

GENERAL DISCUSSION

The final session of the symposium included a general discussion under the chairmanship of Professor J. F. Nye.

J. F. NYE: We have this time allocated to a general discussion and you should feel free to discuss any topic which has arisen from the symposium. Perhaps we might ask ourselves: "What are the unsolved outstanding problems and what are the best ways to solve them?" To keep the discussion focused we could divide it into three rough categories: polar glaciers and ice sheets, temperate ice, and surging. Dr Robin's paper was the first of the symposium and there was not any discussion of its implications. Is there still a problem of reconciliation of past temperatures as deduced from oxygen isotopes and past temperatures as deduced from the present temperature measurements?

G. DE Q. ROBIN: On the time scale of decades, in terms of looking at the Greenland variations and relations with respect to temperature-depth profiles, we clearly have a moderately close relationship between temperature variation and time. I think the "Byrd" bore-hole results which Dr Budd presented show pretty conclusively that there is not a bad relationship between the surface temperature and the past isotopic values on the scale of centuries. So I do not think there is a problem there at the moment. It is clear that the major changes that we see in the isotopic profiles, provided we average over long enough time periods, are primarily climatic fluctuations, and it is a question of how accurately we can tie the two together. I think the bigger problem comes on the longer time scale when we want to apply our results back into the Wisconsin and still earlier where there were major changes of circulation which mean that we cannot rely so much on the isotopically derived temperatures. At the Cambridge Workshop, when we reviewed the broad field before the Vostok results were available, we came to the conclusion that there was something like 5% variation in δ common to almost all our bore-hole ice-core results which we felt fairly certain went back into the Wisconsin and was due to climatic changes. In addition to that there were changes in size of ice sheets and effects of flow which varied with location and could be estimated approximately. Since the Workshop, the results from Vostok have been published and they also show 5% change. Vostok is one location where we expect the change in surface elevation or flow pattern to be very small with time, so we can look on climate, and especially temperature, as the main variable. So for this combination of reasons we can say that, provided we go to the right location, we have a very strong dependence of isotopically derived temperatures on the actual climatic changes.

NYE: By climatic changes you mean temperature rather than accumulation?

ROBIN: Yes. Isotopic changes are reflecting temperature changes.

W. S. B. PATERSON: Why would you expect the temperature change at the end of the Wisconsin in Antarctica to be the same as in Greenland?

ROBIN: It is an empirical observation. They all look to be the same.

PATERSON: Well, Century is not the same and the Barnes Ice Cap is not the same. Century is around 11%.

ROBIN: I am talking about the change after you have eliminated local effects such as motion of the ice, change of elevation and of flow pattern.

PATERSON: You have to watch for a circular argument. You assume that 5% represents the global temperature change during the Wisconsin; then, because the change at Century is 11%, you say that 6% is due to elevation change, and then this is quoted as evidence for an elevation change.

ROBIN: It is not quite circular. The 4–5‰ number was arrived at before the Vostok results became available and the Vostok results support the hypothesis. In addition there is independent evidence such as total gas content.

PATERSON: I think that is as yet a bit unsound.

NYE: Is there any other evidence for elevation change besides the atmospheric gas content?

ROBIN: There is also the comparison of the δ values between Century and Devon Island.

PATERSON: Yes. I have talked to meteorologists and they claim that the temperature change at Devon and Century could well be completely different. During the Wisconsin, Century was miles away from any open water and Devon probably not much farther than it is now.

W. F. BUDD: I am still not convinced that all the isotopic variation could not be through elevation change alone; the problems of surging of both Greenland and Antarctica could be associated with this. There is hope for gas content as a measure of elevation change and there is good evidence in Greenland that there has been a substantial elevation change, perhaps at "Byrd" as well. Our own results from Cape Folger also have low gas in the lower part. Gas content will give useful information when we have the technology to prevent gas loss from our cores. Until then I think we should be quite open-minded and try to eliminate one variable at a time to distinguish between temperature variation and elevation change. It seems they are somewhat synchronous in time when you say 15 000 \pm 5 000 years ago.

PATERSON: Again on this point, from the isotopes the elevation change at Century is 500–600 m while for the gas content it is 1 300 m. That is not terribly good agreement!

ROBIN: I agree. What Dr Budd says applies on the long time scale, but certainly on the short time scale you could not have the ice sheet pumping up and down and changing in elevation the way the isotopic values change over the last few centuries.

BUDD: No. I think over the last ten thousand years it seems to be fairly constant.

