THE FUTURE LINES OF PROGRESS IN GLACIOLOGY*

A SYMPOSIUM

held at Bedford College, London, 8 December 1955

Mr. G. Robin (Physics Department, University of Birmingham) opened the discussion:—

I fear that this is a difficult discussion to introduce to-night. Instead of experts talking about their knowledge of specific problems, I hope they will concentrate on emphasizing the serious gaps in our knowledge of the various fields of glaciology.

I believe considerable impetus to glaciological studies after the recent war came from the work of the Glacier Physics Committee of the British Glaciological Society, formed at that time, including members who were able to draw on pre-war research experience on the Jungfraujoch and elsewhere. Their influence was largely successful because they were able to specify precisely some of the physical properties of ice about which we were ignorant. As a result of outlining the exact problems to be solved, certain crucial experiments in the laboratory and field were suggested and carried out. The interest created by this work led in turn to most valuable theoretical studies.

In order to advance our understanding of glaciology in the future, we must keep in mind three general principles which are widely applicable to scientific work. The first essential is to understand the basic scientific principles associated with the behaviour of glaciers. For example, we need to understand the physics associated with the plastic flow of materials under simple conditions before we are able adequately to comprehend the problem of the flow of glaciers.

The next step is to develop various hypotheses depending on sound fundamental knowledge, and to decide how we can best test such hypotheses. Laboratory experiments may help here, but tests on glaciers are, on the whole, preferable, as glaciers form the primary interest of glaciologists. Finally, when our hypotheses have been sufficiently verified to be classified as theories, we should see how we can apply such knowledge to advance other fields of glaciological study.

The primary need to understand the basic principles is illustrated by the study of the relationship of glacier recession to climatic change, effects which may initially appear to have a simple relationship to one another. As studies have progressed, our lack of knowledge of the basic physics of glacier flow, and of the basic meteorology on which our knowledge of accumulation and ablation depends has become apparent. It now appears a difficult task to move from our observed qualitative connexion between the two effects to a useful quantitative relationship. I hope the later speakers will point out such difficulties in their particular fields.

I should like to mention one specialized field with which I am acquainted, the use of seismic shooting in glaciological studies. Our basic knowledge of the propagation of elastic waves through solids is generally satisfactory. As a tool for measuring ice thickness it may produce results which are difficult to analyse at times, but it is such a useful tool that I believe it should be widely used for increasing our knowledge of the dimensions of glaciers. On the other hand I doubt if our basic knowledge is sufficiently sound to justify wide use of seismic methods for studying properties of glaciers such as ice temperature and crystal orientation. Some laboratory work is being done on these problems now, and I hope the method may soon be sufficiently understood to be used as a tool in these studies.

Much also remains to be done in co-ordinating and filling in the gaps between different

* In connexion with the report of this Symposium, it has been suggested that it would be helpful to read an account of the state of knowledge shown by the research done during recent years. The Presidential Address at the Rome Meeting of the Commission on Snow and Ice in 1954 entitled "Recent trends in glaciological research" deals with the work done up to about the end of 1953; it contains 82 bibliographical references. A limited number of copies of this Address is available on application to the Editor.

studies. Physicists recently have made big strides towards understanding the flow of ice. Have the geologists applied such new knowledge to help them with their ideas on the extent and properties of Pleistocene ice sheets? If they have not, what is the reason? Do the physicists need to carry out further experimental tests, and if so of what type, in order to convince the geologists that their work can be of considerable use? If we look at a similar problem in the reverse direction, have the physicists taken sufficient note of the large amount of geological evidence left behind by glaciers? It may be because of the experimental difficulties of direct observation of glacial erosion that physicists have not made much contribution to this aspect of glaciology, but I feel that the alternative starting point of geological evidence has been rather neglected by physicists as being outside their field. It appears worth while to add such borderline regions between different disciplines to the field of our discussion.

