INTERPRETATION OF STRATIGRAPHY:
A REVIEW OF THE ART

Proceedings of a Conference held on 18th June, 1992
at the City of Lincoln Archaeology Unit

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Interpretation of Stratigraphy: a Review of the Art

Proceedings of a Conference held on Thursday 18th June 1992 at City of Lincoln Archaeology Unit, The Lawn, Lincoln

Compiled by Kate Steane

with Jen Mann and Helen Palmer-Brown

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Foreword

These proceedings represent part of a continuing process in the attempt to understand stratigraphy. The origins of this conference echo all those who have worked with stratigraphic sequences including Edward Harris and Michael Schiffer, but I also had personal reasons for initiating this forum for open debate in Lincoln which I shall attempt to explain.

As one of the post-exavation field officers with the City of Lincoln Archaeology Unit, I work on the stratigraphy of backlog sites. Strong emphasis is laid here on the necessity of integration of stratigraphy with pottery and other finds and therefore I have attended several pottery and small finds meetings (MPRG, SEMPER, SFRG). The lively debate engendered by the meeting of these specialists is always impressive, both during formal discussion time as well as in the pub! After one such meeting it occurred to me that I had never presented or discussed my own work in open debate. I knew Tim Williams (monitor for the Lincoln post-exavation project) had a strong interest in the stratigraphic issues and discussed with him the possibility of holding a small informal meeting. He suggested several people who might like to be involved; these I approached at the IFA conference (April 1992). Here Jez Reeve drew my attention to ‘Arch-Form’, a group which had been active a couple of years ago but was now defunct. A call for papers was sent to those known to be interested in stratigraphy (apologies to anyone who did not hear). From being a small group, the number of people interested grew to over 120 and the venue had to be moved to a larger hall. I found it a very exciting experience being surrounded by people who were all involved in similar work. It was also very challenging as I found we all had different perceptions and expectations of the stratigraphy. The day taught me a lot and I was also relieved to have some of my unvocalised thoughts expressed by others. Since then I have found some of the ideas presented very useful for my own work with the Lincoln backlog.

The papers in this volume are presented in the order in which they were given; the texts are largely unedited as I felt that the papers should be published much as they were sent to me (which explains the different methods of referencing).

Kate Steane

Acknowledgements

The conference held on June 18th was largely financed by the City of Lincoln Archaeology Unit in an endeavour to support open debate in stratigraphy.

Many thanks to those who chaired the sessions: Ian George from Lincolnshire Sites and Monuments, Lucy Bown from the City of Lincoln Unit and Tim Williams from English Heritage. It was great to hear such a variety of speakers. The number of people who attended was very cheering - it seems we all need this forum for discussion.

Thanks to Sven Schutte who flew in from Cologne for the day and gave an international flavour to the proceedings.

Helen Palmer-Brown undertook all the desk-top publishing. Jen Mann painstakingly sorted out the grammar and punctuation of the proceedings. Alan Vince played a part in advertising the day (although he could not be present) by spreading the word not only in Britain, but also abroad while attending a conference at Ribe in Denmark. Thanks also are due to Jane Cowgill, Lisa Donel, Mick Jones, Paul Miles and the rest of the Unit.
Future Days

John Barber has planned the next meeting in Edinburgh for 1992

25th November, 1992

Contact John Barber, AOC Scotland Ltd, The Schoolhouse, 4 Lockend Rd, Leith, Scotland, tel 031 5554425

Andrew Westman and Dick Malt from MOLAS together with Peter Clark from Canterbury are setting up the third day of stratigraphy at the Museum of London with a more international emphasis

17th June 1993

Contact Andrew Westman, MOLAS, The Museum of London, 1 London Wall, EC2Y 5HN, tel 071 972 9111, fax 071 972 9112

Can your organisation provide a room for a future meeting? Contact Kate Steane, CLAU, The Lawn Union Rd, Lincoln, LN1 3BL tel 0522 545326
The Use of Soil Micromorphology for Investigating Site Formation Processes

by Hal Dalwood

Introduction
This paper concerns the impact of the application of modern techniques of soil micromorphology to the interpretation of stratified archaeological deposits. The focus is the use of the technique in the city of Worcester. The method has proved extremely useful in checking and correcting interpretation of archaeological soils made in the field, especially of extensive deposits interpreted as ‘buried soils’ or ‘dark earth’. Field interpretations were based on data observable in the field, such as the density, fragmentation and abrasion of artefacts contained in soils. Such interpretations have obvious weaknesses when they are not based on empirical studies or experimental data.

There is a widespread but largely undocumented problem in interpreting extensive and uniform archaeological deposits. The terminology used in describing and interpreting such deposits is limited; they may be called ‘layers’, ‘dumps’ or ‘soil accumulations’. The definition of these deposit categories is inexact, generally being based on the morphology of the deposit (ie its extent or thickness) or on the analysis of the artefacts and ecofacts contained within the soil, rather than on an analysis of the soil structure itself. The analysis of artefacts and ecofacts is often inconclusive, which may lead to the assumption that these deposits contain no definitive information. In some instances, such deposits have been excavated without being fully and carefully recorded, in order to reach the more comprehensible archaeological deposits below, thus losing important information. Anecdotal evidence suggests that this still occurs.

Elsewhere extensive layers have been the basis for interpretations of the development of a settlement as a whole. Dark earth deposits have been interpreted as natural soils and therefore evidence of prolonged periods of settlement abandonment. It can be argued that it is not possible to make direct inferences about the origin of such soils from the presence or absence of artefacts alone, without a firm basis in the analysis of soil micromorphology.

There is a small but growing literature on the application of soil micromorphology in Britain. The major reference for the range of current applications is Courty et al (1989). The present paper is derived from the writer’s work on projects in Worcester with Dr Richard Macphail (Macphail 1992; forthcoming a). A summary of the results of recent work in Worcester and other towns is in press (Macphail forthcoming b), and the utility of the technique in investigating the origin of dark earth deposits in London has also been described (Yule 1990). The application of the technique to different types of sites in Scotland has been reviewed in a recent paper (Davidson et al 1992).

Soil micromorphological analysis in Worcester
Work in Worcester since 1988 has provided examples of the type of interpretation problem that can be addressed through analysis of soil micromorphology. The technique has been formally applied to two excavations, at Deansway and Farrier Street, although observations of archaeological sediments have been made at a number of other sites by soil scientists. Large-scale excavations were carried out at Deansway between 1988 and 1989, in advance of redevelopment as a shopping centre (Fig 1; Dalwood et al forthcoming). Soil micromorphology was used widely to address a number of questions (Macphail forthcoming b), mainly regarding the nature of the soil predating Roman occupation deposits, and the origin and nature of the dark earth that separated the Roman and Late Saxon deposits over a wide area of the excavation.

Pre-Roman soils
A thin soil deposit was truncated by the earliest Roman features on the site (late 1st century). This deposit contained no artefactual material. The soil micromorphology indicated that this was the remnant of a cultivated soil, predating evidence of intensive occupation. It is likely that this soil represents prehistoric tillage. A similar interpretation of this soil was made in the field, largely based on an absence of data. No plough marks were found, and the soil produced no environmental evidence. There is evidence of Iron Age settlement at Worcester, and it could be anticipated that the site was agricultural land in this period, when the gravel terraces of the River Severn are known to have been intensively occupied.
In this instance the soil micromorphology helped to confirm an archaeological hypothesis, and thus the interpretation of settlement origins is based on a wider range of data.

**Early Roman soils**

Early Roman soils were shown to contain evidence of agricultural activity in the form of burned organic material. This evidence was corroborated by environmental evidence of crop processing on the site.

**Late Roman soils**

There were extensive spreads of homogeneous soil, described in the field as ‘dark earth’, which overlay earlier Roman deposits. These soils contained large quantities of fragmented Roman pottery and other artefacts. Analysis of the soil micromorphology indicated that these soils originated from the penning of herbivores, and large quantities of silty soil and other materials brought in from the nearby river. Mixed with these ingredients were human and dog coprolites. These deposits in some places underlay Roman trackways, as well as overlying them. The stabling of animals was not detected from other analyses, although evidence of rubbish deposition (middens) was found in the form of pottery and animal bone.

This evidence for stabling of stock can be used to support the interpretation of some of the archaeological features, such as small post-built structures that may have functioned as animal pens. It is thought that cattle were grazed at meadows on either side of the River Severn, fordable at Worcester. Furthermore, this evidence may be understood within the context of the interpretation of Worcester as a small town in the Roman period, acting as the market centre for the local region. The soil micromorphological data is thus important in constructing models of the economic role of the Roman town.

**The reworked dark earth**

The soil deposits produced by stabling were reworked by enchytraeids (wire worms) and earthworms to produce a grassland soil. Gradual accumulations of organic material led to the formation of a dark earth 250-300 mm thick. The distinctive dark colour of the soil was due to the biological reworking of the later Roman deposits. Soil micromorphology indicated no direct human activity in this formation process - except probably pastoralism. The interpretation of this phase of site formation is particularly clear because the dark earth was sealed by a clay, earth and turf rampart that formed part of the defences of the late Saxon burh.

Other archaeological evidence datable to the period between the 4th century and the late 9th century is limited. A single archaeomagnetic date, AD 717-792, was obtained from a hearth and a single sceat was found dating to c AD 715. This evidence postdates the foundation of the See of Worcester c AD 680. The only evidence of possible earlier activity was a small sherd of a samian vessel bearing a runic inscription, but this is a problematic inscription.

Soil micromorphology therefore provides some basis for understanding the development of Worcestershire between the fourth and the ninth centuries. There is good reason for believing in the continuity of sub-Roman and post-Roman Christian communities in Worcester, based almost entirely on documentary rather than archaeological evidence (Bassett 1989). It is likely that the hypothesised post-Roman Christian community was contained within the small area of the later Roman defences. The evidence of continuity of pasture from the late Roman period to the ninth century thus takes on a new significance: rather than evidence of abandonment, dark earth at Worcester is interpreted as evidence of continued use as farmland.

Other excavated evidence suggests that Roman boundaries were maintained through the post-Roman period, probably in the form of hedges, that eventually were fossilised as boundaries and building lines in the late Saxon and medieval town.

The occasional evidence dating to the eighth century can be understood as the gradual spread of occupation areas beyond the late Roman defences after the creation of the see c AD 680. The bishop of Worcester had a haga or enclosure, recently located west of the Deansway excavations adjacent to the river (Baker et al 1992). In this instance soil micromorphology can be integrated with historical interpretations of the settlement development.

**The transformation of dark earth into urban soils**

The dark earth was sealed by the construction of the late Saxon burh defences, dated to c AD 890. Here, soil micromorphology showed that the clay and turf ramparts were erected on a green-field site, comprising the pasture fields outlying the occupied area. In other areas the dark earth was transformed by intensive human activity. Much organic material was trampled into the soil, including midden spreads, wood and grass ash, charcoal, bone, human and dog coprolites. There was also evidence of building material, in the form of alluvial silt probably used for floors. Fen peat was used for fuel or for walls.

The onset of human activity post-dating the construction of the burh rampart was dramatically demonstrated by soil micromorphology. The modification of the soil was also demonstrated by other kinds of data, with many artefacts trampled into the dark earth in areas not protected by the burh.
Fig 1: Location of sites 1-4 at Deansway excavation, Worcester (drawing by L Templeton, HWCC Archaeological Services)
rampart. Buildings were constructed and cess pits dug, truncating the soil layer. In this instance soil micromorphology added detail to an archaeological interpretation based on other kinds of data.

Conclusions
In Worcester the main application of soil micromorphology has been to the enigmatic dark earth deposits. The results of analysis demonstrate the fundamental importance of soil micromorphology in understanding these deposits, which are found on many urban sites. The human and natural agencies at work in the formation of dark earth deposits are not necessary the same at any two sites, even within the same town (Macphail 1992: forthcoming b). The apparent similarity is due to the biological reworking of anthropogenic soils containing charcoal, but this often masks disparate depositional histories.

A dark earth type of deposit once seen is immediately recognisable in the field. This has perhaps fostered a blasé attitude to dark earth that should be rejected. It would seem necessary to carry out soil micromorphological work wherever this type of deposit is encountered, in order to determine both the human and biological factors involved in its formation. It is also important to use the technique at different locations within a settlement, because it may be possible to detect different types of human activity concealed within visually similar deposits.

Even from small excavations useful results can be obtained. It is perhaps a more reliable measure of the broad processes of site formation than some other types of data from small excavations. The technique has great potential in the assessment of the archaeology of small towns and other sites that are not well known archaeologically. Soil micromorphology has proved valuable at numerous sites in determining the nature of the “natural soil” over lain or truncated by archaeological deposits. It has also been important in determining the origin of both localised and continuous deposits on urban sites, and can be used to determine whether these soils are formed by dumping or natural accretion.

Soil micromorphology’s strength lies in its ability to detect human and natural processes that contribute to site formation, but do not leave evidence that can be detected through other analytical procedures currently in use. The technique is capable of testing and confirming interpretations of soils and sediments made in the field, since it is firmly based in experimental data. It is likely that in the future archaeologists will continue to make informed interpretations of stratified soils and other deposits based on correlations between, for example, characteristics of artefact assemblages and depositional activity. These interpretations should be based on the evidence of soil micromorphology, or there is a danger that interpretations will only be based on the archaeologists’ own expectations.

Acknowledgements
The writer would like to thank Richard Macphail for his enthusiastic input to current fieldwork in Worcester, as well as Clare de Rouffignac and Malcolm Cooper for making many useful comments on earlier drafts of this paper. The figure was drawn by Laura Templeton.

