

The Fatal Shore, the Long Years and the Geographical Unconscious.

Considerations of Iconography, Chronology, and Trade in Response to Negbi's 'The "Libyan Landscape" from Thera: A Review of Aegean Enterprises Overseas in the Late Minoan IA Period' (JMA 7.1)¹

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The scope and broad-ranging coverage of the paper by O. Negbi (1994) deserves commendation. Few scholars today risk venturing from their secure specialisations. The paper harks back indeed to the days of Furumark (1950) and a previous generation who felt able to be general scholars of the eastern Mediterranean Late Bronze Age (LBA). Negbi has bravely sailed the Aegean, peered down into the caldera at Thera, crossed the eastern Mediterranean, stepped off her landing craft, and taken on a minefield. Sadly, however, despite the merits of her attempts at synthesis, her study is flawed and inadequate in many respects. At a general level, there is a strong temptation merely to note that this study belongs to the now discredited mode of pre-processual historico-

descriptive archaeology (critiques in the Aegean field by Renfrew 1980; Snodgrass 1985; Dyson 1981; 1993). But far more important are the weaknesses and gaps in the key discussions: a lack of familiarity with important data and arguments is apparent throughout, accompanied by a reliance on secondary literature, often not well chosen. While there is no single or 'correct' view of the past, nor approach to its study, scholarly rigour remains a requirement. A brisk and critical response to Negbi is necessary in order to prevent this paper being seen, incorrectly, to represent what it purports to be.

I. The Akrotiri Frescoes

By naming the frieze of the eastern wall of the West House at Akrotiri 'The Libyan

'Landscape' in the title of her article, Negbi indicates that she has accepted already 'the hypothesis that [the work in question] portrays a genuine landscape of a North African oasis'; yet the critical assessment of this fresco is stated as her aim in the ensuing introduction. Unfortunately, this is a true herald of things to come, as far as her article is concerned, for such logical failings escalate in the following discussion of the iconography of the 'Miniature Frescoes'. The full extent of Negbi's argument is that the fresco was painted by a Thera/Minoan artist (the two appear to be interchangeable here) who witnessed the landscape while travelling to Egypt as part of a tradition of palatial exchange of skilled craftsmen in the LBA Eastern Mediterranean.

Negbi's discussion appears to stem from the confusion (inherent in much of the secondary material she uses) between the archaeological view that the quantified and systematised study of the artefacts in question can tell us something about the producing society concerning past social, political and economic processes, or contexts or the 'analytical individual'; and the traditional art-historical view that they can tell us something – straightforward about a specific individual – the 'creative' artist, or the patron/consumer. Negbi adopts the latter view, with little regard to the former. The validity of such an assumption – which, amongst other things, forces the compression of processes into the temporal limits of fixed entities (e.g. generations), and ignores the social structuration involved in the production of art – has been criticised on both archaeological (Snodgrass 1985) and art-historical grounds (Hill and Gunn 1977; Wolff 1993). Likewise, whereas archaeology no longer regards art and artefacts as merely neutral or passive, but considers the functions and roles of such instances of embodied signalling in wider

Aegeocentric approach is unwarranted on present evidence. We cannot therefore assume that diplomatic exchanges of the kind discussed by Zaccagnini (1983) took place. Second, the distinction between the 'Aegeanising' frescoes of Tel ed-Dab'a (painted by Minoan palatial artists according to Negbi) and Kabri (painted by freelance Cycladic artists according to Negbi) is based on no evidence whatsoever (and, to follow the argument to its foundation, there now is evidence for Linear A administration at Akrotiri [as reported in June 1994 by Reuters]). If one were to pursue the very plausible scenario of the co-existence of different levels of interaction and different spheres of value, more carefully substantiated evidence would be required. If anything, the stylistic differences between Thera and Minoan wall-painting, which utilise many of the same iconographic conventions, would seem to indicate the existence of local 'schools' within a common ideational and ideological tradition, rather than widespread circulation of craftsmen.

