

GENERAL DISCUSSION

THE final session of the symposium took the form of a general discussion under the chairmanship of Dr O. Orheim.

O. ORHEIM: We all hope that any symposium, especially one like this, will show us the way forward, and we hope that a general discussion will summarize some of the things that have happened and also point out some of the difficulties for the future. Now, this particular symposium has contained much ice modelling and we have heard about a number of present-day ice sheets in terms of both our understanding and the problems of modelling them. So I thought that one way of structuring this discussion would be to look at how we fail to reproduce Nature in our laboratories. There may be many reasons for this failure: it could be that the models are wrong, or the input data may be incorrect. Another approach to the discussion (and this ties in with the Symposium on Glacier Beds: the Ice-Rock Interface from last week (*Journal of Glaciology*, Vol. 23, No. 89, 1979)) could be to look at large ice masses, their problems, and the conditions at their beds. I want to provoke Dr G. K. C. Clarke on this topic. Is one of the reasons why we get the strange results we have heard about this week because the input data which relate to the bed are wrong?

G. K. C. CLARKE: I have been thinking that, whereas last week we concentrated on the ice-rock interface, in this present symposium we have become familiar with two other interfaces: the interface between models and observations, and the interface between models and physical reality, that is, between physical descriptions of ice and its mechanical behaviour. I have been very struck by the divergence between the predictions of several different models, and also the character of the conflict between what geological input has to say about these things. It seems to me that when ice modelling becomes a rather more mature discipline, then we can hope to see the model predictions converge toward each other. Also, eventually, those predictions could converge toward what we imagine reality would be like.

Something I think would be worth considering before we launch ourselves on this grand path would be to standardize our notion of what an ice sheet is. I know that other disciplines have developed standards, for example, the 1930 gravity field as a reference; I wonder if we should not consider making a reference ice sheet which has specified rheological and thermal parameters, then we would know that discrepancy is due to either errors in the computation or differences in input and not present simply because people are actually describing ice in a different way. So, even if we do not believe that the Glen body is a good description of ice but we are going to accept it for the moment, we ought to agree on an activation energy, a creep coefficient, an exponent, and also on an ice density as a starting point. If we could all agree on those things then at least if we were going to make departures from those assumptions we could say so. I would propose, by the way, that we call this the zero B.P. ice sheet or "the one we decided on today". If you wanted to try what happens when you take the zero B.P. ice sheet and put $n = 20$ you could, and you would be in a position to say exactly what you were doing, it would be very much less confusing for the rest of us.

ORHEIM: Do we know this much about ice sheets?

CLARKE: I do not think that matters, I think that it is a matter of agreeing on a common starting point; even if we are all wrong, it would be more enlightening to begin with a uniform set of assumptions about ice behaviour. What does Dr Budd think about that, is it a good move?

W. F. BUDD: On the contrary, I think there is a problem if everyone does start off with the same thing. I think one thing which this Symposium has emphasized is that there seems to be a great deal of variety in what people do start with. I think there is more chance of getting the right answer more quickly by allowing some measure of variety there. But I think it is very important that the assumptions are exactly specified and that comes back to what Dr Clarke wants: an exact statement of the basis on which people are putting forward their models.

ORHEIM: Do I take it then that you think the difference between all these models is just a matter of disagreement between the background information? I think it is much more than that. Is it not that nearly all the modellings we have been seeing here are kinds of steady-state modelling?

BUDD: No, I do not think so, not steady-state. I am thinking about the paper by Waddington who was looking at changes in glaciers as responses to variations in input. The modelling we have been doing on surging glaciers works that way too, and with surging glaciers, if one keeps the climate input constant, glaciers can still build up and surge with their own internal instabilities.

W. D. HIBLER III: I tend to agree with Dr Orheim's contention that most glacier models are effectively steady state. In particular, it is my understanding that they do not include inertial and momentum advection terms in the dynamics. In atmospheric models these terms are the ones which most often produce instabilities. Consequently, it seems that unless such terms are included, you do not have a chance of introducing instabilities as a natural part of the model.

