GLACIER MECHANICS IN THE PERFECT PLASTICITY THEORY

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A REPLY TO DR. J. F. NYE'S COMMENTS ON MY PAPER* AND SOME FURTHER DEVELOPMENTS

ABSTRACT. The author agrees with Nye's correction of two errors in a previous paper; but disagrees with his other objections. Evidence is put forward for the significance of a second approximation in the theoretical equation for the profile of a glacier; for instance, it explains why bulges of increased thickness travel down glacier faster than the ice. Features of the front of the Glacier de Saint-Sorlin are reported. An attempt is made to explain crevasse formation in glaciers and the upturning of blue bands at the foot of an ice fall.

RÉSUMÉ. L'Auteur reconnait avoir commis dans un article précédent deux erreurs, qu'a relevées Nye, mais il n'accepte pas ses autres objections. L'interêt d'une deuxième approximation dans le profil d'équilibre d'un glacier est mis en évidence. Elle explique par exemple l'existence de vagues descendant un glacier plus vite que la glace. Des aspects du front du Glacier de Saint-Sorlin sont signalés. L'on essaie d'expliquer la formation des crevasses, et le relèvement des bandes bleues au pied d'une chute du glacier.

INTRODUCTION

I am indebted to Dr. Nye for rectifying two mathematical errors which crept into my paper; however I still believe that they do not weaken my conclusions. I must first state what my aim was in writing the paper in question. Nye finds that there is much common ground between our ideas. It would have been a sad thing had this not been true, as my intention was to present, to develop and to complete Nye's ideas concerning glacier mechanics. Nye's papers are brilliant expositions of mechanics, but they are still pretty far from the explanations which glaciologists ask for when they look at glaciers.

I do not believe any more than does Nye that ice is a perfectly plastic material. It is but a rough scheme. But, before forsaking it (as Nye has since done in a masterful paper†), it is convenient to find out all that it can tell us. For instance, morphologists must pay attention to the fact that, in the plasticity theory, the frictional force of a glacier on its bed becomes independent of the velocity, and thus that glacial erosion becomes independent of velocity. If the glacier moves more slowly, the debris-laden ice which forms the ground moraine nevertheless exerts the same force on the rock bed. Each debris particle would do less work per unit time, but they would be proportionately more numerous. In this way one can explain overdeepening by a glacier; in a basin the velocity is less than over a rock bar, but in spite of this the basin goes on being dug, and the rock bar continues notwithstanding. Such over-deepening could not be explained by the viscous theory.

THE EQUILIBRIUM PROFILE, FIRST APPROXIMATION

To state that the "hydrostatic pressure" (or more exactly the octahedral stress) in a glacier is ρgh, where h is the vertical distance to the free surface, is only a first approximation. All treatises on soil mechanics state this, and I am well aware of it. If I repeated with this hypothesis Nye's classical calculation of the glacier profile, it was more to expound it for the first time to French readers than to make a trifling correction.

Something left undeveloped in Nye's papers which I tried to elucidate, is that the friction on the bed (*i.e.* the shear stress very near to the bed) is approximately 1 bar only if the ice

* L. Lliboutry. La mécanique des glaciers en particulier au voisinage de leur front. *Annales de Géophysique*, Tom. 12, Fasc. 4, 1956, p. 245-76.

J. F. Nye. Glacier mechanics; comments on Professor L. Lliboutry's paper. *Journal of Glaciology*, Vol. 3, No. 22, 1957, p. 91-93.

† "The distribution of stress and velocity in glaciers and ice-sheets." *Proceedings of the Royal Society*, Series A, Vol. 239, No. 1216, 1957, p. 113-33.

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is moving right to the bottom of the glacier. Thus the formula for glacier depths, $e \approx 10/\alpha$ metres, is only valid for a glacier active to the bottom. It is not true, for instance, at the summit of an ice cap ($\alpha = 0$), where there is stagnant ice at the bottom. However the formula remains valid if we take the surface of the stagnant ice as a fictitious bed, and ask for the depth of the active layer, perpendicular to this fictitious bed.

The plasticity theory leads us to the new concept of a glacier which is stagnant because it has not reached its equilibrium profile, just as a reservoir does not overflow before it is full. This is essential in the study of glacier fluctuations, and to an understanding of their response to climatic changes. For instance, I suppose that the central part of North Greenland and perhaps also all of Antarctica, are stagnant to-day; thus with the same area their ice cover could have been much thicker than it is to-day, and the ocean surface lower.

