
Richard Bradley and Mark Edmonds, Interpreting the axe trade: production and exchange in Neolithic Britain. Cambridge: Cambridge University Press 1993, pp. xiv + 236, 24 pls, 64 figs.

Reviewed by Alan Saville

Interpreting the axe trade is divided into three main sections: 1. Neolithic Britain and the study of exchange systems (pp. 3–58); 2. Axe production in the Cumbrian mountains (pp. 61–153); and 3. Exchange systems and the study of Neolithic Britain (pp. 157–206); with the rest of the volume comprised of tables (pp. 207–17), bibliography (pp. 218–232), and index.

Undoubtedly the core of the book is Section 2, an account of an ambitious fieldwork and research project to study the Great Langdale complex of rock extraction sites, high in the mountains of the Lake District, north-west England. The epizoidized tuff of Great Langdale, known to British archaeologists and petrologists as Group VI rock, was used during the Neolithic to manufacture axeheads, which have been found widely dispersed in England, and occur also in Scotland, Wales, the Isle of Man, and Northern Ireland. Although, in a general sense, Great Langdale was already one of the best known of all the axehead-producing locations, and its products probably the most numerous among the stone axeheads of Britain, relatively little work had been undertaken on the production site itself.

The authors accordingly embarked on a bold, innovative, and in many ways highly successful programme at Great Langdale from 1985 to 1987, involving field survey, sub-surface survey, pollen analysis, test-pitting, excavation, and experimental work. Two patterns of extraction were revealed: one of open-cast quarry pits, with blocks of tuff prised up, roughly shaped, then taken to working floors for processing into more regular roughouts; the other of rock-face removal, both by direct flaking and possibly by fire setting, with regular roughouts made on the spot. A third element, subsequent exploitation of previously abandoned debitage, is suggested for one of the quarry sites. Radiocarbon dates on charcoal suggest this activity was taking place between 3800 and 3100 in calibrated years BC.

It is the account of this work and its results which is presented, in a form more narrative than scientific, in Section 2. Section 1 presents a wide-ranging background to the project, including a summary review of exchange systems, which seems to favour Marcel “The Gift” Mauss as flavour-of-the-month anthropological guru. (Guru fashion changes very rapidly; in a subsequent review of the same topic, Edmonds 1993, Mauss is not cited). Precisely how Mauss’s ideas relate to Section 3 of the book is less clear, since this comprises a classic Bradleyesque dance through the British Neolithic evidence of a kind now increasingly familiar since his first tour de force of 1978. As ever, the distillation (or concoction) of abstruse data and interpretation is dazzling; one feels the authors must have seen a film of which this is the book. As usual, as reader one either goes with the flow, enjoying this latest version of the screenplay for what it is worth as a possible picture of prehistory, or one resists in annoyance at the selectivity and papering over of cracks.

It is interesting that one reviewer of this book did not bother with Section 2 — the boring data bit — but was stimulated by the rest to write his own mini-novel of what was going on in the Neolithic (Sherratt 1994). My own approach is instead to focus on this section, picking up from the quote the authors include as the last line of the book: “books are not made to be believed, but to be subjected to enquiry” (p. 206).
But first, what about the title? Surely this must be an invention of the publishers? Trade does not feature in the index and the authors nowhere use the term without inverted commas. They clearly do not believe in trade as a concept relevant to the Neolithic, yet they are prepared to publish with this as the title. This might, unkindly, suggest that the over-riding concern is to publish the book at all costs and that loftier constraints can be jettisoned. This would be surprising, since the authors are undoubtedly academics and this is surely a specialist professional work? Or is it? The book sends out very confusing signals.

What is nowhere made clear is whether or not this book constitutes the definitive publication of the authors’ fieldwork at Great Langdale. A very good interim report on the 1985–7 fieldwork was published with enviable speed (Bradley and Edmonds 1988), but there is no mention of it in this book — it does not even feature in the bibliography. There is, however, an intriguing statement buried on page 98 of the book: “it is impossible to provide a full account of the observations made at every site; those who wish to examine the information in greater detail are referred to Mark Edmonds’ doctoral thesis, which forms the site archive”. This implies the present work is in fact the definitive published account, and begs many questions.

