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Tell el-Dab^ca and Levantine Middle Bronze Age Chronology: A Rejoinder to Manfred Bietak

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Manfred Bietak's excavations since 1966 at Tell el-Dab^ca have brought to light the ancient Hyksos capital of Avaris. The material culture of the early, "Asiatic" level in Strata G-F has significant correlations with Palestinian MB IIA. Bietak's "ultra-low" chronology, however, is 50 to 125 years too low. This paper challenges his latest (1984a) evidence for absolute dates and defends the "middle chronology."

Since the appearance of the first preliminary reports of Manfred Bietak's excavations from 1966 on at Tell el-Dab^ca in the Eastern Delta, now identified certainly as the long-lost Hyksos capital of Avaris, the site has increasingly been recognized as being of major significance for Levantine interconnections.¹ It has been clear almost from the beginning that the material culture of the lower levels—Strata H, G, and F—is "Canaanite" (in my opinion specifically Palestinian) in character and represents an early Asiatic settlement in the Delta that is fascinating because it is "pre-Hyksos." Specialists have pointed out that in archaeological terms the material ranges from Palestinian early MB IIA through an intermediate phase that several scholars call "transitional MB IIA/B." Bietak himself has acknowledged that this relative assignment of the material is correct. In most recent treatments, however, he has drastically lowered the absolute dates of Dab^ca Stratum E3 and on that basis has sought to adjust the entire Syro-Palestinian archaeological-chronological framework downward by as much as 125 years (especially for MB IIA).

Bietak's view in several synthetic works (1981; 1984b; and especially 1984a) have been received with some skepticism, pending fuller publication of the data upon which he bases his dating; meanwhile I had challenged his "ultra-low" chronology (Dever 1985).² The symposium reported in this issue of the

Bulletin has brought this extremely important controversy further into the open. In what follows I shall try to update the discussion, with specific reference to Bietak's latest and fullest publication (1984a), which seemed reflected without substantial change in his remarks at the symposium on "Egypt and Canaan in the Bronze Age," in Chicago, November 1988.

The most efficient way to grasp the essential problem may be to outline our respective views in chart form (fig. 1).

NEW EVIDENCE FROM TELL EL-DAB^cA

Beitak's absolute dates (1984a) differ from his previous dating scheme (Bietak 1981) only by giving what is apparently a "median date," rather than a chronological range, for each stratum; and by having a stronger tendency to prefer the "low" absolute Egyptian chronologies of Helck, von Beckenrath, and Hornung, or of Krauss (see also Dever 1985; Dever in press; Bietak 1984a: 472-73, references; Ward in press). Since I have already challenged (Dever 1985) the evidence for those dates as presented in Bietak's earlier treatments, here I shall simply summarize the argument while concentrating more on the new data he brings forward.

In Bietak's latest publication (1984a: Table 2) he lists nine "datable objects" that can be correlated with Tell el-Dab^ca Strata H through D/2. I shall

Strata of Tell ed-Dab^ca, with comparative phases and chronology.

<i>Dab^ca</i> (Tell A)	<i>Character</i>	<i>Palestine</i>	<i>Bietak</i> (1984)	<i>Dever</i>
H	Open Settlement, Eg. MK material; ruined at end	Early MB IIA (12th Dynasty)	<u>1750 B.C.</u> 1720 B.C.	ca. 1950–1875 B.C.
G/4–2 ↓	Dense settlement; cist-tombs; Palestinian material	Late MB IIA (12th Dynasty)	<u>1710 B.C.</u> 1690 B.C.	ca. 1875–1825 B.C.
F ↓	New town plan, numerous tombs; Palestinian material dominates; tr. to “Hyksos”	Trans. MB IIA/B (13th Dynasty)	<u>1680 B.C.</u> 1660 B.C.	ca. 1825–1775 B.C.
E/3	Consolidation of “Hyksos” town; Canaanite temples; first Asiatic rulers?	Early MB IIB (13th Dynasty)	<u>1640 B.C.</u> 1620 B.C.	ca. 1775–1725 B.C.
E/2	Large settlement; temples continue; major “Hyksos” period	Late MB IIB (13th Dynasty)		ca. 1775–1675 B.C.
E/1	Houses overflow; cemeteries and sacred area continue; = “Hyksos” 15th Dynasty	Trans. MB IIB/C (15th Dynasty)	<u>1600 B.C.</u> 1590 B.C.	ca. 1675–1625 B.C.
D/3	Increased occupation; burials below houses; = mid. “Hyksos” rule	Early MB IIC (15th Dynasty)	<u>1570 B.C.</u> 1560 B.C.	ca. 1625–1575 B.C.
D/2	Little Yehudiyeh ware; tombs with Eg. material; wholesale disruption at end	Late MB IIC (Late 15th Dynasty)	<u>1540 B.C.</u> 1530 B.C.	ca. 1575–1525 B.C.
D/1	Erosion during gap; 18th Dynasty retaining wall	Trans. MB IIC/LBI	Post- 1540 B.C.	ca. 1525–1475 B.C.
B	Temple refortifications of 18th–20th Dynasties; Ramesside “Per-Ramesses”		ca. 1310– 1080 B.C.	
A	Small Ptolemaic settlement		3rd century B.C.	

