ing the swelling and shrinking of near-surface material (caused, for example, by variations in moisture content) as an improbable cause, he goes on to suggest that the tilt variations arise from changes in the load of the ice field. Variations in the total ice mass are known to be large because precipitation at higher elevations exceeds the equivalent of 400 cm of water a year and the climate in the region is relatively mild, but making an estimate of these variations is no easy matter. Nevertheless, by making various assumptions based on actual weather data and a few "arbitrary decisions", Tryggvason is able to demonstrate quite convincingly that variations in the ice budget correlate well with changes in the radial tilt component.

This may not be a particularly interesting conclusion in itself, but its consequences are altogether more significant in the light of the mechanism by which ice load is converted into tilt. Here there are apparently three possibilities; the variation in ice mass may change the direction of the gravitational pull, deform the crustal layers elastically, or deform the near-surface layers plastically. The change in the direction of gravitational pull may be calculated quite accurately if the distribution of the load is known; and by using appropriate data Tryggvason shows that the effect is to produce a tilt of only about 0.04 μrad. In other words, this mechanism underestimates the observed tilt by a factor of about 100. Most of the observed tilt must therefore be the result of ground deformation.

Elastic deformation of the crust and mantle is difficult to estimate, but basing calculations on a model developed by Slichter and Caputo (*J. geophys. Res.*, **65**, 4151; 1960), Tryggvason is able to show that the tilt caused by elastic deformation is unlikely to exceed 10% of that observed. This, then, only leaves plastic deformation or liquid flow at depth. But the problem here is more complex in that the behaviour of the layers beneath Iceland is not well known. Thus it is not possible to proceed as in the other two cases by calculating a tilt which is then compared with the observed tilt. Instead, it is necessary to reverse the process by using the observed tilt to derive an appropriate Earth model. Tryggvason therefore attempts to determine the characteristics of a model in which an elastic plate overlies a fluid substratum and which is consistent with the tilt observations.

What this analysis shows is that the elastic plate, presumed to represent the lithosphere beneath the glacier, has a thickness of only 6.5–8.5 km. This is much thinner than the average oceanic lithosphere (about 70 km) but is not unexpected. Palmason (*Crustal Structure of Iceland from Explosion Seismology*, Reykjavik, 1970), for example, concluded that the elastic crust beneath south-west Iceland is only 8 km thick (though rising to 15 km beneath north-west and south-east Iceland), and a melting point depth of about 10 km or less for basalt is a not unreasonable inference from heat flow data. As far as thickness is concerned, therefore, Tryggvason's result seems to be consistent with other evidence.

But viscosity is quite another story. The correlation between the estimated ice load and the observed tilt is apparently best when there is assumed to be little, if any, time lag between a change in load and the resulting tilt. In short, the response time of the system is close to zero. Feeding this information into a viscosity formula originally derived by Haskell (*Am. J. Sci.*, **33**, 22; 1937), Tryggvason finds that the maximum viscosity of the fluid layer beneath the elastic plate is only approximately 10^{13} poise.

Most other estimates of upper mantle viscosity (generally calculated from the rate of uplift of deglaciated areas) lie in the range 10^{21}–10^{22} poise, although Einarsson (*Jökull*, **16**, 157; 1966) estimated the viscosity below Iceland to be about 10^{20} poise. Thus Tryggvason's new viscosity is, remarkably, at least seven orders of magnitude lower than previous values. It is just possible, of course, that the value of 10^{13} poise refers not to any sub-lithospheric layer but to Katla's magma chamber. But this would make the chamber rather large, for the arrays lie about 15 km from the volcano.

G. truncatulinoides in Dispute

A FEW months ago Theyer (*Nature phys. Sci.*, **241**, 142; 1973) presented palaeomagnetic and micropalaeontological data which seemed to invalidate the timing of what was thought to be one of palaeontology's most reliable datum planes—the first appearance of the planktonic foraminifer *Globorotalia truncatulinoides*. This plane had always been taken to mark the onset of the Pleistocene about 1.8 million years ago; and on this basis *G. truncatulinoides* would be expected to appear during the Matuyama reversed geomagnetic polarity epoch. The gist of Theyer's study, however, was that in the southern hemisphere south of 36° S, the foraminifer in question appears within the earlier Gauss normal epoch, or some 1.0–1.5 million years before its previously known appearance in lower latitudes. This is a serious matter for, clearly, if Theyer is right a great many studies based on the validity of this particular datum plane must surely have led to incorrect conclusions.

But Theyer is not to be allowed to get away with it, for in *Nature Physical Science* next Monday (July 16) he is attacked by Watkins *et al.* with a vehemence that is much less common in science than it was many decades ago. Thus Watkins and his colleagues claim that Theyer's conclusions are "totally wrong", that his palaeomagnetic work is "extraordinarily subjective" and fails to reach "even minimal acceptability standards", that some of the foraminifera illustrated by Theyer are misidentified, and that Theyer "has brought discredit to radiolarian biostratigraphy". These are hard words indeed, but they are also backed up by detailed criticisms. As far as the palaeomagnetic work is concerned, for example, Watkins *et al.* have examined not just Theyer's published article but also his unpublished thesis in which the methods are described at some length. As a result, they find that Theyer has compiled polarity logs using a combination of demagnetized and undemagnetized directions—a degree of selectivity they find unacceptable—and has misinterpreted certain published results. Furthermore, they criticize Theyer's micropalaeontological work by giving chapter and verse on a series of supposed misidentifications and miscorrelations.

In his altogether more sedate reply, which follows the critique, Theyer defends his palaeomagnetic work largely in terms of later demagnetization studies which seem to justify the application of his original techniques to the particular samples concerned. He thus sticks to his interpretation of stratigraphy, where appropriate, in terms of the Gauss–Gilbert, rather than the Brunhes-Matuyama, polarity pattern. As far as the micropalaeontology is concerned, Theyer argues the point on what he takes to be the most crucial of his supposed misidentifications and in the end begs to differ with Watkins *et al.* on the question of interpretation. But he also notes that, criticisms of method and competence apart, a fundamental problem still remains. Why is it that, in at least twelve widely separated cores from the south-west Indian Ocean *G. truncatulinoides* overlaps distinctive late Miocene–Pliocene foraminifer and radiolaria?

In his original article Theyer rejected reworking as an unreasonable explanation and instead proposed the appearance of *G. truncatulinoides* during the Gauss epoch. He now admits, however, that other explanations are possible and require testing. Thus, by implication, he agrees with Watkins *et al.* to the extent that "one should not lightly deny the conclusions from many years of painstaking effort by many people".