CORRESPONDENCE

The Editor,

Journal of Glaciology

SIR,

Theory of glacier variations

J. F. Nye's reply to my criticism of his theory of glacier variations presented at the Obergurgl symposium ([Union Géodésique et Géophysique Internationale], 1963, p. 53–57), shows that he has misunderstood most of my comments. My doubts about the possibility of using this theory to obtain conclusions which correspond to reality stem from what I believe to be the excessive simplifications both of the initial equations of the theory and of the method of solving them.

In his reply Nye acknowledges that in a complete theory it is necessary to start from the equation

$$q = q(x, h, \alpha, \partial^2 h/\partial x^2, \ldots, \theta) \qquad (1)$$

instead of $q = q(x, h, \alpha)$, where q is the ice discharge through the cross-section at distance x, h and α are the thickness and surface slope of the glacier, and θ is the ice temperature. However he denies the necessity of taking into account as independent variables the slope of the glacier bottom β, its width B, and the shear stress at the bed τ, assuming that all their effects are included in hidden form in the variation with x. (At Obergurgl I erroneously included B among the independent variables recognized by Nye; it should be noted that one version of the theory suggested by Lighthill and Whitham is able to take into account only the variation of B with x.)

It is easy to show that, in this question of independent variables, Nye contradicts the initial statement of his own theory, since in general the characteristics mentioned change at a given point in the course on time. The width of a glacier (but not of a glacier valley) usually changes at the same time as its thickness and length, though at a much slower rate; the inclination of the lower surface β changes with any change of regime of floating glaciers and ice shelves even more rapidly than α, and as for the shear stress on the bed τ, although Nye (1960, p. 563) had earlier assumed that $\tau = \rho g h \sin \alpha$, he now believes ([Union Géodésique et Géophysique Internationale], 1963, p. 55) that it is strictly determined by x, h, α, $\partial^2 h/\partial x^2$, etc. This is evidently wrong, since τ undoubtedly changes with time due to changes in the state of the bottom layer: its temperature, water lubrication, bottom roughness, the kind, quantity and distribution of morainic material, etc. It would be desirable, of course, to determine τ as a function of the physical characteristics of the bed and the state of the glacier, but until there is a sufficiently complete and reliable theory of these phenomena, τ must be taken into account as an independent variable. Its value is indeed strictly determined as a boundary condition at the lower glacier surface, but for this it is necessary to know not only the shape and size of the glacier (x, h, $\partial h/\partial x$, $\partial^2 h/\partial x^2$, ...) but the ice velocity V (and temperature) as well. To determine both τ and V from data on glacier shape and size alone, as Nye hopes, is impossible in principle precisely because τ is a function of the physical conditions at the bottom of the glacier.

Neglecting τ as an independent variable is connected with an excessive simplification of glacier mechanics in which it is believed that the state of stress and the strain-rate are dependent only on the thickness and surface slope at a given point. Accordingly, effects of changes in stress and strain-rate transfer from one cross-section to another only indirectly, through the gradual spread of changes in thickness and surface slope. Theories based on such assumptions break down not only at the end of a glacier, as Weertman noted in his contribution to the Obergurgl discussion, but at ice divides, where $\alpha = 0$, and yet $\partial V/\partial x > 0$. In some places we even have flow in the opposite direction from the surface slope, i.e. places where $\alpha < 0$, $V > 0$ (and $\tau > 0$). These widely occurring phenomena prove that the complicated stress state of a glacier is not controlled simply by the values of h and α at a given point, but with direct stress transfer through the whole body of a glacier. Therefore changes can spread from one part of a glacier to another both by means of slow changes in thickness and surface slope ("kinematic waves" and diffusion) and also by rapid changes in the state of stress, and therefore of strain-rate and velocity.

