Dr D. N. Collins' (written communication in 1978) recent SC work at the Gornergletscher terminus has shown that such measurements must be almost continuous (avoiding water discharge rating curves for SC, solutes, or suspended sediments) to infer the detailed workings of subglacial plumbing systems. Since each glacier's liquid-water content, bedrock composition, and jointing, plus ice velocity and geometry combine to establish a subglacial water system, there is no basis for any speculation on a universal, detailed SC pattern to be expected in glacier streams before and after jökulhlaups. Instead, detailed SC patterns for individual glacier systems should be rigorously determined by continuous observation.

Remote monitoring of SC in glacial streams in Washington has already been demonstrated in conjunction with the recent thermal events on Mount Baker (personal communication from D. Frank in 1977). Park officials at Mount Rainier are quite interested in testing my hypothesis; the time is right for an enterprising glaciologist.

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SIR,

Origin of foliation in glaciers: comments on a paper by R. L. Hooke and P. J. Hudleston

The origin of foliation in glaciers has occupied the minds of glaciologists and geologists for over 150 years, yet there is still argument as to how the structure actually develops. The controversy as to whether foliation was a primary or a secondary structure, which raged periodically until the 1950's, finally seemed to have been settled in favour of the latter following detailed work on glaciers by, for example, Schwarzacher and Untersteiner (1953), Meier (1960), and Allen and others (1960). A thought-provoking paper by Hooke and Hudleston (1978) questions these later conclusions by suggesting that bubbles and debris in glaciers are unable to migrate sufficiently rapidly to be responsible for the alternating layers of bubbly and clear ice, or dirty and clean ice which constitute foliation, and that the structure is inherited from earlier layering, such as stratification, crevasse fillings, or debris-layers frozen to the glacier bed.

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The authors are right to point out that the relationship of foliation to earlier layering is critically important. I have gradually come to the same conclusion independently, first following detailed work on Charles Rabots Bre, Norway (Hambrey, 1975, 1976[b]) where the foliation in part is parallel to stratification, then later on Griesgletscher, Switzerland (Hambrey and Milnes, 1977) and on the White Glacier, Axel Heiberg Island (Hambrey and Müller, 1978) where the foliation is parallel to rotated or transposed crevasse traces and other structures. I would not, however, go as far as to say that all foliations are derived from earlier layering, and would dispute the assertion that bubbles and debris cannot migrate in glacier ice; the field evidence often suggests otherwise. For example, the amount and size of debris often entrained parallel to vertical foliation in marginal areas of glaciers is often too great to be explained in terms of wind-blown debris being deposited on the ablation surface and subsequently incorporated in the stratification. It is also difficult to envisage foliation representing strongly rotated and deformed crevasse fillings—sometimes no crevasses are present in the expected areas, so the only conclusion I can come to is that by some process we do not understand, shear in marginal ice near the glacier surface enables debris to be brought down from a moraine-covered surface. Furthermore, longitudinal marginal foliation very often is not parallel to the sides when observed in three dimensions. Although the surface trace of foliation may be parallel to the sides, the structure is usually vertical even where the valley sides are gently inclined, and this would not be so if it were merely rotated stratification. A two-dimensional analysis of strain reveals a fairly simple relationship with such foliation, as discussed below, but the three-dimensional picture is bound to be more complex, and even if obtainable, it would probably be difficult to interpret.

Another objection I have concerns the distribution of bubbles. In stratified ice, the coarse bubbly variety, representing the original accumulations of snow, generally makes up over 80% of the total ice, whereas clear ice, representing frozen lenses or layers of melt water is subordinate. Yet in foliation, clear ice usually forms a much greater proportion of the total, often 50% or more. The question arises: "Where have all the bubbles gone?" I suggest that recrystallization has played a major part in eliminating bubbles, but precisely how I cannot explain. Possibly, fine-grained ice layers, developed by the process of mylonitization due to localized, intensified shear, "absorb" them, since they have the appearance of being extremely bubbly and firn-like. Obviously, much work is needed on the behaviour of debris and bubble-layered ice in deforming ice.

