

## OL Zh 1: QVOVSQVE TANDEM?

This volume<sup>1</sup> presents the full detailed report on the excavation in spring 1994 of a late Middle Helladic building near the village of Kafkania, 5 km. north of Olympia, specifically on a southwest-northeast-running ridge known as Agrilitises. The location of Agrilitises in relation to other Early, Middle and Late Helladic settlements in the area is well laid out by Arapojanni on pp. 53-58 (cf. site no. 1 on Abb. 12).

The excavation attracted widespread attention, and excited considerable controversy, because of the discovery on April 1, 1994 of a pebble interpreted as bearing genuine inscribed Linear B signs on both sides (see FIGURES 1 and 2 here). The oval-shaped pebble is 4.9 cm. by 4.08 cm. in length and width with a maximum thickness of 1.62 cm. It weighs 48 grams. It is dark greenish-grayish in color and has a lighter sandy-colored core.

If the piece is genuine, a fact which many Mycenologists still doubt, at least privately, it would pre-date the earliest group of Linear B documents (those from the Room of the Chariot Tablets assigned to LM II 1450-1400 BCE by J. Driessen<sup>2</sup>, the dating of which is not itself without controversy) by two centuries or more. We discuss some of the implications here below.

The inscribed pebble is also unique as a category of inscription —the parallel to the stone bar or weight inscribed with Linear B characters discovered by V. Adrimi-Sismani in Mycenaean Dimini (ancient Volos) is inexact<sup>3</sup>.

One side of the Kafkania pebble is dominated by the incised drawing of a canonical 'Minoan' double axe. It has what appear to be three strokes emanating radially from the rounded blade edges both on the left and on the right. These are widely spaced and are not drawn parallel to one another, as is usual with Linear B numerical signs for '1' (always drawn vertically) and '10' (always drawn horizontally).

They resemble the rays that radiate out from light bulbs in modern cartoons, especially when the light bulb is used as a figurative metaphor for a 'bright idea'.

<sup>1</sup> Xeni Arapojanni, Jörg Rambach and Louis Godart, *Kavkania: Die Ergebnisse der Ausgrabung von 1994 auf dem Hügel von Agrilitises*, Deutsches Archäologisches Institut, Abteilung Athen, Mainz, Philipp von Zabern 2002, pp. XXIV + 252, 16 plates. 78.00 Euros.

<sup>2</sup> *The Scribes of the Room of the Chariot Tablets: Interdisciplinary Approach to the Study of a Linear B Deposit*, Suplementos a *Minos* 16, Salamanca, Ediciones Universidad de Salamanca, 2000.

<sup>3</sup> V. Adrimi-Sismani, «Μυκηναϊκή Ιωλκός», *AAA* 32-34, 1999-2001, pp. 71-100, esp. p. 84, figure 5. The fragmentary text reads: *e-qe-qi*[-

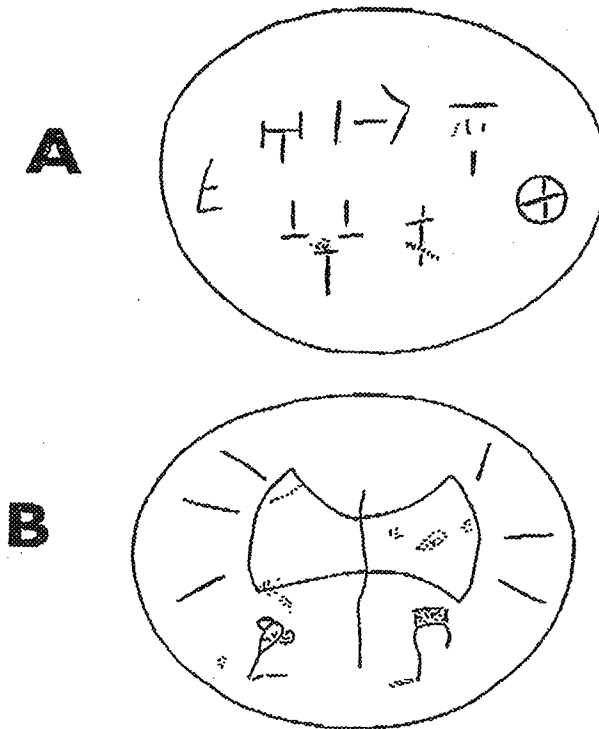


Figure 1: *Drawing of the Kafkania Pebble*, by L. Godart, *Praktika Akademias Athenon* 70, 1995, p. 252. The middle sign of the bottom row of signs on face a most resembles LB \*03 pa.