It is quite clear that as soon as θ, B and τ (for glaciers on a solid bed) or β (for floating glaciers) are included in the number of independent variables as well as h and α, it also becomes necessary to include the derivatives of these quantities along the length x of the glacier, because of the concept of interaction between different parts of the glacier. Thus in its complete form the basic equation of the

theory should be much more complicated than that used by Nye. In some cases this equation can probably be simplified by neglecting effects that are slight under certain conditions, but for this convincing, or at least stated, reasons should be given, and Nye has not given these. In his lecture at Obergurgl, Nye obtained quite different results when first-order diffusion of kinematic waves was taken into account, nevertheless in his reply to comments ([Union Géodésique et Géophysique Internationale], 1963, p. 55) he insists on neglecting the dependence on $\partial^2 h/\partial x^2$ because it "merely leads to higher-order forms of diffusion of the kinematic waves".

Despite Meier's ([Union Géodésique et Géophysique Internationale], 1963, p. 52–53) optimistic conclusions from data on the Saskatchewan and Nisqually Glaciers, most observations show no correspondence between changes of ice velocity and discharge on the one hand and glacier thickness and surface slope on the other. As examples one can consider all rapid glacier advances and many slow glacier changes. A well-known example is the advance of the Vernagtferner between 1893 and 1900, when an increase in thickness of 25 per cent was accompanied by a velocity increase of 16 to 17 times at constant general surface slope (Hess, 1904). This means that the velocity would have to increase in proportion with the twelfth power of the increase in thickness instead of the second to fifth power of Nye's theory!

Let us now turn to the method of solving the basic equations. Nye asserts that for the small changes in a (the rate of accumulation), h and q, it is permissible to use perturbation theory and to calculate with a constant datum value of the wave velocity c_0. Taking into account changes in c_0 would merely introduce unwanted second-order terms, and whether the datum state with wave velocity c_0 is ever achieved or not does not matter. One can agree with this if the limits of applicability of the method are clearly defined and not trespassed, but these requirements have not been fulfilled by Nye. According to his own theory when $m = 2$ and $n = 3$ to 4, the wave velocity c is proportional to the $2 \cdot 2$ to $5 \cdot 5$ power of the glacier thickness h. It follows from this that if the error in the determination of h_1 due to change in the wave velocity alone is not to exceed for instance 5 per cent, then, from equation (37) in Nye (1960) the real thickness of the glacier must not differ from h_0 by more than 1 to 2 per cent during the whole time we are interested in. If other sources of error are taken into account it turns out that the prediction is unsatisfactory even for changes as small as this. In any case the method of calculating with a constant c_0 is limited to such small changes that it has no practical value. (The necessity of taking changes in wave velocity into account seemed so obvious to me that in my question at Obergurgl I did not consider the case of very small changes, which has little practical significance, and this caused the misunderstanding over the factor 3 referred to by Nye in his reply.) Despite this limitation, Nye uses his theory to draw conclusions about very large changes in the thickness of glacier snouts (for instance in Nye (1960), p. 568–70).

Nye also fails to understand my doubts about the instability of glaciers in regions undergoing longitudinal compression; the reasons for them are as simple as they are convincing: there are lots of glacier tongues subjected to longitudinal compression and yet which, in spite of this, remain for long periods in a quasi-stationary state. At all events there are no "kinematic waves" on them moving from the accumulation limit and restoring the disturbed balance; instead seasonal fluctuations take place: small advances in winter and retreats in summer. No matter how simply the instability is explained by Nye in Appendix A of his paper (Nye, 1960), if it contradicts reality it must be wrong. In that appendix it is probably the dependence $u \propto h^m$ that is not correct, and in the basic theory, as I have discussed above, there are many doubtful simplifications. I have already noted ([Union Géodésique et Géophysique Internationale], 1963, p. 54) that the precise solution coincides with Nye's only when $u = 0$ and therefore $c = \text{const}$.

The most attractive feature of Nye's theory is the availability of analytical solutions in closed form. However, in Nye's (1960) paper they were obtained because of the extreme simplifications and the particular glacier model. Nye has acknowledged that, with his model of an "ideal glacier" it is difficult to have a realistic, simple and continuous function $a_0(x)$ and a realistic function $h_0(x)$ both at the same time, though he insists that there is no violation of the equation of continuity. However, it is quite evident that with the condition (Nye, 1960, p. 566)

$$c_0(x) = \begin{cases} \epsilon x & (0 \leqslant x \leqslant \frac{1}{2}) \\ \epsilon(1-x) & (\frac{1}{2} \leqslant x \leqslant 1) \end{cases}$$