G. S. BOULTON: One of the things we must think about very clearly is precisely why people are concerned with past ice sheets. Sometimes the reason given is that there is a demand from the climatologists to give a boundary condition to their circulation models, but that leaves the question—is the exercise useful in itself? I would answer that by saying "Yes it is", and the reason for doing it I believe is simply to test the assumptions we are making. Let us have a model and then run it, in order to test the assumptions we made in building it.

We have been doing some modelling of ice-age ice sheets where we take some boundary conditions which are defined by other palaeoclimatic indicators and then use a modern analogue—say Greenland—to give us patterns of accumulation on the ice sheet. We then apply this to a flow model for the ice sheet. We often find that we cannot reproduce an ice sheet of the size indicated by the geological evidence. We believe that the principal reason for such failure is incorrect atmospheric boundary conditions. In this way, we feel that glacier models can be used to test estimates of past glacial climates reached by other means. For instance, we recently attempted to produce a model of the European ice sheet of 18 000 years B.P. and we found that, using geological estimates for snow-line elevation, maximum length of the ice age, and patterns of accumulation and ablation, which we predicted from general circulation models, we simply could not get our glaciers into steady state. If the ice had attained steady state it would have reached Madrid rather than stopping near Berlin.

R. H. THOMAS: There can be a danger with the modelling exercise and that is this: One can begin by building an ice sheet for some particular purpose and then find that one has become so devoted to the model that one applies cosmetic to cover up the defects. Eventually, the model which started life as an approximation becomes apparently more and more precise and one begins to believe more about it than is actually there. This meeting has been healthy in that some of the cosmetic additions have been demonstrated for what they are. This applies to all models and it is important that we test them as Dr Boulton has suggested against what we know about the geology or against what we think we know the ice sheet has done in the past, and I think that what we have got to do is to find out more about what the ice sheet is

doing today. We still have an argument as to whether the Antarctic ice sheet is in steady state or not, and until we have got a feel for what is happening there I am not sure that models are going to be of any use to us. We cannot test our models against existing ice sheets until we know exactly what the existing ice sheets are doing.

I. M. WHILLANS: I agree with Dr Thomas, modelling may not be particularly valuable unless we understand the physics better. In particular, we do not know what is controlling the bottom sliding of the ice sheets. Dr Thomas has shown some of the problems in trying to address the question of sea-level changes and although this line of work has been started the problem has by no means been solved.

We have been talking at this meeting about mechanical controls on the ice sheets, but there are also climatic controls. What controls the rates of snow accumulation and ablation? After all this is the reason why the ice sheet is there, and if the patterns of snow-fall change then the ice-flow must change also. What we need is a better understanding of both climatic and mechanical controls on the ice sheets and we can do much by studying present-day ice sheets.

THOMAS: Dr Whillans is already demonstrating how the problem can get out of hand. Until we can say whether the ice sheet is more-or-less in steady state, whether it is getting thicker or thinner, then there is little point in understanding what the bottom is doing or what the atmospheric physics is all about. Agreed, these problems have to be solved eventually and results from our model may help to do this.

WHILLANS: It is a straightforward procedure now to find out whether the ice sheets are thinning or thickening, but we have not done this to any great extent yet.

THOMAS: Exactly. The point is we know how to do it but we are not doing it yet. The techniques are available to us now and it is just a matter of getting down there and doing it. It is going to be a long-term project but the sooner we start it the sooner we will know.

ORHEIM: This is getting back to the original point about input data. It is clear that there are different viewpoints of this concept of modelling and testing.

D. J. DREWRY: I should like to add that I have found the ideas of the Maine group in respect of the 19 000 years B.P. ice sheet very stimulating. This is what has led me to think of an alternative way to explain many of the lines of evidence of which I was aware. I would say that the value of their research is that it combines into a single, but not necessarily unequivocal, model several sets of ideas which others may view critically from their particular standpoints. The Maine work has certainly led me to consider alternative ways of explaining available evidence for the Antarctic Late Wisconsin reconstruction.