Let us emphasize another point: an ideally plastic material obeys two laws: (1) it flows only if the maximum shear stresses reach a certain critical value; (2) it flows in such a way that the strain tensor is proportional to the stress deviator at every point. This second law is not necessary for the computation of the equilibrium profile, so this profile has more chance of being correct than have the calculated velocities.

Equilibrium Profile, Second Approximation

As we improve the calculation of the stresses, we can improve the calculation of the equilibrium profile. It would be surprising if extending and compressive flow did not lead to different profiles.

We take the origin at the front of the glacier, Ox along the bed (which makes an angle β with the horizontal), and Oz upwards. The first approximation for the normal stresses was

$$\sigma_x = \sigma_z = \rho g h + H.$$

The atmospheric pressure H is transmitted by the ice as a hydrostatic pressure. This is obviously true for a glacier at melting temperature with interstitial water, and probably true for a cold glacier owing to stress release. The stresses connected with the movement are however continually renewed and so do not release. Thus we have at the surface

$$\sigma_x = \sigma_z = H$$

and not, as Nye thinks,

$$\sigma_x = H \sin(\alpha - \beta), \qquad \sigma_z = H \cos(\alpha - \beta).$$

This first approximation for the normal stresses leads us to a first approximation for the differential equation of the profile $e(x)$, namely

$$e' = \frac{\tau_0 - \rho g e \sin\beta}{\tau_0 \tan\beta + \rho g e \cos\beta}$$

(which gives a parabola if $\beta = 0$), and to a first approximation for the shearing stress

$$\tau_{xz} = \tau_{zx} = \tau_0 \left(1 - \frac{z}{e}\right).$$

According to Nye himself the second approximation for the normal stresses is

$$\sigma_z = \rho g\, (e - z) \cos\beta + H + \frac{\tau_0}{2} e' \left(\quad - \frac{z^2}{e^2} \right),$$

$$\sigma_x = \sigma_z \pm 2\tau_0 \sqrt{2\frac{z}{e} - \left(\frac{z}{e}\right)^2}.$$

I cannot understand why Dr. Nye then denies me the right of writing a second approximation for the profile equation. Let us write down the normal force per unit breadth acting on a cross-section perpendicular to the bed; it is

$$P = \int_{z=o}^{z=e} \sigma_x \, dz$$

where σ_x has its new value. We then proceed with the calculation as before and obtain

$$\frac{e'}{3} = \frac{x}{e} - \frac{\rho g}{\tau_0}\left[\frac{\int_0^x e\, dx}{e}\sin\beta + \frac{e}{2}\cos\beta\right] \mp \frac{\pi}{2}.$$

We could continue and write a second approximation for the shear stress τ_{xz}, which would not be zero at the glacier surface.

I was wrong when I then wrote in my paper equation (16)

$$\cos 2\theta = 1 - \frac{z}{e}$$

which is no longer valid in this second approximation, and in drawing on the same figure my second approximation profile and the first approximation lines of maximum shear. In fact, as Nye says, the lines of maximum shear must reach the surface at an angle of 45°. However, the second approximation for the profile remains unchallenged.

Another objection against my second approximation profile is that it gives a front angle between surface and bed of 29° 36′. Nye thinks it should be 45° as the bed is a line of maximum shear. I disagree with this assumption. The origin is obviously a very singular point, and the slope of the maximum shear lines can probably take any arbitrarily chosen value provided we approach this singular point along a convenient path. If we approach the origin along the bed, this value is 0; but if we approach along the glacier surface it can be 29° 36′—45°= —15° 24′, and so on. In my paper I sought an approximate solution of the flow function near the origin by means of a polynomial in x and z. This led, incidentally, to a value of the front angle of 60°. However, if the origin is an essentially singular point, the flow function is not analytic and cannot be represented by a polynomial near the origin.

Let us now examine a more serious criticism. This method of successive approximations assumes that there is flow as far as the origin. Perhaps there is not, and the only possibility is a solution similar to that which I drew in Fig. 7b of my paper (here reproduced as Fig. 2b, p. 165), with a wedge of stagnant ice at the origin. I cannot offer a mathematical demonstration of the existence of flow at the origin, any more than Nye can demonstrate the contrary. All that I can do is to prove the movement as Diogenes did; I can argue that, just at their front, clean glaciers do move, and their ice does flow.

