

On Jan Best's "Decipherment" of Minoan Linear A

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For slightly more than a decade now, Jan G. P. Best has been publishing material towards the decipherment and interpretation of Minoan Linear A. This work has culminated in *TAAANTA: Proceedings of the Dutch Archaeological and Historical Society*, Volume XIII/1981, Supplementum Epigraphicum Mediterraneum (Middelie, The Netherlands: Studio Pieter Mulier, 1982). A monograph such as this, amounting to 68 pages, would normally receive a brief review in a scholarly journal. Yet because the subject is so controversial, and because the author's philology is so outlandish, and perhaps most of all because the work seems in large part to appropriate the work of another scholar, a longer review assessing the extent of Professor Best's contribution seems in order.

The main thrust of this *TAAANTA* issue is that the Minoan Linear A inscriptions are Semitic, closely related to Ugaritic. Anyone with but a superficial knowledge of Minoica or Semitics or classics or Mediterranean archaeology will immediately realize that such a statement is nothing new and revelational, rather merely in agreement with the published work of Cyrus H. Gordon during the last quarter century. At first glance, those who accept Professor Gordon's decipherment¹ might delight in finding a Dutch scholar concurring with the Semitic identification of Minoan Linear A. However, when it is realized that the material presented is virtually the same as that published by Gordon, and yet Gordon's Minoan studies go uncited, and that the additional material is so poorly presented from a linguistic standpoint, one cannot remain silent. Best's volume begs a response, and I am happy to comply.

The volume is divided into four sections, though only the first of these "Von Piktographisch zu Linear B—Beiträge zur Linear A-Forschung," will be reviewed here.² This section is in turn divided into four parts: 1) "Cretan Writing: Origins"; 2) "Yaššaram!"; 3) "Nochmals: Yaššaram!"; and 4) "Von Linear A zu Linear B".

1 I include myself in this group as is evident, for example, from my "Late Biblical Hebrew and the Date of 'P'," *JANES* 12 (1980), 69, where Gordon's reading of an Eteocretan inscription is cited.

2 The other sections are "Zur Herkunft des Diskos von Phaistos," "Bilingual Inscriptions on the Stele from Lemnos," and "Zur frühindoeuropäischen Sprache in Lusitanien." The subject of the first of these is related to the problem of Linear A, but I refrain from discussing the Phaistos Disc issue due to a desire to keep the present article as short as possible. The last two sections of Best's book deal with problems in Indo-European which lie outside the area of expertise of the reviewer.

Best begins his work by delving into the origins of Cretan writing, concluding that some Linear A signs derive ultimately from Egyptian hieroglyphs, that some come from Sumero-Akkadian cuneiform signs, and that still others have “their corresponding ancestral signs . . . only in Syro-Palestinian documents” (p. 14). There are problems with each of these suggestions. Best presents only six examples of Linear A signs which he believes derive from cuneiform symbols (pp. 10–12):³ *pa* (L2) < normal cuneiform *pa* (153); *na* (L26) < normal cuneiform *na* (43); *te* (L92) < Boğazköy-Ugarit cuneiform *tāš* (192); *nì* (L60) < Amarna-Boğazköy-Ugarit cuneiform *nì* (323); *di* (L51) < Alalakh cuneiform *dí* (231); and *pe* (L1)⁴ < a very rare *pēš* (193).⁵

First it must be stated that to my eye, only Linear A *pa*, *te*, *di*, and *pe* resemble their presumed cuneiform forebears. Apart from this rather subjective criterion, however, is the further difficulty that the cuneiform *tāš*, *nì*, and *dí* signs are attested mainly from Amarna, Boğazköy, Ugarit, and Alalakh. Since the heyday of Akkadian in these Western centers was 1500–1200,⁶ it is difficult to imagine how these signs would have influenced Linear A which is estimated to have been in use beginning c. 1660.⁷ We are left with only *pa* < cuneiform *pa* and *pe* < cuneiform *pēš* as plausible derivations. But since the latter is so rare in the Sumero-Akkadian syllabary, in reality we are left with only the former as a possible correspondence. From such slim evidence one cannot build a theory.

