species, *A. robustus/boisei*). Indeed, fine morphological traits distinguish the Taung teeth from those of Sterkfontein and Makapansgat and, in a few instances, align them with those of the robust australopithecines[7]. If Taung proves to be robust, the confusing position set forth by Olson would follow, namely that the robust australopithecines would need to be added to the hypodigm of *A. africanus*, and the gracile australopithecines from Sterkfontein and Makapansgat would either remain as part of the hypodigm of a vastly more variable *A. africanus*, or need to be removed to form the hypodigm of another species (for which, as Olson points out, the nomen *A. transvaalensis* is available. Since there is now abundant evidence that the gracile and robust australopithecines are taxonomically distinct, the last-mentioned course would probably need to be adopted.

Such removal of the gracile australopithecines of Makapansgat and Sterkfontein from *A. africanus* would cause immense confusion, for the image of the species, *A. africanus*, is based largely on the hominids from these two sites, rather than on the skull of the Taung child, albeit the latter is the holotype.

No less of a muddle would ensue if palaeoanthropologists were now required to call the robust australopithecines *A. africanus*. It was to avert the resulting ambiguity that I raised the possibility of the holotype (the Taung skull) being removed from the paradigm of *A. africanus*—though I assumed and should have added, "if an appeal to the Commission under Article 79 of the Code were successful". In these unusual circumstances of extreme instability and confusion, not only is it reasonable to expect that such an appeal would be permissible under the Code (as Olson points out), but it might well be justified.

Meantime, the entire discussion has helped to establish the case for a comprehensive restudy of the Taung skull. Professor R. A. Dart, discoverer and nominator of the Taung skull, has generously invited me to undertake such a study.

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Paradox of Earth's core resolved

The paradox of the Earth's core is hardly resolved by the recent comments of your Geomagnetism Correspondent[1]. Even your correspondent states that, because of uncertainties in the values of some of the physical parameters in the Earth's core, it is "not possible to say whether convection in the core is feasible or not". He also states that "an origin (of the Earth's magnetic field), other than core convection, has never proved viable", and again "so sure are geomagneticians of the reality of core convection . . .". It is certainly true that no completely satisfactory explanation has been given for the origin of the Earth's geomagnetic field. Most geophysicists, however, would support the idea that it arises from some kind of dynamo action in the core, but what drives the dynamo is very much a matter of contention. The two most reasonable proposals are thermal convection in the core and the Earth's precession; but in neither case has it been possible to establish that the mechanism would work.

Malkus[2] quoted experimental work and order of magnitude arguments to suggest that precession may produce turbulent motion in the core and thus drive the geodynamo. Such reasoning can, however, be seriously misleading, and contradictions can often result from over simplification. Malkus[3] had earlier suggested that precessional torques may drive the geodynamo. Unfortunately, there are some errors in that article—the detailed theoretical investigation of a dynamo in a precessing turbulent core is extremely difficult and as yet no full treatment has been given. Rochester *et al.* (unpublished work) are working on this problem. At present it is impossible to reject precession as the driving mechanism for the geodynamo—it is as likely as thermal convection in the core.

Finally, your correspondent summarised the "flaw" in the Higgins–Kennedy argument that the core is stable against thermal convection[4]. That, however 'has already been pointed out (even more forcibly) by me' both in an earlier letter to *Nature*[5] and elsewhere[6].

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Blocking one-way maternal-foetal MLR

The findings of Jones and Curzen[1] both that lymphocytes from most mothers had low response to stimulating related cord blood lymphocytes in mixed leukocytes reaction, and that only autologous maternal plasmas but not homologous plasmas had an inhibitory effect among those that had relatively high responses, are interesting. Despite the fact that the mixed lymphocyte reaction as measured after 6 d of incubation reflects the sensitisation and responsiveness *in vitro* rather than the state of previous sensitisation *in vivo* as demonstrated by us with the macrophage migration inhibition technique[2–4], the results are consistent with our previous report[3]. It seems that the plasma blocking factors can also inhibit specific sensitisation *in vitro* of maternal cells as well as prevent the expression *in vitro* of cell mediated immune reactivities of lymphocytes previously sensitised (*in vivo*) to solubilised placental antigens as we reported.

On the other hand, the findings of low maternal lymphocyte response to related foetal cells does not warrant the conclusion that maternal lymphocytes may somehow be affected by previous exposure to the plasma blocking factors. The results could as well be accounted for by the abnormality of the foetal cells in their effectiveness as stimulating cells. In fact, if previous exposure had any effect at all, the surface antigens of the foetal cells are a more likely target. The intact responsiveness of the maternal lymphocyte to stimulating related cord or unrelated adult lymphocytes in one-way mixed lymphocyte reaction was indicated by studies by Carr *et al.*[5].

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