SIR, *Response to Dr G. de Q. Robin's plea for fixed axes of reference in glacier mechanics*

Dr Robin and I both agree that the essential point is whether the use of surface-oriented axes makes the phenomenon easier to understand. Robin maintains that with this system the variables, such as σ_z, change in meaning from point to point, whereas with horizontal and vertical cartesian axes variables like σ_z "have the same meaning throughout the analysis of a complete profile". I do not think that such a distinction can be made. Both my variables and Collins's variables retain their meanings throughout, simply because both of us define them precisely. It is true that Collins's σ_z retains the same direction while my σ_z does not. But my σ_z retains the meaning that it is the stress component normal to the upper surface, while Collins's σ_z retains the meaning that it is the vertical stress. Both meanings are readily understandable, but when it comes to interpreting changes in the variables I believe that it is, in fact, more useful to define the variables on my system of axes; for example, in my system σ_z is always the same (equal to atmospheric pressure) on the upper surface, whereas in Collins's system σ_z on the upper surface changes in a complicated way. Similarly, in my system it is easy to understand the fact that τ_{xz} on the upper surface is constant (zero in fact), whereas in Collins's system τ_{xz} on the upper surface undergoes changes that are comparatively difficult to comprehend. Or take $\dot{\epsilon}$; in my system it is the surface strain-rate measured parallel to the surface, and it retains this meaning throughout the analysis of a profile; it is at least as simple to think of changes in this surface strain-rate as it is to think of changes in horizontal strain-rate. Such considerations are merely suggestive of the value of a surface-oriented coordinate system; the full justification is the comparative simplicity of the final result when expressed on this system.

The same point may be made in another way that is not as frivolous as it may at first appear. Robin's plea is for "fixed axes of reference", that is for a set of rectilinear cartesian axes that are fixed in direction throughout the whole profile. But he does not, in fact, obey his own prescription. Owing to the curvature of the Earth the direction of the vertical changes along his measurement line by 7×10^{-3} radians, which is seven times his maximum error in slope measurement. Tacitly he allows his reference frame to rotate from point to point (and he neglects the small additional terms this produces in the equations of equilibrium) because he realises that there is nothing to be gained, and much to be lost, by adherence to a strictly rectilinear set of axes. He is thus really using axes that change in direction as one moves along the profile. A correspondent might then complain that since his axes are not fixed it is difficult to comprehend the magnitude of the changes in the parameters he uses. Robin could quite properly reply that it is easier to comprehend, for example, the change in σ_x on his system, that is the change in the horizontal stress defined with respect to the local horizontal, than the change in σ_x on a system of strictly rectilinear axes, where the x axis would make a varying angle with the horizontal. Thus, by choosing axes following the upper surface of the ice sheet, I am carrying one stage further a procedure that Robin himself uses when he takes rectilinear axes following the geoid. The reason it is worthwhile taking this further step is that the final equation is a local equation in the sense that it contains only quantities at fixed x, and not, for example, any quantities integrated over x. The local equation appears to take its simplest form on surface-oriented axes, and the changes in quantities defined on these axes seem to me to be, if anything, more comprehensible than the changes in quantities defined on fixed axes. Whether the changes in surface slope are rapid or not does not seem to make any difference to these arguments.

Dr Robin also questions the practical advantage of surface axes; he states that the field measurements "all use a fixed system of coordinates". This remark is puzzling, for the measurements Robin uses appear to consist of slope values, values of accumulation rate, and thickness measurements along a line in Greenland. Whether Δx is measured horizontally or parallel to the upper surface can make little practical difference in the computations. Similarly, I question whether the thickness measured vertically is significantly different from the thickness measured perpendicular to the top surface. The practical application to Robin's data of the equation in my coordinate system would therefore appear to be virtually identical to the computation he has already made.

