Notes and News

Two notebooks of General Pitt Rivers

PLATES VI—VIII*a*

In some ways General Pitt Rivers's classic excavation volumes form a monumental blue and gold façade behind which little appears of the field methods upon which they had depended. Little additional information can be found to set these final, masterful formulations against the quality of the original site records, and so the relatively few excavation photographs have formed the basis of much of the discussion and reassessment which have followed in this century. In September 1971 however, two of the General's notebooks came to light in Dorset County Museum amongst a newly acquired collection of Harold St George Gray's papers relating to his own excavation at Maumbury Rings. I am grateful to the Museum's curator, Mr R. N. R. Peers, for permission to discuss these here.

These two notebooks cover work carried out between 1893 and 1897 and refer mainly to the Handley Hill enclosure, the nearby barrows and urnfield, the Angle Ditch and Wor Barrow, all of which were published in the final volume of the excavations (Pitt Rivers, 1898). In addition to notes upon these sites, there are drawings alone which relate to King John's Hunting Lodge, Iwerne Courtenay, South Lodge Camp and Martin Down. There are also detailed drawings of a decorated Roman lead coffin from a quarry at Marnhull in Dorset. Each notebook was chiefly compiled for the General by St George Gray, and accompanying correspondence reveals that these two volumes were taken by him from Farnham after the General's death, when similar material there was being dispersed without authority.

The first notebook consists of 57 pages of field drawings interspersed with occasional rather indifferent landscape and figure studies. The presence of these suggests that a fair copy was to be delivered to the master. They relate to each of the sites set out above and are the drawings referred to by Gray in his memoir of the Iwerne excavation (Gray, 1947). Of particular interest are field plans of ten of the secondary burials at Wor Barrow, scale drawings of the material accompanying the Marnhull coffin and a series of sensitive watercolours recording the Roman wall plaster at Iwerne itself. With these are two finely executed watercolour section drawings, apparently of the 'granary' on the latter site. These must be one of the earliest attempts to preserve an accurate record of soil colour and closely prefigure current practice. They are printed here in black and white (PL. VIII*a*). [A difficult, but we thought worthwhile, task. *Ed.*]

The second notebook is more informative and contains field notes on the period between 10 August 1893 and 27 April 1895 when Gray records: 'Finished levelling & beating the ground at Wor Bar & made everything ready for sowing the grass seed.' The notebook consists of 133 pages of notes including fifteen with sketches of excavated features or finds, the majority covering excavation on the latter site. The most informative of these are discussed further below (p. 49; PLS. VI and VII). The notes are in several hands, though the majority are clearly by Gray himself. Most of the remaining passages must be the work of other 'men of good character as well as energy', whose duties, the General told the Archaeological Institute, included 'the surveys, the contouring, . . . the drawing of the plates and the close supervision of the workmen on the ground' (Pitt Rivers, 1898, 28). The General himself was responsible for only seven pages of the original field notes, those describing most of the primary burials and seven of the secondary burials at Wor Barrow. These are both the most incisive and the most illegible passages in the book.*

* Gray recalled to Professor Piggott that the General had difficulty in reading his own handwriting and would often rely on him for a definitive version.

The notebook as a whole takes the form of an extended diary in which the General's 'clerks' recorded indifferently the attendance of the workmen, the hours spent on the site, the day's discoveries and the state of the weather. The third item, which is not always the longest, is frequently linked to a specially prepared plan, section or photograph, not of course represented among this material. On occasion, however, photographs mentioned in this way may be identified in the published report. This format rather reflects that of the excavation volumes, in which much of a site report may take the form of an extended commentary or caption to a series of plans and photographs. Though individual layers were often accurately recorded in section drawings, the lists of finds which punctuate these notes are usually recorded only by depth. A surveyor's staff was used for this purpose giving measurements in decimal feet. These inventories went to make the Relic Tables which were compiled by Gray for the final publication. Here, one suspects, retrospective attention might be paid to natural stratigraphy, perhaps with the aid of the drawings. Only rarely is this rather constricting format relaxed in favour of a more comprehensive description of groups of excavated features. Of these the most extended is probably Gray's record of the urnfield accompanying Barrow 24 on Handley Hill. Normally it appears that the main record of such features was contained in the plan. It is interesting to notice that up to twenty years later Gray was to use an identical layout in his notes on Maumbury Rings.

For this series of excavations the General employed a labour force of between seven and fourteen and a staff of up to three. In 1894 there is an additional reference to 'a boy named Cook who is engaged to wash pottery'. Work took place six days a week and normally commenced at 7 a.m., though this was occasionally relaxed in bad weather. Sometimes the men had already started work at 6.30 at the General's Museum. Their attendance seems to have been rather irregular and there is a useful reminder of their rustic background when Gray remarks that the missing Hebdige had 'left to go sheep shearing near Wimbourne'. The General's appearances on the site were regular but equally unpredictable. At times he seems to have arrived even earlier than his men (Gray, 1905, xxvii). Though some had worked with him before, their reaction to such strange employment is nowhere explicit. There are hints, however, of some unrest at the master's requirements on the Angle Ditch where four workmen left the site in eight days. The work was carried on at an extraordinary pace throughout and it may be well to remember this in assessing the published results. The Handley Hill enclosure, for example, was commenced on the morning of Thursday 10 August 1893 with seven men. By 3.30 on the Saturday the 900 sq. yds. (750 sq. m.) of the interior had been dug to the bedrock a foot (30 cm.) below the surface, and by 19 August the entire bank and ditch had been removed. Barrow 24 with its associated urnfield of 51 burials was totally excavated in less than seven days, including an area of 1,250 sq. ft. (115 sq. m.) beyond the lip of the ditch. Another example is given by Wor Barrow where the upper filling of the entire ditch, including four secondary burials, was removed in a week. The complete ditch filling, itself over 12 ft. deep and 100 yds. in length (3·5 m. by 90 m.), was removed in little more than a month with the careful examination of thirteen burials. After an interval, a labour force, increased to 14 men, took just six weeks to clear the entire mound to a level nearly 2 ft. (60 cm.) within the natural chalk. This period included interruptions from bad weather and the excavation of 16 more skeletons and the internal timber enclosure. In spite of the rapidity of the work, the ageing General's attention never flagged and special arrangements were adopted in his absence: 'Several urns were found but in excavating them they came to pieces except one which was got out nearly entire & sent to Rushmore in the carriage.' Nowhere is this scrutiny more apparent than in one memo inserted by Gray in the main notes: 'Scale required for Wor Bar, Bar 27 & 26 & Angle Ditch = 70 ft. = 1 inch size of plate $8\frac{1}{2}$ inches square.' Understandably the final report appeared just three years after the campaign was complete.

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It would be surprising if such an extensive series of notes were not to include some significant detail which did not find its way into this final volume. In this case the most valuable material concerns the most important site, Wor Barrow. This seems to shed light upon two disputed areas, the possible mortuary house covering the primary burials, and the timber mortuary enclosure beneath the mound.

One sketch, not previously reproduced (PL. VI), shows a section of the central burial area. From this it appears that a possible pit, not included in the final report, had been cut into the turf pile and perhaps refilled before the building of the mound. If so, it is unlikely that any wood and turf structure over the burials can have collapsed beneath the weight of the barrow. At the same time the notebooks record each of the two pits in the buried land surface, taken by Ashbee as the supports for a pitched mortuary house (1970, 126). From this it appears that the larger of these 'was discovered beneath the skull of skeleton 4'. This would seem to rule out this pit as a component of any structure to enclose the bodies unless some lateral movement of the skeleton could be established.

A second useful drawing (PL. VII) provides the first full section of Pitt Rivers's 'Funeral Enclosure'. Previous discussion has necessarily been based upon two excavation photographs, each showing the spoil against the wooden uprights mainly in longitudinal section (Pitt Rivers, 1898, pl. 255, fig. 1; Piggott 1954, pl. IIb and 56; Atkinson in Vatcher, 1961, 167). This sketch again shows a vertical break in the barrow stratigraphy and the site of an upright post, but suggests that the bank of material tipped against its outer face may have been less substantial than hitherto supposed.* If indeed this cross section is typical, it is hard to see how the bank can contain the entire spoil from the early enclosure ditch isolated by Atkinson (in Vatcher, 1961, 167). In fact, assuming Pitt Rivers's own section of the truncated ditch to be representative (1898, 65), the volume of spoil would be a little over 14,000 cubic feet.† The bank on the other hand might account for as little as 15 per cent of this. Taking the cross section of the palisade trench in this sketch, the volume of chalk not returned to this as packing would be roughly 1,550 cubic feet, almost 75 per cent of the estimated volume of this bank. On the other hand, both the notebook and the subsequent report give much greater dimensions for this trench as a whole (Pitt Rivers, 1898, 65) and on this basis the volume of upcast would easily match the estimated volume of the bank. The amount of apparent turf in the sections might suggest its use as a precaution against the firing of the exposed timbers. If this tenuous interpretation were favoured, however, the spoil from the early ditch would remain to be explained. Here the disconformity in the section along the line of the timber uprights could suggest the tipping of this material into the mortuary enclosure. This reading has already been considered by Piggott (1954, 56). The timbers were sufficiently substantial to support such an arrangement and by this means a revetted rectangular mound about 4·5 ft. (*c* 1·5 m.) in height might have been created.‡ If this were so, the ratio of height to length would be approximately 1:21 compared with 1:18 at Fussell's Lodge (Ashbee, 1966, 32). The excavation photograph originally published by Piggott could be read as showing a revetted mound of this height (1954, pl. IIb). although Pitt Rivers's own published view, while apparently showing a similar break in stratigraphy, might require a higher mound (1898, pl. 255, fig. 1). With all these qualifications it is unlikely that this interpretation, based as it is upon a purely temporary record, can ever be raised above a lawyer's argument.

The second notebook ends with a week's accounts which are quoted in full (p. 50):

* Alternatively this bank may extend to either side of the uprights and occupy the full width of the drawn section. If so the remaining arguments will not apply. I owe this suggestion to Professor Atkinson.

† This calculation makes allowance for the consolidation of the upcast and for the different expansion of chalk and topsoil. These refinements I owe to Professor Atkinson.

‡ These figures refer to the mound before consolidation.

