

## THE RELATIONSHIP BETWEEN INSCRIPTIONS ON HIEROGLYPHIC SEALS AND THOSE WRITTEN ON ARCHIVAL DOCUMENTS \*

### Introduction

First of all, I have some good news to report. In April of last year, speaking in Madison, Wisconsin at the Burdick-Vary symposium on "Problems in Decipherment" in honour of Emmett L. Bennett, Jr., on the subject "The Problems in Deciphering the Hieroglyphic Cretan Script" <sup>1</sup>, I first stressed that the main problem concerning this script was the shortage of texts. To quote my own words :

"The quantity of the documentation is, and seems to have a tendency to remain, dramatically poor. The paucity of the material can be illustrated by quoting some figures which are to be compared with the corresponding figures for Linear A and B. [...]

So, for the Cretan Hieroglyphic Script, we have less than 300 documents and a little more than 1500 signs, representing respectively 4% and 2% of the whole Minoan-Mycenaean epigraphical material.

Moreover, not only is the number of documents low, but the rate at which they are being discovered is slowing down in comparison with the period of the earliest discoveries. [...]

As you know, Evans found the first Cretan Hieroglyphic archives at *Knossos* in 1900. This archives consists of 61 inscriptions with 615 signs.

Incidentally, an erratic tablet—and only one—in hieroglyphic script was found in 1901 at Phaistos.

After that we have to wait until 1923, when Renaudin and Charbonneaux found in the *Palace at Mallia* 24 inscriptions with 189 signs.

But after this we have to be patient until 1968 for the beginning of the next significant group of discoveries : 21 inscriptions with 79 signs excavated by Poursat in the *Quartier Mu of Mallia*; the ultimate document of this series appeared in 1977.

Of course, it is perhaps a coincidence... But if we extrapolate the trend, we shall deal with negative numbers in 1992..."

Fortunately, the trend was inverted last summer, and that is the good news to which I alluded : Alexandre Farnoux found, at the North of Quartier Mu of Mallia, three fragments of tablets bearing all together 10 signs <sup>2</sup>.

Of course, it is neither a library nor even an archive deposit. The area was disturbed *both* in Late Minoan *and* in modern times. But the three documents are exactly of the type of the "lames à deux faces percées d'un trou de suspension" of which two very similar (nos. 3 and 7), one rather similar (no. 6) and three fragments (nos. 1, 4 and 5) were found in Quartier Mu.

So, we are permitted to entertain some hopes. Since excavations will resume this year, we

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\* I thank Miss Patricia Robins and the editor of this volume for considerably improving the English text of this paper.

1 See Y. DUHOUX, T.G. PALAIMA and J. BENNETT (eds.), *Problems in Decipherment* (BCILL 49 1989) 39-58.

2 These fragments are published in *BCH* 113 (1989) 97-100.

can be less pessimistic than in the beginning of 1988. These excavations perhaps will offer us the opportunity of celebrating very much the centenary of the official rediscovery of the Cretan scripts of the second millennium B.C. in general and of the Cretan hieroglyphic script in particular.

Since the date (1889) of this second birth and its circumstances are explicitly given by Arthur Evans himself in 1909 in *Scripta Minoa I*<sup>3</sup>, an unfortunately insufficiently used book, the least I can do is to cite our honorable ancestor in his own terms :

"The possibility that a form of writing closely resembling, or identical with, the Hittite had been introduced into prehistoric Greece, was first brought before me in a practical form in 1889. In that year a four-sided bead-seal of cornelian, bearing on each facet a series of figures as shown in Fig. 4, was presented amongst other objects to the Ashmolean Museum by that well-known antiquarian traveller the late Mr. Greville Chester"<sup>4</sup>.

But enough so far as the history of discoveries is concerned. Let us return to the present reality. This reality must first be described accurately in **quantitative** terms, not only because not everyone is familiar with the Cretan hieroglyphic script but also because some people have the sad habit of offering a biased presentation of the evidence.

In short, our Corpus is composed of two distinct parts :

1. Seals and sealings (*ca.* 150 documents)
2. Other documents (mainly archival inscriptions) inscribed on clay (*ca.* 120 documents).

The *seals and sealings* represent about 307 distinct sign-groups, consisting all together of  $\pm 832$  signs. The *other inscriptions* represent about 274 distinct sign-groups, consisting all together of  $\pm 723$  signs.

In the first group, I do *not* take into account in my calculations either the *decorative* elements or the possible "symbolic" elements (representations of human beings, of animals or of other entities, etc.) which add perhaps something to the *general* meaning of the sign-groups but have certainly no *syllabic* value. In the second group, I do *not* take into account signs which are obviously not syllabic, i.e., the logograms, the fractions and the numbers which represent about 280 signs which are part of the script, of course, but not of the syllabary. In both cases, I omit instances of the so-called "initial cross" (115 in the archives and about 72 in the seals)<sup>5</sup> since it is a diacritical mark, not a syllabic sign.

