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Across the generations: a long-term computing project

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Abstract

Most archaeological computer-aided work falls into one of two categories: a one-off project of relatively short duration with a quite definite termination (most excavations, for instance) or a longer-term enterprise such as the creation of an archive intended for use into the indefinite future. The work described here is somewhat unusual, in that it was indeed a 'closed' operation concerning a single site, while lasting for what may be taken as a very long period within the short history of electronic computing. Consequently the equipment on which it was based was twice replaced; it was necessary to contend not only with changes in software but in the perceived capabilities and role of computers in academic life and society at large. Of course many of the problems could be tackled differently today . . they often cropped up a year or two before an appropriate means of solving them became available through commercial or other sources, so things had to be done the hard way (I suspect this is a common enough experience). If many of the nuts and bolts of the operation can be consigned to archaeology's past, the wider issues which had to be faced are recurrent ones.

32.1 Introduction

The Old Stone Age site of La Cotte de St. Brelade is situated on the southwest coast of Jersey, Channel Is., and consists of a deep T-shaped ravine system cutting through a small granite headland. The ravine filling extends back in time some quarter of a million years. Worked flints were discovered there in 1881, and since then extensive excavations have taken place—the only lengthy breaks in the program of work were 1919–36 and 1940–50. From 1961–78 Dr. (later Professor) Charles McBurney of the University of Cambridge continued the excavations at the request of the site's owners, La Société Jersiaise, the final stages of fieldwork being carried out by me in 1980–2 after McBurney's death. A substantial report[1] on the investigations has now been published (Callow & Cornford 1986).

Archaeologically, La Cotte is extraordinarily rich. McBurney's excavations alone yielded over 100,000 stone artefacts, effectively doubling the corpus of Palaeolithic

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material recorded from the British Isles (compare Roe 1968).[2] It is also exceptionally complicated in stratigraphic terms. Because it is open to the sky and rain, its deposits are susceptible to erosion by runoff and to post-depositional chemical alteration by percolating water (this sometimes resulted in changes of colour, and even of texture, which make it difficult to recognise stratigraphic boundaries). Moreover, during the last interglacial, around 125,000 years ago, marine erosion removed a substantial part of the earlier deposits, creating a cliff c. 10 m high which was later buried under a further 30 m of sediments (Fig. 32.1). The total thickness of the stratigraphic accumulation and the site's sensitivity to climatic change over a period of about a quarter of a million years add to La Cotte's importance, but create considerable theoretical, practical and logistical problems in its study. One of the project's hallmarks, therefore, was the formation of an unusually large and varied group of researchers from both sides of the English Channel; one of its most time-consuming aspects, from my point of view, was the organisation of this group and distribution of information among its members.

The sheer quantity of finds and the need to retain as much flexibility as possible in the face of developing evidence and shifting ideas posed enormous problems for the routine archaeological methodology of the 1960s and early 1970s. McBurney was a man of considerable foresight, however, recognising that by harnessing the power of computers it might be possible to get round these. As early as 1968 he attempted to find a student to take on, as a doctoral research topic, the registration and analysis of finds and stratigraphic data with the aid of the University of Cambridge's prototype Atlas II machine, Titan. In this he was at first unsuccessful, though Titan was used for secondary statistical analysis of data tabulated and summarised by more traditional methods[3] (McBurney & Callow 1971). I myself began work on a Ph.D. centred on the La Cotte artefacts in 1970, but was reluctantly forced to abandon this topic for another at the beginning of 1973; the excavation techniques in use during the earlier 1960s had proved incapable of coping with the site's stratigraphic complexity and new samples would have to be obtained. Shortly before his death in 1979, I was asked by McBurney to take over the completion of the research for publication as he was already too ill to see it through.

It must be appreciated that McBurney himself had no great personal familiarity with computers and computing. This undoubtedly led to problems in reconciling the techniques of excavation and recording with efficient data processing and analytical methods—in particular, he was over-optimistic regarding the ease with which difficult stratigraphic questions might be resolved after rather than during excavation. Despite this, his early recognition of the possibilities of electronic processing, and the considerable start he made in cataloguing the artefacts, laid the foundations on which the subsequent work rested.

One further point which may have to emphasised in today's working environment is that almost all the work was done on mainframe equipment (Titan was replaced by an IBM 370, and that in turn by an IBM 3081). There were several reasons for this:

- The computing connected with the project was well under way during the 1970s

[2] This statement is admittedly a little misleading. Through the accidents of history La Cotte is British, yet geographically it is clearly French; moreover it has to be seen as one of the greatest of European rock-shelter sequences, rather than as typical of a marginal area of settlement during the Pleistocene.

[3] H. and M. Jarman seem to have been the first in Cambridge to use the computer for large-scale processing of finds, from Knossos, in 1970.

