

and, in so doing, he trivializes the data. Second, and just as importantly, how can one throw out supposed "outliers" from an analysis when in fact, because of sample size, these "outliers" may be potentially the most significant and representative levels? In other words, given the admittedly preliminary and bounded nature of the evidence, how does one know what is pattern and what is anomaly? Discarding "outliers" is permissible, but only when much more is known about *data structure*. Helmer's failure to recognize this fact exposes a fundamental weakness in his argument and reveals a curious misunderstanding of scientific procedure.

I have no doubt whatsoever that sample size may be influencing, to some extent, diversity in the Peace Point data; artifact richness and diversity seem to be affected by sample size in many archaeological contexts. However, Helmer has yet to prove it. Although I believe there are better, more plausible explanations of the data — namely, the one I chose to advance — Helmer could have employed a number of robust techniques to flesh out the effects of sample size on diversity at Peace Point. Kintigh's (1984) simulated frequency distribution method comes readily to mind. More upsetting is the fact that Helmer doesn't offer any solution to the diversity problem at Peace Point. Let us assume, for the sake of argument, that sample size does indeed account for most of the assemblage diversity at Peace Point. What then? Do we throw up our hands and walk away in defeat? I, for one, would want to know the slopes of the regression lines that characterize the relationship between classes of lithic material types and tool types or debitage types for each level. Why does the slope of one level differ from that of another? Why are raw material types in one level being added at a much slower rate than another? These are the types of issues that Helmer should have addressed if he was truly concerned with advancing the diversity problem at Peace Point beyond the level of rhetoric.

Contrary to Helmer's claim, I was very aware of the problem of sample size and its bearing on interpretation. A more careful reading of page 94 establishes this fact. Even so, I did not let this limitation deter me from building a *provisional* model of site use and resource utilization. A considerable amount of archaeological work has been undertaken in northeastern Alberta. Yet, significant insights into the prehistory of the region have not been forthcoming. Peace Point, however, offered an excellent opportunity to formulate a chronology of site use, not by traditional type-based projectile point systematics, but by concepts that have the potential to elucidate the cultural dynamics of Peace Point's prehistoric inhabitants. Regardless of the ultimate utility of the model, we now have a direction and a focus for future research at Peace Point. I can only hope that, as new data come to light, this model will be rejected in favour of more robust theories and models. Such is the stuff of good science.

I never claimed that faunal remains were exclusively a reflection of changes in cultural behaviour through time, as Helmer maintains. Differential preservation obviously accounts for some of the data, as I stated unequivocally on page 90. Nonetheless, because delicate bird bone was found in abundance in some of the lowest and highest levels, I felt that faunal diversity, when considered in combination with other data, indicated a general shift through time towards a greater range of species being exploited over longer episodes of occupation.

Helmer's comments about the nature of the Peace Point data and my interpretations of them, in light of sampling procedures, unmasks a fundamental confusion in his logic. On the one hand, arguments of sampling bias and size are used to denounce the integrity of the Peace Point data and my interpretations, some of which provided a foundation for theory building in subsequent sections. On the other, he states that I "provide a very useful, and potentially quite significant, insight into possible temporal trends in flake and blade core morphology." Which is it? Helmer can't have it both ways. This exposes a lack of logical consistency in the structure of his arguments.

Nowhere did I assume, as Helmer implies, that the Peace Point data are statistically representative, i.e., representative of the totality of activities carried out at Peace Point during any one occupational event. The fact that different levels sampled space around hearths differentially is irrelevant; I was conducting an inductive search for spatial patterning based on site formation theory. What was important was the discovery of similar patterns in those levels in which space around hearths was sampled equally (e.g., Levels 1 and 5). I attributed this patterning — the occurrence of dense concentrations of smaller items near hearths and dispersed arrangements of larger items away from hearths — to the same size-sorting processes observed around hearths in numerous ethnographic settings. The fact that this pattern was detected visually, not derived statistically, does not make it any less real. This is not to say that such patterns cannot be expressed in statistical fashion. The tendency for larger items to occur away from areas of intensive use in Levels 1 and 5, for example, is significant at the .001 level of confidence,  $X^2 = 20.01$ ,  $df = 2$  (see Stevenson, 1985: Table 2).

