A Philosopher on Relativity.

By Sir OLIVER LODGE, F.R.S.

AT the twenty-third annual meeting of the Eastern Division of the American Philosophical Association, held at Brown University, Providence, R.I., in December 1923, a presidential address on "The Einstein Theory and a Possible Alternative" was delivered by Prof. William Pepperell Montague, professor of philosophy in the Columbia University of New York. In this address Prof. Montague shows himself to be a philosopher rather especially interested in physics; and he claims the advantage of having discussed certain aspects of the subject with Prof. Bergen Davis and other members of the physics department at Columbia. He reviews and criticises parts of the Einstein theory in a lively manner, thereby arousing interest among philosophers; and his contentions are deserving of notice.

Apparently Prof. Montague raises no objection to Einstein's General Theory, so far as it deals with matter and gravitation, and so far as it replaces the idea of force by the idea of space-curvature. He seems to consider these apparently diverse ideas as only different methods of expressing the same thing; much as you might say that it does not matter whether you represent an ellipse by drawing it, or by writing $x^2/a^2 + y^2/b^2 = 1$. Both modes of expression represent the same thing. Some might hold that a picture is a nearer approach to reality than a formula, but both call up relevant ideas in the mind. Moreover, a picture,—which regarded from a strict point of view is millions of molecules of one substance superposed upon billions of another,—must depend helplessly on the human mind for recognising in it any of the properties of an ellipse!—must, in fact, depend just as much on mind for its interpretation as does the algebraic convention. After all, as regards precision of measurement, the mathematical rather than the pictorial presentation is the more explicit and accurate of the two.

I must make it clear that Prof. Montague expresses appreciation of pure mathematics and feels no philosophical objection to those more striking and revolutionary ideas of Einstein which were borrowed from the pure mathematicians. In the geometrical way of regarding matter and force, the infinite finiteness of the universe, the representation of impenetrable specks of matter as impassable distortions of space,—in all this Prof. Montague rather rejoices; as the following extracts will show:

"... bodies moving with accelerated velocities are subject to forces, hence the later Einstein theory consists, first, of a new though Cliffordian conception of the nature of *force*; and, second (growing out of that), of a new though Riemannian conception of the *universe itself*, as a domain at once boundless and finite. . . .

"Some will undoubtedly feel stifled and penned in at the thought that if they were to travel in a 'straight' line for only a few septillions of miles they would find themselves on the way home without ever having turned around. Others will find the new Riemannian hyper-sphere large enough for all practical purposes; and they will delight in the possibility of relating in some curious way the curvature or size of the universe as a whole to the dimensions of its minimum elements."

Prof. Montague does not think that the hyper-geometrical conceptions of reality need be upset by what he urges as failure in the simpler basis on which the original doctrines of relativity were founded—doctrines admittedly not so well established by actual *ad hoc* observation of predicted results as those of the more general theory have been.

"It is not, I think, too inaccurate to say that while the Special Theory reduces geometry to physics and is offensively destructive and phenomenalist in its conception of the world, the General Theory reduces physics to geometry and its tone and temper are rationalistic and constructive. The muddy prose of Hume and Mach gives place to the clear poetry of Riemann and Clifford."

Prof. Montague goes on to pay a tribute to the late Prof. Clifford, with which, as an old student and junior friend of that remarkable genius, I cannot but sympathise:

"It was Clifford, working under the inspiration of the new geometry of Riemann, who, I think, first suggested that matter could be conceived as a non-Euclidian wrinkle in space. . . . It is pleasant to think that Clifford, whose mind was of such rare beauty and who in his appreciation of spiritual values possessed what the musicians call a sense for absolute pitch, should have been the first to hold a conception which now seems destined to have most far-reaching consequences."

So it turns out that Prof. Montague's criticisms are directed, not at the more recondite parts of the subject so finely developed and expounded by Eddington, but at its foundations as ordinarily expressed. His main objection seems to be to the Larmor-Lorentzian law for the composition of velocities, and to the absolute constancy of the velocity of light as supposed to be measured by different observers. He objects also to this velocity being regarded as a maximum velocity which cannot be exceeded. His view is that light is not really emitted and abandoned to a stationary ether, as a water splash is abandoned by a ship, but that light is continually associated with the lines or field of force belonging to and moving with the source,—somewhat after Faraday's idea of ray vibrations; an idea which, in modified form, has obtained weighty support of late, and about which it is best not to be dogmatic.