R. M. KOERNER: You may be right about the 19 000 years B.P. model for the Antarctic, but I find the Laurentide ice-cap model somewhat depressing (psychologically, not isostatically!) in that it includes elements which are in many ways unacceptable to geologists, this means that they have to spend a certain amount of their time drawing attention to already published data in order to eliminate this model, rather than going forward to uncover new data.

THOMAS: No, this model is valuable precisely because it forces the geologists to state their arguments as to why it is wrong. This is what we need: plenty of interaction between the geologists and glaciologists, and, within reason, the more controversial a model is, the better.

DREWRY: Another point on this, you must, at least when you begin modelling take account of available information and not just let a model run without constraint. If this is not done the exercise is open to excessive, often fundamental, criticism, which by focussing upon detailed and avoidable inconsistencies detracts from the usefulness and credibility of the general model.

ORHEIM: Let us now turn to modelling techniques, there have been quite a few papers at this meeting concerned with this topic. Have there been any conclusions drawn at this meeting as to which techniques are definitely the best?

S. C. COLBECK: I am disturbed that so many people would like everyone to co-operate and follow the same lines. They would seek to identify a set of standard problems and establish a standard ice sheet which we would call *the* standard ice sheet. A small amount of this is acceptable but for the most part I feel it would be very destructive. To me, the thing that people should do is attack the problems that interest them and if those problems happen to be concerned with the top of the ice sheet that is nice, and if they happen to be concerned with the bottom of the ice that is a lot harder, but nevertheless, if that is really what they want to do that is what they should do. In the long run (or even the short run) that is how the maximum amount of progress will be made.

CLARKE: I think that this represents an implied criticism of my earlier leading remarks which I ought to clarify. I am not trying to suggest that the scientific community should dictate very much about the ice sheet. I do not, for example, personally care how thick the ice is when a model is switched on. But it seems to me that there are some common parts to all models such as ice density, thermal properties, and rheology. It seems to me sensible to agree on those parts. You do not think that?

COLBECK: I do not think that you can agree on the nebulous things, and, indeed, it would be positively dangerous to agree on anything and close our minds to the possibility that we might be wrong. In fact, it is helpful if people *cannot* agree on an activation energy. I would hate to see a decree come down which said that the activation energy to be used by anybody who wanted to publish in a scientific journal had to be such-and-such a number.

S. F. ACKLEY: There is some merit in using a simple rheology to look at particular types of problems and a complex rheology when you wish to use the physics to examine a particular mechanism. In sea-ice there is a benefit in looking at different rheologies and proceeding along separate lines in this way.

BUDD: The model which we constructed treated the longitudinal stress-strain-rate relationship as unknown and we determined the appropriate exponent (the simplest case has $n = 1$, but any functional relationship can be used). Having done this we found that, for surging glaciers, the unknown parameter turns out to be a value appropriate for temperate glaciers, but the most important thing for the surging model was that it should possess a multi-valued sliding relationship, in other words, we cannot have a unique velocity for a given shear stress, thickness, and so on. A multi-valued relationship is required, in fact, to have the multi-valued solution a surging model demands. The multi-valued relationship we found from modelling the surging glaciers appears to be much the same as we get from laboratory-sliding experiments. In the field this effect could be associated with many things: cavitation, water underneath, and all other sorts of physical properties for which we must find the appropriate relationships.

ORHEIM: But does this not ultimately mean that we shall need one model and one set of conditions for each glacier?

BUDD: I do not think so. We found practically the same "lubrication factor" from four different glaciers. "Lubrication factor" is a parameter which allows the stress to decrease as velocity increases rather than increase. So, on average, one glacier bed may be very similar to another on the large-scale. But, clearly, things like water production and annual variation need to be taken into account in individual cases.