A second important point made by Hooke and Hudleston is that inhomogeneities are modified by strain to produce foliation, so that at the very great total strains expected in glaciers they have been flattened, stretched, and rotated to give a layered structure roughly perpendicular to the direction of maximum total shortening. The validity of this statement is shown by data we have obtained from Griesgletscher (Hambrey and Milnes, 1977). On this glacier the dominant structure is an arcuate foliation originating in a small ice fall. (We prefer to use the term "arcuate" as opposed to "transverse" as it more accurately expresses the geometry of the structure.) The strain history of marginal ice is very different from that of ice in the middle of the glacier. If one analyses the change in shape of a circle near the margin of the glacier, we find that it deforms into an ellipse orientated initially at a moderate angle to the foliation, which here is longitudinal. With progressive deformation, the ellipse rotates towards approximate parallelism with the structure (non-coaxial strain), and becomes extremely elongated. In contrast, in the centre of the glacier, a circle deforms into an ellipse with its long axis parallel to the foliation, here transverse, as it passes through the ice fall (coaxial strain), after which it changes very little in shape or orientation with respect to the structure. The foliation is best developed in zones that have undergone the greatest strain, and it bears very few traces of earlier structural inhomogeneities. As Hooke and Hudleston suggest, they have been stretched and rotated into the plane of maximum total shortening, parallel to the foliation.

Having thus far to some extent agreed with Hooke and Hudleston's conclusions, I would now like to turn to a somewhat different matter over which our views are at variance, namely concerning the mode of incorporation of debris into a glacier from its bed. This is a controversial topic which should be clarified since many glacial geologists regard the incorporation of debris from the bed by shearing as a fact, without being aware that some glaciologists regard the process as mechanically unlikely (e.g. Weertman, 1961; Hooke, 1968). Hooke and Hudleston (p. 22) cite me as misquoting Swinzow (1962) and Weertman (1968), a statement with which I must partially disagree (this, incidentally, in a paper not listed in their references (Hambrey, 1976[a])). The offending sentence is that "several workers have suggested that debris may be incorporated into glacier ice by shear (thrust) planes extending from the bed". Whilst Swinzow, referring to moraines at the margin of the Greenland ice sheet, does say that the question as to how debris is incorporated is somewhat separate from the processes discussed in his paper (as mentioned by Hooke and Hudleston), he does, nevertheless write as part of his hypothesis that "a flow plane loaded with bottom material constitutes a shear plane" (p. 228). Elsewhere (p. 226–27) he says that in the early stages of the development of these moraines "ice below the silt band moves slower than the ice above or in some cases does not move at all". I do not believe that my statement misrepresents Swinzow. I do, however, extend my apologies to Professor Weertman for implying that he suggested that debris could be incorporated by shear. In fact he was referring to earlier ideas and went on to draw attention to the fact that the mechanism could only occur in regions of compression, if at all. Furthermore he raised fundamental objections to the shear hypothesis in general. I am grateful to Drs Hooke and Hudleston for pointing this out. A point I might raise here is that my reference to dirt-bearing foliation in Charles Rabots Bre concerned vertical, longitudinal foliation exposed at the surface. This is a rather different problem to the incorporation of moraine from the glacier bed and entrainment along shear planes being discussed in this section of their paper. Even so, I disagree with the authors and maintain that debris can be incorporated by a mechanism that does involve shear along discrete planes—not by rolling along the planes but by overriding of debris-laden basal ice along surfaces representing favourably orientated planes of weakness, e.g. crevasse traces. However, the debris may originally have been frozen to the bed by Weertman's (1961) mechanism. We have recently argued the case for the shear process with reference to sub-polar glaciers on Axel Heiberg Island (Hambrey and Müller, 1978), hence there is no need to repeat the arguments here. I might add that structural evidence in many cases suggests displacements of from a few millimetres to a metre or more in both cold and temperate glaciers. I would like to mention two more contrasting examples. First I have noted on Griesgletscher the displacement of a crevasse trace by 20 mm along a plane parallel to longitudinal foliation. Secondly, at the snout of Glacier de Tsijiore Nouve, Switzerland debris evidently is incorporated along low-angle planes parallel to a rotated arcuate foliation, a structure initially of vertical attitude formed in an ice fall, and these planes displace by up to 1 m slightly-rotated crevasse traces. When these crevasse traces are rotated further to a medium angle, they themselves become reactivated as shear planes and cut a later set of crevasse traces by a similar amount. I am convinced that discrete shearing is a widespread phenomenon, but develops on pre-existing planes of weakness. I do not think it can be explained away mathematically because ice is structurally inhomogeneous and no set of equations can take the inhomogeneities into account, but I would be interested to hear of other explanations for the features noted above. I would qualify these statements and say that the amount of debris involved is generally small and normally confined to the snouts of active glaciers.

In conclusion, despite my objections raised above, I believe Hooke and Hudleston have provided us with an excellent review of the foliation problem and have enabled us to focus our attention on those aspects in need of further research.

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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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