

linguistic near-metaphor is overstated. Relatively monotypic fish taxa may well fit his claim, but the heads of relatively polytypic humans are remarkably variable *if you look closely enough*. This counterclaim of mine is based, not incidentally, on empirical findings based on a large battery of 80 craniofacial traits ranging from traditional larger-scale "covering" measurements of two or more bones to small-scale measures of the developmental end products of individual ossification centres.

The more exhaustively and comprehensively one describes the morphometry of skeletal form, especially developmentally complex skeletal components like human crania, the more one appreciates variety at both the intra- and inter-group levels. The very fact that, in certain computer runs, 100% of my study crania were correctly identified through ("blind") classificatory multiple discriminant analysis bespeaks the morphological distinctiveness of (even) closely related regional groups of Inuit, Yuit and Aleuts.

As to Bookstein's cautions about the form of crania being a joint function of gene action, epigenetic interaction of developing tissue systems, sundry environmental effects including nutrition and climate, and — I would add — "plastic" remodeling in response to biomechanical stress, I am fully cognizant of these. Indeed, there are some extended discussions in my monograph on the "meaning" of craniofacial form, i.e., the multiple factors that literally shape it. Further, I am fully aware of the dangers inherent in making phylogenetic inferences from dendrograms/cladograms, owing to converging/diverging effects of natural selection.

Contrary to Bookstein's claim, my Figure 1.2 is not a representation of the "linguistic" history of Eskaleuts, but rather is a composite representation of affinities based on linguistic *and* spatial considerations, which furthermore is consistent with the cultural, historical and archaeological data on inter-group affinities. In my monograph I state the case for considering the "benchmark" of inter-group affinities, as presented in Figure 1.2, an accurate portrayal of the population historical relationships of these populations. Boas and Sapir need not roll over in their graves, for there are independent lines of evidence that the linguistic, cultural and biological attributes of populations *have* substantially co-differentiated in the western North American Arctic and Subarctic.

As to the "traits which emerge(d) as best" in my study: one particular 18-trait battery (out of many batteries of variable trait number that were empirically tested) was found to produce inter-group generalized distances most concordant with the benchmark (Figure 1.2) of approximately known historical relationships. These traits were hardly "a random selection", as claimed by Bookstein. Rather, this battery was composed of variables that were (a) found *a posteriori* to be the most powerful multivariate discriminators, less (b) those that were found or reasoned to be most prone to imprecision, age-related change and subtle intentional occipital deformation, less (c) those that are not morphometrically efficacious, less (d) those that cover areas of the craniofacial skeleton that are biomechanically related, or physiologically responsive, to the function of mastication. Hardly a random selection of traits!

The above results constituted my "principal finding" (not the predominance of neurocranial breadths, as claimed by Bookstein), *viz.*, that when one carefully pares down a large trait battery, discarding variables susceptible or labile to various sources of noise, one is left with a reduced trait battery that yields morphometric distance results that have apparent phylogenetic (population historical) meaning for, at least, an approximately synchronic group of late prehistoric/early historic Eskaleutian skeletal samples.

By extension, I argue that this taxonomically optimal battery can be used to advantage in tracing the threads of population historical continuity farther back into the prehistoric past, in a fashion analogous to the direct historical approach in archaeology. I emphasize in my monograph that my particular findings, i.e., the specific 18-trait composition of my taxonomically efficacious battery, are *not* generalizable. In other words, this particular battery "works" for the restricted region studied — no more and no less.

However, the above position may be overly conservative. Could the very battery, out of many tested, that was constructed of the most powerful non-redundant discriminators, less those traits that were demonstrated or reasoned to have been noise-modified by measurement error, age regression, deformation, poor morphometric meaning and biomechanical functions, *just coincidentally be the one that performed best in a taxonomic application?* While my particularistic findings are not generalizable to "other studies more extensive in space or in time," as Bookstein correctly indicates, my methodology/approach to quality control screening of variables may *de facto* yield a reduced trait subset that may be generally efficacious, taxonomically.

Obviously, and I think for good reason, I am not so pessimistic as Bookstein about the prospects for squeezing information about population origins, migrations and affinities out of the sizes and shapes of head bones. That we have not done an especially good job of addressing these problems, for some of the reasons that Bookstein articulates, I would agree. Likewise, I fully concur with Bookstein that morphology is a covariate, a joint function of many interacting genetic, epigenetic and extragenetic factors. But this reality need not lead to the

raising of hands in submission. Rather, the challenge is to figure out ways of teasing apart those traits that are informational vs. those that lead us down false paths in our attempts at population historical reconstruction.

