Chapter 6

Computers and Prehistoric Archaeology

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I should begin by explaining that by prehistoric archaeology I do not mean the study of peoples who are necessarily remote from us in time, but rather work on cultures of any era which have perished without leaving written records. My topic is limited in this way because I do not wish to discuss the roles of computers in dealing with ancient texts. This is an important use for computers, but my preference is to concentrate on applications which I would not expect historians or linguists to deal with. I will merely remark that for the archaeological study of historic cultures, computers could be used for everything the prehistorian can use them for, in addition to their uses for textual materials.

An inherent condition of archaeological research is that it produces vast amounts of data, and much of an archaeologist's life is spent in coping with masses of details; keeping track of them, organizing them, taking many relevant things into account at once, and extracting some kind of orderly interpretations. Considering how much effort has recently gone into developing electronic equipment intended to deal with data processing problems in general, it seems inevitable that these new machines can lighten some of the archaeologist's burdens, and very probable that some quite new horizons in data analysis can be opened up.
Nearly twenty years ago, long before archaeologists thought of computers, J. O. Brew pointed out, in one of the few truly important discussions of concepts and terms used in archaeological description and classification, that there is little to be gained by using them for this purpose alone. Archaeologists do not usually work under the time pressure that businesses do, and there are often adequate alternatives, including desk calculators (many of which are portable enough to be usable in the field where work is in progress) and the relatively simple and accessible machines which punch and sort cards, make tabulations, and print card data or tabulation results. If anything, archaeologists could profit by making more use than they do of extremely simple aids in the field, such as slide rules (to figure percentages and ratios). The point about computers in relation to this role is that, if a computer is set up to do something more sophisticated with the data, for which simpler equipment is inadequate, it is very easy to use it in addition for producing routine tabulations.

A second role for computers in archaeology is in the general field of data storage and retrieval. What I have in mind here is making it easy for the archaeologist to inspect all the available data relevant to some specified topic, or relevant to objects or sites of some specified category. When emphasis shifts to what the archaeologist does with the data he inspects, one moves out of data retrieval and into data analysis. Of course the two fields are intimately linked, and one cannot always make a sharp distinction between them.

In part, the archaeological data retrieval problem is simply the need for fast and comprehensive indexing of the literature in a way that is flexible and informative enough to be helpful to users with many different specific interests. There is nothing distinctively archaeological about this problem; it is just one aspect of the growing bibliographic difficulties of scholarly literature in general, and by and large the same methods and machines that come to be adopted for other fields should also work well for archaeologists.

A more specifically archaeological problem is involved when the data search is aimed less at finding publications relevant to a given topic than at discovering other objects comparable to a given specimen, or objects comparable to any or all of a given collection of specimens. At present, anyone trying to do this, using published literature, unpublished manuscripts and notes, or museum collections faces a discouraging mess. Much less comparative work is done than might be, and what is done takes too long and is often needlessly spotty and inconclusive.

What is crucial for any real progress with this problem, whether computers are used or not, is a great clarification and standardization of the concepts and terms used in archaeological description and classification. Nearly twenty years ago, long before archaeologists thought of computers, J. O. Brew pointed out, in one of the few truly important discussions of archaeological theory, that there is really more to good archaeological classification than a certain intuitive astuteness as to which things “go together” and which things are best put into separate categories. Brew argued for a multiplicity of different classifications in order to serve different purposes. I agree with this, and would emphasize that what we must standardize are the concepts and terms themselves, not the classifications. One great opportunity that computers offer us is that, rather than forcing greater rigidity upon us (as I suspect many archaeologists assume), they will make possible much more of the flexibility that Brew asked for.

In particular, one controversy has involved “lumpers” and “splitters.” Several different points are at issue here, but one of them is the “lumper” argument that some systems of artifact typology are so complicated that they can only be learned by a few specialists, and that therefore they should be simplified. To this I would reply that one task of archaeological reporting is to describe historical phenomena; that historical phenomena are inherently complex and “messy;” and that there is just no such thing as a simple model of them which does not oversimplify to the point of falsification. Simple schemes and broad outlines are vital for introducing a student to an area and for the edification of the interested person without research interests in the area. But, for research by the area specialist, and, also, for most research involving comparisons between areas, we absolutely require systems of handling the data which are compendious and expressive of minor contrasts, yet systematically organized, and not merely lists of item-by-item descriptions.