This summer I studied the Glacier de Saint-Sorlin in the Grandes Rousses (French Alps). It ends in a flat valley of slope 3°, on a thin, loose layer of glacial drift. Its broad front is pretty flat for the first 100 m., with a slope of 20° to 22°. Thus the front angle is 18°±1°. I have sketched this in Fig. 1 (p. 165) together with the parabolic profile, with Nye's hypothesis, and with my second approximation profile. The agreement between the real profile and the profile given by my "wholly fallacious" theory is worthy of admiration.

The ice is very clean except for the last foot near the bed, and the glacier surface has only some cryoconite. Now at the very end, the signs of actual movement are numerous, and the bed is not flat enough to allow movement without some flow.

However in the last 1 to 3 metres, in summer, a new and local phenomenon occurs, which shows us the futility of these over-academic discussions about the singular point at the origin itself. Solar radiation can reach the ground through the ice, thaw and warm it. So the glacier melts from the bottom and does not touch its bed. The friction on the bed disappears, the critical shear stress is not reached, and the last metres of glacier are held up and borne forward by the mass of the glacier. Several protuberances from the rock bed there give rise to straight, grooved tunnels of very young appearance. Thus Nye's hypothesis is right for only a few metres, while my calculation is done for a few hundred metres.

Of course this example chosen from several others is no proof of the existence of a solution to the plasticity problem, as the ice is not perfectly plastic; but the hypothesis of a great mass of dead ice at the front of a clean glacier is unacceptable for a stationary glacier. Owing to

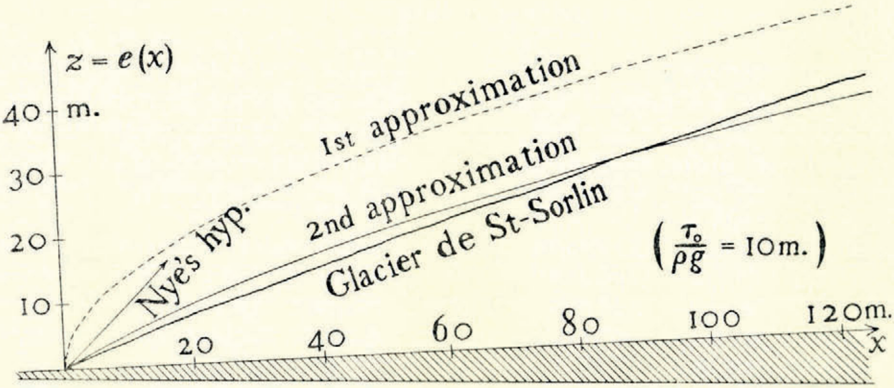


Fig. 1. The equilibrium glacier profile: theory and evidence

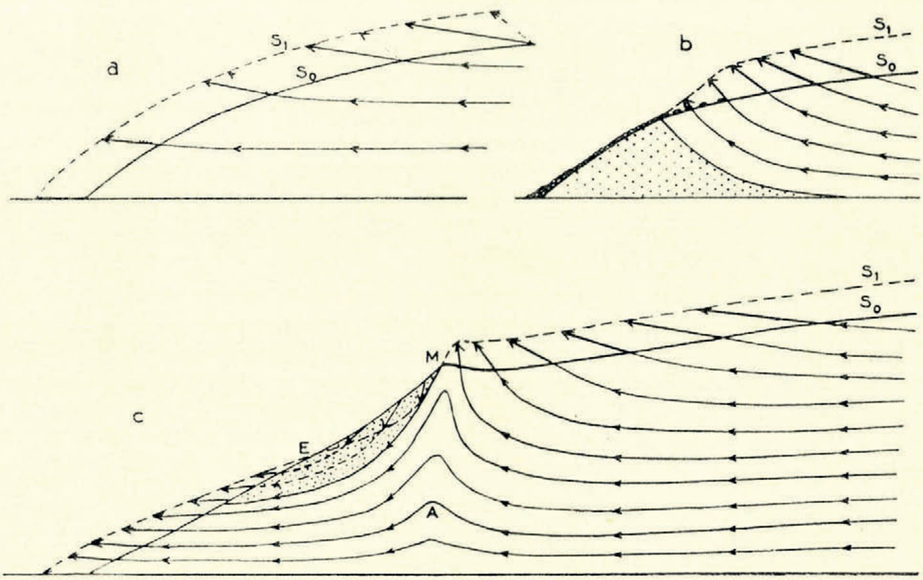


Fig. 2. Flow lines near the front: (a) clean ice; (b) debris-covered ice; (c) "ramp" of an ice sheet, with a superimposed fringing glacier