Thus, we are left with the signs in sign-groups *only*. If we take all of them *indiscriminately* (that means 581 sign-groups containing 1555 signs) we can establish that the seal material represents slightly more than half of the documentation (roughly 53%) and the archival material just less than half (roughly 47%). But, if we count only *once* all the sign-groups which appear more than five times (there are 2 such sign-groups on archival documents and 9 on seals<sup>6</sup>), our statistics change significantly. In archival documents we still have 262 sign-groups and 699 signs, but on the seals we have only 164 sign-groups and 492 signs. All together, there are 436 sign-groups containing 1191 signs. 62% of the sign-groups and 59% of the signs contained in sign-groups occur on archival documents; 38% of the sign-groups and 41% of the signs contained in sign-groups occur on seals.

We can then say that the significant and useful material represents about 60% of the documentation, because the identification of the signs of the syllabary must be based primarily on the archival documents, where signs are genuinely scratched on clay, and not on the seal

3 *SM I*, 8.

4 Fig. 4 shows the drawing of P.44.

5 We cannot be more precise because sometimes there are 2 or 3 initial crosses in front of a sign-group!

6 The most popular appearing more than 60 ( $\text{𐀀} \uparrow$ ), 30 ( $\text{𐀀} \text{𐀁}$ ) and 23 ( $\text{𐀀} \text{𐀂} \text{𐀃}$ ) times respectively.

documents, where they are more or less artificially placed and engraved in order to be automatically impressed on clay.



















Another point is the **qualitative** aspect of the documentation on seals. If I am more strongly convinced than ever that the script on the seals is a *decorative* one, I must confess that I have become more indulgent since my communication to the Marburg Symposium in 1978<sup>7</sup>. This policy is based on a fuller knowledge of seals and sealings and upon systems which I have devised in order to make some apparently useless seals usable (I do not say useful). One example will suffice.

P.22 (Pl. Ia), a white cornelian prism, presents on each of its three faces a sign from the syllabary *and* a sign which is obviously not from the syllabary : the 'gate', the 'single axe' and the 'arrow' are signs from the syllabary, known from archival documents; the 'pig', the 'kid' and the 'scroll' are *not* signs from the syllabary. Just for fun, I shall mention Evans' suggestion that face *a* conveyed a title like "keeper of the swine" and face *b* a "sacrificial function". *If* this document bore a real inscription, only a person knowing the *written* word was able to read it.

For us, there are six possibilities (all from left to right or all from right to left) :

- 1)   
- 2)   
- 3)   
- 4)   
- 5)   
- 6)   

Only one is correct, unless there was no inscription at all or some pun was intended. Since we have no parallel texts, no solution is at hand, except if I present one form in normal size (with a diacritical mark indicating that it is perhaps a ghost form) and the five other ones indented<sup>8</sup> and smaller sized with the same ghost indicator :

  	!
  	!
  	!
  	!
  	!
  	!

The same method can be used for the sign-groups for which the direction of reading is uncertain. In such cases, one form with the diacritical mark for ghost will be full size and the other one smaller size, always with the same indicator. The same applies to 'circular' inscriptions, where we know neither the direction of reading, nor which sign was the first one. So we present P.31b (Pl. Ib) according to the theoretically possible readings :

7 J.-P. OLIVIER, "Les sceaux avec des signes hiéroglyphiques. Que lire ? Une question de définition", *CMS Beiheft* 1 (1981) 105-115.

8 In the index, indentation indicates alternative forms stemming from conjectures in the apparatus. These are not taken into account in the statistics.

- |    |     |                   |
|----|-----|-------------------|
| 1) | 𐤃𐤆𐤏 | clockwise         |
| 2) | 𐤆𐤏𐤃 | "                 |
| 3) | 𐤏𐤃𐤆 | "                 |
| 4) | 𐤃𐤏𐤆 | counter clockwise |
| 5) | 𐤏𐤆𐤃 | " "               |
| 6) | 𐤆𐤃𐤏 | " "               |

Of course, once again, only one solution is correct; and if we do not know which one, we have to resort to the same method as above :

𐤃𐤆𐤏	!
𐤆𐤏𐤃	!
𐤏𐤃𐤆	!
𐤃𐤏𐤆	!
𐤏𐤆𐤃	!
𐤆𐤃𐤏	!

Fortunately, in this specific case, we know the correct answer. It is the third reading : 𐤏𐤃𐤆, because we find this sequence on the clay bar P.117a, as we shall see later.

