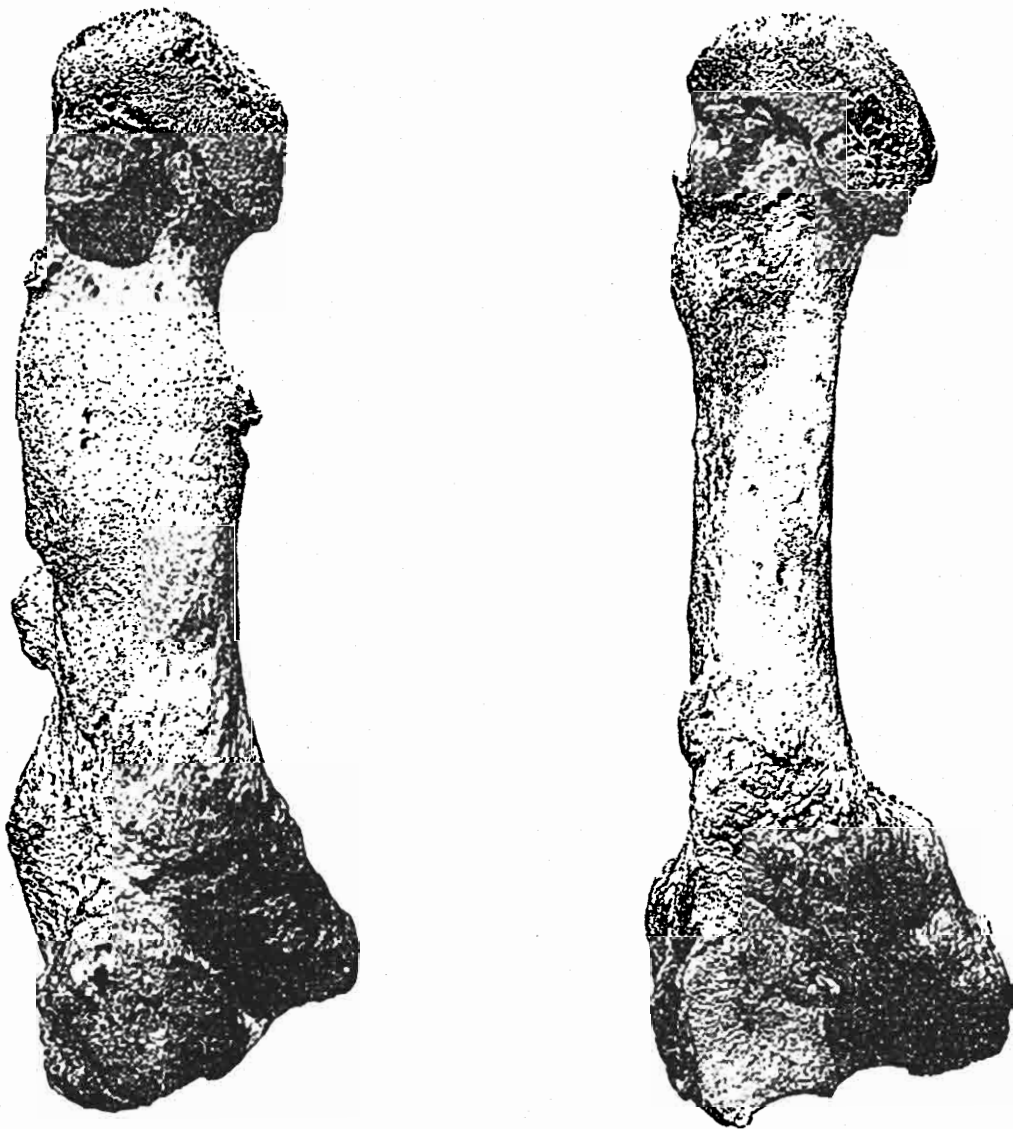


# *Circaea*



## *Circaea*

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*Circaea* is the Journal (formerly Bulletin) of the Association for Environmental Archaeology (AEA) and—as from Volume 4—it is published twice a year. It contains short articles and reviews as well as more substantial papers and notices of forthcoming publications.

The *Newsletter* of the Association, produced four times a year, carries news about conferences and the business of the Association. It is edited by Vanessa Straker, to whom copy should be sent (c/o Department of Geography, University of Bristol, Bristol BS8 1SS).

**Editorial policy for *Circaea* is to include material of a controversial nature where important issues are involved. Although a high standard will be required in scientific contributions, the Editors will be happy to consider material the importance or relevance of which might not be apparent to the editors of scientific and archaeological journals—for example, papers which consider in detail methodological problems such as the identification of difficult bioarchaeological remains.**

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*Front cover: Right third and fourth metatarsals with probable healed fractures, from a human male of 50+. Medieval burial from the nave of the Gilbertine Priory of St Andrew, Fishergate, York. From a photograph by Dick Hunter, Department of Biology, University of York, supplied by Gill Stroud, Environmental Archaeology Unit.*

## Editorial

With the appearance of this issue, we complete volume 7, nominally for 1989, and will thus have reduced to some extent the unfortunate backlog of issues. Providing we can maintain the momentum, we may be able to catch up completely by the end of next year!

Inevitably we must apologise to some of our authors for long delays in dealing with typescripts; we hope this won't discourage intending authors from submitting papers or other material for publication. Since we currently have rather little copy 'in hand', we should be able to achieve a much quicker turn-round for new material. (At the risk of repetitive whinging, we must remind our readership that *Circaea* is, prepared in the Editors' vanishingly little 'spare' time!)

We have been gratified by the response to the new format for *Circaea*; it is certainly much easier to prepare copy by the present means. Once again, we are indebted to John Carrott for patiently typing in copy received as typescripts and to authors who sent copy on PC disks. We should shortly be able to handle 3½" disks as well as 5¼" floppies and Amstrad PCW disks.

Many of you will know that the triumvirate of *Circaea* editors is now one-third depleted: Terry O'Connor has left the EAU to teach archaeological science at the University of Bradford. His contribution to *Circaea* since its inception in 1983 has, of course, been invaluable, and the 'surviving' editors will miss his input sorely. However, Terry has agreed to remain as an editorial consultant.

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### Professor F. W. Shotton MBE, FRS 1906-90: a personal reminiscence

My first encounter with 'Fred' Shotton was on the fateful day in 1969 when I visited the Department of Geology at Birmingham University to decide whether I wanted to become a Quaternary entomologist. Fred was a somewhat shadowy figure to me, as a rather muddled graduate, but my abiding memory is of a huge, gentle man, standing at a distance, with a delightfully

mischievous glint in his eye.

Three years at Birmingham served to convince me that Fred was, indeed, a kindly giant, a straightforward man who regarded members of his department almost as family, treating them with a mixture of sternness and parental indulgence. He could certainly be downright fierce, as illustrated by the possibly apocryphal incident in which he led a group of senior academic staff through a demonstration by students who, until faced with Fred's bulk had a mind to barricade the Senate in its meeting room. Certainly not apocryphal are the stories in which Fred appears in the radiocarbon laboratory, giving forth on the importance of sample purity while the cigarette hanging from his lip liberally sheds ash everywhere!

I knew little of Fred as a geologist when I worked in his department, but I was deeply impressed by his broad and often detailed knowledge of natural history, and of course especially pleased by his interest in insects. Going into the field with him was hard work—even immediately before retiring, he could put us youngsters to shame, whether augering or just striding across the treacherous terrain of a gravel pit. However, the hard work was accompanied by a stream of amusing anecdotes and enlightening information about geology, biology and life in general, which made the physical suffering worthwhile.

My time at Birmingham was marked by upheavals in my personal life, and eventually I left to take up a post at the Natural History Museum, never getting round to writing up. An intermittent correspondence records my communications with Fred on the subject of thesis writing—he managed to convey displeasure, understanding and encouragement in about equal measure. His concern for ex-students was evident in the support he gave while 'that rascal Kenward' was trying to decide what to do with his life, and his behind-the-scenes support for environmental archaeology in general and the York Unit in particular leaves us all in his debt.

If only there could be more like him!

Harry Kenward

## Book reviews

**A. T. Lucas** (1989). *Cattle in ancient Ireland. Studies in Irish archaeology and history.* Kilkenny: Boethius Press. ISBN 0 86314 145 5. Soft cover, 315pp., about 15 punt (approx. £14).

This volume originates from the Rhind lectures given to the Society of Antiquaries of Scotland at Edinburgh University in 1971. Dr Lucas, a former head of the National Museum of Ireland, was still preparing them for publication two days before his death in 1986, and the task was finally completed by Professors E. Rynne and G. MacEoin last year.

The book is based entirely on literary sources, mainly Irish records (the Annals of the Four Masters, for example), some of which date from the 7th century AD and are written in obscure old Irish. However, mainly after his lectures were delivered, Dr Lucas discovered numerous non-Irish accounts, mainly military, dating from Norman times onwards, and a few 19th and even 20th century writings by government officials and amateur historians in the field. These, and the ambiguity of Early Irish, caused Dr Lucas to remark that he was cursed above all cattle.

The Irish Annals only record abnormal events. The military accounts are filled with astonishment at what the Irish apparently thought normal, though that normality must have been considerably disrupted by the presence of an occupying power. Later oral evidence from 'the peasantry' was influenced by the attitude of the recorder to his informer: if he thought the latter was ignorant or slow-witted, he was liable to edit and embellish. Moreover, the recorded was probably told what he wanted to hear, in the hope that 'his honour' would dig deeper into his pocket for a tip. Dr Lucas does not appear to have consulted any agricultural experts, who might have been able to clear up some of his problems.

The history of Irish cattle husbandry is important, since it dominated Irish agriculture and is probably relevant to a considerably earlier period than when it was written down. The Irish had no Roman occupation to make them change

their Iron Age ways, though they had, of course, received Celtic Christianity. The Irish climate also means that this history may not be fully relevant to the rest of northern Europe. In addition, the cattle were almost exclusively used for milk production, and horses were used for traction. Hence most of this work refers specifically to cows, and only occasionally to calves and bulls, 'beeves' having had little importance before the military occupations. Population density in Ireland was probably much lower than in at least the English part of the British Isles and much of Continental Europe.

The cattle seem to have been kept in self-contained herds, probably numbering less than 60 cows, called *creaghta*. (The term also came to be applied to individual animals or their herders and its spelling was very variable). The group may also have included sheep and goats, particularly during the later Middle Ages. Their care was the exclusive task of families of servile herders, the women doing the milking. The vast areas of summer pasture in Ireland, both bog and mountain, meant that transhumance (*booleying*) was widely practised. Winter forage was scarce and consisted largely of uncut meadowland. Such hay as was made was reserved for horses.

Sometimes the *creaghta* were continually on the move, in which case they might have been owned by their herders. Grazing was sometimes rented, sometimes they trespassed, and sometimes they were welcome, no doubt as providers of manure. There seem to have been considerable areas of unoccupied land. A *creaght* might be attached to an army on the move; overlords sometimes 'borrowed' their tenants' cows for this purpose.

The word *creaght* is suspiciously like *creach*, which means cattle raid. Raiding was an institutionalised affair which regularly took place between established enemies. It was, however, considered unsporting to carry out a raid shortly after promising not to do so before a senior clergyman. The herds of the monasteries were usually safe from raiders, as it was believed that the clergy could deliver effective curses. Sometimes clerics were invited to bless a projected raid, and received some of the spoils if successful.

The animals were driven off on foot by herders, whilst the mounted aristocracy held off pursuers. This was considered to be good military training. The cows were rarely harmed; there was a law against killing them unless really old and barren, of equal importance to the law against killing women. Slaughter only seem to have taken place out of spite, when animals had been secreted in a place from which it was impossible to drive them. Many remote valleys were closed off by stockades. On occasion, the raid was a method of collecting taxes. Though some spectacular raids took place over hundreds of miles, notably in and out of Ulster, most animals were only moved about one day's journey, and on occasion cows returned home of their own accord.

Though there is little direct information about the husbandry of the individual cow, many inferences can be made. Shortage of winter fodder would have resulted in a small, slow-growing animal, and cows were not expected to calve before they were four years old. The proportion of bulls to cows was high - one bull to ten cows being mentioned on two occasions - suggesting that fertility was poor. Calving did not take place until June, by which time the cows would have recovered sufficiently from winter deprivation to give a satisfactory lactation. This was probably infinitesimal by present day standards, or even those of 50 years ago, when 600 gallons (2700 litres) was considered an adequate commercial average. The lactation period was probably also short, lasting five to six months, since Christmas was considered to be a good time for raiding.

Because of the raiders and the presence of wolves, cows were returned to the homestead at night. Milking then took place, for which the presence of the calf was usually necessary. The development of the free let-down process, whereby the presence of the milker in familiar surroundings is sufficient, seems to have been slow to develop in Irish cattle. The standard techniques of vaginal stimulation and the disguise of a strange calf by covering it with the skin of the cow's own calf were practised, so that on occasion one fortunate live calf substituted for up to ten dead ones. The supernatural was invoked, and there were stories of wolves which had consumed calves presenting themselves

for the cow's maternal attentions when persuaded by various saints to do so. On occasion, the cow might accept the herdsman's dog. These were large guard dogs, and in general each was considered to be worth six cows. Free milking was first achieved in the monastic herds, in which it was recorded that the cow licked the tonsured head of the milker as a substitute for her calf. Significantly, these herds were disturbed less than the regularly raided ones. With the exception of the divinely motivated wolves, much of what is described above is common to pastoralists the world over, from Mongolia to the ancient Near East and Africa.

Another practice which is carried out in many parts of the pastoral world is the withdrawal of blood from the living animal. This process was described as pagan, disgusting and barbarous, or as a regrettable necessity which reduced the fertility of the cow, according to the degree of understanding of the reporter. The resulting blood was coagulated and preserved with butter for winter consumption, or regularly withdrawn on a Sunday at a particular spot hallowed by ritual for immediate consumption, or was withdrawn for therapeutic reasons (such as the treatment of the parasite-induced haemolytic anaemia known as red water!), again according to the enlightenment of the informant.

By his devoted labours, Dr Lucas has performed a considerable service to archaeozoology. It would be difficult to detect most of the above events by archaeological means, and besides this, Dr Lucas describes the attitude of the early Irish towards their stock. Besides being walking currency and providing food, they were a definite part of the community. On occasion, when an important person died, the cows compulsorily became mourners. They were kept from their calves for up to three days, and the bellows of complaint from both parties were considered a desirable accompaniment to the wake. This is another world entirely from the economic strategies so beloved of present-day scholars.

Reviewer: **Barbara Noddle**

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V. Pflieger and J. Chatfield (1988). A guide to snails of Britain and Europe. London: Hamlyn. ISBN 0 600 55127 X. 216pp., many figures and plates. Hardback £8.95.

T. Warmoes and R. Devriese (1987). Land- en zoetwater mollusken van de Benelux. Gent: Jeugdbond voor Natuurstudie en Milieubescherming. No ISBN. 145pp., numerous figures. Soft cover Hfl15 (about £5).

Snails have their place in environmental archaeology and, like most invertebrates, are an important indicator of the 'health' of existing habitats. The two books reviewed here are very different attempts to make this group more accessible to the amateur naturalist, and may be useful to those who work on ancient mollusc remains.

Pflieger and Chatfield's *Guide* is the English edition of a Czechoslovakian publication, and is basically a photographic guide. Over 150 species of slugs and snails, and a few freshwater bivalves, are illustrated in colour photographs which range in quality from acceptable to quite magnificent. The accompanying text is usefully detailed, and consistent in the information which it provides for each species. To the photographic section, June Chatfield has added text and line drawings to describe British taxa not otherwise mentioned, and a key to British species which is as useful as any such key could reasonably be expected to be. There is also a key to European families, and a substantial introductory text which deals with the morphology, biology and ecology of land gastropods and freshwater bivalves, and with their systematics, zoogeography, and conservation. The parochial Briton, faced with this attractive little book, will complain that many of the species depicted are not found in Britain. This is true, but it does no harm to be reminded of the range of variation in families of which we otherwise see only one or two species, and real conchophiles will enjoy a remarkable number of photographs for under £10.

Warmoes and Devriese's *Land- en zoetwater mollusken* is completely different in appearance. The book is obviously intended for use in the field, and gives an ingenious pictorial and textual key to a range of species which includes most British natives and a few more besides. The pictures are

all line drawings, not always sharply reproduced on the brazenly recycled paper, and the text is in Dutch. The language barrier is not a serious problem, however, as there is a pictorial glossary of technical terms (*lip, sutuur, navel, tuberkel* ...), and the key, like most of its kind, uses a very limited vocabulary. Given a nodding acquaintance with molluscs, and the occasional dip into a Dutch dictionary, most people should find this book quite useful.

These two books deserve attention because they offer a cheap supplement to the standard literature on the identification of non-marine molluscs, and because they demonstrate, in very different ways, that a useful field guide does not need to be expensive or dull.

Reviewer: T. P. O'Connor

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Heinz Ellenberg (1988). *Vegetational ecology of central Europe*. (4th edn) [translated by G. K. Strutt]. Cambridge: University Press. 731pp. price £75.

This is a book about ecology and plant communities, an English edition of what has become the standard work on the subject for the German-speaking world. But is it relevant to the British Isles?

Indeed, is ecology relevant? It was a student story that when Professor Darlington was at the Botany School (as it then was) at Oxford he described the resident ecologist as "the incompetent in pursuit of the incomprehensible". Putting the lapse in manners aside, there is, however, a grain of truth insofar as ecological studies can be hard to understand (so also can the late professor's pet subject, genetics). Academic subjects which are generally poorly explained and bound in jargon deserve to fail to reach wide acceptance (but how to explain the rise of personal computing and all its jargon?).

My own contacts with ecology in the form of vegetation science made me beat an



early retreat from David Shimwell's book (*The Description and Classification of Vegetation* 1971), for example. This is surprising since his supervisor, David Bellamy, has done more than anyone to make ecology available to, and above all understood by, one and all. So, for me, ecology and especially vegetation science lay fallow for many years until, following a favourable review in *New Phytologist*, I ordered an earlier edition of Ellenberg's book for the library. When it came it was a revelation, for there was the vegetation of Central Europe beautifully laid out and clearly explained (in German). I have been an enthusiast ever since.

