

Prologue

We will draw the curtain and show you the picture.

—William Shakespeare, *Twelfth Night*

This book has had a very long gestation period. It began life in 1971 as a book-length manuscript that I used as the basic text for the hunter-gatherer course I taught for twenty years at the University of New Mexico. Some of my former students still recall the tattered, yellowed, foolscap pages, littered with typos, which they photocopied and exchanged with one another in their effort to learn more about the world of hunter-gatherers. By 1974, however, I had given up all thought of publishing “the H & G book” in its initial incarnation. My own field work had convinced me that organizing hunter-gatherer data in a case study or topical format would not be very useful to archaeologists—and besides, there were many other books that presented descriptions of ethnographic groups as examples of the life ways of small-scale societies.

My early manuscript had included tables of comparative data—some based on Murdock’s early cross-cultural research and others assembled from data presented by students in earlier classes, who had reported on those aspects of hunter-gatherer organization that were relevant to their own research. Unfortunately, the ethnographic groups for which there were “good data” were few in number. Even though cross-tabulations of data and some statistical tests filled many pages of my manuscript, I could never figure out how to use the results to develop an understanding of what the hunter-gatherer world was like and, more importantly, how it was organized.

True, I had chapters on demography, the use of space, mean household size, and other topical features that documented domains of cultural variability. In some instances, the patterns in these data even gave me some insight into how segments of a cultural system might be organized. Overall,

however, it seemed as though the harder I worked and the more traits or attributes I tabulated, the more unattainable a systems view of my subject matter became. As the number of paired comparisons—and the combinations and permutations of pairwise comparisons—increased, the less hopeful I felt that my mountain of information would ever lead me to an understanding of how hunter-gatherer systems were organized.

My former graduate students can verify that, from the outset, my efforts to develop a hunter-gatherer data base were accompanied by an exploration of various properties of the environments in which these groups lived. Using weather data, I had calculated various measures of temperature, and I had experimented with indices for evaluating rainfall in a biologically meaningful way. I discovered that more interesting and provocative relationships emerged when continuous environmental variables could be used to organize the comparisons among nominal variables. It also became clear that comparisons between continuous environmental variables frequently resulted in the organization of ordinal cultural variables into interesting patterns, especially when both the environmental and cultural variables were continuous in character. Unfortunately, there were very few reliable values for continuous variables in the ethnographic literature of hunter-gatherers.

One other major frustration in the 1970s was that the computer hardware to which I had access was anything but “user friendly” and the mainframes that I had begun to use while I was still a graduate student were simply not interactive on anything other than a geologic time scale. Today’s generation of graduate students must find it inconceivable that one

would attempt to process data using the dinosaur computers of the 1960s, and there were times when I thought so too, particularly when I was trying to wrangle large boxes of punch cards, which were inevitably full of errors and minor formatting problems—not to mention the problem of lost or mutilated cards.

As a result of these technological difficulties, along with a large body of field-based observations that I had accumulated and was committed to writing up, I had less and less interest in coping with my hunter-gatherer manuscript, and it gradually moved lower and lower on my list of intellectual priorities. I remained convinced, however, that finding a way to maximize the information in the ethnographic literature about the variability among hunter-gatherer societies had to be a major research priority for archaeologists who were interested in the 30,000-year time period between the Upper Paleolithic and the transformation to ways of life not based on hunter-gatherer strategies. The problem was not simply how to maximize the information accessible to archaeologists but, much more importantly, how to use that information as part of a methodology for learning about the past—particularly a past that was different in its range of organized system variants from what we know about the ethnographic present.

I was particularly disillusioned by the way most archaeologists used their knowledge about hunter-gatherers. The standard operating procedure was to extract isolated facts or behaviors from an ethnographic monograph and use them to “interpret” archaeological sites. Interpretations were then assumed to explain the features and properties of the archaeological record at specific sites, and this accommodative fit was then cited as the warranting argument for the accuracy of the interpretation. The dazzling circularity of this explanatory method guaranteed that making more ethnographic sources available to archaeologists would only make the situation worse!

I cannot emphasize strongly enough that the major problem this book addresses is *the development of a method for productively using ethnographic data in the service of archaeological goals*. This is not to say that the method I present in this book is the *only* procedure, but it is the only methodological tool that I have been able to develop which results in knowledge that is germane to archaeological problems. Former students will soon appreciate that this book bears very little resemblance to my 1970s manuscript. Its evolution was definitely gradual and occurred concurrently with the realization that my earlier attempts to record germane information about environments and habitats did not equip me to undertake a realistic study of human adaptations.

