

NEWS AND VIEWS

Credit for Ocean Floor Spreading

WHO deserves the credit for the theory of ocean floor spreading, now almost as central a dogma of geophysics as the Watson-Crick theory is in biology? Nobel prize selectors may take more than a passing glance at a discussion in the *Journal of Geophysical Research* (73, 6563; 1968) where A. A. Meyerhoff argues that Arthur Holmes first proposed the hypothesis of ocean floor spreading in 1931.

The modern form of the hypothesis is usually associated with the names of H. H. Hess and Robert S. Dietz. But Dr Meyerhoff sternly observes that "New" ideas very commonly are very old ones, and it is puzzling why more careful attention is not given by scientists to tracing the history of important concepts". Holmes, he says, published diagrams that "clearly demonstrate the modern concept of the spreading ocean floor" (*Trans. Geol. Soc., Glasgow*, 18, 559; 1931). In the same issue of the journal, Hess and Dietz express their gentle but definite disagreement with the claim for Holmes's priority. Hess points out that Holmes's model would more aptly be called ocean floor stretching, consisting as it does of a thick continental crust thinned out by underlying convection. Holmes believed that the mid-ocean ridges were remnants of continents and Dietz says that he did not envisage new ocean floor being created at a rise and destroyed in the trenches in the true conveyor belt fashion.

Dietz assigns the full credit for the concept of ocean floor spreading to Hess, whilst Hess makes clear that the actual term "sea-floor spreading", "which so nicely summed up my concept, was coined by Dietz after he and I had discussed the proposition at length in 1960". The first formal publication of the concept was in fact

by Dietz in 1961 (*Nature*, 190, 854), but priority, freely acknowledged by Dietz, belongs to Hess because of a preprint distributed in 1960. Both Hess and Dietz acknowledge the importance of the related hypothesis, proposed by F. J. Vine and D. H. Matthews (*Nature*, 199, 947; 1963), that the pattern of magnetic anomalies on the ocean floor can be explained by the two mechanisms of ocean floor spreading and magnetic reversals.

This discussion should interest philosophers as well as historians of science. Is ocean floor spreading merely a hypothesis, or has it now attained the cognitive status of a theory? Meyerhoff goes out of his way to call it a hypothesis, but Dietz is now inclined to regard it as a theory, particularly because of the corroborative evidence afforded by the hypothesis of Vine and Matthews and the palaeomagnetic evidence that their hypothesis explains. Certainly it seems fair to say that the concept of ocean floor spreading is now used as a theory in the sense that it now possesses explanatory powers independent of the evidence that supports it, enabling further hypotheses and perhaps laws to be deduced. Ocean floor spreading and the Vine and Matthews explanation are of course two sides of the same coin and cannot now be considered separately. But historically it seems that the simplicity and elegance of the latter hypothesis, together with the data available at the time, were such that it was seen to support the former rather than the other way round. That, however, is a question which historians further removed from events will wish to debate. The issue will of course seem entirely irrelevant to those who like to quote Sir Francis Bacon as the original proposer of continental drift.

The End Mystery

THE two ends of linear nucleic acid molecules are known as the 5' and 3' ends because of the polarity of the sugar phosphate bonds which link the individual nucleotide subunits together and provide a backbone to the molecule. When nucleic acids replicate or messenger RNA is transcribed off a DNA template, the new molecule is synthesized in the direction from the 5' end to the 3' end. Further, a messenger RNA molecule is translated in the direction from 5' to 3' so that the first amino-acid in the protein chain is specified by a codon near the 5' end and the last is specified by a codon near the 3' end of the RNA.

Information about the sequences of bases in RNA molecules is therefore important, and on page 548 of this issue Dahlberg reports the 3' terminal sequences of three RNA bacteriophages. The importance of his results, and that of some previously published data on

terminal sequences, lies in the questions they raise, not the questions they answer. First, why do all the bacteriophage and viral RNAs so far analysed have the same 3' terminal sequence of CCCA? Second, is it just a coincidence that a 3' terminal sequence of CCA is common both to the RNA molecules in the bacteriophage, which can act as messenger RNA, and also to transfer RNA molecules which all have a 3' sequence of CCA no matter what amino-acid they specify? Third, because the CCA residues in transfer RNA are known to turn over in the sense that they are added and removed by a specific enzyme in the process of protein synthesis, do the CCA residues in phage RNA also turn over? Finally, it is known from other experiments that for at least one of these bacteriophages, the last base—the A—is added to the end of the molecule after the rest of it has been replicated from a complementary strand