REVIEWS

Fluid Models in Geophysics, edited by ROBERT R. LONG. Washington: U.S. Government Printing Office, 1956. 162 pp. $2.25.

The book consists of papers presented at the first symposium "on the use of models in geophysical fluid dynamics", at Johns Hopkins University in 1953. All deal with hydrodynamical problems with some relation (occasionally rather distant) to geophysics. The first, by Stanley Corrsin, is a general introduction to dimensional analysis, with special reference to the non-dimensional parameters that appear in the statements and solutions of the problems, and must be the same in the model as in the original if the solutions for one are to be adapted to the other. The principle is used regularly in aeronautics, but geophysics is still cluttered with experiments that are irrelevant because similarity conditions are not satisfied. They are, I think, satisfied in all the problems considered in this book except possibly some that are explicitly stated to be of an exploratory character. This chapter would have been clearer if some passages that suggest that the theory of dimensions depends on units had been omitted; the formal analogy obscures the essential difference between the applications.

The remaining papers are applications of hydrodynamical methods to problems of meteorology and oceanography and of motions in the central core. The last subject has probably some connection with terrestrial magnetism and is considered by R. Hide. The Elsasser–Bullard theory of the magnetic field supposes the field maintained by a dynamo action in the fluid core, the energy being supplied by thermal convection. A full analysis has not yet been carried out, but a model based on a rotating cylinder of fluid with an internal source of heat gives motions that are interesting for their own sake, and suggests possible actual motions in the core that may be relevant to the secular variation. Two types of motion are specially noted and an explanation is suggested by E. N. Lorenz.

The other papers are very difficult to summarize, because they are already highly condensed summaries. The bibliographies given should be most valuable for further reading. They largely concern motions produced thermally either by long-period disturbances or by instability. One specially interesting suggestion, due to Rossby and mentioned by von Arx, is that some problems of ocean circulation can be reduced to laboratory conditions by a change of the form of the boundary. If this can be extended we might get an approach to a theory of the tides in the actual ocean.

One point that needs amendment concerns Proudman's theorem that in a rotating fluid every small steady motion must be two-dimensional. What is proved is $\partial w/\partial z = 0$, with suitable axes; it does not follow that $w = 0$ everywhere unless there is a surface across the fluid such that $w = 0$ at all points of it. Usually, of course, there is such a surface, namely the bottom of the ocean or of the tank, and the conclusion follows. But it would be possible to lift the tank up, and the fluid must then move with it, I came upon the point in discussing certain motions of the earth that involve changes of direction of the axis of figure; the ellipticity of the core boundary produces motions in the core parallel to the axis of rotation, which can be comparable with the transverse motions.

As an attempt to compensate for a very inadequate summary, perhaps I might be allowed to mention a few points that might be considered at future symposia.

How can we reconcile the phenomenon of ground mirage with the present theory of thermal instability? There is a variation of temperature of several degrees in the lowest centimetre of the air. If there were fixed surfaces above and below, or a fixed surface below and a conducting free surface above, this state would be stable. But with air on top it seems that the steady state can be maintained only if the heat is carried away by turbulence, and a full understanding would apparently involve a turbulent atmosphere with the mirage layer as a sort of boundary layer.

In an early paper on travelling atmospheric disturbances (*Phil. Mag.* **7**, 1919) I took a cyclone of the type then considered by Sir Napier Shaw, superposed on a general circulation, and studied the rate of change of pressure, taking higher powers of the velocities into account. The surprising fact was that with ordinary values of the velocities the rate of motion of the cyclone would be far too fast, and actual rates seemed to require that the actual isobars are much more nearly circular than in the model adopted. I can think of several respects in which the early model departs from what we now know about cyclones; probably modern meteorologists will think of others. Still, I should like to know just what properties of actual cyclones permit them to travel so slowly.

It is generally believed that the apparent secular acceleration of the Moon is due to tidal friction in shallow seas, though the data have never been as consistent as we should like. Recent work, especially by Urey, Holmberg, Egyed and C. A. Murray, has claimed alterations in different ways. A possibility that needs serious consideration is that the dissipation of energy in the tides may be from 3 to 7 times what has been thought. The known shallow seas seem unable to explain this; what can be done with tidal currents along the open coasts of continents? It was always difficult to see how so much energy got into the shallow seas at all. In my estimate about half the dissipation was in the Bering Sea, for which the data were rather rough; but now that Alaska is a region of scientific activity better information should be available and a better estimate should be possible.

Finally, I venture to suggest that something should be done about the language next time. Most of the papers are extremely difficult to read, with involved constructions that recall old-fashioned German. There is no need for this. I should certainly not claim that English physical writers are perfect, and some American scientists certainly could write intelligibly; for instance, H. N. Russell and R. A. Daly. Why, for instance, should the word 'celerity' be dug up for what we have for a century been calling wave-velocity?

HAROLD JEFFREYS