These have no exact parallels in Mycenaean or Minoan iconography or epigraphy. Godart in his commentary<sup>4</sup> entertains the idea that these rays are numbers, referring to ‘six’ axes in a transaction (more on this below). But the non-vertical orientation and the non-parallel disposition of the un-grouped strokes would make this unlikely.

Arapojanni therefore rejects this idea. She sees<sup>5</sup> the more or less radial strokes as pure decoration. But the parallels she cites (decorative dots surrounding a ball-like fixture atop the haft of double axes drawn on early Mycenaean vases *or* rows of dots used as separators for double axes painted frieze-like on other early Mycenaean vessels) are not real parallels.

The side with the double axe is considered in this volume to be side **b** of the pebble. This is an assumption based on a rather dubious ‘reading’ of the texts of

<sup>4</sup> P. 225.

<sup>5</sup> P. 64.

Dimensions de l'objet : largeur 4,9 cm. ; hauteur 4,08 cm. ; épaisseur 1,62 cm.  
 Poids : 48 grammes

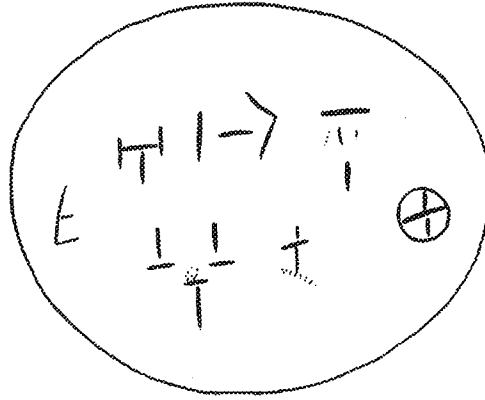


Fig. 1

OL Zh 1	FACE A	.a	a- <u>so</u> - <u>qa</u>	08-12-06	
		.b	qo-ro-ka	37-02-77	que nous lirons
			ka-ro- <u>qo</u>	(77-02-37).	

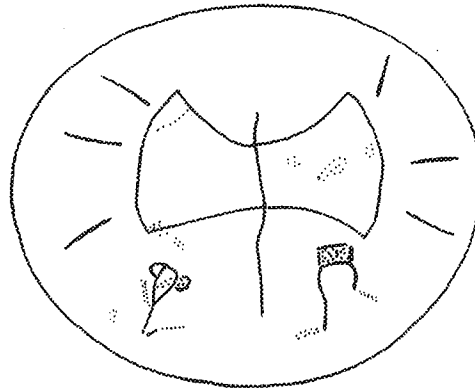


Fig. 2

FACE B      qa-jo      16-36  
 Les deux signes sont gravés de part et d'autre du dessin de la haste verticale d'une double hache.

2 E. L. BENNETT, *Ex-13 vacanti* (FY Tn 316.7-10, v. 13-16), *Colloquium Mycenaicum*, Neuchâtel 1979, 222.

Figure 2: Revised Drawing with Transcription of the Kafkania Pebble, by L. Godart, in X. Arapojanni et al., *Kavkasia: Die Ergebnisse der Ausgrabung von 1994 auf dem Hügel von Agrilitses*, 2001, pp. 213-214. Note that the lower part of the vertical hasta of the sign read now by Godart as LB \*02 ro has been removed.

the incised signs on the two sides. The drawn double axe is certainly the most distinctive feature of the pebble; and, with its cartoon rays, it is a real attention-grabber much like the prominent images found on the obverses of ancient and modern coins. Side **b** has two incised signs, one below each blade edge, to the left and to the right respectively of the central shaft or handle of the double axe.

