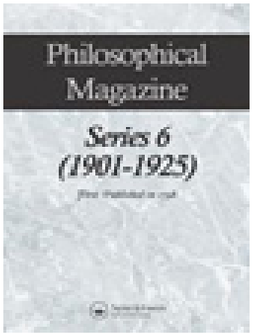

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LXXXIX. Ether, light, and matter

Sir Oliver Lodge

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would be required to remove one of these two electrons than is required to remove one from the outer shell. If, therefore, we interpret our results on the basis of this theory, each of the three ionization velocities found in neon would correspond to the removal of one of the eight outer electrons. Our results therefore indicate that in the outer shell of the neon atom electrons occupy three dissimilar positions with regard to the nucleus and inner shell. This conclusion is in contradiction to the theory of Lewis and Langmuir in its present form, but it is possible that the essential features of the theory might be retained without necessitating absolute similarity of position among the electrons in any particular shell.

LXXXIX. *Ether, Light, and Matter.*

By Sir Oliver Lodge *.

IN the October 1913 number of the Phil. Mag. vol. xxvi. pp. 636–673, there is a remarkable though highly speculative paper by Professor Bruce McLaren (killed, alas! in the war) in which, among other things, he attempts to explain gravity by treating matter as a source or sink of ether. The flow of ether which is thus necessarily postulated is supposed to transfer momentum from the destroyed portion to the boundary of its destruction, and thus virtually to exert a stress on the surface of transition, notwithstanding that matter is unlike a foreign body immersed in a stream, but is a peculiarity of the ether itself. He seems to think that physicists will object to locomotion of the ether, as perturbing to the rays of light; but so long as motion is irrotational it can be shown that rays of light are not affected, though the waves are carried along and made excentric. In other words the path of a specified unit of luminous energy is not altered by any irrotational drift, whatever happens to the wave fronts; for in a moving ether the rays, *i. e.* the paths of energy, are not normal to the wave front, and nothing perceptible need happen. That nothing happens is conceded by the Principle of Relativity; though an explanation of why nothing perceptible happens, and the idea of drift of wave fronts, are foreign to that theory.

That nothing happens on ordinary theory, at least to the first order of aberration magnitude, follows thus:—If the

* Communicated by the Author.

drift v is inclined at angle θ to the ray, and if the consequent aberration angle between ray and wave-normal is ϵ, so that

$$\frac{\sin\epsilon}{\sin\theta}=\frac{v}{\mathrm{V}}=\alpha,$$

the resultant velocity is

$$\mathrm{V}'=\mathrm{V}\cos\epsilon+v\cos\theta,$$

and the path of the ray between any two points A and B is determined by

$$\mathrm{T}'=\int_{\mathrm{A}}^{\mathrm{B}}\frac{ds}{\mathrm{V}'}=\text{a minimum},$$

which can be written, without approximation,

$$\mathrm{T}'=\frac{\mathrm{T}\cos\epsilon}{1-\alpha^2}-\int_{\mathrm{A}}^{\mathrm{B}}\frac{v}{\mathrm{V}^2}\cdot\frac{\cos\theta}{1-\alpha^2}\cdot ds. \quad . \quad . \quad . \quad . \quad (1)$$

So if $v\cos\theta=d\phi/ds$ (that is if the drift has a velocity potential ϕ) the second term—the only one obviously containing θ—is proportional to $\phi_{\mathrm{B}}-\phi_{\mathrm{A}}$ and is independent of path. In other words, the time is unaffected by the drift, to the first order of v/V.

In another medium, V becomes V/μ, and, if v becomes v/μ^2 in accordance with Fresnel's law, the same velocity potential ϕ still holds good. And this accounts for the negative results of a large number of last century's experiments with prisms and water-filled telescopes, &c., some of which are described in 'Nature,' vol. xlvi. p. 500.

Even proceeding to the second order of drift velocity, only the first term of (1) is effective—at least so long as the drift is constant; because for a closed contour, such as is required for any feasible interference experiment, the second term vanishes. The necessary condition may not be satisfied under the peculiar conditions of destruction of substance and failure of the continuity equation; hence perhaps McLaren's caution. But a second order effect, dependent on $\cos\epsilon$, *would* be expected, even in a uniform drift; for the duration of the drifted to-and-fro journey in any direction, compared with the time of the same journey when everything is stagnant, is, by (1)

$$\frac{\mathrm{T}'}{\mathrm{T}}=\frac{\cos\epsilon}{1-\alpha^2}=\frac{\sqrt{(1-\alpha^2\sin^2\theta)}}{1-\alpha^2}; \quad . \quad . \quad . \quad (2)$$

an expression which may be regarded as the simplest theoretical summary of Michelson's experiment. For in

that experiment the times of a to-and-fro journey are compared, when $\theta=0$ and when $\theta=90°$; and, as everyone now knows, the null result of that experiment, combined with my experiment establishing the absence of convective or viscous etherial drift ('Nature,' vol. xlvi. pp. 165 and 501, or Phil. Trans. 1893 and 1897), necessitated some novel explanation, and led to the FitzGerald-Lorentz contraction.

