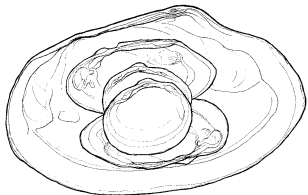


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• circaea



This Issue: Turner and Levitan discuss bone quantification - Payne on tooth eruption - Boyd about the physical environment - Moffat on wood sage - Maloney on testing for Dutch Elm disease - Tomlinson on clearing plant material - and more flies from Phipps (.....not forgetting Miscellany and Burhinus.....)

The Bulletin of the
Association for
Environmental Archaeology

CIRCAEA is the Bulletin of the Association for Environmental Archaeology, and is published three times a year. It contains news and short articles as well as more substantial papers and notices of forthcoming publications and conferences. Editorial policy 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, such as papers which consider in detail methodological problems like the identification of difficult bioarchaeological remains. Circaea is edited by Allan Hall, Barry Kenward and Terry O'Connor, and is assembled and printed at the University of York. Circaea is distributed free to members of the AEA and available to institutions and non-members at £6.00 per annum. At present, copyright resides with individual authors. Circaea is published by the Association for Environmental Archaeology, c/o Room 530, Portress House, 23 Savile Row, London W1X 2AA. Enquiries concerning membership of the AEA should be sent to N. G. Balmain at that address.

Notes to contributors

Articles for inclusion in Circaea should be typed double spaced on A4 paper. Line drawings should be in black ink on white paper or drawing film to fit within a frame 165 x 245 mm. 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 check-list. 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 has a definite preference. For guidance as to the preparation 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, The Royal Society). Text proofs of papers will be provided and should be returned within three days of receipt. **Ten free reprints will normally be supplied to the authors of scientific articles: further copies will be available, if requested at the time proofs are returned, at a charge of 3p per side plus postage.**

Back-numbers and a limited supply of articles can be purchased at the following rates: back-numbers - £2 per part; articles - 3p per side, plus postage.

Copy dates: January Issue - 15th November; May Issue - 15th March; September Issue - 1st July.

The Editors, CIRCAEA, c/o Environmental Archaeology Unit, University of York, York YO1 5DD, U.K.

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AEA**circaea**

As this number follows so hard on the heels of the last, the Editors see no good reason for a protracted Editorial. Copy is flowing in at a steady rate - witness this issue - though this is no excuse for the membership to sit back complacently. In particular, we welcome book reviews and short articles, as well as more substantial contributions.

Another area in which the membership might be able to help production is in supplying artwork for the cover. Striking, contrary photographs which have a story to tell would be most welcome - although of course we can't promise to use material sent, especially since line-drawing covers are a lot cheaper to reproduce than photographs. Normally, we would expect not to return artwork unless specifically requested.

Astute readers will be aware of yet another breakthrough in reprographic technology embodied in this issue. The use of a more elaborate word-processing and printing system has enabled us to underline and **embolden** text - italics are yet a long way off, though! We are also availing ourselves of the University of York Printing Unit's new volume photocopying machine - a thing of beauty indeed - which gives a reasonable standard of reproduction more cheaply. Photographs will still be printed by offset litho.

Another change about which we've made an executive decision is the date of publication. We've abandoned the original pious hope of getting issues out in January, May and September; such precision is impossible bearing in mind our need to adapt to the University Printing Unit's cycle of gross overwork and mere overwork. Rather, we shall prepare copy for printing in 'Spring', 'Summer' and 'Autumn', taking these seasons in a meteorological rather than sidereal sense. Being optimists, we refuse to take account of winter!

Miscellany

'Those whom God has not equipped ...'
A personal report on the 1984 AEA Conference
Murlough House, Co. Down, N. Ireland, 5-8 April 1984

When the notion of holding the 1984 Conference under the auspices of The Queen's University, Belfast was first mooted in Dublin in 1982, reactions were mainly cautious. Some spoke hesitantly of the distances involved, or of the cost of crossing the Irish Sea; others feared alienating members in Continental Europe or weren't sure that there was any environmental archaeology in Ireland anyway. Underlying these concerns, but voiced by virtually nobody, was an almost superstitious fear of 'The Troubles'. The reality when it finally happened was a splendidly diverse and informative meeting in a physically perfect setting, an unusually well-attended conference which was academically valuable and also good fun.

The Queen's University field centre, Marlough House, has to be the most attractive setting upon which the AKA has yet descended. Although the building itself is somewhat functional in style, the surrounding sand-dune terrain (Northern Ireland's first National Nature Reserve) and atmospheric views across to the Mourne Mountains 'sold' County Down to many participants. The conference foregathered on the evening of Thursday April 5th, with the now traditional scenes of just-controlled chaos as Dot Gernard and Martin Munro distributed bodies between rooms to everyone's satisfaction. Dinner that evening made clear to all the intention of the catering staff to overflow everybody at every possible opportunity, with Jon Pillecher forsaking his timbers to give a virtuoso performance with the soup-ladle.

Then the lectures began, with an introduction to the history of Queen's Palaeoecology Centre by Basil Wilson, and an interesting talk by Mike Yates which underlined the fundamental role of rocks in Irish prehistory. The evening finished in style, the Marina Bar, normally in Dundrum village, having temporarily established itself in one corner of the lecture hall.

Friday morning's lectures served to show the considerable amount of environmental work which has been going on around Ireland at the hands of a few determined people. Some subjects, such as deducing crop husbandry from plant remains, were familiar from elsewhere, but Helena Feighan and Brian Williams introduced categories of site - such as raths and fuilachta fiadh - which were new to many of us and which raised new interpretative challenges. Chris Lynn appealed for more detailed consideration of thin and complex occupation layers, a point which recurred throughout the conference.

After a mountainous lunch, the minibus convoy moved us all into Belfast for a series of ad hoc tours around the Palaeoecology Centre, conservation laboratories and the Ulster Museum. Remarkably, nobody seemed to get seriously lost and all reassembled for a convivial posters-and-sherry session. By general consent, the star turn appears to have been the dendrochronology laboratory, an international reputation built up by sheer ingenuity in two small and thoroughly unsuitable laboratories.

Back to Marlough for dinner, then the evening session nicely linked the themes of environment and archaeology, beginning with Frank Mitchell summarising his work on urban plant macrofossils from Drogheda, and ending with a thoroughly enjoyable talk by Fred Hamond in which he cleverly and convincingly used an environmental determinist argument to account for the siting and design of wind- and water-mills in Ireland.

The programme for Saturday's field trips had been scheduled with a precision which seemed to imply English rather than Irish planning. However, a late start from Marlough House soon removed the veneer of order, and an enjoyable time was had by all. We started by exploring the keep at Greencastle and were given an informative talk about the archaeology of the building by Finbar McCormick, who had just read it up in the guide-book. Coffee, very good coffee at that, was then served at Mourne Grange, a Camphill community for mentally handicapped adults. The Grange farms sufficient land to produce much of its own food, collecting seaweed locally for manure. The quality of their rhubarb attracted some envious comments. From Mourne Grange, a scenic drive through the mountains took us to a portal dolmen and thence to Granena casbel and sputerrain: some real archaeology. This had been mooted as a site for lunch, but by general agreement the conference decamped to a

car park in Castlewellaun which was surrounded by diverse places of refreshment. Sandwiches were distributed from a large box in Barbara Noddie's car, the most vital element of the conference convoy.

The party split up for the afternoon, some returning to a sites and monuments survey of the hinterland whilst others availed themselves of a conducted tour of the Murlough nature reserve. The substantial dose of fresh air and exercise thus received put the company in the right physical and mental state for the evening. According to the schedule, dinner at 18.30 hours would be followed by an A.G.M. at 17.30 hours, an A.G.M., furthermore, which would not fall due until the autumn. At this juncture, to borrow Barbara Noddie's besuited turn of phrase, 'the ad hocery nearly broke down'. Only nearly, however, as the compromise was to move straight from dinner to lectures and then to hold a business meeting in the back room of the ever-accommodating Marina Bar until such time as everyone had had enough. Somewhere along the way, Siobhan Geraghty talked about her botanical work at Dublin's Fishamble Street ('The Real Viking Dig'), and Mike Bailie successfully merged dendrochronology with a form of cabaret. Both were impressive.

Sunday dawned, bringing with it a talk on archaeology and pollen analysis which was mainly the former, but which served well to remind the audience that ideal sampling strategies never happen in the real world. Following coffee, the animals were allowed to penetrate what had become a rather green scene, with a short report on shell middens from South West Ireland and a rather more detailed paper from Finbar McCormack (without a guide-book) which linked bones from a crannog with contemporaneous laws controlling tithes and the like.

To take the conference up to lunch, Jon Pilcher chaired a plenary discussion, and we witnessed the only serious sectarian incident of the weekend. Following comments from the floor, Jon summed up the feelings of many present by saying that it was high time that environmental archaeologists selected and excavated sites themselves, on environmental criteria. Amidst the murmurs of assent, John Barber (an archaeologist) loudly asserted his belief that digging a site properly is a highly skilled business which should be undertaken by real field archaeologists, and not by a bunch of scientists who have not been so equipped 'by God'. Given that the discussions of the previous night had shown that a large proportion of AEA members were qualified to become members of the Institute of Field Archaeologists and had every intention of joining, Barber's comments seemed to be those of a dyed-in-the-wool digger of the old, and narrow-minded, school. He was gently but firmly set upon by Jenny Coy, and we all tramped off to lunch feeling vindicated and slightly seag.

After still more splendid catering, the conference dispersed. Some travelled South to visit tombs in the Boyne valley, others made tracks for the ferry back to the 'Other Island', while a few meandered away into the countryside in search of scenery and poteen. The general feeling seemed to be that the conference had been a great success in its stated aim of bringing British and Irish colleagues together, not forgetting Willy Groenman van Waateringe. The grusally under-financed state of archaeology in Ireland had been very apparent throughout the meeting, although the persistence of the people involved gives cause for optimism. The diversity of the lectures (and of the lecturers) was unusual, and one hopes that this might become a feature of future meetings. Above all, though, this was a conference to enjoy. The setting was highly conducive to a relaxed atmosphere and the whole business was controlled with just enough organisation to keep to an

approximate schedule. Dot Gennard, Martin Munro, and the other Queen's University staff who ran the meeting are to be thanked and congratulated.

Terry O'Connor

Symposium: Conceptual Issues in Environmental Archaeology

The Editors have received notice of 'An international symposium' with the above title, to be held at the University of Oxford, 22-24 February 1985. The first circular reveals that the following discussion themes will be its base:

Conceptual issues at the site scale
 Conceptual issues at the microregion scale
 Conceptual issues at the macroregion scale
 Conceptual issues associated with cultural, spatial and environmental change

The organizers are: Drs Bintliff, Davidson, Grant and Shackley, and those interested in receiving the second circular are asked to contact Dr D. A. Davidson, Department of Geography, University of Strathclyde, Livingstone Tower, 26 Richmond Street, Glasgow, G1 1XH.

Book Review

Grigson, C. and Clutton-Brock, J. (eds.) 1983. Animals and archaeology: 2. Shell middens, fishes and birds. British Archaeological Reports International Series 183. 260 pp. 14 pounds.

