MASS BALANCE STUDIES IN KEBNEKAJSE

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Abstract. The mass balance of Storglaciären in Kebnekajse, Swedish Lappland, has been studied in some detail since 1946. Out of sixteen budget years, fourteen have given a deficit, one a surplus and one has been balanced. It is emphasized that more attention should be paid to the accuracy needed for significant mass balance studies. At present the density of the observation network on Storglaciären is 120 observations per km.^{2} for accumulation and 13 observations per km.^{2} for ablation measurements. The net loss during the sixteen years of observation amounts to nearly 10 per cent of the total glacier volume, and it is suggested that the principal cause of this loss is the increase in the mean summer temperature.

Résumé. Depuis 1946 on a étudié en détail le bilan de masse du "Storglaciären" dans le Kebnekajse, Laponie Suédoise. Sur 16 années de bilan, 14 ont donné un déficit, une un gain et une a été équilibrée. On insiste sur le fait que l'on devrait accorder plus d'attention à la précision requise pour des études significatives du bilan de masse. Actuellement la densité du réseau d'observation sur le "Storglaciären" est de 120 observations par km^2 pour l'accumulation et de 13 observations par km^2 pour les mesures d'ablation. La perte nette durant les 16 années d'observation s'élève à près de 10% du volume du glacier, et on suggère que la cause principale de cette perte est l'accroissement de la température moyenne d'été.

Zusammenfassung. Der Massenhaushalt des Storglaciären auf Kebnekajse, Schwedisch Lappland, wurde seit 1946 eingehend untersucht. Von 16 Haushaltjahren hatten 14 ein Defizit, eines einen Überschuss und eines war ausgeglichen. Es wird darauf hingewiesen, dass der Genauigkeit, die für zuverlässige Haushaltuntersuchungen notwendig ist, grössere Aufmerksamkeit geschenkt werden sollte. Zur Zeit hat das Beobachtungsnetz auf dem Storglaciären eine Dichte von 120 Beobachtungen pro km^2 für die Akkumulation und von 13 Beobachtungen pro km^2 für die Ablation. Der Nettoverlust während der 16 Beobachtungsjahre beträgt fast 10% des gesamten Gletschervolumens. Es wird vermutet, dass die Hauptursache für diesen Verlust im Ansteigen der mittleren Sommer-Temperatur liegt.

As a continuation of Professor Ahlmann's studies of the glaciers along the North Atlantic coasts, a glaciological research programme was initiated in Kebnekajse in 1946; its main object was a detailed study of the mass balance of Storglaciären. This study has been going on for 16 consecutive years and a reasonably accurate record of accumulation and ablation since the winter of 1945–46 is now available.

Before going into any details it may be useful to state some basic facts. The glacier area is now 3·1 km.^{2}, the average accumulation is $4 \cdot 0 \times 10^6$ m.^{3} of water equivalent or 130 g./cm.^{2}, the average ablation is $6 \cdot 2 \times 10^6$ m.^{3} or 200 g./cm.^{2} and the average net loss is $2 \cdot 2 \times 10^6$ m.^{3} or 70 g./cm.^{2}, i.e. 55 per cent of the total income. The glacier is obviously retreating rapidly, which can also easily be observed at the front and along the sides and by observing the altitude to which the last winter's snow recedes every autumn. It is thus easy to show the existence of a large deficit, but it is quite difficult to give accurate figures for this deficit expressed in, say million cubic metres per year.

Without putting a lot of effort into the field work, one can get very large errors. We have chosen a glacier which has a very simple geometry—a well defined accumulation area, parallel sides, no ice falls, etc.—but still, the accumulation is so unevenly distributed that a very great number of measurements have to be made in order to plot an accumulation map accurate enough for the computation of the total accumulation within 10 per cent of its true value—and, in fact, an accuracy of 10 per cent is far from satisfactory for a mass balance study.

