JOINT MEETING

of the

BRITISH GLACIOLOGICAL SOCIETY, the BRITISH RHEOLOGISTS' CLUB and the INSTITUTE OF METALS

This meeting was held at the house of the Institute of Metals and the Iron and Steel Institute on 29 April 1948. Members of the Iron and Steel Institute were invited to attend.

At the afternoon meeting Mr. G. Seligman showed a film illustrating the methods of glacier research in the field used by the Jungfraujoch Glaciological Research Party of 1938. He also dealt briefly with the next stage of his researches on the growth of the glacier crystal—the development of large crystals in the tongues of glaciers and in dead ice. Sir Arthur Smout, President of the Institute of Metals, presided.

At the evening meeting Dr. E. Orowan, F.R.S., and Dr. M. F. Perutz of the Cavendish Laboratory, Cambridge, opened a discussion on "The Flow of Ice and of other Solids." At this sitting Dr. Richard Seligman, Past President of the Institute of Metals, took the chair.

The Chairman: Before I invite the speakers to address you, I ought perhaps to explain the reason for my presence in this position. A great many years ago my brother was explaining some of his work to me and the complete analogy between what he was telling me and some of the work which Dr. Pearson had done and exhibited to the Institute of Metals during my Presidency seemed to me to call for a joint effort on the part of the glaciologists and metallurgists. I therefore brought my brother into contact with Dr. Desch who, with his inexhaustible knowledge of these things, was, in my opinion, best able to guide him.

Since then the close connexion between the two fields of work has always been apparent. In 1938 I spent some days on the Jungfraujoch with the team of which you saw pictures this afternoon. Once more I was able, if my memory serves me correctly, to draw attention to the bearing of current metallurgical work on some of the glaciological problems under investigation.

It can be for no other reason than these gentlemen have done me the honour of asking me to preside over their meeting to-day. With these opening remarks I will now call upon Dr. Perutz to open the discussion instead of Dr. Orowan, because, as Dr. Perutz put it, he will describe the problems and Dr. Orowan will later tell you how they are solved!

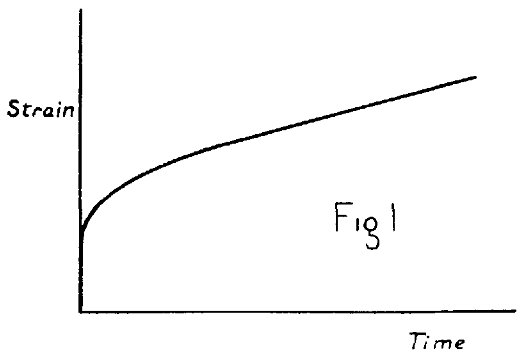
Dr. M. F. Perutz opened the discussion by reviewing some past and present theories of glacier flow. His remarks were largely based on his "Report on certain problems relating to the flow of glaciers." * Dr. Perutz showed that the viscous flow theories of Somigliana and Lagally had become untenable, because it had been discovered that ice does not behave as a liquid of constant viscosity; on the contrary, its viscosity decreases as a high power of the applied shear stress. The sensitivity of the flow rate to slight changes in the thickness of the glacier could thus be explained. The speaker briefly described the recent extrusion theories of Streiff-Becker † and of Demorest ‡ and concluded by showing some photographs by Winterhalter (unpublished) of the behaviour of thin ice strips under sustained tensile stresses.

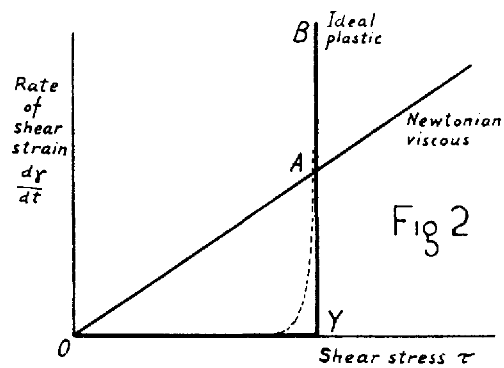
Dr. E. Orowan: Several characteristic phenomena of glacier flow (for instance, the tendency of glaciers to form terraces and to excavate hollows in their beds) appear unexpected, because expectation is based on everyday experience with the flow of viscous materials. Like all crystalline solids, ice is not a viscous but a plastic material; its law of deformation is not a proportionality between the shear stress and the rate of shear strain. The laws governing the deformation of