For his second source for the Linear A signs Best posits Egyptian hieroglyphics (pp. 12–14). Certainly this has much to commend itself, especially in light of the evidence presented by Sir Arthur Evans as to Egyptian influence on early Crete.⁸ But here again Best presents only limited evidence, forwarding only two examples of Cretan signs which purportedly derive from Egyptian hieroglyphic prototypes. The first of these appears to confuse two signs, L75 = syllable *wa* and L82 which has been proposed as ideographic “wine” based on its similarity with the Egyptian hieroglyph *irp* (M43⁹).¹⁰ The two signs L75 and L82 are similar, but to jump from the Egyptian hieroglyphic *irp* to Linear A ideographic “wine” to Linear A syllabic *wa* (based on the reconstructed form **wainu* > Arabic *wain*, Hebrew *yayin*, etc.) is one too many jumps on which to construct much of a theory. Theories are fine, but they should be grounded in hard evidence.

3 The sigla in parentheses refer to the numbering of the Minoan signs by G. P. Carratelli, “Le iscrizioni preelleniche di Haghia Triada in Creta e della Grecia peninsulare,” *Monumenti Antichi* 40 (1945), 467–74, reproduced in convenient table form in M. Ventris and J. Chadwick, *Documents in Mycenaean Greek* (Cambridge, 1959), 33; and to the numbering of the Akkadian signs by W. von Soden and W. Röllig, *Das akkadische Syllabar* (Rome, 1976).

4 Many Minoan scholars follow P. Meriggi, *Primi elementi di Minoico A* (Salamanca, 1956), 15, in labeling this *pà*.

5 This is the usual *ma* sign. The reading *pēš* is not listed by von Soden and Röllig, but cf. R. Labat, *Manuel d'épigraphie akkadienne* (Paris, 1976), 157 (Labat #167); and F. Thureau-Dangin, *Le syllabaire accadien* (Paris, 1926), 62.

6 W. von Soden, *Grundriss der akkadischen Grammatik* (Rome, 1969), 4, §21.

7 Ventris and Chadwick, *Documents in Mycenaean Greek*, 28.

8 A. Evans, *The Palace of Minos at Knossos* (4 vols.; London, 1921–1936), *passim*. See especially Vol. II, p. 28: “. . . the indebtedness of Crete to late pre-dynastic and proto-dynastic Egypt is now substantiated by a cumulative mass of evidence.”

9 Number according to the system of A. H. Gardiner, *Egyptian Grammar* (Oxford, 1957), 484.

10 First suggested by J. Sundwall, cited by Ventris and Chadwick, *Documents in Mycenaean Greek*, 35.

Best's second example of corresponding Egyptian and Minoan signs are the respective symbols U38¹¹ and L85. Both of these are a set of scales, the former used as a determinative for *mḥ3t*, "balance,"¹² and the latter as an ideograph for "talent."¹³ But should this example be utilized to establish the Egyptian origin of Cretan writing? No one argued for Egyptian-Minoan connections more than Evans, but even he admitted that much of the parallelism between the two writing systems "is the result of conditions that underlie all systems of developed picture-writing."¹⁴ Among the objects which are of universal usage, according to Evans, are "simple implements and weapons, domestic utensils,"¹⁵ in which categories would be included a set of scales.

The third derivation of Minoan writing according to Best is Syria-Palestine (pp. 14–16). Here his evidence is weakest of all. In actuality he does not have a specific Syro-Palestinian script in mind, rather a) he refers to M. L. and H. Erlenmeyer's suggestion that Minoan sign L58 stems from a similar design on a Syrian seal (origin unspecified, presumably undetermined),¹⁶ and b) he posits a connection between L52 = *a* and a double axe design. The Erlenmeyers' proposal is plausible, but Best's own suggestion is incredible. He states that the earliest double axe from Tell Arpachiyah in Iraq is "a cult object in connection with a dove goddess" (p. 14), that "protohistorical Assara [occurs] as a snake/dove goddess being the obvious ancestress of the later historical goddess Asherah" (p. 13), and that based on the acrophonic principle the double axe sign L52 is therefore the graph for *a* (the first syllable of said goddess). I trust nothing need be added to upset this example of Best's "transitive law" of archaeologico-religio-linguistics.

