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.

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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, in the third and fifth papers of the same series (the complete set is Nye, 1960, 1963[a], [b], in press [a], [b]; Shumskiy's criticisms are directed at the first).

The alternative approach, which Shumskiy seems to favour, is to write down basic equations that include more effects of lesser importance. The equations naturally become unmanageable and one remains unenlightened. I prefer to select the one or two dependences which seem the most important, and to try to understand their implications as fully as possible before adding complications. I am sure that this is the better way to make progress.

So much for general principles. Now let me try to reply to Shumskiy's detailed points one by one. He is first concerned that I do not take the width B of the glacier as an independent variable. If we regard the glacier valley as fixed, the width B at the upper surface of the ice at the position x is a function of the thickness of ice h at that place. Thus, if x and h are specified, B is fixed. So in the relation

$$\mathbf{q} = \mathbf{q}(x, h, \alpha),$$

where $\mathbf{q}$ is the discharge and α is the surface slope, there is no need whatever to bring in B; the dependence on B is already included.

There is likewise no need to bring in the slope β of the lower surface of the ice, for this is a function of x only, and is therefore also already included in (1). Shumskiy's remark that "β changes with any change of regime of floating glaciers and ice shelves . . . " suggests that he supposes the theory to be applicable to floating ice. It is not, and was never intended to be.

Shumskiy also wants me to include the shear stress τ on the bed in equation (1). It is quite true, of course, that $\mathbf{q}$ could change because of changes in the state of the bottom layer of the glacier. I readily concede the possible influence of changes in the quantities Shumskiy mentions; we discussed them at Obergurgl. For cold glaciers temperature changes may be important. Changes in water lubrication may be responsible for the so-called catastrophic glacier advances. I should have thought that changes in bottom roughness and in bottom moraine would normally be of less importance. But I continue to think that for the great majority of temperate glaciers, over not too long periods of time, the dependence of $\mathbf{q}$ on h and α probably overshadows any dependence on changes in these other quantities. (On the question of catastrophic glacier advances, which my theory does not cover, Austin Post tells me that on the basis of aerial photographs in Alaska he can recognize about 1 per cent of Alaskan glaciers as being of the type that suffer periodic sudden advances.)

But, if, as a result of observations, one decided that it was essential to bring in changes in the state of the bed, I do not think that taking τ as an independent variable would be a sensible way of going about it. For, even if τ is specified, one still needs to specify the bed roughness, moraine content, and so on, before the discharge due to bed slip is fixed. So Shumskiy's proposal to take τ as a further independent variable does not do the job he wants.

In his fourth paragraph Shumskiy raises the fundamental question of how changes are transmitted from one part of a glacier to another. I have already dealt with this question in my reply to the Obergurgl discussion ([Union Géodésique et Géophysique Internationale], 1963, p. 55) and in the various papers of the series (particularly Nye, 1963[a]), and I do not want to take up space by repeating those arguments here. Instead let me develop a slightly different line of reasoning which I hope may clarify the issue, for I know that Shumskiy is not alone in his doubts on this point.

The question is whether changes can spread along a glacier not only by means of slow changes of thickness and surface slope handed on from one point to another (kinematic waves and diffusion) but also, as Shumskiy asserts, by rapid changes in the state of stress, or by "direct stress transfer through the whole body of a glacier". I do not think this latter type of mechanism is possible. Crudely speaking, the question is to what extent a glacier can transmit a longitudinal stress. My own opinion is that, if you put an extra longitudinal compressive force on a section of a glacier, the glacier will respond by thickening at that place, and that the force will only be transmitted down the glacier for a distance of the order of the ice thickness. Let me now try to make this notion more precise.