For example, I have learned that temperature is not just temperature. Some plants are more responsive to the prevailing temperature throughout the growing season, whereas others react to extreme temperatures during the nongrowing sea-

son. Similarly, rainfall occurring at the beginning of the growing season may be more important to horticulturists in some climatic settings than rainfall that comes at the end of the growing season. This understanding of some aspects of what the world was like forced me to take a very different approach to the documentation of environments. I needed to devise many different ways of measuring temperature and available water in order to understand the differences among habitats and the diverse challenges faced by hunter-gatherers seeking subsistence security in different settings. It was clear that I needed to return to methodological square one, and I began to research in a much more comprehensive way the environments in which ethnographically documented hunter-gathers have lived.

It took me two years to develop the data bases dealing with the world’s environments and the geographical distribution of documented hunter-gatherers. Once this aspect of the work was completed, it became clear that the limited range of hunter-gatherer characteristics upon which traditional cross-cultural studies had focused was not really relevant to most of the issues that I hoped to address in my book. I spent a great deal of time researching traditional anthropological interests, such as kinship, as well as the phenomena representing variability among hunter-gatherer educational systems. I also targeted areas of special interest to archaeologists, such as mortuary practices and the character of public rituals, as well as housing, settlement size and distribution, mobility, subsistence practices, and demographic properties.

Quite early in my research, I realized that many of these areas of interest were very difficult to describe dimensionally.¹ In other words, I needed continuous ethnographic variables that I could relate to the many continuous environmental variables that documented the diversity in hunter-gatherer habitats. The lack of precise information in many of the ethnographies that I consulted was disappointing, but I concluded that continuous variables with substantial noise would probably work better in analysis than ordinal variables. This decision meant that I spent many hours developing quantitative estimates and guesses to augment the observational data sometimes provided by ethnographers and observers. As a result, I was able to obtain a number of mean or normative values for settlement size by types, house size by types, mean household size, age at marriage for males and females, percentage of polygamous marriages, mobility variables, demographic variables, and many other categories of information.

These data were very important since I assumed that habitats varied quantitatively within and between nominal or ordinal classifications of habitat. If it were possible to identify truly adaptive responses to continuously varying environmental variables, there should be significant variability within so-called culture or ethnic areas as well as between them, in which case “Galton’s problem” (Naroll 1961) would

lose its problematic status (Harner 1970:73). It is also possible that the geographic distributions of entities and formal properties that archaeologists refer to as *diffusion* may really represent fine-grained adaptive adjustments to environmental conditions. If this is so, I would expect changes in the synergistic social relationships among adjacent groups to result not from the spread of a good idea but, instead, from conditioning processes rooted in adaptive variability when studied analytically across geographic space. The idea that diffusion could be explained—that it might be possible to say when it would occur, when it would not, and how it would pattern—was an appealing possibility.

I also concentrated on other properties usually considered to be indicative of system state differences. I was well aware of the ecological principle that the same climatic changes acting upon a population of mice and elephants would have very different effects on each species. This is another way of saying that one would expect that the prior state of a system could potentially condition very different responses to similar or identical environmental variables. Throughout my intellectual journey, I realized that I would not be able to categorize a priori many important features of hunter-gatherer life as either continuous or even ordinal variables.

The preceding comments are part of the background or scenery revealed by opening the metaphorical *Twelfth Night* “curtain,” but they do not present a comprehensive “picture” of what to expect in this book. Three of the points presented previously are, however, central:

1. The primary problem that this book addresses is the development of a method for productively using ethnographic data to serve archaeological goals.
2. The possibility that the patterning which has been termed “diffusion” can be explained and that one might be able to predict when it would and would not occur—and what shape it would assume—has great appeal.
3. It is reasonable to expect that the prior state of a system may condition very different responses to similar or identical values for environmental variables.

These three statements can be visualized as providing the defining coloration in the intellectual background of the analytical hunter-gatherer picture, but details about the actors and their visages remain unclear. In an effort to sharpen some of the picture’s blurred outlines, I offer the following comments about material that is *not* included in this book. I do not analyze any archaeological data directly, I do not discuss the ideas of many contemporary archaeologists, and I do not attempt to cope with paradigmatic diversity in the field of archaeology.

This book is unapologetically written from a scientific perspective. It is largely an exercise in inductive reasoning, in that it asks questions regarding the character of the world of

organized variability among ethnographically documented hunter-gatherer groups. As such, it addresses many alleged empirical generalizations that appear in the archaeological literature which turn out to be inaccurate or, at best, only marginally useful.