Now, several editions later, there is a translation offered by Cambridge University Press. It is an amazing book. One can look up any plant community in Central Europe or neighbouring regions such as the British Isles, and there is a tidy summary of the ecology and often its history too. One can either read it through, or dip into sections of interest. Heinz Ellenberg must be an amazing person with an insatiable appetite for ecology to be able to gather all this knowledge and sort it out in this way; there cannot be many people who could do this and get it all summarised and written down. In doing so, he has kept a good style that does not deal with community after community in a repetitive manner, and the level of illustrations is good.

Now for the tricky bit: translation from German is not entirely straightforward. For one thing, German is sometimes written with a particular syntax that is quite different from English and leads to a dilemma: whether to translate too literally or too interpretatively. Too-literal translation makes the result seem rather Teutonic after the style of "I have been here since half an hour, when do I become a sausage?", and interpretative translation departs from the style of the original. Another problem area is that of botanical and other rather specific terms which can be very hard to translate well, especially when there is no real equivalent in English. Here it is a matter of fine judgement whether to use the German word (maybe in quotations) or to coin a new term as appropriately as possible. The third matter is more a technical one involving the sheer size and complexity of a task with more than 700 pages and captions and text to

500 figures and the overall editing. The reviewer confesses to not having read quite every page thoroughly.

The translation mostly flows well, but sometimes there are lapses into Teutonic where the translator "becomes a sausage", for example: '...before ever man came on the scene burrows of wild animals or Deer tracks could have enabled those plants which would have found it impossible to grow in woods or swampy meadows and other perennial dense formations, to survive and spread' (p. 622). It is not strictly wrong, but sticks to the German original too closely to be good plain English.

As for botanical terms, some ecological terms such as 'therophyte' are not used here, although they are explained early on. Too-literal translations of botanical and other terms are scattered throughout, such as 'needleleaved' and elsewhere 'needle trees', (conifers); 'reaction value' (pH); 'social fallow' (sounds fun?); 'nival belt' and 'hook plough'. Some of the plant names are also translated too literally, such as Blue Cornflower and Sycamore Maple and Black Alder, these being the German common names translated rather than English ones. These are just a representative sample of botanical points, which show it is unlikely that the translator is a European botanist.

I am afraid that I am even less satisfied with overall editing and production. The book is peppered with small faults at a rate of about one per page on casual reading, which become irritating. I am not going to be boring and list them all (I can be just as tedious without), but to justify the comment, I provide these examples: I find the practice of capitalising vernacular plant names annoying if not downright wrong and, combined with literal translation of plant communities, the results are sometimes comical—I very much hope that 'the ruderal community of Good King Henry' (p. 621) will not become regarded as a proper term here, except in jest. There is editorial inconsistency, too, so that most of the geographical names are translated where there are English equivalents, but strangely not Kärnten (Carinthia), and there are a number of other things that have escaped translation. The inverted commas are back-to-front, unless this is the latest

fashion in Cambridge. The photographs are muddy compared with the originals, the re-lettering of the diagrams is often smudged (as on p. 60; it looks like the sort of thing I do myself!). The book is glued rather than sewn together, and the binding does not look as if it will survive library life for long.

The overall impression is one of carelessness. I know all too well how hard it is to rid my own work of silly little faults (let alone the big ones), but one should expect better from a company which is not exactly a back-street publisher, and for £75 (twice the cost of the German edition). When CUP produced Godwin's *History of the British Flora* one didn't look for mistakes because there weren't enough to make the search worthwhile. When the third edition of the *Flora of the British Isles* appeared, one really started to notice mistakes (well I did anyway), particularly obvious ones such as missing plant families. Unfortunately this tradition continues here.

This is basically a good book or I wouldn't bring it to the attention of *Circaea* readers. It is a very important one because it offers such a good grounding in ecology and vegetation classification (phytosociology). Polunin and Walters' *Guide to the Vegetation of Britain and Europe* of 1985 covers a larger region including the Mediterranean, in less detail, but consciously excludes phytosociology because it was written for the layman. Myself, I find Ellenberg's classification very convenient in helping me to understand vegetation. Although Ellenberg wrote about Central Europe he does add the surrounding regions and I believe the content mostly applies to British vegetation, although the more warmth-demanding, continental or montane vegetation is naturally less well represented here. The purely British viewpoint is an excessively narrow perspective in many matters. To view vegetation on a European scale seems to me to be worthwhile because many plant communities are far better developed in other areas—our alpine vegetation is a relict curiosity; on the Continent it is a subject in itself. As far as vegetational classification goes, I believe it is far better for botany and archaeobotany to consider the plant communities of northern and Central Europe according to the same general classification, and then

compare the detailed local differences, rather than to go for a confusing plethora of regional classifications—the latter seem to me to be about as pointless in vegetation science as is the endless splitting of taxa that don't separate into true species, in systematic taxonomy—a positive hindrance to those who want to *use* the results. Ellenberg's book is here now, and the system is widely used and understood. A word of warning, however: these are modern plant communities, and great caution is needed when comparing them with archaeobotanical results.

Reviewer: **James Greig**

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T. Amorosi (1989). A postcranial guide to domestic neo-natal and juvenile mammals. The identification and ageing of Old World species. *British Archaeological Reports, International Series* 533. Oxford. 380 pp; numerous figs; ISBN 0 86054 678 0. £23-00.

This book is an attempt to collate and synthesize data pertaining to a complex subject. As is often the case with such syntheses, one's joy that the task has been undertaken must be tinged with regret that it has not been done well. The intention has been to summarise the published data concerning dental and skeletal maturation in horse, cattle, sheep, pig, dog, cat and rat, with tables of dental eruption and epiphyseal fusion data, and illustrations of immature post-cranial elements for these taxa. As such, this is a useful volume which brings together diverse and disparate sources of information. That alone should assure regular sales.

Unfortunately, whilst Amorosi's book succeeds in terms of quantity of data, it falls down badly on quality of presentation. The grisly printing quality which seems to have become *BAR's* stock-in-trade has rendered all but illegible many of the tables which Amorosi, perhaps unwisely, has presented as lineprinter output. The line illustrations vary in density, and fine lines have certainly been lost on some figures. That much, and the repetition in



the review copy of pages 159/60, can be blamed on production. The other shortcomings are attributable to the author. The text cannot have been copy-edited, unless in the pitchy dark, and typographical errors abound, as do those interesting changes of syntax which occur when words are electronically processed hither and yon around a text, ending up in the wrong place. More disturbing are the numerous academic and technical errors. To mention all of them would require too many pages, and would enter the realm of salt-rubbing. Briefly, chordate taxonomy is treated with scant regard. Ruminants and lagomorphs are described as classes of mammals, which presumably makes Mammalia a sub-phylum or something (p. 195). In discussing the distal attenuation of the ulna in equines, Amorosi equates the term "*Equus*" with species, not genus, when he possibly means to refer to the whole genus, not merely to one species (p. 226). Pigs are credited with a "complete set" of metacarpals (p. 235), when they have only four. The terminology is sometimes bewildering, as when bones are described as "cylindrical in cross-section" (p. 205 and elsewhere), or when we are told that on some femora "the greater trochanter can extend over the femoral head ..." (p. 257), which must inhibit joint articulation more than somewhat.

On the credit side, there are droves of references, many of them to works unfamiliar on this side of the Atlantic, and a bibliography of keyworks to the identification of major taxonomic groups. The bibliography mentions none of the "Munich bird bone theses", perhaps because they are unpublished, though there are some references to other unpublished material.

Can the volume be recommended? Up to a point, if only for the convenience of having so many sources summarised in one volume. It is a great pity, though, that laudable intent was not met with greater care and competence, and it would be disastrous if the existence of Amorosi's book inhibited the production of something similar but better.

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K. Griffin, R. H. Økland, A. K. G. Jones, H. Kenward, R. W. Lie and E. Schia (1988). *De Arkeologiske Utgravninger i Gamlebyen, Oslo. Bind 5: "Mindets Tomt"- "Søndre Felt". Animal bones, moss-, plant-, insect- and parasite remains.* (E. Schia, ed.) Øvre Ervik: Alvheim & Eide. ISBN 82-90359-37-3. 200 pages. 120 N.Kr. (about £11).

This is the 'environmental' volume on the medieval excavations at two sites in Oslo, Norway, presented in English. 'Gamlebyen' is the old town, and frequent fires created destruction layers that could be dated, thus simplifying the phasing of the archaeology.

Erik Schia (the archaeologist) wrote the chapter on the archaeological background which provides just that. Some of the main reports on the building remains and find groups have been published in volumes 1-3 of this series, and volume 4 is expected and may even have appeared.

Kerstin Griffin's botanical report is quite long and very satisfying. All the data are listed and there are eight pages of plates with good photos of the seeds so we can all see what they looked like. I find this most valuable since the subfossil appearance of many seeds is somewhat unlike the modern material, *Rhinanthus* losing its wing and Umbelliferae shedding layers, to give just two examples. It is also good to become visually familiar with the material from another site. Illustrations also improve the appearance of the whole book. The description takes one through all aspects of the interpretation very clearly. The discussion in connection with other Scandinavian finds is also very useful and readable. The complexities of understanding the various mixtures of rubbish found in medieval towns are well dealt with. The only things that Kerstin does not give are identification notes on critical taxa, as in the German style of doing things, which is very useful but also time-consuming.

Rune Økland writes about the mosses in a shorter report dealing with the remains from six moss-rich finds. It is interesting that all the mosses are still to be found in the Oslo area, while in polluted Britain taxa such as *Neckera complanata* seem to have retreated considerably towards the damp, clean West. Helge Høeg's pollen analysis results were not included in this

report, which is a pity, although they are quoted from publications. Some of the texts have been translated from Scandinavian, though to read them through one would never guess.

Harry Kenward's beetles bring a few more technical terms which non-English speakers would probably have difficulty in locating in their dictionaries, although indeed most of them are explained (such as 'obligate synanthropes') and the report passed the test of being intelligible to a non-beetle specialist (myself). He has a difficult job of presentation, since statistical analyses and terminology are by their very nature hard to express simply and clearly. The results are valuable, for it is important to have comparative beetle results from different places, and in the early medieval period particularly for comparing different parts of the former Viking empire, with Harry Kenward's own results from York and elsewhere, and now these results from Norway. He leans heavily on the evidence he has obtained from Britain, although when he wrote the report that was all that was available; other Scandinavian beetle results from Gothenburg in Sweden and Paul Buckland's results from Iceland and Greenland have since appeared. Fly puparia were just noted, not identified or discussed, but then their study is a specialism in its own right.

Andrew Jones' parasite report is particularly useful for the identification criteria that he gives to establish as far as possible the likely species of *Trichuris* present. He also provides evidence of the faecal content of the samples.

Rolf Lie's animal bone report makes sense to me and reads well, although of course I am not an expert in this field, so I cannot judge the state of the analyses. I think the 'Norwegian Pony' discussed is the Fjordhest, a very distinctive breed with upright mane, of which some examples were seen on the AEA Denmark Conference excursion, usually flashing by too quickly for Sue Stallibrass to photograph. 'Mons' (as a cat may be called in Norway in the way we use 'puss') was used for fur (according to the evidence for skinning), along with a range of other animals, but of rats and mice for his dinner there was no trace, only the vole *Arvicola terrestris* and some unspecified

rodent remains. There was quite a lot of seal, probably caught locally, and walrus from further afield. Rolf Lie also identified a good bird fauna, including a peacock, and eleven fish taxa. All these are discussed with the English vernacular names, even of breeds of dog, a feat of linguistics that puts us to shame. There is not very much comparison with other sites but, judging by the bibliography, not much was published in this field when the report was written.

A volume such as this inevitably invites comparison with the 'environmental' fascicules from York. The Norwegians in some ways had a much harder job putting all this work together, with half the contributors in York and half in Oslo, and changing publication plans. The reader never sees half the difficulties that must be overcome before a publication appears, but even without them this book represents a lot of work. In other ways, the compilers may have had a freer hand in what and how to publish than is the case with the York series. The CBA which publishes the latter seems to me to add an unnecessary layer of bureaucracy and delay to the whole difficult business of publication. The Norwegian book's A4 format may also be better for species lists and illustrations than the York fascicules' non-standard size, and it is especially useful to see so many of the data presented in the Oslo text where they should be. A further advantage Scandinavians have over us, I think, is that they have to think more carefully about English grammar and wording than we do, and therefore tend to write clearly. The book costs the same as a couple of beers in Norway, and is of noticeably better quality than the (always improving) BAR series, which makes it a good book indeed. It is curious that, when I offered copies of the book for sale at an archaeobotanical workgroup where people could look at the book (and make sure it really was all in English), it sold quickly but when I offered the book for sale unseen through the AEA Newsletter I had only a single reply. This book deserves to be better known.

To summarise: I think the good presentation of these results provides many useful lessons over and above the interest of the data. Wouldn't it be wonderful if this example encouraged the production of further such combined environmental

reports from other sites?

I would like to thank Kerstin Griffin for providing me with a copy of this book.

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**D. A. Hinton** (1990). *Archaeology, economy and society. England from the fifth to the fifteenth century*. London: Seaby. 245pp; about 50 figs. and plates. ISBN 1 85264 049 9. £14.95.

The realisation over the last few years that medieval England was really quite interesting has prompted a flurry of textbooks, of which David Hinton's study is one of the more synthetic. The author takes a resoundingly culture-historical approach, and moves through chronologically ordered chapters from the fall of Rome to the Renaissance. So why review the book for *Circaea*? Because Hinton draws on many lines of evidence, including studies of sediments, biota, and human remains, and thus provides an example of how bioarchaeological data can be used by 'cultural' archaeologists. Interpretations of

bone and botanical data are used somewhat uncritically in assessing diet, trade and husbandry, and the sensitive reader might at times become alarmed by Hinton's cavalier abandonment of the caution with which we are accustomed to hedge about our scientific prose. However, it is good to see the bioarchaeological evidence being used, not merely published, and being set in a wider context. For the non-specialist, discussions of the archaeological recognition of historical events and of the strengths and weaknesses of decorative metalwork typologies are useful, and the summarising of Metcalfe's work on sceattas almost makes numismatics interesting. There are the old flaws, not least the infuriating use of numbered notes for references, and Hinton has trouble with topography in remote northern areas, shifting Gauber High Pasture southwards into West Yorkshire (p. 68) and ceding Whitby to Northumberland (p. 57). Neither of these important sites, incidentally, merits an entry in the index. Gripes aside, this is a timely and readable book, which ought to provide a useful background text for anyone working on sediments or biota from medieval England.

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## Report on the International Workgroup for Palaeoethnobotany, 1989

Every three years there is a meeting of (mainly European) archaeobotanists. Looking back on the sweeping changes that are still taking place across Eastern Europe, last summer's conference in Czechoslovakia seems a long time ago.

But back to the conference. The *International Workgroup for Palaeoethnobotany* was invited by the Slovak Academy of Sciences (headed by Academician Chropovský), to hold the conference at their establishment at Nove Vozokany, in southern Czechoslovakia, in June 1989. Just sorting out visas, money, etc. proved a major research project, let alone doing anything at

the conference itself. The venue was a country house, set in beautiful grounds (none of your 1960s decaying concrete British universities!), which was the right size for a meeting of this size and made it easy to wander round and talk to people. This was a very well-organised and successful meeting, at which the organisers from the Slovak Academy of Science led by Dr Hajnalová and Dr Ambros did everything possible to ensure a good conference. There were probably many difficulties in getting things done that us westerners would simply fail to comprehend (like getting photocopies made), yet the whole thing went like

clockwork: lectures were on time, projectors worked, the organisers sorted things out in many languages, the food was good and above all the conference was inexpensive (I don't think any of the British participants got a conference grant).

The programme was quite a full one with about 40 papers delivered, and there were also poster sessions. The session headings were: general views, interpretation, bibliography, data on geographical regions or single taxa, Asia, Africa, methods, etc. I shall not attempt to summarise the wide range of work presented—however the papers and posters will be published in *Monumentae Archaeologicae* and I shall doubtless give details of the appearance of the publication, when available, through the AEA newsletter. Judging by the way they do things in Czechoslovakia, we should not have to wait too long. Some papers, Professor Behre's in particular, were masterpieces in the art of presenting a piece of research.

It was particularly good to meet a large contingent from Eastern Europe, of whom very few had been able to attend either of the last two Workgroup meetings, which were held in the Netherlands and Britain (or, indeed, Professor Körber-Grohne's meeting in Stuttgart) because of difficulties of getting passports and western money. Often it was a matter of putting a face to a well-known name. Most of the time was spent talking to people, which was very interesting, but occasionally acutely frustrating—when one could not communicate with an obviously interesting Russian colleague beyond her few words of English.

On a lighter note, a choir came and sang us songs, and there was a barbecue evening. Some of the Dutch contingent are keen on birds, and they spread word around that there were storks nesting nearby, so some of us walked into the village—past the rusting steel red star (probably on some scrap heap now) set in the verge to encourage travellers towards the glories of socialism—to the churchyard, from where we could see the birds on special nest sites set up in the state farm, while local people collected the lime blossom in the churchyard. The conference excursion took us to various sites of archaeological and botanical interest, the

latter including woodland with wild grape vines and forest steppe. Some people may have been surprised that I was enthusiastically photographing plants such as the thistle *Onopordon acanthium* that were common (to them) but scarcely seen in Britain. Prof. Willem van Zeist summed up the conference.

These international conferences provide a very important opportunity to find out there about the research going on around Europe, the ways of doing it, both from papers and posters given and from conversation, without the inevitable delays of publication. A great many research angles are being investigated by various people in different places, and without such communication, it is all too easy to fail to take in important results from elsewhere, especially when they are published abroad. Meeting people goes with exchanging offprints, which is very important when so much is published in relatively obscure local journals that are hard enough to trace in their country of origin, let alone from abroad. Still on this topic I would like to mention Jurgen Schultze-Motel's bibliography which covers the whole subject of archaeobotany and which is published in *Die Kulturpflanze* annually and sent out to those who send him publications. He very much depends upon people to send him offprints or photocopies of their work so that he can include it in the current year's crop, so as to speak. Archaeobotanical publications are hard to trace and obtain, over and above the difficulties of obtaining western literature in East Germany (for the time being). His address is: Dr J. Schultze-Motel, Zentralinstitut für Kulturpflanzenforschung, Correnstrasse 3, DDR 4325 Gatersleben, East Germany.