Consider a long uniform bar of ice, or other solid material, resting on a horizontal plane. Now push the end longitudinally. The force of friction, opposing the motion, will be proportional to the length of the bar. If the bar is fairly short it will simply move along rigidly in response to the force. But, if the bar is longer than a certain critical length, the force needed to overcome the friction will exceed the yield strength of the bar, and so, instead of translating, the bar will deform plastically. In this case the bar could not be used for transmitting a force along its length. Now, which of the two conditions is appropriate for a glacier? It will be agreed that for a bar of ice of laboratory scale, resting, say, on a laboratory bench at room temperature, the length would have to be very many times the thickness before it ceased to transmit stress. Why should a glacier be any different? An elementary calculation shows that $\sigma = \tau(l/h)$, where σ is the force per unit area applied to the end of the bar, τ is the shear stress set up by friction at the lower surface, l is the length and h is the vertical thickness. When the bar of ice slips on the laboratory bench τ is very small and so l/h can be large before σ is high enough to cause plastic yielding. But we know that in order to cause motion over a glacier bed τ must normally be of the order of 1 bar.* Hence l/h can only reach the value 2 before σ equals 2 bars, which we know produces fast yielding. So a horizontal, parallel-sided, portion of a glacier whose length is only twice its thickness is already unable to transmit a stress. Notice that the crucial point is not the weight of the glacier, but the fact that the "yield stress" has to be reached at the bottom layer if it is to move. The same result would be obtained for the bar of ice on the laboratory bench if it were frozen to the bench; it could not then transmit a longitudinal stress if $l \gtrsim 2h$. In the glacier the greater size gives rise to a substantial hydrostatic pressure that acts in addition to the forces I have been discussing, but, being hydrostatic, it has no effect on the argument. Thus, for a portion of a glacier with a horizontal surface on a horizontal bed to be pushed along by a neighbouring part seems to be mechanically impossible if l is more than a few times the thickness. What would happen is that the level region would thicken, so as to form a sloping surface, which would then provide a shear stress on the bottom. This in turn would constitute a driving force for forward motion. This is precisely the type of transmission mechanism implied by kinematic waves. In view of these arguments I cannot agree with Shumskiy that there can be direct stress transfer through the whole body of a glacier. If Shumskiy can produce examples of flow against the direction of the surface slope over a distance substantially greater than the ice thickness, let him give the exact references. He does not seem to realize that such places would constitute a first-class mechanical problem worthy of the closest study.

In Shumskiy's fifth paragraph he says I obtained "quite different results" when first-order diffusion of kinematic waves was taken into account, and so I ought not to neglect higher-order forms of diffusion. As each new dependence in formula (1) is brought in it leads not to "quite different results" but to a refinement of our understanding. If we take $\mathbf{q}$ as a function of h alone, we learn about the existence of kinematic waves and how to estimate their velocity. If we add the dependence of $\mathbf{q}$ on x we learn that the wave velocity depends on x, and we see how this can lead to temporary instability. When we add the dependence of $\mathbf{q}$ on α we learn about diffusion of the kinematic waves. The fact that there is diffusion does not nullify the concept of kinematic waves—it means that we know more about their behaviour. If higher-order forms of diffusion were taken into account we should know still more. In this type of approach obviously some results are more trustworthy than others. For example, one knows, without doing any calculations, that corners on profiles will not be infinitely sharp; diffusion will see to that. Similarly, abrupt changes of curvature of profile will be prevented by higher-order diffusion. A modicum of physical intuition is needed in assessing the results from any physical theory.

On Shumskiy's paragraph 6 let me repeat that I do not think the kinematic wave theory in its present form is applicable to "catastrophic advances". If he would bring together and give references to the "many slow glacier changes" which refute the basic assumption of equation (1), and also those that support it, he would be doing a service of real value.

