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SUMMARY OF FINAL DISCUSSION

by

W.S.B. Paterson

(Box 303, Heriot Bay, British Columbia V0P 1H0, Canada)

The symposium closed with the traditional final discussion under the chairmanship, in this instance, of John Nye. He defined its purpose as to identify the major themes and issues of the meeting and summarize any conclusions reached about them. The last objective was not achieved, probably because most participants were suffering from mental indigestion after some 60 papers presented orally, and another 100 in poster form, during the preceding four and a half days. Because the proceedings were not recorded, this summary of what, in my view, were the main points of discussion is based largely on my memory, which is unable to attribute some of the points made to specific persons. I have therefore omitted the names of all discussion contributors. Moreover, a few comments from discussion of individual papers have crept in.

Several papers, mainly modelling studies, had dealt with conditions for the initiation of ice sheets; others had discussed their decay, while yet others had treated variations in ice and climate over a single ice-age cycle. This discussion, however, was focused on the familiar 100 ka cycle of ice ages or, more strictly, of concentrations of oxygen-18 and calcium carbonate in ocean sediments. Although the eccentricity of the Earth's orbit also varies with a period of 100 ka, the resulting changes in the distribution of solar radiation on Earth are generally considered to be much too small to account for the switches between glacial and interglacial climates. P. Fong expressed the opposite opinion, based on his belief that the climate system is in a state of *neutral* equilibrium; however, most participants appeared to be skeptical about this idea. If the orbital and climatic cycles are related, the climate system must contain feedback mechanisms that amplify the orbital effects and, in particular, produce the rapid decay of ice sheets that characterizes the glacial cycle. Nye suggested three specific questions:

1. What started the 100 ka ice-age cycle?
2. How is it sustained?
3. How long will it last?

Some suggested answers to the first question were movement of land masses and uplift, either in regions such as Arctic Canada where ice sheets start to grow or the Tibetan plateau where high ground has a major effect on atmospheric circulation. Although these may well account for the formation of the Antarctic ice sheet, or the beginning of glaciation in the Northern Hemisphere about 2.5 Ma ago, they cannot explain the start of the 100 ka cycle some 0.7 Ma ago. Before that, world ice volume appears to have fluctuated with a period of 41 ka, corresponding to variations in the tilt of the Earth's axis, and an amplitude of only half that of the subsequent oscillations.

Several types of model can reproduce, perhaps even explain, the 100 ka cycle:

1. W.R. Peltier and his associates have developed a climate model that acts as a relaxation oscillator with a period of 100 ka when forced at a frequency of $1/20\,\mathrm{ka}^{-1}$, an approximation to the precessional frequencies of the Earth's orbit. The feedback mechanism is the delayed isostatic rebound of bedrock. As an ice sheet retreats into the depression it created, its surface elevation decreases and so the ablation rate increases. This increases the surface slope and therefore the ice discharge, so that the ice sheet continues to thin and retreat. New work, presented by G. Deblonde, showed that, contrary to previous results, this model could not produce sufficiently large oscillations at 100 ka. Another feedback loop, plausibly interpreted as the effect of variations of atmospheric CO_2 concentration during the ice-age cycle, has to be added.
2. B. Saltzman described his model in which CO_2 concentration is one of three control variables. The others are global ice volume and a measure of the thermal-biological-chemical state of the ocean. There are also seven adjustable parameters. With suitable choice of values, the system exhibits *free* oscillations with a period of 100 ka. The variations in CO_2 concentration over the past 150 ka, implied by the values chosen for the adjustable parameters, are consistent with those measured in the Vostok ice core. With this model, the Milankovitch variations are not even a necessary condition for ice ages. Saltzman also suggested that the 100 ka cycle began when the atmospheric CO_2 concentration fell below a critical threshold value.
3. R.M. MacKay presented a model, simpler than Saltzman's in that CO_2 concentration was not included. This can also exhibit free oscillations that reproduce the major features of the climate record of the last 250 to 500 ka. MacKay pointed out that each ice-age cycle does not last exactly 100 ka; the duration varies. This again emphasized the need for improved dating of ocean and ice cores, a point made by other speakers. Existing time-scales for ocean cores are "tuned" to the Milankovitch frequencies.
4. Some models of M. Ghil and his colleagues, merely alluded to at the symposium, can also produce free oscillations, but at a period of only 10 ka. The 100 ka cycle arises as one of several combination tones when the model is forced at the Milankovitch frequencies. Specifically, it is the difference frequency ($1/109\,\mathrm{ka}^{-1}$) between the two precessional frequencies ($1/19$, $1/23\,\mathrm{ka}^{-1}$). In these models, as the ice sheets grow, the Earth's albedo increases and so temperature decreases. The consequent growth of sea ice eventually starves the ice sheets of precipitation so that they start to retreat and the cycle is reversed. Some models also include delayed isostatic rebound.
5. In their paper, T.J. Crowley and G.R. North suggested that major climatic changes, such as the onset of glaciation in the Northern Hemisphere or the start of the 100 ka cycle, reflect instabilities in the climate system. These might be triggered by changes in boundary conditions such as CO_2 concentration. They further suggested that the 100 ka cycle might represent oscillations between two stable states and so could be a transient phenomenon.
6. W.F. Budd and P. Rayner assessed the importance of ice-albedo feedback with an energy-balance model. This study tells nothing about the causes of the 100 ka cycle, because the variations in ice extent in North America over the past 160 ka, derived from an earlier model of Budd and Smith (1981), were used as input. However, the results suggested that the increase in the Earth's albedo resulting from an ice sheet in the Northern Hemisphere was, by itself, sufficient to explain why glacial cycles in the two hemispheres are in phase, even though the long-term peaks in solar radiation at mid-latitudes are not.

