Past, Present, and Future of Quantitative Methods in United States Archaeology

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Abstract

I briefly review the history of quantitative methods in American (mainly US) archaeology and assess their present status. I then identify topics that seem most likely to be most active in the near future and express my opinions about the directions that I think will be most productive over a somewhat longer term. What topics, what methods, and what general approaches should be given special attention and encouragement? What roles should quantitative methods play in the archaeology (or archaeologies) of the twenty-first century?

Key words: quantitative methods, United States, typology, seriation, chronology, "resampling", Bayesian inference, electronic knowledge, text-books

1. Introduction

I very much appreciate the invitation to address this plenary session. It gives me an opportunity to update earlier reviews that I published in the 1980s (Cowgill 1986, 1989). In preparing it I have been greatly helped by some more recent reviews, notably that by Aldenderfer (1998), who astutely covers a number of topics in more detail than I can here. I will not attempt to speak for all the Americas. I concentrate on North America, and mainly on the United States. I must also admit that I have reviewed recent literature less thoroughly than is desirable, and I may have missed some important developments. I focus on those of which I am most aware, and which I find most interesting. Non-quantitative computer applications are abundant, but I have little to say about them here.

Quantitative methods in US archaeology most certainly do have a past, a present, and a probably expanding future, but they have never had a really large role in US archaeological theory or practice. I think this is partly because all but the simplest require mathematical skills that are difficult and uncongenial for most archaeologists. It is also because they have never yet achieved results important enough to make most archaeologists feel they had no choice but to overcome their aversion to mastering the concepts and methods. To identify oneself as a "new" or "processual" archaeologist, one didn't really have to learn much. Read a little Hempel, a little Kuhn, a little Binford, maybe a little popular systems literature, acquire a few key words like "technomic", "positive feedback", and "homeostasis", and one could join the bandwagon. More recently, one can read a little Giddens, a little Bourdieu, a little Foucault, a little Hodder, expand one's vocabulary with words like "inscribed", "decentred", "essentialist", "metanarrative", and "agency", and one can proclaim oneself "postprocessual". This is not to say that nothing useful has been done under any of these headings or that none of these terms express useful concepts – it is just that in addition to the good work there has been much that is facile and shallow. In contrast, it has never been that easy to validate one's credentials as a quantitative person – or at least not for long – fakers are too easily identified.

At the same time, inept or downright wrong applications of quantitative methods are still easy to find, even in peer-reviewed publications. At first this sounds paradoxical. I think the explanation is that people are cautious about claiming to be qualified to propose innovative methods and the work of those who do claim to innovate is likely to be subject to scrutiny, while the majority who think they are only applying routine and unproblematic methods are unlikely to be reviewed by anyone who knows better about the quantitative aspects of their work. Also, much of the problematic use of quantitative methods is not so much wrong as needlessly limited – available possibilities are not taken advantage of.

I also note that very few people in the US spend most of their time on quantitative methods in archaeology. I judge that I put somewhere around a quarter to a third of my total professional effort on quantitative topics, including research, classroom teaching, and advising graduate student research. It is not so much that I couldn't do more along quantitative lines as that it would have to be at the expense of other things I also want to think about and work on. I think this is fairly common in US archaeology. Often individuals go through a phase where much of their work is quantitative and then, without precisely becoming disenchanted with quantitative methods, they find other research interests increasingly competing for their attention and increasingly rewarding. Also, except for Dwight Read at UCLA, I do not know of any statisticians or mathematicians in the US who have devoted sustained attention to archaeological topics. We generally try to do it by ourselves, or else get some *ad hoc* advice from statisticians on specific projects, which tends to mean that they rummage through their particular tool-kit of techniques they like to see if there is anything they can recommend for our problem, or our problem as they understand it.

At least in the UK, there are a few archaeologists who seem to be spending most of their time on quantitative issues, and a few statisticians and other mathematicians who have spent years seriously addressing quantitative issues in archaeology and developing methods appropriate for these topics. This is probably why most, though not all, of the good English-language textbooks on quantitative methods in archaeology have been written by scholars in the UK.