The criticism then turns to the method of solving the equations (paragraph 7). In Nye (1960) the treatment is not restricted entirely to small perturbations; for example, equation (24) has no restriction; likewise, for no diffusion, p. 569 gives a method applicable to large perturbations. Nevertheless it is true that the greater part of the paper and all the subsequent papers are based on small perturbation theory. This does not in the least mean that the theory has no practical value. You must learn to walk before you can run. If we do not understand how small perturbations behave, what hope is there of understanding large ones? So far as applications are concerned the only example Shumskiy gives where I am supposed to have applied linear perturbation theory outside its proper range is on p. 568–70 in Nye (1960). Here the response to a uniform increase a_1 of accumulation rate is discussed. But the response can be kept as small as one wishes simply by keeping the driving term a_1 as small as is necessary. So there is certainly no transgression up to this point. One then finds that the linear term (the response given by linear perturbation theory) is much larger near the snout of the glacier than higher up. It seems quite permissible to conclude qualitatively that there will be a comparatively large response near the snout of a glacier. It is, of course, conceivable, but surely very unlikely, that when higher-order

*This may not be true during a catastrophic advance and the argument then fails.

perturbation terms are taken into account this result would be changed. If the linear term is large one can be fairly sure that the total effect will be large.

I remain quite unconvinced by Shumskiy's arguments in his paragraph 8. I think some of the trouble may arise because he tries to press one particular model, that on p. 566–570, too far. This model was chosen because it was easy to analyse, and because it shows very clearly how it is possible for a stretch of glacier undergoing longitudinal compression to be unstable, and yet at the same time how the glacier as a whole is stable. In one sense kinematic waves arise at *all* points and all times. It happens that because of the artificially sharp change in wave velocity gradient at the mid-point a sharp step-wave arises at this point. We both agree about this. I should not expect to find such a sharp wave on a real glacier; diffusion would flatten it out, and the variation of wave velocity with distance is more complicated than in the model. This does not mean that the model is of no value. On the contrary, it increases understanding of what would otherwise be a puzzling question.

The fact that Shumskiy cannot find kinematic waves on glacier tongues is not at all surprising. If one uses the word wave in the sense of a travelling undulation it becomes apparent that because the driving term $a_1(x,t)$ is usually rather uniformly spread over the whole length of the glacier the wavelength of most waves present will be quite long—comparable with or greater than the length of the glacier itself. Page 578, para. (i), of Nye (1960) explains exactly how the seasonal fluctuations of a glacier tongue, which Shumskiy describes, take place in a way quite compatible with the kinematic wave equations. It would not normally be easy to detect waves by casual observation. The analytical solution in Nye (1963[a]) is helpful in forming a mental picture. The response of a particular glacier to different frequencies is there expressed in a single equation, (44). Although the equation is a solution to differential equations which describe wave propagation and diffusion, it would not be very obvious to the eye examining successive profiles that one was really observing these two processes at work (for one thing, the profiles of $h_1(x)$ are all linear). This is an extreme case, but it must be understood that wave propagation down glaciers, especially since it is combined with diffusion, is not quite the clear-cut process that it is sometimes supposed to be.

Shumskiy's repeated remark that there is some "precise" solution that coincides with mine only when $u = 0$ leaves me baffled.

It is good of him to say that the most attractive feature of the theory is the availability of analytical solutions in closed form. I feel myself that an equally strong point of the theory is that, with the aid of computers, it can be applied numerically to real glaciers (Nye, 1963 [b], in press [a], [b]), and thereby can be directly confronted with observation.

In Shumskiy's paragraph 9 he says it is "quite evident" that it is "simply impossible" to have realistic functions $a_0(x)$ and $h_0(x)$ without violating the continuity equation. If he just means that my choice of $c_0(x)$, with a discontinuity in dc_0/dx, violates continuity, I fully agree with him and simply repeat that the discontinuity was never intended as a literal representation of nature. But I think he means more than this—that this choice of $c_0(x)$ violates the equation of conservation of volume. Here I must continue to disagree. Although the question is really quite academic, as I explained before, I cannot resist accepting his challenge of the "simply impossible". There are innumerable ways of doing it. Consider, for instance, $a_0(x) = A - Bx^2$, where A and B are positive constants. Then h_0, defined by

