

# Antiquity

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## Editorial

THIS issue, the first number in our thirty-sixth volume, contains, among many other things of interest and value, a discussion of three matters which demand some editorial comment. The first is the account by Dr Weiner and Dr Napier of the evolution of man in Africa in the light of Dr Louis Leakey's new discoveries—an article which we invited them to write developing the thesis set out by them in *The New Scientist* some while ago. The second is the publication by Professor Grahame Clark and Professor Godwin of the C<sub>14</sub> dates from Shippea Hill, with all the significance they have for our dating of the beginning of the Neolithic in Britain. The third matter relates to the controversy over the dating of the Knossos tablets, started eighteen months ago by Professor Palmer in an article in *The Observer* which we criticized adversely at the time (ANTIQUITY, 1960) and continued since then in and out of this and other journals, and in and out of common rooms and combination rooms all over the world. No less than twelve pages of our current issue deal, in various ways, with the controversy to which, whatever else he may do, L. R. Palmer has had his name firmly attached in the minds of scholars and of that large general public in and out of the British Isles interested in the past, the truth, and scholars at war.

The article on the finds from the Olduvai Gorge by Dr Napier and Dr Weiner discusses the bearing of the remarkable discoveries by Dr and Mrs Louis Leakey in 1959 and 1960 on the general problems of human origins. It is only fair and just for these two authors to describe the discoveries as 'a major contribution to human palaeoanthropology, ranking with Dubois' discovery of Java man and Dart's recognitions of the adolescent australopithecine with Taung', and to say that 'unquestionably there are human relics still to be found at Olduvai at both higher and lower geological levels than those already known'. Let us all hope that the work on which the Leakeys are now engaged in Tanganyika may yield these and other human relics: it would be a fitting reward for a career of devoted, inspired and unremitting work in East Africa.

Until recently the duration of the Pleistocene has been spoken of in a general way as a million years and the appearance of the first human beings, that is to say the first tool-making animals, as '600,000 years ago' or 'between a half and two-thirds of a million years ago'. A revised date for the volcanic rocks of Bed I at Olduvai (in which *Zinjanthropus boisei*, the juvenile mandible and the stone artifacts of the Oldowan culture were found), was announced on 29 July, 1961 by Leakey, Evernden and Curtis: it was obtained by the potassium-argon method and was 1.75 million years 'B.P.' In a paper in *Nature* for 25 November, 1961, Professor von Koenigswald of Utrecht and Professors Gentner and Lippolt of Heidelberg announced a potassium-argon date of 1.3 million years B.C. for the

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basalt *below* Bed I, but also dates for two basalt pebble tools from Bed I of between 2.25 and 1.4 million years B.C.

The technique of potassium-argon dating has, as yet, only been very briefly discussed in this journal (ANTIQUITY, 1960, 213 and 1961, 59) and there is surely no certainty about the published dates. In a letter to the *New Scientist* published on 23 November, 1961, Leakey refers to some of these problems, and says that 'the beginning of the Pleistocene may well have to be thrown back to 2½ to 3 million years' while Napier and Weiner are agreed that 'it is highly likely that the earliest known australopithecines may have to be back-dated to perhaps 1,000,000 before the present time'. The late Abbé Breuil is supposed to have said—it may be part of the growing archaeological mythology—when visiting Reid Moir's sites in Ipswich, 'Today has certainly aged mankind'. Potassium-argon dating is making that ageing a surprising reality, but we wait for more dates and for a resolution of problems such as the apparent conflict of dates at Olduvai.



The publication of the excavations at Shippea Hill in 1935 seemed to most of us at the time a major contribution to Britain archaeology, and so it was. Now, a quarter of a century later, the publication of the C14 dates from this site is an equally major contribution but one which also shows what has happened to archaeology in the last twenty years. Radiocarbon dates have replaced the guesses and reasonable suppositions of a pre-C14 period, and, where they give a consistent and understandable picture with a pattern of dates provided from many sites and determined in many laboratories, cannot be set aside. Until recently many of us had thought that the Neolithic began in the British Isles 'about 2000 B.C.' Now we know we were wrong and that it began at some date soon after 3500 B.C. The Neolithic in north-western Europe was a long period of at least 1500 years. These are the problems worried out into the open by the article of Professor Clark and Professor Godwin and foreshadowed by our earlier publication of the C14 dates from elsewhere (ANTIQUITY, 1960, 111, and 1960, 212).

