

# *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: Flower heads of wild and fullers' teasels (cf. p. 9); drawings by Mike Hill, Environmental Archaeology Unit.*

## Editorial

This item is almost otiose in an issue which should appear together with at least one other.

May we remind you, *however*, that material for subsequent issues is always welcome. We no longer have 'copy deadlines'—it's easier for us to deal with papers as they come in and produce an issue when there is enough material to fill it.

The copy in this and issue 8(2) has nearly all been provided on computer floppy-disk and it has made production very much easier. Files were sent on Amstrad PCW disks in LocoScript, and on PC 3.5" and 5.25" disks in WordStar and WordPerfect. We should be able to read documents written in Word and some other word-processing formats; the most intractable problem is the format of the disk which (apart from Amstrad PCW) must be readable on an IBM-compatible PC. It should also be possible for us to read documents from Macintosh computers, but it is probably best to confirm with the Editors before sending electronic copy to us. We still need hard copy, of course, for reading and refereeing.

Readers may have noticed the insinuation into *Circaea* of a *Short Contributions* section. This has been adopted in order to encourage the submission of short or rather informal pieces which would not sit happily elsewhere, but which merit publication for some reason or another. We will be fairly catholic with regard to the content, but note that we also intend to introduce a *Notes and Queries* section. This should be a place for simple announcements of the discovery of unusual taxa or the illustration of mystery objects. Items of this kind have been published in *Circaea* in the past under the now defunct *Miscellany* heading.

On the subject of past issues, may we advise that some are now effectively out of print, especially vol. 2 (2) and (3) and the whole of vol. 3? Although we reprinted some parts of vol. 1 many years ago, the cost of reprinting early issues would now be prohibitive unless we were assured of a substantial demand.

This issue includes two short contributions arising from a bone taphonomy workshop held at the University of York in September 1991; further papers will appear in the next issue of *Circaea*.

## Conference Reports

### The wetland revolution in prehistory

This was the title given to a conference held at Exeter University under the auspices of the Prehistoric Society and WARP (Wetland Archaeology Research Project) on April 5-7th, 1991. First of all, full marks to Bryony Coles and her team for organisation, accurate maps, spot-on timing, very comfortable and supportive hall of residence, etc. All the speakers turned up, with the exception of two Estonians, and they ranged from the interesting to the fascinating. It was exceedingly international, with only five British speakers, and the subjects of two of these were early agriculture in Latin America and New Guinea (Professor Harris and Dr Bayliss-Smith). John Evans had a more theoretical subject and John Coles summed up, leaving only Francis Pryor with a British site (he was the only speaker common to this and the AEA's wetland conference held in Norwich in 1986).

Some people objected to the conference title, pointing out that wetland archaeology really started a long time ago in Switzerland, but the only reference to this came in the account of a commemorative exhibition held in Zurich, given by Dr Ruoff in a suitable post-prandial style. There were two major fires at this exhibition which defeated the local fire brigade, but it was well insured and the accidents converted into useful experiments.

North America was well represented (particularly at question time). On the north-west coast they excavate by power-hose, revealing villages completely buried by mudslides (Professor Croes). In Florida they have to contend with site developers who make ours in Britain seem like angelic conservationists; one of them thought it a huge joke to have the bulldozed bodies (not just skeletons, whole bodies) of 58 individuals in his spoil heap. Other, more conscientious citizens, who offer recently unearthed canoes and the like to Dr Purdy and her team at the University of Florida, can only be asked to put them back where they found them, as conservation funds will only cover the superb carved wooden works of art which are also found in large quantities.

Finance is a problem nearly everywhere. Dr Goren-Inbar struggles on a hand-to-mouth

basis with an Acheulian site on the River Jordan, complete with *Elephas antiquus*, which has strata tilted to 45° by subsequent tectonic movements. There is little prospect of further work, let alone publication, in Poland, apart from the tourist attraction of Biskupin (Drs Brzezinski, Piotowski and Newiarowski). Dr Andersen is fortunate to have the services of amateur divers in Denmark (sounds of approval from Andrew Selkirk). Dr Andersen trained the only speaker with adequate finance, Mr Matsui from Japan, where there is a full-time team carrying out rescue excavations by the thousand. A coffer-dam has been built to allow the excavation of a lake-bed site at Asoka, where there are many tons of food deposits.

Wetland excavators must, of necessity, pay more attention to the environmental sciences than some of their dry-land colleagues. However, they remind me of Winston Churchill's dictum that scientists should be on tap and not on top when they casually refer to 'our botanist' and 'getting the dendro done' (an honourable exception was Francis Pryor).

Europe did get a bit of a look in. There were interesting sites in North Germany (Dr Gramsch) and on the Seine (double act by the brothers Mourant). Dr Dolukanov demonstrated the coincidence of many Russian epipalaeolithic sites with the shores of glacial lakes, in a paper which I thought the most impressive of the meeting. The Irish have formed their own wetland group, and have found a number of trackways (Dr Rafferty), but whatever happened to the Dutch, who only managed to field a chairman of a session?

All in all, this was a most enjoyable and informative weekend. AEA members were not in evidence, though some Sheffield students were. WARP is an interesting organisation; some of its members are WAVES (Wetland Archaeology Volunteer Environmental ... I didn't get what the S stood for!). They seem to take life less seriously than the Prehistoric Society, to judge by their logo, a delightful little man in a coracle, with a basketwork hat, drawn by John Coles.

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## **Biological anthropology and the study of ancient Egypt**

This colloquium, held at the British Museum, London, in July 1990, was sponsored by the Department of Egyptian Antiquities at the museum and the Bioanthropology Foundation, Sausalito, California. The first two days covered a variety of topics concerned with physical anthropology, while the final day was devoted to bioarchaeology (zoology and botany). Although the geographic and cultural theme was ancient Egypt, studies from other regions illustrated approaches which could be usefully applied. The final session was an appraisal of bioanthropology in general for the study of ancient Egypt.

### **Groups of human remains**

The first papers discussed skeletons, from whole populations of varying dates in Sudanese Nubia (Prof. George Armelagos, University of Florida), to a specific group of four skeletons buried close together in Abusir (Eugen Strouhal, National Museum, Prague). These studies concentrated on the relationships between individuals, using the information which each yielded to make broader deductions about the group. Comparisons between skeletal material and grave goods from Naga-ed-Dêr were made to discuss burial practices (Patricia Podzorski, Lowie Museum). Dr Simon Hillson and W. A. Scott (Institute of Archaeology, London) illustrated a new statistical model for describing the range of variation within a population, using ancient skulls from Sedment (9th Dynasty) and Badari (predynastic).

Incidence of disease and injury and their effect on individuals and populations was discussed by several participants. The diseases looked at in detail were: tuberculosis (Dr Jane Buikstra, University of Chicago), schistosomiasis (Dr Robert Miller, University of Cambridge), and cranial injuries of Canary Islanders (Drs Martin, Anton and Gonzalez, Tenerife), while a survey of incomplete ancient Nubian remains excavated early this century from Aswan covered a variety of rather gruesome ills and injuries (Theya Molleson, Natural History Museum, London). In the course of this session, Corinne Duhig described the very large collection of ancient Egyptian human remains held at Cambridge, and appealed for investigators to come forward to study them.

### Categories of evidence from human remains

Dr Benson Harer (San Bernadino) began the second day with a well-presented overview of health in ancient Egypt. The next two papers dealt with dental evidence. These were a general discussion of dental anthropology (Prof. Jerome Rose, University of Arkansas), and an interesting study relating tooth development to growth (Simon Hillson, again). Prof. Fawzia Hussein (National Research Centre, Cairo) presented a reassessment of the controversial skeletal remains which some have thought to be those of Akhnaten (tomb 55, Valley of the Kings). She concluded that the bones are not those of the so-called heretic pharaoh.

The remainder of the day was given over to Dr Svante Pääbo (University of California), Drs Robert Hedges and Robin Sykes (University of Oxford) and J. H. Goudsmit (University of Amsterdam) who discussed a (then) relatively new field: the study of ancient DNA. These were challenging presentations for the non-specialist. Nonetheless, the speakers made clear both that this technique is still very much in the process of development, and that DNA analysis will be a valuable tool for many of the questions which are asked about human remains.

### Zooarchaeology and archaeobotany

The first session on the last day illustrated the range of techniques which can be applied to the study of archaeobotany. Taking advantage of the excellent preservation in Egypt, combined with modern collecting work, a survey is being compiled of the development of Egypt's flora, through the Holocene to the present day. This valuable work is under the direction of Dr Nabil el-Hadidi (Herbarium, University of Cairo). Drs John Edmonson and Piotr Bienkowski (Liverpool Museum) followed with the results of a chemical analysis, a technique only recently used in archaeobotany, to identify essential oils from a Graeco-Roman funeral wreath. The final paper of the morning was presented by Alan Clapham (University of Cambridge) on the wide-ranging goals for archaeobotanical research which can be applied to a settlement site.

Bioarchaeological case studies made up the second session. A wealth of animal remains in graves and village contexts from Kerma has

allowed some fascinating cultural conclusions (Dr Louis Chaix, Natural History Museum, Geneva). The next two papers looked at the preliminary work on the archaeobotany (Mary Ann Murray) and zooarchaeology (Dr Barbara Ghaleb, both Institute of Archaeology, London) of Memphis. Finally, an interdisciplinary approach to the study of bread and beer in ancient Egypt was described by this reviewer (University of Cambridge).

### Final appraisal

A theoretical paper (Friedrich Rösing, University of Ulm) and a deliberately provocative discussion by field director Dr Mark Horton (University of Oxford) presented two very different overviews. As with all the papers, these provoked questions and comments, although Horton's sparked particularly lively debate.

Several themes emerged over the three days of presentation, and these were further developed during the final discussion. Many people emphasised the need to apply standards to data recovery, recording and analysis. This is a prerequisite for cross-comparison between sites, otherwise it is difficult for any individual study to be set in a wider context. However, before broad comparisons can be made, it is essential to establish the range of variability within populations or assemblages of biological material. It was suggested that physical anthropologists should concentrate on small local populations, before trying to compare widely-dispersed individuals or groups. Much attention was given to the problem of ageing human remains, and the desirability of conducting a study of an ancient and documented population as was done for bodies in the crypt at Christchurch Spitalfields, London. However, this leads to another problem: the frequent lack of suitable material available for study and the difficulty of post-excavation research on Egyptian material.

It is unusual to find the broad spectrum of bioanthropology presented in one conference, and valuable for those attending to hear of work which is normally outside their immediate reference. As the colloquium progressed, it became clear that information exchange amongst project participants is crucial. Breadth of outlook was strongly advocated throughout the meeting. After such

a stimulating discussion, it is good to hear that the colloquium will be published. Speed is aimed for, particularly because the field is changing so rapidly.

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### **Review of the fifth Institute of Field Archaeologists Conference, April 1991**

This was my inaugural IFA meeting. It promised a range of papers on diverse subjects from the traditional (the Iron Age) through methodology (research design and report preparation) to the more ethereal (visions of archaeology). The meeting was, as always, held in the University of Birmingham, this time concurrently with major structural repairs to the Howarth building, where most sessions were given.

Inevitably only a selection of sessions could be attended and, failing to discover the role of archaeology in Green politics during the first session of the trendily titled 'The Green Debate: What Place Archaeology?', I moved to the pertinent session on research designs in archaeology. Coming from the relatively protected environment of archaeology in York I had been under the illusion that most projects were preceded by at least a rudimentary research design, based on discussion with specialists and subject to reappraisal as necessary. Apparently not so, as Andrew Lawson, speaking in place of Peter Chowne, illustrated. Most speakers stressed the importance of planning for contingencies, and retaining an element of fluidity and formulation of appropriate questions for post-excavation analysis. It was gratifying to find that at last the importance of assessment of material during excavation, and immediately subsequent to it, is being recognised as important by the archaeological community. *Archaeology and Planning* (DoE 1990; PPG 16') was recognised as the document providing the impetus for the rationalisation and improved presentation of research designs. Research Priority, it seems, has replaced Rescue as the catch phrase for the 1990s.

Sir David Wilson gave the requisite witty post-dinner speech, accompanied by

unspeakable wine (where do some universities buy their wine from?), and alcohol flowed freely afterwards (to drown out the earlier sample). Rumour has it that in past years the bars have run dry early, but fortunately a contingency bar was set up. Despite the brisk walk from the hall of residence to the lecture theatres next morning, the session (on burial archaeology) was entirely appropriate to the fragile condition of the participants.

The attitudes and roles of the Home Office, church, coroner, and the law to the discovery, study and disposal of skeletal remains were discussed by representatives from the various bodies. The results of a questionnaire circulated by Charlotte Roberts and Jacquie McKinley had shown how woefully ignorant of the procedures most archaeologists were. It was refreshing to see non-archaeologists contributing to an archaeological conference, and all speakers were entertaining. Francis Green ended the session with a paper on the ramifications following on from his father being buried beneath a tandoori restaurant!

Perhaps it was the hangover, but the session on the Iron Age seemed to the uninitiated to say very little new, several papers reworking old interpretations concerning hillforts and their role in society. J. D. Hill provided the only contentious paper in the sessions I attended, attacking Cunliffe's interpretation of Danebury. Unfortunately Professor Cunliffe was not present to enliven the debate. Are archaeologists becoming more conventional?

The evening's drink and disco entertainment followed an evening dinner up to the usual University of Birmingham catering standard. All conference attendees should be warned about the food and advised to seek an alternative venue, unless nostalgic to relive the experience of school dinners. For the price charged for over-salted and over-cooked offerings one could enjoy a three-course restaurant meal either locally or in the centre of Birmingham itself.

The session on 'Environmental Archaeology: Integration or Specialisation?' began at 9.00 am on the final morning. Admirably organised and chaired by Mark Maltby and Mike Allen, questions of how to integrate environmental reports with other aspects of archaeology, and what audience to target, were addressed.

Although it was encouraging to see environmental archaeology assume a frontline



role in an archaeological conference, the subject matter was equally relevant to all aspects of archaeological publication. Papers by Mike Allen and Sebastian Payne emphasised the need to target reports to the intended audience. Successful integration requires continuous involvement of all participants in the report from the conception of the research design through to final production of the report. Selectivity of information for publication was stressed by a number of speakers, along with the need to resist urges to impress colleagues with one's grasp of up-to-the-minute jargon and terminology, and by presenting pages of raw data. Sebastian Payne argued for presentation of only as much data as was required for the validity of interpretations to be judged. Julie Gardener, for the Council for British Archaeology, aimed to curb over-enthusiastic report writers by pointing out the costs of producing data-heavy reports, and of correcting errors spotted after submission. Technical terminology and Latin names are clearly a turn-off as far as publishers and the wider British audience are concerned. However as Martin Jones pointed out, reports should also be understandable to an international audience, to whom imprecise common equivalents to scientific names may be incomprehensible. The problem of where to store the raw data and results which were not required in the text was discussed at length, but not surprisingly no firm conclusions were reached. As a first move—at least for those working for/with English Heritage—it was suggested that cheap paper copies of technical reports not submitted as Ancient Monuments Laboratory reports could be circulated to interested colleagues. Perhaps the Association for Environmental Archaeology could support this move and circulate lists of available reports?

One of the highlights of the conference was meant to be a session on 'Archaeology and Politicians'. Being politicians, the speakers did not turn up at the originally specified time, so the session was moved to a slot running parallel with that on Environmental Archaeology. Consequently most AEA members present, including myself, did not attend, so I cannot report.

The IFA, unlike the AEA, does not arrange site trips for its members. Perhaps it should be suggested?

The IFA annual conference is not just open to members and, to judge from the badges worn (red = member, blue = non-member) a fairly even mix attended. The cost ranges according to income and, of course, residential status. Assuming non-residential status, it can work out reasonably cheap. If nothing else it provides a useful way of meeting a wide range of archaeologists informally, and contacts between different sub-disciplines can only be a 'good thing'. Sessions concerning biological concerns within archaeology must help erode traditionally perceived barriers between specialists and field archaeologists, and I hope that environmental archaeology will assume an increasingly high profile.

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## Short contributions

### A question of scale: Material in cave ash from Arene Candide, Italy, was not textile

#### Introduction

Meetings of the Association for Environmental Archaeology provide a useful means of contact between specialists from diverse fields. At the Butser meeting in July 1990, Richard Macphail drew my attention to some shrunken material in Neolithic cave ash from Arene Candide, Italy, which the excavators thought was knitted wool textile. I subsequently received photographic transparencies of the material, from which I was able to report that it was almost certainly not textile. My report was too late to allow alteration of the paper by Macphail *et al.* (1990), which was already in the press. The present note is a repeat of my correction for a wider readership.