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WINDOW ON THE PAST: ARCHAEOLOGICAL ASSESSMENT OF THE PEACE POINT SITE, WOOD BUFFALO NATIONAL PARK, ALBERTA. By MARC G. STEVENSON. Studies in Archaeology, Architecture and History. Ottawa: National Historic Parks and Sites Branch, Parks Canada, 1986. 145 p., illus., appendices, refs. Softbound. Cdn\$8.75; outside Canada Cdn\$10.50.

This volume summarizes the results of two seasons of archaeological survey and assessment at and in the vicinity of the Peace Point Site (IgPc-2) in Wood Buffalo National Park. It comprises eight chapters, beginning with a brief introduction in Chapter I and a short synthesis of the history of archaeological research in northeastern Alberta and southwestern Northwest Territories in Chapter II.

Chapter III focuses on the significance of Peace Point in historic times, on a reconstruction of present and past environments in the site area and on a summary of survey and test excavations done at the Peace Point Site and other nearby sites during the 1980 season. Additional results of the 1980 season test excavations at the Lake One Dune Site (IgPc-9) and a detailed survey of a 32 km section of the banks of the Peace River are reviewed in Chapter IV. It is argued that the area around Peace Point has been occupied by boreal forest-related and plains-related peoples at intervals over the last 7000-8000 years.

Chapter V, the most substantial section of the book, summarizes the results of the 1981 season excavations at the Peace Point Site. This chapter includes a statement about the research goals of the 1981 season, a description of field methods and a detailed summary of the cultural remains associated with each of the 18 stratigraphically discrete cultural levels identified during the excavation. The latter discussion is interspersed with comments on *perceived* patterning in the composition of individual lithic and faunal assemblages and in their spatial distributions.

Broad temporal trends in technology, subsistence patterns and inter-regional affiliations are reviewed in Chapter VI. As well, concepts derived from recent ethnoarchaeological research by L.R. Binford are employed in this chapter to interpret patterns of "regional mobility" and the inferred alternating function of the Peace Point Site as a "base camp" and as a "campsite."

Chapter VII is devoted to developing two "cultural formation models" deemed to be potentially useful for future explication of the behavioral history preserved at the Peace Point Site. The first model, explained in greater detail elsewhere (Stevenson, 1985), examines the dynamics of artefact assemblage formation based on a functional three-stage sequence of depositional and post-depositional events. The second model focuses on the marked dichotomy of male/female activities characteristic of northern hunter-gatherer groups and speculates on the effects that this behavioral duality may have had on the formation of activity-related patterning in the archaeological record.

The final chapter (Chapter VIII) summarizes the major points raised in the preceding seven chapters. The three appendices to this book include the results of a detailed soil analysis of samples gathered in 1980 and 1981 from the Peace Point Site and its immediate environs (Appendix A), a macro- and microfloral analysis of soil samples (concentrated on the lower levels from the exposed cliff face at the Peace Point Site) gathered in 1980 (Appendix B) and a summary table of metric and non-metric attributes of lithic artefacts from selected levels of the Peace Point Site (Appendix C).

According to the author (p. 9) this work was designed to accomplish three things: a) to illuminate the importance of the Peace Point Site, b)

to present major research findings from each season and c) to put the cultural, methodological and theoretical contributions the site promises in proper perspective. Stevenson is by and large successful in achieving the first two objectives. He falls short of the mark, however, in his efforts to realize his third and potentially most substantive goal.

The descriptive summaries contained in this book represent a major contribution to northeastern Alberta prehistory, which "is at present poorly known" (p. 11). *Window on the Past* contains a substantial body of useful raw data presented in tabular and descriptive form, supplemented by ample and impressively detailed illustrations. Stevenson clearly establishes the basic stratigraphic integrity of the Peace Point Site — a unique feature in itself, considering the rarity of stratified sites in northern North America — and provides a very useful, and potentially quite significant, insight into possible temporal trends in flake and blade core morphology.