* *Journal of Glaciology*, Vol. 1, No. 2, 1947, p. 47.
† *Journal of Glaciology*, Vol. 1, No. 1, 1947, p. 12.
‡ *Journal of Geology*, Vol. 46, 1938, p. 700.

plastic materials are more complex; a first approximation is represented by a functional relationship between shear strain and shear stress, examples of which are the well-known stress-strain curves of metals. The main feature of these curves is that the strain is vanishingly small up to a more or less sharply defined value of the shear stress, and then increases rapidly with further increase of the stress. In the mathematical treatment of plastic stress and strain distribution problems, it is usual to make the idealizing assumption that plastic deformation sets in at a sharply defined value of the shear stress (the "yield stress"), and that the shear stress remains constant at this value in the course of further deformation.

The stress-strain curve approximation cannot account for flow (or "creep") phenomena because time does not occur in it. If the stress-strain curve were an exact law of deformation, the strain would reach its final value (corresponding to the applied stress according to the stress-strain curve) as quickly as the stress is applied. The real behaviour of crystalline solids, however, is different. The application of a stress is followed by a very rapid deformation; this, however, does not stop instantaneously, but goes over into a rapidly decelerating flow, the rate of which converges asymptotically towards a more or less constant value (Fig. 1, below). This phenomenon was first analysed by Andrade who discovered that the decelerating component of the flow ("transient creep") is physically different from the constant-rate flow ("quasi-viscous creep") that remains



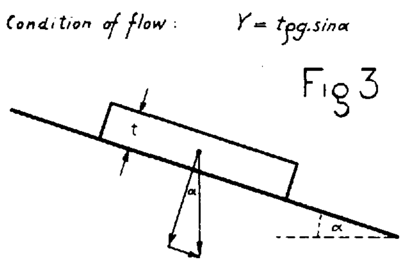


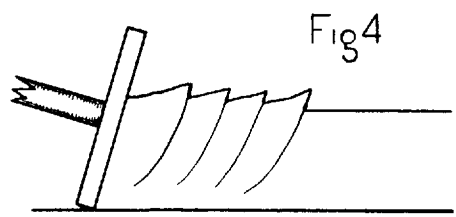
when the transient flow has died away. Transient flow is considerable only after a relatively rapid change of the stress; we can disregard it in dealing with the simplest features of glacier flow.

Experiments with metals have shown that the quasi-viscous component of flow is negligibly small below a certain value of the shear stress, provided that the material has not been strongly distorted before the flow experiment; the quasi-viscous flow rate increases very rapidly as the stress is raised above this critical value. If the rate of the quasi-viscous flow is plotted as a function of the shear stress, curves of the type of the dashed curve in Fig. 2 (above) are obtained. For dealing with a few simple phenomena of glacier flow, this curve can be replaced by one consisting of two straight lines: a horizontal line from the origin to the yield stress Y, and a vertical YB line representing a constant stress Y for any velocity of flow. In the same graph, Newtonian viscosity would be represented by a straight line OA passing through the origin (the reciprocal of the slope of this line is the coefficient of viscosity). It is seen that the flow properties of ice cannot be characterized by a more or less constant coefficient of viscosity; in a narrow stress interval the rate of deformation increases from zero to very high values, and so the stress/strain-rate ratio may have almost any value, according to the strain rate at which the measurement has been made. The very rapid increase of the flow rate with the stress is the cause of the phenomenon mentioned by Dr. Perutz that the Aletsch glacier flows twice as fast in winter as in summer: the slight increase of the weight of the glacier by the winter snowfall, amounting to about 1 per cent., can raise the flow rate by roughly 100 per cent., in spite of the slight decrease of the temperature.

The preceding considerations refer to the simple case of uniaxial (*e.g.* simple tensile or compressive) loading, or to a pure shear stress. In glaciers, the state of stress is bi- or tri-axial, and the question arises how this affects plastic flow. According to a general theorem, any state of stress can be considered as a superposition of pure shear stresses and a hydrostatic (isotropic) stress. It is an empirical law of great importance, the generality of which can be recognized from theoretical considerations, that a hydrostatic pressure or tension cannot influence plastic deformation considerably unless its value is very high. Very high pressures, however, never occur in glaciers; even an ice cap of 3000 m. thickness would only produce a pressure of about 300 bars, *i.e.* only about 1 per cent of the pressures commonly used in Professor P. W. Bridgman's laboratory. It can be said, therefore, that in practical cases the occurrence of plastic yielding is determined entirely by the shear stresses and is not influenced by the hydrostatic stress unless this produces some structural or textural change (*e.g.* extensive partial melting). In particular, there is no reason to expect that ice would become more plastic under hydrostatic pressure; according to Bridgman's experiments, the very slight influence of the hydrostatic pressure is in the opposite direction (it raises the shear stress needed for plastic yielding) if other factors do not change with the pressure.

