ON THE PROPAGATION OF LIGHT IN ROTATING SYSTEMS, A REJOINDER TO DR. A. C. LUNN

By
LUDWIK SILBERSTEIN

Dr. A. C. Lunn in his comments[1] on my paper on this subject[2] proposes to deal "chiefly" with my main contention, but before doing so points out at some length a number of "minor items."

That contention was that a fractional shift effect as a possible outcome of the discussed terrestrial optical experiment, now conducted by Prof. Michelson, would be crucial, namely against Einstein's relativity theory *such as it is*[3] (*i.e.* with the express inclusion of $ds=o$ as the law of light propagation and of $\delta\int ds=o$, with the same ds, as the law of free motion), and favourable to the revival of an aether sharing in part the earth's rotation. Since Dr. Lunn has in the meantime admitted the essential correctness of this result, in a conversation at the recent Lorentz Colloqium at Madison,[4] I need not insist here any more upon it. But the "minor items" are of such a nature as to call for an explicit reply.

In the first place then, Dr. Lunn, while granting readily the claimed necessity of a reference frame for rotation "in spite of appearances to the contrary" (5, p. 291), wonders what those appearances are. Now, such a clause (made, moreover, rather incidentally) has at the time of writing seemed worth making in view of the usual presentation,—even in such fine books as Poincare's 'Science and Hypothesis,'—which is apt to leave the average reader under the impression that it has after all a sense to

[1] This Journal, *6*, p. 112-120; March 1922.

[2] *Ibidem*, *5*, p. 291-307; July, 1921.

[3] It is manifestly impossible to assert anything against it as it might be, *i. e.*, against a modification of Einstein's theory which Dr. Lunn may have vaguely in mind but which he does not specify.

[4] University of Wisconsin, where the subject was brought up by the present writer on March 30 in a paper entitled "The rotating earth as a reference system for light propagation," its conclusions being fully adhered to by Prof. H. A. Lorentz.

speak of the rotation, say, of a perpetually clouded earth, without reference to some assignable framework, such as that of the fixed stars. (See, *e.g.*, p. 141 of Poincare's book, French edition.) But is it really necessary to say any more in justification of a few words of warning inserted in my paper against a possible misconception?

Secondly, in saying that the relativity theory proved unable to deduce the terrestrial *ds* as a gravitational effect, my intention was not to emphasise it as a "flaw in that theory," as Dr. Lunn thinks, but simply to refer to it as a matter of fact. I may be permitted perhaps to mention that, though not a fanatical relativist, I am the last man to be blind to the boldness and beauty of Einstein's theory, and certainly not hostile or prejudiced against it. As to my calling Thirring's solution, in this connection, a "complete failure" (p. 304), though, as I added, mathematically interesting, I do not share Dr. Lunn's impression that I have been unjust to Thirring. It is true that his solution is the result of an avowedly approximate method only, and in view of this one would certainly have to be lenient to some numerical discrepancies. But if these go so far as to yield for the numerical factor of the Coriolis force, as compared with that of the centrifugal one, the value *eight* instead of *two*, and more recently even *ten* instead of *two*,[5] the solution is no more an approximation but simply a misrepresentation of the experimental facts, even if (as I did) one closes his eyes to the superfluous longitudinal force twice as large as the transversal or proper centrifugal force. And the failure seems "complete" indeed when one remembers how simply the correct formula for those experimental facts follows on the classical kinematics. Thirring's hollow spherical shell, rotating around our planet, is certainly not known to represent anything approaching the actual distribution of celestial matter. Yet it seems very doubtful whether anything short of a homogeneous distribution of matter throughout the whole space, which, moreover, has to

[5] In Thirring's original paper of 1918 the factors of the two "forces" (accelerations) were 8/3 and 1/3, but after the amendment of an arithmetical error (Phys. Zeitschrift *22*, p. 29, 1921,) they turned out to be 8/3 and 4/15, bearing to each other the ratio 10 instead of 2.