If measured, rather than estimated, longitudinal strain-rates are to be used, a direct method would be to make repeated measurements (by tellurometer) of the distances between stations along the line. This gives $\dot{\epsilon}$ in my notation directly, and hence $\Delta\dot{\epsilon}$ as the difference in strain-rate between successive intervals. The stations selected define a smoothed upper surface, and it is with respect to this surface that the axes are defined. If, on the other hand, you first measure by some means the horizontal and vertical velocity components for the stations (which is a much more difficult task) a direct computation

https://doi.org/10.3189/S0022143000023637 Published online by Cambridge University Press

gives $\dot{\epsilon}$ parallel to the surface for each interval, and hence the difference $\Delta\dot{\epsilon}$ between successive intervals. Thus no transformation of coordinates appears to be necessary.

In short, I believe that my equations (which are the same for small slopes as those of Budd, but with the variables more precisely defined) *can* be applied directly to the field measurements. Indeed they have already been applied—by Robin and Budd.

H. H. Wills Physics Laboratory,
University of Bristol,
Tyndall Avenue, Bristol BS8 1TL, England
13 November 1969
J. F. NYE

SIR, *Glaciation of the north-western part of the Canadian Arctic Archipelago*

In a recent paper (Paterson, 1969), I stated that the islands in the north-western part of the Archipelago are marked "unglaciated" on the *Glacial map of Canada*. I am grateful to Dr W. Blake, Jr for pointing out that, whereas my statement is true for the 1958 edition of the map, it is not true for the latest (1968) edition. On the 1968 map, only Banks Island and a small part of southern Melville Island are marked "unglaciated". It is now believed that the remainder of the islands were covered by a large ice sheet during the last glaciation (Blake, 1970).

Polar Continental Shelf Project,
Department of Energy, Mines and Resources,
Ottawa, Ontario, Canada
17 December 1969
W. S. B. PATERSON

REFERENCES

Blake, W., *jr.* 1970. Studies of glacial history in Arctic Canada. I. Pumice, radiocarbon dates, and differential post-glacial uplift in the eastern Queen Elizabeth Islands. *Canadian Journal of Earth Sciences*, Vol. 7, No. 2, Pt. 2, p. 634–64.

Paterson, W. S. B. 1969. The Meighen Ice Cap, Arctic Canada: accumulation, ablation and flow. *Journal of Glaciology*, Vol. 8, No. 54, p. 341–52.

SIR, *Distorted ice stalactites as indicators of glacier movement*

During August 1969 we were working in the Mt Castleguard locality, Alberta, Canada. The mountain stands at the eastern end of the Columbia Icefield and hosts a number of small temperate glaciers of its own. One of these terminates at an altitude of 8 200 ft (2 500 m) upon a broad gentle limestone bench. There was an ice cave at the snout which could be followed up the line of ice flow for approximately 100 m. A cross-section is given in Figure 1. Inside the cave, rock and glacier-ice surfaces were decorated with abundant sublimation ice deposits.

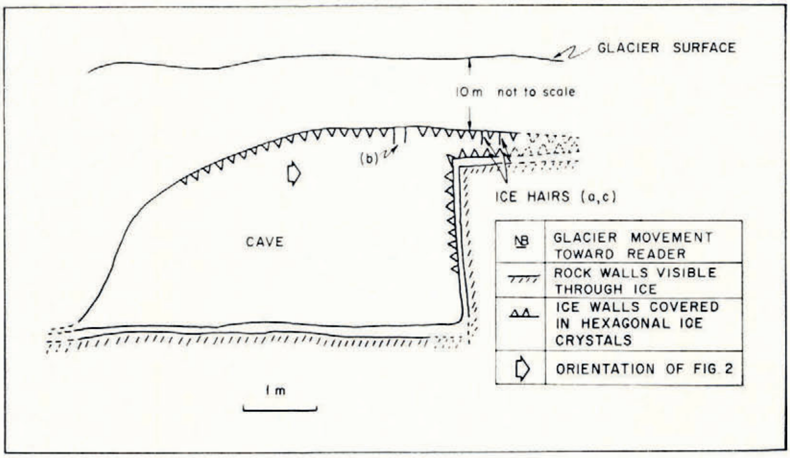

Fig. 1.

https://doi.org/10.3189/S0022143000023637 Published online by Cambridge University Press