PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Introduction

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INTRODUCTION

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When, about fifteen years ago, I along with many other new workers in the field of rock magnetism, started to read ourselves into the subject of continental drift, we found a complex, controversial and perplexing situation, with a very long history. In about 1620, Francis Bacon, in his search for regularities in nature, wrote: '...the very configuration of the world itself in its greater parts presents Conformable Instances which are not to be neglected. Take for example Africa and the region of Peru with the continent stretching to the Straits of Magellan, in each of which tracts there are similar isthmuses and similar promontories; which can hardly be by accident. Again, there is the Old and New World; both of which are broad and extended towards the north, narrow and pointed towards the south.' Though Bacon thought the similarity of shape could not be by accident, he did not explicitly suggest that the two continents might have once been together. This hypothesis seems to have been first mentioned by von Humboldt about 1800: he also suggested a possible mechanism as to how the continents might have drifted apart:

'Our Atlantic Ocean shows all the signs of a valley structure. It seems as if the rush of the water had directed its thrust first against the north-east, then north-west and then again north-east. The parallelism of the coasts north of 10° S, the projected and retracted parts, the convexity of Brazil opposite the Gulf of Guinea, the convexity of Africa under the same latitude as the Gulf of the Antilles speak for this daring opinion. Here in the Atlantic valley as nearly everywhere in the structure of large landmasses, retracted coast lines, rich in islands, face convex coasts on the other bank.'

Occasional references to the hypothesis of drift were made during the nineteenth century: of particular interest is the map by Snider about 1860 which showed South America shifted into close contact with Africa. It was not, however, till the second decade of the twentieth century that the hypothesis became the subject of detailed scientific study, chiefly due to the work of Tayler in the U.S.A., Wegener in Germany, and du Toit in South Africa.

During the 30 years from 1920 to 1950, continental drift appeared to a few geologists and geophysicists as the obvious explanation of a wide range of phenomena: but to most it seemed wrong-headed and impossible. The controversy was often violent and sometimes almost abusive. One feels that it was only the politer manners of our own age which prevented some discussions by geologists of the evidence for and against continental drift from ending as did the Society upon the Stanislaus in the days when the Wild West was really wild. As related by Bret Harte, the controversy then was not, of course, about continental drift, but about the question as to whether some bones dug up by Brown were those of a very rare animal or of one of his neighbour Jones's lost mules.

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'Now, I hold it not decent for a scientific gent
To say another is an ass—at least to all intent:
Nor should the individual who happens to be meant
Reply by heaving rocks at him to any great extent.'

'Then Abner Dean of Angel's raised a point of order—when
A chunk of Old Red Sandstone took him in the abdomen;
And he smiled a kind of sickly smile, and curled up on the floor,
And the subsequent proceedings interested him no more.'

It is interesting to speculate on some of the reasons why the concept of continental drift should have aroused such vehement objections among some geologists in the early decades of the twentieth century, whereas, for instance, it clearly seemed a quite natural hypothesis to von Humboldt at the end of the eighteenth century. I think one of the reasons may have lain in the vast observational and intellectual achievement of the geologists of the intervening century, in bringing such a high degree of order out of a fantastic diversity of facts. The thought that, if the continents had not always been in the same positions relative to each other, then perhaps some part of this vast edifice of geological synthesis might need rebuilding, made for a very natural and understandable conservatism. For instance, in the mental frame of reference of the fixity of the continents, the occurrence of evaporite deposits during the Permian in the Arctic as well as in Europe and North America implied that a large part of the surface of the globe, if not most of it, must have then been hot and dry. So inevitably there arose the concept that different geological ages may have been characterized by different climatic conditions.

Now it was a major achievement of Wegener and du Toit to show that if the continents have moved suitably, then the climatic zoning throughout geological history may not have differed much from what it is today. But to accept this one would also have to accept the necessity of rewriting quite a bit of some geological text-books.

As Professor Read comments: '...It is not to be wondered at that Continental Drift is capable of arousing violent and vituperative discussion when more than two geologists are gathered together.' To the 'anti-drifters', the fixity of the continents seemed the natural hypothesis. To the 'drifters', the constancy over geological time of the climatic zoning seemed equally natural. Of course, the reality must lie somewhere between the two extreme assumptions of (*a*) no movement of the continents but changes of the global climate, and (*b*) movement of the continents but no change of the global climate. On present-day views the second hypothesis seems to me nearer the truth than the former.

Another and very important aspect of the controversy between the 'drifters' and the 'anti-drifters' lay in the possible mechanisms for drift. It must be admitted that some proponents of drift weakened their case for it, derived from very impressive observational material, by rather weak theorizing about how it might have occurred. When their opponents demolished the theoretical models of the drifters, they tended to ignore the weight of observational facts which had to be explained. However, the 'anti-drifters' weakened their own case by producing theoretical arguments why drift cannot have occurred, which, though perhaps more sophisticated, were probably as fallacious as those theoretical arguments put forward by the 'drifters' that it must have occurred.