This is the second of four volumes of collected papers from the 1982 ICAZ Conference, and its title will serve as a summary description of the 19 papers contained within. To review each paper separately would be tedious for reviewer and reader, and would miss several important general observations.

Overall, this is a very useful collection, even better than the first volume in this series. The papers are mostly of a high standard. Many have important ideas or results to contribute, and a few will become 'classic' papers, referred to time and again. One such should be Betty Neehan's account of shell-gathering strategies amongst native people in Northern Australia. Her observation that food-gathering groups will target on one species for a particular day while not completely ignoring non-target species is important in view of the almost mechanical randomness which some other workers attribute to human groups. Towards the end of this eminently sensible paper she comments

'Amongst [a] plethora of biologically sensible and neat explanations for man's behaviour, Anbarra shell-gathering has a welcome, human untidiness about it.' (p.16).

Fishy papers by Nee-Nygaard and Heinrich give excellent examples of well-argued deduction from cleverly elucidated data, although few workers will have either the time or the facilities to emulate

Noe-Nygaard's meticulous inter-disciplinary procedures. Amongst the wider-ranging review papers, Sanger discusses archaeozoology in the Gulf of Maine area (albeit accompanied by a map of the Caribbean) stressing the importance of co-operation between zoologists and archaeologists in research planning. He also echoes Meehan's cautionary comment with

'Monocausal explanations for cultural change may be economical but they are rarely satisfactory ...' (p.230).

Quite so!

Perhaps a reviewer who is not without sin should cast only plaudits and not stones, but it must be said that amongst the many good papers in this collection there are a few duds. Several are merely unconvincing, leaving behind them a feeling of 'all right, but so what?'. Amongst these is Priscilla Turnbull's contribution, enticingly entitled 'The study of clams and snails from archaeological sites', which turns out to be a very routine report of shells found at one Harappan site. An accurate but succinct title would have commended the paper to readers, bibliographers and abstracters. More profoundly disappointing is Colley's account of her work on fish bones from Orkney. There is too much jargonised theory-building and nothing like enough integration of archaeological results with these theories. Despite a useful survey of historical sources, the paper appears both over-long and incomplete, and the theorising is often rather self-defeating. The pity of it is that Colley clearly has plenty of hard data to present, but fails to do so.

Other authors, conversely, argue too much from too few data. Reitz and Cordier demonstrate an interesting application of allometric scaling, but perhaps accept the results too uncritically. Glaassen's examination of growth lines in North American bivalves is weakly argued, and some ingenious interpretations are squeezed out of decidedly inconclusive results. Despite this, her conclusion that communities gathered bivalves when there was an 8 in the month will please ostreophages. Weakest of all, perhaps, is Wing and Scudder's account of resource exploitation at the tropical marine edge. Their paper fairly bristles with biomass estimates and catchment area reconstructions, but a close inspection reveals the biomass figures to be estimates based on estimates unto the fourth generation, and there is also a naive failure to consider the relationship between recovered sample and killed population. The result is a facile paper overlain by a gloss of figures, the whole being disappointing and forgettable.

It is hard to know what to say about Arlene Fradkin's attempt to integrate linguistic data with archaeozoology. One can see what she is trying to say, namely that interdisciplinary integration of ideas can be fruitful and that linguistics and folk legends may give clues as to the status of a species in the socio-economic fabric, but this is hardly a presentation which inspires confidence in the methodologies involved. I was constantly reminded that pre-Christian Finnish mythology has the world created from a duck egg, and idly wondered about the incidence of bones of Bucephala clangula on sites in Lapland.

On the whole, the mollusc papers (pace Meehan) are the weakest in this collection, although papers by Deith and Shackleton show the potential of studies of shells. Bird specialists have given a good account of themselves, with the trend away from mere species-listing well to the fore in the work of Coy and Mouser-Chauviré. Northcote's account of size variability in mute swans suffers from sloppy referencing. One observation is attributed to the editors of a large

volume of collected papers with no details of author or page number. Taken collectively (as a shoal, perhaps), the fish-bone papers are most impressive, and it is evident that a lot of creative work is being done in this field. It is to be hoped that such advances will inspire bird and mammal specialists to take a fresh look at their procedures and methods.

Once again, the editors have assembled a valuable opus which will be an important source book for years to come. The exchange of location maps between Sanger and Reitz and Cordier was an unfortunate but understandable lapse, and this volume does have a few more misprints than its predecessor, but generally the standard of presentation is good and the contents appropriately meaty. Above all, to have half the papers in print within two years of the conference is a splendid achievement.

T. P. O'Connor.

Addendum to paper on epiphyses

Further to my suggestion that epiphyseal closure was only delayed in the male castrated soon after birth, I have now acquired a specimen which provides supporting evidence. This was a pedigree Chianina steer aged 2.5 to 3 years in which the distal femoral epiphysis was in the process of closing (standard data 3.5 years). Such an animal is likely to have been intended for the pedigree bull sales, and would have been castrated at about 4 to 6 months (information supplied by a breeder of such cattle) when it was realised that the required standard would not be achieved.

Barbara Noddle, Dept. of Anatomy, University College, Cardiff CF1 1XL.

Letter

Jennie Coy suggests that a videotape should be made to show the archaeologist what the archaeozoologist can do. But how does one rub his/her nose/eyeballs in the result? Nearly 20 years ago I suggested that the average archaeologist asked to work on the early motor car would produce a splendid typology of styling and dash board layout, but if asked about the engine would say 'But of course they all had engines'. For engine read food supplies or production. Little has changed apart from lip-service. The archaeologist would nowadays ask for an engine report from a 'specialist' and might even bother to read it in case he didn't like the grammar or suspected spelling mistakes.

The question is, who do they think we are? The medical specialist is a person of authority to whom difficulties and rarities are referred, and in this context the excavator is the GP. But what the excavator seems to have in mind is the specialist's technical staff, who prepare the slides or grow the bacteria, reporting on whether a standard set of tests is positive or negative. In the excavator's mind, any trained operative can do this, and the idea that the tests might be improved or reinterpreted neither interests nor concerns him. The technician can even be changed in mid test; incredibly some persons calling themselves environmental archaeologists accept this, having sold their souls to

the floppy disk.

I suggest that we can only convince our supposed masters otherwise by results. Our Roman army must march much faster on its stomach than a series of forts with square or round ends or whatever, give or take a bit of Samian. Circaea is a splendid outlet for our work, but how the hell do we make the excavators read it?

Barbara Noddle, Dept. of Anatomy, University College, Cardiff CF1 1XL.

P.S. I wish to except from these remarks the majority of amateur excavators and my old friends with whom I have been working for nearly 20 years.

**Sub-sampling animal bone assemblages:
reducing the work-load or reducing the information?**

Alan Turner *

Introduction

In a previous issue, Levitan (1983) discussed sampling methods which might reduce the time needed for the analysis of bone assemblages. His emphasis on cost-effectiveness makes a valid point, but I would like to examine the possible results of the proposed approach in a little more detail. Specifically, I should like to consider the question of whether sub-sampling may have the effect of reducing the work-load involved in analysis at the cost of the information available from the material.

Why sub-sample?

I should make it clear that I do not disagree with the spirit of Levitan's proposal that a judicious reduction in the amount of material actually examined might save considerable time for the purpose of answering certain questions. The example given by Gamble (1978), to which Levitan refers, shows very clearly the existence of a cut-off point at which further examination of a very large bone sample may give little if any additional information about the number of species represented at a site. Nor do I number among those who distrust any kind of statistically-based 'messing about' with the raw data of the bone counts, and I fully accept the theoretical justifications behind an appropriately conducted strategy of sampling or sub-sampling. But I do believe that the sampling approach may be of rather more limited use than an acceptance of its theoretical validity might at first sight imply.

The effect of sub-sampling

Levitan stresses the extent to which the various questions asked of a particular assemblage may each necessitate a different sampling strategy, contrasting the results of looking for relative species abundances with those of looking for age classes within a species based on counts of a given anatomical part. He rightly stresses the need for different sample sizes as a proportion of the total within the excavated assemblage depending on the question posed, and argues that experimentation will eventually produce guidelines for sampling techniques. In the course of that discussion, he uses various comparisons between the contents of an assemblage and any sub-sample of it to support his argument. But such comparisons largely serve to

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demonstrate the fact that appropriately drawn samples of sufficient size will adequately represent the population from which they were taken (using the term population in the general sense of the data-set from which a sample is drawn). The fact that a sub-sample of the excavated assemblage may preserve the relative proportions of, say, sheep to pig bones in that assemblage is not at issue. What concerns me is the potential loss of information about the production of those relative proportions which may result from an analysis which examines them in smaller absolute quantities.

At the heart of demonstrations of sub-sampling utility lies the assumption that proportions of species, anatomical parts, age classes or whatever within the assemblage reflect those within the killed population and that this reflection is maintained within any sub-sample drawn. This assumption is made, explicitly or implicitly, despite the fact that during an analysis one is clearly examining material which is the end product of selection processes, yet often drawing inferences appropriate to the material before those processes began. I am thinking here of attempts to quantify relative species or age-class abundances, and thus to reconstruct stock kill-off or hunting activities, usually achieved by one or other method of counting bone specimens or arriving at figures for minimum numbers of individuals in the excavated assemblage. Such approaches seem more likely to offer, at best, an idea of what was deposited at the site after the animals had been killed and butchered and the meat and other products disposed of. Actually getting from there to an understanding of what was killed and in what proportions is not an easy task, particularly when one considers that these activities may have taken place in any combination of on- and off-site localities.

It has been suggested elsewhere (Fieller and Turner 1982; Turner 1983) that a solution to the problem of quantification in archaeological analysis lies in the method of estimating the likely killed population from which a given assemblage component has been drawn based on observed distributions in the assemblage. Full details of the method, referred to as the Petersen Index, may be found in the papers referred to above, but in brief outline the approach considers the distributions of potentially pairing elements. For example, with 20 left and 25 right humeri of a species, with 5 matches between left and right, the method would give an estimate for the population from which they were drawn of

$$20 \times 25/5 = 100 \text{ animals}$$

with the added advantage that confidence limits may be attached to the estimate (Fieller and Turner 1982). The method depends upon the fact that if one draws a random sample from a population of paired elements then one will expect to get distributions of, in this case, left and right as well as pairing elements appropriate to the sample size drawn. Thus if we took the 100 left and 100 right humeri from 100 animals, placed them in a thoroughly mixed heap and drew around a quarter of the bones randomly, then upon finding that we had 20 left and 25 right bones we should expect to have around 5 pairs.

