$5 \cdot 3 \times 10^{-3}$, I believe that the coefficient for any Arctic-type ice of density 0·88 or so (compared with 0·92 g. cm.$^{-3}$ for laboratory ice) will be substantially less, due to "resistance" to heat flow by included air bubbles (the extreme case, snow, is a great thermal insulator). For the same reason κ, the thermal diffusivity, will also be less. So with a much larger supply of bottom heat, and a lower thermal conductivity and lower diffusivity, I believe that the family of curves in Wexler's fig. 2—at least all curves for times over 10,000 years—will ground themselves at a temperature just below 0° C., implying that the basal region of any accumulating, thick glacier which is in a stable state will consist of isothermal ice, a conclusion of importance in considering the even thicker Pleistocene ice sheets.

In order to give some idea of the thermal conductivity of bubbly ice, I have had my guide, Armand Perron of Valtournanche, make a series of measurements which should approximate a comparison of the thermal conductivity of normal (isothermal) glacier ice of density 0·92 in the Gornergletscher (altitude 2,800 m.) with that of bubbly (isothermal) glacier ice, density 0·87, in the same glacier. Illustrations of small specimens of these two types of ice from the same area were reproduced in an earlier paper.[2] These tests were made by inserting aluminum tubes containing refrigerated brine mixtures at about −9° C. into close-fitting bore holes in the two types of ice *in situ*, and by measuring the time interval for the respective tubes of cold brine to warm up to the temperature (0° C.) of the surrounding glacier ice. The tubes inserted in normal glacier ice required on the average 21 min.; those in the bubbly ice, 42 min. It would seem that there is a very substantially lower thermal conductivity for bubby ice compared with normal ice, of the order of one half.

Also, on this same subject, Birch and Clark [3] report substantially lower thermal conductivity for limestone, marble, and even for gabbro and diabase compared with the conductivities, suitably weighted, of their constituent minerals—due, they show, to the minutest films and wedges of air or other gases between the mineral crystals. Such differences of conductivity run up to some 20 per cent for these rocks; if in rock the minute remnants of air persisting between mineral crystals after millions of years of exposure to thousands of tons pressure at high temperature cause such differences, it seems probable that substantially greater diminution of thermal conductivity occurs in cold firn, in which occluded air has only had a few thousand years to escape under only a few thousand pounds pressure. Both in rock and ice it is the breaking up of continuous paths by multiple minute air spaces, rather than the resulting slightly lesser density, which decreases thermal conductivity. I am planning measurements in the field of the thermal conductivity of this cold firn, probably at 4,000 m. on the Monte Rosa, this summer or next.

But the original method of attack by Dr. Wexler has my admiration! I thank him.

JOEL E. FISHER

25 West 43rd Street,
New York 36, N.Y., U.S.A.
16 July 1959

SIR,

I appreciate the opportunity to comment on Mr. Fisher's interesting suggestions.

In my calculations I assumed the ice to be contained and motionless within the Marie Byrd Land basin—a non-steady state condition both with respect to temperature and mass. If a layer of ice is moving then certainly friction will introduce another heat source; but whereas geothermal heat is supplied at the bottom of the ice, this is not true for frictional heating released by sinking ice layers. It is difficult to see how surface layers could make their way to the bottom of the ice as Mr. Fisher postulates. An ice mass moving horizontally would have to be of infinite extent to enable layers deposited on the surface eventually to move close to the bottom. For an ice mass moving down a slope, the ice trajectories within the glacier would dip down in the accumulation zone, but move up again to the surface in the ablation zone. Thus it is likely that the heat of friction released by vertical motion of the ice layers is not concentrated at the bottom of the ice.

With regard to the thermal conductivity of glacier ice, I found Mr. Fisher's description of thermal conductivity experiments on "normal" and "bubbly" glacier ice to be of considerable interest. If the thermal conductivity of the bubbly ice is only half the normal value of $5 \cdot 3 \times 10^{-3}$ c.g.s. units, and *if* this ice is the same as that resting on bedrock under 3,000 m. of ice in Marie Byrd Land, then after 10,000 years the bottom temperature would become −13·0° C. instead of −18·5° C. as originally computed for the case of no loss of geothermal heat through the ice. This new value is based on an ice density of 0·87, as given by Mr. Fisher. Using the density of pure ice, 0·92, the temperature would be −13·4° C.

