

## REVIEW

Berres, Thomas 2017: *Der Diskus von Phaistos, Grundlagen seiner Entzifferung*. Frankfurt am Main: Vittorio Klostermann GmbH, XIV + 336 pp., 64 figs., 18 tables. ISBN 978-3-465-03977-8, Price € 49,-.

The book of Thomas Berres presents an in-depth treatment of the thorn-in-the-flesh of every decipherer, the famous discus of Phaistos (Crete) (Fig. 1). It focuses on internal evidence and, given the subtitle, seeks to provide fundamental clues provided by such a line of approach as to the long-sought-for decipherment of the hieroglyphic text. In line with this observation, the author does not claim to have actually deciphered the text, but limits himself to prerequisites for such an accomplishment. Starting point are the claims to decipherment by Kjell Aartun (1992) and Derk Ohlenroth (1996), the first suggesting the text rendering Semitic and the second as it rendering Greek, from which failed attempts[?] lessons may be learned. Berres does not systematically treat all attempts at decipherment, though, which would be, it must be admitted, a Herculean task.

Key points for the approach which focuses on the internal evidence are: what is the date of the object?, what is the direction of writing?, on which side does the text start?, are we dealing with a syllabary or alphabet?, how was the spider web of lines drawn into the clay, how are the signs stamped and what can we learn from secondary corrections?, what is the function of the so-called “thorn” 46, which is not stamped but incised?, are there any internal clues as to the understanding of the contents or the type of language in the form of prefixes, suffixes, and roots showing evidence of declension?

Most of these issues have been treated in an exemplary fashion by Yves Duhoux in his critical edition of 1977. However, Berres has some ideas of his own, of course, otherwise the whole exercise of writing a book would be pointless. In the following, I will select a number of issues which in my opinion are representative of the author’s approach and, while discussing them in the order of the book, try to determine their validity.

1. *Date* (pp. 1-11). In connection with the issue of the date of the discus, Berres distinguishes between that of its production and its final deposition. According to its traditional dating, the “life-time” of the discus is assigned to the Middle Minoan period or first half of the 2nd millennium BC, say *ca.* 1900-1600 BC. A notable



Fig. 1a. The disc of Phaistos, side A.

exception to this dating is the one proposed by Jan Best. He stresses the fact that the discus has been found in association with a Linear A tablet, PH 1, on which features a personal name, *di-ra-di-na*. In writing variant *di-re-di-na*, the same personal name is found on a Linear A tablet from Hagia Triada, HT 98. Now, the Linear A tablets in question are traditionally assigned to the Late Minoan IA period, say *ca.* 1550-1450 BC, which is considerably later than the Middle Minoan date traditionally assigned to the discus. But Best even goes a step further than this and maintains that the Linear A texts of Hagia Triada are contemporary with the Linear B ones of Knossos, which date to Late Minoan II or IIIA1, say *ca.* 1450-1350 BC. Berres finds the argument by Best unconvincing because the identity of the name does not need to imply that actually the same person is referred to, and he points in this connection to Ventris/Chadwick 1973, 102-



Fig. 1b. The disc of Phaistos, side B.

103, which lists the same names attested for the Knossos tablets of *ca.* 1350 BC and the Pylos tablets of *ca.* 1200 BC. From the valid observation that it is *not certain* that the name *di-ra-di-na* or *di-re-di-na* refers to one and the same person, Berres then jumps to the conclusion that it *certainly does* not refer to one and the same person. He does so because in the end he sticks to the traditional dating and qualifies the attempts at a later dating as resulting from “Willkür und Wunschvorstellungen” (p. 11). The author does not mention in this connection the fact that, in the Hagia Triada and Knossos tablets, there are “linkers” and “big linkers”, persons of the same name which occur frequently in these sets of tablets and do so in close connection with each other on the same tablets (Woudhuizen 2009, 171-175). In these cases, there can be no doubt that the identical persons are referred to, not just persons who happen to have the same name as

in case of the aforesaid names from Knossos and Pylos. Accordingly, the Hagia Triada texts and those from Knossos must belong to one and the same chronological horizon. That is not wishful thinking, but hard fact. In this group of “linkers” and “big linkers” *di-ra-di-na* or *di-re-di-na* features in an admittedly modest way, nonetheless the association of the discus with a Linear A tablet could well suggest a late date for its deposition. Of course, this information does not tell us anything about how old the discus was at the time of its deposition, but it needs special pleading to argue that it was already hundreds of years old.