What is called side **a**, giving primacy to the inscription rather than to the conspicuous drawing of the double axe, has two rows of linear signs, three signs in each row. When these signs are oriented horizontally, clear incised marks that might be part of another seventh sign are seen—and drawn in the illustrations in the volume under review<sup>6</sup>—to the left (see FIGURES 1 and 2 here). These clearly incised marks are disposed in vertical orientation midway between the two rows. They are dismissed by Louis Godart in his separate section on the inscription<sup>7</sup> as “un graffito n’appartenant vraisemblablement pas à l’écriture linéaire B”<sup>8</sup>. If they are not accidental, they can be interpreted (as done by Jean-Pierre Olivier, personal communication, and me) as a simplified form of sign LB \*60 *ra*, made ‘straight line linear’ by the difficulty of inscribing near the edge of the pebble surface. Its degree of simplification is paralleled on the same side of the pebble by sign LB \*12 *so*.

Godart offers no explanation as to why the individual who carefully executed the six signs on side **a**, the two signs on side **b**, and the double axe and cartoon rays on side **b**, would have incised a random graffito in this position on side **a**. If the writer of the main text and drawing did not inscribe the leftmost elements, then we need to explain why he chose to write his text on a pebble that someone else had already marred with a senseless graffito. Alternatively the ‘graffito’ could have been added after the main text and drawing. In this case, we must ask why the writer of the main text or a second party added it. If the marks on the left do make up a sign (LB \*60 *ra* being the best candidate), they are placed rather symmetrically with the right-most sign in row two (LB \*77 *ka*), i.e., vertically midway between the three signs of the top row and the two other signs of the bottom row.

The find circumstances of the Kafkania pebble are carefully explained by Xeni Arapojanni<sup>9</sup>. It was found in Quadrant B2/13 during the cleaning of the west side of a stone wall at depth -111. It came from the seam or gap between the top preserved layer of wall stones and the layer underneath it.

Why was such a small and insignificant object not simply thrown away with other debris in the cleaning process? Why was it examined closely, so that it was then kept for further study after its unique inscription and drawing were noticed?

Arapojanni explains that the workmen studied it carefully because rounded river pebbles are not usually used in wall construction and because a small whorl

<sup>6</sup> Abb. 2, p. 9; Tafel 13; fig. 1, p. 213.

<sup>7</sup> Pp. 213-240.

<sup>8</sup> P. 214.

<sup>9</sup> P. 6.

stone had been discovered earlier fairly close by in Quadrant 2/8. They picked up the inscribed pebble to see whether it had a perforation hole like the whorl stone. Thus did the *daimones* of archaeology smile upon this excavation and bring it great good fortune on what has been known as All Fool's Day or April Fool's Day, in a tradition beginning in France before spreading to England, Scotland and Germany (and thence to other countries, including Greece and the United States) from the sixteenth century CE onward.<sup>10</sup>

It is nevertheless hard to understand why the inscribed pebble was used as filling debris in a stone wall. Let us assume that writing on a pebble in this way was a fairly normal practice, i.e., the pebble was not an extraordinary historical *unicum*. That it was not an aberration is implied by Godart's 'administrative' reading of the text as a document recording the individual responsible for 6 double axes. In this case the pebble might have been discarded when it was no longer needed as an administrative record. We might expect then it to be set aside with other like texts.

In any event, it is a cause for dismay to think of how many such inscriptions have been casually thrown out in excavations since Schliemann first dug at Mycenae. Looking on the optimistic side, just think of how many inscriptions might still await us at Greek excavations sites, either in standing MH walls or in future excavations.

A small and rather primitive marble figurine, broken away at the knees, was found in Quadrant 3/10 at a lower level than the level at which the inscribed pebble was found. The figurine comes from an area with many sherds and considerable traces of burning<sup>11</sup>. In her thoughtful summary of the excavation

<sup>10</sup> The day is celebrated in Greece according to the on-line museum of hoaxes. On April Fool's Day 1982 ([http://www.museumofhoaxes.com/af\\_1982.html](http://www.museumofhoaxes.com/af_1982.html)): "Greece's state-controlled National Radio Network issued a warning that pollution had reached emergency levels in downtown Athens, and that the city would have to be immediately evacuated. All schools were called upon to close immediately, and the children to be sent home. Furthermore, anyone driving a car was asked to abandon it and flee to open ground. Many people took the broadcast seriously and attempted to leave the city, since pollution is a serious problem in Athens. Within three hours the Radio Network had retracted the broadcast, revealing it to be a joke, but by then the damage had been done. One man sued the network for \$820,000, claiming the prank had caused him mental distress. The director of the network submitted his resignation over the incident, and the originator of the hoax was fired." It is further reported (<http://www.museumofhoaxes.com/hoax/aprilfool/P90/>) that on April Fool's Day 1995, "the Greek Ministry of Culture announced that during excavation for the Athens metro system, archaeologists had uncovered what they believed to be the tomb of Socrates near the base of the Acropolis. A vase containing traces of hemlock (the poison used to kill Socrates) and a piece of leather dating from between 400 and 390 BC were found in the tomb. The news agency *Agence France-Presse* immediately issued a release about the story. What it didn't realize was that the Greek Ministry was joking, forcing the news agency to issue an embarrassed retraction a few hours later." It is difficult to know whether these reports in themselves are hoaxes.