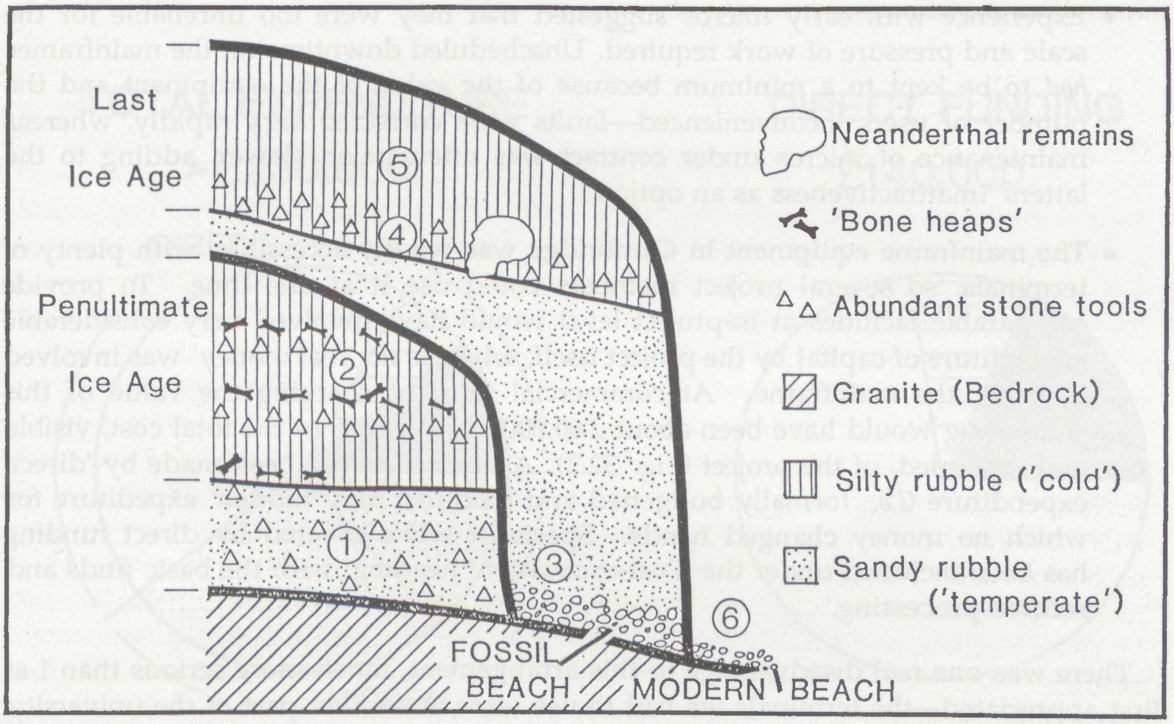

Figure 32.1: A highly schematic longitudinal section through the ravines at La Cotte de St. Brelade.

1. Interglacial deposits with dense early Middle Palaeolithic occupation, TL age 238±35 thousand years;
2. cold phase with intermittent occupation and mammoth butchery;
3. last interglacial marine erosion and beach (dated elsewhere to c. 125 thousand years);
4. early last cold phase, with late Middle Palaeolithic occupation and Neanderthal teeth;
5. late last cold phase;
6. recent marine erosion. The Cambridge excavations were chiefly concerned with 1–3. Depth coordinates referred to in this paper are measured from the top of the deposits at c. 40 m amsl.

(though the quantities of data involved were relatively small until about 1977); the analytical work described in the publication was almost entirely completed by 1985, with typesetting etc. continuing in 1986). Initially, micro-computers simply did not exist, and it was only very late in the work that desktop machines capable of rapid handling of multi-megabyte databases became attainable.

- Experience with early micros suggested that they were too unreliable for the scale and pressure of work required. Unscheduled downtime on the mainframes *had* to be kept to a minimum because of the value of the equipment and the number of users inconvenienced—faults were corrected very rapidly, whereas maintenance of micros under contract was often much slower, adding to the latters' unattractiveness as an option.
- The mainframe equipment in Cambridge was readily accessible, with plenty of terminals, so several project members could use it at one time. To provide comparable facilities at in-project level would have involved very considerable expenditure of capital by the project itself, whereas no 'real money' was involved in using the mainframe. At commercial rates of charging the value of this computing would have been about £60,000—about 25% of the total cost, visible and concealed, of the project (Fig. 32.2). A distinction has been made by 'direct' expenditure (*i.e.* formally budgetted and paid-for) and 'hidden' expediture for which no money changed hands. Specialist work covered by direct funding has been included under the 'Post-excavation' heading, with the basic finds and records processing.

There was one real disadvantage to this arrangement, much more serious than I at first appreciated—the terminals we had to use were in another part of the university. Since from 1979 onwards the great majority of the computer work (apart from data and text entry and editing) was done by me, this sometimes took me away from the laboratory for a substantial proportion of the day. With hindsight, this probably allowed some managerial problems to develop further than would otherwise have been the case, and it would have been better to have attempted to raise (the considerable) extra funds to install our own lines and terminals. But at the time when this should have been done, the scale and duration of the task I faced was not fully apparent; I was at first still influenced by McBurney's estimates of the work outstanding and the resources needed to carry it out.[4]

32.2 The role of the computer in the La Cotte project

This topic is discussed here under the following headings:

- Stratigraphy and provenance
- Cataloguing

[4] As a result of which there were more urgent items for which funds had to be found anyway. At the time of my return to Cambridge in the summer of 1979 I found that no money existed or had yet been sought for supporting staff beyond the beginning of 1980, nor for consumables and other recurrent costs apart from a small component of Science Research Council grants earmarked for particular areas of research.