Over the lower part of the glacier, where the snow lies on solid ice, a number of pits are dug at the middle or the end of May for a determination of the average snow density. At the same time the snow depth is measured with a steel rod at about 200 places in the ablation area. Depths and densities, which in fact vary little from one place to another but change considerably with time, give water equivalents. This method has always been easy to use and it is believed that the accumulation maps are good over the lower half or lower two-thirds of the glacier.

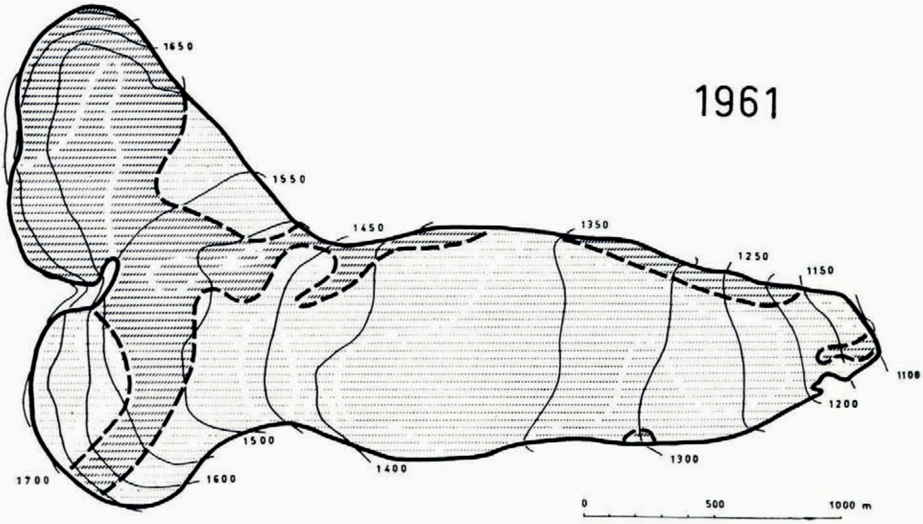
In the accumulation area the problem is rather different. It happens seldom that the autumn surface becomes solid enough to permit soundings with a steel rod; almost every observation has therefore had to be made in pits. Since it is in these higher reaches of the glacier that the maximum snow depth is found, at least the same absolute accuracy is needed as that which can be obtained in the ablation area, and with 4 to 6 m. of snow the digging of one or two hundred pits has been out of question. So, where the observation net ought to be *very* dense, we have few observations, and where we could have accepted fewer figures, we have plenty of them. This is unfortunate, but I assume that the same is true for most accumulation studies wherever they have been carried out.

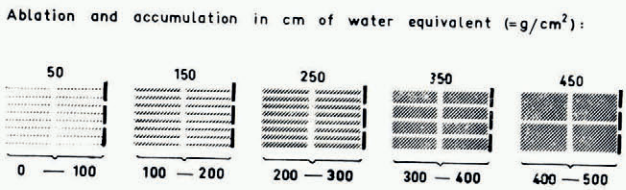
During the last few years we have improved our methods. We still dig a number of deep pits for density measurements and for studies of stratification, but the majority of snow depth observations in the accumulation area are now made by core drilling with a coring auger of the SIPRE type. In this way we have managed to obtain the same accuracy over the whole glacier. The 1961 accumulation map (Fig. 1) is based on 370 observations, i.e. about 120 per km.^{2}.

Even though ablation does not vary as much as accumulation it also has to be measured at a great number of points. Variations can be caused by heat from nearby rock walls, by differences in ice structure and in amount of dirt on the surface, by surface drainage and, which is very important, by the thickness of the original snow cover. In the ablation area the net loss from the beginning to the end of the ablation season is easily measured with the usual stake method making use of the accumulation inventory at the end of May. In the accumulation area of Storglaciären stakes are used, but only combined with often repeated pit observations. The only satisfactory method which we have used for accurate ablation measurements in firn, is to compute it as the difference between two successive accumulation measurements. The density of our ablation observation net has been only 13 per km.^{2} as compared with 120 for accumulation, but probably these densities give about the same accuracy (Fig. 2), since the ablation is more a function of altitude and thus varies less than accumulation and also because the greatest volume of melted water comes from the ablation area, where it is so much easier to measure (more than two-thirds come from there).