#### Material and methods

Two transparencies taken under plane polarised light were supplied of a 25 µm thick cross-section of the material (one is reproduced in the figure here). They had frame sizes of 0.33 mm and 0.116 mm, indicating the width of the field of view in each case and therefore calculation of the magnification. The material had a mesh-like

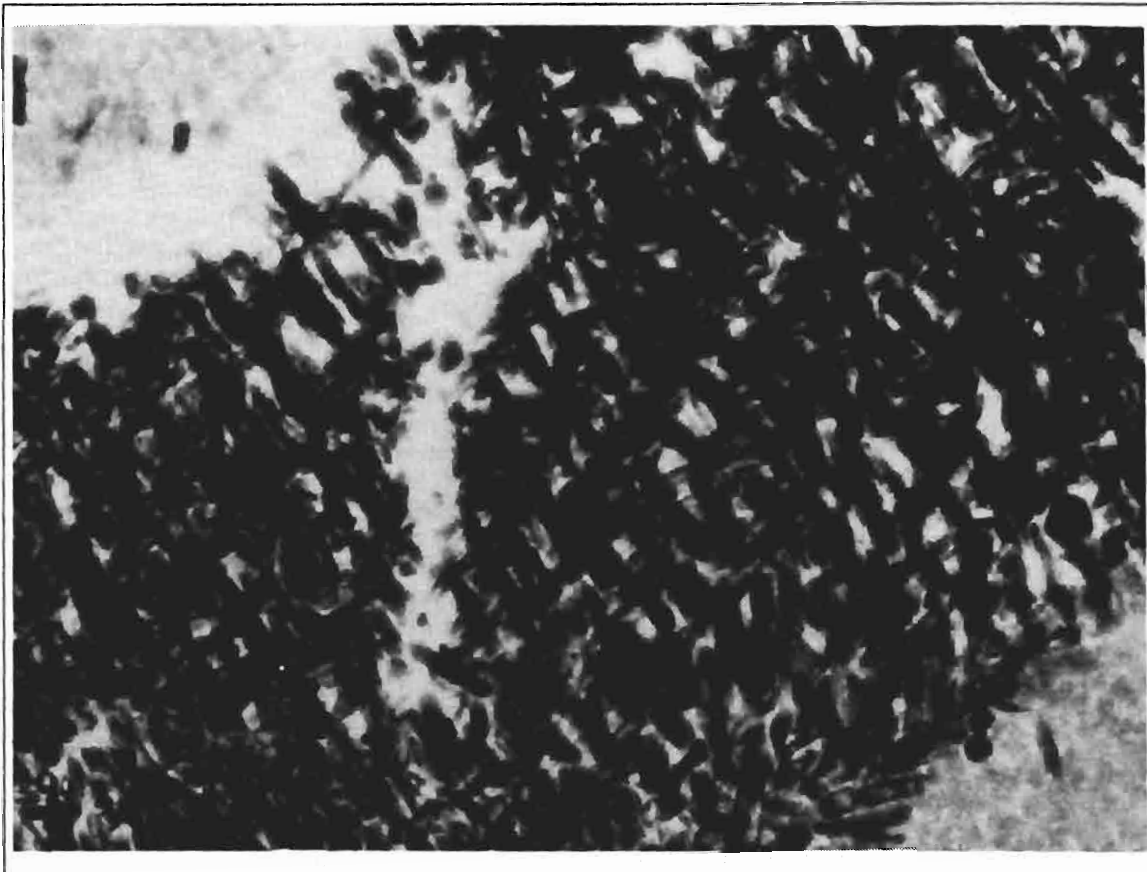


Figure 1. Photomicrograph of the material from cave ash at Arene Candide, Italy, under plane polarised light. The material came from cave ash containing sheep/goat stabling detritus that is thought to have shrunk by 90% on burning. The grey material at the edges is wood ash. Frame length 0.33 mm (from Macphail et al. 1990)

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#### Wool fibres in cloth remains throw light on fleece evolution

I have long used the fibres in textile remains to study the way in which different kinds of fleece developed in domestic sheep during prehistory and history. Notably, large collections of cloth from the Danish Bronze Age, the Roman site of Vindolanda on



Hadrian's Wall and from British medieval towns have been investigated. More finds described in several papers published during 1990 have filled gaps in the record and thrown new light on the first stage of fleece development in the early Bronze Age.

Neolithic sheep had short hairs obscuring very fine underwool in a coloured coat like that of deer. A sheep surviving from this period is the 'wild' (feral) mouflon of Corsica. Haired skins preserved in the Iron Age salt mines at Hallstatt in Austria have now shown that the Neolithic type of sheep without a fleece persisted alongside fleeced sheep into the Iron Age (Ryder 1990a). During the 1960s I used wool remains from the Danish Bronze Age to show that the first fleece to develop was a primitive hairy type (the 'Hairy-medium' fleece), from which developed a primitive woolly type ('Generalised-medium' fleece). The same fleece types are seen in the hairy and woolly, brown Soay sheep that survive on St Kilda, off the north-west coast of Scotland.

What the recent studies have shown is that some of the earliest Bronze Age textiles from Denmark had wool (with no hairs) which was as fine as that of the underwool of the Neolithic sheep. It therefore appeared to have been combed from a hairy coat intermediate between a fleece and a hair coat if not from the Neolithic hair coat itself (Ryder 1990b). Before the development of shears in the Iron Age, primitive sheep (which have a natural spring moult) had their wool removed by plucking or combing.

Iron Age wool had the same two hairy and woolly fleece types as in the Bronze Age, but there was now a greater range of colour—black, white and grey in addition to the brown of wild and Bronze Age sheep. The evidence for this comes from the large collection of the c. 100 AD textiles from the Roman site of Vindolanda that I measured during the 1970s. Since archaeologists thought that the cloth was of local manufacture this indicated that the wools came from local sheep. A surviving sheep with this range of colours and fleece types is the native Orkney breed. The stimulus to breed sheep with white wool was associated with the development of dyes.

The large collection of cloth from Hallstatt, dated up to eight hundred years earlier than the Vindolanda remains, has the same range,

not only of colours, but of fleece types, so confirming that the Vindolanda wools are typical of the Iron Age (Ryder 1990a; 1992).

All Roman sites, including Vindolanda, have also produced a few examples of modern fleece types—'Semi-fine' (shortwool), 'Medium' wool (some of which later became longwools) and 'Fine' wool, and it was thought that these began to develop in the Roman period. An additional find from Hallstatt was the presence of some Semi-fine and Medium wool fleeces, which indicates that these types were already emerging in the Iron Age. The Fine fleece developed in the Near East and the Mediterranean area during the Roman period and later emerged in Spain as the modern Merino breed.

Until the Hallstatt material became available for study, very few Iron Age samples of wool had been examined. One I published in 1961 came from the Scythian, frozen burials at Pazyryk in Siberia, dated c. 400 BC. This was a piece of sheepskin with the wool intact, which therefore indicated the appearance of the fleece. It was a white, primitive hairy type. A larger collection of wools from Pazyryk (kept in the Hermitage Museum, Leningrad) has now been measured (Ryder 1990c). As well as the expected primitive hairy and primitive woolly types, there were also Semi-fine and Medium fleeces, which support the finding from Hallstatt that these modern fleeces were developing in the Iron Age. Of particular interest was one very fine sample, apparently combed (like the Danish Early Bronze samples) from a Neolithic type of coat (seen at Hallstatt only on skins, Ryder 1992).

I have been looking for Neolithic wool for over 30 years and now it seems to be emerging in an unexpected way. More finds are needed to elucidate further the very first stages in fleece development.

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[This contribution is reproduced, with modifications, from *Archaeological Textiles Newsletter* 12, 13–15 (1991).]

#### Appendix: Wool fibre terminology and definitions

Mammals have hair, and the wool of sheep is a kind of hair. But wool biologists divide 'wool' into three types of fibre: short, thick *kemps*; long, less-coarse *hairs*; and finer, true *wool* (which itself can be coarse, medium or fine). Kemp and hair are collectively referred to as 'hairy fibres'. The coat of wild and Neolithic sheep had only very coarse kemp and very fine wool. Such 'hairy' sheep are best described as 'non-fleeced' to distinguish them from woolly, fleeced sheep. Fleeces are primarily composed of wool, but many have varying, smaller proportions of kemp and hair, depending on the fleece type.

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#### The last teasel factory in Britain, and some observations on teasel (*Dipsacus fullonum* L. and *D. sativus* (L.) Honckeney) remains from archaeological deposits

##### The last teasel factory in Britain

I came to hear of the existence of a factory processing the flower heads of fullers' teasel by a rather circuitous route and arranged to visit the factory in suburban Huddersfield, West Yorkshire, with Philippa Tomlinson late in 1989. We were shown round by the manager of Edmund Taylor (Teazle) Ltd., Mr T. J. Ledger. This firm, founded in 1849, is the only one of its kind surviving in Britain and supplies all the needs of the home industries in which teasels are used.

The teasels processed by Taylor's are both home-grown and imported. The British crop currently comes from five farms in Somerset

and one in Kent, each supplier sowing about 0.5 acres (0.2 ha); Taylor's is the sole buyer. These teasels come in bunches cut in the field and have to be trimmed and graded in Huddersfield. The foreign crop is Spanish, the growers there sending the heads ready trimmed and graded.

Taylor's supply teasels to the British market and to Australia, the United States of America, Canada and India, but not to the rest of Europe, who are supplied by firms in Spain. The main market is the woollen industry where wire 'teasing' mechanisms—though now widely used—have never proved entirely satisfactory for the final raising of a nap in the finishing of woollen cloth. Teasels are also used in the paper industry and in the manufacture of felt, and a modern outlet (especially in the United States) is the decorative use of heads as ornamental 'hedghogs'.

At the factory, the teasels for the cloth industry are sorted by machine for length and diameter, though their quality is judged by hand. Quality varies depending on the source and the season of the crop, British teasels (it is said) usually being superior to imported ones.

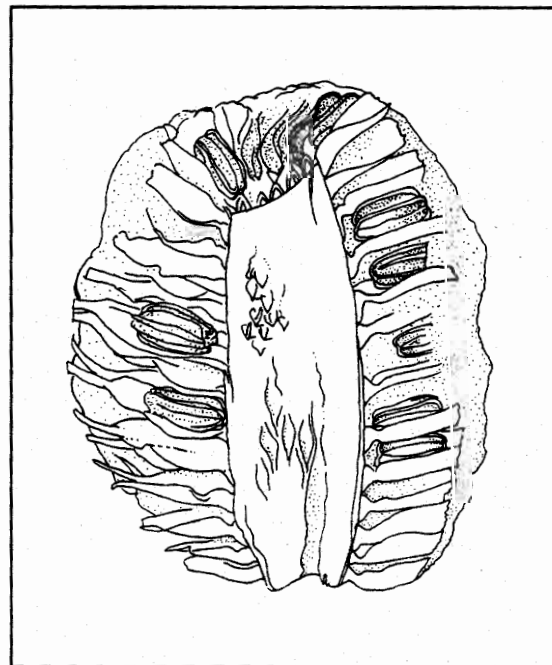


Figure 2. A half-head of *Dipsacus sativus* from twelfth century deposits (layer 185) at Eastgate, Beverley, N. Humberside (site code BE84). Magnification approximately x2.

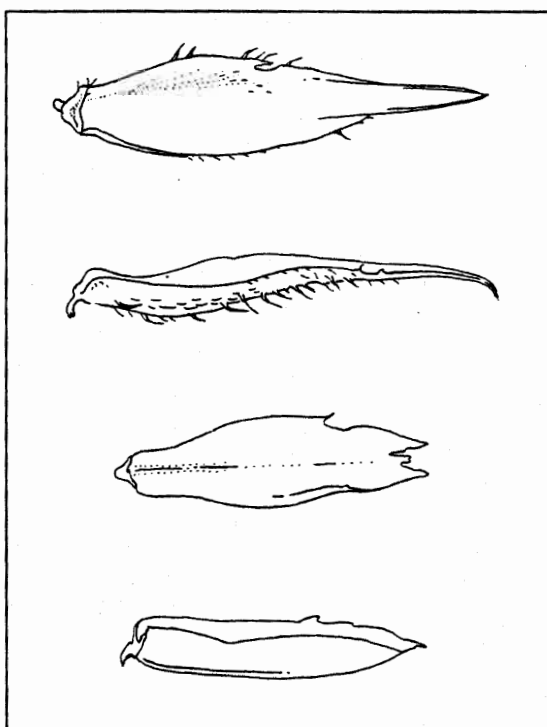


Figure 3. Receptacular bracts from modern reference specimens of *Dipsacus sativus* (above) and *D. fullonum* (below), after boiling in dilute hydrochloric acid. Magnification x3

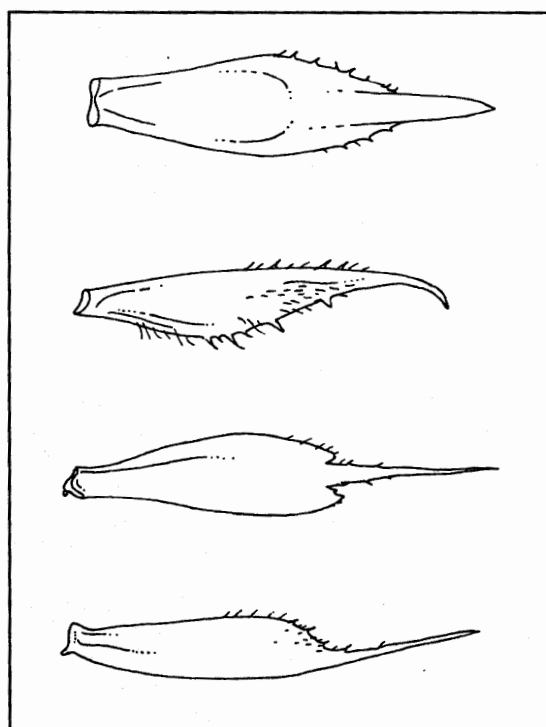


Figure 4. Untreated receptacular bracts from modern heads of *Dipsacus sativus* (above) and *D. fullonum* (below). Magnification x3.

Trimming of the stalks and basal (involucral) bracts is done largely by hand with secateurs. The heads may then be treated in one of two ways: they may be turned on a lathe-like machine to square off the ends, and a hole drilled in either end, so that they can be set on spindle barrels or 'Lana' brushing machines or, with a short length of stem still attached, they are set in two rows between parallel rods, each composite 'rod' taking 120 heads. Twenty-four such rods are fixed to the drum against which the cloth is drawn to raise the nap (teasing usually takes place more than once to raise the nap—often wires are used initially, and teasels reserved for the final raising). Although both sides of the heads are used before the rods are discarded, one-quarter of the rods are replaced on the drum each day. That one firm can supply all the cloth industry's needs at such a turnover of teasels is perhaps an indication of how much the woollen industry has declined in recent decades.

'Seed' from the heads is retained by Taylor's, treated for eelworm (the main crop pest, infestations of which can result in very poor

quality heads), and returned to the growers. The market is currently (early 1992) buoyant, though there is not enough business for Taylor's to employ more than a single worker-manager.

Anyone interested in contacting Taylor's is welcome to contact Mr Ledger at Edmund Taylor (Teazle) Ltd., Green Lea Mills, Cross Green Road, Dalton, Huddersfield, HD5 9XX, W. Yorkshire. (N.B. The spelling of *teazle*, here, is the alternative given by the Oxford English Dictionary; I follow Clapham *et al.* 1989 in using *teasel*.)

#### Archaeological teasels

By one of those chances that I have described in the pages of this journal before as serendipitous, our visit to Britain's last teasel factory preceded by only a few weeks the analysis of medieval deposits from excavations of the Dominican Priory in Beverley, North Humberside (Foreman, forthcoming) in which teasel fruits and receptacular bracts were recorded from a pit fill (context 185). The deposit was part of a

sequence which accumulated outside the line of the Priory precinct and which may in fact pre-date the founding of the House (dating by artefacts is to the twelfth century). That textile working was going on in the area was attested by the presence in other deposits of the sequence of remains of certain or probable dyeplants: root fragments of madder (*Rubia tinctorum* L.), stem fragments of dyer's greenweed (*Genista tinctoria* L.), pod fragments of woad (*Isatis tinctoria* L.), seeds of weld (*Reseda luteola* L.) and leaf fragments and fruits of sweet gale or bog myrtle (*Myrica gale* L.). The plant remains from this site are considered in more detail by Allison *et al.* (forthcoming).