What is most important of all in this entire discussion is that only in passing does the author refer to the derivation of Linear A and Linear B from the earlier Minoan hieroglyphics. Best seems to lose sight of this (although he does include the hieroglyphic representations in his charts). Furthermore, we should add that any conclusions about the origin of writing on Crete must remain tentative. Nothing has changed since Ventris and Chadwick wrote the following nearly 30 years ago: "It is not easy to arrive at a detailed understanding of the way in which the earlier Minoan scripts originated and developed."¹⁷

Best ends this part of the book with the conclusion: "With respect to syllabic values assigned by Michael Ventris to corresponding Linear B signs it was possible to establish original linguistic affinities with North-West Semitic dialects" (p. 16). While this statement is true, it gives the impression that it was Jan Best who established the Semitic character of the Linear A inscriptions. As noted earlier, anyone with but a casual awareness of the Minoan problem knows that Cyrus Gordon published this conclusion as early as 1957. To write as Best does, and not to cite Gordon, is a clear case of scholarly dishonesty.

The impropriety of Best's actions is clearly evident in the second, third, and fourth parts of his work. It is these pages which deal with the inscriptions themselves, and not

11 Again, according to the system of Gardiner, *Egyptian Grammar*, 521.

12 *Ibid.*

13 Ventris and Chadwick, *Documents in Mycenaean Greek*, 50, 57.

14 Evans, *The Palace of Minos at Knossos*, Vol. II, 281.

15 *Ibid.*

16 M. L. and H. Erlenmeyer, "Zu den kretischen Siegeln mit Hieroglyphenschrift," *Kadmos* 4 (1965), 1–4.

17 Ventris and Chadwick, *Documents in Mycenaean Greek*, 28.

with the origin of their writing system. The central focus of parts two and three is a series of dedication formulas. The first three read similarly (p. 17):¹⁸

ya-ta-nV-tV.wa-ya.ya-di.ki-te-te.....ya-sa-sa-ra-me
a-ta-nV-tV. u-ya. a-di.ki-te-te..... a-sa-sa-ra-me
ta-na-nV-tV. u-ya. a-di.ki-te-te..... a-sa-sa-ra-me

The variant opening words of these dedications Best translated as “I have given,” from the verbal root *ytn/’tn*. He cites Ugaritic as an example of a Semitic language which includes both variants, and specifically notes the nominal forms *ytn/atnt*, “gift” (p. 18). The former does occur in Krt 135, 277,¹⁹ but the latter never occurs; perhaps Best has in mind *itnn* (cf. *UT* §19.415a) = Hebrew *’etnān*, “hire, reward.” Regardless of this minor problem, here we note the first explicit example of Best’s appropriation of previously published material without acknowledgment. In Gordon’s major book on the subject, *Evidence for the Minoan Language* (Ventnor, N. J., 1966), henceforth *EML*,²⁰ Minoan *ya-ta-no-/a-ta-no-* is already translated “he has given” (*EML* §121) and the *y/’* interchange is also highlighted (*EML* §148). Best explains the third variant as if it were based on a verbal root *tnn*, “give,” and claims its occurrence in ‘nt vi:VI:12. But the word in this passage is actually *tnn*, nothing more than the 2nd masculine singular imperfect of the normal Ugaritic root *ytn* (cf. *UT* §§10.16, 13.37, where the line is translated.)²¹ Gordon, incidentally, interprets Minoan *ta-nu-a-ti*, to which Best’s *ta-na-nV-tV* may be related,²² from the root *tn’*, “to set up, to donate,” attested commonly in Phoenician dedications (*EML* §122), e.g., *mtnt ’š tn’ lb’l*, “a gift which he donated to Baal,” in a Punic inscription from Constantine.²³

