

Letters to the Editor.

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The Mathematical Theory of a New Relativity.

IN the September number of *Current Science*¹ some criticisms by "B. S. M." of my Theory have been published. I heartily welcome such criticisms because they give me an opportunity to elucidate the points which may not be clear. I am briefly replying to the criticisms in the various paragraphs seriatim, reserving a more detailed examination for a later occasion.

§ 1.

1. I have never claimed that my Theory deserves Harlow Shapley's characterisation of it "as one of the high lights of astronomy," and I myself was surprised at the generosity of his remarks. It is a mere presentation of a new Theory still in its infancy which, applying Newtonian principles, with necessary corrections, claims to deduce the formulæ which are the pride of Relativity. It is not for me to say anything as regards the insinuation that Shapley's reference was not based on a critical study of the article.

D. R. Hamilton's criticism that my of the advance of perihelion to "absurdly erroneous" results

has been met by me in Chapter VI of my paper.²

3. "On the mathematical side not much notice has been taken."—The critic refers to the mention of my article by the *Zentralblatt für Math.*, and omits to refer to the *American Mathematical Monthly*.³ He has not also referred to *Nature*.⁴

§ 2.

4. His objection to the choice of the name "A New Relativity," needs no notice. I am certainly "vehemently opposed" to the extraordinary assumptions of Einstein's Relativity, but in no way to a "principle of Relativity in any form whatsoever". I maintain that Einstein's postulates are only approximately and not exactly true.

5. I gladly welcome the admission that "Relativity is not the only example of a physical theory which appears absurd when its logical consequences are pushed to their very limit." The very fact that the Principle of Relativity (special as well as general) has been unable to make any substantial progress from any other hand except that of its creator, and even not much from him since 1916, shows that it is not the last word on the problems which it claims to solve.

6. The quotation from Jeans' *Mysterious Universe* at page 228 is not objected to, and the page of the book also was correctly given. But the critic has pounced upon a misprint and expressed his curiosity at finding this mistake in the list of references at the end. Unfortunately there were no less than 13 misprints in the first paper already pointed out in the *Errata*, and there are more misprints in the second paper other than the two mentioned by the critic, which have been corrected in the second *Errata*.

7. "There are some misstatements of fact in the author's references to Relativity the most serious of which are in connection with the observational verifications of the general theory of Relativity."—I shall deal with the subject at considerable length in Chapters X, XI and XII of my next paper, which will show that the claim of Einstein's General Relativity that it has been verified is unfounded.

8. He considers that Chazy's observational value for the advance in the longitude of perihelion of Mercury is "the best observational data".—I suppose it is a great pity that although Chazy's value was published in 1926,⁵ it was overlooked by Eddington in his later works. The critic is obviously unaware of the more recent literature to be discussed in Chapter X, which will also show the great uncertainty due to certain assumptions in the calculation of the advance of the perihelion. So much so that von Gleich has actually asserted that the supposed excess of motion of the perihelion of Mercury does not exist and the Newtonian theory of gravitation needs no correction by Relativity.⁶

9. He considers that "the most satisfactory data (on the deflection of light) available at present" are those of Campbel and Trumpler.—This is relying on a rather stale literature of 1923 in ignorance of the bad exposure made by Freundlich (quoted in Chapter II, p. 25) that if necessary corrections are introduced, the three cameras of the American Expeditions of 1922 give the values $2''\cdot20$, $2''\cdot30$ and $2''\cdot10$ respectively.⁷ Freundlich has discussed in detail all the previous works including those of Campbel and Trumpler and shown that the value obtained by the latter was due to an inaccuracy in balancing the observations and that their value of the deflection also is $2''\cdot27$. It will perhaps shock the critic to learn that Lanczos, a great worker on Einstein's Relativity, has also pointed out that with

the necessary corrections Campbel and Trumpler's value comes to $2''\cdot20$.⁸ Of the three tests, the deflection of light is the only one that is reliable, and in this Relativity has already failed.

10. (i) The later observations of St. John⁹ as regards the shift of spectral lines of the Sun are supposed to be "quite satisfactory".