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If the ice is 20,000 years old, then the bottom temperatures in these three different cases would be, respectively, $-4\cdot9^\circ$ C., $-12\cdot7^\circ$ C. and $-5\cdot5^\circ$ C. However because of the loss of geothermal heat through the ice, especially in the early stages, the actual temperatures would be several degrees lower.

There is, however, considerable doubt whether Mr. Fisher's "bubbly" ice would be similar to that found at the great pressure existing under 3,000 m. of ice. At the bottom of the Maudheim hole at 100 m., Schytt [4] found ice of density 0·885 containing air bubbles, mostly round, with a mean diameter of about 0·5 mm. At a pressure 30 times greater, the air bubbles would be even smaller and hence the effect on thermal conductivity probably negligible. It is hoped that further light on this problem can be cast by measurements of thermal conductivity on the ice recovered from 300 m. in the Byrd hole.

United States Weather Bureau
Washington 25, D.C., U.S.A.
28 September 1959

HARRY WEXLER

Dr. G. de Q. Robin has also sent to the *Journal of Glaciology* copies of letters between himself and Dr. Wexler concerning the same article,[1] and with the permission of Dr. Wexler we are publishing the following extracts.

In his opening letter, Dr. Robin says that he does not favour the glacial growth hypothesis for several reasons: (*i*) the age of 10,000 years required seems much too small, since most glacial geologists would probably say that the Antarctic ice sheet has persisted throughout the Pleistocene although it has fluctuated in size; (*ii*) the depth of the glacier makes it probable that this area was initially covered by sea; and (*iii*) the analysis given by Wexler has not taken into account the effect of changing surface elevation on the mean ice temperature. At present ice temperatures appear to decrease by 1° C. for each 100 m. rise in elevation. If this effect was present during the growth of the ice sheet, one would expect the temperature in the bore hole to increase by 1° C. for each 100 m. depth—or about two-thirds of this gradient if the Earth's crust were adjusting itself to the ice load. For these reasons, he (Dr. Robin) thinks that the flow hypothesis gives a better explanation of the approximately isothermal layer.

In his reply, Dr. Wexler says that, with regard to (*i*), he feels that the last 10,000 years have seen a marked growth in ice thickness in the Pacific sector of Antarctica for the following reasons: the storms which over the millennia built up the ice on the Indian Ocean side of Antarctica were more and more deflected by the high obstacle they created and moved with higher frequency into the Pacific sector via the Ross Sea. This resulted in cessation of growth in the original ice sheet, and in certain portions actual wastage—as shown by the retreat of the ice from the Taylor, Victoria and other dry valleys near McMurdo Sound and the raising of beach levels in that area (e.g. Marble Point) by about 20 m. The storms entering the Ross Sea area brought copious quantities of heat and moisture into the interior of the Pacific sector of Antarctica. The prevailing north-east winds through a thick portion of the troposphere and the heavy snows at Byrd Station attest to this prevailing flow from the ocean.[5] The prevailing southerly winds found on the west sides of the cyclones in McMurdo Sound and the Victoria Land plateau would strongly favour ablation. Another sign pointing to the comparatively young age of the ice in this region of Antarctica is the deep trough in the snow surface extending hundreds of miles parallel to, and near, the foot of Horlick Mountains. The bottom of this trough is only about 750 m. above sea level as compared with 1,500 m. in the neighbourhood of Byrd Station. From the strong prevailing surface north-east winds, averaging some 20 knots (10 m./sec.), and the persistent drifting snow entering this trough, one would expect it to fill in about 10,000 years. Using rough figures for the size of the trough and if we take Loewe's [10] snow transport figure of 2×10^{10} g. m.$^{-1}$ yr.$^{-1}$, then 10,000 years would be required to fill the trough. It would not appear that the maintenance of this trough could be explained by an under-ice ocean current between the Ross and Weddell Seas, since the Thiel-Neuberg airborne traverse during the past season showed the ice along the meridian 130° W. to be grounded from lat. $79\cdot8^\circ$ S. to $84\cdot8^\circ$ S.

With regard to (*ii*), if the Byrd basin had originally been covered by sea this would certainly have changed the initial conditions and probably the entire approach used, but as the snow accumulated more and more on the "Byrd Ice Shelf", what would have happened to the water underneath? Would it have gradually frozen *in situ* or squirted up the slopes in response to the tremendous growing pressure, and then frozen? Dr. Wexler leaves this problem to others, but feels he should be allowed to attempt to