Similar macrofossil assemblages, including dyeplant waste and teasels had been identified from excavations of an adjacent property in Eastgate, Beverley (McKenna in press) and it may be that this is part of the same phase of occupation of the area. At first the *Dipsacus* material from the Eastgate excavation, which included a half-head (Fig. 2), was thought to be wild teasel, *D. fullonum* L. (nomenclature follows Tutin *et al.* 1976). The bracts ended in a smooth spine which looked very different from the reference material of fullers' teasel, *D. sativus* (L.) Honckeney (I have rejected

*Dipsacus pilosus* on the basis that it has very different achenes and much smaller receptacular bracts).

However, the whole fossil bracts were very much narrower in their basal portion than were the bracts of either of the two teasel species and this suggested that some tissue had decayed from them. To test this, bracts from reference material of *D. fullonum* and *D. sativus* in the Environmental Archaeology Unit, University of York, were boiled in dilute hydrochloric acid for about 20 minutes. They were then left (unintentionally!) for several days before being examined (representative examples are shown in Fig. 3). Under the binocular microscope it was clear that in both species there is an outer layer of delicate tissue which would easily decay in the ground. In *D. fullonum* the spines themselves are also very flimsy after acid treatment and gentle scraping with a needle caused these to break off, leaving only the basal portion of the bract intact. In *D. sativus* the cilia at the base of the spine (Fig. 4)—which feel stiff and appear likely to be resistant to decay—were found to be no more than processes on the soft superficial tissue and were easily removed by gentle scraping with a needle. The central bristle-like spine that was left was very like

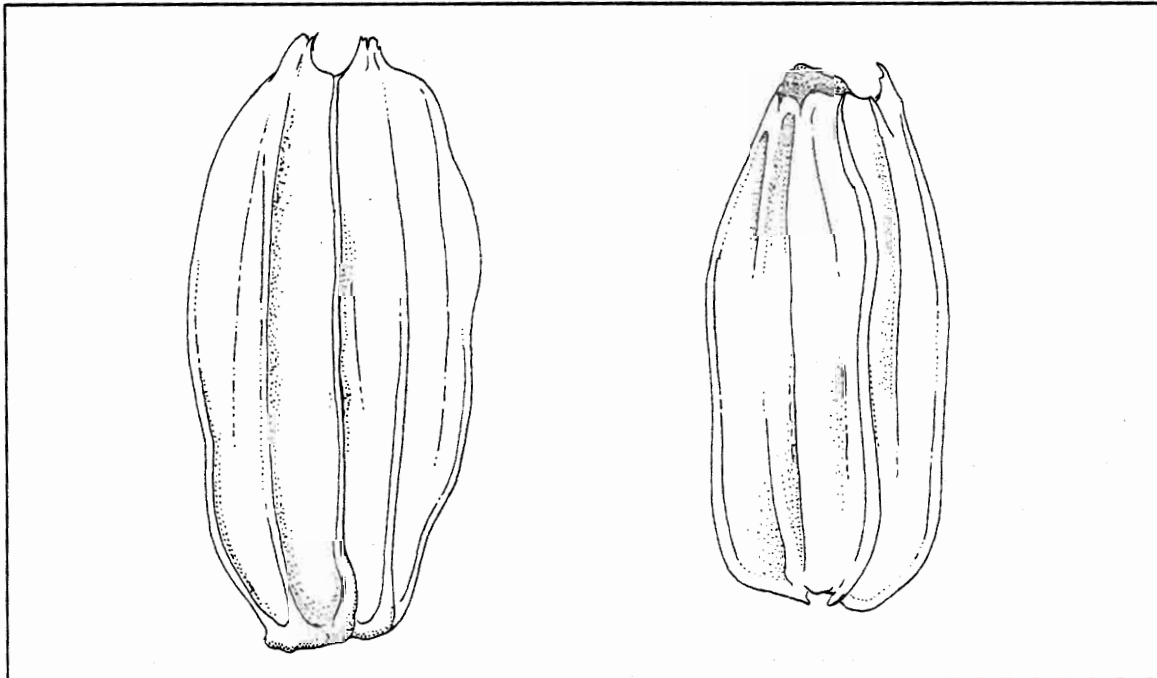


Figure 5. Achenes from 12th century layer 185 at the Dominican Friary site, Beverley, N. Humberside. The right-hand achene has a clear double rib on the left-hand face, indicating that it is *D. sativus*; the left-hand lacks doubling and might be *D. sativus* or *D. fullonum*. Magnification x15.

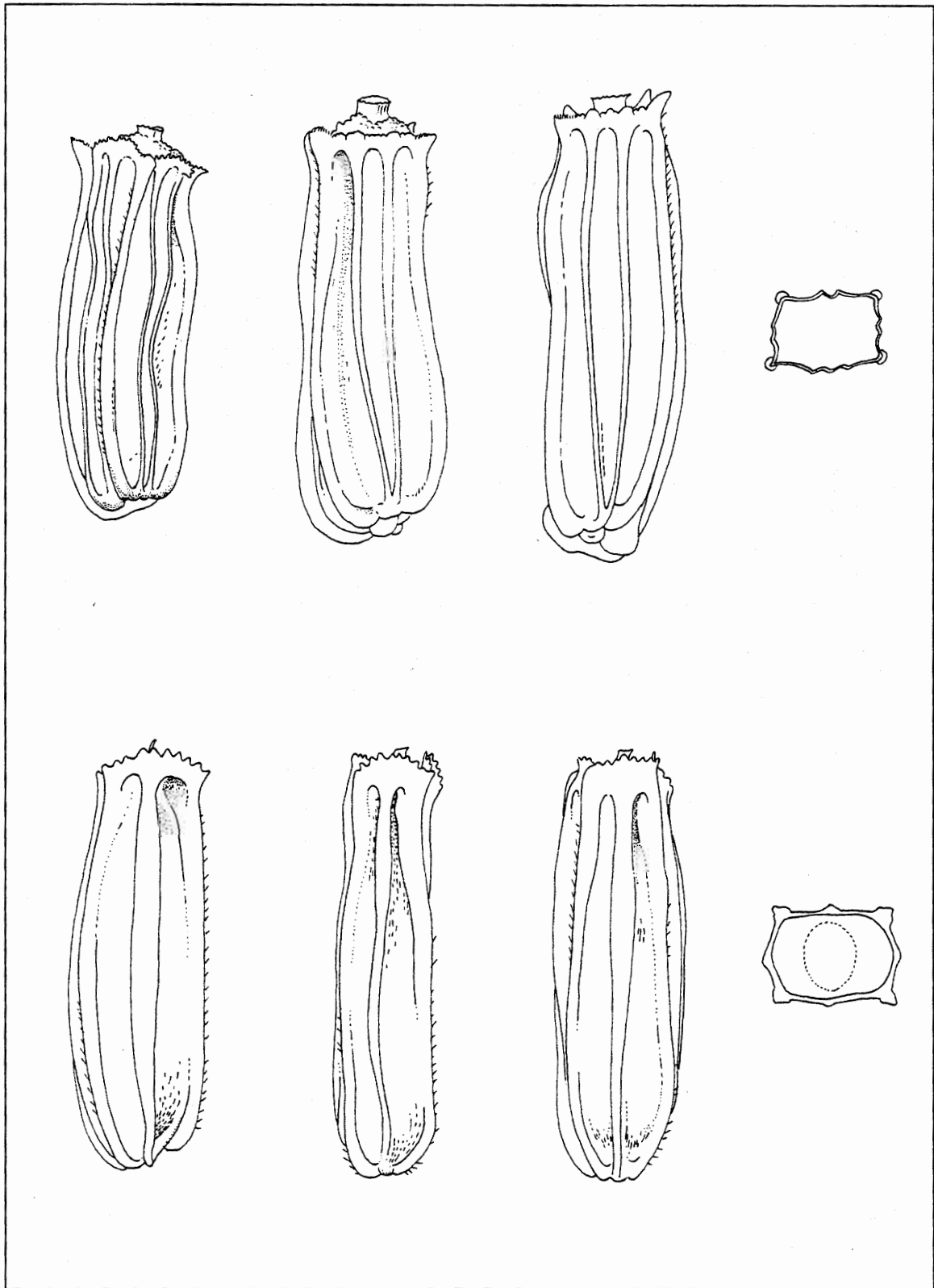


Figure 6. Modern reference specimens of fruits of *Dipsacus sativus* (above) and *D. fullonum* (below). Magnification x15.

the fossil material in retaining a degree of springiness and a slightly recurved form.

The *Dipsacus* fruits from context 185 at the Dominican Priory excavations at Beverley were also scrutinised more closely. The modern reference material available suggested that there were subtle differences in size and shape between the two species, with *D. fullonum* being, on average, a little longer and narrower than *D. sativus*, though the fossil material was usually somewhat flattened or even fragmentary. A difference in pubescence between the two—if it was, indeed, a reliable character—was also unlikely to be of much use with fossil specimens. The most reliable character would seem to be the presence of double ribs on the faces of the achene (Fig. 6) in *D. sativus*. Although not all specimens may show this, and extra ribs may only be present on one of the four faces of the fruit, I have not seen them in the material of *D. fullonum*, which has simple, single ribs on each of the faces.

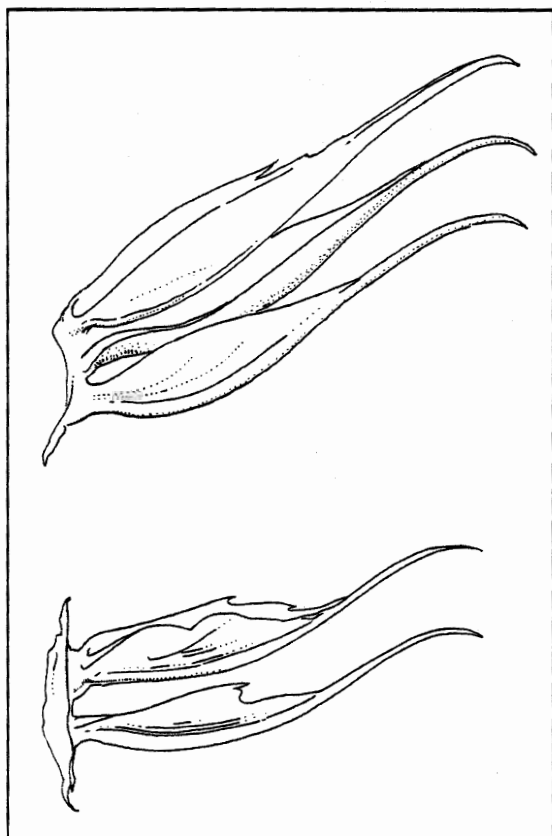


Figure 7. Receptacular bracts of *Dipsacus sativus* from late ninth century deposits (layer 26721, sample 1732) at Coppergate, York (site code 1976-81.7). Magnification  $\times 4$ .

On this basis, I determined the bulk of the achenes and all of the bracts from the Eastgate and Dominican Priory deposits as *D. sativus* (cf. Fig. 5) and undertook re-examination of some more material, from Anglo-Scandinavian deposits at 16-22 Coppergate, York. *Dipsacus* bracts and/or fruits from three contexts of a cess-pit fill from this site, dated to the period AD c.850–c.900 led to the re-determination of the bracts (Fig. 7) and of some of the fruits as *D. sativus*. There were remains of *Isatis tinctoria*, *Diphysium complanatum* (an exotic clubmoss implicated in dyeing as a source of aluminium for mordanting) and weld in one of the layers containing teasel remains.

#### Acknowledgements

I am grateful to Barrie McKenna for bringing my attention to Edmund Taylor (Teazle) Ltd., and to Mr T. J. Ledger both for his kindness during our visit to his factory and for checking several points in an early draft of this paper. Mike Hill (Environmental Archaeology Unit) undertook the painstaking work of drawing the teasel material and is offered sincere thanks.

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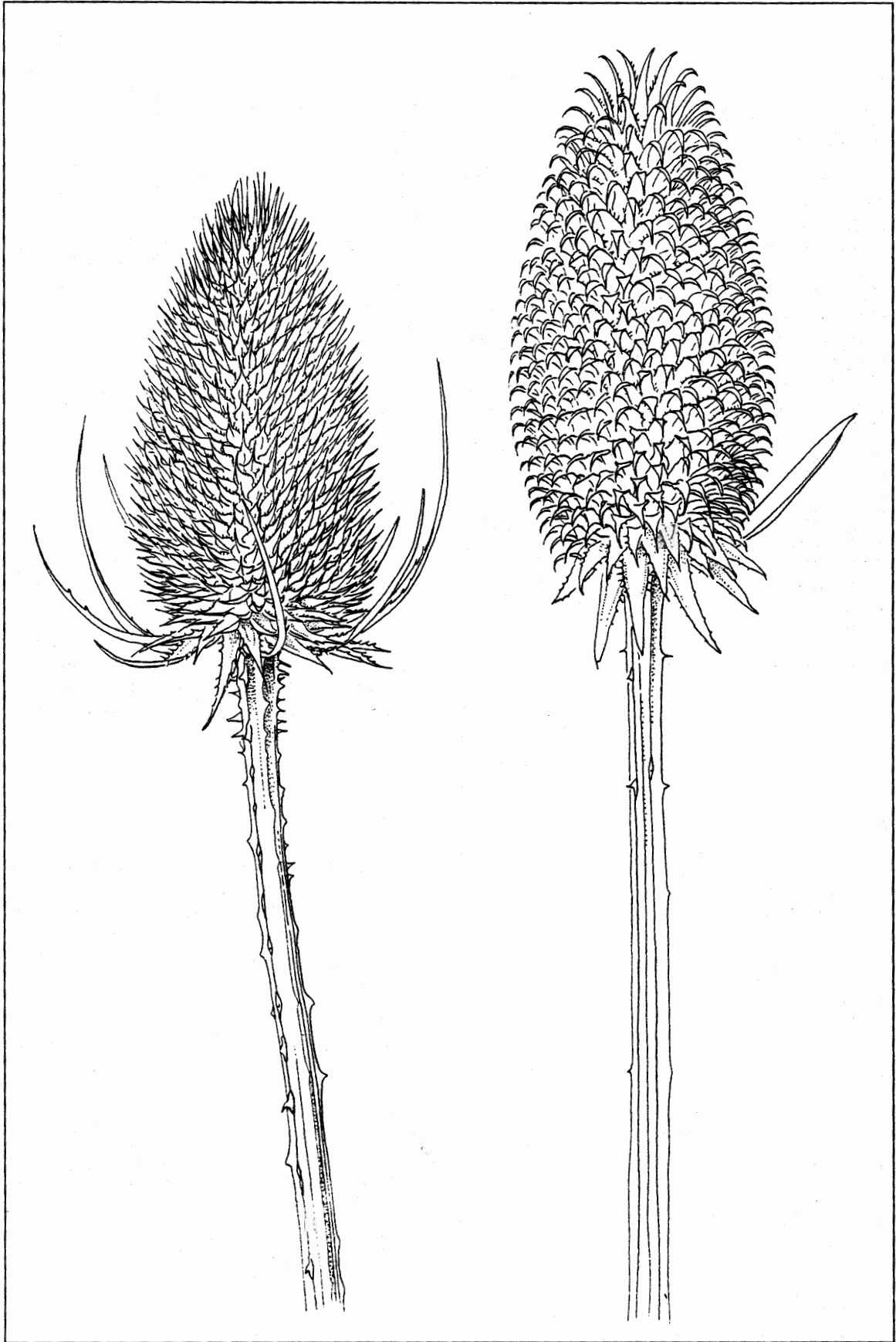


Figure 8. Drawings of modern reference specimens of mature flower heads of *Dipsacus sativus* (left) and *D. fullonum* (right). Scale approximately 1:1.

Postscript to *The last teasel factory in Britain*

Although I have not examined them in detail, the spines present on the stems (and probably also those on the midribs of the leaves of teasels) seem superficially very similar to the prickles of *Rubus* and *Rosa*. They appear to be exogenous, as in *Rosa*, but it is possible that poorly preserved material of these three genera may be confused. —ARH

**Papers from the bone taphonomy workshop at York, September 1991**

**Bones and beyond bones: insects, stains and keratin remains**

While some aspects of taphonomy may now be well understood, there are clearly other areas of the subject deserving further exploration. Two very different topics have recently been occupying my thoughts, and it seemed to me that they deserved mention at this taphonomy workshop.

