

greater knowledge of oceanic volcanoes might put important constraints on geochemical and geophysical models of the lithosphere's evolution, Batiza has looked more closely at some of the other seamount statistics. Thus he finds, for example, that in the Pacific as a whole, and especially in the North Pacific, there is a monotonic increase in the density of non-hotspot seamounts (number of seamounts per unit area of ocean floor) going back to the Eocene (about 37–53 million years ago). If all oceanic volcanoes had been produced at ridge crests and had spread with the lithosphere to their present positions, this would mean that since the Eocene there has been a gradual reduction in the rate of production of oceanic volcanoes. But it would also mean that each volcano would be of roughly the same age as the lithosphere upon which it sits, which is known not to be the case. The more likely explanation, therefore, is that new volcanoes can erupt on lithosphere of any age and that the density of seamounts increases into the past simply because the older lithosphere has been around longer.

In which case, what causes non-hotspot volcanoes to occur away from the ridges? Unfortunately, there appears to be no strong evidence to suggest that non-hotspot volcanoes are produced any differently from recognizable hotspot volcanoes, which means that they could well be the result of deep-mantle plumes that do not live long enough to give rise to volcanic chains as the lithosphere moves over them. On the other hand, to the extent that volcanic chains have been attributed to propagating fractures in the lithosphere as an alternative to the mantle plume hypothesis, non-hotspot volcanoes could just as well arise where lithospheric fractures provide a conduit for the transmission of magma from the asthenosphere below. There is, in fact, some evidence to suggest that oceanic volcanoes are more abundant where lithospheric fracturing is more frequent, but the correlation does not yet preclude plumes. Hotspot and non-hotspot volcanoes may or may not have distinct origins; it is too soon to say.

But to get back to statistics: although there is an increase in seamount density back to the Eocene, there is then a decrease in density going back further from the Eocene to the late Jurassic (about 136–157 million years ago). This is surprising at first sight, for the explanation of the increase during the more recent period must surely also be relevant to the earlier period, and there is no obvious reason why the Eocene should have been singled out for the favour of a seamount density maximum. The cause lies partly with the sediment that accumulates on the volcanic lithosphere and partly with the lithosphere itself.

With increasing age of the oceanic lithosphere there is a gradual decrease in the proportion of the smaller volcanoes observed and a corresponding increase in

the proportion of larger ones. The reason is partly that as the sediment gets thicker it submerges the smaller volcanoes, which therefore become no longer detectable. At the same time, the older and hence the thicker the lithosphere (because of cooling), the greater is the maximum volcano size the lithosphere can support. Thus although there is, beyond the Eocene, a decrease in the number of seamounts per unit area, the average size of the seamounts increases. Moreover, in terms of overall volume, the increase in average size more than compensates for the loss of numbers. The aggregate volume of seamounts per unit area thus increases with age from the present to the Jurassic with a particularly dramatic increase (by a factor of about 7) between the Eocene and the Jurassic.

There seems little doubt, then, that most of the individual volcanoes now observed on the ocean floors erupted away from the

ridges, for Batiza has adduced strong quantitative evidence that this is the case. By contrast, statistics have been unable to demonstrate that there is any difference between the factors controlling the production of non-hotspot volcanoes and those governing the generation of hotspot volcanoes, although this is far from saying that no such difference exists. To this extent Batiza's analysis has provided less geophysical insight into the workings of the lithosphere than he would perhaps have wished. The geochemical implications of seamounts are not yet much clearer either; but at least it can be said, on the grounds of volume alone, that the geochemical consequences of the existence of seamounts are unlikely to be insignificant.

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Onco gen

What has moved into *c-mos*?

from Peter Newmark

Two separate points are worth making this week. The first is to draw attention to the paper beginning on page 262 of this issue of *Nature* in which Nicholas Gay and John Walker point out a strong homology between a probable ATP-binding sequence of the β -subunit of mitochondrial ATPase and the amino terminus of the p21 protein that is encoded by *c-ras*^H, the cellular oncogene equivalent of the Harvey murine sarcoma virus transforming gene. It is within the amino terminus that a single amino acid change has been implicated in a human bladder carcinoma.

For the second point I am grateful to Nobumichi Hozumi and Robert Hawley of the Ontario Cancer Institute who have spotted a very interesting sequence homology, related to the *c-mos* gene that had apparently been activated in a mouse myeloma by the replacement of one of its ends by a movable genetic element (Rechavi *et al.* *Nature* 300, 582; 1982).

Rechavi *et al.* appeared to have identified a 159-base pair sequence that bore many of the hallmarks of the bacterial insertion sequence type of movable genetic element. They plumped for that interpretation because they detected no long terminal repeats, one of the characteristics of retrovirus-like movable elements.

But now it is possible to unearth strong evidence of a long terminal repeat in the Rechavi *et al.* sequence on account of a paper that Hozumi and Hawley, together with Marc Shulman of the University of Toronto and Edward Kuff, Anita Feenstra, Kira Lueders and Leonard Smith of NCI Bethesda, have in press in *Proceedings of the National Academy of Sciences*.

That paper is a follow-up to one by Hawley *et al.* in the same journal (79, 7425;

1982) in which it was shown that the immunoglobulin κ light-chain gene of two hybridoma cell lines that synthesized little of the light chain, contained retrovirus-like sequences within an intron. Restriction enzyme maps suggested that these sequences were intracisternal A particle genes, which are normally present in about 1,000 copies in the mouse genome.

Kuff *et al.* have now confirmed this suggestion and have sequenced the long terminal repeats of one of the sequences inserted into a κ light-chain gene intron.

Remarkably, the 338-base long terminal repeat sequences — which are almost identical — have greater than 90 per cent homology with 350 of the bases that have replaced the end of the *c-mos* gene in the myeloma of Rechavi *et al.* (the discrepancy in numbers is accounted for by a few minor insertions or deletions). The homology is only apparent when the myeloma sequence is read backwards from the junction with *c-mos* and converted into the complementary strand sequence. Then it is clear that it begins and ends with the same 4-base pair inverted repeat as the long terminal repeat of the intracisternal A particle gene and shares with it some other characteristics of long terminal repeats.

Therefore, an alternative interpretation to that of Rechavi *et al.* for what went on in the myeloma is that an intracisternal A particle gene became inserted in the opposite direction to *c-mos*, displacing part of it and, conceivably, activating what was left by a process similar to the activation of *c-myc* by avian lymphoid leukaemia virus in chicken B-cell lymphomas (Hayward *et al.* *Nature* 290, 475; 1981). □

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