Acknowledgements: I am most grateful to Mr R. N. R. Peers of Dorset County Museum for permission to

HANDLEY HILL CAMP

Pay of 7 men from Thurs. Aug/10/93 to Sat. Aug. 19th inclusive

Riggs	..	£1.	2.	6.
Hayter	..	£1.	2.	6.
Thom	..	£1.	2.	6.
Fry, Geo.	..	£1.	2.	6.
Fry, Jos.	..	£1.	2.	6.
Elliott	..	£1.	2.	6.
New	..	£1.	2.	6.
Yard Broom	..		2.	0.
Beer	..		16.	0.
		£8.	15.	6.

publish this material and to Professor R. J. C. Atkinson, Professor S. Piggott and Mr Paul Ashbee for their valuable comments upon this text.

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RICHARD BRADLEY

The destruction of Acrotiri

PLATE VIII*b*

Readers of ANTIQUITY *have recently had their attention drawn by Mr Gerald Cadogan and his collaborators (1972, 310–13) to the revival of interest in Professor Sp. Marinatos's theory that a volcanic eruption on the island of Thera caused the destruction of a number of Minoan sites in Crete in the 15th century BC, and to the considerable literature that has grown up. Mr James Money has a new theory on the sequence of events which, we think, may prove to be of the greatest importance. Mr Money, Fellow of the Society of Antiquaries of London, is a Government official by profession; he has had a life-long interest in archaeology which, he says, has given him an intimate knowledge of soils in the Kent and Sussex Weald. This, he thinks, may have done much to train his eye to detect the humus layer at Acrotiri. Mr Money writes:*

In May 1972 I went on holiday to the Greek Islands, which included a week on Santorini (Thera).

I was armed with Professor Sir Denys Page's paper, 'The Santorini volcano and the destruction of Minoan Crete'* and, having read it, accepted his findings and conclusions, with one important exception. In order to explain why there are no artifacts later than LM Ia (*c.* 1500 BC) at Acrotiri, while LM Ib remains (*c.* 1450 BC) are found in the Cretan sites, he concludes 'that at least a couple of decades intervened between the pumice-fall which destroyed the Santorini settlements and the ash-fall which rendered the eastern half of Crete temporarily uninhabitable' (op. cit., 44). To reach this conclusion he flies in the face of eminent vulcanologists who state that volcanic eruptions of the Santorini type occur 'in a single phase after a very long period of quiescence' (op. cit., 31).

I was worried by what seemed a weak argument, and before reaching Santorini had half resolved to see if I could find some other more probable solution of the chronological problem. Soon after my arrival I visited the quarries at Phira (op. cit., frontispiece), where there are extensive exposures of the ash/pumice layers ejected by the volcano. It is immediately obvious that the ash (including the 'coloured ribbon' layer) lies immediately over the pumice, with no sign whatever of the pumice having been weathered or of any intervening layer of even minute proportions, such as would have occurred if there had been a substantial gap between the fall of the pumice and the fall of the ash. The same situation, of one layer tightly on another, is apparent in several other exposures which I saw later in other parts of the island.

I visited Acrotiri on three occasions and was

* Society for the Promotion of Hellenic Studies, Supplementary Paper No. 12, 1970.

very hospitably received by Mr Christos Doumas, assistant to Professor Spyridon Marinatos. It was on the second visit, when looking at a section where the tumbled masonry and rubble is overlaid by the usual thick deposit of ash/pumice, that I noticed, between the bottom of the pumice and the top of the masonry/rubble, a thin (2–4 cm.), but distinct, brownish layer of what to the naked eye looked as if it could contain humus. The possibility struck me that this had accumulated after the abandonment of the site, following the very severe earthquake which ruined the town, and that it represented the passing of the few decades between LM Ia and LM Ib. If this was so, there was no reason why the Santorini settlements and the sites of eastern Crete could not all have been overwhelmed at the same time by a single vast explosion.

Mr Doumas, who was interested in my theory, allowed me to take samples of this humus-like layer and another sample from the top of the pumice layer where it meets the ash. He also asked me to sample the base of the pumice layer, where the pieces of pumice are noticeably smaller and smoother than those of the main mass. Finally I took a sample from a much thicker deposit of brown earthy material which in a number of places lies in hollows amidst the masonry debris. This, Mr Doumas said, had been dumped by squatters who re-occupied parts of the town after its destruction by the earthquake and abandonment by the former inhabitants. The relative position of these samples is shown diagramatically (FIG. 1).

Soon after my return to England I contacted Dr I. W. Cornwall, University of London Institute of Archaeology, who kindly agreed to do various tests on the samples, with the following results (quotations are from his report):

Sample 1 (from the top of the pumice at its junction with the ash) was 'clean fresh pumice, showing no detectable amounts of organic matter . . . there was nothing at all to show any interval between the pumice and ash-explosions —no charcoal or signs of chemical weathering.'

Samples 2, 5 and 7 (from the thin (2–4 cm.) brown layer between the pumice and the tumbled masonry/rubble). No. 7 contained 4·4 mgs./gram of alkali soluble humus which 'would seem to represent a buried soil at a surface formerly bearing vegetation'. The sample also contained charcoal and 'three tiny fragments of shell and ribbed cylindrical calcareous structures (? sea-urchin spines) which were clearly organic in origin. This might be taken as evidence of occupation with remains of sea-food, but it should be borne in mind that a submarine eruption might well throw up remains of marine organisms among the pumice.' Nos. 2 and 5 contained 0·1 mgs./gram of alkali soluble humus, i.e. only 1/50 of what was found in No. 7. He comments: 'whether this trace of humus was formed *in situ* or was derived, possibly by rain-washing from some other place, it does denote some interval, if only a short one, between the ruination of the building and the fall of the overlying pumice represented by Nos. 4 and 6, which are themselves humus-free.' Charcoal was present in Nos. 2 and 5, 'which seems likely to denote the action of fire *in situ*, not the carbonization of vegetation by the overlying pumice'.

Sample 3 (from the earthy deposit in amongst the masonry debris) contained 5·6 mgs./gram of alkali soluble humus and was similar to No. 7. Charcoal was plentiful in small grains.

Samples 4 and 6 (from a band about 2·5 cm. thick at the base of the pumice layer). The pieces of pumice in this band are much smaller and smoother than those in the main overlying layer. Dr Cornwall found that both samples were completely humus-free. In No. 4 the pumice lapilli were 'well-rounded and many showing signs of weathering—rusty stains from oxidizing magnetite. No charcoal was seen. This suggests that the fine pumice-fall at the base of the coarse pumice was a separate event and that some weathering took place, if not humus-formation, before the fall of the overlying thick, coarse pumice.' No. 6 was 'a very small sample and perhaps not fully representative of the deposit. It contained a few biggish lapilli (over 2 mm.) which were well rounded, many medium-sand magnetites and a few solid-rock fragments. Most was finer material which passed the sieve. No charcoal was found.'

With regard to phosphates in the samples Dr Cornwall says:

'All the samples were qualitatively tested for the presence of phosphate, traces of which were

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found in all, even the fresh pumices. The putative soils contained no more than the rest, so that the phosphate was probably due to small amounts of apatite present in the basic pumices and ash. It results that none of the samples is likely to represent a human occupation-deposit, but only a natural soil in the cases where there is appreciable humus present.'

Dr Cornwall sums up:

'It seems certain that Samples Nos. 3 and 7 represent soils *in situ*, with Nos. 2 and 5 as very immature examples of similar subaerial weathering and growth of vegetation. Nothing in the examination of the sand-grades contradicts these conclusions. In addition, No. 4 (though not the stratigraphically comparable No. 6) shows some signs of oxidation, which is hard to explain save by at least a brief exposure to air and weather before the fall of the overlying coarse pumice. No. 1 shows no suggestion of such, even short-lived, exposure and from the evidence of these samples one would regard the deposition of the coarse pumice and that of the overlying ash as being immediately sequent, without perceptible interval.'

Dealing finally with the time factors involved Dr Cornwall considers that the tests confirm my 'suspicions of a distinct interval between the destruction of the buildings and their burial by the big pumice explosion'. He thinks that the 'fine pellety pumice (samples 4 and 6) represents a first fairly slight explosion, preceding the big one by an interval long enough for some oxidation of magnetite but too short for the growth of any vegetation, i.e. not more than a year or so'. He hesitates 'to guess at the ages of the other two intervals suggested, but one of "at least a few years" for Nos. 2 and 5 and "probably several decades" for Nos. 3 and 7 would not be too incautious.'

With regard to the differing humus content of No. 7 on the one hand and Nos. 2 and 5 on the other, it should perhaps be noted that the samples were taken quickly and at only three random points. Mr Doumas, however, told me that in the excavations as a whole this humus-like layer had usually been present below the pumice and above the ruins of the town, but that no particular notice had been taken of it.

To conclude, it seems, from the evidence of the samples tested by Dr Cornwall and from what is already known and published about

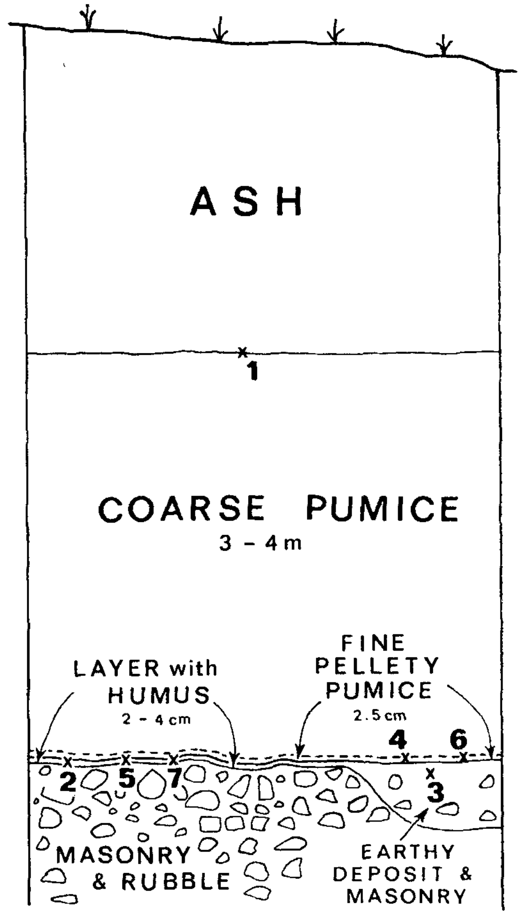

Fig. 1. Acrotiri. Composite section diagram (not drawn to scale), showing stratification and relative position of the seven samples taken

Acrotiri, that the sequence of events was as follows:

1. Destruction of Acrotiri by severe earthquake, followed by its abandonment (LM Ia).
2. Reoccupation of some parts by squatters.
3. The accumulation of a thin layer containing humus over the area and the growth of the sort of vegetation which would flourish on an abandoned site.
4. The burning of vegetation (or other material) *in situ*.
5. The fall, after a substantial interval, of 'fine pellety pumice' ejected by a slight explosion of the volcano.

