TEN REASONS WHY KH 115 ≠ KN 115*

My main aim in this article is to emphasize that we do not have strong enough evidence to support the recent claim that a single scribe wrote Linear B tablets at both Knossos and Khania. I present ten palaeographical arguments

* I wish to thank Alexandra Karetsou, the director of Herakleion Museum, and Maria Vlasakis, the director of the Khania Museum, for permitting Ruth Palmer and me to study the Knossos and Khania tablets in August, a very busy time for them. We were able to examine every tablet of KN 115 and KH tablets Ar 4, Gq 5 and X 6 carefully. The staffs of both museums made our work extraordinarily efficient and pleasant, while Birgitta Hallager offered us wonderful *xenia* in Khania. I thank Erik Hallager and Jean-Pierre Olivier for sharing with me their preliminary drafts of articles and their opinions on my own developing ideas. I have also profited from discussing these texts with S. Hiller, S. Jalkotzy, W.-D. Niemeier, R. Laffineur, P. Carlier, E. L. Bennett, Jr., M. Wiener, and especially J. Driessen whose careful reading of the proofs saved me from several factual errors. In studying the tablets, Ruth Palmer and I followed the following procedure. We had the advantage of having read fully the articles cited below, and I had discussed particular points with the scholars just named. In the Herakleion and Khania museums, we each examined the tablets independently, making our own observations, and only discussing our preliminary conclusions after we had studied the whole material. I had written a preliminary draft, entitled «Khania Tablets and Dionysus», for my graduate seminar at UT Austin in fall 1993. This I had circulated among scholars. I am ultimately responsible for the ideas expressed herein and their manner of presentation. Lastly, I wish to stress that my arguments and criticisms are put forward with *l'esprit de Gif* firmly in mind. I am discussing and examining the ideas and points of view of scholars whose expertise and unique talents I greatly admire and respect. On the other hand, I do not believe in the relativity of truth, so this is not an exercise in illustrating the apothegm *tot homines, quot opiniones.* I am trying to convince my readers that the equation KH 115 = KN 115 cannot be proved on present evidence and should not be used in further reconstruction.

The Primary publications and discussions of the Khania tablets are as follows. For KH Sq 1, HVH1 = E. Hallager, M. Vlasakis and B.P. Hallager, «The First Linear B Tablet(s) from Khania», *Kadmos* 29, 1990, pp. 24-34. For KH Ar 4, Gq 5, and X 6, HVH2 = E. Hallager, M. Vlasakis and B.P. Hallager, «New Linear B Tablets from Khania», *Kadmos* 31, 1992, pp. 61-87. GTz = L. Godart and J. Tzedakis, «Les nouveaux textes en linéaire B de la Canée», *Rivista de Filologia* 119:2, 1991, pp. 129-145. JPO = J.-P. Olivier, «KN 115 = KH 115. Un même scribe à Knossos et à la Canée au MR IIIB: Du soupçon à la certitude», *BCH* 117, 1993, pp. 19-33.

that cast doubt upon this identification. I do not at all doubt that it is possible for a Knossian scribe to have worked also at Khania, or vice versa. With further discoveries we might even find out that KN 115 did so. But since the evidence is so doubtful, we should not build further hypotheses on such an unprovable speculation.

The existing palaeographical evidence does not prove the claim that any of tablets Ar 4, Gq 5 and X 6 from Khania are by the same hand as Knossos Hand 115[1]. This negative conclusion is not trivial, nor is it of interest merely to specialists in Mycenaean pinacology. Rather it calls for prehistorians not to use the claim that the tablets from Khania were written by scribe KN 115 from Knossos as the basis for further theories. In fact, the designation of the scribe of the Khania tablet as KH 115 is an ill-advised departure from the established system of designating scribal hands at any given site by particular unique numbers in clustered sequences beginning with one, some multiple of ten or some multiple of ten plus one: e.g., PY Hands 1-6, 11-15, 21-26, 31-34, 41-45, 91 (cf. *infra* n. 19); MY Hands 51-58*a*, 60-65; TH Hands 301-303; KN Hands 1-141, 201-225[2]. The designation KH 115 for the scribe of Khania tablets Ar 4 and Gq 5 was proposed by Jean-Pierre Oliver. It rein-

The interested reader will find good photographs of KH Ar 4, Gq 5 and X 6 in HVH2, plates 5, 6A and 6B, and GTz where the tablets are given preliminary designations as KH Ar 2, KH Gh 3, KH At 4. HVH2 and GTz also give drawings of the tablets according to their readings of the texts. These both differ in details from the readings of JPO. JPO, pp. 27-29, also provides a comparative table of sign forms (KH Ar 4, KH Gq 5, KN 115, KN 103, KN 104) which should be consulted.

It is perhaps worthwhile to stress here that international cooperation has always been a hallmark of Mycenology, especially in the publication of newly discovered tablets. Only in the last ten years have we seen the presentation of new Linear B inscriptions to the scholarly community confused by competing publications of the Thebes sealings and now the Khania tablets. These present the uninitiated scholar with rival numbering systems and varying readings that are not given the kind of relative weight they would be given by a true collaborative edition with *lectiones variae* in notes or apparatus. In the present instance, only those elite few who have the privilege of seeing the actual tablets under circumstances suitable for study can make a true judgment as to whether Hallager's or Godart's or Olivier's reading of a particular sign or tablet is to be preferred. This does a disservice to our linguistic, archaeological, art historical and historical colleagues. Please let us cooperate in presenting definitive first editions, after which individual scholars can put forward rival readings and interpretations.

I use the standard Mycenological abbreviations: *CoMIK, GORILA, KT5, TITHEMY.*

1 I agree with JPO, p. 30 n. 42, that KH X 6 is virtually illegible and therefore does not enter into the present discussion.

2 For an explanation of this system, cf. J.-P. Olivier, *Les Scribes de Cnossos*, Incunabula Graeca 17: Rome 1967, pp. 39-40; *TITHEMY* 11.

forces by nomenclature his argument that the scribe who wrote these tablets was the same as the scribe who wrote the tablets of Knossos Hand 115. His argument was made necessary by the fact that two preceding articles judged the handwriting of these tablets to be very similar to, but not the same as, the handwriting of KN 115[3]. Since this identity is the subject of controversy among scholars, it would have been better if the assignment of Hand number had followed the normal procedure which preserves the integrity of palaeographical analysis at each site, independent of personal scholary opinions[4]. At the outset I ask readers of this article to pay close attention to the footnotes (especially notes 22, 23, 29-31, 34-36) in which I discuss many details which are important for interpreting the evidence. Table 1.1-.10 ilustrates the individual points discussed in this paper and summarized at the end of it.