PATERSON: There is a trend in the Devon and Century results.

BUDD: No. I think the Century line showed a fairly constant climate-by-isotopes effect with a very slightly warmer origin around 6 000–7 000 years ago. To my mind there should have been a warming associated with the motion down slope and this would depend on the speed of Camp Century, which is not known that well. If it were 10 m a⁻¹, which it may be, then it would be substantial and would mean that there is some other cooling going on. If it is a cooling associated with an elevation rise, it would need to be approximately 3 cm a⁻¹. For an accumulation rate of 35 cm a⁻¹ that is a very slight imbalance, so I think these numbers are getting sufficiently close that we have to be very cautious about saying one thing or the other.

NYE: We started off thinking oxygen isotopes were going to give us a record of temperature and now we are seeing that they tell us about the history of the ice sheets as well.

BUDD: Yes. I think the only way we can get over that is by having sufficiently many bore holes along the flow line that we can actually look at the history of the ice sheet along the line of flow and perhaps remove some of the variables. At the moment I think there are too many possibilities to be very certain about the past history. This is important when people outside our field use our results rather incautiously to infer similarities with what they find about past climate.

L. LLIBOUTRY: What has been done to establish a relation between the temperature and the isotope variations? There is an isotopic equilibrium between the atmosphere and the surface of the snow. What do we know about this problem?

ROBIN: The main data come from comparing 10–15 m ice temperatures over the Antarctic, which may represent 1–10 year mean temperatures with the isotopic composition of 50 year

mean samples of the ice. If you go year by year, the fluctuations are too big to get a very close relationship.

LLIBOUTRY: Dansgaard finds seasonal fluctuations in isotopic content. These come from the surface or from the atmosphere. If they are from the atmosphere, Raynaud's method would not work, so they must be from the temperature at the very surface.

ROBIN: This is an empirical relationship.

LLIBOUTRY: Do you know of any theoretical or experimental work since Dansgaard's? Is there nothing more accurate that has been done? For instance, suppose one has an atmosphere with a given isotopic composition, what is the isotopic composition of the snow which falls? If next you put this snow in contact with an atmosphere of a different isotopic composition, is there an exchange? A lot of experiments can be done.

ROBIN: Some work is being done by Dansgaard and others to examine long-term integrated effects but not the mechanism itself. There have also been some field observations taken in the past. Picciotto and others tried to relate the isotopic ratios for individual snowfalls with the prevailing atmospheric conditions around the edge of Antarctica. This is relevant to the particular problem but I know of no laboratory studies on this scale.

M. M. MILLER: At the symposium on Quaternary Environments at York University last spring this point was brought up with respect to palaeontological evidence: the warm-wet and warm-dry plant types of the Holocene. The work our group has undertaken for a number of years in the interior of northern British Columbia, the Yukon, and Alaska indicates that over the past 10 000 years not only the tropopause has changed, but storm paths have shifted. Within 150 miles (240 km) across the Alaskan/Canadian sector of the Cordillera, we find over the past 10 000 years—working from the present backwards—the following progression: In the interior of northern British Columbia we go from cool-dry in the interior to cool-wet conditions on the coast; then back from warmer-wetter in the interior to warm-dry conditions on the coast; next, around the thermal maximum, we go from warm-wet conditions in the Atlin region to cool-moist on the coast. This is based on palaeontological evidence. Then we have cool-dry inland and warm-dry on the coast, and finally cool-dry and cool-wet. In other words, as storm tracks shift, you can have a complete change in the regional temperatures and precipitation. Maybe the Antarctic is sufficiently stable climatologically that this is not a problem, but I suspect that oxygen-isotope measurements in other areas, the Greenland ice sheet for example, might be subject to this mechanism.

ROBIN: Yes. A point I tried to make in my talk is that the long, gradual slopes of polar ice sheets are the most favourable areas to look for the gradual changes because you expect circulation changes to be less than in other parts of the world. This is the reason for doing the work on polar ice sheets. To sum up, the fit of Dr Budd's isotopic values at "Byrd" station in the upper part of that core are the best fit so far because they are very little influenced by geothermal heat and temperature changes in regions remote from the ice sheet. They are largely the product of conditions in the upper half of the ice sheet: the motion and surface climate. This is the best test and gives the best fit between mass balance and isotopic temperatures. It is a better test than Camp Century which is still influenced by some geothermal and frictional heat fluxes reaching the surface. "Byrd" is a superior location because it was not greatly influenced by possible changes in height of the ice sheet.