There has been a conspicuous tendency, of late, to revolt against the simplicity of the undulatory theory. Something more than that is evidently wanted. And incidentally I may mention that Prof. Benedicks, of the Metallographic Institute of Stockholm, has written an immediately forthcoming small book called "Space and Time," in which he supports some modification of the emission theory. With a corpuscular theory in empty space, the Michelson-Morley experiment would raise no difficulty at all, and probably would never have been performed; for the velocity of light would depend on the motion of the source, and everything would be simple. A similar simplicity is aimed at by Prof. Montague. He does not indeed propose a corpuscular theory—he sees difficulties in that,—but

he is well aware that what he does suggest is quite alien, and indeed contrary, to the early parts of the Einstein theory. He adduces therefore simple imaginary experiments, akin to those so often appealed to by philosophic relativists, by which he thinks the Einstein view might be disproved. He realises the difficulty of doing anything effective with only two signalling observers, who must be dependent on inconclusive to-and-fro journeys, so he arranges a triangular duel, with a third observer, or rather signaller, equidistant from the other two.

Let there be two observers on the same parallel of latitude, with perfect signalling appliances and stop-watches, which, however they are affected by the rotation of the earth, must at least go at the same rate; and let there be a third signaller at the north pole, who by symmetry is able to establish simultaneity between the other two. At the word "Go!" from the pole, the two on the equator signal to each other, and time the interval between sending and receiving. Prof. Montague claims that, on the ordinary wave theory, and as he mistakenly seems to think on the Einstein theory, the one travelling to meet the signal should get it sooner than the one whom it has to overtake. For, as he says, even waves take less time to travel less distance,—... "even Einsteinian light travels a shorter distance in less time than it does a longer distance."

On the other hand, on a corpuscular theory there would be no observable difference, because the increased speed of the corpuscles from an ordinary source would compensate for the recession of their recipient. So both Prof. Montague and Prof. Eddington would, I presume, expect a negative result; and the experiment would not discriminate between them. Unless, indeed, it gave a positive result, to the chagrin of both, and the triumph of a common-sense view based on the behaviour of ordinary waves.¹

It will be observed, however, that Prof. Montague has abandoned translation, as hopeless for his purpose, and has utilised rotation; and about rotation there has always been something with a tinge of the absolute about it, even in relativity doctrine. But whether it is necessary to make this admission, in order to safeguard a positive result from Prof. Montague's imaginary experiment, is very doubtful. His own attitude is hostile to waves in a stationary ether, as well as to the less intelligible doctrine that every observer, at all times, must measure the same velocity for light; he does not actually sustain or mention a corpuscular theory, as Prof. Benedicks does,—and as apparently the Swiss genius Ritz did,—but his postulated entanglement of the waves with the field of the source comes to somewhat the same thing.

Prof. Montague goes on to attack the composition of velocities in a more direct manner, and likewise ridicules several curiosities of time measurement, including queer tales by rapid travellers with return tickets, cited from Eddington and Weyl; but concerning the hypothetical slowing down of physiological processes by motion, and other humorous interpretations of certain equations, I am not myself seriously sym-

pathetic enough to justify my trying to sustain them against criticism.

Incidentally I note that he has a long footnote in which, by means of to-and-fro north and south signalling compared with the result of similar east and west signalling, he claims that we might distinguish between Einstein and Lorentz,—whatever that may mean. It looks as if he thought that Einstein discarded the β factor representing the FitzGerald-Lorentz Contraction (introduced by Larmor in 1900, "*Æther and Matter*," p. 173 *et seq.*, and vigorously used by Einstein), while Lorentz admitted it. Either I misunderstand him here, or he makes a mistake.