examine those objects with reference to both Tell el-Dab^ca and the archaeological phasing of Palestine, which Bietak holds to be dependent upon his “fixed” dates in Egypt.

1. *Cylinder seal*: from Area F, Stratum D/1, roughly contemporary with Tell A, Stratum H. Bietak adopts Porada’s (1984) preferred date of “18th century B.C.,” supported by R. H. Boehmer (Bietak 1984a: 478, n. 1). But comparative art-historical considerations, even when advanced by as great an authority as Porada, do not yield a fixed chronology without external controls. Porada’s argument rests largely upon the familiar “duck-bill” axe of Syro-Palestinian type portrayed on the seal, for which she cites as parallels a miniature from pre-Hammurabi destruction levels at Mari; an example on a seal in the British Museum, dated

by Dominique Collon on stylistic grounds to the 18th century B.C. (Collon); and finally on two examples of “duck-bill” axes in the “Tomb of the Lord of Goats” at Ebla, which she apparently would date to the 18th century B.C., our MB IIB. Yet Porada ignores the overwhelming evidence of specialists that wherever we have controlled contexts in Palestine this type of “duck-bill” axe is confined exclusively to the MB IIA (or the “transitional MB IIA/B” phase at latest), being replaced in MB IIB by the socketed or “shaft-hole” axe (see Oren 1971; Dever 1975; Gerstenblith 1983; Porada cites none of these). As for the “Tomb of the Lord of Goats” at Ebla, Matthiae dates it to his early Mardikh IIIB, which he thinks begins ca. 1800 B.C., although he places the tomb itself in mid-MB IIIB, ca. 1750 B.C. However, the pottery in that tomb is largely

reminiscent of MB IIA styles, as Matthiae himself acknowledges, and thus it need not fall later than ca. 1800 B.C.; some of the metals are even earlier.³ Even if one could document “duck-bill” axes in the early to mid-18th century B.C., their main *floruit* is earlier, in the classic MB IIA period, contemporary with 12th Dynasty contexts in Syria–Palestine. In any case, the example of the axe on the Tell el-Dab^ca seal offers no help. It does not itself yield an absolute date (certainly not 13th Dynasty, as Bietak’s use of it would require); and at best it gives only a *terminus post quem* of ca. 2000–1800 B.C., since that seal, like so many others, may be an heirloom. On present evidence, while the seal would fit better with my date of ca. 1950–1875 B.C. for Stratum H than with Bietak’s ca. 1750–1725 B.C., this piece of evidence remains inconclusive.

2. *Scarab with lotus back*: Stratum G/1–2. Bietak dates these scarabs to ca. 1750–1650 B.C., i.e., 13th Dynasty, citing Tufnell (1970), O’Connor (1974), and Weinstein (1975). But William Ward, Tufnell’s collaborator and perhaps the foremost authority on Egyptian scarabs, has now argued persuasively that lotus-back scarabs are found in MB IIA burials in Palestine and thus can predate Dynasty 13.⁴ In any case, the dating of lotus-back scarabs is still too controversial, too dependent upon subjective typological criteria, to offer the precision that Bietak assumes.

3. *Scarab with private name*: Stratum F. Bietak takes this to be a 13th Dynasty scarab and dates it to ca. 1800–1650 B.C., but he cites no evidence. Whatever the actual date, this scarab is irrelevant, since it could be used in support of either Bietak’s date of ca. 1680/1660 B.C. for Stratum F, or mine of ca. 1825–1775 B.C.

4. *Scarab with human representation*: Stratum F. Bietak (1984a: 478) asserts that this scarab has a corrupt inscription that could be read *rh-nj-ntr*; but it is unpublished, and there is no way of knowing how Bietak derives his date of ca. 1700–1550 B.C. Such data cannot be regarded as definitive.