be assumed to be closed (elliptic), can essentially improve that solution. As I gather from a conversation with Einstein, this would, in his opinion, be the only possible way out. Now, although there are no serious objections to a finite, closed space, as first proposed in his 'Cosmological Contemplations' of 1917, the assumption of a homogeneous distribution of matter throughout the universe is very hard to adhere to. In fact, although Einstein's sensational formula, total mass of the universe equal to $\frac{\pi c^2}{4}$ times the curvature radius of space,[6] seems to be compatible with as small an average density as we like, yet the required homogeneity of its distribution could hold only on such a gigantically macroscopic scale for which the 'volume-element' would be a cube whose sides are ½ to 10 million light years long, this being the order of the mutual distances of Shapley's island universes. Now, such a coarse homogeneity would suffice for the purpose in hand only if the number of those "island universes" themselves would still be enormous, which—for the present at least—is entirely beyond our knowledge. In fine, while Dr. Lunn sees here but a passing difficulty, which he compares with the (Newtonian) retouching of the errors of some early results of celestial mechanics, the present writer is impressed by the gravitational aspect of rotation as a very hard and perhaps unsolvable problem.

Thirdly, concerning the field of competency of special relativity, I must insist most decidedly upon what was said on page 302 of my paper. It is admitted on all hands that Einstein's older or restricted relativity theory, though it can and does consider any non-uniform motions of a particle within any of its privileged, *i.e.* inertial systems, yet does not as a matter of fact deal with any frameworks other than the inertial ones as *reference-systems*, nor has it ever proposed to deal with them. In fact, not a single one of the host of papers, pamphlets and text-books written on that subject deals with any but the inertial reference frames and, correspondingly, with any but the Lorentz transformation as the bridge from one to another such system. So much so that the last

[6] See, for instance, the writer's *General Relativity and Gravitation*, Univ. of Toronto Press, p. 134, 1922.

edition (1919) of Laue's excellent book on special relativity has been entitled by him expressly the "Relativity principle of the Lorentz transformation." But it is not merely the sanction of the said restriction by general usage that supports my thesis. Einstein's older theory is by its very structure the geometry of a metrical four-fold determined by the line-element $ds^2=c^2dt^2-dx^2-dy^2-dz^2$, and has, in harmony with this, all of its material (four-vectors, six-vectors, *lor* and other derived operators) defined in relation to the Lorentz transformations. The latter form a group, and the whole field of this group is exhausted by the privileged class of inertial systems and *vice versa*, leaving no place for other reference systems.

Next, concerning the rule of convexity of light rays, "clockwise" in the footnote on page 295 is a manifest misprint for "anticlockwise," as Dr. Lunn could readily see from Fig. 2 and Fig. 4 where the arrows indicating the rotation are drawn in the correct sense.[7]

Further, with regard to the diagram on page 300 illustrative of the optical circuit, the three pairs of (curved) rays were drawn only for the sake of simplicity between the same points A, B, C, but they need not be taken as splits of originally the same ray arriving from the collimator. As agreed upon in a conversation with Prof. Michelson, the ultimate interpretation of his pending experimental results will have to be based upon a careful tracing of rays or waves through the whole apparatus with due attention, of course, to the finite breadth of the light beam. But it will be time to undertake such a tracing, laborious though offering no essential difficulties, when the effect will, probably next summer, be measured by Michelson. For a first orientation the said diagram has seemed most appropriate, especially as it brings out the essential compensation of the effects of ray curvature upon the ultimate phase difference or shift effect.

Finally, that r on page 303 stands for a cylindrical coordinate, and that this also is referred to in the footnote on p. 306, is really too obvious to call for so many words.

[7] Another shocking misprint, not noticed by Dr. Lunn, occurred on p. 292, where 10^{-5} in the value of $\bar{\omega}/c$ should read 10^{-15}. A few other misprints, attributable to a sudden change of the Press at that epoch, are too obvious to need a special mention.

It may still be mentioned, in reply to Dr. Lunn's last paragraph of p. 117, that speaking of the possibilities of a revived aether in the case of a fractional shift effect I had in mind a *non-rigid* aether, as will appear most clearly from page 292, where it is said that the spinning drag of the aether may vary from point to point.

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