6th AMQUA, 18–20 August 1980, Orono, Maine: 'The Structure of an Ice Age'

The sessions began with presentations of the essential facts and basic interpretations from land and sea, including isotopic, sedimentary, palaeontological, and geomorphic information. Succeeding speakers built on this information, reconstructing and modelling oceanic-atmospheric dynamics and the growth and disintegration of ice sheets. The third stage, then, was consideration of the causal mechanisms that would drive the models proposed, and these, it will be seen, centered on those mechanisms that would cause alternation of glacial and interglacial conditions on Earth, not the more fundamental question of ice ages (Quaternary) vs. the major non-glacial intervals (e.g. the Mesozoic) of Earth history.

This is not the place to assess or debate all the presentations and discussions, which were open, vigorous, frank and by no means limited to the meeting room – in the now-established AMQUA tradition. However, some highlights deserve mention. N.J. Shackleton began the proceedings with a review of highlights of oceanic oxgen isotopic studies. Out of this came a theme that recurred with variations throughout the sessions, namely the relation between oxygen isotopic changes and eustatic sea levels. R.G. Fairbanks pointed out that, according to Duplessy, some 30% of the oxygen isotope signal may be controlled by temperature changes.

On the topic of sea-level changes A.L. Bloom affirmed that our knowledge of the amplitude of eustatic change during a glacial cycle is still uncertain. The best estimate is no better than 100 ± 30m and perhaps no better than 120 ± 60m! On the other hand, he demonstrated statistically that there is no confirmation of a 'mid-Wisconsinan' high sea level ca. 30,000–40,000 BP. R.K. Matthews emphasized just how bad the problem of $^{230}Th/^{234}U$ dating can be, especially sounding the alert for effects of diagenesis and freshwater lenses in uplifted coral reefs.

J. Imbrie reviewed the method that he pioneered for reconstructing sea surface temperatures and salinities by factor analyis of planktonic marine fossil assemblages. The still amazing conclusion is that the interglacial-glacial temperature range is so small on a global average: only 1.7°C in summer and 1.4°C in winter. Imbrie's collaborator, A. McIntyre, then discussed the problems with this method, mainly in assumptions concerning the ecological stability of fossil groups and the post-mortem changes that may have occurred.

Gurdip Singh (Australia) reviewed low-latitude data on 'pluvial' and 'interpluvial' episodes in areas that are now dry. In general, he emphasized the difficulties in sorting out changes in absolute rainfall from data on lake levels, which may fluctuate as a function of other climatic/geomorphic controls as well. Singh pointed out that the sedimentary and vegetational histories indicated that glacial maxima were dry in Australia and Colombia and that interstadials and interglacials were moister. The dry 'glacial' climates were attributed to lowered evaporation from cooler oceans.

In contrast, L. Benson finds a precipitation increase of up to 2.5 times appropriate for the Lake Lahontan pluvial lake (Nevada), and G.I. Smith and J.C. Liddicoat calculate that an eight-fold increase in runoff would have been required to fill Searles Lake (California) to overflowing even with a temperature depression of 10°C. However, their chronology based on a long core, indicates that 'pluvial' conditions in Searles Lake were sometimes in phase and sometimes out of phase with 'glaciations' in the marine oxygen isotope record.

L.E. Heusser reviewed her work on pollen extracted from deep-sea cores, which provides a direct correlation of the terrestrial vegetation history with marine isotopic stages. Moreover, through factor analysis of the pollen assemblages she was able to evaluate the vegetational succession through various parts of the last glacial cycle, as well as identifying temperature-sensitive taxons that allowed her to estimate a range of only 3°C in July temperatures in the Pacific northwest during the last glacial cycle. J.C. Ritchie cautioned against interpretations made from 'descriptive numerical methods' that may suggest cause-and-effect relationships that are unproven.