<sup>11</sup> Pp. 10-12 and Abb. 3.

results<sup>12</sup>, Arapojanni speculates that the building in which the marble figurine and the pebble and the whorl were found might have had a different character from others at the site —there are some features of the building itself, like sea shell flooring material, that might support this. The inscribed pebble and the figurine were found, as she notes, at different levels and in different areas. The figurine was found on the floor with noticeable dark-ash traces of burning destruction. The pebble was found in a higher red ash fill level of the building. Arapojanni speculates that the inscribed pebble and the stone spindle whorl could have come from a niche in the mudbrick construction of the east wall of the building. But this is pure guesswork.

What is clear is that the building was destroyed by fire in the beginning to developed stage of MM III. Thus the Kafkania inscribed pebble, if genuine, would date in the 17<sup>th</sup> century BCE. Godart here gives the dates 1650-1600 BCE, but he suggests elsewhere that the date might be even slightly earlier<sup>13</sup>. So the pebble dates ca. 200 years or more before the earliest of our later Linear B inscriptions and a full 350 to 400 years before our main bodies of mainland tablet inscriptions. The relative chronology of the pebble and other Linear B material is clearly laid out by Godart on pp. 238-239. He also places these discoveries and their chronology into his view of the historical context on pp. 226-232.

There are two important issues here: palaeography and interpretation. Godart discusses palaeography on pp. 215-220. He points out that the forms of the signs on what he now calls OL Zh 1 have their closest parallels in the writing style of the ‘master scribe’ at Pylos: Hand 1<sup>14</sup>.

Indeed this is true of certain of the signs: LB \*08 *a* (version with one horizontal); LB \*12 *so* (version with one horizontal and the angle of the here truncated right element); LB \*32 *qo* (with horizontal instead of arching mid-strokes). One could even argue that sign LB \*36 *jo* (with the top loop of its bottom staff severely curved in, and a short, but strong quasi-vertical stroke coming down from an upper horizontal) is fairly close in shape to the *jo* on OL Zh 1. Godart<sup>15</sup> draws parallels for sign LB \*36 *jo* to examples on a Theban stirrup jar (TH Z 839) and on tablets from the Room of the Chariot Tablets. Godart reads a series of three small strokes coming down from the upper horizontal, but the surface in the area, according to his drawing and insofar as one can make out from the very poor-quality photograph of the pebble presented on Tafel 13 S1, is damaged and throws two of those strokes into question. Still the reversed ‘mirror image’ orientation of this sign on TH Z 839 demonstrates that the sign can be reversed without implying that the sequence of signs in which it occurs must be read in reverse of the normal left-to-right direction of reading Linear B texts.

<sup>12</sup> Pp. 64-68.

<sup>13</sup> Cf. *Praktika Akademias Athenon* 70, 1995, p. 254: ‘ή χρονολόγησή της στά μέσα του 17ου αλ. π.Χ.’)

<sup>14</sup> See T. G. Palaima, *The Scribes of Pylos*, Rome, *Incunabula Graeca* 87, 1988, pp. 35-58, 229-230.

<sup>15</sup> P. 219.

However, the sign interpreted as LB \*06 *na* on OL Zh 1 has no parallels at all at Pylos for its single upper horizontal. All Pylos scribes use two upper horizontals. And the sign for LB \*16 *qa* has its closest parallels, in terms of the execution of the central upright element and the placement of the ears, in Pylos Hands 12, 22 and 41. Again, with regard to what Godart interprets as LB \*06 *na*, the poor-quality photograph does not permit us to see whether what Godart reads as a non-epigraphical mark on the pebble might not be the third short vertical stroke of sign LB \*07 *di* in the form used by scribes of Classes ii and iii at Pylos.

