As I write this it is exactly 40 years since Lewis Binford's famous paper, “Archaeology as Anthropology,” came out in American Antiquity, an event that in retrospect has come to be seen as marking the beginning of the “New Archaeology.” In it he rejected the idea that the goal of archaeology should be the reconstruction of culture histories, to be accounted for by migrations of peoples and the diffusion of cultural influences. Instead, he proposed that its aim should be “to explicate and explain the total range of physical and cultural similarities and differences characteristic of the entire spatial-temporal span of man’s existence” (Binford, 1962: 218).

The goal of explaining variation is a demanding one, involving the specification of causal mechanisms. To say that members of two different cultures did things differently because they had different traditions and histories is not an answer but simply a re-statement of the problem. Providing a solution involves specifying mechanisms that led to the differences and being able to demonstrate why they did so. In other words, it involves what Binford calls “doing science”, an activity entirely congenial to readers of this journal, but increasingly alien to some sections of the discipline of archaeology. As he notes in the book under review (p. 400), 40 years later he is still committed to the same position and trying to achieve the same goal, no longer by delivering programmatic polemical and critical statements (although there are quite a few polemical comments in passing, especially lurking in the end notes!), but through the carrying out of a major programme of substantive research.

The book's focus is the explanation of variation among ethnographically known hunter-gatherers, especially their subsistence patterns and forms of organization, and the role that this can play in explaining variation among prehistoric hunter-gatherers known only from the archaeological record. Binford’s interest in this subject goes back to his attempts to understand the French palaeolithic in the 1960s and has included extensive periods of ethnoarchaeological fieldwork, especially in the Arctic with the Nunamiut (Binford, 1978). In fact, as he explains in the Prologue, the book under review itself started life 30 years ago, as a manuscript used for teaching a course on hunter-gatherers at the University of New Mexico. That version had a long samizdat life in the form of increasingly faded photocopies. I can remember making one of these myself when Binford spent a term teaching at the
University of Southampton in 1981. However, as he also points out, the version that has finally seen the full light of day bears little relation to the earlier one.

Potential readers should be warned that it is a massive work—with the notes and references it can't be much short of 500,000 words, even allowing for all the tables and figures—and Binford makes, clear that he would like his readers to read all of it, rather than just skipping to the conclusion. Its central aim is to serve as a paradigm, an exemplary work, showing, “the development of a method for productively using ethnographic data in the service of archaeological goals” (p. 2, italics in original), and in particular to find a way “to maximise the information in the ethnographic literature about the variability among hunter–gatherer societies” (p. 2) for the benefit of archaeologists interested in prehistoric hunter–gatherers. Perhaps most importantly, the information is intended to be part of a methodology capable of revealing aspects of prehistoric hunter–gatherer systems not known in the ethnographic present. After all, what is the point of doing archaeology if you’re not capable of discovering cases where the past was different from anything known to ethnographers? As Binford explains (p. 3), the approach he adopts to achieving his aim is “largely an exercise in inductive reasoning,” a point to which I will return below.

The first part of the book is entitled “Exploring Prior Knowledge and Belief.” Chapter 1 traces the history of hunter–gatherer studies, starting with the work of Mauss and Beuchat on seasonal variations of the Eskimo and passing via Julian Steward and others to the famous 1966 “Man the Hunter” conference, before concluding with the work of recent behavioural ecologists on issues such as sharing and risk. This review and critique provides the basis for Binford’s rejection of the idea of “primal social forms” (contrast the views of Foley [2001] or Boehm [2000] among others) and his adoption of an ecological approach to understanding hunter–gatherer variation. If we are to understand the existing variability among hunter–gatherers then our starting point should be exploring its relation to patterns of variability in environmental properties.