In British rescue archaeology, the norm is to publish a specialist account of fieldwork in a journal or monograph, and to place the archive in a museum and/or the relevant National Monuments Record. This form of publication involves an editing and refereeing process, generally regarded as beneficial both to the practitioner and to the discipline. It marks a disturbing trend if this process is circumvented for research archaeology by publication in book form and deposit in thesis, a trend towards which one of the present authors has already contributed in the case of the confusing publication of the Cranborne Chase project (Barrett, Bradley and Green 1991a, 1991b; and see pertinent yet restrained comments in the review by Mercer 1992).

Indeed, how can a thesis be regarded as an archive? A thesis, by definition, is a work of original, individual research. Although not spelled out in the book, the interim account states (Bradley and Edmonds 1988:207) that the responsibility for the excavation of the separate sites excavated at Great Langdale lies with various individuals: Julian Thomas, Frances Raymond, Roy Entwistle, Steve Ford, and Peter Hinge, with only one site being excavated by Mark Edmonds, together with Mark Bowden. If it is the case that these individuals were responsible for the excavations, then normal practice would dictate that they must be responsible for writing up their own work and depositing their own archive; they certainly cannot deposit in someone else’s thesis.

This may all seem a bit arcane and pedantic but, apart from the issues of principle involved, it is of significance when considering the technical inadequacies of this book, which would probably have been resolved by editorial control in journal or monograph publication. For example:

1. Dungeon Ghyll is the main extraction-pit site studied by the project (see Claris and Quatermaine 1989:fiche 1.13 for exact location). It is described as a “surface earthwork” (p. 108) and a “distinctive hollow” (p. 119), surrounded by a “bank of upcast” (p. 109), but there is no plan or photo to document this, nor any details of the results of the resistivity survey which apparently revealed its profile (p. 108). All that is presented is the floor plan of the trench across the feature (without showing how the trench relates to the feature in plan), and section drawings of both faces of the trench (figs 6.3 and 6.10). These sections are crucial to the interpretation of two distinct phases of activity here, but cannot be said to be unequivocal. Even in the clearer version of the sections in the interim (Bradley and Edmonds 1988:fig. 1), the “thin soil horizon” said to separate the phases does not show. The sections are given layer numbers (though these do not all correlate across the trench, e.g., layer 4), but there is no key to explain them.

2. Top Buttress is the location of the two ledges close to the summit of Pike o’Stickle, where rock-face extraction took place. The schematic section drawing of one site (fig. 6.13) is, one has to say, a rather slap-dash version of the same drawing in the interim (Bradley and Edmonds 1988: fig. 12). One area of charcoal has been deleted entirely; the shape of other charcoal lenses has changed; the shape and number
of pieces of debitage near the base have changed; and what is presumably bedrock at the base has been given the convention of a solid mass of charcoal.

The text refers to both Top Buttress sections in terms of 10 cm spits with alphabetic prefixes, but these are not shown on the drawings. The locations of the radiocarbon samples from the same sections are referred to by vertical depth, not by spit prefix, and are not shown on the sections either.

3. Stake Beck is one of the working-floor sites. Fig. 6.5, which is the same as the version in the interim (but here over-reduced), has a section drawing which does not correlate with the plan to which it relates; either it or the plan must be at different scales. Again there is no key to the layers in the section.

4. Loft Crag, another working floor, appears to be an extremely interesting site with debitage from the production of two axeheads, yet it is described in very summary fashion in three paragraphs (p. 118) with one schematic plan. There is mention of “an attempt at refitting”, but no cross-reference to table A.3 in the appendix, which shows that two separate groups of 99 and 113 flakes were apparently refitted. Surely this merits more substantial description and illustration?

5. Radiocarbon determinations are not listed together anywhere in the book, but only occur scattered through the text. They are not cited in their BP form with appropriate error terms, only in calibrated BC age ranges, without any indication of the calibration programme used or whether the ranges given are at ±1 or ±2 standard deviations. They are therefore unusable by the professional reader.

6. There are several references in the individual site descriptions to percentages of “retouched material” among the knapping debris, and the figures are repeated on p. 142. Here the conclusion, rather an important one, which these figures are said to support, is that the sites with relatively higher percentages of retouched material were “finishing sites”, which doubled as temporary bases for hunting or summer grazing. However, there is no explanation of what this retouched material might be or what might have been its purpose, and there are no illustrations of it to furnish any clue.