The next IWGP meeting will be organised by Helmut Kroll at Kiel, north-west Germany, in 1992. By then, it is to be hoped, almost all the archaeobotanists attending will be able to travel there freely on Common Market passports and the old alternation of meetings between East and West Europe will be an anachronism.

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## Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies

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### Summary

In order to practise environmental archaeology effectively, it is important to define the subject, and its limits and concepts. This paper represents an initial exploration into the field of definition, recognising two apparently distinct threads in the application of environmental archaeology. On the one hand, the European practice has a strong biological emphasis, whereas there is a more geographical approach applied in North America. The geographical links are not, however, uniquely North American, and are perceived from both sides of the boundary, especially amongst British geographers. Environmental Archaeology has relationships with Quaternary palaeoecology, archaeology, and other analytical disciplines. Using the relationship with Quaternary palaeoecology, it is possible to define a conceptual framework for environmental archaeology.

### Introduction

This paper explores some aspects of definition which arise where Environmental Archaeology is seen as a distinct discipline within scientific research, and revolves around several related questions arising during the practice of environmental archaeology: Is there such a field or discipline as Environmental Archaeology, and, if there, how is it defined and what are its limits? Following this, can some basic philosophy or conceptual model be defined for this discipline known as Environmental Archaeology?

These questions stem from some basic methodological problems. Practising environmental archaeologists often are asked, by archaeologists, for opinions regarding two main problems. Firstly, what can environmental archaeology tell the archaeologist about a site (or, in other words, although it is in vogue to consider the environment in relation to a site, is it really worthwhile?), and secondly, what on-site sampling procedures must the excavator undertake to provide useful environmental evidence? To answer these questions requires a clear view of the scope and limit of environmental archaeology, and this is where a clearly defined

conceptual framework of the subject may provide a useful background.

To assess the value of such a conceptual approach to the subject, and to approach answering such questions as those posed above, it is convenient to group the practical approaches to the application of environmental archaeology into three generalised categories. The first may be regarded as the unfunded or under-funded approach, in which environmental evidence has not been budgeted for, and, therefore, cannot be examined; end of problem. Next is the uninformed or misinformed approach, in which the archaeologist either feels he or she should collect environmental evidence, or genuinely recognises the need to do so, but plans in isolation, i.e. consults an environmental specialist too late. The result is commonplace: collection of too few, too many, inadequate or inappropriate samples, poor storage, lack of research problems and design, and so on. The environmentalist tends to act as a 'service' scientist, and the results tend to be of relatively little use and/or intellectually unexciting. Finally, there is the well-funded, planned approach, in which the environmental specialist is involved in most if not all the relevant planning stages. The excavation strategy includes specific environmental

components, where these are viewed as necessary, and the environmental output may be highly valuable in terms of site interpretation. These three approaches often overlap, and more often than not, are imposed by external restrictions such as the availability of specialist staff, structure of funding and so on.

In order to assess the potential value of environmental research in an archaeological investigation, i.e. whether, given optimal conditions, it is scientifically valid to indulge in environmental archaeology, it is necessary to define the subject. A full understanding of what environmental archaeology is, is critical, especially in the latter two situations, i.e. in the un- or misinformed approach, where it may be possible to amend this for future work, and to optimise the research, and in the last approach, where thorough planning and implementation of plans is critical, this requiring the full understanding of the nature and limits of environmental archaeology. Thus, returning to the two archaeological questions: 'what can environmental archaeology tell us?' and 'how do we sample?', it is apparent that only with full understanding of the scope and limit of environmental archaeology can these questions be answered fully.

There is, however, a basic practical problem. Since much, if not most environmental archaeology is externally constrained (usually by funding limitations), much of it tends to become 'service science', and the practitioners frequently are unable to develop their work as a discipline. Consequently, the theory and practice of environmental archaeology, are often, for practical reasons, some way apart.

### Definitions of environmental archaeology

A review of the literature provides a sense that there are several different views between, for example, Europe and North America, regarding the definition and scope of environmental archaeology.

In the British scene, environmental archaeology is defined by the Association of Environmental Archaeology as "the

general field of the application of the natural sciences to archaeology" (*Circaea*, vol. 1, p.2). This general statement may be extended, such as by Myra Shackley, as in "the application of the biological sciences (bioarchaeology) and earth sciences (geoarchaeology) ... to extend the scope of archaeological research ..." (Shackley 1981, vii). This separation is interesting, since, by the time (1984) that Helen Keeley edited a major review of environmental archaeology in England, the definition stands as: "... the application of the natural sciences to the better understanding of the material remains of man's past, by the analysis of *biological remains and organic deposits* from archaeological excavations" (Keeley 1984, 12; emphasis inserted). This emphasis upon the biological evidence reflects the practical direction in which British and European Environmental Archaeology has developed, which in itself probably reflects the very strong growth of palaeoecology, especially botanical palaeoecology, in this region over the last half century. What is the purpose of this application of natural sciences to archaeology? Again there are several views, but generally they all indicate an awareness that ancient societies did not live in isolation, and therefore if we are to understand, as fully as possible, the nature of these societies, we need to know about the environment in which they operated and their interactions with that environment. The ideal of environmental archaeology is to produce that understanding. Following this is a view, which does not get wide exposure in Britain, that environmental archaeology should be the means to reconstructing past geographies.

Karl Butzer makes some strong comments upon the definition of environmental archaeology, comments reflecting a different attitude to the British school of thought or, at least, practice. It is clear from Butzer's writings that he takes a somewhat broader view of the subject than is largely practised in Britain, and he argues for the field to be called "prehistoric geography", a name which is unlikely to catch on in Britain, if only because the practising environmentalists are mainly botanists, zoologists and other life scientists. In Butzer's foreword to the 2nd edition of *Environment and Archaeology: an ecological approach to prehistory* (1971), he states his case thus:



"Contemporary human groups interact with their environment in many ways and at different levels, depending on their technology and organizational skills. The regional environment provides a resource base that may also be relevant to the development of individual economic traits, primary technology, subsistence patterns, and even social structure. Similarly, on a more local scale, the habitat provides settlement sites and the focus for human activities. Conversely, man leaves his imprint upon the local setting and even on the regional environment. Food-gatherers modify vegetation by fire and accidental dispersal of plants; they exploit and may over-exploit food resources, and they impart their mark upon the land. Food-producers leave a far more conspicuous record. Their structures are common and of some permanence. They clear or destroy forest, displace wild game with domesticated animals or mass crops, deplete or destroy the soil mantle, modify or upset the hydrological balance, and initiate the process of pollution that threatens us today.

"Prehistoric groups interacted with their environments in similar ways. No single discipline can hope to unravel the story in its entirety, but many can contribute. Palaeoecology is essentially an approach based on various classes of data. In searching for an understanding of prehistoric groups, their settlements and their way of life, we must not let data-gathering obscure understanding. Instead our work must be more problem-oriented. Each discipline must contribute directly to ecological synthesis, and the archaeologist must concentrate on deriving more meaningful results from his own data, rather than attempt to integrate all categories of evidence. More explicitly, I would hope for archaeologists who can think as geographers, who can derive more social and economic inference from their material and who can interact with other scientists also interested in prehistoric man."

Butzer writes a great deal more on this subject, and one could do a lot worse than continue quoting from his writings. Suffice to say that he views study of physical

environment to be as important as that of the biological environment in relation to archaeology. This may reflect the greater requirement in North America to understand the geological stratification of archaeological sites than has to some extent, occurred in Britain, although this may also reflect differences in the historical development of the subject in different places (Gifford and Rapp 1985).

Butzer makes the following comments about the practice of "prehistoric geography" (1971, 4-5; emphasis added):

"In view of the many disciplines concerned with the past, there are many different approaches to geography. Basically these approaches are of three kinds:

"(a) Individual research by the natural sciences, usually carried out independently in the field or laboratory by *geologists, geographers, soil scientists, botanists, zoologists, and meteorologists*. Although the range of specific goals or interests may vary greatly, most of our basic techniques and palaeo-environmental data have been obtained in this way.

"(b) Interdisciplinary work by natural scientists in collaboration with archaeologists, particularly in the field. Pleistocene geology, geomorphology, palaeontology, and pollen analysis probably form the most common backgrounds of the individuals concerned. Generally directed toward the study of archaeological sites, such interdisciplinary work is particularly valuable in that it contributes ecological as well as environmental information.

"(c) Palaeo-anthropological work by archaeologists directed toward a fuller understanding of the cultural ecology ... of prehistoric communities particularly of the economy, social organization, and interactions with the environment."

Approach (a) largely reflects environmental archaeology as it stands in many studies, whereas approach (b) is an ideal which is relatively rarely achieved in environmental archaeology. The last approach is probably beyond the scope of the individual environmental archaeologist, although,

environmental archaeology as a discipline has a major potential, and often realised, input to such analysis.

Although Butzer's ideas have been quoted quite extensively above, this must not give the impression that this "prehistoric geography" is a uniquely American idea. Although environmental archaeology in Britain tends to be more of a biological activity, British geography nevertheless has "prehistoric geography", if not by that name but at least in spirit, very much at its core. A recent Institute of British Geographers (IBG) Study Group report (Daniels *et al.* 1987), for example, recognises a flood of research in British Geography including the reconstruction of palaeoenvironments as "a major research focus within physical geography, embracing geomorphology, biogeography, climatology [and] historical geography". However, the interaction between this type of geographical study and prehistorical research appears to be relatively unimportant. The IBG study comments, for example, that:

"...the reconstruction of the physical landscape is also an important element within prehistoric, Roman and early medieval historical geography. In these fields, there appear to be lacunae at present with much of the work currently being done by archaeologists." (Daniels *et al.* 1987, 56).

Despite this, over a decade ago (1976) Andrew Goudie discussed the relationship between geography and prehistory, drawing several strands together which strongly support Butzer's case for a "prehistoric geography". Goudie argued that there are strong links between the study of geography and the study of prehistory, at the personal level (many geographers are prehistorians and *vice versa*), at the philosophical level and at the methodological level. Goudie, in particular, draws five main unifying themes between the two disciplines: (1) both disciplines study distributions and locations; (2) both disciplines study environmental influences and ecosystems; (3) both disciplines aim at the reconstruction of past environments; (4) both disciplines examine the role of the human as an agent of ecological change; (5) both disciplines are concerned with landscape development. In practice, these

links appear, at present to be strongest between archaeology and the 'physical' side of geography rather than the 'human' side of geography (Gamble 1987).

So, given these various views of prehistoric research (so to speak), where have we got to? Firstly, environmental archaeology, as practised in Britain, appears to concentrate upon the biological evidence which may provide insight into aspects of the prehistoric environment. Secondly, there is an American view which suggests that science has the means to investigate "prehistoric geographies", this being an approach which may not be too distinctly removed from, at least, British geography.

### Relationships between environmental archaeology and other disciplines

In looking for a definition for the field of environmental archaeology, several references have already been made of the interdisciplinary nature of environmental archaeology. Turning, therefore, to the relationship between environmental archaeology and the other fields with which environmental archaeological research may be linked, it may be possible to improve upon the definition of the subject by looking at its limits and its links with related fields. There are three major relationships between environmental archaeology and other subjects: (i) the role of environmental archaeology in relation to Quaternary palaeoecology and palaeoenvironmental study, (ii) the role of environmental archaeology in relation to archaeology, and (iii) the interdisciplinary links with other analytical disciplines. Following these, it may also be possible to approach the possibility of defining some basic philosophy or conceptual model behind environmental archaeology.

### The relationship between environmental archaeology and Quaternary palaeoenvironmental study

In the first case, environmental archaeology may be seen, for several reasons, as a subject within the general field of Quaternary palaeoenvironmental study. By

definition, the materials and evidence used in environmental archaeological research all represent the Quaternary Period. In terms of the applicability of general palaeo-environmental study to archaeological problems, it should be noted that most regional environmental research tends to be non-archaeological or only contributing incidentally to archaeological research, although there are, of course, the exceptions of specifically archaeological regional studies. However, some site-specific studies, depending upon the nature of the site, may be explicitly archaeological and it is probably in this area that most environmental archaeology is practised.

### The relationship between environmental archaeology and archaeology

Since environmental archaeology is, by definition, intricately linked with archaeology, it is important to look at the relationships between these two fields. In this case, there are two basic positions that environmental archaeological research may take: (i) internally related to site- or region-specific archaeology, and (ii) externally related to site- or region-specific archaeological research. The first, the internal relationship, is probably the one most practised. In such a situation, environmental archaeology inputs into the description, identification and cataloguing of one or more of the three broad classes of archaeological evidence—structural, artefactual and 'ecofactual'. This evidence is analysed and interpreted by the environmental archaeologist, who may also, but less frequently, be involved at final analytical stages, that is, the site interpretations etc. The second, external relationship expresses itself at these latter stages, where archaeological, non-archaeological, environmental and/or non-environmental evidence, from various sources, input into the archaeological site or region integration. At this stage, one returns to Butzer's ideas, in particular, his third main "prehistoric practice", namely the search for a "fuller understanding of the cultural ecology ... of prehistoric communities—particularly of the economy, social organisation, and interactions with the environment" (Butzer 1971, p. 5). Although some archaeologists have been doing this for a long time, especially for

the pre-agricultural revolution eras, it is only relatively recently that British environmental archaeologists have developed this field. The bulk of environmental archaeology has been conducted within the first framework, i.e. internally, often being practised merely as a service science rather than as a discipline in its own right.

### Relationships with other disciplines

In reality, the practice of environmental archaeology is closely inter-related with many other fields of research, such as human biology, zoology, botany, agricultural science, geology, pedology and many others. The link between each of these and environmental archaeology tends to be a two-way, with a flow of methods, concepts and accepted wisdom towards environmental archaeology, where these are applied and then used to derive a set of information (and, in some cases, new techniques) which feeds back into the non-archaeological discipline. The outputs from environmental archaeology provide both historical and geographical information, and it should be noted that there is this specifically geographical component; the output from environmental archaeology increases understanding of past distributions, since most environmental archaeology research is site-specific. This last point returns to Goudie's first geography/prehistory link.

### Basic philosophy or conceptual model

In 1980, John and Hilary Birks discussed the philosophy of palaeoecology, drawing parallels and distinctions with ecology on the one hand, and geology on the other. Their comments are particularly relevant to environmental archaeology, especially if environmental archaeology is seen to be closely related to, if not a subset of, Quaternary palaeoecology. Birks and Birks (1980, 6–8) outline seven major features, a list which can be slightly amended and added to, to form a possible philosophical framework for environmental archaeology.

1. Environmental archaeology is a descriptive historical science largely depending upon inductive inferences

and reasoning, and often depending upon a multidisciplinary approach.

2. The analytical methodology is opportunistic, reflecting the wide range of types of evidence; experimental investigation is largely restricted to methodological problem solving (e.g. identification techniques etc.), and is not central to data interpretation.

3. The interpretive methodology should entail the method of multiple working hypotheses.

4. Explanation must, logically, embrace the simplest explanation until more evidence is available.

5. Although the main aim of environmental archaeology is not to study taxonomy and processes *per se*, a full understanding of these is necessary, therefore much work often goes into investigating these.

6. Concepts of space, time and evolution are of central importance.

7. The language is mainly, by necessity, that of biology and geology.

8. The data collected are both qualitative and quantitative, invariably complex, incomplete and multivariate, being, in many cases, ideally suited to multivariate mathematical analysis.

9. Uniformitarianism ('The present is the key to the past') provides the basic assumption and philosophical tenant, and, as best working hypotheses, present taxonomy, processes, etc. must be used to explain past situations.

## Conclusions

After these considerations, it is possible to answer the questions presented at the outset. Firstly, what is environmental archaeology? There appears to be no one standard definition, and since this is so, the present author may as well contribute another: Environmental archaeology is the study of the material (usually non-artefactual) evidence which contributes to the understanding of past environments in relation to past human activity, with

particular emphasis on the interaction between social and natural environmental systems. As a single phrase, Butzer's "pre-historic geography" is a most useful definition. Secondly, is there such a field or discipline as environmental archaeology? The answer must be yes, although it appears to straddle, with a common aim such as that defined above, various other fields, notably archaeology, palaeoecology and other palaeoenvironmental disciplines. Thirdly, given that there does appear to be a discipline of environmental archaeology, what are its limits? These are probably undefined as yet, but perhaps best described by its links with other disciplines, that is, environmental archaeology as a subset of palaeoecology, having internal and external links with archaeology, and having input/output links with many other disciplines. Finally, can some philosophical or conceptual model be defined for the discipline of environmental archaeology? This is, in the author's opinion, possible, and the definition and the nine conceptual points outlined above offer a step towards the establishment of a philosophy for this discipline.

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## Cultural landscapes: some thoughts stimulated by Bill Boyd's paper: *Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies*

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Bill Boyd highlights the need for critical reassessment of the objectives of environmental archaeology but some of the points he makes need to be challenged and some need to be taken further. He is right to suggest that because so many of our practitioners in Britain have been employed to report on biological evidence from specific excavations there has been some neglect of broader philosophical horizons including the relationship with our close relative geography and, I would go so far as to suggest, cultural aspects of archaeology. Another factor is that some archaeologists are keener to emphasize their distinctness rather than to embark on the long overdue task of breaking down barriers between disciplines which, like so much in Britain, are all too often fossilisations of Victorian and earlier academic structures.

The paper implies that in Britain little work has been done on the reconstruction of past landscapes. Whilst I accept that in the past too much emphasis has been given by funding bodies to individual site based research and service work, often on a small scale, the comments made do seriously underestimate over twenty years work on the past trajectory of present day landscape types. The success stories seem to have been forgotten: work on Dartmoor, the Somerset Levels, Evan's work on the chalk or Robinson's in the Thames Valley. These and many other pertinent examples are conveniently summarized by Jones (1986) who demonstrates the landscape emphasis of much British environmental archaeology and its links to the work of Hoskins which

led to the development in Britain of the sub-discipline of landscape archaeology. Even so, many of our most successful practitioners have come to archaeology from a background in botany and zoology so the research emphasis of the 'palaeo' branches of these disciplines have tended to predominate in our thinking, and critical thought about the cultural aspect of the equation has to some extent taken a secondary position. This is perhaps suggested by Boyd's statement that "environmental archaeology is seen as closely related to, if not a subset of Quaternary palaeoecology." The problem is not a new one, over a decade ago Butzer (1975) noted that collaborating scientists all too often do not identify with the overriding goals of such projects, the study of man's past. This means that some theoretical archaeologists are increasingly criticizing our work as lacking in 'relevance'. We do not of course need to justify our existence purely in terms of the criteria selected by 'big brother' archaeologists but it should encourage us to look more critically at our own academic objectives. This means abandoning the role of industrious technicians presenting objective 'facts' in the service of cultural archaeology. It is worth quoting Adams (1988) on the subject of facts which, he says are "now understood as compelling interpretative statements reached by comparing the results of more-or-less precise measurements undertaken within a theoretical scheme". The point is, as Butzer (1982) and Brandt and Van der Leeuw (1987) have emphasised, human activity is not constrained by the 'factual' landscape

which can be reconstructed with more-or-less reliability from biota and sediments but by the perceived landscape of contemporary lovers. Thus if we fail to give due emphasis to the cultural perspective the whole study lacks meaning. A subsidiary implication is that, as Boyd suggests, we need to involve ourselves more, not just in data gathering, but in project formulation and interpretation.

