superfluid) liquid ^{3}He, or can be measured from the magnetic ringing which follows a small incremental change in the applied magnetic field. A crucial test of the validity of the ABM and BW state assignments which was suggested by Leggett's calculations (*Phys. Rev. Lett.*, **31**, 352; 1973) involved measuring the ratio f_B/f_A at the A-B transition: according to his equations, the quantity $(f_B/f_A) \times (\chi_B/\chi_A)$ should be precisely equal to $(5/2)^{\frac{1}{2}}$, or about 1.58. Here, χ_A, χ_B are the static magnetic susceptibilities of the two phases.

Subsequent magnetic ringing experiments at La Jolla by Webb, Kleinberg and Wheatley (*Phys. Rev. Lett.*, **33**, 145; 1974) gave a value for f_B/f_A of 1.9 ± 0.1 near the polycritical point (at which the A, B and normal phases are in mutual equilibrium) where $\chi_A = \chi_B$ to an excellent approximation. This unwelcome result received support from work at Helsinki by Ahonen, Alvesalo, Haikala, Krusius and Paalanen who reported (*Phys. Lett.*, **51A**, 279; 1975) that, for similar conditions of P and T, $f_B/f_A = 1.93 \pm 0.05$. These results were disconcerting in that they seemed unambiguously inconsistent with ^{3}He-B being in the BW state, assuming that one had accepted as correct the identification of ^{3}He-A with the ABM state.

On the other hand, Osheroff (the original discoverer of the superfluid phases during his PhD research at Cornell University) and the group at Bell Laboratories, working at the solidification pressure in a compressional cooling cell, were persistently reporting NMR results which seemed entirely consistent with the BW identification of B-phase. In particular, Osheroff (*Phys. Rev. Lett.*, **33**, 1009; 1974) found very close agreement with Leggett's $(5/2)^{\frac{1}{2}}$ prediction. Thus, a situation arose where Bell Laboratories data at 35 bar seemed to support the BW state, whereas data taken at lower pressures in two other laboratories appeared to show that this state assignment was incorrect.

The overall situation was particularly disquieting because, as Leggett pointed out (*Rev. mod. Phys.*, **47**, 331; 1975) it occurred "in an area where neither theory nor experiment appears to have much room to manoeuvre". In the event, the theory and the Bell Laboratories group have stood firm and it is the experimenters at La Jolla who are doing the manoeuvring, as may be seen in the comment from Wheatley's group: their new measurements of magnetic ringing, again near the polycritical point but using a different experimental geometry, have yielded results which appear to be in reasonably good agreement with Leggett's $(5/2)^{\frac{1}{2}}$ factor. It is said that a similar manoeuvre has also been carried out in Helsinki. The reason for the earlier discrepant results is not yet entirely clear, but they can perhaps be regarded as a reminder of how much is still not properly understood about the liquid.

The new measurements at La Jolla, which provide a welcome vindication for the work of Osheroff and the Bell Laboratories' group, may be regarded as greatly strengthening the present state identifications of both superfluid phases of liquid ^{3}He and will, no doubt, have been received with sighs of relief from the many theorists working in the field. □

Dynamo attack

from Peter J. Smith

When the principle of the self-exciting dynamo was put forward in the late 1940s to explain the origin of the Earth's magnetic field, the core motions required to maintain the dynamo were attributed to thermal convection generated by radioactive heating. Much later, Malkus (*J. geophys. Res.*, **68**, 2871; 1963) proposed alternatively that the driving force on the core could be the Earth's precession. The idea of precession-induced flow was not new, for the early dynamo theorists had considered and rejected it. What Malkus did was to show that the original reasons for rejecting a precession-driven dynamo were unsound, since which time both convection and precession have been widely accepted as serious contenders for the role of core driver.

Now, however, Rochester *et al.* (*Geophys. J.*, **43**, 661; 1975) report calculations which appear to prove that the power to be derived from precession is at least an order of magnitude too low to stir the core into stable flow. This disagreement with Malkus would be interesting in its own right; but the report is all the more remarkable in that Rochester and his colleagues go on to criticise Malkus severely not only for his result but for the way he obtained it. Specifically, they accuse Malkus of appealing to dubious analogies, of claiming agreement with previous work where no such agreement exists, of publishing inconsistent equations, of error in mathematical logic, of mathematical oversimplification and of numerical errors, among other things. They further object that in later articles Malkus has not only ignored the few published objections to his 1963 work, but has repeated the errors and retracted nothing.

This is the most severe attack to have appeared in the Earth sciences for many years and raises ghosts from another century. But whether or not one agrees with the form of the criticism, it would be a pity to overlook the serious issue it raises. Many people have come to believe in the feasibility of a precessionary dynamo because they have read Malkus's conclusion but not the arguments on which it is based. The problem is that studies in this field are so esoteric that only a handful of people in the world can understand them; indeed, even Rochester and his colleagues claim not to be competent to deal with all the points involved. As they themselves point out, under such circumstances the rationale for avoiding unpleasant criticism, as described by Ravetz (*Scientific Knowledge and its Social Problems*, OUP, 1971), breaks down and myths develop.

The question now is: is the precessionary dynamo such a myth or not? For the irony is that most Earth scientists will find the detailed arguments of Rochester *et al.* no easier to understand than those of Malkus, and will, as before, have to be content with the conclusion.

Regulatory genes and quantum evolution

from A. Hallam

The abrupt appearance of higher taxa in the fossil record has long been something of an enigma to palaeontologists. Although it is true that temporal sequences of fossils provide some of the best evidence of evolution, Darwin himself was somewhat embarrassed by the sparsity of transitional forms, which he attempted to explain away by invoking numerous erosional breaks in the sequence of strata bearing the fossils. Since Darwin, many of these 'stratigraphic' gaps have been filled as a result of research throughout the world by numerous people, yet the quantum jumps between phyla and lower taxa by and large remain. A few decades ago Simpson made a valiant attempt to grapple with the problem by putting forward an ecological model of 'quantum evolution' consistent with current genetic theory. This involved small populations of particular species crossing some environmental threshold into a new adaptive zone (certain mammals re-entering the sea for example), whereupon there ensued rapid evolutionary radiation and hence pronounced morphological divergence from the parent stock.

Valuable as Simpson's ideas have proved, major problems and uncertain-