

Tixier) who were also of the opinion that they were produced with pressure, although none of them were able to reproduce similar ones. The reconstruction is certainly hypothetical, but so are most reconstructions of flint knapping techniques.

"Although previous work by the author and others had demonstrated that it was not possible to distinguish intentional from accidental breaks or break method on microblades, Owen again attempts this task for the Umingmak microblades and is unsuccessful." This is not true. At the time of this analysis (1981), the literature suggested that intentional and accidental breaks could be distinguished (see page 30). As part of the analysis of the Umingmak microblades I carried out a series of experiments on breaks. The results showed that it was not possible to distinguish intentional from accidental breaks or break methods. As these experiments were published in detail elsewhere, they were only summarized in this book, but they were a part of the Umingmak analysis and not previous work.

"One problem is that the actual differences are very small, ranging from 0.25mm to 1.25mm." Note: the differences actually range from 0.25 to 1.5 mm. Microblades from Arctic Small Tool tradition sites, with the exception of some Independence I collections are extremely small. Differences in the size of samples are therefore also very small. Cultural comparisons have nonetheless been based on similarly small differences. I, however, do not suggest that these differences are due to cultural differences. The smallness of the variations is one of my reasons for arguing that the effects of excavation technique on size are of importance. These differences were larger within the IA1 area than those between areas (ranging between 0.35 and 4.9 mm).

"In addition, the variation in length, which differs the most, is not significant according to the median test, while variation in thickness, which differs the least, is significant." In testing the significance of variations in measurements between samples, it is not the absolute difference that is important but the relation of the difference to the total range of measurements and their distribution. I suggest that a statistics book be consulted.

"Ridge blades, which are thicker and more numerous than "normal" microblades. . . ." Ridge blades are not more numerous than "normal" microblades, but make up only 11.2% of the total Umingmak sample (see pages 19, 96). They do, however, make up a higher percentage of the microblades from IA1. This may partially account for the differences in thickness between the excavation areas of IA1, ID and IA2 as I have stated in the text. It does not, however, play a role within IA1.

"If IA2 (1975) and ID (1973) are included, then the decrease in size does not correspond to year of excavation." The excavations in ID (1973) were carried out in a manner similar to that of IA1 1975. Comparisons of excavation technique were limited to IA1 to rule out the influence of other factors. In addition, no information was available on the number of cubic meters excavated per day per person from these excavation areas.

"In fact, according to chapter 2, excavation technique does not differ as much as recording technique and it is difficult to understand how this type of variation affects the size of artefacts recovered." The largest size variation is between the artefacts discovered during the TAYLOR/MCGHEE survey and those of the later Tübingen excavations, not within the Tübingen excavations. Unfortunately TAYLOR and MCGHEE did not publish a detailed description of the excavation techniques used in their survey. During their two week survey of the whole Shoron Lake area they did, however, sink 13 large test pits alone at Umingmak (see page 2). MCGHEE has recently told me that he alone sunk the test pits at Umingmak within a few days using a shovel. It is not difficult to understand how artefacts recovered during this quick survey differed in size from those of later excavations. The purpose of a survey is also different from that of an excavation. Excavation technique within the Tübingen excavations did not vary as drastically, but the number of cubic meters of sediment excavated per person per day in IA1 was five times higher in 1970 than in 1975 (see pages 34-35 and my comments in paragraph 3 above).

"A study to examine usewear under higher magnification (500×) is planned; inclusion of these results would have been a valuable addition to this investigation." A usewear analysis of the microblades at higher magnifications was already in progress at the time of publication. Preliminary results were not included in the publication because of problems with post-depositional surface modification on the Umingmak microblades and a growing scepticism of the method. To investigate these matters, I organized and chaired a conference on Technical Aspects of Microwear Analyses with G. Unrath in Tübingen in February 1985. The papers presented at this Conference (published as *Technical Aspects of Microwear Studies on Stone Tools*, Linda R. Owen and Günther Unrath (eds.), Tübingen, 1986) suggest that use-wear studies at higher magnifications are not as reliable as previously assumed. In a multi-analyst blind test of use-wear traces on experimental tools action/motion was correctly reconstructed in only 55% of all cases (48% specifically and 7% to the group level) and worked material determined 48% of the time (26% specifically, 24% to group

level) ("An Evaluation of Microwear Studies: A Multi-Analyst Approach" by Unrath, Owen, van Gijn, Moss, Plisson and Vaughan). Other articles discuss how chemicals in sediments and post-depositional movement can change and destroy use-wear polishes. I therefore doubt whether at this time a use-wear analysis at higher magnifications would be a valuable addition to the data presented.

