appointed, consisting of the Chairman of the B.G.S. (W. V. Lewis), R. J. Adie, J. W. Glen, J. F. Nye, and W. H. Ward. The Sub-Committee will be pleased to receive requests for advice from members, which should be addressed either to any member of the Sub-Committee personally, or to the Secretary of the British Glaciological Society, c/o Scott Polar Research Institute, Lensfield Road, Cambridge. Expeditions wishing to deposit duplicated reports of their activities should send them to the Secretary at the above address.

GLACIER MECHANICS

Comments on Professor L. Lliboutry's paper*

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THE Editor has kindly invited me to comment on Professor Lliboutry's recent paper, and I am glad to do so in the hope of clarifying certain differences between my own ideas on glacier flow and those which Lliboutry is putting forward. I think it is worth while trying to do this because there is evidently already much common ground between us.

I begin by quoting the English summary in full:

The mechanics of a glacier is accounted for with a minimum of assumptions. Two facts which seemed inconsistent with Nye's theory are thus explained:

(1) Too gentle slopes at the centre of the Greenland Icecap (northern part). That is because the ice is flowing off from the icecap at the border only, being stagnant at the centre. Opposite to the classical scheme of an "evacuating glacier," the author introduces the scheme of a "reservoir glacier."

(2) Finite gradient of the slope at the front. A more accurate differential equation for the equilibrium profile is established and integrated by successive approximations. This equation leads for the slope at the front, relative to the bed, to an universal value: $0.568 = \text{tg } 29° 36'$, which conforms to the observed values, specially at the Thule ramp.

It is then explained why the blue bands and the shear moraines get very close to the stream-lines near the front. A qualitative explanation of their courses is given. Refuting Nielsen and Stockton's calculations, the author enters upon an approximate calculation of the velocities near the front, according to the theory of perfect plasticity.

Lastly, new general cases of extensive flow are indicated, and Nye's theory on crevasses partially denied.

It is first necessary to clear up a misunderstanding in relation to point (2) of the summary. The slope of a glacier at the snout is indeed something that needs analysis. But the theory I have expressed has never attempted to cover this region of a glacier, because it is precisely here that the approximations are recognized to break down. It is a misunderstanding therefore to suppose that the "finite gradient of the slope at the front" is inconsistent with the theory.

Coming now to Lliboutry's analysis, the first hypothesis (p. 248) is that in the model glacier under consideration the longitudinal stress σ_x is everywhere equal to "the hydrostatic pressure $\rho g . \Delta Z$, ΔZ being the vertical distance to the free surface." This is introduced only as a first approximation and, as such, is free from objection. Lliboutry contrasts it with the slightly different hypothesis which I have used ($\rho g . MA$ in his Fig. 1), but, in fact, to the first approximation our hypotheses are identical. It is true that in the second approximation they are different, but this is beside the point, since both of us agree that our hypotheses are only introduced at this point as being true to the first approximation. Lliboutry goes on to derive his leading formula (2) for the shear stress τ_0 on the glacier bed:

$$\tau_0 = \rho g \frac{S}{p} \tan \alpha \cos \beta, \qquad \qquad (2)$$

where p is the perimeter of a cross section, α is the slope of the surface, and β is the slope of the bed. (S is undefined but is evidently the area of cross section perpendicular to the bed.) For small angles, as he points out, this reduces to the formula I have given, namely

$$\tau_0 = \rho g \frac{S}{p} \alpha. \qquad \qquad (3)$$

Now since formula (2) is in any case only true to the first approximation (being based on a hypo-
thesis which is only true to the first approximation), there is no advantage in using it in preference
to (3). To the first order (2) and (3) are the same. There is in fact a positive disadvantage in retain-
ing (2), because it *appears* to give the dependence of τ_0 on β, which is a second-order dependence.
One cannot hope to obtain such a second-order dependence from a hypothesis which is only true
to the first order. Indeed, one might get all sorts of other β dependences by starting with other
equally acceptable approximations for σ_x.

The three profiles which Lliboutry derives (p. 252) for a glacier on a bed of uniform slope β
are then only correct to the first order; and to the first order they are identical with those already
obtained in the earlier theory (ref. 1, p. 569, and ref. 2, equation (3)).

The next step taken in the paper is to derive a second approximation in order to give a better
representation at points where the first approximation fails badly, namely, near the snout of the
glacier where the surface slope becomes large. The method used appears to me to be completely
fallacious. The prescribed conditions are (1) that the substance is perfectly plastic everywhere
with constant maximum shear stress τ_0, and (2) that the shear stress at all points of the bed is τ_0.
The first criticism of the solution is that it does not ensure that the upper surface is free from shear
stress. This is readily seen from the fact that the slip lines in Fig. 5 do not meet the upper surface
at $45°$, as an exact solution would demand. This criticism (and the further criticism that σ_z at the
surface is wrongly put equal to H) might be met by saying that the solution is only meant as an
approximation. Very good, but the solution is then used to deduce the slope at the extreme limit
of the ice ($x=0$) and it is precisely here that the approximations made break down completely.
In fact, since the upper surface is free from shear stress and the bed is prescribed to be a surface
of maximum shear stress, the only possible angle of the surface at $x=0$ would seem to be $45°$,
in contrast to the angle of $29° 36'$ deduced in the paper.

