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ORDINARY MEETING, JUNE 21st, 1869.

THE REV. WALTER MITCHELL, M.A., VICE-PRESIDENT, IN THE CHAIR.

The Minutes of last meeting were read and approved, and the election of the following Members announced :—

MEMBERS :—Rev. James M'Cann, D.D., Glasgow ; Rev. E. E. Jenkins, M.A., Brixton ; T. W. Masterman, Esq., Hare Hatch, near Twyford.

Also the presentation of the following books :—

Ancient Pillar Stones of Scotland. By Dr. George Moore. *From the Author.*
Anti-Secularist Lectures. By Rev. Dr. M'Cann. *From the Author.*
Proceedings of Royal Instit. of Gr. Brit. No. 49. *From the Council, R. I.*

The following Paper was then read by the Author :—

ON CURRENT PHYSICAL ASTRONOMY. By JAMES REDDIE, Esq., *Hon. Mem. Dial. Soc., Edin. Univer.*, HONORARY SECRETARY, *Vict. Inst.*

ALTHOUGH the subject of this paper is so extensive, that I can ill spare any of the time at my disposal for its consideration, I must still, on the present occasion, beg leave to occupy a few minutes with some introductory words of apology and explanation. I need scarcely say, considering my connection with the VICTORIA INSTITUTE, that I am the last person to think it necessary that any apology should be required, as a rule, for examining with the utmost freedom, whether to confirm or confute, any scientific theory or dogma whatever which may have obtained currency among us. But a dear-bought and bitter experience has taught me, that to this rule of freedom there is one exception at least; for I know that the great hypothesis and subordinate theories I am about to examine and handle quite freely, are regarded almost as sacred and as standing on holy ground, and that I approach them at my peril,—well aware that there is an almost universal consent of prejudice against me, which is supposed to be fully justified by the certainty of scientific and mathematical demonstration, confirmed by subsequent experience and what Bacon calls " the decision of time."

2. For more now than a quarter of a century, I have

known practically what it is to be "a scientific heretic," and how it fares with any who will venture to throw doubt upon the truth of Universal Gravitation, or to question what is supposed to be proved in the *Principia*. So far back as 1842, when I had gone but partially into the whole subject, and knew not what scientific prejudice was; when I fancied that all men of science were lovers of truth, and all able to justify their beliefs; I ventured to send two brief papers to the Royal Societies of London and Edinburgh, containing objections to the demonstrations of the first and second propositions of the *Principia*. That sent to the London Society was never acknowledged; the other was returned from Edinburgh at the end of six months as "not suitable for being read before the Society," with a civil apology for the tardiness of this reply. After an interval of twenty years, early in 1862, I published a small book on the subject,* including those objections elaborated, along with many others, and with counter-demonstrations; and in the same year I ventured to submit a Paper† to Section A of the British Association for the Advancement of Science, when it met at Cambridge, which was "declined with thanks," because Newton's theory was attacked in it. In 1863, I sent another Paper to Section A of the British Association at Newcastle, which was not even acknowledged, and which I afterwards published.‡ And lastly, the following year I tried in vain at Bath to obtain a hearing before the British Association for another Paper, which has remained till now in MS., and which was for some time in the hands of our Vice-President, Mr. Mitchell. The Cambridge paper alone was directly an attack upon the reasoning in the first section, and the demonstration of the first proposition of the second section, of the *Principia*; the Newcastle paper was chiefly an exposure of the astronomical contradictions arising from the first Herschel's theory of solar motion in space, a conception with which neither Copernicus nor Newton had anything whatever to do; and the Bath paper was on the motion of the moon, to show that its actual path and the physical laws that must regulate its motion, according to the Copernico-Newtonian hypothesis, are totally different from the hypothetical suppositions employed in solving the famous mathematical problem of the three bodies, in which—strange as it may appear—not only is the sun, but also the earth, regarded as at rest, with the moon revolving round it in an

* *Vis Inertiæ Victa; or, Fallacies affecting Science.* (Lond., Hardwicke.)