I would welcome an initiative from the Glaciological Society or from this Symposium to work out some standards for mass balance studies, because so many people seem to think that a mass balance can be computed from any scattered observations. The classical examples are of course early publications on the mass balance of the Greenland Ice Sheet. Maybe we could agree upon desirable densities of the observation net (in points per km.^{2}) for studies with different aim, for example 100 accumulation and 10 or 20 ablation observations per km.^{2} for a long term study (of, let us say first order) associated with climatic problems or connected with a small glacier's ice and/or water discharge. For other studies, when just a rough knowledge is needed, one may define mass balance studies of second and third order. Other observation procedures may also have to be defined. It is possible that fewer measurements per km.^{2} may be sufficient on glaciers in the Alps than on the Scandinavian ones, because of the lower wind velocities in the Alps—but all this should first be looked into carefully, so that we can know how far we can trust published results.

We look upon our regime programme on Storglaciären as we look upon a meteorological station—one year's observations are useful, a ten-year period is very valuable, but it is not until it covers some periods with glacier advance and glacier retreat that it appears to full advantage. Data should be published as soon as possible—and for the last two years ours have been delivered to the editor of *Geografiska Annaler* only a couple of months after the end of the ablation season—and when a long enough series of data has become available it should be analysed and one should try to establish which climatic conditions have caused the variations in accumulation, ablation and balance. We now have sixteen years of data, and we have good meteorological records from the glacier and from several meteorological stations





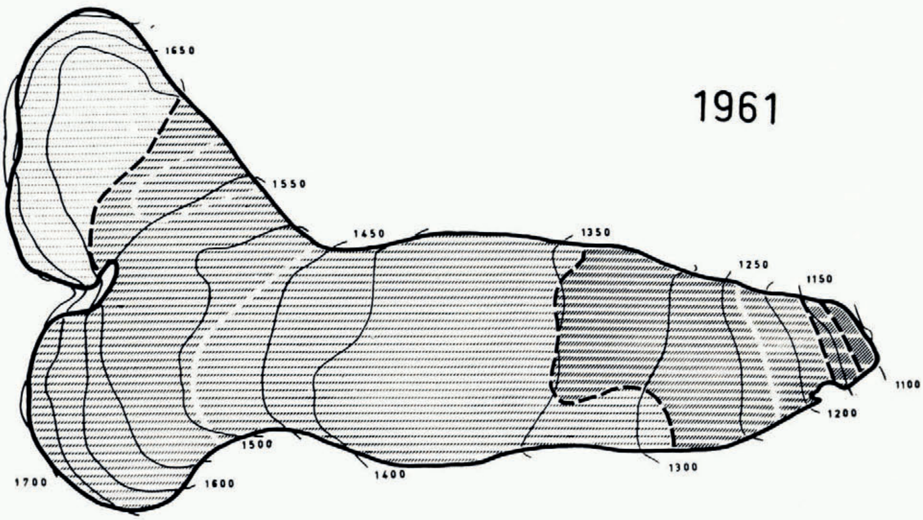


Fig. 1 (top). Accumulation map for Storglaciären for 1961

Fig. 2 (bottom). Ablation map for Storglaciären for 1961

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in the neighbourhood, but at the moment we have no good climatologist and glaciologist available to do the work.

