Meier, M. F. 1960. Mode of flow of Saskatchewan Glacier, Alberta, Canada. *U.S. Geological Survey. Professional Paper* 351.
Schwarzacher, W., *and* Untersteiner, N. 1953. Zum Problem der Bänderung des Gletschereises. *Sitzungsberichte der Österreichischen Akademie der Wissenschaften*. Mathematisch-naturwissenschaftliche Klasse, Abt. 2A, Bd. 162, Ht. 1–4, p. 111–45.
Swinzow, G. K. 1962. Investigation of shear zones in the ice sheet margin, Thule area, Greenland. *Journal of Glaciology*, Vol. 4, No. 32, p. 215–29.
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.
Weertman, J. 1968. Diffusion law for the dispersion of hard particles in an ice matrix that undergoes simple shear deformation. *Journal of Glaciology*, Vol. 7, No. 50, p. 161–65.

SIR,

Origin of foliation in glaciers: reply to comments by M. J. Hambrey

We welcome Hambrey's comments on our paper, and are interested to find that he was independently coming to many of the same conclusions which we reached. Indeed, some of his earlier observations (Hambrey, 1975, 1976[a], [b]) helped convince us that our own approach to the origin of foliation was reasonable.

The deformational history of an element of glacier ice is normally very complex, particularly when changes in regime are considered (see, for example, Hudleston (1976)). Thus we are not disturbed by Hambrey's list of specific examples of features which do not appear to fit our thesis. In the second paragraph for example, has Hambrey considered the possibility that the coarse debris in marginal areas of glaciers (we presume he is referring to valley glaciers) accumulated by falling from the valley sides in the accumulation area, and thus was originally conformable with sedimentary stratification there? Alternatively it could have been eroded from the subglacial parts of the valley sides, presumably by freezing of water in areas where P–T conditions were appropriate. Also, in the second paragraph, has he considered the high probability that the roughly-vertical foliation observed near gently-inclined valley sides took on its vertical orientation some place further up-glacier and at some depth below the surface? In the third paragraph the change in bubble concentration with increasing age can be attributed to displacement of air by percolating melt water, a process which does not imply bubble migration and which is common on the Barnes Ice Cap. Crevasses provide a particularly good access path for such water.

At the end of his fifth paragraph, Hambrey cites several examples of possible shear displacements. A problem with any discussion of such displacements in ice is the lack of agreement on what constitutes a discrete "shear plane". Do the structures to which Hambrey refers involve displacement discontinuities, as in true faults, or do they involve shear zones, a few centimeters in thickness, with continuity of displacement across them? We have observed examples of the latter (Hudleston, 1977) and proposed in our original paper (Hooke and Hudleston, 1978, p. 292) that such structures could be responsible for some cross-cutting foliations. With regard to the former, apparent displacements across discrete planes can result from opening and subsequent closing of tensional fractures (crevasses). Could the structures Hambrey describes be of either of these types? Our main concern is that the term "shear" has been used loosely in the past, and in ways suggesting fault-type displacements. We question whether such displacements occur under appreciable thicknesses of ice, as has been implied in discussions of debris entrainment.

In discussing entrainment of debris by a shear process, Hambrey refers to his paper with Müller (Hambrey and Müller, 1978) in which they discuss movement of dirty ice over clean ice as a result of the fact that ice higher in a glacier moves faster than that below, particularly where the latter is frozen to the bed. We have no problem with this type of process; it is, in fact, central to Hooke's (1973) model for development of ice-cored moraines. It is not, however, the process usually called to mind by the phrase, "The debris was sheared into the ice", which is so often heard, and we think it a disservice to equate the two.

We apologize for omitting Hambrey (1976[a]) from our reference list.