6. The main explosion, which ejected vast quantities of coarse pumice, followed immediately by ash; this totally overwhelmed Thera and simultaneously caused temporary desolation in eastern Crete (LM Ib).

Throughout this enquiry I have exchanged letters with Professor Page, who considers my solution of the chronological problem 'wholly convincing' and has revised his earlier conclusion. It is at his suggestion and with Dr Cornwall's approval that I have written this brief account, which both have read. It has also been seen by Professor Marinatos and Mr Doumas; I am greatly indebted to the latter for his co-operation during my visit to the island. I am glad to be able to make this contribution to an understanding of the marvellous voyage of discovery on which Professor Marinatos has embarked; he first wrote about the subject in ANTIQUITY in 1939 (XIII, 425–39).

An Ogam inscription near Blackwaterfoot

PLATE IX

A couple of years ago Dr Kenneth Steer, of the Royal Commission on the Ancient and Historical Monuments of Scotland, told me of the existence of an Ogam inscription in the cave known as King's Cave on the west coast of Arran, some 3 km. north of Blackwaterfoot. We visited the cave in September 1971 and examined the inscription carefully, measuring and drawing it, and the Commission's principal photographer, Mr G. B. Quick, made an excellent photograph of it (PL. IX).

The walls of this cave are decorated with a number of incised carvings dating from Early Christian to modern times, including an early form of cross and other symbols, human and animal figures, and many graffiti. The cave and its carvings are discussed and illustrated in J. A. Balfour, *The Book of Arran* (Glasgow, 1910), 213–18.

On the left-hand wall, about nine metres from the entrance gate, the inscription is to be seen some two metres above the floor. The base-line runs straight downwards, and appears to be defined by two cross-strokes towards each end of the line and about 50 cm. apart. The ogam letter-strokes are between 3 and 4 cm. in total length, and are mostly perfectly clear.

Starting in the usual way from the bottom, I see no letters after the lower of the two cross-strokes just mentioned for the distance of some 26 cm. There is then a group of four strokes crossing the base-line roughly at right-angles, of which the part of the first stroke below (i.e. to the right of) the base-line is very faint. This should be E. After a short gap there follow two further strokes, rather wider apart than the preceding ones, which however give the impression of a hollow, since the stone has flaked away between them. This looks therefore like an O. Next, a long diagonal cross-stroke will be M; the short line below (i.e. to the right of) the base-line joining it to the M close to the point of intersection can scarcely be a letter. Next come four strokes crossing the base-line; the half of the first below (i.e. to the right of) that line is faint but certain; giving E. After this come five strokes above (i.e. to the left of) the base-line only; the space between the second and third is somewhat wider than between the others, so that it is uncertain whether it is two letters or one. The former is perhaps more probable, and if so it is DT; if not, Q. The last is a group of four strokes crossing the base-line at right angles, the first not quite so long as the others at the top (i.e. the left-hand side); this is E. After that I see no further strokes until what appears to be a closing cross-stroke after about 8·5 cm.

This appears to give us therefore the reading EOMEDTE or EOMEQE; or in the unlikely event that we should read the inscription the other way up, EVLEMOE or ENEMOE. None of these seems at first sight to make any sense in Irish, and we should have to fall back on the convenient explanation that it is 'Pictish'. In the reading EOMEQE, however, the *meq* is striking, since it recalls the MEQQ in the Bressay and St Ninian's Isle ogams in Shetland, which appears to stand for the Old Irish *maicc,* genitive of *macc,* 'son'.* If so, the son's name should precede and the father's name follow,

* On this see the writer in F. Wainwright (ed.), *The Problem of the Picts* (Nelson, 1955), 140.

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the second certainly and the first quite probably in the genitive; but in spite of the fact that MEQ must be Irish, the names need not be so, but could well be Pictish.† However, Irish is more probable in Arran; and it may well be significant that apart from long cross-strokes instead of nicks for the vowels, the inscription lacks any typically Pictish features such as the frequent opposing diagonal slope in the H-series versus the B-series of consonants, the not uncommon line linking the ends of all strokes ('bind-Ogams'), the appearance of *forfeda*, or the use of the 'feather symbol' at the beginnings and ends. However this may be, it is clear that EO and E cannot of themselves be names, and if we accept the reading MEQ = 'of the son of', it must be that some letters have become lost at the beginning and end. There is plenty of room at the beginning and room for three or four at the end, and the condition of the stone does not wholly rule this out, particularly at the beginning (the isolated cross-stroke at the end could of course well be a letter or part of one, and not a closing substitute for the feather-symbol). If the inscription is late enough, say 8th-century, the syllable *-eo* could in fact be the genitive termination of an Old Irish *i*-stem, a form such as *Fedlimtheo*, genitive of the name *Fedlimid*.

The photograph shows what appears to be another Ogam inscription parallel with and close to the first, to its right, but this must be the work of a recent vandal, since it is not cut into the rock but seems to be scribed in pencil or some kind of ink.

I wish to thank Dr Kenneth Steer for his kindness in arranging the visit to Arran and his generous help in measuring and discussing the inscription, and otherwise; and also for permission to reproduce the photograph. (Responsibility for the notes on the reading above is wholly mine.) KENNETH JACKSON

† See op. cit., 141.

The New Zealand Radiocarbon Conference

The 8th International Conference on Radiocarbon Dating, organized this time by the Royal Society of New Zealand, was held at Lower Hutt City, Wellington from 18–25 October 1972. There were about 120 participants drawn from the fields of physics, chemistry, geology, geochemistry, soil science, archaeology and various other disciplines. Not surprisingly, a relatively large proportion (about 25 per cent) of the participants were from New Zealand but some 20 countries were represented in all with some notable absences, in particular China, Russia and East European countries. A message of goodwill for the success of the Conference was received from the Russian laboratories, at the same time expressing a willingness to take part in inter-laboratory calibration work and hopes of attending future radiocarbon conferences. About 40 radiocarbon laboratories, or approximately half the total number of active laboratories listed in *Radiocarbon*, were individually represented and included 5 laboratories from the UK. In 1973 New Zealand is also the venue for the next INQUA meeting and this consideration may have kept some potential participants away from the Radiocarbon Conference, since funds for a major journey of this kind are unlikely to be obtainable in two consecutive years.

The Conference had been well publicized among dating laboratories for some two years in advance and was very well planned by a committee led by Dr T. A. Rafter, Director of the Institute of Nuclear Sciences, DSIR, Lower Hutt who was also the Conference Chairman and whose own very successful radiocarbon laboratory, established some twenty years ago, was among the earliest. Professor W. F. Libby, originator of the radiocarbon dating method, was Honorary President of the Conference.

The aim of the Conference was to bring together a significant proportion of scientists actively engaged in operating radiocarbon dating laboratories, primarily to discuss technical and methodological aspects of radiocarbon dating, the procedures necessary to obtain accurate results and factors affecting the interpretation of the results. This is reflected in the list of topics comprising the 8 separate sessions. These were devoted to (A) secular

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variations of carbon-14, (B) radiocarbon dating techniques, (C) carbon-14 variations in the ocean, (D) radiocarbon in freshwater, (E) radiocarbon in soil development, (F) the use of radiocarbon, (G) sample contamination, and (H) reference standards. In addition the official programme contained visits to scientific institutions and two evening public lectures, the first a survey of radiocarbon dating by Professor Libby and the second a review by Professor N. W. G. Macintosh, of Sydney University, of the evidence for the date of arrival of early man in Australia.

Sixty or so papers were presented and abstracts of these, and paper-bound pre-prints of the two-volume *Proceedings* containing the papers in full were distributed before the Conference. This was achieved very cheaply and effectively, with comparatively few omissions or errors, by photo-reduction of authors' typescripts which had been prepared in a standard way for this purpose, the individual authors being made responsible for editing their typescripts. The corrected final version of the *Proceedings* (Rafter and Grant-Taylor, 1972) is now available at a cost of about £4.00 in contrast with the price of £21.25 for the *Proceedings of the 12th Nobel Symposium* (Olsson, 1970), the previous radiocarbon conference held in Uppsala in 1969.

It can be seen immediately from the list above, as indeed might be expected from the aims of the Conference stated earlier, that most of the sessions were devoted very largely to matters chiefly of concern to laboratories. However, the problem of carbon-14 variations is of direct interest to archaeologists because of the desirability in the long term of establishing a calibration curve (or tables) for conversion of radiocarbon dates to calendar years.

Briefly, the main progress in this area has been along four separate lines. The bristlecone pine chronology has been extended from about 7100 to nearly 8200 years BP so that more early material should become available for radiocarbon analysis and related studies. Calibration tables have been derived from the combined results of the 4 laboratories (Arizona, La Jolla, Pennsylvania and Yale) which have made the largest number of continuous measurements of precisely dated dendrochronological series from the early millennia BC. These laboratories' results when combined together appear to show partial synchronism with some of the more pronounced 'wiggles' in the curve published by Suess (1970). Further theoretical work has been done which seeks to explain the causes of carbon-14 variations in terms of interacting heliomagnetic, geomagnetic and climatic models and the possible pattern of carbon-14 variation for the millennia beyond the present limit of dendrochronological material has been predicted. The occurrence of short-term fluctuations, possibly linked with sunspot cycles, has been suggested. If these are real, an additional error up to ± 120 years may be introduced when short-lived material is dated.

However, at the business meeting at the end of the final session of the Conference it was agreed that no particular calibration curve or table should be preferentially adopted at present and that the use of the conventional 5570 year 'Libby' half-life should also continue as affirmed at previous conferences.

Clearly much work remains to be done and the establishment of a definitive calibration relationship is not yet practical. One may hope that there will be some chance of achieving this at the next international radiocarbon conference which it was planned at this meeting should be held in Philadelphia in 1976 at the invitation of Elizabeth Ralph. Meanwhile the advice given for presentation of radiocarbon dates must remain exactly that contained in the 5 points in the Editorial of the last number of ANTIQUITY (XLVI, 1972, 265).

