LETTERS TO THE EDITOR.

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Helium and Geological Time.

IN NATURE of October 27 (p. 543) a short notice appears relative to some experiments of Prof. A. Piutti, of Naples, on the occlusion of helium from the air by salts in the act of solidification. Prof. Piutti apparently considers his results as throwing doubt on the figures which I have given for the age of different geological formations from the accumulation of helium in them. I wish to give my reasons for dissenting from this criticism.

In the first place, it is not clear from Prof. Piutti's description that the gases extracted from his solidified salts contain any more helium than normal atmospheric air. He has not attempted to show, if I understand his description rightly, that there is any *selective* absorption of helium in preference to the other atmospheric gases; nor is it at all likely that such a selective absorption exists, for we have no knowledge of chemical affinity between helium and other gases, while, in respect of solubility, it would probably be inferior to them. Again, on account of its low molecular weight, it would, if anything, be better able to escape from mechanical retention. But the gases from minerals are in practically all cases many times richer in helium than is atmospheric air. Mere retention of air cannot therefore account for any appreciable proportion of the helium found.

There remains the question, also alluded to by Prof. Piutti, of whether helium can have been absorbed from any source in the interior of the earth. I have already discussed this question, as regards igneous rocks, in Proc. Roy. Soc., A, vol. lxxxiii., p. 298. As regards the bulk of the rock, it is impossible to exclude such an origin, and I have carefully avoided drawing conclusions which might be vitiated by it. My inferences have been drawn from minerals like zircon and sphene, which are immensely more radio-active than the rock in general, and immensely richer in helium. It is without plausibility to assume that the excess of helium in these has any extraneous origin.

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Pwdre Ser.

ON my return from a field season beyond the reach of periodicals, I have just seen, for the first time, Prof. McKenny Hughes's article on "Pwdre Ser" in NATURE of June 23, and the correspondence relating thereto in the succeeding numbers. It may interest your readers to know that a substance of this sort was found by Mr. Rufus Graves (at one time lecturer on chemistry in Dartmouth College) at Amherst, Mass., on August 14, 1819, and by him identified with a luminous meteor which had been seen to fall at that spot on the previous evening. His report of the occurrence appeared in the *American Journal of Science*, vol. ii., pp. 335–7, 1820. The mass of jelly was circular, about 8 inches in diameter and about 1 inch thick. It was of a bright buff colour, and covered with a "fine nap similar to that on milled cloth." The interior was soft, of an insufferable odour, and liquefied on exposure to the air. Some of this liquid was allowed to stand in an open glass for a few days, when it had entirely evaporated, leaving only a small quantity of a "fine ash-coloured powder without taste or smell," which effervesced strongly with sulphuric acid, but not with nitric nor hydrochloric.

Mr. Graves's account was noted by Arago in the *Annal. de Chimie*, vol. xix., pp. 67–9 (1821), who quoted also several similar occurrences cited in earlier chronicles. It is probable, of course, that Mr. Graves was mistaken in his identification, that the meteor actually fell at some other point, and that the jelly was confused therewith only because no other unusual substance was found at the point where the meteor was *supposed* to have fallen. Mr. Graves himself considered that there was "no reasonable doubt that the substance found was the residuum of the meteoric body," but the evidence which he states is hardly satisfactory to the modern, more critical inquirer.

It seems probable that these jellies are, in general, plasmodia of some form or forms of Myxomycetes, and that their common identification with falling stars may have its basis in the frequent recurrence of this error into which Mr. Graves seems to have been led. It is well known that visual estimates of the distance of falling stars are almost invariably far too low. If, then, an untrained observer of a meteor goes next morning to the near-by place where he *thought* he saw the body fall, and finds there no unusual body excepting one of these plasmodia, the jelly and the meteor are almost sure to be associated in his mind. Especially is this probable, since the plasmodia, in general (at least in my experience), have the appearance of having fallen on the grass rather than of having grown there.

EDWARD E. FREE.

United States Department of Agriculture, Bureau of Soils, Washington, D.C., October 17.

On Hydrogen in Iron.

AT the recent meeting of the British Association at Sheffield, Sir Norman Lockyer referred to the relationship between hydrogen and iron at stellar temperatures. Some observations of mine, made several years ago, are of interest in this connection. I also note that at the recent meeting of the Iron and Steel Institute, in a discussion on the influence of carbon in iron, it was suggested that the gases known to be present should also receive attention.

Iron contains ten times its volume of hydrogen, and in many instances 20 volumes of hydrogen; even 100 has been noted. Iron therefore contains from about 0·013 to 0·026 per cent. of hydrogen, 100 volumes equalling 0·13 per cent., all deemed important in metal with like proportions of carbon and sulphur and phosphorus. It is now fully admitted that hydrogen hardens iron, and should therefore be estimated: 1 gram frozen H, *ice*=7·2 c.c., 0·1=0·72 c.c. I note also iron=1 c.c.=7·2 gram iron. 1111 c.c. of solid H=1/10 gram in 100 iron=14·4 c.c.=7 grams per 1000 c.c., and 1000 ordinary pressure=only 0·08961 gram. The figures quoted are apparently in accordance with the periodic law, series 1–7.

As regards the above, more might be said if space permitted.

JOHN PARRY.

October 19.

Research Defence Society.

IN connection with the cases of plague in Suffolk, let me say that this society has lately published an illustrated pamphlet on "Plague in India, Past and Present," by Lieut.-Colonel Bannerman, director of the Bombay Bacteriological Laboratory. It gives a full account of the experiments which proved that fleas carry the plague from rats to man; it also gives a full account of Haffkine's preventive treatment, and of the many thousands of lives which have been saved by this treatment. I am sorry that the Research Defence Society cannot afford to give away this pamphlet in large quantities, but I shall be happy to send it to any of your readers who will send me seven stamps. I shall also be happy to send copies, on sale or return, to all booksellers.

STEPHEN PAGET.

(Hon. Secretary Research Defence Society.)

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British Mammals.

I AM grateful for your reviewer's good wishes for the success of my book (NATURE, October 20). He writes that he has only one fault to find, namely, that a paper of Dr. K. Andersen's dealing with the authority for the names *Nyctalus noctula* and *N. leisleri* is not mentioned anywhere. I beg to state that the title of this paper is given on p. 53. It could not be cited in the synonymy, as the names *Nyctalus noctula* and *N. leisleri* do not actually occur in it. In fact, I believe that my book is the first in which these names occur.

G. E. H. BARRETT-HAMILTON.