Now if, as I think this Symposium may show, the consensus of opinion is moving towards the assumption that drift has occurred, then it seems likely that the ultimate explanation of why it occurs will be much more complicated than the arguments used by both sides in the earlier controversies. Here perhaps one can derive a warning against possible mistakes in the methodology of subjects like the Earth sciences, where the observational facts are highly complex and difficult to reduce to quantitative terms. In such subjects, a highly simplified model which can explain a large number of observed facts is invaluable, especially when it suggests new observations. When the observations are made, it is generally found necessary to make the model more complicated. However, highly simplified models which prove that some supposed phenomenon cannot have occurred, must be treated with caution, for they may discourage new observations.

Round about 1950, as pointed out by Bullard, a certain weariness set in of repeating the old seemingly inconclusive arguments, both observational and theoretical. Perhaps the last pitched battle between the 'drifters' and the 'anti-drifters' took place in 1948 at the meeting of the British Association in Birmingham: the proceedings make entertaining reading.

Since then advances in two virtually new subjects, the study of the magnetism of rocks and the study of the floors of the oceans, have thrown exciting new light on the subject of drift, and have done something to overcome the former widespread objections to it. However, I myself am convinced that the case for continental drift was rather strong quite apart from the new evidence from rock magnetism and oceanography. For instance, the survey by Arthur Holmes in *The Principles of Physical Geology* certainly played a valuable part in convincing the early workers in rock magnetism that the probability that the continents had drifted was high enough to justify an intensive study of the directions of magnetizations of ancient rocks. To a physicist like myself one of the most convincing single pieces of evidence was the Permo-Carboniferous glaciation of the southern hemisphere at about the same time that the great coal deposits of the northern hemisphere were being laid down, I remember the impression made on me by Holmes's remark that there was just not enough water in the world to produce a large enough ice-cap if the continents were then in the same relative position to each other as they are today.

Then there was the evidence of the great transcurrent faults. Kennedy's demonstration of the 100 km shift along the Great Glen in Scotland, together with the probable shifts of 500 km or so at the San Andreas Fault in California and at the New Zealand Fault—not forgetting the several hundred kilometres of compression necessary to produce the great mountain ranges such as the Himalayas—all this suggested that the question was not the qualitative one 'Have or have not the continents drifted?', but the quantitative one 'How much have they drifted and when?'.

So the new evidence acquired during the last decade from oceanography and rock magnetism will, I think, appear in the history of the subject as supporting and making more quantitative an already rather strong qualitative case. The single most striking contribution so far from oceanography seems to me to arise from the study of the oceanic ridges, particularly that in the Atlantic. The last vestige of doubt that Francis Bacon was right when, over 300 years ago, he concluded that the similarity of the shapes of the two opposing coasts of the Atlantic could not be an accident, was removed when it was noted that the mid-Atlantic Ridge has the same shape as, and was equidistant from, both opposing coast lines.

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The contribution of rock magnetism has been to provide, on certain assumptions, the ancient latitudes and orientations of the major land masses throughout geological history. The key assumption is that the Earth's mean magnetic field has always been that of an axial dipole. This has certainly been true for the last 20 My or so, and there is considerable support, both observational and theoretical, for its validity back to 1000 My or more.

The fact that the Pole positions calculated from the direction of magnetization of rocks of the same age from different land masses, on the assumption of an axial dipole field, do not agree with each other, means that the continents must have moved relative to each other. Exactly how they have moved cannot, however, be deduced from rock magnetic measurements alone. For these measurements give only the ancient latitude and orientation of a landmass, but not its longitude. Though Nature has provided the ancient rocks with the magnetic equivalent of a Pole Star to give its latitude and orientation, regrettably it did not also provide it with a chronometer to tell its longitude.

Thus to construct maps of the world at different geological ages, the evidence from geographical shape, geological resemblances and differences, the directions of ancient winds, etc., must all be combined together with the quantitative results of rock magnetic measurements.

However, the rock magnetic measurements alone do allow, in principle, some firm conclusions about the ancient climates of the globe, since they give the ancient latitudes on which climate mainly depends. So far as the existing measurements are reliable, they suggest both that at least for the last 500 My, the Earth's field has been that of a dipole and the mean climate of the globe and its zoning with latitude has been much as it is today: this is in striking contrast with the inevitable deductions from the palaeo-climatological evidence if the continents are assumed to have remained fixed. The above conclusion is, however, still a matter of controversy.

Assuming now that drift has occurred, a major problem ready to be tackled is to determine the *pattern* of drift over the ages. Has the apparent average rate of drift of the different landmasses, now somewhere between 1 and 5 cm a year, always been the same? Were the supposed primitive continents, such as Gondwanaland, a reality in ancient geological times; or were they in some sense only temporary comings together of landmasses, followed by further splitting up? The answer to these essentially observational questions will allow detailed theoretical models to be constructed of the pattern of convection in the Earth's mantle, which seems much the most plausible cause of the drift of the continents. Then the detailed study of the eruption of lavas along the oceanic ridges, such as is now being carried out in Iceland and elsewhere, should yield some firm evidence as to how the splitting apart of the landmasses has actually taken place.

The wide scope of the papers contributed to this symposium and the great interest which it has aroused indicate the importance attached to this formerly highly, and still somewhat, controversial subject of continental drift. These two days of discussion should contribute greatly to a clarification of the present state of our knowledge, and so should help to answer the key question 'Have the continents drifted or not?'; and if the answer is that they have, to construct a quantitative picture of how they have moved, and to work out a mechanism.

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