LB \*07 *di* has only a single top horizontal. Therefore, when I first saw a provisional drawing of the pebble, I interpreted the sign here as LB \*07 *di*. If this is correct, however, this particular form of the sign is not found in Class i at Pylos. Read as Linear B \*06 *na*, the sign finds its best parallel (with the rare single upper horizontal) in KN Hand 116<sup>16</sup>. As for the treatment of the central elements, the best parallels are Pylos Hands 21 and 43.

Finally what Godart reads as LB \*02 *ro* Olivier (personal communication) and others have read as LB \*03 *pa*. The difference again arises from Godart interpreting as unintentional a mark on the pebble where the lower cross-stroke of *pa* would be, i.e., he thinks this mark is not intentionally written by the author of the pebble inscription. The poor-quality photograph gives no help, but the drawing in Abb. 2 would suggest that the upper horizontal stroke is far too high on the vertical *hasta* to be the sole central horizontal stroke of LB \*02 *ro*. Also when Godart published an early drawing of the pebble in the Greek newspaper *Estia* (22 February 1996) and even earlier presented it at the June 15, 1995 meeting of the Academy of Athens (under the sponsorship of Spyros Iakovidis), he drew the sign as having definitely a vertical staff that extended well below the lower horizontal mark in question (see FIGURE 1 here)<sup>17</sup>. If this is so, LB \*03 *pa* is a far more likely identification of the sign. He does not explain in the current volume why he changed his mind about what he sees on the pebble surface.

Why is all this important? Godart uses his interpretation of the central sign of the three signs in the lower row as the basis for reading the signs sinistrowise as *ka-ro-qo* = Greek *Kharopos* or *Kharops*. He claims<sup>18</sup> that the overall disposition of the syllabograms on face a show that the second line of signs is written *boustrophedon*. But there is nothing in the arrangement of the signs that would argue for a sinistrowise reading of line .2 –and a *boustrophedon* reading of lines .1 and .2.

There is no good evidence that Linear B was ever written sinistrowise. Its consistent left-to-right orientation is one of the puzzles of the script throughout its attested use. The few alleged instances of Linear A running sinistrowise (or *boustrophedon*) are all on objects where the writer of the text is following the

<sup>16</sup> Cf. Driessen, *RCT*, p. 320 Plate 31.

<sup>17</sup> *Praktika Akademias Athenon* 70, 1995, p. 252 B.

<sup>18</sup> *Ibidem*, p. 253.

shape or contour of the object, i.e., it is not a conscious choice to write every other line sinistrowise, as it would be here.<sup>19</sup>

Why should we read the second line on the pebble right-to-left? Why in the volume under review does Godart change, without explanation, the drawing of the middle sign to resemble LB \*02 *ro* more than LB \*03 *pa*? One answer would be so that the sequence of signs in the second line could be read in such a way as to yield an attested Mycenaean Greek personal name: *ka-ro-qa* Χάροπος or Χάροψ. If what is now read as *ro* is actually *pa*, this interpretation would be impossible.

The upper line or row of three characters Godart interprets as *a-so-na*, which is also attested in Linear B on PY An 129.6. There it might be an occupational designation<sup>20</sup>. But again this depends on reading the third sign as LB \*06 *na*, not as LB \*07 *di* which it more closely resembles.

Godart<sup>21</sup> adopts a suggestion of Anna Sacconi, that the two signs on side **b** should be read sinistrowise as *jo*, *qa* = δς (έ)βᾶ = the relative clause 'who came', with Kharops or Kharopos on side **a** as its antecedent. The whole text would read then: on side **a**: 'occupational term? / Kharops' and on side **b**: 'who came'. If the pebble were purely an administrative device, like a roundel or nodule, and the double axe were an ideogram used as ideograms are on the Linear B tablets to track economic items, and the rays of the axe were improbably read as numbers, then the pebble could make a strange kind of sense in an administrative context.

The disposition of the elements of the 'record' here and the absence of any surrounding or confirming traces of other such pebble record-keeping devices for over two centuries make this highly dubious. But in the realm of such speculation, the specification 'who came' could be taken as a reference that the delivery of the items in question was done not by an intermediary, but by Kharops or Kharopos as a primary or sole agent.