† Afterwards published with the title—*The Mechanics of the Heavens.*

‡ With the title, *Victoria Toto Cœlo; or, Modern Astronomy Recast.*

ellipse at the comparatively slow rate of about 2,000 miles an hour, instead of with a velocity of upwards of 65,000, as it flies along with the earth, describing a wave line upon *its* orbit.

3. Allow me only further to premise that some of the scientific critics of the press have professed to join issue with me upon this subject. Mr. Augustus De Morgan, late Professor of Mathematics in University College, London, in the *Athenæum*, has magnanimously misrepresented and attempted to ridicule me more than once. Mr. Balfour Stewart, anonymously, in the *Edinburgh Philosophical Journal* (which shortly afterwards became defunct), was quite as successful in misrepresenting me, and nearly as facetious, as Professor De Morgan himself. While in the late journal, *The Parthenon*, when edited by Mr. C. W. Goodwin, the author of the essay on "The Mosaic Cosmogony" in *Essays and Reviews*, there is a kind of acknowledgment that my objections to Newton's demonstrations were valid; for it is mildly observed that "there appears to be a class of writers who imagine that, if they can point out a difficulty in Newton's demonstrations, they have struck a heavy blow at universal gravitation." To which I replied, in my Cambridge Paper, "that it must depend upon the nature of the difficulties, and the demonstrations in which they are found, whether they deal a fatal blow to the theory or not; and that the proper course surely is manfully to face admitted difficulties, and clear them away, if possible, by showing that they do not, *if they do not*, affect demonstrations essential to the theory." * But the writer in *The Parthenon* almost deprecated such inquiry as unnecessary, and somewhat pathetically observed, that, "whatever uncertainty there may be with regard to some other sciences, we are usually taught to believe that the mechanics of the heavens are not uncertain."† We know, too, how implicitly Mr. Goodwin believed in the certainty of the nebular theory of the famous author of the *Mécanique Céleste*, with all its *quasi*-mathematical demonstrations, and how utterly the theory has perished. Strangely enough, a reverend Professor, who gave himself out as an adherent of that evaporated theory in the *Replies to Essays and Reviews*, wrote to one of the foundation members of this Institute, and made it a kind of objection to his not joining it, that the Honorary Secretary "actually did not believe in the theory of universal gravitation"! One of the smart writers, also, in the *Saturday Review* (which professes to be a "journal of science," though it has long since ceased to give anything like scientific articles), in noticing this Society's proceedings, has had his little joke about the author of *Vis Inertiæ Victa*. And I notice

* *Mech. of the Heavens*, § 24. † Ibid., § 16.

these things at the outset, that you may know that I am fully aware of the odium it is possible that even yet may be attempted to be cast upon me for daring to bring this subject before you.

4. I am deeply grateful, under these circumstances, that the Council of this Society has allowed me to read a Paper on this subject. Here, as every author that comes before us knows (and some have felt it very deeply), we are perfectly free and unsparing in our criticisms. But I court, and have always courted, the most unsparing criticism; and I may here repeat what I said in my paper written for the British Association at Cambridge—namely, that "throughout this paper I shall endeavour to use the plainest and most definite language —not arrogantly, but earnestly—and, as it were, to court refutation, if refutation of what is advanced be possible."

5. Only one word more of preface. Fortunately, as regards this subject, no *odium theologicum* need be evolved. Whatever it may once have been, astronomy has long been out of the category of sciences whose teaching is supposed to be contrary to Scripture. The piety of Newton himself, and of many of his most eminent followers, has served to give almost a religious character to his great theory, which is often even used in the pulpit to lead men's minds "from nature up to nature's God"; and, in point of fact, religious objections have actually been urged against my attempt to prove that the theory is untenable! At the same time I am bound to observe, as one who has watched philosophical opinions very narrowly for the last eight-and-twenty years, with my convictions as to this subject always in my mind, that I know of nothing besides in science which so completely buoys up the atheistic and infidel classes of thinkers and public writers, in their almost stolid worship of human science and pride in man's intellectual power, as the faith that they all have, and mostly without the least pretence even of personal knowledge, in the certainty of the demonstrations of the *Principia* of Newton, and of the *Mécanique Céleste* of Laplace. M. Comte has gone so far as to say, with a shocking impiety, that "the heavens declare *not* the glory of God, but of Copernicus, Newton, and Laplace"; while Mr. Darwin, and Professors Huxley and Tyndall, in their writings, though on very different subjects, all glance back, as to a kind of foundation upon which they can lean with confidence, to the astronomical theory which forms the basis of Newton's *Principia*.