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Discussion Session: Feedback Mechanisms from Post-Excavation to Excavation

by Ellen McAdam

Archaeology as a discipline has a uniquely bad publication record, and the last 20 years in Britain have witnessed the growth of a large backlog of unpublished rescue excavations. A number of organisations have addressed this problem by setting up separate fieldwork and post-excavation departments. Unfortunately, this division has tended to exacerbate some of the problems which contributed to the creation of the backlog in the first place, creating an artificial distinction between excavation and post-excavation and interrupting what should as far as possible be a seamless process. It is true that fieldwork and post-excavation call upon different sets of skills, and that employees will have individual strengths and weaknesses, but it is important for the profession as a whole that all its practitioners should have as full as possible an understanding and knowledge of all the processes involved so that overall aims and standards are not warped by narrow specialisation. It was in the hope of soliciting solutions to these problems that the subject of feedback mechanisms was raised for discussion.

As a number of contributors to the discussion agreed, the essential problem is that site records do not always answer the questions which will be asked during post-excavation analysis. A number of units have addressed this by appointing a Records Officer with extensive experience of both fieldwork and post-excavation projects. It is the Records Officer’s job to visit sites in progress and check records in the field, not only for completeness and consistency but also with the requirements of post-excavation in mind. The Records Officer may also be responsible for ordering and curating the record at all stages. One delegate made a connection between the checking of records during the progress of excavation and the concept of units of interpretation, a tier of recording above units of record (i.e. context records) which encouraged the excavator to make explicit his running interpretation of the site.

A number of speakers emphasised the need for structured training programmes which would provide practical experience of all aspects of archaeology. This need was felt to be particularly acute among more junior site staff, who may be responsible for on-site recording but are rarely involved to any significant extent in the interpretation of the records they have created. The problem is, of course, not confined to junior staff. It is clear to anyone who has worked on backlog sites that in the past it has been possible for archaeologists to accumulate extensive field experience and reach the stage of excavating major sites without having had any substantial exposure to the processes involved in post-excavation.

In his notes on this session, Martin Foreman of Humberside has outlined the benefits to be obtained from the transfer of staff from excavation to post-excavation duties, following a project through from the field to the final report: he also makes the point that arranging this sort of transfer calls for advanced administrative and financial skills and a high priority for staff development, including junior employees. For many organisations the present structure of archaeological funding in Britain, which entails both delays between project phases and the necessity of finding continuous funded work for every employee, makes programming the movement even of senior staff from excavation to post-excavation a complex task; the potential for the creation of individual backlogs is almost built into the system.

It is clear from this discussion session that there is widespread acknowledgement of the need for better communications between those involved in the fieldwork and post-excavation phases of archaeological projects. Practical vocational training in archaeology is also recognised as a priority, but many units would face difficulty in designing, implementing and funding such programmes. This is an area in which the units, the Universities, the IFA and the national bodies could and should co-operate.
Preconceptions when Interpreting Stratigraphy

by Tony Barham

Apologies have been given for the failure of this paper to meet the publication deadline (due to Tony Barham's involvement with the discovery of the Bronze Age boat at Dover).

Tony has agreed to bring copies of his paper to the meeting at Edinburgh on the 25th November 1992; otherwise a copy can be obtained from him at the following address:

Tony Barham
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31-34 Gordon Square
London
Assessment versus Analysis

by Dick Malt & Andrew Westman

Introduction
In our session we led a general discussion instead of presenting well-prepared papers and, in taking the present welcome opportunity to write what we wanted to say, we have had the benefit of that discussion, for which we are very grateful. We should of course add that the views expressed here are our own and are not to be regarded as those of the Museum of London Archaeology Service (MOLAS) nor necessarily of anyone else.

The title ‘Assessment versus Analysis’ refers to an important distinction made in English Heritage’s document Management of Archaeological Projects, 2nd edition (universally abbreviated to MAP2), which puts forward a model for managing individual archaeological projects. This model identifies five ideal phases in the life of a project: preliminary assessment and the explicit formulation of archaeological aims, the ‘research design’, essential to every project (Phase 1); fieldwork and the creation of a site archive of records, finds and environmental materials (Phase 2); ‘assessment of potential for analysis’ of the contents of a site archive, presented in an ‘assessment report’ (Phase 3); analysis, research and preparation of reports for publication (Phase 4); dissemination of reports (Phase 5). The model recommends applying a managerial cycle of explicitly documented review, proposal and decision whenever a project comes to the end of a phase, although not every project is expected to go through all five phases. We focussed our discussion on the distinction between ‘assessment’ and ‘analysis’ entailed by the procedural separation of Phases 3 and 4, and the implications of this distinction for post-excavation work on stratigraphic records.

We wanted to do this because of our recent experience of managing projects according to MAP2 as well as our previous post-excavation experience. An early version of English Heritage’s project management model had circulated in 1989. At roughly the time MAP2 was issued in 1991, by coincidence, the Museum of London’s two former archaeological departments were being merged into a single unit, MOLAS. The new unit firmly embraced project management as the businesslike way of the future and, as two among more than a dozen project managers in MOLAS, we are currently managing both developer and English Heritage-funded projects mainly - for historical and practical reasons - in the post-excavation stages of their life.

We would like to say at once that the MAP2 model for managing archaeological projects is welcome and timely: who could argue with its emphasis on competence and efficiency? Promulgation of this model, incidentally, shouldn’t be taken to imply that previously archaeologists were unusually deficient in management skills. We also think it important to note what MAP2 does not make recommendations about: the human factor in work - vital to good and effective management; professional techniques and methods - equally vital of course; and, more difficult to express, exactly what constitutes archaeological value for effort and money spent - will we all recognise the same qualities when we see them? Moreover, very properly, no one claims that MAP2 is the last word on the subject.

Interpreting and implementing MAP2
MOLAS took up the invitation in the preface to MAP2, adapting this model of project management to its own circumstances. A MOLAS working party was set up to interpret and implement the MAP2 model, producing an initial document just in time for us to be able to brandish it at Lincoln. This first MOLAS document deals more thoroughly than was possible in MAP2 with the contents of a site archive and their post-excavation assessment, further details being left to supporting technical specifications and manuals such as the Museum of London’s Archaeological Site Manual (1990) and other documents dealing with interpreting stratigraphic records as well as collecting and processing finds and environmental materials.

The MOLAS specifications for a site archive endorse what was the previous best practice, amounting to everything one should have completed by the end of fieldwork. Stratigraphic and other site records would therefore include a Site Plan - locating areas of excavation, sections and so on internally, and connecting these to the external world, to a uniform degree of accuracy - and a fully checked and completed stratigraphic matrix. Finds would not have to be spot-dated at this stage. Environmental samples should already have been chosen on site from well-sealed contexts, likely to be closely dated and to yield useful information. A summary of results and an index catalogue the site archive, making it accessible.

Although not new these specifications for a site archive are highly important, for no amount of sophisticated post-excavation work - or management -
can possibly compensate for the absence of an explicit research design, bad fieldwork or poor recording.

Post-excavation ‘debriefing’
According to the MOLAS working party the next stages in the life of a project are a ‘debriefing’, followed by whatever work is necessary to produce a post-excavation assessment report, all equivalent to MAP2’s Phase 3.

Presumably managers (or field officers) and site supervisors (or directors) have always thought they knew what they were doing and why. A debriefing directly after fieldwork is what the former archaeological departments did anyway, whether by that name or not, in order to review the results of fieldwork, to decide what to do next and to plan ahead. This is managerial review, proposal and decision, in MAP2’s terms, and in a MOLAS-style debriefing a project manager, site supervisor and specialists do this quickly, ideally with a project’s research design on the table in front of them. Although the research design governs what happens next it can of course be changed, provided this is done in an open, reasoned and explicit way. Work in post-excavation, like work at any time, has in any case to be defined, estimated, costed and timetabled. These basic managerial tasks are nothing new, the Museum’s former departments for example having done them - with more or less efficiency - for more than three hundred projects in the four years 1988-91.

Most projects at present are only evaluations: limited fieldwork for the purpose of making an archaeological evaluation as part of the local authority planning process. In these cases the purpose of a debriefing is mainly to plan the work of compiling the requisite evaluation report, the decision to do so having already been taken, and post-excavation work therefore doesn’t correspond exactly to any of MAP2’s Phases 3, 4 or 5. In different circumstances, perhaps when a project is resurrected and more fieldwork takes place some time after an initial evaluation, a debriefing would correspond in part at least to an ‘assessment of potential for analysis’.

Post-excavation ‘assessment’
At this rate ‘assessment’ must seem like a word in danger of severe abuse, so it’s worth saying that we take it to mean primarily an intellectual act of judgement that one may perform at any time. In this sense one could begin much of an ‘assessment of potential’ while fieldwork was progressing: for example, making an ‘assessment of potential for publication’, in a preliminary way at least, by deciding what to photograph on site. MAP2 employs the term ‘assessment’ to mean both an act - a judgement of this kind presented in the form of an assessment report - and a process, leading up to this judgement.

Assessment as an act of judgement usually depends on some prior mental activity that we would call analysis, although we don’t particularly care what names are given to human cerebrations so long as they don’t mislead, cause mistakes and waste effort. It is clear to us that the MAP2 model’s strict separation of analysis from assessment is all about procedure rather than semantics: ensuring that a considered judgement is fairly made about the value of proposed post-excavation work before embarking on it, especially if it’s time-consuming and expensive. Value is defined as what will answer questions in the research design and should therefore be published.

No one would argue with any of this, as a matter of management. Most site archives would be assessed, in MAP2’s terms, individually - site by site - in order to make a judgement of their value: to decide what to research in more detail and to publish. The technical processes required to make such an assessment aren’t, however, as simple as the MAP2 model seems to assume. As a result the order and definition of Phases 3 and 4 in the MAP2 model are, we think, over-idealised and impractical. In this respect while the MAP2 model seems to us quite appropriate to post-excavation treatment of finds and environmental materials, which easily moves on to substantial local and regional research, it seems less appropriate to explicating and understanding the stratigraphic records of a particular site, a process which may be complicated in itself but may otherwise be limited in scope.

Phase 3 tasks
The MOLAS working party document specifies the tasks that must be accomplished - assuming that a site archive is complete and a debriefing has clarified a project’s aims - to enable a proper assessment report to be written. The stratigraphic sequence should be reliably phased and dated, finds identified and dated, environmental materials identified and sorted and relevant documentary sources identified.

These indispensable tasks differ in their possible complexity. At this initial stage, work on finds and environmental materials is relatively straightforward and easy to estimate - the basic task is one of identification, usually recognising things one already knows about; but work on stratigraphic records may well be complicated and much less tractable - even the simplest structural sequence has to be proved by drawing successive phase plans, which ought not to be a matter of recognising what was already thought to be there.

‘Assessing’ stratigraphic records
There is little one can seriously do with stratigraphic
records, after putting them in order in a site archive, except analyse them. These days professionally-made records aren’t chewed up notebooks, scruffy little-bit-of-everything plans and hundreds of apparently repetitive photographs, whose capacity to yield useful information cannot be assumed. Stratigraphic records, especially those based on single context plans systematically made and checked during area excavation, are designed to be capable of equally systematic and reasonably quick spatial and sequential analysis. Stratigraphic excavation from the top down and post-excavation analysis from the bottom up, or rather from the beginning of a sequence onwards, are two concomitant parts of a single system.

There is no ‘degree’ of stratigraphic analysis in this respect - it’s either done or it isn’t. If it isn’t done, one has no grounds for a reliable statement of what these records mean - whatever one says could have been said during fieldwork, or even before. And if it is done, there’s no point in starting with anything larger or more interpretative than the individual contexts that were recorded, nor in omitting any contexts.

A purely provisional interpretation of a structural sequence, perhaps defining large groups of contexts before their constituent subgroups (in MOLAS terminology), has inherent defects. A provisional interpretation may be little more than a glorified guess, acceptable for the purposes of a debriefing but not to be relied upon by finds or environmental specialists for the purpose of a proper assessment.

Specific dating information should be applied to dependable groupings of contexts, rather than to provisional groups that are liable to change if more analysis is done later. Hasty conclusions as to general periodisation of a sequence - separating prehistoric, Roman, sub-Roman, Saxon, medieval and post-medieval, however such periods are to be defined - are particularly to be avoided. In any case, no one should analyse the same contexts twice: firstly, quickly, to make one set of interpretations and secondly, more slowly and in more detail, to come up with what would almost always be a different set of interpretations.

Selection of data at this early stage is undesirable, but if unavoidable - on grounds of cost, say - it would be feasible to work from the beginning of a stratigraphic sequence of contexts onwards and stop half-way, or select whole areas of a site and ignore others: the effect of this would be like that of stratigraphic truncation. Any other method of selection at this stage, before analysis, would be counter-productive, if not senseless. One would not know what was omitted, nor know the implications of these omissions for what one had decided to retain, undermining whatever interpretation was made.

Apparently well-defined urban contexts lend themselves to straightforward structural analysis. It would be a mistake, however, to accept the stratigraphic unit of record, as defined on site, at face value. In fact, as other contributors to this conference recognised, contexts aren’t always what they seem to be or were believed to be when they were defined and recorded. In tandem with purely structural analysis there should be rather more critical morphological analysis, considering formation processes and phenomena not represented directly by contexts as defined during excavation. If contexts weren’t clearly defined in the first place - as often happens, and not only on more extensive and shallow rural sites - then a purely structural analysis isn’t so easy.