Lastly, because of its general importance, mention must be made of a recommendation by Professor Libby that radiocarbon dating methods could be applied directly to measure the gradual increase in atmospheric carbon dioxide. The existing international network of laboratories provides a unique opportunity to study this important environmental problem and the effects it may have.

Although at this Conference no new decisions were taken that profoundly affect the use of radiocarbon dates by archaeologists at present,

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many other important topics not dealt with in detail here were discussed. The great value of meetings of this kind in maintaining essential international contact among laboratories and individual workers in the radiocarbon field is unquestionable, and much credit is due to Dr Rafter and his colleagues for their faultless organization of a most successful conference, which undoubtedly will have contributed towards a better understanding of some of the present enigmas.

This account would not be complete without a brief mention of the highly successful pre- and post-conference tours made by some of the overseas visitors, led by Tom Grant-Taylor through the North Island and Ian and Mary McKellar through the South Island and each lasting several days. Basically these were specially organized scientific field trips, with a strong geological bias, which also provided an opportunity to see something of the tremendous diversity of New Zealand. A considerable number of sites dated by radiocarbon were visited, ranging from geologically recent volcanic ash-fall sequences in the North Island to features associated with previous glaciations in the South Island. RICHARD BURLEIGH

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Cotton Fellowships

We should like to announce the creation of a new foundation which should prove of great interest to archaeologists, among others. The Dr M. Aylwin Cotton Foundation has been designed, first to provide a research year for established or senior scholars whose interests are centred upon subjects in the Mediterranean area, and secondly to supply a substantial sum towards the publication costs of any work in the same area of study.

The first Cotton Fellow appointed under this foundation is to be Dr Frank B. Sear. He is undertaking research in the field of Hellenistic and Roman architecture. His special interest is in the peristyle buildings of Cyrenaica, public and private, and he plans to investigate their stylistic connexions and origins in the centres of the Aegean in the last two centuries BC.

The first publication grant has been awarded to Dr David Whitehouse, who is engaged in pioneer studies of medieval pottery in central and southern Italy. His present excavations have been undertaken in Tuscania, a town north of Rome, where the monumental centre was entirely evacuated after the disastrous earthquake in 1971. These excavations, sponsored by The British School at Rome in collaboration with the Superintendency of Antiquities of Southern Etruria, have attracted the attention of a BBC Television team and their 'cover' has been included in a programme on British excavations abroad.

Further information can be obtained, by those interested, by writing to Mrs M. A. Cotton, Honorary Secretary to the Foundation's Trustees (*Albany Trustee Company Ltd., Guernsey*), *at 2, Piazza Giunone Regina, Aventino, 00153 Rome.*

Longhouse and roundhouse at Crickley Hill

PLATE X

The excavations of 1969–71 at Crickley Hill have already been described (Dixon, 1972). During the 1972 season work was continued on the Neolithic causewayed enclosure and on an area of almost 2,000 sq. m. in the interior behind the entrance to the hillfort. Within this latter area were found traces of at least two phases of occupation.

In the first phase a series of rectangular buildings, their axes running E and W, extended across the site; the postholes for these longhouses were up to 90 cm. in diameter and

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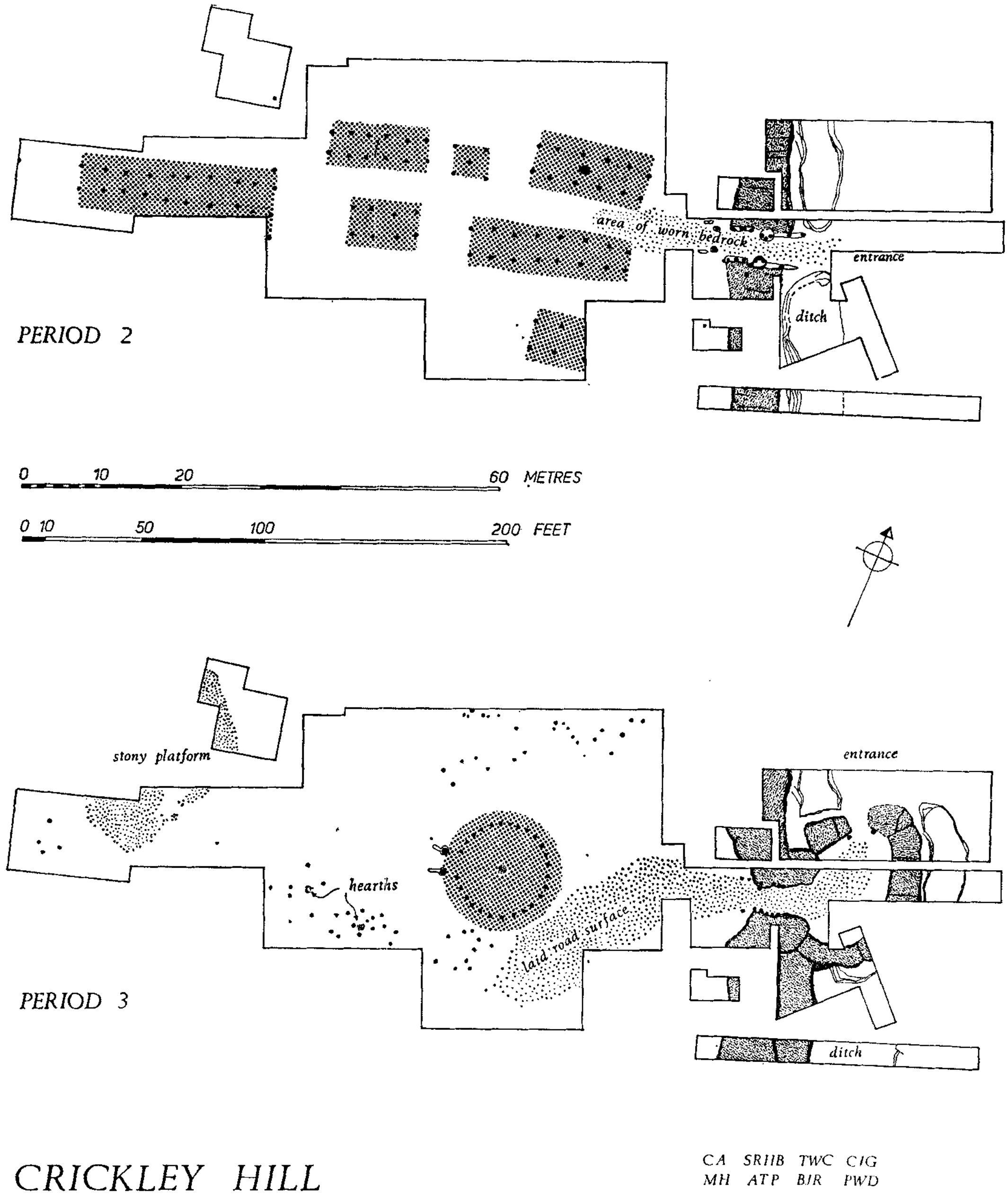

Fig. 1

averaged 60 cm. in depth: to judge from surviving packing stones the scantling of the posts themselves was about 40 cm. Each bay of each building was a square with sides about 2·9 m. between centres. The buildings themselves varied in size from a three-bay house 8·5 m. long to a nine-bay house 24·6 m. in length. Erosion from the top of the hill had removed the floors, but one house preserved in its centre a heavy patch of burning, presumably the base of a hearth, and another, built across the Neolithic ditches to the W of the area shown in the plan (FIG. 1), had a well-preserved central hearth; other houses may have been similarly heated: failure of hearths to survive need not imply that the other houses were used for storage; equally it does not appear possible to say what proportion, if any, of the houses formed byres, and the term 'longhouse' must be taken to refer to length rather than function.

The existence of these hearths makes implausible any suggestion that the postholes belong to rows of contiguous four- or six-post structures, and, while it cannot be proved that all the longhouses were in use at the same period, contemporaneity is strongly suggested by the very regular layout of the settlement. A roadway, hollowed in the bedrock by traffic, ran westwards from the entrance; where it passed between the two easternmost of the longhouses it narrowed to a width of about 3 m., coming no closer than 1·5 m. to the lines of postholes. It therefore seems likely that the post setting formed internal supports for the houses, perhaps made rigid by purlins and by tie beams, across which the roof trusses were laid; the outer walls of the houses would then be simple screens, braced between the eaves and the ground. The structures should therefore be reconstructed as aisled halls, from 7 m. to 7·5 m. in width, and the approximate area of their floor space is shown on the plan by stippling (FIG. 1).

Charcoal and traces of burning indicated that the settlement was destroyed by fire. In the rebuilding a large roundhouse was built above the easternmost longhouses, its postholes cutting through a packed layer of small stones which sealed the longhouse postholes. The post circle of the roundhouse was 11·4 m. in diameter: no outer ring survived, but on the N a slight dip in the bedrock ran from 1·7 m. to 2·4 m. outside the circle, a shelf caused by traffic wear presumably beyond the wall of the house. To the E and S of the post circle the shelf was continued by a long burnt streak and by clusters of burnt daub. It thus appears that the exterior wall of the roundhouse, like those of the longhouses, was a wattled screen on sleeper beams; the overall diameter would then be about 14·8 m. (over 48 feet). On the western side of the house the porch was formed by two large postholes which lay 2·2 m. beyond the post circle, and thus on the line of the exterior wall. In the centre of the house lay a hearth; small postholes may have formed partitions, but no internal supports for the roof were identified. Like the longhouses the roundhouse was finally destroyed by fire.

The previous excavations of the entrance area have revealed three phases of construction: in the first, Period 2, the rampart was timber-laced, and the entrance was an inturned passage closed by two pairs of gates. The centre of this passage had been worn by traffic to a depth of about 15 cm., and this hollowed way continued between the first pair of longhouses; the longhouse settlement must thus be associated with the Period 2 rampart. After the burning at the end of Period 2 the face of the rampart was rebuilt (Period 3a) and subsequently (Period 3b) the entrance area was massively refortified with solid bastions and an out-turned hornwork and outer gate (Dixon, 1972, fig. 1). The cobbled road then laid through the entrance was traced running into the interior, sealing two of the longhouses, and curving southwards to avoid the roundhouse. The destruction of the entrance and abandonment of the fort at the end of Period 3b should therefore be linked with the burning of the roundhouse. Two well-preserved hearths in the centre of posthole clusters to the SW of the excavated area probably belonged to huts associated with the roundhouse: from the intensity of the burning around one hearth some sort of industrial activity may be suspected. Other small posthole groups may be identified as sheds and similar slight structures. All are shown on the Period 3 plan, but for most the period attribution is quite uncertain.