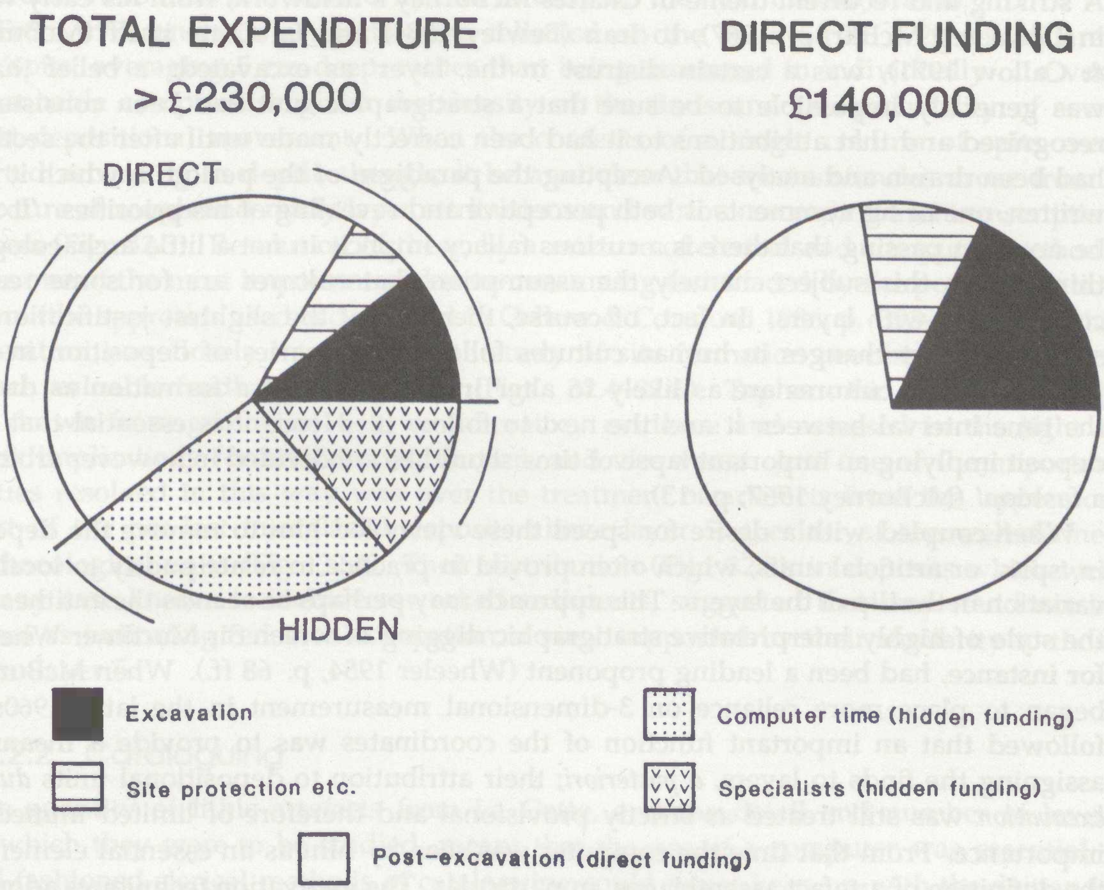

Figure 32.2: Breakdown of expenditure for the La Cotte project.

- Artefact studies
- Environmental studies and dating

Inevitably, reality did not correspond very exactly to such a tidy partition.

32.2.1 Stratigraphy and provenance

A striking and recurrent theme in Charles McBurney's fieldwork, from his early work in Libya (eg McBurney 1967), to Iran (Bewley 1984) and La Cotte itself (McBurney & Callow 1971), was a certain distrust in the 'layer' as excavated: a belief that it was generally impossible to be sure that a stratigraphic unit had been consistently recognised and that attributions to it had been correctly made until after the sections had been drawn and analysed. Accepting the paradigm of the period in which it was written, one of his comments is both perceptive and revealing of his priorities. 'It may be noted in passing that there is a curious fallacy implicit in not a little archaeological thinking on this subject, namely the assumption that cultures are for some reason coterminous with layers. In fact, of course, there is not the slightest justification for supposing that changes in human cultures follow the vagaries of deposition in any particular site; cultures are as likely to alter in the course of a formation as during the time interval between it and the next to follow it. Hence it is essential that any deposit implying an important lapse of time should be subdivided in however arbitrary a fashion' (McBurney 1967, p. 13).

When coupled with a desire for speed these views led him to remove the deposits in 'spits' or artificial units, which often proved in practice to relate poorly to localised variation in the dip of the layers. This approach may perhaps be seen as the antithesis of the style of highly interpretative stratigraphic digging of which Sir Mortimer Wheeler, for instance, had been a leading proponent (Wheeler 1954, p. 68 ff.). When McBurney began to place more reliance on 3-dimensional measurement in the later 1960s, it followed that an important function of the coordinates was to provide a means of assigning the finds to layers, *a posteriori*; their attribution to depositional units *during excavation* was still treated as strictly provisional and therefore of limited immediate importance. From that time the computer was seen by him as an essential element in the definition of artefact assemblages, in particular. The excavation techniques adopted ensured that this had indeed to be the case—without considerable number-crunching the uncertainties and approximations could never begin to be resolved. On the other hand, post-excavation attempts to use the Harris matrix approach (Harris 1979) in an analysis of the stratigraphy had to be abandoned because of the numerous inconsistencies and uncertainties in closely related sections drawn over a period of many years, by many different observers. For the same reasons, attempts at computerised section drawing were not pursued after initial experiments; to have developed software capable of dealing with so many 'ifs' and 'buts', soon enough to be useful, was beyond available resources.

In 18 years of excavation a wide range of field recording methods was tried in an attempt to find a system capable of dealing with the extremely variable conditions at La Cotte. A final and permanent reference datum was only established in 1972; indeed during exposure of a small area of a single layer, in 1969, two unrelated triangulation and a cartesian system of measurement had been in use. Reduction of provenances obtained by different methods to a single 3-dimensional coordinate system was soon recognised as an urgent task. McBurney therefore experimented with artefact distribution plans plotted by hand from the data in the field inventories. A D-MAC digitising table was used with these to transfer coordinates onto paper tape for subsequent transformation via a simple computer program. After considerable expenditure this policy was abandoned when it was appreciated that traditional hand plotting was both inaccurate and redundant; instead, rather more elaborate software was written to perform validation and transformation directly on the field measurements.

