that he really cannot be held for having a high chronology for the Cypriote Bronze Age yet unless he wishes to commit himself to it. But there are many chronologies.

Wiener: On the radiocarbon dates, as Sturt Manning has himself pointed out so successfully, and I owe much of my knowledge to Sturt, the radiocarbon curve becomes smooth again around 1535 BC, 1530 at the latest. Before that it is very hard to distinguish, on a radiocarbon basis, between let's say 1628 and 1535. I noted earlier that a comment was made about how the curve becomes rather flat in the 13th-12th centuries, but the famous back-bending, or as I called it in the book, oscillating radiocarbon curve of the 17th-16th centuries is, of course, much worse, because trees, as a result of sunspot activity or whatever other cause, take in more ¹⁴C than they lose through the half-life decay rate in this period, and the curve turns backward. It is our curse that we have difficulty with radiocarbon dates in that period. Of course, radiocarbon has tremendous uses as Manfred Korfmann has shown with his careful work. He can now pin down Troy II to 50 years whereas in our youth it was dated anywhere from 3400 BC to 2800 BC. It is just that we have this particular prob-

lem for the 17th–16th century BC. On another question, I did note in Sturt's favour that Manfred Bietak said that he could imagine a 60 year D/2 period for Tell el-Dab^{ca}. Now if that period is 60 years, and if the fact that there is no WS I in D/2 is purely fortuitous, then the long chronology might have a faint chance.

Karageorghis: Instead of conclusions I would like to ask some of you to give us your general impressions. Mervyn (Popham), some years after the *SCE* article, which is still our basis for the study of White Slip, how do you feel that this conference has contributed, if it has, or what are your objections, if any?

Popham: From my point of view it has been wonderful because I have been shown to be generally right! I am still not sure whether the important point of whether PWS precedes Proto Base-ring has been solved. For PWS we can now draw on sites like Maroni and Hala Sultan Tekke which were quite out of the picture in my time. It is a little too early to come to conclusions, and we are likely to be influenced by the beautiful material from *Toumba tou Skourou*. I don't think that is necessarily where they were made. Concerning the subdivision of White Slip I still think it is too early, and I still feel that we want more evidence, particularly settlement evidence, before we start to subdivide. It is nice that WS IIA still remains in the Kouklia area probably stretching as far as Episkopi. The one really good thing from my point of view was the Sanidha evidence which has mopped up my oddities. There was the curious flask in *SCE* IV, there was a zigzag line with dots, there were scale patterns, there were loops, all of which did not fit in at all with regular White Slip II. They are all there at Sanidha, so if we want to know where those were made I think we can be pretty certain. Whether we can divide up WS II further I am not sure. Stylistically it is certain that the bar in the front and subsidiary decoration ought to be earlier, but is it? I would like to know the evidence for it being so. I am even more worried about the final stage. We have been shown several White Slip sherds from levels thought to date to *ca.* 1200 BC. If they are regular and not out of the ordinary, they are normal White Slip. How are we going to fit in White Slip III, chocolate White Slip, if it is a chronological thing, between that date and the beginning of, or at least early Maa? I am still not happy about White Slip III, that it is a purely chronological distinction and not necessarily, in part at least, a regional variation.

Karageorghis: A couple of hours ago Mervyn Popham gave me all his colour slides from his archive,

and he is also going to give us his negatives. I would like to thank him most sincerely for his generosity. We are going to put them in good order here at the Foundation, and this archive will be at the disposal of any of you who would like to make use of them. The slides include material that has been lost and is no longer accessible.

Åström: This symposium has provided much new material, many impressive new results from which I have profited. The general outline remains, but we have refined earlier results, and that is how it should be. Let me say that I find Kathryn Eriksson's division of PWS into three phases an excellent move forward. She has studied the stratigraphy of tombs – that is what we have – and I am absolutely convinced that you can make divisions there. That is also a step forward. There are many points upon which I could comment. For instance the new analysis of the technique is new to me; it was very illuminating and instructive. Oren's presentation of the evidence from Israel was excellent and very useful. There have been many excellent papers. One point that has not been mentioned concerns the conclusions reached by Kromholz in his thesis. We were talking about diffusion; his book has not been diffused enough to show his results which were that in the beginning WS II was diffused to the northern part of Syria and Israel and only in the later period to the south. I think perhaps that should be emphasized a little more. To continue: if the terminology which we worked out in the *SCE* does not appeal to people and if you want to change it, then do so as long as we are agreed.