However much the model of a glacier is simplified, the mathematical problems of its plastic





flow remain difficult to solve. At present, the easiest approach is to consider a few simple cases of plastic deformation under gravity forces, and to apply the results to glacier flow.

The simplest case is that of a slab of ice on a sloping plane (Fig. 3, above); it should be regarded as frozen to the slope, so that it cannot slide down except by internal plastic shear. This corresponds to the condition of ice lying on the rough glacier bed. The length and width of the slab should be so large compared with its height that the plastic flow that would occur if it were placed on a horizontal plane could be neglected (*cf.* below).

An elementary consideration shows that the highest shear stress occurs in the bottom layer of the slab; in order to produce a slow downslope movement, the thickness of the slab must satisfy the condition

$$Y = t\rho g \sin \alpha \qquad (1)$$

where Y is the yield stress of ice in shear, α the slope angle, t the thickness of the slab, g the gravity acceleration, and ρ the mass per unit volume. To every slope angle, therefore, there is a definite thickness at which the slab is in "plastic equilibrium."

The slope angle changes in a glacier from point to point, and the thickness of the ice, obviously, cannot vary so as to satisfy the condition (1) at every point of the glacier (in the case under consideration, the glacier will be regarded as infinitely extended laterally; edge effects will be neglected). Even if, at a certain moment, (1) were satisfied, it could not be so a short time later, when the ice has moved on to points of different slope angles. Those parts of the glacier whose

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thickness exceeds that demanded by (1) will exert a pressure or tension upon those the thickness of which is deficient. Thus a longitudinal pressure (or tension) arises, varying from point to point along the glacier, and this pressure keeps the various parts in simultaneous movement. It is easily seen that the longitudinal compressive force F which acts across a cross-section of the glacier is determined approximately by the relationship

$$F = \int_0^x t\rho g \sin\alpha \, dx - Yx \qquad (2)$$

where Y, ρ, g, and t have the meanings explained above, and x is the distance of the section from the top of the glacier (from the bergschrund). In obtaining this expression it is assumed that the change of thickness of the ice per unit length of the glacier is less than the change of height of the glacier bed (*i.e.* the gradient of the bed).

The two terms on the right hand side of (2) have very simple meanings. The first term is the longitudinal force that the parts of the glacier between the bergschrund and the point x would produce if the glacier moved in its bed without friction; the second term is the force needed for moving the part of the glacier between the bergschrund and the point x against the plastic resistance given by the yield stress Y.

If a high gradient is followed by a long stretch of low gradient, the additional longitudinal force required for pushing the glacier along over the low gradient part may be so large that it exceeds the resistance of the ice to longitudinal compression. In such cases longitudinal compression of the glacier takes place, and its thickness increases until the cross-section becomes large enough to transmit the longitudinal force without the longitudinal yield stress of the ice mass being exceeded. For a reason that is not sufficiently understood, the horizontal compression of ice and snow lying on the ground does not take place uniformly; it involves the formation of "thrust planes" the occurrence of which in glaciers has been investigated particularly by Mr. Lewis. The formation of such thrust planes is seen whenever a snowfall is cleared away by pushing tools (Fig. 4, p. 233). It is a characteristic feature of glaciers whenever they come to a longer stretch of low gradient, as is often the case at the end of the glacier. Once formed, such thrust bands appear to persist to the end of the glacier.

The existence of the longitudinal force explains why a glacier can move uphill for a moderate distance; most mountain lakes in formerly glacierized regions are in hollows excavated by a glacier in partly uphill movement.

