

Mochlos, in the Gulf of Mirabello not far from Gournia. The continuity of this practice from EM II to MM II is indicated by examples of silver-riveted daggers from Platanos (EM III-MM I), Kalathiana (MM I-II), and Agia Triadha (MM II). It seems likely therefore that the plating technique evolved in Crete, during MM I-II, and this conclusion might be significant in relation to the development of inlay techniques. As to the centres where the technique of silver-plating may have originated, we might suggest that they would be situated somewhere in north-central Crete where a silver-working tradition is best evidenced during the Early Bronze Age (Branigan, 1968c, 222, 226).

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## The Quest for Arthur's Britain

*Professor Charles Thomas's review-article of (ed.) G. Ashe, The Quest for Arthur's Britain (ANTIQUITY, 1969, 27) has aroused widespread comment. We print here a contribution from Dr C. A. Raleigh Radford, Chairman of the Camelot Research Committee, and a reply from Professor Thomas.*

The March number of ANTIQUITY includes an article 'Are These the Walls of Camelot?' by Charles Thomas. Though formally a review of 'The Quest for Arthur's Britain', it is, in large part, a thinly veiled attack on the Camelot Research Committee, its Officers and its Director of Excavations. As Chairman of the Committee I must ask to be allowed to correct certain errors and implications. I am not strictly concerned with the reviewer's opinion of the book, even though he falls into a number of misconceptions in the course of his strictures. E.g. the Monymusk Reliquary is a portable reliquary and, though I should not ascribe to it a date as early as c. 600 on art-historical grounds, it is inaccurate to say that such reliquaries were necessarily made at the time of enshrinement or later. Professor Thomas seems to have confused the portable reliquary with the tomb shrine—also a reliquary—and in the context his statement is irrelevant and misleading.

The gravamen of Professor Thomas's charge

is that he objects to 'a controlling Committee embracing bodies and individuals, whose interests are non-archaeological and, in so far as eventual interpretation is concerned, certainly not objective.' This can only mean that the final—and presumably also the interim—reports on the excavations at South Cadbury will be coloured—to use no stronger a term—by the views of members of the Committee or of the bodies by whom they were nominated. This charge is supported by no reference to the two preliminary reports, published in the *Antiquaries Journal*, the notes in ANTIQUITY or any other publication. The Camelot Research Committee follows a normal procedure. The Director of Excavations is appointed by the Committee and reports to it; his report is formally received and publication agreed to. The reports in the *Antiquaries Journal* were discussed with the Editor by Mr Alcock and myself. The result was reported to the Committee when the report was tabled and publication agreed to without discussion. I have known an *Editor* insist on alterations to a report; I have never heard of a Committee or its members seeking to exercise such a function. In this case, as Chairman of the Committee, I must absolutely reject the idea that such alterations have even been considered.

Professor Thomas dislikes the publicity

methods of the Committee. This is a matter of taste. My own view is that archaeology is a subject in which the public is interested and that the public, which, directly or indirectly, provides the money, has a right to be kept informed in terms which it can appreciate. At Cadbury the most striking discovery to date is the massive refortification, 1,200 yds. long, associated with pottery of the period AD 450–600. This is something new in the Province of Britannia and can justly be termed sub-Roman in the sense that the adjective has been used in recent archaeological publications, including the *Survey of Cornish Archaeology* published in 1958. It is hardly surprising that this discovery has excited more interest and greater publicity than the valuable results achieved in other periods of the occupation of South Cadbury.

Truly 'there lurks a danger that the project might be brought into a shadow of disrepute—because results can be journalistically misrepresented, or *a priori* theories allowed to colour interpretations given in popular media'. After more than a generation of experience—largely on those early medieval sites, which excite most curiosity—I should have stated the facts bluntly with none of the delicate phrasing of the review. The danger is not confined to South Cadbury. There can be few excavators who would not wish to forget the indiscretion, put out in good faith, but perhaps hastily and unguardedly at the end of a long and tiring day.

The article raises the question of finance. It is, of course, impossible to apportion, within the global figure, the amount spent on each period. But I feel that Professor Thomas, on reflexion, would probably agree that an expenditure of £5,000 per annum for three years is not excessive for the investigation of a major hill-fort, the interior of which covers 18 acres. The total compares, not unfavourably, with an average of £1,300 per annum spent on the comparable site of Maiden Castle, Dorchester, some 30 years ago. That site lacked the complication of two important post-Roman refortifications.

It should not be necessary to point out that

the only connexion between *The Quest for Arthur's Britain* and the Camelot Research Committee is the fact that most of the contributors to the book are members of the Committee. The excavations, to quote Sir Mortimer Wheeler's foreword to the current appeal 'have begun to open an impressive vista of British history and prehistory, ranging from the Stone Age ramparts of 3000 BC or earlier to a final refortification in the 11th century AD, when kings Aethelred the Unready and Canute minted coins here'. A letter from the publishers explains that: 'The book's purpose is two-fold: to state and appraise the legendary tradition of Arthur, and examine this in the light of recent historical and archaeological research; and to summarize the present state of knowledge of the 'dark ages' in Britain.'

