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Quantitative Methods in Archaeology: A Review of Recent Trends and Developments

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This paper reviews recent developments in the application of quantitative methods to archaeological research and focuses upon three major themes: the development of so-called designer methods, which are quantitative methods created to solve specific problems; the resurgence of whole-society modeling through a variety of formal and mathematical approaches; and trends in the the teaching of quantitative methods at the undergraduate and graduate levels. Not surprisingly, different subfields have had greater success than others in the development of useful methods, and the causes of this are explored. Finally, suggestions for improving training in the use of these methods are offered.

KEY WORDS: quantitative methods; statistics; mathematics; formal models.

INTRODUCTION

It has been just over 40 years since Albert Spaulding's (1953) classic paper introduced modern statistical thinking to archaeology, and just less than 30 years since Lewis and Sally Binford's (1968) stimulating, if inaccurate, multivariate analysis of Mousterian assemblages from southern France and the Near East. For me, and I think for many archaeologists of my generation, as Read (1989, pp. 6–7) has shown, these studies were exemplars of what could be done with quantitative methods. Spaulding showed that artifact types could be objectively defined and replicated, while the Binfords' use of factor analysis took a large, complex set of lithic data and seemingly made sense of it in terms of a functional model of activity performance and seasonal changes in land use. In technique, these two papers spanned a continuum—Spaulding's paper used only simple quanti-

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tative procedures, while the Binfords' paper was almost mystical in its complexity.

Since the publication of these papers, archaeologists have been busy at work filling in the continuum between Spauldingian simplicity and Binfordian baroque through substantive applications as well as methodological contributions. Much of this work has been highly successful, while an equal, if not larger, proportion has been abysmally poor. Periodic reviews of the ever-growing and vast body of literature on quantitative methods have dissected the flaws, evaluated the strengths and weaknesses, projected the future, and generally confirmed the place of quantitative thinking in our field (Aldenderfer, 1987a; Ammerman, 1992; Clark and Stafford, 1982; Cowgill, 1986; Read, 1989, 1990). Although we continue to suffer growing pains and the consequences of the historical circumstances of the introduction of quantitative methods to the field—the indiscriminate borrowing of methods from different scientific disciplines (Aldenderfer, 1987a, pp. 23–27, 1991, p. 208; Ammerman, 1992, pp. 250–251)—there are likely few of us who do not believe that the use of quantitative methods in archaeology has matured significantly and that we, as a discipline, are the better for it.

Review papers tend to fall into three broad categories: general, specialty, and book reviews. General reviews, like this paper, make an effort to identify major themes and trends in the use of quantitative methods, and relate them to broader issues of importance within the field. Themes explored in the recent past include historical overviews (Read, 1989, pp. 9–23), progress (or its lack) in the use of quantitative methods for classification research (Ammerman, 1992, pp. 242–245), and spatial analysis at both regional and intrasite scales (Ammerman, 1992, pp. 246–250; Orton, 1992, p. 138; Read, 1989, pp. 49–63). In contrast, specialty reviews tend to focus on a methodological subfield, such as the compositional analysis of ceramics (Baxter, 1992) or sampling (Nance, 1993), a technique or suite of techniques (Aldenderfer, 1991), or the evaluation of the use of families of quantitative methods, such as formal models (Doran, 1990). Book reviews, of course, attempt to summarize edited volumes (Ladefoged, 1996), texts (Aldenderfer, 1995), collections of symposia or proceedings papers, or even combinations of these (Perpere, 1995), and do so, given space limitations, within a broad, analytical context.

In keeping with this distinguished company, I have selected a series of themes for the organization of this review: the continuing development of so-called "designer" methods and the implication this process has for archaeology, the reemergence of whole-society modeling, and, finally, a review of trends in the training of both graduate and undergraduate students in the use of quantitative methods. I use 1990 as a permeable boundary for a discussion of these trends, moving across it to explore antecedents and contextual points of importance.

Before continuing with the review, it is useful to offer some definitions of common terms to provide a basic conceptual framework. Quantitative methods are those that manipulate numbers and use measurement in the research process. Johnson (1978, p. 46) notes that "quantification is concerned with the numerical description of empirical situations . . ."; elsewhere, following Kaplan (1964, pp. 212–213), I have defined the notion of a quantitative idiom, which is simply a recognition that measurement and the manipulation of numbers are seen as a useful way to obtain insight into some phenomenon or process (Aldenderfer, 1987a, p. 14). Measurement is closely related to quantification, for it is the assignment of numbers to some object, entity, or process according to some rule. Measurement is important, because it allows us to use mathematics, which in turn gives us the possibility of creating more precise descriptions of the things in which we are interested. Measurement, however, must take place within a meaningful context. While we must assume we can measure anything of which we can conceive, being able to measure things per se does not make the measurement meaningful. Meaning comes only from a body of theory (Aldenderfer, 1987b, p. 91).

Mathematics is a highly structured form of reasoning that proceeds from assumption to deduction to conclusion (Davis and Hersh, 1981, p. 6). It employs an abstract, axiomatic, formal language that is "content free." That is, mathematics can be about "anything" or "nothing," depending on how it is used. Statistics is a branch of mathematical reasoning that deals with the logic of inference under conditions of variability, uncertainty, and error. As Hacking (1965, p. 1) puts it, ". . . the foundation of statistics is to state a set of principles which entail the validity of all correct statistical inference, and which do not imply that any fallacious inference is valid." Quantification, mathematics, and statistics have each played important roles in modern archaeology, but it is important to distinguish them carefully from one another. The terms cannot be used interchangeably; while in practice both statistics and mathematics are contingent upon measurement, measurement itself in many applied settings is strictly ad hoc, and has no formal mathematical foundation beyond simple counting and arithmetic.

It is also important to note what this paper is not about. I do not discuss the very rapidly growing literature on the application of information technology and its application to archaeological analysis (for an overview see Reilly and Rahtz, 1992), including scientific visualization, image analysis, and database and expert system development. Nor do I discuss certain subfields of the discipline such as paleodemography, bioarchaeology, and remote sensing, including geophysical techniques.