BUDD: I think the Greenland fit is good, too, in so far as the input temperatures from the isotopes give a good fit to the temperature profile. In both cases I think the problem is whether that temperature is associated with the elevation change or the ice-sheet change. As far as whether the isotopes are really telling us about temperature or not, I think this is true but there may be some problem with the factor we are using, whether it is $0.7\delta/\text{deg}$ or

something else. I think this calls for good, long-term calibration using the past records where we have isotopic data of that kind.

T. J. HUGHES: The "Byrd" station record may not be as stable in terms of ice-sheet elevation as you might think. There is a growing amount of glacial-geological evidence that the Antarctic Ice Sheet, particularly the west Antarctic Ice Sheet, was grounded very close to the edge of the continental shelf 14 000 years ago. Last season the Norwegians found ice-cored moraines in the Ellsworth Mountains at the rear end of the Filchner-Ronne Ice Shelf that are 1 700 m above the ice shelf. John Mercer from Ohio State has found ice-cored moraines in the Reedy Glacier area at the extreme south end of the Ross Ice Shelf and also at the lower end of the Beardmore Glacier that are 1 000 m above the Ross Ice Shelf. George Denton and his co-workers at the University of Maine have found ice-cored moraines and raised beaches all along the Trans-Antarctic Mountains from the McMurdo Sound area and northward clear up to Terra Nova Bay. So there is getting to be a great amount of evidence that the western Antarctic ice sheet was very much thicker in the recent past. As a matter of fact the Maine group's conclusion is that 6 000 years ago it was grounded near where the calving barrier of the Ross Ice Shelf is now. This implies that in the Holocene quite rapid changes in ice-sheet elevation occurred.

ROBIN: Even if the elevation has changed quite a lot in recent centuries, the fit of isotopically derived temperatures with the upper part of the observed temperature profile observed indicates there is still good correlation.

HUGHES: Yes, but only in the upper parts.

ROBIN: I am talking about the past few centuries. We are looking for evidence tying the two pieces of data together as distinct from evidence for past changes in elevation, which is a separate problem.

LLIBOURTRY: In the work of Raynaud about gas content, there is an assumption that the close-off always occurs at the same density. There is some work suggesting that the ice crystals were not the same during the Holocene and during the Wisconsin glaciation. Has Dr Paterson found this?

PATERSON: Yes. In our Devon core, the grain size is much smaller for the Wisconsin than for the Holocene suggesting that the close-off of the gas bubbles might have occurred at a different density. It would only take a 2% change in density to account for the whole effect observed at Camp Century.

BUDD: The principle does not depend on knowing what this density or resulting gas volume is. It depends on having a reliable geographical distribution of gas volumes from the surface of the ice sheet being studied that shows a reliable relationship between that gas volume and the site elevation. We have a number of bore holes on the Law Dome which, between the 150 and 1 400 m levels, show a fairly clear relationship between elevation and the gas volume, although there is a slight variation in density.

NYE: Computer modelling has been an important topic at the symposium. I was impressed with Ms Mahaffy's computer model results showing the growth of the ice cap on Baffin. In the work of the Australian group the temperature distribution with depth is very carefully modelled, while in Ms Mahaffy's work temperature is dealt with by simply taking an average value for parameters in the flow law. If one is doing computer modelling simply with the object of understanding when and why ice sheets formed and why they are a given size, how important is it to be really careful about the temperature distribution with depth and to what extent can one legitimately eliminate it by averaging?

LLIBOURTRY: Suppose an accumulation area turns into an ablation area; we have a completely distinct temperature profile and this completely changes the value of n . So I think that this

old method of ignoring the temperature is incorrect although it may give very nice displays. It is good in a qualitative way but for quantitative results we must make a calculation as the Australians do. It is also important to allow for a variable strain-rate with depth because if a constant uniform strain-rate is assumed between the surface and the bottom, the temperature profile changes completely. I have received an abstract of Weertman's communication to Grenoble which is on this subject. Bore-hole measurements of the vertical strain-rate are very useful. We do this in glaciers by placing metal markers at different levels and later detecting them with an electromagnetic sensor. I do not know if this is feasible for ice caps.

NYE: But, if we are going to extend that kind of work to understanding the build-up of the Wisconsin ice sheet, or past ice sheets which are no longer with us, we cannot go about measuring strain-rate in bore holes.