One could even seriously question the starting point of the argument (the 'Libyan' fresco) on more than one count. The iconographic analysis of the Thera frescoes has been the subject of a number of studies. Negbi refers to and relies on several of these. This often leads to internal contradictions in her discussion of genre, reality and imagination in the landscape series. Some of the salient elements singled out in the course of these discussions make it impossible to discuss the Thera works in isolation from the wider world of Near Eastern iconography. Although Near Eastern 'borrowings' are often discussed by Aegean prehistorians, it is almost always on the assumption that the artistic rendering is a garbled message from which we can extract evidence of 'real' borrowings: a depiction of flora in an 'Egyptian' manner

signals the adoption of Egyptian landscape architecture, represented inaccurately by the artists through the distorting lens of 'style' (Shaw 1993). Negbi's variation on this theme falls into the same trap by ignoring the potential role of iconographic loans as part of the ideological dimension of culture contact (see below).

It could be argued, as for instance Morgan (1988: 146–150) has done, that the 'Landscape Frieze' typifies a certain Aegean genre of 'Nilotic Scenes'. However, Morgan's equation of this Aegean genre to similar Egyptian scenes tends to obscure the importance of such fantastic scenes outside their land of origin. The Thera landscape frieze and its few Aegean cousins can be seen as part of a 'Western' tradition of Nilotic fantasies (and indeed general 'Eastern' fantasies) (e.g. De Vos 1980; Curl 1994; Morris 1989: 529 and n.82; Said 1978). The inclusion of mythical creatures (in this case the griffin), the use of Egyptian iconographic devices (perspective), and the individual isolated motifs (papyrus, palm trees, aquatic fauna) often confused in their application, are common themes in the composition of such scenes. It is as if two distinct Egyptian genres, a garden scene and a fowling scene, are combined, but the king/nobleman as cultivator or hunter is absent. What in Egyptian art would have signified the ability of an elite to concentrate and breed exotic species from distant lands and to tame nearby nature, is here amalgamated into a representation of 'Egyptian' nature. All of this points to the conclusion that, far from being a frequent or even one-time visitor to Egypt, the artist based his/her representation on prevalent conceptions concerning a distant and infrequently visited land. As Morris (1989: 529) writes: 'The ultimate source for such scenes is not so much the Nile itself, but Aegean fantasies of Egypt'.

It is also clear, by their appearance in other media (Morgan 1988: 42-43), that certain elements of the scene (e.g. the cat-chasing-waterfowl) belonged to a wider contemporary symbolic system in which animals feature in heraldic and totemic roles, such as are indicated in the associated frescoes in the West House. In conjunction with the narrative elements, it is plausible in this context that such symbolic meanings were part of a contemporary oral tradition (heard especially in elite social contexts), and perhaps one owing much to Near Eastern epics, as has been suggested by Morris (1989; see also Hiller 1990). The position of the frescoes in 'private' (upstairs) space would have restricted their audience to a selected few within the community. This would seem to imply that 'reading' the images on the walls demanded some 'esoteric' knowledge to which only a few had access. Among the components of this knowledge hinted at by the compositions are the specific meaning of the 'borrowed' motifs (not always the same as their meaning), and also the general knowledge bestowed by travel to distant places – as Negbi herself hints – a mental map of the known universe. This is therefore not the depiction of a travelling artisan's experience, but some culturally significant sign(s) or experience like that depicted in the West House frescoes. Maps, images and signs offer a restricted, exclusionary, vocabulary; they offer and embody forms of knowledge and so forms of power within a society (Harley 1988: 287-80; Baudrillard 1981; Bourdieu 1984). This is particularly clear in the case of foreign, or exotic signs and knowledge (Helms 1988; Greenblatt 1991). The representation of landscape in art is also far from neutral, nor simply a case of some polarity (e.g. accurate or inaccurate); instead, it too is a highly political process with important social implications (Daniels and Cosgrove 1988: 4-8). The

suggestion that participation in these experiences was exclusive tells us more about Thera social strategies using the idea of overseas contact, than about the precise mechanisms of that contact.

