It is in any event difficult to believe that the alternative picture suggested by Piddington and Minnett, of a uniform 1-mm. layer of dust, over mountain-side and plain alike, can be more than a convenient analytical model.

With regard to the supposed radioactivity, I am on weaker ground. I had failed to consider the need for high mechanical strength to maintain the observed shape of the Moon, and I am grateful to Prof. Jaeger for pointing this out. It is clear that the average lunar radioactivity must be well below that given for the Earth's crust, unless the thermal conductivity of the solid lunar rock is a good deal higher than hitherto supposed. The obvious way out is to suggest a non-uniform distribution of radioactive material. The likelihood of this depends on what we assume about lunar history, which seems too large a subject to discuss here.

My main contention remains: that I am willing to feel doubt about any of my assumptions, but am unwilling to give up the entire picture until an alternative explanation of Kozyrev's observation is given.

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Magnetostriction and Palæomagnetism of Igneous Rocks

IN a recent communication, Stott and Stacey[1] report on a "crucial experiment" from which they conclude: "This excellent agreement between the dip and the directions of artificial thermoremanent magnetization of the stressed and unstressed rocks indicates that large systematic errors due to magnetostriction are most improbable in igneous rocks of types normally used for palæomagnetic work". This experiment was intended to test the proposals[2] and measurements[3] bearing on the role of magnetostriction in rock magnetism. We present here our reasons for believing that the experiment was not crucial and that the conclusion is not justified.

The magnetic fraction of natural igneous rocks almost without exception consists of mixtures of two or more crystalline phases. These phases may be distinguished magnetically by a number of parameters which are dependent on structure, composition and particle-size. Commonly these segregated phases are intimately intermingled, for they developed from pre-existing simpler phases which were stable only at a higher temperature. As a result, there may be strong interaction of the magnetic fields of the different phases on one another. The magnetic properties of the bulk samples are thus determined both by the properties of the individual phases and by the nature of the magnetic interactions among them. The degree to which the phases have become segregated and the size scale on which the intergrowth textures have developed depend, among other things, on composition and thermal history. Kawai *et al.*[4] have demonstrated that in Japanese effusive rocks, unmixing at low temperatures is a process requiring tens of millions of years, whereas, in contrast, low-temperature unmixing can be detected in as little as three days in a particular sample which was first held at 750° C. for three days and was then quenched to room temperature. As a result of unmixing, the magnetic properties of rocks change conspicuously. At elevated temperatures, the rate of homogenization of unmixed phases varies greatly among different samples; heat treatment at 650° C. for 15 min. is sufficient to cause gross changes in the phase structure and composition of the magnetic fraction of numerous rocks. This fact has been observed repeatedly in many laboratories and is abundantly documented, especially in the Japanese works.

Stott and Stacey ignore these facts, assuming that their experiments on natural samples which have been heat-treated 15 min. at 650° C. and then cooled over a period of several hours while under axial stress give realistic replicas of typical conditions in Nature. Clearly, this assumption is not safe, except possibly for the case of some young rocks which were cooled quickly; it does not permit generalizations, for example, about the possible role of magnetostriction in influencing the directions of residual magnetization of samples of a dolorite sill which cooled over a period of hundreds of years while under load, remained under a load that decreased over millions of years while magnetic phases were segregating from original simpler phases, and finally were brought to the laboratory for measurement.

We feel that this oversight by Stott and Stacey should be pointed out for, in our opinion, there are already too many instances where conclusions founded improperly have given the subject of palæomagnetism a questionable status.

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Borides and Silicides of the Platinum Metals

A GREAT number of intermediate phases exist in the systems of boron with platinum metals[1] but very little information about these phases is available. X-ray crystallographic studies of platinum metal borides have been initiated at this Institute, and we have determined three new structure types (tentatively called Ru_7B_3, RhB and IrB_{1-2} types) which will be discussed briefly in this communication. We have found evidence that the 'Rh_2B' phase with *C*23 structure reported by Mooney and Welch[2] might really be Rh_2Si.

The alloys were prepared by melting mixtures of the elements (99·7–99·8 per cent pure) in an arc furnace. The crystal structures were determined with single-crystal methods, while the lattice parameters were obtained from powder photographs.

The Ru_7B_3 structure. This structure is hexagonal and was recently described in detail[3]. The space group is $P6_3mc$ with two formula units in the unit cell. Ru_7B_3 and Rh_7B_3 crystallize in this structure, which is especially interesting for its close similarity to the Cr_7C_3 structure[4].

The RhB structure. The most probable space group of this orthorhombic structure is *Cmcm*. The four