"DESIGNER" METHODS IN ARCHAEOLOGY

The 1990s, among other things, is the designer decade—designer clothes, cars, houses, drugs, and, for us scholars, methods. In 1987, Keith Kintigh published a very prescient paper on the future of quantitative methods in archaeology. He argued that archaeologists needed to reconsider their tendency to uncritically borrow quantitative methods from other disciplines and, instead, to begin to develop methods sensitive to the unique problems of archaeological inference. He further suggested that this be done in combination with those primarily intuitive aspects of traditional archaeology that have served the discipline well in practice. Since the publication of his paper, many archaeologists have taken his suggestion seriously and have made significant progress toward the development of these designer methods. This can be attributed to a number of positive developments: improved training of archaeologists in quantitative methods (see below), a general willingness on the part of archaeologists to seek out specialists to assist them in the development of new methods, and, perhaps more importantly, a better understanding of the logical and quantitative structure of archaeological data. Particularly impressive progress has been made in the development of methods for faunal, lithic, archaeometric, and spatial analyses.

Approaches to Design

There are three broad approaches to the creation of designer methods, each with peculiar strengths and weaknesses (cf. Read, 1989, p. 23). One is for archaeologists to become adept at the use of quantitative approaches within their particular subfield through deeper training. Another is to engage interested professional statisticians, mathematicians, and other specialists in a joint effort to explore the problem at hand. The third is to rely on the applied work of these same interested statisticians and mathematicians. The former two approaches are certainly not new; we have always had the self-taught as well as the collaborators since the general introduction of quantification to archaeology, but the third is fairly recent in origin, beginning some time during the 1980s. To evaluate the potential as well as the accomplishments of these approaches, I reiterate Kintigh's desire: to develop more useful quantitative methods sensitive to the unique problems of archaeological inference.

The most obvious advantage of the first approach is that the archaeologist, by merit of training, has a deep understanding of the structure of archaeological data within a particular subfield and is aware of his/her ultimate goals for research. The obvious drawback of this approach is that it can be extremely time-consuming to accomplish given the demands of becoming both a good archaeologist and a good mathematician or statistician. What seems critical here is that the nascent specialist devotes his/her time to a subfield, and not to the issue of "quantification in archaeology" as a whole. While we will always need generalists in quantitative methods who are willing to provide overviews of the state of development of the field, my belief is that Kintigh's desire for robust methods will emerge primarily from this pool of subfield specialists with sufficient quantitative training and archaeological expertise. That such individuals are appearing cannot be questioned, as I hope to show in the discussion of different subfields that follows.

Collaboration between archaeologists and quantitative specialists, however, has its place and must not be dismissed. The advantage here is that the archaeologist need not become adept in matters numerical but, instead, can rely the hard-won expertise of someone else. The key, obviously, is finding a specialist with the right training and the flexibility of mind to grasp the essentials of the archeological problem simultaneously with the most appropriate quantitative measures. This tends not to be easy: "Unfortunately, not all archaeologists regard statisticians as useful creatures, and there are, in any case, not enough interested statisticians to go around" (Baxter, 1994a, p. 219). Another drawback of collaboration is that the hired gun will, because of personal interest and training, be prone to fit particular models to archaeological situations with only a cursory understanding of the real demands of the data set and problem, and despite the best efforts of the archaeologist to keep the analysis on track. Baxter (1994a, pp. 222–223) discusses this issue in regard to the application of Bayesian methods to archaeological problems.

Collaboration has been and will continue to be important as cutting-edge mathematics and other quantitative approaches are imported to very specific archaeological problems. However, special caution must be employed in the use of collaborative approaches to insure that there is no repeat of indiscriminate borrowing; just because something can be done does not mean that it should be.

The third source of design, nonarchaeologists building methods for archaeologists, took me by surprise as I began to develop materials for this review. It is best reflected in the pages of *Journal of Archaeological Science* throughout the late 1980s and mid-1990s, although it can be found in other publications. Here we have statisticians and others wading into archaeological waters, often in a highly critical manner. A good example of this sort of design is reflected by a series of papers on bone counts and statistics (Pilgram and Marshall, 1995; Ringrose, 1993, 1995). Ringrose (1993, p.

121), a professional statistician, reviewed the analysis of quantified vertebrate faunal data as performed by archaeologists, and had some very strong opinions on both the strengths and weaknesses of such work:

> It is shown that many of these [quantification methods] lack logical or statistical validity, and that substantial claims have been made with little justification. It is suggested that greater co-operation between archaeologists and statisticians would be an important step towards remedying this situation.

One of the papers criticized was that by Marshall and Pilgram (1991), who used multiple regression to examine vertebrate body-part representations at archaeological sites in the light of meat versus within-bone nutrient contributions to the diet. After give and take, one conclusion drawn by Ringrose (1995, p. 102) was that

> I [Ringrose] was guilty of rushing into print thinking that I knew more about bovid anatomy than I in fact did whereas Marshall and Pilgram (1991) were guilty of rushing into print thinking that they knew more about regression than they in fact did.

In fact, both sides made telling criticisms of each other, but right and wrong (or correct and incorrect) is only part of the issue; what is also pertinent is the whole style of argument. Ringrose, in his original critique and despite often being correct, made the same sort of mistakes as did the New Archaeologists of the 1960s and 1970s during their borrowing frenzy: they assumed they knew more than they actually did. The danger for nonarchaeologists offering us quantitative gifts, then, is that while they may develop useful methods or offer valid critiques, they also will make enough archaeological mistakes to retard or prevent the acceptance of their methods or blunt the force of their observations. Archaeologists of a quantitative bent should watch this source of design carefully and collaborate when possible.