7. The six lengthy tables published in the Appendix contain potentially very useful quantification and technological data about the lithic material recovered from survey and excavation. Tables A.1–2 relate to the survey work finds discussed on pp. 98–104, while A.3–6 relate to the excavated finds. It is presumably the data in Tables A.3–6 which underpin some of the points made in pp. 108–129, yet there is no reference to A.3–6 anywhere in the book (unless the reference on p. 106 to Tables A.1–2 is a misprint for A.3–6).

8. We are told of the arduous nature of the finds recovery process during this fieldwork, with all the lithic samples being brought down from the mountain in rucksacks. It comes as rather a surprise, therefore, that there is only one figure with any of the excavated finds. This shows (fig. 7.1), at a peculiar and totally unusable scale (1:5.6), five “roughouts” from Dungeon Ghyll primary contexts, and four from secondary contexts at the same site. The purpose of the figure is to contrast the irregularity of the earlier roughouts with the symmetry of the later ones. The contrast is certainly apparent, but it is by no means obvious that like is being compared with like. It is hard to accept, on the basis of the unfigured drawings presented, that the objects from the primary contexts are roughouts. (Incidentally, convention is further flouted here by not numbering the nine objects in the figure.)

The absence of finds illustration is very puzzling. Apart from the nine items in fig. 7.1, there is a photograph of a hammerstone (plate 4.11). This absence can be set against the information in Table A.3, which shows a total of 114,938 flakes from the excavated sites (the number of roughouts is not given). The authors are not averse to artefactual representation per se, since they include illustrations of numerous objects from locations other than Great Langdale (figs 2.1–2; 7.6–8; 8.1; 9.2; 9.5 and plates 3.1–2; 9.1). There is thus no obvious explanation for this reticence to illustrate finds from their fieldwork. The authors have excavated an artefact-rich set of sites and, as is clear from the tables in the appendix, have subjected their artefact samples to a great deal of detailed analysis, yet they have chosen not to present any significant visual impression, even by photograph, of the debitage, “typical” flakes (p. 125), roughouts, and “retouched material” they recovered.
9. Several sets of histograms are presented for flakes from various locations and contexts (figs 6.4; 6.6; 6.12). Each histogram is for a separate flake category as defined by the experimental studies. The data within the histograms are shown numerically rather than in percentages (which makes comparison difficult), and are ranged in ratios (1:3; 1:6; 1:9; etc.), which, according to the figure captions, are flake length : platform width. However, all the text references (pp. 114, 116, 124) to these histograms are to flake length : platform thickness ratios, which makes it unclear exactly what aspect of the platforms is being measured. Further confusion is added by references to flake thickness:length ratios (pp. 117–19, 124), which are nowhere itemised; confusion is compounded when the Stake Beck site flakes are said to have low flake length : platform thickness ratios (p. 114) and the Harrison Stickle ratios are said to be similar but are described as relatively high ratios (p. 116).

The ratio being used, whatever it is, is taken to provide an index for the level of skill of the producers (p. 114). Presumably this has to do with flake refinement and position in the reduction sequence, though the flake categories themselves are a measure of this and therefore must to an extent predetermine any patterning revealed.

With reference to the histograms for the Top Buttress flakes (fig. 6.12), we are told that: “the finds from layers with large amounts of quarry debris show the widest range of platform sizes” (p. 124). Putting to one side the question of whether platform size is the same as platform width and/or thickness, there is no indication from the figure of which layers at Top Buttress produced the flakes used for the histogram data.

Enough has been said to make the point that, although ostensibly the final report on this piece of fieldwork, the presentation falls short of what might be expected if published in the normal way as an excavation report rather than in a book. The kinds of problems highlighted are precisely those which experienced archaeological editors and referees would spot before publication, and which copy editors at Cambridge University Press clearly did not.

To be fair, this section of the book does have one major advantage over the interim report, in that it includes a series of site photographs (plates 4.5–10; 5.1–2; 6.1–2), which are highly evocative and do help immeasurably to give the reader a feel for the location and its archaeology. There is also, very much on the plus side, a useful summary (pp. 86–93) of the experimental replication studies undertaken by Edmonds, giving detailed information on axehead manufacturing and polishing, though regrettably with no illustrations of the resulting roughouts or polished axeheads. (This account would benefit from expansion and separate publication in its own right.)