Emphasis is given in the paper to the views of Butzer (1971) and the argument that our central task should be the reconstruction of past geographies. Boyd does not spell out in a precise way what this means or why it should be accorded such emphasis. A curious omission is mention of Butzer (1982) which considerably enlarges on his earlier ecological emphasis. A useful definition of environmental archaeology is the scientific study of the ecological relationships of past human communities. This emphasises the ecological, i.e. interdisciplinary, dimension more clearly than the idea of 'prehistoric geography'. The latter sounds rather like the backcloth to human activity and is redolent of earlier deterministic views of man-environment relationships.

A central task is focusing on the interactions between people and the environment. To some extent, however, the cultural aspects of landscape are not perceived as a major interest of ours. The recent volume *Landscape and Culture* (Wagstaff 1987) integrates geographical and archaeological perspectives in a highly stimulating way. Its emphasis is very much on the contribution of recent developments in humanistic geography which is surprising in view of Boyd's valid point that hitherto archaeology has had the strongest links with the 'physical' side of geography. Even more surprising is the absence of any chapter on environmental archaeology. A key concept which we need to develop and articulate, is that of the cultural landscape, recently given welcome

emphasis by the publication of a conference in Bergen (Birks *et al.* 1988). The study of cultural landscapes necessarily involves the close integration of social and natural sciences so that we can achieve a fuller understanding of their origin and development and the interaction of humanity with environment.

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## Comments on Boyd: *Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies*

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I wonder whether chemists are as worried about the definition of chemistry, and if not why not. They share all sorts of fuzzy boundaries with, for example, physics and biology, yet I'm not sure that this impedes their quest for a better understanding of the nature of matter.

It is my suspicion that environmental archaeologists actually enjoy and flourish along the fuzzy borderlines between more august disciplines, which is why we chose to inhabit them in the first place. So where is sharpening the outline going to get us?

Stamping out the boundaries has of course to do with politics (with a small p) rather than substance. As Boyd comments, the issue is whether environmental archaeology is practised "merely as a service science rather than as a discipline in its own right". From that distinction follows differences in career structure, academic power, research resources and so on. I certainly concur with Boyd's sentiments that, if we consider our intellectual pursuits to be of worth, then we should pay heed to the academic politics of those pursuits. As a foundation to this, Boyd has presented us with a nine point conceptual model.

In presenting his model for environmental archaeology, the insecurity is displayed of a subject that is in a quandary about its own definition. The model presented is, by Boyd's own admission, only slightly amended from one constructed for quaternary palaeoecology. If a subject is to exist in its own right, then its prescriptive statement should not be "slightly amended" from anything. Indeed, the list presented of "major features ... to form a philosophical framework", is so general that it could be slightly amended to apply to any of a whole number of subjects. It says nothing about what environmental archaeologists are trying to do and why.

The only contentious feature in this otherwise bland list is the first. There is in reality no consensus among environmental archaeologists as to the appropriate place for inductive reasoning in our methodology. My personal opinion is that something defined as a "descriptive historical science largely depending upon inductive inferences and reasoning" belongs in any case to some century other than our own. Moreover, I feel this cautious "opportunistic inductivism" is the very thing that fosters an image of environmental archaeology as a rather pedestrian Dr Watson to cultural archaeology's Sherlock Holmes.

Our strength as an academic discipline lies in the questions we can answer, not the structure within which we answer them. If those questions are deemed significant by our academic peers, our subject will prosper; if not, no amount of redefinition and prescriptive statement will save us.

The quotes from Karl Butzer are quite enough to put us on the right track. There are some human groups who have lived independently of pottery, architecture, civilisation, even of post-holes, but all have belonged to the global ecosystem and, try as they may, have been unable to exist, change or develop independently of it. Every transition in the human past has had an ecological dimension, and environmental archaeologists have become steadily better equipped to examine that dimension.

In some areas we have been over-reticent about addressing the broader questions surrounding, in Boyd's words "the interaction between social and natural environmental systems". This is particularly true in the studies of the more recent past with which rescue and contract archaeology has been largely concerned. The site-specific empiricism which has so often characterised such work, and to which Boyd's paper repeatedly alludes, is only

too familiar to many of us. It constrains archaeologists of all descriptions, not just ourselves.

It is vital that we stand above such narrow restraints and follow the example set in palaeolithic and mesolithic studies, in which environmental archaeology requires no justification. We should grasp the nettle firmly, and set about rephrasing and tackling, in ecological terms, key questions on such topics as urbanisation, imperialism, migration and warfare.

Bill Boyd has done us a service by asking us to examine what we are doing, always a salutary task. I think if we look, we can find environmental archaeology's soul, not

in the rules that we follow or the approach that we take, but instead in the destination to which we aspire. Boyd certainly alludes to the destination that I would propose, which is to discover how the social complexity that is a hallmark of our species has allowed us to change our relationship with the global ecosystem of which we are a part. Not only do we have one of the best toolkits for studying the human past, we also have a great deal of support within the wider world of archaeology, which would be keen to see us take a stronger lead than we have hitherto taken.

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## What's in a name? Anyone can be an environmental archaeologist

**K. D. Thomas**, *Department of Human Environment, Institute of Archaeology, University College London, 31-34 Gordon Square, London WC1H 0PY, U.K.*

Bill Boyd raises a number of important questions about the nature of environmental archaeology which should be discussed. However, because both Boyd in his paper and I in this comment cite approvingly from the published work of Karl Butzer, the editors of *Circaea* might feel it appropriate to feed both of our contributions to the shredder and go direct to Professor Butzer for a 'definitive' statement!

Boyd's analysis of the development of environmental archaeology in Britain and North America is marred by highly selective use of the literature. His alleged dichotomy between a 'geographical' tradition in North America and a 'biological' one in Britain is, in reality, either very blurred or non-existent. Environmental archaeology, and archaeology in general, in Britain has made extensive use of concepts from geography: site-catchment analysis (Vita-Finzi and Higgs 1970), landscape analysis (Vita-Finzi 1978; Wagstaff 1987), spatial analysis (Hodder and Orton 1976) and geoarchaeology in general (Davidson and

Shackley 1976), are examples in which British scholars have set the pace. Zeuner's pioneering work, undertaken at this Institute, on the application of geological and geographical concepts and data to archaeological chronology (*Dating the Past*, 1946), laid the foundations for modern geoarchaeology. Ian Cornwall (1958), also working at this Institute, published a pioneering volume on soil science for the archaeologist; this has now been superseded by a book written by Susan Limbrey (1975), another British environmental archaeologist. Equally, one knows of many North American environmental archaeologists who are highly original and pioneering archaeobotanists and zooarchaeologists and who do not use geographical models or concepts in their work. These observations do not, of course, detract from the importance of the other points which Boyd makes.

Boyd clearly approves of the ideas of his mentor, Karl Butzer, whose book *Environment and Archaeology* (1971, 2nd edition) he quotes at some length. I have

two observations to make here: first, it is interesting to note the change in the subtitle of Butzer's book between the first (1964) and second editions, from *An Introduction to Pleistocene Geography* to *An Ecological Approach to Prehistory* (I will return to this subtle distinction later); second, Butzer has written a number of thoughtful works on the nature of environmental archaeology since 1971 (e.g. Butzer 1975, 1978 and, in particular, 1982). This last work, a book entitled *Archaeology as Human Ecology*, encapsulates for me what environmental archaeology should be concerned with and much of what I say below derives directly or indirectly from Butzer's stimulating ideas.

And so on to the substance of Boyd's paper. He provides a useful analysis of the research and funding structure of projects which involve environmental archaeologists; I feel that most workers would agree with this. I think, however, that a further breakdown of environmental archaeology would be useful: into the 'types' of practising environmental archaeologists. In Britain there are three main categories of people who could be classified as environmental archaeologists:

1. Those employed by universities or other educational institutions to teach and research in the area of environmental archaeology.
2. Those employed by national or local archaeological bodies to work as environmental archaeologist attached to local archaeological units, museums, universities or central environmental laboratories.
3. Those employed by universities, other educational institutions or museums to teach and/or research in areas not directly concerned with environmental archaeology (geography, geology, botany, zoology, etc.) but who generate research data which is relevant to environmental archaeology and who might, therefore, choose to designate themselves "environmental archaeologists".

This rather simplified system has probably omitted various other categories of workers, to whom I apologise. It does, however, include the majority of workers in environmental archaeology in Britain and, with only slight modification, would

probably apply to many other countries as well. This scheme is helpful for understanding why the Association for Environmental Archaeology (the AEA) was established in Britain some 10 years ago. Various environmental archaeologists, in particular Don Brothwell and Geoffrey Dimbleby (both of whom belong in category 1, above), were concerned that the subject should develop in such a way that all practitioners were involved (i.e. those in categories 1, 2 and 3) and that a forum for communication should be established. It was felt that workers in categories 1 and 3 were not always aware of the problems faced by the (mainly contract) workers in category 2, or of the important data which they were generating. Equally, workers in category 2, often highly specialised in terms of the materials which they studied, were considered vulnerable to isolation from broader (e.g. conceptual) developments in the subject and that this could limit both their development and the value of their contributions; this is why the Association has been especially concerned to recruit them. Most of the workers employed under category 2 are, inevitably, biologists. I say "inevitably" because most sites in Britain produce large quantities of bioarchaeological materials and funding bodies are usually prepared to employ specialists to work on them. Those environmental archaeologists concerned with non-biological materials are mainly employed under categories 1 and 3, or in central laboratories in category 2. Given this structural background, it is not surprising that Helen Keeley's (1984; 1987) reviews of regional archaeology in Britain, based largely on the work of the local and central units, should focus on the biological or organic materials. Boyd should not take this as confirming his view that environmental archaeologists in Britain think of their subject only, or even mainly, in biological terms (Helen Keeley is a pedologist, by the way!).

What are we to make of Boyd's approval of the term "prehistoric geography" as a definition of (or even a replacement name for) environmental archaeology? As far as *this* British biologist is concerned, Bill Boyd is quite correct in thinking that it is a name unlikely to find favour over here. My prejudice is not because the name fails to include the word "biological" but because it is a hybrid of the too specific and the too

vague. "Prehistoric" would exclude the many people who work as environmental archaeologists in the historical periods which, in some parts of the world, "began" more than 3,000 years ago. In North America there is a flourishing field of study called "Historical Archaeology" (which involves environmental archaeologists) and even post-bicentennial Australia will soon discover, if it has not done so already, that it too has an historical archaeology.

"Geography", on the other hand, is too vague a term. In the 1960s and 1970s, geographers consumed an enormous amount of time, energy and paper discussing the nature of their subject. The upshot of this contemplation of the geographers' communal navel was that no-one could agree where it was located, what it looked like and what it signified. My own impression of the nature of Geography, gained from an admittedly superficial examination of only a few university departments, is that "Geography is what Geography does" and that many scholars currently located in departments quaintly named "Geography" (one of those old-fashioned, 19th century, disciplines) would be equally at home in departments labelled "Biology", "Geology", "Economics", "Politics", etc. Geography has evolved into a thriving and intellectually-exciting multidisciplinary subject-area but I see no merit in *naming* our own subject in relation to such a chimera because it would not clarify in any way what environmental archaeology is, or should be, about.

This is not to deny that geography and environmental archaeology have many methodologies and ends in common, or that some workers located in departments of geography could not legitimately call themselves environmental archaeologists. The search for academic boundaries between so-called disciplines, which Boyd seems to think important for defining environmental archaeology, is surely a waste of effort. The traditional limits between disciplines are increasingly being seen by scholars as irrelevant (historical accidents, to some extent); it is research which straddles the borders of the old-fashioned "subjects" which is most exciting.

In my opinion, the term "environmental archaeology" is, itself, too exclusive in its

implications for many workers. The implied focus on the external physical or biological environment may not be disturbing to geographers or ecological biologists, but how do palaeodemographers, palaeopathologists and students of animal and plant domestication feel about it? It would indeed be a pity if, in our desire to arrive at some philosophically-acceptable (and tidy) definition of environmental archaeology, we excluded many of those practitioners who consider themselves to be environmental archaeologists in the broad sense. We must look for common areas of interest which will be *inclusive* of all relevant specialisms. How can we best achieve this? In my view, by focusing attention on the *objectives* of environmental archaeology (something which is conspicuously absent from Boyd's analysis).

How does Boyd define environmental archaeology? As "the study of (1) the material (usually non-artefactual) evidence which contributes to the understanding of (2) past environments in relation to (3) past human activity..." (numbers in parentheses are my additions). Here, the material evidence comes first, then the environment and finally human beings. This does not necessarily represent Boyd's priorities, but it is interesting to see that people come last. Surely, in any subject which claims to be archaeological, people are the focus of study (this is what distinguishes much of Quaternary palaeoecology, for example, from some aspects of environmental archaeology)?

How would I 'define' environmental archaeology (without imposing a too rigid framework)? As the human ecology of the past; seeking to understand the relationships between past human populations and their environments. The term "environment" is used here in its broadest sense, to include physical, biological and socio-economic aspects. Environmental archaeologists should use any relevant source of evidence (this will most commonly be physical evidence of a geological, geographical or biological nature) including, unlike Boyd, artifacts (archaeological sites are artifacts, and we can hardly ignore them!), ethnographic (especially ethnoecological) data and data derived from experimentation. I would not disagree with Boyd that John and Hilary Birks' philosophy of palaeoecology could

usefully be modified to form a *methodological* philosophy for environmental archaeology; ours is also a historical science. Where our subject diverges from palaeoecology is in its objectives: we don't merely reconstruct the 'diorama backdrop' against which human prehistory and history has been enacted, we attempt to reconstruct the dialogue between peoples and their environments. This is why the academic department in which I have the privilege to work is designated "Human Environment" and not "Environmental Archaeology".

Environmental archaeologists (in the sense which I outline above) should not only adopt the theory and philosophy of Quaternary Palaeoecology for their subject; the theory and philosophy of Archaeology is also vitally important. Environmental archaeologists working with on-site evidence will be aware that it has been transformed by both natural and cultural processes and that both must be understood before any valid reconstruction can be attempted. A discussion of the theoretical basis of Archaeology is beyond the scope of this commentary, but the recent article by Schiffer (1988) provides much food for thought for environmental archaeologists. Schiffer notes the problems of using the term "reconstruction" in relation to any aspect of Archaeology: we are never able to achieve a total reconstruction from the evidence preserved in the archaeological record. This limitation also applies to environmental archaeology, especially when ambitious reconstructions are attempted using one-sided data. An interesting example of this can be seen in the recent attempt by Shackleton and Van Andel (1986) to reconstruct changing patterns of availability (for human exploitation) of species of marine molluscs on early Post-glacial shorelines of the eastern Mediterranean using palaeogeographical data on past coastal morphologies. The hazards of this approach will be manifest to most ecologists and they have been discussed in detail elsewhere (Thomas 1987). Such examples argue for greater dialogue, and increased cooperation, between environmental archaeologists, something which the AEA was established to promote and which will occur only if we take a broader view of what environmental archaeology is, or should be.

Bill Boyd's article raises a number of issues which I am sure will be keenly debated by people who call themselves "environmental archaeologists". The 10th anniversary conference of the Association for Environmental Archaeology is being held at the Institute of Archaeology, University College London, from 30 June to 3 July 1989\*. There will be a whole-day session, provisionally entitled "Aims and achievements in environmental archaeology and closely related disciplines: convergence or divergence?", in which it will be possible to have friendly wrangles about the methods and objectives of our subject; these discussions will, no doubt, continue in the local hostelry in the evening!

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## Commentary on Boyd's *Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies*

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Dr Boyd's paper exploring the conceptual framework of Environmental Archaeology is a welcome contribution to the discussion of the aims of the discipline, especially in the light of one of the themes of the tenth anniversary Symposium of the Association for Environmental Archaeology next year\*. The issues raised in this article will undoubtedly be discussed in detail during the Symposium of the Association next Summer. Dr Boyd presents his contribution as an initial exploration into the field of definition by lengthy quotations from published work, though offering few new ideas.

The author, as a geographer, discusses and contrasts a European approach to the discipline (a biological emphasis) with a North American one (a geographical emphasis), while exclusively quoting *British* and *American* authors. This is a rather unfortunate use of the word 'European', especially in the light of recent developments towards a Unified Europe. In fact, the author may find that the so-called

prehistoric geography approach is more common in Europe than he realises—see, for example, the article by Waterbolk in *World Archaeology*, 1981.

In this short commentary there is no space for a lengthy discussion of the theoretical issues raised in the article, but I will say that I disagree with the author's view that Environmental Archaeology is either a discipline in its own right or a subset of Palaeoecology. The problems which sometimes beset the discipline (described by the author as the "under-funded and misinformed approaches" and the tendency to become a "service science") are likely to have arisen out of the separation of Environmental Archaeology from its core discipline, i.e. Archaeology. I personally see Environmental Archaeology as part and parcel of Archaeology, which I regard to be part of the historical sciences, rather than either the natural or social sciences. These issues will, no doubt, be discussed again and again.