Use of the Petersen Index method means that comparisons between contexts, horizons or sites may then be made without the assumption that the assemblage offers a direct reflection of the distributions within the population from which it was drawn. That assumption is inherent in the use of non-parametric statistical techniques to examine similarities and differences between both assemblages and assemblage components. Use

of non-parametric methods restricts comparisons to the face-value proportions within samples without regard to the population from which they have been drawn, and hence ignores the fact that different assemblages may have been drawn from similar populations or vice versa; in other words, ignores the processes leading to assemblage formation. In contrast, the Petersen Index method not only bridges the gap between the assemblage and the killed population, but may also be employed to explore observed distributions in all manner of potentially pairing elements (besides left and right of particular skeletal parts) and to search for biases. Thus the fact the the method appears to suffer the practical drawback of assuming that observed distributions were produced by random selection processes may actually be employed to investigate biases which have resulted in deviations from expectation (Turner 1983). Such biases may reflect processes which have operated upon the assemblage following the initial selection of the killed population of animals, processes which may otherwise remain unobserved but which constitute the behaviour of those responsible for the formation of the assemblage.

As pointed out in our original discussion (Pieller and Turner 1982), the Petersen Index method of estimation is potentially able to track an original population size through a number of sample selection stages, provided that selection at each stage is random. Thus the effect of reducing sample size (in other words of sub-sampling in the case of Levitan's discussion) should be to preserve the original estimate. However, it is important to note that the original estimate is unlikely to be precisely preserved at the smaller sample size. To continue with the example given above, let us assume that we reduced the 45 bones to around half by a random sampling method such as that advocated by Levitan. We should clearly expect to end up with left and right bones in roughly similar relative abundances to those in the sample of 45 (although that expectation may be unwarranted and I shall return to the point below). Let us assume 10 left bones and 14 right bones occur in the sub-sample. We may use the formula given above to predict the number of pairs - I should stress that this is not a trick designed to give the right answer any more than knowing that a coin has two sides allows us to predict roughly equal numbers of heads and tails in a series of unbiased tossings - because we know what estimate should be made from the observed distributions. We have to calculate the number of pairs in this way because randomly reducing the number of bones by a half does not have the simple effect of reducing the number of pairs by a half also. Thus we may rewrite the formula as

$$10 \times 14/100 = 1.4 \text{ pairs}$$

Immediately we can see one of the major problems introduced by a reduction in sample size. Since we cannot have 1.4 pairs (even allowing for the most casual handling of materials) we must at the sample size now represented expect either 1 or 2 pairs. In that case our estimate of the original population size becomes 140 or 70 respectively, instead of its original figure of 100. It should be stressed here that the original estimate of 100 cannot in isolation be assumed to be the 'correct' figure except in experimental cases where the original number is known. In general cases, all three figures of 70, 100 and 140 simply express the best central estimate at the given sample size and with the observed distributions.

The second problem which presents itself when we reduce the sample size is that the confidence limits for the estimate will also change - in fact they will widen - so that even if the actual estimate itself

remains constant our interpretations of similarity and difference when we come to compare it with other estimates must alter also. The effect might be usefully compared with that produced by widening the quoted standard errors for a radiocarbon determination or an estimate of mean height in a population. And yet a third problem occurs if we consider the actual sample sizes of individual items which are often available in a bone assemblage. Most of us have probably made the depressing discovery that an apparently large sample may produce relatively few bones of a particular type from any one species, even if we add together left and right specimens. But if we wish to consider left and right specimens separately as part of the information about assemblage distributions, then sample size becomes even more critical. I said above that we might expect to preserve the relative abundances of left and right bones in the example given if we reduced the sample by half. With a large enough original assemblage that might be so, but if we consider the likely distortions in left versus right numbers which could be produced in small sub-samples in conjunction with the effects of number reductions on pairings (the latter as illustrated above) then serious problems of interpretation may arise.

In discussing the effect upon the Petersen Index when sample size is reduced, I made the point that one could not generally consider the first figure of 100 animals in the example given to be the 'correct' one. One might feel tempted to extend that argument to suggest that any figure produced, whatever the sample size, is therefore as good as any other since the whole thing is a guessing game anyway and the entire assemblage is itself a sample of what was originally there. But even if there were some validity in that argument, which I would dispute, I would also counter the point by suggesting that such problems do not in themselves warrant the increase in the difficulties posed which we risk when we sub-sample.

So my basic objection to sub-sampling as a general procedure is not that it necessarily does much to alter gross relative proportions of items in the assemblage, assuming appropriate procedures, but that in doing it we may lose information about distributions which will enable us to get beyond the raw data of the assemblage. Generalised arguments about the extent to which the sub-sample represents the assemblage may therefore be correct while at the same time confounding the wider issue. Some of the practical problems which may arise can indeed be seen in the recent paper by Maltby (1982) discussing the effects of unconsidered variations in bone assemblages when one attempts to interpret age-class frequencies. He found that apparent differences over time in the age structure of the kill-off pattern in ovicaprids from the Exeter excavations might be equally well explained by changes in the carcass disposal pattern. Whether Maltby's interpretation of the processes producing his observed patterns is correct, or his methods of quantification valid, are not particularly relevant issues here. What is important is the realisation that any attempt at a correct interpretation is likely to involve a consideration of distributions within the assemblage itself, of the kind discussed by Maltby. It is unlikely that the size of the whole assemblage would lend itself to useful sub-sampling for that purpose, albeit that the overall ratio of, say, ovicaprid mandibles to other skeletal fragments might remain generally the same down to perhaps a 10% fraction. Now that may appear to be no more than a repetition of Levitan's point that ageing data analyses may require substantially larger sub-samples than those which aim to know the relative proportions of sheep and cattle bones. However, I would argue that the objection to sub-sampling is of a more generalised nature.

The question which I suppose then arises is: in what circumstances can one advocate or support sub-sampling? Firstly, of course, in the case of a large assemblage where one simply wishes to know what species are present, although that seems a relatively trivial case. Secondly, in the case of a very large assemblage where one wishes to perform biometrical analyses. In such cases, selection of whole or at least measurable bones from the most secure contexts in each site phase might be argued to provide an adequate sample for the purpose. An example was alluded to by Tyldesley (1983) who quoted the instance of an estimated 3 million bones in store at York, although such cases, where the bones were previously unstudied, are, it is to be hoped, rare. A third and more common case might be one where perhaps 200 metacarpals identified as sheep (*Ovis aries* L.) have been recovered from a context. Let us assume that 90 are left specimens and 110 are right. Clearly, one could take the 110 right specimens as indisputably coming from 110 individuals and estimate, say, mean withers height or perhaps body weight on the basis of those alone (we will ignore inconveniences like sexual dimorphism for the moment). Alternatively, and since it would involve less work, one could simply do the same thing with the 90 left bones. Again, one could simply sub-sample the 110 right bones, thus building an element of extra randomness into the estimate. But a fourth possibility would be to use all the available data, performing calculations on all 200 specimens.

This latter approach is strongly advocated by Fieller (1980) in the similar circumstances of analysing data on the sizes of fish otoliths from archaeological sites. He argues that the increase in estimate precision which may be obtained by using the total numbers is in itself valuable, and outweighs problems posed by the fact that some at least of the left and right specimens may be expected to pair. The possibility of sexual dimorphism, likely to produce bimodality in the data, would itself suggest that the largest possible sample should be considered. Moreover, the fact that large proportions of many British domestic assemblages are persistently referred to the category ovicaprid, despite the existence of clearly applicable criteria for separation (Boessneck 1969; Boessneck et al. 1964; von den Driesch personal communication) implies that some attention should be given to the possibility of tetramodality (it may also imply that the day of judgement will be held in Munich). The point to stress here is that even in what appears to be a straightforward example of time saving through sub-sampling there may be substantial mathematical and biological reasons for retaining the full assemblage sample. The example may be thought simply a further confirmation of Levitan's findings that specific requirements like age-class analyses require larger data sets, but I would again suggest that a wider problem with sub-sampling is indicated.

Some conclusions

I suspect that quite a lot of thought still needs to be given to the entire issue of quantification in archaeozoological work. As regards sub-sampling strategies, I remain doubtful about the utility of such methods for many aspects of bone assemblage analysis. The issue is not whether a sub-sample can adequately reflect the biometric, taxonomic and anatomical patterns within the total assemblage from a site but whether the reduced quantity of material thus considered may obscure the relationships between the excavated assemblage and the killed population from which it was drawn.

I also have doubts about the actual practicalities of sub-sampling, taking into account the need for properly stratified, random samples to be drawn. It is all very well to demonstrate that a sub-sample will adequately represent a known population in hindsight, but the matter of defining that population in advance so that a proper sample may be obtained may present difficulties. A point raised by Levitan (1983, 7) is relevant here. In discussing the reasons why a sub-sample of the assemblage will not bias the analysis in the way that selective retrieval from the site might do, he argues:

'The important point here is that the sampling process can be rigorously controlled; we are dealing with a known population, which is not the case on site.'

The corollary of that statement is of course that one must have in some way examined the assemblage in order to deal with a 'known population' - unless of course one restricts the use of the term to mean something like the bones 'known' to be in the twenty boxes on the first shelf of the store. Levitan details the methods used in his experiments on sub-sampling as involving one person counting out the bones while another selected those whose numbers appeared on a random numbers table. Selection of a given percentage in that manner surely requires prior knowledge of at least the number of bones, since one could hardly take, say, one bone box in ten from a context and claim a randomised 10% sub-sample. But having taken a randomised 10% sub-sample, what does one then do about the bone fragments in the whole assemblage which may well go together to make single and perhaps measurable specimens, or the scattered teeth which may have been lost from a single jaw? In short, while the sub-sample may preserve the relative abundances of discrete items within the assemblage, how does one overcome the distortion and loss of information about distributions and relationships? I suspect the answer may be that one looks at the assemblage, which returns us to the point that certainly an adequate sub-sample may be obtained, but if we have to look at the assemblage to ascertain that, or to recover lost information, just how useful is the method?

And finally, because the use of non-parametric statistics overlooks the need to consider processes of assemblage formation, attempts to compare assemblage patterns within and between horizons and sites will necessitate maximum precision in the calculation of sample statistics and the estimation of population parameters. To obtain such maximum precision will require that sample size be as large as possible.

References

Noessneck, J. (1969). 'Osteological differences between sheep (*Ovis aries* Linné) and goat (*Capra hircus* Linné)'. In D. Brothwell and E. S. Higgs (eds.) Science in Archaeology, 331-58. London.

Noessneck, J., Müller, H.-H., and Teichert, M. (1964). Osteologische Unterscheidungsmerkmale zwischen Schaf (*Ovis aries* Linné) und Ziege (*Capra hircus* Linné). MHn-Archiv 78, 1-129.

Pieller, N. R. J. (1980). 'Appendix: a note on the statistical procedures', in P. A. Mellars and M. R. Wilkinson Fish otoliths as indicators of seasonality in prehistoric shell middens: the evidence from Oronsay (Inner Hebrides). Proceedings of the Prehistoric Society 40, 19-44.

Pieller, N. R. J. and Turner, A. (1982). Number estimation in vertebrate samples. Journal of Archaeological Science **9**, 49-62.

Gamble, C. (1978). 'Optimising information from studies of faunal remains'. In J. Cherry, C. Gamble and S. Shennan (eds.) Sampling in Contemporary British Archaeology. British Archaeological Reports British Series **50**, 321-53.

Levitan, B. (1983). Reducing the work-load: sub-sampling animal bone assemblages. Circoea **1**(1), 7-12.