The implications of this new Neolithic chronology for north-western Europe will be discussed at the Prehistoric Society's Conference in London on 6 to 9 April which we shall report here in due course. Some of the implications of the new C14 dates were discussed at the Congrès Préhistorique de France which met in Rennes and Brest in September, 1961, and there were communicated then several new dates which we hope to publish in ANTIQUITY in due course. Some of them referred to megalithic monuments, and some of these if not archaeologically unacceptable seem prehistorically indigestible at the moment. We shall refer to this matter of the radiocarbon dating of megalithic monuments and the position of megalithic tombs in the new Neolithic sequence in a later issue of ANTIQUITY. We have still too few dates to see established a coherent pattern.

All this has very considerable methodological and historical implications. There are sincere and distinguished archaeologists who are still suspicious of C14 dating and some aspects of these suspicions have been discussed recently in *Germania* for 1961 by Muller Beck, and by Milošević who, *inter alia*, there criticizes Mellaart's use of C14 dates in his article 'Anatolia and the Balkans' published in ANTIQUITY in 1960.

What we need is a vast number of dates from all these new scientific techniques—C14, potassium argon, archaeo-magnetism, thermo-luminiscence—and the clearest possible analysis of the theoretical problems involved in the techniques of dating. Meanwhile we should have no doubt about two points. Dr Deevy, who with Dr Flint, edits the invaluable supplements to the *American Journal of Science*, now to be called *Radiocarbon* (volume 3

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just out) urges that no one should publish any C<sub>14</sub> date without quoting the laboratory number. We in *ANTIQUITY* will try to do this and will not accept for publication any contribution which does not include this information. Secondly (and Mr Deevy might not be with us here) let us all abandon this tiresome habit of sometimes stating dates as B.P. Please let us quote all dates as B.C. We were brought up in an absolute chronological system which thought that the B.C./A.D. line meant something. It may not in terms of human history but it does in terms of human scholars working in the 20th century. B.P. is a nonsense of scientists who cannot see that the last 2000 years are immaterial in a story which goes so far back and into such realms of time as make 2000 years immaterial.

Recently, in *American Antiquity* (1960-61, 557-558), Dr Junius B. Bird of the American Museum of Natural History criticizes the use of B.P., in a short note entitled 'B.P.: Before Present or Bad Policy' and says, 'When B.P. appeared on the scene it seemed reasonable to suppose that it would disappear quickly . . . certainly B.P. is no solution, and I hope that this B.P. concept can be flushed out of the minds of its advocates down to where it belongs before our literature is further complicated or polluted by it'.



Professor L. R. Palmer's criticisms of Sir Arthur Evans's work in the Palace of Minos at Knossos, and of the chronology of the Linear B tablets from Knossos, was first published in an article in *The Observer* for 3 July, 1960, and in a B.B.C. broadcast printed in *The Listener* for 27 October, 1960. These statements prompted Mr Sinclair Hood to write his article on 'The Date of the Linear B Tablets from Knossos' (*ANTIQUITY*, 1961, 4) and the June, September, and December numbers contained rejoinders and counter rejoinders from Professor Palmer and Mr Boardman (*ANTIQUITY*, 1961, 135, 233, 308). The present number contains further notes from Palmer and Boardman, a review of Palmer's *Mycenaeans and Minoans* by Mr John Killen, a Research Fellow of Churchill College, Cambridge, working on Linear B Studies with Mr John Chadwick, and an article summarizing the controversy from Mr Hood. With these we feel that *ANTIQUITY* has done its share, or perhaps more than its share, in this particular controversy and we shall print no more until the publication of the promised scholarly books by Professor Palmer and Mr Boardman demand reviews which may reopen, but we hope, finally close, this dispute, which has been conducted in these pages, and in the pages of the many journals that have reviewed Palmer's book (and where he has castigated his reviewers and they recastigated him), with a splendid 18th-century feeling for the place of invective and abuse in scholarship.

