might imply an origin by weathering or terrestrial contamination.

Perhaps the most hotly debated topic of the conference was the nature of the magnetite crystals found within the carbonates, and the implications for the origin of the nanofossils. Magnetites have been found in the meteorite that are a similar shape to the McKay *et al.* nanofossils, hinting that the nanofossils may be non-biogenic magnetite (J. P. Bradley, MVA, Inc.). They contain screw dislocations, which are inconsistent with low-temperature formation and have not been observed in biogenic magnetites. Another study (K. Thomas-Keprta, Lockheed-Martin) failed to see such elongated forms, but did find teardrop-shaped magnetites only previously found in biological systems. The source of the discrepancy between these studies is unclear, but may lie in differences in sample preparation.

Few terrestrial rocks have been examined on the nanometre scale for evidence of microbial activity, although that is slowly changing. Columbia River basalts contain bacteria with appendages that are similar in shape and size to those in ALH84001 (Thomas-Keprta). And both magnetite and carbonate can be made by bacteria in laboratory experiments (H. Vali, McGill Univ.).

This conference saw the first round of discussions about past biological activity on Mars. An emerging opinion was that the carbonates could have formed through low-temperature, non-biological processes, offering an alternative to the older polarized views of 'hot and lifeless' versus 'cold and alive'. Many participants, even those who disagree with the conclusions of McKay *et al.*, believe that life may have once existed on Mars and that the search for evidence of it should continue. This conference will clearly not be the last word — proposals for new allocations of ALH84001, and for the necessary funding, are already being considered. Soon, we may know whether we once shared our Solar System with other life forms. □

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Seismology

Tracking slabs in the lower mantle

Guy Masters

Sometimes it seems as if geoscientists as a group are exceptionally bad at solving problems. For example, for about 30 years we have been debating whether or not there is a significant barrier to mantle flow at a level close to the depth of the 660-km seismic discontinuity within the Earth. Our whole understanding of the thermal and chemical evolution of the Earth hinges on this fundamental issue. Most geophysicists and geodynamicists lean towards the view that there is significant mass transfer between the 'lower' and 'upper' mantles. Many geochemists firmly believe the opposite must be true. Perhaps the tomographic images presented in the paper by van der Hilst *et al.* (beginning on page 578 of this issue[1]) will help the geochemists to change their minds.

Several factors make this new contribution notable. The first is that van der Hilst and colleagues' images clearly show narrow compressional velocity anomalies in the lower mantle that are geometrically related to the well-known seismogenic subduction zones in the upper mantle. Such images make it hard to avoid the conclusion that there is significant slab penetration into the lower mantle. The authors argue that it is the careful reprocessing of the basic travel-time data which makes these new images possible.

The second factor is the close correlation of the model of compressional velocity anomalies with the independently determined shear velocity model of Grand[2,3] (compressional waves are the fastest of seismic waves, and are known as P (for primary); shear (S for secondary) waves are slower). Grand's model is based not only on completely different data but also on different processing techniques, so the correspondence between the two models is convincing. Furthermore, the location of the fast anomalies correlates quite well with reconstructions of slab geometry inferred from the recent history of subduction. In fact, the authors suggest that some of the discrepancies between the seismic images and the slab models can be used to refine our knowledge of subduction history.

So how might sceptical geochemists wiggle out of this problem? First, they could appeal to the approximate nature of the algorithm used for converting the travel-time data into an image of velocity anomalies. This particular algorithm might lead to the elongation of structures or even the creation of artefacts. However, comparison of the new images with more conventional global tomographic models[4] of structure at longer wavelengths shows that the same basic features are apparent. (This is particularly true of a recent compressional velocity model based on hand-picked, long-period travel times made from modern digital data[5].)

Second, they could point out that many seismologists are themselves sceptical about the exact width of the features being imaged and, in particular, the amplitudes of the anomalies (which are generally underestimated in tomographic inversions of this type). For example, there is good evidence that the relative perturbation in shear velocity is about 1.7 times the relative perturbation in compressional velocity in the mid lower-mantle (consistent with a thermal origin for the anomalies), whereas the images presented in this issue show the compressional and shear velocity anomalies being of similar size. Obviously, further refinement of the images is needed and the velocity anomalies in the lower mantle will change their amplitude and shape somewhat — but they are not going to disappear.

On the other hand, geochemists have quite compelling reasons for believing that a significant fraction of the mantle is in a chemically primitive state, indicating that the mantle is at least partly segregated. For example, the atmosphere contains roughly half of the mass of ^{40}Ar created by the decay of ^{40}K over Earth history and a reasonable place to put the missing argon is in an isolated lower mantle. Similarly, the flux of ^{4}He observed at ocean ridges is only a small fraction of that expected from radiogenic heat production within the mantle — an isolated lower mantle is again a convenient place for the rest. Other lines of evidence point in this direction and Hoffman[6], in a review published earlier this year, concludes that the geochemical evidence still favours some kind of mantle layering, but one in which the layers are not completely isolated.

Can a model which reconciles all the geophysical and geochemical observations be constructed? Many solutions have been suggested but, in the absence of direct observational constraints on the mass flux between the upper and lower mantles, we cannot get a quantitative answer. The images shown in the paper by van der Hilst *et al.* begin to provide these much-needed constraints.

Hoffman concludes his review of mantle geochemistry by saying: "The controversy over the issue of convective layering and the depth of plume sources is likely to be resolved when the so far rather fuzzy seismic imaging of the Earth's interior becomes comparable in resolution to that achieved by geochemical mapping". Even if we accept this rather provocative characterization of the relative merits of seismological and geochemical mapping, I think we can all agree that the images shown in Fig. 3 of the paper (page 581) are far from fuzzy. □

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