From 1974 onwards artefacts were collected on the basis of 20 × 20 cm squares, in 'spits' averaging 5 cm deep, rather than being measured in individually ...a very reasonable compromise given the density of the finds and the probability of some post-depositional movement. When the contents of a single column of spits were sorted according to depth, though, it became clear that inconsistencies arose from a mixture of rough-and-ready layer attributions and various recording and transcription errors (Fig. 32.3). Resolution of these, by a mixture of clerical and computer methods, was one of the most important and time-consuming tasks faced by the post-excavation team (the approach used is described in Callow & Cornford 1986, p. 393–5). In fact this operation was closely integrated with study of site formation processes, from which much valuable feedback was gained (Figs. 32.4–32.6). Taphonomic inferences could be drawn from geological field or laboratory evidence and assessed in the light of their implications for artefact distribution, and vice versa. Just one of the important issues resolved in this way was over the treatment of artefacts from the uppermost part of the pre-last interglacial deposits above layer 5, the last of the well-defined archaeological units (Fig. 32.4). Their distribution (Fig. 32.5), in deposits which were in any case known to include reworked sediments, suggested that they could not be taken as indicating renewed occupation, a view supported by their similarity to those from layer 5.

32.2.2 Cataloguing

The quantity of lithic artefacts from La Cotte, and the detail and number of levels at which they were to be studied, meant that the use of a computer was essential—old-fashioned clerical methods of cataloguing could never keep up with the demand.[5] In the non-publication of some of the richest and most important Palaeolithic sites excavated during the past generation, logistical problems directly arising from the ever more complex questions asked of them appear to have played an important part (Callow 1985, p. 95). But our cataloguing needs were far wider than this, taking in faunal remains, sediment samples, section drawings and plans, information from site notebooks, photographs ...on my return to Cambridge in 1979 the list of possibilities and necessities seemed endless, and in practice not all could be achieved in time to do most good.

Creation of a master artefact catalogue involved the inclusion of some basic observations made in the laboratory: raw material type, a simple typological and technological classification, size measurements and weight. These observations are separable on the

[5]The final three years of excavation, better funded than before and in very rich deposits, produced over three quarters of the studied artefacts. This imposed an immense strain on the existing post-excavation system.

Layer		Depth	Comment
Field	Revised	(cm)	
	A		
A	A	*2525*	
A	A	*2533*	
H	A	*2540*	— Poorly written bag label (actually layer A)
A	A	*2544*	
B	A	*2549*	— Datum error at beginning of new season of excavation (actually 2544)
A	B	*2558*	
A	B	*2565*	— Erroneous layer attribution (change of excavator)
B	B	*2575*	
B	B	*2581*	
C	B	*2590*	Layer redefined from later drawing of adjacent section
C	C	*2594*	
C	C	*2610*	
A		*2621*	— Recorded 1 metre too deep
C		*2628*	— Measured from east side of square (actually 190 E)
D	D	*2628*	
D	D	*2635*	
D	D	*2640*	
E	E	*2648*	

Figure 32.3: Some typical provenance recording errors encountered (taking a fictional case).

The data for some of the spits from a single 20 × 20 cm square, centred on coordinates (110 East, 30 South) have been sorted by depth; the layer attributions are shown before and after correction. Errors like those at depths 2549 and 2565 are particularly damaging, as before correction they raise doubts about their neighbours.

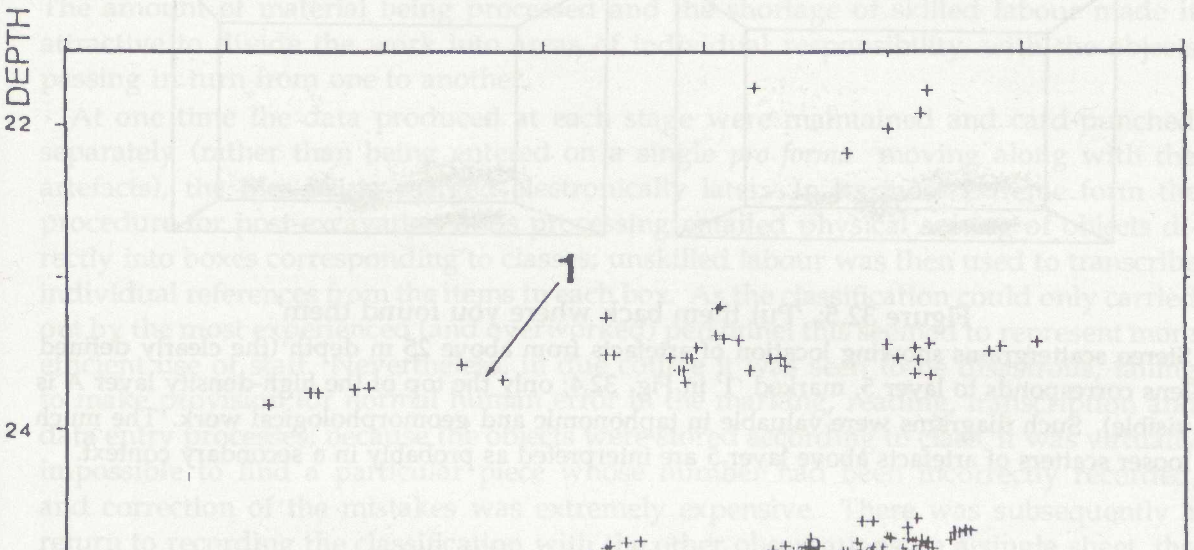

Figure 32.4: Projection of artefacts from a 10 cm 'slice' of the site onto a vertical plane, showing variation in density.
Note the steep dip of the lower layers (as much as 30 degrees). Key:

1. occupation layer 5, sandwiched in artefact-free loess; 2
2. 'void' corresponding to large sediment sample (layer A);
3. intercalated loess (layer B) with low artefact density;
4. voids corresponding to large boulders (layer H).

