nobody should be under any illusions as to the seriousness of the situation." Well, perhaps it is, in the sense that the biological productivity of the world is threatened. But, comparatively, what is the extent of the threat? Is it life-threatening, as in the Sahel? Are whole ecosystems disappearing, as they are in the clearing of tropical forests? Are the lakes being killed, as in Norway and Ontario? No: Sage's concern is that "the wilderness atmosphere of large areas has been destroyed," and "nobody really knows what effects industrial activity in the Arctic will have in the long term on the fauna and flora."

The current, unprecedented rate of degradation of the world, well illustrated in the report of the World Commission on Environment and Development (the Bruntland Commission), is consequential on pressures on the resources of the biosphere caused by high levels of resource use by humans and high rates of human population increase. Solutions, even theoretical ones, are difficult and paradoxical: in practice, the imperatives of political and religious leadership put the problems beyond the capacity of democratic institutions to resolve. However, the Arctic is as well buffered from these pressures as is any geographic zone: indigenous populations were extremely sparse until recently, and industrial growth has been slow.

Now that the populations of arctic peoples are expanding, and material expectations escalating, many look to economic development for their future well-being. Development will indeed entrain some loss of wilderness among the costs. However, the decisions must be made by northerners, and not for them. Southerners can be confident that conservation is close to the hearts of their cousins in the North. The problem will be one of balance.

The Arctic & Its Wildlife is a book most people will like, for its many illustrations and informative text. It is attractively presented and largely free of errors (except for Canadian place names on page 18). Its deficiencies are due mostly to its ambitious scope, its emphasis on cataloguing information and the weak relationship between the factual information it displays and the facile message it attempts to deliver.

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EXPLORATORY HUMAN CRANIOMETRY OF RECENT ESKALEUTIAN REGIONAL GROUPS FROM THE WESTERN ARCTIC AND SUBARCTIC OF NORTH AMERICA: A NEW APPROACH TO POPULATION HISTORICAL RECONSTRUCTION. By GARY M. HEATHCOTE. Oxford: B.A.R., 1986. BAR International Series 301. xiv + 332 p. £12.50.

For at least ten years, judging by his own entries in the 50-page bibliography here, Gary Heathcote has been pursuing the history of arctic populations by consideration of skeletal remains. The present book, a revision of his Ph.D. dissertation (and typed variously in pica and elite), is essentially a large-scale confirmatory study of this kind. From within a sufficiently rich suite of cranial measurements, the author has found a subset that, collectively, are highly concordant with population "distance" scores as reconstructed from "geographic as well as genetic linguistic criteria." He hopes (p. 196) that his findings, after temporal and spatial extension, will "enable a more robust attempt than heretofore allowed at unraveling human population historical relationships in the Arctic and Subarctic zones of North America, Siberia, and Greenland."

As I am a morphometrician by trade, my interest was particularly piqued by the major subordinate theme of this work: the enrichment of craniometrics within the bounds of its present caliper-based tradition.

> The [overriding] reality is that researchers in human osteology will continue, for some time, to have universal access to only the simple tools used in this study. Pioneering works . . . will eventually compel osteologists to abandon their calipers, but for the immediate future, 'old fashioned' osteology will persist. Certainly, a case can be made that there is room for improvement within the constraints of the currently widespread, caliper-wielding approach to morphological questions. This study strives for such improvement. [p. 63-64.]

In this aspect of his project, Dr. Heathcote's timing was most unfortunate. After he gathered his 35 000 measures, but before publication of this volume, there began to appear major revisions of the foundations of morphometrics and its relation to multivariate statistics, changes that could have saved the author a great deal of effort. The emphasis on "nonstandard measures," of which he is justly proud, is but a way-station toward the exploitation of strictly patterned sets of caliper measures as the equivalent of explicitly recorded Cartesian coordinates exploited in turn to construct optimal measurement schemes for particular group differences. Heathcote's principal finding (p. 193) is that his taxonomically optimal trait battery is dominated by breadths, mainly of the neurocranium. This finding could very likely have been generated wholly automatically by a direct construction of distance measures most sensitive to the distinction between Aleut and Inupiaq language groups or between the Kagamil sample and the Kittigazuit. The appropriate method is mean tensor analysis, the ninth of nine "other more rigorous approaches to morphological description" listed, but not adopted, on page 63. And the findings would then appear in a coherent diagram of typical deformations instead of being a list of motley discrete variables.