Elements of the lithic analyses reported in this volume are also of interest. Stevenson capably demonstrates that much can be learned from the detailed study of the often ignored masses of debitage (i.e., stone debris left over from the manufacture, maintenance or repair of stone tools) recovered from archaeological sites. I remain sceptical, however, of the premise that ratios of debitage to finished tools is a reliable criterion for distinguishing the somewhat nebulous concept of lithic workshop from the often times equally arbitrary concept of habitation site (e.g., p. 40).

Less satisfactory in my opinion, though still of genuine interest, is the informal analysis of raw material diversity that partially underlies Stevenson's interpretation of changes in mobility patterning and site use at Peace Point through time (p. 94). The author's principal argument is that differential patterns of resource utilization (i.e., greater or lesser diversity of local and non-local lithic raw materials used) are reflections of changes in the settlement and/or subsistence strategies of the historic and prehistoric occupants of the Peace Point Site (p. 94). There can be no doubt that the recognition of non-local raw materials in the Peace Point collections provides invaluable evidence of human interaction over a broad geographic area. However, I do challenge Stevenson's assumption that changes in lithic resource utilization patterns perceived in the Peace Point data set are necessarily indicative of significant changes in human behavior through time.

A scatter plot (a simple descriptive statistical device for quickly determining if linear relationships between variables may actually exist in a data set) of raw material diversity (a count of the number of raw materials represented in the assemblage) plotted against sample size (the total number of lithic artefacts in that assemblage) for each cultural level in the Peace Point data set shows that a weak but discernible linear relationship exists between diversity and sample size. In other words, as the sample of artefacts increases in size, the number of different raw materials represented also increases. The scatter plot does reveal a single marked outlier to this trend in level 05. Reference to Table 5 (p. 59) indicates that level 5 is characterized by a relatively large sample with low raw material diversity.

A linear regression analysis of the scatter plot (conducted to measure the strength of the linear relationship between the two variables being measured) yielded a correlation coefficient of $r=0.619$, which provides statistical support (albeit weak) for the inferred linear relationship between sample size and diversity. Removal of the outlier value from the calculation of this statistic (done because outliers exert a strong biasing effect on linear regression calculations) increases the "strength" of the correlation coefficient to $r=0.82$. This latter result strongly suggests that, with the possible exception of level 05, the patterning in resource procurement identified by Stevenson is a function of sampling bias and not a reflection of changes in cultural behavior through time. Similar observations apply to Stevenson's treatment of the faunal data from the Peace Point Site.

The simple fact that IgPc-2 has yielded a series of faunal assemblages in stratigraphic context is noteworthy. But bone samples from individual levels at this site are uniformly small. Furthermore, many of the taxa identified in Table 6 (p. 60-61) are represented by only single bone elements. Given these limitations, it is difficult, if not realistically

impossible, to draw more than the most preliminary conclusions about patterns of past economic behavior from the obtained data.

Despite these constraints, Stevenson argues (p. 90, 93-94) that shifts in the diversity of species representation in individual faunal assemblages reflect adjustments in economic strategies practiced by the occupants of IgPc-2 through time. A simple statistical analysis of the raw data presented in Table 6 calls this interpretation into question.

A scatter plot of the faunal information summarized in Table 6 (excluding counts for Unidentified Mammals and Category Uncertain, as these taxa comprise very large subsamples of highly fragmented, unidentifiable bone that significantly skew the calculation of total sample size for several levels) reveals two distinct sets of relationships inherent in the data from Peace Point. For 11 of the 18 levels, a clear linear relationship between sample size and species diversity (the larger the bone sample, the more individual species that are represented) is depicted. Two distinct groups of outliers are also revealed. One of these consists of levels 16, 17 and 18. Reference to Table 6 indicates that the faunal assemblages from these levels are characterized by high species diversity regardless of sample size. The second group of outliers comprises levels 1, 3, 6 and 7, all of which are characterized by relatively low species diversity despite comparatively large sample sizes.

A linear regression analysis of all 18 faunal assemblages yields a correlation coefficient of $r=0.685$, suggesting a statistically weak positive correlation between sample size and diversity in the combined data set. Removal of the two groups of outliers from this equation produces a regression coefficient of $r=0.90$, which is indisputable evidence of a very strong linear relationship in the remaining data. This statistic indicates that mathematical parameters are significantly influencing some of the perceived patterns of resource utilization at the Peace Point Site.