This is perhaps as good a place as any to deplore another tendency in training archaeologists in the US, which is the overwhelming monolinguality of US education. It is quite possible to obtain a doctorate in many institutions without even a working reading knowledge of any language but English. It is simply unrealistic to expect the majority of our students to be able to read anything in other languages. Often enough this applies to professionals as well. Even publications in French have very slight impact unless they are translated into English. Correspondence Analysis, for example, was virtually unheard of in the US until publications about it began to appear in English (notably those by Torsten Madsen and his group in Denmark).

I am afraid I am beginning on a critical and perhaps even querulous note, and perhaps indulging in a little too much complaining. It is perhaps just as well to get that part done with at the outset, so that I can turn from national self-criticism and look at accomplishments and future possibilities.

The physical sciences are noted for the surprising success of quantitative methods in expressing key concepts and formulating explanatory relationships among the entities involved. This has markedly not been the case in the social sciences, except to a limited extent in some domains of economics, geography, and demography. Some think this is because we haven't tried hard enough and still don't have the right concepts, the right data, or both. Others argue that it is because the so-called social "sciences" are about matters that are intrinsically impossible to express mathematically. I will not try to resolve this debate in this paper. For present purposes it is enough to say that quantitative methods are not without their uses for archaeology.

I turn to a more detailed discussion of quantitative methods among US archaeologists under three headings: data organization and description, use of statistical inference based on probability theory, and mathematicization of theory.

2. Data organization and description

A tremendous amount can be learned simply from well-designed tabulations of counts and proportions of relevant categories. Of course, defining categories in reasonably good ways is a major undertaking in itself – or at least it has been considered such in US archaeology. Nevertheless, in many cases archaeologists have succeeded in establishing categories that are clearly useful for them and meaningful in some sense. Even if they are not the best possible categories and do not employ the best concepts for thinking about categories, they have been quite helpful. At least by the second decade of the twentieth century some US archaeologists were using percentage tables to good effect; especially in deducing chronological sequences (e.g. Kidder and Kidder 1917).

Graphics, especially histograms or bar charts, complement descriptive tables and can be immensely valuable for getting ideas about what is going on. For variables measured on interval scales, simple bivariate scatterplots for pairs of variables are also relatively easy to understand and often highly informative. The importance of well-chosen graphics has been emphasized repeatedly (e.g. Tufte 1983, 1990, Cleveland 1985, 1993) but it bears further repeating.

I emphasize the value of descriptive tabulations and graphics for three reasons. First, there is a strong temptation to underestimate them and to move too quickly to more advanced techniques. This is, of course, all the more the case when demonstrating prowess and seeking prestige is more at stake than actually understanding data as economically and as validly as possible. The ever-growing availability of statistical systems for computers makes it all the easier and more tempting to skip the supposedly pedestrian stuff and leap directly to the more fashionable and mysterious. However, if the data do not reasonably fit the assumptions of more advanced methods, the results of these methods can be very misleading. Simple tabulations and graphics that stay close to the relatively "raw" data are not only instructive in themselves; they are also excellent for revealing departures from the assumptions of more advanced methods, as well as for suggesting which advanced methods are likely, or unlikely, to offer further understanding. I strongly agree with Robert Whallon's (1987) call for more and better use of simple statistics.

Second, when simpler methods are in fact used, even today it is all too easy to find inappropriate or needlessly awkward uses. Bivariate rectangular tables of counts of categories for each of a number of collections or other data sets, for example, may be "standardized" by replacing them with percents of the grand total, rather than with percents of row totals or column totals. The insights gained by replacing counts by percents of the grand total are, at best, very slight, and the much greater insights furnished by percents of row and/or column totals are missed. The prevalence of bad practices such as this is perhaps decreasing in US archaeology, but it is still higher than it should be.