5. *Scarab with the name of Sebekhotep*: Stratum E/3. This scarab was published in a photo in 1970. At one point, Bietak (1981: 254) specified “Sebekhotep IV,” whose date, ca. 1720 B.C., would fit his date for Stratum E/3. In 1984, however, he no longer regarded the reading as certain. J. Weinstein concludes that “the name of this scarab could belong to any one of several rulers having the nomen Sebekhotep” (personal communication 23 February 1988). Again, this scarab offers none of

the chronological certainty that Bietak implies: and even if it could be assigned definitively to Sebekhotep IV, it would fit with either his dates or mine for the range of Stratum E/3 (see fig. 1 here).

6. *Rdj-R^c and ^cnr^c scarabs*: Strata E/1–D/3. Bietak regards these scarab types as purely “Hyksos,” i.e., 15th Dynasty, and thus dates them 1650–1540 B.C. In support of this view, he cites O’Connor (1974) and Weinstein (1975). However, Weinstein does allow for earlier occurrences (see above, n. 4). Moreover, Ward (1987: 523–26) has recently challenged O’Connor’s original attribution, upon which Weinstein had built, arguing persuasively that these so-called “Hyksos” scarabs are in use as early as Dynasty 12/MB IIA contexts in Palestine. Meanwhile, O’Connor (1985) has published his own typology and dating scheme for the scarabs in more detail, with reference to Tufnell (1984) but not having seen Ward (1987). Since the date of those so-called “Hyksos” scarabs is still hotly debated, they cannot simply be cited as definitive evidence for the placement of Tell el-Dab^ca Stratum E/1. (Bietak says they are “common” from Stratum E/2 onward.) Meanwhile, even his low estimate of 1650 B.C. would not contradict my date of ca. 1675–1625 B.C. for Stratum E/1.

7. *Scarab with name of Sesostri I*: Strata E/1–D/3. Since Sesostri I’s dates are ca. 1971–1928 B.C., this scarab is obviously an heirloom and contributes nothing to the present discussion.

8. *Two bronze plates with deliberately mutilated images of Neferhotep I*: Stratum D/3. Bietak correctly dates these plates after 1725 B.C., but there is no way of knowing how long after, since they are in secondary use in Stratum D/3. No matter how one views them, they are useless except to establish a very broad *terminus post quem*.

9. *Scarabs of ^cnr^c type or with tripartite patterns*: Stratum D/2. Bietak dates these scarabs to 1650–1540 B.C., but, as my discussion indicates, they have a longer range. Since scholars concede, however, that they do characterize “Hyksos” Dynasty 15, they are irrelevant for chronological purposes here in Stratum D/2.

These nine “datable objects” from Tell el-Dab^ca presumably include all the long-awaited evidence for absolute chronology that Bietak has alluded to for some time. Several points now emerge. There is little new in the 1984 treatment, compared with earlier previews; for the crucial earlier levels, upon which Bietak bases his controversial dating of Syro-Palestinian MB IIA, there is nothing definitive. In

short, Tell el-Dab^ca presents what many scholars have suspected: an admirably well-dug relative sequence in the Delta, whose demonstrably Asiatic materials must be dated by reference to Syria-Palestine, not to a supposedly “fixed chronology” derived from Egypt itself. In particular, Bietak is not entitled to rework the entire Palestinian Middle Bronze Age sequence (as he still attempts) to suit his own notion of the Tell el-Dab^ca phasing, since this is clearly dependent upon seriation of local pottery styles and not on textually-based or “historical” dating. Even if that dating were to have significance for Palestinian Middle Bronze IIA–C, it would nonetheless be necessary for scholars to examine the published material to evaluate whether Bietak’s “ultra-low” chronology indeed has any basis in fact.

TELL EL-DAB^cA AND KEY PALESTINIAN MIDDLE BRONZE AGE SITES

Bietak (1984a) elaborates on an external synchronism that he has used before to buttress his absolute chronology at Tell el-Dab^ca, namely, the categories of the Jericho “Tomb Groups” I–V, as developed by Kenyon and Tufnell. Bietak lists six royal-name scarabs from the Jericho tombs, which he asserts offer a fixed chronology, as follows:

Tomb Group II:	Nubkheperre (Inyotef V), ca. 1650 B.C.
Tomb Group III:	Mayibre (Sheshi), ca. 1625 B.C. Khahetepre (Sebekho- tep V)
Tomb Group IV:	Khakheperre (Sesost- tris II), ca. 1600 B.C.
Tomb Group V:	Khaneferre (Sebekho- tep IV), ca. 1575 B.C. (Mayibre) Sheshi

In a devastating rebuttal, however, Ward (1987: 521–23) has shown that Bietak misread three of those scarabs, Nubkheperre, Mayibre, and Khaneferre. The remaining three names are genuine, but one (Sesostris II) is an obvious heirloom; the other two (Khahetepre and Sheshi) simply confirm the relatively late accepted date for Tomb Groups IV and V. With Bietak, we would equate these two MB IIC groups with Tell el-Dab^ca Stratum D/2, the last “Hyksos” level, ca. mid-16th century B.C.