It is in this section that the flake categories to be used throughout the description of the survey and excavation finds are defined (Table 5.1), on the basis of experimental roughout production, and examples of the experimentally produced flakes are illustrated (Fig. 5.4; but what a pity there is no cross-reference between the table and the figure to show into which categories the experimental flakes fall!).

I have dwelt at length on what I see as the central, fieldwork section of the book, and I could be accused of not having seen “the wood for the trees”. But if one turns to the more general, more interpretative parts of the book, there are also problems.

To get some of the obvious editorial points out of the way first: in fig. 2.4 some of the sites have migrated away from their home base (especially sites 29 and 37); the extent of the chalk in southern England has shrunk dramatically when fig. 2.9 is compared with fig. 8.5; fig. 9.7 is a plan of the surviving surface indications of extraction shafts at Grimes Graves, not of the surviving flint mines; fig. 9.2 has no scale; fig. 8.9 seems to have strayed in from a different book entirely; and there is text missing between lines 2 and 3 on p. 196.

As with the fieldwork section, the rest of the book suffers from a lack of illustration of the artefacts central to its theme. Section 1 has no illustrations of Group VI axeheads at all, and it is not until p. 147 that
there are three pages of axehead drawings (at 1:4.5 scale): firstly 7 roughouts/axeheads from Portinscale (fig. 7.6); then 9 axeheads of the so-called Variant type (fig. 7.7); followed by 8 "Cumbrian" axeheads (fig. 7.8). After this the book has only one more Group VI axehead illustration, a photograph on p. 191 of an oddity with a perforation at the butt (plate 9.1; no scale).

The Portinscale artefacts (fig. 7.6) are described as a possible hoard in the caption, and similarly so in the only text reference on p. 144. This is misleading without further qualification, since the published information otherwise available would suggest two, perhaps even three, separate groups (Annable 1987:3356). The individual axeheads are not numbered in the figure, and are not itemised, but it is the two roughouts shown top right which are recorded as found in a different context a year later than the other four roughouts. The only polished example, bottom left, was apparently found with these four, but possibly in a separate layer.

Hoard titles in general receive rather short shrift in this book, although the distribution and typology of Group VI axeheads and roughouts found in hoards in and beyond the north-west are of considerable research potential; and this despite the known interest in the topic of one of the authors (Bradley 1990). Particular note should be made of the curious discovery, not mentioned here, of a hoard of 5 roughouts on Pike o’Stickley in 1988 (Boyd 1990), presumably very close to where the 1987 excavations took place.

The axeheads illustrated in figs 7.7–8 are drawn in such a schematic way and are at such a small scale, that anyone unfamiliar with these objects in real life would have absolutely no idea from the reproductions in this book of their physical presence and frequent aesthetic beauty. These are qualities, albeit subjective, of legitimate academic concern (p. 49), for example if it is thought that "the polishing of stone tools makes them an ideal medium for the display of stylistic information" (Bradley 1990:44). The "Cumbrian clubs" (Annable 1987:14), with their fancy waisted butts, which are Group VI axeheads at their most elegant, are "power tools" exemplified (e.g., the Belmont hoard: McIntyre 1937; or the Drumour hoard: Neish 1872). In not including an appropriate photograph to show such aspects, the authors have decidedly missed a trick.

When it comes to morphology and typology, the authors use the standard subdivision into roughout, "Cumbrian", and "Variant" forms, and seem to be following Annable’s research (1987:14–7) in regarding the "Variant" axeheads as the result of reworking the "Cumbrian" types. This is not a topic to which the authors give much attention, however. The defining characteristics of these types are not fully explained and they are certainly not obvious from figs 7.7–8. Confusion is created by the use throughout the book of "Cumbrian axe" (without inverted commas) to mean any Group VI axehead irrespective of its typology. Further confusion is created in the discussion of side and butt facets (p. 163); the very strange fig. 8.4, which has schematic sections through unidentified areas of 20 unidentified axeheads, to show their side-faceted form, is cross-referenced in the text to a sentence discussing butt facets.