J. W. GLEN: The thing which has impressed me is the extent to which this subject is becoming more and more complicated as time goes on. After all, glacier modelling started off with Nye's calculation based on ideal plasticity for the Greenland ice sheet (Nye, 1952) and it was astonishingly successful. It predicted quite accurately the profiles which had then just been measured by the French, and it looked as though what we had to do was to find a few additional things to iron out small inconsistencies in what was basically a simple model which worked. Then, as we have gone on, this error seems to have got worse and worse until, by the time I am at the end of this meeting, I am beginning to wonder if any models are of any use for any purpose whatsoever.

Now that may sound a bit harsh, but some of the facts which have been brought up are not in accord with the simple models. For example, one of the elementary things which followed from the Nye model was that if you had a surface slope in one direction then the flow was at least likely to be within 90° of down that slope, and of course we have had examples within this meeting of it being exactly in the opposite direction. All right, that is due to longitudinal strain caused by the longitudinal stress in a glacier so you want to feed these effects in. But another paper we have heard has pointed out that any attempt to feed these effects in produces corrections which are almost exactly equal and opposite to the residual errors when you have finished. I do not know how I interpret this except that we do not seem to be getting nearer and nearer to the truth. Am I wrong here, or is this a picture which other people recognize?

BUDD: This paints a very bleak picture. The results which Bindschadler (1979) showed were that the residuals were of opposite sign but the values were too big if you used the same flow properties which he was using for his shear relationship. I believe that one thing which he did not take into account was the octahedral relationship. I would tend to go the other way and examine the longitudinal strain-rates to see if this can bring us closer to the right result.

To address the question of how good or bad models are, I think that there are a lot of very good models about, but it is important to know the limitations of each of them. One that has not been much discussed in this Symposium is the Mahaffy three-dimensional model (Andrews and Mahaffy, 1976). This can be used with temperature calculations to get, I believe, very good approximations to the appropriate flow properties. However, it does not have a good sliding relationship. The sliding relationship which my group has derived for temperate glaciers from sliding measurements seems to fit many outlet glaciers in the Antarctic. This type of model should be more widely applied and eventually extended to the big ice sheets. When we do that and introduce the data, we have the same problem which Dr Boulton mentioned, that the ice cap will grow too big. Well, one reason why it grows too large is that as the ice cap develops it affects the climate and, in fact, lowers the accumulation at the centre. I think David Sugden did a very good analysis of his idea of a possible Laurentide ice sheet which took a lot of these ideas into account. Whether they are right or not needs to be checked, but I think we are in a position to carry that out now. We can carry out, for example, Weertman's suggestion, develop the Milankovich climate variations, put in the bed, put in the climate, grow the ice sheets, and see how they vary with time. I think this would make a very good programme to try to do for the I.U.G.G. meeting in Canberra in 1979!

G. H. HOLDSWORTH: This is a very interesting problem, but I think it would need to be coupled to a three-dimensional crustal-deformation model.

BUDD: Yes, I think the crustal changes with variations in ice thickness have to be put in. We know enough about the reaction of the uplift to feed in a reasonable response time for the compressions of the bedrock and any subsequent uplift which results from a time integration of the past behaviour of the ice sheet. This can be handled in the computer as one model.

HOLDSWORTH: Treating all this three-dimensionally will be very expensive of computer time. Will we need to wait for the next generation of computers?

BUDD: I did mention this in my review paper last Monday. I believe that to deal with the problem thoroughly one would need to wait till the next generation of machines, but that does not mean that we cannot begin now. There is a lot that can be done with more simplified models. Alternatively, performing one run only over a long time would be very valuable. Just to take an example, there is a lot we can do with a two-dimensional section of some of these ice sheets in terms of modelling and parameterizing for three dimensions before we tackle the complex cases.

ORHEIM: Is the general feeling that for the time being we have to do simplified modelling? From what has been said these models will not take us much further until we are one or two generations of computers along. For the large ice sheets such as Antarctica we know that the modelling work which has been done so far clearly misses the salient features and has to stop before it gets to the outflow glaciers. In addition it has a spacing which is an order of magnitude too coarse.