Merrillees: I think the one thing that has impressed me most is that unlike, it has to be said, quite a number of other conferences which I have attended, this one has brought together participants who have contributed fresh information on a great many aspects of the White Slip problem, if we can call it that, both new and old information. For all of us this has been very much a learning experience, and in that respect it has meant that we are now going to take away with us a great deal of information that has not hitherto been accessible. This we will be able to digest and use in our own studies. It has also demonstrated one other thing, the value of having on an occasion like this the person responsible for classifying White Slip for the Swedish Cyprus Expedition, and Mervyn Popham's presence has enabled us to feel that this process with which we are dealing is itself not just simply personal but also historical and, therefore, can be addressed in a continuing manner. It is also equally valuable to have Paul Åström here because the relative chronology of the *SCE* and indeed the entire

system has not been replaced, challenged or supplanted by anybody who has come onto the scene in this area afterwards. I think that is a great vindication of the foresight both of the classificatory and the chronological systems which we all now adhere to. In that respect it seems to me that we have a particular advantage; if we are in any disagreement or uncertainty about how to apply a certain pottery name or a relative chronological term, we can turn to the *SCE* because it is the one basic frame of reference that we all use. But we have heard today that Palestinian archaeology, which I have known for years, is bedevilled with different relative chronologies. I must have been out of the system for some time since I did not even know that there existed such a thing as MB III, which I now realize, of course, is MB IIB and IIC. So I think we in Cyprus are extraordinarily well off. But it does mean that others who come here to try to understand our system must do it on our terms. I will end by making one point that is very important to us from that point of view: in so many other places which do not have a pottery classification system like ours, there is a tendency to use relative chronology to describe pottery wares, and that, I think, we do not do because we do not need to do it. In that respect we are very well off, and that is why I made my earlier point because I think it is most misleading in a cultural situation like that of Cyprus actually to put chronological labels on various artifacts, material sequences etc. that serve to confuse rather than enlighten. If you know your cultural sequence in the Bronze Age on Cyprus well, you realize that it is very much subject to regional variations. This is a critical point of understanding about all that you deal with here in the Bronze Age at least, perhaps a little bit less in the Iron Age, that the regional variations are the underpinning of an understanding of the civilization. Our terms should facilitate the clarification of those variations rather than obscure them.

Karageorghis: Michal (Artzy), the view from the east?

Artzy: Most of the discussion here has been on the earlier appearances of the PWS and WS I, and I would like to mention that there seems to be a great difference between what happens in the north and what happens in the south. We have been hearing about Tell el-Ajjul correctly compared with Tell el-Dab'a in the south; in the north we are speaking of Akko which, I think, is better than Tell Abu Hawam which did not exist at the time relevant to all of these questions about which we have been speaking. In Akko we seem to have a very different picture so far. We never really excavated the beginning of the Late

Bronze Age or, if you wish, the MB III. In other words, the pieces that we have tend to come mostly from graves, and the graves seem to yield no WS I. On the contrary, there is Cypriote bichrome ware, there is even Chocolate-on-White, there is Tell el-Yahudiyeh ware but no PWS or WS I. I think that although Ian (Todd) said we have spoken so much about the eastern side, we are missing quite a lot of what is going on in the northern part of that area in terms of the very early part of White Slip. However, if we go on to the WS II in the northern part of Israel and further north, we are finding out that we are dealing with large amounts of WS II. Although I hesitated about trying to divide the material into families or into areas of manufacture on stylistic grounds, I think it should be done. I think, for instance, the material from Tell Abu Hawam should be treated in this way and I am sure Jacqueline Balensi will do so. The same is true for Akko. These should then be compared to areas in Cyprus.