Maltby, J. M. (1982). 'The variability of faunal samples and their effects upon ageing data'. In B. Wilson, C. Grigson and S. Payne (eds.) Ageing and Sexing Animal Bones from Archaeological Sites. British Archaeological Reports British Series **109**, 81-90.

Turner, A. (1983). The quantification of relative abundances in fossil and sub-fossil bone assemblages. Annals of the Transvaal Museum **33**(20), 311-21.

Tydesley, J. B. (1983). Cumulative frequencies and sample testing. Circoea **1**(3), 78.

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Editors' note: considering the contentious nature of Dr Turner's paper, we felt that it would be right to submit the manuscript to Bruce Levitan for his comments. He contributed the following reply.

Reducing the work-load - a reply

Bruce Levitan *

I am grateful to the editors for allowing me this opportunity to reply to Alan Turner's paper in the issue in which it appears. I do not think it is appropriate to make detailed comments since it is the general issues which are of most importance, and which I do not think have yet been adequately resolved.

In defence of my own paper, I must say that I did not intend to give any impression that sub-sampling is an ideal solution or that it is always appropriate. It may sometimes be necessary. This is because one is sometimes presented with exceptionally large assemblages. (That of York has already been widely quoted: perhaps Terry O'Connor might feel moved to lift his pen on this subject in the future.) Sub-sampling as I described it is a drastic (and last resort?) method because not all the information available is considered. If it is to be at all successful, the method of sampling chosen should be tailored to fit the particular

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problems under consideration. In such cases as West Hill, Uley, single contexts contain 30,000 bones or more and my prime concern was to speed up the time taken to record the material (and subsequently for analysis). Was sub-sampling the right method? After reading Turner's well-argued paper - and I am not qualified to comment on the statistics - one may conclude not. If I understand him correctly, Turner is suggesting that reduction of the information considered erodes some of the relationship between the bones and the population of animals to which they relate. If this is so, I do not think that he proved his case - if a sub-sample is truly representative of an assemblage, then why should one's interpretation of the data it provides be different from a consideration of the entire context given that the number of bones in the sub-sample is adequate? (Clearly it is foolish to take a 10% sub-sample from 10 bones, but what of 100,000 bones?). Nor does it appear to me that any time-saving alternative has been presented. Therefore, I still maintain that for certain basic questions the intrinsic, representative identity of the context need only be preserved, for example, for rankings of species in terms of fragment counts. Questions of more complex implication - e.g. biometrical studies - may certainly preclude sub-sampling, but usually such questions are concerned with only certain aspects of the assemblage anyway. Thus at one level one might identify, say, a 10% sub-sample in order to answer basic questions, but at another level, all the relevant elements will be considered. An important point is that at this second level not all the bones are considered, but only the measurable specimens in a biometrical study, or the mandibles in an ageing analysis, etc.

At the core of all the debate about sampling is not what method should be used, but the reason why it is necessary. The reality is that one is seldom in a situation where the luxury of choice about the information one is to consider is available. Rather, it is a case of doing one's best by an assemblage within the constraints applied (and these constraints are seldom academic). Personally, I have yet to complete a bone report where I was satisfied that all the avenues of information had been completely explored. I have simply reached what I felt to be a compromise between levels and range of analysis and the constraints set by facilities, time, money, etc. Turner's concern that

'sub-sampling may have the effect of reducing work-load involved in analysis at the cost of information available from the material'

is indeed relevant and laudable. But it is swings and roundabouts. Economies have to be made and the evil choice is where best to do so. My intention in my original paper was to bring forward ideas about one way of making economies in particular situations. I do not argue that this is the best way, but any criticism should, in fairness, put forward alternatives. If the issue is widened to consider whether or not we should have to make such economies, then the implications are far reaching indeed and must be argued at levels far above my own.

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**The use of early 19th century data in ageing cattle
mandibles from archaeological sites, and the relationship
between the eruption of M₃ and P₄**

Sebastian Payne *

The ages given for tooth eruption in sheep, goats, pigs and cattle by late 18th and early 19th century authors are, as has often been pointed out, generally considerably older than those given by more recent authors. Traditionally this has been held to reflect an acceleration of dental development over the past 150 years, resulting from improvements in animal husbandry and deliberate selection for rapid maturation. On this basis, it is widely believed that it is more reasonable to apply the slower timing given by these older authors to animal bone samples from archaeological sites than the more rapid timing given by modern authors for improved stock.

Meltinger (1983), in a recent and very useful compilation of sources on tooth eruption in cattle, reflects this traditional view. The earlier eruption timing given by more recent authors is held to 'result from the progress in cattle breeding', and age determination for cattle remains from prehistoric and early historic sites 'should better be based on the tooth-age teachings of the authors of the early 19th century' (Meltinger 1983, 92).

In two recent papers, however, the reliability of the data given by these early 19th century authors has been questioned, partly on the basis that what seem to be improbabilities in the sequence given for dental eruption cast doubt on the reliability of the observations involved, and thus on the reliability of the timing given (Lemix and Payne 1979 on sheep and goats; Bull and Payne 1982 on pigs).

The purpose of this brief note is to point out what appears to be a similar improbability in the sequence of tooth eruption in cattle as given by these early 19th century authors, to indicate how this can be tested using cattle mandibles from archaeological samples, and to ask for the help of readers of *Circaea* in making available the wider database needed to test this for different periods and areas.

Detailed examination of the tables compiled by Meltinger reveals several points of interest. Two of these are illustrated in Figures 10 and 11. Figure 10 shows that the age given for the eruption of M₃ does not, as might be expected, show a gradual decrease through the nineteenth and twentieth centuries. Instead, there is a rather sharp break between earlier sources, where the age of eruption of M₃ is usually given as between 3.5 and 5 years, and later sources, starting with Simonds (1855), which mostly give the age of eruption of M₃ as between 2 and 2.5 years.

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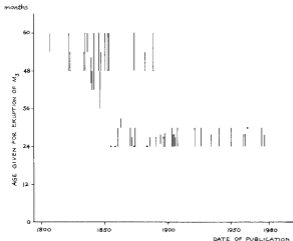


Figure 10. Ages given for the eruption of M_3 by authors at different dates. The information used is taken from Meitinger (1983, table 16), citing the following sources (see Meitinger 1983 for full references): Girard 1807, Schwab 1821, Gurit 1822, Gurit 1833, Schwab 1833, Eisele 1836, Rousseau 1839, Schwab 1839, Baumeister 1841, Baumeister 1845, Graf 1846, von Weckherlin 1846, Leyb 1850, Falke 1852, Müller 1853, Simons 1855, Gurit 1858, Gurit 1860, May 1862, Roloff 1870, Franck 1871, Fürstenburg and Rohde 1873, Adam 1873, Leisering and Müller 1873, Krafft 1881, Franck 1883, Leisering and Müller 1885, Chaveau 1888, Leisering, Müller and Ellenberger 1890, Cornevin and Lesbre 1894, Ellenberger and Müller 1896, Pusch 1896, Chaveau 1903, Ellenberger and Baum 1903, Martin 1904, Mirner 1904, Disselhorst 1906, Krafft 1921, Duerst 1926, Martin and Schauder 1934, Disselhorst and Löwe 1940, Sisson and Grossman 1950, Nickel, Schummer and Seiferle 1960, Habermehl 1961, Koch 1963, Fransson 1974, Barone 1976. (Later editions in which no changes are made have been ignored.)

At the same time, as Figure 11 shows, the relationship between the ages given for the eruptions of P_4 and M_3 also changes: according to the earlier sources M_3 erupts after P_4 , while according to the later sources M_3 erupts before P_4 . Given mandibles at the right stage of development, this can be tested in archaeological bone samples. Grant (1982) provides a summary of data drawn from a variety of sites which can be used for this purpose. Figure 12 clearly shows that, at these sites, M_3 in cattle erupted (i.e. cut the gum, which is about equivalent to Grant's stage '1/2') at around mandible wear stage 25-29, while P_4 erupted after NWS 30 (the mandibles recorded with P_4 at '1/2' are at NWS 35 and 39, but the overall pattern suggests that the range is probably wider - perhaps

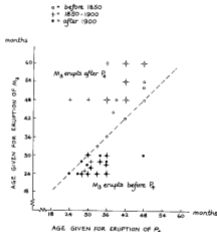


Figure 11. The relationship between the ages given for the eruption of P_4 and M_3 by authors at different dates (sources as in Figure 10). When a source gives a range rather than a single figure, two points have been plotted: thus when an author gives 24-28 months for the eruption of M_3 and 28-34 months for the eruption of P_4 , the early limits are plotted as one point at $M_3 24/P_4 28$, and the late limits are plotted as a second point at $M_3 28/P_4 34$. When a number of sources give the same figures, only a single point has been plotted, since this is often because one source has copied another.

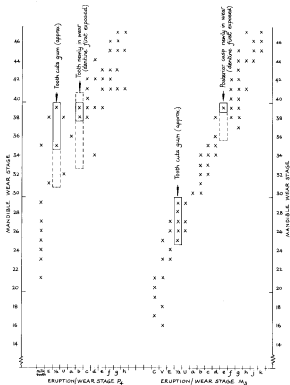


Figure 12. The relationship between the eruption/wear stage of P_4 and M_3 and mandible wear stage in cattle mandibles from a number of British sites, based on data given by Grant (1982, table 2).

around NWS 31-39). This, then, is in agreement with the sequence given by modern authors for 'improved' stock, and at variance with those given by the older authors.

Three possible explanations might be:

1. That M_3 did indeed erupt later than P_4 in the cattle on which these early 19th century sources are based, and that these cattle differed in this respect both from modern stock and from the stock represented by these archaeological samples. This is open to test, using mandibles from early 19th century contexts. Theoretically, such a difference might have either a genetic or an environmental basis. In either case, however, applying early 19th century data to archaeological samples whose sequence of eruption clearly differs from that indicated by the early 19th century sources, and agrees with modern stock, seems highly questionable.

2. That the early 19th century authors are not giving ages for eruption as we now define it - i.e. the point at which the tooth cuts the gum and can first be seen in the living animal - but for something else, possibly the age at which the tooth came into wear. While this interpretation might seem a little strained, it does provide one possible explanation for the difference. If the early 19th century sources are in fact giving the age at which the tooth is 'fully up', and if in the case of M_3 this is understood to be when the whole of the tooth, including the posterior cusp, is in occlusion, this, as Figure 12 indicates, happens at the same time as or later than the time at which P_4 comes into wear. If so, we should be applying the early 19th century data to the time at which this happens, and not to the time of initial eruption as we would now define it.

3. That the early 19th century authors are unreliable. This view was first expressed as early as 1902, by Brown, who said:

'It did not at first occur to the writer to question the truth of the assumption that the improvement of the various breeds of farm stock by selection and high feeding sufficiently accounted for the early appearance of the teeth; but now, after a further period of half a century has elapsed, i.e. from 1850 to 1902, without any marked change having been noted in the development of the teeth - although breeders have been during the whole period steadily devoting their knowledge and energies to the cultivation of the various breeds of cattle, sheep and swine - it is impossible to avoid the conclusion that the original version of the development of the teeth was based on imperfect observation, or on the custom of one writer quoting in good faith from another' (Brown 1902, 4).