LLIBOURTRY: What is important in the problem of the Wisconsin is the sea-level and the problem of the general circulation of the atmosphere. If we make computer models we must simultaneously consider the atmosphere, the oceans, and the ice cap; this would be useful. In this case we could make a very rough approximation for the flow law of ice but must at least take into account the changes in atmospheric circulation.

NYE: But just as a beginning we cannot do everything at once. Would it not be reasonable to do this averaging of the temperature over depth and then deduce flow lines, and compare these with geomorphological evidence of flow lines? The geomorphological evidence may have been made in a retreat stage, so the computer model could be run through the retreat stage to generate flow lines. I realize to take everything in is desirable, but do you not think this is a sensible approach?

BUDD: I think the approach you just described is really the way to do it. The work of Ms Mahaffy is a very good start because the sort of information being sought is very dependent on the input which, in her case, is accumulation rate. The errors you get from that are far larger than one would expect from problems with the flow law. I see little point in modelling the temperature profile in a sophisticated manner when everything depends on the input surface temperature. Who can specify that well enough to give us the right answer? A first attempt of this kind should be done the simplest way. First of all build up the ice cap using the simplest possible input relations, then when you get the feel of how it works, look at temperature distributions. With each step you get closer to reality and eventually introduce things like surges of the ice sheet.

NYE: There was a programme on B.B.C. television some months ago and a book written by Nigel Calder (1974) laying stress on the idea of a snow blizzard. This terrible time comes when it snows and the snow remains throughout the summer and on to the next winter, starting an ice age. I take it this sort of thing could be tested by this sort of computer modelling that Dr Budd is suggesting.

J. W. GLEN: I imagine it could be tested, but I am not sure how valuable that would be until you put in some kind of probability of the equilibrium line in Britain suddenly falling to sea-level. In fact the climate of Britain, as Calder says, is now milder than it was during the "little ice age" and the most likely change, as Calder again says, is for us to return to the kind of climate we had until the middle of the last century. Before running a computer model of the build-up of an ice sheet, it would be necessary to assume where and to what extent the climate has deteriorated to the stage that the equilibrium line has fallen to sea-level. If we were to do a calculation based on that assumption, we probably could see how fast the ice sheet could build up, but it seems most unrealistic to me.

LLIBOURTRY: The problem is very complex. The glaciology is inseparable from the meteorology and oceanography. There are many feedbacks and interactions.

GLEN: I doubt it would be very sensible to run that programme for western Europe. We must ask whether it is sensible to do it for northern Canada where I imagine the equilibrium line is nearer sea-level anyway. Can we now say that we think the build-up is likely to come out of the mountains in Baffin Island and in Labrador as Ms Mahaffy assumes or is it likely to start elsewhere in the Canadian North at lower altitudes?

F. MÜLLER: For this reason I commented after Ms Mahaffy's paper that back-coupling effects must be carefully taken into account or you will get the wrong picture. We must attempt to keep our models free of preconceived ideas, for example that continental glaciation must start over Baffin and in Ungava, because the truth may be quite different. A telling experience for me personally was to fly over the Canadian Arctic in 1964, when the snow line on White Glacier on Axel Heiberg Island remained at 400 m above sea-level. During that summer large portions of the Canadian Arctic Archipelago, i.e. areas as big as western Europe that are usually snow-free, remained snow-covered. The snow patches remained for the next year although 1965 was a budget year which should have produced an average situation on Arctic glaciers. On Axel Heiberg Island, however, it was a positive budget year because of the backlog of firn. In 1965 the radiation component of the heat budget was very much reduced. Ten summers like 1964 and 1965 may create the broad base for a large ice sheet covering not just Baffin Island and Ungava but mushrooming also over large proportions of northern North America.

C. J. STERLING: The B.B.C. film suggested that most of the Great Plains, that corridor coming down the middle of Saskatchewan and into North and South Dakota and Nebraska, was the area of this snow blizzard that, over 100 years, could lead to the formation of an ice sheet. Why was that particular area suggested instead of Ungava and Baffin Island?

NYE: It sounds to me that you saw a different version of the film from the one I saw. In the one I saw it was starting to the south of Ireland.

