EXTRUSION FLOW

Comments on Dr. J. F. Nye's Paper *

By Joel E. Fisher
(New York)

Mr. J. F. Nye has recently published a scholarly paper in which, by direct mathematics, he concludes that plasticity in ice is independent of pressure, and therefore it is necessary, he argues, to throw overboard the "extrusion flow" theory, because that theory requires greater flowability of ice at greater pressure (depth).

It seems to me that in endeavoring to apply to natural glacier ice, *en masse*, the results of experiments on ice, under laboratory conditions, Nye misses the impact of pressure-melting-point temperature on *glacier ice*. The melting-point of all ice drops approximately ·0075° for each added atmosphere of pressure. However, glacier ice (as distinguished from laboratory ice), consists, not of water quickly frozen into ice, but of an aggregate of crystals which have been 10, 20, 100 or more years in growing. Because of this great age of glacier ice, any chemical impurities in the original water are forced out into the thin ice films between its ice crystals. Because these thin films of ice therefore contain more CO_2, chlorides of marine origin, etc. (see André Renaud, *Journal of Glaciology*, Vol. 6, 1949, p. 322–24), *their* melting point is always minutely lower than that of the pure-ice crystals; yet the melting-points of these slightly impure ice films, and of the pure-ice crystals, both drop that very same ·0075° per added atmosphere of pressure.

Consider, then, a block of glacier ice at the highest temperature at which every particle of it would remain unmelted. If one could measure its temperature with *perfect* accuracy, its temperature, T_1, would be very slightly under 0° C. (T_1 is of course the melting-point of those thin films of minutely impure ice, between its pure-ice crystals, slightly below the melting-point of the intervening crystals of pure water).

Now, add pressure to that block of ice; some of those thin films of impure ice, at temperature T_1 would begin to melt as their new pressure-melting-point temperature is now slightly below T. The instant some of these films begin to melt, minute slippage of ice crystals is possible along those films—or, the incipient film of newly formed liquid water refreezes as ice, in an adjacent pressure-free air bubble; or, that slightly saline water drains away, leaching out the salts. In each case, minute movement of two ice crystals relative to each other can take place. To the casual observer, the block of ice appears to yield slowly to pressure. It *looks* like plastic flow.

Obviously, if twice the pressure were added, twice as rapid melting of those thin impure ice films would occur; deformation of the block would be twice as fast. We must conclude that this "flowability" (plasticity Nye calls it) of natural glacier ice at pressure-melting-point is a direct function of pressure.

Even further; movements of ice crystals relative to each other, where the intervening film of ice has been eliminated by melting, may bring about a local concentration of pressure at one or another newly developed point of contact between them, a concentration of pressure possibly sufficient to cause melting of part of those pure-ice crystals themselves, resulting in a further displacement of such crystals with respect to each other.

Nye's conclusions thus can apply only to homogeneous quick-frozen ice—or to natural glacier ice which is so much colder than the pressure-melting-point corresponding to any pressure to which it may be exposed, that added pressure cannot cause any part of it to be above its new pressure-melting-point temperature. That latter condition is the condition of the upper hundreds

* The flow of glaciers and ice-sheets as a problem in plasticity. *Proceedings of the Royal Society*, Ser. A., Vol. 207, 1951, p. 554–72.

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of meters of ice of the Greenland or Antarctic ice sheets, whose mean surface temperatures are necessarily the mean annual ambient atmospheric temperature; *except that interior heat of the earth, flowing up into the glacier from below, cannot flow up through to the surface of a thick glacier fast enough to be completely carried off*, and some of it is then trapped in the *lower levels* of such polar glaciers, eventually warming their *lower levels* up to pressure-melting-point temperature.

Hence it follows that as all glaciers, both temperate and polar, are at least in part at pressure-melting-point, all glaciers, both temperate and polar, do possess flowability which is a function of pressure—and hence, are capable of extrusion flow.

It is true that copious melt water or solar heat on the *surface* of certain temperate glaciers may so accelerate the melting of the impure ice films between crystals close to the surface, as to permit the surface portion of such "over-wet" glaciers to flow actually faster than their lower parts. This does not deny the validity of extrusion flow; it means only that the surface portion may occasionally possess a condition enabling it, temporarily and locally, to outstrip the extrusion flow of its under part. But meanwhile, everywhere, in Alpine and Himalayan valley glaciers or *névés*—or in Greenland or Antarctic ice sheets, *extrusion flow is the mechanism* by which the beautiful principles of pressure-melting-point, aided by the result of concentration of solutes during crystal growth, together permit glacier motion as it operates to-day, as it operated in Pleistocene, in Triassic, and in earlier ages, beneath a (sometimes) thin covering of more friable, frozen surface ice.

NOTE.—The unexplained further finding of Renaud (*supra*) that the amount of solute per cm.^{3} of ice, decreases steadily from firn to snout, bears testimony to the above process that decrease in salts in the outer edge of ice crystals, progressive from firn to snout, is due to their being leached out, as those thin films of "saline" ice are the first to be forced to melt, exactly as would be expected, from the above mechanism.

REPLY TO MR. JOEL E. FISHER'S COMMENTS

By J. F. NYE

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Mr. Fisher's remarks raise two questions:

(1) Does the shear stress necessary to cause a given rate of strain in ice decrease with increasing hydrostatic pressure?

(2) If the answer to (1) is "Yes," could this cause extrusion flow?

I do not think it is possible to come to a definite conclusion on question (1) without more detailed reasoning than Mr. Fisher has given. In order to prove that pressure melting can take place in a glacier it is surely essential to show that enough heat to provide the latent heat of melting can flow in within the time available. This would be decided by many factors, among them the rate of flow of heat from the earth, the mean temperature gradient in the glacier and the length of time it takes a given ice crystal to travel from the surface down to its maximum depth (a period of increasing pressure) and up again (a period of decreasing pressure). In the absence of a detailed examination of these points my own opinion was that extensive pressure melting would be unlikely (I would not be so rash as to say it is impossible) and that was why I thought it right to assume in my paper that deformation was independent of pressure. I did not, as Mr. Fisher says, conclude this "by direct mathematics." I assumed it for the purpose of mathematical calculation.

In answer to the second question, I think that even if ice does deform more easily under pressure it would still not show extrusion flow in the simple cases I discussed. I explained this, perhaps in too condensed a way, on page 565 of my paper. I rejected extrusion flow in these simple cases, not because that theory requires ice to deform more easily at depth but because it is based on unsound mechanics. Let me try to make this clear by an example.

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