In 1989 and 1990 the Greek-Swedish Excavations at Khania under the general direction of Dr Yannis Tzedakis and the immediate supervision of Erik Hallager discovered the first Linear B tablets to be found on the island of Crete at any site other than the Palace of Minos at Knossos. These tablets now number six in all, two of which are still identified simply as «possible tablets»[5]. Of the other four, three (KH Sq 1, KH Ar 4, KH Gq 5) are fragmentary or damaged, but have texts that are sufficient for classifying the tablets by series, deducing the general purpose for which each tablet was written, and making some comparative palaeographical observations[6]. These tablets, despite their fewness and their limited texts, have serious consequences for our reconstruction of the Mycenaean period on Crete. They are important for our undertanding of: chronology, the destruction date(s) of the Palace of Minos, the relationship between different Cretan sites and regions, the internal administration of central and western Crete, the palaeographical tradition of Linear B on Crete and the mainland, and even, possibly, the career of a particular scribe from Knossos. Kh Gq 5 is also important for our understanding of Minoan-Mycenaean and what we conventionally call –simplifying heterogeneous elements and regional variations– Greek religion in

3 JPO, *passim*. The published alternative views of HVH2 and GTz are discussed below.

4 JPO, pp. 24-25, n. 32, adheres to the regular system in his assignment of number 501 to the Hand who makes erasures and writes *pu-na-si-jo* on KH Ar 4. He also explains this system in the cited note.

5 HVH2, p. 61 n. 2.

6 The text of KH X 6 is extremely fragmentary and difficult to reconstruct. Throughout I follow the readings of KH Sq 1, KH Ar 4, KH Gq 5, and KH X 6 presented in HVH1 and JPO. My own opinions on the reading of the first sign of the second sign-group of KH Ar 4.2 are not pertinent to the topic of this article.

historical times[7]. Here I concentrate on the palaeographical evidence of the Khania and Knossos tablets and its broader implications.

The Linear B tablets from Khania establish that Khania (= *ku-do-ni-ja* in the Linear B tablets from Knossos) had a Linear B administrations in the III A:2-III B period. The site had previously yielded a significant number of Linear A administrative documents (tablets, sealings and roundels) from the LM I B period[8]. It also had provided us with a good number (ca. 28) of inscribed stirrup jars (see E. Hallager, *AJA* 91, 1987 pp. 172-176). In the Knossos tablets *ku-do-ni-ja* was found in series dealing with: worker oxen (KN Ce 59.3b); oil (Fh 359.b); monthly rations of barley (G 820.1 *'ki-ri-ta'* LUNA 1); wool and woolen cloth (Lc 481.B, 7377.B) and chariots (Sd 4404.b:]*i-qi-ja ,/ ku-do-ni-ja , mi-to-we-sa-e , a-ra-ro-mo-te-me-na* [); and a clay label unfortunately fragmentary. Tablet KN Sq 1, with its record of 10 pairs of chariot wheels, confirms that *ku-do-ni-ja* was concerned locally at least with the storage of the component parts of chariots:

KH Sq 1]ROTA *ZE* 10[

Since chariots represent the most sophisticated military technology of the period, in terms of design, construction and component materials, and are attested in significant numbers at the palatial sites of Pylos and Knossos, this single tablet fragment reinforces the conclusion that *ku-do-ni-ja* was a site of considerable status and administrative importance in the Mycenaean (and specifically LM III B) period of Crete[9].

The tablets from Khania are related to the problem of the chronology of sites in Crete and to the date or dates of tablets from the Palace of Minos at Knossos. The work of J. Driessen on the Room of the Chariot Tablets archives from Knossos suggests that this peculiar group of tablets, dealing primarily with military equipment, is to be dated LM II-III A:1 and distinguished from the rest of the tablets from other areas of the Palace of Minos[10]. The second group of Khania texts are written in a graphic style that resembles in most details the hand of Scribe 115 at Knossos, to whom are attributed tablets in the following series: Lc, Od, Og, Vc, V and X. These come from areas E5,

7 KH Gq 5 contains a third reference in the Linear B tablets connected with the theonym Dionysus. It is the theonym proper, listed in context with the sanctuary of Zeus and the god Zeus. The other two references come from Pylos. It seems now that one of these Pylos ocurrences (*di-wo-nu-so-jo* on PY Xa 102) is in fact the genitive of the theophoric anthroponym *Diwonusios*. The full implications of these Dionysiac references will be treated by TGP in a separate article. For now, cf. GTz, pp. 143-147, and HVH2, pp. 76-81, 86-87.

8 Published mainly in *GORILA*.

9 On the status of Khania, cf. J. Driessen, *An Early Destruction in the Mycenaean Palace at Knossos*, Leuven 1990, p. 128 n. 453, with references.

10 Driessen (*supra* n. 9), p. 114.

F6-7, F13-14 and F16, i.e., the area of the West Magazines in the Palace of Minos. As Olivier points out[11], these tablets of Hand 115 are connected by findspots (bureaus) and inter-textual references with a dozen other scribes and ca. 1000 tablets from this area of the palace at Knossos. If one could establish an identity between the Khania hand and Knossos scribe 115, one would be able to date a second major group of tablets from Knossos to the period of the Khania tablets Ar 4, Gq 5, and X 6. If, however, the Khania tablets are simply in the style of KN 115, then one could imagine, for example, that one, two or more generations separate the Knossos tablets associated with KN 115 and the Khania tablets[12]. Because KN 115 works in a bureau which concerns itself, among other things, with Khaniote and western Cretan matters, and because scribes can be trained at Knossos within the graphic styles of departments and bureaus, it is easy to understand why KH Ar 4 and Gg 5 have a graphic style that closely resembles the bureau at Knossos concerned in part with Khaniote matters[13]. The arguments focused on palaeography therefore have crucial chronological implications.

We have a clear understanding of the contexts of the Khania tablets. KH Sq 1 was found in garbage pit H directly associated with LM IIIA:2 and LM IIIB:1 ceramic material[14]. But selected sherds from the pit date to MM and LM I and a few to LM IIIB:2. All the Linear B tablets we have were preserved by fire, so it is important to look for evidence of burning destructions at Khania. Burning destructions have been identified in LM IIIA:1, on earth floors of LM IIIA:2, in LM III A:2/IIIB:1 and even one of the rooms of the IIIB:2 settlement[15]. KH Sq 1 which is palaeographically within the Knossian tradition then cannot be dated with any absolute certainty, although it has been reasonably pointed out that it was deposited in LM III B and that LM III A:2-LM III B are the periods of the strongest archaeologically attested Mycenaean influence at the site[16].

KN Ar 4, Gq 5, and X 6 all were found lying on the floor of a room which contained a sealing found in sieving, fragmentary pieces of bronze,

11 JPO, pp. 31-32.

12 GTz, p. 137: «[R]ien ne nous interdit d'imaginer que plusieurs années, plusieurs dizaines d'années ou même une ou deux générations séparent les tablettes en linéaire B du scribe 115 des documents qui viennent d'être mis au jour à La Canée».