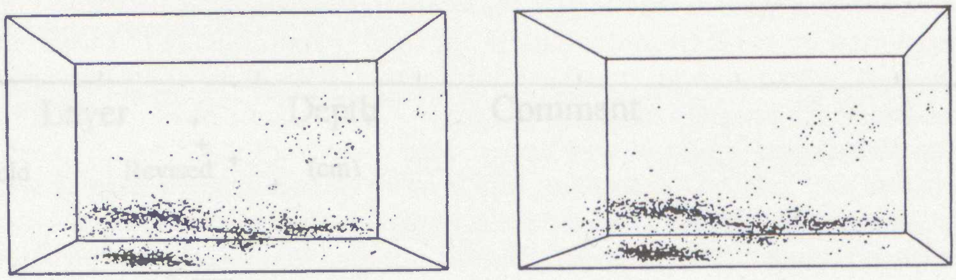

Figure 32.5: 'Put them back where you found them'.

Stereo scattergrams showing location of artefacts from above 25 m depth (the clearly defined lens corresponds to layer 5, marked '1' in Fig. 32.4; only the top of the high-density layer A is visible). Such diagrams were valuable in taphonomic and geomorphological work. The much looser scatters of artefacts above layer 5 are interpreted as probably in a secondary context.

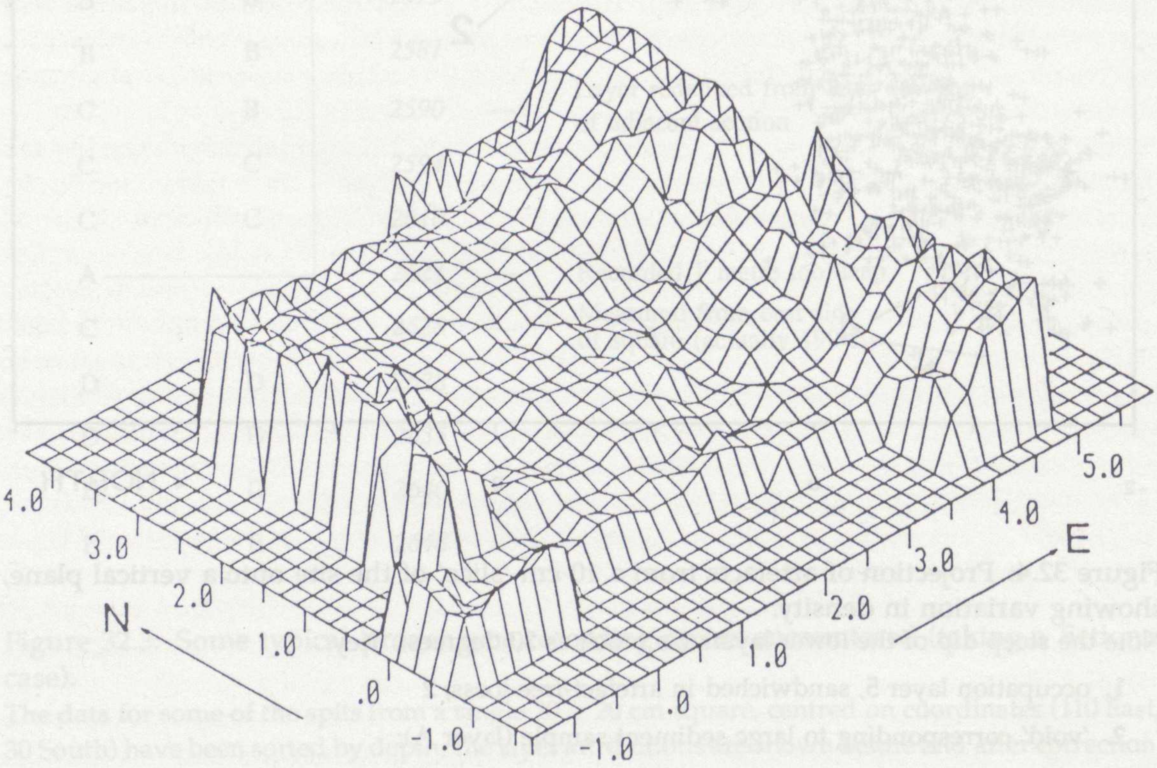

Figure 32.6: Isometric surface plot based on 'cleaned-up' artefact provenances from the layer A/B interface. Diagrams of this type were used both in the final stages of data validation and for studying erosion and other processes.

basis of the skill and equipment that they require—weighing requires a balance, but little operator experience, whereas artefact classification depends entirely on judgment. The amount of material being processed and the shortage of skilled labour made it attractive to divide the work into areas of individual responsibility, with the objects passing in turn from one to another.

At one time the data produced at each stage were maintained and card-punched separately (rather than being entered on a single *pro forma* moving along with the artefacts), the files being merged electronically later. In its most extreme form the procedure for post-excavation finds processing entailed physical sorting of objects directly into boxes corresponding to classes; unskilled labour was then used to transcribe individual references from the items in each box. As the classification could only carried out by the most experienced (and overworked) personnel this seemed to represent more efficient use of staff. Nevertheless, in due course it was seen to be disastrous, failing to make provision for normal human error in the marking, reading, transcription and data entry processes; because the objects were stored according to class, it was virtually impossible to find a particular piece whose number had been incorrectly recorded, and correction of the mistakes was extremely expensive. There was subsequently a return to recording the classification with the other observations on a single sheet, the objects being listed in the serial number order in which they were arranged on the trays passed round the laboratory. It is worth noting that the less satisfactory of these ways of working, involving the accumulation of data in separate files, was only made possible by the (correctly perceived) data management capabilities of the machine. *The lesson, once again, is that the new technology was accompanied by serious problems because its power was more clearly understood than were the discipline and planning that had to go with it* (Callow 1986).