The basic method of work has always been primarily graphic, dealing with plans, sections and diagrams. One has a certain repertoire of questions for different kinds of evidence, but the analytical, interpretative and organisational essentials are pretty much the same. The products are sets of working and interpretative plans at suitable scales, sequence diagrams of contexts and their interpretative groups and subgroups as appropriate, a Land Use Diagram and a comprehensive index. The only reason for writing about the stratigraphic sequence at this stage would be, we think, if the interpretative plans and diagrams weren’t sufficiently self-explanatory: sometimes the way an interpretation was worked out is more important than the solution. If explanations can be verbal, less has to be written down, although the risks in this are obvious.

The MOLAS working party expected that this indispensable post-excavation work on stratigraphic records would be carefully phased with other work on finds and environmental materials - exchanging an initial scheme of phasing from ‘strat’ on one hand and spot dates from ‘finds’ on the other, for example - leading to well-considered interpretations and a definitive phasing to which an assessment report can refer.

As people - and units - gain more experience these analytical processes should be performed with more confidence, efficiency and economy. They could be streamlined, concentrating on areas of interest (briefly) and on areas of difficulty (even more briefly), but only up to a certain point. Many factors helping or hindering this work are external to actual method and technique.

**Types of record and types of site**

As mentioned already, we’ve always had to make estimates for post-excavation work, deciding what is to be done and how long it should take. We have refined our calculations in so far as we expect less work to be done in MAP2’s Phase 3 than was done previously to compile a ‘Level III archive report’, although the results should be of comparable utility.
One can consider ways of estimating the likely difficulty or lengthiness of stratigraphic analysis, combining an appreciation of the type of site and the state of the site archive.

A rural site with extensive but relatively shallow and uncomplicated stratification ought to be understood and phased with much less work than an urban site with complex and deep stratification, although in the former case specific dating information from finds might be necessary to establish any sort of sequence. The state of the stratigraphic records is another variable, reflecting what we could call the efficiency with which the original data were recorded. Again, at its simplest, a series of sections might be an efficient way of recording the stratification of the rural site but very inefficient for the urban site, assuming also adequate finds collection and environmental sampling.

Simple estimates for post-exavation work could be in three logical categories: easy, difficult, and the in-between. We should mention that a current English Heritage-funded project has developed this idea and is applying it to existing site archives, a backlog of unpublished or semi-published projects in London, to enable estimates to be made consistently for subsequent work on them.

The research archive

The MAP2 model seems very suited to a single site that stands alone as a project. This model obviously has to be adapted and clarified to fit the situation in which large urban units operate, where sites arise according to the dictates of modern development, few sites are archaeologically self-contained and everything produced in MAP2's Phases 3 and 4 forms a component part of a single cumulative multi-site multi-project research archive. The principles of the Frere and Cunliffe Reports, precursors of MAP2, were adapted to this situation so that all data from every site was processed to a certain level of interpretation and accessibility ('Level III'), and disseminated publication thereafter ('Level IV') could be more selective. The same principles ought to operate still, at least to the extent that site data is catalogued, digested and an informed decision may be made about what to publish and how.

Taking the MAP2 model further, the process of Phase 3, when complete, would mark the end of work on an individual site archive, and the ensuing Phase 3 assessment report would mark the site's integration into its geographical and historical surroundings - and a unit's publication policy. From this stage on projects would be redefined and possibly their ingredients recombined in research and publication projects.

Premature selection or partial treatment of data in MAP2's Phase 3 is, we suggest, worse than not
Stratigraphy after Harris: Some Questions
by Max Adams

"Why should it take an infinite amount of logic to figure out what one tiny piece of spacetime is going to do?" (Richard P. Feynman, The Character of Physical Law, 1967)

Introduction
Ed Harris wrote the last word on the subject of archaeological stratification. He defined archaeological terms the principle of superposition, and the circumstances in which it offers law-like confirmation of relative chronologies. He suggested guidelines for the denotation and analysis of stratigraphic relations, based on various systems in use in archaeology between the 1940's and 1960's, and his Matrix is enshrined in archaeological systems everywhere.

Stratigraphic analysis provides a mechanism for understanding the formation of archaeological deposits. It offers images of a chronological universe which, but for blurred edges and the odd equivocal relationship, allows the archaeologist to arrange entities in the order in which they were created. Indeed, so essential is this type of analysis now regarded, that it has become for many a simple routine.

Only, there is a problem with stratigraphy. In the same way that physicists and mathematicians have realised that the mechanical universe of Newton and Einstein may be a limited case of cosmic behaviour, Harris's stratigraphy turns out to be a limited case of archaeological formation. A symptom of this problem, as Feynman was to discover in theoretical physics twenty-five years ago, is that the closer you look at stratification, the more complex it becomes. Infinitely. Interfaces cannot be resolved, impacts are no longer diagnostic, there are so many processes operating at so many scales that the little box in the matrix with the number and the two-dimensional relations becomes a parody of itself.

The stratigraphic matrix
In fact, the stratigraphic matrix, as it is commonly used, reflects the multi-dimensional archaeological record in precisely the same way that the London Underground map reflects London. In other words, the matrix is an exercise in topology, that branch of geometry which deals with place and position, rather than quantity and measure. It is concerned with those properties of a figure which remain when given elements are removed. Just as the human brain readily deals with the idea that all stations which lie between Turnham Green and Blackfriars are not equidistant, and do not precisely align with each other in the real world, it is equally able to understand that the order in which one would pass from one station to the other on the District Line is not accurately reflected in the familiar diagram. The London Underground map is entirely inadequate for an understanding of drainage patterns, or the best way to drive across town. It selects an aspect of a system and reduces it to the bare minimum for it to remain understandable.

Archaeologists learn how to interpret matrices with the same critical scepticism. They know that the surfaces of fills in a ditch are unlikely to lie equidistant from one another, just as they know that three-dimensional aspects of archaeological sites cannot be displayed in two dimensions (although there are some archaeologists who persist in the belief that crossed lines on a matrix indicate either a mistake in constructing the matrix, or a mistake in excavating the site!). But they can exploit its reductive aspects to allow an understanding of critical phenomena.

The stratigraphic matrix combines two powerful analytical tools. The first of these is the denotation of concepts which the philosopher Frege (1848-1925) recognised as critical to logical analysis of any non-quantitative, but basically mathematical, information. In creating the stratigraphic matrix we denote two fundamentals of the archaeological record: discrete archaeological events are accorded individual identities, and the order in which they must have been created is denoted by a linear succession in which the earliest event is at the bottom, and the latest is at the top; it is therefore analogous to the logic of superposition.

The matrix itself does not do anything; it has no mathematical or logical function, and it cannot of itself generate archaeological inferences. Its power lies in its capability for displaying concepts, without which stratigraphic analysis is a mere numbers game. The analysis existed for many years before the matrix gave it life. In just such a manner in molecular biology, the idea of DNA only became a reality with the building of the model of the double helix, which reduces the massive complexity of genetic structure to a tangible and accessible model.

The second tool, rarely explicitly recognised, is the application of metaphysical inquiry to archaeologi-
The philosophical parent of topology, metaphysics, deals with problems of identity and change. In archaeology this allows us to examine the relationship between traits, the things that we perceive about archaeological entities, and attributes, the things which make an entity what it is (Adams 1991). In stratigraphic analysis metaphysics allows us to determine the difference between deposits and interfaces, to identify the discreetness of entities and the processes which have led to their formation. Since the artificial division between the subjective and the objective has become such an issue in archaeology, it would be unwise to dismiss metaphysical analysis; after all, the identification of change relies very much on the identification of no change, as it were.

Some problems
It is clear, though, that archaeologists are now concerned with a wider range of concepts than they were when the stratigraphic matrix was first developed during the 1970’s, as Harris himself recognised in the second edition of Principles. The processes of formation, recovery and inference are under closer scrutiny, and the role of the archaeologist in the creation of the record is an issue of great importance. Why, then, is there a reluctance to use the same powerful analytical tools which led to the development of the matrix, to examine with the same rigour the new questions that archaeologists ask of themselves and their data?

Part of the reason must be that there are difficult questions to confront which might at first appear to expose weaknesses in the accepted method of stratigraphic analysis. How do deposits and interfaces behave under different conditions: how fluid can relationships be between depositional entities? How are we to deal with the often fractal nature of processes and interfaces, with boundary effects and non-linear dynamics? We see structure in the debris of human behaviour, and we see correctly, but do we ignore the apparently small problems of fuzziness and uncertainty at our peril? To deduce the structure of an archaeological site simply from examination of its topological characteristics risks a tautological crisis, for in fact very often we are inferring the operation of processes in order to reconstruct them. All archaeological inferences must begin with the recognition of formation processes, but stratigraphic analysis often seems to skip a few inferential steps.

Let me illustrate the point. What is the box that we confidently put numbers in? Is it an event? Is it an archaeological fact? Is it an inference? What information does it carry, and, more importantly, does the reader receive the same information that the writer intends?

What of the lines between the boxes? Are they the passing of time? Do they represent succession, or physical position? Do they imply interaction between boxes?

So what does the matrix mean? What does my matrix mean? Answer: you only know if there is an explicit key to the code. Normally, we assume that we know the key, but there is every reason for the matrix to mean anything that its author wants, so long as the information is correctly received by the reader. The boxes and the lines may mean different things to different archaeologists, so long as those meanings are explicit, and relate to real concepts.

Solutions
It may seem that the solution is standardisation of display and rigorous implementation of ideas, but I think that is not the answer. The matrix is, after all, a tool, and the archaeologist should use it as he or she sees fit. The solution lies rather in extending the power of the tool that we have, to allow access on a variety of levels to much more than just a sequence of events.

We can, in fact, use the ideas behind the matrix to analyse archaeological data in a logical and explicit way, but in order to do so, we must use the matrix to view the archaeological process itself. One problem facing us is that by the time data are ensnared in a stratigraphic matrix all the important inferences have been made. We have to accept that topological denotation as a truth. However, the matrix denies us access to two essential processes of the archaeological operation. Firstly, the process by which equivalences in the ground are rationalised, and entities given identity, is lost. This means that the crucial phase, in which the diagnosticity of physical traits is determined, goes either unrecorded or is not available for analysis. If you want to know why archaeological decisions about entities were made in the field, you have to consult unwieldy text in a notebook or on context sheets, or you have to directly consult the excavator.

Secondly, inferring the archaeological processes which have led to the formation of deposits and interfaces is left to a textual analysis whose explicitness is often difficult to determine. This is odd, because using the tools which give the stratigraphic matrix so much analytical power would surely lend the same authority to assessments of the quality of data retrieval and inference generation.

Once the principles of archaeological stratigraphy were recognised as a canon in their own right, it was generally accepted that the recording of physical relationships observed in the site record was a superfluous operation, to be skipped in favour of the structural, take-it-or-leave-it matrix. That was an essential process of development, because it forced
archaeologists to recognise the power of the new tool, and it made the order of the past accessible. I think that this is now inadequate. As a tool in post-excavation analysis, and as a systematic record of the processes of retrieval, the physical, or trait matrix ought to be indispensable. It is a reflective concept, and should therefore image the following types of information: - type of entity, deposit, interface, lamination, spit, perceived quality, and conferred status. It should also reflect uncertainty and the 'subjective' elements of recording. It images the archaeologist at work in the field.

In this system all relationships ought to be denoted, because although not all may be relevant to the logical succession of events, each relationship has its own archaeological significance in terms of processes: there is potential for each entity to have interacted in the past with all those with which it is in contact; it is a dynamic past that we are confronting.

This raises a wider point, echoed in Feynman's words above. At the smaller scales on which we are increasingly concentrating, boundaries and horizons which cannot be resolved in infinite detail need to be recognised and confronted, or we are in danger of merely caricaturing the past. So with the physical matrix we are recognising the process of retrieval, with all its attendant problems.

I also believe that there is a powerful case for the use of a second intermediate form of denotation, which one might call the metaphysical, or attribute matrix, if this is not too offensive a term. This analysis might consider inferential aspects of the archaeological process: - enhancing and reducing cultural processes; enhancing and reducing environmental processes; operational processes; status of retrieved material; types of event denoted. This set of concepts images the archaeologist in the process of generating the most basic inferences required for archaeological analysis.

It might denote processes which are inferred to have formed deposits and interfaces, including, perhaps especially, the operational process itself. It is at this stage, also, that depositional status ought to be assigned, reflecting the potential for material retrieved in context to be integrated with the structural record. So if the sherds of pottery retrieved from a deposit reflect primary or secondary refuse, this needs to be mirrored in the matrix. Perhaps this sounds as if I am calling for a standardisation of procedures, for a bureaucracy of stratigraphic thought, but really the opposite is true. By using this logical approach, there is much more potential for individual archaeologists to reflect their personal concerns, research interests, and questions. It is an adaptive strategy.

It is at this stage that I think we have the chance to assess the potential, or inference quality (IQ), of an archaeological resource. This intellectual process of assessment of the archaeology and the archaeologist both defines and informs subsequent processes of analysis.

There is, finally, the more familiar topological matrix which distills the essence of the archaeological record into its structural sequence. Topology is reductive, minimal, logical, and ruthless. It is the edited version of the process of inference, but it has little validity if the processes leading to it are not available for examination and critique. It is the equivalent of reducing the Old Testament to just the ‘begats’.

We are now in a position to genuinely analyse the archaeological record as a record. There follows a vast potential for such denotation and analysis: multi-media access to sites; a key to GIS, to relational aspects of archaeological data, and perhaps especially to the archaeologists themselves.

I have deliberately not provided graphics to illustrate the three sets of denotation which I have suggested above. This is because current debate tends to concentrate on semantics of graphic convention, about which most archaeologists have strong opinions, instead of looking at the ideas behind the graphic. This paper seeks to avoid that debate by emphasising the theoretical aspects of denotation. It is hoped, however, that a collaborative project in the near future will be able to provide a practical demonstration of the ideas contained here.