One further point must be raised with regard to figs 7.7–8. All the axeheads shown schematically here were published by Annable (1987:figs 6–8, 10–12) and it seems possible, though it is not stated, that these are versions of Annable’s drawings. If this is so, then what are shown as cross-sections (and hence very germane to consideration of traits such as the side facets), would in most cases actually be misrepresentations of Annable’s end-on-views showing the line of the cutting-edge.

In view of the subject matter of the book, it might be expected that distribution of the products of Group VI rock would come under close examination. Not so. The only distribution plot is the inadequate and over-reduced fig. 3.1, which excludes Ireland. The distribution shown differs in detail from that in the most authoritative recent publication (Clough and Cummins eds 1988:270), without explaining why. Perhaps this lack of concern reflects unspoken reservations about the utility of distribution plots, especially given the low percentage of axeheads sectioned, or unease with the current characterization criteria for assigning individual axeheads to Group VI? Apparently not so either.

One would anticipate very close attention to the problems of the petrological identification of the Group VI source and its products (cf. Woolley 1989), in view of the current debate over the validity of much of the thin-section matching which has been undertaken. As Berridge (1994:41) has recently
articulated this: “increasing numbers of archaeologists are beginning to express serious doubts about some of the concepts, the techniques, and general approach behind implement petrology studies”. This disquiet is glossed over here, as is the erratics question, which by way of acknowledgement gets a Parthian shot (p. 50). The bald statement: “axes made from local erratics were seldom polished completely” (p. 50) comes without any citation. How do the authors know this?

Related to this question are the recent, exciting moves to study flint and stone axeheads in Ireland using a much firmer database and more rigorous characterisation techniques (Grogan and Cooney 1990; Sheridan, Cooney and Grogan 1992). This development, and the issues it raises, are not discussed here (though this perhaps reflects delay between composition and publication of the present book?).

Finally in this long list of concerns, I return to the title of the book. The publishers cannot be blamed for the use of axe rather than axehead, since the authors themselves use this throughout the book. Indeed, in so doing they are only following a now long, if unfortunate, tradition in British archaeological writing since the use of “celt” (e.g., Evans 1897) passed from fashion; a sin of which in the past this reviewer has also been guilty. There are strong reasons for changing this tradition, however. It is not just that axehead is the semantically correct term. Use of the shorthand form “axe”, when only the axehead is meant, potentially excludes several layers of significance relating to the way in which axeheads were or may have been used, whether functionally, “commercially”, ritually, or symbolically. One would not expect the present authors to fall into this trap; indeed, it is clear from various comments in the book (e.g., p. 49) that they are aware of the danger. Nevertheless, their constant use of “axe” precludes certainty at any point in the text as to whether they are conceptualizing the axehead in hafted or unhafted state. When found in boards (plate 3.1) it is obvious that axeheads were unhafted, but when found alone, whether in on- or off-site contexts, this is rarely clear.

Failure to include detailed discussion of hafting and hafts, of evidence for use and wear, of practical considerations of use, and of whether it was actually axes or axeheads which were exchanged, is remarkable enough in a book with this title. But to fail to consider the evidence provided by two of the most remarkable finds of hafted axeheads from Britain — at Ehenside Tarn and Solway Moss (Evans 1897) — both in Cumbria and both probably Group VI rock — or even to illustrate them, is an extraordinary omission.

This review has gone on far too long. I have made plain my antipathy to the book, which I trust the authors will forgive, but they did, quite literally, ask for it (p. 206). Interpreting the axe trade is an academic book, but, judged by rigorous standards, it is not a scholarly one. Is this simply a regrettable reflection of the fast times in which we live, and of the multifarious pressures of the academic industry of which the authors and publishers are part? Does it matter if students of archaeology, presumably the target audience for this book, are not given clarity of factual exposition and illustration? Circumstances have conspired to make this less of a book than it could have been, but at least the authors are such prolific writers that this one disappointment is expendable. My advice would be to avoid this book and instead use their succinct, more readable, and much better edited alternatives on fieldwork at Great Langdale (Bradley and Edmonds 1988; together with Claris and Quartermaine 1989) and on axehead production and exchange (Edmonds 1993). Am I wrong in protesting and demanding too much? Perhaps I am over-empathizing with the Neolithic prospectors who were seeking perfection of their own at Great Langdale?

REFERENCES