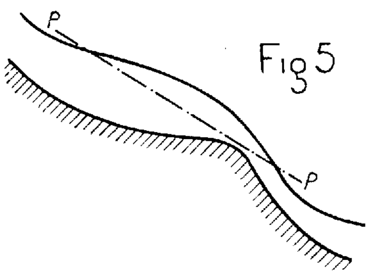
A very interesting case arises when a region of small slope angle is followed more or less abruptly by a steeper gradient. Without a rapid increase of the gradient, the steepest plane along which plastic shear could take place would be the tangent of the glacier bed; any steeper plane would intersect the glacier bed and, therefore, could not act as a shear plane for a simple geometrical reason. If, however, a low gradient is followed by a high one (Fig. 5, p. 235), planes steeper than the glacier bed underneath (*e.g.* P—P) become possible as shear planes, and plastic deformation may take place within the entire volume of the glacier ice just above the sill, not only in its lowest strata. On the steep gradient below the sill, on the other hand, the ice moves almost as a rigid block, as in the case of the slab on the sloping plane in Fig. 3. Since the amount of ice flowing through a cross-section per unit of time is the same above and below the sill, the wear of the bed will be much heavier on the steep gradient where the shear is concentrated in the bottom layer. This would explain the general tendency of glaciers to transform their bed into a succession of terraces. This effect must not be confused with a superficially similar phenomenon which is the formation of "*roches moutonnées*." The steep walls on the downhill side of such rocks seem to result mainly from tensile fracture by the ice load on the top of the rock due to the fact that its downhill flank is not in contact with ice and so not supported by pressure.

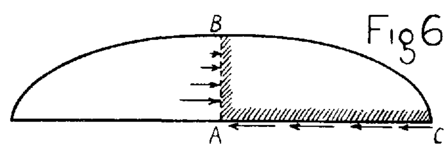
A second simple case of plastic deformation under gravity forces is that of a slender ice column standing on a horizontal plane. Let Y_c be the yield stress of ice in compression (Y_c is about twice the yield stress Y in shear mentioned above). If the height of the column exceeds the value h determined by

$$Y_c = g \, . \, \rho h \qquad (3)$$

plastic yielding occurs at the bottom of the column and the ice is squeezed out sideways, until the height of the column is reduced to that given by (3). For the rates of flow which occur in glaciers, Y_c seems to be between 1 and 3 bars (approximately between 1 and 3 kg./cm.[2]), so that the critical height of the ice column would be between 10 and 30 m.).*

The conditions are very different if the block of ice, although of a height exceeding the value h given by equation (3), is not a slender column but an extended slab the horizontal dimensions of which are comparable with, or larger than, its height. The plastic extrusion of a bottom layer is then opposed by the shear stresses acting between this layer on the one hand, and the horizontal base, as well as the unplastic upper part of the block, on the other hand. Let it be assumed first that the plastic deformation would consist, as in the case of the slender column, in the extrusion of a bottom layer from under the slab. This process would now be opposed by the shear stresses acting against the extrusion between the bottom layer and the horizontal base, and those between the bottom layer and the supposedly rigid upper parts of the slab. In order to overcome these, the





pressure of the rigid upper part would have to be much higher than the yield stress of the ice in compression; an easy calculation shows that the extrusion of a bottom layer is altogether impossible when the width of the slab is hundreds of times larger than its height, as in the case of the Greenland and Antarctic ice caps. Here the plastic flow must extend practically over the entire volume of the slab. The problem of the distribution of stress and strain in a plastic ice cap has not been solved yet; it is easy, however, to estimate the height which the mass of ice must have at its centre in order to flow in its entire volume. Let Fig. 6 (above) represent a cross-section through the cap which is assumed to be a long ridge (perpendicular to the plane of the figure), rather like the Greenland ice cap, and let AB be the longitudinal symmetry plane. In the plane AB there is no shear stress; the normal pressure is equal to the hydrostatic pressure head $\rho g z$, where z is the depth below the surface. If the height of the cap in the middle is $h = AB$, the total horizontal force across the plane AB, per unit length of the ridge, is $\frac{1}{2}\rho g h^2$. Since the part ABC of the ridge is in equilibrium (it is not accelerated) the horizontal forces acting upon it must cancel. Besides the hydrostatic pressure across the plane AB, the only horizontal force is that due to the shear stress acting in BC. In plastic equilibrium, this shear stress is very nearly equal to the yield stress Y in shear (apart from end effects), and so the condition of equilibrium is

$$\tfrac{1}{2}\rho g h^2 = YR \qquad (4)$$

* This seems to be the reason why crevasses in temperate glaciers are never deeper than 30 or 35 m., as mentioned in the discussion.