Conclusion
The stratigraphic matrix may be seen as the essence of stratigraphic analysis, and with good reason. However, the crucial analytical processes involved in its production are often unstated, implicit, and frequently vulnerable. The denotation of concepts must play a more prominent role in the way we analyse entities, or we are in danger of creating a mere caricature of the past. Implicit notions of boundaries, discrete events, and concepts of transformation and change become fossilised in graphic displays, and here there is danger. Matrices are graphic conventions, and whilst they possess great inferential value, they can only be as good as the concepts which lie behind them. It matters little, in the end, how we choose to display these notions, so long as they are explicitly determined, and based on a comprehensive understanding of the role of topology and the metaphysical dilemma.

If we are to continue to distil archaeological entities into two dimensions (and there are still many advantages to doing so), then it is important to enhance the value of the tools of analysis. This may be done by introducing two additional forms of matrix, both with specific roles. The first of these, the trait matrix, displays information relevant to the collection of data in the field, and also reflects the often
equivocal nature of observation. It is axiomatic that all physical relationships are expressed, since we know that it is possible for supposedly discrete entities to interact with each other. On the crudest level, this may involve movement of artefactual material between deposits which have a physical, but not necessarily a structural relationship. On a more subtle level, Tony Barham’s paper in this volume amply demonstrates the dangers inherent in the ‘discrete entity’ mode of thinking.

The second of these matrices, the attribute matrix, reflects processual components of the archaeological record, and must, in addition, account for the intermediate inferences which link what we observe about entities in the field with what we think we can know about them: in other words, it reflects the relationship between traits and attributes. It is only then that there is justification for producing the structural, or more properly topological, matrix.

Reference

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Contrasts in the Recording and Interpretation of ‘Rural’ and ‘Urban’ Stratification

by Peter R Clark

I have used the terms ‘Rural’ and ‘Urban’ in the title not to imply any fundamental difference between the strata encountered in the town as opposed to the country, but rather as a shorthand for a historical separation of the methodological approach employed by practitioners in different environments. ‘Urban’ archaeologists are often confronted with complex, deeply stratified deposits with enormous logistical problems in terms of numbers of artefacts and stratigraphic units, often exacerbated by severe financial and time constraints. ‘Rural’ archaeologists often deal with quite shallow stratification, over large areas and with comparatively few artefacts, but with major problems of depositional and post-depositional processes to take into account when excavating, recording and interpreting their sites. This, of course, is an oversimplification, and any number of exceptions may be described; however, I trust that this simplistic model will be allowed for the purposes of discussion.

The work I am currently involved with requires the co-ordination of field teams working throughout Kent on every kind of site from early prehistoric rural sites to deeply stratified urban environments such as Dover and Canterbury. A field team may be working on a Neolithic/Early Bronze Age site buried under colluvium and with severe problems of post-depositional change one week, and then move to a deeply stratified urban site truncated by later intrusions and with major problems of residuality the next. Training staff and articulating the relationship between on-site recording and effective post-excavation analysis have highlighted these issues of excavation and post-exavation methodology.

Our urban excavations are now undertaken using methodologies developed in the late 1970’s and greatly improved by much debate and practical experience over the last decade or so (Boddington 1978; Harris 1979; DUA 1980; Pearson and Williams 1992). This entails the identification of individual stratigraphic units (or contexts), the comprehensive description and planning of each unit, and the preparation of a Harris matrix to chart the stratigraphic position of each context. Post-excavation studies are based on the hierarchical grouping of contexts allowing interpretive statements to be made, and form a basis for assemblage analysis of artefactual material, a technique that has been used with much success (DUA 1989; Clark 1988; 1992). Those experienced in these techniques will be well aware of their effectiveness and power in deeply stratified situations, and will also be well aware of their shortcomings, as reflected in many of the papers at this conference; such methodological debates are. I feel, a sign that this avenue of archaeological thought is healthy and prospering.

By contrast, rural excavations have no such body of established methodology to call upon, even if only to form a platform for debate. The methodologies adopted on urban sites, with their emphasis on sequence and individual stratigraphic units, are not appropriate on rural sites, where identification of natural processes can be of paramount importance in understanding and explaining the stratigraphic sequence.

Much of our work on rural prehistoric sites has taken place in conditions similar to those usually encountered in urban environments; the creation of the Eurotunnel terminal at Folkestone, for example, entailed a major programme of survey, excavation and other interventions in appalling weather, under severe time constraints, and often working alongside site plant. The simple translation of techniques developed in urban situations was clearly inappropriate; luckily we were able to draw on the skills of an experienced team of field supervisors, but staff training and debate was hampered by the lack of published methodologies appropriate for the types of archaeological phenomena they were examining in such difficult conditions.

The major focus of traditional recording systems has been on sequence; individual stratigraphic units are identified, recorded as independent entities and their relative stratigraphic positions recorded, usually portrayed in the form of a Harris matrix. The technical expression of these concepts, such as the ‘single-context planning’ technique, has proved most successful in excavation and study of complex deeply-stratified sites. However, the potential of these techniques to explore issues of deposit formation processes and post-depositional change has not been fully explored as yet. These observations are not particularly new; studies of residuality, microstratigraphy, artefact distribution, etc. have all been carried out
using the underlying concept of the stratigraphic unit, though these techniques have yet to be woven into an articulated and published methodology.

The very idea of a 'stratigraphic unit' in practical terms is difficult to define; in common sense we all perceive layers of differing nature overlying each other, but when it comes to the crunch the identification of a single stratigraphic unit is a highly subjective and interpretive process. The concept falls between a processual definition (the archaeological manifestation of an 'individual action' in the past) and a descriptive definition (the black stuff overlying the yellow stuff). Harris's seminal work on stratigraphy (1989) falls into difficulties in attempting to identify this concept, particularly in the quest for 'laws of archaeological stratigraphy'. However, we should not be too quick to attack these concepts or the Harris matrix; whilst they may not be the complete answer, they are valuable heuristic models, which have proved their worth and again, and will continue to be useful if their benefits and limitations are understood.

The attempt to understand processes that have acted upon the formation of a context, and the post-depositional changes that it may have undergone, now seems to be near the top of the agenda of modern practical stratigraphic study, and some useful material is beginning to appear in the literature. However, many recording systems rely on the concept of the individual stratigraphic unit as the foundation of their perception of stratigraphic sequence. It is this conceptual base that I suggest should be reviewed.

The use of the stratigraphic unit as the conceptual base for recording is not always adequate for certain phenomena that one may perceive during the course of excavation. Processes may act upon a sequence without destroying the physical attributes of the individual stratigraphic units; bioturbation may sort artefacts without necessarily blurring the interfaces between contexts; differences in underlying subsoil may affect the survival of stratigraphic sequences; the interfaces between stratigraphic units, often not independently recognised, may contain important information. A 'stratigraphic sequence' thus may be viewed as a palimpsest of processes and actions within contexts, between contexts and throughout a group of contexts. A recording system based on the identification of individual stratigraphic units has difficulty in incorporating all these factors in its record of the site. Increasingly at Canterbury we are considering moving away from the concept of the individual context as the basis of our recording and interpretation and more towards a search for a concept of process and action, attempting to identify and record relevant attributes to allow further analysis. At present these are simply subjects for debate; to what extent we can formalise a theoretical base, and translate that into practical methodology useful within the constraints of 'rescue' archaeology, remains to be seen.

A review of the conceptual basis of our recording methodology has only just begun; can we adapt the present system based on individual stratigraphic units, or should we adopt a completely new recording structure? The absence of a practical methodology has hampered discussions of appropriate recording responses and the training of field staff; in practice field staff are expected to acquire knowledge and expertise through 'experience', a woolly and unexplained process. The non-vocational emphasis of many university courses and the perceived lack of consultation between university departments and professional units means that the issue of practical training for field archaeologists is of paramount importance. Given the gradual demise of the large scale excavations that seemed to be a characteristic of the early 1990's, where is an archaeological graduate to gain practical experience and develop expertise? This is an issue that the profession must take very seriously if we are to maintain standards and develop ideas in the future.

Given that an appropriate conceptual basis may be devised to understand the range of processes and actions we believe to be present in the archaeological record, it will be of great importance to turn this into a practical system that our field teams can use in their day-to-day work. We hope that the research currently being undertaken in university departments on depositional and post-depositional processes will also have a practical expression of use to field practitioners. An academic input would be most welcome here in identifying attributes that might signify particular processes or actions. One possibility is that a range of methodological recording 'tools' could be devised by specialists, with clear and unambiguous practical instructions for recording attributes in the field. In this way, specialist advice would be available to often hard-pressed field teams, enabling an increase of the inference potential of their records, the quality of archaeological interpretation and ultimately the job satisfaction of the team members.

Whilst published methodologies have proved highly successful in urban situations, and issues of depositional and post-depositional history may be adequately accommodated within this theoretical structure, the approach is not satisfactory in 'rural' situations where a wide range of depositional and post-depositional processes may act within, between and throughout stratigraphic units and sequences. There is thus no adequate practical, published methodology for excavating such sites in 'rescue' situations.

I feel that the profession should address these issues, perhaps adopting a different theoretical basis for stratigraphic recording based on the concept of
process and action, rather than the present emphasis on units of stratification articulated by sequence. In Canterbury we have embarked on a review of the epistemology of stratigraphic recording, with the hope that a practical approach to addressing the issues I have described may be devised.

Such a change in standardised recording responses, involving a more flexible response and a deeper understanding of archaeological process and post-excavation techniques, will necessarily entail a greater demand for skilled and experienced field excavators. The provisions for vocational training are at present almost non-existent, and this is a most pressing issue for the profession.

One step towards facilitating the publication of methodological debate would be a journal devoted to the subject; to this end a new journal is to be launched in 1993, entitled Advances in Archaeological Practice. At present the general editors are seeking copy for the first issue; more information and notes for contributors are available from CAT, 92a Broad Street, Canterbury, Kent, CT1 2LU.

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Truncation Horizons and Reworking in Urban Stratigraphy

by Brian Yule

This paper suggests that field archaeologists may often not recognise 'reworking' in the excavation of complex urban stratigraphy. The identification of truncation horizons may enable the archaeologist to distinguish between reworking and dumping. It is considered that the standard approach to the construction of the Harris matrix may actually mask the presence of truncation horizons. Consideration of all the physical relationships of a context, not only the latest, would enable truncation horizons to be identified, and allow reworking to be recognised.

My proposition that reworking is often not recognised in complex urban stratigraphy is admittedly not based on a thorough survey of the literature. It is an impression gained during my research on the 'dark earth' in London (Yule 1990), from a small number of available London archives, and from more generalised statements about its formation process in London and other towns. It seems to me that we have been too ready to interpret 'dark earth' horizons as dumped, rather than subject to processes of reworking.

In this paper I have used the Winchester Palace, Southwark site to illustrate several probable reworked deposits.

Interpretation of mixed earth layers relies overmuch, I believe, on the nature of the layers, in isolation from their context in the stratigraphy. Stratigraphic evidence, particularly the identification of truncation horizons, may help us to distinguish between dumped and reworked strata. I define a 'reworked' layer as one which has undergone mixing and post-depositional transformation; at the base of the reworking there is necessarily a truncation horizon.

A truncation horizon at the interface between strata is equivalent to a geological 'unconformity', indicating a break in the sequence. Harris, in Principles of archaeological stratigraphy, refers to this horizon as an 'interface of destruction' (Harris 1989, 68), but gives little guidance as to how such an interface may be recognised, or how overlying layers may be interpreted.

It is generally accepted that in the absence of truncation a layer deposited naturally or anthropogenically would be expected to seal a single chronological horizon. A corollary of this is that more than one chronological horizon exposed at an interface implies truncation and the presence of a truncation horizon. Of course we should not expect all truncations to necessarily expose multiphase strata; eg the truncation horizon produced by the reworking of a thick destruction dump would simply reduce the thickness of the latter, and not necessarily expose earlier strata.

The interpretation of layers that seal a truncation horizon is not straightforward; layers may be dumped - as in a make-up sealing an eroded surface, or, reworked - as in a worm-sorted soil or a plough soil. While it is difficult to be prescriptive when the potential sites and agents are so numerous, it seems clear that biological or anthropogenic reworking should be considered as an interpretation for many well-mixed earth layers sealing truncation horizons.

Some examples of probable reworking are presented from Winchester Palace, Southwark (Yule 1989; research archive and publication in preparation). The accompanying schematic section across the site highlights putative truncated horizons and (hatched) reworked layers; the standard Harris matrix shows the sequence of groups (Fig 1).

The sequence can be briefly summarised as follows:

Group 20 is the 'natural', a sandy gravel, overlain by 21, a pre-Roman waterlain flood clay. Groups 22-25 are three phases of Roman buildings. They are sealed by Group 28, a make-up across the whole area for a masonry building, represented here by Groups 31 and 36, which is part of a multiphase sequence and is in use from the 2nd to the 4th century. Several hypocaustus rooms are demolished in the early 4th century - Group 37 is demolition debris filling one of the hypocausts, and Group 38 robber trenches. Group 40 'dark earth' forms in the post-Roman period, during which there is an erosion event. Group 41, which truncates the Roman sequence at the east end of the site. After infilling there is late Saxon occupation, as evidenced by several pits, Group 43. Medieval Winchester Palace dates from 1150, comprising Group 44, the eastern precinct wall and building range with internal floors, and external Group 45 garden soil.
Examples of reworking:
1. The weathering and development of a soil in the top of the Group 20 'natural' sandy gravel represents biological reworking.