Helmer's statement that visual assessments of spatial data are unacceptable vis-à-vis the array of statistical procedures capable of distinguishing between random and non-random artifact distributions is *dead wrong*. Spatial statistics rely on data of two types: grid cell counts and item point locations, the latter being eminently preferable to the former. No item point technique, not nearest-

neighbour analysis, not Whallon's (1984) unconstrained clustering method, has been devised to detect the type of size-sorting within hierarchical nested clusters of artifacts that I found visually. Helmer's rejection of visual assessments of spatial data is only acceptable when data are so randomly distributed that clusters cannot be detected readily. Their statistical description is another matter altogether. I leave for the reader to decide for her/himself whether statistical approaches to the discovery of spatial patterns at Peace Point was a preferable alternative to piece plotting and visual assessment (see Figures 17 and 22).

Helmer concludes his review by stating that "In the absence of appropriate quantitative analyses, all the behavioral patterns in this volume must be regarded as unsubstantiated and therefore as inadmissible data." On the contrary, what is unsubstantiated and inadmissible are theoretically barren, mechanistic approaches to archaeology. Ten years ago, Helmer's slavish dependence upon statistical pattern recognition procedures would have gone unchallenged. Today, however, archaeologists are striving to *eliminate discordance between data structure and methods of analysis* (e.g., see Carr, 1984). In recognition of the complex, polythetic nature of archaeological data, archaeologists have rejected methodologies that mask variability and information, particularly at initial stages of analysis when data structure is unknown.

Exploratory Data Analysis (EDA), an approach I used to give meaning to the Peace Point data, has replaced the sterile, involuntary techniques of the past. But EDA is more than method, it is a state of mind, it is a way thinking about data and analysis. The underlying assumption of EDA is that the more one knows about the data, the more effectively data can be used to develop, test and refine theory. However, this requires adherence to two principles mechanistic analysts would find difficult to embrace: skepticism and openness: "One should be *skeptical of measures which summarize data* since they can sometimes conceal or even misrepresent what may be the most informative aspects of the data, and one should be *open to unanticipated patterns* in the data since they can be the most revealing outcomes of analysis" (Hartwig and Dearing, 1979:9).

I titled my volume on *Peace Point Window on the Past* for reasons that I had hoped would become clear during its reading. In retrospect, I should have been more explicit about this. The "window on the past" doesn't lie in anything tangible on the ground waiting to be discovered, not even at Peace Point. Rather, it lies in our approaches and our commitment to forging linkages between static archaeological remains and the dynamic behavioural processes that produced them. Helmer implies that I have exceeded the "limits of my data" in *Window on the Past*. But there are no inherent limitations on data, only on imagination. And in the northern forests of western Canada, where the archaeological record, and our knowledge of the people who produced it, are so impoverished, model building and theorizing are what is called for. Adherence to outmoded concepts and sterile, mechanistic approaches can only retard the development of boreal forest archaeology.

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GLACIATION IN ALASKA: THE GEOLOGIC RECORD. Edited by THOMAS D. HAMILTON, KATHERINE M. REED and ROBERT M. THORSON. Anchorage: Alaska Geological Society, 1986. v + 265 p., maps, tables, figs. Softbound. US\$18.00.

This is an invaluable compendium of information and current thinking on the questions of glacial geology in Alaska. This summer I

took the volume with me into my field camp in the St. Elias Mountains in the southwest Yukon but made slow progress because of the wealth of information in the book and because the papers really pointed out many of my own field problems of glacial geology. The changes in stratigraphic nomenclature that have occurred recently, together with the abundant evidence displayed by many authors for different patterns of glacier behaviour in different regions, are requiring a rethinking of ideas on correlation of events. This rethinking is apparent from the book in the discussion being generated between the scientists reporting on each area. With the fluidity of ideas, it is particularly valuable to have so much material assembled in one volume.

The presentations demonstrate the wide range of dating techniques that it has become necessary to employ, the problems of correlation of the different dating techniques and the gaps that still exist in dates of glacial events. They demonstrate the development of a framework of mountain glaciation fluctuations that are not constrained by the continental glaciation framework and point out the need for considerably more research in the field and in the laboratory for the development of new dating techniques.

Some of the field measurement techniques employed can be questioned — for example, the morphological measurements of moraines reported in some detail, the rock hammering techniques and the validity of lichenometrical techniques — but in general the papers are both good syntheses of research conducted and good scientific presentations of ongoing work.

The presentations are divided regionally, with chapters on the Brooks Range, the Seward Peninsula, the Yukon Tanana Uplands, Nenana River Valley, West Central Alaska, the Alaska Peninsula, the Aleutian Islands, Cook Inlet basin, the Gulf of Alaska and southeast Alaska, and a general summary of the process leading to the book and some general conclusions begin the volume. Although a synthesis of the whole of Alaska was not the purpose of the book, a more detailed overview of the implications of the regional findings is probably necessary. The message would appear to be that similar trends throughout Alaska mask considerable differences in detail of glacier fluctuations.

