CORRESPONDENCE

The Editor,
Journal of Glaciology

SIR,

Isotopic fractionation at the base of polar and sub-polar glaciers

In their paper, Boulton and Spring (1986) use the well-known Rayleigh model to interpret the oxygen-isotopic composition of basal ice from Byrd Station and to discuss a possible application for other oxygen-isotopic profiles from polar glaciers. There are some limitations to the use of their approach that I would like to point out.

1. While the fractionation at the water–ice interface is always given by the equilibrium fractionation coefficient (1.003 for ^{18}O), the amount of observed fractionation is dependent on the freezing rate. This is due to the fact that the water close to the ice interface is more or less depleted in heavy isotopes. The controlling factor is the ratio between the diffusion coefficient of $H_2{}^{18}O$ in water and the freezing rate. As pointed out by Posey and Smith (1957), the consequence is that the ice is more or less enriched in ^{18}O. The observed separation will be different from the true value. The isotopic range between the value at a certain percentage of freezing – that is for the authors at a certain value of discharge reduction – and the value of the initial water is thus not fixed unless diffusion phenomena and freezing kinetics are considered. It would only be fixed if water is always completely homogenized during freezing. This limiting case would give a Rayleigh distribution for an infinitely low freezing rate. Of course, this is probably never realized in Nature. The Jouzel and Souchez (1982) approach was concerned with a co-isotopic study – both D/H and $^{18}O/^{16}O$ – and put forward the concept of the slope of the freezing process on a δD–$\delta^{18}O$ diagram. This slope is not dependent on the freezing rate but the relative positions of the points representing a certain percentage of freezing. If we refer to figures 2 and 3 of the paper by Souchez and Jouzel (1984), the apparent fractionation coefficients can be very different from equilibrium values in freezing experiments but the slope $(\alpha - 1)/(\beta - 1)$ is the same. This phenomenon has to be taken into account for a precise interpretation of the isotopic composition of basal ice.

2. If an open system is considered, then another difficulty arises. Souchez and De Groote (1985) showed that the freezing slope increases if initial water is mixed with isotopically more negative water in the course of freezing. Thus, the range of values for a single isotopic ratio between the value at a certain percentage of freezing and the value of the initial water will be different if there is some mixing or if there is none. By a study of a single isotopic ratio, it is not possible to tell whether the isotopic profile in basal ice is only due to fractionation by freezing without any mixing. Obviously, a co-isotopic study of basal ice from Byrd Station would be of great value. I am aware that the δD values of basal ice from Byrd Station are probably not available.

I am not sure that, at the present day, a theory can predict that the difference between subglacial water and the basal part of normal ice at Byrd Station would be $5.4^0/_{00}$ and that the difference between $\delta^{18}O$ values for subglacial water and the glacier sole with which it is in contact would be $3^0/_{00}$ for the two reasons given above. One cannot exclude the possibility that the slight difference obtained by the authors is purely coincidental, as other combinations of factors would allow one to reach the same result. I do not yet see a means of testing this further with a single isotopic approach.

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SIR,

Winter-talus ridges, nivation ridges, and pro-talus ramparts

In a previous letter to the *Journal of Glaciology*, D.R. Butler (1986) drew attention to the apparent primacy of R.A. Daly (1912) in describing "winter-talus ridges", features that are now usually referred to as pro-talus ramparts. Butler also suggested a return to Daly's original terminology. His letter raises a number of interesting issues.

First, R.A. Daly was apparently *not* the first person to provide a written description of pro-talus ramparts, though he may well have inferred their mode of formation independently. It is often difficult to establish primacy in scientific explanation, but it is clear from a number of accounts that certain geologists and geographers working in the British Isles were aware that ridges could accumulate at the foot of perennial or even possibly late-lying snow beds several years before Daly's description was published. For example, in an account of the various types of moraine found in the English Lake District, Ward (1873, p. 426) described a separate category consisting of

> "mounds of scree material formed at the base of a slope, by the sliding of fragments over an incline of snow lying at the base of crags",

and noted that he was indebted for this suggestion to

> "Mr Drew, late of Cashmere ... he having seen mounds of this kind at the foot of snow slopes among the Himalayas".

A quarter of a century later, Marr and Adie (1898, p. 56) described a possible example of what would now be termed

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