The data was held in the form of SPSS (Nie *et al.* 1975) system files, suitable for direct statistical analysis. An alternative would have been to use software specifically designed for database management, selecting and transferring subsets of data for statistical work as required. But it was only in the final stages that the most detailed artefact data had to be incorporated and file structures became particularly complex, and by this time SPSSX was available with its more advanced facilities. Otherwise, data restructuring could be done either within SPSS itself, or with the aid of a fairly small suite of FORTRAN and other programs. This was not an ideal solution, above all because the software's front end was left in a partly developed, user-unfriendly state (and therefore unnecessarily labour-intensive), but as explained below there were undesirable constraints restricting the adoption of efficient working methods.

32.2.3 Artefact studies

Faced with so many artefacts, sampling was unavoidable; on the other hand it had to be carried out in a very controlled manner. Sampling percentages had to be calculated and adhered to so that the samples for each type of study were neither so large that effort was wasted, nor so small that results were unnecessarily inconclusive. To achieve this, a pseudo-random number was generated for each artefact. Once the required percentages had been ascertained for every category/assemblage combination, this variable provided the means of selecting repeatable samples. Lists were then produced of the pieces to be included (sorted to match the storage system, so that they could easily be found and extracted for study).

While the master catalogue contained a fairly cursory record of each object, it was also necessary to create a number of smaller files to accommodate further particulars about major categories (some of them sampled) such as waste flakes, cores, non-flint artefacts etc. Structurally, these were usually quite simple. It was really only with the retouched tools that large quantities of type-specific data existed in a single file; for this, SPSSX proved entirely adequate, even if its imposition of a rectangular structure with a lot of 'system-missing' values was rather expensive in terms of filespace. It provided the full range of statistical methods required, not only for working on the artefact database but for the further analysis of summary information about the assemblages. The little extra 'home-grown' software needed was mainly concerned with graphic output and with tidying-up tables for publication.

32.2.4 Environmental studies and dating

Analysis of faunal abundances etc. was very much as for artefacts. But two applications in connection with TL dating were more unusual. At one stage the viability of the dating programme itself was in doubt because of the immense (1200%) variation in soil radioactivity across the site; analysis of the measurements and sample locations was able to focus attention on an area in which the activity was consistent. Secondly, data compiled from field plans were employed in estimating the quantity of granite fragments contained in a sphere of sediment around each of the flints to be dated; too much granite and the date came out too old.

32.2.5 The publication process

Not surprisingly, the graphics facilities offered by the computer played an important part in the eventual publication. That is not to say that every drawing, or even a majority of them, could be produced in this way. For instance we are far from being able to reproduce flint artefacts automatically to an adequate standard, not least because of the extent (and, to be honest, the inconsistency) of the value-judgments required in interpreting and filtering the information provided by such objects. But it would have been wholly impractical to draw, by hand, illustrations such as Figs. 32.4–32.5, with thousands of points to be accurately plotted (particularly Fig. 32.4, in which artefact counts from 'spits' had to be converted into random points within the area covered by the spit). Again, computerised plotting offered the precision and speed required by stereo pair scattergrams, and cumulative frequency curves for granulometry and artefact typology, of which many were needed both for research and publication. Many of the tables were produced by batch editing of output from SPSSX (alas, the TABLES program for that package was released just a little too late for us).

Flexibility in text processing was of particular value in a project characterised by frequent and often dramatic revisions of interpretation, and consequently of plans for the structure of the site report. But the existence of a typescript stored on magnetic media really paid off at the publication stage. For some years it had been planned to submit it on magnetic tape from which typesetting could be done; some publishers approached, among the largest in the academic field, had proved very suspicious of this advanced (!) proposal. The tape was duly sent off, and pronounced entirely satisfactory by the typesetters. But fate intervened—the contracted publishers realised that the book was too large and complex for them to handle and be confident of a profit. An alternative company was found, and to keep costs down the typesetting was carried out on Oxford University Computing Service's Lasercomp (Marriott 1982). The existing embedded commands (for Cambridge's GCAL text processor) were converted to those required for the Lasercomp using the powerful editor available on the Cambridge mainframe, and the resulting files transmitted to Oxford through the Joint Academic Network. Return of the output to Cambridge, by post, was the non high-tech part of the operation. Regrettably, low cost proofing facilities became available for the Lasercomp just a few months too late for us ... the usual story!

32.3 The computer as stimulus (or the archaeologist as opportunist)

Leaving aside La Cotte's intrinsic importance for Palaeolithic archaeology, and for a wider context of Quaternary studies, for me one of the most striking aspects of the research project is the extent to which the use of computers made it feasible to pursue lines of research which would once have been too labour-intensive to be considered; another is that experimenting with the machine for one purpose sometimes opened up unforeseen possibilities in other directions.

Examples of the former are fairly obvious: heavy use of multivariate statistics, horizontal, vertical and 3-dimensional plots of artefact distribution, even the maintenance of an *analytical* (as opposed to retrieval) database of the size of our master artefact catalogue. Not infrequently the application of mere number-crunching techniques helped to pull together superficially disparate lines of thought. The relationship between raw material availability and raw material procurement strategies had been at the forefront of investigations since the 1960s. Yet it was only when multivariate techniques were applied to the lithic technology data that it became clear just how all-pervasive were the implications of this relationship (Fig. 32.7).

Serendipity, that happy and unlooked-for conjunction of opportunity with need, has always been part of the mechanism of progress. Thus in the La Cotte case, the extent to which the 3-D artefact distributions proved to give insights into geomorphological processes was totally unexpected; this in turn led to a better understanding of taphonomy, and the limitations and possibilities of working with artefact assemblages from so complex a site. For that matter, I had written the stereo scattergram program for plotting a few principal component scores as labelled points; it did not occur to me for some time that it would do just as well to give an impression of density for thousands of unlabelled points—though I soon learned not to do it with a pen plotter when the operators complained about holes in the paper. Speed of response, too, meant that 'brainstorming' sessions were possible. What happened if you lumped certain artefact types together? Was fine quartzite being distinguished from flint by the knapper? And so on. Such flexibility did not exist when McBurney started thinking in terms of using computers for this work in the 1960s (I still retain an abiding hatred of punched paper tape), or in the laborious applications listed, for example, by Clarke 1968). Today's challenge is to open our imaginations to respond to the growing speed and capacity of desktop and portable machines.