13 GTz, pp. 134-136. The implication is that Khaniote scribes would have been part of, or interacted with, the bureau at Knossos which dealt with Khaniote matters. Thereby Khaniote scribes, whether from Knossos or not, would have been graphically influenced by the Knossian 'Khaniote' bureau, and vice versa.

14 HVH1, pp. 26-27.

15 HVH1, p. 27.

16 HVH1, pp. 27, 31. For example, but not only, the inscribed stirrup jars from this period at the site. Cf. E. Hallager, «Khania and Late Minoan III Crete», *Cretan Studies* 1, 1988, pp. 115-124.

several obsidian artefacts, one stone tool, a piece of pumice, a fragment of a figurine and bone remains of butchering or food debris (the remains are of an adult female sheep, a young lamb, one or two subadult pigs and two newborn or very young piglets)[17]. Pottery and stratigraphy indicate a date at the end of LM III B:1 for the tablets and the destruction. It would be to this period that the ca. 1000 Knossos tablets would be dated, if it could be proved that KN Hand 115 = KH Hand 115.

The arguments and evidence are clearly laid out in HVH2, GTz, and JPO. E. Hallager has long been an advocate of the late (LM III B) date for the destruction of Knossos[18]. KN 115 = KH 115 would make it possible for champions of a late destruction of the Palace of Minos to claim that, barring a Methuselan longevity of KN 115, at least the destruction that preserved KN 115 and related tablets must be within the same archaeological period as KH Ar 4 and Gq 5, i.e., LM III B:1. As noted above, however, mere similarity between the graphic styles of Khaniote tablets and a Khaniote-W. Cretan bureau at Knossos would allow proponents of an earlier destruction, by arguing for palaeographical conservatism, to separate the tablets of KN 115 associated scribes from the Khaniote tablets by several generations: an LM III A:2 early destruction at Knossos would still be defensible.

It is clear that the new texts from Khania also very much complicate the current picture of the palaeographical development of the Linear B script. Palaima has cited parallels between the writing style found on a group of isolated tablets from Pylos which he designated as PY 'Hand 91' and that of a number of Knossian scribes as evidence for the existence of an earlier Cretan palaeographical tradition at Pylos, possibly as early as LH III A[19]. The further

17 HVH2, pp. 65-67 and n. 3A.

18 E. Hallager, *The Mycenaean Palace at Knossos: Evidence for Final Destruction in the Late Minoan IIIB Period*, Stockholm 1977, esp. pp. 89-94.

19 T.G. Palaima, *The Scribes of Pylos*, Incunabula Graeca 87: Rome 1988, pp. 111-113, fig. 11. These texts are isolated from the tablets that can be certainly attributed to the uniform destruction of the site. They have linguistic and palaeographical features that distinguish them from the rest of the Pylos tablets and align them with Knossian tablets. JPO, p. 25 n. 32, objects that the assignment of Hand 91 to this group should be eliminated because «il ne s'agit pas d'une 'main' à proprement parler...» However, his objections are only valid with regard to attributing all these distinctive tablets to Hand 91, since they are not linked to one another by find-spot or textual contents. But, given their palaeographical distinctiveness, one of these tablets (Xa 1419, the fullest tablet) should still be identified as Hand 91. The others should then be classified generally as Class iv, i.e., tablets in the general style of the defining Hand 91, but providing too little evidence in terms of numbers of signs, subject matter, find-spot or physical characteristics to support an attribution to Hand 91. We should note that Xa 1419 provides much more palaeographical evidence than the 'corrector' on KH Ar 4 which Olivier designates as Khania Hand 501.

argument is that this earlier Cretan tradition (represented by tablets from the site of Knossos) eventually evolved into, or was replaced by, the general mainland style attested widely in tablets from LH III B:1 and III B:2 destructions at Pylos and other mainland sites.

HVH2, pp. 82-85, emphasizes that Palaima also identifies points of similarity between the style of PY 'Hand 91' and that of painted stirrup jar inscriptions. Since the painted ISJ' s are dated to III B, it might be suggested that the differences between the normal mainland tradition and the Knossian-Cretan tradition are regional rather than chronological. While other factors might explain this similarity[20], it is now clear that the close resemblance especially between the style of KH Ar 4 (dated to the end of III B:1) and that of KN Hand 115 proves that the earlier Knossian tradition was still alive in western Crete in LM III B:1. There are further implications. Tablets of Pylos Hand 91 and Class iv (see *supra* n. 19) are closest in writing style to Knossos Scribes 104 and 137. The handwriting of Xa 1419 and Ae 995 especially only resembles Hand 103 at Knossos in sign *i*. But the Pylos tablets differ significantly from Hand 103 of Knossos in signs *so, ne, jo, to, i, no*, and VIR. Thus the Pylos Hand 91 and Class iv tradition resembles most that of area I3 at Knossos: *Area of the Bull Relief*. The single sign resemblance cannot be used to link the Pylos Hand 91 to Knossos Hand 103, and thereby to Knossos Hand 115 and ultimately to the Khania tablets. Still much depends on the exact identity of KN Hand 115 and the 'scribe' of the Khania tablets.

GTz and HVH2 both stress the palaeographical similarities between the tablets of Knossos Scribe 115 and the Khania tablets KN Ar 4 and Gq 5[21]. Yet each also points out that certain signs, in particular on KH Ar 4, are very unlike those of KN Scribe 115: HVH2, p. 75 (*na* and *pu* on KH Ar 4.2); GTz, 134 (*na*, *si* and *pu* on KH Ar 4.2)[22]. JPO also remarks that the scribe of KH Ar 4 and Gq 5 never cuts the lower rule lines with the vertical strokes of his signs, whereas KN 115 does this almost invariably on his

[20] Even HVH2, p. 84 n. 83, concedes that the isj inscriptions are a special case, perhaps either conservative or archaizing because they are *painted* and executed by persons not familiar with the script and who therefore might be using unsimplified *forms* of the signs, the exemplars learned by novices.

[21] GTz, p. 34; HVH2, p. 75.

[22] GTz, pp. 130-134, give a tablet-by-tablet commentary on sign resemblances between KN 115 and KH Ar 4 and Gq 5. Oddly in this commentary GTz, p. 134, claim that there are no sign matchups between KN V(3) 7620 –omitted from the list of tablets attributed to KN 115 in GTz, p. 130– and the Khania tablets, but in fact they have *po, ro, ta* and *wo* in common. GTz also do not discuss Og 7440 which is tentatively attributed to KN 115 in *KT5*.

many tablets[23]. HVH2, p. 74 and 34, otherwise note that the very size and/or shape of KH Ar 4 is similar to tablets of Hand 115 (V 479, V 655) *and* Hand 103 (As tablets) *and* tablets by other Knossos scribes[24]. Thus these scholars maintain caution, despite the attractive similarities. GTz. 135, conclude that «ces documents ont été écrits par un maître ou un disciple du scribe 115 de Knossos travaillant à La Canée». As we have seen, this is very unsatisfactory for solving important chronological problems. Such a neutral conclusion would make it possible to explain the close similarities between the Khania and Knossos tablets as the result of a conservative tradition (generated by the very structure of the administration at Knossos) spanning the period between LM III A:2 early (to which one group of scholars assigns the Linear B tablets of the West Wing at Knossos[25]) and tablets from the end of LM III B:1 at Khania.

