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## MYCENAEAN INSCRIBED VASES

### III. THE CONSEQUENCES FOR AEGEAN HISTORY\*

In the second part of this series (Kadmos 11, 1972, 27—46) it was argued that the inscribed vases from the Mainland were cogent evidence for relations between Crete and the Mainland in the LM/LH IIIB period. Since that part appeared T. G. Spyropoulos has communicated<sup>1</sup> further important and unexpected evidence pointing in the same direction: he has interpreted the LM IIIB painted larnakes from Tanagra as “part of the amalgam which is the common artistic expression of the Creto-Mycenaean world”<sup>2</sup>.

Our first part surveyed the evidence, starting with the newly discovered inscribed LM IIIB stirrup-jar, which indicated that the Last Palace at Knossos was largely a reconstruction of the LM IIIB period. Here recent work by Popham and Warren figured prominently. Since the completion of the article further important work by Popham has been published.

In a brief additional study of LM IIIB pottery from Knossos<sup>3</sup> Popham underlines the uniformity of pottery of this period throughout Crete and concludes that “there is a strong presumption that this general uniformity indicates, if not some form of political unity,

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\* Abbreviations: AE/NB = Arthur Evans, Note Books. — AoC = Archaeology of Crete (J. Pendlebury). — DM/Pot = Duncan Mackenzie, Pottery Notebooks. — DPK = Destruction of the Palace of Knossos (M. Popham). — NEP = Northern Entrance Passage. — OKT = On the Knossos Tablets (L. R. Palmer and J. Boardman). — PoM = The Palace of Minos (A. Evans). — PP = The Penultimate Palace of Knossos (L. R. Palmer). — SM = Scripta Minoa (I—A. Evans 1909, II—J. L. Myres 1952). — SMK = Stratigraphical Museum at Knossos (see Footnote 5, below). — TP = Test Pit.

<sup>1</sup> Teiresias 1, 1971, 4—6

<sup>2</sup> This echoes the phraseology used in my New Guide to the Palace of Knossos, p. 116; but by “Creto-Mycenaean amalgam” I meant the distinctive culture of Mycenaean Crete of the XIII—XII centuries.

<sup>3</sup> BSA 65, 1970, 195—202 with Plates 47—52

at least surprisingly close communication within the Island. The presence in the Little Palace pottery of fragments from eight vases, which appear to be imports from the Chania region, supports this conclusion".

Thus literate LM IIIB Chania stood in close relation with literate LM IIIB Knossos, both forming part of a Cretan ceramic koine at a period when Crete was exporting inscribed vases to so many Mainland palaces. In this general context, apropos of the new evidence from the Unexplored Mansion, we may pose the question whether it is permissible to suppose that the Linear B script continued to be used independently of a palatial system of administration. General opinion on this key question may be summed up in the words of S. Dow<sup>4</sup>: "Literacy ended . . . when the palaces and all that went with them, particularly account keeping, but also the whole series of activities involved with the palaces, were ended". If this passage is read in the light of the new evidence, which supports Evan's remark apropos of the Little Palace finds about "the survival of similar usages as regards securing documents and possessions", then it will be obvious that some rewriting is necessary in a chapter which accepts a date of c. 1380 B. C. for the final destruction of Knossos.

Of far greater importance to our theme has been the publication<sup>5</sup> of Popham's long-awaited study of the material in the Stratigraphical Museum at Knossos (hereafter SMK). A full study of this monograph would go beyond the limits of the present subject of discussion, and it must be reserved for a later occasion. It is all the more imperative in that, because of publication dates, Popham was not able to consider the facts and arguments advanced in *The Penultimate Palace of Knossos*. In general it may be said that Popham's work may be regarded as a supplement to Pendlebury's *Guide to SMK* and gives it new precision, particularly in the matter of sub-classifications within LM III pottery. It makes a new and, I believe, decisive contribution to the discussion among archaeologists about the history of the Last Palace at Knossos.