After these preliminary explanations, I can now discuss my subject : the relationship between inscriptions on hieroglyphic seals and those on written archival documents. But not directly : some precautions are still necessary.

### Prolegomena

1. I shall admit that the hieroglyphic archival documents (bars, labels, nodules with or without seal impressions) from Knossos and from Mallia (palace and Quartier Mu) come from archive deposits (which is probable, but not proved).

2. I shall consider here as certain, for the sake of convenience, that the inscriptions in hieroglyphic script on seals are *true* inscriptions, even if their decorative aspect must never be lost sight of<sup>9</sup>.

3. I shall take for granted that *the script* of the hieroglyphic inscriptions carved *on seals* and *the script* of the hieroglyphic inscriptions written *on archival documents* is *the same*. Thus, those differences in the forms of the signs that we can detect result only from the use of different supporting media, engraved with different tools in order to perform different purposes (diachronic, social and administrative). We must not interpret this evidence by introducing the notion of a dichotomy between a "monumental" and a "cursive" script (as we rightly do in the case of the hieroglyphic Egyptian and of the hieratic Egyptian).

4. Lastly, I shall imply that *the language* written by the hieroglyphic script is *the same* on the seals and on the archival documents; but this point has little relevance to the present discussion.

The relationship between the two categories of inscriptions can be studied from two points of view : internal (comparative) and external (finalist). The first viewpoint is purely mechanical but implies, to be satisfactorily achieved, not only a good mastery of the overall material but

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9 *Supra* n. 7.








also a clear conception of what a hieroglyphic inscription is. The paucity of our documentation is a serious drawback in these matters. The second viewpoint demands not only a full knowledge of all the seal material of the protopalatial period (and even of the end of the prepalatial and of the beginning of the neopalatial period), but also as accurate a knowledge as possible of the mechanisms of the Minoan administration. I need not emphasize that we are at this conference in order to try to make some progress in understanding the mechanisms of Minoan administration and that my own views are still tentative.

### A. Internal comparisons :

At the present stage of our documentation and of my knowledge, the exact number of certain <sup>10</sup> matches between inscriptions on seals (and sealings) and inscriptions written on clay does not exceed 5. In addition there are 4 possible <sup>11</sup> matches.

#### a. Certain matches <sup>12</sup> :

- |    |   |   |
|----|---|---|
| 1) |  | : already seen by Evans <sup>13</sup> . |
| 2) |  | : already seen by Evans <sup>14</sup> . |
| 3) |  | : already seen by Evans <sup>15</sup> . |
| 4) |  | : new <sup>16</sup> .                   |
| 5) |  | : new <sup>17</sup> .                   |

10 By "certain" I mean *complete* sign-groups whose signs are securely identified, whose sequence of components is unambiguous and whose direction of reading is clear.

11 "Possible matches" means that some problem exists about the correct identification of a sign or the exact sequence of the signs or the direction of reading. The cases cited by Evans and not mentioned here (*SM* I, 261, fig. 116.a.b.e.f.h.k.m.n.o : i.e., 9 out of 14) fall in one or more of these categories and concern mainly *parts* of sign-groups.

12 For the references, see *Appendix*.

13 *SM* I, 260 and 261, fig. 116.c.





14 *SM* I, 260 and 261, fig. 116.d.

15 *SM* I, 261, fig. 116.p.

16 Evans was theoretically able to find this match, since he published in *SM* I the sealing and the nodule on which this sign-group appears. But, if he correctly identified the 'glove' and the 'bee' on the sealing P.51a (Pl. Ic) and on the nodule P.76a (Pl. Id), as well as the 'calf's head' on P.51a, he curiously never made a proposal for the equivalent of this last on P.76a and, worse, he gave two different numbers (122 on the sealing and 93 on the nodule) to the second sign [122 : 'character of uncertain meaning'; 93 : 'heart-shaped figure with specks of grain']. Even if he had correctly identified all the signs, their correct sequence still would not have been evident on the sealing : which sign is to be read as the second and which one as the third ? Only the knowledge of the word gives the answer, and fortunately we have it on the nodule where the arrangement of the signs is linear ! This is another fine case of the ornamental nature of the hieroglyphic script on the seals. This example also shows another characteristic of this script, unfortunately not limited to the seals : the signs have neither real "tops" or "bottoms" nor real "right sides" or "left sides" (the 'glove' is necessarily drawn upside down on one of the two documents; if the 'calf's head' is taken "normally", the 'bee' lies on its back in its two occurrences; finally, if the 'heart-shaped' sign is "horizontal" on the sealing, it is "vertical" on the nodule—and vice versa, of course). Keeping this feature in mind, it becomes impossible to speak about the "normal" or "standard" position of a sign, either on the seals or on the written archival documents. An important consequence of this, is that the isolated sign-groups themselves (and consequently the documents bearing only one sign-group : especially the different faces of the prismatic seals) have neither top nor bottom, neither right nor left. They only have a beginning and an end, and only when we know where to find it. In these circumstances, speaking about "verticality" or "horizontality" of the face of a hieroglyphic seal is probably nonsensical.