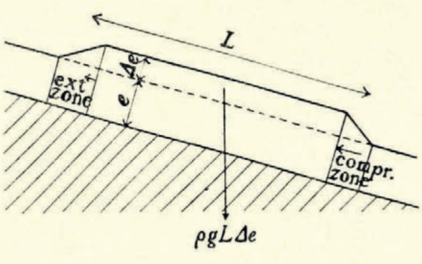
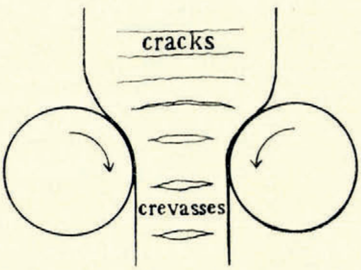


Fig. 3 (left). Schematic bulge going down a glacier

Fig. 4 (right). A strip of metal stretched between rolls is similar to a glacier entering a valley. Opening of previous cracks



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the ablation the glacier would recede. It would be very tiresome to the theorist if a glacier of perfectly plastic material could not be stationary.

The Role of Ablation

I fear that Nye has misread my paper when he accuses me of not taking account of ablation when computing my velocity solution. On the contrary, unsatisfied with "some restrictions on the distribution of ablation" which he invokes, I described extensively (Fig. 7, here reproduced as Fig. 2, p. 165), and put into an equation, the condition at the boundaries prescribed by the ablation.

In a stationary glacier, the velocities diverge in such a way that their vertical components exactly counteract the local balance (ablation minus accumulation). If the velocities do not diverge enough, the glacier thins, a mass of dead ice appears at its front, and then the velocities diverge more. If the velocities diverge too much, the glacier thickens and advances, and then the velocities diverge less.

However, when the adjustment has happened and a stationary state has been reached, the equilibrium profile can be computed almost without taking account of the movement. The movement enters only to determine where the flow is extending and where it is compressive; to determine whether one should take + or − in the last term of the profile equation. There is not "statical determinacy", but there is "variable separation" in our system of equations.

Bulges Passing Down a Glacier

It must be emphasized that this problem can be solved in the perfect plasticity theory only because we suppose that the glacier is stationary. Let us take for instance a flat, rough bed with a uniform slope β and a negligible local accumulation balance. In the stationary state the thickness is

$$e = \frac{\tau_0}{\rho g \sin \beta}$$

and there is plug flow. Flow conditions are reached only at the bed, and the glacier moves as a whole with a uniform velocity u_0. We can compute u_0 if we know the volume of ice which must be passed from the accumulation zone to the ablation zone each year.

Let us now suppose that, owing to a strongly increased accumulation balance, for instance the fall of a hanging tributary glacier, a bulge of increased thickness is formed on the glacier. In the first approximation for the profile, this bulge cannot persist and it will be progressively damped out; it is replaced by a zone of increased velocity, with a transitional zone of compressive flow in front, and another of compressive flow behind. In the second approximation, as the profiles for compressive and extending flow are not the same, a bulge can remain (Fig. 3, p. 165). However I believe that a *critical size* is necessary. According to our computation of the normal forces over the whole cross-section (2nd approximation), the compressive zone produces an extra upwards force

$$\tau_0 \frac{e \tan \alpha_1}{3} + \tau_0 \frac{\pi}{2} e,$$

and the extending zone an extra downward force

$$\tau_0 \frac{e \tan \alpha_2}{3} - \tau_0 \frac{\pi}{2} e.$$

Let us now equate these forces to the excess weight,

$$L.\, \Delta e \;\rho\, g \sin \beta = \tau_0 e \left[\pi + \frac{\tan \alpha_1}{3} + \frac{\tan \alpha_2}{3} \right],$$

whence, since α_1 and α_2 are small, and of opposite sign

$$L.\, \Delta e \approx \pi e^2.$$

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Thus if the volume of the bulge per unit breadth reaches πe^2, the bulge would go down at any speed without dying away. Such bulges exist in fact on several glaciers, and travel down 3 to 4 times faster than the ice;[1] however I think that perfect plasticity theory is unable to give us this speed owing to its oversimplified assumptions.

