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THE DATE OF THE KNOSSOS TABLETS

There is no problem about the dating of the Knossos tablets. But a problem has been invented, and in view of it I would like very briefly to remind you of the facts which led the excavators to assign them to the period which they did, and which have led archaeologists since them to accept that dating—pace Professor Blegen's polite enquiry in *Minoica*.

The matter falls into two parts. First, the stratigraphy of the site; secondly, the archaeological evidence bearing on the date of the tablets, apart from their findplaces.

For the stratigraphy our primary published evidence is in the reports published during the excavations, in B S A. Evans' later survey of Minoan civilisation, *Palace of Minos*, in which he incorporated an account of the excavation, was not designed as an excavation publication, nor is it one. There is in fact no full excavation publication of the site—and this is a pity because it was dug with more care and better records than any other Greek site in those years. So good are these records that such a publication could—and should—still be attempted.

The notebooks of Evans and Mackenzie supplement the published reports in many details, although they do not affect the general conclusions. With them, with the published reports, with the finds in Heraklion and with the Stratigraphical Museum, which was a unique institution in Greek archaeology before the American museums at Corinth and Athens, the story of the dig can be re-told, even re-told in some detail. You have already heard today some account of these documents; and may have observed how they can be used both for the eliciting of more precise information about finds and findplaces; and how they can even be used—or perhaps abused—to tell a story other than that which the excavators, with their years of firsthand experience of the site, knew to be the truth.

A detailed study of the published and unpublished records reveals that the state of the Palace of Knossos in its last centuries was somewhat confused, but still clear enough—clear to the excavators and clear to us today. Briefly, we see clear evidence that the

Palace was destroyed violently, and by fire. With the burnt debris of the Palace we find the Linear B tablets, preserved by the conflagration which baked them, clay sealings, pottery and other objects. The date of the destruction is given by the pottery and other objects—and indeed the sealings. In current terminology we can say that it happened some time after the start of LMIII—in about 1400 B. C. or soon afterwards. And I think it is time we called it a LM III A destruction and not a LM II destruction, since it happened an appreciable time after the floruit of the Palace Style. This is made clear by renewed inspection of the pottery in the Stratigraphical Museum. And anyone who has looked at the pottery in the boxes there will be left with little doubt about the date of the last major period of occupation on the site, with even some vase fragments bearing the marks of the flames which brought the Palace down and baked the tablets and sealings.

But as well as this clear evidence of a datable destruction we can see that parts of the Palace were again occupied. Rooms and apartments around the edges of the building were cleared of their burnt debris and the remains of their palatial furnishing and pottery. Not all parts, probably, were reoccupied at the same time, nor any of them for very long. But even if we cannot be sure about the exact length of time of reoccupation in different parts the areas of reoccupation are obvious. Wherever these areas of reoccupation are clear, and their vases, floors and architectural or stratigraphic relationship to the ruined palace unquestionable, we find no burnt debris, only late vases, of latest LM III A or LM III B styles, which were abandoned unburnt by the end of the Bronze Age.

The reoccupied parts are characterised by their comparative poverty. Earth floors were used over the old palace paving. Rubble walls were thrown up, corridors were blocked and reused as store-rooms, the spacious South Propylaeum was turned into a pithos store, the imposing North Entrance Passage was blocked and turned into a magazine. But most of the parts of the Palace, as excavated, were not reoccupied, although they were certainly plundered, in ancient and modern times. And wherever scraps of later material appear in the burnt Palace, there we can see other clear signs of late interference, some as recent as the 19th century, for which we have also documentary evidence.

And what of the tablets? In the reoccupied areas of the Palace some tablets are found stratified beneath the reoccupation floor,

or, as in the North Entrance Passage, involved with intact and unburnt late pottery which, from its condition, clearly had suffered no such conflagration as had the clay tablets, and, indeed, the structure in which they were eventually found.