This began with Blegen's article of 1958<sup>6</sup>, in which he asked for the excavation data to be looked into. Hood, after his preliminary

<sup>4</sup> *The Linear Scripts and the Tablets as Historical Documents*, by Sterling Dow and John Chadwick, *Cambridge Ancient History* Vol. II, Ch. XIII, p. 26

<sup>5</sup> *The Destruction of the Palace of Knossos* (*Studies in Mediterranean Archaeology* XII), 1970

<sup>6</sup> *Minoica* 61-66

rejection, came to see that the tablets and sealings (to say nothing of the numerous artifacts) had been found in association with pottery classed as LM IIIB. This basic statement of the facts of discovery, first proclaimed in 1964 and steadfastly maintained down to Hood's most recent publications, has never, to my knowledge, been challenged by Popham or by any other archaeologist. They have simply rejected his proposal to reclassify the LM IIIB pottery as LM IIIA 2. After this important insight into the nature of the pottery in the horizon of destruction, scrutiny shifted to the sub-floor deposits, which should provide a terminus post quem for Linear B at Knossos<sup>7</sup>. It is here that Popham's most important contribution lies.

In fact it may be said straight away that we are now in touching distance of final agreement on this long sustained and hotly disputed issue: so much so that every effort should be made to state clearly and simply the remaining points of difference. I accept Popham's ceramic classifications and distinctions: they are properly a subject for discussion among archaeologists. It may be said, however, that discussion will be more objective if we use classificatory terms like LM IIIA and IIIB and avoid pseudo-descriptive terms like 'destruction pottery' and, still worse, 'reoccupation pottery', for this has a built-in historical conclusion. The crucial question is the stratigraphic siting of the LM IIIA 2 and LM IIIB pottery which Popham has selected from the boxes of SMK. It seems to me that the sources of information adduced by him uniformly indicate that the pottery in question, including much LM IIIB, came from sub-floor deposits and is evidence not for the final destruction of the Last Palace but for its constructional date. Not a single LM IIIA vase in Popham's monograph can be unequivocally assigned to the supra-floor destruction deposit in which the tablets and sealings were found.

The issue now clarified, it will be evident that what remains to be discussed is not one involving ceramic expertise but a matter of Quellenkritik. Three self-evident principles may be enunciated at the outset. The first (A) emerges from the history of the excavations. The West Wing and the Northern Entrance Passage (together with the 'Prisons Area') were cleared by 1901 and the East Wing by 1902. It follows that the onus of the proof is on the scholar who urges that material, obtained (say) in 1913 or 1928–29, belongs to

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<sup>7</sup> This thesis was the main purpose of my Penultimate Palace.

the 'destruction deposit'. Still worse would be 'reoccupation deposit', for this would involve a double error, the misplacing of the material and a *petitio principii*, for the 'reoccupation' is precisely what is under discussion. Another important principle (B), which constantly arises, concerns the nature of sub-floor deposits. Evidently, if an area is cleared and levelled for a new construction (e. g. the Throne Room and the Central Court), then the sub-floor deposit may contain material of all dates from Neolithic down to the time immediately preceding the date of constructional work. It will not do either (a) to dismiss such evidence as 'mixed' simply because some of it is inconveniently late or (b) to impose an arbitrary stratification on material found in one and the same box. Our third principle (C) comes into operation if the evidence from a given test is so surprising as to provoke incredulity. We should then suspend judgment and seek confirmation from other tests carried out in the vicinity. If it should turn out (as is frequently the case) that tests carried out at different dates by Mackenzie, Evans, and Pendlebury produced consistent results, then rejection of the testimony would be difficult, all the more so if re-examination of the material supports conclusions drawn by the excavators themselves about the constructional history of the Palace. The application of these principles to the available source testimony may now be demonstrated by a few important examples drawn from key areas of the Palace.

### 1. The Throne Room and the Central Court (DPK 55-6, PP 32-7, Pt. I, 77)

Here little need be added to previous discussion. Evans showed the area of this 'revolutionary intrusion' into the Palace in his plan PoM I, 449, Fig. 322. The decisive tests bearing on the construction were:

- 1) F I 3 (1903) in the Anteroom to the Throne Room to .55 down. In addition to Neolithic sherds and bones, the lot contained the stem of a tall plain kylix, which Popham determines as "more likely to belong to the phase IIIA 2 to B rather than to early IIIA".
- 2) I II 5, with a label in Evans's own hand, contains the sherds from the decisive TPs 59-60 of 1913. The key sherd, which figures in AE/NB 1913 and was first drawn by J. Raison<sup>8</sup>, has a motif called

<sup>8</sup> OKT 247

by Popham "alternating arcs", which, he states, "persists from early LM IIIA into IIIB<sup>9</sup>. The black colour of the paint and the poor execution of the design are more typical of IIIB, but the sherd has been overfired or burnt and its small size precludes any certainty".