2. The cut through the Roman sequence at the east end of the site is an obvious truncation horizon. However its interpretation is not certain; how do we explain the recorded cut line, and the filling in of the cut with 1 m of unstratified, homogeneous dark earth? The absence of tip lines makes it difficult to interpret the infill as dumping. The original cut may have been the result of erosion of the bank of a stream, assumed to have flowed just east of the site. The cut may have been infilled naturally (probably over a period of centuries) by soil development due to biological reworking of exposed strata, and the process of soil creepage and slippage down the slope resulting in natural soil accumulation.

3. Group 28 is also probably a reworked deposit, though there is no obvious truncation horizon at its base. The deposit appears similar in character to the 'dark earth' but has significant quantities of charcoal and slag, particularly towards the base of the layer. The charcoal and slag do not seem to be associated with any on-site process, and may represent industrial waste brought onto the site as bulk material in levelling up dumps. Subsequently during the life of the building represented here by Groups 31 and 36, that part of Group 28 which was external to the building was relatively much more mixed than that part which was sealed by the building. Mixing would have been the result of biological reworking - the process of soil formation, and anthropogenic reworking - the presence of late 2nd-century pottery may indicate the digging in of kitchen waste to a garden area.

4. The Group 45 dark brown earth deposit was external to, and contemporary with, the Group 44 medieval building range. There is documentary evidence for this area being the Bishop's 'privey garden'. What is the source of the earth? Dumping of imported earth is a possibility, but seems less likely than reworking. Favouring the latter interpretation is a clear truncation horizon.
which exposed earlier Roman strata, and the presence of much residual Roman pottery. Reworking may have comprised digging over of the ground and subsequent biological mixing, via root and worm action.

5. The Group 40 post-Roman ‘dark earth’ is probably also a reworked deposit, but is little represented on this site.

I believe that on many sites ‘dark earth’ formation results in part from biological reworking of earlier deposits during site abandonment. The identification of truncation horizons at the base of such layers which prompted their interpretation as reworked rather than dumped deposits (Yule 1990, 621).

Do we record stratigraphy so that we can recognise truncation horizons? It seems to me that our concern with establishing the sequence, as in the standard Harris matrix, masks the presence of such horizons. Harris’ Law of Stratigraphical Succession states:

A unit of archaeological stratification takes its place in the stratigraphic sequence of a site from the position between the undermost (or earliest) of the units which lie above it and the uppermost (or latest) of all the units which lie below it and with which the unit has a physical contact, all other superpositional relationships being redundant. (Harris 1989, 34)

This has led to the failure of most excavators to record the relationship of a layer to any but the latest of the underlying layers. I agree that in drawing the Harris matrix this is good practice - the matrix should be kept as a vehicle for displaying the sequence, and would be crowded with ‘cross-overs’ if all relationships were to be shown. But I believe we should be at pains to record all physical relationships of a context, because this would enable truncation horizons to be identified.

The Group 45 medieval garden soil at Winchester Palace is a good example: although the Harris matrix shows no relationship with earlier Roman layers, Groups 28 and 37, the fact that Group 45 physically seals them is significant to its interpretation as sealing a truncation horizon, and therefore raising the possibility that the deposit is reworked.

The failure to identify truncation by reworking means that we may not recognise the extent to which earlier stratigraphy has been lost.

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References
Interpretation of the Formation Processes of One Context on the North Downs in Kent

by Andrew Hutcheson

It is perhaps more appropriate to add 'problems with' to the beginning of the title, as it was the lack of an accessible method for interpreting this specific context that led to thought on ways in which interpretation could have been better served. It was realised, after the fact, that the information necessary to the interpretation of the context had not been collected in the field and only questions as to its nature could be posed.

The subject, context 2, was a constituent of a site known to the Canterbury Archaeological Trust, rather prosaically, as F72; one of a large number of sites discovered during survey work by C.A.T., prior to the construction of the Channel Tunnel Terminal. F72 lay at the bottom of the southern slope of Castle Hill near Folkestone, at about 70 m OD.

On top of the hill itself, Caesar's Camp, is a Norman keep, excavated last century by General Pitt-Rivers. There is also a likely Neolithic causeway enclosure, evidence of which comes from both the shape of earthworks on the hill and a small excavation carried out by C.A.T. in 1987.

F72 was excavated by C.A.T. between October 1991 and March 1992 in advance of an extension to the A20, made necessary by the expected rise in traffic with the imminent opening of the Channel Tunnel. The excavation was funded by English Heritage, with agreement for access coming from the Department of Transport and Transmanche Link.

The excavation took place during the coldest (and wettest) winter months on an exposed hill slope; indeed, conditions could not have been much more adverse for detailed archaeological work. In addition the 'window' or period of time allocated for the excavation was, initially, three weeks. This meant that what realistically could have been expected in terms of a record was a very basic salvage of the site. The amount of time for excavation was then extended, however by this point context 2 had been excavated. Therefore what follows here is to an extent wishful thinking from post-excavation with the benefit of hindsight.

Most of the features perceived on F72 were cut into underlying chalky colluvium or hillslip. The most prominent of these features were three ring ditches (cuts 102, 103 and 104), first seen through aerial photography, and which were the original reason for the rescue excavation. Most of the rest of the features were interpreted as either pits, post-holes or stake-holes. Pottery from these features has been variously dated to the Late Neolithic/Early Bronze Age Transition, or Beaker period, to which the ring ditches are dated; Deverel Rimbury or Middle to Late Bronze Age from one feature, and quite a lot of Early Iron Age material from the later features. Truncating all three ring ditches and many other cut features was a very large unbounded cut (101), interpreted as a terrace of Early to Middle Iron Age date; filling this unbounded cut was the context in question.

Context 2 was defined upon stripping off the top stratum, which was a redeposited and much abused soil layer. It lay underneath a service road, which serviced Channel Tunnel work on the portal at Holywell Coombe and then had a massive spoil heap placed on top. This left context 2 so compacted that when it was struck with the point of a mattock, the mattock bounced back, leaving a small diamond shaped impression on the surface.

From its surface, context 2 looked remarkably uniform and undifferentiated. It was homogeneous in colour, compaction, in its most common inclusions and fine components. The initial, on-site, interpretation of context 2 was as a single unit of relatively slowly accumulated colluvium caused by soil erosion up slope. This may have been the result of intensive agricultural use in prehistory. At the time it was very difficult to predict its information potential for reasons that hopefully will become apparent.

F72 was a rescue excavation and, as mentioned above, time was therefore at a premium. The options for the excavation of context 2 were therefore limited, particularly as the major overriding aim of the excavation was the investigation of the ring ditches. Taking it down in spits by hand was not a feasible option, given the time limitations, the number of available archaeologists and money. It was already known that in places context 2 was almost 1 m deep and that its compaction was formidable. The decided method of excavation was in effect to sample the context by hand. Three slot trenches 1.5 m wide were excavated running down slope from the edge of the cut to the site edge, located to the south. The
remaining vast majority of the context was excavated by machine, with recovery of materials when spotted.

During the slot trench excavations a few lenses with slightly different components were discovered. Within a couple of these lenses whole, though crushed, pottery vessels were found. None of the lenses had been visible from the surface of context 2, indeed some were nowhere near the surface. They were all surrounded by the homogeneous material which made up context 2 and were discovered in a 'quarrying' fashion. These inclusions were loosely interpreted as either 'occupation' episodes which had occurred during a slow colluvial accumulation, or as material that had been engulfed and transported by a quick colluvial accumulation.

Apart from the pottery content of the lenses, there was also a large assemblage which came from the general context. This consisted of quite fresh looking material of Early to Middle Iron Age date together with relatively abraded sherds from both the Early Bronze Age and the Late Bronze Age.

Another, conceptual, context was delineated when darker patches were uncovered within an area of context 2; there was no definite boundary to this area. This context (context 14) upon excavation proved to be basically the same as 2, with the exception of the dark patches of fine silty loam which contained a far greater density of pottery than was generally the case within 2. The frequency of these inclusions and their size increased with depth, until it was discovered that they lay above pits which could be seen as cutting natural. It seems likely that these pits were cut through the colluvium at a mid-point in its accumulation, this relationship then becoming blurred through unknown processes, guessed at as being bioturbation.

Many of the features that could be seen to cut the chalk had upper fills which were very similar, if not identical to 2. There was no sharp definition, only a diffuse boundary between any of the layers filling features directly underneath context 2.

What can be said about the formation of the context from this rather meagre data set?

The original interpretation, of there having been a long term accumulation of colluvium is still the most credible. This interpretation is supported by the inferred evidence for pit cutting from a mid-point in the colluvium. Alternatively, if the dark patches or context 14 are given a different inferred process of formation, one where an unknown process of bioturbation brings up material from underlying contexts (namely the pits) into the body of context 2, then the colluviation could be explained as occurring quickly, incorporating occupation materials lying in its path.

The data to discern deductively between these two interpretations, or inductively to create new ones, does not exist. In either case there is a dislocated 'occupation' sequence, for which we have very little recorded contextual information. For instance, using the first framework of interpretation, any cuts within the colluvium became filled with very similar material to that into which they were cut; even if they had additional fills of other materials, they may only have been recorded as lenses or inclusions in context 2 and their spatial position may have been left unrecorded.

If the assumption is made that the ceramic material within context 2 dating to the Early to Mid Iron Age is in situ, since it occurred in groups that seem to represent whole or large proportions of vessels, then the rest of the material can be considered as residual. The spread of the in situ materials within the context may have some pattern which would perhaps facilitate an interpretation of the cultural nature of the 'occupation' that enacted their deposition. Without this spatial information on these materials it is difficult, if not impossible, to discern the nature of the context of deposition. In Schiffer's terms, the archaeological context gives no interpretive handle with which to know and understand the systemic context from which the artefacts originated (Schiffer 1972).

If the first assumption is accepted then a further assumption can be made: the context of deposition for the Iron Age pottery within 2 was as secondary refuse, although with the proximity of the ring ditches latent ritual deposition cannot be completely ruled out. Either way, these assumptions then logically lead to a third assumption: there were cut features within the body of 2 or prior to its accumulation but lying further up slope. More tenuously, if the pottery represents secondary refuse then it is also probable that structural remains of settlement may have likewise existed, in fairly close proximity to the refuse deposition areas.

There were many features cutting the chalky hilltop that could have originated at a higher, and to all intents and purposes invisible, level within the body of context 2. Since the point from which these features were cut cannot be ascertained, they all coalesce into one group, in a relative chronology based on the visible stratigraphic sequence.

It was clear from the artefacts recovered from some of these features below the level of the chalk that there were at least three phases of activity on the site. However, for many of the features, particularly those that may have represented the remains of structures, it was unclear to which phase they should belong, since no artefacts of diagnostic date were found within them.

Purely on its own, the stratigraphic sequence for this site does not help in producing a relative chronology for all the contexts excavated. The stratigraphy has effectively been erased or, at the very least, blurred through other processes.

With hindsight, there were several options for
Fig 1: Context 2, site F72 cut by ring ditches
applying tools at the time of excavation which would have increased the information content. Crucially, the soil micromorphology of contexts 2 and 14 was not examined. This would have solved the enigma of the duration of colluvial deposition and hence whether the ‘occupation’ was in situ or re-deposited from further up Castle Hill. Other methods for dealing with blurred sequences have been recommended; one example was discussed by Needham & Stig-Sorensen (1988). Their blurred sequence was a ‘midden’ deposit at Runnymede, a Late Bronze Age settlement site situated on Thames gravels between Berkshire and East Surrey, which suffered from a similar homogenising of stratigraphic relationships, this time in alluvial deposits.

At Runnymede in situ pottery groups were recorded by planning at 1:5, and each sherd was given a number. This level of detail allowed processes to be inferred from the position, condition and relation of the sherds to each other. This allowed interpretation of the nature of the original depositional surface and post-depositional changes which might have occurred. In effect, pottery groups which appeared to be in situ were treated as separate contexts, each representing at least one particular cultural event.

Potentially, a similar data set on the pottery groups for F72 would have allowed the inference of the upper parts of features and hence the upper levels of the stratigraphic sequence and a relative chronology for the site. This in turn may have led to the delineation of structures and/or cultural activity zones within the body of context 2. In practice this may in part still be possible with detailed analysis of the pottery recovered from 2, looking at ‘cross-fits’ between sherds, but the spatial information will obviously not be recoverable.

The individual aspects that were missing from the recording of context 2 in terms of requirements for the interpretation of its formation and relationships with other contexts were:
1. A series of soil micromorphological samples from 2 and 14.
2. Detailed positional information on the in situ pottery groups.
3. Spatial information on lenses and any large inclusions within the context.
4. A full contour survey of the surface and perceived bottom of the context (necessary in order to relate the positions of the inclusions to an overall position in the context).
5. Full recovery (or as close as possible) of artefacts with three-dimensional information on the position of each.

The above are considered necessary for the in-depth interpretation of the context. How feasible is the collection of such a data set within the confines of a pressurised rescue excavation? Excavating the whole context by hand, in this case, was never going to be an option; quite literally it would have taken years. A quick method of excavation and recording would have been necessary.

A way of achieving this would have been to excavate shallow spits with a machine, one area at a time, leaving a series of sections at various locations across the context from which soil micromorphological samples could be taken. Following the machining, artefacts and inclusions could be spotted, recorded and further excavated. The recording would consist of plotting the position of the anomaly in three dimensions, which would most effectively be done using an Electronic Distance Measuring device, or in the case of a pottery group recording in plan at 1:5 and separately numbering each sherd. Using such a methodology a greatly enhanced data set could have been provided for context 2 in a relatively short amount of time.