Leading off the second-stage reconstructions W.F. Ruddiman discussed general circulation models (GCMs) and the CLIMAP input to these models. Essentially, he concluded that there is the need to simulate climate via GCMs over longer periods of time and through changing conditions, rather than at a single moment in time (18,000 BP) under 'equilibrium' conditions. In detail, Ruddiman presented results of spectral analysis of a core from latitude 40°N where the 23,000-year precession cycle is very strong, and he emphasized the lag of oxygen isotope peaks behind those of sea-surface temperature at times of rapid ice-sheet growth (c. 115,000 BP). (These results have recently been published in *Science 212*: 617–627 [1981]).

From oceans and atmosphere the discussion moved to models of ice-sheet growth and disintegration. J.T. Andrews reviewed models, pointing out that the proponents of terrestrial-based, precipitation-controlled ice sheets have largely worked in the northern hemisphere while those who prefer the marine-based, temperature-controlled model are Antarctic specialists. The former school sees the northern ice sheets as composites of initially independent ice domes that never form a single-ridged ice mass, in other words, a minimum-ice-volume model. The marine-based school visualizes much more extensive ice sheets, as discussed by T.J. Hughes at AMQUA and by G.H. Denton and Hughes in their recent volume *The Last Great Ice Sheets* (Wiley-Interscience, 1981), where inherently unstable marine-based ice sheets are buttressed by floating ice shelves. Such a model leads to a maximum-ice-volume estimate, equivalent to 160m sea level lowering at 18,000 BP. Andrews admits that there may be a good bit of truth in both models and that more field evidence is necessary to confirm or refute the underlying hypotheses.

What drives these models? J.D. Hays and J. Imbrie reviewed their recent conclusions on orbital perturbations ('Milankovitch mechanism') as the 'pacemaker' of glacial-interstadial-interglacial fluctuations with fundamental frequencies of 100,000, 41,000 and 23,000 years. They added, however, that the climate system may also include internal ('stochastic') variations that will occur even at times of no change in the external ('deterministic') forcing functions. The problem of the weak amplitude of the 100,000-year eccentricity cycle, which nevertheless dominates the geologic record over the last 700,000 years, was tentatively explained (N.G. Pisias) as a non-linear response of the climate system. Hays suggested that certain frequencies may be related to solar (rather than orbital) forcing, but M. Stuiver has looked at the record of solar variability (sunspots, radiocarbon) over the last 1000 years and finds no evidence of solar forcing over that period.

R.G. Johnson emphasized the role of the Gulf Stream as a feedback mechanism within the North Atlantic oceanic-ice sheet system, beginning with its initiation about 3.1 million years ago when the Straits of Panama were closed by uplift and continuing as a major means of heat and moisture transport to the high latitudes.

Having discussed the structure of an ice age as well as we understand it today, the meeting turned its attention to questions of mankind's adaptation to ice-age conditions and changing environments. A.J. Jelinek emphasized from the outset the slow pace of cultural change, largely devoid of recognizable variations at all. Only since ca. 40,000 BP is the archaeological record sufficient to evaluate man's adaptations to his environment in detail, and even here the role of climatic change (for example, in plant domestication) is generally played down by many archaeologists. Jelinek described cultural changes in the coastal Near East (et-Tabun cave, Israel) that do appear to correlate with environmental change. Here several types of late Lower Palaeolithic stone-tool industries alternate repeatedly and Jelinek suggested that the alternations may be related to climate and/or sea-level position as interpreted from the standard oxygen isotope curve for the period 120,000 to ca. 40,000 BP. Chronological control in archaeological sites for this period must be improved, however, before the correlation will be convincing.

As parting shots both I. Rouse and A.L. Bryan agreed that the cultural changes described by Jelinek for western Eurasia may have little bearing on the question of the adaptations that occurred in eastern Asia and led ultimately to man's migration into the New World.

Abstracts of papers by the principal speakers and discussants as well as those for numerous poster papers are available from the AMQUA treasurer, J.E. King, Illinois State Museum, Springfield, IL 62706, U.S.A. for $3.00 postpaid.

Geological Sciences,
University of Michigan,
Ann Arbor, MI 48109, U.S.A.

William R. Farrand