3) The material from Evans's TP 56 of 1913 below the paving of the Central Court (E III 18). According to Popham the box contains "four certain sherds of LM II to early IIIA type, three probable others and nothing later".

To this we may add, by way of confirmation, I II 11 (1904) with LM III in the first and second metres and I II 1 (1928)<sup>10</sup>, just west of the NEP, again with LM III. Cf. also Hood's report on I II 5: "with at least four LM III sherds including one plain kylix rim with signs of burning and the LM IIIA sherd drawn by Raison" (see PP 33).

Here we have a group of tests (our principle C) by Mackenzie, Evans, and Pendlebury giving clear evidence for a reconstruction when 'IIIA 2 to B' pottery had become stratified. We may seek confirmation for this surprising result in the adjacent Northern Entrance Passage.

## 2. The Northern Entrance Passage

(DPK 43—44, PP 38—43)

Here we are particularly fortunate in that there is complete agreement on the pattern of facts except for a single, but decisive, point. It will be recalled that Evans sited (with the full support of all the available sources) the Great Deposit of tablets on an earth floor (overlying an earlier paved floor) intermingled with double amphoras, which Popham, like Evans, classifies as LM IIIB. The deposit stretched as far south as the passage opening west. It was at this point that Evans sank his test pits 77, 78, and 79 of 1913. Popham's study of the material from these tests confirms those by

<sup>9</sup> Note that this design figures exclusively as LM IIIB in Popham's article BSA 62, 1967, 348, Fig. 6, 7 and also in Fig. 2 of the more recent article cited in note 3. These purport to give a selection of motifs diagnostic of LM IIIB. If the motif persisted from early IIIA to IIIB, why was it chosen and presented as illustrative of IIIB, and why does it not appear in Fig. 5 of the first article (IIIA)? A similar vacillation occurred with the classification of the IIIB strainer after its true find place (Gallery of the Jewel Fresco and not Room of the Stirrup-Jars) had been revealed by DM/Pot 1901.

<sup>10</sup> One of Pendlebury's tests; see p. 71 for his excavation strategy.

Raison and Hood (see PP 39—40). There can be no doubt that TPs 77 and 78 within the side passage were sunk below the floor uncovered in the earlier years of the excavation: the earliest sherds are Neolithic, but the greater part are Middle Minoan; there are in addition not only large fragments of Palace Style jars, but also a small amount of LM IIIA, including sherds from four decorated vessels (so Popham). If we now turn to the third test of this group (no 79 = SMK J II 5), sunk in the NEP itself just outside the passage opening west, we may presume that this too was devised by Evans as a scientific test with the same purpose as the other two. That it was also sunk below the paved floor that underlay the earth floor on which the tablets had rested is immediately indicated by the quantity of Middle Minoan recorded in the Guide and confirmed by Raison's subsequent inspection (see OKT 247). Popham gives further precision to Raison's catalogue of the LM III sherds contained in the box: there are two sherds, a tall kylix stem and a kylix stem with rill handle "which could well belong to the re-occupation phase" (note the lapse into the *petitio principii* and substitute "LM IIIB").

This is a finding of the utmost importance. Here we have the find spot of the most concentrated deposit of tablets with a massive association of LM IIIB pots which is beyond dispute. Add the fact that immediately to the south another deposit of tablets along with the 'Great Seal' was found high above the LM IIIB floor from the surface down to 90 cm. Unquestionably this is the 'destruction deposit', the Linear B deposit. We now have the testimony of Popham for the presence of material as late as LM IIIB from the 1913 test at this very spot. Only one point remains to be settled. Was this test made underneath the underlying paved floor of the NEP, as the Neolithic and Middle Minoan sherds would suggest, or did Evans simply remove an earth baulk in 1913, which had been left standing since the beginning of the excavations in 1900? This would mean separating TP 79 from TPs 77 and 78. The suggestion about an earth baulk was first made by Boardman in 1960, and it is embraced with a slight hesitation by Popham: "Apparently this same year, in 1913, a small baulk of earth, outside this opening, left there in 1900, was cleared (OKT 47 and Pl. XII) and the sherds kept" (p. 43).