Chapter 2 introduces the metaphor of the ecological theatre and the evolutionary play that runs throughout the rest of the book, and goes on to distinguish between habitat, niche, and population as different dimensions of ecological variation. Habitat variables are such things as temperature and rainfall and can be measured independently of any consideration of the capacities of the actors in the ecological space. This is not the case with niche, which requires a consideration of the specific properties of the actors and the way they interact with properties of the habitat. This discussion leads on to a consideration of the characteristics of human actors and the role of human intentions, about which Binford is sceptical:

“In a tactical situation, outcomes are always caused by external conditions and their states—stable or unstable—coupled with the accuracy of human projections of these prior conditions to some future time. Knowledge of the state of the external conditions makes successful volitional action possible, and therefore cause is to be
sought in those external conditions, given the human actor’s capacity for learning” (p. 36).

In the context of discussing risk and uncertainty, Binford raises the question of whether humans are maximizers, and if so, with respect to what. He rejects the Darwinian argument that humans, like other animals, seek to maximize their reproductive success, proposing instead that: “A constant characteristic of human actors is that they attempt to maximise their vital security in any habitat, limited only by their capacities and means” (p. 41). Of key subsequent importance in Binford’s argument is his insistence on the extreme plasticity of human behaviour and the lack of significance he attaches to culture. While the human capacity for culture is important, it is a constant, and cultural variation is to be the object of explanation, not explanatory in itself, a view on which Binford’s position has not changed in all those 40 years.

In Chapter 3 he presents his strategy for gaining archaeological knowledge. The key idea here is that of dimensionalizing data. We can’t just describe the world in any old way if we want to understand it. We have to describe it in terms of criteria we believe to be relevant to our knowledge goals. Analysis involves exploring the relations between different dimensions. The dimensionalization of the data is in a sense the creation of the observer, but this in no way predetermines what the relations between different dimensions will be once we start to explore them. It is here that Binford introduces his concept of frame of reference. Specific frames of reference are not right or wrong; they are relevant to particular issues and involve the juxtaposition of knowledge from one domain against knowledge from a different, less well-known one about which we wish to find out more. Binford’s primary frame of reference is a global environmental one in which he looks at the relationship between ethnographic and (to a much lesser extent) archaeological observations and environmental properties of their locations.

Part II of the book is concerned with methods for using prior knowledge, starting with the generation of habitat variables to which hunter–gatherer variation can be related. The basis for the construction of the variables is a database of more than 1400 weather stations around the world. Again, however, we can’t just use any old information about climate. If we are interested in exploring how aspects of hunter–gatherer groups relate to habitat variation then we need to define relevant aspects of the habitat, in this case variables that predict the production and biomass of plant producers and consumers. This involves the development of equations that use the data provided by the weather stations to generate new relevant variables, such as effective temperature. Further calculations lead to an equation for estimating the net annual above ground plant production in grams per sq.m. per year for anywhere in the world, as well as a whole series of other variables related to plant production.

Binford notes in the prologue to the book that it took him two years to develop the databases of the world’s environments and the geographical distribution of documented hunter–gatherers. In the circumstances, that sounds quite quick. The
result of the habitat data collection and the generation of relevant derived variables is a set of data tables for 339 ethnographically-known hunter-gatherer groups. It represents a remarkable achievement. Clearly, there is a degree to which the content of the tables has to be taken on trust, but it seems a safe bet that they are going to be an indispensable source of information for scholars interested in hunter-gatherer variability for years if not decades to come.

Chapter 5 presents equally valuable information for the same set of 339 groups on the organizational variables that are to be explored, including population size, percentage contribution of gathering, hunting and aquatic resources, the mean size of the group that camps together during the most dispersed phase of the year, the main vegetation type where they occur, and many others.

By relating the population densities and areas of the hunter–gatherer groups to the specific world vegetation zone in which they occur and multiplying up the result by the size of the zone, he is able to come up with an estimate of world hunter–gatherer population, around 7 million. Further than this, however, by taking the area occupied by known individual hunter–gatherer groups and developing a multiple regression equation using climate and other variables to account for variation in the size of these areas, he is able to generate predictions of group areas for parts of the world where hunter–gatherers no longer exist but where weather station information is available.