Up to the Middle Ages, science made very slow progress because philosophers laid down theories and did not bother to put them to the test of experiment. Nowadays it is possible that we are going to the other extreme, as some people, including glaciologists at times, collect and publish masses of data as if this is an end in itself. It seems that a trend back towards the philosopher is desirable, so that we concentrate our collection of data on well planned experiments. It would be an improvement if instead of hearing the frequent and very reasonable question "We are running an expedition to so and so, what useful glaciological observations can we take?" we were also occasionally asked, "We want to plan an expedition to tackle a worthwhile glaciological problem, can you suggest suitable problems and suitable locations in which to tackle them?"

Mr. W. V. Lewis (Department of Geography, Cambridge University):—

The aspect of the subject which I wish to stress as being profitable for future research, is related to the work we are at present doing in Norway. A team largely from the Department of Geography at Cambridge had completed a programme of work on small cirque glaciers in Midt-Jotunheimen, and so the stage was set for the investigation of a larger complex glacier. Our findings in Midt-Jotunheimen confirmed, among other things, that a form of rotational movement about a horizontal axis plays a very real part in the movement of certain small cirque glaciers. Readily observable, and on the whole reliable, evidence of this came from a study of the dip of the annual accumulation layers as they moved down the glacier. Systematic study of such layers and kindred structures by direct observations of their dip and strike, and of crystal size and packing, on the surface and in crevasses, stream channels and melt-holes, should therefore receive attention in future glacial research, preferably in association with direct measurements of movement.

I can best illustrate this need for further work by referring specifically to Austerdalsbreen and showing slides of the different parts of its course. The gathering ground is 1800 m. above sea level on Josterdalsbreen from which two 1000 m. long ice falls, Odinsbreen and Thorsbreen, feed the main gently sloping tongue, 3 km. long, which ends at an altitude of less than 300 m. Austerdalsbreen, in possessing both ice falls and gently sloping sections, is therefore a representative complex glacier. At the foot of such ice falls movement is complicated and little understood, and probably includes an active element of rotation, a form of movement which has been almost entirely overlooked until recently. Lastly the lower tongue possesses a fine twin series of ogives—arched bands which stripe the glacier surface and are reminiscent of tree rings both in appearance and in annual spacing. The twin series is due to the glacier being fed by the two ice falls. Ogives have puzzled generations of alpinists and their close study is long overdue, especially as their elucidation should advance considerably our knowledge of glacier movement.

As the accumulation layers in the *névé* move towards the top of the ice fall the increase in the speed of flow, careful measurements of which are needed, causes crevasses to open and so to expose the layers beneath. One crevasse which was descended 22 m. revealed eleven dirty bands probably marking the accumulation of summer dust, and so representing about twelve seasons' accumulation. We need to know the full depth of the *névé* at this point so as to be able to estimate the volume of ice, and perhaps the number of annual layers, which flows gradually over the lip of the ice fall. The exposures in the seracs near the top of the ice fall show the increasing tilt of the dirty seams as the ice turns over the lip. Some step faulting also occurs. Further down, the ice fall presents a strong appearance of chaotic movement and here, in spite of the difficulty of access, is another major need for more information. A sequence of photographs on fixed bearings from fixed points would greatly help.* As the crevasses open certain major blocks undoubtedly twist and heel over in an erratic manner, but if the ice fall is deep (and here again we need more data) as is probable in this case, the deeper layers may well flow downhill in a more orderly manner, moving roughly parallel with the steep rock bed. During the three years that the ice probably takes to traverse the Odinsbre ice fall between 20 and 60 m. of ice may be lost by ablation. It is the crevassed and jumbled surface layers that are lost, leaving the lower, and perhaps less disturbed, ice to reach the glacier below.

The tunnel which we dug 50 m. into the foot of this ice fall showed no sign of faulting or erratic distortion during the two months of digging. In the main the inner end tilted downwards at a fairly uniform rate of 1° in four or five days. This suggests an annual rotation of about 70°. The one clearly defined dirty band occurred at the mouth of the tunnel and dipped up-glacier roughly parallel with a somewhat irregular series of blue bands which could be readily distinguished along the full length of the tunnel. Do these represent, in part at least, the somewhat disarranged and modified accumulation layers of the *névé*, or are they formed mainly or wholly in the ice fall? My present view is that they represent elements of both. The need for more precise observations in such ice falls is very real to help us to answer these and other related questions.