17 Evans ignored the two seals on which this sign-group is attested.

### b. Possible matches <sup>18</sup> :

- 1)  : already seen by Evans <sup>19</sup>.
- 2)  : already seen by Evans <sup>20</sup>.
- 3)  : new.
- 4)  : new.

### General remark

All the matches involving seal impressions and clay inscriptions are from Knossos <sup>21</sup> (the origin of the seals is of course most of the time unknown or subject to doubt). Given the paucity of the material, we are unable to say if this is due to chance or not. It is certainly interesting to notice that there are no exact matches in the clay archives between Knossos and Mallia <sup>22</sup>, although the internal similarities are relatively frequent : 8 at Knossos and 3 at Mallia <sup>23</sup>.

### B. External comparisons :

Let us only consider the "certain matches", and let us make a firm distinction between the first two sign-groups (overabundant and obviously of a special character) and the three last sign-groups.

Nos. 3 and 4 appear once impressed on clay sealings and once written on clay archival texts (a label and a nodule respectively) at Knossos. It is perhaps interesting to notice :

- 1) that neither the label (but this is the general rule) nor the nodule (and this is quite unusual) bear any seal impression;
- 2) that the label presents all together three different sign-groups and the nodule four different sign-groups (with no logogram or number in either case);
- 3) that each of the two clay sealings bears two different seal impressions (in one case from different seals, in the other possibly from the same seal);
- 4) that the first of the clay sealings (the one with the impressions from two different seals) has no graffito inscription, whereas the latter presents a flat face with three incised signs;
- 5) that the two sealings, although of different forms, were probably used the same way : they were securing a thread of which the hollow channel is still visible.





All these remarks certainly do not hold the same level of interest. But it would perhaps not be too incautious to suppose that the similar sign groups (written in one case with a stylus on a


<sup>18</sup> For the references, see *Appendix*.

<sup>19</sup> *SM* I, 261, fig. 116.g : not taken as certain here because the sign-group is incomplete on the tablet.

<sup>20</sup> *SM* I, 261, fig. 116.l : not taken as certain here because there are problems about the secure identification of the first sign.

<sup>21</sup> Two exceptions : 1) one seal impression with the 'trowel and eye' comes from House A of Zakros (on 3 sealings) and another from Palaikastro (on a loomweight, printed before firing); 2) one seal impression with the 'trowel and arrow' comes from Quartier Mu of Mallia (no. 42 : on a fragmentary amphora handle, likewise printed before firing).

<sup>22</sup> The only possible exception is  on a 'cone' from Quartier Mu (no. 21) and  in P.104c. [Note of correction : the 1988 material from Quartier Mu gives another parallel between  and P.77  ].




<sup>23</sup> Incidentally, there are about a dozen similarities between sign-groups on the seals and sealings alone (*i.e.* not taking into account the ones already occurring in the matches studied here); only one occurrence concerns a sealing from outside Knossos :  which appears in Quartier Mu (no. 31).

clay lump without a seal impression and impressed in the other case on a clay sealing bearing other information than that transmitted by a single seal) are personal names (perhaps some kind of "signature" ???).







No. 5 is perhaps, at one and the same time, sufficiently and insufficiently frequent (2 times on seals and 7 times written on clay archives) to allow any speculation. This is certainly not the case with nos. 1 and 2, which raise the most vexing problems in hieroglyphic epigraphy. My goal here is not to try to solve these problems, but to give some indications which can lead towards a solution.

1) These two sign-groups are the most frequent in our corpus : there are more than 60 attestations of the first <sup>24</sup> and more than 30 of the second <sup>25</sup>.

2) These two sign-groups are certainly related, in meaning and in function. The best indication is that they appear together on the same face of the same seal <sup>26</sup>. They are also "mixed" once : the 'trowel' is in the middle position with the 'arrow' on one side and the 'eye' on the other <sup>27</sup>. Moreover, one should not neglect the fact that these two sign-groups appear at least 11 times on two different faces of the same seal <sup>28</sup>.

3) Their meaning <sup>29</sup> was "absolute" or "self-sufficient" <sup>30</sup> :   appears alone on the fragment of an amphora handle from Quartier Mu of Mallia (no. 42) impressed before firing.  is found on a loomweight from Palaikastro <sup>31</sup>, likewise impressed before firing.

