LETTERS TO THE EDITOR

ASTRONOMY

Distribution and Origin of Lunar Craters

THE communication by McCall¹ leads me to make some further comments. It is certainly true that the non-random distribution of the large craters is a serious and probably fatal objection to the impact hypothesis so far as the major formations are concerned, though no doubt many small meteoric craters exist. The Mare Crisium seems to be nothing more than an exceptionally large crater-like formation, and is a member of the great western chain which also includes Petavius and Langrenus, in which case it seems that the other circular-type maria are basically similar. Moreover, in the case where one crater breaks into another, the wall of the broken formation is perfectly preserved up to the point of junction; Thebit is a good case. This could not be so were the second, younger crater produced by a violent impact. I directed attention to this in 1956², and the same point has since been made independently by Kopal and others.

It seems, however, that this argument can also be used against the idea of violently explosive vulcanism, and it may be argued that the process of crater formation was relatively gentle and prolonged. In making this suggestion³, I had in mind an uplift and subsidence process which, if repeated several times with interior remelting, would result in the production of a terraced crater.

PATRICK MOORE

Glencathara, Worsted Lane,
East Grinstead, Sussex.

¹ McCall, G. J. H., *Nature*, **196**, 255 (1962).

² Moore, P., *Formacion de los crateres lunares, Urania* (Tarragona, 1956).

³ Moore, P., *J. Brit. Astro. Assoc.*, **71**, 362 (1961).

I HAVE elsewhere¹ expressed agreement with Mr. Moore's view that the lack of shaking-down effects provides an important clue to the origin of lunar craters, and at the same time expressed my own view that the structure of the crater Porter is inexplicable in terms of explosive evisceration, a process which I have never favoured, though simple dish-craters in maria could be produced by a bubble-burst through a thin plastic veneer. The fact that craters appear to have been the product of slow and gentle processes does not, in my view, invalidate my hypothesis. Certainly the large craters stem from subsidence, and, as on Earth, the initiating upwards push may not necessarily have been explosive in every case. The blisters seen in some craters probably reflect gentle pressures without break-out. It remains possible that many of the large cirques were initiated by point-focus explosion, forming axial vents and confocal fractures, not craters. This seems most likely in the case of early structures, since, once fractured, the retaining crust would allow slow gas leakage rather than explosive discharge. It is interesting to note that at least one carbonatite complex reveals some evidence of a recessive stage involving cauldron subsidence². Moore's suggestion of gentle uplift and subsidence, repeated several times, fits in well with patterns of history in terrestrial volcanic foci, but, as on Earth, one must expect no uniformity of detailed history, and I do not think point-focus explosion can be ruled out, entirely, because of the objection made.

One of the most potent arguments for a volcanic origin of lunar craters is the progressive decrease in size with age. A large crater never shows evidence of developing on the same focus as an earlier smaller structure. This is the essential pattern of history displayed by terrestrial calderas: I do not know of a single recorded case where a secondary caldera has not been of diminished magnitude

compared with its predecessor. I would be interested to know if other geologists can record such a case.

G. J. H. MCCALL

University of Western Australia, Nedlands.

¹ McCall, G. J. H., *J. Brit. Astro. Assoc.*, **72**, 7, 356 (1962).

² McCall, G. J. H., *Geol. Mag.* (in the press).

PHYSICS

Re-entrant Motion in Special Relativity

THE argument by Dr. T. E. Phipps¹, in which he claims to show that the velocity at the edge of a rotating disk is not ωr and that ωr can be greater than c , contains a simple fallacy from the point of view of relativity theory.

The fallacy lies in the assumption that if the radius of the rotating disk is r then the "rest length" circumference of the disk will be $2\pi r$. This relation is true only when the disk as a whole is at rest in the inertial frame S . Of course, the radius of the disk when it is stationary in S will in general be different from its value when rotating. In the actual rotation considered an element of the boundary which has rest length δx^* in S^* suffers the Lorentz contraction when witnessed by the observer in S . Thus the observer in S ascribes to the circumference the total length $\Sigma \delta x = 2\pi r$, which is appropriate to the Euclidean geometry in his frame; on the other hand, the aggregate of measurements by observers in the local frames S^* on the boundary will be $\Sigma \delta x^* = 2\pi r / (1 - v^2/c^2)^{1/2}$, albeit the disk dwellers would also ascribe a total length r to the radius. The non-Euclidean character of the geometry in accelerating frames of reference was inferred in this way by Einstein long ago and led him to adopt the Riemannian metric in general relativity.

Subject to this correction the rest of Phipps's calculations are now seen to be consistent with $|v| = |\omega r| < c$. Thus his equation (2) should read $2\pi r/T = -v$ and since, as Phipps agrees, $T = 2\pi/\omega$, (3) then becomes $v = -\omega r$ and because of the appearance of v in the factor $\sqrt{1 - v^2/c^2}$ we can conclude as usual that $|\omega r| < c$.

Thus there is no question, as Dr. Phipps goes on to suggest, that the much discussed identical twins age equally and that both "stay young". The experiment of Hay *et al.*², discussed by Sherwin³ to whom Phipps refers, is actually in reasonable agreement with the expected age discrepancy proportional to the factor $\sqrt{1 - v^2/c^2}$.

Note added in proof. Since this communication was submitted letters from Dr. R. H. Boyer and Dr. D. B. Lichtenberg, pointing out the error in Dr. Phipps's argument that I have indicated above, have appeared in *Nature* together with Dr. Phipps's reply⁴. Nothing that has been said in these communications alters my view of the correctness of the interpretation of relativity theory given here.

W. DAVIDSON

Department of Mathematics,
Battersea College of Technology, London, S.W.11.

¹ Phipps, T. E., *Nature*, **195**, 67 (1962).

² Hay, H. J., *et al.*, *Phys. Rev. Letters*, **165** (1960).

³ Sherwin, C. W., *Phys. Rev.*, **120**, 17 (1960).

⁴ Boyer, R. H., *et al.*, *Nature*, **198**, 886 (1962).

Observation of Mixing at Optical Frequencies in a p-n Junction

MIXING of light corresponding to adjacent axial standing wave modes of a ruby laser has been effected in a silicon p-n junction. The junction was prepared by grinding down the side of an S.T.C. RS-284 power rectifier diode