This note, then, has two purposes. First to suggest that there is enough doubt about the applicability, the precise meaning, or the accuracy of the information given by these early 19th century sources for us to be very cautious about using this information to age mandibles from archaeological sites. Second, to ask for the assistance of readers of Circaea working on sites with cattle mandibles at the appropriate stage of development (covering the eruption and early wear of M_3 and

P₄) in making available data with which to look at the relationship between the eruption of M₃ and P₄ at different periods and in different areas. Any information would be most gratefully received.

References

Bull, G. and Payne, S. (1982). 'Tooth eruption and epiphyseal fusion in pigs and wild boar.' In B. Wilson, C. Grigson and S. Payne (eds.) Ageing and sexing animal bones from archaeological sites. British Archaeological Reports British Series 109, 55-71.

Brown, G. (1902). Dentition as indicative of the age of the animals of the farm. (Revised edition). Royal Agricultural Society of England.

Deniz, E. and Payne, S. (1979). 'Eruption and wear in the mandibular dentition of Turkish Angora goats in relation to ageing sheep/goat mandibles from archaeological sites.' In M. Kubiakiewicz (ed.) Archaeozoology, 1, 153-63. Agricultural Academy, Szczecin.

Grant, A. (1982). 'The use of tooth wear as a guide to the age of domestic ungulates.' In B. Wilson, C. Grigson and S. Payne (eds.) Ageing and sexing animal bones from archaeological sites. British Archaeological Reports British Series 109, 91-108.

Weitinger, E. (1983). Die Zahnalterbestimmung beim Kind in ihrer Bedeutung für die Osteoarchäologie. Eine Literaturstudie. Institut für Paläoanatomie, Domestikationsforschung und Geschichte der Tiermedizin, Munich University.

Simonds, J. B. (1855). On the teeth of the ox, sheep, and pig, as indicative of the age of the animal. Journal of the Royal Agricultural Society of England 13, 276-362.

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**Environmental archaeology and research into the
physical environment**

W. E. Boyd *

At the AEA Sheffield meeting in September 1983, Judith Shackleton presented a most interesting lecture in which she discussed the importance, for archaeological research, of research into changes in sea level and coastal configuration (van Andel and Shackleton 1982; Shackleton in press; Shackleton et al. in press). Her work concentrated on the Mediterranean area, but such research is applicable elsewhere, and need not be limited to sea-level and coastline changes.

I was particularly interested in this theme, because the research I undertook for my doctoral thesis concentrated on Late Quaternary coastal changes in Scotland (Boyd 1982a). Towards the end of this research I discovered, and am still finding, archaeological applications for the results of what was primarily a geological study. Although the results of geological and geomorphological research have been widely used in archaeological studies, I often wonder how such research into the physical environment (as opposed to the biological environment) is done explicitly to contribute to archaeological research or to solve archaeological problems. Consequently, I thought that it may be useful to describe some of the examples of such work, partly to supplement Judith Shackleton's material with data from Britain, and also to illustrate the great potential that research into the physical environment holds for archaeologists. The three examples which I will describe below represent a development in thought; the first one represents the solution of an archaeological problem which followed as a consequence of the geological research, whereas the latter two were approached explicitly as archaeological problems which required geological answers.

Example 1. In the area around Shewalton Moss, on the coast of North Ayrshire, West Central Scotland, Mesolithic flints are found on the raised bench surface. These have posed a major problem since by conventional thinking the flints lie either seaward of the coastline which was presumed to have existed during the period of Mesolithic occupation of the area, i.e. they were deposited under water, or they represent anomalously late Mesolithic occupation of the area (Morrison, personal communication). Both possibilities are improbable.

This problem was resolved by geomorphological and stratigraphical study of the coastal (mainly onshore) deposits, most of which have little direct contact with archaeological evidence. The results of this study (Boyd 1982b) indicate that during the Flandrian marine transgression, which occurred in this area during the period of

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Mesolithic occupation, the inland limit of the shoreline lay nearer to the present coast than previously considered (cf. Jardine 1971), and therefore that the distribution of Mesolithic flints was anomalous neither in space nor in time. Another consequence of the research was the provision of a detailed palaeoenvironmental model which indicates the potential of this area for Mesolithic occupation (cf. Bonsall 1980) and further contributes to archaeological study of this area (Norrison 1982).

Example 2. Immediately to the north of the area of example 1 lies the town of Irvine, a town whose roots go back at least to early medieval times. Indeed its early history was dependent on the fact that the coastal morphology was, even as recently as AD 1600, considerably different to that at present (Boyd, in Strawhorn, in preparation). The problem to be approached was where would a possible Roman port have been located? Its presence is only hinted at in documentary sources, and although there have been a few finds of Roman coins, there are no substantial Roman finds, let alone any indication of the presence of a port (J. Strawhorn, personal communication). So where would the port have been had it existed?

The solution is, as yet, only a partial one, but at least several realistic possibilities have been identified. Based on the coastal environment research in the area south of Irvine (Boyd 1982a), on models which emanated from that research, and on consideration of fluvial and coastal processes, it became apparent that during the Roman period, the coastline lay beside the present site of Irvine, now an inland town, that there was a small estuary running south to north immediately to the west of Irvine, and that parts of the Roman coastline have probably been removed subsequently by fluvial erosion and replaced by fluvial flood plain sediments. Consequently, the Roman port of Irvine either lay on the site of the present town, for which there is as yet no evidence, or it lay immediately to the north of the town, in an area of coastland which has subsequently been eroded and replaced by fluvial sediments. The search for a Roman port at Irvine may, therefore, be fruitless one, and this rather negative solution must be borne in mind during any future archaeological work.

Example 3. At Shiels, near Glasgow, an Iron Age ditched enclosure has been excavated (Scott 1973, 1978, in preparation; Robinson 1983). This enclosure lies at c. 7.35 m above Ordnance Datum, on the flood plain of the River Clyde. Being in an area in which relative sea level has lain, at times, higher than this elevation, it is important to understand the behaviour of the water levels, both of the sea and of the River Clyde, during the period of occupation at the Shiels enclosure. Such information may contribute to the understanding of the nature of the occupation.

Using sea level index points (known elevations of dated sediments whose exact relationship to some former tide level is clearly understood) from elsewhere in this region (Boyd 1982a), it is possible to calculate the changes in altitude of the local high tide level during the period of occupation. Also, consideration of fluvial processes and sedimentation during this period contributes to a model of the physical environment at this time (Boyd, in Scott, in preparation). Being able to quantify certain aspects of the physical environment and accurately identify others in this manner allows further understanding of the role of this site in the landscape and emphasises certain of the advantages and disadvantages incurred during the occupation of the site.

These three examples represent only the tip of the potential iceberg of archaeologically-based geological research. Obviously the results of such research, as in other lines of environmental archaeology, may be of varying satisfaction. However, the examples serve to illustrate the potential for 'archaeological geology'. All the examples could be expanded upon, and clearly for these, and many other cases, geological research programmes based on archaeological problems could and should be formulated.

References

Bonsall, C. (1980). The coastal factor in the Mesolithic settlement of north-west England. Veröffentlichungen - Museum für Ur- und Frühgeschichte Potsdam 14/15, 451-72.

Boyd, W. E. (1982a). The stratigraphy and chronology of Late Quaternary raised coastal deposits in Kentirewshire and Ayrshire, western Scotland. Unpublished Ph.D. Thesis, University of Glasgow.

Boyd, W. E. (1982b). Archaeological implications of a new palaeoenvironmental model for part of the Ayrshire coast. Glasgow Archaeological Journal 9, 15-17.

Jardine, W. G. (1971). Form and age of Late Quaternary shore-lines and coastal deposits of south-west Scotland: critical data. Quaternaria 14, 103-14.

Morrison, A. (1982). The Mesolithic period in south-west Scotland: a review of the evidence. Glasgow Archaeological Journal 9, 1-14.

Robinson, D. E. (1983). 'Pollen and plant macrofossil remains from the ditched enclosure at Shiels, Govan, Glasgow', in Jones, M. (ed.) Integrating the subsistence economy. British Archaeological Reports International Series 181, 123-34 (Symposium of the Association for Environmental Archaeology 3).

Scott, J. G. (1973). Shiels. Discovery and Excavation in Scotland 1973, 66-7.

Scott, J. G. (1974). Shiels. Discovery and Excavation in Scotland 1974, 82-3.

Scott, J. G. (in preparation). The excavation of the Iron Age ditched enclosure at Shiels, Govan, Glasgow.

Shackleton, J. C. (in press). Paper on the coastal palaeogeography in the Mediterranean and its archaeological implications, to be published in N. Fieller, D. Gilbertson and N. Ralph (eds.) Palaeoenvironmental Investigations: research design, methods and interpretation. British Archaeological Reports International Series (Symposium of the Association for Environmental Archaeology 5).

Shackleton, J. C., van Andel, J. H. and Rummels, C. W. (in press). Coastal palaeogeography of the Central and Western Mediterranean during the last 125,000 years and its archaeological implications. Journal of Field Archaeology

Strawhorn, J. (in preparation). The history of Irvine: Royal Burgh and New Town.

van Andel, J. H. and Shackleton, J. C. (1982). Late Palaeolithic and Mesolithic coastlines of Greece and the Aegean. Journal of Field Archaeology **9**, 443-54.

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A discussion of the status of wood sage in pollen analysis

Brian Moffat *

The wood sage or wood germander (*Teucrium scorodonia* L.) is a member of the Labiatae, the family which includes the mints and calamints, dead-nettles, thyme and woundworts. Labiatae pollen is rarely abundant (>5% of non-tree pollen) in U.K. pollen records, and *Teucrium* genus pollen is not usually distinguished although Faegri and Iversen's standard key shows this to be practicable. With a full type-slide collection the writer finds it straightforward.

This note argues that the species merits fuller consideration. In the standard national flora (Clapham, Tutin and Warburg 1962) 26 genera of native and naturalised Labiatae are listed. A look at the habitats listed there shows a preponderant grouping in a few particular habitats: wetlands and watercourses; woodland and woodland edge; arable fields; waste ground; and calcareous soils (these particularly in southern England). Plainly these are, in ecological terms, restricted. Furthermore, the calcareous areas of southern England are notorious for the scarcity of deposits suited to pollen analytical work; the preservation of pollen is poor and unreliable. Notable exceptions, long-term and radiocarbon-dated, are studies at Leven Brooks, East Sussex, and Fringholt and Wingham, Kent (Thorley 1981, Godwin 1962). The writer knows of no analogous work connected with any archaeological site on chalklands and the like.

Wood sage, in contrast to these, inhabits grassland, particularly where lightly grazed, heath and sand dunes, the broken ground of slopes and scree. Such habitats are distinctive, extensive and widespread. There is some overlap in habitats of woodland and woodland-edge.