Third, I think we still have a good deal to learn by way of making graphics even more effective than they usually are today. I expect that development of improved graphic practices will be important in the near future. I hope that inappropriate and relatively unhelpful choices will be made less often. There is also too much unthinking reliance on default options offered by computer systems, and often not enough enterprise and imagination in "playing" with ways of showing the data. I don't, of course, suggest that we should fiddle with the display so as to make it seem to mean what we would like it to mean, but rather that if we really want insight we can learn a lot by looking at the data in different ways. Also, the exercise of exploring the extent to which different graphics choices make the data look different is itself instructive.

3. Statistical inference

Unlike quantitative description, statistical inference scarcely existed in US archaeological practice before the 1950s. Archaeologists always, of course, made use of intuitive notions that some observable patterns were too strong and based on too much data to be at all likely to be merely accidental. But very rarely did they make any use of formal statistics grounded in probability theory. Sometimes this resulted in failing to perceive, or at least failing to trust, patterns unlikely to be simply accidental. I suspect that it has more often resulted in treating patterns as "real" even when their empirical support was quite problematic.

In the 1950s interest in methods based on statistical inference was considerably increased through the efforts of Albert C. Spaulding, then at the University of Michigan. Especially notable was his use of what we would now call discrete multivariate analysis for the empirical discovery of patterning in sets of artefacts (e.g. Spaulding 1953). His general approach was to characterize individual objects in terms of a number of variables. Univariate distributions of interval-scale variables were searched for multiple frequency

modes, and each such mode was used to set up a nominal category. That is, if a tabulation of measured object lengths revealed three distinct modes, these might be defined as "short", "medium", and "long", and a second "length" variable created that was treated as nominal with three categories (even when, as in this case, ordinal information is preserved). These new variables derived from interval measurements were combined with other variables that were intrinsically nominal, and the set of variables subjected to discrete multivariate analysis (DMA). Spaulding's earlier work generally used only three variables at a time. For example, one could explore the relations and interactions among surface treatment, shape category, and tempering materials for some set of pots; usually those from a single period of a single site. Chi-square was the principal technique used by Spaulding in his earlier papers. Later, as computer-aided (DMA) techniques became available, it was feasible to consider more variables jointly.

In retrospect, I think Spaulding's main contribution was to make US archaeologists far more aware that they might be missing something important if they didn't know a little about statistical methods. On the whole, I do not think that the specific approach used by Spaulding has proven very productive. For one thing, he persisted in reformulating DMA results in terms of "types". The issue here is *not* the ontological status of types, whether they are *discovered* (as Spaulding argued) or *imposed* (as James Ford argued). The answer to that particular debate is that Spaulding and Ford were doing different things with different kinds of data sets (Spaulding working within single periods of single sites and Ford working across multiple sites and periods) and they were both right in describing what they did as discovering or imposing. For present purposes, the relevant point is that it seems DMA results are best and most naturally expressed in terms of the relations and interactions among the variables, and expressing the results in terms of type categories seems forced and less useful.

Also, Spaulding's approach to formulating types has found very little use in US archaeological practice. Nor have other quantitative approaches to typology. For example, I have worked a fair amount with the ceramics of Teotihuacan (an immense prehistoric city in central Mexico), and I have felt fairly comfortable with a typological system that was developed by workers who used methods quite different from Spaulding's. I am interested in improving on this system, but mainly in ways other than through DMA (Cowgill 1990).

There was a surge of interest in quantitative methods in the 1960s, connected with the advent of mainframe computers that could be programmed in languages, such as Fortran, that were not beyond the skills of a few archaeologists, as well as early versions of statistical systems, some of which are still with us in descendant versions. Much of this was connected with the aspirations of the "new archaeology" to make archaeology scientific. A number of multivariate techniques were used or misused, notably multiple regression methods and "factor analysis", which usually turned out, in those cases where the work was well enough documented to allow one to tell, to be some form of principal components analysis. Most of this 1960s work has not stood up well to subsequent criticism, which is probably one of the reasons why the use of quantitative methods has not expanded more rapidly in the US.