First of all, there is the question of unusual states of preservation. Do we as yet fully recognize their research potential, and indeed might special states of preservation offer to be especially valuable in studying the chemistry and molecular biology of organic remains? This point can be illustrated by means of three examples:

(a) Special states of preservation as a result of the change of body fat to adipocere is well known to forensic pathologists, but to what extent can it help in the preservation of animal remains and would such a process assist in the preservation of the bone chemistry? An example of adipocere was accidentally produced in a partly defleshed pig carcass I buried in sand some five years ago. On excavation a year ago, many of the well preserved bones were covered in deposits of white cheese-like adipocere. Given further burial time (but how much?), the adipocere would have slowly decomposed, but it could have had a long-term effect on the quality of bone preserved.

(b) Although it has long been known that bronze/copper staining on bone, because of its antibiotic properties, leads to an area of very well preserved bone, no significance has been attached to this fact. Also, the excavator tends to overlook the fact that hair and leather seems to preserve in such an environment (and there is one case of helminth preservation). Again, it would seem worth questioning whether such well preserved areas might have special value in chemical or biomolecular analyses.

(c) Finally, as regards special states of preservation, it seems to me that we may be

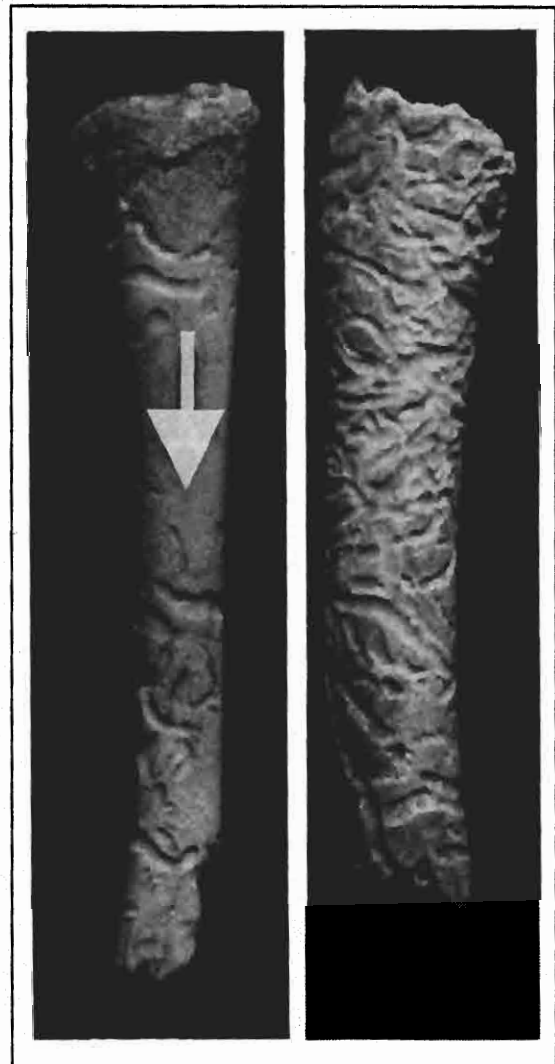


Figure 9. General detail of two Roman chicken long-bones from Uley, Somerset, displaying two degrees of severity of channelling damage.

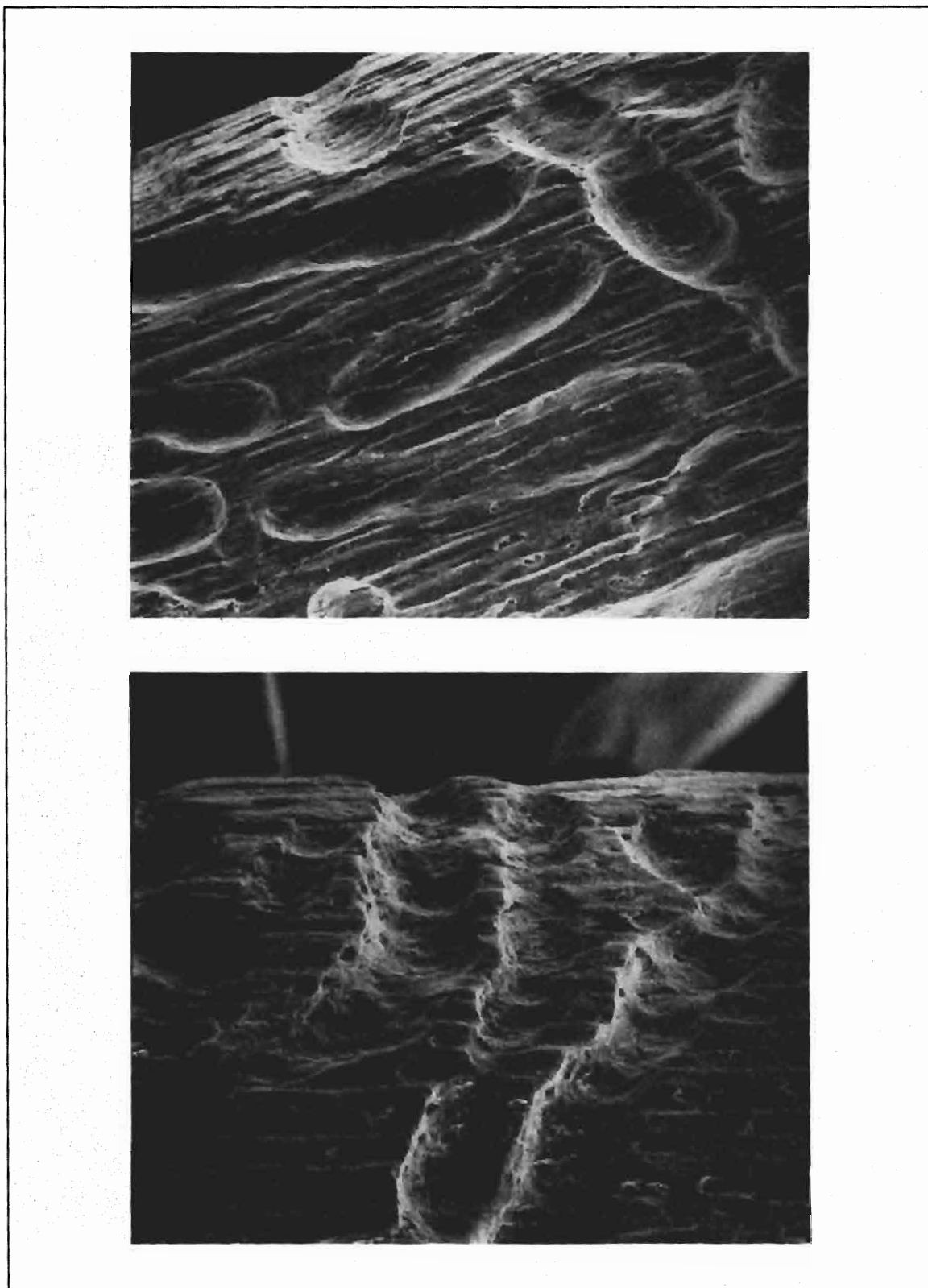


Figure 10. Scanning electron micrographs of possible insect damage to Roman bone from Uley, Somerset (WH79, 1242). Above: x35; below: x50.

missing keratin preservation in bog and other anaerobic environments, simply because we do not alert the excavators to the possibility of hair, hoof and horn at some sites. This fact has been driven home to me recently, not only by the bog body experience, but also by an Orkney peat cutter noting hair and indeed handing over to Keith Dobney parts of a sheep (leg pieces only). As aspects of the chemistry of nutrition are locked up in keratin as well as bone, we may thus be missing out on valuable analytical material.

The second taphonomic topic I want briefly to mention is that of insect damage to bone. My previous experience of this has been limited to damage on bones from Nubia and the island of Socotra, the latter case apparently being the result of ants. Others have been aware of potential insect damage, but there is still not enough recognition of the fact that such species might differentially eliminate bone. But this isn't the only problem. The question I want to ask here is, can we yet recognize different kinds of insect damage? The example I want to give is of Roman chicken bones from Uley, Somerset, U.K. Of the large sample of *Gallus*, many displayed well defined tracking across the bones (Fig. 9) and in some instances this had led to fragility, breakage or collapse of the bone.

Because initially I had suspected root damage as the cause, a simple experiment was carried out with the body of a dead cat which I had just buried. Directly over and onto the corpse, I seeded various plants with a view to encouraging roots to 'etch' into the bone. While such experimenting is limited and inadequate, it was not possible to set up a more rigorous experiment at the time with multiple chicken corpses seeded with a wide range of plant species! On excavation, the cat skeleton was found to be covered in large and fine-meshed roots. While the bone surface was damaged and eroded, the destruction did not simulate the distinctive narrow channelling as on the Roman bones. In form (Fig. 10), the damage to Roman bone is remarkably similar to that caused to wood by engraver and bark beetles (Scolytidae). So could it be beetle damage, say by dermestids? As far as I can ascertain, dermestids remove un-mineralised tissue but do not damage bone? Do any of my colleagues know otherwise?

## Acknowledgements

I am grateful to Stuart Laidlaw and Sandra Bond for photographic and SEM assistance. Keith Dobney kindly provided for study the Orkney peats with enclosed pieces of sheep feet.

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## On using *Bonerec*: Bruce Levitan's computer recording program

### Summary

*Bonerec* is a bone recording program based on dBASE III PLUS. It is easy to set up and use, and has a manual. Records are made onto four, linked files: the main record (context, species, etc.), measurements, age data and comments. There is validation, a supporting macro, and a method for ordering the records. Analytical programs have been written for listing by species and by context, and for summarising.

### Introduction

When Bruce Levitan worked at Bristol City Museum, he used the Ancient Monuments Laboratory bone recording system. When he moved to Oxford University Museum and had access to an IBM-compatible PC, he decided to write his own recording system based on the most widely used commercial database program, which at that time was dBASE III PLUS. *Bonerec* was developed during his time at Oxford. Bruce having now left archaeology (to work with computers in the Health Service), it seemed important that information about the program be presented; it is a large piece of work, with a manual of 82 pages and a program and related files of similar length. There are aspects which could be altered and improved, but this author has found it easy and reliable to use, and it has many useful, time-saving and well-thought-out features. Others may want to use it, or use parts of it (for example, the species and anatomy codes) or ideas from it.

## Getting started

The program requires dBASE III PLUS to run it (it is not compatible with dBASE IV), and a computer with a hard disk and at least 640K RAM. Help is given in the manual about how to set up the dBASE and Bonerec directories, and the DOS AUTOEXEC.BAT file. The user then types 'bones', and the setting up is done through a series of questions:

'Are you using a colour monitor?'

'Do you wish to read the directory set-up and information screens?'

'Do you wish to use a new sub-directory?' (If so, there are questions and information about doing so, and if not there is a prompt for entering the sub-directory needed, and if you've forgotten its name, a listing may be requested.)

'Do you wish to use the validation procedure?' (If used, only valid entries for species, anatomy and zone will be accepted; if 'No' is answered, you are free to use these three fields as you like).

This is followed by a menu screen, with the choices:

- 1 - CREATE NEW SITE FILE (see below)
- 2 - USE EXISTING SITE FILE (You will be given a modified version of 1, including a list of existing files.)
- 3 - BACKUP SITE FILES (You are talked through copying files, in dBASE or comma delimited format.)
- 4 - PROGRAM INFORMATION (The information screens are useful both for bone recording and for computer learning.)
- 0 - EXIT

Assuming you have chosen 1 above, you will be given help in naming the four files needed, a list of existing files (if any) and will be asked:

'Which main file do you wish to use?'

'Which measurement file do you wish to use?'

'Which ageing file do you wish to use?'

'Which comments file do you wish to use?'

and then, 'Do you wish to modify number sequencing?' (of which, more later).

This is followed by the main BONE RECORDS MENU:

- 1 - Information about the system
- 2 - Append records to existing file
- 3 - Edit records (This uses dBASE's EDIT function with a screen form which shows all the fields on one screen.)
- 4 - Renumber records
- 5 - Display records on screen (dBASE BROWSE)
- 6 - Locate for specified characters (a search facility)
- 7 - Add new taxon/anatomy codes
- 0 - Exit

Of these, the user will usually want to choose 2: append new records.

## Bone recording

Having chosen 2 above, the Context screen will appear, in which the context number should be entered and four other references may be entered, e.g. feature, feature type, phase, small find number. On pressing the key <PgDn> (page down), the main Bone Recording Form will appear (Fig. 11). Here, it is filled in to show the context information already entered, and one, complete, left sheep or goat radius. In the lower half of the screen, 'Yes' has been entered for 'Measured', 'Age', 'Butchery' and 'Comment'.

On pressing <PgDn>, the program will check that the entries for species, anatomy and zone are valid ones (if not, help may be requested). It will note whether 'Yes' has been answered to any or all of 'Measured', 'Age' or 'Comments'. In this case (Fig. 11), all three have been flagged, and the following screens will be shown: a measurements information screen giving a suggested measurement list for the common species; a measurement entry screen (up to 14 measurements per bone); an epiphysial fusion screen (if a long bone; or, if

**B O N E R E C O R D I N G F O R M**

Context: 1227 Ref [2]: F120 Ref [3]: PIT Ref [4]: 3 Ref [5]:

Taxon: SG

Anatomy: RAD

Number of specimens in this record: 1

Symmetry: L

Zone: 123456

Fragment size: 5

Indicate whether or not each of the following are recorded: Y for yes, N for no

Measured: Y

Age: Y

Butchery: Y

Comments: Y

Chewing: N

Pathology: N

Horncore or antler description: N

READ

||<C:>||TEMR

||Rec: 6/6

||Num

Figure 11. The main bone recording form for Bonerec.



a tooth or mandible, two screens, for recording teeth using Payne's (1973; 1987) and/or Grant's (1982) method(s)); and a comments screen, which will take up to three lines, each of 60 characters. This is followed by:

'Do you wish to keep the same context?'

If so, there is no need to repeat the context entries, and a new Bone Recording Form is presented.

Species and anatomy are recorded using abbreviations. The recording method for the zone field is defined in the manual, but can be changed to suit the user (the field is seven characters wide). The fragment size field is a modified Ancient Monuments Laboratory coding (1: <25% complete; 2: 25-49%; 3: 50-74%; 4: 75-99%; 5: 100% complete). This field is not validated and any four characters could be entered.

The program is supported by a macro (Superkey, a commercial macro program), so that, for example, in normal use the entries needed in the above 'Getting started' section will all be done by pressing F1 and then F2; and the common species and parts of the anatomy will be entered using 'hot keys', the control key for species and the alt key for anatomy, e.g. <Ctrl>p for PIG and <Alt>f for FEM (femur). The macro does not have to be used, but it saves time.

Editing is straightforward for most fields, but where a species, anatomy or zone is changed, some knowledge of the system is required since the species, anatomy and zone order numbers will also require editing (see below). And where 'No' has been incorrectly recorded for 'Measured', 'Age' or 'Comments', as the program stands at present some knowledge of dBASE is necessary: the record file has to be altered to 'Yes' (easy); and, using dBASE III PLUS, a record needs to be appended in the Measurement, Ageing or Comments file showing the specimen number and data required. In practice, I chose to do most editing using dBASE rather than Bonerec's edit options, but this was out of an intention to maintain and extend my familiarity with dBASE. It is possible to use Bonerec with only a limited knowledge of dBASE itself, but clearly the greater one's knowledge, the more one is in control.

## Structure

The program was designed to be easy to use and to be as flexible as possible within the limitations of a very structured database. The use of four files saves space: where no measurements are taken, no empty fields are stored; and—similarly—for the age data and comments. Conversely, where there is a comment to make there is sufficient space. The file structure is shown in Appendix 1. The fields in the Main Record File are identical with those seen in Fig. 11, with the exception of no. 18, SPEC\_NO, and nos. 20-22, SPORDER, ANORDER and ZORDER.

The SPEC\_NO (specimen number) is added by the program to the Main Record file. When 'Yes' has been entered for 'Measured', this number will also be written to the Measurements file, thus providing a link between the files. The SPEC\_NO can be started at any numeral. The question 'Do you wish to modify number sequencing?' when setting up a new set of files, allows the user to start the new set at a higher number than the last number of the previous set. This will prevent duplication of specimen numbers when, for example, a new set of files are used for a new phase—a useful feature.

