

CONTINUED DISCUSSION OF THE ASTRONOMICAL  
AND GRAVITATIONAL BEARINGS OF THE  
ELECTRICAL THEORY OF MATTER.

BY

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## PART I.

MY short summary in the December number of the Phil. Mag., page 519, put prominently forward the idea that the expected effect required that the additional inertia due to motion should be independent of gravitative influence; for the conclusion seemed obvious that if weight and mass varied together there would be no change in acceleration, and that in that case it did not matter how much the mass of a revolving body varied. But I soon perceived that this was only attending to the transverse acceleration and neglecting the longitudinal, which is taken into account in Professor Eddington's completer theory in the October number of the Phil. Mag., page 322. He there re-determines the fundamental equation of particle dynamics, with momentum a function of speed, and shows that not the ratio  $F/m$ , but the product  $Fm$ , enters into the absolute term of that equation, so that it becomes  $\frac{d^2u}{d\theta^2} + u = \frac{F/m_0}{h^2u^2} \cdot \frac{m}{m_0}$ .

I take up the thread again here, and point out that that being so, the unexpected result follows, that if the additional inertia is acted on by gravity, in accordance with the ordinary

Newtonian law  $F = \gamma mm'/r^2$ , the varying factor  $m$  will enter twice into the equation of motion, and the whole perturbation will be increased instead of being annihilated. In other words, if the gravitative pull on the planet increases in the same way as the inertia increases, the effect is not to cancel, but to double the perturbing effect. On the other hand, if the extra inertia is not affected by gravity the perturbing effect is as already calculated. Consequently from this point of view some perturbation seems inevitable;—either the value reckoned by me in August, with the extra inertia independent of gravity, or else double that value, if the extra inertia is fully subject to the Newtonian law of attraction.

The question arises therefore, rather pressingly, how much dependence can be placed on the theory? It will be granted I think that the fact that a correct value for Mercurial apsidal progress can be deduced from the electrical theory of matter by a reasonable assumption of solar drift is not a negligible fact. For if the theory were completely inapplicable the value of drift required might have been of an altogether unreasonable order of magnitude. The fact that the same drift gave a Martial apsidal progress also of the right magnitude (see August Phil. Mag. pp. 91 & 92) seemed to me at the time very confirmatory. But I admit that the changes in excentricity are not thus accounted for correctly, and that the calculated perturbations for Earth and Venus exceed any probable value for those planets.

I perceived in my August paper that a difficulty of this kind would arise, but thought that it might be got over by choosing a particular longitude for the projected component of the solar drift which should almost nullify the result for those two planets; and so I chose the longitude  $294^\circ$  as being half-way between the perihelia of Mercury and Mars, and inclined to their major axes at a reasonable angle, while at the same time it happens to be practically half-way between the perihelia of Earth and Venus, though inclined to their major axes at a much smaller angle, and so being less effective. I hoped therefore that it might be possible to contrive to get rid of the calculated too great progress of perihelion for Earth and Venus, especially as the roundness of their orbits must make the exact position of perihelion difficult to determine.

Professor Eddington, however, countered all this contrivance, in September, by working out the theoretical changes of excentricity more thoroughly than I had done, and showed that it was highly improbable that the calculated perturbations could be admitted for some and evaded for