However, I think a still more serious criticism of the solution is its optimism. It is surely quite
possible that under the two prescribed conditions, numbered (1) and (2) above, no solution exists.
Such a state of affairs is well known in plasticity theory, and what is one to make of an approxima-
tion to something which may not, or does not, exist? I would guess that if a perfectly plastic sub-
stance were placed in this situation it would show a "compressive flow" type of behaviour up to a
certain limiting slip line, and beyond this slip line the material would be rigid, so that neither
condition (1) nor condition (2) would be fulfilled for the extreme end—a flow similar to that
illustrated in Fig. 7b of the paper. I wonder if Professor Lliboutry would agree that this is at least
a possibility to be reckoned with?

Furthermore, it is said in the paper that one can calculate the profile entirely from the stress
solution and without reference to the distribution of velocities (bottom of p. 264). But surely the
profile must be determined to some extent by the distribution of ablation. When working out the
first approximation to the solution of this problem I pointed out (ref. 1, p. 570) that if the equili-
brium profile deduced purely from considerations of stress was to be maintained there had to
be *some* restriction on the distribution of ablation or accumulation. The restriction turned out in
that case to be very weak (that the rate of accumulation should change only slowly from point to
point), but it always has to be considered. Otherwise one runs into the fallacy of "statical
determinacy" (ref. 5), which used to cause so much trouble in plasticity theory. I think that
Lliboutry may be running into this fallacy when he computes his velocity solution.

The suggestion that the northern part of the Greenland Ice Sheet is stagnant is an interesting
one (although I do not see why one should be surprised to find that the shear stress on the bed is
0·6 bars, bearing in mind that values for alpine glaciers range from 1·5 to 0·5 bars). It may be helpful
to point out in conjunction with this new hypothesis of Bader and Lliboutry that it is no longer
necessary, although it may still be useful, to look at such problems in terms of perfect plasticity.
It is quite possible with the more general theory of ref. 3 to calculate the differential motions in
the Greenland Ice Sheet if enough data are available, and I am now attempting this.

I think that ref. 3 now supplies the general theory which Lliboutry is seeking on p. 266 of his
paper.

Lliboutry introduces on p. 272 some new cases of extending flow. He gives the name extending
flow to all cases when the glacier extends longitudinally, that is when $\partial u/\partial x$ is positive, even though
the width, depth and velocity may vary with respect to position and time. This seems to be a matter
of definition. However, he goes on to say that transverse crevasses will be associated with places

where $\partial u/\partial x$ is positive. This criterion leads him to deduce that crevasses will be formed at places where the glacier valley narrows, because here, he says, the velocity increases. I think this criterion for the formation of transverse crevasses is wrong. I would suggest that a better criterion is that the longitudinal stress σ_x at the surface should be more tensile than atmospheric pressure. When there are no transverse strain rates the two criteria are identical, but in general, using the theory of ref. 3, the criterion I propose gives not $\partial u/\partial x > 0$ but

$$\frac{\partial u}{\partial x} + \frac{1}{2}\frac{\partial v}{\partial y} > 0,$$

where x and u are longitudinal, and y and v are transverse. Where a glacier narrows there is a transverse compression and $\partial v/\partial y$ is negative. $\partial u/\partial x$ may be positive, but it does not follow that $\partial u/\partial x + \frac{1}{2}\partial v/\partial y$ is positive; hence it does not follow that crevasses will be formed. A simple illustration of the point is given by a strip of metal passing through a rolling mill, the axes of the rolls being vertical. The strip is extended longitudinally by a great amount; but this is accomplished not by a longitudinal tension but by a transverse compression.

I think that the further deduction that crevasses will be formed in a glacier tongue which is growing is also invalid, but this time for a more mathematical reason. First of all the equation $\partial\Phi/\partial x = -VL$ is only true for a steady state. Secondly, the differentiation for obtaining $\partial\bar{u}/\partial x$ seems to have been wrongly performed. I would rewrite equation (41) as

$$\frac{\partial\bar{u}}{\partial x} = \frac{\bar{u}}{\Phi}\frac{\partial\Phi}{\partial x} - \bar{u}\left[\frac{1}{L}\frac{\partial L}{\partial x} + \frac{1}{E}\frac{\partial E}{\partial x}\right],$$

without any terms in $\partial/\partial t$. If $\partial\Phi/\partial t$ is introduced one should also bring in terms in $\partial\bar{u}/\partial t$, $\partial L/\partial t$ and $\partial E/\partial t$, which cancel it out again.

There remains the question of the origin of crevasses discussed on p. 273. Lliboutry says that my theory can only produce a tension of 1 bar and that this is quite insufficient to break the ice. Since atmospheric pressure has no effect (ref. 4) the effective tension is 2 bars, but even so it may well be maintained that 2 bars is not enough to break the ice. But there is perhaps no real dispute between us here. I would start by postulating that the ice of a glacier always contains a sufficient number of places where the tensile strength is effectively zero, and I would then invoke a longitudinal tensile stress to start crevasses at these weak places. Lliboutry is going one stage further back and is proposing mechanisms by which the weak places can form in the first place.

To sum up, I submit that, so far as the glacier profile is concerned, Lliboutry's first approximation is correct, but not new, and that his second approximation or refinement of the theory is fallacious. The basic theory which Lliboutry has used, that is plasticity theory, furnishes a sound basis (although not the only one), if properly applied, for attacking problems in glacier mechanics. At the same time it is important that, the foundation being sound, the application should be done properly, and therefore that this paper should not go unchallenged.

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On going to Press it is understood that Prof. Lliboutry does not fully agree with the above comments. His reply will be published in the next issue. *Ed.*