II. Chronology

Chronological matters feature regularly in Negbi's discussion, but the key for her is the dating of the Late Minoan (LM) IA period. She argues that the so-called 'high' chronology is not supported by the evidence. However, her case does not stand up to scrutiny. At the same time, this is a welcome opportunity (also see Manning 1995) to update and revise earlier views expressed by one of the present authors (Manning 1988). In particular, the relative position of the Thera eruption is now seen as within mature LMIA, and not at its end (see below), and the chronology of the LMIB period has been rethought (noted as a problematic area in Manning 1988: 57).

Negbi acknowledges that finds of Thera tephra or pumice from the Aegean demonstrate that the eruption occurred before the LMIB period (i.e. in LMIA), just as the final ceramic assemblage buried by the eruption is also mature LMIA (Niemeier 1980; Marthari 1990). However, she disputes the view of Warren (1990/91: 30; Warren and Hankey 1989: 214), based on Lolos (1987; 1990), that the eruption may be placed before the end of the LMIA period because of the absence at Akrotiri of the latest class of Late Helladic I pottery. Negbi (1994: n.5) states that this cannot be confirmed due to a lack of LM/LHI vessels in dated contexts in Egypt. This is absurd. Problems of timelags in transmission and deposition would never permit Near Eastern data to resolve such an issue; instead, it is the clear and substantial relative chronology in the Aegean which offers our evidence. Further, the evidence

amassed by Lolos is confirmed by the data at Phylakopi, where Thera tephra first appeared before the end of the LMIA period (Davis and Cherry 1984; 1990: 193-96). Thus we can place the eruption of Thera during mature LMIA, but before its end. Since a clear post-Thera LMIA horizon has yet to be recognised anywhere in the Aegean (although there is a possible abandonment phase at Trianda: Marketou 1990; and work at Palaikastro may offer hope: MacGillivray *et al.* 1992: 134-37), the length of this post-Thera LMIA phase is unknown. The 45-55 years employed by Warren and Hankey (1989: 215), who regard the eruption as 'well before the end of LMIA', may be too many, about right, or too few.

The relation between the eruption of Thera and the finds of pumice at Tell ed-Dab'a requires scrutiny. The evidence is that some probable Thera pumice has been found at Tell ed-Dab'a dating to the early 18th Dynasty (after Ahmosis and before Tuthmosis III, according to Bietak). Thus the eruption of Thera occurred at some time *before* this dated context. But the length of the 'some time before' is unknown. This pumice is not from contemporary airfall deposition from the eruption itself. It has been transported by some means (natural or human) to Egypt in the period after the eruption. In the Aegean, in the ceramic periods following the eruption of Thera, pumice from the eruption has been found in several similar workshop/craft-use contexts. Many of these contexts are several generations to a century or more after the period of the eruption (e.g. Faure 1971; Pichler and Schiering 1980: 26-30; Evelyn 1984: 290 nn.33, 35). The appearance of the pumice in Egypt does not date the eruption, but the exploitation of this post-eruption resource. This might have been exploitation of eroded and naturally drifting Thera pumice washed up in Egypt, but is perhaps more likely to be exploitation of pumice

acquired by people in the Aegean and then traded as a craft resource. The appearance of this new craft resource in early 18th Dynasty Egypt might then be linked with much other evidence for cultural contacts with the Aegean region at this time (and especially with the LMIB/LHIIA Aegean). The eruption occurred at an unknown earlier point.