It takes a lot of hard work to follow the arguments in both this chapter and the previous one, as Binford himself is aware: “I have described in perhaps greater detail than many will find palatable the steps I have taken to assemble environmental and ethnographic frames of reference that would, I promised, eventually be applied to questions stimulated by archaeological patterning [!]” (p. 204, my exclamation mark).

In chapter 6 Binford uses the information to develop a minimalist model of hunter–gatherer responses to environments, based on the use of terrestrial resources only and the assumption that food resources are included in the diet in proportion to their abundance in the environment. This involves the use of environmental variables to construct equations that give estimates of accessible plant production that could be used by human consumers, and similarly for animal production. These in turn feed into equations specifying the number of people/100 sq.km. unit who could be fed by plant resources alone and animal resources alone in that unit, as well as number of quantities derivable from these, such as expected human population density. By establishing this baseline model Binford has a means of comparing the corresponding figures for ethnographically known hunter–gatherers and establishing the extent to which they deviate from the expectations. In many cases, he suggests, the deviations represent the results of intensification processes, where intensification refers to the obtaining of increased amounts of food from a given unit area. The findings are used to model aspects of the hunter–gatherer
populations of mesolithic Europe and to suggest hypotheses about the processes that led to the appearance of agriculture there.

In Part III of the book we turn from pattern recognition to theory-building, and chapter 7 is provocatively titled, “Twenty-one generalisations in search of a theory.” The core of the chapter is the exploration of relationships between subsistence, biogeography, the population size of ethnic groups and the size of the areas they exploit, and intensification processes. It leads to the development of a model of hunter–gatherer group size, taking into account male–female task differentiation and the number of dependants, as well the size of the foraging area.

Chapter 8 uses ethnographic information to explore how group size really does vary in different circumstances. Tables of invaluable data are provided on such topics as group size and mobility, household size and the extent of polygyny. It turns out that the model fits quite well for groups mainly dependent on terrestrial plant resources, but not for those largely dependent on terrestrial hunting or aquatic resources. The attempt to understand why this should be the case leads to an exploration of the implications of such strategies as food storage, and the role of mobility. As Binford explains (p. 309), variability in minimal hunter–gatherer group size is an indication of the scale of cooperation during the part of the year when people are at their most dispersed. In some parts of the world this is the most productive season, in others it is the least productive, but this apparent paradox does not mean that it is impossible to offer general explanations for the variation. What we need to do is look at the costs and benefits of increasing and decreasing the scale of cooperation in different circumstances, and see what patterns emerge.

In Chapter 9 Binford examines different theoretical frameworks to account for the variation in group size that has been observed, starting with Greg Johnson’s (Johnson, 1982) proposal that the optimal size of group for human decision-making is six people, which turns out to match the average number of basal family units making up the minimal size group. However, he concludes that this number actually arises because group sizes relate to work schedules. These require six subunits to produce three different male or female workers per day: the key relationship is between labour and the size of the units needed for pooling risk of subsistence failure (Winterhalder, 1990). The factors affecting the size of the maximal annual group are much less well-understood. Nevertheless, once they are bigger than c. 50 people internal differentiation may start to occur.

Chapter 10 is concerned with the implications of intensification and what happens as hunter–gatherer ranges become increasingly packed and reduced in size. It also raises the question of how such intensification may have led to the origins of agriculture. The implications Binford explores include not just subsistence changes but changes in social organization and restrictions on rights of access to resources. The forms of likely intensification vary from region to region depending on climate and the availability of different kinds of plant and animal production, but at high latitudes the most viable response is likely to be a shift to aquatic resources, the use
of which may be further intensified by investment in technology. Binford is able to use his global database to develop two general measures of the extent of intensification for any given ethnographically-known group. The first is a measure of "niche effectiveness," which is obtained by dividing the actual population density of the group by the value predicted by the Terrestrial Model for that location. The second is a measure of spatial packing obtained by relating population density to the size of the foraging radius. Packing index values greater than 1.0 indicate that the foraging radius is circumscribed and therefore the region is packed, cutting out the possibility that mobility can be used as part of a resource exploitation strategy. Responses to packing differ according to the resources that become the focus of the diet.

