CONTINENTAL DRIFT

Dissent on Split

from our Geomagnetism Correspondent

In a sharply worded and provocative note (*Earth Planet. Sci. Lett.*, **10**, 271; 1971) Wright takes issue with Larson and LaFontain who recently suggested (*Earth Planet. Sci. Lett.*, **8**, 341; 1971) on the basis of palaeomagnetic evidence that both the North Atlantic and South Atlantic began to open up about 200 million years ago. The burden of Wright's criticism is aimed directly at Larson and LaFontain; but rightly or wrongly he chooses to extend his target to geophysicists in general and what he clearly takes to be their arrogance in apparently refusing to take full account of geological data when it comes to such things as continental drift. Whether the imputation of guilt by association will be taken humbly by geophysicists remains to be seen.

In their original article Larson and LaFontain made an attempt to test the continental fit of Bullard *et al.* (*Phil. Trans. Roy. Soc.*, A, **258**, 41; 1965) using up to date palaeomagnetic data. Their technique was to hold North America stationary and rotate the other continents into the relative positions indicated by the Bullard reconstruction. When this was done, Carboniferous poles from Eurasia and North America, which were widely separated before the rotation, moved to near coincidence. So did Permian poles from North America, Eurasia and Africa. From this Larson and LaFontain inferred that during the Carboniferous and Permian, Europe, North America and Africa were contiguous, or almost so. The situation with regard to South America was less certain; but in so far as the few available poles moved towards the common pole when rotated, the same conclusion could, according to the authors, justifiably be drawn.

In the Triassic the polar coincidences after rotation were not quite so good, which seemed to indicate that this was when the continents began to split. This was confirmed by the behaviour of the Cretaceous poles. Rotation of the African poles to the Bullard fit produced first a convergence and then an increased scatter—an effect to be expected if the rocks had been magnetized since drift began. Moreover, the closest coincidence of North American and African Cretaceous poles occurred about halfway along the rotation path which, according to Larson and LaFontain, must mean that by the Cretaceous the South Atlantic had already opened up by about 50 per cent. Rotation of the Eurasian poles, on the other hand, produced divergence right from the start. Thus by about 100 million years ago the North Atlantic above 40° N must have been about as wide as it is now.

Wright's general objection to all this is "that in 1970 it is still possible for geophysicists to make pronouncements about Earth history without considering geological evidence at all". More particularly he points to evidence already published which suggests that the South Atlantic, at least, must have opened much later than the Triassic. The distribution of salt deposits on both sides of the South Atlantic, for example, has apparently shown that marine conditions did not reach positions about halfway up Africa and South America until about 110 million years ago. Palaeontological evidence from Brazil and West Africa also suggests a final separation of the two continents about 90 million years ago. And the tectonic history of the Benue trough in Nigeria, in turn, gives a split about 80 million years ago.

In quoting this specific evidence Wright is on pretty strong, if somewhat qualitative, ground and would probably have been wise to leave it there. His more general pronouncements are, however, altogether more contentious. He believes, for example, that "geological criteria are *still* (Wright's italics) the most reliable when it comes to reconstructing pre-drift configurations and establishing when they broke up". The irony of this statement becomes apparent when it is remembered that geological evidence was available for at least the first half of this century and yet failed to convince all but a handful of geologists that continental drift was anything but science fiction. Then again he quotes a statement from a colleague in the United States to the effect that "95% of geologists would agree that the South Atlantic opened around 110 million years ago and not 200 million years". To which the only adequate reply would seem to be that thirty years ago 95 per cent of geologists would have agreed that the South Atlantic never opened at all—it was there since creation.

Carbonic Anhydrase may Regulate Photosynthesis

Now that the dust has, to a large extent, settled on the battles fought over the mechanisms of photosynthesis, the centre of interest seems to have shifted from carbon pathways and electron movements to the problems of how the process is regulated. In next week's *Nature New Biology*, D. Graham and M. L. Reed suggest that carbonic anhydrase, an enzyme which facilitates the combination of water and carbon dioxide to yield HCO_3^- and free protons, may play an important, if not the most important, part in the control of photosynthetic processes.

On the face of things this may seem an ambitious proposal, but few workers will deny that enzymologists have always had the happy knack of spinning the most remarkable webs of biochemical pathways on the evidence of finding unexpected enzymatic activities in plant or animal systems. The new hypothesis is based on an unusual photosynthetic phenomenon which can be observed in the unicellular green alga *Chlorella pyrenoidosa*. Cells grown in high partial pressures of CO_2 (5 per cent by volume) show very low rates of photosynthesis if transferred to a CO_2 concentration some ten times less than that in air. An induction period of about 2 hours seems to be necessary before the normal Calvin cycle of CO_2 fixation is restored. In contrast, cells grown in air (0.03 per cent CO_2) do not show this lag on transfer to a low CO_2 environment.

Graham and Reed discovered that the level of carbonic anhydrase activity was very low in cells grown in high CO_2, but that the activity of this enzyme slowly increased, concomitantly with photosynthetic activity, when the cells were transferred to low CO_2. Furthermore, this adaptation to the low CO_2 environment could be inhibited by a prior treatment with 'Diamox', a specific inhibitor of carbonic anhydrase. On the other hand, cells grown in high CO_2 showed no depression in photosynthetic activity if this was measured in high CO_2, even if 'Diamox' was present.

What all this suggests is that the rate of photosynthesis is regulated by the activity of carbonic anhydrase when the concentration of CO_2 is limiting. How this regulation may be achieved is not yet certain, but Graham and Reed have a novel proposal. They suggest that the enzyme may make available the protons which are required to maintain a *p*H gradient across the thylakoid membranes of the chloroplast, or to regulate the proton gradient in the chloroplast. Such a *p*H gradient is, of course, an essential feature of photophosphorylation according to Mitchell's chemiosmotic hypothesis.

Of course, other functions for carbonic anhydrase can be put forward—Graham and Reed mention the possibilities of increasing the concentration of free CO_2 at the site of carbon fixation or changing the permeability of the thylakoid chloroplast membranes—but evidence for these ideas is as scarce as it is for Graham and Reed's hypothesis. And of course the Mitchell hypothesis has not yet found universal acceptance—persuasive as the carbonic anhydrase scheme appears, it is unlikely to prove the last word in the regulation of photosynthesis.