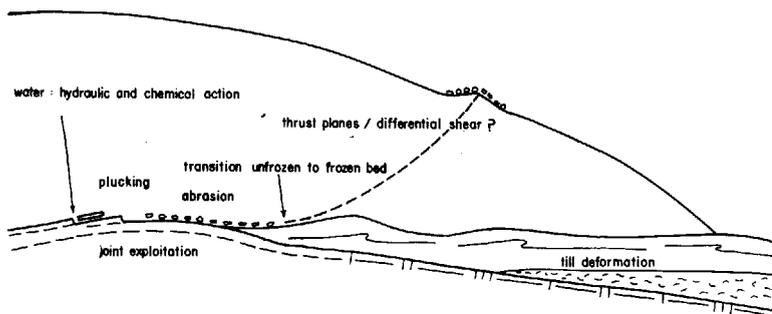# GENERAL DISCUSSION

Chairman: B. Hallet

GLACIER BED EROSION, EROSION FEATURES, AND
PROCESSES AT THE TERMINUS
H RÖTHLISBERGER: Before we start a heated
discussion which will be difficult to stop, I
have made a little drawing showing the transi-
tion zone from unfrozen bed to frozen, about
which various questions may be raised:

The first question concerns differen-
tial shear. If we have bottom sliding further
up- and no sliding down-stream, do we have,
between these two zones, an area in the ice,
which may be more or less extended, of discrete
shear, or what could better be called a thrust
plane or fault? We will then have to consider,
as we did in the first session, water at the
bed, what effect it may have in the various
models, and where it goes. Incidentally,
observations from the Axel Heiberg expedition,
that have not been mentioned yet, show that
water does come up exactly along the shear
zones. Then we heard about the flow of ground
water below the glacier bed. We may come back
to discuss water in channels or sheets, and what
its effects amount to. If we go further up-
glacier, there may also be some intermediate
storage, as is well-known from Antarctic radar
records. Similar water bodies may also occur
closer to the edge where we have a continuous
flow and some storage in between. So there is
a diversity in natural phenomena, but even more
diverse are the methods applied to the study of
them. Among them stands out continuum
mechanics, which may be wholly or partially
applicable. Then there is the approach
referred to, among friends, as speculative
discontinuum mechanics. We may even use
observations, equally intuitively or specula-
tively interpreted. Please let us have more
discussion of these points.
R L HOOKE: I would like to try to clarify
further my thoughts on shear and debris
transport in glaciers, discussed at some length
earlier in the week, and to invite further
comment from participants.
    Firstly, the existence of a foliation
defined by alternating bands of debris-bearing
and clean ice is not evidence for discrete
shear along the debris bands, nor for shear
being responsible for incorporation of debris
into the ice. Two specific lines of evidence
mitigate against such an interpretation:
(1) Experimental data (Hooke and others 1972)
suggest that debris concentrations in excess of
about 10% by volume stiffen ice rather than
soften it.
(2) Measurements and theoretical calculations
indicate that foliation exposed at the surface
near a glacier terminus is not, in general,
parallel to the direction of maximum shear
strain-rate (Hooke and Hudleston 1978).
    Secondly, two of the most commonly cited
lines of evidence in support of the shear
hypothesis can be readily explained by alterna-
tive processes for which solid field evidence
exists:
(1) Down-glacier facing steps in a glacier
surface are frequently the result of differential
ablation, with ice down-glacier from the step
melting faster due to a thin dirt cover.
(2) Clean ice beneath debris-bearing ice in
the type localities of the Thule-Baffin

moraines can be explained without resorting to
a thrust-type mechanism in which exceptionally
high shear strain-rates occur across relatively
thin zones (Hooke 1973).
Alternative explanations should be considered
for other features which suggest thrusting in
ice. For example, some up-glacier dipping
discontinuities overlain by recumbent folds may
actually represent the strongly attenuated
lower limbs of these folds.
    In conclusion, the question is not one of
whether shearing occurs as a normal process of
glacier flow: it does. Nor is it one of
whether zones of higher shear strain-rate in
clean ice, which are a few centimetres in
thickness, exist: they do (Hudleston 1977).
Nor is it one of whether thrusting can occur,
particularly in ice under low hydrostatic
pressure near the glacier surface: it probably
does. Rather the question is whether dirt is
either incorporated into the ice by such
shearing or, once incorporated, is transported
to the glacier surface along such shear zones.
This has not been demonstrated. To demonstrate
it, we need, among other things, detailed
velocity profiles across debris bands
demonstrating that shear strain-rates are much
higher in the dirt band than in the adjacent
ice.
A IKEN: You said that the observed thrust
planes are usually not in the direction of
maximum shear. If I remember rightly, Haynes
from CRREL (Haynes 1973) did laboratory
experiments on fracture of ice and got the
same result. For a sample under compression,
the directions of maximum compression and the
smallest principal compression are at 90°, so
maximum shear would be at an angle of 45°.
From Haynes' results, however, it follows that
the normal to the fracture plane ideally makes
an angle of 29° with the tensile principal
stress if the other stresses are compressive.
I wonder whether this direction suits your
observations better? My second point is that
the shear fracture does not occur inside the
dirt zone, but beside it, on top of it. So
maybe it is a sequence of processes, that
debris gets into a fresh shear plane which
later is abandoned, and then a new one forms.
HOOKE: This divergence of angle is more than I
was referring to. What we find is that if
you make measurements close to the glacier
surface the foliation dips steeply up-glacier
at the surface, but decreases in dip rapidly
with increasing depth. We cannot come up with
a specific angle between the foliation
direction and the direction of maximum shear
strain-rate as that angle varies in space.
Near the base of the glacier the two are
probably very nearly parallel. We have no
measurements, but certainly feel that is the
case. It is near the surface of the glacier
that the two begin to diverge.
    I think the second point is a valid one,
but if the shear is above the dirt band, it is
a question of what the role of that shear is in
moving the dirt to the surface.
R A SOUCHEZ: I am working on the variations of
δD (deuterium) and δO^{18} in ice of a glacier
basal sequence from Bylot Island in the
Canadian Arctic. There are numerous dirt

layers in the ice and it appears that previous
investigators believe that the entire basal
sequence was incorporated by freeze-on at the
base of the glacier. If you study both δD
and δO^{18}, not simply δO^{18}, you can distinguish
between ice that is refrozen at the bed and
glacier ice. If you do that, you find that
the ice which is refrozen at the bed is only a
thin layer and the layers between are glacier
ice. So you have a repetitive sequence of
thin layers of ice which refroze on the bed
and glacier ice. Looking closely at the
structure you find very elongated folds which
give rise to this repetitive sequence. This
point favours major folding at the glacier base
including glacial ice within the basal sequence,
along the lines of Hooke's idea.
HALLET: With reference to folding of basal ice,
I think it would be appropriate to ask Dr John
Shaw to express some of his ideas on the
subject.
J SHAW: I have been wondering a little bit
listening to the discussions what the motivation
for studying glacial erosion and sedimentation
is. I would have thought that much of the
motivation would be to understand the landforms
and sediments that result. But I have not
noticed the glaciologists paying much attention
to either landforms or sediments. I was very
surprised to hear a point being brought up by
several people that folding in the ice could
possibly cause foliation; this is an idea that
has been around for a long time and we are just
recycling things. However, another important
element is that, if we do get folding, we
increase the debris volume over an area of the
bed. Possibly this could directly lead to the
formation of a landform. There are two ways
in which it could not: if deposition occurred
by lodgement on the bed and these folds were
continuously dragged along, then the structures
would be completely destroyed, and,
alternatively, if the surface of the ice were
to melt down and this soft sediment was let out
at the surface, it would flow, and the structures
would be destroyed. The final possibility is
that we could simply melt out the ice *in situ*
from a stagnant body. In this case the
structures and the form will be preserved, but,
of course, compacted.
    Rogen moraines are an example of forms
produced in this way. They appear in Sweden
quite close to here [i.e. Norway]. The reason
I bring them up is because I talked to several
glaciologists at the beginning of the week and
they did not know what they were. There are
several important points about Rogen moraines.
First of all, they are transverse ridges.
Second, they tend to be "drumlinized" at their
up-stream end. Third, they have flutings on
the surface. Fourth, eskers which run through
fields of these features tend to be of a younger
age. Given these observations, the Swedish
geomorphologists long ago decided that Rogen
moraines in some way formed as a result of
active ice. The proximal side of Rogen
moraines is always extremely plane, and the
distal side irregular. However, morphology is
not sufficient to determine the mode of
formation. We must also look at the internal
structure. If we look at the internal
structure of the moraines, we can recognize
folded beds of clearly differentiated till.
Cross-cutting these folds are bands of sorted
sediment. It seems that as we have folding of
the till, but the stratified layers are
horizontal, the stratified layers did not exist
when the folding occurred. They must have
developed subsequently. So we have folding
occurring in the active stage, and then
deposition by melt-out in an englacial position,
probably caused largely by water flowing
through a debris-rich ice system. At the
time these observations were made, I knew of
no modern-day analogue for Rogen moraines.
However, some close similarities exist between
the observations of David Croot on modern
glacial processes and the above deductions
based on Pleistocene landforms and sediments.
    The point I would like to make is that by
studying Pleistocene sediments we have access
to things that are probably inaccessible in the