Chapter 11 explores the evolution of complexity and wealth differentials among hunter–gatherers by using the world database to examine how they relate to the exploitation of particular kinds of resources and to population density, especially the degree of packing. As intensification increases, the shift from high dependence on terrestrial animals to increased dependence on plants has implications for the gendered division of labour and new male roles. Interestingly, too, the form of intensification has a bearing on the nature of internal social differentiation. Internally ranked and stratified societies seem to be more associated with the exploitation of aquatic resources, while in high-density groups dependent on terrestrial plants complexity is seen in the existence of such organizations as secret societies.

Chapter 12 is entitled, “The Last Act Crowns the Play,” and in it Binford uses the models and data he has been building up throughout the book to offer an explanation of the origins of Near Eastern agriculture. He uses local climate reconstructions both to create a terrestrial model for these areas and to project subsistence data from the ethnographically known hunter–gatherer cases. By means of the climate reconstruction he is able to model change over time in the key environmental variables and the implications these would have had for foraging populations. In particular the model predicts that the Younger Dryas period would have led to a return to maximum mobility, following the increased stability of the early Natufian, with packing and intensification thereafter, in the period corresponding to Pre-Pottery Neolithic A. Not only do the models provide predictions for the human occupation sequence of the southern Levant that closely match archaeological observations in terms of population density and settlement stability (see e.g., Bar-Yosef, 1998), they also predict successfully the very different sequence from Zeribar in Iran.

As Binford points out (p. 461), all these arguments follow from his theory of packing, which generates predictions of the kinds of changes that are likely to occur by using the global habitat data and its derivatives and the global hunter–gatherer sample. However, “what cannot be anticipated accurately is the range of systemic variability that may be generated during the period of criticality when the laws of self-organization themselves also change” (p. 461). The models may have predicted
a high degree of intensification and packing for the southern Levant after the end of the Younger Dryas, but they did not predict the origins of agriculture.

The book concludes with an epilogue in which Binford asks whether he has been successful in establishing a general archaeological research procedure and answers with a qualified yes. The key is pattern recognition research. “This means comprehensively projecting what is known about a focal subject matter against a frame of reference made up of variables that are known or suspected to be relevant and about which there is some secure knowledge about the factors conditioning variability in each” (p. 471). We then have a basis for using, in this case properties of ethnographic hunter–gatherers, as frames of reference for the comparative study of the archaeological record.

Despite this attempt to provide a comprehensive summary, a review such as this barely begins to do justice to the richness of Binford’s book. The amount of work and thought that have gone into it are truly remarkable. As I have noted already, the process of constructing the derived environmental and ethnographic variables was clearly a major research enterprise in itself, never mind the subsequent exploration of the relationships between them. The tables of raw and derived data are certain to be pillaged by researchers for years to come. They are not all that easy to use though. In many cases it is not at all clear what the variables mean and since the tables do not provide a key to this you have to search in the text to find the places where their construction and meaning are described. In many ways, though, a book is no longer the best way to present such information. In an ideal world we would now have a Binford hunter–gatherer website that would not only contain all the information in the tables but also the original weather station information on which the climatic patterns are based. The maps, too, would be far more useful as GIS databases that would enable researchers to analyse the spatial patterns presented, as well as generating their own. Still, it is more than a little churlish to complain about this in the light of the riches Binford has provided.

Apart from the tables presenting the data, the volume includes large numbers of scattergrams that are used to present the pattern recognition work exploring the relationships between the variables. For example, minimal group size against the log of niche effectiveness, subsistence diversity against log population density, latitude against log of net above ground productivity, or mean household size against maximum group size. The hunter–gatherer groups represented in the scattergrams are often coded into different categories, indicating, for example, the extent of social differentiation, the degree of investment in storage, and many other variables. The patterns Binford infers from these are identified purely by eye in most cases, without statistical support in terms of numbers such as correlation coefficients. In many cases this is fair enough. The patterns apparent in the scattergrams are complex: often it is clear that one relationship holds for part of the range of values and a different one for the rest of the range. Moreover, it is not simply a matter of looking for linear patterns but also of seeing whether particular hunter–gatherer categories fall into particular parts of the scattergram. However, it has to be said
that in some cases at least the interpretation Binford puts on the patterns, indicated in many cases by differential shading of areas of the scattergram, seem to me to be tendentious to say the least. I would suggest that fig. 10.04, for example, is a case in point.

