Radio-activity of Potassium and other Alkali Metals.

PROF. MCLENNAN'S letter in NATURE of May 14 (p. 29) makes it desirable that I should record that my experiments continue to be in contradiction to those he describes. Whether the activity of potassium is due to an extraneous impurity or to the element itself, it is to be expected that some separation of the activity should be possible. For the last year and a half I have been trying to effect such a separation without result. All samples of the same pure salt which I have procured or prepared are identical in activity. In one experiment a sample of the sulphate was crystallised twenty-two times, in another a sample of the nitrate was crystallised eighteen times without introducing any difference between the final products; in a third a sample of potassium sulphate was prepared direct from wood ashes, and found to agree with a commercial sample prepared from the Stassfurt deposits. I propose now to prepare a third sample direct from felspar.

The only difference which I can detect between Prof. McLennan's experiments and my own lies in the fact that he places the active material inside the ionisation vessel, while I place it outside an aluminium window. If he were measuring the effect of very soft rays, the difference between our results might be explained.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, May 14.

On Dispersion and Spectrum Series.

IN reply to Mr. Campbell's letter of April 30, it must suffice to point out that my letter of March 5 was limited to infinite spectrum series and to luminous gases. This seemed to me at the time of writing obvious from the context, as well as from the express reference to the finiteness of the refractive index of *luminous* hydrogen. Apparently I was mistaken; anyhow, Mr. Campbell's suggestion that I confused the emission lines of luminous with the absorption bands of dark hydrogen rests on a misconception of my meaning.

To avoid further misunderstanding, I will add that by "electrical theory of absorption and dispersion, of magnetic rotation and Zeeman effect," I mean, of course, the theory of Drude, together with its extension by Voigt to *all* magneto-optic phenomena; I do not mean theories such as those of Lorentz and Ritz, which deal with the Zeeman effect alone. I know no reason for confining that theory to one member only of a series, *e.g.* the D lines of sodium, and contend that it leads to contradictions when applied to all the members of an infinite series of similar lines.

The remaining points raised by Mr. Campbell seem to me foreign to my argument. I cannot discuss them in the space at my disposal here, but hope to do so elsewhere.

Bonn, May 8. G. A. SCHOTT.

Secondary Waves of Light.

IT has hitherto been held that, so long as the diffraction apertures used (cut in perfectly opaque or perfectly reflecting screens) are large compared with the wave-length of light, Fresnel's expression for the amplitude of the disturbance due to a surface-element gives us a close approximation to the observed diffraction effects, and that the exact value for the obliquity factor is of little importance (*e.g.* see Schuster's "Optics," sec. 48). That this is true only in the special case in which the apertures are held normal to the waves of light, and not in other cases, is shown by some new diffraction phenomena that I have made the subject of study.

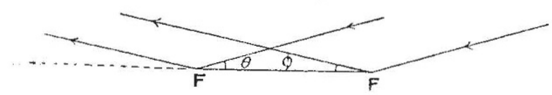
The only experiment so far known which might seem to show effects due to the obliquity factor is the well-known one with the circular disc, but it is really inconclusive. The observed fact, that the illumination along the axis of the disc decreases as the disc is approached, is more or less entirely due to minute irregularities in the rim of the disc, and not, as is sometimes stated, to the increasing obliquity of the secondary waves producing the illumination.

The theoretical grounds on which my experiments were based were these:—if diffraction bands are produced and observed in a direction in which the amplitude of the disturbance in the secondary waves varies rapidly from point to point, we might expect effects due to varying obliquity. Such effects would obviously not occur if the diffraction aperture or mirror is, as is usual, held normal to the waves of light incident on it, but might if it be held obliquely.

In the *Philosophical Magazine* for November, 1906, I showed that the diffraction bands due to a rectangular aperture held very obliquely are not equidistant, that the band-width increases progressively from one side of the pattern to the other, and that the number of bands on one side of the pattern is limited. They are most easily seen on the spectrometer if the image of the slit of the instrument formed by light reflected very obliquely from the face of a prism is observed. The positions of the minima of illumination, actually observed, are closely in agreement with those calculated from the usual formula $(\cos\theta - \cos\phi = \pm n\lambda/a)$, θ, ϕ being the complements of the angles of incidence and diffraction. Further observations have elicited the following:—from the expression for the intensity of the illumination in the pattern deduced by the ordinary method

$$I = a^2 \sin^2 \frac{\pi a}{\lambda} (\cos\theta - \cos\phi) \div \frac{\pi^2 a^2}{\lambda^2} (\cos\theta - \cos\phi)^2,$$

it would appear that the maxima of illumination in corresponding bands on either side of the middle one should be of equal brightness. This is flatly contradicted by observation, both visual and photographic. It is found that the bands on one side are considerably fainter than those on the other, and this difference becomes very large as the light approaches grazing incidence. The illumination in the diffraction pattern (with a given angle of incidence) decreases and dies away as we approach the limiting plane of the fringes, which is the plane of the reflecting surface (FF in the diagram).



This effect is inexplicable if the question of the variation of the amplitude in different directions of the secondary waves, supposed to be sent out by the elements of the reflecting surface FF, is not taken into account. It must be remembered that we are not dealing with apertures small compared with the wave-length; both the aperture and its projection are large compared with λ, and there are no polarisation effects observed. The question may be attacked analytically, and it can be shown that an element of the surface of a reflecting body is equivalent in its effect to a double source of appropriate intensity which, it is known, produces zero effect in its equatorial plane and a maximum along its axis. The effect of an element of the surface FF is therefore zero along the line FF, and in other directions increases as we move away from the line FF. Remembering that the elements are not in the same phase, and integrating their separate effects, we get an explanation of the phenomenon observed.

A fuller discussion and a mathematical investigation will be published in due course. I found that similar effects are observed when the transmitting aperture is used. Some experiments with coarsely ruled gratings are in progress which seem to point in the same direction.

C. V. RAMAN.

Science Association Laboratory, Calcutta, April 2.

The Corrosion of Iron and Steel.

IN NATURE of May 14, Dr. J. Newton Friend alludes to *Gallionella ferruginea* as obtaining its life's energy by oxidising ferrous carbonate and organic ferrous salts, causing the precipitation of rust, or ferric hydroxide. May I point out that *Gallionella ferruginea* can live and grow well without any iron at all, and so cannot be a vital factor in the metabolism of the bacterium, using the term "bacterium" in its widest sense? The oxidation which takes place can be simply explained by the fact that ferrous carbonate in solution is very unstable, becoming very rapidly oxidised.

W. F. MACFADYEN.

54 Dunard Street, Glasgow, May 16.