(Parenthetically I take this opportunity of correcting a rather confusing misprint or slip in the summary of my historical survey communicated to the Physical Society in May 1892, which appears on page 165 of 'Nature,' vol. xlvi. Of the two terms in the expression for T', above the middle of the first column, the drift angle θ should appear only in the second term, while in the first term the angle should be the much smaller aberration angle ϵ, which is essentially of the second order in v/V.)

Hence a flow of ether need not be objected to on the ground of a perceptible disturbance to rays of light; but instinctively one feels that a Le Sage-like hypothesis concerning gravity is not likely to be on right lines. There must admittedly be a stress in the ether between two particles of matter, but it should be a static rather than a kinetic stress, and should not be accompanied by locomotion. Locomotion of the ether is objectionable for a variety of reasons: the only motion permissible in a universal medium of infinite extent is a circulatory or vortex motion, such as might occur along lines of magnetic force. Magnetic lines are always closed curves; there is no known way of generating them; they always pre-exist, though they may be of atomic or molecular magnitude, and in a magnetic field are opened out so as to enclose a perceptible area. This is generally admitted to be the process of magnetisation; and when the magnetism ceases the lines shrink up into infinitesimal, or practically infinitesimal, orbits again. That the *quantum* is associated with these ultimate magnetic units is exceedingly likely (*cf.* Dr. H. Stanley Allen, Proc. R. S. Edin. November 1920); and an association of electric units with the magnetic ones is all that is necessary to account for radiation.

In the Phil. Mag. for April 1921, I ventured on a speculation that matter is a sink as well as a source of radiation—a sink not of ether but of the radiation movement of ether. Annulling of the electric component in an ether wave, though the process commonly generates heat, may also under some conditions liberate the magnetic component, which, at a distance of $\lambda/\pi\sqrt{2}$ from a resonating particle, is left behind by the electric component. By analogy it

should not be obliterated. The electric component there attains for a moment an infinite speed and gets out of phase with the magnetic component. When the phase difference is 180° the energy-flow is reversed in sign ; when the phase difference is 90° there is no energy-flow, and only a stationary vibration existing inside a certain boundary.

The problem is whether part of the magnetic circulation, left stranded, could not cease to be oscillatory and become continuous and permanent; and whether the synchronous electric pulses of myriads of successive waves could not accumulate as a separated pair of opposite electronic charges.

Meanwhile we may roughly estimate that to generate such a pair requires energy comparable to 10^{-6} erg, which would be supplied by a length of a few hundred kilometres of ordinary sunshine if the area of wave contributing to the result were comparable to λ^2/π, as suggested by Lord Rayleigh's "Sound" investigation in the Phil. Mag. for August 1916 (*cf.* 'Nature,' vol. cvii. pp. 169 and 203). But to generate a weighable amount of matter, say a tenth of a milligramme, in the laboratory, a beam of sunlight a square decimetre in section, even if all of it were effective, would have to shine for seventeen years.

P.S.—In the current Proc. Roy. Soc. for February 1921, vol. xcix. No. A 697, p. 118, Professor Eddington shows, in a manner terribly difficult to understand, that it is "impossible to build up matter or electrons from electromagnetic fields alone,—some other form of energy must be present." The other form of energy postulated above is the previously existing particle on which the electromagnetic waves impinge; and an operation, such as Prof. Eddington is probably competent to envisage, is supposed to go on in its immediate neighbourhood—*i. e.* in a region where the electric field is exceedingly intense, and where the product of certain S tensors becomes important. It may be worth just noticing that the ordinary normal surface tension $(2\pi\sigma^2/\kappa)$ on an electron, which the structure of the electron is somehow able to resist, approaches to within a twentieth of what I have elsewhere supposed to be its limiting or critical value 10^{33} dynes per square centimetre, or what might be called the weight of a thousand earths per square inch.