At the point where <PgDn> is pressed on completing the Bone Recording Form, the species, anatomy and zone entries are checked against three databases. At the same time, three numerical codes for the entries are written to the SPORDER (species order), ANORDER (anatomy order) and ZORDER (zone order) fields, which allow the records to be put in order. Extracts from the three databases are shown in Appendix 2. The lists probably cover most of the species and anatomical parts needed; 254 species of mammals, birds and fish, and 219 anatomical parts (for mammals, birds and fish) are listed. Where new species, bones or zones are required, this is easily done by altering the relevant databases, which are independent of the program itself. A likely situation for wishing to add new codes is for either/or categories, e.g. RED/FAL could be added, with a number between Fallow deer and Deer. [These databases could be used as adjuncts to other computer recording systems].

This author has not so far altered BL's program. Several improvements could be made, and this is not in principle difficult. The

program is not compiled, and is clearly set out, with titles and explanations.

#### Listing and summarising

Bonerec is a bone recording and not an analytical program. (BL intended to tackle the analytical side at a later date.) Records made using Bonerec are ordinary dBASE files, which can be manipulated in dBASE III PLUS or dBASE IV, or read by other programs, e.g. Paradox, SuperCalc5. BL wrote some programs, for example, a search facility (see 6 of the Bone Records Menu) and a program for totalling species and showing percentages.

This author's first requirement, on having recorded a bone assemblage, was to have a sensible, ordered copy of everything which had been entered, on A4/21 cm-wide paper, both to browse through and to be the bone archive. This necessitated learning some dBASE programming, and BL's help in doing this is acknowledged. (De Pace's 'dBASE III PLUS' (1987) has proved useful). A brief description of the work done is given. At present these programs are not user friendly, but the variables (path, file names, titles) are clearly marked in the programs.

List by species. The records are ordered by species, anatomy, symmetry and zone. The site name, phase and column headings are shown at the top of each page; the species sub-titles are shown in full ('Horse' not 'HOR', 'Large unidentified mammal' not 'LAR') and the total for each species is given. It proved possible to get most of the information recorded onto a single line per record: context, other references, specimen number, N, anatomy, zone, fragment state, fusion (from the Age file), an abbreviation (M A B Ch P) where 'Yes' had been flagged (to Measured, Ageing, Butchery, Chewing or Pathology) and the Comments (running onto a second or third line where necessary). There is an option to start each new species on a new page.

List by context: a similar list of all the records, but by context, and showing totals for each context and sub-totals for each species within the context. This could be used to print out particular context(s), for example at the end of a recording session. (It had been decided that computer recording should replace not duplicate a manuscript record).

Measurements and tooth data. These are shown separately, but repeat relevant data, e.g. the measurements list shows the context, specimen number, fusion and zone data, and a 'c' if a comment was made.

Anatomical analysis. This is a detailed anatomical analysis showing for each bone of each species: total number of bones, number classed as fragments, the total for each individual zone for the left, right and left+right sides, the total number of zones per bone and the average number of zones present.

Any of the above can be used to show particular groups, e.g. the pathological bones, a given area, building, type of feature, or group of contexts. It has been useful, while writing up a site, to be able quickly to print out a detailed anatomical analysis of, for example, an important group of post-holes, and see species and anatomy totals, minimum numbers, zone totals and numbers classed as fragments; and to be able to refer to the context printout for the primary record and comments made.

A number of other programs have been written, for example: to remind me how to index the main record file; to test whether the species, anatomy and zone entries match correctly their respective numerical codes (a mismatch can occur for example where the species has been edited but the SPORDER field left unchanged); and to join two sets of files.

#### Access

The Bonerec program is available free from the author (address below) at the time of publication. Users should send a formatted 5¼" or 3½" disk (IBM-compatible, with at least 650k space) which will be returned with BL's Bonerec and GJ's Boneanal files loaded. A copy of the manual can be supplied on disk or a paper copy may be borrowed and freely photocopied. A copy of dBASE III PLUS is essential (current price c. £395 + VAT) and a copy of Superkey is recommended (current price c. £65 + VAT).

#### Acknowledgement

I am very grateful to Bruce Levitan for help and support in this work, and for commenting on a draft of this paper.

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Appendix 1. Bonerec files structure

Main record file

	Field name	Type	Width
1	SPECIES	C	6
2	ANATOMY	C	6
3	NO_SPEC	N	4
4	CONTEXT	N	4
5	REF2	C	6
6	REF3	C	4
7	REF4	N	2
8	REF5	N	4
9	SYMMETRY	C	3
10	ZONE	C	7
11	FRAG_STATE	C	4
12	MEASURED	L	1
13	BUTCHERED	L	1
14	PATHOLOGY	L	1
15	CHEWING	L	1
16	AGEING	L	1
17	HC_ANTLERS	L	1
18	SPEC_NO	N	6
19	COMMENT	L	1
20	SPORDER	N	7, 3
21	ANORDER	N	7, 3
22	ZORDER	N	6, 2
Total			84

Measurements file

	Field name	Type	Width
1	SPEC_NO	N	6
2	MEAS1	N	5
3	MEAS2	N	5
and so on to:			
15	MEAS14	N	5
Total			77

Ageing file

	Field name	Type	Width
1	SPEC_NO	N	6
2	PROX	C	2
3	DIST	C	2
4	PDP4	C	3
(Payne dp <sub>4</sub> )			
5	PP4	C	3
6	PM1	C	3
7	PM2	C	3
8	PM3	C	3
9	GDP4	C	1
(Grant dp <sub>4</sub> )			
10	GP4	C	1
11	GM1	C	1
12	GM2	C	1
13	GM3	C	1
Total			31

Comments file

	Field name	Type	Width
1	SPEC_NO	N	6
2	COM1	C	60
3	COM2	C	60
4	COM3	C	60
Total			185

Notes: C—character, N—numeric, L—logical; '3'—to 3 decimal places.

*Appendix 2. Examples of the species, anatomy and zone abbreviations and codes used in the bonerec program*

**Taxon codes and names**

Code number	Taxon code	Full name
<b>Common mammalian taxa (archaeological order)</b>		
1.000	COW	Cattle
2.000	SHE	Sheep
2.010	GOA	Goat
2.020	SG	Sheep/Goat
3.000	PIG	Pig
4.000	HOR	Horse
4.010	DON	Donkey
5.000	DOG	Dog
6.000	CAT	Cat
7.000	DRAB	Rabbit (domestic)
10.000	RED	Red deer
10.010	ROE	Roe deer
10.020	FAL	Fallow deer
10.990	DEER	Deer
<b>Other mammalian taxa (taxonomic order)</b>		
100.000	HEDG	Hedgehog
101.000	MOLE	Mole
102.000	CSHREW	Common shrew
102.010	PSHREW	Pygmy shrew
102.030	WSHREW	Water shrew
102.990	SHREW	Shrew

... and so on.

**Anatomy codes and names**

Code number	Anatomy code	Full name
9.000	ATLAS	atlas
9.100	AXIS	axis
9.200	CERV	cervical vertebra
10.000	THOR	thoracic vertebra
11.000	LUMB	lumbar vertebra
12.000	SAC	sacrum
13.000	CAUD	caudal vertebra

13.500	PGS	pygostyle
14.000	VERT	vertebra
15.000	RIB	rib
15.100	CC	costal cartilage
15.200	RAY	fin-ray bone
15.300	SPN	spine
15.400	BRANCH	branchiostegal ray
16.000	STERN	sternum
16.500	FUR	furcula
16.700	COR	coracoid
16.800	CLV	clavicle
17.000	BAC	baculum (os penis)
17.100	CLT	cleithrum
18.000	SCP	scapula
19.000	HUM	humerus
20.000	RAD	radius
21.000	ULN	ulna
22.000	CAR	carpal
23.000	MC	metacarpal
23.100	MC1	metacarpal I

**Key to zone codes**

Zone	Zone number
<b>Long bones and mandible:</b>	
123456	6.01
12345-	6.04
-23456	6.05
1234--	6.09
-2345-	6.11
--3456	6.12
123---	6.13
-234--	6.15
--345-	6.16
---456	6.18
12----	6.19
-23---	6.21
--34--	6.22
---45-	6.23
----56	6.25
1-----	6.27
-2-----	6.29
--3----	6.30
---4---	6.31
----5-	6.32
-----6	6.35

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## Short contribution

### A working classification of sample types for environmental archaeology

The staff of the Environmental Archaeology Unit (EAU), York, have for some years now used 'in house', and in conjunction with York Archaeological Trust and some other excavation organisations, a standardised classification of samples for biological analysis. The need for such a classification arose for two reasons: to ensure that appropriate samples were collected for the intended analyses and to facilitate control of laboratory processing, data interpretation and report preparation using a computer-based data interrogation system.

The sample types described below have been used successfully by the EAU for a good number of years. It must be remembered, however, that the treatments of samples summarised here do not represent a template for all archaeological sites. They represent the methods which have been developed in the Unit to deal with deposits which are primarily from urban sites with complex, commonly 'waterlogged' stratigraphy. They have evolved considerably and will doubtless continue to do so, and some aspects of them (for example, the typical sizes for bulk samples and the size of mesh used) are inappropriate to some kinds of sites or recovery needs.

The kinds of samples used by the EAU, together with the processing methods typically applied to them and the biological remains for which they are analysed, are discussed in turn (note that the abbreviated forms **GBA**, **BS**, etc. are used in computer databases containing sample catalogues and information concerning the progress of processing). It is EAU policy that sample material (other than, for example, timbers and pollen samples) is stored in plastic tubs; the use of polyethylene bags makes storage very much more precarious and increases handling time very substantially, and is to be discouraged (see Appendix for details of plastic tubs and indestructible labels). Samples usually comprise 'whole sediment', with nothing removed.

We have found it essential to follow this classification of sample types rigorously; failure to do so causes serious interpretative problems as well as administrative difficulties (especially in terms of database management).

### 1. GBAs (General Biological Analysis samples)

*Nature of sample:* These comprise 5-10kg of sediment from a layer, preferably with the sampling location accurately recorded. Current EAU policy is that *all* layers are sampled in this way—redundant samples can be discarded but, once excavated, unsampled sediment is lost for ever. GBAs are normally taken from some convenient or significant point(s) within a layer but may be collected as a 'column sample' traversing a series of vertically contiguous layers (for molluscs from a buried soil for example), when layer boundaries *must* be respected. GBAs furnish subsamples for a variety of other analyses as well as permitting laboratory description and investigation of the sediments themselves. *Function:* GBAs are used for analysis of plant macrofossils, invertebrates (particularly insects, molluscs, and eggs of parasitic worms) and (rarely) for small vertebrate remains. They also provide a 'voucher' of the original sediment for lithological description and long-term storage and as an insurance against unforeseen requirements for analysis. *Storage:* Cool storage, preferably in the dark, is essential if samples are to be kept for more than a month or so—and they usually are! Sample material, especially of sediment rich in organic matter, commonly degrades in poor storage. *Processing methods:* GBAs are normally inspected and described in the laboratory using a standard *pro forma*. Subsamples from them are mostly processed using a minimum sieve mesh of 0.3 mm aperture, following procedures of Kenward *et al.* (1980; 1986), but small subsamples are also routinely used for analysis of parasite eggs. *Resulting physical archive:* This may comprise small vouchers of original sediment; glass or card slide mounts of various small biological remains; dried residues; residues stored wet in preservative; insect 'flots' (*sensu* Kenward *et al.* 1980) in 'alcohol' (industrial methylated spirit, IMS) in glass jars; and vials or other containers with assorted remains, dry or in various preservatives.

### 2. BSs (Bulk Sieving samples)

*Nature of sample:* BSs are collected once the GBA sample has been taken. Optimal size varies according to the nature of the site and layer, but generally a minimum of around 50 kg (approximately 30-50 litres) is taken. The maximum practicable size is, perhaps, 100 kg. Small contexts may of course produce less than the optimum quantity, in which case all the material remaining after the GBA sample has been collected will normally be bulk sieved. If it is desirable to process very large quantities of material from a single layer, a series of separately numbered samples is taken; the location of each sample *should* be accurately

recorded! *Function*: BSs are taken to allow a general assessment of the coarser component of the layer, to recover small artefacts, and for analysis of larger plant remains, large insects, molluscs and vertebrates (primarily fish, birds and small mammals). *Storage*: Unprocessed samples are too bulky for easy storage, so sieving normally takes place on site. The resultant residues and washovers (*sensu* Hall and Kenward 1990, 296) are best stored in tubs. Whether they are stored wet or after drying will depend on their nature, the research objectives of the project and, more fundamentally, on the practicability of large-scale drying. Dry material can be stored almost anywhere; organic material (and, of course, certain artefacts such as iron objects!) in wet residues/washovers will eventually degrade unless kept cool and preferably also in the dark. *Processing methods* (see Kenward *et al.* 1980): BSs are normally sieved in water using mesh of 1 mm apertures, whether plastic mesh or conventional metal sieves. Mesh of 300 µm is sometimes substituted where smaller remains—e.g. charred cereal chaff or, exceptionally, insect fragments—are to be recovered. Sieved material is normally dried, and often re-sieved using mesh of 2 mm aperture before sorting. *Resulting physical archive*: Wet (subsequently usually dry) residues and washovers, preferably stored in tubs, and a variety of different kinds of biological remains recovered from the samples (usually stored dry in vials, polyethylene bags or boxes).

### 3. SRs (Site Riddled samples)

*Nature of sample*: Ideally, whatever remains of the layer after the above sampling procedures have been carried out; this may be only a few tens of kilogrammes or as much as a tonne or more of sediment. If the layer is very large, it should be divided into a series of separately numbered samples (perhaps using 10–50 buckets as a maximum according to the nature of the deposit). Each of these samples should be separately located to allow analysis of variation through the layer. *Function*: SRs are processed to recover a wide variety of medium-sized artefacts and to provide the principal vertebrate assemblages. *Storage*: Since SRs are normally riddled and sorted on site; typically only dry bone is retained for analysis by the environmental archaeologist. Cool, dry conditions are preferable. *Processing methods*: SRs are sieved using a coarse mesh, usually 10–12 mm aperture and normally on a riddling frame incorporating a powerful water spray. Bone, artefacts and any other components required are sorted on site, the residues being discarded after recording. *Resulting physical archive*: Normally dry bone, often in very large quantities.

### 4. Hand-collected Material

Although traditionally the principal method of recovering bones (as well as artefacts), hand collection has repeatedly been shown to provide a biased sample and ideally should be avoided. If it is desirable to collect particular bone material, for example articulated elements, whole skeletons, or especially poorly preserved material, it is preferred that a SPOT sample (below) should be taken and appropriately cross-referenced in the record. Routine hand collection of other material such as mollusc shell or nutshell is similarly undesirable and a source of confusion in subsequent analysis since it represents an unknown proportion of what was in a particular layer.

### 5. SPOTs (Spot samples)

*Nature of sample*: SPOTs are taken to represent unusual or interesting material observed during excavation. Typical subjects for spot samples are caches of fly puparia or large seeds, articulated or otherwise obviously associated bones, or small patches of charcoal. They *cannot* substitute for other sample types. *Function*: Although SPOTs may be taken for a variety of purposes, they are usually intended to provide material simply for identification. *Storage*: As for GBAs; SPOTs are often of very delicate material and many need to be treated at least as well as artefacts composed of organic substances. *Processing methods*: Extremely variable, from visual inspection and immediate identification to complex analysis. *Resulting physical archive*: Depends on nature of samples; sometimes biological specimens stored as for those sorted from GBAs and BSs, but often requiring special conservation techniques.

### 6. POLs (Pollen samples)

*Nature of sample*: Pollen analysis might be carried out on subsamples from GBA or SPOT samples, but more conventionally a series of separate samples is collected using special techniques, e.g. by coring or from monoliths or open sections. *Function*: For analysis of pollen and—as appropriate—other microfossils. *Storage*: Samples for pollen analysis must be stored in cool, dark, anoxic conditions (cf. Moore and Webb 1978, 21). *Processing methods*: There are standard preparation techniques (*ibid.*). *Resulting physical archive*: Pollen 'count' slides, residues from preparations. These may require appropriate museum curation in the long term but are usually stored without detriment under normal laboratory conditions.