The dip of the ice structures was measured from the tunnel to the end of the glacier, following, so far as we could estimate, flow lines of the ice. The dip increased nearly 30° in the first 60 m. down-glacier from the tunnel where the surface gradient was still steep, and presumably resulted from active rotation. The slope of the surface then became more gentle and the dips increased much more slowly, and somewhat erratically, to a maximum of 90° 500 m. from the tunnel. Thereafter the dip *decreased* by about 5° each 300 m. down-glacier, until near the snout where it fell off rapidly. The vertical dip occurred where the pressure waves at the foot of the ice fall were well developed. The decrease in dip down-glacier from this zone presumably revealed that the glacier flowed more quickly at the surface than at depth, as one would expect except where rotational movement predominates. In this zone of normal flow the very fine series of ogives occurred at intervals of about 60 m. The alternate dark and light bands were distinguished by dirt on Thorsbre ice, but mainly by the structure of the ice on Odinsbreen which we examined more closely. The light bands consisted of approximately 75 per cent white bubbly ice, and 25 per cent blue bands, but in the dark bands the proportions were roughly equal. Some dirt was present also in these Odinsbre bands both distributed in depth and concentrated on the surface by ablation. Dirt was also being actively and erratically distributed by streams, but the way in which the dirt was largely confined to the dark bands throughout the three kilometres of the glacier tongue does not suggest that its distribution is controlled by the streams.

So may I end by posing the greatest puzzle of all—are these ogives the upturned ends of the parcels of annual layers which spill over the lip of the ice fall? The concentration of blue bands in the darker portion of each ogive suggests, I think, that they represent the deeper moving ice of the ice fall in which the annual layers have been strung out, and probably much augmented by ice layers induced by shearing in these lower layers which feel the drag of the rock bed. If this is so, the whiter bands represent the curved outcrops of the ice which was uppermost in the

* This suggestion is interesting. It has been advocated for the whole of a glacier from a distant point, in order to demonstrate its surface flow by a series of, say, weekly still photographs converted into a moving picture. For a specific feature as proposed by Mr. Lewis rather fewer photographs taken over a longer period would suffice. There would be no need for a moving picture. This technique should prove of great value for many features of glacier movement. Ed.

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ice fall, above the zone of rotation. In travelling through this zone these layers, which were roughly parallel with the rock bed, may well be tilted so as to crop out at the surface at the steep angles we observed right at the foot of the ice fall. But much of this is speculation aimed at pointing to some of the matters worthy of careful study on glaciers to-day.

Professor G. Manley (Bedford College, London):—

In what I have to say to-night I am trying to present general principles. Future workers should be able to see where their researches lead, in order better to design their experiments. The ice at the earth's surface can be studied in the following ways:—

(1) The first I call geography. Where is the ice? In what amount (area and volume)? What is its surface relief, character, banding? The technique is essentially that of survey and, of course, as Mr. Robin's seismic work has shown, rather elaborate effort is needed to elucidate volume.

(2) The second field of research we can call history. How did the ice originate? What are its vicissitudes in time, daily, seasonal, secular?

(3) The third stage we may call geophysics. How is the present ice nourished? How does it waste? Accumulation, ablation, movement and possibly additional wastage through descent into unfrozen water require investigation.

All these demand techniques of physical measurement on the ice, rather than around it, and, from the large-scale processes, we come:—

(4) Fourthly, to the physics. How does the ice move in detail? How does movement vary with temperatures, load, slope?

At this stage the field worker meets the laboratory investigator.

Each of the above demands different types of personnel and technique and each may find its application:—

(1) Mainly now in distant ranges, e.g. the Himalaya.

(2) Proceeding wherever records are available based on photographs, surveys, historic and archaeological records and other methods of dating.

(3) Has evolved in Norway, Iceland, Alaska, the Alps: where, indeed, the more detailed work

(4) is now being carried on.

It would appear that there is still plenty of scope for (1); (2) is needed and is beginning in areas where the details of the behaviour of the glaciers differ appreciably from those already investigated nearby, e.g. Swedish glacier movements compared with Norway; north compared with south Baffin Land; northern Labrador compared with south Greenland.