4) It is not excluded—although not proved—that their meaning could be "supplemented" by a "badge" (*vel sim.*), like the representation of a 'cat's head', of a 'seated man', or of the logogram for WINE or WHEAT. But even if that is really the case, we remain unable to explain this phenomenon, although the supplementation by an *economic* logogram is certainly of interest.

5) The very simple fact that concerns us last is perhaps the most significant of all. In the clay archives these sign-groups were really read and had a precise and unambiguous meaning. Of course, the fact that  appears only twice reduces the strength of the statement that each time it appears it directly follows another sign-group. Nonetheless the fact that on the bar P.104  appears once and   twice reinforces statement 2 above. However, the multiple occurrences of   on a single document are difficult to explain (twice on P.100 and P. 104,

24 Among which 10 in written archival documents.

25 Among which 2 in written archival documents.

26 On CS 170 (AM 1938.793) : each of them is preceded by one (or even two !) 'initial cross' and they are separated by a double vertical line.



27 In CMS IV no. 135 one could rightly argue that we are in front of a single and complete sign-group, having a full value by itself; in P.27d (Pl. Ie), where the same disposition appears, the presence of two vertical lines on each side of the 'trowel' constitutes a strong argument, I think, in favour of a 'divergent' reading. Not to mention the rather extraordinary three-sided prism bead CS 101 (AM 1938.797) which bears on one face the 'trowel', on the other the 'eye' and on the third the 'arrow' (although the "reading" of the last representation is perhaps not entirely certain) : if my interpretation is correct we would have here some kind of "printing matrix" able to deliver two different messages by the different combinations of its faces.

28 In combination with one or two other sign-groups : the study of these combinations would be too specific to be undertaken here. *N.B.* : there is only one example of these two sign-groups without at least a third one : on CMS XII no. 9D, a (rightly, I think) *gemma dubitanda* (but cf. the case of CS 101 in previous note).

29 Unless we consider that nothing was intended to be seriously "read" ... which was probably the case for the sealing from House A of Zakros, dated 'sub-LM I A' (for the time in which it was used !).

30 So a "relative" meaning (like 'son' for instance) would be extremely doubtful.

31 R.W. HUTCHINSON *et al.*, "Unpublished Objects from Palaikastro and Praisos", BSA 40 (1939-40) 48-49 [HM 4815].

three times [perhaps four <sup>32</sup>] on P.116 ); while the numbers which follow   on labels (2 and 634) and bars (from 5 to 420) don't seem significant enough to allow any guess.

From such evidence we may form a general impression. Perhaps we are confronted with two "entities"—if not two physical persons called by a generic name, then two "institutions"—extremely "popular", having activities in the same sphere, perhaps the one complementary to the other, acting through the whole of "hieroglyphic" Crete (for the moment from Knossos to Zakros) and concerned with most of the economic subjects which are attested in our current epigraphical and archaeological documentation. Something like "palace" and "temple" would not be unsuitable, but it would perhaps be difficult to explain their conjunction, at least in the present stage of our knowledge. In any event the widespread occurrence of the terms through *time, space and types of supports* <sup>33</sup> will probably remain—and for a long time—a challenge for our studies.

### Conclusions

The relationship between inscriptions on hieroglyphic seals and those on written archival documents is evident for several reasons :

1) because seal impressions executed from hieroglyphic seals were found together with written archival documents *both* at Knossos *and* at Mallia;

2) because the script (and probably the language) is the same in the two sets of documents;





3) because similar sign-groups, containing from two to four signs, are found in the two different classes of documents.

Moreover, there are two other significant points to be stressed :

1) two of these similar sign-groups (one of three signs, another of four) might be interpreted as personal names on the seals (in fact they are printed on seal-impressions found amid the archival documents) and in the written archives. If so, we would have proof that some of the seal-bearers were playing some role in the archival activity on clay. This point may seem commonplace, but it is not, in fact, self-evident.

2) the two most frequent sign-groups on the seals are also found (*both* written *and* impressed from seals) in the archival documents (not to mention other supports of the script at sites other than Knossos). This last point denotes probably the expression—and so the existence—of a large power which extended over the whole of Crete, for *at least* the last part of the protopalatial period, unless similar institutions had everywhere in this period the same means of expression and administration, through seals, sealings and written archival documents. Centralized or decentralized, we are obliged to speak of some kind of administrative *koine*. And this is perhaps the major lesson we have to learn, at the present stage of the development of our studies, from the hieroglyphic material, taken as a whole <sup>34</sup>.

32 Cf. n. 35.
















33 Is it really mere coincidence if   appears on an ivory seal from Chrysolakkos (prepalatial), and   on 3 identical sealings from House A of Zakros (neopalatial) ?