I write this letter with regret. I have known Charles Thomas for many years; his contributions to our knowledge of early medieval Britain are many and valuable. In the present case he seems to be labouring under some misapprehension.

*Professor Thomas writes:*

This will be as brief as possible. I was not attacking either Dr Radford, or Mr Alcock (whom, indeed, I *defend* in my last paragraphs), nor any other archaeological friends. I was however voicing, perhaps a little obliquely, a view which I know to be held by a good many fellow-students and colleagues; a protest against the association of what we might call 'the Arthurian fringe' with a major field-project. This is a dangerous step, and no amount of casuistry about the needs for funds, the ends justifying the means, or what Dr Radford regards as 'the public's . . . right to be kept informed in terms which it can appreciate' will make me change my mind. The public media are of course always with us now, but there are ways of employing them which side-step some of the pitfalls. Of course the site is a big one, requiring a lot of money, and I imagine the long-term Iron Age results are quite sufficient to justify all the expenditure without the bonus of later periods.

As for the book and the Camelot Research

Committee being in some way linked—for example, by a common central interest in ‘Arthur’, by Mr Ashe’s roles as editor of the first and secretary of the second, and by Dr Radford, Mr Alcock and Mr Rahtz being common to both—I can’t see why critics of my review seem so keen to dissociate the two. What is the book about, if not mainly about ‘Arthur’ and ‘Camelot’? The publishers may like to think that it is a summary of the present state of knowledge of the ‘dark ages’ (horrible term!) in Britain. It is a most lop-sided summary, in that case.

Dr Radford has avoided two of my serious criticisms. The first was on the impropriety of the use of the word ‘Arthurian’, as a cultural or chronological label. I need not repeat my strictures on this. The second was on the shaky validity of the imported pottery, in what I believe to be a ‘secondary find-spot’, as a chronological guide to an event or events (the *floruit* of ‘Arthur’, Mons Badonicus, the battle of Camlann, etc.) which themselves lack any firmly agreed chronology. The plain truth is that, barring some quite unforeseeable narrative inscription, or wondrously preserved MS, turning up, a question like ‘Was Arthur

here?’ or ‘Was this Arthur’s stronghold in the year 500?’ is unanswerable in terms of archaeology. Nor is it possible to give a satisfactory answer in terms of legitimate inference so long as we lack general agreement as to the existence, dates, career, and locale, of ‘Arthur’; and it should be borne in mind that a respectable academic view (which I myself happen personally to share) would locate the entire Arthurian episode, if it be an historic one, in North Britain. It is because of such factors that I regard the statements and claims put out about South Cadbury—and I accept that these are not necessarily made, supported, or authorized by all the Committee or their Director on all or any occasions—as exceeding the limits of inference, and liable to recoil on us all to the detriment of future projects. Why not have a try at projecting the Iron Age image—the oppidum of the Durotriges Lindinenses, or whoever it was involved with in pre-Roman times? Is it old-fashioned of me to suppose that the public’s right to be kept informed is, if a right at all, a right to a straightforward critical evaluation of *all* the evidence? Or does the Iron Age lack the monetary appeal of ‘Arthur’?

## Radiocarbon Dates for the Newgrange Passage Grave, Co. Meath

In the interim report on the Newgrange excavation in ANTIQUITY, 1968, 40, mention was made of the fact that the tomb builders had caulked the roof joints with burnt soil and sea sand. Where the arrangement of this material could best be observed, it was clear that the sea sand had been put into the joint first followed by the packing of burnt soil, the aim evidently being to prevent the ingress of water which would percolate down through the cairn. This precaution was additional to the system of water grooves described in an earlier interim report (ANTIQUITY, 1964, 288).

The sea sand is identical in its constituents with that on the present sea shore around the mouth of the River Boyne, about 20 km. down river from the site. The burnt soil seems to have come from a domestic area because it contained a few fragments of worked flint

similar to those found in the course of the excavation and some scraps of animal bone. Perhaps turves into which these items had been trampled had been removed from an adjacent area and fired. There was so much of the material present that it looked as if it had been burnt specially for the purpose. The burning was not done *in situ* on the roof of the passage. The material was light grey in colour and when wet was very sticky—almost putty-like to the touch.

Two large samples of it were collected—Sample no. 1 from the caulking of roof-slab 3 (i.e. the third from the passage entrance) and Sample no. 2 from under the cross-lintel which supports the boulder cap at the point of junction of the passage roof with that of the chamber (FIG. 1). The material could have been put into these positions only at the time the