It will be shown in Chapter XII that the agreements are found only for different arbitrarily chosen levels for the centre and the edge, there being marked positive and negative residuals for higher and lower levels. The discrepancies range between ± 25 per cent. which can hardly be called "quite satisfactory". The discrepancy at the edge is conveniently explained by some unknown "edge effect". The discrepancy is greater when the average of $M\Sigma \left(\frac{ad\lambda}{\lambda}\right)$ is taken, which alone is the correct method. It is well known to every worker on solar physics that wave-lengths on the Sun may differ from those measured on territorial sources on account of a large number of causes.

11. As to the companion of Sirius the assumption of the "quite satisfactory" result is based on Adam's observation.¹⁰ But Vysotshiy¹¹ has shown that the density of the companion of Sirius is much less than what has been assumed so far and therefore the radius much larger. If Einstein's value is correct, then so is mine!

The following are stated to be "misstatements of a minor nature":—

12. (1) "Relativistic invariance holds in vacuum only".—Schwarschild's particular solution is of $G_{\mu\nu} = 0$. As to this Eddington says, "This is the state of the world in an empty region—not containing matter, light or electromagnetic fields, but in the neighbourhood of these forms of energy."¹²

13. "Einstein arbitrarily assumes $C + v = c$ and $c - v = c$."—In relativity the velocity of light is absolute and relative to every frame of reference is the same. Even if two systems S and S' be moving with relative velocity v , "the resultant velocity relative to S' is c , the velocity relative to S is also c , whatever the direction".¹³

14. "Milne's theory ignores gravitation and evades collisions."—Compare "The new theory shows that the expansion is in reality a natural phenomenon taking place in the absence of gravitation and proceeding in

spite of it" (p. 2) and "consider a swarm of particles moving in straight lines, each with a uniform velocity without collisions or other interactions possessing an entirely arbitrary velocity-distribution" (pp. 6-7).¹⁴

15. "In Relativity time is wholly imaginary and space illusory."—The unit of time is really $\sqrt{-1}$, and space has four dimensions. What more imaginary and illusive character is wanted than the confusion of space and time?

16. (1) The mathematical part of the work is "quite elementary and does not go beyond the solution of an ordinary differential equation of the second order".—No reference to any book or paper is supplied where even an approximate solution of my equation (5.9) was given before, much less the exact solution. The so-called elementary solution is by successive approximations covering two pages and a half.

17. "Looking from an æsthetic-mathematical point of view one searches here in vain for such concepts like (as) groups, tensors and generalised spaces characteristic of Relativity or functional equations, sets of points, and Finsler spaces relevant to Milne's new Relativity."—The author thinks that paper which does not use Tensors, Groups and other branches of higher mathematics is not a valuable paper at all. Such a suggestion will condemn workers like Maxwell, Bohr, Sommerfeld and many other illustrious investigators, both dead and living. In fact the claim of the New Relativity is that its formulæ are deduced on simple dynamical principles without recourse to such methods. It is not easy to appreciate what objection there can be to its simplicity.

18. Some differential equations are considered to be "not relevant".—Apparently the critic thinks that the complete theory is contained in the five Chapters already published. More have come out since.

19. (iv) The superiority of method over those of Forsyth, Morley and Pierpoint is said to have been "achieved at the cost of a little wrong mathematics". It is conceded that the solution (9.5) is "correctly obtained" but it is asserted that the next solution has been obtained wrongly. The critic says, "It is, however, absurd to use this theorem here since it cannot apply to non-linear differential equations, and moreover is not a particular integral."—It was pointed on pages 10 and 11 that for Mercury

$\frac{\mu}{h^2} = 1.73 + 10^{-13}$ and u also is nearly the same, and that k was of the order 10^{-4} . Also θ varies from 0 to 2π . It was accordingly considered too obvious to mention that the second power of $\frac{\mu}{h^2} (1 - 2k\theta)$ as well as the product $\frac{\mu}{h^2} (1 - 2k\theta) \cdot u$ were negligible.

If this obvious fact were borne in mind, it will be found that the general solution given by me is perfectly valid. The correctness of the solution can be easily verified by direct substitution in the differential equation. It is some consolation that the solution in elliptic functions is not called "quite elementary". The critic has not noticed the misprint $\frac{D^4}{\mu^3}$ for $\frac{D^6}{\mu^3}$.