Decorated sherds from the roundhouse postholes closely matched the finds from the Period 3b entrance, including white infilled pottery with incised chevrons, comparable to the very early Iron Age material in the Upper Thames and Wessex. The longhouse material, on the other hand, was of coarse thick fabric without decoration. The break in the ceramic tradition appears to come between Period 2 and Period 3a, a time of change from longhouse to roundhouse. Large roundhouses are found elsewhere in Early Iron Age contexts (Hawkes, 1958; Harding and Blake, 1963; cf. Harding 1972, 22 f.), and small square or rectangular houses are being identified in increasing numbers (for a summary see Stanford, 1970). A parallel for the Crickley longhouses may be sought in the house, perhaps a ten-poster, perhaps two contiguous smaller buildings, at Park Brow, Sussex (Wolseley *et al.*, 1925, 32) or a possible floor 50 by 16 ft. (15 by 4·8 m.) in extent at Langford Down, Oxfordshire (discussed by Harding 1972, 34–5), but for *comparanda* for extensive and regularly planned groups of longhouses one must still turn directly to the Bronze Age and Iron Age settlements of continental Europe.

PHILIP DIXON

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Fossil shell observed by Acheulian man

PLATE XI*a*

It is only through studying all aspects of their culture that we can build up any concept of the mentality of our palaeolithic forerunners. I suggest it is notable that the Acheulian hand-axe people in Europe are the earliest men known to have paid attention to fossils when they encountered them. These tool-makers belonged to the primitive subspecies of *Homo sapiens* represented by the Swanscombe and Steinheim skulls.

A few years ago Mr K. D. McRae of Oxford noticed a conspicuous fossil shell in a flint hand-axe in the collections of the University Museum of Archaeology and Ethnology in Cambridge. Later it occurred to him that this might interest me in connexion with a survey I am making of the decorative uses of fossils from palaeolithic to modern times, so he asked the Curator, Mr P. W. Gathercole, if he would look for the specimen. Early in 1972 Mr Gathercole reported that he had found the hand-axe in question, and on my next visit to Cambridge he made it available for me to study.

The hand-axe (PL. XI*a*) was collected in 1911 at West Tofts, Norfolk, a locality lying 8 km. north east of Brandon. It is a biface of bluntly pointed linguate form, 135 mm. long, with width of 78 mm. and thickness of 35 mm. The flaked surfaces are patinated blue-grey with touches of rusty staining along ridges which form an inverted Y above the fossil shell. A large portion of the butt of the hand-axe retains the original cortex or weathered surface of the flint. The style of flaking is typical of the 'developed' Acheulian.

The fossil shell is situated at the middle of one face of the hand-axe, near the edge of the unflaked cortex. The fossil is an example of the Cretaceous bivalve mollusc *Spondylus spinosus* (J. Sowerby). Each valve in this species is characterized by fine ridges radiating from the umbo (apex). The ridges originally had spines, but these have commonly been broken off, and then traces of them may be, as in this case, almost imperceptible. On the hand-axe only one valve of this fossil shell is seen, the other lies embedded in the flint. The umbo of the exposed valve is slightly damaged. Originally this valve measured *c.* 24 mm. in length from the apex to the slightly frilled margin; and in width it measures 22 mm.

The most significant fact which emerges from examination of the hand-axe is that the fossil shell distinguishing it is on a weathered portion of the block of flint out of which the implement was fashioned. We must infer that the Acheulian tool-maker not only saw the fossil when he selected the block of flint, but that as he worked

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this into a bifacial hand-axe he deliberately avoided flaking the area containing the fossil and left it occupying a central position.

It is, I believe, no more than coincidence that recent shells of the same genus (*Spondylus*) were extensively used for decoration by the Neolithic inhabitants of the Aegean region.

I suggest that it was the fine fan-shaped markings of the Cretaceous fossil shell which appealed to the mind of the maker of the West Tofts hand-axe, about a quarter of a million years ago. Three other examples of fossils worked as artifacts by Acheulian people in Europe are on record.* In all these specimens a conspicuous feature of the fossil structure is the radiation of lines from one or more centres.

I am much indebted to Mr K. D. McRae for calling my attention to this implement which is illustrated here by courtesy of the Curator of the University Museum of Archaeology and Ethnology, Cambridge. I wish to record my thanks to Miss Mary D. Cra'ster for checking some of the details while this Note was in preparation. KENNETH P. OAKLEY

* K. P. Oakley, 1971. Fossils collected by the earlier palaeolithic men, in *Mélanges de préhistoire, d'archéocivilisation et d'ethnologie offerts à André Varagnac* (Paris), 581–4.

The Kalnes Bronze Age boat

Dr John Coles of the Department of Archaeology in the University of Cambridge wrote the following comment on Paul Johnstone's article 'Bronze Age sea trial' ('Antiquity', 1972, 269–74):

I was much interested to read of Paul Johnstone's Bronze Age boat, and agree with many of the things he says about its manufacture, its ease in propelling, and its resemblance to Bronze Age rock art of the north. I wonder, however, if he has really managed to prove those points made by him in the paper?

Seaworthiness was not tested in terms of lengthy voyages, in the ability of the crew to avoid swamping, and in the capacity of the boat to carry anything but sacks of sand, stones or grain. How would the skins have stood up to prolonged seawater and pounding of seas? How would the relatively low freeboard have resisted spray and waves? How would the skin and wood of the boat have reacted to the uneasy shuffling or rolling of cattle being pitched about, whether tied or not? Would any such boat have made headway in rough adverse seas and winds?

I ask these questions from the point of view of great interest in ancient transport and in archaeological experiments in general, and I do not intend in any way to detract from the work done and some of its conclusions.

Paul Johnstone replies:

Dr Coles points accurately to two drawbacks in our experiments. Because it was not feasible for us to remove the hair from the cowskins, we did not do any waterlogging tests. We knew from umiak experience and Irish Dark Age literature that the problem of keeping hides water-resistant for considerable periods was solved, but there are no exact data and tests could and should be done to establish how long various types of treatment to skins are effective.

I wanted to take our Kalnes replica on a voyage of some length, say from Frederikstadt to Oslo, but unfortunately the Norwegian TV budgets are so low that we could not afford the necessary escort craft, etc.

I do not see cargo as any great problem. Good, unshiftable stowage is the same in any sort of craft. The only additional problem would be the risk with hooves and horns if animals are carried in a skin boat. I have seen in the West of Ireland two cattle or several sheep being taken out to an offshore island in a curragh. The farmer simply tied the beasts' feet, and dumped them on their backs in the craft: on arrival they trotted off apparently none the worse. On a long emigratory voyage it might be much more of a problem. Putting the beasts in sacks tied round their necks has been one suggestion. Thirst seems to me a likely problem but possibly some primitive people have found a simple practical answer. Anyway Humphrey Case has gone into this in far more detail than I have (*Antiquity*, 1969, 176–86).

Adverse weather is an interesting point: one

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of the characteristics of skin or hide vessels is that it is virtually impossible to row them into a strong head wind. They float so high and are so wind borne that they are blown back almost as much between strokes as they are propelled forward by the thrust of the oars. This has interesting and varied implications too lengthy to go into here, but I have considered them in some detail in my forthcoming *Sea-craft of prehistory* (London, Allen Lane, autumn, 1973). It does not, anyway, affect the sea-keeping capacity of these craft, except on rare occasions on a lee shore.

Freeboard is not much of a problem either. If Dr Coles has seen either the film *Man of Arran*, or our BBC film, I think he would agree that the buoyancy and dryness, so far as spray is concerned, of skin boats are among their outstanding qualities. Seaworthiness must to some extent be a question of comparison. I have had a varied experience of small boats and a number of curragh trips, including a 45-mile (72-km) row across the Irish Sea in one—an episode that ended in a Force 6 Gale. I have quite often felt quite alarmed, but never in a curragh. I think that if our Kalnes replica had had a little more flare to its sides one would have had as much confidence in it. This is purely a subjective impression but I pass it on for what it is worth. In any case our Kalnes craft is sitting in the Sarpsborg County Museum, and I am sure that the Curator, Professor Marstrander and myself would be delighted if anyone would organize some more trials.

I did try to persuade the Curator to let us see how long it would stay afloat on the museum's fine lake, and measure the water intake, but alas, he declined. He said that, in the absence of a museum night-guardian, vandals would sink it long before waterlogging did.

Radiocarbon dates for Northton, Outer Hebrides

Three radiocarbon dates have been obtained from the Neolithic and Beaker settlement site at Northton in South Harris, Outer Hebrides, briefly reported on in ANTIQUITY some years ago (Simpson, 1966). The measurements were made by the British Museum Research Laboratory on the protein (collagen) fraction of animal bone from the three main occupation horizons:

Beaker II	1531 ± 54 bc (BM-707)
Beaker I	1654 ± 70 bc (BM-706)
Neolithic II	2461 ± 79 bc (BM-705)

The occupation horizons were separated from each other by layers of virtually sterile wind-blown sand (FIG. 1). The pottery from the Neolithic II horizon comprised regional forms of Western Neolithic wares similar to those from sites on the adjacent island of North Uist; there were also sherds of a number of Unstan Bowls. (A few sherds were recovered from the Neolithic I horizon, and these were not dissimilar to those from the Neolithic II.) The Beaker I horizon was the main occupation level of this period, and where classifiable the pottery appears to belong to Clarke's Northern British series. There were two stone house structures associated with this phase (Simpson, 1971). Similar pottery styles were present in the Beaker II horizon, but there was less material.

In the Neolithic II levels the contemporaneity of Unstan bowls and the local multiple carinated Hebridean wares with incised decoration had already been demonstrated. The mid-third-millennium date for this horizon suggests a considerably earlier date than had hitherto been suspected (Henshall, 1972, 178) and would add support to the argument that this style first developed in north-west Scotland, later to be transmitted to the Orkneys (McInnes, 1969, 22). The form and decoration of these bowls has been compared with vessels in the Fengate style but the Northton date suggests that the northern forms developed several centuries before the appearance of Fengate wares in the south, and must represent a quite separate tradition developing on parallel but independent lines.