In a few years we will be getting data over the large ice sheets with such a detailed precision that it will perhaps be beyond us to handle. Dr Zwally gave a fascinating paper on the data from the geosatellite over Greenland saying how the profile over Greenland is now known to within a couple of metres in the vertical direction. They have a plan at the Goddard Space Flight Center, which he would have been quite happy to tell us about had he been here, to get a satellite dedicated to glaciology and sea-ice research up into the air by 1985. By that time they will be profiling the Antarctic ice sheet with a precision of perhaps 10 cm together with all sorts of other sensors. Now you who are doing practical field work in and modelling of the Antarctic, how do you feel about this, and how do you want to handle these data?

THOMAS: Perhaps those of us who are doing field work will have to look for another job.

ORHEIM: It probably will not be that bad. I suspect that we will be out taking ground-truth data for the satellite. It will be a different kind of field work. Seriously though, can you anticipate being able to handle these data and will you be able to cope with models for which you have input on the kilometre grid spacing?

BUDD: Two comments on that: One important point is that if one wants large-scale motion then one can neglect the small-scale motion for many purposes. That was another very important result of Bindschadler's work, he found that if you use large-scale averages for surface slopes then you can model large-scale dynamics reasonably well. We found this too.

The same applies on the Antarctic ice sheet. We are getting these surface waves of wavelength several times the ice thickness, but again they are a small-scale feature; we believe we understand them in terms of ice flow over undulations but they do not come into the large-scale dynamics.

The other point is that one of the most important pieces of data we need in order to check the sliding is the surface elevation and ice thickness along the outlet glaciers together with measured velocities. The scheme which I described the other day for relating velocities to a formula involving the shear stress and the normal stress above buoyancy is something which can be used in this check.

THOMAS: I agree with Dr Budd, certainly when we are looking at these fast outlet glaciers. I am not too sure whether you will be able to test your relationship because I am still not convinced that one can calculate the water pressure simply by extrapolating sea-level up-glacier. I think the water pressure must decrease inland, and I do not think this is calculable. But, in principle, the more data we get from these fast outlet glaciers, the more likely it is that we will be able to understand what is going on.

I would also agree with the inference Dr Budd made that modelling the big ice sheets is easier than modelling the very small ones. In modelling the small glaciers you have to concentrate on the small-scale effects (which you can ignore with the big ice sheets) because these are the principal things you are looking at.

ORHEIM: Are the small-glacier people happy about that? I think that there is frequently an order-of-magnitude difference with Antarctic ice sheet modelling also, but hopefully this will improve. One thing which may have struck many of us—it certainly struck me—we saw in Dr Whillans' work a very small datum on the map of the Antarctic ice sheet from which he got a considerable amount of information about the stability of a very large piece of the Antarctic. Is this a good way to proceed?

M. M. HERRON: One problem is the lack of long-term accumulation data. Are the modellers willing to use 24 years worth of data as is obtained from fission-product dating, stand as an acceptable average rate for the last 5 000 years? Long-term data are not easily obtained.

WHILLANS: That is a real problem. Data can be obtained back to the sixteenth century but that is still a short time for accumulation-rates. If we are modelling the Antarctic ice sheet we need to know how constant the climate has been. What we desperately need is a good time series for accumulation-rates for some central location, and perhaps also details of long-term glacier variations. On well-dated cores the annual layers can be identified going back thousands of years.

KOERNER: The good resolution which Dansgaard and his co-workers get, going back over thousands of years, depends on the accumulation-rate being sufficiently high. They aim to find a good standard and then to look for various signatures at different depths, such as a volcanic ash layer which they can identify in cores from other places. Whether you can do this in the Antarctic or not I do not know, presumably you cannot cross-correlate events in the Northern Hemisphere with layers in the Southern Hemisphere.

WHILLANS: There are definite possibilities in this kind of stratigraphic study. Ellesworth Land is a likely place as I do not believe that anyone has looked at the stratigraphy here and it is a place where the accumulation-rates are comparable with those of Greenland.