The following observations are regarded as contrary to scientific investigations:—

20. "Nature's limits are not fixed by our capacity to observe them."—Does Nature exist only when seen by man?

21. "Relative velocity cannot mean relative velocity as actually observed, and we cannot go by measurements only."—This is a rather bad paraphrase of my remark "if relative velocity only means relative velocity as actually observed and we go exclusively by measurements only, then the relative velocity between two bodies would depend on the particular method of measurement which is chosen". This remark was in anticipation of Chapter VII, which will throw fuller light on the point.

22. "A certain concept in Relativity is unacceptable because the concept is philosophically an impossible one."—The concept referred to is, in physical language, that there exists a mysterious cosmic force of repulsion, which not only acts at a distance, but increases in intensity as the distance between the two bodies increases.

I am not the first critic to find fault with his conception. Even Einstein has been aware of the weakness of his argument and has been trying to change the assumption. In fact in a paper published in the *Proc. Nat. Acad. of Sciences* jointly by Einstein and de Sitter, λ was taken to be zero. In *Cosmos*, de Sitter concluded that the theory of the expanding Universe is less definite than it used to be and that we do not even know whether the curvature is positive, zero or negative, whether the universe is finite or infinite.

23. A whole paragraph is devoted to commenting on a reference to Jordan's paper in a short note at the end. It has been repeatedly emphasised over and over again in my papers that the existence of gravitons is not necessary for the Mathematical Theory.

§ 3. GENERAL RELATIVITY.

(a) *Advance of Perihelion.*

24. The critic says that there is "no novelty of ideas" as they go back to the work of Laplace, whose theory produced secular perturbations or, in the alternative, a high velocity of gravitation.—Obviously the critic, who has quoted profusely from the *Encyclopaedia*, is not aware that Lorentz and Eddington have shown that even on Laplace's own theory perturbations could vanish to the first order terms (see Chapter VII). It has also not been mentioned by the critic that Laplace's theory was based on the idea of pressure caused by a liquid ether owing to which the velocity of propagation was directed from the attracted body towards the attracting body, whereas in my theory, the gravitational influence is propagated equally in all directions outward from the influencing body towards the influenced body, the change of sign making all the difference.

25. It is irrelevant to refer to other theories including Anding's which assumed a new law of gravitation quite *arbitrarily*, and yet did not succeed in getting any satisfactory results. It is one thing to assume arbitrarily a new law of gravitation, and it is quite another thing to deduce a new law from a single assumption of the finiteness of the velocity of propagation. I may mention that if one were to assume a new law of gravitation, I would take it to be $-\frac{\mu}{r^2} - \frac{3\mu h^2}{D^2} \cdot \frac{1}{r^4}$ which will give all the results we want and avoid all difficulties (see Chapter VII).

26. "Remembering that Sulaiman's correction factor does not depend on r , the criticisms levelled against Gerber's theory therefore apply to Sulaiman's theory equally well."—Gerber's theory (referred to by me in Chapter I, p. 4) involved the propagation of potential with finite velocity which Seeliger, Laue and Oppenheim rejected as being impossible. Gerber had also misapplied the Lagrangian equation as pointed out by Pauli. Neither of these defects can be found in my theory. Also my factor is a function

of dr/dt which is a function of r , and is therefore not derivable from Gerber's potential.

27. Following in the footsteps of D. R. Hamilton the critic has emphasised at several places that my theory would lead to unforeseen perturbations; and either Mercury would go off in three centuries or the velocity of gravitation should be thousands of times more than that of light.—My replies to Hamilton's criticisms are to be found in Chapter VI, which may be referred to. As Laplace's theory led to a retardation, he was unable to explain the perturbation. My theory leads to an acceleration of motion, which can be adequately explained by the resistance of the medium through which planets pass, as is now becoming clearer from the explanations of Zodiacal light and Gegenschein phenomena. This material difference saves my theory from foundering on the rock on which Laplace's theory did.

28. It is wholly unnecessary for me to discuss the other theories relating to the advance of Mercury's perihelion. The principal defect in Einstein's theory is the unconvincing nature of its fundamental assumption of the absoluteness of the velocity of light.