For the next word, *wa-ya/u-ya*, Best suggests both variants of the Semitic conjunction “and,” *wa* and *u* with an emphatic *-y*. He cites Ugaritic as an example of a language where both *wa* and *u* exist, though if Best could control all of Semitic or even all of Northwest Semitic, he certainly would have cited Hebrew. In Ugaritic, *w* is the regular conjunction

18 For the source of these inscriptions, Best cites P. Meriggi, “Kleine Beiträge zum Minoischen: 2. Minoische Widmungsformeln,” *Kadmos* 13 (1974), 85–94. A comparison of Best’s and Meriggi’s transcriptions shows that the two do not accord at every sign. Since Meriggi’s readings are taken directly from W. C. Brice, *Inscriptions in the Minoan Linear Script of Class A* (Oxford, 1961), his are obviously correct. Where Best’s stem from is difficult to say. Regardless, for the sake of criticizing Best’s philology, and not his transcribing, I accept his readings for the nonce.

19 Ugaritic texts are cited according to the system of C. H. Gordon, *Ugaritic Textbook* (Rome, 1967), henceforth *UT*.

20 For simplicity’s sake, I cite only *EML*, though the reader should be aware that many of Gordon’s proposals appeared in earlier works, e.g., “Notes on Linear A,” *Antiquity* 31 (1957), 124–30; “Minoan Linear A,” *JNES* 17 (1958), 245–55; “The Language of the Hagia Triada Tablets,” *Klio* 38 (1960), 63–68; “Minoica,” *JNES* 21 (1962), 207–10; and “Towards a Grammar of Minoan,” *Orientalia* 32 (1963), 292–97. For a complete listing of Gordon’s works on Minoan, see the bibliography in G. Rendsburg, *et al.*, eds., *The Bible World: Essays in Honor of Cyrus H. Gordon* (New York, 1980), 293–294.

21 See also C. H. Gordon, “Poetic Legends and Myths from Ugarit,” *Berytus* 25 (1977), 84; and H. L. Ginsberg *apud* J. B. Pritchard, *Ancient Near Eastern Texts* (Princeton, 1969), 138.

22 If there is a relationship between the two, it is not without difficulty. Best’s *ta-na-nV-tV* has a second *nun*; this could represent the 1st plural suffix of the perfect verb, but then what becomes of the last syllable?

23 Cf. A. Berthier and R. Charlier, *Le sanctuaire punique d’El Hofra à Constantine* (Paris, 1955), 153, inscription #250. For other examples, see conveniently R. S. Tomback, *A Comparative Semitic Lexicon of the Phoenician and Punic Languages* (Missoula, Mont., 1978), 120–121.

(*UT* §19.799) and *u* is a *hapax legomenon* in 118:6 (cf. *UT* §19.3). In Hebrew, as is well known, both forms occur, with specific phonological rules governing when *wa/wā/wě* is used and when *û* is used.²⁴

For emphatic *-y*, Best refers to K. Aartun's discussion of this particle,²⁵ though in truth its existence is not fully accepted in Ugaritological circles.²⁶ Moreover, Best cites *uky* in 1018:5 as a parallel where conjunction *u* can be followed by emphatic *-k* and emphatic *-y*. But to use this rare and enigmatic form to elucidate Minoan is an example of *ignotum per ignotius*.

Best interprets the next word, *ya-di/a-di*, as variants of "hand" (p. 19), a sound conclusion, especially given the already attested *y'* interchange in Minoan mentioned above.²⁷ Minoan *ki-te-te* Best analyzes from the root *ḥt'*, "sin, compensate." There is no major problem with this reading, but one would like some corroborating evidence that Semitic etymological *ḥ* can appear in Linear A script as *k*-. The pharyngals are not represented in Minoan orthography, e.g., *pi-te* = *ptḥ* (*EML* §124), *re* = *r'* (*EML* § 127). According to Best's interpretation, the voiceless velar fricative /ħ/ would be represented in Minoan script, but certainly some comment in this direction is desired.