7. **WOODs** (Timber or other wood samples, including 'waterlogged' wood and larger charcoal fragments)

*Nature of sample:* The sampling and storage of waterlogged wood has been considered by, for example, Coles *et al.* (1990); the handling of wood samples is very inconsistent between sites and requires particular attention during project planning. In general, small charcoal is recovered from sieved samples (GBAs, BSs, SRs), but patches of charcoal or large pieces are frequently collected as SPOTs and very fine particulate charcoal may be recorded from POLs. *Function:* Samples of wood may be collected for purposes of identification or for information concerning woodland management or timber conversion. In addition, identifications of large structural timbers and wooden artefacts may be required. *Storage:* The storage of timber samples in both the short and long term presents special problems with resultant cost implications, not least because waterlogged timber must be kept wet and cold. *Processing methods:* Work on biological aspects of waterlogged wood and charcoal is usually inextricably linked to the needs of conservators, finds researchers and dendrochronologists and poses particular problems of organisation and communication. In the EAU the normal action taken is identification of tree species and recording of insect damage but, where appropriate, ring counts and measurements may also be made. *Resulting physical archive:* Organisation of long-term storage of wet wood is not normally the responsibility of the environmental archaeologist.

8. **OTHERs** (Other kinds of samples)

Samples may be collected for various other specific purposes, for example block samples to represent a buried soil horizon or a series of small samples for diatom or phytolith analysis. As such, samples in this category may have almost any form and have very varied requirements for processing and storage.

It is also our experience that what have been termed 'context' samples, usually a small quantity of deposit collected for no clearly defined reason, are normally of little value; a GBA sample would be appropriate, providing enough material for a wide range of analyses.

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Kenward H. K., Hall A. R. and Jones A. K. G. (1980). A tested set of techniques for the extraction of plant and animal macrofossils from waterlogged archaeological deposits. *Science and Archaeology 22*, 3-15.

Kenward H. K., Engleman C., Robertson A. and Large F. (1986). Rapid scanning of urban archaeological deposits for insect remains. *Circaea 3*, 163-72.

Moore, P. D. and Webb, J. A. (1978). *An illustrated guide to pollen analysis*. London: Hodder and Stoughton.

#### Appendix

The following addresses may prove useful:

(1) **Thurgar Bolle**, Telford Way, Kettering, Northamptonshire NN16 8UY, U.K. (0536 410111)

Suppliers of 10 l. **plastic tubs** (manufacturer's code: PO200); minimum order is 600; price per 100 at time of writing: £95.25+VAT (i.e. approximately £1 each).

(2) **IML Ltd.**, 6 Thorncliffe Distribution Centre, Brookdale Road, Thorncliffe Park Estate, Chapeltown, Sheffield S30 4PH, U.K. (0742 465771)

Suppliers of 'Tyvek' white woven **plastic labels** (prices on request); these can be supplied pre-printed with site names/codes and so forth, and with washered or punched holes. They are effectively indestructible and can be almost indelibly marked with waterproof, spirit-based black markers.

Although the outlay per sample is higher with tubs than polyethylene bags, they are reusable and only one is needed per sample. They are also very much easier to handle and store than bags!

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## Bones that cats gnawed upon: a case study in bone modification

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### Summary

The role of cats in producing gnawing damage to mammal bones in human occupation debris is briefly discussed. The results obtained by allowing a domestic cat to chew discarded sheep bones *ad lib* are presented, and it is concluded that cats produce a characteristic pattern of damage which may be distinguishable from that produced by dogs.

### Introduction

It has long been recognised that scavenging carnivores and omnivores are an important factor in the taphonomy of archaeological and palaeontological bone assemblages. The recognition of surface damage caused by scavengers has been of particular importance in the examination of Pleistocene assemblages, when hominids may have been among the scavengers (Shipman 1981). In more recent archaeological assemblages, the emphasis has been more on recognising the pattern of bone destruction, and thus removal from the archaeological record, which may result from such scavenging, and observations have been made on the destructive effects of bone chewing by, *inter alia*, foxes (Stallibrass 1984), dogs (Stallibrass 1990, Payne and Munson 1985), pigs (Greenfield 1988) and humans (Jones 1986). The result of most of this work has been to increase awareness of the potential attrition of deposited assemblages before burial, and to indicate the form of the surface damage which may be taken as characteristic of the gnawing and chewing of bones by different species.

In all of this work, the domestic cat has not figured large, even though cats have a long history of mutualistic co-existence with people, and observations of modern cats would suggest that they are just as liable to scavenge discarded refuse as are dogs or pigs. There may be a contemporary social factor at work in this exclusion, namely the Western premise that one feeds bones to dogs, but not to cats. In order to investigate the possibility that cats will produce a characteristic pattern of surface damage, a long-term feeding experiment was devised. The experiment was

undertaken by TO'C; the bones were examined and the results collated by NCM.

### Materials and methods

Limited empirical observation suggested that it would be a mistake to start by feeding too many different types of bone to too many cats, as the results would be complex to collate and to standardise for any one variable. Accordingly, it was decided to limit the experiment to two types of bone, with a standard treatment prior to feeding them to one cat. Pragmatism dictated that the bones should be readily available as domestic refuse, so sheep scapulae and humeri were chosen. These were available at roughly fortnightly intervals as debris from a roasted shoulder of lamb, thereby standardising the treatment undergone by the bones (around 90 minutes at 200°C in a closed container; the presence or absence of rosemary or garlic was not thought to be significant!). The rather long interval between each feeding ensured that neither the cat nor the experimenter became bored with the procedure. The same cat, an elderly neutered male, co-operated enthusiastically throughout the period of feeding experiments.

Following cooking of the shoulder of lamb, and removal of most of the meat, the bones were allowed to cool, and were then offered to the cat. The cat was always given the bones in the same place, an area of grass close to 'home' and known not to be frequented by other cats or dogs. The cat was left to do whatever it wished with the bones, with no interference and minimal direct observation. Interest in the bones generally waned after an hour or so, but the bones were left in the same place for up to 24 hours to ensure that any

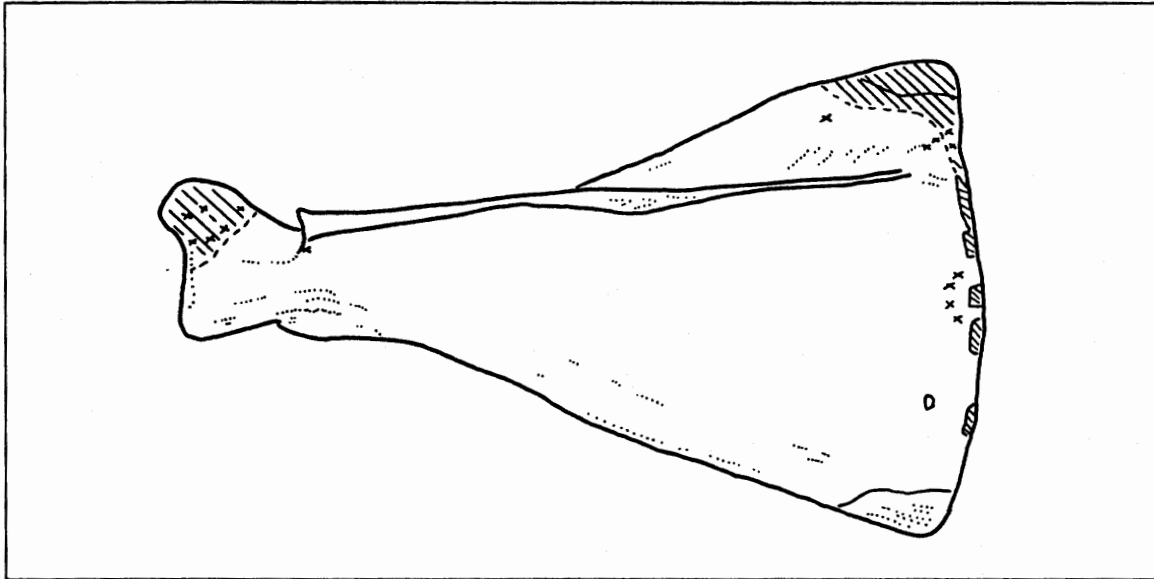


Figure 12. Diagram to show the principal areas of surface damage on the scapula. The experiment included both left and right specimens: where necessary, the records of individual bones have been laterally inverted to produce this summary. Hatching shows areas of general surface damage and destruction: discrete isolated 'punctures' are marked by a cross.

scavenging activity had ceased. The bones were then removed to a protected corner of the garden between a fence and a densely prickly shrub, where sub-aerial weathering could occur for several weeks without further disturbance by mammalian scavengers. The purpose of this weathering was to ensure the decay of any remaining soft tissue and fat. Finally the bones were collected and cleaning was completed by a period of simmering in water, followed by drying in a ventilated cabinet.

By mid-1991, sufficient specimens had accumulated to justify some collation of results. The increasing age of the cat also made it advisable to terminate the experiment.

## Results

Thirteen sheep scapulae and thirteen sheep humeri were examined macroscopically for surface damage which might have resulted from chewing or gnawing. At least some damage was found on nearly all of the specimens examined. In a small proportion, this took the form of the destruction of particular areas of bone, usually the edges or protruberances. More commonly, an area of grooves and pits would be apparent, evidence

of repeated biting on some parts of the bone. Away from these concentrated areas of damage, isolated pits and 'punctures' in the bone were noted.

Each specimen was recorded by noting the location of areas of damage and isolated punctures on a *pro forma* which provided outline diagrams of different aspects of the bones. The individual records were then merged by tracing all the observations onto one diagram of each of the elements. These concatenated diagrams are presented as Figs. 12 and 13.

On the humerus, gnawing activity concentrated on the epiphyses. This was presumably because the epiphyses have a large quantity of attached soft tissue and are composed of relatively soft bone. However, this pattern of damage may also reflect the cat's habit of placing a paw on the diaphysis to keep the bone still during gnawing. At the proximal end, the tubercles showed the most frequent damage, with many small areas of destruction. The proximal articular surface also commonly bore patches of grooves and punctures. On the distal epiphysis, the pattern of damage was generally similar, with tooth marks distributed across the epiphysis.

Damage on the scapulae was concentrated along the suprascapular margin, particularly at the superior angles, and around the coracoid. Surprisingly little damage was noted along the scapular spine.

Examples of typical tooth marks and damage are shown in Figs. 14-17. Tooth marks typically show clearly defined margins, and are narrow in proportion to their depth. Paired punctures, such as those seen on the suprascapular margin in Fig. 16 are frequent, and may reflect the distinctive form of the feline lower first molar.

## Discussion

It is clear that a cat, even when not driven by the extremes of hunger, can inflict appreciable surface damage to the bones of a medium-sized ungulate. Considerable destruction of bone is unlikely, and the damage typically takes the form of more or less dispersed tooth marks. In this respect, the damage differs from that produced by canids, which seem more inclined to 'mumble' bones, producing heavily gnawed areas on which individual tooth marks merge into a mass of shallow pits and grooves. The more dispersed and clearly

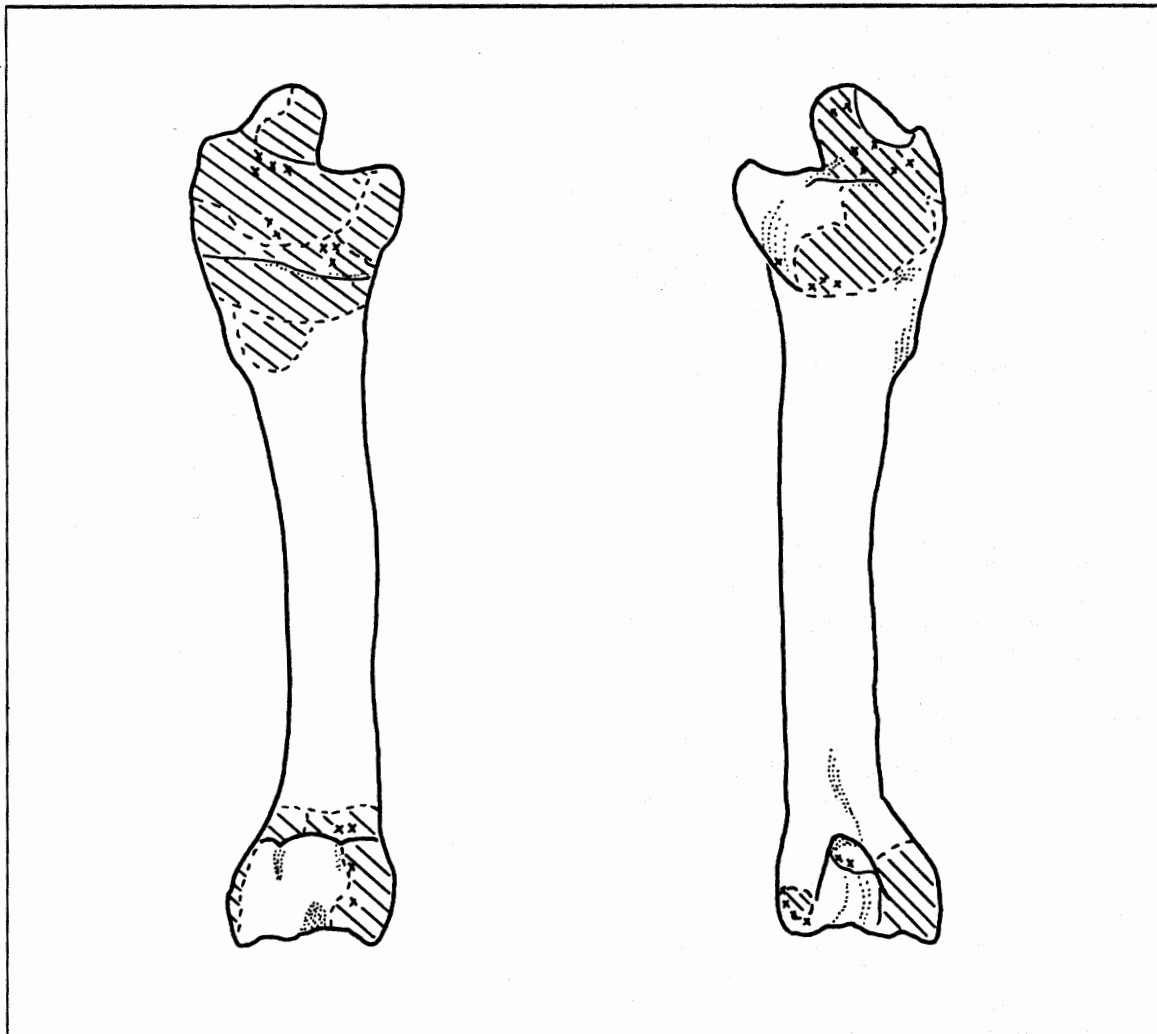


Figure 13. Diagram to show the principal areas of surface damage on the humerus. The experiment included both left and right specimens: where necessary, the records of individual bones have been laterally inverted to produce this summary. Hatching shows areas of general surface damage and destruction: discrete isolated 'punctures' are marked by a cross.

defined tooth marks produced in this experiment by a cat would seem to be distinctive, as would the relatively deep and narrow profile of those tooth marks. Some of the damage caused, for example, to the suprascapular margin and the coracoid would appear to be the consequence of attempts to remove conjoining soft tissue, but some of the other tooth marks may represent 'handling' of the bones or, perhaps, a form of play.

Clearly, this paper presents only the results of one rather *ad hoc* experiment, and illustrates the surface modifications caused by one cat on two skeletal elements of one ungulate species. Without wishing to encourage the undue proliferation of gnawing experiments, there is evidently a need for further work to establish whether bone modification by cats is consistent enough to be reliably distinguished from that caused by dogs, and to determine whether cats can, or more importantly will, wholly destroy the bones of smaller vertebrates. Having established the criteria by which to recognise feline tooth marks, the next step will be their routine recognition on archaeological material, thereby adding a little more detail to our knowledge of assemblage formation processes on occupation sites.

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Figures 14-17 appear on pages 31-4.



*Figure 14. Humerus, proximal epiphysis, showing damage apparently caused by repeated biting on a small area.*





Figure 15. Humerus, proximal epiphysis, showing clearly defined deep and narrow tooth marks.