modern environment: the bottom of the ice sheet and processes which may not be occurring at present. I think that if the glaciologists would turn their attention to solving problems as to how such things as folding occur and the physics of melt-out, it would be helpful. The geomorphic conclusions presented are derived more on the methods of Sherlock Holmes than John Glen. Maybe the John Glens of this world can now apply their more quantitative skills to similar problems.

J T TELLER: Roger Hooke stated that debris strengthens ice rather than softens it and therefore a debris-rich plane is not a likely avenue for shearing or thrusting. I would like to ask if a shear plane that was initiated in clean ice, but which progressively became enriched with debris, might still continue to be the weakest plane within the glacier? That is, it seems that once movement along a shear plane develops, the minimum stress required for continued movement will coincide with that plane, even if debris is moving along it from a near-basal position to the glacier surface.
HOOKE: Firstly, a plane is a two-dimensional feature but ice crystals and debris particles are three-dimensional. This distinction may not seem important, but it has always been a stumbling block for me. A shear *plane* exists, for example, between two cards moving one over the other. In ice, however, the mechanical problem is easier to visualize if we think in terms of shear zones, a few millimetres to perhaps a few metres thick.

On the Barnes Ice Cap, Hudleston has measured crystal orientations in shear zones a few centimetres thick and finds strong single-maximum fabrics with c axes oriented perpendicular to the zone. Such fabrics no doubt tend to soften the ice. This could possibly more than offset the hardening effect of any debris that might be present in the zone. (There has not been any debris in the zones we have studied, however.)

Of greater importance is the question of the role that such zones might play in entrainment and transport of debris. Regarding transport, our measurements, and I believe those of many others, have failed to demonstrate a significant difference in shear strain-rate across debris-rich bands that have produced ice-cored ("shear") moraines at the glacier surface. As ice beneath such bands is not stagnant, shear zones are not necessary for formation of such moraines. Regarding entrainment by shear, so far as I know no one has presented a clear mechanical model for this process, so let me suggest one. Possibly ice surrounds a debris particle resting on the bed and then moves it forward over stagnant or slowly deforming ice. (If the latter ice is slowly deforming, the flow must be three-dimensional to avoid a space problem.) Detailed strain measurements would be necessary to demonstrate the reality of such a process and especially the role of shear zones in it. If ice with a transverse flow component simply closes beneath the particle, as may occur during plucking, the process might better be called entrainment by plastic flow, thus contrasting it with entrainment by regelation.

R P GOLDTHWAIT: I have a couple of observations to make. One is in connection with John Shaw's remarks. Near the margin of Casement Glacier, tunnels came in behind what was essentially a *roche moutonnée* form or knob (Peterson 1970). The tunnels intercepted an open space in the lee of the *roche moutonnée* over which ice was sliding at 2.9 cm d⁻¹. The ice looked like dirt, and indeed the bottom 0.1 to 0.2 m was till-like debris. When we tried to work there it warmed up a little and the debris started to fall off on us. In such a situation on the lee side, you lose some of the diagnostic items that have been attributed to lodgement till. Lodgement-type debris with oriented pebbles gets all mixed up as it peels off. As you go back from the margin you can imagine closure rates which might make large cavities impossible. What happens there I am not quite sure, but I am sure that the fabric under thin ice gets all mixed up over boulders and bumps.

I should bring up a second point which is pertinent, but old. We tunnelled into the Red Rock ice cliff (Goldthwait 1960) beneath 30 to 41 m of ice. In that tunnel we drove two shafts down and found that the first centimetre of ice above the bottom, which lay on miscellaneous rocks and vegetation, moved not at all. We put in steel pegs and measured flow rate. As we came up 20 mm above ground the ice moved a little (0.48 mm d⁻¹) and still more rapidly above (2.72 mm d⁻¹ at 5 m above ground). The surface of the glacier was moving at a rate of 12.1 mm d⁻¹. This differential motion is what I think of as an internal shear. Now at that particular point, more than 30 m back from the cliff under fairly thick ice, we had no significant amount of dirt except a few little lenses coming along in the lower 1.3 m of ice. We do not know where they came from because this basal ice is obviously frozen to the ground

beneath it and doing no erosion. Out in front, however, and in the outer part of the tunnel and side tunnels, there were great intersecting folds of thin dirt bands and, in some cases, pods of dirt along the bands to as much as 10 m above the ice base here and 30 m in other places. Detailed petrographic work in the tunnel shows that the dirt was accumulated in front of the ice cliff in snow-drifts which build up annually and into which a certain amount of dirt is incorporated. As the ice front advances it incorporates this material as part of its body. On the inclined toe below the ice front, bands of dirt increase in slope as they come under the ice cliff. By the time this ice is overridden 8 m into the glacier, these bands have overturned and are well-folded. We believe the dirt rises and is accumulated and recycled right at the front of the glacier. We have no evidence on the basis of materials found that any of the debris came from far up-glacier, but it did come sometime.

We dated some of the organic debris in the pods and the age was around 4 760 ± 220 ¹⁴C a. On the stones underlying the glacier 16 to 30 m back, we found lichen which our lichen expert, Dr Wolfe, tried to revitalize. He felt that it was viable although we never got much growth. The material around those rocks dated around 200 years old, so presumably the advance occurred over here within the last 200 years.
D J DREWRY: I would like to make additional comments to those of Roger Hooke in regard to whether shear planes bring up material, or whether material already in the ice encourages strong differential motion. There are one or two observations that we can take from ice sheets away from the very complicated marginal zone (where flow lines move up towards the surface and complications arise due to complex transverse and longitudinal strain). Observations from Greenland and Antarctic ice cores show that very fine-grained sediment layers tend to be associated with very fine-grained ice having a high degree of c-axis verticality, and that there is strong differential motion associated with these layers. The fine-grained sediments have not been brought from the bed, which may be both several thousand metres deep and several hundreds of kilometres up-stream, but have been deposited on the ice-sheet surface (eventually forming deep isochronous horizons) by such activity as vulcanism. These fine-grained sediments locally enhance the ice creep rate. At Byrd station, for instance, there are thin bands of volcanic ash between 1 200 m and 1 800 m. Fabric studies of the ice column by Tony Gow show that, in the fine-grained debris band, the c axes have a very high vertical orientation and that there is strong differential movement in this zone. Nobody who has looked at the Byrd core or the dynamics of Byrd station in relationship to the west Antarctic ice sheet would interpret these debris horizons and strong shear motion as having anything to do with material brought from the bed; it is a volcanic horizon enhancing creep in the ice sheet.
HOOKE: Could you comment on the concentration of debris in those bands?
DREWRY: The concentration was very, very low. Debris formed cloudy bands, containing volcanic glass of less than 5µ size. In this case we are not actually stiffening the ice but introducing extra dislocations which enable the ice to creep more rapidly.
RÖTHLISBERGER: In relation to David Collins' concept of various melt-water routings through the glacier, I would like to point out that I expect the main drainage channels to be located at the bed. This can be inferred from the fact that the bed is undoubtedly the principal longitudinal discontinuity extending over the whole length of the glacier, as well as from the consistent occurrence of heavy sediment load even in cases where the glaciers end in lakes or fjords. Indeed, the bulk of the melt water is successfully captured in subglacial intakes of existing hydro projects, although, as happened at Argentière, sometimes at changing locations of the bed. An englacial water conduit has nevertheless been reported temporarily, 0.5 m above the rock bed, at Argentière also. The englacial flow, however, occurred in this case in a severely disturbed zone where seracs are moving down a major rock step. It represents therefore an exception rather than the rule!
I would like to add that the picture outlined in connection with the plucking mechanism is part of the story only where subglacial drainage is concerned. At the large pressure fluctuations of the melt season, channels incised from below into the glacier sole can form over and over again in the same location relative to the glacier bed, so that water follows more or less the same course. During the steady water flow of winter, to the contrary, the channels are more likely to move along with the ice, thereby coming continuously into contact with new areas of the bed

(provided there is sufficient bed slip). This might be one possibility for explaining the high concentration of solutes in winter, and I am interested to hear from David Collins whether this idea is compatible with his findings or not.

