given in that paper and only one adds a useful new argument. The best evidence in each direction does not impinge on that for the other, and neither side has a satisfactory explanation for the opposing evidence.

The arguments against the effect of an impact, all discussed in my review, are:

(1) A *Protunqulatum* community lacking dinosaurs.
(2) Appearance and progressive evolution of the *Protunqulatum* community just before the boundary.
(3) Freshwater community unaffected.
(4) Last occurrence of dinosaurs being detectably below boundary in Montana and vicinity
(5) Existence of transitional floras below boundary.
(6) Putatively extraterrestrial material below boundary.
(7) Absence of effects of elimination of stratospheric ozone.
(8) Coincidence with culmination of very large regression.
(9) Marsupials but not placentals nearly eliminated; most arboreal multituberculates and birds survived.
(10) Darkness expected to occur too often.
(11) Concentration of Ir, etc., in reduced material, with associated timing anomaly.
(12) Dinosaurs occurring well above the palynological boundary in New Mexico.
(13) Effect of predicted cooling not seen.
(14) Effects of predicted acid rain not seen.
(15) Absence of turbidites at boundary (if marine impact) and apparent absence of large terrestrial crater.

The first two items refer to a community of Paleocene aspect, whose members were ancestral to much of the Paleocene fauna and which appeared in Montana 4×10^5 yr before the boundary. Smit and van der Kaars[5] claim that the sediments involved postdate the boundary, but their work ignores the field relationships given in a detailed geological map[6] (other evidence and will be refuted more adequately in a paper in preparation by several people). That my list of evidence is one-sided, reflects merely the greater publicity which given to evidence favouring an impact as the cause of the extinction.

The results of Bohor *et al.*[4], who found quartz fragments at the boundary which had been shocked at very high pressure, are at first sight strongly favourable to an impact. But until possible alternatives such as a uniquely powerful explosion (perhaps analogous to the smaller ones of kimberlites) are considered, some scepticism seems appropriate. Unless there were mutliple impacts[2], for which there is no evidence (despite considerable search) except an unconvincing [3] interpretation of osmium isotope ratios[7], it is not apparent how the impact hypothesis can be modified to resolve the major conflicting evidence. Yet volcanism, even broadly conceived, has equally severe problems[3,4].

Coincidence is not a satisfactory basis for theory.

On the "late Eocene mass extinction", I know of no evidence that it exists. For instance, there are as many last appearances of marine families recorded in the middle Eocene as in the late Eocene[8], neither value being unusually large; species extinctions are not concentrated at one horizon[9]; and the mammalian turnover is later[9] and clearly involves at least a major biogeographic interchange[10].

LEIGH M. VAN VALEN
Biology Department (Whitman),
University of Chicago,
915 East 57th Street,
Chicago, Illinois 60637, USA

1. Maddox, J. *Nature* **308**, 685 (1984).
2. Davis, M., Hut, P. & Muller, E.A. *Nature* **308**, 715-717 (1984).
3. Van Valen, L.M. *Paleobiology* **10**, 121-137 (1984).
4. Bohor, B.F., Foord, E.E., Modreski, P.J. & Triplehorn, D.M. *Science* **221**, 867-869 (1984).
5. Smit, J. & van der Kaars, S. *Science* **223**, 1177-1179 (1984).
6. Bell, R.E. thesis, Univ. Minnesota (1965).
7. Luck, J.M. & Turekian, K.K. *Science* **222**, 613-615 (1983).
8. Van Valen, L.M. *Nature* **307**, 50-52 (1984).
9. Keller, G., D'Hondt, S. & Vallier, T.L. *Science* **221**, 150-152 (1983).
10. Russell, D.E. *et al. Palaeovertebrata, Mem. Extraord. (1982).*

Did komatiitic lavas erode channels on Mars?

SIR — Huppert *et al.*[1] show that terrestrial Precambrian komatiitic lavas may have erupted as very hot, highly fluid, turbulent flows capable of eroding deep channels and melting and assimilating rocks over which they flowed. We have argued[2] that the chemical composition of martian fines, measured by Viking Lander spacecraft[3], indicate derivation from mafic igneous rock, that terrestrial Precambrian komatiitic lava would be a good candidate for an analogue. Using our derived Mars composition, Schonfeld[4] calculated that martian lavas had viscosities of about 0.5 Pa s. The new findings of Huppert *et al.*, that komatiitic lavas could have been as hot as 1,700° C and as low in viscosity as 0.1 Pa s, renew our interest in the possibility of martian channel formation by lava[5].

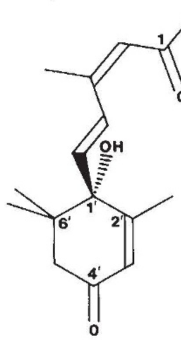
Various channels on Mars have been ascribed to tectonism, earthflows, wind action, glacial ice, and flowing basaltic lava, but the majority have features (braided channels, terraced walls, sculpted islands, dendritic tributaries, meanders, increase in size downstream) that have forced most observers to conclude they were formed by running water. However, the source of the water, and where the water is today, remain vexing problems. (Most workers believe that water had to be more abundant on Precambrian Mars and what water remains is locked principally in ground ice). Most of the previous objections to lava as the principal erosive agent on Mars (see ref.6) are based on observed characteristics of basalt. Such lavas erupt at around 1,200° C and have viscosities more than fifty times that found by Huppert *et al.* for komatiites. If martian lavas were komatiitic, however, a water-like fluid would have been available and such a fluid would explain not only channel formation, but also the enormous lateral extent of martian flows and the absence of apparent outflow deposits (sediments) in the basins into which the large channels discharge. Finally, if water did not carve martian channels, the complex schemes to explain the present lack of surface water, including gross changes in the atmosphere/climate of the planet, are not needed.

ALEX K. BAIRD
Geology Department,
Pomona College,
Claremont, California 91711, USA
BENTON C. CLARK
Martin Marietta Aerospace,
Denver, Colorado 80201, USA

1. Huppert, H.E., Sparks, R.S.J., Turner, J.S. & Arndt, N.T. *Nature* **309**, 19 (1984).
2. Baird, A.K. & Clark, B.C. *Icarus* **45**, 113 (1981).
3. Clark, B.C. *et al. Science* **194**, 1283 (1976).
4. Schonfeld, E. *EOS* **57**, 948 (1977).
5. Cutts, J.A., Roberts, W.J. & Blassius, K.R. *Lunar planet. Sci.* **209** (1978).
6. Baker, V.R. *The Channels of Mars* Ch. 3, 34-55 (University of Texas Press, Austin, 1982).

A question of chirality

SIR — Hanke writes (*Nature* **310** 272; 1984) "It is quite remarkable that immunoglobulins should be able to discriminate between the enantiomers because, as can be seen from the diagram, the molecule of abscisic acid is almost symmetrical...".



To my eye, the degree of chirality shown by abscisic acid is almost exactly the same as that between my feet. Perhaps Hanke is more supple than me, but I cannot put my right foot into a left shoe. Why should a globulin be more flexible?

N.W. PIRIE
42 Leyton Rd,
Harpenden, Herts, UK

Scientific Correspondence

Scientific Correspondence is intended to provide a forum in which readers may raise points of a rather technical character which are not provoked by articles or letters previously published (where Matters Arising remains appropriate).