The date of the Thera eruption, and the date for the LMIA period, must therefore be derived from other sources. Negbi simply ignores the 'natural scientists', and explores some circular archaeological and stylistic linkages involving Alalakh, Tel Kabri and Tell ed-Dab'a. Here there is no certainty, as Negbi concludes. All we can say is that frescoes with reasonable stylistic similarities to LMIA examples in the Aegean (although noting the lack of MMIII and LMIB comparanda) derive from contexts within the Hyksos era. Whether the linkages are with late, or early, or mature LMIA is unknown. Similarly, since there is evidence of reuse of plaster and thus for phases of paintings at Tell ed-Dab'a, even if the last fresco phase belongs to the late Hyksos period, the earlier frescoes may be placed in the mid- or early Hyksos era. The lack of LMIA/LHI objects or influences in anything but early 18th Dynasty contexts in Egypt (Kerna sherd: Warren and Hankey 1989: 138; Kom Rabia sherd: Warren and Hankey 1989: 139 [if LMIA; it is in fact probably LMIB: see below]; tomb of Aahotep: Niemeier 1985: 185 and n.1435), and the indications already from Akrotiri that the LMIA period at least began during the Second Intermediate Period (Warren 1979: 106-07 and n.2), might suggest indeed that LMIA had ended by the early 18th Dynasty at the latest, and was largely contemporary with the Hyksos era.² If the earliest possible date for the stratification of the Khyan lid at Knossos is LM I based on the associated stone vases (Niemeier 1994: 73 and n.21), then this too

may offer further support. Objects bearing the name of Khyan also come from Boghazköi, Baghdad, and Palestine (Negbi 1994: n.12), so a direct (contemporary) trade or diplomatic contact with Knossos is plausible (in LMIA, after the major second phase of New Palace construction during the MMIII/LMIA transition: Niemeier 1994: 83-84).

The date for the close of the LMIA period is not clear at present, as Negbi observes. Cline (1994: 5-6) argues that a converted 18th Dynasty baggy alabastron found in a later LHI context in Shaft Grave V at Mycenae (Warren 1969: 104, MN829) provides a minimum terminus *post quem* for the close of the LMIA period early in the 18th Dynasty; however, Hankey (1987: 43) has already stated that this vase (given there as Warren 1967: 44 no. Q2, pl. VIII.Q2; cf. Cline 1994: 201 no.597 and bibliography) is 'a drop vase of Middle Kingdom date, not, as stated, XVIIIth Dynasty'. Hence it is excluded here (and was accordingly not discussed in Manning 1995).

Some LMIB or possible LMIB material from Egypt, and other links to the Near East, might be argued to offer synchronisms with the Hyksos or early 18th Dynasty, but this is disputed or unclear (see e.g. Kemp and Merrillees 1980: 252-54; Manning 1988: 32-36; 1990a: 32-33; 1995: Appendix 8; Niemeier 1994: 73-74 and n.32; Warren and Hankey 1989: 138-42; Warren 1990/91: 34). The Kom Rabia Sherd (Warren and Hankey 1989: 139), probably of LMIB date (with a careless or degenerative foliate band motif ['sub-LMIA' = LMIB: Niemeier 1980: fig. 17] and perhaps from a LMIB bridge-spouted jar?) (a LMIB date is also favoured by Prof. P.P. Betancourt and Dr. K.O. Eriksson: pers. comm.), probably offers evidence for an early 18th Dynasty terminus *ante quem* for some point in LMIB. Other mature LMIB (as found in the LMIB destructions on Crete) or later LHIIA material offers synchronisms with the

reign of Tuthmosis III. Similarly, the Red Lustrous wheel-made ware spindle bottles from Gournia and Kommos offer a likely correlation for later LMIB with the Late Cypriot IA.2-IB periods of the earlier 18th Dynasty in Egypt (Cline 1994: 214 nos 717, 718; Eriksson 1993: 57, 96-98, 136-37).