6. Having thus cleared the ground, it may be a relief to many, though it may startle most of my hearers, if I now, in the first place, observe that the "Current Physical Astronomy" of the day, as, for instance, we find it taught in the Astronomer

Royal's Lectures, and as accepted in the Royal Astronomical Society, is not actually in accordance with anything that purports to be demonstrated in Newton's *Principia!* Let me not be misunderstood. I am not saying that our modern astronomers do not profess to believe in Kepler's laws and in Newton's theory and demonstrations. But I do say, that whatever they may vaguely and inconsistently profess, they do not hold Newton's conclusions, and that the conclusions he has professed to establish are not in accordance with what is now believed. And yet I am bound to add further, however paradoxical it may sound, that Newton is in a certain sense responsible for even what the moderns believe, though discordant with his professed demonstrations, and not in accordance with what either he himself or Copernicus or Kepler believed.

7. Let me now endeavour to reconcile and explain these apparently conflicting assertions. In the first place, we are all accustomed vaguely to speak of our believing in the truth of the Copernican system of astronomy as opposed to the Ptolemaic; but we do not literally believe what Copernicus taught, namely, that the sun is at rest in space, and that the orbits of the earth and planets round the sun are circular. Then, again, we still talk of believing in the truth of Kepler's laws of the elliptical orbits of the earth and planets round the sun; but Kepler, too, believed the sun to be at rest, though not in the precise centre of the planetary orbits. And yet we ought to remember that an ellipse as well as a circle is a curve that returns into itself, and that no ellipsis can possibly be described round a moving centre or focus that is travelling rapidly onwards in space; but this, according to Professor Airy, is now believed, as regards our sun, "by every astronomer who has examined the question carefully."*

8. Once more. Sir Isaac Newton, in the *Principia*, professes to establish upon a mathematical basis what Kepler taught were the motions of the heavenly bodies, superadding a physical cause or law to account for those motions after they have once been set agoing. That law, as is well known, was gravitation. The theory of universal gravitation (as I have already stated in this Institute) was previously propounded by Halley and Hook to the Royal Society of London, ten and twelve years before the *Principia* was published.† How the original conception of the theory came to be assigned to Newton, and the mythical story of his apple to be invented, I do not know;

* Airy's *Lects. on Astron.* (4th edit.), p. 173.

† Vide *Journ. of Trans. of Vict. Institute*, vol. i. pp. 413, 414; and *Phil. Trans.* there cited, vol. ii. pp. 126, 127, and 326. (Lond. 1809.)

and how honest persons, if well instructed, can repeat the story, I do not understand. I can only once more point to the printed *Transactions* of the Royal Society to prove that it is a myth. All that Newton had to do with the theory was to give it mathematical countenance, and (as is popularly believed) to demonstrate its truth. But even if Newton had proved —which I beg leave to deny—that gravitating bodies could revolve in circles or ellipses round their centres of attraction, he must surely have done so in vain, if the real motions of the planets are now held to be neither the one nor the other; and, if the sun moves onwards in space, it is simply impossible that they can be so. But Newton, also, like Kepler and Copernicus, held the sun to be at rest; the primary hypothesis of the Third Book of the *Principia* being, "That the centre of the system of the world is immovable."

9. Again, all the demonstrations in the *Principia* are based upon the supposition that the heavenly bodies are moving *in vacuo*, or "in spaces void of resistance"; whereas, at the first meeting of the British Association for the Advancement of Science, it will be found that Professor Airy, the present Astronomer Royal, in his *Report on Astronomy* (1832) stated that "the existence of a resisting medium has been once more established in this century by Encke." So here, again, modern astronomers do not believe what Newton taught in the *Principia*. I may observe, in passing, that when Newton wrote, the notion of a resisting medium, or of what was called a *plenum* throughout the universe, as formerly taught by Aristotle, was then in vogue, and was the foundation of Des Cartes' system of vortices; and there is a curious letter from Voltaire to a friend, written when he came to England to visit Newton, in which he says, in allusion to this change of theories from the *plenum* to a *vacuum*, (now again reversed in our day!) "I left the world *full* in Paris, but found it *empty* in London. In France the earth is believed to be shaped like a melon [referring to the lemon-shaped water-melon, no doubt], but here it is flat like an orange."