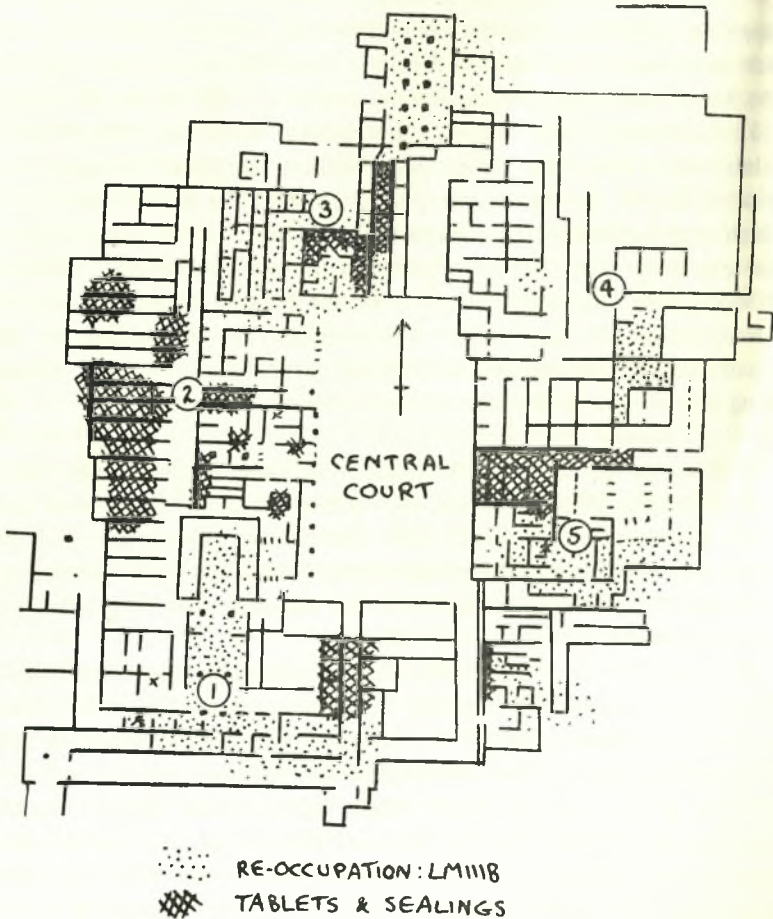
This is the simple picture. Not, I agree, as clear as though it happened yesterday, but the excavations are now 60 years old, the excavators dead. The records they left can still help us, and anyone who has himself kept an excavation notebook will realise the value and limitations of the Knossos notebooks. There are still obscure points, but it will take more than brilliant casuistry to upset the evidence which the excavators presented, and the basic conclusions which they inevitably drew from that evidence. This is not a matter that can be judged only on arguments devoted to the more obscure or incompletely documented parts of the Palace. It depends on the interpretation of the whole, and can only be challenged by an equally comprehensive and convincing interpretation of the whole, using the same evidence—and all of it.

Let us briefly consider the record of finds in the Palace itself. In the sketch, FIG. 1, I have shown the areas of the Palace for which there is clear evidence that floors were reoccupied, and the places where tablets and sealings were found. A simple glance reveals the story clearly enough, but we may consider in passing the state of various parts of the building.

In the S. W. (1) the South Propylaeum was converted into a pithos store and partly blocked, and the row of basement rooms on the steep slope south of it was reoccupied. The destruction rubbish cleared away by the reoccupiers was thrown into the gap between the Palace and the South House where it was stratified below reoccupation rubbish. Just to the east there is an area where reoccupation and the debris of the burnt Palace overlap. Here the finds of complete late stirrup jars and the burnt debris including tablets and sealings clearly indicate two levels of occupation, since, as everywhere, the reoccupation pottery suffered no final conflagration and destruction. There are even signs of late high foundations. Just to the north the old «Central Clay Area» and the ruins of the stairs to the Piano Nobile were, it seems, built over by the reoccupiers, high over the Palace floors and debris.