The volume undoubtedly fills a need for all of us interested in glacial geologic problems of the cordilleran northwest of America and should be a basic reference book on the desk of all glacial geologists and glacial geomorphologists. The editors have, as they point out in their introduction, seen the need for a synthesis volume, and they must be congratulated for their successful completion of the task.

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ICE SEAMANSHIP. By GEORGE Q. PARNELL. London: The Nautical Institute [202 Lambeth Road, London, England SE1 7LQ], 1986. 87 p., 35 figs., index, recommended reading list. Softbound. £17.

The aim of *Ice Seamanship* is to provide a handbook for navigators and masters of ships operating in ice-infested waters. The author, George Q. Parnell, as a master mariner of the company of Master Mariners of Canada and a member of the Nautical Institute, is presumably well qualified to write such a book, although he gives no autobiographical details. As a reviewer who is more familiar with sea ice than with seamanship in sea ice, I found this annoying, as the reader has no information as to the experience of the writer.

Quite rightly, the hazardous aspects of operating ships within an ice cover or near the ice edge are stressed in great detail. Also the very sensible recommendation — do not proceed unless you are sure about what you are going to encounter — is to be found time and time again in the handbook, advice that cannot be over-stressed. Radar, although a valuable asset in pack ice and just off the ice edge, is not to be relied on,

and the author is careful to point this out. Suggestions on how to trim the ship, what to do if beset in the ice, the correct track through pack when under escort, iceberg avoidance, and what to do to minimize damage if collision is unavoidable are all provided, along with many other valuable pieces of information.

There are several points in the book that I am unhappy about, however. My principal objection is that the author is really not too informative about pressure ridges. Indeed these features, which are very common within the ice cover, are mentioned only once, and their significance to ships is severely understated. A 10 m sail and a 30 m keel will really not do a ship too much good if collision occurs. The omission of a detailed discussion on pressure ridging and other features of sea ice deformation is serious for another reason. Pressure ridge sails offer the best indicator of the age of sea ice, rather than colour as the author suggests. It is always difficult to tell the difference between first year and multiyear ice floes, but it would usually be near impossible to do so by colour alone, given the ice will almost certainly be snow covered. Shape and degree of consolidation of pressure ridge sails, combined with other morphological features of the ice cover, are the best indicators. On the whole, the account of ice properties in the handbook is incomplete and rather naive, indeed sometimes erroneous. Fortunately the errors would not impact greatly upon ice seamanship. Grease ice, for example, as a herded slurry of frazil crystals, can reach over a metre in thickness; all salt does not drain from multiyear ice; the word "height" applied to ice is ambiguous — "freeboard" should be used; the raised rims of pancake ice are mainly the result of wave pumping. There are more.

Mention of waves brings me to another point. There is only one mention of there being reduced wave and swell action in pack ice. This would seem to be important information to the ice seaman. However, there is also no mention that several vessels have been destroyed by entering the pack in search of calm seas, only to find the worst conditions imaginable within the first km of the edge: high waves throwing ice floes at the ship.

Finally, I should add that there is no description whatsoever about ice chart interpretation. I would have expected this to be an essential section in a handbook of this type.

The handbook is clearly presented with many illustrative diagrams. It is typewritten rather than typeset.

Sadly, I cannot recommend *Ice Seamanship* as a stand-alone handbook on the operation of ships in pack ice, but as a complement to other manuals on this subject perhaps, since the experience of the author as a master mariner and his recommendations are presumably valuable to ships operating in these inhospitable seas.

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HOW TO FIND INFORMATION ON CANADIAN NATURAL RESOURCES: A GUIDE TO THE LITERATURE. By GABRIEL PAL. Ottawa: Canadian Library Association, 1985. 182 p. Softbound. Cdn\$25.00.

Natural resources have received increased attention over the last several decades, stemming in part from escalating energy prices, rapid depletion of energy and other resources, and a growing interest in conservation and ecology. A growing body of natural resource literature has reflected this heightened interest, and the number of books, articles, serials, and government publications relating to natural resources has become large and unwieldy. This is especially true in Canada, a nation both rich in natural resources and dependent upon their export. Gabriel Pal's book attempts to make sense out of this expanding literature and provides a useful guide to gathering current data on natural resources.