LETTERS TO THE EDITOR.

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An Electromagnetic Problem.

IN the application of general principles to special cases it is sometimes found that the result is a seeming paradox, which is not always easy to remove. Such problems, although involving no new principle, are nevertheless of considerable interest, and after attaining their satisfactory solution we often realise that we did not before appreciate the full import of the general law.

The following question has been discussed with considerable interest among some of the writer's friends, and therefore it seemed not improbable that other physicists might be interested.

If two spheres of positive electricity are near together and are suddenly released, it is clear that their potential energy decreases as they separate and goes over into kinetic energy of motion. This kinetic energy is, of course, the energy of the magnetic field which results from the motion of the charges.

It seems possible, however, to arrange a system so that this magnetic field shall vanish because of symmetry, and the question then presents itself, Where is the energy? Suppose we have a sphere of positive electrification placed as the water is in a soap bubble, and capable of expanding under the mutual repulsion of its parts. The potential energy of the electricity certainly decreases as the sphere expands, and *if the electricity be considered continuous* there is certainly no chance for a magnetic field, as is easily seen from consideration of symmetry. If the sphere be allowed to expand, where does the energy go? The obvious answer is that the electricity is not continuous, but exists as discrete particles, *i.e.* as electrons; but if we try to escape the difficulty in this way, it is equivalent to admitting that the electrical laws, together with the conservation of energy, *require in themselves* the discrete structure of electricity. If, on the other hand, we say that the electricity is associated with matter, *i.e.* with ponderable mass, and the energy appears as ordinary mechanical energy of motion, then we are admitting that the electrical and energy laws *require the association of electricity with matter.*

There seem to be no other solutions to the problem than those above given, and if we admit either of them we reach a conclusion which certainly is striking when we consider that we have only used the general laws of electricity and energy.

The writer does not state the above as a fundamental paradox, but only as an interesting problem.

D. F. COMSTOCK.

Institute of Technology, Boston, Mass., November 3.

The Progress of Aviation.

I HAVE read with great interest the article on the above subject by Prof. Bryan in NATURE of October 29.

May I be permitted to direct especial attention to the necessity for finding the displacement of the centre of pressure on all kinds of surfaces and at all angles therein referred to? The paper by Prof. Bryan and Mr. Williams on the subject of longitudinal stability, and Captain Ferber's article in the *Revue d'Artillerie* (November, 1905), both assume the truth of Joëssel's law. There is, however, every reason to suppose that there is a certain critical angle below which Joëssel's law ceases to be true, the displacement decreasing with the angle instead of increasing.[1]

Consequently, the numerical conclusions arrived at from the stability formulæ of Captain Ferber and Prof. Bryan may be very wide of the mark.

I hope to experiment in this direction myself, but my time is very limited. There can be no doubt whatever that a thorough investigation as to the centre of pressure would be of the greatest practical use.

HERBERT CHATLEY.

32 Britannia Road, Southsea, October 31.

I AGREE strongly with all that Mr. Chatley has said. It cannot be too emphatically pointed out that the object of our stability investigations was to show that the subject is capable of being treated mathematically, and that, *given the requisite experimental data,* the conditions of stability of any system of planes or surfaces can be calculated out in the form of numerical results. The cases in which this was done were intended merely as *examples* illustrative of the general method, and for this purpose Joëssel's law furnished the simplest assumption available at the time. It will be noticed, too, that arbitrary values were assumed for the moments of inertia of the systems. To draw inferences from the results of examples worked out with this object would be an unfortunate mistake.

It is to be regretted that want of time has prevented my attempting to work out any examples based on the Turnbull results, though the idea suggested itself when I saw the paper in the *Physical Review*. The theory of stability has thus been somewhat at a standstill. Those who, like Mr. Chatley and myself, would like to see that theory advanced are prevented from doing this by pressure of other duties, while those who have the necessary time and money have been mainly occupied of late in breaking records. Mr. Lanchester's theory of stability starts from so different a standpoint that it must be discussed at a future time.

G. H. BRYAN.

Potato Black Scab.

THE discovery this autumn of black scab in the potato crop in two localities in co. Down was the means, through the Irish Department of Agriculture, of supplying me with excellent material of diseased tubers for examination. I have kept the resting "spores" of the chytridian fungus *Chrysophlyctis endobiotica*, Schilb., causing the disease, under varied conditions of temperature, nourishment, moisture, and light, and have succeeded in causing the "spores" to germinate, especially by cultivation in potato juice. Each "spore" proves to be a zoosporangium, full of zoospores or zoogonidia, seen in active swarming motion before rupture of the sporangium. The zoospores, 1·5–2 μ in diameter, escape through a slit-like opening in the wall of the sporangium 30–60 μ in diameter, and have the usual characters of a chytridian zoospore.

Since the publication of Schilbersky's short preliminary account in 1896 in the *Berichte der deutscher botanischen Gesellschaft*, and Potter's account of his discovery of the pest in Cheshire in 1902, we have learnt nothing of the life-history of this injurious fungus.

T. JOHNSON.

Royal College of Science, Dublin, November 17.

The Nature of γ Rays.

EXPERIMENTS by Prof. Bragg and myself upon the secondary kathode radiation which proceeds from matter through which γ rays are allowed to pass, taken in conjunction with the similar result announced by Mr. Cooksey in NATURE of April 2 (vol. lxxvii., p. 509) for X-rays, support the theory of the material nature of X and of γ rays originally advanced by Prof. Bragg.

The modification of the ether-pulse theory recently advanced by Prof. Thomson may possibly furnish a partial explanation of these effects, but in the light of some experiments which I have lately carried out upon the secondary γ rays, even this modification seems quite insufficient. A brief summary of these results is appended.

(1) The γ rays of Ra, and probably of Th, appear to consist of two distinct homogeneous bundles, the value of λ/Δ (where λ is the absorption coefficient and Δ the

[1] Spratt-Moedebeck's, "Pocket-book of Aëronautics" (1901); Wilbur Wright, Smithsonian Report, 1902, pp. 133–148 (*Journal of Western Society of Engineers*, December, 1901); Turnbull, *Physical Review*, vol. xxiv., No. 3, 1907.