In the West Wing (2) there is no sign of reoccupied floors. A stirrup jar in the Room of the Tall Pithos is not dated (see below).

The Magazines had been partly plundered since antiquity, and some even of the floor cists broken into. There are clear signs of this disturbance in places where late pottery, some even Geometric and Hellenistic, intruded; but the mass of the burnt debris, with vases



of clay and stone, tablets and sealings, gives us the date of the destruction and the context of the tablets.

In the N. W. quarter (3) there was extensive reoccupation. In the area of the Room of the Stirrup Jars tablets were stratified beneath the reoccupation floors. The mistake of the 1900 excavation, in failing to distinguish the two significant floor levels, was

rectified in 1901 by further excavation. We have clear evidence here of tablets on a floor earlier than that of the final Bronze Age occupation. Palmer would even have them three floors down but fails to explain how they can then belong to the last period of occupation, to a final conflagration (which burnt and destroyed none of the objects by which he would date it!)¹.

In the North Entrance Passage burnt and broken tablets were found beside intact and unburnt late vases. This is manifestly no dating context, and at any rate, in the reoccupation period the Passage was blocked at its north end, and no longer used as an entrance to a Palace.

In the N. E. quarter (4) most of the Palace was eroded down to MM levels. Rooms by the School Room were reoccupied, and a corridor leading off it blocked and used as a store. Long after the final abandonment of the site the great terrace wall west of the School Room collapsed and deposited high over the debris of the Court of the Stone Spout fragments of the Toreador Fresco together with pre-destruction pottery and sealings.

In the Domestic Quarter (5) tablets and sealings were found in the Grand Staircase, the Hall of the Colonnades and the East-West Corridor, where there is no evidence for reoccupation, and indeed little to attract occupiers. Many of the apartments to the south were reoccupied and here tablets and sealings are stratified below reoccupation on floors by the Service Stairs and beneath a late wall on the balustrade of the Queen's Bathroom.

The story of the stratigraphy in the Corridor of the Sword Tablets is confused, but no account leaves the tablets in a reoccupation context, and it seems quite possible that the corridor was no longer in use after the destruction and that the adjacent Shrine of the Double Axes was approached from the east, down the slope.

This is the general picture as we can see it, and as the excavators saw it—in even greater detail. Can we really believe that the excavators could have been so completely wrong, and so often, about the context in which they found the tablets all over the

¹) The problem of a plaster floor here is explained in *Antiquity* 1961, 233 f. There is in fact (as Palmer has pointed out) a sketch in Evans' notebook (not Mackenzie's, and for 1900, not 1901) which suggests that tablets were found in the south part of the Room of the Saffron Gatherer; but, as Palmer agrees, this is an unimportant feature. The tablets are dated by the earliest context in which they appear, not the latest.

Palace? Over details we may still disagree with them—especially in the light of the accumulated knowledge of the last 50 years. But for this broader issue we—who can only study the excavation through their notes and admittedly incomplete publications—cannot contest the considered judgement of the men who watched the digging; who could make mistakes, certainly, but who could correct them as the evidence and their experience of the site grew; and who at the end and on calm reflection had no doubts.

But this is of course only half the story. There are other matters which bear upon the date of the tablets and I shall mention briefly some of them.

Clay sealings. Clay impressions of Minoan and Mycenaean gems were found wherever tablets were found in the Palace. They were preserved by the same fire and some of them even carry short inscriptions in Linear B. That they are contemporary with the tablets is quite clear. The dating of Late Bronze Age seals depends on a series of finds in Greece and Crete—Vaphio, Mycenae, Prosymna, and in Crete the tombs at Knossos and elsewhere. Using this evidence as a yardstick we find that none of the seals used for the Knossos impressions are as late as those in the LM III B tombs. And that the latest of them can be assigned to the early part of the LM III A period. This should surprise no-one since the same is observed of the pottery in the burnt debris of the Palace. Nor is there any danger of circular argument here, for the sequence of Late Bronze Age pottery is itself well enough established, even though we may still argue about details. Unless, that is, it is alleged (as it seems to be) that Palace Style pottery and other objects were still being made beside LM III B vases. And this does not deserve an answer, since it can only be maintained by ignoring all the evidence of many excavations over the last century.