The point must be considered with all the greater care because the opposing sides are now within touching distance of complete agreement and the whole issue of the date of the tablets and the

constructional history of the Last Palace can be decided here. We recall the basic principles enunciated above. The onus of the proof is on the scholars who would assign material from a test of 1913 to the 'reoccupation', and acceptance of this thesis is all the more difficult if it includes Neolithic and Middle Minoan. Moreover, we are dealing with a group of tests which provide mutual support. We must therefore examine with the greatest care the photograph adduced to assert the survival of an earth baulk at this key point in 1913. In the first place we note that the fine photograph OKT Pl *XII* was taken in 1902, and it clearly shows the site of the 1913 tests in the NEP and the side passage. Popham has kindly informed me that he interprets as an earth baulk the rectangular patch on the photograph which protrudes into the NEP just south of the side passage and runs north almost as far as the next narrower opening west.

It is most gratifying that we now have the help of a new source in interpreting the photograph: this is the newly discovered plan (1903) of the Palace by the architect Theodore Fyfe<sup>11</sup>. Within the side passage he plots the foundations<sup>11a</sup>, which protrude some little distance beyond the west wall of the NEP, which rests on them, these foundations extending almost as far north as the next narrower opening in the west wall. It is these protruding foundations which are so clearly visible in the photograph, and in the light of the architect's plan the rectangular patch cannot be interpreted as an 'earth baulk'. It is perhaps superfluous to add that a photograph taken in 1902 cannot be accepted as evidence for the state of the NEP in 1913, to say nothing of the difficulty presented by the Neolithic and Middle Minoan material.

The evidence for LM II and LM III material at this stratigraphic depth is consistent with other tests we have listed from this part of the Palace, and we can now add Pendlebury's test J II 1 (N. Bastion foundations: W. side of N. corner, 1929): "... some LM I and LM III". The overall picture of the finds can be schematically represented as in Fig. 1. The consistent testimony of the chain of tests extending from the northern end of the NEP up to the Central Court and across to the Throne Room block leaves little room for doubt not only about the LM IIIB context of the tablets but also for the late constructional date of the Last Palace.

<sup>11</sup> See PP Pl. I A

<sup>11a</sup> Better still in PoM I 397, Fig. 286

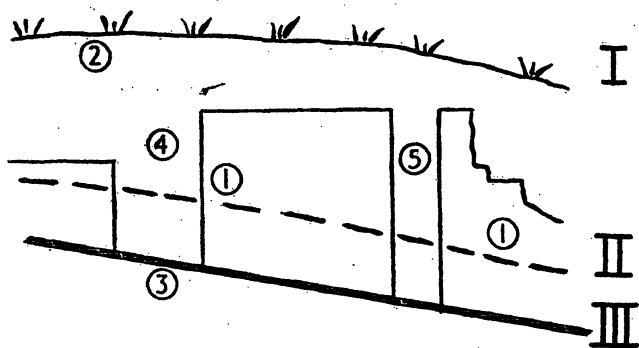


Fig. 1. I Surface before excavation. II LM III B earth floor. III Paved floor  
1. Great deposit of tablets with LM III B double amphoras. 2. Tablets and Great Seal. 3. LM III A—B sherds. 4. Passage opening west. 5. Narrower opening west.

### 3. The West Magazines

(DPK 44—54, PP 59—70, Pt. I 78—82)