Additionally, some post-exavation analytical tools would be necessary in order to interpret the data set, most importantly a method for viewing and manipulating the data in three dimensions, namely a three-dimensional computer mapping system. Also standards of pottery abrasion for the various periods and fabrics involved should be generated so that models for sherd movement can be established. Such tools would have applications in any situation where stratigraphic blurring is suspected.

References
**Finds Context and Deposit Status**

by Steve Roskams

**Introduction**

In recent archaeological excavation, but particularly since the advent of the ‘Rescue’ movement of the 1970’s, the case for government funding has been made in terms of the need to ‘preserve by record’ those sites threatened by redevelopment. If the archaeologist was pressed further, justification for allocating often substantial resources was based on the need to gather artefacts not just as individual finds, but in coherent assemblages - hence the requirement for a regulated context for their recovery. Despite the changes from unit- to project-funding, and latterly to developer finance, these broad lines of argument continue to be used when trying to acquire material support for excavation projects. Given this, it is more than a little surprising that, when finds are recovered in such controlled circumstances, very little work goes into linking site and finds evidence during the subsequent stages of analysis. This paper considers why this is the case and what needs to be done about it.

It is reproduced here essentially in the note form of the original presentation - ‘warts and all’. The central points which it seeks to make would no doubt have benefited from wider reading and from incorporating comments made at the seminar itself. However, this was not possible in the limited period of time between giving it and the present volume being produced. Therefore it comprises, even more than is usual in archaeology, some preliminary conclusions from thinking in progress, rather than a finished item.

Of course, part of the explanation for the continuing dissociation of finds and field research mentioned above is obvious. As several other contributors to this volume make clear, organisational matters within the profession militate against specialist integration. Thus site staff move from place to place, putting pottery in trays and soil samples in bags, but have only limited contact with the artefactual or ecofactual workers who process the results of their endeavours at a later date. Similarly, further up the hierarchy, site directors write up the structural sequences according to one timetable, with or without access to pottery spot dates, coin lists etc., and finds specialists work to a different schedule, with different objectives and often in a different place altogether. The end result of this divergent research activity is the publication of site and finds in isolated sections of a report, or even in separate volumes which appear at very different times.

However, organisational matters are only part of the story, and might be better seen as a symptom of underlying conceptual problems rather than a cause in their own right. These fundamental issues result from a failure to theorise adequately the relationship between a find and its associated deposit. The calls of Hodder et al for contextual archaeology (Hodder 1987) have fallen mostly on deaf ears within the field profession. This is partly a function of the general divorce of theory and practice in archaeology, as in many other spheres of modern life, and partly because of the inherent weaknesses in the formulation of the ideas themselves. But it is mainly because nobody is putting in the theoretical groundwork necessary to enable our sophisticated field data to contribute to that contextual archaeology. If this can be done, the field profession is then surely capable of instituting the necessary organisational responses to enable it to flourish. This paper considers some ways forward for future practices.

**The Conceptual Problem**

In mentioning this lacuna in theoretical development, I do not wish to imply that none of the necessary work has been done. Indeed, issues such as site formation processes and the various transformations of the archaeological record have been looked at in considerable detail. If Schiffer’s work (1976, 1987), with its potentially useful distinction between curation and rubbish survival, remains the *magnum opus*, many other archaeologists, including several involved in the seminar, have contributed to discussions of site evidence. Furthermore, articles by Stein (1987), Beronius-Jorpelund (1992) and Fedele (1984), amongst others, show that the challenge is also being taken up in North America, Scandinavia and on the continent. Equally, on the artefact side, some research has been published. Thus seriation has been promoted (Carver 1979 and 1983). Orton and Orton (1975) some time ago published a method to assess statistically artefact date against date of deposition, an approach recently taken up to good effect by Evans and Millett (1992). Vince (1985) has traced dated sherd through stratigraphic sequences, Bradley and Fulford (1980) have considered sherd size in specified deposits and Moorhouse (1986) has looked at cross-joins from some
Yorkshire sites. As a result of this endeavour, there have been some inroads into the problem in terms of the character of the finds assemblage itself.

However, if one turns from the consideration of artefacts and ecofacts as discrete entities to their investigation in relation to site evidence, the record is less impressive. Furthermore, when such work is attempted, the conventional approach is to define the 'status' of a deposit in terms of its physical characteristics and thereby suggest that the finds from that stratigraphic unit are of similar status. Thus a spread of grey, charcoal-flecked silt is interpreted as trodden occupation debris and assigned primary status, along with the carbonised grain which it contains. A dump of redeposited clay, on the other hand, is seen as secondary, as is the pottery within it. Such a procedure is unfortunate on three counts.

Firstly, the impetus to make these decisions on status often comes from funding agencies anxious to cut costs. They want to know which small part of the artefactual assemblage to investigate properly, and which to throw away (or, at best, leave to accumulate dust in 'the archive'). Given this situation, it is not surprising that some people, both archaeologists and non-archaeologists, are starting to question why precious resources were expended in recovering the non-researched material in the first place.

Secondly, and not unconnected with the issue of funding, a decision on deposit status based on site evidence is usually considered as an end in itself. Any judgements made are rarely compared to assessments derived from studying the finds along the lines given above. Thus the work of the expert looking at wear indices on coins, or rim percentages on pottery, is not compared to that of the site stratigraphist. The latter's work has provided the answer, so why bother wasting time and money repeating the exercise? However, if various specialists utilise such different criteria to identify residual material, there is no guarantee that they are speaking the same language, or even have the same concept of residuality in mind.

Thirdly, and most fundamentally, any study which attempts to define status directly from the physical characteristics of a stratigraphic unit is making a basic error, and this is the nub of the matter. Status is a property of neither the deposit itself, nor of the assemblage derived from it. It is a property of the 'relationship' between these two entities. The recent, but now conventional, use of the word 'context' in place of the old-fashioned 'layer' to mean stratigraphic unit has done much to muddy the waters over this issue. An example will explain what is at stake. Consider a piece of wall plaster derived from a wall on site. It makes no sense to ask 'What is the status of the wall, based on its characteristics?' or to enquire 'Is the plaster of primary or secondary status?'. The only valid answer to such questions is 'It all depends', and what it depends on is how the plaster related to the wall on the ground. If it was applied to a prepared surface immediately after the wall's construction, this may be considered a primary relationship. If the plaster is mixed up in its core with reused stone, having been unintentionally redeposited from disturbed levels several centuries earlier in date than the wall, it lies in a secondary relationship. To repeat, status is a relational property. To define it in a particular circumstance, one requires information on two elements - the find itself and its specific context on the site. Both are necessary; neither is sufficient in isolation.

Some Definitions of Status
With the above conceptual issues in mind, it should be possible to define several such relationships, several different types of status, which go beyond the simple division into primary and secondary usually adopted. A preliminary attempt is made below. In each case functional, chronological and spatial information on both find and associated stratigraphic unit is needed, variations in the relationships between these factors producing the different types described. Some of the most common have been picked out, though with more thought and investigation, one could no doubt produce finer distinctions and subdivisions.

Type A - finds contemporary with, and functionally connected to, the stratigraphic unit from which they were derived. This is the closest form of relationship and would apply, for instance, to ironworking slag found in the ash deposit in the bowl of a furnace. To prove its existence, one requires specialist identification of the material and secure interpretation of the function of the associated feature from its structural characteristics.

Type B - finds broadly contemporary with, yet functionally and perhaps spatially distant from, the context in which they were found. This would apply, for example, to the same type of industrial residue mentioned above, this time lying in a deposit spread on the surface of a yard outside the building containing the hearth. It has chronological significance for the period concerned, but no specific functional implications for activities on the spot at which it was recovered.

Type C - finds functionally and chronologically unrelated to the context in which they were found, but derived locally. Thus on a deeply stratified, multi-period site occupied from the Roman period to the present day, second century pottery found in a twelfth century rubbish pit, having fallen in from its sides during the medieval digging of the feature, would qualify. Such material has functional, and perhaps even spatial, implications for the site in question but neither of these concern the period of the
pit fill in which it was found.

Type D - finds functionally unrelated to the context in which they were found, imported to the place of deposition, and earlier in date than that context. Thus, for example, boat timbers, provably of early medieval date by dendrochronology but incorporated into a late medieval structure, would fall under this category. Though these may be of great interest to those studying medieval carpentry, and relevant to an investigation of processes of timber recycling, they tell us nothing about the later structure nor of vernacular architecture at that time, except in so far as they were modified in order to be accommodated in the new building.

This last example makes one final thing clear. Strictly speaking, the property which we are identifying when assessing status is not even that between the find in its entirety and its associated stratigraphic unit, but between a defined aspect of the data embodied in the find and that unit. Thus the information derived from the now redundant early medieval carpenters' marks on the timber of the last example are irrelevant to a study of fifteenth century prefabrication, whereas the complex joint on the same timber cut when it was reused may be of vital importance.

Conclusion
If the haziness surrounding the concepts of residuality and deposit status could be removed, and the types of distinctions outlined above could be constructed more thoroughly and used more widely, archaeological research and archaeologists themselves would benefit in a number of ways. Firstly the finds assemblages recovered in controlled conditions from our excavations would be more fruitfully investigated. Also funding agencies would have more return for the resources invested in their original retrieval. Finally, and perhaps most important for the health of the profession, a more fulfilling and productive working relationship between field, artefact and ecofact specialists would be developed. We have a duty to ourselves, our colleagues and our sites to see that this is done.

Bibliography
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Spot Dates as Qualitative Data?

by Barbara Davies

This paper is not intended to be definitive, rather it is presented to encourage the audience to ask questions about how these data can be applied to meet individual needs. The discussion is confined to the Roman period and the examples are drawn from London and Lincoln where similar systems of data gathering and site interpretation are in use.

According to MAP2 every site excavated should have a basic archive. In order to deal with large numbers of backlog and current sites, systems of 'spot-' or 'pot-dating' have been developed using mnemonic codes for fabrics, forms and types of decoration (eg VRW = Verulamium region white ware). Thus a basic but comprehensive pottery archive can be collated within a short time scale. Ideally every site should have two tiers of recording:

1) spot-dating - the basic archive,
2) quantification, using at least two measurements (in preference EVEs and weight), of well stratified or ceramically important assemblages.
3) in particular cases a third tier would be implemented for kiln material and primary or single depositions of chronological significance where a higher level of recording is required, such as base and handle measurement and the nuances of rim types.

Unfortunately many sites will not have sufficient funding to take pottery research further than tier 1, therefore generating no reliable statistical data. However, the basic archive produces a vast body of data and this paper explores possible applications of these data.

The recording methods used in Lincoln and London (Figs 1-3) demonstrate the process by which the basic archive is converted to qualitative data. The Lincoln recording system has more fields than the London system, where they are condensed within the comments field, but the two systems provide the same amount of information. The Lincoln system takes more time to complete and computerise the information but the extra fields are particularly useful for accessing forms and decorative features and providing a sherd count statistic. The use of an emperor code (NERO), or condensed numerical date (ML4), provides a fast method of grouping all types of pottery from a particular date range for assessing the overall ceramic content of the period.

The primary use of the data is to provide relative dating for the site, but also information for site assessments such as: the quality of the depositions; whether the pottery is burnt, highly abraded, smashed, crushed, residual or particularly homogeneous; whether there is an hiatus in the occupation of the site or a chronologically long sequence. Here it is important to stress the level of communication between site and pottery specialists so that obvious anomalies such as the presence of medieval pottery in the earliest Roman levels (X in Fig 3) can be explained and depositional factors, such as residuality, can be taken into account before the site is phased.

Fig 1A: London, spot-dating data - information stored

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<th>Size</th>
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<th>Latest Date</th>
<th>Fabric</th>
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Fig 1B: London, quantified data - information stored

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Fig 2: Lincoln, spot-date recording sheet

PHASE: Postro? SITE: hg72 CONTEXT:AK SHEET NO 1 of 25

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<td>V</td>
<td>42</td>
<td>RIM-BASE PROF</td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>ZZZ,</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>SMALL SHS</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ZDATE,</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>SOME BURNT,</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>ML4</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Fig 3: Example of pot-dating from London as a computerised printout.