Crevasse Formation

The last part of my paper was hastily drawn up so that it could be published before the glaciological programme of the International Greenland Glaciological Expedition (E.G.I.G.) had been settled. As a result an error crept in to the computation of $d\Phi/dx$ for an advancing glacier. I apologize to the reader for this. As regards my ideas, I will here draw up a more elaborate statement.

Nye's theory leads to tensions of about 2 bars at the glacier surface. He specifies that one must not take atmospheric pressure into account as it is a hydrostatic pressure. I agree with him, and note the contradiction with his previous statement that σ_z is not equal to H at the surface. A tension of two bars may at times be sufficient to produce an intergranular crack at a weathered surface, but generally 7 to 12 bars are necessary to break ice. I quote Nye's opinion:

"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."

I think that the tensile strength cannot be zero if there is no crack, no lack of continuity; and a crack is nothing else than an incipient crevasse. To open the crevasse we need then a *strain*, not a *stress*. Thus I would say:

"Depending on several factors, and essentially on bed roughness, the maximum stresses at the glacier surface fluctuate strongly about the mean value of about 2 bars given by Nye's theory. Thus in many places cracks can appear, but only when the longitudinal acceleration $\frac{\partial u}{\partial x}$ is positive in the neighbourhood can the transverse cracks so formed open."

To take up Nye's analogy again, if we stretch a strip of metal between two rolls, we cannot of course form transverse cracks, but if the cracks already existed they would open, as sketched in Fig. 4 (p. 165).

Therefore to apply plasticity theory to crevasse formation, we must take the friction on the bed as fluctuating between 0 and τ_c, with a mean value τ_0 of about 1 bar, and a critical shear stress of the ice τ_c of several bars. This statement is more in agreement with the measured values of the yield stress of ice (see ref. 1, p. 823). The glacier equilibrium profile would not change on the mean, neither perhaps would the mean stresses and velocity, but on a smaller scale, according to the roughness of the bed and its distance from the surface, we should pass from a compressive flow to an extending flow and vice versa. Now at every point where there is extending flow the tensile stress at the surface is not $2\tau_0$, but something between $2\tau_0$ (the value for a very thick glacier) and $2\tau_c$ (the value for a very thin glacier). In both extreme cases Nye's theory can be applied; if very thick we should take it in its original form, if very thin we should divide the glacier into many small regions with different types of flow. The general case seems to be a very hard problem to resolve.

I shall now proceed to the correct computation of $\frac{\partial u}{\partial x}$ for the general case.

The volume of ice passing through a cross-section (i.e. the discharge) is $\Phi = \bar{u}LE$, where L is the breadth of the glacier, E its mean thickness, and $\bar{u}$ a mean velocity through the section. This can be differentiated to give

$$\frac{\partial \Phi}{\partial x} = \frac{\partial \bar{u}}{\partial x} LE + \bar{u} \frac{\partial L}{\partial x} E + \bar{u}L \frac{\partial E}{\partial x}.$$

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If V is the local balance (ablation minus accumulation, expressed as a height of ice), we can write

$$\frac{\partial \Phi}{\partial x} + \frac{1}{\bar{u}} \frac{\partial \Phi}{\partial t} = -VL.$$

The term containing $\partial \Phi / \partial t$ is introduced because we must differentiate following the ice in its movement. Whence, eliminating $\partial \Phi / \partial x$,

$$\frac{\partial \bar{u}}{\partial x} = -\frac{V}{E} - \frac{1}{\Phi} \frac{\partial \Phi}{\partial t} - \frac{\bar{u}}{L} \frac{\partial L}{\partial x} - \frac{\bar{u}}{E} \frac{\partial E}{\partial x}.$$

Now $\partial \bar{u} / \partial x$ must be positive for extending flow. This equation (which should replace eq. 41 of my previous paper) shows that one or more of the following conditions must be fulfilled:

$V \ll 0$: strong accumulation,

$\frac{\partial \Phi}{\partial t} \ll 0$: strong reduction of discharge (but a stagnant state not yet reached),

$\frac{\partial L}{\partial x} \ll 0$: narrowing glacier,

$\frac{\partial E}{\partial x} \ll 0$: flow lines getting closer (for instance in the upper part of an ice fall).

Now we find crevasse formation just behind a bulge of increased thickness (as stated previously), or just after a "glacier flood". Generally the glaciologist arrives after the glacier flood has been reported, and by then the decrease has begun; nevertheless there is some evidence that new sets of crevasses appear as soon as the glacier gets swifter, *i.e.* when $\partial \Phi / \partial t > 0$. I believe the explanation is to be found as follows: the disturbance in stress introduced by a protuberance in the bed extends further when the velocity is greater.