(b) *Gravitational Deflection of Light.*

29. "Gerber's equation does not yield the value for the deflection which is certainly true, and the same should also be true of Sulaiman's equations if properly handled."—The critic has undertaken to rehandle in two ways my equations which, as shown in Chapter VII, was obtained for heavenly bodies where v'/D is small.

30. (i) He does "the straightforward thing...to write the equations of motion *ab initio* using the relation $v = D$ " and curiously gets $\frac{d^2u}{d\theta^2} + u = 0$. He has apparently forgotten that, as noted on p. 8, v is the velocity along S'P = $\frac{dr}{dt} \cos \alpha - \frac{rd\theta}{dt} \sin \alpha$ and not along the tangent.

31. (ii) He has verified his result "qualitatively" by putting $v = D$ in $-\frac{\mu}{r^2} \left(1 - \frac{v}{D}\right)^3$. [The critic has been freely using such expressions as "absurdly erroneous", "completely false impression", "most serious misstatement", "quite elementary", "drab", "absurd to use this", "results completely against all observed values", "arbitrary", "truly amazing".] But I would refrain from using any epithet for the

critic's mathematics according to which for a light particle no matter howsoever much inclined it may be to the radius vector the form always is $-\frac{\mu}{r^2} \left(1 - \frac{c}{D}\right)^3$. This is the supposed "consonance with Gerber's equation"!

32. "When the (author) states that $r \frac{d\theta}{dt}$ can never exceed the *tangential velocity* c , he assumes unconsciously that the tangential velocity is constant, but this certainly cannot be true."—There has never been any such assumption which would be contrary to my theory. I said on page 29, "it disproves the assumption in Relativity that the velocity of light in a gravitational field remains constant". But undoubtedly the change in the velocity c is extremely small.

33. "The assumption of a constant tangential velocity is equivalent to taking the central orbit as circular, and it becomes meaningless to talk of the deflection as the angle between the asymptotes of the orbit."—With the disappearance of the constancy of the velocity, the "meaninglessness" vanishes and so does the circular orbit.

34. The "third mistake" is said to be that "by showing that the least value of the expression is...the conclusion is drawn that the deflection is exactly $4/3$ times the Einstein value".—It is not clear where the word "exactly" has been got from. He has apparently misread the word "nearly". On the equation, that was the minimum value only, and the critic has himself quoted my words "at least".

35. "One would naturally inquire what would be the maximum deflection possible, but the work is silent on this point."—It is shown in Chapter VII, section 5, that on that equation the maximum deflection would be $3/2$ times Einstein's value. But it will be shown in Chapter XI that in the case of light the equation is only approximately true and not exactly so, thus giving a smaller value for the deflection.

(c) *The Shift of Spectral Lines.*

36. "No remarks appear to be necessary in this case, for according to the author's own showing the corrections provided by the New Relativity are not appreciably large and the value of the ratio is the same as Einstein's."—Apparently the critic considers that there is no credit whatsoever in obtaining on purely Newtonian principles the same value for the spectral shift, as is

obtained in Relativity with the help of extraordinary assumptions and cumbersome mathematical apparatus, and by treating an electron at the Sun as being "momentarily at rest", a self-contradictory assumption.

37. The value of the shift was the same as Einstein's for light from the centre of the disc.—In the Appendix to Chapter II it is shown that as regards the shift from the limb, there is an appreciable difference in the two values. In Chapter XII (see No. 10 above) St. John's average values will be examined.

38. "No one would seriously think of adopting it as an alternative to the General Theory of Relativity for the explanation of gravitational phenomena."—I can never expect that my theory will be accepted until more accurate future observations are forthcoming to disprove Relativity still more thoroughly.

§ 4. SPECIAL RELATIVITY.

(a) *Relative Velocity.*

39. (1) As the Theory is a new one, it is not at all surprising that critics should feel some difficulty in the early stages of its development. In Chapter VII it has been explained that relative velocity must vary with the method of observation. The whole object of obtaining the formula for the compounding of velocities was to show that *if a messenger's double journey method be adopted*, a formula can be obtained which as an approximation would reduce to a form similar to Einstein's. Einstein has assumed his formula to be rigorously true, from which unconvincing results follow in the extreme case. If his formula were taken to be approximately true only so long as the velocities of the bodies are small compared to that of the messenger, I would have no quarrel with it. Indeed, all the results of Relativity can be incorporated in the new Theory within the limits of such an approximation.