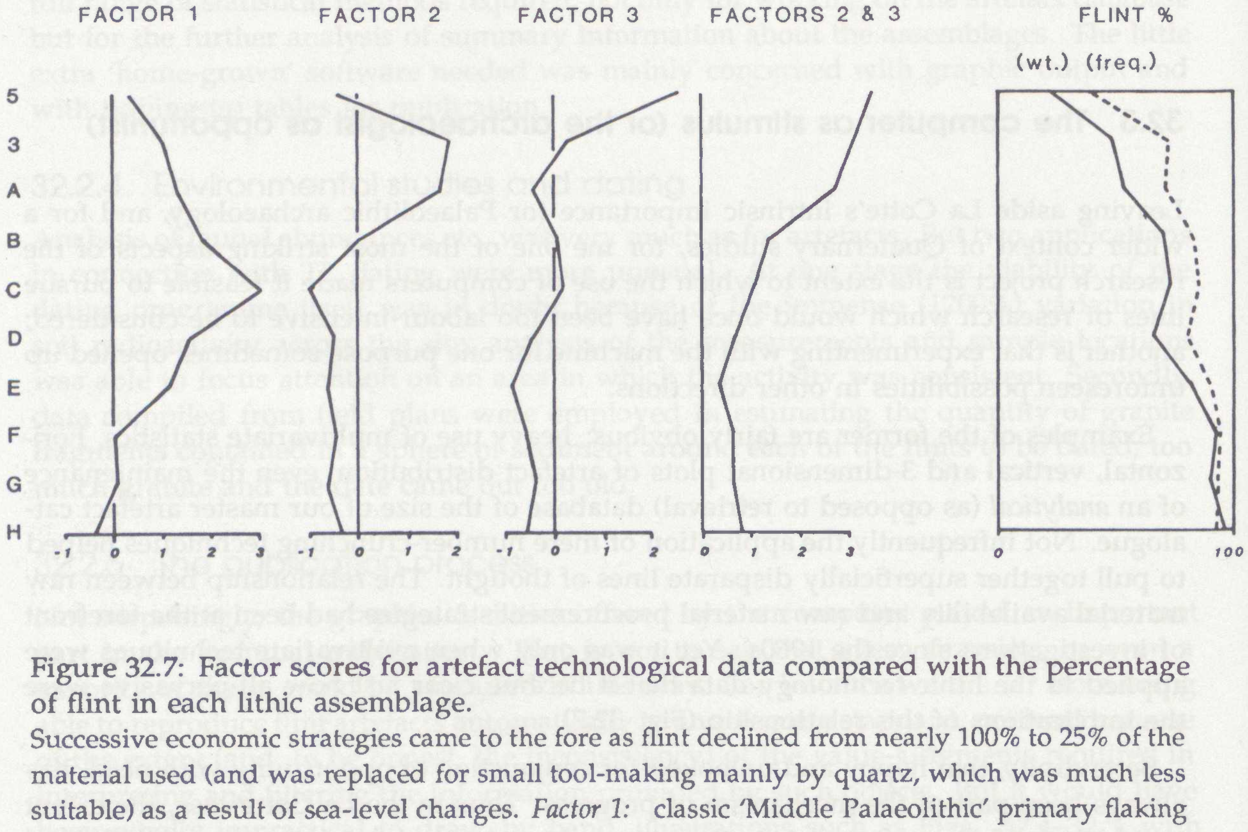

Figure 32.7: Factor scores for artefact technological data compared with the percentage of flint in each lithic assemblage.

Successive economic strategies came to the fore as flint declined from nearly 100% to 25% of the material used (and was replaced for small tool-making mainly by quartz, which was much less suitable) as a result of sea-level changes. *Factor 1:* classic 'Middle Palaeolithic' primary flaking technology (based on discoidal and Levallois cores). *Factor 2:* intensive exploitation of the available flint: resharpening and elaboration of tools; use of flakes or flake-tools as cores; smaller blanks acceptable for tool-making. *Factor 3:* importation of finished tools: increased artefact size, excessively high percentage of retouched tools; contrasting technologies for imported and locally-made artefacts.

32.4 Luddism and ignorance?

Humankind in microcosm, perhaps most researchers are content to wait for someone else (bolder or more foolhardy as you will) to lead the way in exploiting innovation, for all the world like cautious skaters faced with new ice on a pond. But besides the pioneering and the merely prudent there will always be others whose vested interests, or simple mistrust of the novel, work against the exploration of new ideas and new possibilities. Of course this is a persistent and well recognised problem in the refereeing of grant applications, papers submitted to journals, and the like. But it can affect almost any research work, if the opportunities exist.

It would be fair to say that one of the biggest obstacles to successful completion of the La Cotte project was the attitude of just a few members of an older generation to our objectives and to the methods we were using. In the present context there is no need to dwell on the surprisingly determined opposition to renewed investigation of the site's stratigraphy and environmental sequence after McBurney's death, as I think this had little to do with science of any kind. But, in the light of what I have described above, the words of one distinguished authority on a later period of prehistory[6] seem particularly ill chosen: 'I do not know what is to be done to impress upon you that the writing of a book cannot be done by computer'. I hardly suppose this injunction was intended to be taken literally, as if I was waiting with my arms folded for the machine to do the thinking for me! If it was not, though, it shows an extraordinary lack of awareness of the growing impact of computers in the academic world. Desktop word processing was already a fact. The Computer Board had provided Oxford University Computing Service with its phototypesetting facilities for the benefit of the whole British academic community. For that matter, my own doctoral dissertation, itself very much concerned with exploiting the new computational power to open up new lines of questioning in prehistoric research, had been produced by means of a primitive text processor program and a lot of simple computer graphics many years before (Callow 1976).