The alternative which Prof. Montague seriously proposes to the relativity treatment of light is not the corpuscular theory,—for, as he says, "light is in almost all respects wave-like rather than corpuscular"—but it is this,

"... that the medium or carrier of light waves is not a doubtfully existent ether-substance, but the certainly existent field of force which each electron and proton carries with it."

He thus desires to abolish a stationary ether, as Einstein does not, and claims that because an electric field is centred on its charged particle, it follows that when a charged particle moves, its field must move with it.

"The velocity *in* the field will be constant and absolute, but the velocity of the field has to be added to it."

Thus he tries to combine the advantages of the wave and emission theories in this respect. But the full treatment is not so simple; a great deal is known about the way in which a field moves, and he would find himself immersed in plenty of complexities, unless he discarded electromagnetic theory altogether.

Finally Prof. Montague proposes an experiment to test his crucial assumption, namely, that the velocity of light is affected by the motion of the source, and is not independent of it as it would be in a stationary ether and as it is in Einstein's theory. His proposed experiment depends on rotation, again; and, to summarise it briefly, it employs the idea that light from the sun's advancing limb ought to come to us quicker than from the receding limb. His proposed rotator is not the sun, however, but a mechanical cylinder, sphere, or disk, projecting light from opposite points of its circumference, through a couple of narrow slits or tunnels, on to a moving sheet of sensitive paper. The slits or narrow tunnels are carried by a pair of drums, which are separated by a considerable distance and are revolving about an axis at right angles to the main axis and parallel to the path of the two beams of light. (He makes these drums revolve in opposite senses, but that is surely a mistake or a needless complication.) It follows that if they are revolving quickly enough, the light may be unable to get through both slits, because of the time taken in the journey between them, and he claims that with increasing speed the fading of the spot from one beam should precede the fading of the other. His experiment therefore is really a Fizeau-like experiment to measure the velocity of light, or rather to compare the velocities from two oppositely moving sources without the use of a mirror or any return journey: the fading and disappearance of the traces on the photographic film at a certain

¹ May I direct attention to a clear pre-relativity description of all this class of problem, in an old volume of *NATURE* for 1892 (vol. 46, pp. 497-502, and also on pp. 164 and 165; the latter with a misprint of θ for ϵ in two places).

speed of rotation being looked for, and the times of fading compared. But presumably any mode of determining the velocity of light would serve: the essence of the experiment is not the mechanical details, which are crude, but the suggestion that it is possible to find a difference in the velocity of light according as it comes from the approaching part of a rotating wheel or from the receding part.

Well, that is a definite suggestion for experiment, if any one really thinks it likely (as I do not) that the velocity of light depends on the motion of the source: though it would seem as if measuring the velocity of light from an approaching and receding star would be more likely to decide the question. At any rate, the difficulties encountered by employing a non-terrestrial source would be of another order. Prof. Montague fears, however, that a final entanglement of the light in the fields of atmospheric matter might interfere with a correct determination of velocity from an astronomical source. It might also be argued that the known Doppler effect, depending on stellar advance and recession, gives a relative frequency accurately calculable on the basis of a constant velocity for light, and that to supplement this by the peculiarities of a projectile kind of light would be merely perturbing. To this he would doubtless reply that a fair and square initial consideration of light as projected from its source will give the Doppler effect and explain the M.M. result with perfect ease. He might also claim, perhaps, that his variety of ray-wave would evade the usual Foucault objection to emission theories, and might give the Fizeau result in dense substances as well. These things are not elaborated in his paper.

But now—seeking what can be politely said in support of any of his contentions—it must be admitted, I think, that the relativity explanation of the Doppler effect can be made to look as if it had a weak point. Algebraically it comes out right, of course, but it has too much the air of the ordinary explanation, which attributes an obvious meaning to $c \neq v$. The ordinary explanation, for an observer approaching a source, is simply that the observed frequency increases in the ratio

$$\frac{n}{n_0} = \frac{c+v}{c}$$

by simple composition of velocities: which is still Professor Montague's, and used to be everybody's, argument. But, with the relativity law for compounding velocities, deduced from Larmor's transformation, this so described ratio would be unity, and there would be no Doppler effect at all if it had to depend on an apparently changed speed of arrival of the waves. Relativity, however, does not attempt to proceed by compounding velocities for the purpose of explaining the Doppler effect: it knows that c cannot be added to or subtracted from, by anything a source or an observer can do; so it simply applies the Larmor-Lorentz transformation to the space and time periodicities of wave motion, and deduces algebraically