The lifting up and rearranging of channels can lead to more severe changes, as in Argentière where the main channel shifted from one side of the glacier to the other leaving the subglacial water intake without a source of supply. The ice thickness changed and the glacier gets a big push. Something similar also occurs locally, of course, and this may account for the highly variable concentrations of sediments with time. For example, new moulins develop and we get new crevasses, so at these places it is clear that when these events occur, we get a surge of fresh material.
D N COLLINS: I would agree with much of what Hans Röthlisberger has just proposed. One thing to think about is the idea that at the bed not only is the glacier moving, and delivering sediment into small channels, but also that the channel system itself, particularly smaller channels, can move across the bed. At least channels which are incised upwards into the ice can move. However, I imagine that the main arterial channels will be incised down into the bed and therefore fixed in position. To actually explain how sediments and solutes vary with discharge, there must be a multi-component channel system at the bed. Perhaps big channels incised downwards are continuously scoured out of any sediment except during winter. In spring, sediment is flushed through and those channels never again contribute sediment during the season. But then the smaller channels have to be envisaged. These are always acquiring sediments and solutes as they migrate, and feeding them into the main channel from where they are effectively flushed from the system in summer.

I also like the idea of cavities at the bed which may change their sizes with differing water pressures in the main channels. Melt waters are led into temporary storage in cavities, and subsequently water returning into the main channel system has increased amounts of solutes and sediment. I also agree with Hans Röthlisberger that englacial drainage in ice-walled channels above the bed is improbable. Where before I have talked about the difference between englacial and subglacial systems, my englacial system transports water which is not chemically enriched and includes the basal arterial channels in which I presume that further chemical enrichment does not take place.
HALLET: We have been mapping pro-glacial areas where the exposures have allowed us to decipher much about subglacial processes. Extensive networks of closely associated cavities and channels exist in these areas. In some places as much as 30 to 40% of the glacier sole was not in close contact with the bed. Therefore there is a tremendous potential storage of water under the glacier, and it seems from a number of lines of evidence that all these cavities at some time interconnect. Presumably as the water pressure drops or as the sliding velocity drops, the cavities will fill in with ice and can reform subsequently. Occasionally the areas where the glacier was in intimate contact with the bed seem to be flushed out, presumably as the water from the large-scale hydraulic network communicated with the basal film. Much of this story fits rather well with the solute record that Dr Collins presented.
SHAW: I would like to respond to Dr Röthlisberger's and Dr Collins' point that englacial channels are improbable. That may well be the case if the ice is active, but once the ice is stagnant, channels within the ice become very common. Much glacial sedimentation occurs at this stage.

The second point that I would like to make is that it seems that the people who are working with glaciological processes are looking at two scales: the very small scale on one hand, and a jump to the large scale of the whole ice mass. In doing so they neglect the transport of debris in the basal portion of the ice where we may have 40 to 60% by volume of debris. I would like to ask them why this is neglected, because so long as they neglect it they are not much use to those glacial sedimentologists and glacial geomorphologists dealing with depositional landforms.
HALLET: I would argue slightly with that. I think that for a glacial geomorphologist interested in erosional surfaces, it is quite appropriate to consider ice with sparse debris. Several of you probably have comments about the volumetric content of debris in basal ice, and others may want to comment on efforts on modelling debris-rich ice.
DREWRY: One of the most valuable contributions to be made is communication between glacial geologists, glacial sedimentologists, and glaciologists to discuss what parameters are

needed as input to models and how to measure them. When I started trying to find data on basal debris concentration, I found little of practical use. We have heard at this meeting many bulk values quoted, 0.1% up to 50 to 70% by volume. What we really require are vertical and horizontal variations and the disposition of debris; whether in stacked horizons, whether sediments extend for a considerable vertical distance within the ice. I would make a plea for glaciologically relevant measurements of sediment in ice masses.

D E SUGDEN: I would like to follow up what John Shaw said when he pointed to the difficulty of evolving effective links between those working on process studies under present glaciers and those working on landforms and sediments in areas formerly covered by ice in the Pleistocene. Two fields occur to me as being potentially very interesting: one is esker patterns. In many parts of Scotland it is quite common to find a very complex anastomosing pattern of eskers. When you map them in a bit more detail you find they seem to have a different morphology. Often in a whole system you might find one rather complex ridge with a very sharp-crested and clear form. The others may be successively less clear, with more signs of over-running and streamlining by ice. All I am suggesting is that perhaps these represent the channels that Hans Röthlisberger has been mentioning. An esker may build up in a channel at the end of one season, then the next season it may not reopen and another may open. In the meantime the original esker deposit is beginning to become moulded by moving ice, and so on. Perhaps this line of attack might allow Pleistocene field studies to contribute to studies of melt-water flow in glaciers.

The other point I would like to make concerns water tables. In areas where down-wasting ice was a common feature, a lot of the landforms seem to be explained by geomorphologists and glacial geologists in terms of sediments deposited beneath a water table. These studies imply water is not necessarily flowing at the bottom of the glacier when there is a water table. Streams issuing from calving glacier snouts in Greenland fjords often emerge at the water level. I have seen this in many places in the South Shetland Islands, too. The channels emerge from the glacier at the water surface rather than the bottom of the glacier. Perhaps there is hope for interchange of ideas here.

HALLET: I think the esker problem is one that is being explored rather fruitfully from a theoretical point of view. This is work that is being done by Professor Ron Shreve, who is considering the physics of tunnels in glaciers, calculating the equipotentials under the ice, and studying the different patterns of eskers. In places, eskers form complicated networks, and in others eskers occur by themselves. So the disposition and shape of eskers is a fascinating problem. I think we can look forward to some very interesting advances in that field following Shreve's introductory work on the subject (Shreve 1972).

J W GLEN: I feel I should respond in some way to what John Shaw has been saying because I do not believe that glaciologists or modellers are deliberately avoiding the evidence which glacial geologists want to put to us. I am continuously struck, not by the complexity of what we are asked to explain, but by the relative simplicity. For example, people talk about measuring the debris content in a glacier. Surely we would expect this to depend enormously on the nature of the rock over which the glacier was flowing, yet I do not see much discussion of that. Equally, the kinds of forms, be they erosional forms like *roches moutonnées*, or depositional forms like eskers, or, dare I say it, drumlins (if they are a depositional form and not a depositional-erosional form, which is being debated), are relatively similar irrespective of very different types of rock over which the glaciers concerned have flowed. I for one got the message that these things were really rather similar irrespective of rock form and therefore what we had to do was to explain why that was so. That, I think, is what we have been trying to do. If we have got the wrong message, then tell us.

You say we work on a smaller scale. A thing like a *roche moutonnée* I take to be what you mean by something on the small scale, and then, on a very big scale, I suppose you mean something like the Laurentide ice sheet. But I do not know what the phenomena are that we have to explain on some sort of intermediate scale in a way that we can go away and do it. Perhaps a valley glacier is one; plenty of people have worked on that. What is it that we are not doing?

