complexity of surface deformation and the super-abundance of Recent volcanic products. Over large areas it is, within limits, a matter of choice as to where it is put—and that alone will keep the arguments going.

Support for deep mantle convection

from Peter J. Smith
Geomagnetism Correspondent

ASSUMING that the engine driving the Earth's lithospheric plates involves some sort of convection in the mantle, do the convection cells reach down to the core-mantle boundary or are they relatively shallow? Views on this point have changed significantly since the idea of motions in the mantle was first proposed. Put briefly and simply, convection as originally envisaged was regarded as mantle wide, with the currents moving the passive lithosphere by viscous coupling. As the new global tectonics developed during the 1960s, however, the nature of the asthenosphere became clearer and the lithospheric plates themselves came to be seen as possible driving forces (acting under gravity, for example). The result was that convection cells became conceptually shallower, being limited to the upper mantle or even to the lithosphere–asthenosphere. More recently, sporadic attempts have been made to reverse this trend by reviving mantle-wide convection but have generally received little support. The latest such attempt is by Robinson (*Earth planet. Sci. Lett.*, **21,** 190; 1974) who has managed to refute the two serious criticisms of mantle-wide convection put forward some years ago by Ringwood (*ibid.*, **14,** 233; 1972).

Robinson's two-dimensional model involves steady cellular convection driven by heat from below. The cells are 3,000 km deep (the thickness of the mantle) and 1,400 km wide (which is within observed trench-ridge separations), but their most important characteristic is that their limbs are narrow. In other words, the convection of heat between the core-mantle boundary and the Earth's surface layer takes place within a narrow (100–200 km) boundary layer, the vertical segments of which form narrow ascending and descending 'plumes'. This immediately avoids Ringwood's first problem, which is that the necessary 200° C mean temperature difference between the ascending and descending currents in the conventional wide-limbed mantle-wide convection model would imply gravity anomalies at the Earth's surface more than 100 times greater than those observed. In Robinson's model the corresponding temperature difference is of the order of 1,000° C but is confined to the narrow plumes. The resulting gravity anomalies are thus comparable to those actually observed.

The second problem that Ringwood pointed to was that of "re-establishing a superadiabatic gradient by thermal conduction after completion of half a turn of the cycle", the point being that superadiabatic gradients are necessary to drive the convection currents. According to Ringwood, the time required for conduction through 3,000 km of mantle would be 2.5×10^{10} yr, so that "only one convective overturn could have occurred throughout the history of the Earth". But according to Robinson, this problem does not arise in his model because the time scale for conduction through a narrow boundary layer is much shorter than that of the convective cycle. In any case, the superadiabatic gradient in the plumes (the temperature gradient inside the cells is adiabatic) could be maintained by energy stored in the core. Ringwood rejected this notion on the grounds that unacceptable radioactive abundances would be required in the core, and that even if the necessary deep heat could be found it would lead to unacceptably high surface heat flow. Robinson argues, however, that the narrow plumes of his model avoid the need for high surface heat flow and that one can envisage heat sources in the core which do not involve radioactivity.

Advances in lichenology

from a Correspondent

PROGRESS and problems in lichenology were considered at a meeting at the University of Bristol on April 8-10. It was the first international symposium held in Britain devoted solely to lichens and was organised jointly by the Systematics Association and the British Lichen Society.

Some fundamental questions as to the nature of lichens were raised by P. W. James (British Museum (Natural History)) and A. Henssen (Botanical Institute, Marburg) who reported convincing evidence that a single fungal partner with different types of algae (green and blue green) can produce morphologically very different individuals traditionally placed in different genera. The production of some chemical components used in lichen taxonomy may also be affected by the algae present. The use of chemical characters in lichen taxonomy was reviewed by D. L. Hawksworth (Commonwealth Mycological Institute, Kew) who suggested guidelines for the taxonomic treatment of chemical races in lichens, and R. W. A. Oliver (University of Salford) demonstrated the value of mass spectrometry and high pressure liquid chromatography for the accurate determination of substances in small amounts of herbarium specimens. Lichen taxonomy is also being clarified by studies of ontogeny (Henssen), algal components (E. Tschermak-Woess, Botanical Institute, Vienna) and by scanning electron-microscopy (M. E. Hale, Smithsonian Institution).

On the ecological side F. Rose (King's College, London) showed the value of lichens as indicators of ancient woodlands in relatively unpolluted parts of Britain, M. R. D. Seaward (Trinity and All Saints' Colleges, Leeds) reported on the performance of *Lecanora muralis* in urban areas and A. Fletcher (University College of North Wales, Anglesey) described the ecophysiology of marine and maritime lichens. Distributional studies on a world scale (P. W. James) and in Britain (B. J. Coppins, Royal Botanic Garden, Edinburgh) were also reported. A novel technique for measuring lichen growth rates was proposed by R. A. Armstrong (University of Oxford) and R. H. Bailey (Department of Extra-Mural Studies, London) pointed to gaps in knowledge of the establishment and dispersal of lichens.

Physiological studies formed the basis of several contributions. Sulphur uptake and metabolism using radioactively-labelled sulphur — subjects relevant to the use of lichens as air pollution indicators—were reported by B. W. Ferry (Bedford College, London) and the uptake of heavy metals in lichens was discussed by D. H. Brown (University of Bristol). Nitrogen fixation and transfer in lichens continue to provide an interesting field for research as was shown by J. W. Millbank (Imperial College, London). The physiology of symbiosis itself and the transfer of carbohydrates were discussed by J. F. Farrar (Imperial College Field Station, Silwood Park) and D. J. Hill (University of Newcastle upon Tyne).

D. C. Smith (University of Oxford) compared the lichen symbiosis with other symbiotic systems; he pointed to many similarities and stressed that from the laboratory standpoint lichens were ideal organisms in which to study symbiosis itself. The concept of controlled parasitism of the algal partner by the fungus may, however, be preferable to one of symbiosis in lichens.

In conclusion R. Santesson (University of Stockholm) pointed to the resurgence of interest in lichens and the stimulus which this meeting provided for further work. It is clear that lichenology is a field in which many problems, both old and new, require further research, but also one in which it is still relatively easy to make important original contributions.