Faced with this uncertainty, we may turn to the close of the LMIB period. Representations of *Keftiu* (Vercoutter 1956; Wachsmann 1987), who are widely assumed to be Aegeans (e.g. Sakellarakis and Sakellarakis 1984; Knapp 1985; but cf. Furumark 1950: 223-46; Strange 1980; Merrillees 1972; 1982), painted before Tuthmosis III Year 28, look like LMI Minoans (Wachsmann 1987: 43; Warren and Hankey 1989: 145). This does not necessarily mean that LMI Minoans visited Egypt until this year, but that the current copybook image of Aegeans in Egyptian Thebes was of LMI style until then (see Wachsmann 1987: 12-55). In contrast, the infamous Aegean figures in the Mencheperresonb tomb, and as repainted in the Rekhmire tomb, are dressed like those in the (likely) LMII Procession Fresco at Knossos, and carry LHIIb-style metal vessels (Wachsmann 1987: 44; Warren and Hankey 1989: 145; H. Matthäus, public lecture, London, 1994). And, indeed, the designs on the kilts, and some of the other painted designs in these tombs, in fact best date to the LMIIIA.1 period (Barber 1991: 330-51, esp. 347-48; Prof. P.P. Betancourt pers. comm. and paper in prep.); one may also add the Katsampas stone vase and the Aniba alabastaron (Manning 1988: 34, 36). The decoration of the Mencheperresonb tomb appears to have been the earlier, and so suggests the painting of LMII-III.A.1 style figures by the last decade of the reign of Tuthmosis III. Now this painting need not correlate with the very start of the LMII-III.A.1 period in Crete. We do not know how

often diplomatic visits were made to Egypt by the *Keftiu* (Wachsmann in fact suggests just two such missions), so the LMII-III.A.1 period may have started a few years, even a decade or so, earlier than this painting (conceivably, on a minimalist model, the LMI 'image' could have been painted in Egypt in the later part of the first half of Tuthmosis' reign even after LMII-III.A.1 had actually begun in Crete, but before the next *Keftiu* mission to the Pharaoh).

All the painting shows is that, when painted, LMII-III.A.1-style Aegeans were by then the contemporary copybook image. Nonetheless, despite the quibbles and the potential range of the exact correlation, it would appear likely that the LMII-III.A.1 period began on Crete somewhere between the middle to later part of the reign of Tuthmosis III (with LMII during the main part of his reign, and LMIIIA.1 beginning at least a decade or so before its end). In turn, the later LMIB period may be correlated with the first part of the reign of Tuthmosis III. The beginning remains problematic. In the past, many scholars regarded the LMIB period as a short one, but recent archaeological evidence appears to suggest that it was rather longer. A beginning for the period might therefore be suggested from around the start of the 18th Dynasty, or during the earlier 18th Dynasty, depending on such opinions.

A chronology which has LMIA commence in the earlier 17th century BC and close in the first half of the 16th century BC, and LMIB follow from there until during the first part of the reign of Tuthmosis III, is compatible with a range of scientific evidence, whereas a 'low' chronology is not. The eruption of Thera was a very large volcanic eruption, and is likely to have left some indication in at least one of the ice-core or tree-ring records from the northern hemisphere which provide evidence of major explosive volcanic events over the last several

thousand years (Manning 1992a and re This is why it has been suggested that a var of signals in the later 17th century BC could be the Thera eruption, with 1628 BC preferred the best bet (Manning 1992b and refs.). agreement, a new ice-core has again found volcanic signal suggested to be Thera c.1623 BC (Zielinski *et al.* 1994: 950, Table What is missing in all of these records is very large, or significant, volcanic signal the 16th century BC or shortly thereafter. Thus it seems unlikely that the Th eruption occurred during this period. This not to say that there is yet a definite or direct link between the Thera eruption and records from the later 17th century BC, an is of course possible to dispute the link between Thera and any one of these records (e.g. see Manning 1990b: 91-92 and re However, we can say that, whereas the late 17th century BC offers a suitable period for very large volcanic eruption, the 16th century BC looks unlikely.

A large body of radiocarbon evidence from Akrotiri on Thera would also support a similar conclusion, although critics can and will raise the issue of a possible volcanic effect due to the potential incorporation of volcanic carbon dioxide into samples. This is despite the fact that such an effect seems unlikely to have widely and consistently affected different plants from different growing locations, given evidence that such effects are most notable only near fumaroles, and given the widespread nature (rapid atmospheric mixing) of Thera island. Further, the presence of volcanic 'decarbon' in the archaeological samples should be detectable through differing or atypical $\delta^{13}C$ measurements (Hubberten *et al.* 1990: 18). This is not the case where known. There is also the cloud of doubt concerning the meaning and reality of the 'contaminant' debated between the Simon Fraser and Oxford laboratories (Housley *et al.* 1990: 21).