SHAW: Yes, you are working on a small scale at the scale of a *roche moutonnée*, and an even smaller scale when you are considering the effect of the single particle eroding the bed. And yes, you are dealing at a large scale with ice sheets such as the Laurentide ice sheet. An example of the intermediate scale is exactly what you mentioned, that is the drumlin. For a long time geomorphologists have been considering drumlins in terms of flow patterns in the ice. I think we have very good evidence that there are complicated flows associated with drumlins, but all we can do is establish a kinematic argument for such flows. I would hope the mechanicists could at least tell us if these flows are reasonable, and that is what I hoped they would do.

T J KEMMIS: I would like to provide a partial answer to Dr Glen's question about what to investigate at the intermediate scale. To date we have looked at the scale of the individual process, but not at the intermediate scale of processes in combination. For an example, why not look at processes occurring in combination under temperate ice conditions? We know that under such conditions a large number of processes can take place. And yet we know temperate glaciers do not all produce the same product. Obviously there are different sets of processes taking place in various glaciers. We need to know what sets of processes may occur essentially simultaneously, what may not, and why.

S HALDORSEN: I am always a little disappointed when I listen to lectures concerning very clean ice moving upon a very clean bed. In that connection, I have a comment on the paper of Bernard Hallet. Glacial abrasion does not only include abrasion of bedrock, but also abrasion of material in drift and already deposited till. When you are out in the field, you commonly find that the thickest and most massive tills in many cases are dominated by abraded clast material and such till usually is classified as lodgement till, i.e. till deposited from a sliding glacier. It is quite clear that much of the abrasion occurred *in situ*, just before, during, or after deposition. The very massive structure shows that the material hardly was deposited from debris-poor ice. In such cases I find it difficult to apply the idea that a debris-rich ice is not abrading.

HALLET: I would like to respond to that by saying that it really depends on your perspective. I have worked mostly in alpine areas, and there it is quite appropriate to look at ice that is relatively clean. The field evidence for this is clear. One can look at glaciers that are retreating and, except for terminal moraines where there is a bit of debris, only widely scattered rock fragments can be found on extensive bedrock exposures. Furthermore, on those rock surfaces extremely delicate features (like fragile spicules of subglacial carbonates) occur that would be readily destroyed by pro-glacial waters. By the fact that they are preserved almost intact, you can practically preclude the removal of any coarse debris. Looking at the occasional scattered rock fragments on a pro-glacial rock surface, and knowing something about the sliding velocity of the glacier and the retreat rate, one can figure out concentrations of debris in the basal ice. I can assure you that you have to stretch it to get 1% by volume in some of these glaciers. Now I am not saying that we do not have glaciers with thick debris bases, and I am not even saying that they cannot abrade. In fact one of the intriguing peculiarities of the mechanics of abrasion is that as glacier sliding slows down, the drag imparted by rock debris decreases. Slow sliding appears possible even when the glacier bed is entirely covered with debris. Perhaps this could lead the discussion in another direction. What is it that glacial geologists would like theoreticians to model in terms of the mechanical properties of materials at the base of a glacier? Should we be thinking of a till layer that is very soupy, as I have heard mentioned? What is known about the mechanical properties of this material?

D G CROOT: Following on from Bernard Hallet's comment, I would like to refer to my own work on glacier surges in Spitsbergen. I hesitate to suggest that surging glaciers are everything we want the glaciologist to model, but I would like to say firstly that most of the Pleistocene sequences which the glacial geologists deal with tend to be in lowland areas, not Arctic or alpine high-altitude cirque glaciers or small valley glaciers. Secondly, a simple question I would like to put to glacial mechanists or glaciologists is: how do we explain some of the thrust and fold structures observed at the snouts of surging glaciers? These features are very common, and often include sorted, bedded sediments which are totally undisturbed. I have seen "shear zones" 0.3 m wide with beds completely intact within them, with 30 m or so of ice beneath. I cannot envisage differential movement within that shear zone; it would completely destroy the bedding which cuts across the lineation of the shear zone. If I were to go into a Pleistocene environment and find a till sequence containing such a zone of bedded material, it would be very difficult to interpret in terms of the glaciological conditions that formed it if I had not already seen it in a present-day environment. Having seen it, I can perhaps try and interpret a Pleistocene sequence,

but very often in a Pleistocene situation, as in the Laurentide ice sheet, we have no model on which to work. Perhaps someone would like to try and explain in glaciological terms how this kind of zonation of debris can occur.

M SEPPÄLÄ: We glacial geomorphologists would be very grateful for even more detailed information of the glacial erosion processes under the present ice sheets to help us interpret the features found under glaciated surfaces and to reconstruct the conditions under the past continental ice sheets. There are still too many unknowns in the mathematical formulae to make this possible.

GLEN: I think some of the problems which are involved here are extremely complicated and this is the reason why there are the unknowns. For example, the whole question of surging glaciers is incomprehensible; they do not form a single class. There seem to be surging glaciers of all different sorts, valley glaciers and wide fronts of ice caps. Sometimes there seem to be a lot in one place, as I think in the Pamirs, sometimes there are just one or two. They have posed very many questions, but I think we must contemplate that there may be many different causes of surging glaciers. If that is so, it is very difficult to use the evidence to interpret Pleistocene deposits. To take the particular question, that Dr Croot asked, of how you get folds: one quite good way of getting folds is to have a layer of rheologically different material and compress it. Ramberg (1964) did work compressing samples of differing mechanical properties, and showed that if they differed by more than a certain amount, then when you compressed a planely-layered sample, folds developed. I think Roger Hooke's remark about debris layers strengthening ice may be sufficient reason why if ice is compressed it should then fold.

On the other hand, there are things which we have suggested which I think are capable of experimental tests, but as far as I know have not been proven. For example, over twenty years ago I made some postulations about the way in which stones move in ice as an attempt to understand till fabrics (Glen and others 1957), and suggested that the longitudinal tendency of stones in till was due to the stones undergoing a rolling action, with the stones spending a much longer time in the sub-horizontal position than in the vertical. This certainly is what you would expect from a continuum mechanics theory of a rigid body embedded in a shearing material. If that shearing material was also being compressed, you would expect the axes to be tilted slightly away from the horizontal, very much like the till fabrics found. I am told we never actually see the stones doing the turn-over. I think this is an interesting question and would love to know whether people have ever found stones moving away from this position and over. It ought to be visible; it is only a small fraction doing it, but a small fraction should be doing it. Similarly the transverse maximum was interpreted in that old paper of mine as being due to collisions between stones. It ought to be more common when the stone density is higher. Again, I think some field measurements on this sort of thing would be helpful. Now I am not suggesting that that is the be-all and end-all of till fabrics. I am just saying that that is an attempt I made to try and solve a problem, and I hoped to see more papers which pulled it to pieces than I have seen in the last twenty years.

D E LAWSON: I have measured pebble orientations in glacier ice (Lawson 1979) and found that some pebbles within basal ice do have near-vertical orientations, probably 1 in 200, or 1 in 300. The remainder were very close to horizontal or within 10 to 30° of horizontal.

GLACIOMARINE PROCESSES

DREWRY: The study of glaciomarine sedimentation has involved several disciplines which have remained quite separate in their interests and methodologies. Sedimentologists have looked at the petrographic characteristics of continental and deep-ocean sediments; glaciologists have concentrated on looking at ice and have ignored the sediments; and the oceanographers have been essentially in the background as far as looking at any interactions between ice or the sediments. It is now time that we brought these three fields together to interpret the very large volume of glaciomarine sediments that occur around the world. Some 10% of the world's oceans today have glaciomarine sediments forming in them. During large-scale ice expansions of the Pleistocene, that area was probably doubled to 50 to 60×10⁶ km². For the geologist (to go further back in time) the most likely evidence of former glacials will be materials preserved in the oceans. Subaerial weathering and surface geological processes are likely to strip away most of the terrestrial evidence of former glaciations. The items that I would highlight for essential understanding of processes involved in glaciomarine sedimentation are, first, evaluation of the varying roles of ice masses,

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such as ice shelves, ice streams, and tidewater glaciers. We have little quantitative data in regard to mass flux, velocities, thickness, dynamics, and thermodynamics in general. Secondly, we have little useful information about the sediment content of any of these ice masses that can be used for modelling purposes. Thirdly, we need to understand the physics of melting and freezing and the release of sediments when ice enters shallow water, in the case of the grounding line of an ice shelf, or deep water, as in the case of a calved iceberg. Here, surely, is an area where theoretical glaciology can help considerably our understanding. Let us hope that the pursuance of iceberg conferences can lead to practical investigations along these lines. Fourthly, one of the major problems arising from the discussions at this meeting relates to water issuing from beneath ice streams and tidewater glaciers near to the grounding line. Where does this water go? How much sediment is in suspension? What are the density contrasts that govern whether the water moves up to the surface, along the bed, or is inter-stratified? This problem needs examining in a fjord environment, as well as in large embayments such as the Ross and Weddell seas in Antarctica. It is here that the oceanographers can play an important role so that sediment transport by these waters can be fitted into a unified theory. Fifthly, although we can now provide reasonably realistic two-dimensional models, the next step, and Ross Powell's beautiful three-dimensional diagrams must surely point in this direction, is to consider realistic three-dimensional analyses. Finally, we must look at processes through time. How, for instance, do frontal oscillations and mass variations in ice sheets and glaciers affect the production of sediments in an oceanic environment? I would now like to ask people for their suggestions for future research in glaciomarine sedimentology, which, I believe, will have an increasingly significant contribution.