2. *Corrections and 02-12* (pp. 57-78, 85-100). The secondary corrections by the scribe can best be discussed in combination with the identification by Berres of the combination of “man’s head” (02) and “shield” (12) as a determinative. The most important corrections by the scribe are also discussed by Duhoux in his edition of the text of 1977. But Berres uses the discussion of these corrections as an argument for his identification of the function of the common element 02-12, which occurs as much as 13 times in the text on the discus, 12 times on the front side and 1 time on the back side. He rightly observes that a number of corrections entails the writing of 02-12, which was at first omitted and later added in segment A5 and A29. Sometimes, after writing 02 the scribe at first forgot to write 12 and first imprinted the next sign, as in segments A10 and B32. From these observations, Berres draws the conclusion that the element 02-12 on the one hand is unimportant and easily to be subject to oversight and on the other hand important enough to be added as yet by means of corrections. In his opinion this can be explained only when 02-12 does not render a phonetic value but functions as a “stumm” (unphonetic) determinative. More specifically, it is argued that 02 functions as the determinative of a personal name and 12 specifies the person in question as being armed. In fact, this analysis is one of the main insights of Berres’s book, often repeated and leading him to the exhortation on p. 261 that “Die Ergebnisse der bisherigen Forschung sind weitgehend wertlos, da man im Allgemeinen Kopf (...) [02] und Schild [12] (...) nicht als (stumme) Determinative erkannt hat”. I have my doubts whether this inference is correct, for three reasons:

(a) in copying a text from a preliminary draft, the focus of the scribe is not on the contents of the text but on the lay-out. Hence, omitting an element in this procedure does not necessarily say something about its importance in a phonetic or linguistic sense. For instance, when I recently copied Beyköy 2 from my draft on the computer, I omitted the verb in one of the total of 50 phrases, which from a linguistic point of view is a vital element for proper understanding of the contents.

(b) determinatives are not soundless (unphonetic) elements, but express their respective values. Thus, in Luwian hieroglyphic the determinative for “land”, L 228, renders the value *UTNA* (plural *UTNAi*, Yalbur § 5) and the determinative for “town”, L 225, renders the value *UMINA*. It can even be argued that Sumero-grams like LÚ “man” and GAL “great” from cuneiform scripts were actually read as /lu/ and /gal/, as in the Byblos script the tile LUGAL “king” occurs as

*lu-ka<sub>1</sub>-lu* (Woudhuizen 2007, 727) and in Cretan hieroglyphic LÚ “man” is expressed by E112 *lu* or E30 *ru* (Woudhuizen 2016, 155; 162).

(c) there already is a determinative of personal name, a stroke attached to what in my opinion is the name of the recipient of the letter, Nestor, when first mentioned in A3. This device, identified as sign 47, is amply attested in the legends of Cretan hieroglyphic seals (14 times, see Woudhuizen 2016, 59-60) and can positively be traced back to the Luwian hieroglyphic determinative of personal name, L 383, 1. See further below sub 4.

3. *Total of signary* (pp. 101-118). With reason, Berres argues that, with a view to the number of individual signs (traditionally 45), the signary of the discus is more likely to be identified as a syllabary than as an alphabet. With a special formula, he estimates that the total number of signs of the discus script, including signs not used in the text but theoretically part of it, could be 56. This would be comparable to the Cypro-Minoan script, which consists of vowel (V) signs and consonant + vowel (CV) signs. Whatever the merits of the mathematics, the signary of the discus can be positively shown to be a constituent part of the Cretan hieroglyphic script more in general (Woudhuizen 2016, 42-44; 94-106), and this script entails well over 100 signs and therefore may safely be assumed to include, alongside syllabic signs, logograms, in other words to be of a logosyllabic nature.