where ϵ is a positive constant, it is not "difficult" but simply impossible to have realistic functions $a_0(x)$ and $h_0(x)$ without violating the continuity equation. Having no possibility of doing anything

about this, Nye suggests other models in which ϵ is not a constant and dc_0/dx is a continuous function of x, becoming equal to zero at the boundary between the accumulation area (where there is extension) and the ablation area (where there is compression). Naturally these more realistic models are not open to the same objections, but the conclusion that "kinematic waves" arise at the boundary between the extension and compression zones (Nye, 1960, p. 564) does not follow from them. This conclusion arises from the fact that in Nye's "ideal" model there is a violation of the condition $\partial h_1/\partial x = 0$ at the boundary between the areas of uniform extension and compression (i.e. areas where $dc_0/dx = \text{const.}$) which takes place because there is a discontinuity in the function $h_1(x)$ when there is a discontinuity in the derivative dc_0/dx. There are no such discontinuities in reality; the curve of longitudinal strain-rate always passes smoothly through a zero, where, consequently, $dc_0/dx = 0$, and the solution of equation (17) in Nye (1960) becomes $h_1 = a_1 t$ whether one approaches from the positive or the negative values of dc_0/dx. As for the result $\partial h_1/\partial x \neq 0$ with $\partial a_1/\partial x = \text{const.}$, this will be obtained at any point of the glacier where $d^2c_0/dx^2 \neq 0$, quite independently of whether dc_0/dx at the point in question remains positive or negative or whether it changes sign. Thus the formation of moving waves theoretically (and this is in full accord with experience) takes place both at the boundary and also within the accumulation and ablation areas. Thus giving up a physically impossible model while retaining the concept of interaction between the extending and compressive areas is not misleading, but restores the true physical sense of the phenomena.

It should be mentioned that in reality the functions $a_1(x)$ and $u_0(x)$ or $c_0(x)$ are so complicated that this one fact is usually a sufficient obstacle to prevent one obtaining closed analytical solutions with Nye's method, depriving his theory of its most important, but imaginary, advantage.

In view of the unjustified oversimplifications of both the basic equations and the method of their solution, one can affirm that Nye's theory of glacier variations is suitable only for rough evaluations of some components of these variations and cannot be used for any precise analysis. The problem can be solved only by solving the system of equations including the kinematic and dynamic equations (the equations of continuity and equilibrium). If an appreciable change in ice temperature and/or density takes place, the system must also include the equations of energy and/or the thermodynamic equation of state.

Soviet Committee on Antarctic Research,
1-y Akademicheskiy pr. 30A,
Moscow, B-333, U.S.S.R.
7 December 1963

P. A. Shumskiy

REFERENCES

Hess, H. 1904. *Die Gletscher.* Braunschweig, Friedrich Vieweg.

Nye, J. F. 1960. The response of glaciers and ice-sheets to seasonal and climatic changes. *Proceedings of the Royal Society*, Ser. A, Vol. 256, No. 1287, p. 559–84.

[Union Géodésique et Géophysique Internationale.] 1963. Colloque d'Obergurgl (suite). *Bulletin de l'Association Internationale d'Hydrologie Scientifique*, 8e An., No. 2, p. 50–142.

Sir, *Theory of glacier variations; reply to Dr. Shumskiy's letter*

I cannot find any justifiable criticism of my work in Dr. Shumskiy's letter. No one would dispute that the complete equations of the theory, if they could be formulated, would be much more complicated than those I have used. But such a statement can be made of almost any physical theory. Physical theories develop by a process of successive refinement. It may be that Shumskiy is expecting too much of a theory of glacier variations in the present state of our knowledge. Indeed I have some sympathy with his remark that "Nye's theory of glacier variations is suitable only for rough evaluations of some components of these variations and cannot be used for any precise analysis". My own opinion, for what it is worth, is that the theory is suitable for rough evaluation of the major components of these variations. So far as using it for precise analysis is concerned, the best way of testing any theory is to compare its predictions with observation. Then we can look at the discrepancies and try to refine the theory in the places where it most needs improvement. I have made this comparison with observation, with encouraging results,