MILLER: Our field measurements reveal that the firn line has risen and fallen between 750 m and 1 250 m along the Alaskan coast in something around 30 years, there is that much latitude in the vertical displacement of the zone of maximum snowfall. In addition, storm-track shift can bring very heavy snowfall to a region which formerly did not have as much. This could be the basis for the concept of the Russian climatologist M. I. Budyko that increased snow cover and polar sea-ice cover increases albedo, which tends to have a chilling effect, and may be a factor in the onslaught of global glacial conditions. I think that the B.B.C. film and the report which preceded it got very confused press in the United States and Canada. The basis of the film was that there is glacial geological evidence that an ice tongue came from the south in the English Channel. The main point is that we could be back in full scale ice age within 200–300 years. I presume the figure of 300 years comes from the recent estimates of Murray Mitchell, a research climatologist with the U.S. National Weather Service. He has said that if the present atmospheric pollution trends continue, CO₂ in particular, the greenhouse effect can change to an albedo effect, and this would lower freezing levels. These are rather speculative ideas but if atmospheric pollution is actually proven to be a factor in climatic change, it probably could happen.

NYE: Could I turn the discussion once again? We have seen extremely detailed and very careful work on temperate glaciers, measuring temperatures to 0.01 deg or even 0.001 deg. When we think of the scientific object of the work on the polar ice sheets we say it is to understand past climate and we all agree that that is a great goal. When we come to this very detailed work on temperate glaciers I ask myself what is the real purpose of this work. It is an interesting academic exercise; there are some extremely interesting scientific problems, but has it got the same importance as the other work? I am intentionally being a bit provocative. I wonder how accurately Dr Raymond would like to know the temperature around a

bubble in a temperate piece of ice and when he would be satisfied in his understanding of the detailed thermodynamics of these bubbles and inclusions?

C. F. RAYMOND: I can comment on the general nature of the question, though perhaps not on bubbles specifically. One of the major problems is glacier sliding. Dr Robin discussed how rather small temperature effects, in the range of 0.01 deg, are probably important at the bottom of a glacier. Certainly when one considers these effects one needs to understand the thermal behaviour of the ice. One cannot neglect possibly important consequences from the liquid and gas phases. Another aspect is the problem of water motion in glaciers and its effect on sliding. How water arrives at the glacier bottom is probably a major consideration. When Dr Nye newly introduced the idea of permeability through a vein network, this opened up the possibility that considerable amounts of water might reach the bed homogeneously rather than at isolated points through moulin or other conduits. The behaviour of the liquid phase in the ice depends on very small temperature differences, and one's conclusions might easily be upset by neglecting effects of salts or gas.

LIBOUTRY: In our work with temperate glaciers in the Alps we looked for some kind of stratigraphy to examine problems like the fact that the vertical strain is not uniform in glacier dynamics. We were not interested in temperature, it was a question of having a new method to distinguish one temperate ice from another temperate ice by measuring water content as well as grain size. I gave this as a thesis subject to Dupuy and he made more careful experiments and long calculations than field work. Later I published a paper on the temperature of ice in temperate glaciers. Our first aim was to understand stratigraphy but we were faced with the problem of what controls the variation in water content. At first we supposed that there was a seasonal variation of rime deposition at the glacier surface which caused a seasonal variation in salt content and that the water content was related to this. Now it appears that the water content is related to the grain size and the smaller the grain size the larger the water content. One explanation, supported by the work of Duval, may be that there is a concentration of the strain-rate within layers of high water content.

GLEN: I was in fact going to make the point that Professor Lliboutry has just made. One reason why we would want to know this is that although laboratory experiments can tell us quite a bit about the stress-strain-rate relations in ice a little below the melting point, there is every indication that they have been rather unsuccessful in telling us this as we get up to the melting point. I think Dr Budd brought that out in slides he showed where he indicated that in order to get good agreement he could assume laboratory results until he got near the melting point but there he had to choose something a little different. There are also the results of Duval. Maybe we have a very small temperature region within which, nevertheless, there are quite large changes in mechanical properties which have quite appreciable effects on glacier dynamics. If we have got quite large amounts of water involved in this way, it may have something to do with glacier hydrology. There is another side of glacier hydrology which was not mentioned when we were talking about surging glaciers but which is related to them and that is the extent to which surging glaciers are related to major water effects. I am thinking particularly of the fact that Lednik Kolka, which was quoted as one of those which gave evidence for regular surging, has been associated on most of its surges with a quite vast amount of water coming out. Where does it come from?