13), and the worrying fact that many of these (and other Akrotiri) samples so far dated were soluble in NaOH (Nelson *et al.* 1990: 200; Housley *et al.* 1990: 209), and thus 'unsafe', since the full and effective removal of contaminating humic matter is impossible in such circumstances. The latter may well account for the former. The alternative ultrasonication procedure employed by Simon Fraser 'can hardly be considered a safe extraction method for humic matter' (Friedrich *et al.* 1990: 193), and the Oxford laboratory concluded that the technique 'was simply a mild way of dissolving the whole sample [not the objective], rather than a procedure which was removing a specific contaminant' [the objective] (Housley *et al.* 1990: 210). Therefore, of recent determinations, only those from the Copenhagen laboratory, which were carried out solely on carefully selected, fully carbonised, short-lived samples from the final volcanic destruction and $\delta^{13}\text{C}$ corrected, relate definitely to the last human occupation at Akrotiri in mature LMIA shortly before the eruption (Friedrich *et al.* 1990: 193-95), may be considered as unequivocally 'good'. The Copenhagen determinations clearly favour a 17th century BC date for mature LMIA (and the eruption of Thera). The consistent weighted average of the determinations is 3355 ± 32 BP (Friedrich *et al.* 1990: 195), which offers, at 1 σ , an 87% probability for a calibrated calendar date within the range of 1680-1605 BC on the bi-decadal calibration curve, and a 72% probability for a calibrated calendar date within the range 1688-1601 on the decadal calibration curve (Stuiver and Reimer 1993).

Radiocarbon is also about to impinge on the chronology of the subsequent LMIB period (work in progress by R.A. Housley, G. Cadogan, S.W. Manning, R.E. Jones and R.E.M. Hedges). New determinations from Crete suggest a date of ca. 1490 BC for the end

of LMIB, consistent with the archaeological evidence discussed above. The beginning of the period must lie in the earlier to mid-16th century BC.

In sum, archaeological and scientific evidence is compatible with a LMIA period beginning in the first half of the 17th century BC, and finishing in the earlier to mid-16th century BC (exact point unknown at present; this depends on the exactitude of the correlation between the 18th Dynasty 'historical' chronology and the New Kingdom art-historical and stylistic sequences, and the date chosen for the beginning of the New Kingdom [and against the necessity of a 'low' choice, see Ward 1992]). The LMIB period, notable for its contact with earlier 18th Dynasty Egypt, then follows.

III. Trade and Exchange

To the seemingly trivial end of determining whether the painter of the West House scenes was portraying something experienced first-hand or a product of his [sic] imagination, Negbi attempts a comprehensive review of recent literature and evidence regarding political, cultural and commercial interaction, including the movement of individual artists between regions of the LBA Eastern Mediterranean. Specific attention is focused on the possibility of direct relations between Egypt and Neo-palatial Crete.

It is an understatement to say that problems abound in Negbi's discussions of trade and exchange. There are numerous factual errors or weaknesses in logic, but it appears more useful here not to list these in a pedantic fashion, but instead to discuss the broader topic.³ In particular, in the sections of her paper devoted to LBA trade, Negbi fails to articulate a coherent model of LBA trade in the eastern Mediterranean, and follows, instead, a mode of unfocused generalising description in which 'factual' account is

interspersed with occasional secondary theoretical discussion. The lack of critical evaluation does not assist the reader in determining which ideas and opinions are Negbi's, and which derive from her extensive reading of the literature. Negbi is correct that the LBA witnessed more 'complicated patterns of interaction'; however, this requires greater emphasis and explanation, with, for example, discussion of the differing levels, spheres and modes of interaction and circulation of both material and non-material goods.