The presence of two discrete groups of outliers in the faunal sequence suggests that sampling bias *may not* be the only valid explanation for the changes in the faunal record of the Peace Point Site. It is conceivable, as Stevenson suggests (p. 90), that the occupants of the site shifted from the focal exploitation of large terrestrial mammals to a more diversified economy during the fur trade era. It should be noted, however, that the levels comprising the first group of outliers — levels 16, 17 and 18 are the three most recent occupations of the Peace Point Site (all likely post-dating the mid-18th-century A.D.) — and levels 1, 3, 6 and 7, which comprise the second group of outliers, are the four earliest occupations with acceptable faunal samples (all likely pre-dating circa 1400 B.P.). The possibility that differential bone preservation is responsible for the perceived differences between the two groups cannot be ignored, despite Stevenson's argument to the contrary (p. 90). Certainly, the fact that the most obvious differences between the species profiles of levels 16, 17 and 18 and levels 1, 3, 6 and 7 are in the presence/absence of bird, fish and small fur-bearing animal remains (all of which are less likely than large terrestrial mammal bones to survive for long periods of time in a buried context) lends indirect support to the position that the variations in species diversity reflected in the faunal materials from Peace Point are largely artefacts of the samples themselves and not measures of changing economic behavior through time. In the final analysis, the equivocal results of the preceding statistical tests suggest that the faunal data from the 1981 season excavations at the Peace Point Site are incapable of supporting the level of interpretation attempted by the author.

A substantial portion of this book is devoted, either directly or indirectly, to the formulation of models "to account for artifact patterning thought to have been produced by site formation processes during occupation and by division of labour by sex" (p. 9). While sympathetic with the author's desire to exploit the full potential of the Peace Point Site and to pursue more sophisticated problems of prehistoric hunter-gatherer behavior, I feel that Stevenson has been far too uncritical of his current data set and has relied far too heavily on intuitive, as opposed to quantitative, pattern recognition in his analyses to succeed in his goal of making this volume a substantive contribution to the method and theory of hunter-gatherer studies.

Examples of problems arising from an uncritical assessment of the lithic raw material and faunal data from the Peace Point Site have already been discussed. More serious criticisms of a similar nature can be applied to Stevenson's intuitive analysis of assemblage patterning on a level-by-level basis, the results of which are ultimately used as empirical justifications for the two key cultural formation models presented in this work.

Excavations during the 1981 season concentrated on the block excavation of a judgementally selected circa 4×3 m area of IgPc-2 (p. 46). The actual exposed area of individual levels varies, as the block excavation was located on a terrace edge. This block excavation represents only 10% (or less) of the extant portion of the Peace Point Site (estimated by the author to be circa 20 m long and 6 m deep (p. 25-26). Given the low sampling fraction (10%) and the fact that excavation units were judgementally and not randomly located, the excavated data set from IgPc-2 *cannot* be considered statistically representative of the Peace Point Site.

Stevenson (p. 95-96) acknowledges that sampling problems may impose some limitations on his data, but he does not specify what these limitations might be. Unfortunately, the non-random nature of the 1981 sample of artefacts from IgPc-2 has a critical, and perhaps fatal, impact on his studies into variations in assemblage composition patterns and spatial distribution patterns.

Implicit in all of the analyses reported in this volume is the assumption that the individual cultural levels intersected by the 4×3 m block excavation are comparable analytical units. That is to say, the data from each level are considered to be representative of the totality of activities carried out at the Peace Point Site during that depositional episode. As a corollary to this, the author also tacitly assumes that variations perceived in assemblage characteristics both within and between levels are the result of human behavior and not of differential sampling biases.

In fact, there is no reason to believe that individual occupation levels at the Peace Point Site are comparable units of analysis at all. Evidence in support of this position can be gleaned from the excellent level plans provided and from Stevenson's own arguments concerning the stratigraphic integrity of IgPc-2.

The majority of occupation levels identified as representing episodes of intensive site use are associated with possible hearth features. The presence of these features is central to most of the behavioral reconstructions presented in this volume, as it is intuitively assumed that patterned primary and secondary disposal/loss activities were carried out around the hearth area (e.g., p. 50-56). An informal comparison of floor plans for levels 1 (p. 49), 6 (p. 68) and 16 (p. 85) reveals that the 4×3 m block excavation does not sample the space around each hearth feature equally, as hearth locations shift from a central position in level 1 to the southwest edge of the unit in level 6 to the northern edge of the excavation in level 16. Assuming that behavior around hearths was in fact patterned, each of these levels has sampled a markedly different subset of behaviors and are, therefore, *not* directly comparable.