Even more striking is the same person's later statement that McBurney 'did not dig in order to feed a computer' (whatever that means)—with the implication that I did, apparently, or else erroneously thought that McBurney did. As I was working very much along the lines that McBurney and I had agreed before the latter's death, and had discussed over a great many years, this was puzzling. I concluded that

either he really knew very little about McBurney's approach to his work, and eagerness to exploit new and productive ideas and techniques,[7]

or he himself was having difficulty coming to terms with the rapid technical changes in archaeological practice.

Such hostility in the higher places of British archaeology, not merely to the tasks that McBurney and I had set ourselves, but to the means we had decided to employ,

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[6] Written at a time when he was unsuccessfully attempting to persuade the Jersey Heritage Trust that the re-excavation they were funding at La Cotte was 'academically unreal' ... much to the surprise, outrage and finally amusement of specialists engaged in the research.

[7] Apparent even from the very start, in his doctoral dissertation (McBurney 1948), and later in his being one of the first western experimenters with microwear; the scale of his ambitions for La Cotte is anticipated in *The Haua Fteah* (McBurney 1967). For an account of the man and his work, see Clark & Wilkinson 1986).

was enormously damaging to the efficiency of the project in its vulnerable state after McBurney's death. In its more overt form the opposition manifested itself in letters refusing to support laboratory accommodation needs beyond a three year period after my commencing work (remember, this had served as a Department of Archaeology training excavation over much of the previous eighteen); pressuring me to limit the book to 100 pages and some microfiche; later, flat refusal of support for a publication subvention to make it possible to stay with our original publisher, rather than change to one which (though technically good) had more limited marketing arrangements; and so on. Certainly I had no suspicion, when I started, that McBurney might have any enemies who were willing to see the eventual fruits of his work buried in this way—let alone ones so foolish as to expose themselves on paper, for that matter. And certainly these carryings-on lend themselves to such an interpretation. But if it was not malice, then what inability to comprehend!

There is a lesson here that has to be heavily underscored. As the research increasingly showed that the site required reinterpretion in almost every respect, determined attempts to force a premature closedown clearly had to be resisted to prevent a serious scientific loss. A comment made by Ascherson (Ascherson 1986) in an article about the World Archaeological Congress is no less apposite here: 'Academia is not a temple but a battlefield in which final victory may require tactical retreats'. To borrow the imagery of chess, I was forced to sacrifice not just pawns, but knights and bishops, to save still more valuable pieces. Perhaps inevitably, among the principal and most serious casualties were system design and development, since effort conspicuously devoted to anything not yielding rapid and tangible results merely stimulated opposition. To anyone familiar with these aspects of research the irony must be apparent ... of course the time not spent on them at the appropriate moment had still to be spent later, *many times over*. Thus a much higher degree of automation of some of the data processing could easily have been achieved through better integration of the various separate pieces of software used, or by programming the computer to take on more of the decision-making when the artefact provenances were being resolved. These steps alone could have saved many months of work—and comparable examples could be offered for many other (non-computational) aspects of the project.

For the purposes of discussion, let us suppose that the motives in this placing of obstacles were reputable ones. Then, if McBurney's unfamiliarity with the methods he was advocating had created problems in realising his vision of the project, it is fair to say that others' lack of confidence in their potential (if not plain mistrust) came close to destroying it altogether.

32.5 Concluding reflections

I hope I have shown the immensely important part that computing technology played in the development and eventual accomplishment of a major field and laboratory project. Much has already changed, with machines increasingly in on-site use, for instance. More and more archaeology students, for that matter, arrive at university already computer-literate, or soon become so. But I think some general lessons may be drawn.

Firstly, any technology's potential for exploitation has to be understood in the light of, and set against, the restrictions and discipline required if that exploitation is to be wholly successful. In practice, true equilibrium between these is unlikely to exist unless the technology is already well established, but an obsolescent technology may not be capable of handling demand. So it is hard to avoid under- or over-targetting, depending on which way the balance lies. As in manufacturing, so in research. The responsible investigator is therefore obliged to maintain sufficient familiarity with developments to be able to exploit them both appropriately and effectively; no easy task. But it is not enough to go hell-for-leather after 'results' without giving careful thought to the process and means of research. Nowhere is this more true, I suspect, than in facing up to the growing impact of computers.

Secondly, there remain serious problems of credibility and thus education. Notwithstanding best efforts to change this situation, I suspect it will always be with us in some form, continuing sometimes to frustrate the efforts of those who are trying to be at the leading edge of research. In the La Cotte case the worst was averted, and after years of gestation the work was not aborted or (just as important) born deformed. Even so, the consequences were extremely serious in terms of long and unnecessary delays to the research and publication, in lowering the quality of work done by staff whose morale was inevitably affected, and in loss of time to follow up important lines of investigation. Perhaps we are still living with the lingering product of an unfortunate association, in some peoples' minds, between the application of computers and the worst excesses of 'The New Archaeology'—despite years of responsible statements attempting to refute this, such as that by Doran and Hodson (Doran & Hodson 1975, p. 343): 'It seems that the less archaeology becomes scientific, in this sense, the better'.

Finally, it is worth stressing that colleagues in different countries have commented on the smallness of the budget on which the La Cotte project operated. This is all the more striking in view not only of the project's scale but its admitted inefficiency. The hard fact is that nothing comparable could have been achieved *without* computers (even admitting that savings were made by other means, too); it is inconceivable that adequate post-excavation funding could have been found for the same objectives to be met by more labour-intensive methods. But this is not a point that needs labouring, for anyone reading the present volume.

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