To some extent this dilemma can be resolved by hierarchical taxonomies, using a “fine-grained” system of numerous categories based on minor differences for the experts, and lumping these into a smaller number of more inclusive, higher level categories for nonspecialists. This is not entirely satisfactory either, for the hierarchy itself has a simple structure in which there is no easy provision for expressing multidimensional relationships in which a given category resembles a second in one way, a third category in a different way, a fourth in still another, and so on. Here again, there is a built-in pressure toward oversimplification and a Procrustean treatment of historical complexities.

With a computerized data file, any feature that is recorded can be used as a basis for searching the file, and one can retrieve data in a form that groups together all specimens which share any one of the enormous number of different logically possible combinations of features. Different sets of searching criteria imply different systems of classification, and a limited number of criteria can be combined in several different ways to generate a great many schemes of classification. The only limitation is the obvious one that the file cannot be searched for any features that were never recorded in it in the first place.
This raises a related problem in the retrieval of archaeological data. To what extent is it feasible or desirable to establish a single basic code for describing all specimens of a given class of material, such as pottery? No matter what varying philosophies about the defining of types they adhere to, all archaeologists would surely agree that features which mark important contrasts in the material of one area or period may be of very little importance for other times or places. A code designed to be useful both for Mississippian pottery of the eastern U.S. and for Anasazi pottery of the Southwest would have to provide for many attributes which would be observed exclusively in one area, and never in the other. It would probably also force one to record a number of minor details which were important in one area, and trivial by any possible criteria in the other. For many purposes it would be simpler to use different codes in each area.

But what about the person who specifically wants to search for similarities in the pottery of these two areas? Or suppose one wanted to do a comparative study of a north Texas collection which had affiliations in both directions. Must one make separate computer runs, coping with two or more distinct coding systems? This coding problem will require much study, and its outcome will depend upon the extent to which archaeologists in different areas can agree not to make things needlessly difficult for one another. At the very least, there should be no more disparity between systems than is really required by disparity in the data.

In this connection, the work of the New World Lithic Typology Project sounds like exactly the sort of thing that is needed, and something similar should be begun for ceramics. I suggest that we develop on paper a large and extremely detailed “master code” for ceramics, including provision for every descriptive variable which may be important anywhere; provision for infinite subdivision of all measurable or rankable variables; and provision for unlimited additions to the range or number of states (“attributes”) of all variables. Undoubtedly, new variables and new ranges or states for old variables will have to be added from time to time, but I believe this could be done in an orderly way such that every new thing has a new designation, and all old designations are either unchanged in meaning or totally discarded.

One could then set up files of data which, depending on circumstances, could cover anything from one component of one site to the whole world, using as an actual working code whatever simplification of the master code was most convenient. Under no circumstances should the working code provide more detail than the master code; it should always be some simplification, in which some master code variables are ignored, and in which some ranges or states of the variables used are lumped together (for example, in the case of length, by rounding off exact values to the nearest whole centimeter).

A vital individual responsibility will be to specify for every working code exactly how it relates to the master code. By doing so, the codes used for any two files, however different in themselves, will be related to one another through the one underlying master code, and one could be specific about the extent to which data from two or more files were unambiguously similar. One could also be specific about the nature and extent of ambiguities in such comparisons. To avoid needless ambiguities, it should be required that all working codes must provide at least information on some minimum list of variables and states or intervals of these variables, whatever else is provided in addition.

The master code is roughly analogous to the linguistic concept of a universal system for describing phonetically all possible speech sounds, but I would stress that the working codes need not, and often will not, approximate the criteria which were “important” in the cognitive systems of the makers or users of the artifacts. That is, in the terms of Pike, all our codes may be more or less “etic” rather than “emic.” It may become possible to convincingly demonstrate in some cases that we are close to the categories important for a prehistoric people, but even if we could we would still want for many purposes to take account of other features in our analyses.

As in the case of tabulation, there are some simpler alternatives to computers for data retrieval, including hand-sorted, edge-punched cards and optical coincidence cards. In my experience they are less desirable than computers, either if one wishes to put all important data directly into the punched code, or to integrate data-searching directly with statistical analysis; or indeed, any time that the data are really numerous. Their best use is for multiple indexing of a few hundred items, including illustrations, descriptive text, or both, where the records themselves are to be inspected for much detail that is not included in the coding system per se.