Karageorghis: Sophocles Hadjisavvas, the view from Cyprus?

Hadjisavvas: Last month I was asked by the organizers of yet another conference on Cypriote archaeology to summarize the results. I told them directly that we are not yet ready to have conclusions. New material is coming to light day after day; some very important excavations, including my own at Alassa, are not yet published. The Kouklia material is not yet published. I think that we must publish all the important excavations...

Karageorghis: Or perish!

Hadjisavvas: You know my ideas on that subject. Perhaps I am a bit stricter than you were in your time. I am also pressing my Cypriote colleagues to publish, not only the foreign colleagues. I think that we have to wait for all these sites to be published, and material which was excavated before and not properly published to be re-published in the light of the new evidence which we have. The sciences will also come to our help; in that way we are luckier than our predecessors. I believe that scientific analyses will be of assistance, especially with absolute chronology. The Department of Antiquities is ready to collaborate with any scientific centres that wish to undertake scientific research on finds from Cyprus. We are also ready to collaborate with any foreign institutions which want to come and work on the island. My impressions from this conference are excellent. I fully agree with what Paul Åström and Robert Merrillees have said about our meetings. It is yet another bridge on the road we are trying to construct on Cypriote archaeology.

Karageorghis: Kathryn (Eriksson), the view of the younger generation (not that others are not young!)?

Eriksson: It is wonderful to be in the company of so many experts on White Slip. The situation with regard to PWS and WS I is very interesting, and this is the area upon which I concentrated. I am also interested in WS II, and I would like to remark that in Egypt, at the settlement of Memphis at Kom Rabia, we do have WS II around the time of Horemheb. We also have White Shaved as well. Sturt (Manning) brought up the point why do we not have more White Slip in Egypt? Irmgard (Hein) suggested that we do not have enough excavated settlements, but we have so many tombs which Robert Merrillees detailed in his thesis of 1968. We have lots of Base-ring I, we have lots of Red Lustrous. Why the situation in Egypt is different from that in Palestine is a very interesting question but I do not have the answer as to why the patterns change. In relation to the chronology of these early wares, we have supported the view that Mervyn Popham put forward, and I would like to say that Pendaria and Akhera only have PWS; there is no WS I in these tombs, and Mervyn Popham classified all of these as PWS in the 1972 classification. At Pendaria there is no Proto Base-ring or Base-ring; at Akhera there is Proto Base-ring/Monochrome and this is supported by the evidence at *Toumba tou Skourou* Tomb III where WS I and Base-ring I are missing. This is supported by the stratified settlement material from the same site. Support is also forthcoming from the Maroni-Vournes sequence, and I was really interested to hear about the Hala Sultan Tekke evidence as well. One further point concerns Myrtou-Pigadhes and the

White Slip sequence there which we have not discussed. In the publication Catling only illustrated pottery from Periods III and IV. We do not know the nature of the White Slip from the early levels, and it is possible that there might be PWS together with the 97% of the Middle Cypriote type vessels. It would be instructive if we could look at that material again.

Karageorghis: It remains for me to express my gratification that you are not too displeased with the results of this conference. I thank you all for your participation, for your patience, for your wisdom, both those who read papers and those who took part in the discussion. I would also like to thank Linda Hulin and Anna Lekka who helped with the projection of the slides, the technicians who hopefully will give us many tapes which Ian Todd will take over. I am sure that you will agree that the one person who deserves particular thanks is my secretary Maria Georgiou. You have heard Manfred Bietak telling us the good news that the National Research Council of Austria has approved the whole project which he himself conceived. This means that we will go on with the project, and together with Paul Åström we have started thinking of the next conference which will be in Stockholm.

Åström: I am thinking of a meeting on Bichrome Wheelmade ware and Base-ring ware, one day for each, in October 2000.

Karageorghis: Those of us who may be optimistic will put down this date. On a serious note, the proceedings of this conference will be published, and you are urged to collaborate in the timely submission of your papers. We aim to achieve publication within one year of the event. Thank you all very much.