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## Reply to Bell, Jones, Thomas and Van der Veen

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Where do I start? In the style of most "replies to response to ..." I will first say some suitably polite things, in particular a "thank you" to the respondents for your comments. However, unlike many other replies, I do not intend now to replace politeness with criticism of the respondents. The ideas that I have floated in my paper are, after all, speculative and exploratory, and are certainly not dogmatic assertions to which I or anyone else should necessarily adhere, and I consider that their greatest value is in the responses which they have elicited from Bell, Jones, Thomas and Van der Veen. When I received the commentaries on my paper, the covering letter from the editors of *Circaea* contained words to the effect of "...we enclose all the responses to your paper...over to you now—enjoy the challenge!". I will admit that my first reaction was "What challenge?". In many respects, the respondents have risen to the challenge (for want of a better word) that my paper offers, but rather than thinking in terms of challenges, I prefer Thomas's suggestion of "friendly wrangles". The importance of a discussion such as this is that it is a discussion, and a friendly one at that, and that we as environmental archaeologists (whatever we are) do not reach the levels of disagreement which is often encountered in "debate" in other fields of academic endeavour.

My initial reading of the responses invoked two reactions. The first was the eminently modest reaction that was I not being presumptuous in raising this issue when there are others better qualified than I am to do so? The second reaction was a slight sense of smugness and a definite sense of relief that my raising of these issues has not been in vain. The responses and their

contents justify my attempts at conceptual navel gazing (I use the term advisedly), and I have no doubt that the comments, both regarding ideas and facts, made by the four authors will be of interest and use to most, if not all, readers of *Circaea*.

So with the backpatting out of the way, let us look at the content of the responses. In general, I have little difficulty with most of the points raised in the responses, and indeed concur with many of them. In particular, my evidence does range rather narrowly, and I admit there are many examples which could have been cited; it is easier, after all, to make bold assertions with little evidence. However, whether my bold assertion is correct or not is largely immaterial; in this case it acts as a stimulant or hypothesis, and in that respect has served its purpose by extracting the responses of Bell, Jones, Thomas and Van der Veen. These respondents have raised many interesting points, some of a detailed nature and others of a broader nature, and in doing so, have provided a list of useful and relevant references to which I or any other environmental archaeologist can add. Although it is tempting to respond to these points in detail, or cite further references to argue the case one way or another, I do not intend to do so, since I think it is probably more useful at this stage to comment on some of the general overriding themes evident in the responses to my paper.

Firstly, it is clear that environmental archaeology is a diverse subject. Such a view is, of course, neither novel nor contentious. This diversity is one of the major strengths of environmental archaeology, and is certainly appealing to the practitioners who may or may not call

themselves environmental archaeologists. In this respect, whether one prefers Butzer's terms "prehistoric geography" or "human ecology" as summary descriptors of the subject, both of these phrases emphasise the powerful and exciting theme of the unifying, wide-ranging and multi-disciplinary approach to understanding past human-environment interaction. However, I would still argue that this diversity tends to be only vaguely defined. This lack of definition, furthermore, may place environmental archaeologists at a disadvantage against, for example, traditional archaeologists, many of whom may be unclear about what environmental archaeology can contribute to archaeological investigations.

The second apparent theme is that there is a perceived need for a discussion regarding the nature of environmental archaeology. All the respondents agree on this, and the convening of a symposium session at the AEA 10th Anniversary Meeting in 1989\* seems to confirm this general view. Different environmental archaeologists will see different reasons for this need, and most would, I suspect, concur with Thomas's reluctance to indulge in "navel gazing", but preferred that we should examine the whole, and not the hole, of the body! However, I still consider that it is important to know what we are doing, as environmental archaeologists, in order to undertake environmental archaeology investigations effectively. In this respect, Jones makes some succinct comments on an issue that I also consider to be important, where he refers to the necessity of paying heed to academic politics. Whether we like it or not, it is now common practice within higher education and research institutes to think in terms of business methods, and a common and sensible theme in this context is that a vital precursor to success is the careful definition of aims and objectives. All I am suggesting is that if we are to be successful within the science and non-science community we must consider these.

The third theme which is relevant in this discussion revolves around the definition of our subject area. This is clearly where most of the discussion, and certainly some debate will lie, and probably where individual opinions will part company. The first key issue is whether, if environmental archaeology is to operate successfully, we

need to know and/or define what we, as a corporate body of scientists, do. If the answer to this is negative, then the discussion can end. However, if the answer to this fundamental question is in the affirmative, as is likely, judging by the respondents' view, then we must discuss how we set about defining "environmental archaeology", and what we include in or exclude from such a discussion. For example, do we need philosophical or conceptual rules or guidelines, or is it perhaps more useful to follow a less formal path? How do we identify the future direction of environmental archaeology? What does environmental archaeology expect to produce? And so on. In this context, Jones' image of comparing the "rather pedestrian Dr Watson [environmental archaeology] to cultural archaeology's Sherlock Holmes" is, I think, a useful one, since it in particular begs the question of whether this is not actually the case in many situations. Such an analogy may force us to consider the relative roles of data collection and data analysis and integration which are apparent throughout the "environmental archaeology" literature. For example, how more or less valid is the basic data collection and consequent production of species lists (for example) that occurs at many sites, compared with the relatively fewer higher level analyses and integrated studies reported by, for example, authors cited by Thomas, Bell and Van der Veen? How does one assess these different types of approaches? I suspect that Thomas's "friendly wrangles" are, at present, the place to address these types of issues, and I regret that I was unable to be part of them in London last year.

To conclude, it is perhaps rather obvious to state that the discussion regarding the definition and direction of environmental archaeology is on. However, this is not because I have raised it, but because there is a sense amongst environmental archaeologists, expressed in the nature of last year's AEA 10th Anniversary Conference in London, that such discussion is worthwhile and timely. Indeed the participants of any field of scientific endeavour ought to engage in such discussion, since to be a good practitioner requires one to understand the context of one's expertise. Many reasons may, if deemed necessary, be forwarded to justify such discussion, these ranging from a

purely esoteric interest in the philosophical position of any field of enquiry, to the thoroughly pragmatic needs encapsulated in Jones' "academic politics". The debate, at least for environmental archaeology, is not whether such discussion ought to take place (all the respondents to my paper agree that it should) but should concern itself with what ideas ought to be discussed.

Since I now reside in Australia, it is pertinent to finish this reply by paraphrasing one of many Aboriginal comments on life. Amongst traditional Aboriginal Communities, camp sites show a high degree of spatial organisation, with the relative locations and orientations of individuals' humpies (temporary shelters) being determined by, and thus reflecting the social structure of the group living at the site. Derived from this is a saying which goes something like: "Show me where you live, and I will tell you who

you are". To me, this summarises the reason for my preliminary efforts at discussing the conceptual framework of environmental archaeology and, I suspect, the respondents' willingness to react to my efforts.

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[\*Editors' note: the 'manuscript received' dates on these six contributions indicate a very long period between first receipt of Dr Boyd's original paper and publication; much of the delay has been a result of dilatoriness by the Editors. We apologise to all the authors and to the readership of *Circaea* for the unduly long gestation of this set of papers. Please note that, in particular, the 10th Anniversary Meeting of the AEA took place during this period but we have preserved the authors' original text rather than amending the tense accordingly.]



## Post-medieval cattle horn cores from the Greyfriars site, Chichester, West Sussex, England

Philip L. Armitage, 1972 Roseate Lane, Sanibel Island, Florida 33957, USA

### Summary

Osteometric study of an excavated group of horn cores from Chichester, West Sussex, believed to be the discarded waste products from a local butcher's slaughteryard, has revealed considerable heterogeneity in the size and appearance of Sussex cattle in the post-medieval period. This unexpectedly wide variation in what has hitherto been regarded as a geographically isolated (and therefore uniform) population of cattle most probably reflects marked differences in the standards of livestock husbandry as practised by stockmen throughout the region.

### Introduction

Excavations carried out by Alec Down in 1984 at the Greyfriars site, Chichester, uncovered a post-medieval pit (Area D, feature A50) filled with cattle horn cores. The date of this assemblage is suggested by the associated pottery, which is late 16th century (A. Down, pers. comm., 1990). A sample of 118 horn cores from the pit was sent to the author (then working at the Booth Museum of Natural History, Brighton) in order that a detailed study could be undertaken. Apart from being briefly mentioned in the paper by Bruce Levitan (1985), which describes an assemblage of 18th century cattle horn cores found during archaeological investigations at Alphington Street, St Thomas, Exeter, in 1984,

this group of Sussex cattle horn cores has until now remained unpublished. The main purpose of this short paper is therefore to bring this Sussex material to the wider attention of fellow archaeozoologists, and to provide them with sufficient data for use in comparative metrical analyses.

### Methodology

#### (i) Age of the cattle at death:

Using the method of Armitage (1982b, 40-3) the specimens can be classified into three age classes on the basis of size, surface texture and appearance of the bone (Table 1). No juveniles (age class 1: 1-2 years) or old adults (age class 5: over 10 years) were identified.

Table 1. Greyfriars, Chichester, 1984. Cattle horn cores, summary of the attributed ages of the specimens.

Age class	Suggested age range (years)	Number of specimens	% total
1. Juvenile	1-2	0	0
2. Sub-adult	2-3	33	28
3. Young adult	3-7	42	35.6
4. Adult	7-10	43	36.4
5. Old adult	over 10	0	0

(ii) Sex of the horn cores:

Tentative determinations of the gender of the young adult and adult specimens (age classes 3 and 4) were made on the basis of a visual appraisal of the shape, curvature and angle of attachment of the core to the frontal bone, after the method of Armitage and Clutton-Brock (1976, 332) and Armitage (1982b, 43). The following identifications were made: female 22 (25.9%), male 3 (3.5%), castrate 31 (36.5%) and indeterminate 29 (34.1%).

(iii) Size of the cores and classification into the groups small/short horned, short horned, short/medium horned and medium horned:

Measurements taken from the young adult and adult specimens are summarised in Tables 2 and 3, below.

As discussed by Martin (1847, 56) it is common to subdivide cattle into the broad categories: short, medium and long horned, on the basis of horn length. This is the classification system adopted by Armitage (1982b) to describe cattle horn cores from British post-medieval sites. It should be noted that the method can only be applied to adult

cores over 3 years of age (age classes 3 to 5); in younger animals (age classes 0 to 2) it is not possible to determine their potential adult length, and so they are omitted from the analysis.

Using the classification system of Armitage (1982b, 43) the young adult and adult cores (complete and broken) from Greyfriars, Chichester, have been assigned to their respective groups (Table 4). Even though many of the Greyfriars' specimens are broken, with only between one third and one half of the core remaining intact, it proved possible to derive estimates of the original complete length of the outer curve (measured from the tip to the base) by projecting the dimensions of the surviving basal portion. Although the estimated values so obtained allowed these incomplete specimens to be classified, they are not considered sufficiently accurate for use in metrical analysis with measurements taken of intact specimens and have therefore been omitted from the tables of measurements given in this report. Estimates made from 'virtually' complete cores (i.e. specimens with only the very tip missing), however, are believed to be sufficiently close to the original values to justify their use in the metrical analysis.

Table 2. Greyfriars, Chichester, 1984. Cattle horn cores, summary of the metrical data. All measurements are given in millimetres.

Key to Measurements: LOC—length of outer curve; BC—basal circumference; MxD—maximum diameter of base; MnD—minimum diameter of base; ½BHC—half breadth between horn core bases (see measurement 31, von den Driesch 1976, p. 29).

Age class	Measurement	Number of specimens	Mean	Range	Standard deviation
3. young adult (3-7 years)	LOC	24	137.1	90.0-205.0	40.1
	BC	41	146.8	98.0-208.0	32.0
	MxD	41	51.3	34.0-74.0	10.8
	MnD	42	40.5	25.3-56.8	9.2
	½BHC	4	85.4	82.7-93.1	-
4. adult (7-10 years)	LOC	23	128.7	86.0-300.0	42.2
	BC	35	130.8	83.0-189.0	27.1
	MxD	36	47.0	34.7-68.5	8.7
	MnD	38	37.1	23.0-55.5	8.8
	½BHC	6	71.3	57.4-105.5	-



Table 3. Greyfriars, Chichester, 1984. Cattle horn cores, frequency distribution for the basal circumference (mm). Young adult and adult cores only (age classes 3 and 4).

Number of specimens = 76, mean = 139.47 mm; range = 83–208 mm; standard deviation = 30.72 mm.

Basal circumference class interval (mm)		Number of cores
80–89	x	1
90–99	xxx	3
100–109	xxxxxxxxxxx	11
110–119	xxxxxxxxxxx	10
120–129	xxxxxxxxxxx	12
130–139	xxxxxxxx	8
140–149	xxx	3
150–159	xxxxx	5
160–169	xxxxxxxx	8
170–179	xxxxxx	6
180–189	xxxx	4
190–199	xx	2
200–209	xxx	3

Table 4. Greyfriars, Chichester, 1984. Cattle horn cores, summary of the length classes identified. Young adult and adult cores only (age classes 3 and 4).

Notes: [a]—exceptionally small-sized cores but otherwise generally similar in shape and curvature to cores of the short horned group; [b]—specimens with LOC less than 220 mm but with larger (more 'robust') bases than the 'true' short horned cores; [c]—includes one male core (LOC 205 mm), best classified under the medium horned group.

Group (length class)	Length of outer curve (class limits in mm)	Number of specimens	% of total
Small horned/short horned [a]	under 100	10	11.8
Short horned	100–220	50	58.8
Short horned/medium horned [b]	(200–205)	11	12.9
Medium horned [c]	220–360	13	15.3
Long horned	over 360	0	0
Indeterminate	-	1	1.2

## The problem of classifying late 16th century cattle

As discussed by Armitage (1984, 6) the late 16th century was a time of significant advances in cattle husbandry, which is reflected in the very wide variety of size and general appearance of cattle horn cores found at archaeological sites of this period. In the light of these changes, the existing horn core classification system devised by Armitage (1982b) is proving inadequate to deal with the intermediate forms encountered in deposits of this date and there is clearly a need to revise the system in order to provide a more precise typology which will accommodate *all* late 16th century cattle. Until this revision has been carried out, the intermediate types in the Greyfriars sample have had to be assigned to two temporary classes: small/short horned and short/medium horned (Table 4).

## Interpretation and discussion

### (i) Source of the cattle horn cores:

Deposits of cattle horn cores found at archaeological sites generally derive from one (or a combination) of the following three sources:

- (a) slaughteryard (butcher's shambles)
- (b) tanyard
- (c) horn-worker's premises

The connection between deposits of cattle horn cores and the crafts of butchery, leatherworking and horn-working may be explained as follows:

### (a) butchery

During preparation of the carcass for disjoints, the butcher would leave the horns attached to the hide. Occasionally, however, he would remove them for sale directly to the horn-worker; either as complete horns (i.e. outer sheath and bony core) or sheath only (inner core removed). Evidence for this dual practice appears in a late 15th century petition from London horners to the Lord Mayor where mention is made of the purchase by horn-workers of 'hornes in the bones' and horns 'oute of the bones' from City 'bochers' (Fisher 1936, 23). If the butcher sold the horners horn sheaths only, he soon accumulated large quantities of the inner bony core (horn core) which were then

thrown away along with the other unwanted slaughteryard waste.

### (b) leather-working

Pictorial evidence showing that tanners bought hides of cattle which still had horns attached is provided by an early 19th century engraving of the 'skinmarket' at Leadenhall, City of London (Wilkinson 1825) and a photograph of a modern leather market (Cooke 1917, 17). As discussed by Prummel (1978, 399-402) this practice is well documented and there is archaeological evidence showing that the tradition is long established and may be traced back to medieval times. Having purchased hides, the tanner's first task in preparing them for the tanning process was to cut out the horns (see Thomson 1981, 162) which he would sell to the horn-workers either as complete horns (sheath and core) or as outer sheaths only. If the latter procedure was followed large quantities of horn cores quickly accumulated as demonstrated by the excavation of a 16th century tannery site in St Albans, Hertfordshire, where there was found a pit filled with oak bark and cattle horn cores (Saunders 1977).

### (c) horn-working

If the horner purchased complete horns from butchers and tanners, his first task was the removal of the inner bony core (Armitage and Clutton-Brock 1976; Prummel 1978, 409). In this way the horner soon accumulated a large quantity of horn cores which he disposed of with the other horn-working waste. Archaeological evidence for the association between cattle horn core deposits and the horn-working industry was found by Wenham (1964) during excavations at Hornpot Lane in the city of York, which uncovered a 14th century horn-soaking pit containing over 500 cattle horn cores. Further evidence of this comes from Stamford where excavations on the site of a 16th/17th century horner's workshop revealed ten horn-soaking pits filled with cattle horn cores (Cram 1982).