where R ($=AC$) is half the width of the ice ridge. It was mentioned above that the yield stress of ice in compression is probably between 1 and 3 kg./cm.^{2}; if the mean value 2 is assumed, the yield stress Y in shear is 1 kg./cm.^{2}, and this gives, with the mean half-width $R=6.10^7$ cm. of the Greenland ice cap, a height $h_0=3\cdot7.10^5$ cm. at the centre of the cap. This is of the correct order of magnitude; the actual maximum height is $3\cdot3.10^5$ cm., from which, however, a certain amount has to be deducted to allow for the effective height of the coastal range over which the ice flows. If the land surface rises towards the interior, a slope effect (represented by equation (1)) arises in addition to the spread effect. With a thickness of, say, $2\cdot5.10^5$ cm., and $Y=10^6$ dyn/cm.^{2}, a gradient of about 1 in 220 would maintain the flow according to equation (1). The slope effect represented by equation (1) and the spread effect to which equation (4) refers are not essentially different phenomena; the spread can be imagined as a downslope movement of the ice over slip surfaces situated within the ice mass.

Mr. W. Vaughan Lewis (Department of Geography, Cambridge): I should like to thank Dr. Orowan and Dr. Perutz on behalf of those who study the land forms produced by ice. The interest now shown by physicists and metallurgists in the movement of glaciers promises completely to transform the study of glacier motion. Much dubious mechanics has crept into the literature and geomorphologists will be grateful to the physicists for help in clarifying these matters which they are so much better fitted to judge.

Mr. W. H. Ward suggested in the discussion on a paper by Mr. G. Seligman * that the rotational slipping of clay on unstable slopes may have analogies with the movement of glaciers. This linked up with observations I had made on overthrusting in glaciers in Iceland and Norway. In 1946 and 1947 I was able to make further investigations. It seems that a simple cirque glacier with a surface gradient of the order of 15 degrees moves partly by rotational slipping along a series of glide planes. (Mr. Lewis then showed some slides including some of the Skauthöe cirque, Jotunheimen, Norway, Fig. 7, p. 282).

The cirque glacier at the Juvashytte ends on the lake side at an ice wall roughly parallel with the direction of flow of the ice. This reveals a convenient section showing the annual layers of accumulation to the lee of the north-facing boundary wall. The winter snows pile high in the angle between the rock wall and the *névé* surface, and after loss by ablation during the summer a wedge-shaped addition remains each autumn. The successive lower layers can be seen to have tilted more and more by the lowering of the ends adjacent to the rock wall, in the manner envisaged by Streiff-Becker, but not by the process of extrustion flow that he favoured.

No convenient section occurred in the Skauthöe cirque, but the illustrations show the series of thrust planes and ice bands rising steeply to the surface towards the lower end of the glacier. These dipped down into the glaciers at about 45 degrees from the horizontal, where the glacier surface inclined at 22 degrees. Slabs of gabbro rose to the surface along these thrust planes and always emerged at this angle of about 45 degrees (see Fig. 7, p. 282). The largest block must have weighed 50–100 tons. Blocks of gabbro of similar shape and size could be seen breaking from the head wall directly up-stream of where they emerged. Some of those incorporated in the moraine were 10 m. long.

Figure 8 (p. 237) is drawn on the assumption that this evidence indicates that the glacier moves, at least in part, by rotational slipping. The motive force is the wedge-shaped annual addition at the top of the glacier, assisted by melting of the ice at the lower end. Dr. Orowan encourages me in this interpretation and has calculated that a surface gradient of the order of 12 degrees would be sufficient to cause such a glacier to move in this way. Plastic yielding along specific thrust or shear surfaces—ideally along a single surface following the bed of the glacier—seems to accord with the type of movement that Dr. Orowan has advocated this evening.

* *Journal of Glaciology*, Vol. 1, No. 1, 1947, p. 19.

Over-thrusting was particularly active on the Skauthöe glacier because of its steep surface gradient and the resistance offered to the forward movement of the lower layers of the ice by the substantial "1760" moraine. It is also active at the steeply sloping ends of glaciers, as was shown many years ago by T. C. and R. T. Chamberlin. The slides showing the tongue of Heillstuggubreen illustrate similar overthrusting, again bringing up material to the surface of the glacier.

An immediate attraction of this hypothesis of glacier movement is that it could account for the scouring out of basins in the rock floor which, on the disappearance of the ice, would contain lakes. Lakes have long been recognized as a characteristic of glaciated valleys, but their formation has not been adequately explained.