Another alternative, in my view, would be to interpret the two signs read normally, i.e., dextrorse, as a single word *qa-jo* = βαιός 'little', 'small', i.e., a description of the axe (scil. a masculine noun like πέλεκυς or \*λάβρυς). The axes would be the well-attested miniature double axes. The adjective βαιός has no clear etymology, and the first element could well come from a labiovelar. But again, I must stress that I am only trying to make sense of the inscription in case it proves to be authentic.

Another difficulty is that it is clear from the palaeographical history of Linear B, that the handwriting styles of Class i at Pylos (Hand 1 and related scribes) are developed styles, simplified from the more elaborate styles in use earlier on Crete. The notion that a highly simplified, i.e., developed, style could be attested

<sup>19</sup> See T. G. Palaima, «The Development of the Mycenaean Writing System», in J.-P. Olivier and T. G. Palaima eds., *Texts, Tablets and Scribes*. Suplementos a *Minos* 10, Ediciones Universidad de Salamanca, 1988, pp. 269-342.

<sup>20</sup> Godart, p. 221; Aura Jorro, *DMic* 1, s. v.

<sup>21</sup> P. 224.



in the MM III period on the mainland and continue in use for over four hundred years and be the style of the most dominant scribe at Pylos at the end of LH III B is *a priori* improbable.

The Kafkania pebble then has many odd singularities —not shared, by the way, by the other cited stone oddity (the stone bar or weight from Dimini)— its date, its form, its palaeography, its iconography, the meaning of its text, the difficulty of interpreting two of its signs, and the need to reject what seems to be another sign as a meaningless graffito. The interpretation of the text on what Godart calls side **a** requires questionable readings of two of the six signs, the unparalleled reading of the second line as sinistroverse, and the rejection of a sign that two Mycenaean palaeographical experts think might have been intended to be LB \*60 *ra*. Likewise, Godart's interpretation of the ray-strokes on side **b** as digit-strokes is rejected by his co-editor Arapojanni, and the sinistroverse reading of the two signs as *jo*, *qa* is suspect.

Where does that leave us? Now that over a decade has passed since its discovery and passions have cooled, it is reasonable to state publicly that all the above enunciated peculiarities of this text have led to the opinion that the Kafkania pebble may in fact be a forgery. That it was discovered on April Fool's Day may not be a coincidence. One privately circulated reading interprets the signs on face **a** to yield three personal names:

1. *a-so-na* = Ἰάσωνας = the first name of the son of Xenī Arapojanni and Jörg Rambach;
2. *ra-qa* = Rambo = a nickname of the same son;
3. *ra-pa-ka* = Rambach = the family name of the same son.

Following this line of speculation, it might even be that a sinistroverse reading *jo-qa* (indicated by the prankster-forgery of this document reversing the orientation of sign LB 36 *jo*) on face **b** is a not entirely successful attempt to convey the name Jörg, the father's name.

Continuing with our reasonable speculation, we would then have to ask from where the forger derived his knowledge of Linear B signs and their forms. Oddly enough, this is no simple matter. It is true that the shapes of many of the signs on face **b** conform to the shapes of the signs given in the standard sign-list, arranged systematically by phonetic values in what is known as a syllabic grid, given by Emmett L. Bennett, Jr.<sup>22</sup> This Wingspread sign-list (see FIGURE 3, column 1) according to Bennett, "imitates a hand of Pylos, Class I."<sup>23</sup> But the signs for LB \*06 *na* and LB \*16 *qa* are just enough dissimilar from the forms on the Kafkania pebble to call into question the use of this list as the source for the sign forms.

Also oddly enough, no one sign-list has become the canonical version used everywhere. Early on in Linear B studies, standard handbooks like those by M.

<sup>22</sup> In *Mycenaean Studies*, Madison 1964, p. 257.

<sup>23</sup> *Ibidem*, p. 254.