The titles of some of the *ANTIQUITY* contributions have been, in themselves, intriguing. Mr Boardman's note in September was called 'The Knossos Tablets: An Answer', Professor Palmer's note in December was entitled 'The Knossos Tablets: Some Clarifications', and Mr Hood has been courageous enough to call this article in this present issue 'The Knossos Tablets: A Complete View'. Only a few days before we received Mr Hood's manuscript we got a letter from a distinguished Oxford scholar begging us to get someone to straighten out the controversy and explain the rights and wrongs to non-specialists. We think that Mr Hood's article does this, as also did a very interesting and clear talk given on the Third Programme by Mr Boardman on Sunday, 3 December, 1960, entitled 'Knossos and Evans'.

*The Listener* unfortunately did not print Mr Boardman's broadcast, but the broadcast itself led to a Third Programme Discussion between Professor Palmer and Mr Boardman with the Editor of *ANTIQUITY* in the Chair. We felt like saying 'On my right Professor Palmer, on my left Mr Boardman, Seconds out of the ring', but are there any seconders

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for Professor Palmer? No one seems to have written in support of his case up to date; the dispute as we see it today seems to be Palmer *contra mundum*. Of course this does not mean that Palmer is wrong. The lone voice crying in the wilderness has often been proved to be right; there is establishment and orthodoxy in archaeology, prehistory, ancient history and philology as in everything else. The Palmer case must be examined on its merits. Here we would only make, editorially, the following few points.

First, there is no personal element in this dispute. At one stage Palmer was represented as saying that Evans had virtually faked his results but this he has strongly repudiated in the press and in conversation with ourselves. This is not a dispute about the reputations of one or more scholars—although some reputations may tarnish before it is over—it is a dispute about the establishment of facts in antiquity derived from archaeological evidence. As such it is an interesting and valuable exercise at a time when we see how uncritical complacency makes one man's hypothesis another's fact, and something completely proven to his devoted pupils. The Palmer challenge is particularly stimulating at a time when a glut of facts and excavations leave few people time and energy enough to step aside and ask theoretical questions about the nature of archaeological evidence.

Palmer's challenge to Arthur Evans's work is a challenge to the correctness of Evans's archaeology. It is true that Palmer is not an archaeologist, but one need not be an epigrapher to understand the value of inscriptions, a numismatist to understand the value of coins, a trained historian to understand the value of written sources. Any one is entitled to use archaeological evidence to produce historical facts, if he uses this evidence correctly and if he understands it. Palmer has been driven to disputing the archaeological publication of Knossos in the hope of finding archaeological support for his philological arguments. He has eagerly sought in the Knossos Day-Books and elsewhere archaeological support for the post-1400 B.C. date of the Knossos tablets, and in so doing has revealed that he does not understand the name and nature of archaeological evidence.

Of course the notional basis of Palmer's argument is perfectly sound. All excavations—that is to say all excavation reports—can and must be reassessed from time to time and sites virtually re-excavated in the light of current knowledge. We have a very good example of this in the work which Professor Hawkes did in reinterpreting the work of General Pitt Rivers in Cranborne Chase. There is no reason to suppose that Arthur Evans's excavations were perfect any more than those of any other excavator, no reason to suppose that they are less liable to criticism and re-interpretation than those of anyone else. The giants of the past—Schlieman, Pitt Rivers, Petrie, Arthur Evans—will have their work reassessed and sometimes turned upside down as must the archaeological giants of the present be prepared to be reassessed and inverted.