However, it is time to return to Binford’s overall framework. The theoretical position taken by the book is somewhat ambivalent. As we have seen, the author himself says that the book is largely an exercise in inductive reasoning—he wants the data to speak for themselves—which in some respects it is, but of course such a claim is also disingenuous. It is his basic assumption that variation in human behaviour arises from variation in ecological circumstances—justified in the outcome—that leads him to define the particular variables he has chosen to create and analyse. This assumption is shared by the discipline of behavioural ecology or sociobiology, but Binford explicitly rejects the fundamental basis of that approach, which is that humans, like other animals, act in ways indicating that they are trying to maximize their reproductive success. Moreover, while he quotes with approval a number of studies based on optimal foraging theory, a key part of behavioural ecology, he does not use the approach himself. He settles for a more general argument that humans attempt to maximize food security, which seems reasonable enough but lacks the theoretical consequences that follow from the more conventional evolutionary assumption. In arguing that humans attempt to maximize different currencies at different times—energy at one time, prestige at another—he fails to acknowledge that, depending on the context, both may be ultimately linked to reproductive success.

On the whole, Binford’s intention is to avoid as far as possible any metaphysical commitment to a particular view of the world, whether it be humanistic or evolutionary. Instead, he advocates that we let patterns emerge from the data. However, despite his disavowal of evolutionary mechanisms, virtually all the conclusions from his analyses make perfect sense in behavioural ecology terms and could have been predicted by optimal foraging theory. Indeed, if further support for the power of optimal foraging theory was needed, Binford’s results supply it. This is not to say that all the provocative patterns that emerge from his data analysis would have been revealed by a purely theory-based, hypothesis-testing approach, as opposed to Binford’s pattern recognition, but that they only really make sense in the light of the behavioural ecology framework. Indeed, it seems to me that one of the most important future roles for Binford’s data and results is in providing an empirical basis for behavioural ecological studies of prehistoric hunter-gatherers. One of the problems of such studies is that they usually lack the information to create a baseline for evaluating the actual hunting decisions whose results the archaeological record reveals to us. What we do not often have is any sense of the options that were available. Now, for example, if we have reason to believe that the group whose remains we are studying were living in a packed landscape as opposed to one that still had potential for mobility, we may evaluate the evidence for their activities very differently.
Neither Binford’s view of the world, nor behavioural ecology attach much importance to culture (cf. Smith, 2000). What matters to both of them is human behavioural plasticity: if people find themselves in new circumstances they learn and adapt. This is certainly a welcome antidote to the belief that cultural variety or change can be explained simply by virtue of cultural difference itself. Furthermore, as the book demonstrates, it provides a very powerful learning framework because of the possibilities it offers for generating theoretically-based, clearly testable hypotheses. Nevertheless, that does not mean it corresponds exactly to the way the world works, and in my view it has shortcomings, especially when it comes to understanding change. This is because learning cannot be assumed to be automatic. Not all human learning is the kind of trial-and-error learning from the environment Binford effectively assumes.