HERRON: One more point about the implication by Dr Budd that it is simple to insert changing accumulation-rates and temperature patterns into physical models which are themselves growing and changing; it is not. The accumulation-rate at C-7-1 on the Ross Ice Shelf is very close to that at the South Pole, despite the fact that C-7-1 is nearly 3 km lower in elevation and over 1 Mm closer to the ocean. The relationships governing accumulation-rate variations are not well understood. For modelling the Wisconsinian Antarctic ice sheets, I do not think it will be adequate to apply simple accumulation-rate–elevation factors.

THOMAS: To get back to the Antarctic stratigraphy. I believe that the chemistry approach is getting somewhere. I read that Wilson (1978) counted layers back for many thousands of years by some chemical technique. Presumably these methods are now being developed for the Antarctic stratigraphy, even allowing for the fact that the accumulation-rates are relatively low.

HERRON: There are many methods to overcome the Antarctic problem of small annual layers. I disagree with Dr Koerner in that I think it is very easy to cross-identify volcanic layers with Northern Hemisphere dates. In addition to that, there is the possibility of using nitrate techniques to provide the information.

BUDD: Just to comment on the question of Antarctic accumulation-rates: I was referring to the high rates which Mahaffy and colleagues (Andrews and Mahaffy, 1976) had to put in to get

the ice sheets to grow large enough in a short enough time to lower the sea-levels. Their accumulation-rates were far too high for present-day conditions. However, the things they did not include were the long-term Milankovich variations. The appropriate time scale is not the first 5 000 years but the range 10 000 to 15 000 years. We found when we included this variation that the accumulation-rates did not have to be so large.

ORHEIM: To move to quite a different field. I had not anticipated how many papers would deal with the ice shelves around Antarctica as well as the importance attached to ice shelves in the reconstruction of past ice sheets. Does this mean that it is really the fringe areas that we have to concentrate on in our modelling and that we can forget about the main ice sheet?

WHILLANS: The fringe areas of glaciers are known to be inherently variable and the outer edges may well be the places to look at if we want to know what the ice sheets as a whole are doing. What is needed is to go to an area for which we have the expertise to understand the dynamics.

THOMAS: I agree that we need to look at the edges, particularly those of the ice sheets. Most models cut off where the ice shelf begins so there is no interaction between ice shelf and ice sheet; that has to be wrong, we should include the interaction between sheet and shelf. As regards Dr Whillans' point, there may be an inherent trend for the ice shelf to thicken and thin more easily than the ice sheet, but we should take this as a warning. If the ice shelf starts to thicken or thin in a regular manner—not just a bump going through—then that is a warning of things to come, especially if we take the CO₂ warming effect on climate seriously (I am not sure that I do!). If we consider a quick warming by 5–10 deg in 50 to 100 years then the edges are going to be the first things to respond; so we have to keep looking. Of course ice shelves are easy things to look at and study anyway because of their very simple dynamic nature. We have already come across the problems that arise when we analyse ice sheet behaviour because we do not know what is happening underneath. Although we also do not know what is happening under an ice shelf, we can make some fair assumptions about the dynamics.

ORHEIM: I feel that we have not spent too long on this discussion, but we have covered many points. I now want to open it up to anyone who wants to say something. Things we have not covered are the internal parts of the ice sheet, we have some glacier-chemistry people here and we have heard about the radio echo-sounding of layers, and so forth. Are there any comments people want to make about the internal structure of ice sheets?

THOMAS: Together with all the work we want to do on the tops of ice sheets and ice shelves, we need to do more of the kind of thing that Dr Budd and Dr Baker are doing, that is, to work on the ice that we get out of the ice sheets. Dr Baker's work has been looking at the effect of small particles on the creep of ice, and Dr Budd has been looking at the effect on creep properties of the fabric that develops in the ice core. This kind of work is important because there appear to be regions in the ice core that are much softer than the ice around them. It seems to me obvious that we cannot just assume a flow law for the ice core, we have to look at real ice cores and find out how the fabric forms. There seem to be order-of-magnitude differences between the hardnesses of ice *in situ* and unless we get a handle on this our modelling is largely a waste of time.