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REFERENCES

Hambrey, M. J. 1975. The origin of foliation in glaciers: evidence from some Norwegian examples. *Journal of Glaciology*, Vol. 14, No. 70, p. 181–85.
Hambrey, M. J. 1976[a]. Debris, bubble, and crystal fabric characteristics of foliated glacier ice, Charles Rabots Bre, Okstindan, Norway. *Arctic and Alpine Research*, Vol. 8, No. 1, p. 49–60.
Hambrey, M. J. 1976[b]. Structure of the glacier Charles Rabots Bre, Norway. *Geological Society of America. Bulletin*, Vol. 87, No. 11, p. 1629–37.
Hambrey, M. J., *and* Müller, F. 1978. Structures and ice deformation in the White Glacier, Axel Heiberg Island, Northwest Territories, Canada. *Journal of Glaciology*, Vol. 20, No. 82, p. 41–66.
Hooke, R. L. 1973. Flow near the margin of the Barnes Ice Cap and the development of ice-cored moraines. *Geological Society of America. Bulletin*, Vol. 84, No. 12, p. 3929–48.
Hooke, R. L., *and* Hudleston, P. J. 1978. Origin of foliation in glaciers. *Journal of Glaciology*, Vol. 20, No. 83, p. 285–99.
Hudleston, P. J. 1976. Recumbent folding in the base of the Barnes Ice Cap, Baffin Island, Northwest Territories, Canada. *Geological Society of America. Bulletin*, Vol. 87, No. 12, p. 1684–92.
Hudleston, P. J. 1977. Progressive deformation and development of fabric across zones of shear in glacial ice. (*In* Saxena, S., *and* Bhattacharji, S., *ed. Energetics of geological processes*. New York, Springer-Verlag, p. 121–50.)

SIR, *An alternate statistical interpretation of the strength of snow: comments on the paper by H. Gubler*

Gubler (1978[a], [b]) presents very interesting work which may lead to a greater understanding of the physical processes involved in snow failure. However, he raises a point in the second paper which I do not believe to be valid. To quote Gubler "From the strong dependence of the strength distribution on sample size, it follows that the link definition of Sommerfeld is very critical". In fact, extreme-value statistics show that the large-volume strengths of materials do not depend much on sample size, but are critically dependent on the distribution of the weakest strengths in the material. Gubler's calculations show this very clearly, as did Epstein's (1948) analytical work. The way different distributions tail off at the low end critically determine the predicted large-volume strengths. If the samples are large enough, the volume dependence of the strength is low.

Actually, the most accurate way to determine the large-volume strength of a material is to measure the strengths of large volumes, not as Gubler seems to imply, very small volumes. In large-volume measurements, all questions of detailed material structure, like Gubler's "link definitions" are necessarily ignored. Large volumes of snow are very difficult to handle and it would be very convenient if the large-volume strengths could be derived from measurements on smaller volumes. As I have shown (Sommerfeld, 1974) practical test-sample sizes probably do not measure the lowest strengths accurately. These inaccuracies result not from theoretical definitions but from the practical consideration of sample-wall interference with material flaws.

Concerning shear strengths, Gubler asks "But what happens if the measurements are performed with a different shear-frame size?" One answer, of course, is that Daniels' statistics predict that the larger the frame size the lower the mean strength and the lower the standard deviation of the measurements. If Daniels' statistics accurately apply to snow, the same large-volume failure stress would be predicted no matter what the test-sample size. This is clearly seen by considering Daniels' example of a bundle of a large number of threads. The bundle will fail at some stress. This stress is predicted, according to Daniels, by sampling the strengths of the threads. It does not matter if we consider the strength distribution of single threads or, for example, pairs of threads. If the theory is correct, the predicted failure stress for each analysis would be correct and both equal to the actual failure stress. This is true of the prediction up to the trivial case of the sample consisting of one test on the whole bundle.

A properly designed test using different size samples would be one way to test the applicability of Daniels' statistics to snow. Perla (1977) presents one such experiment comparing 25 tests each with 0.01 m^2 and 0.25 m^2 frames. He found a decrease in mean strength and standard deviation as predicted by Daniels, but did not determine the Daniels strength for each case.

Both Daniels' and Gubler's use of integral expressions implies they are dealing with a continuous medium and not a body made of discrete elements. With a large number of "threads" or "fundamental units", the distinction is not important, but then, neither is the exact character of the elements so long as they are described accurately enough by the chosen distribution.