JPO is designed to refute this conclusion and to justify the equation KN 115 = KH 115. The arguments are forceful. JPO, p. 21, notes with approval the opinion of GTz, p. 134, that, except for the three syllabograms, *pu, na,* and *si* on KH Ar 4, if the Khania tablets had been found at Knossos by Evans, there would be no hesitation in assigning them to Knossos Hand 115. HVH2, p. 75, go even further by claiming that the tablets, if found at Knossos, would have been assigned to KN 115 *despite* these palaeographical differences[26]. I

23 JPO, p. 30, nn. 46-47. This is a key difference in the habitual process of writing quite unrelated to forms of signs, i.e., it is an automatic detail of personal handwriting which external factors are unlikely to influence. Olivier explains this difference between the Khania and KN 115 tablets as resulting from the tablets being written at two different stages of the scribe's career. This type of *ad hoc* argument, if applied universally, would wreak havoc upon the entire method of scribal analysis. One could, for example, attribute the few habitual and constant differences between Hand 1 and Hand 2 within the chronologically limited collection of tablets from the major Pylos destruction to changes between one week and the next.

24 JPO, p. 20 n. 6, insists that KH Ar 4 and KN V 479 were manufactured by the same tablet-maker. The practical consequences of this suggestion –if tablets at Knossos really were manufactured by young children or senior citizens *and* if the Khania tablets were done at a later stage in a Knossian scribe's career– were not explored. Unfortunately only four tablets by KN 115 have meaningful papillary line traces, and these are not sufficient to identify a specific flattener or whether the flattener was a child. Cf. K. E. Sjöquist and. Åström, *Knossos: Keepers and Kneaders,* Göteborg 1991, pp. 46-118, tables I-III. V 655 in fact is larger, being 5 cm. high, while V 479 is a more exact match at 4 cm. high. For physical descriptions of the Knossos tablets, see Olivier (*supra* n. 2), pp. 57-58.

25 Driessen (*supra* n. 9), pp. 114, 117-123.

26 HVH2, p. 75, concentrate on *na* as the most crucial sign differentiating KH Ar 4 from the style of KN 115. Yet the same opinion is reached: «Had the tablet been found at Knossos it would undoubtedly have been assigned to scribe 115, in spite of the mentioned differences».

very much disagree with these highly rhetorical and dramatic opinions of GTz and HVH2 and the use which JPO makes of them. The stronger claim that one would attribute tablets to a particular scribal hand *despite inexplicable differences in sign shapes* does violence to the very principles of palaeographical analysis in Mycenology.

JPO tacitly recognizes this by insisting that one must explain these differences in order to justify such an attribution. Scribal identification depends on close examination and consistent comparison of details: how individual signs are conceived, how they are executed, what peculiar diagnostic tendencies scribes have in executing elements that one sign might share with another, how the signs are disposed on the tablet (e.g., their size, spacing, relationship to rule lines), the overall format of the text on the tablet (e.g., presence or absence of ruling above or below the first and last lines of text, width of margins, disposition of ideograms and texture of tablet) and so on. These features of the tablets of KN 115 and KH Ar 4 and Gq 5 are different enough to make the attribution of the KH Ar 4 and Gq 5 to KN 115 an incautious judgment even if the tablets had been found at Knossos. In fact the differences exceed those that distinguish Hand 1 from Hand 2 at Pylos, a distinction which was made both by Bennett and Olivier in their edition of the tablets and then treated in detail by Palaima, and which has never otherwise been challenged[27]. Scholarly respect for such differences is what called for the original identification of scribes within the *Room of the Chariot Tablets* as 124, '124' and '124' a-s. These tablets shared a peculiar graphic style, but also had small differences within their limited repertories of signs that made attribution to the same hand methodologically unsound. If the conclusions reached by J. Driessen in his restudy of these tablets are correct[28], then we have proof that this scholarly caution was essential for interpreting this group of documents correctly. We should not ignore our methodological principles now simply because the conclusions might have more important historical consequences.

27 E. L. Bennett, Jr. and J.-P. Olivier eds., *The Pylos Tablets Transcribed* II, Rome 1976, pp. 11-12, based on E. L. Bennett, Jr., «Tentative Identification of the Hands of the Pylos Tablets,» *Athenaeum* NS, 1958, pp. 328-331. T. G. Palaima, *The Scribes of Pylos*, Rome 1988, pp. 59-68, and esp. 66 n. 82, where it is pointed out that the very close similarities between different scribes at Knossos proper is due to work within the same bureau or department, each of which maintains and teaches its own graphic tradition.

28 Namely that these sub-divisions really do represent different scribes. J. Driessen, *The Room of the Chariot Tablets at Knossos: Interdisciplinary Approach to the Study of a Linear B Deposit* (Ph. D. diss.: Katholieke Universiteit Leuven 1989). The palaeographical section of this dissertation has not yet been published, so we shall refrain here from discussing what must be considered work in progress.

In my opinion, the scribe (or scribes?) of KH Ar 4 and Gq 5 exhibits serious differences from KN 115 in the forms of individual signs other than *pu, na,* and *si*. Even limiting ourselves to signs which are clear enough so that their pertinent elements can be read, these diagnostic differences include *(1) habitual tendencies in executing similar elements of different signs* and *(2) habitual tendencies in disposing elements of signs one to the other* and *(3) personal habits of ruling and layout and sign placement*[29].