Also in the 1960s there were a number of efforts to use quantitative methods of matrix-ordering to seriate archaeological assemblages (Brainerd 1951, for reviews see Cowgill 1972 and Marquardt 1978). Overwhelmingly, the goal was to derive improved chronological orderings. My impression is that this activity took place rather independently of New Archaeology, and was motivated more by the recognition, at least in some quarters, that archaeology of any persuasion suffers greatly when chronologies are uncertain or imprecise. The methods were generally inherently unidimensional and intrinsically dependent on iterative heuristic searches that (a) could not be guaranteed to arrive at optimal arrangements and (b) could not reasonably cope with datasets in which two or more independent axes were required to give a good representation of the patterning. The methods of chronological ordering of the 1960s have been largely replaced by correspondence analysis.

"Numerical Taxonomy" methods also date back to the 1960s and 70s, largely inspired by the influential books of Sokal and Sneath (1963) and Sneath and Sokal (1973). Mainly this meant various forms of agglomerative hierarchical clustering. The tree diagrams generated by these methods may have considerable value for questions of biological phylogeny, but they rarely can capture more than a fraction of the interesting patterning in archaeological data sets. They can have real value as a sort of rather quick and rough exploratory technique, early on in an analysis, but they are too limited (and often misleading) to be respectable end products. It is notable that, nonetheless, they are still treated as end products of analysis by some US anthropologists, though perhaps rarely any longer by US archaeologists.

Of a number of developments in the 80s perhaps most notable was, in a pre-GIS environment, several methods for spatial analysis. Important examples are the use of k-means clustering in the work of Whallon (1984), Kintigh and Ammerman (1982), and Kintigh (1990). Kintigh (1988, 1989) also created programs to control for sample size in comparing collections on the two diversity measures called "richness" and "evenness". These programs avoided some of the problematic assumptions of other methods by using multiple Monte Carlo simulations - what we now call "resampling".

I turn to the present and future, and to methods and topics that particularly interest me. I have little to say about GIS, simply because it hasn't interested me much personally, although I and my students have done a fair amount of spatial analysis, mostly in pre-GIS days (e.g. Cowgill et al. 1984). A number of my colleagues at ASU have used it, mainly as an adjunct to more mathematical methods (Robertson 1999). Kvamme (1998) provides a recent overview of this topic. GIS certainly has a very wide appeal in US archaeology. I think this is mainly because archaeologists tend to be visual more than mathematical, and GIS can produce marvellous pictures that are often very interpretable to the innumerate. Sometimes, perhaps often, such pictures can tell the story so clearly that anything more seems hardly needed. Nevertheless, more mathematically sophisticated methods of spatial analysis have great promise and are still considerably under-utilized in the US. This situation will probably improve, especially if computer systems for these analyses become more available.

I think that many varieties of so-called "postprocessual" archaeology have been too neglectful of chronology building, perhaps because it seems too scientist and perhaps even ethnocentric – if linear time didn't matter to *them* why should it matter to *us*? But it does and should matter to us. Some of my colleagues who work in parts of the US Southwest where dendrochronology enables them to date phenomena to within a decade or so are able to ask – and

answer – important questions that are simply beyond us in many parts of the world. Many of these questions concern matters of agency and context that I think are on the cutting edge, conceptually. So, I both urge and predict that we continue, by every means available, to develop quantitative methods that will enable us to refine our chronologies.

Methods of exploratory data analysis (EDA) are becoming quite popular. This, again, is probably because many of the methods produce such readily comprehensible results. Most anyone can correctly grasp concepts like median and midspread, and box-and-whisker plots are proliferating like rabbits. This is all to the good. I might have mentioned this topic under the heading of data organization and description. It surely has a place there, but the results from EDA also often have a bearing on statistical inference. I hope it is becoming generally recognized that EDA methods belong in our tool kits as partners to so-called "confirmatory" techniques, rather than as replacements for them or somehow in opposition to them.

Multivariate statistics will continue to be used a good deal. I do not know whether any radically new techniques are in the offing.