Popham's researches have provided a firm cross-reference between the NEP and this quarter of the Palace. In his comment on the fine pieces of Palace Style jars from the sub-floor tests inside the passage opening west (where no earth baulk is claimed) he notes (p. 44) that most of these belonged to a vase which in all probability "Evans reconstructed and allotted to the W. Magazines". The indubitable sub-floor provenance of this material, supported as it is by Pendlebury's LM II sherd from under the foundations of the East Bastion (J II 7, 1929), is an additional confirmation of the 'penultimate' origin of the Palace Style sherds from the Magazines. The strange muddle which arose in 1900—04 over this scanty material has been traced elsewhere (PP 13—16). But Popham records still later sub-floor material from the West Palace quarter. Pride of place must be given to the sherds emanating from the lower cists of Magazine XIII and from those under the pavement of the Long Corridor, apropos of which Popham abandons the 'plunderers' hypothesis advanced in OKT (p. 39): "the arched kylix foot, the rather straight-sided cup rim and the stirrup jar fragment can hardly be earlier than the second phase of LM III A" (p. 53). Material recorded as obtained from between two paving slabs in Magazine XIV included "the foot of an LM III B type of plain goblet and a kylix fragment with palm decoration which is not earlier than LM III A 2". Further, a test "from 16th Magazine from under floor level" (1903) yielded one bowl fragment of LM III B style. All this is perfectly harmonious with the evidence reviewed so far. Popham admits a large-scale reconstruction at a time when



pottery "not earlier than LM IIIA 2" had become stratified. The still later date suggested by the LM IIIB pottery in the sub-floor deposits can be countered only by objecting to its presence.

#### 4. The South Propylaeum

(DPK 56—57, PP 71—83, Pt. I 83—85)

Popham notes a number of cross-connexions between this area and the West Magazines in the shape of joining sherds, and it is important to note that in both areas the material is of sub-floor provenance. This fact is of considerable methodological interest because it enables us to carry out independent tests of the stratigraphic pattern which has already emerged in no fewer than three localities of the Palace. Noteworthy is Popham's communication relating to the material obtained from the wall spur on the west side of the Propylaeum. This, it now appears, included not only the LM IIIA amphoroid crater already discussed but also "three sherds of a bowl with whorl-shell design of Mycenaean type which must belong to LM IIIB". If we now add that the tests of 1903 from the walls of the Central Clay Area (= Pelasgian Megaron) yielded, along with much Neolithic, some of LM IIIA type, and that Mackenzie's probes of 1925 along the foundations of the Propylaeum walls produced some as late as LM IIIB (see PP 145—46, Testimonium 9),

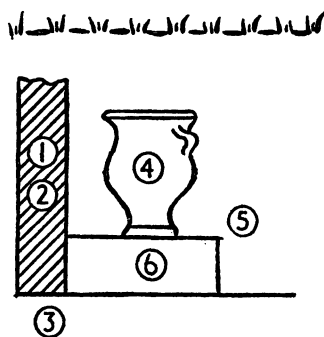


Fig. 2. Schematic section in Northwest corner of South Propylaeum. 1. LM IIIA sherds. 2. LM IIIB sherds. 3. LM IIIA and IIIB sherds. 4. LM IIIB pithoi. 5. Carved stone rosettes and fresco. 6. Shelf.

it will be seen that once again the results of tests at different dates were consistent. They can be represented in simple diagrammatic form (Fig. 2). All are agreed that the famous carved stone rosettes,

like the similar piece of fresco, must be regarded as part of the 'destruction debris'. The issue may be posed in the form of some simple questions. Can we dissociate the piece of carved stone (5) from the adjacent LM IIIB pithoi (4) above whose bases it was found? Can we associate it with the sherds of the amphoroid crater (1), some found within the wall spur and others in the sub-floor tests of 1923—25? Can we associate LM IIIB sherds from inside the wall spur (2) and from the Propylaeum foundations (3) with the pithoi alongside the wall spur (4) and assign all this material to the 'reoccupation'? Such drastic rearrangement of the excavation data is implied in the assignment of the carved stone rosettes to 'a LM IIIA context'. In this connexion we may recall that the stone basin from the corridor north of the Throne Room, found virtually touching the LM IIIB 'pilgrim flask', has also been described as found in a LM IIIA context<sup>12</sup>.

### 5. The East Wing

(DPK 22—30, PP 84—97)

We may now leave the West Wing of the Palace, where Popham has made so many valuable additions to the evidence indicating a major rebuilding with an LM IIIA 2/IIIB terminus post quem, cross the Central Court, relaid at much the same period, and enter the East Wing. Here we face Evans's statement about the signs of restoration on a large scale in the 'Reoccupation Period' (SM I, 55) and the conclusion of Pendlebury (AoC 239) about the clearance of virtually the whole Domestic Quarter, supported by his reference to SMK N. and O. *passim*.