Best's analysis of the last word in these inscriptions, *ya-sa-sa-ra-me/a-sa-sa-ra-me*, is the most crucial and yet it is also the one fraught with the most difficulties. He interprets these variants as the divine name Asherah (Ugaritic *atrt*, Hebrew 'āšērā^h) plus vocative *-m*. Both ends of this parsing require major rethinking. We start with the reading "Asherah," where in fact two problems exist (the lack of the feminine indicator *-t* and the double writing *-sa-sa-*) and then move on to vocative *-m*.

If *a-sa-sa-ra-* is "Asherah" (for the variant *ya-sa-sa-ra-* see below), we must first ponder what happened to the expected feminine suffix *-(a)t(u/i/a)*. Every Northwest Semitic language attested in the 2nd Millennium B.C.E. retains this suffix. Ugaritic consistently has *-(a)t* (cf. *UT* §8.3). Amorite personal names include the suffix, viz., *du-uh-ša-tum*, *ni-ig-ḥa-tum*, *ti-is-pa-tum*, *sa-la-ma-tum*.²⁸ The Amarna tablets include such vocables as *še-ti*, "hour," in *EA* 138:76, *la-bi-tu*, "brick," in *EA* 296:17, and the very divine name under discussion *a-šar-ti*, *passim* in the theophoric personal name *abdi-ašarti*, literally "servant of Asherah." If we accept W. F. Albright's decipherment of the Proto-Sinaitic inscriptions, then here too we witness the ending *-t* in feminine singular nouns.²⁹ Minoan itself includes one feminine singular noun ending in *-atu*, namely, *ki-re-ya-tu*, "city, town," in libation table I, 3³⁰ (*EML* §152), and a variety of feminine personal names ending in *-atī* (*EML*

24 The Tell Mardikh tablets reveal that Eblaite is another language which uses both *wa* and *û*, though the specifics of when *wa* is to be used and when *û* have not been worked out yet. See D. O. Edzard, "Neue Erwägungen zum Brief des Enna-Dagan von Mari (TM.75.G.2367)," *SEB* 4 (1981), 89–90, n. 4.

25 K. Aartun, *Die Partikeln des Ugaritischen* (Neukirchen-Vluyn, 1974), 46–47.

26 E.g., emphatic *-y* is not mentioned in *UT*.

27 As cognate evidence, Best cites Ugaritic *yd* (*UT* §19.1072) and fossilized **d* (*UT* §19.633). Hebrew, perhaps, affords a closer parallel with the regular form *yād* and the variant form 'ād in *Psa.* 68:19. For the latter, cf. M. Dahood, *Psalms II* (Garden City, N. Y., 1968), 143.

28 H. B. Huffmon, *Amorite Personal Names in the Mari Texts* (Baltimore, 1965), 142–43 and in Glossary, 182, 236, 247, 270.

29 W. F. Albright, *The Proto-Sinaitic Inscriptions and Their Decipherment* (Cambridge, Mass., 1969), 35.

30 Number according to Brice, *Inscriptions in the Minoan Linear Script of Class A*.

§153). A list of slaves on an Egyptian papyrus from c. 1740 B.C.E. includes a variety of Northwest Semitic names whose parsing reveals feminine singular nouns ending in *-t*.³¹ Furthermore, archaic Hebrew poetry can include feminine singular nouns with *-t*, e.g., *zimrāt* in Exod. 15:2 and *pôrāt* in Gen. 49:22. Finally, we may note that Phoenician,³² Moabite,³³ and the Hebrew of the Samaria ostraca³⁴ retained *-t* even into the 1st Millennium. The evidence is thus so weighty against even the possibility of Minoan “Asherah” without *-t* that Best’s interpretation can be readily dismissed. Moreover, his appeal to *UT* §8.4 that some feminine singular nouns do not end in *-t* is totally inappropriate since Ugaritic *atrt* is an example of a feminine singular noun that does end in *-t*. As any Semitics scholar knows, there are nouns in each language which are grammatically feminine even without the typical feminine termination *-(a)t(u/i/a)*, e.g., *’tn*, “she-ass,” *’m*, “mother,” but “Asherah” is not one of them.