34 The existence of an administrative *koine* is well established for the end of the neopalatial period : the major difference between these two phenomena is perhaps not so much, in the second case, the change to another script (linear A) than the relinquishment of the use of seals (and consequently of sealings) as support of writing.
















## APPENDIX : References



## a) Certain matches :

1.   : about 50 times on seals and sealings; at least 10 times <sup>35</sup> inscribed in clay archives (P.80b, P.83b [labels], P.100b, P.100c, P.104c, P.104d, P.109c, P.116a, P.116b, P.116d [bars]), most frequently alone <sup>36</sup>.
2.   : about 30 times on seals and sealings; 2 times inscribed in clay archives (P.54b [nodule] and P.104b [bar]), each time immediately after another sign-group.
3.    : P.69a.1 [sealing]; P.94a [label].
4.     : P.51a [sealing : Pl. Ic]; P.76a [nodule : Pl. Id] <sup>37</sup>.
5.     : CMS XXI no. 109b and CMS II 2 no. 256b <sup>38</sup> [seals]; P.89b [label], P.103d, P.109a and P.118[a,b],c,d <sup>39</sup> [bars].

## b) Possible matches :

1.    : P.31b [seal; circular disposition of the signs]; P.117a [bar; broken after the 'crossed arms'] : completeness, order of the signs and direction of reading not completely certain.
2.   : P.50a [sealing]; P.89a [label] : the animal's head is not absolutely the same in the two cases ('wolf's head with protruding tongue' [] on the sealing, 'fore-quarter of a boar or a pig' [] on the label (but the significant differentiation between the signs no. 73 and no. 70 is perhaps less the protruding tongue than the open mouth <sup>40</sup> and it is entirely a matter of opinion to decide whether the sketch in P.89a presents an open mouth or not).
3.   : P.31c [seal; no direction of reading indicated and careful drawing of the 'sistrum' ()]; P.100b [bar; initial cross before the 'plough' ()] : if the equivalence of signs no. 28 and no. 27 is accepted, and if the direction of reading on the bar is adopted for the seal, the two groups match.
4.   : CS 169 <sup>41</sup> [seal; no direction of reading indicated and the "inscription" seems far less important than the decoration]; P.50b [nodule; inscription probably complete before the 'ox-head' (perhaps room for an 'initial cross' ?) and considerably empty space after the 'mallet' : so it is possible that the sequence is to be read in that order ?] : if one agrees with the direction of reading on the nodule and applies the same direction to the seal, the two groups match.



Jean-Pierre OLIVIER

35 Broken at right in P.117a and perhaps to be restored in P.116a (where  stands isolated, on the same face as a complete ).

36 It has to be stressed that it appears twice on P.100 and on P.104 and three times (perhaps four : cf. previous note) on P.116.

37 About the reading of this sign-group see supra n. 16.

38 Albeit followed, but *after* a decorative motive, by an 'initial cross'.

39 In the four last occurrences, the 'serpent' is drawn with four curves () instead of two () : but Evans himself had already seen that it was the same sign.

40 But when the tongue is drawn, the opening of the mouth does not seem absolutely necessary : cf. the sign on the Chamaizi vase from Malia (HM 11.149).

41 = PM I, 277, fig. 207c.

### Response by Thomas G. PALAIMA

All Aegean prehistorians are fortunate that Jean-Pierre Olivier has now turned his attention to producing a corpus of Cretan hieroglyphic inscriptions. I was going to say "his pinacological attention", but this master of pinacology (the study of inscribed clay documents) now finds himself in somewhat unfamiliar territory. For, as his paper makes clear, the very origin of the Cretan hieroglyphic writing system is connected with the carving of the signs of the script on stone seals. The existence of some 150 seals or impressions from seals with characters in a glyptic tradition raises the critical problems which he addresses in his paper <sup>1</sup> and which center on the relationship between hieroglyphic seals and hieroglyphic clay documents.

I think that we should consider four particularly difficult and interrelated questions raised by Jean-Pierre's paper :

- (1) the origin of the hieroglyphic script;
- (2) the significance of the characters on the stone seals;
- (3) the purpose of the "inscriptions" on stone seals;
- (4) the evolution of the clay "archival" script, i.e., hieroglyphic linear writing.