W H MATHEWS: I would like to respond to your last remark. Having worked in glaciomarine sediments, I find that they can be an extreme source of frustration. Indeed, I have one student that I can think of particularly who just about chucked in the sponge and went back to some other activity because of the difficulty of distinguishing glaciomarine sediments from till. All I could say to give her some kind of support was "join the club". We need criteria: geochemical, fabric, something of this sort, that help us to go to a site and say with some confidence "this is glaciomarine, that is till". I can only cite as an example one that comes very close to home, the excavation for our geology office building at the University of British Columbia, in which three of us who had some experience with both glaciomarine and glacial sediments examined it, proclaimed it glaciomarine, and then did a fabric analysis and found that it was beautifully developed - as well developed as you would get in any till. Criteria are what we need first of all.

O ORHEIM: There was one point I wanted to make concerning David Drewry's very episodic model of iceberg sedimentation. He showed a slide of an iceberg that had rolled 90° to expose a cliff of debris and from this he suggested that iceberg sedimentation was episodic. I would make the point that even if you turn an iceberg on the side you have got approximately 8/9ths remaining underneath the water, so most of the sediments are going to continue to melt from the sides at much the same rate as from the base. Therefore most of the sediment is going to come out at the same rate as before, unless you turn the iceberg 180° so the sediment lies on the top; then for a little while you will not have sediment melting out at all.

I think the grounding line is exceedingly important and we can expect, with a high degree of confidence, that practically all the sediments that are not incorporated in the ice will be deposited at the grounding line. There is good evidence to indicate that for an ice sheet most sediments are not in the ice but underneath it. However these sediments are transported, whether by melt water, traction, or whatever, they are going to be deposited near the grounding line. Changes in grounding-line position, e.g. following sea-level changes, will then move sediments with it.

The thermal effects of grounding ice shelves are also important. When an ice shelf goes aground, the bottom temperature will fall below the melting point. For a glacier that was 200 m thick, with little surface melting and mean annual surface temperature of -20°C, the temperature at the base would typically be -12°C. If we suddenly ground an ice shelf by changing sea-level, then the temperature has to change inside the ice shelf and, of course, bottom freezing occurs incorporating sediment which up to that point had not been frozen. It is almost impossible to calculate the basal freezing rates. People have tried to solve how sediments melt in the Arctic when permafrost is uncovered. That seems almost an intractable problem, and going the other way is even worse. For a

selected combination of sediment and (saline) water, and making some assumptions, we found that if it takes a time, t years, to change linearly from 0 to -12°C, the freezing depth in metres is approximately 0.01t, in other words if the change takes 1 000 years, 10 m of permafrost will form. This suggests that sea-level changes of that sort of order and much longer can actually manage to freeze a considerable amount of sediments beneath an ice shelf. The phenomenon is an interesting one and it would be very useful to see the modellers tackle it and solve the heat-flow problem.

DREWRY: I agree with Olav Orheim that the grounding-line zone is extremely important. Present knowledge shows that the grounding line of large ice shelves is highly complicated. This means that simple two-dimensional models will be inadequate where there is high differentiation of the ice-sheet margin. For instance, the Ross and the Filchner ice shelves are the world's largest ice shelves. On one side of the Ross Ice Shelf we have a range of mountains with ice discharging from the ice sheet in outlet glaciers. On the other side we have an ice sheet which is grounded principally below sea-level where ice flow is differentiated into a series of ice streams and grounded domes and ridges. The ice streams are moving at the order of hundreds of metres a^{-1}. Thermal calculations indicate that the bottom of the ice streams are at the pressure-melting point and are sliding. The intervening ice domes and ice ridges are frozen to bedrock, so flow here is extremely small. Thus the ice that is important for sedimentation beneath the floating Ross Ice Shelf is almost entirely contributed by ice streams. The position of the ice streams in the ice shelf is governed by relative geometry and discharge. If these ice streams are crucial to the ice shelf and hence the sedimentation, we are looking at a zone which is comparable to normal sliding, temperate glaciers having sediments at the bed. Therefore differentiation of the grounding line and the varying processes which operate there make modelling difficult, but the available data provide us with clear boundary conditions and focus upon ideas regarding sediment-water-ice interactions.

MATHEWS: I would like to comment, Dr Drewry, on your discussion of the ice streams coming off from Marie Byrd Land. On the British Columbia coast and also on the Norwegian coast, we find trenches leading across the continental shelves which may very well be an expression of Pleistocene ice streams. The big question I want to raise is "is a shelf a necessary adjunct to these or could these be simply streams coming out and terminating in perhaps the deep water at the edge of the continental shelf?"

DREWRY: The ice streams that we observe in Antarctica are associated, in most cases, with bedrock channels. Some of them are fairly shallow and have a relief amplitude of only a few tens of metres to maybe 100 to 200 m. Usually these channels are eroded by the ice in soft semi-consolidated sediments. In the case of ice confined by rock outcrops, it is certain that outlet glaciers are producing very deep trenches (such as at Byrd and Beardmore glaciers, etc.). These continue for long distances, even continuing out onto the continental shelf. This is common in the Ross and Weddell sea embayments. Channel formation is thus dependent on how transient the ice streams are and how much time there is available to erode channels. There is an enigma, however. We see channels extending over several hundreds of kilometres which relate to the ice streams, but there are some ice streams that have no bedrock channels!

ORHEIM: I do not think you meant to say that ice streams were common in the Weddell Sea. In fact, the bathymetric data show only one large over-deepened channel, although people who have postulated surging, or a collapsing west Antarctic marine ice sheet, have published several channels going out from the Weddell Sea. Therefore our concept of grounding in the Weddell Sea gives ice of fairly uniform thickness instead of highly complicated as in the Ross Sea.

IKEN: My question refers to Dr Orheim's earlier remarks on the temperature at the bottom of grounded ice shelves. I was surprised by your basal temperature estimate. White Glacier on Axel Heiberg Island fits your example nicely. It is about 200 m thick and the mean annual temperature is about -20°C, but the glacier is sliding over the bed. Now of course the glacier is on a slope, so the analogy is not perfect, but bottom temperature appears to depend on velocity. Could this not also be true for ice shelves, so the grounding line might be at the melting temperature instead of cold?

ORHEIM: I do not think grounded Antarctic ice shelves could be sliding. The situation is quite different. There is practically no surface melting and a surface mass balance of 0.5 to 0.6 m a^{-1} brings the cold surface temperature deeper into the ice.

Could I make the general comment that in our attempt to understand glaciomarine sediments

in the Antarctic and parts of the Arctic, we are looking at exceedingly small pin-points on the ocean bottom? We have been fortunate in the Norwegian expeditions to have obtained much seismic evidence together with cores, so we are beginning to reconstruct some kind of regional picture. In the Arctic there are certain areas which have been very heavily studied, but even the most studied areas have nothing like the evidence available above water. I think we have to be exceedingly careful in claiming that, based on a few cores here and there, we know the story. We all recognize this, but it does not hurt to say it again.