10. So, then, if there be really solar motion in space, and if there be a resisting medium, through which all the heavenly bodies must move, there is not a single demonstration in the *Principia*, whether sound or fallacious, which is in accordance with our "Current Physical Astronomy"; and no conclusion at which Newton arrived by "demonstration" in his "immortal work" is now really accepted by modern astronomers.

11. But I have said (§ 6) that, nevertheless, Newton is in a sense responsible for even what the moderns now believe in physical astronomy, though discordant with his professed demonstrations. And here I must first beg your attention to

what these demonstrations purport to prove, and what they do not.

12. In order to establish the probability of the theory of universal gravitation, it must be perfectly obvious to any thinking person, that the first thing to be done was to prove that a gravitating body could possibly revolve round a centre of attraction. Now, there is no attempt whatever to prove this mathematically in the *Principia*.* The theory merely rests upon some vague reasoning in the first section, under the definition of a centripetal force, founded upon the inapplicable illustration of a ball held mechanically by a string and swung round; to which we shall hereafter revert. In the first proposition of the second section it is simply assumed that gravitating bodies could revolve; and the demonstration purports to prove, by a certain mode of measuring the areas of a polygonal figure, described by radii drawn to a fixed point at intervals, that such bodies will describe equal areas in equal times: in other words, the first proposition of the *Principia* purports to demonstrate that revolving bodies gravitating to a centre (for that is meant) will move in accordance with Kepler's second law, and describe by their *radii vectores* equal areas in equal times. The two "forces" employed to produce this motion are a so-called centripetal force, intended to represent the constant force of gravity, and the innate force ("*vis insita*") with which a body perseveres in its state of uniform motion in a right line, according to the first law of motion.

13. But in this proposition the "revolving body" is supposed to move in free space, "void of resistance," and the areas are described "in one immovable plane;" and it is to these two points I now especially desire to direct attention. In the first four corollaries, also, that follow the "demonstration," the same supposition, that the bodies are moving "in spaces void of resistance," is logically and expressly repeated; and this is necessarily implied in the two additional corollaries. But in the last of these it is said—"6. The same things hold good when the planes in which the bodies are moved, together with the centres of force, which are placed in those planes, are not at rest, but move uniformly in a right line."

14. This is indeed an astounding corollary; and I need scarcely say that it is not supported by any attempt at demonstration. Yet what it thus illogically, and, I venture quite plainly to say, falsely and absurdly asserts, is coolly introduced into the second proposition, which is simply the converse of the first *with that addition*. There is no fresh demonstration

* Nor elsewhere. Vide *Mech. of the Heavens*, § 29.

of the second theorem, which merely reasons backwards on the first, purporting to show that when the *radius vector* of a revolving body describes equal areas in equal times, it is moved by a centripetal force; and the conclusion drawn (from the same polygonal figure) is that "it"—the so-called centripetal force—"acts, therefore, always in the direction of lines tending to the *immovable* point S.—Q. E. D." And then we have, instead of any demonstration, merely this astounding assertion :—

"Case 2.—And it is indifferent whether the surface in which a body describes a curvilinear figure is quiescent, or moves together with the body, with the figure described, and its point S, uniformly in a right line."

