CORRESPONDENCE

The Editor,

Journal of Glaciology

SIR, *Englacial debris in glaciers*

One or two comments are necessary in view of Boulton's (1971) letter in reply to my original commentary.

The first point is that my observations on the sharp contact between ice and the underlying till is not irrelevant because I am talking about continuous ice-cliff sections cut by lateral melt-water channels that are 1–3 km in length. These sections indicate that the englacial debris planes for these glaciers are derived from within the first 400 m of the margin. In one or two instances, I have noticed *isolated* debris bands further up-glacier that probably do reflect the incorporation of debris by a freeze-thaw mechanism. The second point is that my reason for commenting on Boulton's paper was to point out that in terms of glaciers on Baffin Island, the *absolute* amount of englacial debris is not large. In fact, I have recently calculated on the basis of the volume and age of moraine debris that the average rate of glacial erosion in the area is only 50 mm/1 000 years. If we take a glacier 1 km^2 and allowed 100 years occupancy time, and further say that all englacial debris is restricted to the ablation zone, then on the basis of these figures there is *only* 5 000 m^3 of debris entrained in the glacier at any one time. This means that the average concentration of the debris is approximately 0.05% by volume in the ablation zone.

I realize that Boulton's statement about the very considerable difference between sub-polar and temperate glaciers is partially couched in relative terms, but I am concerned that this *relative* expression should not be construed to indicate large *absolute* amounts in the case of Baffin Island glaciers. The third point is something of a query, but I think it does have relevance. On a qualitative basis, as a result of looking at late-glacial end and lateral moraines in the Rocky Mountains National Park, Colorado, Arctic Canada, Norway, Italy and the United Kingdom, there appears to be a direct proportional relationship between the size of these moraines and the present activity index. In other words, moraines that I have examined in temperate areas are considerably larger than those in arctic glaciers (that is, for late-glacial moraines that have no ice core). If these end- and lateral-moraine complexes are formed from englacial debris, as I think they must be, then this relationship appears to be opposite from that suggested by Boulton.

In case there is any doubt, I should add that I think Boulton's paper is extremely important and any questions that I am raising should be construed as being made in a constructive manner.

Institute of Arctic and Alpine Research,
University of Colorado,
Boulder, Colorado 80302, U.S.A.
11 May 1971

JOHN T. ANDREWS

REFERENCE

Boulton, G. S. 1971. Englacial debris in glaciers: reply to the comments of Dr J. T. Andrews. *Journal of Glaciology*, Vol. 10, No. 60, p. 410–11. [Letter.]

SIR, *Englacial debris in glaciers: reply to the comments of Dr J. T. Andrews*

Allow me to make three points which I hope will clarify my position for Dr Andrews:

1. As a matter of observation I have suggested that cold glaciers and temperate glaciers transport subglacially derived debris in rather different positions. In the former, it is disseminated through a relatively large thickness of basal ice (100–200 ft (30.5–61 m) on the Barnes Ice Cap), and in that ice has bulk concentrations which in most cases average about 5% by volume, although individual debris bands may contain much higher concentrations. In temperate glaciers, the debris tends to be restricted to a thin basal layer rarely more than 1 m thick. I have not attempted to contrast the debris discharge of these glaciers, but the position in which debris is carried. Dr Andrews' calculation of a 0.01% debris content in the whole of the ice of the ablation area is interesting but not pertinent to my suggestion.

2. If the above contrast exists, what is the reason for it? I have suggested that the difference in thermal regimes may be responsible, and have proposed mechanisms of inclusion beneath cold ice in Spitsbergen. Dr Andrews says that the debris in the Barnes Ice Cap is incorporated in the terminal zone. It would be very interesting to have details of the sections quoted by him and an opinion of whether they are compatible with Weertman's (1961) hypothesis for Baffin Island glaciers of an origin by basal freezing.

3. In answer to Dr Andrews' query. It is true that many temperate glaciers have large terminal and lateral ice-cored moraines. I believe this stems from the fact that many such glaciers are valley glaciers in which englacial debris is introduced not from the bed but from valley sides, nunataks and cirque headwalls. Where these latter features do not occur, there is almost no englacial debris above the basal layer. Cold ice caps with no source of supraglacial material do, however, contain englacial debris and produce large terminal ice-cored moraines.

School of Environmental Sciences,
University of East Anglia,
Norwich NOR 88C, England
25 May 1971

G. S. BOULTON

REFERENCE

Weertman, J. 1961. Mechanism for the formation of inner moraines found near the edge of cold ice caps and ice sheets. *Journal of Glaciology*, Vol. 3, No. 30, p. 965–78.

SIR,

On present-day glaciers in the U.S.S.R.

In a recent paper Grosval'd and Kotlyakov (1969) review various Soviet glaciological projects. In particular, they discuss mass balance figures for Lednik IGAN, in the Polar Urals, computed for the period since 1818 using meteorological data recorded at the weather station at Syktyvkar. They place stress on the fact that there appears to exist a 22-year periodicity in the plot of the 10-year running mean of net mass balance and they speculate on the relationship this bears to the 22-year fluctuations in solar activity. In view of the interest of the current IHD in the glacier-climate problem these results of Grosval'd and Kotlyakov merit some discussion.

First, the relationship used (maximum snow accumulation *versus* sum of the average monthly temperature and the total ablation *versus* the sum of mean monthly temperatures for the summer) seem rather too simple *a priori* to give a good "explanation" of the data observed over only 11 years of actual field work. The authors claim that the correlation is "satisfactory" but do not mention the form of relationship (regression equation) or discuss the "goodness" of fit of the data to the predictive equations. It is not stated whether the computed correlation coefficients were reduced to take account of the probability of an observed higher correlation arising randomly from a universe with a lower true correlation. With a sample size of 10 an observed correlation of 0.90, for example, must be reduced to 0.72 (which would "explain" just less than half of the observed data) for significance at the 5% level (Ezekiel and Fox, 1967, p. 294).

Secondly, it is not clear whether the meteorological data, used in establishing the relationships mentioned and valid for 11 years, were collected over or near the glacier or at Syktyvkar, but in the former case there would be need for a relationship between meteorological parameters at the glacier and at Syktyvkar. In this connection it might be mentioned that Syktyvkar is apparently located about 900 km away from Lednik IGAN and on the opposite side of the Ural Mountains (the authors do not say where Syktyvkar is).

Further, by plotting the extrapolated mass-balance data in 10-year running mean form, the authors encounter difficulties due to the Slutzky effect (Slutzky, 1937; Mitchell and others, 1966) whereby a running mean acts as a mathematical filter. In general, a time series will consist of a sum of Fourier components (a purely random series will have a white spectrum) and the filter will selectively amplify or damp constituent components according to frequency to give rise to a new filtered or distorted time series. The authors' graph of the 10-year running mean net budget represents a filtered time series and the prominance of the 22-year cycle is probably due to this filtering effect, likewise the phase difference between the 10-year mean for Lednik IGAN and the 5-year mean for the Grosser Aletschgletscher.