Formerly, another advantage of hand-operated systems was their much greater accessibility. It appears that this is being greatly changed by the development of remote-time-sharing computer services; whereby a number of users at consoles many miles apart can simultaneously run their problems on the same large high speed computer. Archaeological data retrieval would require scanning, perhaps repeated scanning, of files containing several hundred thousand to several million characters, and I am not sure whether this is feasible with present time-sharing facilities. At any rate, it seems reasonable to expect that it will become possible soon. Certainly it will be possible to write programs for searching such that the individual user need know nothing about programming, and need only supply the machine with a request for specific data files and a specification of the criteria to be used in searching. It should be possible for an archaeologist to learn a system about as complicated as that he now must know in order to get one book out of a
library (assuming the book is not already out or lost), go to a console within a few yards or a few blocks of his office, and in a few minutes do a comparative study that would take him months by precomputer methods.

This assumes that the archaeological data are stored in a computer facility which is used primarily for business or scientific purposes, which has the advantage that the same facility could be used very easily for data analysis. In some cases it may be better to use one of the microfilm machines being developed for library or archival purposes, and this might especially be true if high-resolution pictures of artifacts are desired as part of the search output.

Whatever kind of machine is used, the big and costly bottleneck will be getting the data into the file to begin with. Besides working out a suitable code, much tedious, meticulous, and rather skilled labor will be required to produce good descriptions of the material in a language readable by the machine. There is not much hope of automating this process. One can, for instance, imagine an electronic scanner automatically sensing and recording minute color variations in an object, but the day is still very distant when a computer can cope with the problem of deciding which colors on a pot are properly characteristic of it, and which best ought to be recorded as "firing clouds," "altered due to erosion," or the like.

The third role for computers in archaeology is in the statistical or mathematical analysis of data. Archaeologists have not very often gone much beyond intuitive interpretations based on inspection of tabulated counts or percentages. There have been some instances of ill-founded judgments based on statistically inadequate evidence (too small a sample, a badly selected or biased sample, or inadequately defined or poorly chosen variables) but probably most interpretations have been valid as far as they went. The exciting things about statistical methods are the possibilities they offer for new and richer interpretations of data; especially for better methods of generating categories and taxonomies, for dealing with complex interactions between different traditions, and for convincingly validated reconstructions of prehistoric social and cultural systems. Archaeologists have been making some use of statistical techniques at least since Spier's work at Zuñi but the use has always been very limited. However, since 1955, statistically-oriented papers have appeared with increasing frequency. Perhaps there has been some justification for this lack of enthusiasm, for most of the really interesting problems for archaeologists require multivariate techniques for dealing simultaneously with many variables, such as cluster analysis, factor analysis, multiple correlation, etc., which are discouragingly tedious without a computer.

Today the stumbling block is not in the execution of the computations, but in the education of archaeologists in the methods, so that we can have some idea of what technique or techniques might be available for a given problem, and of what interpretations are justified, and not justified, on the basis of a given computer result. There is probably still too much of a tendency for most archaeologists either to flatly reject the notion that "a machine can do your thinking for you" or else to put a somewhat premature and excessive trust in computer results. My own position was expressed when I earlier used the phrase "exciting . . . possibilities."

Roughly, three general things can happen in any particular study. First, the mathematical model may fit the data so poorly that actually wrong results are obtained, even though the computations themselves are technically correct. Second, the results may not be wrong, but may yield so little beyond what was already apparent that the effort is not worthwhile. Finally, the results may be both valid and an important addition to what can be obtained by more familiar methods.

I believe that results of this third kind are beginning to appear in print. To my mind the greatest weakness in these, and in all other studies to date, is that archaeologists have been too dependent on statisticians whose own outlooks have mostly been derived from experience in other social sciences, particularly psychology and educational testing. In many ways it is true that multivariate data are multivariate data, and there are many logical and formal similarities whether they concern archaeological assemblages, responses to a battery of tests, or anything else. But there are problems peculiar to archaeological data, and, especially, the formal, logical structures of the kinds of interpretations we want sometimes differ importantly from what a psychologist wants from his data. As long as we just borrow multivariate techniques which have been developed primarily to meet the needs of psychologists and educators, we are almost certainly missing many of the possibilities of these methods for specifically archaeological problems.

Clearly, one thing which is needed is that at least a few archaeologists learn enough statistics to be able to talk to, and sometimes talk back to, the statisticians. Equally, the bulk of mathematics-allergic archaeologists must learn, in a general qualitative way, what the names of the various multivariate techniques are, what assumptions about the data each presupposes, what general sorts of problems each is useful for, and what major peculiarities, pitfalls, and ambiguities are associated with each. As far as I can tell, many archaeologists who are tackling problems by less suitable methods are not consciously rejecting multivariate techniques but simply are unaware that anything better than their present methods is available.