4. *Determinative of personal name* (pp. 132-134). Berres challenges the identification of the determinative of a personal name by Achterberg *e.a.* 2004(, 139) in segment A3, as it seems in the first place because it is incompatible with his identification of the “man’s head” 02 as such. His arguments are the following:

(a) the determinative of personal name 47 occurs only in A3, not when the name it is allegedly attached to occurs later on in the text and not in connection with the other names assumed.

(b) the second argument is worth citing as it is revealing about Berres’s knowledge of Luwian hieroglyphic: “Seltsam auch, dass das Namensdeterminativ für *Nestor* nicht wie [02] am Anfang sondern im Innern einer Zeichengruppe steht” (p. 133). In Luwian hieroglyphic the determinative of a personal name, L 383, 1, always occurs in front of or in direct association with the first syllable of the personal name it determines, not as the first element of the phrase, which is usually a sentence introductory particle. Evidently, Berres lacks knowledge of Luwian hieroglyphic, as, frankly, he himself admits when on p. 118 he declares “dass ich keine Sprache mit Silberschrift beherrsche” (p. 103). The situation on the discus, in which only the recipient of the letter is marked by the determinative of personal name and then only when mentioned for the first time, whereas all the other personal names go unmarked, is exactly paralleled in Beyköy 2, where only the name of Muksas or Muksus is singled out as such in § 26 and not when mentioned later on §§ 36, 38 [damaged], and 44 (Zangger/Woudhuizen 2018 [this volume]).

5. *The “thorn” 46* (pp. 137-188). In connection with the “thorn” 46, scratched into the clay, Berres argues that it is not a syllabic sign because of its different nature from the stamped pictorial signs 01-45. After a lengthy exercise in which all possible functions are discussed, he comes to the conclusion that “Übrig bleibt nur die Konjunktion *und*” (p. 170). In this manner, the personal names, which in his opinion are identifiable as such by the determinative of personal name 02, or, when armed, 02-12, are coordinated by the conjunction 46 “and”. This ultimately leads Berres to the assumption that the text is a treaty between the persons mentioned (pp. 280-281). Internal evidence is decisive, however, that 46 functions as a syllabic sign. It so happens, namely, that it features in doublets and triplets, from which it may be derived that the alternating final signs, 12, 35, and 46, render a syllabic value of which the consonant is the same but the vowel changes: CV<sub>1-3</sub> (see Fig. 2 below). From the resulting grid, it is possible, in like manner as Michael Ventris did with the Kober-grid, to bring about a connection with a related script (= external evidence, a category Berres is not interested in) as the prediction CV<sub>1-3</sub> serves as a reliable verification. This related script can thus be shown to be Luwian hieroglyphic, as in this script the corresponding sign of 12, L 181 *TURPI* “bread”, renders the syllabic value *tu*<sub>6</sub> according to the acrophonic principle and the corresponding sign of 46, L 383, 2 *+r(a/i)*, may also be assumed to originally express a dental value before the time it became subject to rhoticism ([d] > [r]). Notwithstanding that the vowel is not fixed in the Luwian case, it lies at hand that the exact value of 46 is *ti*, from which it follows that 35 renders *ta*. Berres counters the syllabic nature of 46 by the following remark: “Dann aber würde man nicht begreifen, warum die Diskusschrift diesen Konsonanten [oder Silben] nicht wie die anderen Phoneme mit einem Bildzeichen, sondern mit einem abstrakten Zeichen wieder gibt” (p. 152). He does not seem to realize that the same applies to its Luwian hieroglyphic counterpart, L 383, 2! Just like the determinative of personal name, the “thorn” sign can also be traced in other Cretan hieroglyphic texts, like the double-axe from Arkalochori (explained away by Berres as an incidental scratch, pp. 215-230) and the largest seal #294 (Woudhuizen 2016, 60-61). Note that the fact that Luwian hieroglyphic L 181 depicts a bread and not a shield, as assumed for its counterpart from the discus, 12. This nullifies Berres’s theory that in combination with 02 it specifies an armed person, which on the sign-level is nothing but “Kling-Klang” etymology.