Since these questions all bear upon one another, it is not possible for me to discuss them point by point. Let me plunge *in medias inscriptiones*. I refer to inscriptions. This is, as I have expressed elsewhere <sup>2</sup>, a most delicate issue, certainly not resolved yet in this paper. For Olivier states at one point that he is "more convinced than ever that the script on the seals is a *decorative* one", while at the same time he adopts as a working principle "that the inscriptions in hieroglyphic script on seals are *true* inscriptions, even if their decorative aspect must never be lost sight of". He is able to document parallels between sign groups on seals and those on clay inscriptions, some fairly regularly recurrent, which lead to his further qualification. It is mainly the existence of a number of signs, most of which are quite naturalistic and pictorial (e.g., 'pig', 'kid' and 'scroll' : all unattested *so far* in clay inscriptions), which prompts him to want to see the seal signs as no real script, but decorative symbols. We are faced with something of a paradox which allows Olivier to satisfy the standard definition of an intelligent person : someone able to hold two contradictory ideas in his mind at the same time. What are the questions raised by this paradox, the ramifications of it, or the alternatives to it ?

First, if the hieroglyphic signs on seals are decorative in origin and primarily decorative throughout their existence, how did the genuine hieroglyphic script originate ? Must we believe that a real writing system came into being only when these pictorial symbols—and only a selection from them—were incised on clay for clearly administrative, if not totally archival, purposes? How exactly would the process of attaching values, presumably phonetic, to the *decorative* seal signs have worked? And what would have been the value or meaning of the signs inscribed on the stone seals both *before* and *after* the hieroglyphic administrative script

1 And earlier in his paper in *CMS Beiheft* 1 (1981) 105-115.

2 T.G. PALAIMA, "Ideograms and Supplementals and Regional Interaction among Aegean and Cypriote Scripts", *Minos* 24 (1989) forthcoming.

was developed? This also has practical implications. How would a Minoan seal-owner, after the genuine hieroglyphic script came into being, have viewed a seal which had been carved pictorially before the advent of true script? The seal might now be read to have nonsense meaning. Would it therefore be less highly prized or even discarded? Or is Olivier prepared to go so far as to say that the signs attested in administrative texts only appeared on seals after the clay linear hieroglyphic was developed? Can the evidence possibly support this? Are there any seals from the period of the earliest hieroglyphic seals and clay texts which are composed, like the earlier 'Arkhanes script', entirely of pictorial symbols unattested in hieroglyphic clay script? This would lend support to the possibility that clusters of purely decorative symbols, perhaps imitating the signs and arrangements of an already existing administrative script, were used on stone seals. In any case, it is the hybrids which mix "pictorial" and "real" signs that are puzzling.

Olivier has admitted that one or two naturalistic and pictorial signs originally classified as merely decorative elements have subsequently been attested in clay inscriptions. It is therefore hazardous, in dealing with a very representational script and considering the extremely limited data, to catalogue automatically every pictorial sign that does not occur on clay texts as "decorative". It is fine to cite Evans "just for fun". But this should not be allowed to mislead us. We could cite some preposterous suggestions the father of Minoan scripts made about Linear B signs. Moreover, we should note that many Linear B ideograms have very pictorial styles (e.g., EQU, SUS, ROTA, BIG, CUR), and we would be entirely mistaken not to assign them presumably logographic values in the Linear B documents. The "decorative" signs on seals could be the equivalent of newly coined Mycenaean logograms or relatively rare Mycenaean phonograms which were never subjected to the simplification or stylization produced by repeated use.

At the end of his paper, Olivier makes two highly significant and I think entirely valid conclusions—again quite an accomplishment given the nature of the evidence. These conclusions bear upon our four points. First, two sign groups are found in seal impressions and inscribed on clay texts. Thus he concludes that some seal-bearers were involved in a Minoan administrative process. This also implies that at least these—may we call them literate?—seals also had an administrative function. Is it permissible to extrapolate and to conclude that all seals in this period had this as their primary purpose? This, of course, would lead us into the whole question of sphragistic vs. non-sphragistic uses of seals. I would also like to ask Olivier how many seal *impressions* are comprised of (1) entirely "decorative signs"; how many of (2) mixed decorative and clay-script signs; and how many of (3) entirely clay-script signs (there are at least two). These statistics would shed light on all our previous questions. For example, if there were no seal impressions of (1) entirely decorative signs, this might imply a rigid dichotomy of applications or uses of seals of different types. If in addition there were some impressions of type (2), this would increase the probability that the so-called "pictorial" signs in these cases had real values.

Second, the occurrence of frequent sign-groups on seals and archival documents from various sites implies for Olivier the existence at least of similar institutions throughout Crete operating within something like an administrative *koine*. On the basis of Near Eastern parallels and even reasonable extrapolation from later Linear B records, such recurring sign-groups could be any of a number of vocabulary terms necessary to a functioning administrative bureaucracy. These terms could express: family relations, social or professional status, titles, geographical names, religious or political institutions, and so on. Has Olivier given any thought to reviving an old ghost, namely the view that hieroglyphic on seals is related to religious institutions, on clay primarily to secular institutions? There is always some overlap between the two spheres (religious and secular), as the Linear B tablets well attest.