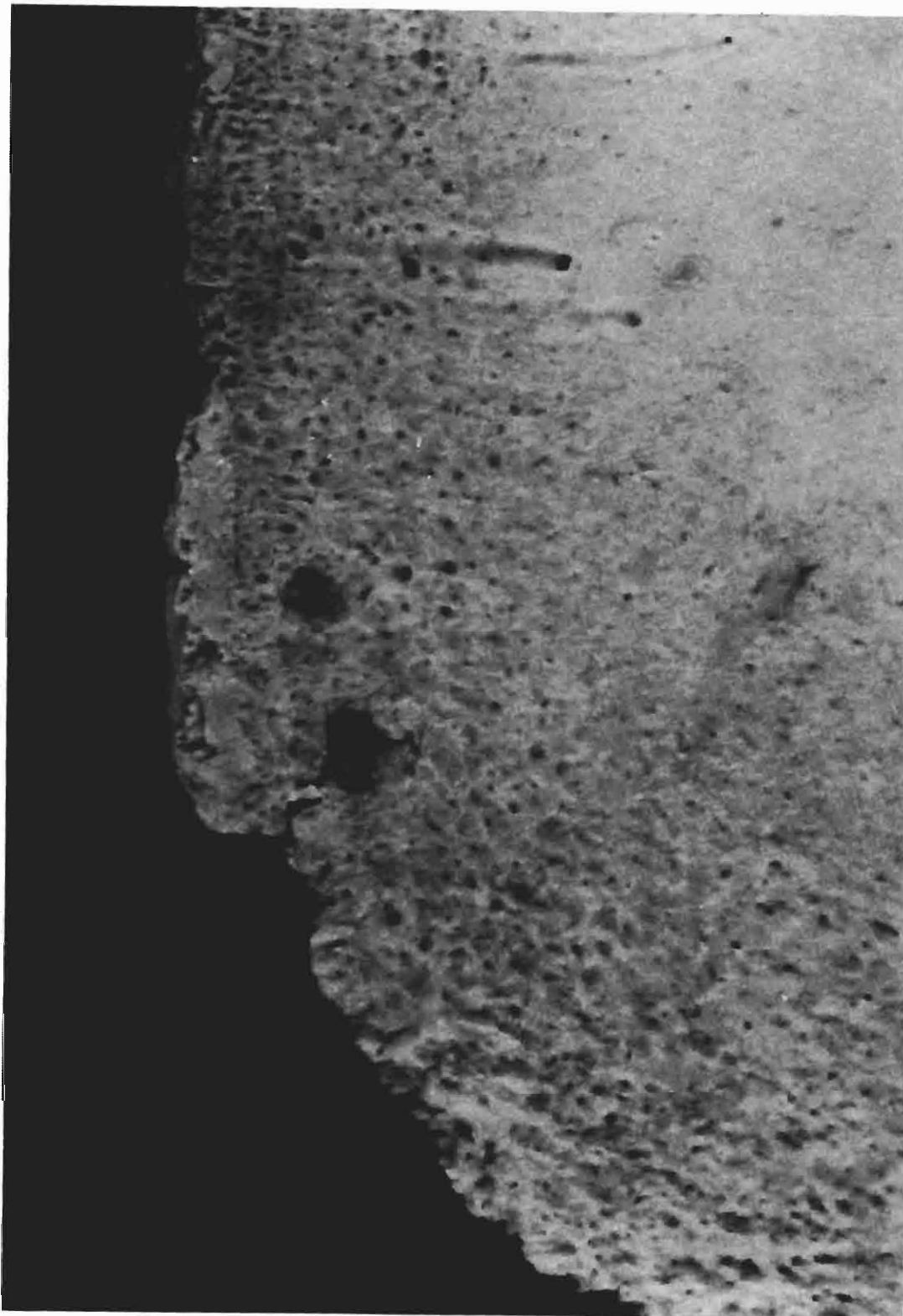


Figure 16. Scapula, suprascapular margin, showing a paired puncture. The centres of the two holes on this specimen are 2.9mm apart.



Figure 17. Scapula, coracoid process, showing appreciable destruction of bone.

## Identification, classification and zooarchaeology

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### Summary

Identification of preserved biological materials is often regarded as a skill which has little to do with analysis and interpretation. This paper argues that in zooarchaeological studies—here with particular reference to vertebrate remains—identification procedures deserve more detailed consideration, because these procedures have a significant effect on the results of faunal studies. It is suggested that most identifications are made within a system of usually unspecified rules which vary from one analyst to another. Improvements in comparability between faunal studies will result if these rules are considered before beginning an analysis, and if the rules are made explicit in publications.

### Introduction

Most archaeological studies employ typologies as descriptive and analytical devices. The conscious use and analysis of typologies dates from the publication of Krieger's (1944) paper, and a large, complex, and sometimes acrimonious literature has been devoted to typology in general and artefact typology in particular (Hill and Evans 1972; Whallon and Brown 1982). In spite of the continuing typological debate there would appear to be a general consensus that typologies are artificial devices designed to expedite research in specific areas (Hill and Evans 1972; Hayden 1984) and that 'types of types' (Steward 1954) exist.

Typological debates continue in many sub-disciplines of archaeology, and these generally concern the appropriateness of certain typologies for solving certain archaeological problems. For example, typologies of microchipping have been called into question by Vaughan (1985) on the basis of experiments which suggest that the correlation between microflake form and the material worked by the stone artefact is not as good as once thought. Similarly, the utility of some typologies of lithic debitage have been questioned by Sullivan and Rozen (1985).

There has been relatively little debate about typology in the analysis of animal remains from archaeological sites. This is because most

zooarchaeologists have assumed that the system with which they describe specimens may be imported intact from zoology. As a result most methodological developments have been in the interpretation of organic remains rather than in their classification and description. The one important exception to this is the discussion concerning the identification of cut marks and breakage patterns on bone (e.g. Behrensmeyer *et al.* 1987; Binford 1981; Johnson 1985; Morlan 1986; Shipman 1981). Typologies of these phenomena are concerned with the identification and classification of humanly-produced modifications rather than the identification of the faunal element on which they are found. They therefore resemble artefact typologies, and share all the problems and advantages inherent in such methods.

In this paper I will briefly consider the theory of identification, then examine the use of classificatory systems to describe and 'identify' faunal specimens from archaeological sites. It will be suggested that zooarchaeologists should consider their identification systems more carefully in order to increase the degree of standardisation of data presentation and reduce the possibility of interpretive error resulting from misapplication of identification methods. Examples will be drawn largely from vertebrate zooarchaeology. It is in this field that problems of identification are most likely to occur, because zooarchaeologists are

generally concerned with identifying elements or parts of elements of complex endoskeletons. Analysis of other animal remains, such as molluscs or insects, is usually concerned with identification of relatively complete shells or exoskeletons. This is not to say that many of the problems discussed below will not occur; however, the problems are probably less acute than in the field of vertebrate zooarchaeology.

I should point out at the start of this paper that I have deliberately avoided discussing 'case studies' which I consider to be examples of poor identification procedures or data reporting. Most zooarchaeologists, including myself, have made errors of the types discussed below. It will not serve any purpose to select a few examples from the many to illustrate the points made here.

### Identification, classification and typology

The initial stage of any zooarchaeological analysis is to group specimens into meaningful categories. Although this may appear to be similar to the creation of artefact typologies, which also group objects into meaningful groups, there are differences between the two processes. These differences stem from the distinction which must be made between classification and typology on the one hand and identification on the other. Classification is the process of grouping objects or other phenomena into groups based on similarities and differences (Hill and Evans 1972, 233). Typology is a special form of classification, in which phenomena are assigned to the same type if they share consistent patterning of attribute states (*ibid.*). Biologists have distinguished identification from classification (Sneath and Sokal 1973, 3), noting that identification is the assignation of an organism to a previously established classificatory system.

Archaeologists who study artefacts may wish to use previously established typologies and 'identify' their artefacts by reference to those systems. However, they are always free to modify such typologies or to develop new typologies if existing systems are inadequate for their research design. As a result, there may be debate about the relative merits of different typological systems to assist in the solution of the same research problem. Alternatively, one may apply two completely

different typologies to the same artefact assemblage if one wishes to investigate two different areas of human behaviour. For example, typologies of ceramics or lithics which are useful for constructing culture history may be inappropriate for analysing site function.

Archaeologists who study animal remains, or any other largely unmodified organic material, generally organise their specimens into groups by a process of identification. No matter what the research orientation, it is commonly assumed that the initial step of a faunal analysis is to group species according to well-defined attributes preserved in chitin, shell, bone or teeth. This accounts for the widespread establishment of comparative collections and the publication of identification guides and keys. Most zooarchaeologists believe that pre-existing classificatory systems can be employed in the analysis of organic remains. This view is further enforced by fairly frequent pleas for standardisation of data reporting in zooarchaeology (e.g. Clason 1972; Grigson 1978; Driver 1983), such standardisations being impossible without a general agreement that there is a single appropriate classificatory system.

This attitude is certainly reasonable, and many specimens can indeed be grouped using two biological schemes. The first of these is the standard binomial nomenclature; the second is a fairly well standardised system of anatomical description. Using these systems '*Bison bison* left femur' is likely to be well understood throughout the English speaking world and (with one translation) throughout the entire world. This stands in contrast to artefact typologies which, in some areas, have become so cumbersome as to become almost unworkable, and which contain few standardised terms acceptable in more than one language.

If one accepts some of the assumptions (discussed below) inherent in the classification '*Bison bison* left femur' then this is a reasonable way of describing faunal remains. In fact, most vertebrate remains can be described quite precisely by three variables—species, element, and part of element, the latter following a system such as Brumley's (1973) butchering units or Watson's (1979) diagnostic areas. Some specimens may be described further, using categories such as age, sex or pathological condition, but these

are usually a distinct minority of the entire assemblage.

Are faunal identifications a form of typology? In some ways they do resemble artefact typologies. Bones are grouped by considering a variety of attributes, with multiple attribute states. The groups are exclusive, and can be defined by non-random associations of attribute states. However, there are important differences between a system of bone identification and artefact typology. The binomial system assumes phylogenetic relationships between animal groups, which is not the case with artefact typologies. The binomial system is hierarchical, while many artefact typologies are not. The basic unit of zoological classification - the species - is essentially defined by its reproductive behaviour, while the basic unit of typology - the type - does not exist as a population and has no capacity for perpetuation. Finally, modern artefact typologies are designed to solve specific research problems, while zoological systems of classification are often used as descriptive referents in research which does not deal with phylogeny.

### Methods of identification and their effects on bone groups

Of the three major attributes defined above (taxon, element and modification), the third will not be discussed in this paper, as it is often describing an artificially induced condition of the bone, and consequently most zooarchaeologists have to be explicit in developing non-zoological typologies to describe bone fragments or other aspects of bone modification. Identification of specimens is essentially a matter of grouping specimens by taxon and element.

The methods by which bone fragments are identified ought to be relatively simple. First, it is necessary to identify the element represented by the complete bone or bone fragment. Unless one can identify the element represented, it is usually impossible to justify identification of taxon. It may be possible, using such criteria as bone thickness or surficial characteristics to identify some fragments to the class level without first identifying the element. For example, long bone fragments with cortical bone thicknesses over a few millimetres are unlikely to be anything except mammals (unless one is

working in an area with large reptiles or large flightless birds), and many cranial bones of fish display distinctive surficial characteristics which distinguish them, as a class, from other vertebrate classes. However, I strongly suspect that in many cases the assignment of bone fragments to categories such as 'unidentifiable mammal' or 'unidentifiable bird' is the product of wishful thinking. This is particularly likely in the case of birds, where size ranges and cortical thickness of bone fragments frequently overlap with the smaller mammalian species.

It is worth emphasising that assignation of any bone fragment to all but the most general taxonomic group cannot be undertaken without identification of the element. Generally, once one considers specimens below the level of the class, there are no readily observable features of the gross morphology which permit identification of the taxon without prior or concomitant identification of the element. Terms such as 'small ungulate long bone fragment' are meaningless, although they are sometimes encountered in the zooarchaeological literature. If the features on the fragment are sufficient for identification as a small ungulate (as opposed to a medium-sized carnivore, for example), then they will certainly be sufficient to identify the element from which the fragment derives.

The second stage of identification is to assign the identified element to a taxonomic group. Such identifications may range from very general (e.g. the order or family) to the particular (species or subspecies). Regardless of the specificity of the identification, it follows that the identification guarantees distinction from other taxa at the same level of specificity. Thus, the identification 'Canidae' should guarantee that the specimen could not belong to any other mammalian family, such as Felidae or Cervidae. Similarly 'Canis lupus' implies that no other members of *Canis*, such as *C. familiaris* or *C. latrans* are represented.

The use of such a classificatory system depends upon the following:

1. Zooarchaeologists employ the existing binomial nomenclature used by zoologists.
2. Identification to the given taxonomic level is justified by the methods employed.



These principles are investigated further below.

### Use of binomial nomenclature

The International Code of Zoological Nomenclature (ICZN) provides rules for the classification of animals by order, family, species etc. and, like many artefact typologies, is a way of simplifying an incredible array of diversity (Jeffrey 1977). It is organised in such a way as to suggest degrees of relationships between phenomena; for example, animals of the same genus are thought to be more closely related (i.e. they diverged more recently from a common ancestor) than other members of the family to which the genus belongs. The zoological classification is also an artificial classificatory device, as are archaeological typologies. With the possible exception of the species, all other hierarchical levels of the system are imposed by zoologists, rather than by nature.

One must remember that, because the binomial system defined by the ICZN is artificial, there are other ways to develop classifications of animals. For example, one could describe groups based on diet, locomotion and size, such as those used by some paleoecologists (e.g. Van Couvering 1980). The emphasis in zooarchaeology, paleontology and paleoecology on identification of taxonomic groups defined by the ICZN is because of the general belief that identification of the species allows one to infer a wide range of other information, including tolerances to a variety of climatic conditions, habitat types utilised, and various behavioural traits (e.g. social behaviour; migrations etc.). The reason for the continued use of the binomial system of nomenclature is probably because most other possible classifications of vertebrates will operate at a more general level than the species, and identification of bones using standard zoological categories allows them to be regrouped into other classificatory schemes if required.

In most cases the use of the binomial system does not cause problems, but one must recognise that zooarchaeologists frequently modify the system, usually by recognising size classes which cross-cut established taxonomic divisions. The most widely used example of this would be a designation such as 'large ungulate'. Such an identification for Late Pleistocene/Holocene faunas of Canada might

include bones of horse, bison, musk ox, camel, wapiti and moose, from two separate orders and four separate families. From the same fauna one might also recognise 'small ungulates', which could include deer, caribou, sheep, mountain goat, pronghorn antelope and possibly even saiga antelope; in this case the taxonomic category includes two families from a single order. Thus, while bones with many diagnostic features might be assigned a taxon based on established zoological classifications, bones with fewer diagnostic features may be 'identified' using a system which groups specimens from separate lineages into a single category based on an attribute (size) which is not relevant to the zoological system. Thus, some cervids (moose, wapiti) are separated from other cervids (deer), but grouped in the same 'large ungulate' category as bovids, camelids and equids. This is somewhat analogous to the provisions in the International Code of Botanical Nomenclature which allow the category 'form-genus' to describe superficially similar fragmentary plant fossils which may derive from a variety of different families (Jeffrey 1977, 40).

The implications of this methodology are probably not critical to zooarchaeology, although one wonders whether it is really worth making these types of identifications, as virtually no inferences or deductions are ever made from such information. However, as will be discussed below, if one begins to make assumptions about which species are really represented in these very general taxonomic categories, the potential interpretive value increases and new problems arise.

### Identification systems

As a zooarchaeologist, one is occasionally stopped in hallways or, more disconcertingly, in conference receptions and asked to identify a specimen. After a few instances of embarrassingly implausible identifications, one learns to ask some critical questions before making a pronouncement. "Where does it come from?" and "How old is it?" are the two I have used most frequently. Such preliminary questions reveal something rather interesting about our identification methods—we frequently rely upon the context of the specimen to aid our identifications. It would appear that our methods do not simply depend on recognising 'diagnostic' characters on bone fragments, but also on other



assumptions which are rarely stated. These assumptions are worth examining in some detail.

*Assumption 1: Although taxonomic groups are defined by a host of characteristics, most of which are not preserved archaeologically, single bones exhibit sufficient diagnostic characteristics to allow identification, frequently to the species level.*

This assumption is the basis for zooarchaeological identification. Yet very few bones in the post-cranial skeleton are diagnostic of the species if one has to select one species from the entire animal kingdom. For example, the presence of a large bovid femur fragment on a 3000 year old site from the Canadian plains virtually guarantees the identification *Bison*, and in many cases analysts will identify *Bison bison*. However, on a historic period site from the same area, many femur fragments would be indistinguishable from domestic cattle, and would be recorded as *Bos/Bison*. What zooarchaeologists really mean when they identify a bone fragment is that, given our knowledge of what animal species are likely to have been found in an area during a particular time period, one can identify a fragment based on a combination of size and morphological characteristics. In the above example, the bison femur fragment is probably not distinguishable from those of European bison or some African and Asian bovinds. However, given the likely geographic range of fauna, the possibility of there being an Old World bovid in the assemblage is considered so unlikely as to be dismissed.