As examples of *(1)*, KN 115 shows a disproportionate lengthening of the upper of two horizontal elements of signs, whereas the scribe of KH Ar 4 and Gq 5 does not. Compare their respective treatments of *to* and *po* (Table 1.6-1.7). KN 115 shows the same tendency in signs not attested on the Khania tablets: *pa* and *za* (cf. KN V 492, V 503, V 655) and *na* (Vc 7518) and *a* (Kn Vc 569) (Table 1.7)[30]. *This is a constant and habitual mannerism of the writing of KN 115 that we would expect to be present in the Khania tablets if they were written by him.* KN 115 also exhibits a conspicuous inward curving of the outer strokes of signs *ti* and *e* at the bottom, whereas the scribe of KN Ar 4 and Gq 5 does not. This feature (Table 1.3-1.4) is attested on KN 115 V 479 (*e* twice), V 503 (*ti*), V 7512 (*ti*), and even on the *ti*-like ear-element of *au* on Od 666 (!!!!), thus refuting the counter-argument by Hallager that this observation is based on a single occurrence of sign *ti* within the texts of KN 115, so that «[n]o palaeographer would thus argue...that our sign [i.e. the Khania version of *ti*] differs from the 'standard of KN 115'[31]». *This is another constant and habitual mannerism of the writing of KN 115 that would be present in the Khania tablets if they were writing by him.*

Olivier discusses one example of point *(2)* where the characteristic features of disposition of elements of a sign –in this case *di*– support his hypothesis that KN 115 = KH 115[32]. *However, there are other signs, e. g.* nu *and* po,

29 For a full discussion of criteria for defining scribal hands, cf. the remarks of Bennett quoted in J.-P. Olivier (*supra* n. 2), pp. 26-32, along with Olivier's discussion. Cf. also Palaima (*supra* n. 27), pp. 20-31.

30 The chart in JPO, p. 28, does not represent accurately how consistently and in many cases exaggeratedly this disproportional lengthening of the top element of *to* by KN 115 occurs: cf. KN V 482, V 488, V 655, V 7539, V 7620, Od 487, Og 7440, Od 486, Vc 7537 and all instances of *to* in *to-u-ka* on Lc 504, Lc 512, Lc 581, Lc 7377. This is a habitual, characteristic and conspicuous feature of KN 115's writing style which is not found in the version of the sign on KH Ar 4. In some cases, e.g., Lc 504 and Lc 512, the upper horizontal is nearly double the length of the lower. The only apparent exception occurs on Od 487 where the tablet breaks to the left of the *to* in line .a, thus giving the appearance that the strokes are somewhat equal in length. In fact the *ductus* of the upper stroke runs into the break, indicating that it extended further to the left. This perhaps is the particular instance of the deceptive *to* on the JPO chart.

31 *Per litteras* (February 5, 1994). Insertion mine.

32 JPO, p. 30.

where this same feaure is present that refute the attribution of KH Ar 4 and Gq 5 to KN 115 (Table 1.5-2.6).

Thus the argument should not be set in the terms in which JPO sets it, namely that the only serious difference that challenges the attribution of KH AR 4 and Gq 5 to KN 115 are the signs *pu, na,* and *si* on KH Ar 4. Nonetheless, let us review his line of argumentation, although it begins with what we consider a faulty premise. My shorter observations appear in italics.

JPO maintains that if one can find an explanation for the three anomalous signs on KH Ar 4, then the equation KN 115 = KH 115 is proved. He notes the remarkable fact that these three signs occur in the single sign-group *pu-na-si-jo* on KH Ar 4.2. This seems too much of a coincidence to be attributable to chance. A careful re-examination of the tablet indicated that *pu-na-si-jo* is written *in rasura. While I do not disagree that this sign group is over an erasure, the entire tablet is palimpsestic, and it was not possible to distinguish between the nature of the erasure and smoothing beneath these signs and the erasure and smoothing in patches beneath the text elsewhere on the tablet surface*[33]**.** Thus this erasure in and of itself does not prove that a second scribe intervened to erase the original text and write a new sign-group *pu-na-si-jo.*

The sign group *pu-na-si-jo* differs from the rest of what JPO, pp. 23-24, chooses to call KH 115 in four ways:

1. the forms of the four signs are different from those of KN 115;

2. the stylus used seems slightly more pointed or sharpened than the stylus used to execute the rest of the signs on the tablet, but only a study with a microscope might determine this conclusively;

3. The way in which the stylus was held produces a different angle from the other signs on the tablet (more than 45° vs. less than 45° relative to the table surface), but this could be the result of writing more slowly in a correction vs. writing quickly and normally in originally producing the rest of the text; *[It also might have to do with holding the stylus differently to avoid marring the text already written on the still moist tablet surface when the 'corrector' intervened.]*

4. the space between these four signs is greater than the between signs elsewhere on the tablet; the 'corrector' took advantage of the space available to separate his signs one from the other. *[Note that the spacing between* si *and* jo *is exactly the same as that between* to *and* so *written by the original scribes on line .3, while the space between* pu *and* na *the same as that be-*

[33] Emmett L. Bennett, Jr., shares this observation (*per vocem telephonicam*) with Ruth Palmer and me.

tween *56 *and the visible traces of the following sign on line .1 (and comparable to the spacing between* di *and* wi *in* di-wi-jo-de *on KH Gq 5). One should also note that the signs involved (*na, si, *and* jo*) have shapes which normally give the appearance of greater spacing between them and preceding or following signs.]*

JPO, pp. 25-26, likens this corrector scribe at Khania to Hand 1 at Pylos, the masterscribe and archivist who intervenes clearly in the work of other scribes[34], and identifies him as KH 501. He notes that the styles of the individual signs are palaeographically heterogeneous: *si* is written much as in Linear A; *na* resembles KN 125 and 225, and some Linear A forms; *pu* is closest to the styles of KN 103 and 221, but also PY Hand 15 and Hand 44; *jo* is like those of KN 113 and 141, and PY Hand 11 and S49 (Ciii).

Here I wish to assume the role of *advocatus diaboli.* As we have seen, HVH2 and GTz, when confronted with signs *pu, na,* and *si* on Ar 4.2, came to the conclusion that they were unlike the corresponding signs of KN 115. But KH Ar 4 and Gq 5 have so few total signs upon which to make a palaeographical identification (see below) that HVH2 and GTz concluded that these three signs were characteristic of the way the scribe of KH Ar 4 wrote, i.e., that these signs were his distinctive anomalies within the general palaeographical tradition. Keeping this in mind, JPO point (1) can only be used to prove that these signs were written by a corrector if one accepts that all other diagnostic signs on Ar 4 are so similar to those of KN 115 that KN 115 must have written KH Ar 4. We have already pointed out ways in which a significant percentage of the signs on Ar 4 differ in style from KN 115. We are dealing here with 15-18 total signs. I am not willing on such limited evidence to make large assumptions about how a scribal tradition at Khania would have developed. The four Classes of writing style at Pylos show heterogeneous and mixed features once one moves away from the 'type scribe'. How can we assume that a Khaniote scribe who is generally influenced by a Knossian bureau tradition in a mere fifteen documented signs –but even then does not follow the style slavishly– might not have preserved and used forms of *si* and *na* which descended from the strong Linear A tradition at the site? How can we assume that he, or the tradition of which he was part, might not have absorbed influences from other Knossian departments and bureaus? We might use an analogy from manuscript tradition. If manuscript X resembles an archetype in most ways, but shows some variant readings, do we then claim that the variants in X must be the products of contemporary influence

[34] Palaima (*supra* n. 27) pp. 111-113. JPO makes clear that the find context of the Khania tablets is such that one should not give this second hand the same status of master-scribe given to PY Hand 1 on the basis of his role in the central archives at the Palace of Nestor.

upon X or the writer of X? No. We assume a merging of the influences of various traditions through time that X then incorporates.