NYE: I shall put the question directly to Dr Hodge. At the Cambridge Symposium on the Hydrology of Glaciers the term "water table" was popular, and some of us, I was one, had the feeling that there really was no such thing as a water table in a glacier. It was something which depended on the size of the hole you used to explore it with. If you used a bore hole you would get one value, if you used a hole which was only the size of a vein between three ice crystals you would get another value, if you used a moulin you would get yet another value. What

is the present status of the "water table" in a glacier, and what is the physical interpretation of the surface which in your paper lies at about 66% of the glacier thickness?

S. M. HODGE: When I first started this work I referred to it as a "water table", but have now ceased to call it that because I no longer think of it as a water table in the same sense that there is a water table in the firn layer. As for the physical interpretation of the surface, it is the level of water in 5 cm diameter bore holes which connect with the basal water system. I believe it represents the potentiometric surface of the water at the bed.

LLIBOURRY: It is the piezometric surface. I suppose that there may be both isolated cavities and interconnected cavities and there may be some individual cases where all the cavities are interconnected. In this last case there is a piezometric level. According to some experiments by Gillet in Glacier de Saint-Sorlin by about mid-June all the cavities interconnect because there is much water. At that moment one has a piezometric surface and the pressure can be defined; before this there are different values at points which are very near. Of course, Röhlisberger's measurements cannot give this because he puts so much water in his holes that he makes an artificial moulin and forms waterways which link the bottom of his hole with the vein network. He cannot, therefore, measure pressure at the bottom of one hole. Pressure has been measured by Gillet, by inserting a pressure gauge and then closing the hole with a core of cold ice, but there are many problems. First, because all this instrumentation can move within the ice owing to regelation, and next because the measurement is not at a fixed point but one that is sliding with time. We found a pressure which varies with time, but cannot say if we are measuring, for instance, the pressure in the water film on the up-stream side of an obstacle and next in a cavity behind the obstacle where there is another pressure. This is one possible explanation, but perhaps we are measuring the variations of pressure in the waterway. We cannot say.

W. D. HARRISON: My comment relates to temperature measurement and water circulation. One justification for going to the trouble of making very accurate temperature measurements is that if they are done over a period of time, they tell you something about the circulation. For example, in Blue Glacier the negative temperature of -0.02°C near the surface does not seem to change through the summer. If water were flowing rapidly through the veins then the temperature would be much closer to 0°C . We can make these measurements in temperate ice and can describe situations as they are in the glacier, but we have not really begun to say why they are this way.

NYE: Though we happen to be glaciologists we should not forget that ice is only one material and what we are learning about ice here is far ahead of what is known about any other material so close to its melting point. One looks to the day when these things will be thought about in detail with other solids.

C. L. HRONEK: I would like to point out that there are groups of amateur speleologists who would be delighted to assist with glaciological research. In addition to carrying instruments into caves near glacier margins they could help with the exploration of moulin and studies of internal drainage channels. Compared to glaciology, speleology is a young science but I can see tremendous benefits in a marriage of the two. The more people you have working on a problem, the better the overall picture you can get.

HODGE: May I add that "water table", as defined by the U.S. Geological Survey, is simply the level one finds in wells which just penetrate a porous medium sufficiently to give standing water. It is more correct, I believe, to refer to the bore-hole water levels as defining a "potentiometric surface". Such a surface must refer to another particular surface in the medium. Thus one would refer to the surface defined by water levels in holes which connect to the glacier bed as the potentiometric surface of the bed. The second point I would like to

make is in regard to whether you would get a different level for a different diameter hole. I am going to try to investigate this in the South Cascade Glacier. I am considering pouring water down a bore hole as Röthlisberger has done.

GLEN: The whole idea of the water table really only makes sense if the water system is relatively static, surely? If the glacier is basically draining its water out rather fast in time, then putting a bore hole in will change the ability of the water to move about and will itself be a major influence on the height of water. I do not know what the hydrologists do if they have an aquifer which is being drained rather fast. Clearly there are problems. If you have a porous medium with fluid in it and underneath you have some rather large cavities, down which the water is going, the standing height in those cavities will not be the same as the level to which water rises in the porous medium above because of the viscosity.

M. F. MEIER: We are really talking about two coupled but separate systems. One is the motion of water through the bulk of the glacier. Whether it is porous-medium flow through veins or small conduits or whether it is a karst flow through large conduits, we do not know. This system can be tapped with a bore hole. In Dr Hodge's talk he mentioned that in the firn there is a water table that stands quite close to the glacier surface and does not vary much with time. Obviously this firn water table is not the water table of the bed or else the glacier would float. The other system is the one at the bed, which is only indirectly connected to the in-flow. Dr Hodge has shown that there is good evidence that the water level in holes which communicate with this bottom system acts as a large manometer and measures the head of this system. But it is a separate system from what you measure with the porous-medium approach.

