detecting cosmic ray particles, is, of course, a simple matter, but completely negative results were obtained by Fleischer *et al.* Because of the long time during which the collector—the sea bed—has been in operation, it is clear that the flux of free monopoles arriving at the Earth must be less than $4.2 \times 10^{-18} \text{ cm}^{-2} \text{ s}^{-1}$, up to an incident energy of $\sim 2 \times 10^{19} \text{ eV}$, a total of less than $1/2$ monopole cm^{-2} during the entire age of the Earth.

A search for tracks left by monopoles in natural minerals has proved similarly unrewarding (R. L. Fleischer, P. B. Price and R. T. Woods, *Phys. Rev.*, **184**, 1398; 1969). The only remaining possibility for the existence of monopoles therefore seems to lie in the region of very high mass particles, but this must be a slender chance.

MOON

Mascons may be Plates

by our Astronomy Correspondent

THE mass concentrations beneath the lunar maria which P. M. Muller and W. L. Sjogren reported in 1968 may have a plate-like structure, according to M. F. Kane of the US Geological Survey in the *Journal of Geophysical Research* (**74**, 6579; 1969). This result, which should please people who have been saying that the mascons can be understood in terms of lava flows, was obtained from an analysis of the data for Mare Imbrium. The discovery of mascons came, of course, from detailed tracking of the lunar satellite Orbiter 5, and what Kane has done is to compare the form of the gravity anomaly over Imbrium with that expected for several mascon models.

In their discovery paper, Muller and Sjogren assumed rather arbitrarily a depth of 50 km for the mass concentrations, which meant that the Imbrium gravity anomaly required 20×10^{-6} lunar masses. For a nickel-iron sphere such as an asteroid this would correspond to a diameter of about 100 km, but it raises all sorts of problems about the survival of an object of that size striking the Moon.

Clearly other configurations are possible, and Kane claims that a plate-shaped mascon suits the data better. The problem with a spherical mascon is that although the fit to the data can be improved by bringing it closer to the surface, if the radius is to be kept equal to the depth of the centre the density becomes inordinately high. A suitable mascon, according to Kane, would be a 10 km thick layer at the surface, and if it were 400 km in diameter it need have a density excess of only 1 g cm^{-3} over its surroundings.

Unfortunately although the plate mascon is a good approximation near the centre of the gravity anomaly, it is as unsatisfactory as the spherical mascon elsewhere. Kane suspects that the answer might be found when the topography of the lunar surface is allowed for in the calculations. The ring of mountains around Imbrium, for example, has yet to be taken into account.

It is satisfying that this approach fits nicely with what people have been saying at Houston. Given that the maria were once vast lakes of lava, it seems possible that high density crystals might have sunk to the floor. This kind of structure is seen on the Earth, and density differences of the right size ought to be possible.

The principal implication of Kane's report, however is that the distinction between a plate mascon and a spherical mascon is clearer in data taken from satellites nearer the surface. In the eighty orbits considered by Muller and Sjogren, the closest approach to the Moon by Orbiter 5 was 100 km. A 30 km orbit would be better for observing mascons. The point is that this kind of relatively simple satellite would give a lot of information about the interior of the Moon which, apart from pickings from deep impact craters, is otherwise inaccessible.

SEA FLOOR SPREADING

Is the Rate Constant?

from our Geomagnetism Correspondent

THE magnetic evidence in favour of sea floor spreading was almost certainly the most important single geophysical result of the 1960s. The discovery that the pattern of magnetic anomalies normal to mid-oceanic ridges is similar to the polarity-time scale derived from continental rocks was the first significant support for sea floor spreading, and led to the first really convincing tectonic model for the Earth as a whole. But there are still a few details to clear up. For example, the rates of sea floor spreading calculated from almost all of the magnetic anomaly patterns covering the past 3 to 4 million years involve a decrease about two million years ago followed by an increase about 0.9 million years ago. Why should this be?

Because the two discontinuities in the rate of spreading appear in all profiles from the Atlantic, Pacific and Indian Oceans, it has generally been assumed that they are real—as indeed they may be. The theory of plate tectonics, for example, which implies that all ridges are interrelated suggests that any change in spreading rate should be world-wide. But Emilia and Heinrichs are suspicious (*Science*, **166**, 1267; 1969). They point out that even if all ridges are related, it is unlikely that a universal discontinuity in the rate of spreading would be in the same sense at every ridge. Since this seems to be the case, it is much more likely that some of the accepted relations between the anomalies and the palaeomagnetic scale are incorrect. In this case, all spreading rate curves would contain the same error. They would all tell the same story, but it would be wrong.

The evidence that Emilia and Heinrichs adduce for this hypothesis is rather convincing. The first ocean magnetic anomalies were compared with the continental time scale before the discovery of the Gilsa event, which occurred about 1.65 million years ago. As a result, one particular anomaly was identified with the Olduvai event 1.96 million years ago, a practice which was continued even after the discovery of the Gilsa. But is this the correct thing to do? It turns out that the spreading rate discontinuity 2 million years ago depends solely on this one identification. If the particular anomaly in question is now identified with the Gilsa event, the rate of spreading between 0.9 and 3.32 million years appears constant.

There is, of course, no *a priori* reason why the rate of spreading should be constant. But not only does it make the solution neater, it also ties up one or two other loose ends. Between the anomaly previously associated with the Olduvai event and the end of the