Do the other arguments in JPO add force to the claim of a 'corrector' scribe? No. Point (2) by Olivier's own admission is not certain. There is a reasonable alternative explanation of point (3): erasing and rewriting a text, particularly in mid-tablet and near the beginning of a particular line, is going to necessitate that the scribe hold his stylus at a different angle in order to avoid smudging the still moist text he wishes to preserve. With regard to point (4), the spacing within *pu-na-si-jo* is paralleled by the 'first' scribe elsewhere on the tablet. At the very beginning of the tablet he leaves an inordinately large amount of space after sign *po*, just as is seen after *pu-na-si-jo* (Table 1.10). Thus there is no compelling reason to accept that the sign group *pu-na-si-jo,* albeit written over an erasure, was written by another scribe. This only follows if we assume *a priori* that KN 115 = KH 115.

Nonetheless assuming that these anomalous signs have been explained away, JPO, 30, makes several other observations pertinent to both KH Gq 5 and Ar 4:

1. Sign *di* on tablet Gq 5 has a slight drift to right of the lower vertical relative to the upper three verticals; and this feature is shared with KN 115 alone among the 21 scribes at Knossos whom Olivier has repertoried. *[I agree. But, as mentioned above, I also insist that we must apply this same palaeographical criterion to all cases where it is applicable. Signs* nu *and* po *show consistent differences in this regard between the Khania tablets and tablets of KN 115. See further discussion below.]*

2. The first sign of [•]-*ne-wa-ta* on KH Ar 4.2 is difficult to interpret in a way consistent with the handwriting style of KN 115: *ko* and *wo* are difficult or nearly impossible.

3. The last sign of *di-wi-jo-de* on KH Gq 5.1, which is certain from context, is hard to reconcile with the *de* of KN 115. *[This difference is noted and then ignored!]*

4. KN 115 almost always cuts the rule line with vertical strokes, but the 'scribe' of the Khania tablets never does so. Olivier would attribute this to different psychological dispositions or two different stages in the life of the same scribe. *[This is clearly a form of special pleading (cf.* supra *n. 23) and only seems to have logical weight here because JPO has constructed his argument in advance to arrive at a point where all other objections to the attribution of KH Ar 4 and Gq 5 have seemingly been refuted –or ignored (*de *on Gq 5,* nu *on Gq 5,* po, to, qa, ti, e *on Ar 4). Thus the only remaining objection and a serious one because it falls into the same category of habitual, yet non-essential detail of writing style as the disposition of elements of signs* di *and* nu *and* po– *can be removed by*

an ad hoc *explanation that would never be accepted if applied within the more chronologically limited tablet groups from other sites.*[35]*]*

I do not doubt the reading of the erasure on KH Ar 4. Nor do I question the close resemblance of KN 115 to the style of KH Ar 4 and Gq 5. But I hope by now to have made clear that there are some serious problems in making the attribution of KH Ar 4 and Gq 5 to KN 115. In the remainder of this paper I shall discuss these and related problems further.

First, we should notice that both GTz and JPO treat KH Ar 4 and Gq 5 as an ensemble in comparing forms with KN 115. Only HVH2, p. 76, express some doubts on the palaeographical identity of these two tablets. HVH2 observe that KH Gq 5 contains too few signs for a palaeographical identification and only shares two signs with KH Ar 4. KH Gq 5 only offers ten relatively legible phonetic signs total[36]. The two signs that KH Ar 4 and Gq 5 have in common are *jo* and *so*. Olivier's identification of *jo* on Ar 4 as one of the four signs of Hand 501 means that *KH Gq has only one phonetic sign in common with first scribe Ar 4,* and that one-sign (*so*) is more different in form in its versions on Ar 4 and Gq 5 than the sign chart in JPO, p. 28, makes it appear.

Excluding *pu-na-si-jo,* KH Ar 4 contains only 11 signs that are sufficiently preserved to be used with confidence in palaeographical comparison

35 JPO, p. 30 n. 46, cites the observations on the writing of Pylos Tn 316 made by E.L. Bennett, Jr., «PU-RO *vacant* (PY Tn 316.7-10, v. 13-16),» *Colloquium Mycenaeum* (1979), pp. 232-234, as providing a parallel wherein the disposition or the temperament of a scribe affected his system of textual layout and his writing.Whether or not this scribe (Hand 44) went out to lunch between writing the *recto* and the *verso* of this tablet, the differences on Tn 316 are produced by difficulties in devising a format for recording some very complicated and unique information. By the end of the tablet the scribe has solved his formatting problem satisfactorily through experimentation. This tablet then is in no way comparable to the simple texts and entries on KH Ar 4 and Gq 5, nor does the 'lunch break' on Tn 316 cause the scribe to change his normal placement of signs on the rule lines, or to forego a bottom rule line. Quite simply Tn 316 is not a pinacological *comparandum* for KH Ar 4 and Gq 5. I have discussed in a recent paper how the formatting characteristics of Tn 316 are paralleled on PY Fr 1223, the only other tablet by Pylos Hand 44. Cf. «The Last Days of the Pylos Polity», in W.-D. Niemeier and R. Laffineur eds., *Politeia* (*Aegaeum* forthcoming).

36 Here HVH2 are observing what had been considered a standard principle in palaeographical identification, namely that, except in extraordinary cases such as a group of texts presenting sign forms, linguistic features and find-spots that are unique at a given site (e.g., Hand 91 and Class iv at Pylos), a certain critical mass of attested signs was necessary for assigning a scribal hand. J.-P. Olivier (*supra* n. 2) p. 40, used 30 signs as a general standard for the minimum number of signs necessary to make a certain scribal identification.

(*e, jo, ne, po, ro, so, ta, ti, to, wa, *56*). To this group we could add 4 signs that have traces preserved that could be diagnostic (*ka, qa, *83,* VIR). We have then 15 signs total, plus the layout and format of the text, the *ductus,* and the positioning of signs relative to one another and to the rule lines. Gq 5 offers even less: at most 9 complete or mostly complete signs (*di, jo, me, nu, ri, so, we, wi, wo*). There is another sign *de,* which, as we have seen, Olivier also considers diagnostic, although he ignores its implications. The standard target figure for a secure identification of Hand was set by Olivier as 30 signs (*supra* n. 36). Although we agree with Olivier that this 'target figure' should not be applied slavishly and that much in scribal identification depends on the experience and judgment of the pinacologist[37], nevertheless we must keep in mind here the scanty evidence with which we are working: 15 and 10 signs respectively, and only a single sign in common to establish that the scribe of KH Ar 4 is the scribe of KH Gq 5.

