

Mitchell and others (1966) and Lliboutry (1964–65, Tom. 2, p. 829–32) outline procedures by which the unfiltered time series could be analysed for periodicity and trend if this were felt desirable in the light of the objections previously raised.

With regard to the question of a relationship between solar activity and variations in glacier net balance it should be axiomatic that, if a relationship does exist, it is by way of the atmospheric circulation. However, the problem lies in relating variations in radiation from the sun in the ultra-violet region of the spectrum (absorbed chiefly by ozone and oxygen in the stratosphere and thermosphere respectively) and variations in corpuscular radiation (interacting with the magnetic field of the earth) with gross circulation changes in the troposphere.

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SIR,
*Refraction seismic investigation at Zemu Glacier, Sikkim:
comments on the paper of R. N. Bose, N. P. Dutta and S. M. Lahiri*

It happens rather often that former glaciological work has been done in vain because succeeding explorers do not know about it. This is particularly bad in cases when such things as special glacier maps remain unknown, which could provide valuable information and data for following studies.

The seismic investigations at Zemu Glacier, Sikkim, (Bose and others, 1971) may be an example. The authors have carefully studied and used the work of several predecessors, but have obviously not known and therefore not used the glacier map of the Zemu which has been published together with a detailed article on the same glacier (Wien, 1933). The map shows the state of the glacier in 1931 and, together with other data in the article, can be compared with the results of the recent work. To give an example: the seismic cross-section measured in 1965 could be studied together with the velocity profiles across the glacier measured by photogrammetry in 1931 at altitudes of 5 250 and 4 600 m, the latter rather near the recent seismic section at 4 500 m. The ice depth of 310 m at this section (Bose and others, 1971, fig. 9, p. 119) shows that the thickness of 220 m which Finsterwalder (1933) computed by using Somigliana's and Lagally's formula for a place higher up and nearer the equilibrium line (at about 5 400 m) is probably much too small. This is another example of the fact which has become well known in the meantime, that the ice depth cannot be determined from values of the surface velocities.

Perhaps a bibliography of glacier maps and other existing data arranged in a geographical order could change the present unsatisfactory situation, as a result of which valuable glaciological work remains unused. Such a bibliography could be confined to remote regions where literature is obviously difficult to discover and to obtain. It should contain old reports, papers, sketches, pictures, and maps, together with complete references and short notes about the information which can be expected. Such a survey on former literature would be a valuable contribution to the IHD.

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SIR, *A technique for producing strain-free flat surfaces on single crystals of ice*

The paper by Tobin and Itagaki (1970) reminds me of a possibly useful experiment made many years ago in a cold room at the U.S. Snow, Ice and Permafrost Research Establishment. Rods $5 \times 1 \times 0.5$ cm were cut from a Mendenhall Glacier single crystal with the long dimension approximately parallel to the c -axis. At -10° C the rods were broken by slowly pressing over the edge of a bench. They usually broke in cleavage normal to the c -axis, yielding apparently perfectly flat mirror surfaces.

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REFERENCE

- Tobin, T. M., *and* Itagaki, K. 1970. Instruments and methods. A technique for producing strain-free flat surfaces on single crystals of ice. *Journal of Glaciology*, Vol. 9, No. 57, p. 385-90.

SIR, *A technique for producing strain-free flat surfaces on single crystals of ice :
 comments on Dr H. Bader's letter*

I have tried the method described in the letter by Dr Bader although my previous experience with the method never produced satisfactorily flat surfaces. Usually the upper half to two thirds of the fractured surfaces were flat and mirror-like. However, careful observation of about a dozen surfaces examined revealed that none were crystallographically flat and smooth, unlike the cleaved surfaces of NaCl. The surfaces were always slightly curved and often showed surface patterns resembling conchoidal cleavage or Lüder lines. Consequently, the method was inadequate for the surface self-diffusion study. I wonder if the method can produce perfectly flat surfaces of appreciable size and if so, with what probability.

Micro-hardness tests were conducted immediately after preparation by Bader's method and again about 24 h later. I found no appreciable difference between them and the value agreed with the freeze tap method, which indicates that the surface is strain-free. The thermal etch pit density was $3 \times 10^4/\text{cm}^2$, typical of the bulk density of this type of specimen and no appreciable change is observed during sublimation, which supports the conclusion that the method is strain-free.

Bader's method may be useful in certain cases because it is strain-free, although the size and orientation of the surface is limited, and the waste of the material is quite large.

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