NEWS AND VIEWS

Can Geomagnetic Polarity Intervals be Classified?

THE problems of nomenclature in a rapidly progressing scientific field are three-fold. For one thing, any system of nomenclature adopted in the early days of a science should ideally be open ended in the sense that it should be able to accommodate new discoveries of fact without upheaval. In practice this is usually extremely difficult to achieve and in many cases is theoretically impossible. Second, it may well become apparent at some stage that a system of nomenclature which seemed rational and logical at the time it was proposed will seem less so in the light of more recent knowledge. This is more than merely a matter of accommodating new factual data. Apart from the simple case involving the naming of unrelated objects or events, a nomenclature is likely to carry within it certain assumptions about the way nature behaves—and it is unlikely that all these assumptions will prove to be valid in the light of subsequent discoveries. When new data have accumulated to the extent that a new, more natural scheme of classification can be perceived it becomes a case of having to revise the whole system of nomenclature. An increase in the quantity of data may well lead to a qualitative change in the theoretical framework within which the data fit. And, finally, there is the problem of human nature. Once a system of nomenclature has come into common usage it is not the easiest thing to overthrow.

All of these problems have arisen to a greater or lesser degree in the naming of geomagnetic polarity intervals. When Cox and Doell came to describe the first few well-dated polarity intervals within the range 0–4.5 million years, they immediately had to wrestle with the problem of open endedness. Simply to have numbered the intervals then known in sequence starting with the youngest would have led to a non-sequential series as soon as another magnetic interval was discovered; and it was clear, even at that time, that further discoveries were likely—an assumption which proved correct. It was equally evident, however, that the known magnetic intervals could be divided into two natural groups—one comprising intervals of the order of 10^6 years (which Cox and Doell termed "epochs") and one comprising intervals of 10^5 years or less ("events") which fell within epochs. The delights of this discovery were quickly apparent. The longer epochs were comparatively easy to delineate and were soon named after early workers in the fields of magnetism and palaeomagnetism (Brunhes, Matuyama, Gauss and Gilbert). The shorter events were then named after the sites from which came the first rocks in which the events were detected (Jaramillo, Olduvai, Réunion, and so on). On the face of it this system was perfectly open ended.

Unfortunately, it later became clear, both theoretically and experimentally, that the simple division into epochs and events was an illusion based upon incomplete data. In reality the lengths of magnetic intervals form a continuous spectrum from 10^7 years down to intervals too short to be resolved by potassium–argon dating. Clearly then the only system of nomenclature which is viable in the long term is one which assigns a different name to each magnetic interval, irrespective of its length. But by the same token such a system can only be devised when every single interval has been discovered. In the light of experience it is obvious that to have adopted a perfectly open ended system right from the start was impossible (and is still impossible). Cox and Doell's scheme, though adopted in a spirit of optimism which later proved unjustified, was nevertheless the best that could be devised under the circumstances. As a result, however, palaeomagnetists have inherited a scheme which is nonsense logically but which is likely to obtain for a long time to come.

So the first thing to be said about the Mesozoic and Tertiary polarity nomenclature proposed by McElhinny and Burek on page 98 of this issue of *Nature* is that it suffers from the same (and even more) disadvantages as the Cox and Doell scheme for the more limited period. To say as much is merely to state the inevitable; but it should also be said that McElhinny and Burek have made the best of what is, in the final analysis, an impossible job. What they have done is to analyse all reliable Tertiary and Mesozoic palaeomagnetic polarity data and thus show that a logical classification scheme is possible based essentially on reversal frequency. Thus they divide the last 280 million years of geological time into six intervals of 10^7–10^8 million years each (which they term magnetic intervals). Three of these intervals (230–200, 150–120 and 70–0 million years, respectively) are regarded as "mixed" in the sense that reversal frequency is so high that no predominant polarity can be discerned. The other three clearly have a predominant polarity but may contain much shorter "zones" of opposite polarity. Thus the reversed Kiaman interval (280–230 million years) has no zones, the newly named normal Graham interval (200–150) has three short reversed zones and the newly named normal Mercanton interval (120–70) has two reversed zones.

There is no doubt that this scheme will be of great practical value if only because it clarifies the existing state of knowledge and provides a formal framework into which new discoveries can, for the time being at least, be fitted. But ultimately it is likely to come to grief for the same reason that the original Cox and Doell scheme has come to grief—new discoveries will probably show that the picture is a great deal more complicated than current knowledge suggests. There must inevitably be a nagging feeling that McElhinny and Burek's beautifully clear classification is the latest palaeomagnetic equivalent of deducing a linear law from two experimental points and that just as the distinctions between the epochs and events of the past 4.5 million years have become a meaningless blur so will the distinctions between the new intervals and zones. And because magnetic periods on the geological time scale cannot be defined with anything like the same precision as those of the last 4.5 million years, a good case can be made for suggesting that the disparity between the current picture and reality for the past 280 million years is greater than for the past 4.5.

Of course, none of this is to suggest that McElhinny and Burek have not performed a valuable piece of work but only that its limitations should be borne in mind constantly. The real danger—and there is an obvious precedent for it—is that once the McElhinny–Burek scheme becomes accepted it will be difficult to get rid of.