fact that it seems to be unnecessary. In other words, few are convinced that there are any major phenomena that cannot be explained by plate tectonics.

But this is far from saying that the expansion has not occurred. The question remains unresolved; and it is still open to anyone to test the hypothesis against any appropriate data. This is precisely what Stewart (*Geophys. J.*, **46**, 505; 1976) has now done using hot spots and mantle plumes. Of course, mantle plumes are as controversial as the expanding Earth. There are certainly hot spots at the Earth's surface, but whether they reflect underlying crustal–upper mantle phenomena or whether they are due to thin columns rising from the core–mantle interface is still a matter of debate.

One of the main attractions of mantle plumes, however, is the possibility that they might be laterally stable and thus provide a frame of reference fixed to the mantle. Evidence on this point is mixed, with some workers claiming to have shown that plumes remain fixed with respect to each other and others claiming to have detected relative motion. But as Stewart points out, all such exercises have been carried out assuming the Earth to have maintained a constant radius. So how do the data appear against the background of an expanding Earth?

To find out, Stewart has determined the great circle angular distances between the members of pairs of hot spots for three times—the present, 50 Myr ago and 120 Myr ago—assuming a constant terrestrial radius. For points within the same plate, the average ratio of the 50 Myr angles to the present angles is 0.98 ± 0.04 which is not significantly different from unity. The average ratio of the 120 Myr angles to the present angles, however, is 0.88 ± 0.03. The errors on these figures are estimated to be up to 5%, largely because of the difficulty of locating accurately the centres of hot spots which may be 150 km or more across. But taken at face value the older figure suggests that the average great circle separation between members of hot spot pairs could have increased by 11–17% over the past 120 Myr and even up to 6% over the past 50 Myr.

One explanation of these remarkable results is that the hot spots are moving with respect to each other at relative speeds of 0.5–2.0 cm y^{-1} across the surface of an Earth with constant radius. But in that case it would be strange that the members of most pairs are moving away from each other, for in a random system one would perhaps expect as many hot spots to be converging as diverging. On the basis of Stewart's evidence, therefore, it seems much more likely that the geocentric angles between the supposed mantle plumes remain roughly constant whilst the Earth expands to produce divergence of the hot spot locations at the surface.

As Stewart admits, his results can hardly be said to prove that the Earth has expanded. But they are a small step in that direction. □

Balancing the nutrient budget

from Peter D. Moore

THERE have been many commendable attempts to reduce to quantitative terms the influence of catastrophes on the flow of energy through and the cycle of nutrients within ecosystems. Some of the most carefully monitored experiments have come from the Hubbard Brook ecosystem studies in New Hampshire (for example, Likens *et al.*, *Ecol. Monogr.*, **40**, 23; 1970) and the Coweeta catchment area studies in North Carolina (for example, Swank and Douglas, *Science*, **185**, 857; 1974). In both these experimental areas, deciduous forest cover has been destroyed and the resultant changes in runoff water and nutrient losses have been observed. At neither of these sites, however, have the consequences of these hydrological and chemical changes been considered from the point of view of those ecosystems which receive the effluent generated by the catastrophe.

A study has recently been undertaken in Minnesota by Wright (*Ecology*, **57**, 649; 1976) in which the chemical impact of forest fire upon the nutrient budget of receptor lakes has been determined. The forested catchments of Meander Lake and Lamb Lake in the north-east of Minnesota were extensively burned in a serious fire in 1971. Changes in the nutrient budget and hydrology of these lakes were compared with those of Dogfish Lake, a similar site whose catchment lay outside the burned area.

The percentage increase in runoff attributable to the fire was 30% at Lamb Lake and 60% at Meander Lake. The latter value is considerably larger than values obtained after clear felling catchment areas at Coweeta and Hubbard Brook (15% and 40% increases respectively) and the reason for this may be the loss of the absorptive moss and humus layers after the fire in Minnesota. As it moves through the system, water may receive and may also lose various ions. On arrival at the ground, precipitation may move for a short distance over the surface, particularly if the soil is frozen. In burnt areas this water becomes enriched with phosphates. On percolating into the soil these phosphates are adsorbed onto soil particles, but the general cationic load of the water is increased. Stream water is considered to be equivalent chemically to this subsurface flow of water in the soil.

Wright determined nutrient budgets of phosphorus and various cations by monitoring input from precipitation and output in streamflow. The difference between these two rates is equivalent to the weathering input less any change in storage capacity in the system (including biomass growth). It is regrettable, though understandable, that these two components in the budget cannot be separated, but considerable additional measurements

The ecology of dragons: a reply

I AM glad that Robert May thought my article (Hogarth, *Bull. Brit. ecol. Soc.*, **7**, 2; 1976; May, *Nature*, **264**, 16; 1976) seminal; I must, nevertheless, disagree with him on one of his criticisms. In classifying the wyvern and cockatrice separately from the dragon, he attaches more significance to limb number than to other characteristics which these three types held in common. In particular, I refer to their supernatural qualities which seem to me to be of sufficient importance to justify setting them apart in a taxonomic category separate from that of the vertebrates. Four legs do not make a tetrapod; it is no more logical to include wyverns and cockatrices with conventional vertebrates than to classify a 12-legged dragon as a myriapod. Convergent evolution has indeed occurred to a remarkable extent, but between dragons and their allies on the one hand and vertebrates on the other: not, as May suggests, between vertebrates (including the wyvern and cockatrice) and dragons.

My estimate of 5,000 years BP for the emergence of dragons is therefore feasible. The total dependence on human imagination for their existence (a unique ecological relationship) precludes an origin earlier than, say, the late Pleistocene. The absence of convincing representations of dragons in upper Palaeolithic art, and their frequent occurrence in literature and art from the time of Babylon onwards, indicates the chronological limits on the possible time of emergence. Between these limits, 5,000 years ago seems a reasonable, albeit imprecise, estimate.

P. J. HOGARTH