Also of no help is Best’s attempt to explain the double writing *-sa-sa-*. He adduces as a parallel the cuneiform writing of both Ašur (usually *a-šur*) and Aššur (usually *aš-šur*) to show that “Asherah” can be written with either one sibilant or two. This is obviously ludicrous. As any first-year student in Akkadian knows, a double consonant may be written either singly or double.³⁵ “Assyria” has etymological double *š*, as is confirmed by Hebrew *’aššûr* with *dagesh* in *shin* marking the double consonant. “Asherah,” on the other hand, has etymological single *š*, as is confirmed by Hebrew *’āšērāh* and by the consistent cuneiform representation with but one *š*, e.g., Amarna *a-šar-ti* cited above.³⁶ For Best’s interpretation to be correct, he would need to find an example in all of ancient Near Eastern writing where a single consonant is written doubly in a syllabic orthography (cuneiform, hieroglyphic, etc.), but one does not exist. These two points, the lack of an expected feminine *-t* and the double writing *-sa-sa-*, force us to conclude that Best’s analysis is plainly wrong.

Nor is Best’s explanation of the final syllable of this word as vocative *-me* cognate to Ugaritic vocative *-m* (p. 20) defensible. The problem is the same as Best’s recourse to emphatic *-y* discussed earlier, namely, that Ugaritic studies has not confirmed the existence of vocative *-m*. Some scholars have argued for its presence in Ugaritic³⁷ and hence in Hebrew,³⁸ but the bulk of scholarship denies a vocative *-m* in Ugaritic.³⁹ Some Ugaritic

31 W. F. Albright, “Northwest-Semitic Names in a List of Egyptian Slaves from the Eighteenth Century B.C.,” *JAOS* 74 (1954), 222–233. Note especially nos. 15, 17, 35, 87.

32 Z. S. Harris, *A Grammar of the Phoenician Language* (New Haven, 1936), 58–59.

33 A. H. Van Zyl, *The Moabites* (Leiden, 1960), 171.

34 Specifically *št* = *šatt* < **šant*, “year.”

35 Von Soden, *Grundriss der akkadischen Grammatik*, §7d; R. Caplice, *Introduction to Akkadian* (Rome, 1980), 12; D. Marcus, *A Manual of Akkadian* (Washington, 1978), 32; etc.

36 Cf. the presumably related common noun *aširtu*, “sanctuary, offering,” also always with one *š*, in *CAD* I/2 (A), 436–42.

37 See A. D. Singer, “The Vocative in Ugaritic,” *JCS* 2 (1948), 5–6; and Aartun, *Die Partikeln des Ugaritischen*, 39.

38 E.g., M. Dahood, *Proverbs and Northwest Semitic Philology* (Rome, 1963), 12.

39 There is no entry for vocative *-m* in *UT*. See more specifically the denial of D. A. Robertson, *Linguistic Evidence in Dating Early Hebrew Poetry* (Missoula, Mont., 1972), 92; and the remark by M. H. Pope, “Ugaritic Enclitic *-m*,” *JCS* 5 (1951), 123, n. 8. The evidence presented by Singer, “The Vocative in Ugaritic,” 5–6, is itself very revealing. Four of the examples he cites are labeled “doubtful.” Two of his three sure examples have vocative

nouns in the vocative may take enclitic *-m*, but the *-m* itself is not a sign of the vocative.

The variant form *ya-sa-sa-ra-me* is explained by Best as possessing not only the vocative *-me* but also the vocative *ya-* cognate to Ugaritic *y-*. While it is true that Ugaritic does use this particle (*UT* §§12.5, 19.1060), since *a-sa-sa-ra-* cannot be "Asherah," the argument for a preceding vocative falls. In sum, Best's interpretation of *ya-sa-sa-ra-me/a-sa-sa-ra-me*, which is for him a most critical word, is a total failure. Instead, we should accept Gordon's analysis of *ya-sa-sa-ra-mV* as "votive offering" based on its parsing as a nominal form of Š-stem of *šlm*, "render, deliver" (*EML* §§122, 160, 163). The variant form with *a-* would be another example of the interchange of *ya-* and *a-* in Minoan Linear A texts (*EML* §148).