There is nothing in the Jericho tomb scarabs that would give credence to Bietak’s low chronology for his earlier, controversial strata, G–E.⁵

Bietak has attempted to use pottery from Jericho and elsewhere in Syria-Palestine to support his dating of Dab^ca, especially Strata G–F, thereby lowering the dates for our MB IIA by as much as 125 to 150 years. Yet here again, he labors under false assumptions about the chronological utility of tomb-based typologies, which (like those at Jericho) are far from certain, and at best offer only an approximate, relative sequence. He also shows overconfidence in the last, “low chronology” of Albright and Kenyon, which despite the weight of their authors’ reputations, are now regarded as idiosyncratic and require much updating.⁶

From the late 1960s, Bietak has failed to consider the consensus of Palestinian experts regarding the chronology in the area—despite the manifestly Palestinian character of the early Tell el-Dab^ca material. He has cited only those scholars—whose work is now outdated—who appear to support his low chronology. He also insists that the material culture of Tell el-Dab^ca Strata G–E is not really “Palestinian” but derives from coastal (and perhaps inland) Syria (see Bietak 1984b). Finally, his drastic lowering of the dates for his pre-“Hyksos” levels has obscured the early, peaceful process by which Asiatics infiltrated the Delta, beginning already in the 12th Dynasty (see Dever 1985).

For example, Bietak now hints at his seriation of one type of Syro-Palestinian ware, the Tell el-Yehudiyeh juglets. He alludes to “the earliest examples of the Tell el-Yehudiyeh ware” in Complex 7 of Dahshur, which he would date well after the beginning of Dynasty 13, comparing it then to Dab^ca Stratum G. Although he does not define his typological criteria, he concludes that “this system seems to be a very fine tool for relative and absolute dating in Egypt” (Bietak 1984a: 480). How so? Whence does he derive an *absolute* chronology, even if Dahshur Complex 7 does date to 1700 B.C. (as D. Arnold holds)?⁷

Bietak apparently does not accept that Tell el-Yehudiyeh juglets, all of which Albright originally thought were MB IIC, can now be found in clear Palestinian MB IIA contexts. And he seriates them on strictly typological grounds, something Palestinian ceramic specialists do not do. Such use of a typological “system” does not inspire much confidence. The contention needs the support of much more evidence.

Bietak argues that a sequence of local Egyptian hemispherical cups can be correlated with the Yehudiyeh series. He provides an elaborate chart (Bietak 1984a: 481) to quantify a shift in the relative proportions of those vessels, arguing that they develop from shallow to deeper forms. Perhaps they do; but again, he does not explain how this produces an absolute chronology.

CONCLUSION

One of the difficulties is that Bietak, while agreeing that his material from the early levels is representative of the Syro-Palestinian Middle Bronze Age (as de Vaux and I pointed out long ago),⁸ has published very little by which we can judge for ourselves. In 1985, I gleaned for comparative purposes about all that one could. Since almost no subsequent pottery or metal implements have been

published, I assume that the conclusions reached there are still valid. Bietak selectively quotes Palestinian archaeologists when they are at variance with one another (Bietak 1984a: 482, n. 49), as inevitably happens. But he overlooks a rather striking consensus on the main points of ceramic typology and chronology. I stand confident that my redating of his Palestinian material is mainstream. We may all be wrong, of course, and Bietak right. But until more convincing evidence appears, I shall continue to use the chronological framework that has been worked out over the last two or three generations for Syria-Palestine. It is internally consistent; it provides a relative sequence into which virtually everything (including Tell el-Dab^ca) will fit rather nicely; it has at least as much claim to being "fixed" in absolute terms as Bietak's scheme; and it serves us well. What more can be asked of chronology, which, after all, is hardly a science?

NOTES

¹See especially Bietak 1981; 1984a; 1984b, and references therein. Since the Chicago symposium, there has appeared Bietak 1989, with fuller data but no appreciable change of view.

²The response in Dever 1985 has warm appreciation for Bietak's work in general, but a detailed refutation of his "low chronology" and its implications for Syro-Palestinian archaeology. My defense of traditional dates would be in agreement with the position of virtually all Syro-Palestinian specialists—including even those as idiosyncratic as Kempinski (1983: esp. 150, 151; see also Dever in press; Ward in press).