$$h_0(x) = \frac{4}{c_0(x)} \int_0^x a_0(x)\,dx,$$

will be found to be given by

$$h_0 = \begin{cases} C(L^2 - x^2) & (0 \leqslant x \leqslant \tfrac{1}{2}), \\ \dfrac{Cx(L^2 - x^2)}{1 - x} & (\tfrac{1}{2} \leqslant x \leqslant L < 1), \end{cases}$$

where L is the length of the glacier and C is a positive constant. This gives a non-zero h_0 at $x = 0$, as at an ice-divide, and then a decrease. At the terminus $x = L$ it gives $h_0 = 0$, with a rounded wedge-shaped profile of the conventional sort. If one wishes to have dh_0/dx positive at $x = 0$ one simply adds a positive linear term to $a_0(x)$. All sorts of variations on this theme are possible.

I have answered Dr. Shumskiy's criticisms point by point; so far as I can see there is not one that stands up to examination or advances our knowledge of these questions. Realizing that many of the criticisms arise from a plain lack of understanding of my general approach, I have done my best to bring us into closer accord, at the risk of taking up too much space from an indulgent Editor. My purpose is not only to make quantitative predictions, but also to enhance understanding of the mechanism of glacier variations. This latter purpose is best achieved by making judicious simplifications. For the quantitative predictions I refer Dr. Shumskiy to the later papers of the series.

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Sir, *Water-spouts on the Britannia Gletscher, north-east Greenland**

Wiseman's (1963) letter to this *Journal* describing a water-spout on the Aletsch Gletscher reminded me of the water-spouts encountered by members of the British North Greenland Expedition (Simpson, 1955) near the snout of the Britannia Gletscher in the summer of 1954, and prompted me to exhume two photographs from my files (Figs. 1 and 2). These water-spouts were not intermittent like those described by Wiseman (1963) and Rucklidge (1956), but were continuous gushers lasting for several days, and forming an integral part of the drainage pattern of the glacier. They are thus more akin to the spouts described by Glen (1941), who stressed the role of crevasses in englacial and subglacial drainage and stated that sometimes the water carried in this way from higher levels "attains such a pressure that it literally bursts its way through the ice, sending up a small water-spout which may continue for as along as an hour, then dying down into a more gentle fountain".

The Britannia Gletscher in Dronning Louise Land is about 14 km. long and 8 km. wide, with a snout fanning out in piedmont fashion (now much reduced). The eastern side of the glacier flows into Britannia Sø. A detailed map of the glacier and its environs is given in the account by Hamilton and others (1956) of the expedition's research, and in a paper by Lister and Wyllie (1958) there is a good view (fig. 24) of the lower part of the glacier photographed from a vantage point 500 m. above it. The map and the photograph show a well-defined radial drainage pattern, with many melt-water streams deeply incised. Roughly concentric with the snout of the glacier, and transverse to the radial drainage, there is a series of markings on the surface which appear at close quarters to be small scarps, with dip slope down-glacier. Small features of this kind are visible in Figure 1, trending from lower right to upper left of the picture. These are probably the surface expression of shear planes dipping up-glacier. The Britannia Gletscher is not heavily crevassed, and one can walk over the greater part of it without encountering crevasses more than a foot or two (half a metre) in width at the surface. Only one moulin was observed by expedition members, at a high level on the glacier.

The largest spout encountered is shown in Figure 1. This occurred about 0·5 km. from the snout, on the eastern half of the glacier, and its direction followed the radial drainage pattern. It was observed to flow continuously for several days after it was discovered, and although there was presumably some diurnal variation in response to changes in ablation rate, this was not observed. The trajectory of the water gushing from the glacier indicates that the englacial stream rose through the glacier at an angle of about 30 degrees to the horizontal. It is unlikely that a crevasse would guide exit at this angle, and it is more likely that the feature controlling the upward flow of water is one of the shear surfaces described above.

Figure 2 shows a smaller spout near to the eastern side of the glacier, only a few hundred metres

* Contribution No. 64–9 from the College of Mineral Industries, The Pennsylvania State University.