One final point about the role of computers in archaeology should be made. It has become quite common for most anthropology departments to have a few students who already have substantial computer programming and operating experience. This competent and motivated student energy is a major resource for the fuller and more effective use of computers by archaeologists.
I have made no attempt to review specific current computer projects, but I should mention briefly my own work, which is more fully reported elsewhere. Since the spring of 1965 I have been collaborating with René Millon in a pilot study for computer analysis of data obtained by him and his associates in the course of a detailed mapping and surface reconnaissance of Teotihuacan, a huge prehistoric city in central Mexico. It has been clear for many years that gross contrasts exist between different parts of the city, and, like any other preindustrial city, it must have been occupied by a complex and richly differentiated society. One major aim of the mapping project has been to bring the evidence on differentiation into sharper focus and to get a better idea of the city as a going concern. Much that is new can already be said without recourse to computers, but given the enormous quantity and the detailed character of the data which are being accumulated, not all its implications can be brought to light by ordinary inspection.

One important use for multivariate analysis involves treating each artifact as a "case" and generating a typology or classification of the artifacts, based on comparisons of their attributes. So far, we have not been working on this level, but have instead been treating each of the roughly 5,000 separate tracts from which we have materials as a "case." The variables used to characterize these cases include architectural and other features which may be observed as present or absent, rankings of extent or intensity of other features, and counts of various categories of pottery and other materials collected from the surface.

At present, it is often difficult to say if a feature is worth including in our code or not. We have been guided by the idea that in a pilot study we should run the risk of including too much rather than too little, and have developed a code which requires eight 80-column punched cards for each site. We think of this as primarily a format for data storage, which can be recoded by machine in many different ways, to serve as input data for many alternative statistical programs. In recoding we have great freedom to select or omit different variables, to build new variables as functions of the old, or to collapse or simplify variables in different ways; all far more easily than if we had to return to the original data sheets each time.

We have not, of course, any intention of trying to provide in the code for everything observed in each site. Data on features which are unique or extremely rare are best retrieved by merely listing the sites which have them, and are too sparse to be useful for multivariate analysis. Much of our work so far has been concerned specifically with the development of a code which retains a good deal of the detail of the original data without becoming impossibly cumbersome. We have also been very concerned with establishing the validity and reliability of our data. Comparisons of frequencies and ratios of materials collected and analyzed from the same tracts by different workers at different times have so far turned out to show surprisingly good reliability. However, sampling problems inherent in scarce categories require that for some things we must do our studies on the basis of aggregates rather than individual tracts and thereby lose some sharpness of detail for these categories. This is one of the inescapable limitations of surface-collected data that can be overcome only by digging and not by computing.

Our major goal is to search for clusters of features (including artifact types) which are similar in their distributions, and for clusters of tracts which are similar in their features. The first procedure is what in factor analysis is called R technique, while the second is Q technique. We plan to compare results and costs of various clustering techniques.

We do not expect all the resulting clusters to be self-evident in their interpretation (although some of them probably will be). Rather, they are to be seen as themselves important kinds of data which will demand the fullest exercise of intuition, judgment, and control of both mathematical and anthropological concepts and materials for their best interpretation in cultural terms.

Other useful applications of the computer include simple tabulation of the data, and the machine production of distribution maps. For the latter purpose, the SYMAP program of Howard T. Fisher, of the Graduate School of Design, Harvard University, looks most promising. For tabulations and statistical analyses, the DATA-TEXT system of programs being developed for use on the IBM 7094 by Arthur S. Couch, Department of Social Relations, Harvard University, has many very attractive features, including great flexibility in recoding variables.

REFERENCES

1. Among the various people who have made useful comments on this paper, Drs. J-C. Gardin and Irving Rouse in particular have offered suggestions which influenced its present form.


5. For an excellent discussion of this, see J-C. Gardin, "On a Possible Interpretation of Componential Analysis in Archaeology," in Formal Semantic


8. Krieger, in *op. cit.*, expresses a similar opinion.


13. Prof. Millon's mapping project is being supported by the National Science Foundation and sponsored by the University of Rochester. For a preliminary report on this project, see Millon, "The Teotihuacan Mapping Project," *American Antiquity*, XXIX (1964), 345-352. The pilot computer study is being supported in part by the NSF and in part by the Wenner-Gren Foundation for Anthropological Research, with Brandeis University as the sponsoring institution.