In regard to (3) Ahlmann has particularly emphasized the need for further work on the budget of tropical glaciers, where the radiation factor assumes much greater importance; while the detailed physics (4) has obvious significance in relation to the flow of other solids. Yet, if we knew more under (3) and (4) we should be better able to answer the many questions of the past, e.g. did the ice advance very rapidly and steadily over, say, Russia and equally fast over the Irish Sea? Such work becomes important as we need to know the impact upon meteorology; for how long can a diversion of the atmospheric circulation persist?

We can summarize all this as a need for increasing precision of measurement, improved instrumentation. Here I agree cordially with Mr. Robin. As far as I can see future research will be most productive if it proceeds along the boundaries or contact zones between the sciences.

Natural ice constitutes a special form of the earth's surface with peculiar properties, possessing a degree of uniformity and hence more predictable qualities in respect of the behaviour of the air above. We are supposed to be the people who know about the ice; having assembled our knowledge, we therefore should meet and co-operate with meteorologists, hydrologists, geologists and botanists.

Lastly, I have said nothing about the interesting question of where there *might* be ice, based on estimates from existing snowfall data and snow cover; this, in turn, offers a further realm of enquiry of some promise. On the whole, I an inclined, therefore, to think that collaboration with the other field sciences should be aimed at but, so long as we do not have a complete understanding of glacier movement, the pure physics of ice must remain paramount in many minds; while for others there is still abundant opportunity for the simple exploratory account where remote areas are concerned.

Professor S. E. Hollingworth (Department of Geology, University College, London):—

We are indebted to physicists for many of the recent advances in our knowledge of glaciers. These advances fall into two main categories; first, there is knowledge of crystal structure of ice and of the physics of deformation based on laboratory data and the extension of this to the movement of large ice masses; secondly, comparative surface surveys giving volume changes—the net result of absolute movement plus differential ablation—but little clue to internal movement. Between these two approaches lies a difficult topic, now being probed in various ways, of the actual deformation of the ice by continuous and discontinuous movements. My impression is that in this field little advantage has been taken of the excellent opportunities that crevasses and surface of the glacier together provide for three dimensional studies of deformation.

With the limited time available this evening it is as a structural geologist that I would approach this particular problem. I would like to exemplify my views by particular reference to Austerdalsbreen, which has been so admirably illustrated by Mr. Lewis' lantern slides. Longitudinal crevasses across the pressure ridges at the foot of the head of the Austerdalsbre glacier tongue below the Odinsbre and Thorbre ice falls provide excellent sections through the glacier, revealing a great range of internal features that appear to be of considerable significance to glacier structure and movement.

I should like to mention two of these by way of illustration. The first is that at one place in a crevasse wall the following structures are observable—alternations of blue and white ice dipping steeply up-glacier crossed at a high angle by a white ice band which is almost certainly a closed-up snow-infilled crevasse. A later blue ice band crosses these structures and both of the latter are displaced across the band. A second and still later white ice band (snow-filled fracture) crosses this dislocation zone of blue ice; this white band and all the earlier structures are cut across by a second shear band of blue ice along which measurable displacement has also taken place. Assuming that the white ice bands are crevasse infillings, there would appear to be little doubt that these, and the subsequent zones of shearing, have developed in or below the ice fall. An identical sequence of structures was observed in two other crevasses in the same general position, suggesting that the type of disturbance was common to a considerable width of the glacier. It is reasonable to conjecture that up-stream of the observed location within the pressure ridge zone some of these structures may not have developed and that down-stream they may be eliminated or obscured by ablation or further differential movements (see below). A systematic recording and analysis of the evidence available in crevasses could not fail to give a substantial amount of valuable information concerning the nature of differential movement within the ice.

The second point concerns the spread of the mélange or breccia of avalanche ice from Thorsbreen on to the Odinsbre ice below the ice fall. It consists of blocks or fragments of coarsely crystalline ice in a matrix of powdered snow and ice. This material survives longest in the depressions between the pressure ridges. The axial ratios of what were essentially equi-dimensional blocks, as seen in crevasse walls, show an intense progressive deformation of this material by pressure directed down-glacier as traced progressively down the glacier in a consecutive series of depressions. I will only comment that it will be obvious that such deformation, if general, would produce a sub-parallel deposition in blue bands and other planar structures that were originally of widely contrasting orientation.