The problem of comparability is further exacerbated by the presence of multiple hearths in some levels. The existence of two or more of these "behavioral loci" within less than a 1.5 m radius begs the question of mixed assemblages and further reduces confidence in the integrity of the sample.

Stevenson (p. 90) is on the right track, I believe, when he discusses the possibility of treating hearths as the primary analytical unit in what he refers to as an "episode analysis." Unfortunately, he does not attempt to apply this potentially more rigorous approach to his investigations. If he had done so, *Windows on the Past* could have stood as a major methodological contribution to hunter-gatherer archaeology.

My final criticism of this volume concerns the methodology (or lack thereof) actually employed to define the significant patterning of artefact distributions. Very simply, Stevenson relies on the subjective visual assessment of level plans to isolate meaningful spatial aggregates of artefacts. This is completely unacceptable given the array of statistical procedures available today capable of distinguishing between random and non-random artefact distributions. In the absence of appropriate quantitative analyses, all the behavioral patterns identified

in this volume must be regarded as unsubstantiated and therefore as inadmissible data.

The preceding review has been extremely critical of much of the substance contained in the book *Window on the Past*. This is because I am convinced of the very real importance of the Peace Point Site as one of the few stratified sites in northern Canada. As Stevenson suggests, the site *has* great analytical potential. Before this potential can be realized, Stevenson must take a harder look at the true nature of his data set and define the acceptable parameters within which he, and others, can work. Additional research at Peace Point, based on a well-designed sampling strategy, is clearly called for. Hopefully, this research will be carried out in the near future.

Window on the Past, despite the reservations expressed above, will be useful to the professional archaeologist interested not only in northeastern Alberta prehistory but in hunter-gatherer studies as well. Stevenson's models of assemblage formation and male/female activity sets, though in my opinion totally unsupported by the preliminary analysis of the Peace Point Site assemblage, are nonetheless original contributions to theory and provide ample food for thought. Further refinements and testing of these hypothetical constructs by Stevenson, and others, are eagerly awaited.

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Response from the author

In his review of *Window on the Past: Archaeological Assessment of the Peace Point Site, Wood Buffalo National Park, Alberta*, Helmer condemns my analyses and interpretations of the Peace Point data. Although his objections may seem credible to some, I will demonstrate that they are based on something other than a careful reading of the book and an adequate understanding of archaeological procedure. I normally ignore rhetorical reviews. However, Helmer's condemnations of *Window on the Past* deserve response, lest they hinder the growth of boreal forest archaeology. Helmer's review is replete with confusion and contradiction. Yet, rather than dwell on each point, I will concentrate on what I believe to be the fundamental flaws in his analyses and logic.

Helmer has severely criticized me for my interpretations of the Peace Point lithic and faunal data in the absence of rigorous statistical procedures. He then conducts several *simple* linear regression analyses to ascertain the relationship between sample size and faunal and lithic diversity. A correlation coefficient of $r = .619$ is said to provide evidence that sample size, not human behaviour, accounts for the number of lithic material types in Peace Point's levels. Yet, any introductory textbook on statistics will tell you that this r value accounts for only 38% of the data. To strengthen his argument, Helmer throws out one of the potentially most important levels at Peace Point — Level 5, in contrast to bracketing cultural levels, contained no exotic lithic materials and a broader range of species, suggesting an intensive use of the local environment.

Helmer continues this questionable form of analysis when he examines the relationship between faunal diversity and sample size. Again, a relatively insignificant correlation coefficient, $r = -.685$, is obtained and disregarded in favour of dropping "outliers" from the analysis. Regrettably, the *faux pas* this time is more serious; two groups of *perceived* "outliers," comprising 7 out of 18 levels, are discarded.

Both Helmer's regression analyses and curious habit of throwing out data that do not conform to his expectations are unacceptable. When Helmer considers only *simple counts* of lithic types and animal species from each level, he masks important sources of variability and information. My interpretations of changing land use patterns at Peace Point in regards to lithics were based on not just the number of different types of lithic materials present, but whether they were *local* or *non-local* in origin and *how many of each type* were associated with each level. Helmer's simple linear regression analyses ignore this crucial information

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