resting spores of *Ceratocystis ulmi* which may be found in fossil lake sediments in culture. Spores of other fungi from sediments as old as the Tertiary have hitherto been cultured successfully[3,4], so viability may not be a problem.

As cereal-type pollen has been shown to pre-date the elm decline[5,6], the real problem of interest to palaeoecologists now is probably not so much why the elm declined but how to find corroborative evidence for early agriculture. I suggest that a study of grass silica remains (phytoliths) could provide the answer. These may be washed into lakes from cropped areas and become incorporated in the sediments. Recent work form the United States[7,8] indicates that the careful, detailed study of phytoliths can provide rather more palaeoenvironmental information than was once supposed. Some papers on British phytoliths have been published[9-11] and these provide a foundation from which research could start. Additionally, it may be easier to determine phytoliths than pollen using automated methods, as the number of morphological types which occur is that much lower and their overall structures are simpler. It is always possible that identifiable portions of leaf cuticle, and not just phytoliths deriving from cuticles, can be found in fossil contexts and analysis of these might make research easier.

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Protein antigenicity and protein mobility

SIR — In an article in *Nature*, Westhof *et al.*[1] discuss the proposition that the antigenic determinants of proteins could belong to regions which show high mobility. In developing this correlation they have inadvertently overlooked work by ourselves[2]. Using NMR methods which detect relatively slow motions as well as the fast motions seen in crystals, we observed that "the region of cytochromes *c* around Ile-57 is very flexible and it is striking that the best characterized antigenic site involves amino acid residues 58–62" (ref.3).

We go on to state that for myoglobin and lysozyme, protein flexibility in charged surface segments is also important for antigenicity.

While not wishing to detract from the work of Westhof *et al.*, we think it right that attention should be drawn to this earlier work, since in the Oxford Enzyme Group we are engaged in a very detailed examination of mobility in relation to antigenicity in the cases of both cytochromes *c* and lysozyme. These studies, using both NMR[4] and crystallographic methods to analyse mobility, show that the idea that such a correlation exists is confirmed but that mobility must be considered in the broadest context of motions on the slow time scale, perhaps even down to seconds, as well as at $<10^{-9}$ s. This means, of course, that the antigenicity need not be due to the ground state conformation or its mobility but can be related to a conformation of slightly higher energy[5]. Such states will be missed unless both crystallographic and NMR methods are used together.

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Doubts over the Colorado Plateau

SIR — I was puzzled by your comment in a News and Views leading article[1] on calderas "Can anyone now doubt that the Colorado Plateau . . . is a magmatic structure of the same kind?" Frankly, yes! I am wondering if it was Dan McKenzie's letter to *Nature*[2] which triggered your suggestion. If so, I think that you read something into his discussion that he did not mean to be there. He was speculating on the possible effects of injecting very large amounts of magma (complete provinces) into or beneath the crust throughout regions such as the Colorado Plateau, in order to account for their uplift without any internal deformation. Calderas are related to single volcanic centres and, although some of them are uncomfortably large (from the volcanic hazard point of view), they are all an order of magnitude smaller than the sort of multicentre regional phenomena that McKenzie was considering. Similarly, resurgent doming is a phenomenon of individual volcanic centres, rather than entire provinces. It is (as the name implies) a dome-like uplift, which may be related approximately to a point pressure source, as magma reoccupies the reservoir below. In contrast, the Colorado Plateau has risen like a lift floor, with essentially all the deformation around its margins. The Flat Tops area, in northwest Colorado, shows a similar style of uplift on a smaller scale than the plateau; strata are flat-lying throughout its uplifted area but almost vertical along the monocline at its western margin.

I think that McKenzie's explanation of the plateau's uplift is probably correct. The region escaped the thermo-tectonic events which affected most of the western United States during the Mesozoic. At about 25 Myr, it was penetrated by scattered, explosive, low-volume, alkaline magmatism which was very loosely related to kimberlite. This carried up lithospheric xenoliths of garnet-lherzolite from depths as great as about 140 km (similar to South Africa), and seems to have originated from the top of the asthenosphere[3]. Subsequently, there have been large amounts of magmatism all around the plateau but virtually none upon it. The surrounding magmatism is compositionally essentially the same as that which characterizes ocean islands, except for local input from crustal fusion in areas of intense volcanic activity. If such liquids come ultimately from the asthenosphere (as must clearly be the case in ocean basins), then the existence and nature of asthenosphere beneath the plateau becomes the vital question. It appears to be both present at >140 km, and seismically normal[4,5]. Thence, following the McKenzie model, one deduces that abundant magmas have left the asthenosphere beneath the plateau during the late Tertiary but none has reached the surface. The only point of McKenzie's analysis that I doubt concerns heat flow. This seems rather too low on the plateau to accommodate the massive amounts of geologically-recent intra-crustal and Moho-depth magmatism he envisages. Perhaps the uprising liquids consolidated deeper, within the sub-Plateau lithospheric mantle. Here they would ultimately become an episode of "mantle metasomatism", of the type (one of the types) that Hawkesworth and his colleagues study[6].

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Mouse Igκ sequence elements in *Drosophila* (erratum)

A letter under the above title from P. A. Tsonis appeared in Scientific Correspondence in the 22 November issue (*Nature* **312**, 314; 1984) with the author's name omitted. □