40. "Where u and v' are velocities relative to an observer who is at rest in his own system."—In absolute space, "rest in his own system" is not the same thing as absolute rest. *If the messenger's double journey method be employed*, the relative velocity as measured must necessarily be slightly different from Newton's relative velocity.

41. "As an example of confused thinking it is hard to find anywhere in relativistic literature a parallel to the author's derivation of the equation."—Failing to grasp the essence of the theory, the critic has become

angry and uses strong language. Of course, what is not grasped must necessarily appear to be confusing.

42. "An absolute distance between two moving points is assumed as r independent of all measurement."—For Newton's absolute space and time, such an assumption is perfectly sound. My point of view is Newtonian, and the statement is in strict logical agreement with the whole trend of such thought.

43. (1) The critic "deduces an absurd consequence" by putting $v' = D = c$.—Apparently he seriously thinks that for a double journey method also $v' = c$, as if a messenger travelling with velocity c can overtake another body travelling away with the same velocity, and thereafter return to his source! But if $D > v'$, then on this double journey method the value for a receding body tends towards the limit $v = c - \frac{u}{2}$ which is simple dynamics. The notion of "hybrid form" is the outcome of ignoring that the method of ascertainment is a method of the double journey of the messenger.

44. (ii) Fresnel's Formula.

The relative velocity formula according to the double journey method is not only a function of the difference between two absolute velocities, but also a function of the individual absolute velocities themselves. It was only as an approximation that it was applied to Fresnel's experiment. For a higher approximation the formula derived on the messenger's double journey method would be inappropriate for Fresnel's experiment, because there the two parts of the beam though travelling in opposite directions relative to the moving water perform only one and not a double journey.

This clears up the discrepancy. The object of the new theory is to explain phenomena by applying well-known rational principles without the postulates of Relativity. So far as Fresnel's water experiment is concerned, he had himself explained it on a simple physical principle of a change in the velocity of light.

45. The transformation formulæ on pages 247-48 have been summarily dismissed as "ridiculous analogues".—They merely indicated a different method of arriving at nearly the same formula, and have not yet been actually applied. The significance of the difference will appear from Chapter VII.

(b) The Principle of Aberration.

46. "It is difficult to see any justification for the reduction in the intensity of force along its apparent direction. It really makes no sense to say that when the velocity of flow is D , the effective component of force observed along the apparent direction is $D \cos \alpha$."—It makes no sense because the word "apparent" which did not occur in the text (p. 250) has been interpolated. The dynamics of a force moving with a finite velocity is unique, and difficulty of its conception is great. As a matter of fact, it will be seen from Chapters I and VIII that the equations have been obtained on the hypothesis that there is no change in the magnitude along the resultant; and the consideration of a change in magnitude, as pointed out in Chapter VIII, has been postponed.

47. "There is an utter confusion here between velocity and force. This confusion is also responsible for the meaningless phrase 'the velocity of light ON a body moving with velocity v '."—The critic has made a capital out of the misprint in which the word "falling" was unfortunately left out after the word "light".

48. "The claim of universality is belied by assuming that in the case of light the velocity is reduced while in other cases the intensity of force is changed (for example, H in the explanation of Bucherer's experiment)."—The effective magnitude in both of these is changed because it is only their component that is effective.

49. "There is yet another inconsistency The universality claimed would certainly require Newton's law of attraction to be $-\frac{\mu}{r^2} : \left(1 + \frac{v^2}{D^2}\right)^{\frac{1}{2}}$ leading on to Gerber's equations, but the author uses, instead, the factor $\left(1 - \frac{v}{D}\right)^3$."—A reference may again be made to the Appendix (IV, pp. 259-60) which shows how the factor is derived. A further reference to Chapter VII, section 5, is invited.