I support very strongly the remarks of the previous speaker that it is not a question of whether there is too much observation or whether there is too little, but whether the observers have been directed towards the points which will most likely be productive of results of value; this is the significant thing.

I feel confident that detailed studies of internal structural features, of which those mentioned above are but two simple examples, would yield a rich harvest in the development of our knowledge of glacier movement. One final speculative suggestion I might make is the possibility that the pressure ridges developed by discontinuous differential movement along closely spaced parallel shear planes to produce an effect known to geologists as shear folding rather than by continuous *bending* of an anticline or arch type.

Mr. W. H. Ward (Building Research Station, Watford, Herts.):—

The recent advances in our knowledge of the flow of glaciers spring from the theoretical ideas of Drs. Orowan and Nye, the laboratory work of Dr. Glen and the application of those ideas to observations from a number of glaciers by a few field workers. One step forward has been made and that has pointed to further gaps in our knowledge. There is now fairly convincing evidence that most of the displacement of a glacier occurs in a narrow zone or a plane at the rock bed, which develops a minimum shearing resistance of the order of one bar, and that the distortion within the ice is not a major contribution to the discharge or the mass balance of the glacier. No doubt the geologists will be delighted with that conclusion because it points to the source of glacial erosion.

We do not know what controls the rate of displacement of ice in a boundary zone in contact with rock. Apparently in glaciology we have become involved in boundary layer problems akin to those in other flow problems. If my understanding of the position is correct a physical study of the problem in the laboratory is needed. I do not wish to discuss in detail how this might be pursued, possibly the ice should contain varying amounts of rock debris, and perhaps the surface friction specialists will have contributions to make. No doubt it will be found that when the ice contains an optimum amount of rock debris, the shearing zone moves into ice with a lighter loading of debris.

It is difficult to follow up this point in the field because of the inaccessibility of the rock bed. Something might be achieved by measuring the gradients of the surface strain rates of ice in close proximity to a rock wall where there is a lateral thrust from the glacier and the ice is obviously sliding past the rock.

Progress could also be made in this direction by making a detailed survey of the rates of strain of a very large number of points all over a glacier surface together with an equally detailed survey of the changes in its mass distribution. This will be a big task and very difficult to organize; before it can be achieved some improvements are needed in the techniques of measuring accumulation and ablation.

Dr. J. F. Nye (H. H. Wills Physical Laboratory, University of Bristol):—

Mr. Ward has just mentioned the problem of calculating how fast a glacier will slide on its bed. The surface movement of a glacier is due partly to bottom sliding and partly to differential motion in the ice itself; at present the second of these is much better understood than the first. Glaciologists could perhaps learn something about the mechanism of bottom sliding from what is already known about friction between metals—for the sliding of glacier ice over a rock surface may be analogous, under certain thermal conditions, to the motion of a soft metal over a hard one.

A great many measurements are made of the speeds of glaciers, often with the object of making deductions about régime. However, the quantities that are usually needed for testing present theories of glacier flow are not so much the absolute velocities of points as the differential velocities between neighbouring points; it is necessary to know the rate at which the glacier is deforming,

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as well as the rate at which it is moving down as a body. On the surface of glaciers differential measurements are considerably easier to make than absolute measurements, and more field data of this type would be of great value.

As regards other "future lines of progress", something that might well be fruitful is the study of the pattern of surface velocities on ice caps. It would be an ambitious programme, but nevertheless a fitting sequel to the fine work that has already been done in measuring the depths of the ice caps in Greenland and elsewhere. Such velocity studies would doubtless be used in many ways, but they could be applied immediately to check the theoretical prediction that, provided certain conditions are satisfied, the velocity vectors are at right angles to the surface contours.

A further theoretical relationship which stands in need of field test is one which connects the maximum depth of crevasses in a given region with the strain-rate on the ice surface.