Faunal Studies

Arguably the most robust development of quantitative methods in any subfield of archaeology is in the area of faunal analysis. Methods specifically tailored to the analysis of faunal data were developed during the 1950s (White, 1953), reviewed during the late 1970s and early 1980s, (Casteel and Grayson, 1977; Gilbert and Singer, 1982; Grayson, 1979), summarized by Grayson (1984) and others throughout that decade, and reevaluated once again in the 1990s by Brewer (1992), Lyman (1994a), and others. Hundreds, possibly even thousands, of articles creating new measures and applying them to specific assemblages have been published since the 1950s. Lyman (1994a, pp. 40–47) lists 112 different measures defined and employed by zooarchaeologists in the English-language literature over the period 1977-1992. While the validity and importance of some of these measures can be questioned, it nevertheless remains the case that few other aspects of archaeological inquiry can match this level of productivity directed at applied analysis. Among the topics that have benefited from this effort include the development of utility indices, which measure in some way the economic "value" of the faunal materials present in an assemblage (Binford, 1978; Lyman, 1992; Metcalfe and Jones, 1988), the role of bone density and how it affects skeletal element survivorship in the archaeological record for a wide range of species (Lyman, 1984, 1992, 1994b), the application of diversity analysis to faunal assemblages (Cruz-Uribe, 1988; Meltzer *et al.*, 1992), the use of statistical procedures to define intraspecific variability so that males and females can be reliably distinguished (Crabtree, 1993; R. Thomas, 1988), the determination of whether a faunal assemblage is the result of scavenging, hunting, or some mixture of the two (Stiner, 1991), and the measurement of various taphonomic effects on faunal assemblages (Marean, 1991; Marean and Spencer, 1991; Marean *et al.*, 1992).

What appears to explain this apparent success, especially when compared to other methodological foci, such as lithic analysis? As a student of both lithics and quantitative methods, I have always had a certain envy of my zooarchaeological colleagues, and I recall the jealousy I felt when reading Grayson's book. While the jealousy might have been misplaced, there are good reasons why faunal studies are more quantitatively precocious.

One obvious advantage is that animal skeletons are finite entities. That is, depending on the species, they are composed of fixed and invariant numbers of bones that are articulated in a single manner. In one sense, nature has "standardized" the object of study. Thus animal bones of sufficient size or possessing identifiable landmarks can be identified as to their place in the skeleton (i.e., the skull, left humerus, etc.). These invariant properties are, of course, contingent upon the successful identification of the species, but once this is achieved, quantitative estimates of numbers of individuals can be made.

Despite inevitable disagreements, faunal analysts have come to a remarkable level of consensus on basic definitional issues. While scientists from other fields may express shock and possibly some amusement at this statement, such agreement on foundational issues in archaeology is often rare. For example, while confusion has not been totally eliminated, faunal analysts know for the most part what it is they are counting and, perhaps more importantly, why. Lyman (1994a) discusses the varied, but generally consistent, definitions of MNI (minimum number of individuals), MAU (minimum number of animal units), NISP (number of identified specimens), and MNE (minimum number of elements), for example. Most faunal analysts are now aware of the different contexts for the proper use of these different ways of counting.

The recent dramatic increase in the numbers of quantitative measures in faunal analysis is, in part, due to a change in the goals of analysis. As Lyman (1994a, p. 50) has noted, the goals of analysis during the period 1950 through roughly 1980 were concerned primarily with methods useful for the assessment of the taxonomic abundance of different species in archaeological assemblages. In contrast, the past two decades have focused their attention upon evaluations of the taphonomic condition of assemblages, thus requiring new measures and new definitions. I expect that this trend of ever more specific and well-considered development of quantitative measures of faunal analysis will continue unabated.

Lithic Studies

Although progress in the development of robust quantitative measures useful for lithic analysis does not compare to the success achieved in faunal studies, the past decade has seen the emergence of a number of potentially important methods. One of the problems with lithic analyses is really a problem with the medium: it is subtractive, and except for a very limited number of reduction strategies, most notably blade manufacture, little can be known directly of the prior state of the object before reduction. Everyone knows that the human body has 206 individual bones, but how much debris (or blades, or scrapers, for example) does a core have?

Those methods that have had the greatest success, or potential for it, have engaged in basic definitional issues, albeit with mixed success. One area of great promise is the analysis of flaking debris (Shott, 1994). Debris is ubiquitous at most archaeological sites, and archaeologists have for generations attempted to make sense and find uses for this abundant data class. Earlier generations of archaeologists, perhaps best characterized by Binford and Binford's analysis of Mousterian assemblages, simply included raw counts of debris or debitage as a column or row entry for submission to a multivariate method (choose your poison: factor, cluster, or correspondence analysis, principal components, etc.). The mutual covariation of this data class with others was said to have meaning and, thus, was interpreted. These sorts of studies have declined since their heyday in the early to mid 1980s (Aldenderfer 1987b, pp. 95–102).

They have been replaced by a new generation of studies that seeks to find meaningful behavioral structure within the data class itself. There are two basic approaches to the analysis of reduction debris: individual flake analysis (IFA) and mass analysis (MA). The former, perhaps best characterized by Sullivan and Rozen's (1985) approach, uses counts of debris classes and measures of certain key attributes to define differences between assemblages, usually with the aid of some multivariate method. IFA is labor intensive, and, as Shott (1994, pp. 78–79) has pointed out, subsequent experimental study has cast doubt on many of its assumptions. Mass analysis, in contrast, is a more rapid approach that looks at debris in the aggregate, and uses counts and ratios of different size and weight classes, often with the assistance of discriminant analysis and the results of a series of well-defined experiments. Although Ahler's (1989) methodology is the best known, variants of MA have been developed by Stahle and Dunn (1982) and Patterson (1990). The latter models are interesting, in that they suggest different sorts of reduction strategies may have different distributional forms: thus Stahle and Dunn found that the Weibull distribution was a good descriptor of a number of debris size/class assemblages, while Patterson suggested that biface reduction is best described by a concave distribution of percentage of flakes as graphed against flake size, which becomes log-linear after transformation. Analysis becomes an exercise in curve fitting. Unfortunately, experimentation has uncovered a number of serious flaws in Patterson's method. Shott (1994, pp. 97–98) has suggested that some debris size class distributions may best be modeled by the log skew Laplace model, which has had great success in sedimentology (Fieller *et al.*, 1992). The ultimate success of these models of debris analysis will depend on the ability of their developers to move beyond simple mathematical description of an assemblage of size classes and give behavioral meaning to these distribution forms.