No radiocarbon dates exist for North British Beakers but Clarke tentatively dates these vessels to a period which would equate quite happily with the Northton determinations, and in the old style chronology he would see them as

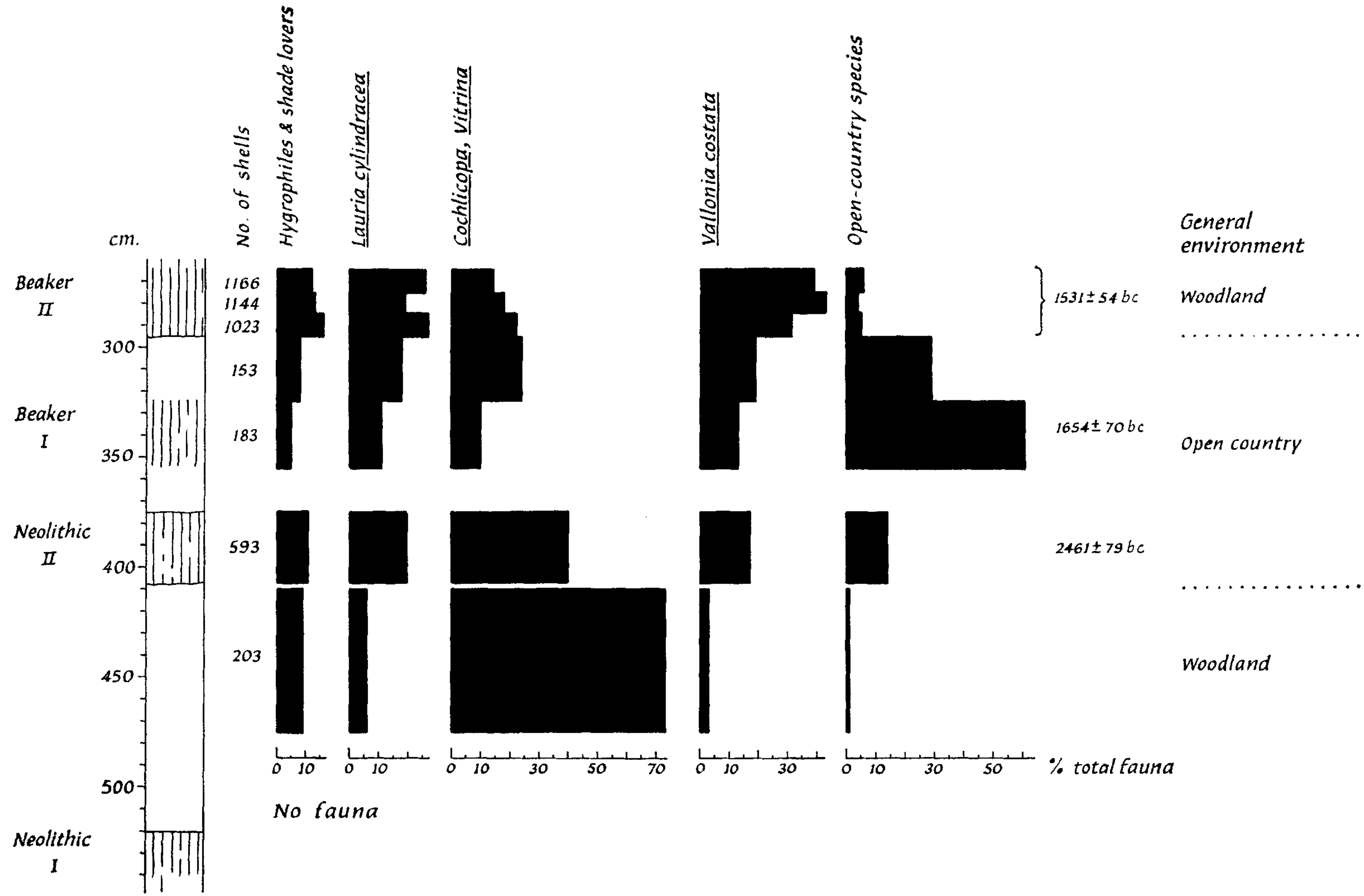

Fig. 1. Land snail diagram from the Neolithic and Beaker occupation horizons at Northton

broadly contemporary with the Wessex Culture. Two finds from the Beaker levels at Northton would support this view. From Beaker I came a small bone disc decorated with lozenge and chevron patterns produced in a *kerbschnitt* technique. Similar designs occur on cylindrical bone beads accompanying a Yorkshire Vase Food Vessel from Folkton LXXI (Simpson, 1968, 199, fig. 45, 5) and discs found with a cremation and a small bronze knife dagger in Hoare's barrow 20 at Lake, Wiltshire (Hoare, 1810, 212, pl. xxxi). In Beaker II the primary female inhumation in a corbelled cist was accompanied by a highly polished bone pin with an expanded perforated head which finds close parallels in the Wessex Culture (e.g. Annable and Simpson, 1964, 110, fig. 359) and are generally considered to be copies of Únetiče metal pins. On the other hand, three recent dates for Wessex material (*Current Archaeology*, No. 32, 1972, 241) would suggest a rather later date for some of the grave groups.

The environmental sequence on this site, as deduced from the evidence of land snails, has already been published in detail (Evans, 1971), and the part which is relevant to the Neolithic and Beaker horizons is reproduced here (FIG. 1). Essentially, there was an episode of deforestation which we can now say took place shortly before 2461 ± 79 bc. This was followed by a period of about 800 years during which the landscape remained open. By 1531 ± 54 bc the site had once more become wooded. It is not certain whether deforestation was an entirely artificial process brought about by Neolithic man, or a natural one caused by the overwhelming action of wind-blown sand, but there can be no doubt that it was closely associated in time with man's presence on the island.

Leaving aside this problem, there are two points worthy of note. In the first place, the phase of open country, however brought about, was not in the nature of a short temporary clearance of the kind described by Turner (1965) but was of long duration. In this respect it is similar to the situation at several sites in southern England (Evans, 1971), as for example at Durrington Walls (Wainwright and Longworth, 1971, 329) where woodland clearance took place in the middle of the 3rd millennium and was followed by a period of about 500 years when an open grassland environment, probably maintained by sheep grazing, obtained.

The second point of interest concerns the phase of woodland regeneration. At several sites in southern England, which comprise long barrows and causewayed enclosures, snail analysis of ditch sediments has demonstrated an episode of woodland or scrub regeneration, often associated with late Neolithic or Beaker occupation horizons: e.g. South Street (Evans, 1968), Wayland's Smithy and Nutbane (M. P. Kerney, pers. comm.), Horslip (Connah and McMillan, 1964), Thickthorn Down (Drew and Piggott, 1936), Knap Hill (Connah, 1965, 19) and Windmill Hill (Smith, 1965). In these cases it is sometimes held that regeneration is a local phenomenon engendered by the favourable environment of the ditch and confined to it. But at Northton there can be no question of this, for the shells in the Beaker II horizon are probably culled from a wide area by wind action, and the virtual absence of open-country species is thus good evidence for widespread woodland regeneration on this site in the middle of the 2nd millennium BC.

RICHARD BURLEIGH, J. G. EVANS and D. D. A. SIMPSON

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Early Bronze Age faience beads from Central Europe

With the friendly cooperation of several museum directors in Czechoslovakia and Hungary, a number of faience or glass paste beads from Early Bronze Age contexts were collected for analysis by the thermal neutron activation method. This technique, which is non-destructive, has been described in detail elsewhere (Aspinall *et al.*, 1972). The analyses give information on sixteen chemical elements, of which seven: copper, arsenic, antimony, silver, gold, tin and cobalt, are used for discussion here. With the possible exception of cobalt, the last six are usually associated with the first, copper, which is normally responsible for the blue or blue-green colour of faience.

The article mentioned above included re-analyses of two Czechoslovakian beads, originally discussed by Stone and Thomas (1956, sample numbers 55 and 56), the very high antimony and cobalt contents of which suggested the possibility of local manufacture. The same conclusion was reached on typological grounds by Harding (1971), who also discussed the archaeological associations of the following beads which have now been analysed. To add to the one bead from Moravia (Ostrožská Nová Ves) we now have samples from four more: Kyjov, Holešov, Jiříkovice and Němčice nad Hanou. We also have samples from two sites in Slovakia—Branč and Košice—and from three sites in Hungary—Szöreg, Pitváros and Dunaujváros (formerly known as Sztalinváros or Dunapentele). This represents a total of at least eleven finds from nine sites, with thirty-one beads or fragments of beads having been analysed. Broadly speaking, the Moravian finds are from Únětice cemeteries, the Slovakian (with Holešov) from 'Nitra group' graves (including its eastern relative, the Košt'any group), and the Hungarian from early Pecica or Nagyrév cemeteries. Most of this material should date to Br A1, though some at least of the Moravian finds seem to be from 'classical' Únětice contexts.

The most significant features of the Czech bead analyses are high antimony and cobalt for Kyjov and Jiříkovice (cf. Ostrožská Nová Ves), and high gold for Branč. With the help of a multiple regression analysis (Newton and Spurrell, 1967) it is possible to distinguish between the beads from the six Czechoslovakian sites on the basis of their elemental composition. The level of discrimination is increased if the beads from Kyjov and Jiříkovice are assumed to have a common origin. This is illustrated (FIG. 1) where the antimony and cobalt contents are expressed as parts per million of the whole beads. There is a high degree of correlation between these two elements and they are therefore shown combined. It may also be noted that the Stone and Thomas sample number 56 from Polepy in Bohemia has a similar composition to those from Němčice.