There is no evidence for either tanning or horn-working activity in this part of Chichester in the early modern period (Down, pers. comm.) and it is very unlikely therefore that the Greyfriars horn cores came from a tanyard or horn-worker's premises. The presence of butchers in the area is, however, well attested; documentary sources examined

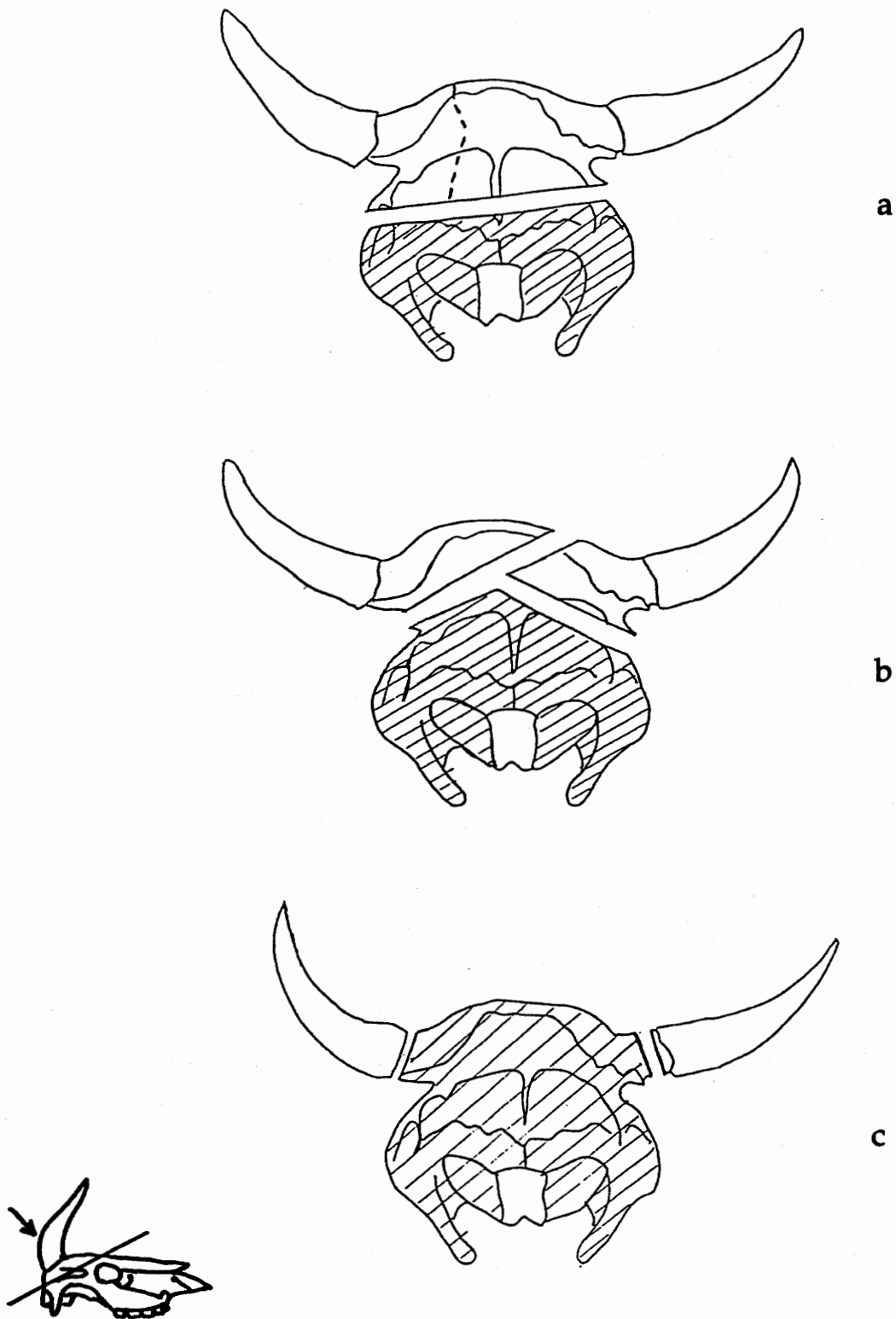


Fig. 1 (top to bottom: a-c). Methods of detaching horns from cattle heads. Shaded area—portion not present in pit. Inset: arrow indicates nuchal view seen in (a)-(c). Broken line in (a)—subsequently broken (in antiquity).

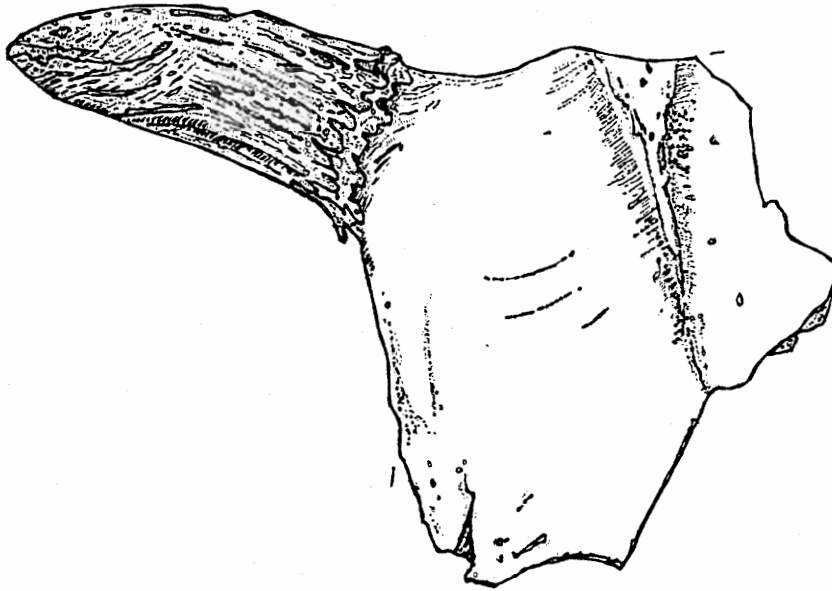


Fig. 2. Frontal view of right horn core and part of frontal bone of small/shorthorned cow from Greyfriars, Chichester. Length of outer curve 90mm. (Drawing: Kate Armitage)

by Morgan (1984) revealed that the Greyfriars site in the 16th and 17th century—then a garden—belonged to the Exton family who were engaged in the butchery trade. It seems very probable, therefore, that the cattle horn cores in the pit came from a local slaughterhouse run by this family.

(ii) Evidence for the removal of the hide:

Twelve specimens (10.2% of the total: Table 5) have small superficial cuts on the frontal bone, across the intercornual protuberance or at the base of the skull (Table 6). These marks are recognised as having been made by a skinning knife and provide evidence for the removal of the hide (in this case by the butcher prior to disjuncting the carcass).

(iii) Marks made by cleaver or axe:

Almost all the specimens examined show evidence of having been hacked off the skull by means of a cleaver or axe. In the majority of the specimens the right and left horns had apparently been removed together (as a single unit) from the head by a sweeping blow

directed across the back of the skull—possibly when the animal's head was positioned on the ground (Fig. 1a).

Subsequently (in antiquity) the portion of the cranium bearing the two horn cores broke in two; whether this was done purposely or accidentally during deposition and burial in the pit is, however, unclear—but as no conjoined core was found it may be suggested that separation occurred prior to deposition.

A few specimens show evidence of the right and left horns having been removed separately, by blows directed to the side of the head just below the base of each horn in turn (Fig. 1b).

A third method for detaching the horns from the head is indicated by the core of a young adult shorthorn, in which the blow delivered by the cleaver—or axe—fell across the base of the horn, severing it completely from the parietal and frontal bones (Fig. 1c). As this method is represented by only a single core it was probably the least favoured; it would

Table 5. Greyfriars, Chichester, 1984. Cattle horn cores, evidence of skinning. Note: in the specimens where the description of the evidence for skinning is 'uncertain', it is not possible to ascertain whether knife marks had originally been present because the horn core only has survived or because the frontal and/or parietal bones are poorly preserved.

Age class	Description	Number of specimens
2. sub-adult (2-3 years)	with knife marks	3
	without knife marks	10
	uncertain	20
3. young adult (3-7 years)	with knife marks	6
	without knife marks	13
	uncertain	23
4. adult (7-10 years)	with knife marks	3
	without knife marks	18
	uncertain	22

have created problems when the time came to extract the bony core from its outer sheath. In the other two methods (Figs. 1a and b) the surviving portions of frontal and parietal bones left attached to the core would have functioned as 'hand-holds' when the horn sheath was being pulled off.

(iv) Kill-off pattern (age at slaughter):

Over two-thirds of the Greyfriars' horn cores are from animals over three years of age (Table 1). This preponderance of fully grown cattle fits very well with the picture of urban centres being principally supplied with culled draught animals. Such animals would not have been sent into the town for slaughter until at least six years of age. Draught cattle generally started their working lives when about two or three years of age (Fussell 1952, 63; Cornwall 1954, 73) and after working for a period of between three to five years they were then fattened ready for the meat market (Oschinsky 1971, 162). According to Leonard Mascall (the author of *The Governemete of Cattell* of 1587 and owner of Plumpton Manor, near Lewes), working oxen could be kept till ten, and then fattened for slaughter (Fussell 1952, 63). In view of Mascall's advice, it is somewhat strange to discover that the sample of horn cores from Chichester does not include at least a few old adults (i.e. animals over 10 years of age); the reason for

this discrepancy is unclear.

The presence of immature (sub-adult) horn cores (28%) in the Chichester sample (Table 1) suggests that at least some of the cattle reaching Chichester in the late 16th century were supplied by livestock farmers specialising in the rearing of fat cattle (cf. Sir Thomas Pelham of Laughton—referred to below).

Very young veal calves may also have been slaughtered in significant numbers in Chichester in the late 16th century but as the horns of these animals would have been very little developed (i.e. were no more than horn buds) evidence for this is unlikely to be found in excavated horn-core deposits such as that discovered at the Greyfriars site.

(v) Cattle husbandry—evidence of heterogeneity in Sussex cattle:

Coefficient of variation (variability) in the length of outer curve, calculated after the method of Pearson (Simpson *et al.* 1960, 90) was 29.2 for young adults and 32.8 for adults (data from Table 2). These high values indicate that the collection of horn cores from the Greyfriars site is heterogeneous in composition. A 'pure' (homogeneous) sample would be expected to have a coefficient of variation between 4 and 10 (*ibid.* 91).

Table 6. Greyfriars, Chichester, 1984. Cattle horn cores, details of specimens showing evidence of skinning.

Key to Length class: SH—short horned; MH—medium horned (classification of Armitage 1982b).

Age class	Side	Length class	Number of marks made by knife (per specimen)	Location of marks
2. sub-adult (2–3 years)	R	-	1	on surface of frontal bone
	R	-	1	on surface of frontal bone
	L	-	1	on surface of frontal bone
3. young adult (3–7 years)	R	SH/MH	1	on surface of frontal bone
	R	SH	2	on surface of frontal bone
	R	SH	2	on surface of frontal bone
	R	SH	1	base of skull
	L	SH/MH	5	on surface of frontal bone
	L	SH	1	on surface of frontal bone
4. adult (7–10 years)	R	SH	4	around base of horn core, anterior surface
	L	SH	1	across intercornual protuberance
	L	SH	2	on surface of frontal bone

The high variability within the Chichester horn core sample can in part be ascribed to the presence of male, female and castrated animals, but it also clearly demonstrates that more than one type of stock is represented. Indeed, the very wide variety of horn cores includes those from exceptionally small shorthorned animals (see Fig. 2) reminiscent of the dwarf ('scrub') cattle of the high middle ages (see Armitage 1980, 406; 1982a, 53) as well as those from individuals of similar size and horn conformation to the modern Sussex breed. (A few of the larger Greyfriars cores are not much different in size from the horn cores of the two adult Sussex cows in the modern comparative osteological

collections of the Booth Museum of Natural History, reg. nos. 100026 and 102040 whose length of outer curvature measure 310 mm and 238 mm, respectively).

The very wide variety recorded in the Greyfriars sample is all the more remarkable when one considers that cattle in Sussex in the 16th century formed a geographically isolated population. Although recognised as an important cattle rearing district, very few Sussex farmers had sufficient capital to fund long distance movement of breeding stock and they could not therefore afford to import animals, except in a few, rare, instances from outside the county (Cornwall 1954, 77).



Virtually all cattle found within the county at this period were therefore 'native' bred. This situation may be contrasted with other farming areas at this period where cattle populations comprised a mixture of local and non-local (sometimes even foreign) stock. The cattle population of Lincolnshire in the 17th century, for example, included recently imported Dutch shorthorned cattle as well as black longhorns from Lancashire and Yorkshire (Markham 1657, 69; Mortimer 1707, 166).

If the variety in the Greyfriars sample cannot be ascribed to the presence of more than one regional 'race' of cattle, the alternative explanation must be that the wide range in size reflects different standards of livestock husbandry practised by Sussex stockmen, i.e. the more progressive farmers reared animals of reasonable size and quality while others, who largely neglected their stock, produced smaller and inferior quality animals, though even a wealthy landowner such as Sir Thomas Pelham of Laughton—who specialised in beef production in the first half of the 17th century—had many runts in his herd; these poor quality animals (mostly females and castrates) were only half the value of the better sort of cattle, when fattened and sold for meat (Cornwall 1954, 73–4). Fussell (1952, 95) also considered that the majority of Sussex cattle remained little improved until comparatively recent times, and that the Sussex cow was especially 'tiny ... inspite [sic] of the efforts of ... breeders'.

### Acknowledgements

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[A copy of the complete Level III Archive Report on the cattle horn cores from the Greyfriars site, Chichester, is held by The Booth Museum of Natural History, Brighton, where it may be inspected, on request. Alternatively, any

archaeozoologists seeking a copy of the measurements taken of these specimens may write directly to the author at the address shown at the beginning of this paper.]

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## The distribution of the land snail *Vitrea contracta* (Westerlund) in a calcareous soil on Martin Down, Hampshire, England

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### Summary

Detailed analysis of the land snails from a rendzina on Martin Down, Hampshire, England, identified fresh shells of *Vitrea contracta* (Westerlund) throughout the soil profile. This indicated that live snails were present in the soil and not just on the surface. *V. contracta* has not been recorded as a burrowing species so it is suggested that it is living within the soil voids. This conclusion has implications for the interpretation of fossil land snail assemblages from similar soils.

### Introduction

The ecological interpretation of land snail assemblages from soils and sediments depends on many assumptions about the origin and history of the assemblages. One of these assumptions is that the sample assemblage originated at the surface as a contemporary death assemblage of snail shells. As a result, it is standard practice to exclude the burrowing species *Cecilioides acicula* (Müller) from an analysis as it may be intrusive and therefore considerably younger than the other shells in the sample (Evans 1972). This is often illustrated in modern soils by the presence of fresh, shiny, translucent shells of *C. acicula* deep in the profile, that must be alive or have died within the previous year. The living appearance of the shell is lost soon after death as the organic periostracum that covers the shell is destroyed (Evans 1972; Cameron and Morgan-Huws 1975).

A modern rendzina (Avery 1980) on Martin Down, Hampshire (NGR SU053184) was sampled in 10 mm spits with an area of 0.4 X 0.4 m (1.6 l). As part of a detailed analysis of the land snail assemblages (Carter 1987), fresh apices of all species were identified and their distributions plotted in 10 mm spits down the soil profile. The extraction procedure used did not discriminate between living and recently dead individuals.

### Results

The modern snail community at Martin Down consisted of seven species which reflect the short chalk grassland habitat of the Down. Two types of distribution can be identified in Table 7, either having most shells concentrated in the surface 20 mm, or a uniform spread down the profile to the top of the C-horizon at 170 mm (the lower limit of 10mm spit sampling). *Cecilioides acicula* is the most abundant species and is spread down the profile, as would be expected from its burrowing habit. Five other species (*Cochlicopa lubricella*, *Vertigo pygmaea*, *Pupilla muscorum*, *Punctum pygmaeum*, *Nesovitrea hammonis*) are concentrated at the surface, reflecting a distribution in life only on or above the soil. The outstanding species is *Vitrea contracta* which, although present at low frequencies, has a distribution similar to *C. acicula*, indicating the distribution of live individuals throughout the soil profile.

### The distribution of *Vitrea contracta*

Of the seven species distributions in Table 7, only that for *V. contracta* is unexpected, as it is not referred to in the literature as a burrowing species. With only one or two specimens present in most samples, contamination during sampling must be considered as a possible cause of the distribution. The collection of each 10 mm

Table 7. Frequency of fresh shell apices in 10 mm spits from a soil profile at Martin Down, Hampshire. Nomenclature follows Kerney and Cameron (1979). Key: Cl: *Cochlicopa lubricella* (Porro), Vp: *Vertigo pygmaea* (Draparnaud), Pm: *Pupilla muscorum* (Linnaeus), Pp: *Punctum pygmaeum* (Draparnaud), Vc: *Vitrea contracta* (Westerlund), Nh: *Nesovitrea hammonis* (Ström), Ca: *Cecilioides acicula* (Müller).

Depth (mm)	Cl	Vp	Pm	Pp	Vc	Nh	Ca
0-10	92	11	16	4	1	6	28
10-20	41	2	1	2	2	5	46
20-30	4	1	-	-	1	-	30
30-40	4	-	-	-	8	1	31
40-50	2	-	-	-	1	2	35
50-60	-	-	-	-	1	-	43
60-70	-	-	-	-	2	-	50
70-80	1	-	-	-	2	-	57
80-90	2	-	-	-	1	-	61
90-100	-	-	-	-	-	-	24
100-110	-	-	-	-	2	-	25
110-120	-	-	-	-	-	-	26
120-130	-	-	-	-	-	-	32
130-140	-	-	-	-	1	-	35
140-150	-	-	-	-	-	-	36
150-160	-	-	-	-	5	-	39
160-170	-	-	-	-	2	-	23

Table 8. *Vitrea contracta*: the frequency of fresh and old shell apices in 10 mm spits from a soil profile at Martin Down, Hampshire.

Depth (mm)	Fresh	Old	Depth (mm)	Fresh	Old
0-10	1	19	90-100	-	9
10-20	2	30	100-110	2	14
20-30	1	52	110-120	-	3
30-40	8	79	120-130	-	2
40-50	1	43	130-140	1	1
50-60	1	52	140-150	-	-
60-70	2	28	150-160	5	-
70-80	2	19	160-170	2	1
80-90	1	14			

spit took at least 30 minutes, so soil and shells could have fallen from the sides of the sampling pit on to a spit during its removal. The degree of contamination caused by this method of sampling is best illustrated by the distribution of *Cochlicopa lubricella*, which is present in large numbers in the surface 20 mm. If it is assumed that all fresh shells

of *C. lubricella* below 20 mm are the result of sample contamination, it can be calculated that an average density of 67 shells per 1.6 l in the surface 20 mm produced only thirteen shells in the rest of the samples. This represents an average of only 1.3% contaminants per sample from the surface 20 mm, and clearly cannot account for the

distribution of *V. contracta*, which lacks a surface concentration of shells. Therefore, the distribution is not an artifact of sampling and must reflect individuals of *V. contracta* living within the soil. This conclusion has implications for the analysis of fossil snail assemblages in archaeology. If *V. contracta* is behaving like *Cecilioides acicula*, then it may be necessary to treat it in a similar manner and exclude it from the analysis.

The presence of *V. contracta* within the soil is perhaps not entirely unexpected, as it is a characteristic species of cave and rock-rubble communities (Ellis 1969; Evans and Jones 1973). At Martin Down it may be treating the well-structured soil profile with abundant voids like the clast supported rock-rubble habitat—a soil with a well-developed crumb structure is, in effect, clast supported. Of the 29 fresh shells of *V. contracta* recovered, only three had a diameter greater than 2 mm, and half were smaller than 1 mm, so movement within this type of soil without active burrowing is possible. It is interesting to note that most shells of the other species present at Martin Down are within this size range and therefore could, but apparently do not, enter the soil during life.

Evans and Jones (1973, 121) suggested that the presence of *V. contracta* in the cave and rock-rubble assemblages reflects the fact that it is a facultative carnivore, as these habitats may totally lack vegetable matter. This is not the case in the Martin Down rendzina, which contains abundant plant organic matter down to a depth of 160 mm (Carter 1987). Evans and Jones (1973, 128) also suggested that the snail communities of caves and rock-rubble are the product of uniform high humidity in these habitats and this factor may explain the behaviour of *V. contracta* at Martin Down.