The paper is completely silent on virtually all the major debates of the past century regarding the nature of, and practice of, trade in prehistoric times. At the most fundamental level, there is a need for explicit definitions of terms such as trade, exchange, and so on, given the large baggage of connotations associated with these words. Negbi does not consider the positivist/formalist v. substantivist/primitivist debate (e.g. Sherratt and Sherratt 1991: 351-56; Curtin 1984; Finley 1985; Snodgrass 1991; Garnsey *et al.* 1983; Hodder 1982b; Dalton 1961; 1975; Polanyi *et al.* 1957), nor related debates concerning the divides between gift, barter, and commodity exchange (Appadurai 1986), or the relative importance of preciosities versus bulk trade (e.g. Schneider's 1977 critique of Wallerstein 1974; also Sherratt and Sherratt 1991); worse, at varying points Negbi happily subscribes to contradictory views (e.g. non-substantivist Sherratt and Sherratt versus substantivist Zaccagnini [see esp. 1987]). There is no examination of specialised 'exchange', nor of the role and ideology of such long-distance trade (cf. Helms 1988; 1993). The lengthy discussion by Negbi of 'navigational dilemmas' assumes that the balance of investment and gain (which only exist in monetised economies) are the deciding factors in long-distance travel and exchange.

At the specific, or micro-level, modes of exchange are not considered. The movement of ideas and styles without the necessary movement of people is not discussed. The specific case of actual art images and artists being exchanged is one rich in possibilities, and an extensive literature could have been discussed. Whereas exchange of some objects or resources might be dismissed under the categories of 'action at a distance' (Renfrew 1975), exchange of the signing practices and codes themselves is a very different matter. Archaeology has lately become very aware of the ideological and ideational aspects of long distance trade (Helms 1988), and of the social construction of values and meanings in trade

Problems of differential survival and recovery affecting archaeological and literary evidence on LBA trade, leading amongst other things to the bias towards prestige goods, are never acknowledged by Negbi. Social theory, and a view of trade and commodities beyond the materialistic level, or the trait, is entirely absent. At the macro-level, the relations between the Aegean, east Mediterranean, and the Near East, fall into the ambit of 'world systems' theory, or core-periphery analysis (Kohl 1989; Frank 1993; Sherratt 1993; Edens 1992; Algaze 1993; Rowlands *et al.* 1987; Chase-Dunn and Hall 1991; Hall and Chase-Dunn 1993), and other models and theories of interregional interaction and social, political and economic change (see among much literature e.g. Schortman and Urban 1992; Sherratt and Sherratt 1991; Scarre and Healy 1993; Renfrew and Cherry 1986; Brumfiel and Earle 1987; Hall 1985; Liverani 1990; Junker 1990; Knapp 1993; Hardt *et al.* 1988; Ericson and Earle 1982; Earle and Ericson 1977; Melas 1991a; Sabloff and Lamberg-Karlovsky 1975; Renfrew and Bahn 1991: 307-38), but Negbi offers no discussion, despite the clear evidence to her themes of Aegean-Near Eastern relations and royal ceremonial exchanges.

(Appadurai 1986; in an Aegean context, see Melas 1991 a; 1991 b). There seem at least two possible avenues to develop here. The first is that such exchange among rulers of kingly crafts and their producers is a special form of the exclusionary political economy described by Baudrillard (1981; also see Bourdieu 1984). In this case, the specific semiotics of the images are not necessarily directly or initially important. Moreover, it may be the actual travelling or distance to foreign lands which was important. The second is that the language of the specific images is desired. The images either have a cultural value in the recipient world (exotic, specific role, etc.), or are part of a valued signing system (for example epic poetry as recited at elite feasting occasions) common to a number of elites (consider Morris 1989). The most striking failure is the lack of theoretical discussion on the very model of artisan exchange which Negbi proposes.

In the light of the rich evidence regarding interregional interaction in the LBA (Cline 1994), it is particularly regrettable that, all too often, Negbi's arguments consist of a meandering pastiche of direct quotations or insufficiently incorporated citations uncritically accepted from other scholars. In particular, the available evidence allows for greater chronological subtlety than Negbi's collage of 18th to 13th century BC sources implies. Within her article lies the suggestion of what a successful study might be, for example in the contrasting patterns or evidence relating to different types of exchange or differing types of influences and artefacts over different periods between the Aegean and the Near East (for example the contrast between a very small number of LMIB/LHIIA imports to the Near East to select mortuary or religious contexts, versus the active LHIIIA Mycenaean expansion and exportation of commodities such as perfumed olive oil in the ubiquitous stirrup jars). However, this was not to be.