50. The critic says that "it is impossible to see how $c_1 = c \cos \alpha$ and $\tan \alpha = v/c$ can be both simultaneously true where c_1 , the apparent velocity of light, has its direction perpendicular to that of v , and α is the angle of aberration".—If the critic means that $c_1 = c \cos \left(\sin^{-1} \frac{v}{c}\right)$ and $c_1 = c \cos \left(\tan^{-1} \frac{v}{c}\right)$ cannot both be simultaneously true exactly, it is of course so. When v is small, the

first approximation gives $c_1 = c \sqrt{1 - \frac{v^2}{c^2}}$ which can be transformed into Minkowsky's equation, in which v cannot be greater than c .

But when v is large, $\tan \alpha = v/c$ is the more accurate formula and then the second approximation gives $c_1 = c \cos \left(\tan^{-1} \frac{v}{c} \right)$ which yields

$$c_1 = \frac{c}{\sqrt{1 + \frac{v^2}{c^2}}}$$

for which v can be even greater than c , even up to ∞ .

51. "It is on the basis of such 'flawless' mathematics that the possibility of velocities up to ∞ is deduced, and one might well suggest to the author the derivation of his first universal principle when one of the bodies is moving with such a velocity, for example, the velocity greater than that of the messenger employed."—Apparently the critic labours under the impression that even when the velocity of recession is greater than that of the messenger the double journey method would hold good. Comment on this is unnecessary, because there can be no double journey in such a case.

(c) *Michelson and Morley Experiment.*

52. "By using the author's own universal principles it can easily be shown that this explanation is untenable."—The critic says that the formula should be replaced by

$$\frac{l}{c - \frac{v}{2}} + \frac{l}{c + \frac{v}{2}} = \frac{2lc}{c^2 - \frac{v^2}{4}}$$

(1) This is an obvious misapplication of the formula obtained by the double journey method to two single journeys of light. If the times for the two journeys to and fro are to be calculated separately, the double journey formula cannot possibly be applicable.

(2) If a double journey formula is insisted upon then the whole double journey should be taken as one journey and the Corollary (IV, p. 247) which gives the ratio of the apparent and real velocities should be applied, giving instead of the apparent time $\frac{2l}{c}$ the real time $\frac{2l}{c \left(1 - \frac{v^2}{c^2} \right)}$.

53. The critic says that the second universal principle has been applied wrongly and that after the direction of flow has been once shifted by an angle α and the velocity changed from c to c_1 , "it is therefore wrong

to again compound c_1 with v , and hence the time should be replaced by $\frac{2l}{c_1}$."—In reality there is no second compounding at all. When falling on the moving surface the light is shifted forward, and its component along AB' which alone will reach

$$A'' = c \sqrt{1 - \frac{v^2}{c^2}}$$

and the time is given by

$$c_1^2 t^2 = l^2 + v^2 t^2 \text{ or } 2t = \frac{2l}{\sqrt{c_1^2 - v^2}}$$

In the New Relativity the reflection of light from a moving mirror is different from that from a stationary mirror. The effective incident ray is deflected forward and the effective reflected ray deflected backward by the same angle; but the ray which reaches A'' is the component of the deflected ray along AB' .

54. According to the critic the difference in times should be $-\frac{l}{c} \cdot \frac{v^2}{2c^2}$ which he says "can certainly be measured, but is contradicted by the null result of the Michelson and Morley experiment".—It has been shown above that his deduction of this difference is wrong.

(d) *"Fine Structure of Spectral Lines."*

55. There was no attempt made to give any quantitative explanation, but only an indication was given in anticipation of what was to follow. The critic has wasted unnecessary words on what he naturally regards as "really a complete mystery". It must be so, until the method is announced.

5. COSMOLOGY.

56. "The only positive result is the derivation of Hubble's famous velocity-distance law."—Apparently the validity of the derivation of the equation is not challenged.

57. "This cosmological principle can be considered as a particular form of Milne's principle of equivalent observer's."—This is perhaps some admission that the principle is sound even from the relativist's point of view, though it is a particular case. In fact my principle of the constancy of the ratio of the acceleration to velocity is quite different from Milne's principle of "equivalent observers".

58. "It is quite redundant for the purposes of deriving Hubble's law to invoke the aid of an emission theory of matter which calls to aid supernatural agencies for the