Seals, we know, can be used for a long time, but it is incredible that these hundreds of impressions could every one have been made by seals some 200 years old.

Next there are the ideograms, the sketches of objects inventoried which appear on the tablets. Of the metal vases shown on the tablets the Vaphio cups and animal-head rhyta can only be matched by finds of earlier than 1400, and in representations on Egyptian paintings also of this date.

For the bronze corslets our only example in corpore is the

remarkable recent find at Dendra, again in a context of before 1400 B. C.

Stirrup jars are shown in some quantity on the tablets. They are most common in the Aegean world at the end of the Bronze Age, but no archaeologist will be surprised at their numbers at Knossos before 1400, since the shape itself was invented in Crete well over a century before, and plenty of LM I, LM II and LM III A examples have been found in the island. The boxes in the Stratigraphical Museum yield fragments of many of pre-destruction date from the Palace itself.

They lead us to think of stirrup jars elsewhere. The Linear B inscriptions on stirrup jars at Thebes have led Palmer to believe that they are imports from Crete. Furumark's inspection of the pottery from the floor of the parts of the Palace in which they were found—a Palace which had been adorned with very Cretan frescoes—led him to conclude that this part of the building was destroyed in 1400, at about the same time as Knossos. Added up this looks as though we have Cretan stirrup jars inscribed in Linear B before 1400 B. C. Again, this should not much surprise us, but this is not a point which can be laboured since the details of the Thebes excavations are if anything even more obscure than the reading of Cretan place names in Linear B.

What, then, of Knossos in LM III? Where is Idomeneus? Not, it seems, in the reoccupied rooms of the Palace, although it must be admitted that some Mycenaean structure might have stood over the ruins of the Palace, to be eroded away and leave slight if any impression on the site. The Little Palace (whose problems can only be tackled when the notebooks are published) seems to have been a centre of some importance in LM III, and there were certainly other villas of importance in the Knossos area which could have been occupied by a Mycenaean princeling. The graves tell more clearly the state of Knossos' fortunes.

The palatial sites in the rest of Crete had been overwhelmed before Knossos—the Knossos of the Linear B tablets—flourished for a generation under, it seems, foreign rule. Then Knossos fell—to Greeks or Minoans, we do not know—and the island developed freely again in its native tradition rather than any Mycenaean one, having already lost much of its foreign trade and trading posts to the Greeks.

But these broader historical issues lead us steadily farther and

farther from the basic archaeological facts, and we must be careful not to let our prejudices about where we think Greeks or Cretans ought to be at any time colour our interpretation of the evidence. This evidence may enable us later to theorise about the history, institutions and language of the Cretans and Mycenaeans, but the evidence—unvarnished and unedited by commentators, or—if need be—by the excavators themselves—must come first. On the subject which has occupied us in these last minutes the evidence points one way only, and this is the way archaeologists, philologists and historians must follow.

ΣΥΖΗΤΗΣΙΣ

Palmer: I should like to remind you of the title of my lecture: «The Documentation of the Knossos Excavations». I was not arguing a case. My theme was simple; documents exist in Oxford which contain important unpublished facts relevant to the stratification of the tablets. I gave some examples and urged scholars to suspend judgement until the full publication.

Meanwhile, Mr. Boardman continues to make inaccurate statements about the material entrusted to his confidence on the 4th July 1960 on his acceptance of an invitation to act as coeditor in the publication. I remind you of his allegation 1) about the absence of tablets in the south part of the Room of the Saffron Gatherer and 2) about the position of the reoccupation plaster floor. How true these statements are you saw from Evans' own sketch plan.