Sporadic attempts have been made to create a quantitative basis for microwear analysis. Some scholars, dismayed by what they see as a deplorable lack of rigor and replicability in microwear studies [Newcomer *et al.* (1986); but see a solid rejoinder by Bamforth (1988)], have sought to create quantitative descriptions of microwear polishes, in particular using interferometry (Grace *et al.*, 1985) and fractals (Rees *et al.*, 1991). These approaches have had very limited success, leading latter authors to make the strong claim "that there was no possibility of identifying worked materials from microwear polishes alone" (Rees *et al.*, 1991, p. 639). While a severe statement, it does suggest that the quantification of microwear polishes is a more complex business than has been heretofore appreciated, and indeed, a fractal approach combined with structural studies at the molecular level using the atomic force microscope might be profitable.

Simple, more empirical approaches, however, have had some success. Kuhn (1990) describes a very simple heuristic for the measurement of an index of reduction for unifacial side scrapers. Limited experimental data suggest that the measure is robust and can, thus, provide a useful measure of the extent of tool reduction in a number of applied examples. The success of this simple index suggests that similar treatments of other classes of scraping tools would likewise produce useful results. Wynn and Tierson (1990) have developed a similar approach to describe Achulean handaxes. Similar quantitative studies of artifact form and shape have had a long history in archaeology (Read, 1989, pp. 30–34), and have been of general interest to statisticians as well.

Archaeometric Studies

As the name implies, archaeometry measures things within an archaeological context, and although definitions vary somewhat, most agree that archaeometric studies are principally concerned with the compositional analysis of archaeological materials ranging from ceramics to metal to glass to obsidian (Baxter, 1994a, pp. 12–15; Bishop and Neff, 1989). Once the elemental composition of a set of artifacts is determined, it is frequently possible to use these data to investigate a variety of archaeological problems, including intraregional production and exchange, long-distance trade, and craft specialization. Compositional analysis also is commonly used in authentication studies of artifacts in cases of fraud, identification of forgeries, and theft of national patrimony.

Unlike faunal or lithics studies dependent upon quantitative analyses, compositional analysis is heavily dependent upon multivariate statistical methods. There are a number of reasons for this, with the most obvious being that large suites of elemental data are required to make fine distinctions among the various chemical compositions of different artifact types derived from distinct raw material sources. While descriptive methods are commonly used to explore data structure, these are seldom sufficient to make a determination of provenience.

Baxter (1994a, pp. 12–15) found in a review of the archaeometric literature over the period 1968-1991 that the most commonly used methods were some form of cluster analysis, which appeared in over 70% of the papers, discriminant analysis (31%), and ordination (28%; the total is greater than 100% because some papers used more than one method). This reflects the reality of archaeometry: clustering tends to be used in situations in which there is little or no prior information regarding the number of possible sources of the material in question; discrimination is used when at least some of the sources are known and the goal is to assign new specimens to existing sources; and ordination is used to reduce dimensionality in complex data to create new variables, investigate aspects of intercorrelation between elements, or to represent these data in a graphical manner in either two or three dimensions (Bishop and Neff, 1989, pp. 63–64).

In the strictest sense, archaeologists don't design new multivariate methods for archaeometric analysis, but instead apply existing methods to their collections. This, of course, can be done poorly or well, and there has been much discussion within the archaeometric literature concerning the development of useful models for specific kinds of research problems or for specific data types. Indeed, the successful development of such models serves to satisfy Kintigh's desire to develop methods attuned to the niceties of archaeological inference. For example, Bishop and Neff (1989, pp. 69–70) make a strong case for modeling, rather than simply summarizing, compositional data. That is, since any multivariate method will discover and often impose some sort of structure on a set of data, it behooves the investigator to think deeply about just what might account for elemental composition in the sample. They illustrate the difference between modeling and summarization with an example of ceramic analysis focused upon distinguishing the effects of tempering versus natural elemental compositions in clay sources when attempting to determine the sources of different pottery types. They show that, without careful model development, it is very easy to confound their effects and make serious errors in interpretation. Arnold *et al.* (1991) have developed this argument further.

Other authors have reviewed the use of various aspects of compositional analysis with the goal of examining details of specific methods or details of analysis. Baxter (1992, 1994b) has reviewed the use of discriminant analysis in compositional studies, Harbottle (1991) has examined differences in the use of Mahalanobis and Euclidean distances in compositional studies of ceramics, Baxter (1991) and Baxter *et al.* (1990) have compared principal-components and correspondence analysis as applied to compositional data, while Baxter and Heyworth (1991) discuss correlation matrices in the context of compositional studies. Baxter (1994a) provides a very thorough collection of references of applied analyses in compositional studies that spans more than two decades of research.

Spatial Analysis

Archaeological data are inherently spatial, and as a consequence, archaeologists have been long interested in methods for the analysis of spatial data (Aldenderfer, 1996). Indeed, archaeologists did much of their earliest borrowings of quantitative methods from human geography. Hodder and Orton's (1976) *Spatial Analysis in Archaeology* is an excellent summary of the kinds of quantitative methods borrowed and the problems to which they were applied during this period. Since then, archaeologists have expanded the range of techniques used as well as the kinds of problems they have chosen to investigate using them. However, the basic themes have remained the same: the identification of spatial anomalies (much more or much less of a phenomenon observed in some set of locations), coincidence (phenomena observed "together"), proximity (the strength of coincidence), dependence (the degree to which some spatial pattern is autocorrelated), and heterogeneity (how varied is pattern across some sample or population) (Goodchild, 1996). Within archaeology, these themes have been explored traditionally at either the regional or intrasite level, although a number of recent spatial approaches have advocated the use of a nonsite methodology that blurs such a facile distinction (e.g., Ebert, 1992; Ebert *et al.*, 1996; Kvamme, 1996).