Professor R. Haefeli (Versuchsanstalt für Wasser- und Erdbau, E. T. H., Zürich) communicated:

I. As a connecting link between laboratory research and the metamorphosis of ice with artificial ice (England, Weissfluhjoch, SIPRE) and tests *in situ* with living ice (Jungfraujoch) it would be of the greatest interest if laboratory tests could be made in bulk with polycrystalline *glacier ice*. For this purpose the technique of removing and transporting undisturbed samples from the glacier would have to be developed and improved.

II. As a theoretical basis of glacier flow the theory of creep in connexion with rheology can be of great service, since movement depends for the most part upon metamorphosis without fracture. The theory of plasticity is only applicable where shear planes and glide planes exist. The solution of the problem, therefore, depends on a combination of several functions (flow curves, conditions of fracture, etc.).

III. The possibility of studying glacier flow on a model perhaps has a great future. By this I mean not only models of opaque plastic materials (kaolin, asphalt, etc.) but transparent materials and by the use of photo-plasticity.

IV. Since, for the last ten years, single Alpine glaciers have been checked from top to bottom (that is to say at the same time in the firn area, at the firn line and in the ablation area) steps should be taken to do the same on the Greenland Ice Cap and in polar regions on the great ice fields. Since this investigation, like that in the Greenland Ice Cap, is very extensive and difficult and all countries are interested in the subject, work of this kind should be carried out on an international basis and put on a firm and reliable footing for the future. The observations from the surface to the inside of the glacier are always pressing further into the interior of the glacier. Only when the glacier *bed* has been reached, if possible from the inside of the bed itself (i.e. by tunnels) can glacier erosion be observed *in situ*.

With future glacier observations the change in height of the glacier surface at the firn line should receive greater attention.

Professor A. Bauer (École Nationale d'Ingénieurs de Strasbourg) communicated:—

I want to lay stress on two widely different points of view:—

I. *Measurement of displacement, deformation, creep and all that concerns the rheology of ice*

(*a*) It is necessary that these measurements be made in such a way that the displacement vectors are determined by their three components orientated in space. The precision of determination must be the same for the three components. It is essential that a detailed estimate of errors, allowing for the conditions of observation, is made so that the accuracy of the results can be checked. We know that it is more difficult to determine vertical than horizontal displacements in measurements of the movement of glaciers. Thus, the former are often neglected if not forgotten.

(*b*) It seems to me highly desirable to publish, in addition to the calculated results, all the miscellaneous data obtained by observation and measurement from which the results have been derived. The measurements themselves and the method of measuring often constitute an interpretation, if not a theory, which is not always evident from the calculated results themselves.

Furthermore, other persons than the author must also be able to make use of the results. The interpretation may not change but the measurements must remain to be used again. As an example may I give an instance from geodesy, namely, the study of terrestial refraction made by Bauernfeind in 1880. His measurements were published *in extenso* and have been used since on many occasions.

It must be the same in glaciology, even if we have to load our publications with columns of numbers. A solution might be to record the pure observations on microfilms, which would be cheaper than to publish them in a journal.

II. *International organization*

All progress in glaciology depends on co-operation between research workers of many countries. One has only to think of the waste of time of scientists of different nations, working on the same question because they know nothing of each other! We do not have to criticize the causes of this but to remedy it. We must recognize that the *Journal of Glaciology* has already contributed to a good deal of improvement in this respect.

This lack of contacts in space exists also in time; research work of earlier times is often neglected if not ignored. We should do well often to look back for the first research on a subject and not to repeat measurements and observations which have already been made and then to believe we have made a discovery.*

My proposals are:—

(*a*) That there should be an international organization to make contact easier between the glaciologists of the whole world.

(*b*) That recent fundamental works be published by this organization in four languages at least (the two languages of the I.U.G.G. are not sufficient).

(*c*) That an analytical bibliography of the fundamental publications recent as well as old be established in four languages at least.

(*d*) That old fundamental work which is out of print should be at the disposal of research workers on microfilms.

We must not only make measurements and observations but also work in groups and know what the others do. Progress in glaciology is bound up in the organization of international co-operation.

Mr. H. H. Lamb (Meteorological Office, Air Ministry, Harrow) communicated:—

As a meteorologist, I would like to reinforce the plea of Mr. Robin and others for glaciological field studies designed from the outset to make a contribution towards the solution (or even towards the understanding) of *specific* problems.