The only common Labiatae which share habitat to some degree with wood sage are: thyme (*Thymus drucei* Koenig) on dry grassland, heath, dunes, screes, among rocks; self-heal (*Prunella vulgaris* L.) on grassland, clearings in woods, waste places, mainly on basic and neutral soils; betony (*Betonica officinalis* L.) in open woods, hedgebanks, grassland, heaths, usually on lighter soils. Although 'common', wild marjoram (*Origanum vulgare* L.) is primarily associated with calcareous soils and therefore not with pollen analytical sites. It is argued that no other species need be considered. Wood sage, thyme and self-heal are common throughout the British Isles. Yet wood sage is alone in being a provenly persistent perennial (with large clones at 50-100 years old) which grows in large, often exclusive, patches. The largest seen by the writer covered 1.8 m², while 1.0-1.2 m² was common (Clapham, Tutin and Warburg 1962; Hutchinson 1968). The erect growth of stems en masse would promote the free dispersal of pollen; in no other species considered was this the case.

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I can amplify the habitat description of wood sage in two ways, both of archaeological significance. There is a marked association of the plant with cleared areas in deciduous woodland (intrinsic to the traditional coppicing cycle) including areas where there has been burning (so-called 'fire-patches') and ground cover has not been restored (it has extensive subterranean rhizome growth). This has been recorded in several studies of post-coppice floristic change in south-east England (Table 10); these amount to a substantial number of the works on post-coppice floristics yet traced by the writer.

Table 10

wood type	soil type	remark	reference
oak-hornbeam (Hertfordshire)	clay-with-flints light clays stiff loams	frequent; common where coppiced; frequent on light soils	Salisbury 1916
oak-hazel; some scrub (Ditcham Park, Hampshire)	'chalklands'	in lighter phases frequent or locally abundant	Adams 1921
oak-Spanish chestnut (Han Street NRH, Kent)	not stated	29% of ground vegetation production in year 2 after cut	Ford and Newbould 1977
oak-birch- Spanish chestnut -hazel (High and Pear-tree Woods, Saxhill, E. Sussex)	sandy loams; Tonbridge Wells Sands	common, especially on lighter soils	Moffat, unpublished

Table 11

	Clones:	
	number	%
Classified by slope:		
Ground level or with slight slope - <20°	58	12.1
On slopes >20° but <40°	382	79.6
Ground of marked slope >40°	40	8.3
Sum:	480	100
The clones were distributed with respect to other criteria thus:		
At break in slope; at base of a turf or rock	442	92.1
In continuous turf or tussock	17	3.5
Where there is recent evidence of burning (blackened gorse stems)	112	23.3

There is a curious association with broken ground on Scottish upland. Wood sage may be seen in profusion on Whinny Hill, Crow Hill

and Salisbury Crags (in Holyrood Park, Edinburgh) and in the Ochil, Sidlaw and Pentlands Hills. In Holyrood Park, some field notes were taken on established clones (>0.5-0.4 m² in area, and >30 flowering heads) along transect lines. 480 such clones were recorded in July and October 1982 (Table 11).

Whinny Hill was the core of the area surveyed. Its area, bounded by encircling road and paths is about 0.5 km². It is a 'crag-and-tail' feature with crag on north-west, west and south-west sides of an 'asymmetrical dome', and 'tail' elsewhere. The summit is at 178 m OD. Classes in slope morphometry at the macro-scale are estimated to be:

Slopes at -	<20°	ca. 60%
	>20° and <40°	ca. 25%
	>40°	ca. 15%

No further refinement in the scale of preoccupation is appropriate.

There is plainly a high correlation of the incidence of wood sage with moderate slopes, broken and open and burned-over ground (on the micro- and larger scale). This post-fire abundance was also recorded in Shieldaig, Wester Ross, after a pinewood took fire; it had about 50% frequency in 120 plots over 6 years (Sykes and Morrill 1981).

Grazing pressures seem to influence the distribution of wood sage. Much of the evidence is anecdotal - sheep and goats eat it readily, and cattle sparingly. It adulterates milk, making it garlicky. On Whinny Hill it seems not to grow in the closely grazed-over areas by rabbit burrows, nor on heavily trampled paths (as on much of Arthur's Seat itself). Sheep grazing was stopped in the park in the early 1970s, from which time wood sage may well have expanded in distribution. There are numerous immature plants. The plant may also have been cropped, both for its varied herbal uses and as it is a 'natural bitter' and a substitute for the hop (Pratt 1899; Anon 1789; author's observations).

Further to this, the cover of wood sage may be rapidly surveyed by photograph. In the months September to December, the flowering heads wither to upstanding stems of a highly distinctive rich chestnut-



Figure 13. A stem of wood sage (*Teucrium scorodonia* L.). To 60 cm tall; pale yellowish-green petals; softly hairy leaves.

brown. With a view over the area to be surveyed, wood sage in large clones is discernible at distances up to about 250 m. At this time of year, the distinct colouration of common associate plants, such as sward- and tussock-forming grasses, gorse, elder, stinking groundsel, and rosebay willowherb, is striking. This photogenicity enables an overview of the abundance and distribution of present-day vegetation to be gained. Thus the vegetation local to pollen-bearing deposits may be summarily described by photographs, and the relation to the most recent spectra discussed and ascertained.

The candidacy of wood sage for more detailed study has been discussed and found to be worthwhile.

References

- Adams, R. S. (1921). The woodlands of Ditcham Park, Nants. Journal of Ecology **9**, 114-219.
- Anon (1789). Annals of Agriculture **1**, 247-8; **2**, Index.
- Clapham, A. R., Tutin, T. G. and Warburg, E. F. (1962). Flora of the British Isles. (2 ed.) Cambridge.
- Ford, E. D. and Newbould, F. J. (1977). The biomass and production of ground vegetation and its relation to tree cover through a deciduous woodland cycle. Journal of Ecology **65**, 201-12.
- Gedwin, H. (1962). Vegetational history of the North Downs as seen at Wingham and Frogholt. Veröffentlichungen - Geobotanisches Institut Alpbach **37**, 83-99.
- Hutchinson, T. C. (1968). Biological Flora of the British Isles: Teucrium scorodonia L. Journal of Ecology **56**, 901-11.
- Pratt, A. (1899). The flowering plants of Great Britain, **4**, London.
- Salisbury, E. J. (1916). The oak-bornbeam woods of Hertfordshire. Journal of Ecology **4**, 83-117.
- Sykes, J. M. and Borrill, A. D. (1981). Recovery of vegetation in a Caledonian pinewood after fire. Transactions of the Botanical Society of Edinburgh **43**, 317-25.
- Thorley, A. J. (1981). Pollen analytical evidence relating to the vegetational history of the chalk. Journal of Biogeography **8**, 93-106.

Taxonomic note. Teucrium scorodonia now has 3 congeneric species in Britain. T. chamaecrys L. (wall germander) is an introduction of the last few centuries. T. scordium L. (water germander) and T. botrys L. (cut-leaved germander) are native, rare and highly localised. Habitats include:- chalky ground and banks; dune slacks; walls. Noting the absence of work in the Northwest European Pollen Flora (NEPF), these 3 taxa are judged to be of negligible palynological and archaeological significance.

Disease and the elm decline: a method of testing the hypothesis?

B.K. Maloney *

Introduction

Why elm should have declined suddenly in north-west Europe during the few centuries centred on 3000 B.P. has been a source of controversy for years. A variety of possible explanations (cf. ten Have 1968) have been suggested: climatic deterioration, forest clearance by man, lopping of trees for animal fodder, disease, or a combination of factors. The likelihood that man was partly or wholly responsible makes the matter of considerable importance to the environmental archaeologist, although the evidence has necessarily been accumulated by pollen analysis of sediments from basin environments and not archaeological sites. The aim of this paper is to discuss briefly if and how the disease hypothesis can be effectively tested and to stimulate discussion by those who have, perhaps, better information on the subject than I as a palynologist have.

It is very easy to argue for or against the disease hypothesis in explanation of the elm decline in prehistory without ever trying to test it. The theory seems to have been proposed initially by Aletsee (1959) while Smith (1960) suggested that the activities of man, damaging elm trees, may have contributed to disease intensification: the interaction of man and nature. Heybroek (1963), a forest pathologist, stated that native pests and diseases in general have a remarkably small influence on the composition of more or less natural vegetations but introduced diseases may annihilate susceptible species. He subscribed to the view that Dutch elm disease was probably introduced into Europe in the first or second decade of this century but Rackham (1960, 262-4) challenged this, presenting records of possible incidences of the disease in Britain dating back as far as the 15th century and suggesting (*ibid.*, 266) the feasibility of an even earlier origin.

The pathogens

Dutch elm disease (Heybroek 1963) is caused by a fungal pathogen (*Ceratocystis ulmi* (Buism.) C. Noreau) dispersed by elm bark beetles of the species *Scolytus scolytus* (Fabricius) and *S. multistriatus* (Marshall). It interferes with the sapstream so that the canopy eventually wilts and often dies. Trees can recover from the disease but pollen production obviously decreases or ceases altogether while they are under attack.

Phloem necrosis is apparently the only other disease of elm which occurs in epidemic proportions and Heybroek (1963, 3) claims that this viral disease is carried by *Scaphoidea luteola*, a species which I have

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been unable to trace in the entomological literature, but which is said not to occur in Europe. Smith (1972) states that no vector of phloem necrosis (elm zonate canker disease) is known, however, and I cannot verify Heybroek's statement that the American elm (*Ulmus americana* L.) was severely affected by an outbreak in 1870: Swingle and Bretz (1950) labelled zonate canker as a previously undescribed disease.

Heybroek also reported that several pathogens, e.g. slimeflux (*Nectria cinnabarina* (Tode ex Fries) Fries), and parasites (*Polyporus squamosus* Fr., for instance) may shorten the lives of lopped trees while Rackham (1980) claimed that elms are probably attacked by more wood-rotting fungi than any other tree. These wood-rotting fungi were said to include many Polyporaceae and species of *Pleurotus* (Fr.) Kummer. Two taxa, *Lycophyllum* (*Pleurotus*) *ulmarium* (Ball ex Fr.) Kühner and *Rigidoporus ulmarius* (Fr.) Imazeki, are specific to elm. The commonest disease of elm was stated to be of bacterial origin however: bacterial wetwood, attributed especially to *Erwinia nimpressuralis* Carter. This invades the heartwood via the roots or through wounds.

When a serious new epidemic of Dutch elm disease began in the United Kingdom in about 1970 it was discovered that there were two strains of the fungus (Brasier 1979): a highly pathogenic 'aggressive strain' thought to have been imported from North America, and responsible for the outbreak, and a less pathogenic 'non-aggressive strain' which may be endemic to the United Kingdom and adjacent areas of Western Europe. Additional research demonstrated that there were two distinct races of the 'aggressive strain' which entered Europe independently. Aggressive isolates from the United States, Canada, Holland, France and Germany grew into regular, striate colonies under culture while those from Italy and Iran were uneven and less striate. The two races had a low fertility when hybridization was attempted. By the end of 1978 the Eurasian race was in the Soviet Union, Bulgaria, West Germany, Denmark and Ireland as well as Italy, while the North American race remained concentrated in the United Kingdom and adjacent areas of Western Europe. Already some 25 million elms have been killed in Southern England. However, a virus-like agent which attacks the fungus has been discovered (Brasier 1983) and bark beetles could be bred to spread spores contaminated with this, and so counteract the disease.