The methods employed in these archaeological analyses of spatial data have varied along a familiar continuum—from relatively intuitive, pattern search methods best exemplified by *k*-means clustering of intrasite data, to more formal, deductive methods that range in complexity from the simple, such as calculated measures of spatial autocorrelation (Kvamme, 1990b), to the difficult, such as log-linear models of site location preferences (Maschner and Stein, 1995). Spatial statistics, that is, formal statistical models adapted to spatial data, have been used infrequently, a situation that appears to be due to lack of education rather than lack of interest in these methods (see below).

Predictive or locational modeling now dominates the analysis of regional-scale archaeological data. While locational modeling has always been of interest to academic archaeologists, its recent development has been of considerable interest to cultural resource managers (Judge and Sebastian, 1988; Kvamme, 1990a). At the most general level, predictive models attempt to develop a statistical description of site location on the landscape with the goal of predicting for unstudied but similar areas the probability of the presence of archaeological sites. The attributes used to develop predictions are usually environmental in nature, but in theory, they could consist of any attribute set. Simple predictive models tabulate the occurrence of sites across these environmental variables, while more complex models, such as those by Maschner and Stein (1995), attempt to develop a more rigorous statistical basis for predicting site locations. As Read (1989, p. 60) notes, however, predictive modeling is fraught with many problems relating to sampling, population definition, and spatial heterogeneity, which, if not considered, are likely to lead to the development of an inadequate or very incompletely specified set of predictions. Many critics of predictive modeling have noted there is a strong air of environmental determinism with the whole approach, especially with those models that work within a geographic information systems (GIS) context. This critique has led to the development of other theoretical bases for modeling locational choice, such as ritual, boundary maintenance, and cost–benefit analysis. While most of these also are conducted within a GIS framework, each of them calculates something, such as a cost surface using a simple empirical estimate of movement up and down slopes (Limp, 1991), sets of Theissen polygons that define presumed political boundaries within the Valley of Mexico (Ruggles and Church, 1996), or combinations of viewsheds from barrow mounds that appear to define social boundaries in Neolithic England (Wheately, 1996). While many of these models are elegant, most suffer from a failure to connect that which is calculated (the empirical measure used as the basis of the model) to the anthropological process being modeled.

Intrasite analysis, in contrast, is now exploring in depth the connection between method and anthropological process. The early development of intrasite analysis was almost wholly empirical and was devoted to the borrowing or discovery of methods that could discover spatial pattern primarily in terms of anomaly and coincidence. Most of the methods borrowed came straight from human geography (Dacey, 1973), or were novel inventions on the part of archaeologists, like Kintigh and Ammerman's (1982) *k*-means clustering or Whallon's (1984) unconstrained clustering. As a number of authors have noted, the failure to connect spatial pattern with anthropological process led to frustration and outright rejection when these methods were applied to real archaeological data and led to a number of reviews of foundational issues and comparisons of method performance using either simulated or ethnoarchaeological data (Blankholm, 1991; Gregg *et al.*, 1991; Kintigh, 1990). Others, such as Orton (1992, p. 138), have called for new statistical approaches that seek to define edges of artifact distributions and segregate them from others. It seems clear that the continued development of intrasite methods will take place within a solid anthropological context and that useful and robust methods appropriate for empirical research will emerge from this effort.

THE RETURN OF WHOLE-SOCIETY MODELING

One of the theoretical hallmarks of the "New Archaeology" was the systems approach (Aldenderfer, 1991), and a result of its adoption was the use of computer simulation to model whole societies or significant portions of them. Considerable effort was spent to identify appropriate subsystems and the variables and attribute states thought to describe them. There was a belief that models should be complex so as to capture as much of "reality" as possible. These models were then programmed and were experimented with to understand their behavior under a variety of conditions. Although strong claims were made that this sort of dynamic modeling would lead to the development of innovations in archaeological theory, this desire never really materialized, and instead, simulation modeling found a different, perhaps more useful, niche as an adjunct to middle range and formation process theory (Aldenderfer, 1991).

During the 1980s, relatively few archaeologists continued to advocate whole-society modeling, the most prominent of them being James Doran, who has called for the "formal modeling" of societies, especially the interaction of their political and sociological subsystems. Doran's (1990) models are more formal than quantitative, in the sense that while they can be simulated on a computer, they are not "calculated" and are not dependent upon some set of mathematical or statistical formulae for their operation. Instead, they depend mostly upon rules, decision trees, and the effects of probabilistic outcomes of different choices of rules under changed circumstances. While much of Doran's work has been widely cited within the relatively small community of mathematically inclined archaeologists, his work has had relatively little influence beyond this small circle. This has less to do with the quality of his models than their theoretical provenience: his work has always been strongly influenced by cognitive and computer science and artificial intelligence (AI) models, scientific fields that are poorly understood by most archaeologists.

The decline of whole-society modeling can be attributed to a number of factors, the most prominent being the collapse of the systems approach as a well-regarded theoretical perspective and its replacement by new theoretical approaches that have emphasized methodological individualism and the resurgence of interest in decision making at a disaggregated level. These new approaches span a continuum of interest, from so-called cognitive archaeology to neo-Marxism. Many of these newer theoretical perspectives are avowedly postprocessual; those with more stridently antiscience flavors pointedly reject modeling efforts like those associated with systems thinking, labeling them as "objective," and therefore artificial and imposed. In these circumstances, then, it is no surprise that whole-society modeling is not common since the theoretical goals of many archaeologists are no longer sympathetic to this end.