Rotational slipping is only put forward as one of the many ways in which glaciers move. It probably occurs when gradients, loading and thickness are specially favourable, such as at the head of a glacier, at the terminus, and possibly below ice falls in the middle of a glacier's course.

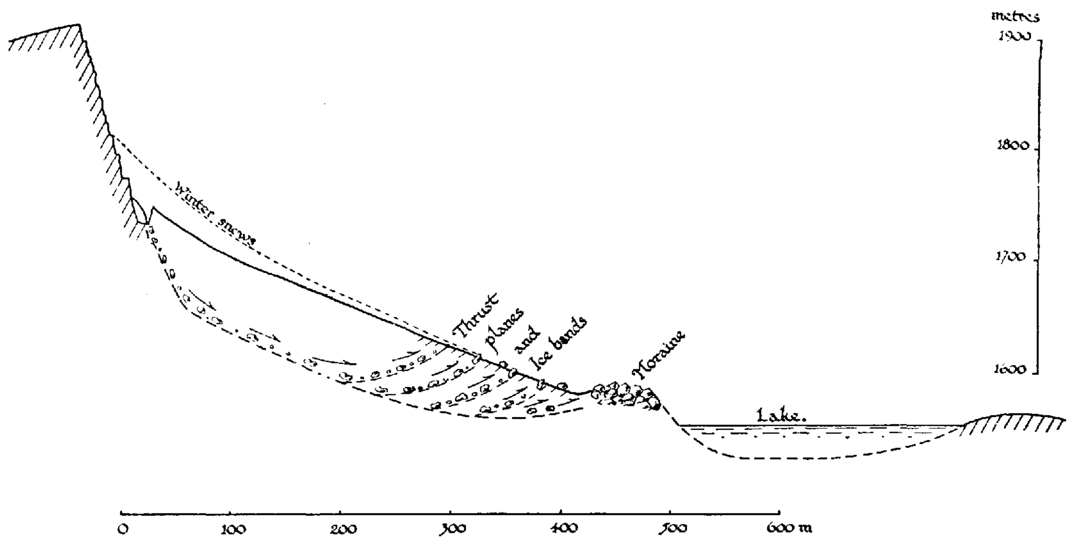


Fig. 8. Estimated section through Skauthöe Cirque and Glacier

Dr. C. H. Desch, F.R.S.: As this is a joint meeting with metallurgists perhaps I might say a word or two on the application of the work of metals.

An analogy is often drawn between the flow of ice and the flow of metals, but there is an important distinction—the volume change which takes place on freezing. Since water expands on freezing and metals, with few exceptions, contract, it is difficult to draw a close analogy. I have suggested that it would be interesting if laboratory experiments were carried out with bismuth, that being a metal which expands on freezing, has a crystal structure not too far from that of ice and shows in many respects a behaviour like that of ice. It is brittle, so that it fractures easily if hammered. On the other hand, you can extrude it through orifices and even produce a flexible wire. Therefore, if experiments were made at a temperature a few degrees below the melting point of bismuth, I think some useful information would result.

I am glad that reference was made to Professor Andrade's work, because that has been overlooked. In the case of low-melting metals he showed that deformation could always be represented by an equation containing two terms; one of the plastic deformation virtually independent of time, and one of the viscous term depending on time. These results are interesting, but in making a

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comparison between the deformation of metals and of ice, one must always bear in mind the difference in the sign of the volume change because that is fundamental.

Mr. C. GURNEY (Royal Aircraft Establishment, Farnborough): I would like to add to Dr. Desch's remarks about the effect of pressure on the melting point. If both solid and liquid are under the same hydrostatic pressure, then the relationship between melting temperature and pressure is given by

$$\frac{dp}{dT}=\frac{L}{T(V^L-V^S)}$$

where V^L and V^S are specific volumes of liquid and solid. The sign of $\frac{dp}{dt}$ therefore depends on the sign of the specific volume difference and as Dr. Desch has said, it is different for water than for most metals. If, however, the solid is under uniaxial compression, and the liquid, if formed, is pressure free, then the relevant formula is

$$\frac{dp}{dT}=-\frac{L}{TV^S}$$

where p is now uniaxial compression. In this case $\frac{dp}{dT}$ is negative for all substances and independent of the sign of the specific volume change.