As much as I would like to dilate on the new morphometric developments (see Bookstein *et al.*, *Morphometrics in Evolutionary Biology*, 1985), it would be inappropriate to dwell overmuch on them here. But I must caution the reader not to adopt certain of Heathcote's "unconventional" variables, notably the perpendiculars from chords to arcs of the vault. His goal, the representation of curving form, is sound, but it is not achieved by a suite of measures all confounded with the position of Bregma, Lambda, or both. I should point out that any analysis, however modern, of these skulls would be well served by the author's immense caution and competence in matters of measurement *execution*. The approach to data screening and precision testing recounted here is superb.

It is more useful to turn from the slightly obsolete morphometric details of this project to a consideration of the contribution that morphological data, according to whatever biometric canon, might make to studies of population history and prehistory, arctic or otherwise. Let us inquire generally whether morphology has any special contribution to make to such studies. In my morphometric view, the answer is a somewhat qualified "no," for two reasons.

1. Paradoxically, morphometrics offers too great a richness of measurements for the a-posteriori association of variable lists with predetermined classes to be meaningful. From any reasonably well-distributed scheme of landmarks (the author's 80 measures here are roughly equivalent to the digitizing of 28 separate points), almost any group separation having a biological basis can be corroborated by a suitably constructed morphometric descriptor extracted via analysis of deformation. But these are no more automatically meaningful than are the variables of a precisely analogous set, ratios measured at 45° to the first set, which are variables on which a pair of populations precisely agree in mean value: the "invariants" of the comparison, by contrast with the "covariants" found by Heathcote. The existence of both such sets is guaranteed by theorem, regardless of the nature of the populations.

2. The human head is highly constrained in its morphology. There exist mutually unintelligible languages, but, so to speak, no mutually nondeformable heads. The variability of normal heads is quite small, and much of that is epigenetic. Then morphological distances measured using skulls are too unreliable a function of variable selection to serve as evidence of interjacency in lineage studies. Indeed, the subject of Heathcote's book is in effect the *un*reliability of morphometric distance as adumbration of population history.

I would argue, instead, that morphology serves most usefully as a *dependent* variable in human biological studies. It is morphology that is to be "predicted," and ultimately explained, by group membership, not the other way 'round. Heathcote studied skulls deposited before major population catastrophes, such as European intrusions. Can we find the evidence of those intrusions in the skulls? Effects of climatic changes? Dietary developments? Advances in hunting technology? Wars and intermarriages? Would one do better to study post-cranial remains?

As in studies of protein evolution, traits having general selective value can be expected to conflict with a cladogram, being determined instead by convergence in response to environmental factors. Thus traits that emerge as best "corroborating" the "true" (linguistic) history of Eskaleut populations, Figure 1.2, will tend to the biologically meaningless. Even if these traits are truly the right indicators of the divergence in question, they represent a random selection from trait space, likely never to be replicated. There is thus no meaning to the author's findings that could reasonably be expected to generalize to other studies more extensive in space or in time.

It would be better to study morphological correlates of all the environmental and anthropological features that might *cause* modification of form during descent or migration. The author is aware of such correlates but considers them nuisances that must be subjected to "statistical control" to arrive at a "taxonomically optimal subset" (p. 196). On the contrary, these are the main meat of a morphological analysis: morphology as covariate. The present work, while its data are extensive and its statistical workup is heroic, does not persuade me that osteometrics will serve as a potent source of independent evidence in population historical studies.

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Response from the author:

I have been aware of, and admired, Bookstein's ground-breaking works on morphometric analysis since the late 1970s. While his review is predictably scholarly and insightful in many respects, there are some issues raised and allegations made that prompt me to respond:

1. Regarding morphometric methodology and the characterization of my approach as "slightly obsolete": Bookstein notes correctly that I discussed both the limitations of the caliper-based tradition in osteometry and the advantages conferred by certain newer approaches to quantifying and analyzing morphology, including his own. Here I would like to balance these frank admissions by illuminating some current limitations of Bookstein's methodology and strengths of my own. The distinction between morphometric methodology at the *analytic* vs. *observational* (data production) level is central to this critique.

First of all, it should be emphasized that Bookstein's mean tensor analysis method is arguably the most powerful approach to the quantitative *analysis* of comparative morphology currently available. Conceding this begs the question as to why I did not adopt his approach in my study. Bookstein provides the historical reason in his review. However, if I redesigned my study today, I still would not follow his example for essentially two reasons that relate to what I perceive as current limitations of his methodology at the *observational* level: Bookstein advocates the employment of (a) digitized *two-dimensional* coordinate data, produced on (b) *images* (e.g., photographs and radiographs) of the objects under study; in his case, fish (see Bookstein *et al.*, *Morphometrics in Evolutionary Biology*, 1985). In order for a "next generation" approach to yet more rigourous morphometric inquiry to evolve, a better integrated (data generation and analysis) system is needed. Such a system needs to have the capability of producing and making sense of *three-dimensional* data that can be produced *directly on the objects* under study.