Seeing that what is now under examination is a supplement to Pendlebury's work, we can focus on certain key tests where Popham has given new precision to the ceramic classifications of the Guide to SMK. We choose one where the evidence is particularly complex and important: N II 23 (= boxes 1289—90) of 1928, from the top step of the Private Staircase in the Queen's Megaron. This is all the more interesting because of the new fact contributed by J. Boardman to the evidence bearing on the context of the tablets and sealings found in this quarter. The photograph, taken during excavation in 1902 (OKT Pl XIV b), shows two LM IIIB stirrup-jars lying in the deposit above the balustrade of the Queen's Bath-

<sup>12</sup> P. Warren, *Minoan Stone Vases*, 6—7

room, which is where Evans located some of the Archives Deposit of sealings. That the bulk of these were found above the line of the upper flight of stairs (the Service Stairs) overlying a deposit of LM IIIB pottery has been demonstrated over and over again (Fig. 3). This pattern was observed in the south half of the staircase. Popham discusses the pottery assigned to the north half (1901). This he classifies as LM IIIA, and after considering three possible inter-

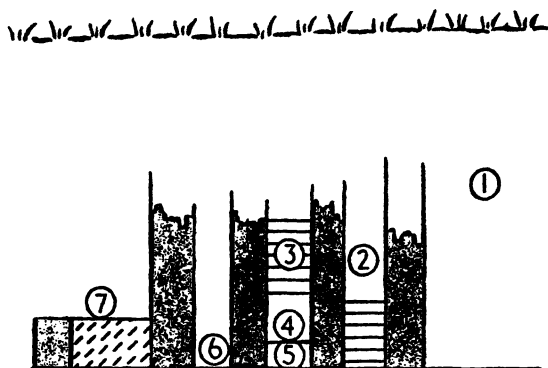


Fig. 3. Schematic section of Service Stairs. 1. Great Tablet. 2. Demon Seals and Shield Fresco. 3. Sealings of Archives Deposit. 4. LM IIIB pots. 5. 'Mature Mycenaean' sherds. 6. Tablets and sealings on floor of corridor. 7. Sealings and LM IIIB stirrup-jars. — N. B. The stair steps are conjectural.

pretations he opts for it being 'a destruction deposit'. However, the context of the sealings and tablets must be considered in its entirety, and Boardman's photograph puts us in the position of eye witnesses. The associations of the sealings are indubitably LM IIIB.

In Pendlebury's probe into the Private Staircase we are not concerned with a 'destruction deposit' but with evidence bearing on the large scale reconstruction which the excavators had deduced. Here a test of 1905 under the seventh step from the top produced sherds as late as LM II. The results of Pendlebury's probe of 1928 at the top of the staircase were still more remarkable. The Guide lists the material as ranging from Neolithic to LM III, a characteristic sub-floor deposit. Here Popham has once again given new precision to 'LM III': the material includes a large quantity of LM IIIB: 55 feet and 9 stems of LM IIIB 'champagne glass' goblets, 18 bases and 9 stems of shallow bowls, etc., etc. (p. 29, n. 27, with Fig. 17).

The excavation data relating to the Queen's Megaron area are now firmly established, thanks to the supplementary contributions by Boardman and Popham. All that remains is to pose certain questions germane to the assessment. We may assume that Pendle-

bury was a competent excavator, and it emerges from a study of the Guide that in his tests of 1928—29 he was carrying out a consistent excavation policy<sup>13</sup>, much as we should expect so long after the Palace had been cleared. Can we maintain that twenty-six years later he simply gathered up this mass of material lying above the top step of the staircase? If so, how did it come to include so much Neolithic and Middle Minoan? Such questions must be posed and appropriate answers given before we can pass judgment on the submission that what Pendlebury found in 1928 was “an unsorted sample of the kind of material the excavators were finding in the reoccupied upper levels” (DPK 29).