SOVIET PAPERS

V KOTLYAKOV: I would like to thank the organizers of this symposium for selecting very interesting and important topics which bring together glaciologists and geologists. I am a glaciologist and I will say a few words from the point of view of present-day glacier investigations. Many of the papers delivered considered new concepts and approaches to the studies of glacier and glaciomarine deposits. The studies of glaciomarine sediments, which incorporate the long history of glaciers, although not yet finally clarified, are of the greatest significance. In particular, there are many data obtained in the western hemisphere and in our country testifying to the existence of vast ice sheets over areas that are now water-covered. In our studies, we proceed from the assumption that a number of methods should be applied simultaneously for the study of glacial erosion and sedimentation. This enables us to conduct some comparatively accurate analyses. The results of such studies are presented in the paper by Serebryanny and Orlov, which unfortunately was not delivered here as the authors are now working on an expedition in Spitsbergen. The balance method occupies the basic position in the investigation of sedimentation and debris transport. For some glaciers of the Soviet Union we have calculated all the components of debris input and output with reasonable accuracy. This is most important for the Central Asian glaciers where, as you have seen from the slides of Dr Chizhov, many glaciers have a very thick debris cover. In this respect, special attention should be paid to surging glaciers in whose regime the debris balance seems to play an important role. One of our theories states that overloading of glaciers by debris may work as the triggering mechanism for surges. From direct surveys, we know now that many end moraines of the Central Asian glaciers, which were recently believed to have formed thousands of years ago, were generated by big surges only a few decades ago. The problems of glacier surges are very important for glacial geology, and that is one of the reasons for their being included in the programme of Soviet astronauts' studies at the Salut 6 orbital station.

Finally, remote radio echo-sounding of glaciers has recently become a very efficient method for studying glacier structure and geological activity, as can be seen from several papers here. We now pay much attention to the development of radio echo-sounding equipment in the same way as does the Scott Polar Research Institute. In the near future we hope to have equipment which will allow us to echo-sound temperate glaciers of various thicknesses and to get reliable data on their internal structure. That is very important for this point of view.

GLEN: There is one thing that impressed me very much in looking at Dr Chizhov's photographs, and that was the way in which his glaciers were not flowing over the full width of the valley. They were flowing through broad moraine walls and outside the moraines there was a further valley with streams flowing along it. My memory is that this is also what I have seen for other Central Asian glaciers on the south side, from Pakistan and India. I wonder if this is itself, perhaps, a feature which is peculiar to surging glaciers? I have no experience on which to say this, and wonder if either Dr Chizhov or somebody else could comment on it because it could be a feature which, if found in relic form, might help us to tell if we were looking at the relics of a surge.

O P CHIZHOV: I cannot answer this question. The valley is very large and was not made by previous ice-age glaciers, but is tectonic. The glacier lies in the bottom of that great valley and the lateral moraines were made by the glacier when it surged and the ice surface bulged. Before the surge, the glacier surface in the lower zone is down-wasted with large elevated lateral moraines. It gives the appearance of the glacier not filling the valley. But during the surge the glacier flows over the greater part of the valley width, and the appearance of the surface is entirely different. I do not know the exact cause.

HALLET: Dr Glen's question is a very intriguing one because some glaciers that are clearly non-surging show this pattern. In fact during his presentation, Dr Small showed a beautiful

picture of Glacier de Tsidjiore Nouve with the moraines clearly encasing the glacier, some distance from the valley wall.
CROOT: The surging glaciers in Spitsbergen which I have examined occupy the full valley width when they surge. However, with a year or two of ablation, because of the distribution of debris-rich and debris-poor ice, one attains a profile which would appear from the air to give the valley-within-valley situation described by Dr Glen and Dr Chizhov. Valley-side streams coming down towards the lateral margins produce erosional valleys, by incising themselves between the valley side and the lateral debris-rich ice margin giving exactly the situation described, although the original surge did fill the valley.

HISTORICAL EVIDENCE OF GLACIAL EROSION AND SEDIMENTATION

D M MICKELSON: I have benefited a great deal from the discussions we have had this week and I think both glacial geologists and glaciologists here have profited from this interchange. One concern that I have, and something that I think glacial geologists and glaciologists should keep *in mind*, *is the temporal aspect of the processes* we have been considering. For instance, if we glacial geologists look at a stratigraphic section anywhere back from the terminal moraine, we are certainly looking at a point in the landscape which has undergone a number of processes during time. In fact the evidence that we have may not be a complete record of the processes that have been on-going. I had a feeling a number of times this week that most of the discussion was revolving around an ice mass that was out at its maximum position and people were trying to look at what was happening in certain places. We really have to keep in mind that the zones are moving across the landscape, and it is only by recognizing that and keeping in mind this temporal aspect, that we will be able to understand from the glacial geology what was happening in the ice mass itself.

I have an intuitive feeling that, for the most part, in marginal zones, sometimes only 100 m wide and sometimes, in the case of ice sheets, 10 km or more wide, we have primary deposition taking place. We can look at some places well back from the margin and see evidence that we have a period of time when deposition took place, a period of time when erosion took place, and then deposition again as the ice margin retreated.

One question that arose in discussion was whether in marginal areas it is possible to preserve very delicate things like organic deposits if the toe of the ice was not frozen. It has been suggested a number of times by glacial geologists that it is necessary to have the glacier frozen to its bed to preserve these kinds of deposits. I would suggest that it is not necessary, and that in marginal areas, whether or not the bed is frozen, you are quite likely to preserve pre-existing organic materials. I would cite the Two Creeks Forest in Wisconsin as a case in point, where we have over a dozen localities, all within a few kilometres of the outer front, and where, because of the [evidence of] surrounding vegetation, beetle remains, and a variety of other things, we must have had warm-based ice.

KEMMIS: It seems to me that many of the problems discussed here arise from the incomplete and inadequate questions asked and the lack of perspective given to the answers. The shear zone-debris band problem discussed all week is a case in point. A very simple question is asked: "does shear take place in these zones?" as if that is the only factor involved. People then answer this question with a simple yes or no based on field observations or laboratory calculations. But the problem is greater than simply "does shear occur?" There must be a host of factors affecting whether or not shear may take place in the ice, factors such as thickness of the debris band, spacing of the bands (if important), particle size and concentration in the debris band, ice dynamics (i.e. velocity, thermal regime variations at the bed, ice thickness, etc.), and so on. Until each of us includes information on these factors with our yes or no, that is, until we provide a perspective for our answer, we will never be able to satisfactorily answer such questions as: "does shear take place in these zones, and if so, what factors are important to its occurrence and where is such shear likely to occur?"

The same inadequacies in outlining a problem apply to the study of glacier beds comprised of sediment. A question was asked earlier, should we model till beds as "muddy"? But till beds are much more complicated than that! Tills may vary greatly in particle size and distribution from the very cobbly tills at Hardangerjøkulen to the matrix-dominated tills of the Laurentide ice sheet in midwestern U.S.A. So particle size is an important factor to consider in modelling. Moisture content and consolidation characteristics (in the geotechnical sense) are additional factors to consider. Until our questions include analysis of all the important factors it will be impossible

to arrive at meaningful, comprehensive answers.

A final example of not considering all the relevant factors is that of the preservation of organic deposits and buried soils, just mentioned by Dave Mickelson. Organic deposits or buried soils may indeed be "soft" when subjected to a triaxial test. And one way to preserve such material might be to freeze it beneath the "cold" snout of a glacier, thus increasing its strength. But is shear strength the only variable which is important? Organic soils commonly have low permeabilities and virtually no discontinuities such as joints. They therefore are probably very difficult to pluck and may persist as easily beneath a thin film of melt water under a temperate glacier as under a frozen toe. So, until we ask questions that encompass all of the factors relevant to a problem, and until we put our answers into the perspective of the system from which our answers came, it will be very difficult for us to understand the spectrum of conditions over which different glacial processes may occur.

J EHLERS: I should like to ask a few questions which might be answered one way or the other by the glaciologists present here. One of the *main questions seems to be: how did the inland ice manage to get so rapidly from the centres of the glaciation to the marginal areas*, for instance from Scandinavia to north Germany? If we estimate the time the inland ice had to travel this distance, we see that it must have proceeded with an average speed of roughly 50 to 100 m a^{-1}. Allowing for halts, it may have advanced at an even faster speed. During its advance many interesting things happened and some of these created the phenomena Wickham and I described this morning. The ice dynamics are by no means solved yet. Did the processes which were envisioned by Boulton play an important role? While I think they offer a very interesting solution to the speed question, the field evidence makes it unlikely that they played such an important role as in Iceland where he made his observations (Boulton 1979).