But this room is in fact of slight importance for the tablets. As I told you, it contained only a few. The great mass lay in the Northern Entrance Passage. Here there is fortunately no conflict between the Notebooks and the published statements. To the last, Evans sited the «Great Deposit» above a clearly defined postdestruction surface along with the double amphorae.

Consider now Mr. Boardman's treatment of Evans' account of the history of the Northern Entrance Passage. You will remember that there were three phases. In MM III it was about 7 m wide; subsequently it was narrowed by the building of bastions; then in LM III some bastions were removed and the passage restored to its original width, with the earth floor above which the tablets were found. This account is faithfully repeated by Pendlebury. Consider now Mr. Boardman's attitude: on the strength of an old photograph he rejects the account of Mackenzie, Evans and Pendlebury. At the end of LM III, as represented by the double amphorae, he now claims, the whole entrance had been blocked by a wall and used as a pottery store. You will find nothing of this in Evans. As for the tablets, Mr. Boardman says they were not on the earth floor, where Evans sites them, but in

it. This is the difference between us : I accept all Evans' statements about this deposit and its environment. Mr. Boardman, in defence of Evans' dating, rejects Evans' account.

Now I turn to another serious inaccuracy in Mr. Boardman's presentation. He showed us a plan of the Palace with the reoccupied and non-reoccupied areas in different colours. This plan is a pure fiction. I will demonstrate this by one example. First, let us consider what Mackenzie says about stirrup jars. In J H S 1903, he remarked that virtually no trace of such vessels was found in the LM II Palace, whereas they became extremely common in the following period. The excavator also traces the degeneration of the ornamentation. At the close of LM III it had been reduced to coloured bands. Evans, too, repeatedly insisted on the absence of stirrup jars, and we find the same general picture in Pendlebury [1939].

Now for my example. We enter the Lobby of the Stone Seat from the Central Court. On our right, in a recess of the northern wall, a nest of tablets was found in a burned deposit. At the west end of the north wall, a door leads to the Room of the Tall Pithos. Just inside that door Mackenzie found lying on its side a stirrup jar, which he describes in the same terms as he uses for the latest stirrup jars in the article cited. Now this important find does not appear on Mr. Boardman's plan. It is evident that the vessel dates the latest occupation of the floor. If we accept, as I do, Mackenzie - Evans' account of the stirrup jars, the floor was in occupation at the end of LM III B. At all events, it is a fact which must be taken into account.

Now let us consider Evans' statement about the use of these rooms in their latest phase. The shallow vats, he said, point to their use for the storage and disbursement of oil. With what subject does the nest of tablets found here deal? With one subject only: the storage and disbursement of oil. You see, there is perfect harmony between the tablets and their archaeological environment, which includes an intact stirrup vase.

Here, then, we have some more recorded facts which Mr. Boardman has excluded from his presentation. Once again, I appeal to scholars to suspend judgement until all these recorded facts—and they affect most parts of the Palace—are published.

*Boardman: I will only take a minute. I hope Professor Palmer's second lecture will also go on record. I have already demonstrated his inaccurate use of the material in articles in *Antiquity*.*

Palmer: This is simply abuse. I am concerned with facts. I ask Mr. Boardman only one question. Does Mackenzie record or not record the stirrup jar in the position stated by me? Please answer «yes» or «no».

Boardman: He does.

Hood: Any archaeologist who has kept a daybook knows that later results will necessitate revision. I should also like to remind you of Mackenzie's statement that no tablets or sealings were found in reoccupation deposits.

Palmer: What was the date of this statement?

H o o d: 1904.

P a l m e r: *By all means let us ignore the statements about the Room of the Stirrup Jars. I have already said that this is only the fringe of the deposit. I repeat again that there is no discrepancy between the Notebooks and the published statements concerning the Great Deposit in the Northern Entrance Passage.*

[There was no time for further discussion].