Testing the hypothesis

Remains of macrofungal parasites are most likely to survive where woody material is preserved. *Fomes fomentarius* (L. ex Fr.) Kickx has been found at archaeological sites (Clark 1954; Shackley 1981; Purdy and Purdy 1982) but *Rigidoporus ulmarius* has not. However, macrofungi are unlikely to have been the cause of epidemics. The most detailed study of fossil microfungal remains from the Holocene of Western Europe (van Geel 1976) did not report *Ceratocystis* at the generic level, let alone at the species level, and this is not really very surprising as the various types of spore are very small and unlikely to be readily identifiable by light or phase contrast microscopy. (Scanning electron microphotographs of conidia are available in Harris and Taber (1973) and Hiratsuka and Takai (1978) and of chlamydospores in McNeel et al. (1983).) *Ceratocystis ulmi* adopts a variety of reproductive strategies (cf. McNeel et al. 1983). It can grow from vegetative hyphae, budding yeast cells, ascospore-producing perithecia, conidiospore-producing synnemata and it has a conidiospore-producing sporothrix stage. Ouellette and Gagnon (1960) have claimed that it produces microendospores too.

The difficulty of identifying fungi from the various types of spore produced at different stages in their life cycle is by no means rare in mycology but this can be overcome by growing the plants in culture from the spores provided that they are still viable. This approach may be the only way of assessing the incidence of Dutch elm disease in the past as not only is it not possible to identify the spores, but it cannot be assumed that the insects consistently carried the disease even if fossil remains of bark beetles are preserved. However, it has yet to be demonstrated if Ceratocystis ulmi spores are preserved in anaerobic environments and how long they retain viability. They are insect- and not wind-dispersed and, in the same manner as pollen, one might expect spores of insect-borne species to be produced less abundantly than those of wind-dispersed taxa. Our only guide to viability is that actinomycetes micro-endospores appear to preserve and remain viable for thousands of years (cf. Unsworth et al. 1977).

Fungal spores from peat have been grown in culture before. Moore (1954) took samples from two profiles, unfortunately neither C14 dated nor relatively dated using pollen analysis, in the Wicklow Mountains, Eire, and found that viable taxa occurred down to 70 cm depth in one peat ('A') but disappeared below 25 cm in another ('B'). In these instances the viable species were mainly in the genus Penicillium Link.

Collection of samples under sterile conditions is essential in this form of analysis as fungal spores are abundant in the atmosphere. Moore (1954) recognized this and extracted plugs of peat from a cleared face using sterile glass specimen tubes which were subsequently tightly stoppered. It is not feasible to sterilize a pollen corer before and after collection of each core segment in the field but this should not prove necessary providing that the samples cultured are from the centre of each core (cf. Unsworth et al. 1977), although control experiments are desirable to verify this. The upper few centimetres of lake sediments are frequently too fluid to be collected using conventional corers and can be important in testing recent incidences of Dutch elm disease, but Shapiro (1958) has given details of a freeze-corer which will not only collect the samples but will retain them in a frozen, and therefore sterile, condition. However samples are collected, it may be advisable to take them back to the laboratory in a cool box to reduce the possibility of germination of various micro-organisms in transit.

If the samples are not to be prepared immediately on return to the laboratory it is important to ensure that they are kept under both cool and sterile conditions. It is common practice among palynologists to store cores under cold conditions (4° C or less) or, if they are not to be worked on for some time, at a constant temperature in a deep-freeze. Freeze-drying is a much-used method of maintaining viable culture collections of microfungi (cf. Unions 1983) but storage in a deep-freeze at -20° C. should not kill the viable spores in fossil deposits and involves less effort.

Maintenance of sterility during laboratory work is also essential. All glassware must be cleaned properly and the culturing medium must be sterile. Alexopoulos and Benseke (1955) show how this can be achieved. Gibbs and Brasler (1975) used a 2% oxid malt agar to culture Ceratocystis ulmi and insertion in an Arnold sterilizer is necessary to ensure that this is kept sterile.

Culturing can be carried out by mixing the culture medium (using sterile tools) with some fossil lake sediment or peat on a sterile petri

dish. The petri dish should then be placed in an incubation chamber and incubated at 18° C. following the method of Gibbs and Brasier (1973). Identification of the growth colonies which result is a specialist task which will require the assistance of a trained mycologist.

It is absolutely essential that replicated control experiments are carried out to ensure that the viable *Geratocystis* spores do not derive from the atmosphere or other sources in the laboratory. These can be effected by simply using 2% oxidant malt agar on sterile petri dishes.

Fortunately neither *Geratocystis ulmi* nor its imperfect form *Graphium ulmi* Buis appear to occur in the soil (Gilman 1957; Donach and Gams 1972) so colonies cultured from lake sediment or peat are likely to have arisen from spores which have been transported in from plant sources. It is reasonable to conclude that the disease hypothesis is not easy to test, but the best prospects of success seem to lie with the culturing of *Geratocystis ulmi* spores or micro-endospores using sediments from anaerobic environments. If this was carried out, it might give evidence of Dutch elm disease in historical times pre-dating the beginning of the 20th century as well as in prehistory.

The growth of collaboration between palynologists and mycologists, and between archaeologists and mycologists has much to recommend it: although one has some doubts about their preparation techniques, Stewart and Robertson (1968) claimed that fungal spores of taxa saprophytic or parasitic on cereal crops occurred in the matrix of plant-tempered pottery from Jarmo; and there are taxa confined to animal remains, animal coprolites and burnt-over land (Hudson 1980) which might survive in sediments where no other evidence remains, or may suggest data from other sources. Thus the study of microfungal remains as a tool of environmental archaeology appears to have some potential, as the old bogey, that of contamination, is one that can be resolved given rigorous application of the scientific method.

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References

- Altaeus, L. (1959). Geschichte der Moore und Wälder des nördlichen Holsteins. Nova Acta Leopoldina **21**, 1-51.
- Alexopoulos, C. J. and Meneke, E. S. (1955). Laboratory manual for introductory mycology. Minneapolis.
- Brasier, C. M. (1979). Dual origin of recent Dutch elm disease outbreaks in Europe. Nature **281** 78-80.
- Brasier, C. M. (1983). A cytoplasmically transmitted disease of *Geratocystis ulmi*. Nature **305** 220-3.

Clark, J. G. D. (1954). Excavations at Starr Carr: an early Mesolithic site at Seamer near Scarborough, Yorkshire, Cambridge.

Donsch, K. H. and Goss, W. (1972). Fungi in agricultural soils. London.

Gibbs, J. N. and Brasier, C. M. (1973). Correlation between cultural characteristics and pathogenicity in Geratocystis ulmi from Britain, Europe and America. Nature **241**, 381-3.

Gilman, J. C. (1957). A manual of soil fungi. (2 ed.) Iowa State University.

Harris, J. L. and Taber, W. A. (1973). Ultrastructure and morphogenesis of the synnema of Geratocystis ulmi. Canadian Journal of Botany **51**, 1565-71.

Heybroek, H. M. (1963). Diseases and lopping for fodder as possible causes of a prehistoric decline of Ulmus. Acta Botanica Neerlandica **12**, 1-14.

Hiratsuka, Y. and Takai, S. (1978). Morphology and morphogenesis of synnemata of Geratocystis ulmi. Canadian Journal of Botany **56**, 1909-14.

Hudson, H. (1980). Fungal saprophytism. Studies in Biology 52. London.

MacNeel, D. J., Kulkarni, R. K. and Nickerson, D. W. (1983). Pleomorphism in Geratocystis ulmi: chlamyospore formation. Canadian Journal of Botany **61** 1349-52.

Moore, J. J. (1954). Some observations on the microflora of two peat profiles in the Dublin Mountains. Scientific Proceedings of the Royal Dublin Society **26**, 379-95.

Oelons, A. H. S. (1983). 'Preservation of fungi', in Smith, J. E., Berry, D. E. and Kristiansen, E. (eds.) The filamentous fungi 4: Fungal technology. London.

Ouellette, G. B. and Gagnon, G. (1960). Formation of microendospores in Geratocystis ulmi (Baism.) C. Moreau. Canadian Journal of Botany **38**, 235-41.

Purdy, L. H. and Purdy, B. A. (1982). Ancient polypores from an archaeological wet site in Florida. Botanical Gazette **143**, 551-3.

Rackham, O. (1980). Ancient woodland: its history, vegetation and uses in England. London.

Shackley, M. (1981). Environmental archaeology. London.

Shapiro, J. (1958). The core-freezer - a new sampler for lake sediments. Ecology **39**, 758.

Smith, A. G. (1960). The Atlantic-Sub-Boreal transition. Proceedings of the Linnean Society of London **172**, 38-49.

Smith, K. M. (1972). A textbook of plant virus diseases. (3rd edn.) London.

Stewart, E. B. and Robertson, W. (1968). Fungus spores from prehistoric potsherds. Mycologia **60**, 701-4.

Swingle, R. U. and Bretz, Z. W. (1950). Zonate canker, a virus disease of American elm. Phytopathology **40**, 1018-22.

ten Hove, H. A. (1968). The Ulmus fall at the transition Atlanticum-Subboreal in pollen diagrams. Palaeogeography, Palaeoclimatology, Palaeoecology **5**, 359-69.

Unsworth, B. A., Cross, T., Seaward, M. R. D. and Sims, R. E. (1977). The longevity of thermactinomycete endospores in natural substrates. Journal of Applied Bacteriology **42**, 45-52.

van Geel, B. (1976). A palaeoecological study of Holocene peat bog sections, based on the analysis of pollen, spores, and macro- and microscopic remains of fungi, algae, cormophytes and animals. Ph.D. thesis, University of Amsterdam.

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**Tested, rapid techniques for clearing botanical specimens
for the study of waterlogged archaeological plant remains**

Philippa Tomlinson *

Introduction

Rapid and simple, but effective, techniques were required for the study of a wide range of archaeological specimens and comparative modern reference plant material. Many methods for clearing plant parts are described in the literature (O'Brien and McCully 1981 is most useful) and these range from very gentle techniques to very drastic ones. 80% Lactic acid (Kiger 1971) was found to be the best clearing agent for gentle purposes such as dealing with very delicate fragments of waterlogged leaves and flowers and grass stems. It has also been found to be the best mounting medium for all types of fragment. Jeffrey's solution, which is made up with 10% chromic acid and 10% nitric acid (Kiger 1971), may appear to be rather a fierce medium, but can produce an effect similar to rotting in waterlogged soil for hundreds of years on tough modern plant fragments such as legume pods, fibrous stems and evergreen leaves. The epidermal features can be revealed in a very short time. The main problem is an overbleaching effect where the cell walls become extremely pale if the fragments are left in Jeffrey's solution for too long (staining may be helpful here). Autoclaving in lactic acid (O'Brien and von Teichman 1974) may also be a useful technique for moderately soft specimens, particularly fresh material. For tough, fresh material, soaking in sodium hypochlorite and scraping with a razor can produce a very clean epidermis in grasses, sedges and the like. (Clearing some archaeological specimens of putative horsehair was most successful when the thin cross sections were left to soak in lactic acid for at least 24 hours.)