All these questions and observations arise from Olivier's sober and discerning presentation of the limited hieroglyphic data, which we can say is becoming available to us because of his hard efforts. I perhaps invite too much speculation. If so, it is only because I have through his paper really seen this material for the first time. We must all thank and encourage him greatly.

## Discussion

### Olivier :

I would like to reply briefly to many of the points raised in Palaima's response to my paper. Most are critical questions which even the completion of the corpus of our limited data may not be able to answer fully.

- 1) We know nothing about the *actual* origin of the Cretan writing system : all that we have is some documentation which provides us with the *oldest* attestation of this system. Consequently, we ought not to be allowed to say that its origin was primarily decorative (even if this now seems to be the most likely explanation).
- 2) 150 seals and impressions of seals are almost nothing, on statistical grounds, and if we have them it is only due to the material (less perishable than others) on which they are preserved.
- 3) «Contradiction» is not a characteristic of the human mind, but of nature (perhaps I should write "Nature").
- 4) Any distinction between "true" script and "decorative" script may be misleading. Sumerian and Egyptian are true scripts from their very beginning, but they have full "pictorial" or "decorative" characteristics.
- 5) The "hybrids" which mix "pictorial" and "real" signs are perhaps «puzzling», but certainly no more so than the entire corpus of seals from the period of the first palaces which mix seals engraved with signs of the Cretan hieroglyphic script and seals without any signs of this script.
- 6) It seems to me better to accept, at the first stage of devising the hieroglyphic signary, too few signs than too many. It will be easier, afterwards, to add signs to a list than to subtract those which were wrongly admitted.
- 7) The Linear B ideograms which have very pictorial styles (e.g. EQU, BIG, CUR) are Mycenaean innovations for Mycenaean *things*, and so are outside the hieroglyphic debate (ROTA and SUS are not especially pictorial : they are just naturalistic).
- 8) All "hieroglyphic" seal-bearers were, at some moment in their lives, involved in an administrative process; if not, why possess a seal ? That does not imply that they were literate any more than it implies the contrary ! We just do not have sufficient evidence to determine the degree of literacy among users of hieroglyphic seals.
- 9) Statistics ('counting' would be perhaps a more appropriate term) about "written" faces of seals and "decorative" faces are impossible for the moment (even with so small a number of items). The relative position of the faces is still unknown (alas!) for most of the seals and will remain so until a fresh examination of all the objects in all the museums in the world will be completed.
- 10) We cannot read the Cretan hieroglyphic script, so any parallels with Linear B are uncertain. Of course, there were probably religious "patterns" in the hieroglyphic script. But we are unable to decide exactly where these occur.
- 11) The only certain and reliable conclusion, a century after Evans' discovery, is that we know, if not nothing, at least very little, about all these things and that we have to be very humble and patient in studying such frustrating material.

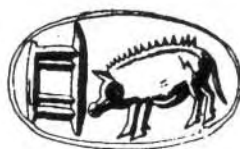


**Blasingham :**

I have a question about Olivier's distinction between decorative motifs and syllabic or meaningful motifs. Why do you think that geometric motifs, like the j-spiral whorl on Knossos P.22 side C, cannot convey meaning even if they are not syllabic ? That particular motif and a number of others which appear on hieroglyphic seals also appear on the earlier seals from the Mesara tombs. And the links are unmistakable since the motifs do not evolve very much between the Mesara seals and the hieroglyphic ones. Is it not possible that what might have been a purely decorative motif could have been taken over as a unit of meaning, whether syllabic or not, in the hieroglyphic seals ? In other words, must we rule out any kind of link between the pre-palatial seals and the hieroglyphic seals ?

**Olivier :**

Certainly not. But, in my experience, these special characters never appear inside sign-groups, but always at the beginning or at the end. This is a strong argument that they are not part of the sign-groups and so are not to be read syllabically. But this does not exclude their having had some semantic function which will almost certainly always escape us. It is difficult to distinguish motifs from actual hieroglyphic signs, given the present state of our documentation.



a



b



c

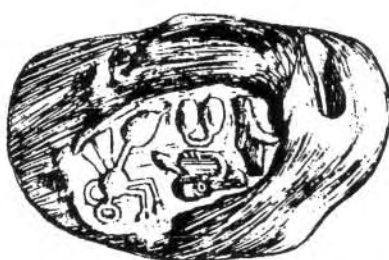
a



b



e



c



d

Origin of the illustrations :

a-c and e : *SM I.*

d : drawing by L. Godart.