Because of the resemblance of the writing style(s) of these two tablets to KN Scribe 115, JPO and GTz group them for purposes of comparison with the Knossian scribe. Otherwise the laudable caution of HVH2 should and would have prevailed. *In order to stress the need for cautions here, we should note that on the basis of the same selection of 25 signs from the whole repertory of tablets of the major Hands 1 and 2 at Pylos, one would not be able to distinguish these two hands from one another!* The master scribe and his close associate at Pylos share most features of writing style, text layout and ductus, but they differ, and differ consistently, in the rendering of four key signs (*ja, o, ki,* and ra_2)[38].

Here is a summary and analysis of ten critical points of difference between the signs and writing style on KH Ar 4 and Gq 5 and those on tablets of KN 115. First I present the most obvious and certain differences between the signs of KH Ar 4 and Gq 5 and the signs of KN 115. In each case, the sign forms and peculiar stylistic characteristics of KN 115 are sufficiently attested to establish them as reasonably diagnostic for scribal attribution. That is, in no case is a characteristic identified by a *hapax* sign or single occurrence. Moreover, most of our observations can be seen in the tables of sign forms drawn by Olivier in JPO, pp. 28-30, or in the tablet drawings in *CoMIK*. I stress then that this is not a debate about how to read the shapes of particular signs. Rather I am pointing out important diagnostic details that JPO or HVH2 or GTz excluded or undervalued in their arguments.Table 1 provides

37 In this regard, the art of scribal identification resembles the attribution of vase paintings to particular artists and schools.

38 Palaima (*supra* n. 27), pp. 66-68, and 229-232.

illustrations of each of the following points, arranged according to subsections 1.1-1.10[39].

1. As JPO, 30, acknowledges, the *de* on KH Gq 5 is 'difficult to reconcile' with the standard version of the signs by KN 115 (V 488, V 655). Even if one allows for the fragmentary state of *de* on Gq 5, enough of the sign remains to show that it must have lacked the broad central-lower horizontal, the long arcing upper left oblique of the central element, and the secondary stroke at upper right in a position documented for KN 115.

2. More problematical still is the shape the composite ideogram *ṂḚ+ṚḮ* on KH Gq 5. Its squarish right-angle upper element is radically different from the standard forms of *me* or *ri* of KN 115 (V 479, V 482, V 655).

3.-4. The outer elements *ti* and *e* on KH Ar 4.1 do not curve inward at the bottom as do the standard *ti* and *e* (and the ear of *au*) of KN 115 (V 479, V 503, V 7512, Od 666).

5.-6. Most tellingly, however, JPO, rightly stresses the placement of the bottom vertical of the *di* to the right of the central of the upper three verticals of the sign as a distinctive habit of KN 115. However, he fails then to point out that KN 115 habitually places the central curves of sign *nu* so that the upper left of the lower curved stroke nearly bisects the upper curved stroke (V 482, V 503, Od 485). The upper curved stroke is also well off-center and nearly up against the left vertical stroke. Both these features are radically different from those of the *nu* on Gq 5 where the upper left of the lower curved stroke in fact sits clearly to the left of the entire upper curve and where the upper curved stroke itself is centrally positioned. Moreover, the elements of *po* on KH Ar are disposed in such a way that the upper two parallel horizontal elements terminate at the same point above the vertical stem of the sign, while there is a significant gap between the top of the vertical stem and the lower of the two horizontals. KN 115 writes this sign (V 492, V 503, Vc 7518, V 7620) always with the upper horizontal extending longer than the lower and to the right of the vertical stem. On all versions of *po* on tablets of KN 115 the vertical stem joins the lower of the two horizontals or extends up above it. One cannot invoke a habitual practice as confirming evidence for one sign (*di*) and ignore it as refuting evidence for two others (*nu, po*).

7. Another telling feature is so omnipresent and characteristic of KN 115 that it is remarkable that it has not been acknowledged. As mentioned above and just now seen in the case of *po*, KN 115 draws the upper of two horizontal elements much longer than the lower element. This is most conspicuous in sign *to* (V 466, V 482, V 488, V 655, V 7539, V 7620, Od

[39] The sign forms are taken from *CoMIK*, JPO, and, for tablets above 4495, my own drawings. The drawings of tablets V 479 and KH Ar 4 are from *CoMIK* and HVH2 respectively.

487, Od 486, Vc 7520, Vc 7537, Lc 504, Lc 512, Lc 581, Lc 7377), but it also occurs in other signs with horizontal pairs: *pa* (V 492, V 503, V 655), *na* (Vc 5523, Vc 7518, X 5538), *a* (Vc 569). *to* and *po* on KH Ar 4 both lack this well-attested idiosyncrasy KN 115.

Seven out of the twenty-five potentially diagnostic signs from the Khania tablets reveal significant differences from the ways these same signs are written by KN 115. In all cases the differences have to do with details of shape, conception and relative placement of component elements. That is, *these are features which characterize the peculiar writing styles of different scribes!* We can add here a possible eighth difference.

8. The discernible traces of sign *qa* on Ar 4 indicate that the bottom elements of the sign flange outward to the left and right. The left 'ear' of the sign also is affixed to the left curved stroke high up near the apex of its arc. This is very different in conception from the *qa* of KN 115 which has a perfectly vertical central stem and where the left 'ear' is attached lower down and entirely below the apex of the arc (V 466, V 7512, X 5538, X 7845).

If we take these significant differences into account, *we can no longer ignore the most conspicuous constant and habitual difference between the maner of writing on KH Ar 4 and Gq 5 and on the tablets of KN 115.*

9. *KN 115 almost invariably cuts the lower rule-lines in writing his signs. The writer of the Khania tablets –including the supposed 'corrector'– does not.* As mentioned above, we *could* invoke a temporal development within a single scribe's style as a desperate explanation of this glaring difference in habitual writing style. But this would only be reasonable if this were the only feature in which the Khania tablets differed palaeographically from the tablets of KN 115. However, as we have seen, there are many clear differences. This glaring difference in handwriting style should have been taken as an initial clue to the non-identity of the Khania and KN 115 tablets. Then the palaeographical features of sign forms just reviewed would have reinforced how different the two groups are within the general tradition of the Khaniote-W. Cretan bureau. Instead the general resemblance of the mere twenty-five signs of the two Khania tablets to the Knossos style of Hand 115 was given such weight that extravagant claims about their similarities, even their virtual identity, were made. HVH2 and GTz remained skeptical about the identity of the two groups, but based their skepticism chiefly on the sign-group *pu-na-si-jo* on KH Ar 4. This made it possible for JPO to adopt a line of argument that would make this sign-group the *only* major obstacle to attributing the Khania tablets to KN 115. Once it was removed, a feature like the cutting or non-cutting of the rule line could be explained away in chronological or psychological terms as a minor discrepancy.