The greatest difficulty for climatology in interpreting the evidence of ice studies is that glaciers and ice sheets grow and decay in response to *two* main variables—temperature and precipitation. It is impossible to learn much from a given ice report, unless it can be clearly shown to mark a response primarily to a change in *either* temperature *or* snowfall. We need evidence which is unambiguous, and those glaciers which respond to both variables may hardly be capable of giving it. Presumably the glaciers on the high mountains in low, and relatively low, latitudes respond principally to temperature changes whereas those in high latitudes should be most affected by changes in the yearly amounts of precipitation collected: these two regions are *a priori* the most promising for evidence of effects which lend themselves to confident interpretation.

Nevertheless, I believe it would be a useful (though seemingly endless) programme to establish how each and every important glacier and ice sheet responds to variations of temperature and rainfall—using the very pronounced variations that have occurred during the past fifty to one

* See footnote on p. 694.

hundred years and more for which detailed climatic records are available, at least from the neighbouring lowlands, in nearly all parts of the world.

It has to be admitted that the independent variations of individual glaciers may be due to causes that are either too localized or non-meteorological (*e.g.* due to stresses and strains within the ice or between the ice and the bed-rock) to be of any interest to climatology. This branch of science will generally be interested only in the large-scale results of glaciology, which point to an influence affecting wide regions simultaneously in a similar manner. This would, however, include cases in which large changes in the extent of a great ice sheet on land or sea resulted from the passing of (or failure to pass) some difficult topographical barrier.

One special plea—for a search for any possible frozen-in evidence of the former wind currents prevailing when any given layer of ice is on the surface—may appear to glaciologists only as a revelation of a comfortable, back-room theoretician's ignorance of the difficulties of observation. Unravelling of the type of atmospheric circulation prevailing at any time in the past, when ice sheets were laid down or glaciers grew, depends however on ultimately discovering the important features of the main wind-streams of that epoch. This impels me to ask whether it is possible either to find reliable traces of the former surface snow dunes or *sastrugi* in the stratification below the surface near the middle of ice caps, or to find (and discover the geographical origins of) any pollen frozen in.

Mr. J. M. Hartog communicated:

I. *Heat Balance.* Much attention has been devoted in recent years to studies on ablation and heat absorption, but little to the *net heat balance* of a large ice mass for which we need to know in addition the *heat losses*, above and below the firn line. Although nourishment of a glacier is dependent on accumulation, its continued existence and life is due to cooling in one of three ways: (*a*) by net radiation losses, (*b*) by latent heat losses in surface evaporation, (*c*) by forced convection (cold air currents at surface).

Some measure of the heat losses due to (*c*) is the depth of penetration and temperature amplitude of the annual winter cold wave, below which there is a zone of ice of unchanging temperature until one reaches (if present) a large "reservoir" near 0° C. This does not necessarily imply that there is zero heat flow through the "reservoir" although the thermal gradient may be very slight. To take an analogy the surface of Lake Victoria in Africa is sensibly level, but the lake has a large input and output of water, and is the chief source of the Nile.

We require investigation of surface and sub-surface temperatures over the whole year, and preferably over a period of years. Knowledge of air temperatures at the time of accumulation by precipitation or by drift is important since the initial temperature of new solid accretions to a glacier system is likely to be a major factor in determining its gross heat content (there will be thermal equilibrium between the solid matter and the atmosphere at the time of accumulation).

The general aim of this programme of research should be to determine the heat content, régime and the factors influencing the heat losses of a glacier system, and then to deduce how likely variations will affect the long term rates of growth or diminution of glacierization. This will extend and amplify the inferential work of C. E. P. Brooks and the practical measurements of H. U. Sverdrup and B. E. Holtsmark in this field, as well as measurements shortly to be published by two Oxford expeditions to Nordaustlandet.

II. *The theoretical and the practical.* Aircraft designers who know and understand much theoretical aerodynamics and aircraft behaviour are not usually pilots with daily flying experience. So it is not entirely strange that theoretical glaciologists are not usually those who derive most pleasure from glacier surroundings, nor do they even spend much time on glaciers. There is an especial need in this Society and outside to marry the practical and theoretical approaches of those who try in their different ways to understand ice behaviour.