Another problem associated with this assumption is the concept that the zoological taxonomy is immutable, whereas in fact it is in a constant state of revision. For most vertebrate zooarchaeologists this is not a major problem, because revisions tend to be rare and minor. However, it can lead to some embarrassingly over-confident identifications. For example, until recently ornithologists identified two species of flickers in western North America, the red-shafted flicker (*Colaptes cafer*) and the yellow-shafted flicker (*C. auratus*). These are now considered as subspecies of a single species, the common flicker (*C. cafer*). If one reads zooarchaeological reports from the 1960s and 1970s one can find bones of both original 'species' identified. One suspects that, in reality, the skeletons of these two types of bird exhibit so much overlap that one cannot separate them, and certainly today few people would attempt to separate bird

subspecies on osteological characters. The fact that the two types were originally divided into separate species probably produced a state of over-confidence in zooarchaeologists, who felt that osteological differences ought to be found. Today no one attempts to make the distinction which was made a decade or so earlier, because the taxonomy has changed, not the birds.

Assumption 1 therefore requires some modification. Bones are not identified solely by their morphology and size. Rather, a great many possible species are excluded as candidates by virtue of their position in time and space. Furthermore, species which can be separated by zoologists are not necessarily separable on the basis of osteology.

*Assumption 2: The methods for identification are sufficiently well tested that one does not need to justify most identifications, except in relatively rare circumstances.*

In most zooarchaeological publications there is little discussion of identification methods. Perhaps zooarchaeologists feel that their methods of identification are so easy to use that the methodology requires little discussion. Perhaps they rely to so great an extent on 'experience' that they cannot describe their methods. Generally, discussion of identification methods is confined to relatively rare species, when it is important to demonstrate that the identification is justified. In addition to personal experience, zooarchaeologists use three methods for identifying fragments:

- (a) comparative collections;
- (b) published guides or keys;
- (c) measurement systems.

The use of comparative collections is widespread, and probably forms the basis for most identifications made by zooarchaeologists. However, most comparative collections (including the one I use) are really inadequate for their intended purpose. Returning to an earlier example, the identification '*Bison bison* left femur' is usually arrived at through the following type of mental process: 'clearly a large ungulate, based on morphological characteristics and size; perissodactyls can be eliminated on the basis of morphology, so it must be an artiodactyl; the only artiodactyls of this size on the Canadian plains at 3000 BP are

bison, moose and wapiti; specimen was compared with an old male bison which died in a zoo, a juvenile moose donated by a game farm, and a mature female wapiti culled from a national park; characteristics most resemble the bison'. While this may exaggerate the deficiencies of comparative collections, there are few which contain sufficient numbers of specimens to cover age and sex variation, individual variation, or variation resulting from life in different habitats. Most identifications using comparative collections are therefore 'best guess' approximations, usually based on inadequate comparative samples.

The use of identification guides and keys also poses problems. A key is a formally laid out system of identification, usually organised in such a way that presence or absence of characteristics can be used to identify a species. Keys usually have a branching form, so that one begins by looking for features characteristic of gross taxonomic groupings, and then proceeds to finer divisions (Pankhurst 1978). Such keys are rare in vertebrate zooarchaeology or paleontology, because each species possesses hundreds of bones, and bones are generally found as fragments. Consequently, a formal key would be required for each part of each element of the skeleton, or at least for those areas generally considered most useful for separating taxonomic groups. While attempts to do this have been made (e.g. various keys in Gilbert *et al.* 1985), most published aids to identification cannot be described as keys. In most cases they are usually collections of illustrations, sometimes with notes discussing diagnostic characteristics (e.g. Gilbert 1980; Olsen 1964; 1968; Smith 1979; Schmid 1972). As I have suggested (Driver 1987) the existence of such guides is somewhat anomalous. For the frequently occurring species in an area, one can anticipate that most zooarchaeologists will have access to comparative collections which contain those species, and 'hands on' inspection is likely to be better than illustrations for the purposes of identification of fragments. For rare species, on the other hand, it is surely better to take the specimens to a comparative collection which contains the species than to rely on an illustration to identify a rarity. The only guides which have any real value to zooarchaeologists are those which summarise the results of observations of large numbers of specimens and discuss distinctive diagnostic characteristics which consistently occur (e.g.

Olsen 1960; Brown and Gustafson 1979; Lawrence 1951). Such publications are relatively rare, and even those which are based on observations of many specimens rarely provide information on how many specimens of each species were consulted or the locations from which specimens were obtained. Nevertheless, they are quite important as a supplement to a comparative collection, because they point out consistent diagnostic differences between morphologically similar species.

Most zooarchaeological identifications are made through a combination of comparative collections and illustrated guides, generally used in a complementary fashion. Good illustrated guides will be the result of examination of many specimens, and should partly solve the problem of most comparative collections—insufficient representation of intra-species variation. The comparative collection is essential for the identification of fragments, and for examining details of bone morphology.

Measurement systems of varying degrees of complexity have been used by zooarchaeologists. At the most simple level, all analysts use gross size to eliminate certain taxa from consideration. Thus, to return to the example of the bison femur, sheep is excluded on the criterion of size rather than morphology, because both sheep and bison share many morphological features. More complex systems of measurement involve taking multiple measurements on a single specimen, and are generally only used to separate closely related species. These measurements may be compared using a bivariate plot (e.g. Davis 1987, figure 1.12) or by using multivariate statistics (e.g. Morey 1986). While such methods appear to be sound, as they are based upon measurements which discriminate between modern specimens of known taxonomic affiliation, they can be misleading. Many modern species exhibit considerable geographic variation and, while a system of measurements may discriminate between two closely related sympatric species, it is not necessarily the case that the method can be applied in other regions or in the past. Identification by measurement also requires relatively complete specimens, and can only be applied to a relatively small proportion of fragments.

Assumption 2 therefore requires some qualifications. We do not systematically test

reported assemblages. Similarly the ratio of L to all ungulates (U1 + U2) changes from 1:1 in site X to 5:2 in the reported assemblages from site Y, even though the actual ratio remains constant from one site to the next.

Cases such as this will not necessarily arise, provided that zooarchaeologists are aware of such problems in the data. However, unless the analyst of site X clearly differentiates between specimens which can be identified positively as species U1 and those which can only be identified on their own merits as large ungulates, the data produced by the analysis will be of limited value in any comparative studies, because it will not be possible to sort out which bones are really identifiable to the species level and which are assumed to belong to that species.

One could argue that such a problem would not arise if the analyst of site Y reported values for an extra category—'large ungulate'. Indeed, this is a fairly common procedure in zooarchaeology. While this would solve the problem of looking at ungulate to lagomorph ratios, it still creates problems. For example, the importance of U1 in the Site Y assemblage still cannot be compared with U1 values from Site X because criteria used to identify the bones differed from one assemblage to the other. If, on the other hand, the site X analyst had used the 'large ungulate' taxon for specimens which could not be identified positively as species U1, the assemblages would be comparable.

One other possible solution would be to calculate the ratio of U1 to U2 in the site Y assemblage, and then make the assumption that this same ratio applies to the 'large ungulate' category. The 'large ungulates' could then be assigned proportionately to species U1 and U2, and comparisons could be made with site X. Again, there are serious problems with this method. For example, if butchery practices differed between the two ungulate species, then more 'large ungulate' fragments would derive from the species which had undergone more frequent bone breakage and comminution. The situation could be further confused if we added more sites to the example with new species of small ungulates and lagomorphs at some of the sites.

There are other problems with 'identification by association'. The practice almost certainly encourages complacency in identification

procedures. If one begins with the assumption that all bones found in a supposedly monospecific assemblage are indeed from one species, then the likelihood of identifying the rare bone of another species of similar size is considerably diminished.

The practice of 'identification by association' is of little value to zooarchaeology. Apart from being dishonest, such identifications can lead to either confusion or unwarranted conclusions. The practice should be discontinued. Zooarchaeologists should identify to a particular taxon only those bones which can unquestionably be assigned to it.

### A set of procedures for zooarchaeological identification

Identification of specimens by zooarchaeologists is an attempt to place them into taxonomic and anatomical categories used in zoology. In view of the general robusticity of the system of binomial nomenclature, and (with the possible exception of fishes) the system for naming individual bones, this method of classification would seem to be the most appropriate for the initial stages of any zooarchaeological analysis in which knowledge about species representation is important. Even if one does not wish to use the binomial system and standard anatomical terms, most other imaginable classifications require prior knowledge of the taxon and element. Consequently standard zoological descriptors will continue to be important in zooarchaeological classification.

It is important for zooarchaeologists to realise that the evidence used by zoologists to establish their classificatory systems include a wide range of data which can never be observed in the archaeological record (Ross 1974). There is no expectation that all, or any, bones or bone fragments will be sufficiently distinctive to identify unequivocally the species defined by consideration of whole specimens. The classification that zooarchaeologists use was developed to meet the needs of zoologists who almost always have many complete specimens of the animals they are attempting to classify. It is inevitable that many zooarchaeological specimens will be recorded as 'unidentifiable'.

If most zooarchaeologists accept the use of zoological terms to identify bone fragments,

one might expect unanimity on standardised methods for data reporting. However, it is unrealistic to propose this. Individual zooarchaeologists have different confidence levels (with a tendency for the more experienced to be less willing to differentiate between closely related species). Since comparative collections differ in quality, one's ability to identify bones is partly a function of where one works. Furthermore, different research goals may require different approaches towards identification. For example, if research is primarily oriented towards analysis of subsistence, it might well be a waste of time tracking down the occasional passerine bone in an assemblage dominated by large mammals. Alternatively, palaeoenvironmental studies require species identifications, and bone fragments which cannot be identified to that level can often be ignored, even though in other contexts they might provide information about element frequency or butchery. However, although we cannot expect complete standardisation of data reporting, it is nonetheless necessary to inform other archaeologists of how one has implemented the system of identification. In order to do this, one has to follow certain procedures, and these are outlined below.

Prior to beginning an analysis one should develop a set of rules about how identifications are to be made. I suspect that very few zooarchaeologists do this, although many assume that they have done so. In most cases, one has a fairly good idea of the type of fauna which will be recovered from a site, and can predict fairly well what sorts of decisions will be required during the course of the analysis.

The first rule of virtually any analysis must be that each fragment will be identified on its own merits, so that 'identification by association' does not occur. However, one may decide to make exceptions to this rule (although I personally do not). For example, a complete articulated skeleton might contain some bones which are identifiable to species, while other are only identifiable to genus if found as individual specimens. In such a case, one might decide to allow the identification to species of all bones which are clearly articulated. Similar decisions must be made in the case of bone fragments which can be glued together. If one finds twenty fragments of a moose tibia which can be reconstructed, should it be identified as a single fragment of moose? Should each individually identifiable

fragment be counted? Should each fragment be counted as a separate identifiable piece? One can make arguments for all procedures, but whichever is to be followed must be established prior to the beginning of the analysis, and should also be reported (briefly) in the faunal report.

One must also make decisions about how one will make taxonomic distinctions. As noted earlier, assumptions are always made about what species are represented in the fauna. If one begins with no assumptions, then identification is virtually impossible, because every fragment will have to be checked against far more species than is realistic. For example, on Canadian high arctic sites dating to the last 5000 years, the only Canidae likely to occur are *Canis lupus*, *C. familiaris*, *Alopex lagopus* and *Vulpes vulpes*. For most analysts these form the universe from which any specimens identified as Canidae must derive. Such North American species as *Canis latrans*, *Vulpes velox* or *Urocyon cinereoargenteus* will be excluded from consideration by most analysts prior to attempting to identify canid bones. Decisions not to include certain species as possible sources of fauna result in a greater proportion of specific identifications. For example, using the Arctic example cited above, a canid femur which was demonstrably larger than a big fox but much smaller than a small wolf would have to be identified as a dog, *Canis familiaris*. However, if one was to include *C. latrans* in the list of 'possible' species for the area, then the specimen would probably be identified as 'dog/coyote sized canid'.

In addition to deciding what species might be present in the area, analysts must also decide what elements of the skeleton can provide specific identifications. This varies from one taxonomic group to another. For example, identification of the various species of *Canis* must be undertaken on fairly complete mandibles or crania; distinctions between mule deer and white-tailed deer can be made only on the antlers. On the other hand, many bones of *Castor canadensis* can be identified to species because there are no closely related species in the region being studied. If one is willing to produce a list of species which are likely to occur in the site (which I have argued above is essential), then one should be able to predict in advance which species are likely to be difficult to separate. This will allow one to decide prior to the analysis which elements exhibit so much overlap in morphology and

size that distinctions between species cannot be made. Once such decisions have been made, they should be adhered to, and should be reported in the published analysis.

Finally, it is very important that zooarchaeologists attempt whenever possible to report identifications in more detail than is usually done, so that the nature of identification methods can be understood by other archaeologists. As noted above, this should include brief notes about what taxa were considered separable, and what elements were used to separate taxa. Ideally, descriptive zooarchaeological reports which provide the basic information about a site's fauna should also include tables in which numbers of elements (or parts of elements, or butchering units, etc.) are recorded for each taxa. This not only allows other analysts to manipulate data on element frequency, it also provides a very good guide to the identification procedures utilised. For example, if a zooarchaeologist practices 'identification by association', these tables will show elements such as ribs identified to fairly specific levels; on the other hand, tables produced by a zooarchaeologist who does not use the method will show ribs and other less diagnostic elements relegated to a more general category. Admittedly, such tables take up space. This problem can be solved by carefully constructed tables and a lot of fine print. It can also be solved by the somewhat controversial use of microfiche appendices or even floppy discs. The introduction of many tables of data is not generally approved by editors and publishers, but without them much of the information recorded by zooarchaeologists is lost. Such data are often vital to future researchers, and zooarchaeologists should promote their use.

## Conclusions

The classification of specimens by element and taxon is a preliminary step of most zooarchaeological analyses. Zooarchaeologists generally use classificatory systems borrowed from zoology. It has been shown that the assumptions made by zooarchaeologists when using these systems, especially binomial nomenclature, are partly invalid. Furthermore, the procedures for actually identifying specimens are rarely made explicit, nor are most zooarchaeological identifications susceptible to testing or critical evaluation. We can place no confidence limits on identifications.

While it is desirable to begin testing our abilities to provide correct identifications, using carefully constructed blind tests to assess the reliability of the methods, we can make zooarchaeological data more trustworthy by following some simple procedures. We must make explicit which species have been considered as the 'universe' from which identifications have been made. We must outline the way in which identifications were made, including details of comparative collections, keys, guides, and measurement systems used. We should avoid 'identification by association'. Data reporting should include more than a list of taxa accompanied by NISP and MNI values. Publication of data should, at the very least, include lists of elements identified to various taxa, preferably organised by provenance.

The arguments for these recommendations are unambiguous and easily defended. Zooarchaeological analysis does not stop at the site level. Any attempt to work with data compiled by other researchers requires that one assess whether data sets are comparable, and this means that details of identification procedures and results must be made explicit. If zooarchaeology has any claims to be scientifically based we must adopt procedures which make the methodology of data production clear to other researchers. Only then can past research contribute to future syntheses.

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### Notes for Contributors

**Articles for *Circaea* should be typed double-spaced on A4 paper with generous margins.** Line drawings should be in black ink on white paper or drawing film, to fit within a frame 153 x 250 mm maximum. Captions should be supplied on a separate sheet of paper, and labelling on figures should either be in 'Letraset' (or an equivalent) or should be in soft pencil. Half-tone photographs can be accommodated, but authors wishing to make extensive use of photographs, or colour, should note that they may be asked to contribute towards the high cost of production. The editors will modify short contributions to fit the layout and convention of *Circaea*. The same principle will be applied to idiosyncrasies of spelling and punctuation. Scientific articles will be submitted to referees; authors may, if they wish, suggest suitable referees for their articles.

**TWO COPIES** of scientific articles should be submitted. Authorities must be given to Latin names, either at their first mention or in a comprehensive list, and species lists should follow a named checklist. References should follow the so-called 'modified Harvard' convention, but with journal titles preferably given in full, not abbreviated. *World List* abbreviations will, however, be acceptable if the author indicates a definite preference. For guidance as to the preparation and presentation of material for publication, contributors are referred to the British Ecological Society's booklet *A Guide to Contributors to the Journals of the BES*, and The Royal Society's *General Notes on the Preparation of Scientific Papers* (3rd ed., 1974). Text proofs of papers will be provided and these should be returned to the Editors within three days of receipt.

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