Ventris and J. Chadwick,<sup>24</sup> Leonard Palmer,<sup>25</sup> and the standard grammar by Ebbe Vilborg,<sup>26</sup> each had their own stylized way of presenting the signs. And each used forms that would have caused a novice forger to write a few signs differently than on the Kafkania pebble. Of these, however, Vilborg (see FIGURE 3, column 2), which would also have been useful for explaining values and spelling rules, comes closest to being the prototype for the Kafkania forms. Its forms of LB \*02 *ro*, LB \*03 *pa*, LB \*07 *di*, LB \*12 *so*, LB \*16 *qa*, LB \*32 *qo*, LB \*36 *jo*, and LB \*60 *ra* would all be workable exemplars. Only LB \*06 *na* is difficult, but its minuscule size in the Vilborg sign-list might have suggested a form with a single top horizontal.

Since the area of Olympia has traditionally been an area for excavation under the sponsorship of the Deutsches Archäologisches Institut, we might consider a German-language source for the sign forms on the Kafkania pebble. The forms of signs given in the standard German handbook on Linear B, S. Hiller and O. Panagl,<sup>27</sup> are ultimately derived from one of the Wingspread sign lists (and the chart was taken from *Acta Mycenaea*, Salamanca 1972, p. XVIII). However, these give poor exemplars for LB \*06 *na*, LB \*12 *so* and LB \*32 *qo*.

Another possibility is the sign-list of J. Chadwick<sup>28</sup> (see FIGURE 3, column 3). This sign-list has splendid exemplars for all signs except LB \*06 *na*.

But it is also possible that LB \*06 *na* and LB \*07 *di* were confused by the forger who was using a sign-list that presented the signs in their standard *numerical* order where the two signs appeared side by side or one on top of the other, as in fact they appear in my FIGURE 3 here. Such a mistake would be easy for the forger to make when working under pressure, especially if she or he were looking at a small-scale sign chart. In this case, the chart from the widely available *Decipherment of Linear B* would provide almost perfect models for all of the signs on the pebble. If the number of small verticals in the upper portion of sign LB \*36 *jo* in the damaged area of face b is really only one (1) in number, then the Chadwick chart would provide excellent prototypes for *all* the signs. Either way, it is a reasonably good source for the signs, if the Kafkania pebble is a forgery.

Another possible source is the sign list in the exhibition catalogue published by the Greek Ministry of Culture in conjunction with the celebration of Athens as the cultural capital of Europe (1985). This exhibition toured Greece for several years and included special workshops where Greek children could learn how to write different scripts, including Linear B.<sup>29</sup> The sign list (see FIGURE 3, column

<sup>24</sup> *Documents in Mycenaean Greek*<sup>2</sup>, Cambridge 1973, p. 23 and 41.

<sup>25</sup> *The Interpretation of Mycenaean Greek Texts*, Oxford 1963, p. 25.

<sup>26</sup> *Tentative Grammar of Mycenaean Greek*, Göteborg 1960, p. 24.

<sup>27</sup> *Die frühgriechischen Texte aus mykenischer Zeit*, Darmstadt, 1976, p. 68.

<sup>28</sup> *The Decipherment of Linear B*, Cambridge 1967, end figure 17.

<sup>29</sup> See S. Khrysoulaki ed., *Η Γέννηση της Γραφής*, ΚΕΝΤΡΟ ΕΚΠΑΙΔΕΥΤΙΚΩΝ ΠΡΟΓΡΑΜΜΑΤΩΝ ΥΠΠΟ 1990, pp. 50-51, 96, for the children's workshops, and p. 88, for the Linear B sign list.

Kafkania			1	2	3	4	5
A	B						

Figure 3: Comparative Table of Linear B Signs Written on the Kafkania Pebble

SOURCES:

- A = From *Drawing of the Kafkania Pebble*, by L. Godart, *Praktika Akademias Athenon* 70, 1995, p. 252.
- B = From *Revised Drawing with Transcription of the Kafkania Pebble*, by L. Godart, in X. Arapojanni et al., *Kavkania: Die Ergebnisse der Ausgrabung von 1994 auf dem Hügel von Agrilitses*, 2001, pp. 213-214.
- 1 = Signs from *List of Linear B phonograms*, from E. L. Bennett, Jr., ed., *Mycenaean Studies*, 1964, p. 257.
- 2 = Signs from *List of Linear B phonograms*, from E. Vilborg, *A Tentative Grammar of Mycenaean Greek*, 1960, p. 24.
- 3 = Signs from *List of Linear B phonograms*, from J. Chadwick, *The Decipherment of Linear B*, 1967, end figure 17.
- 4 = Signs from *List of Linear B phonograms*, from S. Khrysoulaki ed., *Η Γέννηση της Γραφής*, ΚΕΝΤΡΟ ΕΚΠΑΙΔΕΥΤΙΚΩΝ ΠΡΟΓΡΑΜΜΑΤΩΝ ΥΠΠΟ, 1990, p. 88.
- 5 = Signs from *List of Linear B phonograms*, from J. Chadwick, *Γραμμική Β και συγγενικές γραφές*, 1992, p. 30.