"No evidence is presented to suggest that other archaeologists have excavated older sites more carefully or with a different technique than more recent sites. Even if the excavation or recording method is responsible for differences in microblade size noted at Umingmak, this conclusion cannot be generalized to the entire Arctic without substantial investigation." As stated on page 55, it was not possible to discuss the excavation techniques used at other Arctic sites as they are generally not included in site reports. Nowhere do I mean to suggest that excavation technique is responsible for all size differences in Arctic microblade samples. Only that comparisons of size attributes should only be made between similarly excavated samples.

Further research has been carried out since the completion of the Umingmak manuscript in 1981. On the basis of the analysis I received a two and a half year scholarship from the Volkswagenwerk Foundation to investigate microblade and blade technology and use in the North American Arctic and the Upper Paleolithic of Europe for my doctoral dissertation. The results will be published this year (see also *World Archaeology* 17(1) 1985). In the course of this analysis, I had the opportunity to analyze Arctic collections and to talk to Arctic archaeologists in Ottawa, Edmonton, Fairbanks, Anchorage, Washington D.C. and Copenhagen. 10,000 microblades and blades from over 65 sites were analyzed. In addition, refitting, work on use-wear analysis and experimentation were carried out. Unfortunately this time-consuming work was not possible within the Umingmak analysis.

This research has supported the belief that artefact size is easily affected by non-cultural factors of which excavation technique is only one. On the basis of this later research, I also feel that one of the most important findings of the Umingmak analysis was the method of platform edge preparation used in the production of the Umingmak Pre-Dorset microblades.

I am always interested in exchanging ideas on microblade and blade technology or on Arctic prehistory in general.

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ARCTIC WHALING. PROCEEDINGS OF THE INTERNATIONAL SYMPOSIUM: ARCTIC WHALING, FEBRUARY 1983. Edited by H.K. S'JACOB, K. SNOEIJNG and R. VAUGHAN. Netherlands: Arctic Centre, University of Groningen, 1984. 191 p. Softbound. No price listed.

Arctic whaling has become a popular subject for scientists and lay people. The adventures and hardships of commercial and aboriginal whalers provide intrigue to the layperson who vicariously relives these events. The aboriginal culture that evolved around the hunt provides information to the anthropologist for understanding the operation of northern native societies. The products of the hunt (i.e., the whale catches) offer cetologists a vital source of data for understanding the biology of marine mammals. Because of the ever increasing pressure to exploit the arctic environment, it is important that laypeople and scientists understand the position of marine mammals in the culture of native societies and in the oceans. Failure to achieve this could jeopardize the viability of the native cultures and the marine mammals uniquely adapted to the arctic environment. This book attempts to convey the current knowledge on the people, marine mammals, and environment associated with arctic whaling.

The book is a compilation of eleven papers and a summary of a panel discussion on the conflict between commercial and aboriginal whaling. The papers address the arctic climate and sea ice; the biology and ecology of whales; the history of native, European, and North American whaling; and the archaeology of native whaling societies. The last technical paper examines current policies and catch quotas concerning exploited whale populations and the methods used to hunt them.

The papers in the book vary in detail. The two papers on climate and sea ice summarize the factors that shape the weather and climate and that account for past, present, and possibly future sea ice cover of the Arctic. Historical whaling records are examined as a source for reconstructing past sea ice conditions. The biology and ecology of whales are described in three papers that explain the evolution, adaptations, and possible mechanisms of avoiding competition for the beluga, narwhal, and bowhead whales. The two papers on the history and archaeology of native whaling trace the influence of whaling on settlement locations, subsistence patterns, behavior and social identity of natives as well as describe their methods for hunting whales. Three papers describe the history of non-native whaling and present the chronology of the bowhead whale fishery in the eastern and western Arctic and Sea of Okhotsk and the countries involved in the fishery. The history and current management of cetaceans and the scientific basis of the management procedures are described in the last technical paper in the book.

While the papers are informative, they are not without faults. Typographical, spelling, and grammatical errors occur throughout the book. Some sentences are incomplete, awkward, or have substantial parts missing. Citations in some papers are incomplete and headings for tables or figures are occasionally missing or confusing. Because of these deficiencies, the contributing authors do not always provide the reader with a clear understanding of their papers. Some of the papers are also very site-specific with little attempt to more broadly apply the findings. If the Arctic Centre hosts another symposium, more careful editing and broader interpretation of the results would significantly add to the scientific value of the proceedings.

Arctic Whaling is a paperback book that is moderately priced. Despite the above-mentioned problems, the book contains useful information for any biologist, anthropologist, archaeologist, or anyone interested in arctic whaling.