$$\frac{n}{n_0} = \sqrt{\frac{c+v}{c-v}}.$$

Neglecting v^2/c^2 this has the same value as the ordinary expression above, and no experiment yet made can discriminate between them. But am I wrong in

imagining that a relativist would dislike writing it in that form and would prefer the expression

$$\sqrt{\frac{1+v/c}{1-v/c}}?$$

For this, though identical, conveys no inappropriate suggestion of compounded velocities.

In conclusion, it is always satisfactory when controversialists can get up against a question of fact. Stripping it to bare bones, we may put these two questions:

(1) Does the velocity of light really depend on the speed of its source?

Wave theory, Einstein theory, ether theory, all say "no." Corpuscular theory answers "yes": and Prof. Montague's adopted conception of a ray vibration also, in his opinion (but as I think doubtfully), answers, "yes."

(2) Does the measured or apparent velocity of light depend on the motion of an observer?

Corpuscular theory, and Prof. Montague's theory, reply, "Yes if the source is stationary," "No if the source is moving with the observer." Ordinary wave theory would say "Yes in any case," for what the source is doing does not matter; that can only affect wave-length, not speed. Einstein's theory would loudly say "No in any case": for if one could discriminate between motion of observer and motion of source, relativity as commonly stated would be upset, and attention to a stationary ether would be made inevitable. Experiment so far has declined to answer: for a to-and-fro journey involves squares and can be dodged. So then at present we have no discriminating reply, unless an answer in favour of Einstein is to be taken as implied by the general consistency and sufficiency of his theoretical results both old and new.

One who is impressed with the mysterious way in which the Larmor-Lorentz transformation thoroughly formulates all known results without any trouble or hesitation, and yet who is not helped thereby to form a clear physical conception of the process, might urge that to clinch matters an experiment is desirable which should aim at determining $c \neq v$; when v is the speed, not of source, but of observer. That is, an experiment is wanted which would measure the speed of an observer relatively to light, and therefore relatively to a stationary luminiferous medium. Direct determination of the velocity of light at different seasons of the year, by any method, would at first sight seem able to settle this question, provided an accuracy greater than one in ten thousand could be attained.

But consider this further. A fixed space-interval and a time-keeper constitute the real observer: the FitzGerald contraction does not depend on whether motion is positive or negative, nor does direction of motion affect the rate of a clock. Hence if the experiment is made now and six months hence, x/t will equal x'/t' , and no difference in the speed c will be perceived.² That seems right from one point of view; and yet, physically, waves *ought* to travel down an ether stream quicker than up, unless group velocity has something objectionable to say.

To sum up the position, still in an interrogative

² Because $\frac{x'}{t'} = \frac{x/t - u}{1 - ux/c^2}$, which equals c if $x/t = c$, and not otherwise.

manner without dogmatism:—change of frequency depends solely on relative motion of source and observer: combined motion of both through a medium does not affect frequency on any theory, so long as the motion is steady. Ordinary aberration is wholly and solely caused by motion of observer relative to path of ray. Is observed velocity of light dependent on motion of observer, too? The strength of the relativity position is that no way of performing the

experiment is likely to give a positive result, unless truly relative motion is introduced, as by mounting the whole receiver on the end of a revolving arm; and questionably even then. The strength of the ether position is that a relative ether stream past fixed "stations," though undemonstrated, is at least not negated by this or any other experiment, and may therefore nevertheless be a reality. A philosopher may be able to explain what "reality" means.

Obituary.

DR. ROBERT KIDSTON, F.R.S.

BY the unexpected death of Robert Kidston on July 13, palæobotany suffers an irreparable loss. Dr. Kidston was still at the height of his activity, and had probably never been busier in his life than when the end came. He was then in South Wales, engaged on his fossil investigations. Equally distinguished in systematic and structural palæobotany, Kidston was the veteran leader in his science, worthy to rank with the great continental masters, such as Zeiller, Grand'Eury, and Nathorst, who had already passed away.