Mr. W. H. WARD (Building Research Station, Watford): I noticed that during his talk earlier to-day, Mr. G. Seligman mentioned that alpine crevasses were seldom deeper than about 30 m., and that Dr. Orowan referred to a rough calculation giving the yield stress of ice as 1 kg. per sq. cm. A simple piece of arithmetic

$$S=\frac{\gamma h}{2}=\frac{0\cdot 8\times 10^{-3}\times 30\times 10^2}{2}=1\cdot 2\text{ kg./cm.}^2$$

shows that these two figures are in rough agreement. I was wondering if anybody had made actual measurements of the yield stress of ice.

Dr. PERUTZ: Nobody has made reliable measurements on glaciers, but from certain experiments made during the war one can form a rough estimate and can confirm that yield stress is of the order of magnitude mentioned by Mr. Ward.

Sir GEOFFREY TAYLOR, F.R.S.: Does Dr. Orowan's theory give any explanation of the way in which the curved lines on the surface of the glacier shown by Mr. Lewis are formed?

Dr. OROWAN: In the middle of the glacier the shear stress is uniaxial (the axis of shear is horizontal and perpendicular to the gradient of the glacier bed); flow is produced here by the weight of the ice as it lies on the sloping bed, according to equation (1), possibly assisted or restrained by the longitudinal stress (*cf.* equation (2)). In its lateral parts, however, the thickness of the glacier is not sufficient to cause flow according to equation (1), and the longitudinal stress obviously cannot make up for this deficiency. An essential factor in producing flow in these thin lateral parts is the shear stress transmitted from the central parts across vertical planes parallel to the glacier gradient; in other words, the thick central parts drag the lateral parts with themselves. As one goes from the centre towards the sides of the glacier, the shear stress component with the horizontal axis decreases, and a shear component with a vertical axis appears and increases. Even if the ice were an ideally plastic solid, therefore, the rate of shear around the vertical axis, which is zero in the centre, would increase towards the sides of the glacier according to the Lévy-Mises equations, provided only that yielding obeys the Maxwell-Mises (Huber-Mises) quadratic yield condition. This increase is just what is shown by the typical curvature, which initially straight transverse lines on the surface of the glacier assume in the course of time. The form of these lines, therefore, is no indication of any viscous property of the flow.

Mr. Lewis (in reply to a further question by Sir Geoffrey Taylor): It seems that under certain conditions overthrusts bring up debris to the surface of the glacier. These dirty layers may subsequently be drawn out to form the remarkable Forbes' Bands shown by Dr. Perutz.

Mr. J. F. Nye (Cavendish Laboratory, Cambridge): I should like to ask the glaciologists whether there is any theory of the mechanism by which the thrust planes are able to bring debris to the surface. Is the process envisaged, for instance, as a rolling of the boulders between the two more or less rigid blocks of ice on each side of the thrust plane, or are they perhaps worked up to the surface by sliding taking place at the thrust plane, first in one direction and then in the other? The rolling mechanism would seem to require a very large relative displacement of the ice on each side of the thrust plane, and such a large displacement would give rise to steps on the surface of the glacier far higher than the ones shown in Mr. Lewis's slides.

Mr. Lewis: All I can be definite about is that the slabs lay parallel with the thrust or shear planes. I do not think those huge rectangular blocks would roll.

Mr. G. W. Scott Blair (National Institute for Research in Dairying) enquired whether the model suitable for the plastic flow of ice in a glacier would be more like that for a Bingham body than for a material obeying St. Venant's law of plasticity.

Dr. Orowan in reply indicated that the existence of transient creep was evidence that ice was not a Bingham solid. However, the existence of creep in general showed that it did not exactly obey St. Venant's law either.

Dr. E. W. J. Mardles (Royal Aircraft Establishment, Farnborough): Rheologists are always interested in threshold yield values and would like to have full information about the ratios of shearing stress to rate of shear for ice under various conditions. There has been controversy about the question of yield values; for example, does glass flow under low stress and does ice behave in the same way, or is there a definite yield value before ice changes from an elastic solid to a plastic material? It could be settled by laboratory work and I am wondering whether anybody has done any work in this direction.

Dr. Orowan: If the temperature is high enough for glass to show any permanent deformation, such deformation can take place at any value, however low, of the shear stress. Ice, on the other hand, undergoes practically no permanent deformation before the shear stress reaches a fairly well defined critical value. This is indicated by experiments on ice which are in accord with more accurate and more extensive experiments with many other crystalline solids.