10. There is one more difference between the KN 115 tablets and the Khania group that confirms that they were written by different scribes.

HVH2, p. 74, and JPO, p. 20 n. 6, correctly observe how similar tablet KH Ar 4 is in size and shape to KN 115 tablets of the V series: particularly V 479, but other fragmentary tablets like V 503, V 492, V 466, V 431, and V 429 can also be included here[40]. How do the KN 115 tablets compare to KH Ar 4 in formatting? *KH Ar 4 has three lines of entries. It is ruled into three lines by two rule lines, so that the final line of writing has no lower rule.* These lines are approximately 1.3, 1.3, and 1.6 cm. in height. The KN tablets of comparable size, ca. 4 cm. high, also contain three lines of text. V 479, which both HVH2 and JPO identify as most like Ar 4 in physical features, has texts on both *recto* and *verso. On V 479* recto *and* verso, *KN 115 has divided the same space into four lines by three rules lines, so that the final line of text has a lower rule.* These rule lines are approximately 1.2, 0.9, 1.1, and 1.1 high (*recto* and *verso*). This is no aberration. Wherever KN 115 has to place three lines of written text on a tablet approximately 3.5-4 cm. high, he does so by dividing the tablet into four lines by means of three rule lines: V 429, V 466, V 479, V 503, V 7620[41]. He then writes his text on the first three lines and leaves the final line (often a much narrower line than the first three) *vacat*. This is a trademark characteristic of KN 115. He likes to place his text on ruled lines, and he does so automatically. It is just as automatic a tendency of an individual writing style as (**.9**) his cutting of the rule lines by the lower elements of his signs.

Conclusion

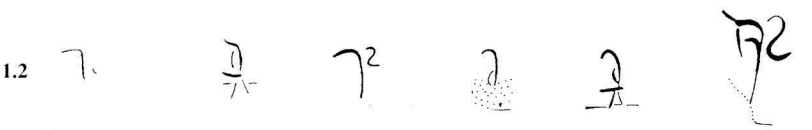
Why does the scribe of KH Ar 4 not follow this automatic habit of KN 115 about ruling a tablet (.10) in writing a text of the same length on a tablet of the same size? Why does the scribe of KH Ar 4 and Gq 5 not write his signs on the rule lines in the same way as KN 115, nearly invariably cutting the rule lines (.9)? Why does the scribe of KH Ar and Gq 5 write eight (.1-8) of his twenty-five signs in ways that vary conspicuously from the habitual ways KN 115 writes those same signs? On the present evidence, the only answer is that we do not have sufficient evidence to conclude that the scribe of KH Ar 4 and Gq 5 is the same scribe as KN 115. For now we must reconstruct the palaeographical and administrative histories of Knossos, Khania, Pylos and other mainland sites in LM/LM II-IIIB with the assumption that KH 115 ≠ KN 115.

Austin, TX 78712-1181 USA Thomas G. Palaima
PASP Classics WAG 123
University of Texas at Austin

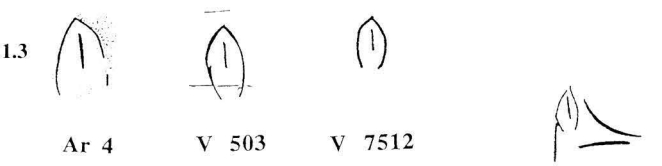
40 V 655 can also be included although it is slightly larger in size. Cf. Olivier (*supra* n. 2), pp. 57-58.

41 V 431 and V 492 are clearly designed as if to accommodate three lines of text.

1.1 Gq 5 V 488 V 655



1.2 Gq 5 V 479 V 482 V 655 V 479 Od 666

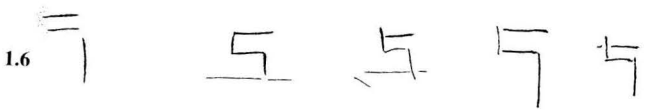


1.3 Ar 4 V 503 V 7512 Od 666

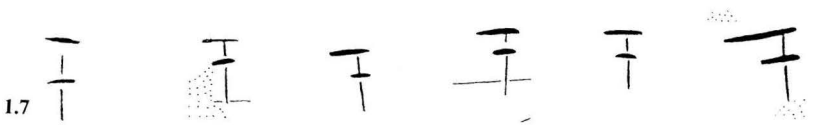
1.4 Ar 4 V 479*v*. V 479*r*.

1.5 Gq 5 V 482 V 503 Od 485

TABLE 1
1.1-1.5



1.6 Ar 4 V 492 V 503 Vc 7518 V 7620



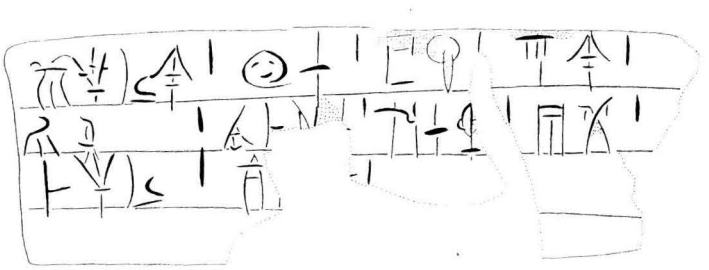
1.7 Ar 4 V 466 V 482 V 655 Od 486 Lc 504

V 492 Vc 7518 Vc 569

1.8 Ar 4 V 466 V 7512

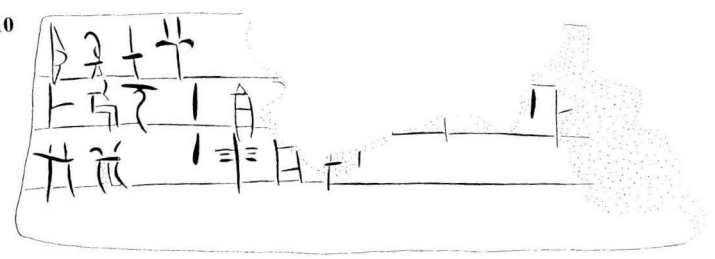
TABLE 1
1.6-1.8
1.1-1.5

1.9

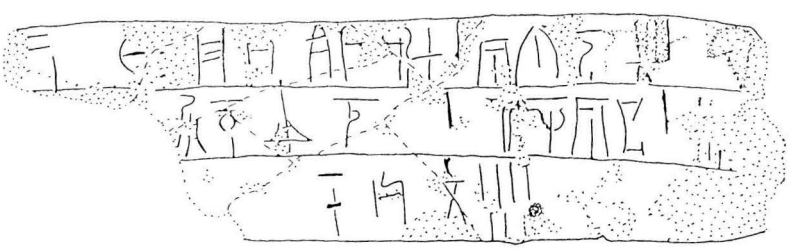


V 479*r*.

1.10



V 479*v*.



Ar 4

TABLE 1
1.9-1.10