Notes

- 1 Apologies to Robert Hughes for borrowing from the title and chapter headings of his book *The Fatal Shore* (1986). Where Negbi's 1994 JMA paper (hereinafter all references to Negbi refer to this article) provides adequate references they have not been repeated here (e.g. to the frescoes and sites at Akrotiri, Alalakh, Tel Kabri and Tell ed-Dab'a). The present authors note that they had not seen *Ägypten und Levante* volume 4 at the time of writing. This paper was composed 'in the field' at Maroni, Cyprus, by the named members of the 'Tsaroukkas, Mycenaean and Trade Project' during July-August 1994; the forbearance of other team members is appreciated.
- 2 The find of a Late Cypriot I Base Ring jug in a secure Second Intermediate Period context at Kom Rabia also confirms that this Cypriot period (contemporary with LMI) began during the Hyksos-Second Intermediate Period. (Merrillees 1992: 50-51; Bourriau 1990).
- 3 In order to avoid the charge that this statement is an assertion, a few of the failings in the construction of Negbi's uncritical and discursive discussion are highlighted here. (a) Her lengthy views on 'navigational dilemmas' are presented as being as valid as a few key assumptions or a rationale. News that 'during every month of the year one can encounter favourable winds for every direction that one chooses', ignoring the obvious overall patterns of weather and society which dictate sailing seasons in pre-modern circumstances in the Mediterranean, thus merits more attention in her discussion than issues of actual quantified imports and exports. (b) Rigour and structure come and go. After referring to Zaccagnini through the work of Niemeier (1991), and noting, at least via Niemeier, that Zaccagnini examined a special gift-giving framework at royal level and involving the exchange of kingly crafts (see also Helms 1993), Negbi then happily suggests that it is tempting to accept from an 'immediate impression' and by 'judging from distinctive Knossian scenes' that 'close ties may have existed between the royal

families at Knossos and Avaris'. Never mind the lack of a known royal family at Knossos; or the lack of comparative material apart from a couple of frescoes at Knossos; or the 'certain peculiarities' in the Tell ed-Dab'a frescoes; or the fact that there are no definite LMIA Knossos comparisons – only a hypothesis of a painting tradition, or the plentiful evidence of Egyptian (and Anatolian-Near Eastern) bull cults. However, in contrast, another paragraph ends by referring to Hankey's (1993) view and stating that 'it is premature to attribute the Tell ed-Dab'a fragments to Minoan artists...'; yet by the end of the paper we are told of 'evidence attesting to the presence of Aegean artists at the palaces of Alalakh, Kabri, and Tell ed-Dab'a'.

(c) Chronology and context rarely meet. Negbi wishes to argue that Hyksos rulers and their contemporaries had contacts, even a marriage alliance, with LMIA Crete. There is, however, a critical absence of LMIA/LHII items in the Near East at this time, which Negbi explains away by references to the 'large number of Egyptian stone vases from Knossos...', apparently oblivious to the fact that the vases in question are (earlier) 18th Dynasty and mainly from LMI-II contexts. (d) Logic and structure simply do not exist. Data from many centuries are enmeshed together; reviews on the Hyksos, Crete, and Cyprus leave us little the wiser on their modelling in political, social or economic terms; and, after no explanation at all (and in fact a confused summation of the situation at Enkomi), we are suddenly told that: 'Archaeological and textual data thus seem to indicate that the "formative phase" of long-distance maritime trade... was based mainly on the provision of high-valued raw materials (copper and tin) in exchange for high-valued manufactured goods (luxury pottery vessels and artefacts of precious metals)'. But, just when one might hope for a change in approach and some consideration of economic theory or processes of regional interaction, we are presented only with a wandering discussion of debates about various types of artefacts or categories of evidence.

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