Despite this, a number of archaeologists have been working quietly to integrate what they see as interesting and useful commonalities across a number of scientific disciplines, ranging from mathematics through ecology to computer science, about the behavior of so-called complex adaptive systems (Holland, 1992), and how a deeper understanding of these systems might lead to new and robust theories of social change. Unlike the older systems approach, which modeled systems under conditions of homeostasis and equilibrium, where change originated from "outside," complex adaptive systems approaches exploit the power of nonlinear dynamical mathematics to create models that exhibit instability and chaotic behavior, and where change is generated endogenously. Nonlinear dynamics theory (also known as chaos or complexity theory) has been around for some time. Because nonlinear effects are computationally difficult for all but the very simplest systems, model behavior can be observed only through computer simulation, although it is often possible to obtain qualitative insights about system behavior through their heuristic application [e.g., see Aldenderfer (1998) on the sensitivity of small animal populations to nonselective, nonconserving hunting].

Regardless of their origin, what is of greatest importance concerning models based on nonlinear dynamics is to understand that they are capable of dealing with large, complex, and disaggregated systems. This goes far to satisfy the demands from archaeological theory that we move to actor-based approaches and away from models of aggregated behavior. Depending on the source of the model, however, archaeological applications of nonlinear modeling have taken distinctly different forms. Biskowski (1992), for example, models resource procurement as a decision model from an expert system-AI approach, in which individuals evaluate the probability of successfully obtaining their goals depending upon complex webs of individual relationships, availability of surplus, and future requirements of both the borrowers and those borrowed from. McGlade (1995), using nonlinear mathematics, shows how modeling individual decisions about land use in a large, disaggregated population can lead to qualitative estimates of the sensitivity of the system and the likelihood that it will become unstable and fall into chaotic behavior. Kohler (1995) has developed a very interesting and very complex model that seeks to understand the process of village formation in the American Southwest. His model is based explicitly upon Holland's (1992) work on complex adaptive systems and uses an agent-based simulation modeling methodology—Swarm—developed at the Santa Fe Institute, a leading center of research into the application of nonlinear models across a number of scientific fields. The model is structured around microeconomics and decision making under constraint and incorporates sharing, resource availability and abundance, and agricultural productivity under environmental variability. It also is explicitly agent-based, in that decision making is modeled at the household level, and the number of potential households is very large. An important feature of the model is that its behavior will be compared to a fine-scale archaeological record of a small area in southwestern Colorado. Doran *et al.* (1994) and Palmer and Doran (1993) have modeled various aspects of social change in the European Upper Paleolithic using models derived from the theory of distributed

AI. A review of online lists of forthcoming conferences reveals that models based on artificial life approaches, cellular automata, and genetics are on their way.

What should we make of these models? One of their principal advantages is that they are consistent with the demand of archaeological theory for disaggregated models of human behavior. This alone should help them gain at least a reading, if not widespread acceptance. Their true test, though, is whether or not they will really model social change as advertised. Although archaeologists have long argued that our field is in a unique position to examine trends and consequences of change over the long term, we have delivered relatively few convincing and useful models of that change. Few of our models have been used by other disciplines, let alone our colleagues in other branches of anthropology. Perhaps these interdisciplinary models, if they provide real insight into the past, will give archaeology a broader intellectual hearing. Working against this possibility is the sad truth that there are still relatively few consumers in our field, let alone in others, that are capable of developing these models and interpreting their results. In the field of whole-society modeling, then, we archaeologists are going to be very much dependent upon collaborators to help us create these models. But if these models are to have any real effect on anthropological theory, the archaeologists involved are going to have to be a special breed of person, sophisticated in complex mathematics and computer simulation, but also connected to the field and empirical data and capable of dealing with new anthropological theories: a daunting task, but there is promise, as I hope I have shown.

TEACHING QUANTITATIVE METHODS IN ARCHAEOLOGY

Quantitative methods, of course, are now firmly a part of the archaeological establishment. Introductory texts, such as that by Renfrew and Bahn (1996), provide numerous exemplars of the application of quantitative methods to actual archaeological data, ranging from the simple (MNI, NISP, and relative bone weight) to the complex (multidimensional scaling used to reconstruct Mycenean regional systems). While these examples are not meant to teach students how to use these techniques per se, they do show the novice that measurement and numbers are an essential part of the research process within the discipline.

It is one thing to show undergraduates (or graduate students, for that matter) clear examples of sound research using quantitative methods but quite something else to teach them to use them effectively. While many authors have noted that the use of quantitative methods has improved markedly since the publication of Thomas' (1978) indictment, there are still plenty of mediocre and downright poor applications of quantitative methods to be found in our journals and books.

Although misapplications and fundamental errors in the use of quantitative methods will never be fully eliminated from the field, it is reasonable to ask whether or not the currently mixed situation can be improved. There are three areas to consider: what sorts of materials are actually being used in graduate and undergraduate education today, what we should be teaching, and how we should be teaching it.

Materials

In the spring of 1996, I sent out a request for information to the two major, general listservers for archaeology—Arch-L and Arch-theory. Among other things, I asked the participants on the list to tell me if their departments or programs required courses in quantitative methods for either their graduate or undergraduate degrees, what texts they used, and what general instructional approach was used. I received approximately 50 unique responses, mostly from North America (i.e., different departments and programs). This represents about one-sixth of the undergraduate programs offering majors or some focus in anthropology but about five-eighths of the graduate programs offering the Ph.D. While this is not a statistically valid or comprehensive sample, it provides us with a point of departure.

Almost no program required undergraduates to take a course in quantitative methods. This is no surprise, in that at many universities anthropology is seen as a service discipline with relatively few majors, and required quantitative courses, rightly or wrongly, are often seen by department chairs as a detriment to obtaining more majors. A paucity of majors is anathema to an administrator: witness what happened to geography at many major universities throughout the 1980s. About one-half of the undergraduate programs did offer some sort of course in the anthropology department designed to teach quantitative methods, and most respondents said that students were encouraged to take them. Two texts dominated undergraduate courses: Thomas' (1986) *Refiguring Anthropology* and Shennan's (1988) *Quantifying Archaeology*.