Another important question may be: what happened to the melt water? In some cases it was confined to tunnels under the ice and led to the creation of tunnel valleys. Mickelson told me that in the Laurentide glaciation this process took place during the whole Pleistocene whereas in our northern European glaciation it was restricted to the Elsterian, our oldest glaciation. In the discussion, Röthlisberger has offered a possible solution to that problem by suggesting that during the later glaciations we had permafrost in the region, and thus no such in-cutting could occur. That point is worth discussing: during the Saalian and Weichselian (Illinoian and Wisconsinan) glaciations the process of sheet-like accumulation of melt-water sands prevailed, while the incision of channels or tunnel valleys took place on a much smaller scale or was completely missing. Without permafrost it would be hard to solve the question of how this sheet-like sand accumulation could take place. In permeable deposits, as they occur in northern Germany, water either infiltrates into the ground or collects in streams. Run-off which is not concentrated in streams only happens on impermeable ground. Such were the conditions when permafrost prevailed, and consequently the sheet-like sand deposits could be formed. These are some of the questions which still remain for me.

HALLET: I think some of those questions are going to remain for all of us, but it seems to me that you have really pointed to the kind of information useful to theoreticians in terms of modelling the subglacial system. If you have tunnel valleys and if much of the subglacial drainage is actually concentrated in these valleys, then it is quite a different problem from considering subglacial drainage by a uniform flow of water through a porous till. If we are trying to understand the subglacial water pressure, and especially the pore pressure in a till, which figures in a very important way in determining whether or not till will deform, it is quite critical that we be able to say something meaningful about the flow of water. So the tunnel valleys are beautiful evidence that you are dealing with channellized water very much as we would expect on a completely different scale from some of the work on valley glaciers.

One thing that I found very interesting about your map of tunnel valleys is that if you took that map and reduced it by two orders of magnitude, you would get almost exactly the same type of map that we have compiled for bedrock areas in front of a present-day glacier. The drainage networks appear irregular with non-arborescent and non-converging channels.
GLEN: I was very impressed by Dr Ehlers' figure of 100 m a^{-1} and wonder if he could say whether this is a firm figure and also whether it applies to both kinds of advance, the one with channels and the one without.
EHLERS: It refers to the advance without big channels and is an estimate, not a firm figure. I have taken into account the Weichselian (Wisconsinan) interstadial deposits in Sweden and

Norway, and if you do that you see that the ice had to advance to northern Germany and to retreat again with that very high speed.
GLEN: I think it is important to get a feel for these figures because they give people who are attempting to model past ice sheets something to go on, just as the models can feed back and tell people what to look for. If it is something like 100 m a^{-1} advance, then we are talking about something which is much more like a surging-glacier advance than anything else we look at today. Perhaps some of these questions about what surging glaciers do become more relevant. Of course, it is not a valley-glacier advance we are talking about, but on a broad front. Some of the features which are most distinctive of current surging glaciers are, I suspect, due to the fact that they are valley glaciers. But it does give added impetus to looking at what glaciers which are advancing at speeds of the order of 100 m a^{-1} are doing.
GOLDTHWAIT: There is one answer to John Glen that he may not be aware of. We are possibly in the most fortunate position in Ohio to determine how fast the last Wisconsin glacier which reached Ohio was moving. We have a combination of two things: wood just beneath the drift, still more or less in place, and wood in the lowermost till of the Wisconsin time. I have had this wood dated at over 50 places and calculated the net rate of advance between half a dozen points down each of two lobes. The rates vary between something on the order of 20 m a^{-1} and a little in excess of 100 m a^{-1}. The lowest figure is for the outermost portion of the advance, when it got almost to Cincinnati (Goldthwait 1958). The controls are figures from the north side of Lake Erie which come largely from the area of Dreimanis' work, from Cleveland, from intermediate central Ohio, and at the terminal position. Depending on the lobe, the terminus was reached at around 21 500 BP for the outermost moraine.
MATHEWS: Dr Goldthwait, you are not the only people who have wood. What I will come out with is almost a repeat of what you said. In the Vancouver area, we have evidence that the ice was still somewhere to the north of Vancouver about 18 000 BP. It moved southward to the southern end of the Puget Sound lobe, a distance of 350 km, by some time between 15 000 and 14 000 BP. In other words, the average velocity was just about 100 m a^{-1}. If you want figures, John, there you have it. Like Dr Goldthwait's, that is a pretty firm number.
HOOKE: The hypothesis of instantaneous glacierization, presented by Ives and Andrews, is perhaps pertinent to this discussion. Snowbanks and snow-patches in hollows gradually expand because summer melt is insufficient to remove all the snow in such hollows. Thus instead of ice from the northern side of Lake Erie, in Dick Goldthwait's situation, actually having to advance across the lake and move all the way down through Ohio, ice could have been formed in Ohio and in the Lake Erie basin by accumulation in pro-glacial snow banks, and the glacier grows in place.
GLEN: Surely they did not claim it at the southern margin? They claimed it for the middle, at the beginning.
HOOKE: That is correct. They claimed it for the central part. But a similar process contributed to the advance of temperate valley glaciers under the climatic conditions found in northern Scandinavia in the early 1900s (W Karlén, personal communication).

Another very brief comment: moraines formed in localities where ice did not expand when it came out of narrow valleys, but instead continued to go straight, are quite common along the east face of the Sierra Nevada, in California. I do not think anybody has proposed that any of those glaciers were surging glaciers, but this is a fairly common moraine form.

Several people have made comments about the interchange that does not take place between modellers and glacial geologists. The modellers were accused of ignoring the glacial evidence. I plead guilty to some of that, but at the same time I think that it is incumbent upon the glacial geologist to make a real effort to understand glaciological theory. Not necessarily so they can apply it, but so they know the important parameters to measure, and what critical observations could be made to test the theories. You do not have to be a theoretician or modeller to look at the model, see what the assumptions are, and try to develop experiments that can test some of these models. The interchange obviously has to take place in both directions.
SHAW: For a moment we were back to talking about the stability of large ice sheets and surges, and got away from historical evidence of glacial erosion and sedimentation. Maybe we should stick to that for a moment. This morning Dr Glen said in response to Mr Dardis' lecture on drumlins that he did not believe in the convection theory and that it was inappropriate to apply to landforms. Dr Glen referred us back to geomorphologically-based and geologically-based theories which some geomorphologists find wanting. We are just throwing the baton back-