HODGE: The surface defined by the level of water in holes which connect with the basal water is called the potentiometric surface. To get a water table you just dig down just far enough to get standing water.

NYE: It seems to me that Dr Meier has described essentially a two-slab system. One slab at the top is porous on a very fine scale and has its own water table. Then there is another system in which the connecting conduits are an order of magnitude larger. It seems to me what you have done here is to break down a system which actually has a continuous spectrum of capillarity size quite artificially into a two-slab system.

MEIER: The two slabs are defined by the water level in bore holes. For the first 99.99% of the drilling there is one water level which behaves in a certain way. In the last centimetre of the drilling, just before the bed, the water level drops by tens of metres and behaves completely differently. I think this is pretty good evidence that the drill has broken out of one system and into another.

NYE: At the same time Dr Hodge proposes to drill holes of different sizes to see what happens. Is this just a precaution to make sure that this theory is right?

HODGE: I am also going to plug one of the holes and cut off the supply of water from the surface and see what effect that has.

LLIBOURTRY: [Professor Lliboutry showed two slides, not reproduced here.] At the ice-bedrock interface there are two very different extreme cases. In the normal case there is a diurnal fluctuation in the waterway. The second case is when there has been a great variation in the pressure within the waterway. For instance, at the beginning of the ablation season there is much water and the water surface rises and may even reach the glacier surface. At the end of the ablation season it is the reverse. All this has nothing to do, of course, with the water in veins within the ice which perhaps do not communicate. In my opinion there is not a communicating vein network and ice is not a porous medium. In spite of this the water moves within the ice because there is constant recrystallization and so there is a water flux but you cannot speak in terms of Darcy's law for a porous medium.

NYE: Have you got more information about this movement of water by recrystallization, because for me this is a new idea?

LLIBOURTRY: No. It is only an idea, although there are some isotopic studies by Oeschger at the front of the Gornergletscher which support it.

GLEN: It seems to me that there are really basically three systems. There is the one we were talking about first, which I think is responsible for the level which Dr Hodge gets when he drills through firn. This, I would have thought, has some connection with the permeable medium and Darcy's law. The very fact that the level is steady during the drilling seems to suggest that water can flow in and out in order to maintain that level. This is probably associated with the wet firn height and is a perfectly reasonable and probably Darcian water system. Secondly, there is the thing which he gets down to afterwards, and which is represented by Professor Llibourtry's slides. Thirdly, there is the remaining water which I believe is probably in some cases quite considerable. At the Moscow meeting the South Cascade Glacier results were shown to be quite inconsistent unless we assumed there was some great body of water, perhaps high up in the glacier at certain times, which could find its way out if it were connected with moulin. Dr Clarke's sealed-off crevasses may be an extreme case of this.

MEIER: First, I'm glad you brought that up because we do have this problem of isolated water bodies which tend to be quite appreciable. In the South Cascade Glacier our calculations show that something like 1 m of water was lost from storage as the season progressed from May to November. With regard to Lednik Kolka, there are problems there of a local nature due to thermal hot springs. We were rather excited when Åke Fleetwood from Sweden measured ionic conductivity of water of Lednik Kolka right at the surface and found extremely high conductivities. Apparently these stem from thermal springs high in the basin of Lednik Kolka and these may have something to do with the large floods that have been observed there.

GLEN: The floods are associated with the surges, are they not?

MEIER: Yes. Also in Iceland the surges of Vatnajökull and other ice caps normally are accompanied by a flood.

HODGE: On Nisqually Glacier in a normal outburst flood the magnitude of the water released is again of the order of 1 m of water.

MEIER: There is radio-echo-sounding evidence, too. Scattering due to water-filled cavities is evidence of comparatively large amounts of water.

NYE: Professor Frank pointed out to me that the Icelandic word for glacier outburst, *jökulhláup*, does not really mean that, it means that the ice has leapt forward. Does it reflect some folk memory of surges being connected with water outbursts?

K. PHILBERTH: With regard to the problem of temperature measurements, I feel that, for temperate and quasi-temperate glaciers, temperature is not always a nice parameter for calculating mechanical properties. Moreover, it is difficult to measure temperature to 0.001 deg accuracy. Perhaps one should look for parameters which are related to the water content and which can be measured *in situ*. Dielectric constant is a better parameter than resistivity because resistivity depends on salt and on surface conditions of the probe. Has work been done on the relationship between the mechanical properties of the ice and the complex dielectric constant?