During the sixteen years of measurements the accumulation has varied between 2·1 and $6{\cdot}9\times10^6$ m.^{3} (68 and 220 g./cm.^{2}) and averaged $4{\cdot}0\times10^6$ m.^{3} (130 g./cm.^{2}). The ablation has varied between 3·2 and $9{\cdot}6\times10^6$ m.^{3} (100 and 300 g./cm.^{2}) and averaged $6{\cdot}2\times10^6$ m.^{3} (200 g./cm.^{2}). We had a surplus in 1948–49 because of maximum accumulation followed by the next lowest ablation, we measured a balanced regime in 1947–48 and all the other fourteen years have given a deficit. The average thinning of the whole glacier has been 15 m. since 1946 (nearly 10 per cent of its volume has melted away). It is also of interest to know

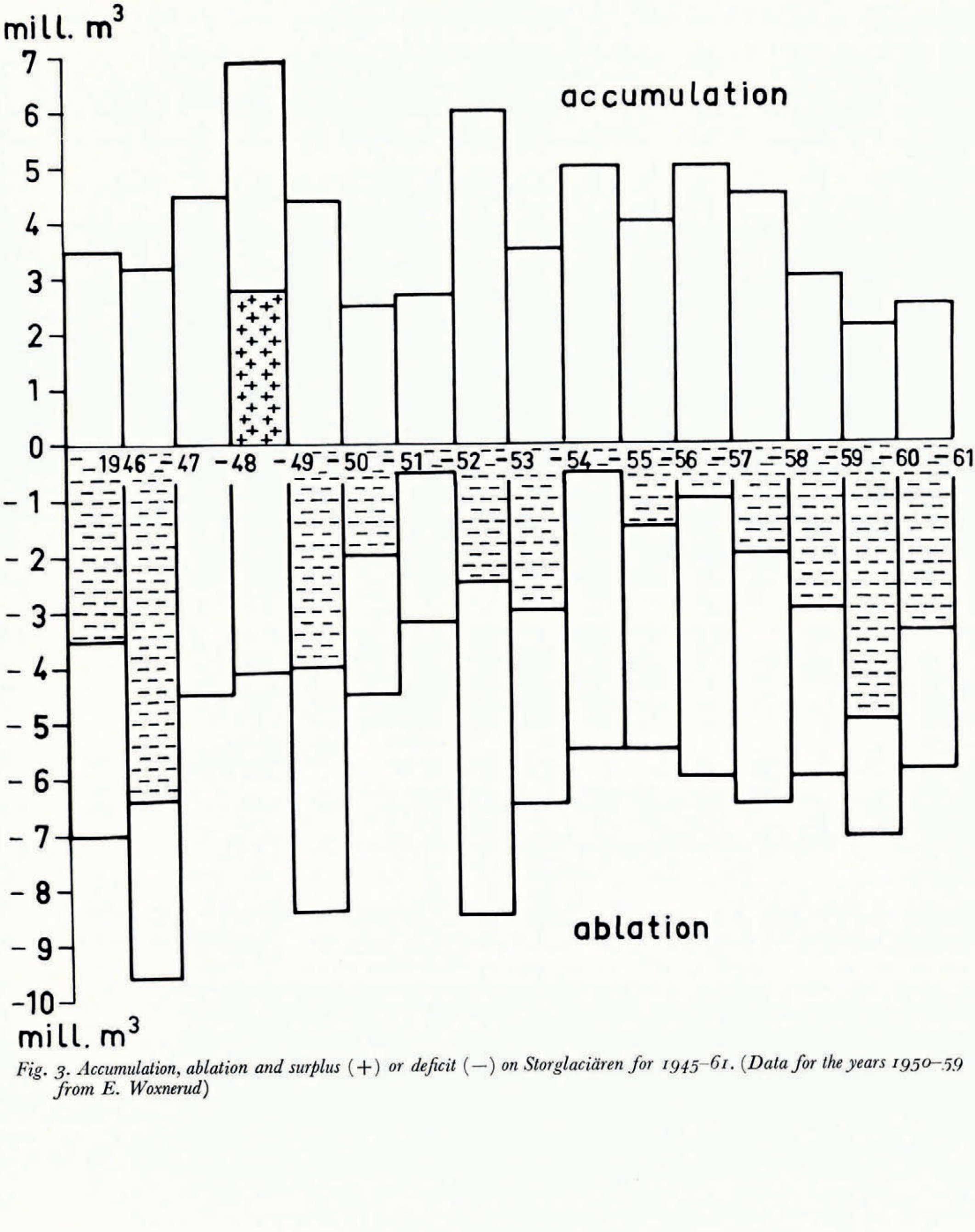

Fig. 3. Accumulation, ablation and surplus (+) or deficit (−) on Storglaciären for 1945–61. (Data for the years 1950–59 from E. Woxnerud)

that the loss during the last three-year period has been greater than during any other three-year period under observation.