In contrast, about 60% of the graduate programs required archaeologists to take at least one course in quantitative methods, and of these, more than half had at least one course specifically designed to teach quantification within an archaeological perspective. Most of those programs that had their own quantification course used Shennan's text or a combination of papers gleaned from major journals and edited volumes; Thomas' book was little used. Programs without specialty courses suggested that their students take quantitative methods from departments of sociology, geography, statistics, and psychology.

There is good and bad here. Undergraduates, except for the most motivated, are unlikely to ever see a quantitative class, and therefore, many are poorly prepared for graduate study. Those who choose to go into cultural resources management also will be poorly trained, a perennial complaint from the managers who hire them. In contrast, graduate programs seem to be making a serious effort to provide their students with this training. However, the number of programs that do not have their own specialty courses suggests that Kintigh's desire for useful methods to be developed primarily by archaeologists may well take some time to realize more fully.

What Methods Should We Be Teaching?

Orton (1992, p. 139), while not identifying specific methods or techniques, has made a clear call for a "bottom-up" or "basic needs" approach to the training of archaeologists in quantitative methods rather than the "trickle down" approach he believes has characterized how our students learn about quantification. While his intent is to ensure that students are well grounded in fundamental concepts of quantitative analysis, there are many possible ways in which this goal might be realized.

At the undergraduate level, students should be given a firm grounding in basic probability and statistics. Just how to deliver that grounding, though, remains controversial (see below). Ideally, this would include basic descriptive and inferential statistics and data modeling through simple regression. Both texts in current use today accomplish these ends. A course like this also might focus upon methods and analytical approaches of current importance to archaeologists, such as diversity analysis and simple spatial methods. Graduate-level courses should follow two tracks: an overview of multivariate methods of great utility in our field (principal-components/factor analysis, log-linear modeling, correspondence analysis, cluster analysis, and discriminant analysis). Baxter's (1994a) text is an excellent overview of these methods in archaeological contexts. The second track should consist of training, as required, in the methods of specific subfields. Such courses should emphasize model building and validation as well as provide an overview of current technique.

The degree to which exploratory data analysis (EDA) is integrated into this basic curriculum also should be considered. Developed originally by Tukey (1977) and his colleagues as a more robust approach to data description that was said to allow pattern to emerge more clearly from complex data, archaeologists have elevated EDA to something of a philosophy of data analysis that is often seen as contradictory or opposed in some sense to classical, or confirmatory (or inferential), methods. Despite this, relatively few archaeologists have used EDA extensively (Voorrips, 1990, p. 8). Until recently [but see Drennan's (1996) excellent book], there has been no text of archaeological statistics that has integrated an EDA approach with classical methods.

I strongly believe that EDA has a major role to play in archaeological inquiry. While admittedly many of the methods espoused by Tukey are somewhat obscure, his and others' emphasis on defining more data types beyond those accepted by classical methods, the use of transformations, and the ways in which graphical methods are integrated into the research process are enormously helpful and should become part of every graduate student's analytical repertory.

What about spatial methods? No graduate program surveyed requires their students to take any sort of spatial statistics, analysis, or modeling course, although they strongly encourage them to do so. This is ironic, in that our literature is filled with articles on the application of this or that spatial method to archaeological situations. I think this lacuna exists because there are so very few archaeologists with formal training in this field, and that there remains much for geographers to do regarding basic definitional issues. As Kvamme (1993, 1994) continues to remind us, spatial statistics are quite different, and generally more complex, than their nonspatial counterparts.

The role of geographical information systems (GIS) remains equivocal as a substitute for training in spatial analysis. GIS enjoys something of a vogue in archaeology today and for good reason, given the real assistance it has provided archaeologists in a plethora of applications (Aldenderfer and Maschner, 1996; Maschner, 1996; Petrie *et al.*, 1995). However, geographers and others continue to debate whether or not GIS is its own analytical strategy with revolutionary potential (Marble, 1990) or simply a tool that makes life easier for anyone working with spatial data. There is no question that, until very recently, analytical options in the sense of spatial statistics were essentially nonexistent in most major GIS packages and that the methods employed by them were ad hoc and relatively simplistic. As more GIS packages strengthen their analytic capabilities, we can expect more emphasis by archaeology programs for a solid grounding in basic GIS skills that, ideally, will integrate spatial analytic methods more effectively into the curriculum.

Finally, more comprehensive training in the use of radiocarbon dates should be integrated into the curriculum, probably in the basic undergraduate class. Most professionals are aware that dates are actually statistical phenomena, but I think most of us would agree that the systematic and widespread abuse of radiocarbon dates abounds. If anything, this situation is worsened by the improved quality of calibration programs, such as that by Stuiver and Reimer (1993). Although Thomas (1986) has an extensive section on radiocarbon dating in his introductory text, few students seem really to understand dates or know how to use multiple dates from a single provenience successfully in the most effective manner or to connect these multiple dates to the calendar scale. While many of the statistics are indeed complex, especially those methods that advocate a Bayesian approach (Buck *et al.*, 1992), there are a number of recent papers that attempt to make these innovations more accessible to the end user (Aitchison *et al.*, 1991; Buck *et al.*, 1991, 1994; Dehling and Plicht, 1993).

How Should We Be Teaching Quantitative Methods?

I suppose I should really not admit this, but my first experience with quantitative methods, a statistics course in sociology as an undergraduate, was a disaster. Whether it was the instructor or my own thickness, I simply had a difficult time understanding just how these methods would make me a better archaeologist. While I look back on those days with some chagrin, I firmly believe that many archaeologists of my generation and those subsequent have had the same feeling: statistics, in particular, and quantitative methods, in general, were something that had to be suffered through and could be quickly forgotten once the course was over.