Best next presents two more Linear A inscriptions which he reads as follow (pp. 23–24):⁴⁰

a-ta-nu-ti.wa-ya.u-su.qa-le.ya-sa-sa-ra-me
ni-ka-te-ya-me.u-qe-ti.ya-sa-sa-ra-me.ta-nu-ni-ki-na.ni-nu-qa-ya(?)-tu-i

The first he translates "I have given a fallen gift, O Asherah," with *u-su* cognate to Ugaritic *uš* (*UT* §19.117) and *qa-le* cognate to Ugaritic *qll* (*UT* §19.2231). Both are well attested words in Ugaritic, but it is not readily seen how their collocation fits the context here. Regardless, Best's statement that *u-su* reflects the nominative case ending *-u* suffers from the problem that in his translation an accusative would be more appropriate.

The second of these inscriptions is translated by Best as "My offering I have inscribed, O Asherah, your presenter, Ninuqayatui." *Ni-ka-te-ya-me* is derived from the root *nkt*, "offer" (cf. *UT* §19.1650) with 1st singular possessive suffix *-ya* and enclitic *-me* (p. 25); *u-qe-ti* is derived from the root *hqq*, "inscribe" (p. 25); and *-ki-na* suffixed to *ta-nu-ni-* is seen as the 2nd feminine singular possessive suffix (p. 25). I limit myself to two criticisms. First, it has not been proved that enclitic *-ma* can follow a possessive suffix. H. L. Ginsberg questioned such interpretations of various Ugaritic passages 35 years ago.⁴¹ The one example of *-m* following a possessive suffix which Ginsberg accepted, 125:60, is apparently due to a special reason; here the *-m* is used to indicate direct quotation, as with Akkadian *-mi* (a particle distinct from *-ma*).⁴² In Hebrew, H. D. Hummel proposed numerous examples of enclitic *-m* following a possessive suffix,⁴³ but each one has been questioned

y- prefixed to the noun and *-m* suffixed to it. Since a double vocative would be quite extraordinary, this also should tell us that *-m* is merely enclitic and not vocative. His one remaining example, III AB A:8–9, is too little evidence on which to posit the existence of vocative *-m* in Ugaritic. Vocative *-me* does exist in Akkadian (see von Soden, *Grundriss der akkadischen Grammatik*, §123d), but it is rare.

40 The former appears in Brice, *Inscriptions in the Minoan Linear Script of Class A*, Table XXa/XX, though it is not perfectly clear exactly which inscription Best is quoting. The latter appears in S. Alexiou and W. C. Brice, "A Silver Pin from Platanos with an Inscription in Linear A: Her. Mus. 498," *Kadmos* 15 (1976), 18–27. The transliteration and discussion which follow are based on Best's assignment of values to the signs, but it should be pointed out that a few of these are questionable, e.g., the 16th sign which Alexiou and Brice read as L102 = *de* or perhaps L45 = *ko*, but which Best reads as *ni*.

41 H. L. Ginsberg, "Review of J. Obermann, *Ugaritic Mythology*," *JCS* 2 (1948), 141. Cf. also the cautious words of Pope, "Ugaritic Enclitic *-m*," 123, that "in a few uncertain cases [*-m*] seems to be added to suffixed pronouns" (italics added).

42 H. L. Ginsberg, *The Legend of King Keret* (New Haven, 1946), 46.

43 H. D. Hummel, "Enclitic *Mem* in Early Northwest Semitic, Especially Hebrew," *JBL* 76 (1957), 99–100.

by D. A. Robertson in his thorough treatment of this particule.⁴⁴ The second point to be critiqued in Best's analysis of this inscription is his claim that *-ki-na* represents the 2nd feminine singular (!) possessive suffix cognate with Ugaritic *-kn*. Clearly, this is incorrect, since *-k* is the form he has in mind (*UT* §§6.7, 19.1184a).