³For the material from this tomb, especially pottery, see Matthiae 1979: especially figs. M–P. For the three fenestrated axes in question, see Matthiae 1980: figs. 10a–b (a broad "eye" axe), 11a–b (two elongated "duck-bill" axes). Matthiae dates all three axes to MB IIB, ca. 1750 B.C., on the basis of their occurrence in this tomb alongside a vase bearing the name of "Hotepibre," possibly the ninth pharaoh of the 13th Dynasty, who would date to ca. 1775–1765 B.C. (on the "high" chronology); see further Scandone-Matthiae 1979; Matthiae 1980. However, the "eye" axe is unquestionably earlier typologically; and wherever it occurs in context in Syria-Palestine, it is always found with EB IV materials (ca. 2200–2000 B.C.). The "duck-bill" axes, which succeed them, are exclusively in the MB IIA period, ca. 2000–1800 B.C., in some cases coming together with pure 12th Dynasty material. The combination of those two successive axe types can only be explained by assuming that the "eye" axe is an heirloom and the other two would belong to ca. 1880–1775 B.C. at the latest (Oren 1971;

Dever 1975: 30; Tubb 1982; Gerstenblith 1983: 89–91). Bietak accepts Matthiae's date, citing none of the contradictory evidence. As for the pottery, there are many classic MB IIA types, like the two jugs of "North Syrian Painted Ware"; see Matthiae 1979: fig. P; 1982: fig. 20. Unless the "Hotepibre" can be shown beyond doubt to be identical with the 13th Dynasty pharaoh's name, I prefer to date the "Tomb of the Lord of Goats" to late MB IIA, ca. 1850–1800 B.C.—exactly like the "Tomb of the Princess" in the other chamber (the latter date agreeing with Matthiae; cf. Matthiae 1982: 8–10).

⁴See especially Ward 1987: 523–26. Bietak's subsequent attempt to refute Ward (1987: 46–49) seems far from convincing. For O'Connor's earlier opinion, dating those design scarabs all to the "Hyksos" period (Dynasty 15), see O'Connor (1974; 1985). Weinstein (1975: 10–11) posited "overlap" between the end of MB IIA in Palestine and the beginning of the 13th Dynasty in Egypt; that would seem to leave open the possibility of late Dynasty 12 occurrences of *ʿnr*^c scarabs. Finally, Tufnell (1980) discusses Tomb 303B, with a related *rdj-R*^c scarab, together with pure MB IIA pottery. Tufnell also cites other early occurrences.

⁵In addition to Bietak's misreading and misuse of the Jericho tomb scarabs, there is the problem of his overconfidence in the tomb typology itself, as developed by Kenyon. Without full control of the Palestinian comparative material, it is impossible to make use of the Jericho tomb typology—and certainly impossible simply to adopt it uncritically for all of Palestine (see below).

⁶See Dever 1976: 10–12 and references there; Gerstenblith 1983: 101–8 (the latter not cited by Bietak). Bietak

has finally reacted to the facts I had put in evidence (see Bietak 1989: 96–99), but in a fashion that does not confront the evidence.

⁷Arnold's range for Dahshur Complex 7, however, is ca. 1760–1650 B.C., which indicates how imprecise the correlation is. Furthermore, Bietak's frequent comparisons (cf. Bietak 1984a: 480) of Dab^ca with Dahshur imply that Arnold has seen his pottery and concurs that it is similar; in fact, there is no indication in any of the Dahshur publications that such is the case or that Arnold agrees with the absolute dates given to the Tell el-Dab^ca pottery (J. Weinstein, personal communication 16 April 1989). Once again, we are confronted with misleading comparisons, unpublished and therefore unverifiable data, and unfounded assertions. I suspect that the Dah-

shur material, if anything, would tend to support the "middle," not the "low" chronology.

⁸See de Vaux 1971: 80, 81; Dever 1975: 29, 30; Kempinski 1983: 148–51. Bietak (1984a: 474), however, continues to derive the cultural impetus for the Asiatic presence of Tell el-Dab^ca not from Palestine—where the best parallels lie by far—but from coastal Syria, particularly Byblos. Van den Brink (1982: 93), in a preliminary publication of Tell el-Dab^ca tomb types and burial customs, argues that the cist and vaulted tombs, especially well paralleled in Palestine, are actually Mesopotamian in character. Why such desperate searches for distant comparisons, when the obvious explanation of the "Asiatic homeland" is much closer at hand? (see further Dever 1975: 79, 80).

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