If the distribution of fresh *V. contracta* shells is compared with that of old shells (Table 8), the latter category is seen to be concentrated in the upper part of the A-horizon (10-70 mm). This distribution indicates that at least most individuals died at or near the soil surface. A uniform distribution at death would produce at least equally high concentrations of old shells at the base of the A-horizon (110-170 mm). This suggests that *V. contracta* is only temporarily present deep in the soil profile, perhaps when surface conditions are too dry, and, unlike *Cecilioides acicula*, it will return to the surface.

It may be argued that—in the particular example presented here—because *V. contracta* is dying at or near the surface, it does not create any problems for the interpretation of fossil assemblages. However, it does serve to illustrate the point that, at the scale of a snail shell, a well-structured soil can function like a clast-supported rubble layer. In archaeological buried soils, no fresh shells will be present to demonstrate that species other than *Cecilioides acicula* were living in the soil, and it is worth bearing this in mind when trying to interpret ecologically improbable assemblages of shells.

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## A method for investigating bone fragmentation and anatomical representation

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### Summary

This paper describes and discusses a method for investigating bone fragmentation and anatomical representation by means of a Fragmentation Index. This index is calculated using fragment size and is felt to be of potential use for investigating relative degree of fragmentation in different anatomical elements for different taxa and sites.

It has become apparent in recent years that quantifying bones is not a simple matter of counting or weighing fragments. Over the past two decades a number of papers and bone reports have given space to the consideration of how to calculate anatomical frequency in archaeological samples. These have ranged from simple, but effective methods such as that of Grant (1975) who advocated the 'epiphysis count', and that of Watson (1979) who was one of the first to advocate 'zone' counts, to extremely complex methods based upon recognition of a multitude of characteristic zones (Dobney and Rielly 1988). Methods for quantifying bones continue to proliferate. In some respects this seems to add to the confusion, but it does serve to show that analysts continue to actively question their methods and design new ones to cover new circumstances.

Some workers have taken the view that the method they are advocating is the best option, whilst others have taken the view that different situations call for different solutions, and that a particular method may be appropriate in one case but not necessarily in another. The latter is the view supported here. After all, a prehistoric hunter-gatherer settlement bears little resemblance to a medieval waterfront. The former site can reasonably be expected to reflect the activities of the inhabitants in a fairly straightforward fashion: the bones were the remains of meals and secondary usage (bone working, hide preparation, etc.); the latter may contain deposits that

did not originate from the site but which were brought in as rubbish from elsewhere, and even the point of origin of the rubbish may have represented the end point of a complicated series of events that began with raising the animals at some remote location, included droving and marketing, then butchery and retailing, and finally domestic consumption. Clearly the questions posed in any analysis of anatomical elements will depend upon the type of deposit, and it follows that the method of analysis will in turn be dependent upon those questions. It should never be said that any one method is the best, and it may often be appropriate to use several methods in a single report. So long as the way in which the calculations are made is clearly defined, and the raw data are available (though not necessarily in the published report), there should be no restraint upon the analyst in using 'non-standard' methods, or in creating new ones.

The aim of this paper is to put forward a method of calculating anatomical frequency that the author has found to be useful on a number of urban sites and also on a range of high status medieval sites (abbeys, castles, etc.). The intention is not to propose that this method is better than any other, but simply to add to the suite of tools of analysis. The method is not entirely new or entirely original, but is a refinement of methods that have been described elsewhere. It evolved from the use of the Ancient Monuments Laboratory

(AML) coding for fragmentation of bones (Jones *et al.* 1980). The analytical software provided with the AML system did not support analysis of anatomical representation using the fragmentation codes, except in a very basic and 'raw' state which still required a large amount of manipulation. It was felt, therefore, that a method for dealing with the data generated by the use of the coding was needed.

Methods described by O'Connor (1984), Payne (1979) and Watson (1979) provided the basic idea, i.e. that bone counts can be standardised for skeletal frequency and account can be taken also of fragmentation (Watson's 'zones'). The present method employs a modification AML fragmentation scoring rather than the diagnostic 'zones' of Watson. An example of Watson's method is that, for a longbone such as a humerus, there are four zones: 1—right humerus (head), 2—left humerus (head), 3—right humerus (distal) and 4—left humerus (distal) (Watson *op. cit.*, table 1). In some respects this resembles the epiphyses counts of Grant (1975), though there are differences, particularly in the zones of non-long bones, and zones may be defined that use shaft fragments without epiphyseal parts present.

Payne's approach, which he called MINDEX and used for his analysis of bones from Towcester, was to use finer basic units with a built-in correction factor for fragmentation (Payne *op. cit.*).

The AML method subdivides a bone into the following fragmentation categories: <25% complete, 25%, 25–49%, 50%, 51–74%, 75%, 75–99% and 100%. These eight divisions can be supplemented with a coding for proximal end present (P), distal end present (D) and midshaft (M).

In a recent paper O'Connor rightly criticised some of the methods of quantification, particularly those based upon pairing of elements (O'Connor 1985). Quite apart from the problems he discussed, such methods seem inappropriate for sites with complex stratigraphy and which are themselves part of a more complicated economic framework (e.g. urban sites in particular). Elements from single individuals may become widely dispersed, and only some of them may be represented on the site. A good example of

this is the specialised processing of animals in Exeter (Levitan 1989). Furthermore, butchery and other processes can result in differential fragmentation of bone elements, so that simple counts, for example, would be biased in favour of the more fragmented bones.

### Fragmentation Index: method of recording

The method is actually a system for providing a measure of the degree of fragmentation in an element. It is not primarily a means of calculating anatomical frequency (though see below), and is not intended as a replacement of other methods which address that need (eg the zone count and epiphysis count methods). It has, therefore, been named a Fragmentation Index (FI). This involves recording the proportion of the bone that is present. It is scored on a five-point scale that is rather subjective, but which is wide enough for the overlap not to be significant. The five-point scores are based on the AML method summarised above, but have been simplified into five categories rather than eight:

- 1—less than a quarter of the bone represented (<25% complete);
- 2—between a quarter and a half complete (25–49% complete);
- 3—between a half and three-quarters complete (50–74% complete);
- 4—three-quarters or more complete, but not complete (75–99% complete);
- 5—complete (100% complete).

The scoring is subjective in that a bone that is close to the border between one score and another may be placed in the wrong category. It is difficult to see, however, how this can be overcome. Absolute size does not provide an answer because without knowing the size of the whole bone it would be impossible to weight the measurements, and two fragments of equal size may not represent equal proportions of the bones. It is the opinion of the author that the 'bias' of wrongly recording some bones using the subjective scoring will not be great. In any

case if a bone is close to the borderline between scores, then it probably does not matter too much which score it is given as the *FI* itself is only an approximation.

### Fragmentation Index: calculation

Having recorded the bones in this manner, the *FI* for a particular element may then be calculated using the following formula:

$$FI = (n_2/4 + n_3/2 + n_4 + n_5)/NE (\%)$$

where  $n_{2-5}$  = total number of specimens for that element in fragment categories 2...5 and

$$NE = n_1 + n_2 + n_3 + n_4 + n_5.$$

Score 1 (<25% complete) is not included in the first part of the index because the factor for division is so uncertain, fragments may be nearly 25% complete, or only 10% complete, etc. For this reason, a separate index for this score ( $FI^2$ ) is calculated, this being a simple percentage based on the total number of fragments for that element:

$$FI^2 = n_1/NE (\%).$$

For example, two sets of 150 humeri with the following scores: (a) 66 score 1, 53 score 2, 20 score 3, 10 score 4, 1 score 5; (b) 42 score 1, 20 score 2, 17 score 3, 28 score 4, 43 score 5 may be calculated:

$$(a) \quad (53/4 + 20/2 + 10 + 1)/150 (\%) \\ = FI \quad 22.8\%$$

$$66/150 (\%) \\ = FI^2 \quad 44.0\%$$

$$(b) \quad (20 + 17 + 28 + 43)/150 (\%) \\ = FI \quad 56.3\%$$

$$42/150 (\%) \\ = FI^2 \quad 28.0\%.$$

If all the bones were complete, an *FI* of 100% would result; thus the smaller the *FI*, the greater the degree of fragmentation. The two *FIs* give an obvious indication of the relative fragmentation in the two examples. In the first case  $FI^2$  is low, indicating that most of the humeri were very fragmented; the  $FI^2$  is quite high. In the second case, the  $FI^2$  is low and the  $FI$  is high (this is not to suggest the cor-

relation of  $FI^2$  and  $FI$  is always of this kind). Whilst these results are self-evident before calculation, it would not be so obvious in larger and more diffuse assemblages which occur in the real world. It will be obvious from the formula applied above that the index is only approximate. For instance, bones in score 2 are assumed to be about a quarter complete and thus are divided by 4; similarly bones in score 3 are assumed to be about a half complete and so are divided by 2. For the present purpose, it does not really matter that this index is not a precise measure of fragmentation because what it does is provide a standardised method of assessing relative fragmentation across the skeleton and, for any one element, across the site. In recording the bones, the scoring method also employs some measure of 'zone' by adding the suffix *P* if the proximal end is present, *D* if the distal end is present and *M* if the middle part only is present (*P* and *D* are also used for cranial and caudal respectively). Thus the method can easily be used for the 'epiphysis' count advocated by Grant (*op. cit.*), whilst a simple summation of all the bones (the *NE* of the formula) provides a simple fragment count of the elements. The method described above is the result of a process of evolution through work on bones from several sites analysed by the author which have all referred to the present paper as 'forthcoming'. The appendix lists those sites where this method has been employed so that the reader may look at some real data and real cases in order to form a more complete opinion of this method, though it should be noted that previous versions of this method differed slightly in detail (though not intention—in the main, the differences relate to the definition of the fragment classes): e.g. Levitan (1987); Rielly (1988).

### Uses of the Fragmentation Index

(i) the  $FI^2$  is only a single measure of overall fragmentation in an element, but it does serve as a useful tool for intra- and inter-site comparisons. In a series of reports on sites from the Upper Thames valley, Wilson has commented on the fact that some sites display distinct patterning in fragmentation (see, in particular, Wilson (1978) and Wilson and Levitan (forthcoming)). Unfortunately Wilson did not use

the method described above, but employed a cruder fragmentation index. Had the present method been used, a far more detailed picture might have emerged.

(ii) in addition to using this method in terms of the  $FI$ , the individual fragmentation scores could be used in the way the  $FI$  is calculated, and distribution maps produced for each score. This would produce a far more precise and detailed way of mapping fragmentation than the  $FI$ .

(iii) the  $FI$  can be used as a sort of anatomical frequency indicator, although it should be employed alongside other methods of calculation in such cases (Levitan 1987; 1989). Used in this sense it is particularly useful because it reduces all elements and all taxa to indices which are directly relatable. When used in this sense, the formula given above is modified to the extent that the '/NE (%)' factor is removed. In the examples given, the results would be: 34.3 and 78.0 respectively. The raw fragment counts indicate that both sets of humeri are the same, but the  $FI$  counts indicate that the second set potentially represents a greater number of complete humeri. The greater degree of fragmentation in example (a) has led to the same number of fragments which may have originated from a smaller number of bones than example (b). In such cases it is often important to quantify the bones in two ways—one to show the main zones of fragmentation, since these relate to the processes which preceded deposition and, in part, led to the fragmentation pattern (e.g. butchery), and one to compensate for the degree of fragmentation. The former can be tackled using Watson's zones, as O'Connor has shown (1984), and he is presently working on further refinements of this idea (T. P. O'Connor, pers. comm.). The present method is an attempt to deal with the latter problem. It reduces the fragments to indices calculated for each body element, i.e. as if they are 'whole bones', and allows direct comparison of these across a site, or between sites, as well as comparisons between species.

(iv) the fragmentation indices described above may also be used to calculate the relative abundance of the species, the individual anatomical indices simply being summed (the indices being used without

being transformed into percentages). An example of this is given in Levitan (1987, figure 7). These results may be presented as individual percentages or as aggregate counts (*ibid.* 67, 69). The aggregate method is useful because it provides a direct view of how the species inter-relate.

## Discussion

Is there really a place for yet another method of anatomical analysis? Clearly the author feels there is otherwise this paper would not have been written! The method presented here is felt to be a valid one because it addresses anatomical representation from a different viewpoint to the papers cited above. Normally the analysis is trying to reconstruct the patterns of anatomical representation in order to discover whether certain elements have been selected rather than others; from such patterns, conclusions about butchery and animal by-products, such as horn-working, can be made. The present method, however, is more concerned with patterns of fragmentation. The indices produced give a measure of how fragmented different elements have become. The indices can be used to compare fragmentation in a single element in several different locations, or overall patterns for different taxa, and so on. Not only is such information of intrinsic value in certain circumstances (e.g. for the Thames valley sites quoted above), but it may also be of facility in helping to understand anatomical representation in the first sense above. Something of this can be seen in the third of the applications described.

'Worked examples' were given in two papers by Levitan (1987, 65–9; 1989, 163–5, 174–8). The reader may like to refer to these papers (which used an older version of the  $FI$  formula) and consider the following points. Three obvious problems with this method are:

(i) epiphyses and symmetry are ignored;

(ii) it is not sufficiently precise in weighting for fragments of scores 2 and 3;

(iii) small fragments <25% complete are not included in the  $FI$ , yet these often form the bulk of the sample.

Table 9: Anatomical representation for the major domesticates from Narrow Quay, Bristol (AD 1580-1600).

Key: SKL = skull, JAW = mandible, HC = horncore, CAR = carpus, TAR = tarsus, MC = metacarpus, MT = metatarsus, PHAL = phalanges, RIB = ribs, VERT = vertebrae, SCAP = scapula, PEL = pelvis, HUM = humerus, RAD = radius, ULN = ulna, FEM = femur, PAT = patella, TIB = tibia, FIB = fibula; N = number of fragments, R = rank; %P = proportion of epiphyses (proximal and distal) which are proximal.

Anatomy group	N	FF <sup>1</sup>	FF <sup>2</sup>	R	%P (of P+D)
<b>Cattle:</b>					
SKL, JAW	222	20.17	69	9	
HC	-	-	-	-	
CAR, TAR	51	16.73	14	11	
MC, MT	200	57.17	24	4	22,45
PHAL	73	9.01	1	14	
RIB	696	2.94	68	19	
VERT	411	7.95	61	16	
SCAP, PEL	386	35.62	60	7	
HUM, RAD, ULN	633	113.92	26	1	36,62,-
FEM, PAT, TIB	407	57.75	32	3	48,-,46
<b>Sheep/Goat:</b>					
SKL, JAW	172	26.67	49	8	
HC	6	2.00	33	21	
CAR, TAR	4	0.88	0	23	
MC, MT	106	37.46	5	6	47,54
PHAL	-	-	-	-	
RIB	845	8.11	23	15	
VERT	94	4.49	26	17	
SCAP, PEL	250	70.21	18	3	
HUM, RAD, ULN	285	110.79	3	2	19,74,-
FEM, PAT, TIB	262	53.46	15	5	55,-,40
<b>Pig:</b>					
SKL, JAW	110	13.50	68	12	
CAR, TAR	5	1.88	0	22	
MC, MT	21	2.54	0	20	-,50
PHAL	-	-	-	-	
RIB	46	0.65	26	24	
VERT	72	4.16	21	18	
SCAP, PEL	47	10.30	13	13	
HUM, RAD, ULN	43	12.05	19	12	67,50,100
FEM, PAT, TIB, FIB	61	19.42	13	10	29,-,45,100

To deal with these points:

(i) Epiphyses counts may be utilised by calculating the proportions of proximal epiphyses out of the proximal plus distal total. This may be used as an additional indication of intra- and inter-species variation. The example given by Levitan (1989, table 8) of a group of late 16th century AD bones from the site of St Nicholas' Priory, Exeter, illustrates that the  $FI$  values may be ranked to show their relative abundance between, as well as within, species. The skeletal elements have been lumped into groups (similar to those of O'Connor (1984)), and it should be noted that ideally the indices should also be given for each skeletal element separately. The table also gives the  $FI$  results as <25%. A useful addition would be proportions of proximal epiphyses present (out of total epiphyses per element). An example of this is given in Table 9, which shows the results from a late 16th century deposit from the site of Narrow Quay, Bristol. Thus, for example, cattle have low proportions of proximal metacarpals and high proportions of proximal metatarsals, but in sheep (where the proportions of proximal metapodia are higher) the pattern is reversed. The problem of ignoring symmetry is felt to be less important as in a complex urban deposit, for example, there is little chance of paired elements occurring on a regular basis (O'Connor 1985).

(ii) In the case of a lack of precision for the 2 and 3 scores it was felt that the rather subjective nature of assigning degree of completeness to a bone is not sufficiently precise for the formula to take account of fragments which are, for example, 33% complete. To divide the scores into finer fractions would introduce spurious accuracy into a method which mainly seeks to provide standardisation for the  $FI$  rather than precision.

(iii) In the case of fragments less than 25% complete, it is clearly impossible to assign a reasonable weighting factor. One possible way of including these fragments is the weight method of quantification, but this method is not in common usage and has many problems attached to it. In order to include some indication of the representation of this size fraction, the  $FI$  is employed. It is then possible to gain an

impression of which body-parts might still be under- or over-represented. In the example in Table 9 the  $FI$  results are given. Here, for instance, it can be seen that fragmentation of skull was greater for cattle and pig than for sheep, so the relatively higher  $FI$  obtained for sheep may be something of an over-representation. In conclusion, this method should be useful in mapping fragmentation patterns across sites, and in helping to understand anatomical representation patterns.

