NEWS AND VIEWS

Disputes about the Earth's interior

Sir Harold Jeffreys and Dr R. A. Lyttleton are again at loggerheads. Both are mistaken to pretend that plate tectonics does not exist. Lyttleton is right to give another airing to Ramsey's theory.

The most conspicuous of those who reject the theory of plate tectonics is Sir Harold Jeffreys, now 91, who founded the school of geophysics at the University of Cambridge and who, with successive editions of his monumental book *The Earth* (the sixth was published in 1976), has spread his influence far and wide. Jeffreys is not so much an iconoclast as the opposite; far from being prepared to break the icons of the past, he would keep them bright and shining, as can be gathered from his insistence that the successive periods of mountain-building recognized by classical geologists must be explained by a shortening of the Earth's crust in turn caused by the contraction of the body of the Earth in the past 4,500 million years.

As it happens, Cambridge is also the base (some would say refuge) for another distinguished supporter of that view, Dr R.A. Lyttleton, who may nevertheless be better known for his contributions to celestial dynamics and to the theory of comets. So has Cambridge (where Vine and Matthews first demonstrated that the sea floor really spreads (see *Nature* **199**, 947; 1963)) become a place in which ideologically motivated men conspire to undermine the theory of plate tectonics? Not a bit of it. Far from making common cause, Lyttleton and Jeffreys have become persistent critics of each other's work. Their discontents, with plate tectonics but also with each other, are nicely illustrated by their latest contributions to the scientific literature.

First, Jeffreys. This month's issue of the *Geophysical Journal of the Royal Astronomical Society* (**71**, 555-566; 1982) includes Jeffreys's article "Tidal friction; the core; mountain and continent formation" in which he concedes an important point that Lyttleton has been urging on him for the past five years — that in attempting to relate the observed deceleration of the Earth's rotation to the influence of tidal forces, it is essential to allow for the possibility that the moment of inertia of the Earth may be changing. This important issue has its origins in Newton's theory of the motion of the Moon and in Halley's observation, in 1695, that compared with the predictions of that first serious essay in celestial mechanics, the Moon appeared to be decelerating in its orbit about the Earth. By the mid-nineteenth century, the reality of the deceleration (and that of the Sun and planets) had been firmly established and ascribed to the consequences of tidal friction. There are two effects — the Earth's rotational velocity is decreased, but (because angular momentum is conserved) the Moon retreats from the Earth (by about 3.5 cm a year) into an orbit in which the proper motion is decreased.

As it turned out, the first accurate estimates of the quantities concerned appeared only in the 1930s, when Spencer Jones (then the Astronomer Royal of England) was able to distinguish the secular deceleration of the Moon from the apparently random fluctuations of the Earth's rotational speed — a task then complicated by the use of the Earth's rotation as the standard of time. He estimated the secular acceleration of the Moon as -22.4 4.0 seconds of arc per century per century, well within a standard deviation of the values more recently obtained by laser ranging, from the longitudes on the surface of the Earth at which ancient eclipses were observed and by combining accumulated accurate observations of the planets. Jeffreys (like others) prefers a larger value, agrees with Lyttleton that the data do reveal changes of the moment of inertia of the Earth but considers that these may amount to a few parts in 100 million over the course of a decade. The explanation of recent fluctuations, Jeffreys says, is to be found in climatic change and the consequent redistribution of ice and water over the surface of the Earth. But attempts to pin down the long-term non-tidal contribution to the acceleration of the Earth's rotation should wait on more accurate observations even though the crustal shortening of at least 200 km needed on Jeffreys's view to account for mountain-building must have gone some way to offset the deceleration caused by tidal forces.

Lyttleton has no such inhibitions. His latest book, *The Earth and its Mountains* (Wiley, 1982), published in November, is a splendid *tour de force* intended to bring together his own interpretation of the secular acceleration of the Moon with a theory of the Earth's interior first put forward thirty years ago by the late W.H. Ramsey. As a contribution to the literature, Lyttleton's book has the virtue of being neither a didactic work nor an exercise in popularization but an argument addressed to people in all disciplines able to stomach some elementary algebra. On the basis of the ancient eclipse data, he concludes that the intrinsic acceleration of the Earth's angular velocity due to its decreasing moment of inertia is between a fifth and a quarter of the deceleration expected from the tidal forces of the Sun and Moon. (Lyttleton acknowledges R.H. Dicke's estimate in 1966 of an intrinsic acceleration only a little less.)

Lyttleton bravely and persuasively puts his faith in Ramsey's theory — the view that the molten core of the Earth differs from the mantle above it not in chemical composition but because of a phase transition from solid insulating rock to material of essentially the same composition in a liquid and electrically conducting phase. The theory originally advanced as a means of accounting for the average densities of the terrestrial planets (the Moon included) has the great advantage of consistency and simplicity. One of its particular virtues was to avoid the implausible assumption, current in the 1940s and 1950s, that the molten core consists solely of an alloy of iron and nickel. Shockingly, the papers presented at the Royal Society on the Earth's core in January this year include not a single reference to Ramsey's work (see *Phil. Trans. R. Soc.* **306**, 1-289; 1982) even though most recent attempts to account for the composition of the core involve attempts to "lighten" it with the admixture of elements such as oxygen, sulphur and silicon. The chief objection, that shock-wave and high pressure measurements with materials such as olivine have failed to demonstrate the required phase transition, may be irrelevant. Lyttleton is entirely right to carry a torch for Ramsey's theory, which is in that category of theories too neat not to be true. Needless to say, the steady growth of the Ramsey–Lyttleton core neatly accounts for the observed decrease of the Earth's moment of inertia, the contraction of the Earth's diameter in past aeons and thus for mountain-building.

Where does plate tectonics fit in? Lyttleton does not mention this other way of building mountains. Jeffreys refers with regret to the defection of Sir Arthur Holmes to "Wegener's theory" and says that attempts to account for the subduction of oceanic plates must be likened to "cutting butter with a knife also made of butter". Time will probably show that all the participants in this many-cornered quarrel are correct, or partially so. Lyttleton has scored an important point off Jeffreys, but Jeffreys is probably right to insist that the uncertainties about the motion of the Moon and composition of the Earth's core are still too great for a simple resolution of this long-standing dispute. Both of them should also admit, however, that the case for plate tectonics is too strong to be ignored, and both should admit that Ramsey's theory could yet turn out to be more interesting than they acknowledge. In its original version, the theory supposed that the solid rock of the mantle and the molten material of the core were identical in composition. But every chemist knows that multi-component systems in equilibrium with each other may differ in composition.

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