Resampling methods will surely continue to increase in popularity. They avoid the need for many of the assumptions about shapes of distributions that are often so dubious for archaeological data. Many such methods are already very feasible on antiquated (i.e. last year's model) desktops and laptops, and as computer power per dollar continues to increase exponentially, these methods will become all the more feasible and attractive. An interesting book by the late Julian Simon (1997) expounds this approach on an elementary (perhaps too elementary) level. There are, of course, numerous more advanced books on bootstrapping and other resampling methods (e.g. Efron and Tibshirani 1993).

This leads into a topic that particularly interests me; that of Bayesian concepts and inference. It has long seemed to me that the "classical" (i.e. Neyman-Pearson) approach to statistical inference uses a tortured logic that simply doesn't get across to most students and may be one reason for thinking statistical inference an arcane and perhaps not very applicable topic. Certainly, in practice, most archaeologists who think they are following the classical logic of inference tend to be, in fact, folk Bayesians. Especially, the probability of a sample statistic, conditional on the truth of some hypothesis about the actual value of the corresponding population parameter, is routinely confounded with the probability of the value of the population parameter, conditional on the observed value of the sample statistic.

Shortcomings of a classic outlook on inference can be seen in the otherwise excellent text by Shennan (1997). I feel that he tends to be a little too dubious about formal statistical inference in general, and then, when he does use it, to do so in a rather too formulaic and rigid way that, indeed, warrants some scepticism. In this regard, the text by Drennan (1996) is considerably better in its general outlook on inference, although it does not explain how to carry out any Bayesian methods and covers a more limited range of topics than does Shennan. I have been experimenting with using both books jointly in a first-semester graduate course.

As I see it, Bayesian inference offers both conceptual and practical advantages. With "uninformative" priors, as I understand it, the results are usually the same, or nearly the same, as those obtained by classical inference, so there is little or no practical ad-

vantage. The main advantage is probably the conceptual one; that one understands that one can properly speak of the probability of the population parameter, but only because one is knowingly accepting a particular prior probability distribution for the parameter.

When there is a good basis for an informative prior, however, there can also be considerable practical advantages. The impression still seems to be prevalent that Bayesian methods, even if logically preferable, are too difficult computationally to be practical. This may still be true for many statistics, but there are certainly some important exceptions. One such is using proportions of artefact categories in collections to make inferences about the proportions of these categories in the populations represented by the collections. Using beta distributions (Robertson 1999) or Dirichlet distributions (Neiman et al. 2000) the computations are not excessively difficult for modern computers.

In other cases the Bayesian computations are more difficult and require resampling methods, but these have also been put to very good use for important topics, notably by Buck et al. (1996 and numerous papers). An outstanding example is their application to the problem of making optimal chronological inferences from a combination of calibrated radiocarbon dates and other kinds of evidence, such as stratigraphy. The facts that issues of chronology are important and that some of their methods are incorporated in the OxCal program combine to make this Bayesian approach to chronology very attractive in the US, as well as elsewhere. I understand that, in its present version, the OxCal program does not give satisfactory results with some datasets. It seems, however, that this is a problem that could be overcome before long.

The biggest problem I see that impedes wider adoption of Bayesian concepts and methods is that I know of no elementary book in English that explains them. Even the little book by Iversen (1984) is not easy for our students and covers a limited range of topics too briefly. However transparent and elementary the book by Buck et al. may seem to mathematicians and statisticians, the obstinate fact is that it is far beyond the level that can be grasped by any but a tiny fraction of students or professional archaeologists in the US. Coupled with an absurdly high price set by the publisher and essentially no advertising of the book to US archaeologists, I think its very existence has been a rather well-kept secret, except where a few subversives, such as myself, spread the knowledge by word of mouth or through citing it in publications.

By fits and starts I have been working for some years toward a really introductory book that would explain Bayesian concepts, explain useful methods for a few simple cases, and prepare students for books such as that by Buck and her colleagues. I wouldn't like to predict how soon, if ever, the book will be ready to seek a publisher. If any of you know of such books that already exist, or are in preparation, please let me know.