From a methodological perspective, the picture is built up gradually, chapter by chapter. Because some of the material is detailed and requires considerable concentration, I think it is important at the outset to give readers a brief guide to the book’s contents. Unlike a novel, whose writer tries to keep the ending of the book a secret until the climactic chapter, this book will mean more to the reader who has been given some idea about where the argument is heading at any given moment.

The metaphorical picture that develops in this book is one in which particular devices for structuring data—called *frames of reference*—appear in the foreground as the way to organize prior knowledge and make it useful to archaeologists. Two major frames of reference are featured, one of which is designed to document the primary variables conditioning habitat variability. Use of this frame of reference in turn permits archaeologists to relate archaeological facts to a multitude of environmental variables so that the characteristics of adaptive responses to habitat variability can be documented and identified.

As I have already indicated, very different responses to environmental variables can be expected, depending on the prior conditions extant within the cultural systems that are experiencing environmental fluctuation, change, or variability. This problem brings us to the second frame of reference developed in this book. The variability documented among ethnographically known hunter-gatherers is organized into a basic frame of reference for comparison to archaeological remains. In principle, from the equations developed in this book, an archaeologist can anticipate many of the properties of hunter-gatherer groups that might be expected to occur at a given location, at an archaeological site, or at a series of sites. These anticipated characteristics can then be used in a variety of ways to learn more not only about hunter-gatherers but also about the archaeological record. And, since one of the goals of this book is to explain variability among hunter-gatherers, the explanatory theory that I have developed is available for archaeologists to use deductively by reasoning to or simulating changing conditions and thereby providing patterns of change that can be expected to occur in the archaeological record at specific locations.

Most of this book is concerned with the development of procedures and methods that can be used directly by archaeologists or that can be used as models by other scientists. At the same time, I hope to illustrate the general principles of and the benefits to be derived from using environmental frames of reference to study patterning among hunter-gatherer cases. I hope that the archaeological reader realizes

that the same procedures could be used with archaeological data. The organizational insights derived from using environmental frames of reference to study hunter-gatherers also make it possible to construct a *hunter-gatherer frame of reference* with which archaeologists can productively study the archaeological record. Last, I address the subject of cultural responses to changed climatic conditions by applying a hunter-gatherer frame of reference to changing climatic sequences at locations where there are provocative archaeological sequences.

Although there are many graphs in this book that display the relationships among the several data sets that I have assembled during my research, the “picture” that this material presents is essentially an intellectual one. It becomes “graphic” to the degree that the reader understands the organization of the data and my tactics and strategies for transforming patterning into new knowledge. The book is divided into four parts, each of which uses—in different ways—the prior knowledge available to me in the pursuit of different goals.

The three chapters in *Part I* survey some of the prior knowledge available to me about hunter-gatherers. Although I spent years assembling and ordering this ethnographic information, earlier researchers devoted whole lifetimes to observing and recording the life ways of the small-scale societies that are at the core of my information base. In chapter 1, I summarize the work of several important anthropological researchers whose ideas about hunter-gatherers have shaped the thinking of their intellectual descendants up to and including the contemporary era. Following this “founder’s effect” chapter, I address contentious assumptions and ideas about the role of human actors in the explanatory process. I also use the stage in chapter 2 to clarify my position on many of the issues that are of great concern to humanists. Chapter 3 is devoted to a discussion of science as the learning strategy whose precepts will be implemented in subsequent chapters when I actually develop frames of reference and use prior knowledge within the broad framework of inductive research.

Part II of the book is fairly well described by its title: “Methods for Using Prior Knowledge: Building Frames of Reference and Models.” Chapters 4 and 5 describe the mechanics of building an environmental frame of reference and developing the means for making projections from hunter-gatherer data. In chapter 6, I demonstrate how to use prior knowledge to build models with which to analyze one’s subject matter. I conclude this section with an illustration of how to apply these models, using the constructed frames of reference for the analysis of European archaeological data dealing with the appearance of domesticated plants and animals. These three chapters outline the logic and actual construction of intellectual models and frames of reference and are central to the tactical exploration of the procedures developed in subsequent chapters.

In *Part III*, the dialogue becomes more complicated. Instead of presenting a linear sequence of information, strategies, reasoning, and warranting arguments, I begin to demonstrate some strategies that have strong philosophical implications for such controversial subjects as objectivity and the ability of science to go beyond the circularity of “theory-dependent” observations and reasoning. These issues reflect complicated interactions in what the reader will discover is the drama unfolding in the metaphorical “scientific theater,” where researchers engage their colleagues in a debate of ideas.