Methods

Table 12 shows which methods have been found to be the best to use for different types of specimens. As necessary:

(a) Cut the plant specimens to the required size, if necessary separating the parts into different tubes, and label. Boil in a water bath with industrial methylated spirits to remove air and/or chlorophyll, wash and drain.

(b) Either boil the material in Jeffrey's solution in a water bath or leave at room temperature for 1-4 days. Boiling requires only 5-30 minutes depending on the nature of the material. Care should be taken that the fragments are not left in the solution for too long - watch the colour change (brown - green - blue) to see how long is needed. If the

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material is very full of tannins and resins change the solution, but do not change after it has ceased to become discoloured.

(c) Place in lactic acid to finish the clearing process (leave to soak if necessary), scrape with a scalpel to remove unwanted tissue, and mount in fresh lactic acid. Semi-permanent mounts can be made by removing the excess lactic acid with a tissue soaked in meths and ringing the coverslip with nail varnish or a solution of poly-vinyl acetate in acetone, adding further coats when dry. These mounts last at least a year.

(d) Soak fresh fragments, cut to size, in sodium hypochlorite on a glass slide, scrape using a sharp razor blade, holding the fragment down with a small cork.

(e) Autoclaving in lactic acid is a rapid technique and an ordinary pressure cooker can be used. Place the fragments in Macartney bottles in the autoclave. (Not tested by the author but could be a useful method for difficult specimens.)

(f) Mounting in lactophenol is essential for seeing the silica bodies in grasses and *Carex* species; semi-permanent mounts can be made as above.

Results

The photographs (Figures 14-15) show the characteristic epidermal cells of the pods of *Vicia faba* L. The modern reference material is 'broad bean' whereas the archaeological specimens from early medieval deposits at 16-22 Coppergate, York, are more likely the field bean (or horse bean) which is a smaller variety of the same species. The fossil material was cleared and mounted in lactic acid, the modern herbarium specimens were treated with Jeffrey's solution and mounted in lactic acid. Note the arrangement of cells around the stomata and the peculiarly shaped trichomes (hairs). When the trichomes fall off their attachment cells (cicatrices) remain visible.

I should be grateful to hear from other workers of their 'pet' methods - equally, I should be interested in reports on how effective the methods outlined here have been for those who may like to try them.

References

Clark J. (1960.) Preparation of leaf epidermis for topographic study. Stain Technology 35, 35-9.

Kiger R. W. (1971.) Epidermal and cuticular mounts of plant material obtained by maceration. Stain Technology 46, 71-5.

O'Brien T. P. and McCully M. E. (1981). The study of plant structure. Principles and selected methods. Termercarphi Ltd. Melbourne, Australia.

O'Brien T. P. and von Teichman I. (1974). Autoclaving as an aid in the clearing of plant tissue. Stain Technology 49, 175-6.

Table 12

	Class of material						
	ARCHAEO- LOGICAL		FRESH MODERN			HERBARIUM	
	A	B	C	D	E	F	G
Boil in INS (a)	-	-	*	*	-	*	*
Jeffrey's solution (b)	*	-	*	-	-	*	-
Sodium hypochlorite soak + scrape (d)	-	-	-	-	*	-	-
Lactic acid to clear cold (c) (or autoclave - (e))	-	*	-	*	-	-	*
Lactic acid mount (c)	*	*	*	*	*	*	*
Lactophenol mount - grasses + sedges (f)	-	(*)	-	-	(*)	-	(*)

Table 12. Methods for clearing and mounting modern and archaeological plant material. * indicates cases where treatment as indicated is suitable ((*) works less well). The letters in parentheses refer to paragraphs in the methods section. Classes of material: A, C & F - THICK/TOUGH; B - SOFT/FRAGILE; D, G - FRAGILE/THIN; E - MEDIUM TOUGH/THIN

A further note on archaeological fly puparia

John Phipps *

In my outline (1983) I indicated that puparia of Muscina have the posterior spiracular slits bowed and radial, i.e. they are coded as c1 (Figure 1b). The spiracular plates are separated by a distance about equal to their diameters, 0.2 mm. From time to time I have found archaeological puparia which I interpreted as bowed and peripheral, no examples of which are listed in my account. According to Skidmore (1979) Atherigona laevigata Loew has such slits but does not occur in Europe. I was therefore inclined to regard these slits as aberrant Muscina sp. Such specimens have been noted from 16-22 Coppergate, York (9th and 10th century AD deposits); Walmgate, York (9th-10th centuries AD) and Carlisle (late 1st century AD).

Recently I have found more of these and in some at least the slits have an indication of a hook at one end, so that they may be regarded as sinuate and become b3, characteristic of Haematobosca stimulans (Meigen) and further agree with this species in other characters mentioned by Skidmore (1979); e.g. the spiracular plates are small (0.12 mm) and are separated by about 3 times their diameter (Figure 17), the anal sclerite is the right shape (though not very different from that of Muscina) and there is a number of short spines along its posterior margin. Muscina also has a row of spines, but it is less complete and nearer to the anal sclerite. In view of the difficulty in seeing the sinuate nature of the slits in many of these puparia, especially those in which they are thick (Figure 18), I am inclined to regard as H. stimulans all those puparia in which the slits are disposed peripherally and the plates are small and widely separated. These observations and uncertainties only emphasize the need for large collections of reference material to establish the range of variation.

It is particularly useful to be able to recognise H. stimulans as its larvae are confined to cow dung; in fact the female oviposits only in fresh cowpats. The larvae pupate in soil, near the pat. This habit is characteristic also of other species of Haematobosca, so that one can draw similar conclusions from their occurrence even if specific determination seems doubtful.

I have also recently recognised some puparia as those of the anthomyiid Paragla radicum (L.). Material has been found at the two York sites listed above, as well as at The Bedern, York (3rd/4th century AD) and Carlisle (pre-Roman or late 1st century AD). The family Anthomyiidae is closely related to the Muscidae and the two are often regarded as a single family. The puparia of some anthomyiids, including that of P. radicum, have been described by Stork (1936), and my specimens agree so well with her description that I am fairly convinced of this determination. The puparium is shown in Figure 19. The posterior

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spiracular plates are fairly small and separated by a distance about equal to their diameter. The spiracular slits are straight and radial, i.e. would be classed as a(s)3. The posterior end bears 7 pairs of papillae, one of which is small and placed below the spiracles. The anal sclerite is elongated and in the majority of specimens has fallen out. The dorsal surface of the last two segments is thickly covered with spines.

The larvae of many Anthomyiidae live in the roots of plants, especially Cruciferae. Those of P. radicum have been recorded from such roots but also from decomposing material including dung. Its occurrence is thus less informative than might be hoped.

I am grateful to Alan Robertson for drawing the figures.

References

Phipps, J. (1983). Looking at puparia. Circus 1(1), 13-29.

Skidmore, P. (1973). On the immature stages of Palaearctic Muscidae with particular reference to taxonomic and biological aspects. M.Phil thesis, University of York.

Stork, M. B. (1936). A contribution to the knowledge of the puparia of Anthomyiidae. Tijdschrift voor Entomologie 79, 96-167.

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Figure 16. Posterior spiracular plates and anal sclerite of Muscina sp.
Scale 0.2 mm.



Figure 17. Posterior spiracular plates and anal sclerite of
Haematobosca stimulans (Rg.).



Figure 18. Posterior spiracular plate of many Haematobosca stimulans.

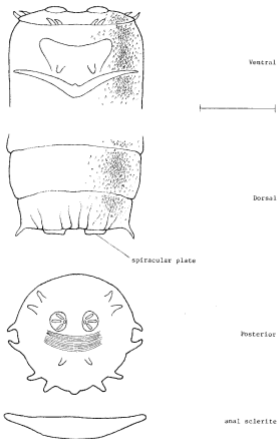


Figure 19. Posterior end of puparium of *Pirene radicum* (L.) in dorsal, ventral and posterior views. Scale 1 mm.

The Inside Back Page

Sounds of jubilation have rent the sleepy air of East Anglia lately, signalling to startled coots and bitterns yet another great breakthrough at the University of Lowestoft. For many years now, academics at the University have felt that too much of their time was expended on labour-intensive technical jobs, and have longed for greater automation to free their cerebral capacities for higher things. Step forward, then, William Freiborg-Prentiss, the hero of our tale.

Known to his colleagues as 'Old Bill', not so much for his name as for the nature of his Senior Common Room account, Dr. Freiborg-Prentiss operates from the University's Department of Improbable Computer Applications. His first target was to reduce the hours which bone specialists spend sorting, identifying, recording and tabulating their grubby raw material. The solution was to boldly interface a three-dimensional image analyser with the famous IPAGX computer, this latter device carrying a complex bone recognition and accounting program called USSER. After much trial and error, the system has been perfected to the point where an industrial robot (or graduate technician) passes bones through the image analyser, the IPAGX central processor considers what it has been shown, and a high-speed printer rattles off a complete set of data tables and an analytical report. Meanwhile, the highly-trained specialist can be doing ... well ... something else. But this is not all!

An outbreak of innovative peripheral engineering has permitted the image analyser to be interfaced with a variety of microscopes so as to allow digitised and chip-efficient recording of very small things. Even now, the paracybernetic Old Bill is working on seed and pollen recognition programs, respectively known as MACRO and PALLY. The day of the wholly-computer-generated-pollen-diagram looms very near... Perhaps the longer term holds a promise of dedicated palynological computers processing etiolated cylinders of peat and sharing their results directly with each other through a complex network. Never again will those tiny, spiky diagrams creep across the screen of a conference room. Instead, a succinct summary of the results could be delivered, accompanied by pleasant transparencies of sylvan countryside or palm-fringed beaches.

Let the palynologists should feel that this penultimate page is having rather too much of a go at them, it should be said that USSER will have some interesting fringe benefits. Bone specialists tend to be decidedly unclean types, a fact which they blame on the subjects of their study. They would, wouldn't they? Well, during a proving trial of USSER on the IPAGX, research technician Mahalonobis T. Firefly recorded over 2000 specimens during a six-hour period, and remained sufficiently clean to be tolerated in the staff tea-room, providing the windows remained open. The implication is clear: with Royalty maintaining an almost improper interest in archaeology, institutions will be able to face the possibility of an impromptu Ducal walkabout without fears about the cleanliness of staff. Indeed, one of the events which stimulated Freiborg-Prentiss to his beneficial works was the unfortunate whitewashing of the slightly sordid Dr. Myfanwy Crepuscular one foggy day when a visiting Inspector of Ancient Kelics was mistaken for a Royal Personage.

Thanks to Old Bill and his inspired labours, academics in archaeological science can look forward to a clean, restful, largely indolent future consonant with their place in the cosmos - Burhinus