Dr. Mardles: With most soft solids, if the ratios of shearing stress to rate of shear are calculated, it will often be found that they increase at higher rates of shear until rupture of some kind occurs. Possibly this complex behaviour with thixotropy and dilatancy might occur with ice.

Dr. Orowan in reply to a question by Mr. Peckman said that he did not know of cases where the superposition of a hydrostatic pressure would lower the shear stress necessary for yielding. If materials showed more deformation with a hydrostatic pressure superposed to uniaxial tension than in pure tension, this was not because the hydrostatic pressure promoted plastic yielding, but because it counteracted fracture much more than it hindered yielding.

Dr. Orowan in reply to a question by Mr. Campbell as to the possibility of there being several hollows along a glacier bed, explained that these might arise in every terrace in the glacier bed, not only at the end of the glacier.

Dr. Mardles enquired what was the radius of their curvature. He believed it was a common occurrence among civil engineers to measure the actual radius of curvature when an embankment slipped, and it might be possible to correlate curvature with ice pressure. In reply Dr. Orowan said that this question was treated in detail in soil mechanics. The arc of slip was not really circular, but the assumption of a circular arc very much simplified stability calculations without introducing considerable error.

Mr. B. Webster Smith, F.G.S. (Copper Development Association): Are we not trying to prove too much with too little? A glacier is an enormous thing and most Alpine valleys contain many kinds of rocks, hard and soft, so that to ascribe a hollowing out action to any particular cause may be misleading. I was particularly interested in Mr. Lewis's beautiful photographs of blocks which had been dropped into the glacier and brought out again. The larger blocks seemed to be sharp-edged and uneroded as if they had been carried practically undamaged for years. May it not be that these had sunk to a certain layer and that what has been called the overthrust is simply the movement of successive layers of accretion over one another? These continually take place but not necessarily at the bottom of the glacier and if that is so, they cannot account for the hollows. As a basis for future experiments, if one went down into the bergschrund and there froze in some indestructible materials of different colours at various levels, it might be possible at a later date to see if the pieces emerged in the order in which they were put in.

Dr. Perutz: These movements are, I think, very slow, and as to where the thrust takes place, I do not think we know. The fact is material seems to be brought up from the bottom.

Dr. Orowan: It is not very surprising that big blocks arrive with fairly sharp edges, because the only thing they have undergone is one slide down the side of a mountain, and that should not damage them very much.

Dr. Desch: I wonder whether any members have seen the model at Glasgow University of a glacier composed of cobbler's wax. It shows many features very well, including crevasses giving the right curvature. I always think that in these discussions when you get a certain confusion between viscosity and plasticity, one might go back to Ruskin and remember his chapter on honey and butter! There is confusion between plasticity and viscosity; in a glacier you have in addition to plastic deformation a viscous deformation with a time factor.

Dr. Philip Bowden, F.R.S. (communicated): I am sorry to have to leave the meeting just a few minutes before its close, but I would like to take this opportunity of paying a tribute to Mr. Gerald Seligman. I have had the privilege of some association with him from the very early days of this work and, like all who have come into contact with him, I have a great admiration not only for his enthusiasm and foresight, but for the devoted attention and really hard prolonged work which he has put in. He is always very modest and retiring about his own contributions, but we all know that the planned attack on these problems, and the British Glaciological Society itself, are the children of his imagination. It must be a great source of satisfaction to him to see that they have grown to such a large, noisy, quarrelsome, able and effective family.

Mr. Scott Blair: I should like, on behalf of the Rheologists' Club, to thank our Chairman, Dr. Richard Seligman, for presiding over our meeting. You, Sir, and I meet on rather different occasions, and generally when we do meet it has more to do with butter than with ice. It is, however, interesting how one can integrate these things, even from the mountain to the valley or from butter to ice, and it has been one of the functions of the Rheologists' Club to be interested in all kinds of materials geologically. I think we have found another material to-day about which most of us knew nothing but which has tremendous interest for rheologists. I do thank you for presiding over our meeting this afternoon.

The Chairman: I must thank Dr. Scott Blair for his friendly words; it has given me great pleasure to take part in to-day's proceedings. I, for my part, should like on behalf of the audience as a whole, to thank Dr. Perutz and Dr. Orowan particularly for their introduction of the subject, and for their many explanations which must have contributed very much to the enjoyment of the evening.

The meeting then terminated.