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SYLLOGEUS 55 — CLIMATIC CHANGE IN CANADA 5: CRITICAL PERIODS IN THE QUATERNARY CLIMATIC HISTORY OF NORTHERN NORTH AMERICA. Edited by C.R. HARRINGTON. Ottawa: National Museums of Canada, 1985. 482 p., figures, tables, maps. Softbound. No price indicated.

This volume is a compendium of papers presented at the 4th meeting of the National Museum of Natural Sciences Climatic Change Project. The aim of the meeting was to focus on "critical periods" of the Quaternary climatic history of northern North America. Apparently no such periods were defined prior to the conference; thus the authors took it upon themselves to both define and describe these periods. The resulting critical periods range from instantaneous to 11 000 years in length! The book suffers somewhat from this lack of definition.

As is common for conference reports, the essence of each paper must be discussed separately. The introduction, by C.R. Harrington, largely introduces and summarizes the remainder of the volume. The book is confessed to be organized "somewhat arbitrarily" by disciplines, rather than by critical periods as might be expected from the title. The disciplines included and number of papers from each are bibliographies (2), instrumental records (2), historical records (6), prehistory (1

abstract), tree rings (1, 1 abstract), palynology (7), glacial geology and geochronology (1 each), and paleoclimatology and glaciology (1, 1 abstract). There is also the text of a special lecture by M.K. Thomas citing the importance of paleoclimatic data to a climatologist and urging paleoclimatologists to advertise the availability and significance of their data.

The Bibliographies section includes summaries by M. Andrews and C.R. Harrington of their bibliographies on Holocene Paleoclimates and Quaternary Climatic Change in Canada respectively. Andrews gives an intriguing look at the mechanics of a search strategy, by itself a useful concept. Both Andrews and Harrington also provide a hint of the uses of the bibliographies by showing the concentration of past work by geographic area (Baffin Island represents about 10% of global work) and by subject (glacial geology and palynology include >25% of all references). No mention is made of "critical periods."

The Instrumental Records papers address the 1930s drought (M.O. Berry and G.D.V. Williams) and the effects of major volcanic eruptions on Canadian climate (W.R. Skinner). The first paper, using water-based wheat yield as an indicator, concludes that the thirties drought, described at the time as "one of the worst droughts in history" in Saskatchewan, was in fact about a 20-year event. An implication might be that a "critical period" may be in the eye of the beholder. The next paper examines the effect of volcanic eruptions in the past 100 years using nationwide temperature and precipitation data. The approach is a good one, but the analysis is flawed. The many histograms are interpreted with a bias and the statistical testing is inappropriate. (Only composites having "an apparent dust veil signal" were tested as to the significance of the signal!) Even in the period of instrumental data volcanic eruptions are not shown to be "critical" in the causation of climatic change. M. Parker, in the Tree Ring section, draws a similar (inconclusive) conclusion of the effects of volcanic eruptions during the 1800s, but also fails to subject her data to rigorous statistical testing.

The Historical Records section examines climate between 1620 and the present. The papers involve proxy data, cover from 1 to 360 years, and conclude the following:

- 1) that the summers of 1816 and 1817 were exceptionally severe in central Canada (A. Catchpole; C. Wilson),
- 2) that the period between 1818 and 1860 was characterized by greater sea-ice cover than at present (M. Dunbar),
- 3) that 1715-1802 data indicate 1760 as a critical year, perhaps marking the end of the Little Ice Age in the Hudson Bay/James Bay region (T. Ball), and
- 4) that the climate of New England since 1620, although only partially reconstructed, shows general warming in the 1900s overprinted by major high-frequency signals (<20 yr) and variability among indicators (W.R. Baron and G.A. Gordon). The ingenuity displayed by all of these workers in data collection and interpretation is obvious and praiseworthy.

The Prehistory section and the second paper of the Tree Rings section (the first is that of Parker, mentioned above) are abstracts. These are tantalizing but offer little hard information. "Critical periods" are not mentioned.

The Fossil Pollen section occupies nearly a third of the book, and deservedly so. The papers (by J.B. Macpherson, R.J. Mott, P.J. Bartlein and T. Webb, III, L.V. Hills and others, and R.W. Mathews) offer an excellent blend of synthesis and new work and cover not only northern North America but the eastern United States as well! Unfortunately, only Mott clearly defines and attacks the problem of a "critical period" of climatic history, in this case the postglacial transition ca. 11 000 B.P.

In the Glacial Geology and Geochronology section, the paper by J.T. Andrews takes a multiparametric approach to the problem of climatic reconstruction. Pollen, niveo-eolian sediment, and marine molluscs are used to infer a glacial chronology since 11 000 B.P. He also warns of the filtering effects of sampling (pollen, sediment) on the inferred climatic record. J.V. Matthews, Jr., and C.E. Schweger take a different tack, defining "critical" on the presence not of climatic change but