This perception, that statistics are of little daily value and that students suffer them for the most part only because they have to, is not a phenomenon limited to archaeology. Indeed, this is a problem that has worried professional statisticians for at least three decades: what is the proper way to teach statistics to both graduate and undergraduate students such that once the course is complete, they will both understand and then use statistics in the correct manner to solve everyday problems? (Simon and Bruce, 1991, p. 22).

One approach for statistical education, first developed over almost thirty years ago by Julian Simon, an economist charged with teaching statistics to undergraduates, is the resampling method of data analysis, which is only now gaining a measure of broader acceptance (Peterson, 1991). Resampling, also known as the bootstrap method (Efron and Diaconis, 1983), is a computer-intensive method of data analysis that uses simulation to create many (often thousands) of samples of a known data set to extract as much information as is possible from it while avoiding the use of statistical formulae. Through this process, the user is able to assess the degree to which a result of an experiment is a likely or unlikely result. Because it focuses upon the data at hand, it avoids making any sorts of statistical assumptions (i.e., Gaussian curve) and lets the data "speak for themselves." While critics note that the quality of the analysis is dependent wholly upon the adequacy of the sample of observations at hand (the primary assumption of resampling), its proponents argue that, although this is true, resampling forces the user to think more carefully about data and their variability in a creative manner instead of trying to force data into a probably ill-chosen formula selected on the basis of rote learning. In short, the resampling method emphasizes the process of reasoning and the use of intuition rather than the details of formulaic approaches (Simon, 1993, 1994, p. 290).

If sampling and the bootstrap method are so appealing, why have they not seen more use in introductory statistics classes? Simon suggests that the apathy and hostility resampling has faced is in part generational: most teachers of introductory statistics learned it the old way and generally see no reason for change (Peterson, 1991, p. 58). While professional statisticians have adopted resampling as an approach for solving particularly difficult problems in mathematical statistics (Edgington, 1995; Efron and Tibshirani, 1993; Good, 1994), there has been little trickle-down to the undergraduate level. Although not discussed, I also imagine that the demands of the job market, which places priority upon classical, inferential methods in virtually all fields, has retarded the development of a comprehensive approach to the use of resampling in undergraduate courses.

I should stress that resampling is somehow not antithetical to classical inferential methods, and the two can be taught simultaneously. Simon (1993) has published a text that does just this; the examples he provides, ranging from confidence intervals through probability theory, convincingly show that the two approaches can work together successfully. It is important to note, however, that resampling does demand a different logic, and instructors should be prepared to revise their courses accordingly.

While I have seen no mention of resampling per se in the archaeological literature, especially as it pertains to educational issues, a few archaeologists have experimented with it. Kintigh (1984) has used Monte Carlo sampling to generate pseudo-confidence intervals around the results of diversity analyses and *k*-means clustering of spatial data; Ringrose (1992) uses the bootstrap to evaluate the results of correspondence analysis in a similar fashion. This use of resampling will likely continue to grow in popularity in our discipline. As to resampling's potential effect on the educational process, I think it is difficult to predict. I do believe, though, that given the problems we face with the interpretation of variability in our data sets and the inability of classical methods to assist us under normal circumstances (i.e., small samples, nonnormal distributions, poorly specified models, etc.), resampling, if it can help us and our students to think more carefully about data and its analysis, should be given a try.

CONCLUSIONS AND SUMMARY

I hope this review has shown that the application of quantitative methods to archaeology is very much alive and, in some subfields, very robust and of considerable utility. It seems clear that the most important determinant of the relative success in the creation of *useful* quantitative methods is the development of a quantitative idiom within a subfield that connects theory, goals, method, and practice. In effect, this is the context within which Kintigh's desire for methods appropriate to archaeological research will be realized.

From where, though, do we get our quantitative idioms? As I have pointed out elsewhere (Aldenderfer, 1987a, pp. 15–17), they emerge in great part from our theories. Theory defines what is of interest to us, and through theory we identify data useful in solving our problems. The choice of method, at least in part, is an exercise in the exploration of the implications of our theories. Clearly, those theories that emphasize the role of empirical data in archaeological research will be more likely to develop robust quantitative idioms than those that do not. Designer methods fit well into this context, because they are usually directed at solving some practical problem regarding data interpretation and evaluation within a problem-oriented context. As I noted, however, differences in the success of different subfields of archaeology in the development of these designer methods is highly dependent upon how well theory is articulated with empirical expectations derived from that theory.

Understanding the theoretical origins of the quantitative idiom also can help us understand the prospects of the long-term viability of quantitatively based variants of whole-society modeling. Although the theories from which these models have been developed are numerous, each of them has a strong articulation with empirical data; therefore, they actively seek useful methods that can help make sense of these data. Thus while Doran's formal models are very distinct from Kohler's Swarm-based models in quantitative structure, they both make predictions about the archaeological record. This willingness to use quantitative methods to develop predictions about some phenomenon within a theoretical context distinguishes these modeling approaches from those that appear to be content simply to develop new glosses or interpret some body of data. But the strength of these models—their "objective bias"— may be their greatest shortcoming, especially to those who have their doubts about the reality of empirical data.

Given recent trends in anthropology as a whole, the quantitative perspective these models employ almost insures that many anthropologists and increasing numbers of archaeologists will find their predictions either irrelevant or impenetrable (i.e., they don't understand the models because they have limited or no quantitative training).

Given this situation, it is imperative for the discipline to pay far more attention to both undergraduate and graduate training than it has done to date. Undergraduates will need early exposure to a broad range of quantitative methods, while graduate students will require in-depth training as well as mentoring in the subfields of their choice. Collaboration with non-archaeologist specialists will always be necessary since mathematics, statistics, and other sources of our borrowing are not static and will require interpretation no matter how sophisticated the archaeologist may be. However, as the pool of ever-more quantitatively adept archaeologists increases, we should expect that both the range and the quality of these useful methods developed primarily by archaeologists should increase dramatically.

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