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## Appendix

List of sites where the Fragmentation Index has been employed:

(a) Sites analysed by the author (np = not published):

Hazleton, Gloucestershire (Levitan 1990a)

St Katherine's Priory, Exeter (np, but see Levitan 1987; 1989)

St Nicholas' Priory, Exeter (np, but see Levitan 1989)

Narrow Quay, Bristol (np, but see Table 9)

Brean Down, Somerset (Levitan 1990b)

(b) Other sites:

The Ditches, Gloucestershire (Rielly 1988)

Potterne, Wiltshire (A. Locker) (np)

Wroxeter, Shropshire (B. Meddens) (np)



## A note on the systematic recording of organic-walled microfossils (other than pollen and spores) found in archaeological and Quaternary palynological preparations

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### Summary

A simple system for recording organic-walled microfossils other than pollen and spores, chiefly fungal spores and algal cysts, which has been successfully applied to diverse palynological assemblages, is reported. Its basis is the creation of informal form-species or 'types' by the systematic description of the morphological characteristics of each microfossil. The system enables the accurate description of entire assemblages of microfossils without the need for the formal identification of each '*incertae sedis*' microfossil. It is therefore possible to characterise such assemblages and recognise recurring patterns of possible palaeoecological and/or palaeoeconomic significance even when the taxonomic identity of specific form-species remains unknown.

### Introduction

In addition to the pollen of flowering plants and the spores of higher cryptogams and some mosses, many other organic-walled microfossils are present in palynological preparations. Such microfossils, of unknown or uncertain affinities or origin ('acritarchs'), together with fungal spores and algal encystment structures, have been used for many years in pre-Quaternary palynology both as stratigraphic zone fossils (Elsik 1968a, b; 1976; Ediger 1981) and, when used as part of a group or facies of palynomorphs, as environmental indicators (Batten 1973; Hart 1986; Jarzen and Elsik 1986).

'Acritarchs' are also common in archaeological and Quaternary palynological preparations (Faegri and Iversen 1975, 216). Many appear to be of algal or fungal origin. However, while the general affinities of these microfossils may be clear, many have not been successfully systematised and relatively few have been identified to the level of species or genera (Godwin and Andrew 1951; Van Geel and Van der Hammen 1978; Brinkkemper *et al.* 1987).

The specific, formal, identification of such organic-walled microfossils would appear

to be difficult because:

(1) The taxonomy of algal and fungal bodies is commonly based on the complete living organism and its life cycle and not upon the individual spore or resting stage (cyst) alone (Graham 1971; Elsik *et al.* 1983); in consequence the morphology of cysts, fruiting bodies or spores in isolation plays a minor role in the classification of taxa into families, genera or species.

(2) At present no systematic corpus of subfossil Quaternary fungal spores, algal cysts and other organic-walled microfossils is available. The classification of these microfossils has therefore remained confused and is in need of clarification.

(3) The literature of algology and mycology is unfamiliar to palynologists and archaeologists.

In consequence this large and varied group of microfossils has generally been neglected in the reconstruction of past environments or the search for evidence of subsistence activity (Dimpleby 1978, 121). Yet this group offers considerable potential to that end (Graham 1962; Van Geel 1976). They occur in considerable numbers in conventional palynological preparations, they are thought to be resistant to decay,

they are morphologically distinct and ultimately derive from taxa with largely known ecological tolerances and whose association with human activities, both directly and indirectly, is well documented (e.g. Clark 1952; Ashbee 1957; Helbæk 1958; Watling 1974; Seaward 1976; Van Geel 1976).

To overcome the above problems several workers have created morphologically distinct microfossil 'types' which have then been recorded in the same way as other, formally named, taxa (Van Geel 1978; Brinkkemper *et al.* 1987). In Van Geel's system each new microfossil encountered is described, recorded and given a type number. Each type "... can be considered as a provisionally, but not formally named type species" (Van Geel 1978, 47). Consultation with suitable authorities enables the subsequent provisional identification of many of the taxa concerned. This form-species approach is ultimately derived from pre-Quaternary palynology (cf. Traverse 1988).

This 'informal form-species' or 'type' approach was adopted by the author during the palynological investigation of Quaternary cave sediments (Coles 1988). Initial difficulty was experienced with the systematic description of this diverse group of microfossils. The characteristics described below were found to be a useful means of describing morphologically distinct types of organic-walled microfossils and may in consequence be of interest to other palynologists.

### The recording system

The system is based on a simple pro-forma record sheet (shown in Fig. 3). The sheet is designed to be photocopied back to back on a single sheet of A4 paper and stored in ring binders. The characteristics recorded permit the differentiation of the type from other, similar, 'taxa'. The sheet also records the location of the informal holotype (a single type specimen selected to show its main characteristics), and informal paratypes (specimens selected to show additional characteristics), of the type, enabling its subsequent examination for comparative purposes.

Each worker using the system uses an

independent running series of numbers prefixed by a three letter abbreviation of the workers initials or name (workers within a single laboratory may care to erect a single 'laboratory series' for convenience). This provides for the unique identification of each series. Where a type subsequently resists formal identification, publication of the *incertae sedis* type as a form species may be appropriate (for an example see Hunt, Andrews and Gilbertson 1985).

The definition of descriptive terminology suitable for all organic-walled microfossils is beyond the scope of this note and readers are advised to consult Kremp (1965), Elsik *et al.* (1983) and Traverse (1988, 297-306) for suitable terms. It should be noted, however, that the terminology employed for the description of pollen and the higher cryptogam spores is generally inappropriate for the description of algal cysts and fungal spores since apparently similar features may have quite different functions and origins.

Each major characteristic on the record sheet has a separate field number. Related characteristics are described by sub-fields. As some information (e.g. fields 20 and 22, below) may have to be added after the analysis of a given site is completed, it is convenient to hold the records as a 'working file'; following which the new records can be checked, photographic plates added and collated within the main record. The descriptive fields are defined below:

### Side 1: morphological description (Fig. 3a)

01 Type Number: the number of the type within the running series, always prefixed with the recording worker's initials, e.g. GMC 217, etc.

02 [Informal] Type Name: the name given to the type by the worker. This is an informal name and is not necessarily of taxonomic significance. If the taxon is subsequently identified, the formal name should be used.

03 Drawing: detailed drawing of the type specimen, showing (if possible) polar and equatorial views. May be annotated. Scale (in micrometers,  $\mu\text{m}$ ) must be shown.

04.1 Transparencies: used to record the film

and frame number if transparency film is employed. A note of the storage location of transparencies should also be given.

04.2 Photograph: monochrome plate(s) of type. Again may be annotated.

04.3 Notes: used to record the film/negative number and frame number of the monochrome film used. Film type/speed and exposure information may also be given.

05 Shape: the shape or overall structure of the palynomorph (see Elsik *et al.* 1983).

06 Overall Dimensions:

06.1 length of *a*-axis (generally the polar or longitudinal axis).

06.2 length of *b*-axis (generally the equatorial or transverse axis).

06.3 length of *c*-axis (applicable to non-sphaeroidal irregular palynomorphs).

07 Internal Divisions (septa): the cross wall or any wall layer partitioning the inner space of a palynomorph. Typical forms of internal divisions (septa) are shown in Elsik *et al.* (1983).

07.1 Description: the form of the division, whether simple or complex with one or more openings (see 07.4). In some cases the dimensions of divisions (in particular the thickness) will vary, this should also be noted.

07.2 Location of divisions: transverse, longitudinal, or complex (transverse and longitudinal combined).

07.3 Number: number of internal divisions.

07.4 Apertures: location and structure of openings within internal divisions.

08 Wall Structure: the arrangement of layers within the palynomorph wall.

08.1 Description: complex (many-layered or ornamented) or simple (hyaline).

08.2 Number: number of wall layers discernible in transmitted light.

08.3 Distribution: distribution of wall layers, or differentiation in the thickness of the layers across the palynomorph; uniform, thinning towards poles, etc.

08.4 Dimensions: thickness of each layer; maximum and minimum (if applicable).

09 Wall Sculpture (ornament): features which stand out in relief on the wall surface.

09.1 Description: type of sculpture.

09.2 Distribution: orientation or distribution of sculpture.

09.3 Dimensions: dimensions of sculpture forms.

10 External Appendages/Processes: hyphal, hair- or spine-like projections which anchor or otherwise ornament various fruit bodies or spores. Generally any process or outgrowth which is elongate; whether straight or flexuous. The boundary between 'sculpture' and 'appendages/processes' is obviously arbitrary; generally an appendage is considered to be more than twice as long as it is broad. Differentiation of sculpture and processes is necessary for palynomorphs which have a sculptured surface together with distinct and separate appendages.

10.1 Description: type of appendage: hyphal, hair or spine like.

10.2 Distribution: distribution of appendages across surface of microfossil.

10.3 Dimensions: dimensions of appendages/processes.

10.4 Number: number of appendages; if greater than ten give estimated numbers or 'greater than'/'less than' figures.

11 Apertures/Attachment Scars: (Aperture—a pre-formed opening in the palynomorph wall; attachment scar—an interruption of the spore wall at the site of palynomorph). In practice it is difficult to distinguish between apertures and attachment scars since this involves an interpretation of the function of a particular opening which may not be immediately

	<b>01 Type Number:</b>		<b>02 Type Name:</b>	
	<b>03 Drawing:</b>			
	<b>04.1 Transparency:</b> <b>04.2 Photograph:</b>		<b>04.3 Notes:</b>	
	<b>05 Shape:</b> <b>06 Dimensions:</b> 06.1 a:		06.2 b:	06.3c:
	<b>07 Internal Divisions (septa):</b> 07.1 Description: 07.3 Number:		07.2 Location: 07.4 Apertures:	
	<b>08 Wall Structure:</b> 08.1 Description: 08.3 Distribution:		08.2 Number: 08.4 Dimensions:	
	<b>09 Wall Sculpture (ornament):</b> 09.1 Description: 09.2 Distribution:		09.3 Dimensions:	
	<b>10 External Appendages/Processes:</b> 10.1 Description: 10.3 Dimensions:		10.2 Distribution: 10.4 Number:	
	<b>11 Apertures/Attachment Scars:</b> 11.1 Structure: 11.3 Number:		11.2 Dimensions: 11.4 Distribution:	
	<b>12.1 Colour:</b>		<b>12.2 Stain uptake:</b>	

Fig. 3 (above and opposite). Sample of incertae sedis microfossil recording sheet. (a) (above) Side 1—morphological description.



<b>01 Type Number:</b>	<b>02 Type Name:</b>
<b>13 Similar to/different from:</b>	
<b>14 Provisional Identification:</b>	
<b>15 Holotype Location:</b> 15.1 Slide Number: 15.3 England Finder Ref: 15.4 Micrometer (microscope):	<b>15.2 Sample Number:</b> hor.      vert.
<b>16.1 Storage Location of Slide:</b> 16.2 Accession Number:	
<b>17 Sample Site Location:</b> 17.1 Site Name: 17.3 Country: 17.5 Publication:	<b>17.2 Region:</b> <b>17.4 Grid Ref:</b>
<b>18.1 Sample Preparation Methods:</b> 18.2 Sample Mounting Medium: 19.1 Analysis by:	<b>19.2 Date:</b>
<b>20 Palynological Association:</b>	
<b>21 Ecological Association:</b>	
<b>22 Provisional Age of Sample:</b> <b>23 Frequency of Occurrence:</b>	
<b>24 Specimen/Sample Cross Reference:</b>	
<b>25 Literature Cross Reference:</b>	
<b>26 Additional Notes:</b>	

(b) Side 2—information on affinities/identification, find location, storage location and recovery methods.

apparent in the fossil state. In consequence they are best described together.

11.1 Structure: circular, elongate ('pores'), slit-like ('furrow'), etc.

11.2 Dimensions: length by breadth by depth.

11.3 Number: number of openings.

11.4 Distribution: random, zonate, polar, etc.

12.1 Colour: several types of palynomorph, notably fungal spores, exhibit various shades of brown due to the presence of melanin and/or other pigments. The presence or absence of colour and an estimate of the colour (using the Munsell colour system or similar) should be recorded.

12.2 Stain Uptake: state type of stain used (fuchsin, safranin, etc.) and the degree of uptake relative to pollen present in the assemblage.

**Side 2: information on type affinities/identification, find location, storage location and recovery methods (Fig. 3b)**

For ease of use, the Type Number (01) and Type Name (02) fields are repeated at the top of the form.

13 Comparable with/different from: list of types similar to (and hence possibly confused with) the described type. If possible note major distinguishing feature(s).

14 Provisional Identification: the affinities of the type and possible identifications.

15 'Holotype' Location: the accurate location of a given informal holotype is essential if the specimen is to be relocated for subsequent study and comparison with other, possibly homologous, taxa.

15.1 Slide Number.

15.2 Sample Number.

15.3 England Finder Reference: reference

using England finder (see Traverse 1988, 483-6).

15.4 Micrometer: give the microscope used and the horizontal and vertical stage micrometer co-ordinates. The stage micrometer is to be used only if an England finder is not available since the micrometer reference will only apply to a particular microscope. All locations recorded by stage micrometer should be transcribed to England finder references before archiving.

16.1 Storage Location of Slide: the institution where the slide is permanently curated or stored.

16.2 Accession Number: institution catalogue number of the slide (if different from the slide number).

17 Sample Site Location: the locality from which the sample containing the recorded type was collected.

17.1 Site Name.

17.2 Region (or county if in British Is.).

17.3 Country.

17.4 Grid Reference (if applicable).

17.5 Publication: if the described specimen has been published, or the site from which the specimen has been recovered has been published, give the full reference of the paper.

18 Sample Preparation Methods: several authors have noted the possible effects of preparation techniques and mounting media upon the definition of surface sculpture and size of pollen grains (e.g. Faegri and Deuse 1960; Cushing 1961; Praglowski 1970). In the absence of comparable information, similar strictures may apply to other organic-walled microfossils and hence it is desirable to record the preparation techniques used and the mounting medium employed.

18.1 Preparation Methods: state methods used, give reference if applicable.

18.2 Mounting Medium: state type of mountant.

19.1 Analysis by: person making record.

19.2 Date: date of first record.

20 Palynological Association: give major pollen and spore types with which the type is associated. e.g. *Betula-Coryloid-Gramineae*. This should preferably be synonymous with the local pollen assemblage zone, if so this information will obviously need to be added after the completion of the conventional pollen and spore analysis.

21 Ecological Association: give postulated environment of deposition or if taken from a modern (known) environment, describe (for example, 'Leaf litter beneath mature oak woodland'). In addition the source of ecological association data should be indicated (i.e. inference or observation).

22 Provisional Age: of the sample or the age range through which the type is present at the type site (indicate which). For example: 'Modern comparative (1990)', 'circa 3500 bp.' or 'Flandrian-Godwin Zone VI to VIII'. Again such information may only become available following the completion of the analysis or after radiometric dating and will need to be added retrospectively.

23 Frequency of Occurrence: estimate of frequency (in type sample or at type site); single specimen or abundant in many samples.

24 Specimen/Sample Cross Reference: note the occurrence of the described type in other samples or in other sites and contexts.

25 Literature Cross Reference: possible synonyms in published sources and comments as to the degree of similarity (e.g. 'Compare with type 98 of Van Geel 1978').

26 Additional Notes: any additional information not recorded above.

### Application

In practice, new 'types' are created as they are encountered. Thus it will be apparent that the type number does not reflect the morphology of the palynomorph (since the type numbers are allocated as each type is discovered) and hence cannot express the morphological (generic) relationship of the taxon in question. While the number of described types remains small this does not present an undue problem. However as the

	Type Number:	Type Name:	Type Group:	File:
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Fig. 4. Part of incertae sedis microfossil type number allocation sheet.

number of types grows it becomes increasingly difficult to locate previously recorded taxa and establish whether they are similar or a new, and undescribed, type. It is therefore necessary to impose a morphological order on the types.

To enable the storage of records in morphological order, an additional record of the numbers allocated must be kept to prevent duplication. This is simply a sheet listing the type numbers used, in the order of allocation, and the morphological group and sub-groups into which they have been placed (Fig. 4).

The records are allocated to morphological 'type groups' on the basis of four orders of characteristic. These are:

1st order

1. Shape/overall structure.

2nd order

2. Number of apertures/attachment scars.

3rd order

- 3a. Internal divisions.
- 3b. External appendages/processes.
- 3c. Wall sculpture.

4th order

- 4a. Wall structure.
- 4b. Wall colour (pigmentation).

Types are allocated to morphological groups using the rank order of characteristics. The records for each type within the first order group are held together, subdivided according to the second order characteristic, and so on. Where a type does not possess a given characteristic the next lowest rank of feature is used. For example, where sphaeroidal, inaperturate, internally undivided palynomorphs are encountered, they may only be separated on the basis of fourth order characteristics, such as the presence or absence of pigmentation and size. It should be noted that size is generally a low rank characteristic because of uncertainties regarding the possible effects of different processing techniques and mounting media.

It should be apparent that as the number of records increases, so will the potential

number of subdivisions within a given group. It will also be apparent that this morphological classification of palynomorphs does not necessarily reflect the true taxonomic relationship of the living taxa from which these organic-walled microfossils are ultimately derived. Nevertheless, such classification has considerable value in permitting the accurate recording of palynomorphs of uncertain origin.

## Conclusion

The system outlined above has proved sufficiently flexible to record many forms of palynomorph. The system at present forms the basis of a catalogue of *incertae sedis* organic-walled microfossils held in the Department of Archaeology at the University of Edinburgh. It is hoped that this will eventually form the subject of a published corpus of material together with a morphological key to the principal types.

## Acknowledgments

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## Contents

<b>Editorial</b>	53
<b>Book reviews and Conference Report</b>	54
<b>Papers</b>	
W. E. Boyd — <i>Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies</i>	63
M. G. Bell — <i>Cultural landscapes: some thoughts stimulated by Bill Boyd's paper: Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies</i>	69
Martin Jones — <i>Comments on Boyd: Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies</i>	71
K. D. Thomas — <i>What's in a name? Anyone can be an environmental archaeologist</i>	72
M. van der Veen — <i>Commentary on Boyd's Towards a conceptual framework for environmental archaeology: environmental archaeology as a key to past geographies</i>	76
W. E. Boyd — <i>Reply to Bell, Jones, Thomas and Van der Veen</i>	77
Philip L. Armitage — <i>Post-medieval cattle horn cores from the Greyfriars site, Chichester, West Sussex, England</i>	81
Stephen Carter — <i>The distribution of the land snail <i>Vitrea contracta</i> (Westerlund) in a calcareous soil on Martin Down, Hampshire, England</i>	91
Bruce Levitan — <i>A method for investigating bone fragmentation and anatomical representation</i>	95
G. M. Coles — <i>A note on the systematic recording of organic-walled microfossils (other than pollen and spores) found in archaeological and Quaternary palynological preparations</i>	103

# *Circaea*