The most significant features of the Hungarian beads are the association of copper with gold, arsenic, antimony and tin and the good discrimination among the three sites on the basis of the gold and tin content. The group taken as a whole cannot be distinguished from the Czechoslovakian group. However, when both groups are taken together and compared with the published analyses of British and Egyptian beads (Table 1), clear differences emerge; silver and antimony are relatively high

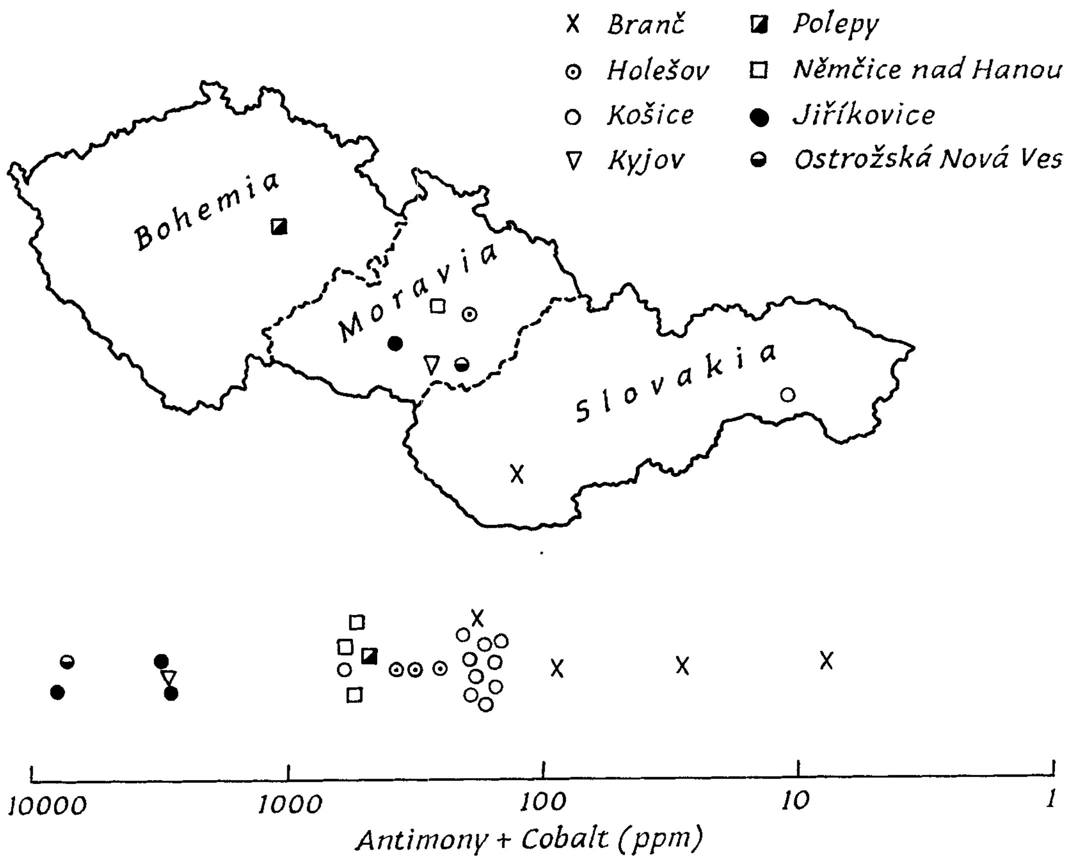

Fig. 1. The combined cobalt and antimony content of the Czechoslovakian beads in parts per million, and the location of the sites from which they came

Table 1. Geometric mean values of concentration (parts per million)

Geometric mean values are quoted in preference to arithmetic means since the logarithms of the concentrations are used in multiple regression analysis. (*Ref. Aspinall *et al.*, 1972)

ELEMENT	SOURCE		
	Britain*	Egypt*	Central Europe
Copper	9100	11200	10000
Silver	1·9	0·4	26
Antimony	5	5	190
Cobalt	1·5	2·6	3·8
Tin	5000	350	250
Arsenic	35	30	40
Gold	0·01	0·07	0·05

for the Central European beads and tin for the British beads. This finding lends support to the suggestion of Aspinall *et al.* (1972) that the Central European beads are of a different origin from the rest.

Further, within the analyses of the Czechoslovakian beads, there is tentative evidence for localized site groupings, as follows:

(*a*) Kyjov, Jiříkovice, Ostrožská Nová Ves
(*b*) Němčice nad Hanou, Polepy
(*c*) Holešov
(*d*) Branč
(*e*) Košice

Considered from an archaeological point of view, these results are quite acceptable, and in

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fact bear out the conclusions based on typological and other considerations reached by Harding (1971) who stressed the regional differences in types. Most of the beads analysed were of the simple annular shape, though there were three four-pointed beads from Hungary, and the Košice beads are short cylinders. The obvious conclusion is that faience or glass paste was produced in a number of different places, where not only the raw materials but also the techniques used and shapes created differed to a more or less marked degree. Both the analyses, which group together three Moravian sites not far distant from one another, and consideration of the local bead-types (in Moravia annular and segmented) support the idea that the beads came from a few local workshops, and were then distributed round the adjacent countryside. More samples of beads will be required to improve the statistical picture and to determine the extent of the distribution; for instance, in the case of the four-pointed beads, which seem typically Hungarian, samples will be needed from south-west Slovakia and from north-east Jugoslavia, where they also occur. We address these last remarks to Museum Directors in those areas, in the hope that they will provide additional samples for us to continue these investigations. In the meantime, we register a plea that Central European scholars publishing finds of faience will cease to describe them as imports from Egypt or the Near East unless they find new and compelling evidence for doing so.

ANTHONY HARDING AND STANLEY E. WARREN

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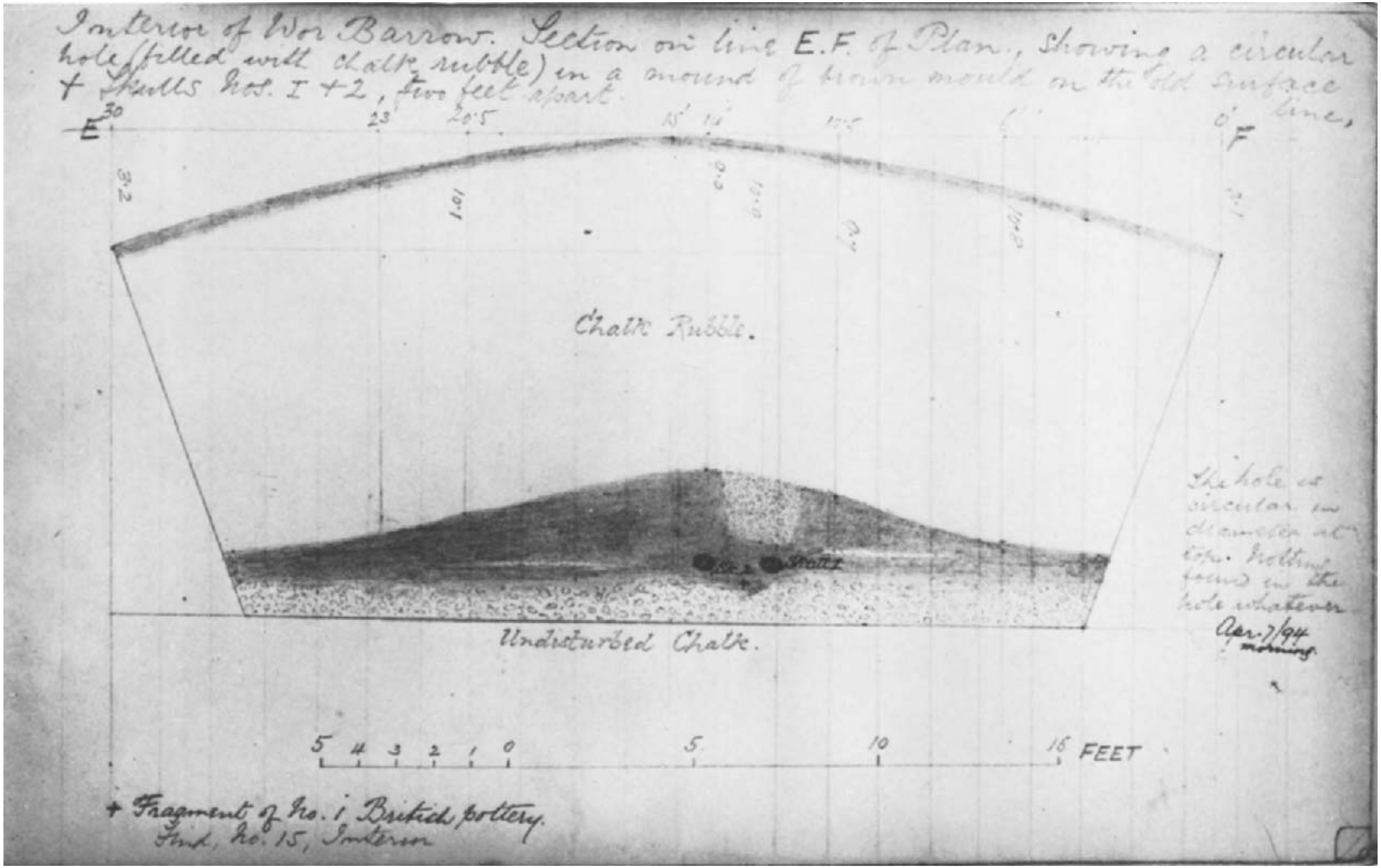