Here, too, we are fortunate in being able to resolve doubts by the application of principle C, that is by reference to other tests carried out in the vicinity. Particularly cogent is Pendlebury's test of 1929 in the south light-well of the Hall of the Double Axes. The location is also relevant to the Queen's Megaron suite. Here, on the west side of the wall of the east light-well, there was a great heap of LM IIIB stirrup-jars, while on the other side of the wall (i. e. in the south light-well of the Hall of the Double Axes) was found the bulk of the fragments of the Dolphin Fresco, above the floor level. It was at this point that in 1929 Pendlebury dug under the terrazza surface of the light-well (N IV 14). No doubt can be entertained that this was a sub-floor test. Along with Neolithic and Middle Minoan he recorded “an appreciable amount of LM II-LM III”. It would have been interesting to have had Popham's commentary on the LM III material, but this does not figure in his monograph. The same is true of the other 1929 test under the foundations of the east light-well of the Hall of the Double Axes (N IV 15), the location and depth being confirmed by a label in Greek. This likewise yielded some LM III. In the light of these three tests we can turn to another on which Popham has again shed new light. This is yet another by Pendlebury, and it concerns the area of the Shrine of the Double Axes. The pattern of the supra-floor finds has been dealt with repeatedly. Popham has discussed elsewhere<sup>14</sup> the LM IIIB pottery from the corridor west of the shrine, where tablets and sealings were recovered in the ‘upper strata’ overlying the late pottery on the floor. To the south of the shrine there was a double corridor, and it is here that Popham has made an interesting observation in correction of Evans's extremely inaccurate

<sup>13</sup> For Pendlebury's excavation strategy, see PP 96.

<sup>14</sup> Last Days of the Palace of Knossos 7—8

account in PoM IV: in the stratigraphy presented there the measurements "appear to have been mistakenly copied by Evans from his account in the preliminary report 1902, where he is seemingly referring to the two corridors immediately south of the shrine".

From the precise measurements given there it is clear that a floor level was reached in 1902, and this in any case natural presumption is borne out by the presence of door jambs in the more southerly of the two corridors<sup>14a</sup>. It was here that Pendlebury locates his test of 1928 (O I 6 = boxes 1372-74). Popham has studied the material and reports "Apart from the jug at Fig. 8, 1 [with reed decoration] the sherds . . . are almost entirely plain, some being of IIIA type, but most IIIB in character, as though in this area there were two superimposed deposits". It should be added that much of this material shows marks of severe burning. Once again it remains simply to pose the questions bearing on the stratigraphy of this deposit. We recall the critical principles laid down at the outset. If we wish to assign material from late probes to the 'destruction deposit' or the 'reoccupation deposit', the onus of the proof is on us. The test is located in the corridor where the floor level was measured in 1902. Can we assume, in the light of Pendlebury's consistent excavation policy, that here too he gathered up a mass of 're-occupation' material twenty-six years after the floors had been cleared? And this time we face a difficulty which is all the more severe for scholars who have proclaimed that there is no evidence of fire in LM IIIB. How are we to explain the marks of burning which are visible on bowls, snapped stems and even the bases of LM IIIB vessels?

## 6. The South Front

(DPK 57-58, PP 98-106)

Here too we encounter a conclusion drawn by the excavator himself: Evans assigned certain basement rooms (the Room of the Wheat and the Lapidary's Workshop) to 'LM III', meaning, of course, the 'Reoccupation period'. Since the floor of the more northerly of the two rooms was half covered with burnt wheat, it was immediately clear that this was further evidence for a fire at this period. Later DM/Pot 1901 provided the ceramic evidence on which Evans's conclusion was based. Hood, who sought to save the

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<sup>14a</sup> Cf. T. Fyfe's plan [1903] = PP Plan IB

orthodox chronology by reclassifying much of the LM IIIB pottery, so clearly associated with the tablets, at this point finds himself bound to agree with Popham that these fine decorated vases are of LM IIIB date. So, to save his LM IIIA 2 date for the destruction he has to devise an hypothesis which will remove these late pots from the burnt debris which engulfed these rooms. He follows Boardman's suggestion that this mass of vases had been placed on the surface of the ground as an offering after the ruined palace had become a place of pilgrimage. Later this whole deposit sank into the burnt deposit, which stretched from the surface (tablets and sealings) down to the floor (burnt wheat), thus misleading the excavator into the conclusion that there had been a LM IIIB occupation of the rooms. This 'colourful surmise'<sup>15</sup> would still leave us with the finds at the west end of the South Front, where once again we are indebted to Popham. He has shown that one of the stirrup-jars, which figured in Evan's 'decisive stratigraphy' for the date of the tablets and was located by the excavator in the Room of the Stirrup-Jars, was actually found (on the testimony of DM/Pot 1901) at the west end of the South Front. Here Evans's own record<sup>16</sup> has brought new enlightenment about the associated tablets: "Bügel with octopus . . . on floor level and tablets".