But this must be done with a full understanding of archaeological method and how the material remains of the past, crumbled and ruined in mud and filth, can be made to speak historically. Arthur Evans's father, Sir John Evans, in at one time criticizing the work of another scholar on British coins, said that he was 'unacquainted with the common course of events'. It would appear to us that Professor Palmer is unacquainted with the common course of archaeological events. First he does not seem to realize that all archaeologists make mistakes, muddle find spots and the rest of it. Error of this kind is not confined to archaeology. A catalogue of errors, and this is what Palmer's 'facts' amount to, is no more than a demonstration of human frailty. These must be set against the totality of evidence studied at a site like Knossos and the overall picture of prehistoric chronology in the East Mediterranean. Secondly he does not seem to understand the nature of stratigraphy and of the deposition of objects; things that are found together are not necessarily of the same date, and the exact depth at which objects are found (ah, shades of Sir John Marshall, and of

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that devastating note of Wheeler's in *Ancient India* for 1947 (3, 143), which, incidentally, Mr Pyddoke surprisingly did not include in his *Stratification for Archaeologists*, is not evidence of absolute chronology. But, thirdly, excavation does not proceed by an accumulation of entries made at the end of single days. The excavator is the man who sees the whole picture built up of days and months of daily observations and who can say 'What a fool I was not to see what that level meant a fortnight ago', who uses his daily notes (and many excavators refuse to have day-books), to help his memory in assessing the final picture. If there is an apparent conflict between a daily diary and an annual account or a full account written years later it is not because of real conflict or misrepresentation, it is because of rethinking again the whole picture of a total excavation. The conflicts that Palmer and others find between the day-books, the annual reports, and the final reports at Knossos are not evidence of bad faith but of good archaeology.

Professor Palmer is like Mr Gradgrind in *Hard Times* who said 'What I want is Facts'. The question is 'What is an archaeological fact?' In this issue Professor Piggott urges us all to read Professor Beveridge's *The Nature of Scientific Evidence*, and I equally urge everyone to read E. H. Carr's *What is History?*, a book based on the George Macaulay Trevelyan Lectures which he delivered in the University of Cambridge in the spring of 1961 (London, Macmillan, 1961, 21s.). We will content ourselves with quoting two sentences from this remarkable, clear and cogent book. 'The historian is necessarily selective. The belief in a hard core of historical facts existing objectively and independently of the interpretation of the historian is a preposterous fallacy, but one which it is very hard to eradicate'. It is this fallacy, we suggest, that is confusing Professor Palmer; the facts of Knossos are the *interpretation* by Mackenzie and Evans of what they found and dug, *not* entries in day-books. This is not an attack on Professor Palmer who has given us a great deal to think about and whose thesis about the history of Knossos may well be proved right on philological grounds. We invite comments on the whole problem of the nature of archaeological evidence, while declining further comments specifically on the Palmer-Evans Knossos dispute until the full publications of Palmer and Boardman to which we have referred. If Palmer's writings will set us all thinking more deeply and carefully about *how* we think we establish historical facts by archaeological means, he will have done us all a far greater service than waging a minor Minoan war.



A group of young archaeologists (mainly archaeological students and research students, and interested students in other disciplines) are at present discussing the possibility of a small conference which would give them the opportunity of exchanging views and ideas on archaeology in general, unimpeded by the authority and experience of their elders. Such a meeting which we warmly support might last from one to three days, be confined to men and woman under some such age as thirty, and would consist of papers and discussions about recent excavations, and about techniques of excavation and discovery. It is not intended that there should be at such a meeting analytical or synthetic papers of a general nature, but that the emphasis throughout should be on field archaeology in its widest sense, and particularly excavation in the field. This is not conceived of as a separatist movement but as a semi-private opportunity for keen young people to meet themselves and discuss common problems. We hope this idea will be supported by the Council for British Archaeology, the Prehistoric Society, the Society for Mediaeval Archaeology and other interested bodies. Those interested in what is at the moment only an embryo idea should write to *Barry Cunliffe*, 28, *Carlisle Road, Cambridge*, or *Richard Reece*, 85, *Victoria Road, Cirencester* (not the Editor of *ANTIQUITY*).