A second area of great interest is the development of electronic knowledge bases. It may seem odd that I do not put this under the heading of data organization and description. Much work of this kind surely belongs there. However, what is emerging as a truly big problem in the US, and doubtless elsewhere, is finding ways to make effective use of databases created over the years within a region, by diverse projects using diverse methods both for collecting and for describing their data. Methods for durably archiving databases and "migrating" them to new systems and new platforms when existing ones become obsolete are receiving consid-

erable attention, with good reason. However, the problem I have in mind here is a different one, essentially that of enabling researchers who didn't collect the data themselves to compare apples and oranges – to acquire the background information needed to search files for relevant data and to understand what it means when it's located. These problems can certainly be considerably alleviated by electronic means. To me, however, the problem of comparing disparate datasets can sometimes be put in the form of an estimation problem: "If project A rather than project B had collected these data, what would they probably have reported?" This is the only way I see to escape the need to reduce comparisons to least common denominators, which necessarily wastes much of the information in the individual datasets. Estimates, of course, come with confidence intervals. The hope is that one can find ways to combine diverse lines of evidence to derive confidence intervals that are not excessively large. So far this is merely a suggestion for a line of research that, if it can be made to work, should become immensely useful. "Fuzzy set" concepts and methods may also prove useful.

4. Mathematization of theory

At present I have relatively little to say under this heading, except to note its scarcity. Most archaeological theory is not expressed in mathematical terms at all, and perhaps there is good reason for this, at least as long as we use the kinds of math that have been developed for the physical sciences. I note with some surprise that at the April 2000 annual meeting of the Society for American Archaeology there was a symposium entitled "The Mathematics of Cultural Evolution: Taking Stock in the Year 2000". For better or worse, I didn't attend this session. However, the session abstract indicates that some of the papers used formal modelling by means of differential equations. Since I didn't hear these papers, I really cannot judge but (speaking as a Bayesian) my prior probability is on the sceptical side. Previous efforts to apply differential equations to sociocultural phenomena have not been very successful, in my opinion. Certainly none have become popular. For one thing, how do you do justice to agency with differential equations? To be sure, economists have long used differential equations, but only at the expense of postulating excessively oversimplified rational actors rather than reasonable approximations of human beings.

I think that computer-intensive modelling probably has more to offer. Such methods can cope with non-linear relationships among variables and can deal with multiple "agents". As yet I don't know enough about these methods to know whether the term "agent" here means anything that could be translated into archaeological concepts of agency. But it sounds promising.

5. Teaching

In his review, Aldenderfer (1998) discusses at some length the teaching of quantitative methods to archaeologists in the US. A number of programs, even at the graduate level, do not require any course in quantitative methods and, of those that do, quite a few send their students to other departments for that training. In my experience, courses taught in other departments, even when they cover techniques applicable in archaeology, simply don't get across to the students very well how to apply them to archaeological situations. And, of course, such courses often spend consider-

able time on techniques and issues not very relevant for archaeology, and too little time on techniques and issues that *are* relevant for us. I suspect that those departments that do not require any in-house courses in quantitative methods generally have no faculty member qualified to teach such a course.

I think we are still caught in a vicious cycle in which, without more students who are somewhat sophisticated quantitatively, we have not produced quantitative results of such universally recognized significance that they *demand* that the field as whole pay more attention to quantitative training. Of course, as I've said before, it may also be that our subject matter is simply not very amenable to quantitative methods that can yield really big payoffs. This is still a question for the future to decide.

A welcome change since the mid-80s is the appearance of several books that are on an appropriate mathematical level and written by archaeologists or at least by experts with a real feel for archaeological situations and issues. Notable examples on an introductory level are Shennan, first published in 1988, now in a substantially revised second edition (Shennan 1997) and Drennan (1996). Important more advanced books include Baxter (1994) and Buck et al. (1996). These are all extremely useful for anyone trying to teach quantitative methods to students, and hopefully they will have a substantial impact on practice. One thing still missing is an introductory book on Bayesian concepts and methods.

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