PLATE VI: TWO NOTEBOOKS OF GENERAL PITT RIVERS

Pencil section of the primary burial area at Wor Barrow 1894

See pp. 47–50

Photo: Dorset County Museum

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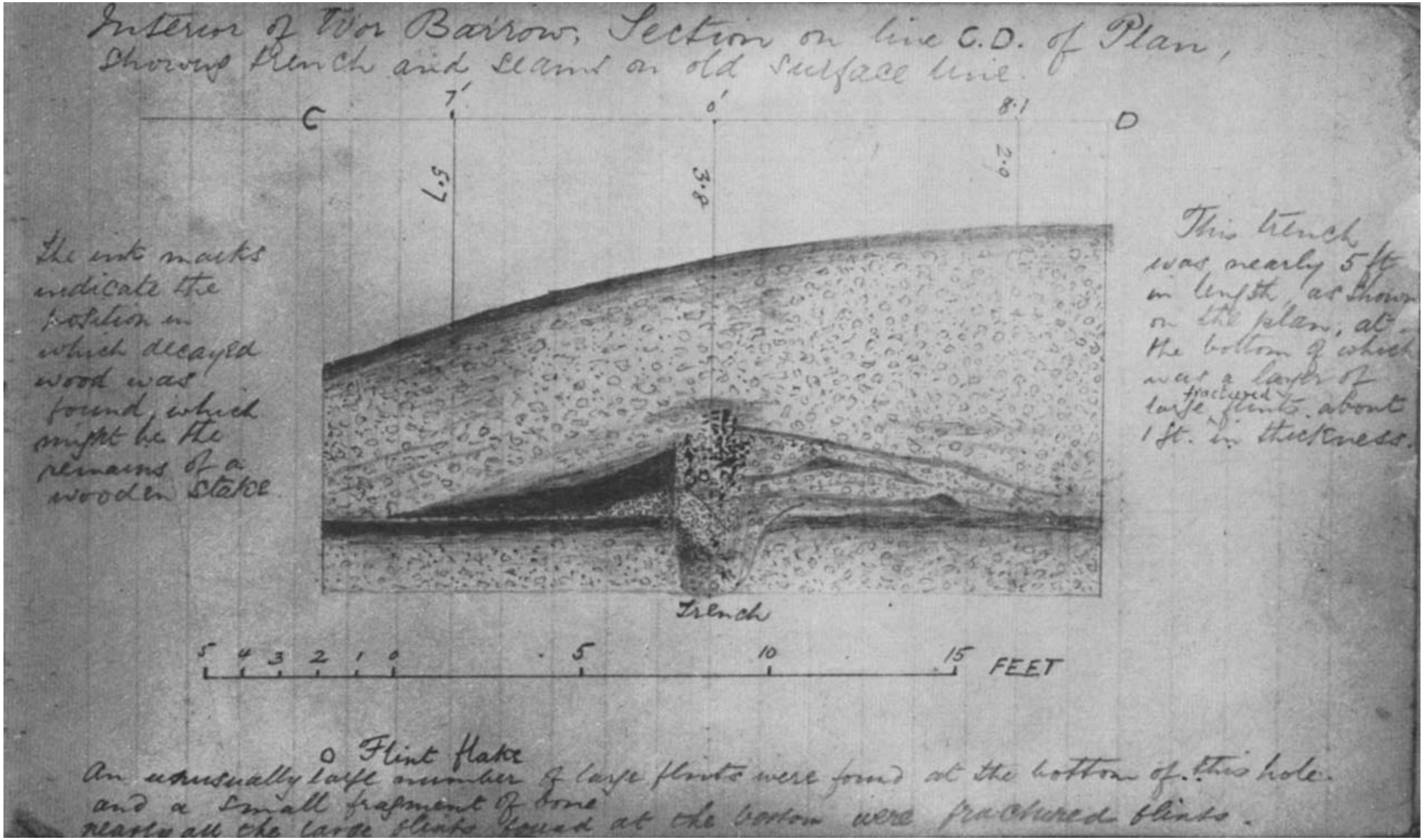

PLATE VII: TWO NOTEBOOKS OF GENERAL PITT RIVERS

Ink and pencil section of the SE side of the mortuary enclosure at Wor Barrow 1894

See pp. 47–50

Photo: Dorset County Museum

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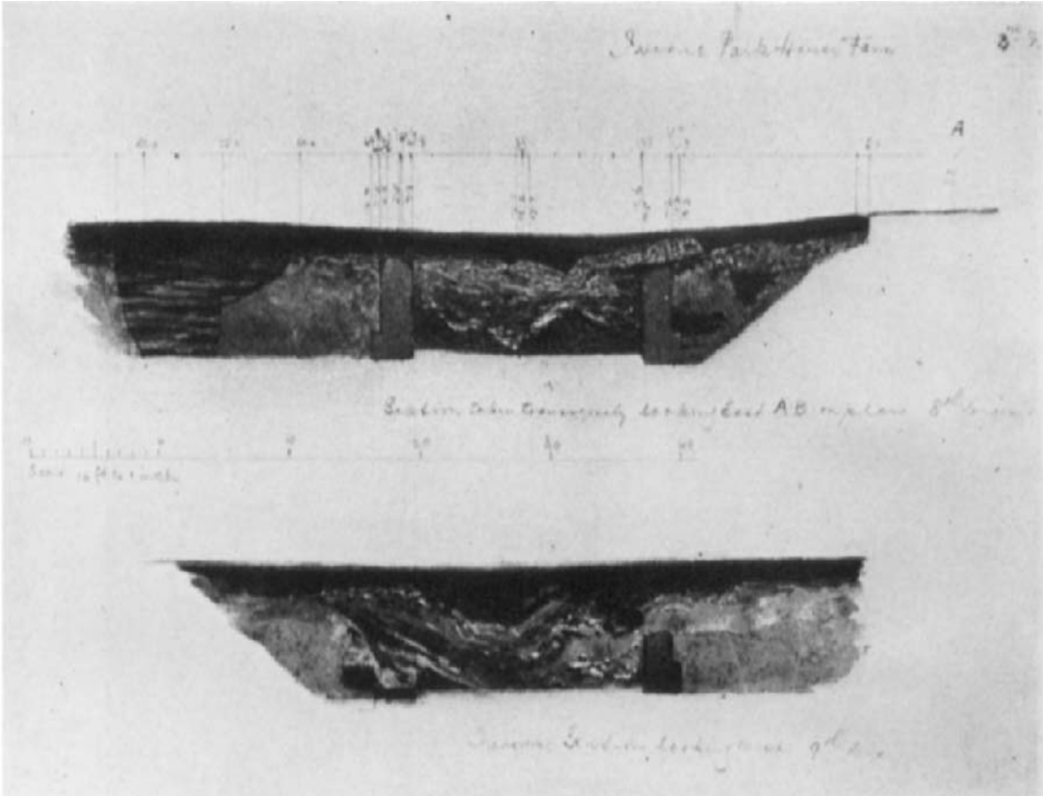

a

PLATE VIIIa: TWO NOTEBOOKS OF GENERAL PITT RIVERS: *Watercolour sections of the Roman 'granary' at Iwerne Courtenay executed for General Pitt Rivers 1897*

See pp. 47–50 *Photo: Dorset County Museum*

b

PLATE VIIIb: THE DESTRUCTION OF ACROTIRI: *Tumbled masonry overlaid by humus, pumice and ash layers. The junction of ash,* a, *and pumice,* p, *is shown;* h, *indicates the humus layer*

See pp. 50–52 *Photo: Richard Dewhurst*

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PLATE IX: AN OGAM INSCRIPTION NEAR BLACKWATERFOOT

The inscription in King's Cave, near Blackwaterfoot on the W coast of Arran

See pp. 53–4

Photo: G. B. Quick

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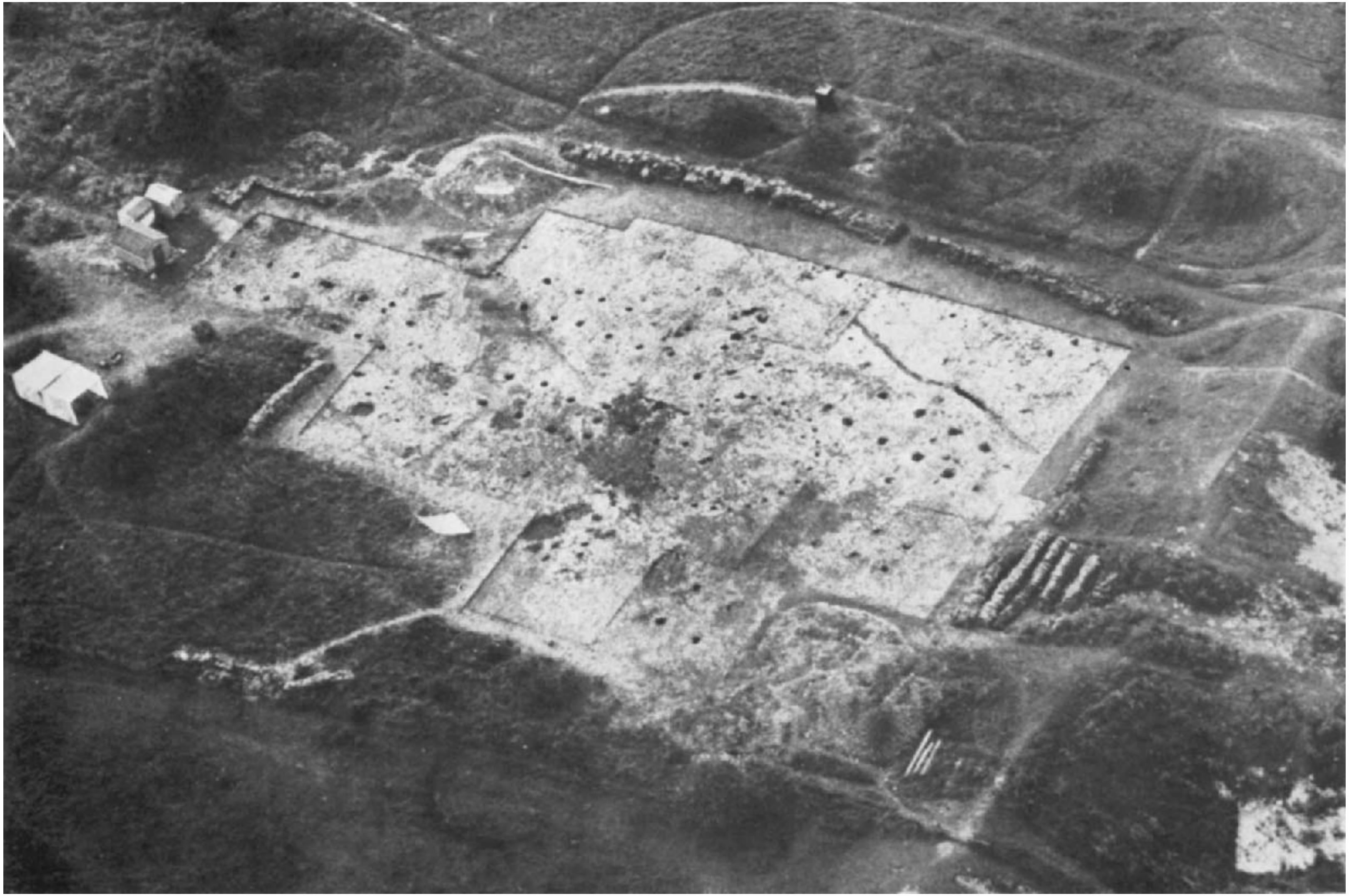

PLATE X: LONGHOUSE AND ROUNDHOUSE AT CRICKLEY HILL

Excavated area 1972; the rampart and entrance are on the right

See pp. 56–9

Photo: Philip Dixon

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PLATE XI*b*: EDITORIAL

Mr Richard Leakey and his wife, Dr Meave Leakey, holding the complete femur and new skull found at East Rudolf, Kenya in 1972

See pp. 1–2 *Photo: Robert Campbell*

PLATE XI*a*: FOSSIL SHELL OBSERVED BY ACHEULIAN MAN

Acheulian flint hand-axe displaying fossil shell, Spondylus spinosus *(J. Sowerby). West Tofts, Norfolk (length: 135 mm.)*

See pp. 59–60 *Photo: Museum of Archaeology and Ethnology, University of Cambridge*

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