One intellectual engagement in the scientific theater has arisen from several points that I first encountered in a fascinating paper delivered by Patty Jo Watson (1986) at the fiftieth anniversary meeting of the Society for American Archaeologists. Watson was responding to an argument that I had made (Binford 1981:29; Binford and Sabloff 1982:149) denying the empiricist assumption that the past was directly and self-evidently accessible. I had argued that the past was only knowable through disciplined inferential reasoning and that, up until then, I had encountered nothing in the archaeological literature that indicated archaeologists could cope with the magnitude of the methodological problem they faced. More specifically, I said that “the dependence of our knowledge of the past on inference rather than direct observation renders the relationship between paradigm (the conceptual tool of description) and theory (the conceptual tool of explanation) vague; it also renders the ‘independence’ of observations from explanations frequently suspect and commonly standing in a built-in relationship, thereby committing the fallacy of ‘confirming the consequent’” (Binford 1981:29).

Watson’s response to this argument surprised me, particularly what she perceived as my skeptical attitude. Although I had publicly confessed that I doubted that many of the forms of reasoning presented in the archaeological literature would get us to the past, I had never doubted that the problem could be solved. In fact, in the monograph in which the preceding quotation appeared, I was working to reduce some ambiguities associated with the problem of inference. This point was overlooked, as was my earlier argument (Binford 1982) that the intellectual independence of propositions formed the basis for a modern idea of objectivity. Not many years later, Alison Wylie (1989) seemed to appreciate this issue and, unlike many other critical voices, acknowledged my attempts to solve some of the problems associated with the secure growth of knowledge in archaeology, at least at the methodological level.

I have long admired the solution developed by the discipline of geology to deal with the fact that it is impossible to observe directly the dynamics that occurred in the past (Kitts 1977:56–68). By means of warranted, uniformitarian assumptions demonstrating the linkages between circumstantial

evidence from the past and direct observations in the present, the dynamic geologic processes that operate in the world today are argued to have also been operative in the past. The use of uniformitarian assumptions, however, implies tactical reasoning and not a dogmatic assumption that the past was like the present, as many have imagined.

Throughout my research career I have been an advocate of the use of cross-cultural comparisons as a uniformitarian strategy for learning in anthropology. Despite the fact that this was the method of choice during one phase of anthropology's development, I always felt that the potential value of this technique had gone unrealized. This is partly because—in my opinion—researchers always asked the wrong questions of their comparative data and also because the standard comparative tactics ensured that the results would be ambiguous. In *Part III* of this book, I use cross-cultural comparisons differently and, I believe, more productively by looking at selected attributes and characteristics of a global sample of diverse hunter-gatherer groups in terms of multiple frames of reference. The patterns I isolate using this technique provide clues to dynamic processes that were operative in the world of nearly contemporary, ethnographically documented hunter-gatherers; they do not simply represent an “interpretation,” with its attached logical fallacy of confirming the consequent.

The title of *Part III*—“Recognizing Patterns and Generalizing about What the World Is Like: The Transition from Pattern Recognition to Theory Building”—fairly accurately describes the challenges of the subject matter of chapters 7–9. In chapter 7, I use generalizations derived from pattern recognition studies to build a model of the factors that *might* contribute to the variability in hunter-gatherer group sizes,

and in chapter 8, I get feedback from the hunter-gatherer data set about how group size actually varies in a wide range of specific circumstances. In chapter 9, I shift focus and discuss the observations and reflections of the scientific observer who is engaged in an effort to understand what the world is like and why it is the way it appears to be.

Part IV of the book, which is called “Putting Ideas, Second-Order Derivative Patterning, and Generalizations Together: Explorations in Theory Building,” places the reader more directly in the scientific theater in which the researcher is engaged in theory building. This section also demonstrates that the learning strategies and tactics that have been focused on ethnographic investigation can have productive results when they are applied to archaeological research. I urge the reader not to jump immediately to chapter 10 (“A Disembodied Observer Looks at Hunter-Gatherer Responses to Packing”), chapter 11 (“The Evolution of System States: Complexity, Stability, Symmetry, and System Change”), and chapter 12 (“The Last Act Crowns the Play”), because much of the value of these chapters will be lost unless the reader follows the development of the patterning and arguments that precede them.

I hope that once the reader has made the considerable effort required to digest the material presented in this book, he or she will be motivated to apply this approach to the many intriguing problems that archaeologists confront in all regions of the world. One could say that the end of this book is, in fact, a beginning, since what has been learned from the research upon which the book is based cries out in the Epilogue for elaboration, expansion, and application to other sources of data that may be organized fruitfully into frames of reference.