6. *Luwian* (pp. 130-132). As we have seen, Berres himself admits that he is not interested in external evidence. I personally do not see how a script or a language can be deciphered without the combination of internal and external evidence. It does not come to me as a surprise, therefore, that Berres’s approach leads nowhere. We have also seen that the author admits “dass ich keine Sprache mit Silberschrift beherrsche” (p. 103). Now, Luwian hieroglyphic is a syllabic script, and one wonders why Berres on pp. 130-132 suddenly, in discussing the results of Achterberg *e.a.* 2004, tries to show off his expert knowledge of Luwian. “Der luwische Diskustext, auf den sich das Autorenkollektiv geeinigt ist,

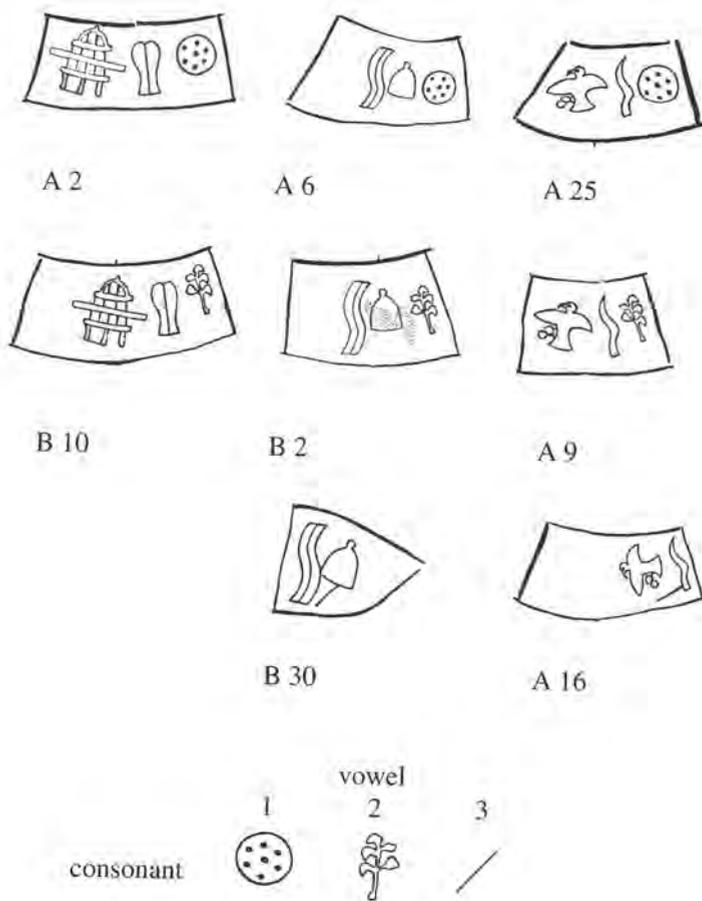


Fig. 2. Doublet and triplets with resulting grid (after Achterberg *e.a.* 2004: 76, Fig. 34).

ist kein Luwisch. Der "luwische Brief an Nestor" ist niemals von jemandem, der die luwische Sprache und Schrift beherrschte, geschrieben worden. Davon legt auch der unsägliche Inhalt des (erfundenen) Textes beredtes Zeugnis ab. Jetzt erweisen sich das zu knappe 'luwische' Diskussyllabar und die üppigen Mehrfachnotierungen sowie die – gemessen an echten luwischen Texten – völlig un- ausgewogene Häufigkeits- verteilung der Silben als (notwendige) Folgen eines verkehrten Entzifferungsansatzes" (p. 131). On the basis of a close study of which "real Luwian hieroglyphic" texts does Berres base his statistical disclaimer? Texts in Early Iron Age scribal tradition, Late Bronze Age scribal tradition, or the Middle Bronze Age ones? I would have liked something like a reference with a specification ... Instead of bashing others, it seems more expedient that Berres brings his own "Grundlagen" up to the standard.

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