4) of Linear B from this volume offers reasonable exemplars for all the signs on the Kafkania pebble except the universally problematical LB \*06 *na* (or LB \*07 *di*). The publication date of this widely circulated catalogue and its general availability make it a prime suspect as a source for the writing on the Kafkania pebble. Likewise timely is the table on p. 30 of J. Chadwick, *Γραμμική Β και Συγγενικές Γραφές*, Athens 1992 (see FIGURE 3, column 5).

Given the popularity and availability of the Khrysoulaki and Greek Chadwick books and sign charts and the specific teaching mission of the exhibition Η Γέννηση της Γραφής, it is not impossible that the writing on the pebble was a proud schoolboy exercise later used as a harmless prank. In this case, we might even read on side **a** *pa-ka* as Πάσχα (Greek Easter) and on side **b** *qa-jo* as βάλων (as a reference to Palm Sunday). The rest of the text on side **a** would read: *Iasonas Rambo*.

There is a law in archaeology and in many sciences: *unus testis nullus testis*, i.e., 'one witness is no witness'. We should certainly apply this law to the Kafkania pebble. Godart argues here that linguistic features within the Linear B script prove that Linear B developed over a considerable length of time before its first attested use. In particular he mentions Michel Lejeune's and Leonard Palmer's observation that sign LB \*62 *pte* must have had at first the value \**pje*. LB \*62 *pte* appears once in the RCT on tablet V 159, already with value *pte* in the word *ra-pte-re*. Unfortunately there is no way of knowing how long a time span it would have taken for this process of development of */pye/* into */ptel/* to become universalized as the value for sign LB \*62. The sign has no attested Linear A prototype. It is even theoretically possible, since it appears only once in the RCT texts, that at that time the sign represented both values and that in other words it might have still retained the value \**pje*.

I had the good fortune to read Thucydides with Nicholas G. Hammond in graduate school. The residue of this thoroughly humanistic experience still stays with me. Also I remember that he once reminded us graduate students that a year in antiquity in any period is as long as a year now. Even a short pottery phase like LM II lasted at least half a century. Who can know how long before our first attested tablets Linear B was invented and where? The same problem plagues the origins of the Greek alphabet. There scholars posit a generation or two (30 to 60 years) before the earliest texts as adequate for the invention of the script.

If we apply something like the same reasoning here and also look for a period when the mainland Greeks would have had strong motives to adopt record-keeping to manage economic organization that had become too complex for non-literate control mechanisms and systems, then LH II (1500-1400 BCE) would be the earliest, in my opinion, that we would want to place the use of the script on the mainland. This is not to deny that certain sites show considerable sophistication even in the MH III period. But they in no way approximate the complexity of Minoan sites, individually or in relation to one another, when they adopted the use of Linear A, or of later Mycenaean palatial sites when

Linear B was in full use. Thus on purely historical grounds the arguments advanced by Godart<sup>30</sup> strike me as unconvincing.

The sealings of the House of the Tiles at Lerna and the sealings at Monastiraki in Crete indicate that fairly elaborate and well-developed sites can function without written texts. Far from creating an *a priori* assumption that some form of writing must have existed between these sealing finds and the full Mycenaean palatial period on the mainland, they re-emphasize, in my mind, the fact that traditional ways of handling transactions and the movement and control of resources and materials were effective and highly conservative. Some quantum leap in social, political and economic complexity or some radical change in political and cultural relations among and within mainland regions and in turn between them and Crete would have been needed to call Linear B into being.

Thus even on historical grounds it seems highly unlikely that Linear B writing existed in MH III in the northwest Peloponnese in any form. That said, I hope to be proved wrong by a carefully controlled MH III or LH I excavation that will find another inscribed pebble or more typical clay documents that are less iconographically, linguistically, palaeographically and textually problematical.

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<sup>30</sup> Pp. 229-231.