Intuitively, we can all appreciate that complex three-dimensional objects are best quantified through considering their form in three-dimensional space. Bookstein in fact reports that, while he had not yet written the computer programs (as of 1985), his mean tensor analysis theorems are valid for three-dimensional space (*Ibid.*, p. 131). I trust that these computer programs are now written. With Bookstein's kind cooperation, I would like to be among the first of his colleagues to empirically test the three-dimensional version of his analytic tool, as long as its application is not tied to the use of images of, in my case, human crania.

Once I am able to produce — accurately, precisely, and efficiently — three-dimensional coordinate data directly on specimens, I hope to experiment with the 3-D extension of Bookstein's method. Hopefully, the appropriate technological developments needed for the production of a lightweight (see below) system for automated, direct production of 3-D data on objects will materialize soon. Currently available 3-D sonic digitizers have not been widely adopted, owing to their practical limitations, and purely manual ways (long available) of producing 3-D data directly on objects are too time-consuming for anyone working with large numbers of specimens. I am encouraged by the fact that mechanical 3-D coordinate measuring machines with computer interface are on the market, but they have been designed for industrial quality-control applications. While they are, in principle, adaptable for osteometric work, they are currently far too cumbersome for those of us who must frequently heft our measuring tools to our work sites. Hopefully, portable models will soon be developed.

While my seeming aversion to image analysis may seem reactionary, it is not a generic aversion but rather is based on some particularistic realities of producing data on, and interpreting, skeletal craniofacial form at the gross macroscopic level. The plotting and registration of landmark and surface outline coordinates from images, as recommended by Bookstein, would produce lower-quality observational data than those produced by a judicious, caliper-wielding student of craniofacial form, for reasons that include: (a) the frequent ambiguity of landmarks and outlines of the component parts of the craniofacial skeleton, (b) frequently encountered visual evidence of cortical bone remodeling, especially of a resorptive nature and most frequently involving the zygomaxillary complex, necessitating the interpolation of original (pre-metamorphic) locations of landmarks and surface outlines, and (c) the fact that certain dimensions — of my rather extensive and unorthodox measurement battery, at least — cover aspects of form that could not be "captured" from standard anatomic view (superior, lateral, inferior, etc.) images, as advocated by Bookstein.

Standard view images of, e.g., fish gross anatomy, may very well serve as adequate sources of raw data in Bookstein's morphometric studies. However, in craniofacial skeletal studies, dependence on images would both (a) introduce additional measurement error noise and (b) preclude the taking of certain finer-grained measurements of the component parts of the craniofacial complex.

In the former case, this is due to the fact that resolution of landmarks, outlines and tell-tale cortical bone surface metamorphosis is usually less problematic when one visually inspects the actual specimens, rather than pictures of same. Regarding the latter objection: Because many of my unorthodox measurements have known or assumed ontogenetic, functional and taxonomic meaning, I am loathe to abandon an "obsolete" caliper-based approach in favour of any "newer" approach not as amenable to the morphometric desideratum of comprehensive coverage of the form under study.

Bookstein's approach, especially its 3-D extension, has much to recommend it in terms of its morphometric data analytic power. However, its widespread application in, at least, human osteology will likely not materialize until better quality data generation complementation is developed.

2. Regarding the characterization of my variables as "motley" and "discrete": Here, I assume that by "discrete" Bookstein means that my (bounded, *continuous*) variables are not spatially interconnected, in terms of each being spatially relatable and referenced to all other variables in two-dimensional space. This is true and I admit that it would be far better, morphometrically, if the case were otherwise (as in the case of mean tensor analysis). However, some of my exploratory batteries were formulated on the basis of stepwise multiple discriminant analysis, and their constituent variables represent those with measured covariation vis-à-vis all other variables. These batteries contain variables with properties of demonstrated, powerful non-redundant discrimination, i.e., they each contribute substantial independent information on inter-group morphometric differentiation. These attributes do not seem wholly consistent with regarding these variables as "motley discrete."

3. Regarding the claims that morphology is ill-suited to the investigation of population history and is best regarded as a "dependent variable" in human biological studies: This issue is likely of greater interest to most readers of this journal than those above. After all, if human osteologists are *not* able to extract information on "where did these people come from" and "with whom are they most closely related," our serviceability as collaborators in population historical reconstruction with archaeologists, historical linguists and cultural anthropologists would be altogether nullified.

First of all, regarding Bookstein's claim that the head is too highly constrained in its morphology to be useful in population historical studies: his 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 incidently, 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 coincidently 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)