After presenting these inscriptions and his exegesis, Best turns to a general discussion of Minoica. Among the further readings he mentions are *ku-ni-šu* (*sic*: there is no *šu* sign in Linear A, only *su*), "emmer," cognate to Akkadian *kunāšu*, and *ku-lu*, "total," cognate to Hebrew *kōl*, Ugaritic *kl*, Arabic *kull*, etc. (p. 34). Again, anyone who has kept abreast of Minoan studies during the last quarter century knows that these were among Gordon's earliest readings and first clues in his decipherment of Linear A.⁴⁵

Best's final part, "Von Linear A zu Linear B," is a general discussion of the orthographic and phonetic details of the two scripts. Of the various points which require comment, I limit myself to one. Best refers to the two "cloth" ideograms at Knossos, one preceded by *ku* and one by *zu*. He suggests that these two syllables are abbreviations for Semitic words for clothing, specifically Akkadian *hūratu* and *zuluhhu* (p. 41). All would be fine, except for two problems. First, both these words are rare and neither has "clothing" or "a type of clothing" as its primary meaning. The former is defined as "a dye from a plant or its parts, the plant itself and its parts,"⁴⁶ and the latter means "a breed of sheep, a type of fabric."⁴⁷ Second, the identification of these *ku* and *zu* syllables as abbreviations representing Akkadian words for clothing was proposed by Gordon 26 years ago. Gordon posited more appropriate words, *kusītu* and *šubātu* respectively, two of the commonest Akkadian words for types of clothing.⁴⁸ This example of Best's "decipherment," the last example I will cite, is paradigmatic of his *modus operandi*. He repeats without acknowledgment material published by Gordon more than two decades ago, though in this case he has altered it slightly, thereby revealing an intractable inability to handle Semitic philology.

In short, Jan Best's work is clearly not the "Entzifferung der Linear A-Schrift" it purports to be (p. 37). If Linear A is Semitic, as I believe it is, Gordon deserves the credit for the decipherment. Finally, it should be noted that Best cannot claim ignorance of Gordon's work on Minoan. In an earlier book on the same subject (*Some Preliminary Remarks on the Decipherment of Linear A* [Amsterdam, 1972]), Best wrote that Gordon "was the first and most ardent advocate" of the Semitic character of Linear A (p. 13), and he actually cited Gordon's "Notes on Minoan Linear A" of 1957 and *EML* of 1966 (p. 14). Moreover, throughout the *TAAANTA* volume, Best cites *UT* wherein Gordon refers to his work on Minoan with specific bibliographic references at §17.14. Clearly, Best's actions

44 Robertson, *Linguistic Evidence in Dating Early Hebrew Poetry*, pp. (in order of Hummel's presentation of the verses) 102 (Deut. 33:3), 103 (Psa. 21:2), 107 (Psa. 22:16), 108 (Psa. 31:12), 101 (Psa. 38:20), 86 (Psa. 102:18), 86 (Psa. 109:13), 86 (Psa. 109:15), 107 (Isa. 3:12), 102 (Isa. 3:13), 106 (Isa. 30:27), 95 (Isa. 40:17), 99 (Hos. 12:15). Some of these are also rejected by Dahood, *Psalms I*, 131, 237 (on, respectively, Psa. 21:2, 38:20).

45 Gordon reads the latter of these as *ku-ro*, not *ku-lu*, the difference arising from Gordon's identifying L22 as *ro à la* Ventris and Chadwick, *Documents in Mycenaean Greek*, 23, and Best's interpreting it as *lu*.

46 CAD 6 (H), 247.

47 CAD 21 (Z), 154.

48 Cf., respectively, CAD 8 (K), 585-87; and CAD 16 (S), 221-25.

cannot be tolerated, least of all in the scholarly community which has brought to the modern world a better understanding of our classical, Biblical, and Near Eastern heritages.