To return to Nye's questions:

Question 1. The only plausible suggestion was an instability in the climate system, triggered when the atmospheric CO_2 concentration fell below some threshold value.

(The long-term trend is a decrease with time, with ice-age and other fluctuations superimposed.)

Question 2. No consensus emerged as to the merits of the different models or the relative importance of different feedback mechanisms. More data are needed, for example on the phase difference between variations in CO_2 concentration and in world ice volume. Some possible feedbacks were never mentioned. For example, phytoplankton in the oceans excrete dimethylsulphide and this escapes to the air to form a sulphate and methane sulphonate (MSA). Aerosol particles of this sulphate form most of the cloud-condensation nuclei in the marine atmosphere (Charlson and others, 1987). Two papers presented at the symposium showed increased concentrations of MSA in Antarctic ice cores from the last glaciation. This suggests the possibility of an interesting biological feedback.

Question 3. Nobody was bold enough to try to answer this one.

Discussion then turned to general questions about modelling. This was appropriate because the symposium brought together two groups that normally work apart; the modellers and the practitioners, to use K. Hutter's expression. Some disciplines, however, were not well represented. There was little or no discussion of cloud physics, for example. This was unfortunate, not only because the representation of clouds is a problem in atmospheric general circulation models (GCMs), but also because the chemistry of ice cores depends on, among other things, poorly-understood processes in clouds.

One striking feature of the meeting was the increase in sophistication of ice and climate models since the symposium on that subject at Evanston in 1983. (see *Annals of Glaciology*, 5.) Two different approaches were apparent. The "minimalist school" try to find the minimum number of variables and fudge factors needed to explain the observations. The other approach was characterized by the remark: "If the physics is good, put it in the model". The chairman posed the question: "If your model simulates a given real situation, do you then understand it?" Some reluctance to give a direct answer was apparent. However, K.H. Cook had described a simple model she had used to study the behaviour and effect of the atmospheric stationary waves that, according to some GCM studies, were generated by the Laurentide ice sheet. This appears to imply that, at least for the case of GCMs, the answer is "no".

The provocative statement, that those who work with GCMs have already used all available data, immediately produced counter examples. For instance, although the existence of leads in sea ice, even in winter, is well-known, the paper by I. Simmonds and W.F. Budd was the first to attempt to incorporate the heat flux from leads into a GCM; as a result, the representation of the south-polar trough was improved. The need to include this heat flux acquired new importance from the observations of E.L. Andreas and others. They found that this flux was not invariably trapped in the near-surface inversion layer as previously believed. If the lead is very wide (a few km) the plume of water droplets and ice crystals, and thus also the heat flux, can reach altitudes of 4 km and distances of 200 km downwind. Again, D. Rind, using a GCM, concluded that Milankovitch variations could not start the growth of an ice sheet in Arctic Canada, contrary to the findings of the modelling study of Budd and Smith referred to above. However, surface elevations on Baffin Island in Rind's model are unrealistically low (less than 500 m) as a result of the wide grid spacing. Moreover, ablation is equated with melting; this ignores the refreezing of surface melt water in cold firn. As a result of this process, the heat required to remove a polar snow cover is significantly greater than that needed to melt it once. The paper by S.G. Warren and S. Frankenstein provided an example of conflict between GCM predictions and field data: a prediction that a warming of 3 K in Antarctica would increase precipitation by 12% compared with the figure of 30% obtained from analysis of the Dome C ice core. This discrepancy has obvious implications for predictions of how greenhouse warming may change the size of the ice sheet.

Pressure to develop more sophisticated models arises from requests for more detailed predictions, among other reasons. Testing such models will require additional data. One participant expressed the view that the quality of data is in fact deteriorating, not only because of the termination of long-term monitoring programs for lack of money, but also because some long-standing "facts" are now being questioned. For example, a recent fresh look at some ice-core data suggests that, in Greenland, the southern dome and perhaps even the whole ice sheet may have disappeared in the last interglacial (Koerner, 1989).

A fundamental question about the value of GCMs was also raised. The output from these models is what we call "weather". It is essentially unpredictable beyond a few days because the results are so sensitive to the initial conditions. If, however, we average weather over time we get "climate" and this may be predictable in a statistical sense. At present, climate predictions are expressed in terms of changes in quantities such as mean temperature, wind speed and direction, and precipitation. But is this climate? And how can a model that cannot reproduce the behaviour of real storms predict climate? Two different views came to light: (1) Future storms will be similar to past ones, except that their frequency, strength, tracks, and duration may change. This should be statistically predictable once the models are able to get the present behaviour statistics correct. (2) Future storms may have a character completely different from that of present ones and so they may be unpredictable until the models can foresee what a future chaos will be like.

The onset of chaos ends my summary.

I thank David Fisher and Uwe Radok for refreshing my memory about certain aspects of the discussion.

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