101,RPOT,L,NERO,55,70,SAND,NJ
101,RPOT,L,NERO,55,70,VRW,1B2,
101,RPOT,L,NERO,55,70,VRW,LID
101,RPOT,L,NERO,55,70,VRW,MORT
101,RPOT,L,NERO,55,70,DR20,-
13,MPOT,S,INT?,1000,1150,EMSS,CPX
13,RPOT,S,PREF,40,70,AMPH,-
13,RPOT,S,PREF,40,70,C186,*RIM
13,RPOT,S,PREF,40,70,SAM,DR27
13,RPOT,S,PREF,40,70,SLOW,II

Spot-dates are preliminary dates assigned to individual contexts. When the site has been phased (Fig 4 below) the basic archive is combined with the phasing to provide tighter dating. Groups of contexts, thus a larger pottery assemblage, yield more information than isolated contexts. The latter may only consist of a few sherds with no diagnostically datable forms or ware types resulting in a very broad date range, whereas the larger group is more likely to include a number of diagnostic forms and ware types. Any redeposited material should be apparent at this stage. At this point there should be further liaison between specialists including those who provide more absolute dating information such as that given by coins, samian and other stamps, and dendrochronological analysis (where relevant) etc. This provides a good dating structure for the site thus helping to refine pottery and other chronologies and typologies such as architectural, building material or leather artefacts.
**Fig 4: example of spot-dating tabulated by phase**

Phase 1-6

<table>
<thead>
<tr>
<th>59 RPOT S LIE2 70 to 130 AD</th>
</tr>
</thead>
<tbody>
<tr>
<td>LOMI IVJ</td>
</tr>
<tr>
<td>LOMI VAR CDR30?</td>
</tr>
<tr>
<td>VRW</td>
</tr>
<tr>
<td>LOMI</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>60 RPOT S LIE2 70 to 130 AD</th>
</tr>
</thead>
<tbody>
<tr>
<td>SAM DR18/31</td>
</tr>
<tr>
<td>LOMI IVJ</td>
</tr>
<tr>
<td>MICA LID</td>
</tr>
<tr>
<td>SAM DR27?</td>
</tr>
<tr>
<td>SAM DR37</td>
</tr>
<tr>
<td>SAND</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>63 RPOT S LJM2 70 to 150 AD</th>
</tr>
</thead>
<tbody>
<tr>
<td>AMPH C185/6 VAR *RIM</td>
</tr>
<tr>
<td>HWC IIE</td>
</tr>
<tr>
<td>HWC IVF</td>
</tr>
<tr>
<td>RWS</td>
</tr>
<tr>
<td>SAM DR37</td>
</tr>
<tr>
<td>VRW I</td>
</tr>
</tbody>
</table>

Statistical questions concerning dating, trade, and topography can be exemplified by using presence and absence (categorical variables: nominal, ordinal and binary) from the spot-dating data and relative percentages (continuous variables: ratio of weight and EVEs) from the quantified data. The data are sorted into:

i) Ware group/broad function (eg FINE; REDUced - cooking vessels; AMPHOrac)

ii) Individual sources within a ware group (eg AHSU - Alice Holt kiln products)

iii) Vessel form/detailed function (eg beaker, bowl, jar and flagon)

iv) by phase - pottery joined with phasing files

v) by site function - pottery joined with dating/interpretation files.

Thus, plotting the rise and fall of fabrics and forms through the comparison of the quantified data from stratified sequences of pottery assemblages, either from a single site or across a number of sites, it is possible to refine the date range of industries or styles. Chronological patterns which elucidate the links between the site, or sites, and Romano-British and/or continental suppliers provide information about trade and trade routes. The comparison of assemblages from several sites of the same date demonstrates topographical differences in function.

In order to provide this information rapidly for one synthetic project (Sites East of the Walbrook, Williams forthcoming CBA), consisting of over 36 sites within a two year time scale, a system of site interpretation was developed. Each context was given an interpretive code so that the nature and/or status of the deposit could be readily assimilated. The overall site formation was presented at a glance and allowed rapid sorting of artefactual and ecofactual data. The site was then discussed as ‘text sections’ (or archaeological events) within ‘landuse’ blocks. Fig 5 illustrates a simple ‘landuse’ diagram with basic dating parameters, both absolute and relative, as well as historically attested events.

An example of a more complex ‘landuse’ diagram, Fig 6, demonstrates that even the most complex of urban sites can be interpreted in this way. The ‘landuse’ blocks provide the framework for further research and project design allowing the value of a site - stratigraphic, ceramic, as well as artefactual and ecofactual - to be rapidly assessed.

By joining the interpretive codes with the pottery data (Fig 7, below), we can refine the dating further and begin to interpret the particular features of a site. At the touch of a button, we can explore the ceramics of a building through its construction, use and destruction, even delving into individual rooms and partitions. Within a short space of time it is possible to provisionally interpret the nature and function of individual buildings or other archaeological events from the types of ceramics found in association with them. In the example given, contamination is denoted by the use of ‘!’. The stratigraphic date (stratdate) is given in emperor or condensed dating codes as precise dates (other than ipq’s provided by absolute dates from coins etc) cannot be assigned to pottery. This method also allows rapid comparison with national and continental sites during the Roman period where activity relates to historical events within an emperor’s reign.

**Fig 5: A simple Landuse Diagram**

- **FORUM**
  - Samian stamp 70-85 AD
  - **BUILDING 2**
    - Pot 70-120
    - **OPEN AREA 4**
    - Coin Vespasion c 69-79 AD
    - **BUILDING 1**
      - Pot 55-70
      - **OPEN AREA 3**
      - Pot 55-70
    - **OPEN AREA 2**
      - Pot 55-70
    - **OPEN AREA 1**
      - D174us Pot 55-70
      - ErDA1 Pot 40-70

- **OPEN AREA 1**
  - D174us Pot 55-70
  - ErDA1 Pot 40-70
Fig 7: pot-date data joined to interpretive data to construct a stratigraphically dated database.

<table>
<thead>
<tr>
<th>Context Group</th>
<th>Phase/Interp Code</th>
<th>Stratdate Period Code</th>
<th>Ceramic</th>
<th>Size (see above Fig 2 for remaining fields)</th>
</tr>
</thead>
<tbody>
<tr>
<td>136, 1.1</td>
<td>D1377usOA1, NERO+</td>
<td>D1377usOA1, NERO+</td>
<td>RPOT, S</td>
<td>ISTC,40,100,AHSU,I</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>MPOT, S</td>
<td>INT?,1000,1150,EMSS,CP</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>RPOT, S</td>
<td>PREF,40,70,AMPH,</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>RPOT, S</td>
<td>PREF,40,70,C186,*RIM</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>RPOT, S</td>
<td>PREF,40,70,OXID,</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>RPOT, S</td>
<td>PREF,40,70,SAM,DR27</td>
</tr>
<tr>
<td>13, 1.2</td>
<td>!ErOA1, NERO!</td>
<td>!ErOA1, NERO!</td>
<td>RPOT, S</td>
<td>PREF,40,70,SLOW,</td>
</tr>
<tr>
<td>173, 1.2</td>
<td>D174usOA1, NERO</td>
<td>D174usOA1, NERO</td>
<td>RPOT, S</td>
<td>1STC,40,100,AHSU,</td>
</tr>
<tr>
<td>173, 1.2</td>
<td>D174usOA1, NERO</td>
<td>D174usOA1, NERO</td>
<td>RPOT, S</td>
<td>1STC,40,100,KOAN,CAMPA</td>
</tr>
<tr>
<td>173, 1.2</td>
<td>D174usOA1, NERO</td>
<td>D174usOA1, NERO</td>
<td>RPOT, S</td>
<td>1STC,40,100,OXID,1</td>
</tr>
</tbody>
</table>

By taking into consideration the stratigraphic sequence and dating evidence a phased dating file is produced, Fig 8. This example is presented in ASCII order of context number but can be sorted on any field - perhaps by date, or by features such as ditches (D prefix) or buildings (B prefix).

Fig 8: Stratigraphic, rather than pottery, dated file

<table>
<thead>
<tr>
<th>Context Group</th>
<th>Phase/Interp Code</th>
<th>Interp</th>
<th>Stratdate</th>
<th>Comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>1,</td>
<td>4.2,</td>
<td>-</td>
<td>LFTR,</td>
<td></td>
</tr>
<tr>
<td>10,</td>
<td>2.1,</td>
<td>?EsOA2,</td>
<td>NERO,</td>
<td></td>
</tr>
<tr>
<td>100,</td>
<td>4.2,</td>
<td>?Co,</td>
<td>LFTR,</td>
<td></td>
</tr>
<tr>
<td>101,</td>
<td>2.3,</td>
<td>EdOA4,</td>
<td>LNFL,</td>
<td>COIN &lt;4&gt; ILLEGIBLE</td>
</tr>
<tr>
<td>104,</td>
<td>2.2A,</td>
<td>B1r4us</td>
<td>NERO</td>
<td></td>
</tr>
<tr>
<td>114,</td>
<td>2.3,</td>
<td>EdOA4,</td>
<td>LNFL,</td>
<td>COIN &lt;1&gt; CLAUDIUS I MID 1STC</td>
</tr>
<tr>
<td>189,</td>
<td>2.1,</td>
<td>EdOA3,</td>
<td>NERO,</td>
<td>SAM _ DR27 PASSIUSUS LG 50-70</td>
</tr>
</tbody>
</table>
The dating file can be readily joined with data from other disciplines, such as building material (Fig 9) - which can elucidate the status of buildings - but also with small finds and environmental data, giving a rounded interpretation of a site. By liaising with specialists in this way a total picture is built up - each specialist communicating with one another to give a rapid assessment of the site instead of working in isolation as many are still forced to do. Ideas are generated and archaeological interpretation progresses. For example an investigation of iron trituration grits on mortaria was a direct result of communication between a specialist in small finds, in this case industrial evidence, and the Roman pottery department.

Fig 9: Building material data joined with phased dating file

(First fields see Fig 8)  BM Period  Fabric Number  Type  Corners  GRAM  Comments
305.1.2.E0A2.LNBF,  RCBM,  2452,  TEG,  0,  300,  
305.1.2.E0A2.LNBF,  RCBM,  2454,  BRIC,  0,  1200,  
305.1.2.E0A2.LNBF,  RCBM,  2454,  IMB,  0,  100,  
305.1.2.E0A2.LNBF,  RCBM,  2454,  TEG,  0,  300,  
282.1.4.E0A3.LNBF,  -,  -,  -,  0,  200,  MORTAR SAMP  
282.1.4.E0A3.LNBF,  -,  -,  MUDB,  0,  200,  
282.1.4.E0A3.LNBF,  RCBM,  2452,  -,  0,  2600,  ? FORM  
282.1.4.E0A3.LNBF,  RCBM,  2452,  IMB,  0,  400  

Turning to synthetic projects, the basic pottery archive can be used to build up a spatial chronology of an area, in this case the City of London east of the Walbrook river, within discrete spans of time. By plotting the incidence of pre-Flavian pottery (Fig 10) found on different sites we can begin to build up a picture of areas of early occupation which is contrasted with the evidence of Flavian wares (Fig 11) showing the expansion of the settlement at this time. With CAD systems the generation of such diagrams can be virtually instantaneous.

Fig 10: Sites East of the Walbrook River, London in the pre-Flavian period
Fig 11: Sites East of the Walbrook River, London in the Flavian period

Part of the aim of a larger study of the ceramics from the pre-basilica buildings at Leadenhall Court with my colleague, Jo Groves, was to place the site within the context of Flavian London. This had to be written to Level IV stage within six working weeks. The survey encompassed over 50 phased and dated sites and investigated the boundaries of the early settlement of Londinium, broad topographic and socio-economic patterns of the Flavian settlement, and the 'status' of individual buildings. The first two elements were achieved using the basic archive as qualitative data, counting each line of spot-dated data as a single occurrence and then expressing it as a percentage of the total count. The third was based on quantitative data, using statistically derived data based on percentages of EVEs and weight in grammes.

The use of spot-dates as qualitative data is more complex. It relies on a sound, but not inflexible, pottery chronology and typology. It is a reflection of, but not a substitute for, a ceramic model postulated through the analysis of quantified assemblages from well-stratified sequences.

Fig 12: Preliminary comparison of qualitative data with quantified data of the same date.

12A: Building 6 use 2 (120-140/160 AD) - Qualitative data as a percentage

<table>
<thead>
<tr>
<th>NO OF RECORDS</th>
<th>%</th>
<th>FAB GROUP SOURCE</th>
<th>FAB/FORM</th>
</tr>
</thead>
<tbody>
<tr>
<td>3</td>
<td>6.67%</td>
<td>AMPH,IMP,MED,</td>
<td>AMPH,</td>
</tr>
<tr>
<td>2</td>
<td>4.44%</td>
<td>AMPH,IMP,SGAUL,</td>
<td>PE47,</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>AMPH,IMP,SPAN,</td>
<td>C186,</td>
</tr>
<tr>
<td>2</td>
<td>4.44%</td>
<td>AMPH,IMP,SPAN,</td>
<td>DR20,</td>
</tr>
<tr>
<td></td>
<td>17.77%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>FINE,RB,-</td>
<td>HWC,JII</td>
</tr>
<tr>
<td>2</td>
<td>4.44%</td>
<td>FINE,RB,-</td>
<td>SAND,V *RODWELL</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>FINE,RB,-</td>
<td>LOMI,IV (Contd.)</td>
</tr>
</tbody>
</table>