Robert Kidston was born at Bishopston House, Renfrewshire, on June 29, 1852. The family removed shortly afterwards to Stirling, where Kidston's life was spent. As a young man Kidston went into business in a bank, but his strong bent for natural history soon turned him towards scientific pursuits. About 1878 he attended botanical classes in the University of Edinburgh. He was then, and remained throughout life, a keen field botanist.

Kidston soon became interested in fossil plants, and his work met with early recognition, for, from 1880 onwards, the plant-remains collected by the Geological Survey of Scotland were referred to him. This gave him his first great opportunity.

In Kidston's work as a fossil-botanist two periods may be distinguished. From the beginning up to 1904 he was predominantly a floristic and stratigraphical palæobotanist, and so indeed he continued to be, but from 1904 onwards he further took a leading part in morphological and structural investigation, and expressed himself with a wise authority on the great questions of affinity and descent. His work in every field was remarkable for that sound judgment which has so generally characterised the best systematists.

From about 1880 a constant succession of papers on fossil plants was maintained; only a very few can be mentioned here. In 1886 Kidston was entrusted with the duty of preparing a catalogue of the Palæozoic plants preserved in the British Museum, a valuable work of reference in its time. Thus he was already recognised as our leading systematist in Palæozoic botany.

Kidston always took a special interest in the fructification of ferns and fern-like plants. A paper published in 1887, "On the Fructification of some Ferns from the Carboniferous Formation," proved to be of exceptional importance, for various reproductive organs described belonged, as Kidston afterwards showed, to the group of the Pteridosperms. Thus, in the case of *Neuropteris heterophylla*, of which he afterwards discovered the seed, the fructification described in 1887 turned out to be the male form.

The memoir on *Lepidophloios* (1892) is the leading monograph on that genus of fossil Lycopods. A paper on the various divisions of the Carboniferous rocks as determined by their fossil flora (1893) is a valuable contribution to the geology of that period. Two important memoirs on the fossil flora of the Yorkshire coal-field, on which he had already published a number of reports, appeared in 1896 and 1897. The second is of remarkable interest, as containing the first complete description of the cones of *Sigillaria*, and thus finally establishing the true nature of that long-disputed genus.

A general summary of the flora of the Carboniferous Period appeared in 1901–1902. This was written at a transitional time, when botanists had recognised the Cycadofilices as a class, but were still ignorant of their reproduction. Kidston's remarks do justice to the position at that date.

The second and more brilliant period of Kidston's work opens with his important paper on the fructification of *Neuropteris heterophylla* (1904). He was the first to demonstrate, by direct proof of continuity, that a fern-like plant of the Coal Age bore seeds. Previous evidence (only slightly antedating Kidston's discovery), however convincing, had been indirect. Kidston, in fact, was a pioneer in this field. A year later (1905) he published a preliminary note on Microsporangia in connexion with the foliage of *Lyginodendron* and was thus the first to identify the male organs of a "seed-fern." This was followed up in 1905 by his great memoir on the microsporangia of the Pteridospermæ, a truly admirable work, showing a rare soundness of judgment in discussing the affinities of the newly discovered group.

A purely anatomical memoir on the internal structure of *Sigillaria elegans* (1908) was the first full and illustrated account of the structure of a ribbed *Sigillaria*. The work was extended to other species a year later.

Other papers on structural subjects dealt with new species of *Dineuron* and *Botryopteris* (early ferns) and with a new *Lepidodendron* from Pettycur, all of Lower Carboniferous age. Kidston had a special predilection for Lower Carboniferous plants, and was among the first to grasp the full importance of this fine flora.

One of Kidston's later memoirs on the fossil flora of the Staffordshire Coal-Fields (1914) includes a number of important discoveries, the most striking being perhaps the pollen-bearing organ of a new *Neuropteris*, very different from that of *N. heterophylla*.

The mere glance we have taken at a few outstanding papers may serve to show how Kidston's work illuminated every side of Palæozoic botany. His work was fully appreciated on the Continent and was by no