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wards and forwards. Could Dr Glen tell us how
he thinks we might cooperate to solve such
problems as the origin of drumlins? As far as
I know, no theoretician has modelled that
problem in detail?
GLEN: My reason for saying what I said this
morning was precisely the point which Roger Hooke
was making a moment ago, that we need to make
sure that the postulations made by glacial geo-
logists are physically reasonable. I do not
believe it is physically reasonable to have
convection, by which I mean a flow of ice driven
in a vertical cell by the differential tempera-
ture and therefore different buoyancy of ice, on
a scale of a drumlin field. I am not sure it
is reasonable anywhere, but I am prepared to
argue it about Antarctica. I am not prepared
to argue it about the drumlin field, because I
am absolutely sure that if you work out the
Rayleigh number you are going to find that
convection is quite out of the question. The
viscosity of ice is far too high for the very
small temperature differences to be able to
drive something on this small scale. My purpose
was to say I do not think we understand sufficient
about the other possible theories of drumlins to
rule them out of court and do a Sherlock Holmes
argument, which I think is what you are talking
about. "When you have considered all the other
possibilities, the one remaining, no matter how
improbable, must be true." I do not think we
have reached that point. I do think we want to
get information about what a drumlin is. Is it
a uniform body? Is it something which always
has a similar structure inside? Or is it only
something which has a similar shape or form, but
in different places has very different internal
structures? I rely on the geologists to tell
me the answer to those questions. We can only
explain drumlins if we know what exactly it is
we are explaining. If they do consist entirely
of till material, then certain theories are
possible. If they do not, or if inside them
there are sometimes complicated structures, then
perhaps we should say that the similarity between
things with differing structures shows that the
form has been developed by the glaciers on
various other different structural situations.
Therefore, as some people have suggested, they
may be an erosional form, not a depositional
form, an erosion of deposits. I am not an
expert on drumlin theory, but I know some of
these ideas are going around. Some people say
it is due to the rheology of the till material
when it has water in it. Some people say it is
due to certain areas of the bed being frozen
and other areas unfrozen. Some say it is due
to deposition around some kind of knob, be it a
rock knob, a previous till knob, or even a bit
of frozen ground. These seem to be the things
we can postulate about. I am not a drumlin
theorist, but those are the questions we should
be debating, not what I believe to be physically
impossible solutions.
SHAW: Let me, as a dull Dr Watson in the presence
of the rather eloquent Sherlock Holmes, try to
express my problems. Roger Hooke is saying that
the glacial geologist or the glacial geomorpholo-
gist has to be able to handle the mechanical
theory. To establish that rather complicated
flows are required to form drumlins took four
years of intensive geological work. To ask us
at the same time to handle the mechanics in an
original fashion is too much. In fact when I
spoke to people about this problem here they
said, "Ah yes, that is a very difficult problem".
Now I wish that someone who feels that this is a
difficult problem would take it on.
ORHEIM: My comment goes back to convection theory
and also to those glacial geologists who have
liked to invoke the concept of surging to explain
anything they cannot explain by any other means.
I would make a plea that glacial geologists and
glaciologists, when trying to explain phenomena,
should never invoke a physical process they
cannot demonstrate happening today. We know,
for example, some glaciers are surging; we know
nothing about surging of large ice sheets. Yet
that is a very heavily discussed topic. There
are other processes where one goes into the
esoteric, along the lines of what John Glen was
saying. Any model that cannot be tested in the
field should not be used.
SOME HONOURABLE MEMBERS: No. No. No. Shame. Poor.
Never.
ORHEIM: I was not trying not to be provocative.
I do think though that it is very important
that you try to test models and, as Roger Hooke
was saying, a model should clarify your thinking.
You do not have to understand the model perfectly,
but a good model focuses your thinking and
improves field experiments. For example, we
have discussed bed roughness for 20 years, since
Hans Weertman's paper on sliding, yet exceedingly
few glacial geologists or glaciologists have gone
out and measured it. There are many obvious
field tests to check whether models are applicable.
The mathematics for the modeller gets more in-
tractable as you get field evidence, but that is
not the fault of the glacial geologist. He can
turn the problem back to the modeller.
D A FISHER: I want to point something out that

comes from ice-core work whenever ice cores have
penetrated the layer that was dropped during the
last glacial period, the Wisconsin or Würm.
There are some unique characteristics of this
ice that suggest that the type of ice you are
dealing with as an ice sheet might not be quite
the same stuff you are looking at today on the
surface. The unique characteristics are that
Wisconsin ice is very dirty in microparticles,
500 to 600 parts per billion by weight, and the
ice crystals are an order of magnitude smaller
than the ice crystals in the Holocene or in the
pre-Wisconsin. The third characteristic which
seems to be unique is that the Wisconsin ice is
rheologically softer. There are measurements
from the Barnes Ice Cap on Baffin Island, the
Devon Island ice cap, and the Agassiz ice cap on
Ellesmere Island which show this. On the
Barnes Ice Cap, where the drill hole goes
through Wisconsin ice it bends more quickly. In
the case of Devon Island and Agassiz, where we
measured bore-hole closures, one can pick out
the ice due to the Wisconsin glaciation because
the bore hole closes much faster; this means
that in various forms of creep the ice is weaker.
Therefore the modellers may want to consider a
different rheology, other than just due to temp-
erature, for old ice.
LAWSON: We have been discussing the difference
between the way glaciologists and glacial geolo-
gists are looking at problems. My training is
more as a sedimentologist, and I see the problem
as something that spans the two. Really what
we are looking at is a complex sedimentary
environment. For the glacial geologists to
interpret their sediments, they need criteria
based upon the characteristics of debris forming
in active glacial environments, with their rela-
tionship to debris properties and the glacier's
mechanisms of entrainment, transport, and deposi-
tion well defined. Investigators of active
glaciers, as well as theoreticians, must present
their results in these terms so that glacial
geomorphologists can go from the sediments as
they find them and interpret, as fully as possible,
the glaciological and sedimentological mechanisms
that formed them.
HALLET: Considering the range of interest in
glacial erosion and deposition, which spans from
very idealized theoretical glacial mechanics to
very detailed unravelling of complicated glacio-
stratigraphic relations, it is not surprising
that, at times, communications may be difficult
between scientifically distant researchers in
this field. This conference, however, has gone
a long way in the direction of sharing central
ideas, delineating key problems that remain to
be solved, and hopefully catalyzing collaborative
work in research on glacial erosion and deposi-
tion. Thank you very much for your participa-
tion in the discussion.
L W GOLD: We have now come to the end of a
successful symposium and five full days of
interesting contributions and discussions. I
would like to take this opportunity to thank on
your behalf the individuals that made this
possible. First, the Papers Committee under
Garry Clarke. This includes David Drewry,
Dick Goldthwait, Bernard Hallet, Hans
Röthlisberger, and Johan Ludvig Sollid. To
them I express our appreciation for putting
together an excellent programme. They also are
working hard as Scientific Editors, along with
Ailsa Macqueen, the House Editor, in putting
together the proceedings of this meeting. Next,
I want to express our thanks to those individuals
that made this all possible: the local arrange-
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Baker from our Cambridge office and Annemor
Brekke from the Norsk Polarinstitutt. And
behind all of them, of course, has been Hilda
and to her I give our special thanks. I wish
also to recognize and express our appreciation
to the Norsk Polarinstitutt and the University
of Oslo for joining with the Society in sponsoring
and running this symposium.
    Finally, I want to thank all of you for
your participation, particularly the authors for
their clear presentations within the allotted
time so that there was a good opportunity for
discussion. There was a real meeting of minds
during those discussions and I know they are
going to continue this evening, during the post-
symposium tours, and whenever there is an
opportunity to meet in the future.

REFERENCES
Boulton G S 1979 Processes of glacier erosion
    on different substrata. *Journal of
    Glaciology* 23(89): 15-38

Glen J W, Donner J J, West R G 1957 On the
    mechanism by which stones in till become
    oriented. *American Journal of Science*
    255(3): 194-205
Goldthwait R P 1958 Wisconsin age forests in
    western Ohio. I. Age and glacial events.
    Ohio Journal of Science 58(4): 209-219
Goldthwait R P 1960 Study of ice cliff in
    Nunatarssuaq, Greenland. *US Snow, Ice
    and Permafrost Research Establishment.
    Technical Report* 39
Haynes F D 1973 Tensile strength of ice under
    triaxial stresses. *Cold Regions Research
    and Engineering Laboratory. Research
    Report* 312
Hooke R L 1973 Flow near the margin of the
    Barnes Ice Cap, and the development of
    ice-cored moraines. *Geological Society
    of America Bulletin* 84(12): 3929-3948
Hooke R L, Dahlin B B, Kauper M T 1972 Creep
    of ice containing dispersed fine sand.
    Journal of Glaciology 11(63): 327-336
Hooke R L, Hudleston P J 1978 Origin of
    foliation in glaciers. *Journal of
    Glaciology* 20(83): 285-299
Hudleston P J 1977 Progressive deformation and
    development of fabric across zones of shear
    in glacial ice. *In* Saxena S, Bhattacharji
    S (eds) *Energetics of geological processes.*
    New York, Springer-Verlag: 121:150
Lawson D E 1979 A comparison of the pebble
    orientations in ice and deposits of the
    Matanuska Glacier, Alaska. *Journal of
    Geology* 87(6): 629-645
Peterson D N 1970 Glaciological investigations
    of the Casement Glacier, southeast Alaska.
    *Ohio State University. Institute of Polar
    Studies. Report* 36
Ramberg H 1964 Note on model studies of folding
    of moraines in piedmont glaciers. *Journal
    of Glaciology* 5(38): 207-218
Shreve R L 1972 Movement of water in glaciers.
    Journal of Glaciology 11(62): 205-214