## 7. Summary

The evidence from all parts of the Palace, from which we have selected a few key examples, in the new and more precise form which it has assumed thanks to Popham's researches, is perfectly consistent and overwhelming. The tablets and sealings, together with all the other rich finds, were observed in a LM IIIB context, as Hood came to see in 1964. What is still more surprising is that the destruction deposit overlay floors and constructions which sealed in pottery as late as LM IIIB, much of it exhibiting signs of burning. Scholars will first wish to check the pattern of facts by careful study of the available sources, bearing in mind the principles enunciated at the outset. Once this has been done, there is an objective way of choosing

<sup>15</sup> I have borrowed this phrase from Popham, who thus designates Evans's 'memorial cult' in the Temple Tomb. This, it will be recalled, was similarly prompted by the appearance of uncomfortably late pottery found along with alabaster resembling those from the Throne Room (see OKT xvi). Add this to the late ceramic associations of the stone vessels (see note 12).

<sup>16</sup> See OKT Pl. XXXI (a)

between interpretations. We can either practise economy of hypotheses by simple acceptance of the stratigraphic pattern, on the principle that sub-floor deposits, with their numerous interconnexions established by Popham, are earlier than the supra-floor deposit, which included the tablets and sealings. Alternatively, we can devise numerous ad hoc escape hypotheses: plunderers, Ruinenkult, squatter-priests, incense-burning pilgrims, and the like. A simple count of these hypotheses will be an index of the 'arbitrariness' of the over-all explanation. But at the end we should still have a literate Knossos, forming part of a literate Crete in the LM IIIB period, which exported inscribed vases to the Mycenaean palaces until shortly before their destruction.

In conclusion gratitude is due to Popham for his meticulous and long sustained researches. His work will have permanent value as a description of material even though the bulk of it comes from sub-floor deposits, contrary to his declared aim of examining "the material found above floors, which will be our main concern". We hope to have demonstrated by this brief examination of some key lots "that the new inscribed jar from LM IIIB Knossos fits into that 'privileged' horizon which is also that of the Linear B tablets from Knossos". In the light of Popham's more recent contributions we may reaffirm what was said in Part I, namely that in his researches "there is a rigour and consistency all the more heartening and meritorious in that the results persistently go counter to the views which he embraced in 1960 and has long defended so strongly".

An immense work of rewriting now faces Aegean scholarship in all its branches, but the basic task was one of the greatest simplicity. These results could have been achieved as soon as Evans published the relevant volumes of *The Palace of Minos*. He gave us the information about TPs 59–60 of 1913. In his Foreword to the Guide to SMK he told us about the tests of that year and his wooden labels. The Guide registered LM III for F I 3. All that was necessary was to search out the boxes stored in Knossos and to check the material. Yet even after Blegen's paper of 1958 asking for the evidence to be looked into, this simple precaution was not taken. Still more curiously, Boardman's Addendum to p. 31 at OKT 101 shows that the second part of this volume was written without the examination of these boxes. Then, when Popham finally came to inspect the material and was inclined to date it even later than Raison and Hood (p. 63 above), he promptly rejected the testimony.

Again, once Pendlebury had published his conclusion that the whole of the Domestic Quarter had been completely cleared of debris after the great disaster of LM II, simple logic should have made it clear that the destruction debris actually found by the excavators in 1901—02 must have belonged to the following period. Here Popham's findings are so consistently in Pendlebury's favour, and agreement between him and Hood is so close, that only slight shifts of attitude will bring complete harmony.

In 1958 Blegen suggested a thirteenth century date for the construction of the Throne Room. It is a striking fact that results bringing strong confirmation of this thesis should have been reached by a scholar active at Cincinnati<sup>17</sup>. Numen inest!

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<sup>17</sup> Mr. Popham now holds an appointment in Oxford.