But how to explain this loss? Is accumulation or ablation responsible, or is it both? A thorough analysis has not yet been made. We know however, that the temperature climate has become considerably warmer during this century. This temperature rise began at the turn of the century and amounts to about 2° C. for the period June–August. Figure 4 shows the observed ablation as a function of the summer temperature anomaly found by taking the average of the mean summer temperature for five nearby meteorological stations—the anomaly being defined as the deviation from the mean value for the years 1901–30. In this case the "summer" is defined as 1 June–30 September since we often have a considerable amount of melting late in the season. The graph says that in order to get equilibrium—with the present accumulation and areal extent unchanged—the summer temperature must fall to 0·7° C. below the 1901–30 average, i.e. to 1·2° C. below the average since 1946. We can also read that the accumulation should have to increase from 4·0 to about $5 \cdot 5 \times 10^6$ m.³ to cause a balanced regime with the 1901–30 summer temperatures and to about $6 \cdot 5 \times 10^6$ m.³ with the present rate of summer melt.

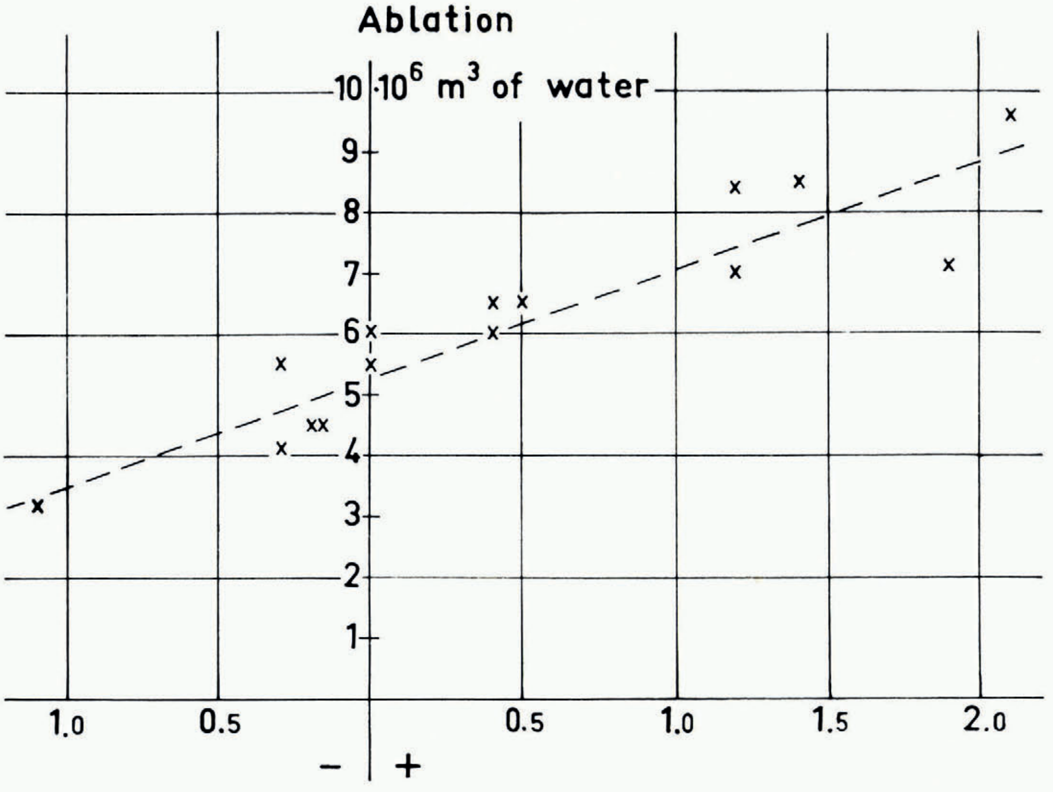

Fig. 4. Deviation of mean summer temperature for five stations in Swedish Lappland from the average figure for the years 1901–30