I would like to give some examples of what I mean. First regarding the theoretical treatment of ice caps. The physicist takes an ideal case, where the ice has the form of a dome lying on a

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horizontal rock-bed. From this assumption, various formulae are derived, which are then applied to real ice caps. An instance is G. Robin's paper (*Journal of Glaciology*, Vol. 2, No. 18, 1955, p. 523), based on such an ideal case, in which it is naturally assumed that the flow in an ice dome is radially outwards from the centre. Now without going further into the assumption, it is worth seeing how far this treatment can justifiably be applied to behaviour of ice caps. Exaggeration of vertical heights in diagrammatic profiles is probably largely responsible for neglect of the physical constraints imposed on the movement in a large ice mass by the form of its rock-bed and rock walls. As inspection of diagrams with less vertical exaggeration will show (for example Robin's Fig. 7 (a) in the *Geographical Journal*, Vol. 120, Pt. 2, 1954, p. 198 (and inset) showing a seismic profile in Dronning Maud Land) the concept of outward movement in radial directions cannot apply to part of the Antarctic Ice Sheet. If the seismic results are reliable, and they appear to be, then the movement of the ice must be channelled.

Similarly in Greenland, where the ice cap is fringed by exposed mountains, rising to three, five or even more thousand feet above sea level, the flow of ice is channelled into glaciers which reach the sea through gaps in the rocky containing wall. It should be remembered that the highest points of Greenland are not in the centre of the ice cap, but on the rocky eastern fringe where peaks of 11,100 and 12,139 ft. rise considerably above the main north–south ridge of ice which in most places is between 9000 and 10,000 ft. (2743 and 3048 m.). Physiographically the ice is contained by a rocky saucer, or dish, which provides mechanical support all round except for the gaps mentioned.

Even Sørfonna in Nordaustlandet, much of which appears to lie on a horizontal basalt platform, has a major discharge of solid ice in a large and much crevassed glacier. There is much other evidence from glaciations to show that channelling of ice flow is in fact universal, except for piedmont glaciers.

Secondly there still remains much to be found out about the movement of glaciers and glacier systems. Both drilling and use of surface markers have been tried, but long term experiments should be started with sub-surface markers, placed in the accumulation area with a view to their recovery after a period of years. Such markers might well take the form of three concentric 4-ft. (1·2 m.) diameter discs, mounted mutually at right angles; construction should be of wood faced with a metal (aluminium or copper perhaps). It is conceivable also that in due course sub-glacial location may become possible.

Thirdly in design of field experiments on glaciers the help and helpful advice available from skilled mountaineers should not be neglected. On the one hand precautions may be required to ensure the safety of workers as otherwise an increased amount of research will certainly lead to a higher accident rate. On the other hand personal knowledge of an area may lead to a constructive suggestion, such as my having been able to assure Mr. W. V. Lewis and Mrs. Grove (then Miss J. M. Clark) after the latter's paper to the Society in November 1950 on Skauthöe, that tunnelling of that cirque glacier was a perfectly feasible and practical proposition, as well as the best way to investigate its internal behaviour. But that would not have been possible but for a day spent in 1948 with Miss Clark's team, when I was able to scramble all over the glacier and its surroundings.

Finally I would like to point out the development in electro-thermal drilling of sub-polar glaciers, which has been achieved by two Oxford expeditions recently. In 1951 the highest rate achieved was a penetration of 18·8 ft./hr. (5·73 m./hr.), and a mean rate of 10·0 ft./hr. (3·05 m./hr.), both in solid ice *below* zero. This was with light equipment (engine plus generator weighing 230 lbs. (92 kg.); power output 1260 watts; fuel consumption 2½ hours at full load to a gallon (4·5 litres) of petrol). More recent experiments in 1955 have fully confirmed the hopes that this would turn out a straightforward method for drilling sub-polar glaciers with a view to determining: (*a*) sub-glacial temperatures, (*b*) ice-thicknesses, (*c*) internal velocity of a glacier. Details will be published elsewhere. It is by this sort of work in the field that we can establish the facts on which theoretical studies can be based, and by which theoretical results may be tested, and proven.