GLEN: I think the simple answer to the question is "No". It might tell you something, but interpretation will be difficult because it will depend crucially on the shape of the water bodies which we are talking about. You get quite a different answer for dielectric constant of a mixture according to whether you have spheres or tubes and so on, and that is one of the

things that we generally do not know. If we did know it, perhaps in some other way, I think dielectric constant could be quite a useful way of controlling temperature by seeing whether, in the course of an experiment, water bubbles were growing appreciably. In other words, that things were not as isothermal or "isohydric" as we hoped.

LLIBOURRY: The freezing content could be measured *in situ* in the same way as the thermal conductivity is measured *in situ* by measuring that rate at which a hot or cold sphere placed in the ice loses heat.

MEIER: I would tend to agree with Dr Philberth and disagree with Dr Glen on the question of using dielectric properties, because Dr Philberth mentioned the *complex* dielectric constant. If you know the loss tangent of the water containing solutes within a glacier, or if you can know this to a reasonable degree of approximation, then you can use a technique such as microwave absorption to measure very small amounts of water quite accurately. We have shown that this is extremely useful in snow and it ought to be just as good in ice.

RAYMOND: If the measurement of the water content is very sensitive to the water salinity, then one should measure the temperature, too.

MÜLLER: I would like to make reference to an intermediate class of glaciers lying between temperate glaciers and large sheets of clearly cold ice. I am speaking of the valley glaciers associated with the big ice caps and the many mountainous regions—the Alps, Himalaya, and so on—where the ice is partly cold and partly temperate. The study of these glaciers has certain advantages. Take, for example, the question of the various water tables mentioned earlier, on White Glacier on Axel Heiberg Island, where the glacier consists of cold ice with pressure melting-point temperatures at the base, one of the three "water tables" is already eliminated, and the one that is linked to the bottom can be studied separately. For this reason water-table fluctuations in moulin were measured on White Glacier and it was found that these fluctuations are related to the surface velocity fluctuations. The influence of different accumulation patterns and thus of melt water on the thermal regime of glaciers can be studied more easily on this glacial type.

HARRISON: There is a common misunderstanding that temperatures cannot be measured to 0.001 deg accuracy. Under certain conditions, this is fairly easy, and the main condition is met in temperate glacier ice; that the temperature is close to 0°C so that calibration standards at nearby temperatures are available. Field calibration is easy in a triple-point cell, or to somewhat lower accuracy in an ice bath. Of all the things we would like to know about temperate ice, temperature is one of the most straightforward to measure.

ROBIN: This means that very accurate temperature measurements close to the bed of a glacier in relation to pressure melting point should not be a very difficult thing, particularly when one is looking for 0.1 deg accuracy.

NYE: I have found this discussion very instructive, and I hope it has been as useful to all of you as it has been to me. I thank all of the contributors to it and now call on Dr Robin to say a word about the organization.

ROBIN: I am very happy to say thank you to everyone who is responsible for this conference, to the Canadian National Research Council, particularly their glaciological advisory groups who set up this conference and helped us get here. For all of those who have had that help, I would like particularly to say thank you. An earlier symposium under the sponsorship of the same group, the National Research Council Symposium on the Causes and Mechanics of Glacier Surging, focused interest on that subject, and in the years that followed we have seen the effects of that carrying on for some time. The group here have done the same job again. They have focused attention on a particular aspect of glaciology and have attracted people here to discuss a very interesting set of topics. I am sure this will stimulate work in the subject

in a way which is not going to suddenly stop at the end of this symposium. I would like to congratulate them, first of all, on their success in organizing this symposium and then for the local arrangements and the scientific organization. Brian Sagar, particularly, has done a magnificent job. I think it has all gone splendidly and we thank you and all the other members of your staff. Next I thank the scientific members of the organizing committee, Garry Clarke, Steve Jones, Bill Mathews, Stan Paterson, Brian Sagar, and Simon Fraser vice-president, Dr Wilson. Coupled with them we should also thank John Glen for taking on the responsibility for publication in association with the scientific committee. It has been a stimulating week for all of us present. It has also been a very comfortable and pleasant occasion, socially and in many other ways. Thank you very much all those of you who are responsible for bringing us together.

REFERENCE

- Calder, N. 1974. *The weather machine*. London, BBC Publications.