We *know* that the summer temperatures have increased with an amount which corresponds well to the 1·2° C. required for equilibrium, but during only one exceptional year have we measured an accumulation of $6 \cdot 5 \times 10^6$ m.³ or more. It is even less likely that accumulation has decreased from about $6 \cdot 5 \times 10^6$ m.³ a year to 4·0 if one notes that according to a paper by Anders Ångström (1941) the annual precipitation during 1901–30 was at least as large or

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probably 5 per cent larger than during 1861–1900, and it can also be shown that the average winter precipitation as recorded by our five neighbouring meteorological stations was 11 per cent larger during 1945–60 than during 1901–30.

It is therefore most likely that, before the present retreat started, the accumulation on Storglaciären was approximately the same as it is today or $4 \cdot 0 \times 10^6$ m.³, possibly slightly more because of the larger area, and that the retreat is a result of a June–September temperature rise of just over 1° C.

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DISCUSSION OF DR. V. SCHYTT'S PAPER

MR. E. R. LACHAPELLE: How much variation in the mean density of accumulation do you find as you move up the glacier?

DR. SCHYTT: One spring we dug sixteen pits in the accumulation area with depths varying from just a little to 2 m. We calculated the mean density for every pit, and the mean density for all these averages was $0 \cdot 46 \pm 0 \cdot 01$ g./cm.³. But, more important, it varies with snow depth, so when we have dug our pits we find the average density as a function of the thickness of the snow, and then use that diagram to find for any snow depth the density to use in order to get the accumulation. That seems to work very well.

PROFESSOR H. HOINKES: I was very impressed by the number of 120 accumulation measurements per km.². That is a tremendous number and represents a tremendous amount of work, but it shows even more clearly the need to have some shorter method to tell us the behaviour of the glacier. You cannot go on for 50 yr. with observations like that!

DR. SCHYTT: You can if you choose a small glacier and have men to work on it!

PROFESSOR HOINKES: Yes, but on a glacier like the Hintereisferner you have walking distances of up to 5 hr. If you can establish by detailed study the relation between total net accumulation at a suitably chosen point, as I discussed in my paper, then by just digging one pit and knowing how much accumulation remains at this one point, we can get some estimate of the total net accumulation. I cannot be sure of the method with only 9 yr. control, but this is one way to solve the problem.

DR. SCHYTT: We have tried this; we found the average accumulation for the period 1946–59 by a similar method, but the trouble is that a winter with a lot of wind gives a very different pattern of accumulation.

DR. F. MÜLLER: If we follow Dr. Schytt's suggestion on how many sites to have on our glacier, we very easily run into the danger of finding ourselves terribly limited as to the problems we can tackle with our available people, and this may limit the size of glacier we choose. In this way everybody may end up with a small glacier. It would be very dangerous if we had a number of glaciers all over the world assessed for mass balance and they were all small glaciers. What conclusions could we draw concerning the bigger glaciers? We know that the smaller glaciers react quite differently in many respects from the medium-sized and big glaciers. Being in this position on Axel Heiberg Island, I tried to choose a medium-sized glacier, and also to do some test measurements on a small one and on a very limited area on a big glacier, and tried to tie these in with the medium-sized one. This is one way to avoid extremes in this.

DR. SCHYTT: My idea is of course not to have Greenland and Antarctica covered with 120 measurements per km.^{2}. It would be good to have a number of such reference stretches spread out, but I also suggest we have different orders of accuracy. The main thing is that when people publish their results they should say how many measurements these results are based on so that we can ourselves judge when comparing them.