I think we must introduce here the mechanism of friction itself, as Weertman did in a recent paper.[2] The ice melts against a protuberance when the sliding is small and deforms around it when the sliding gets faster. A theory of crevasse formation does not need more refinement in the flow law of ice, but a more realistic scheme for the bed rock.

Non-uniform Friction on the Bed and Discontinuous Movements

If the bed is unable to hold the glacier for a long distance, very strong stresses can appear. This is generally the case in an ice fall, that is where the glacier bed slopes very steeply. First, the rock is often smooth on such a rock bar. Secondly, owing to sliding on the bed, as I said in my paper, the glacier flows away from the slope at the top of the fall. (Sub-glacial tunnelling has led to much evidence about this, for instance at the Glacier d'Argentière.) Thus it jumps over protuberances of the bed and there is no friction at all.

Haefeli[3] speaks of longitudinal pressures of about 10 bars at the foot of the slope in the Mont Collon Glacier. In the plasticity theory we must take this value of about 10 bars as the critical shear strength τ_c. But even so the theory as developed by Nye is unsuitable, as sudden movements of the ice take place, and the inertia forces can no longer be neglected.

Lewis spoke of "rotational slip" in a glacier.[4] Rotational slip in soil mechanics is a rough idea used to make the computations easier. In fact, there are generally tensile cracks in the upper part, then sliding of the superficial layer with sinking of the upper part and flow of the lower. The same happens in an ice fall. We observe that in the upper part there are wide transverse crevasses, and the surface is broken into steps. Owing to shear cracks which undoubtedly prolong the crevasses, the glacier sinks like a moving staircase; see for instance the pictures of the Mayangdi Glacier in ref. 3, p. 29, and of the Gorra Blanca Glacier in the same issue of the *Journal of Glaciology*, p. 19. I have sketched this in Fig. 5, below.

Thus the ice does not rotate forwards in the upper part, as quasi-static dynamics would say. This probably explains the strong upturning of the foliation, the blue bands rising almost vertically, at the end of a glacier after several falls.

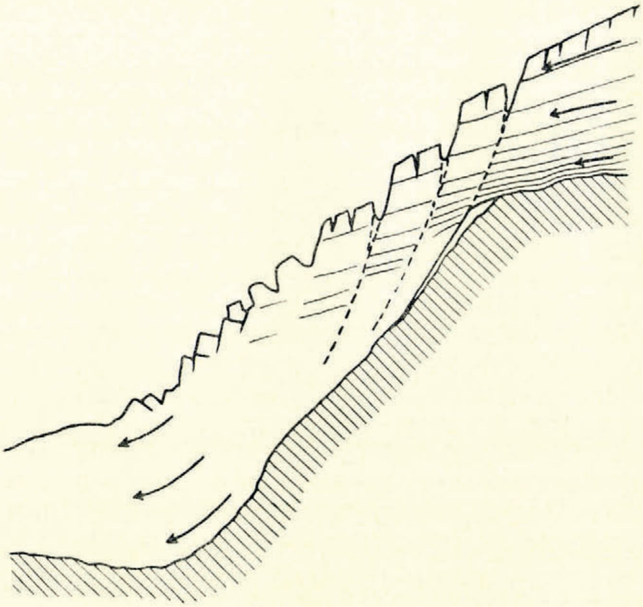

*Fig. 5. Glacier going down a slope discontinuously, and without touching its bed in the upper part (schematic)**

To summarize this contribution to glacier mechanics, which attempts not to be polemics, I think that the approximate scheme of perfect plasticity is adequate to explain many observed facts which Nye did not study: (1) overdeepening, (2) too small a thickness of some ice sheets, (3) an angle between surface and bed of about 18° in the first hundred metres of a broad, clean glacier, (4) existence of bulges moving down a glacier faster than the ice, (5) abundance of crevasses where a valley gets narrower, (6) strong upturning of foliation below an ice fall. For this we must not only improve the computation of the plasticity solution, but also take account of the masses of dead ice, of the slipping on the bed and its unequal roughness, and the possibility of discontinuous movements.

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* After making this drawing Professor Lliboutry found striking similarity between it and the figure in Albert Heim's *Gletscherkunde* (Stuttgart) 1885, p. 195—an early and pertinent view on ice movement and banding.—Ed.