Cultural learning involves learning from others, and the knowledge may or may not be tested against the environment. Indeed, any feedback from the environment in terms of the consequences of taking particular socially learned courses of action may not be at all obvious. The fact that much knowledge is inherited means that it can be out of equilibrium with external circumstances for non-trivial lengths of time. It also has implications for the process of innovation. Henrich (2001) has recently shown that the normal profile of innovation adoption indicates that innovations generally arise not from direct learning from the environment but from modifications—what Boyd and Richerson (1985) call “direct-bias”—of existing traditions. If learning is automatic and causality comes from the environment, then when environmental problems arise, created for example by “intensification pressure,” appropriate solutions are simply found by trial-and-error and applied. If the properties of the cultural tradition, as an inherited body of knowledge affecting people’s actions, are relevant, then we cannot assume that this will be the case. Recognition that there is a problem with existing ways of doing something may make people less risk-averse in their experimentation with novelty (see e.g., Fitzhugh, 2001), but it does not mean that they will necessarily come up with valid solutions. If you believe that your problems are caused by the spirits then the answer may be to increase or diversify your ritual activities rather than developing new technologies.

It seems to me that Binford’s refusal to give any serious role to cultural inheritance and his rejection of the fundamental behavioural ecology assumption that humans strive to maximize reproductive success paradoxically come together in his ideas about the origin of intensification, whose effects, especially those arising from packing, his analyses demonstrate so effectively. He seems to assume that differential intensification arises largely from differential population growth rates, with high growth rates producing “pressure.” This seems to me a problematic view. First, the behavioural ecology assumption that people maximize their reproductive success in the prevailing circumstances (which, of course, includes not just survival and birth but successful parenting) implies that if there is resource room for people to increase it they will do so (cf. Wood, 1998). In other words, population growth is a sign of adaptive success, not of population “pressure,” which is more likely to be
accompanied by a stable population size (Bettinger, 1998). Moreover, it is quite clear that rates of population increase for hunter-gatherers can be as fast as for farmers (Sellen and Mace, 1997). Over archaeological time-scales the reason why the world did not fill up to overflowing until the present cannot have to do with growth rates. A starting population of ten with the not unreasonable growth rate of 1% per year produces a population of more than 30 million in 1500 years. The reason this did not happen must have to do with density-dependent ceilings, coupled with adverse climate change and random fluctuations in population at the regional scale (see e.g., Boone, 2002). These ceilings are not targets of population regulation, nor are they the point at which people are starving; they are the point at which rates of reproductive and parenting success fall to the replacement rate and population stops growing. This does not necessarily mean that the people concerned should be considered under pressure. What it does mean, however, is that if some people start exploiting new resources that give them greater reproductive success, then the new way of life will prevail and the outcome is likely to be processes such as Ammerman and Cavalli-Sforza’s “wave of advance” of population (Ammerman and Cavalli-Sforza, 1984), whether the expansion is based on agriculture or more productive resources obtained by foraging. In some contexts this will not happen without technological innovation, for example, the bow in the late palaeolithic (Stineret al., 2000). In Binford’s view such innovations come into existence as required. However, more innovations arrive at places from outside than are invented locally and they depend on the cumulative history of specific cultural traditions. This is why, in my view, Jared Diamond’s (1997) evolutionary account of human history, which gives a major role to diffusion, is more convincing than one built on Binford’s premises or those of behavioural ecology would be, important though they are as part of the story.

However, these responses are a tribute to the quality of the work under review, and are not intended to be criticisms of it. In conclusion, can we agree with Binford in his assessment that the book is successful in achieving his goals? Without a doubt in my view. The book is extremely stimulating. Anyone with an interest in the issues presented and even the tiniest bit of imagination will think of 10 different research projects they want to carry out in the light of what Binford has demonstrated. Those whose main interests are in horticultural and agricultural societies will wish they had something similar to explore. Moreover, the centrality of the goal itself, of explaining variation, cannot be emphasized too strongly. As to the “frames of reference” approach, it seems to me to be a particularly powerful example of the “comparative method” long used to explain biological variation, particularly powerful because of the strength of the causal understanding of the factors affecting variation in this case. It is a far cry from traditional “cross-cultural” approaches. However, the interesting thing about the comparative method these days is that, albeit reluctantly in some cases, it has had to take on board phylogenetic lineage history in developing its causal explanations for variation (Mace and Pagel, 1994; Borgerhoff Mulder, 2001). Forty years on, this is not an enterprise that the author of “Archaeology as Anthropology” is likely to believe in.