DR. M. F. MEIER: The required density of sampling points cannot be given as so many points per km.^{2} to get a certain accuracy because I see very little evidence that there is more, or less, dispersion say over the Greenland Ice Sheet than there is over South Cascade Glacier, which is about 4 km.^{2}. I think this has to be related to the size of the glacier unless you can show that one glacier has very uniform conditions over it and another has very non-uniform ones. Maybe we should suggest 100 points per glacier for a certain class of accuracy.

DR. MÜLLER: Another possible way of reducing our work is by using a great number of sites for a short time, and once we have found which sites will continuously fall near the average we can reduce our number of stakes. That has been done successfully on the Grosser Aletschgletscher, where only about 15 stakes are used at present to make the yearly assessment, but in the 1940's, when Professor Haefeli started this programme, they had several times as many stakes.

DR. H. LISTER: I have been terribly disappointed to find that on some occasions when one has worked long hours to take many accumulation measurements one finds that the standard deviation to go with the mean of those measurements is often greater the greater the number of observations made. This is really heart-breaking; the spread of values about a mean value of accumulation only indicates the number of measurements made, it does not indicate the accuracy of the mean accumulation. This suggests that an accumulation measurement is correct for that particular site only. How to get over this I do not know. I noticed this particularly in the Antarctic, when we were trying to calibrate at "South Ice" the methods we were to use on the trans-Antarctic journey, knowing that on the journey time would be limited and we could not take n per km.^{2}, but we found that there was as much as 50 per cent difference in the accumulation over 10 yr. measured by interpreting layers at two sites a couple of metres apart, whereas at another two sites 10 m. apart the sastrugi variation alone was enough to cause a 50 per cent change in mean accumulation. This was not due to the method. I think it would help if we could agree on some sort of areal distribution rather than leaving it to the ambitious worker to take as many as possible—and he ends up with a worse-looking figure than the chap who had a few days off!

MR. J. MACDOWALL: I have observed Lister's phenomenon over the ice shelf, and I regard the standard deviations as of interest in their own right and representative of the undulations of the surface. One could see a considerable variation in this standard deviation which tallied with the appearance of the shelf.

DR. J. F. NYE: Surely what the accumulation in Antarctica is in any one year is a question of somewhat limited interest; what we really want to know is the average over a rather long time. Now in these cores you get lots of layers and therefore rather good values for the average over say 80 yr., and that is an interesting figure and the standard deviation for it would be very small.

MR. LACHAPELLE: One year I tried an experiment on my data from the Blue Glacier. I plotted the surveyed accumulation points on an outline map of the glacier, gave it to someone who had never been on the glacier, and asked him to draw the lines of equal accumulation. They came out quite different from the ones I drew, because I had walked over the glacier and I knew what the drifting accumulation patterns were. I was able subjectively to extrapolate from point to point. I am afraid you cannot deal with this in an entirely objective, statistical manner; you have to take into account the personal experience of the investigator on the glacier. This definitely raises the accuracy of the plotted accumulation pattern, but it is a little hard to put it in figures.

Dr. Meier: What was the order of magnitude of the accumulation which occurred during the ablation season and the ablation which occurred during the accumulation season?

Dr. Schytt: This varies very much and I have not calculated any sort of mean, but in the summers of 1949 and 1952 I think we had more accumulation than ablation in some very high spots. We normally continue our ablation measurements until 15 September, which is usually the end of the ablation season. We reckon that very little ablation takes place after that, but it happens every now and then that when we send some men back, maybe in December, we have had 30—40 cm. ablation of ice in the lower parts of the glacier. In those circumstances we have arbitrarily defined the end of the budget year as the time when we finished, 15 September, and have added the ablation during the October period to that of next year. This does not affect the net results of course, but we had to put the limit somewhere; it would be difficult to use the scheme Dr. Meier has outlined, we cannot have people in the field all the time.

Dr. G. de Q. Robin (Chairman): Starting with Dr. Meier's paper on more accuracy in the definition of terms, and ending with a consideration of the accuracy of field observations, we obviously must have a lot more thought and discussion before we can make firm recommendations, but nevertheless we have covered a lot of ground and it has been extremely useful.