35
### NO OF RECORDS % FAB GROUP SOURCE FAB/FORM

<table>
<thead>
<tr>
<th></th>
<th>2.22%</th>
<th>FINE,RB,-</th>
<th>LOMI,V ABR</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>11.10%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>OXID,RB,-</td>
<td>HOO,</td>
</tr>
<tr>
<td>3</td>
<td>6.67%</td>
<td>OXID,RB,-</td>
<td>OXID,</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>OXID,RB,-</td>
<td>OXID,IIA</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>OXID,RB,-</td>
<td>VCWS,</td>
</tr>
<tr>
<td>4</td>
<td>8.89%</td>
<td>OXID,RB,-</td>
<td>VRW,</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>OXID,RB,-</td>
<td>VRW,*ODD AMPH?</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>OXID,RB,-</td>
<td>VRW,I</td>
</tr>
<tr>
<td>---</td>
<td>------</td>
<td>----------</td>
<td>--------------</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>AHSU,II</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>BB1,ERJ</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>BB2,IV</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>GROG,II STB HM</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>HWC+,IIF</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>HWC,II</td>
</tr>
<tr>
<td>3</td>
<td>6.67%</td>
<td>REDU,RB,-</td>
<td>HWC11E</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>HWC,IIF</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>HWC,NJ</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>NKS,9</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>REDU,RB,-</td>
<td>SAND,</td>
</tr>
<tr>
<td>2</td>
<td>4.44%</td>
<td>REDU,RB,-</td>
<td>SHEL,II</td>
</tr>
<tr>
<td>---</td>
<td>------</td>
<td>----------</td>
<td>--------------</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>SAM,IMP,Gaul</td>
<td>SAM,DR35/6</td>
</tr>
<tr>
<td>1</td>
<td>2.22%</td>
<td>SAM,IMP,Gaul</td>
<td>SAM,DR37</td>
</tr>
<tr>
<td>2</td>
<td>4.44%</td>
<td>SAM,IMP,Gaul</td>
<td>SAM,JV</td>
</tr>
</tbody>
</table>

|   | 99.94% | TOTAL   |

12B: General Post Office site Phase F (120-140 AD) - quantified data

<table>
<thead>
<tr>
<th>EVE %</th>
<th>FAB GROUP</th>
<th>SOURCE</th>
</tr>
</thead>
<tbody>
<tr>
<td>5</td>
<td>0.08%</td>
<td>AMPH,IMP, MED</td>
</tr>
<tr>
<td>23</td>
<td>3.66%</td>
<td>AMPH,IMP, SPAN</td>
</tr>
<tr>
<td>4</td>
<td>0.64%</td>
<td>FINE,IMP, CGAUL</td>
</tr>
<tr>
<td>76</td>
<td>12.58%</td>
<td>FINE, RB,-</td>
</tr>
<tr>
<td>171</td>
<td>27.23%</td>
<td>OXID, RB,-</td>
</tr>
<tr>
<td>294</td>
<td>46.82%</td>
<td>REDU, RB,-</td>
</tr>
<tr>
<td>51</td>
<td>8.12%</td>
<td>SAM, IMP, GAUL</td>
</tr>
<tr>
<td>1</td>
<td>0.16%</td>
<td>SAM, IMP, SGAUL</td>
</tr>
</tbody>
</table>

|   | 625 | 100.00% | TOTAL |

I had experimented with using spot dates as qualitative data five years ago (Fig 12A). The individual fabrics were condensed within functional groups (Amphorae - AMPH, oxidised - OXID, reduced - REDU, fine - FINE and samian wares - SAM), and compared with quantified data of the same date from a different site (Fig 12B). Apart from the amphorae (with which there is a specific problem of rim survival versus heavy body sherds) there is some similarity when the qualitative percentages are compared with the EVEs from the quantified data. However, in this example the presence or absence of just one form could significantly distort the statistics. The sample size is clearly too small.

In order to concentrate on the positive aspects of using spot dates as qualitative data it is prudent to discuss any problematical elements at this point. Several factors have to be taken into consideration:
a) The difference in site volumes could create a bias.
b) Where make-up dumps or any secondary material is concerned the source of the pottery is uncertain.
c) It is not viable to use spot dates across multiple periods of time because of the differential in the numbers of fabrics identified within each period, for example the numerous first century fabrics compared with the relatively small amount of fourth century fabrics (Alex Bayliss DUA Archive 'Potdate' discusses the problems and suggests some statistical solutions).
Fig 13: comparison of qualitative data from 50 spot-dated sites across the City of London with quantified data from Roman Ceramic phases (R1b and R2 combined c. AD 60-100) derived from 10 of these sites.

13A: c AD 60-100: quantified data

<table>
<thead>
<tr>
<th>Fabric</th>
<th>EVE</th>
<th>%</th>
<th>Grammes</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>AMPH</td>
<td>608</td>
<td>1.58</td>
<td>193927</td>
<td>38.13</td>
</tr>
<tr>
<td>FINE</td>
<td>4166</td>
<td>10.86</td>
<td>14312</td>
<td>2.81</td>
</tr>
<tr>
<td>OXID</td>
<td>8778</td>
<td>22.87</td>
<td>109826</td>
<td>21.59</td>
</tr>
<tr>
<td>REDU</td>
<td>19916</td>
<td>51.9</td>
<td>171745</td>
<td>33.77</td>
</tr>
<tr>
<td>SAM</td>
<td>4908</td>
<td>12.79</td>
<td>18809</td>
<td>3.70</td>
</tr>
<tr>
<td>TOTAL</td>
<td>38376</td>
<td>100.00</td>
<td>508619</td>
<td>100.00</td>
</tr>
</tbody>
</table>

13B: c AD 60-100: qualitative data

<table>
<thead>
<tr>
<th>Fabric</th>
<th>No of records</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>AMPH</td>
<td>2062</td>
<td>11</td>
</tr>
<tr>
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<tr>
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<tr>
<td>SAM</td>
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<tr>
<td>TOTAL</td>
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The sample size used in the Leadenhall Court project was more viable, consisting of some 18,023 records from phased and dated contemporary sites of the period AD 60-100 (Fig 13A). Comparison of the combined quantified data collated for the study of early Roman pottery (Davies & Richardson, forthcoming CBA) for Roman ceramic phases R1b and R2 dated AD 60-100 compares well with the qualitative data (Fig 13B). When the totals for the main fabric groups are compared in terms of EVEs from the quantified data and the number of records expressed as a % in the qualitative data there is very little difference between the two sets of figures. The apparent discrepancy for the amphorae can be explained by the inclusion within the qualitative data of two sites from the waterfront area which produced very high proportions of amphorae and, within the quantified data, the ratio of rim survival (EVE) in comparison with the amount of body sherds present (grammes).

Although the distribution of amphorae should be treated with discretion it seems reasonable to suggest that both sets of figures represent a ‘mean’ with which individual sites can be compared.

Fig 14: Distribution of the principal fabric groups across the City in broad topographic areas (Qualitative data: + or - indicates to within 3% of the ‘Mean’)

<table>
<thead>
<tr>
<th>Site/area</th>
<th>Amph+oxid imports %</th>
<th>Sam+fine imports %</th>
<th>RB Fine %</th>
<th>RB Oxid %</th>
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<tr>
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</tr>
<tr>
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<td>16</td>
<td>15 +</td>
<td>16 -</td>
<td>44 +</td>
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<th>RB Fine</th>
<th>RB Oxid</th>
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<td>23</td>
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<td>9</td>
<td>11</td>
<td>14 +</td>
<td>30 +</td>
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I used qualitative data to plot the distribution of the principal fabric groups across the City within broad topographic areas to present a broad outline of the topographic and socio-economic patterns for the period c AD 60 - 100 (Fig 14). Imported wares consisting of amphorae, seals and continental oxidised wares were extracted and presented graphically (Fig 15). The diagram suggests that:

"the greatest quantity of imported wares are present in the area of the port (FMO & PDN). Some of the material is obviously backfilling for the Flavian waterfront (PDN) and possible make-up dumps (FMO). In these cases the question of rubbish disposal and the use of amphorae as 'hardcore' must be addressed. Even so, it is reasonable to suggest that a number of these wares arrived via the port and it is likely that some were broken on arrival or that they were either sold or redistributed from the waterfront warehouses. The likeliest route would have been along the adjacent road towards the forum area, and it is worth noting that the majority of the sites with above average amounts of these vessels are concentrated either around the forum area (BRL, CLE, FEN, LIM) or to the south in close proximity to the port (EST, WTT). Sites with average or below average amounts cluster to the north of the forum (LCT, LLO, WTV, BOP, CNL) or on the periphery (BIS, GPO, PUB & RAG).

Although no more than a glimpse of the potential for a more detailed study, a broad examination of the distribution of high status imported wares - samian and other continental fine wares - arriving in London from c AD 60-100 reflects the structural evidence (see Davies & Groves forthcoming for an explanation of status models). On the whole, the results show that sites of probable differing status coexist but with some topographic and socio-economic patterns emerging." ... "None of the sites show evidence of very high status, which is surprising in view of the pre-eminence of the municipium. The majority cluster towards average or above average status which would be expected in a town of Londinium’s stature. Equally, none of the sites appear to be of very low status, although the more peripheral sites and those in the northern sector appear to be of relatively lower status in comparison with those situated towards the centre and along the main east-west roads. However, compared with assemblages of the same date from Chelmsford, a typical Romano-British 'small-town', even the lowest status sites of London contain a far higher proportion of all types of imported wares (Goings, C. 1987 The Mansio and other sites in the south-eastern sector of Caesarea Magus: the Roman Pottery Chelmsford Archaeological Trust CBA Research report 62, p117)."
Pottery alone yielded this information. When other material, such as ecofacts, industrial waste, glass and metal objects etc. are included we can begin to interpret and understand the archaeological evidence appertaining to a particular period of time or a particular topographic area through an integrated approach.

Fig 15: Spatial distribution of amphorae, seals and imported oxidised wares across the City of London c AD 60-100
Reinterpretation: Thoughts from the Backlog

by Kate Steane

During the past four years (with several more years to run) the City of Lincoln Archaeology Unit post-exca-avation team has been working on a backlog of more than 40 sites, all excavated in Lincoln between 1972 and 1987. This means dealing with sites which most of us have never seen, so we are distanced from the close focus of excavation. However, as a team we have accumulated a working knowledge of a wide range of sites all over the city, leading to a new understanding of the stratigraphy.

Interpretation

Archaeologists are expected, even during excavation, to present a clear chronological sequence for each site to the onlooking public. In Lincoln, annual reports produced from 1973 onwards, available to the general public, developer and academic alike, provide initial interpretations of the results of each site excavated. This usually precedes any extensive post-excavation work but is useful as a quick reference to the sites. In Lincoln’s Buried Archaeological Heritage (1990), a publication inspired by Museum of London DUA Archive Catalogue (1987), brief summaries of all the sites excavated between 1972 and 1990 were largely based on the original interpretations, as published in the annual reports.

But transforming a sequence of layers, cuts and features into a site narrative has many pitfalls. There are missing variables in the stratigraphy; truncation removes part of the evidence as does the decay of the strata in situ. We are all human and as such we selectively filter the information; working through all the processes of recording, initial on-site interpretation, post-excavation analysis and publication can change our perceptions on

![Diagram](attachment:image.png)

**Fig 1:** A network of wattle fences in the mid 10th century had bowed out as a result of the still flowing channel (St Benedict’s 1985)
Fig 2: St Paul-in-the-Bail: the Roman forum, the apsidal building and the single cell building with the cist burial

Interpretation. Both working through the stratigraphic archive and integration of the stratigraphy with the finds can lead to fresh insights into the formation of a sequence. It is only recently, in the last four years, that Lincoln has fully enjoyed the benefits of an integrated computer system; this, more than anything else, has facilitated stratigraphic analysis.

Reinterpretation
Post-excavation work in Lincoln has provided many examples of the dynamic nature of site interpretation. A few of these are discussed below.

One of the key elements in reinterpretation is the context descriptions. During excavation at St Benedict's in 1985 one feature was interpreted as a Roman drain to the River Witham which silted up and went out of use in the post-Roman period. During further analysis (examining sections and context descriptions of the silt layers) it was possible to see that, although the drain silted up, the water did not stop running; it merely followed a wider channel and continued into the late Saxon period. The flow of the channel, together with that of the river, had effected the collapse of a network of late Saxon wattles (Fig 1). Its continued existence also explained the later lack of structural features in the middle of the site.

In Lincoln the reassessment of the entire medieval pottery sequence has necessitated revision of the chronological sequence of a number of sites already published. One such site is Flaxengate (1972), particularly the stone phases (Jones R H 1980); some of the context records for this site gave only limited information regarding relationships, which led to the drawing of a matrix where grouped contexts 'floated', ie they did not interconnect horizontally. This ultimately meant that stratigraphic relationships had little influence in dictating the interpretation of the sequence across the site; pottery dating had to take the lead in linking one part of the site with another. With the recent changes in pottery interpretation the framework of the matrix has had to shift vertically either up or down.

St Mark's church in Lincoln (Gilmour and Stocker 1986) is included in the current post-excavation programme (because the earlier part of the
site has yet to be published). This has led to several reinterpretations of the structural sequence, sometimes due to taking a fresh look at the finds. An example of this is an ashy layer with lead waste, interpreted on site as fire debris; recent analysis by one of our finds officers, Jane Cowgill, has shown that this layer is more likely to be part of a programme of refurbishment, leaving fragments of window cames within an ashy occupation layer.

Partly as a result of our reinterpretations of the backlog sites Lincoln's Buried Archaeological Heritage has been rewritten and will soon be available under its new name of Lincoln's Recorded Archaeology.

Alternative interpretations
It is not always possible to reach a definitive interpretation. The stratigraphy can be truncated and reworked with little or no associated pottery as happened at St Paul-in-the-Bail between the late Roman and Late Saxon periods. A timber apsidal building (church) was succeeded by a graveyard; this was then cut by a single cell structure (the core of the late Saxon church). A cist burial which held a 7th century hanging bowl ‘floated’ between all three events; there were no evident relationships. All these activities occurred in a central location with regard the earlier forum (Fig 2). Radiocarbon dates taken from inhumations sandwiched between the two buildings suggested the possibility of a Roman date for the apsidal building, although their standard deviation would also allow the inhumations to be Middle Saxon. There are many possible permutations of the St Paul-in-the-Bail sequence which is very tantalising, giving no clear-cut interpretations; Andrew Selkirk (1992) has examined these options creatively.

Conclusions
Often during excavation an interpretation is outlined and it is difficult to reassess the evidence in a fresh light. Post-exavcation analysis in Lincoln has concentrated on the original record (particularly context sheets, plans, sections and slides), with an emphasis on integration with pottery and finds evidence. It may be possible to build up a history of the successive reinterpretations for many of Lincoln’s sites. However, it is only relatively recently, with the advantages of computer technology, that we have been able to fine-tune the reinterpretation process, melding the stratigraphy with the finds, to reach an integrated understanding of the site.

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Vince, A G & Jones, M J 1990 Lincoln’s Buried Archaeological Heritage, City of Lincoln Archaeology Unit