WHILLANS: It is possible to use radar layering to examine the balance of the ice sheets in times past, but there are two limits to this approach. First, it is not really known, at least to my satisfaction, what is causing these layers; this should be investigated. Secondly, the limits of how far back in time we can push the interpretation of these internal layers in order to work out the mass balance have not been evaluated. Layers near the bottom are always going to be

nearly parallel to the bottom, regardless almost of what the balance of the ice sheet is or has been. So the question that Dr Robin is talking about, the mystery of the radio-echo-free zone, is not a severe blow to us because the layers that he cannot find would not be very helpful anyway.

DREWRY: The absence of radio-echo layers close to bedrock is important in regard to the validity and understanding of depth-time stratigraphies in ice sheets. Close to the centres of outflow, where horizontal motion is small, layers extend to the bottom and follow topography fairly closely. We observe an increasing separation between the lowest visible layer and bedrock as we move down a flow line. This occurs despite the fact that our system performance is sufficient to detect layers to deeper levels theoretically. We believe that this echo-free basal zone is due to the mixing of strain-softened bottom ice during flow over and around rough terrain (Robin and others, 1977). In other words, layering is not visible because it is no longer present, having been destroyed by complex deformation. The lowest observable layers, therefore, represent a minimum depth to which the law of superposition is valid. Below this horizon we should not trust to continuity and a simple depth-time chronology based upon uniform deposition and strain. Thus, wherever we plan to core to bedrock we should conduct a simple radio echo-sounding survey to establish the likely depth of continuity. Perhaps the controversy over interpretation of the deeper parts of the ice cores (e.g. at Camp Century) may be resolved if we have a healthy scepticism over dating?

ORHEIM: What are the radio echo-sounding results from the Byrd Station area?

DREWRY: At Byrd Station we have layers for about 85% of the ice column, at Dome "C" to about 80%. We can see layers down to the bottom (more than 90–95%) on many records from central East Antarctica. We must be careful however, in making sure that the cessation of layering is not due to inadequate system performance.

GLEN: In fact, of course, for rheological purposes it is quite obvious that the ice at the bottom ranks amongst the more important material since it has the highest stresses on it, and another very important thing we have to settle is whether in the Antarctic the bottom ice does have very markedly different properties. The more evidence we can get on that, the nearer we are going to be to solving a great number of problems.

K. PHILBERTH: What I do not understand about the velocity profile of the cold ice sheets is this: On the one hand, there are people who say that the two-layer model is good enough and that it is not even important to know where one layer ends and the other layer begins. On the other hand, it is emphasized by some that it is not sufficient simply to take into account the temperature profile with regard to viscosity and Glen's law, you must take into account impurities and fabrics also. How are these two views reconciled?

BUDD: There are only a few places where velocity profiles have been measured with depth so I think those have to be our guides. We found, on Law Dome, a stagnant layer near the bottom and, in spite of what Dr Glen suggested a minute ago, it appears shear stresses are highest somewhat above the bed. The lower part is stagnant not because fabrics impede the movement, but more because the material seems to be locked in somewhat by the irregularities. In other cases, for example Camp Century, it may be that the bed is smoother and a different result may occur, but until the velocity profiles have been measured then it will be difficult for us to say. Perhaps it will be possible to analyse the core to see how it performs (the same goes for Byrd—but here unfortunately the bottom part of the hole is blocked). The question is still open.

It all comes back to Cray's statement to us of ten years ago, "We must get down and drill, drill, drill" until we reach the bottom and can measure all these things.

ORHEIM: Perhaps that is a good place to finish this discussion of the dynamics of large ice masses, it certainly points to a future for us. This has been an enjoyable discussion, I thank the participants for contributing to it, and I now close this session.

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