15. I crave leave to observe with reference to this, and I do so without meaning to sneer, that it is a too simply mathematical view of the case! The atom of truth in it amounts to no more than this, that *if* the relative motions continued the same, whether the centre was in motion or not, it would not signify! Or this, that if we draw some circles on a sheet of paper to represent the orbits of revolving bodies, it is indifferent whether we carry the sheet of paper while we walk about peripatetically, or study it while quiescent on our desk, for the figures will still remain the same! But as a dynamical or physical proposition it is ridiculously absurd. For, what does it amount to? In the case of our earth it would amount to this. If the earth's orbital motion round the sun is (as we have been taught since September, 1863,) 65,000 miles an hour, the sun being regarded at rest, and if the sun's attraction serves to hold it in its orbit while travelling with that velocity (only varying a few thousand miles an hour when in aphelion and perihelion); then we are to believe that it would be "indifferent," if we were to start the sun off in a right line at the rate of 65,000 miles an hour; and that is a slow rate compared with that which some astronomers have assigned to the sun, for Bessel considered its motion two or three times as great, and Professor Airy's predecessor, Mr. Pond, assigned to it a velocity equal to that of light. Now, if the sun travelled onwards in space at the rate of only 65,000 miles an hour, and the earth kept revolving round it, what would then the motion of the earth necessarily be? Once every six months its motion would be at the rate of 130,000 miles an hour,—and how people can even conceive the sun's attraction could then hold it, I know not!—while every six months afterwards it would for a moment actually come to a dead stop; and yet then, instead of falling into the sun by its gravity, we must suppose it would suddenly hop off again to career wildly round the sun as before; its motion on this sup-

position of solar motion, being precisely and necessarily in a cycloid curve, like that described by a nail in the rim of a wheel as it rolls along the ground! And yet if solar motion in space be true, the earth and planets must all move in curves more or less cycloidal, and all of them always with velocities *plus* and *minus* that of the velocity of the sun in the course of each revolution they make. Such is the incontrovertible result of some of the teachings of current physical astronomy, as it is now to be found in all our orthodox books on astronomy, ever since the first Herschel's time. But it is what rational men will be unable to believe, whenever they come to think. It is utterly inconsistent with all Newton's "demonstrations," such as they are; and yet it has its foundation on the illogical so-called "6th Corollary" to the first proposition of the *Principia*.

16. Following the second proposition of the *Principia*, and the scholium thereon, we have another somewhat extraordinary corollary. It contains one of the rare allusions to be found in the *Principia* to the possible existence of a resisting medium in space as affecting the motions of the heavenly bodies;* and is as follows:—

"Cor. 2. And, even in resisting mediums, if the description of areas is accelerated, the directions of the forces deviate from the concourse of the radii towards the part to which the motion tends."

I have already elsewhere noticed this obscure corollary;† and I only allude to it here to observe that it was scarcely to be expected that Newton would give much attention to the influence of a resisting medium as affecting his theory, (since, as a matter of fact, all his demonstrations are based upon the supposition that the heavenly bodies move in empty space,) and to point out the illogical character of a corollary which supposes the direct contrary. But Newton having drawn such a corollary, we need not be surprised, perhaps, that the re-establishment of the *plenum* by Encke has not disturbed the faith of Newton's followers in his "demonstrations," though they relate only to the motions of bodies *in vacuo*.

17. Before proceeding with further remarks upon current physical astronomy as it clashes with the teachings of the *Principia*, I would beg leave to call attention to some other popular astronomical dogmas. For instance, whenever we now look up to the heavens at night, "to consider the moon

* Compare *Prin.*, b. ii., prop. 53, th. 41, *Scholium.*

† *Vict. Toto Cœlo*, § 24.

and the stars which God hath ordained," we cannot but think of what we have been taught to believe respecting their light, and their distances from "this spot of earth" on which we stand. And, first, let us give our attention to what our modern astronomers have taught us respecting what are called the "fixed stars."

18. The fixed stars are supposed to be suns, like our own sun, and to be the centres of systems, like what is called our "solar system." They are distinguished by their twinkling from the planets, which shine with a steady light; and in the field of the most powerful telescope they present no real measurable disc, however brilliant (which the planets do), but appear only as illuminated points of greater or less brilliancy. Their brilliancy is, as a rule, considered the criterion of their nearness to the earth; and they are divided into stars of the first, second, third, or fourth "magnitude," and so on, according to their decreasing brightness; but this really means (according to the current theory) that they are regarded as stars at greater and still greater distances from the earth; and these distances, I need only add, are enormous. The exceptions to this general rule are so rare, as not to require to be further noticed in a paper like the present. This theory, that the decreasing brightness of the fixed stars indicates increasing distance, involves "the probable supposition that they would all yield the same quantity of light at the same distance;"* and this really means that they are all of the same size, and that they vary in brilliancy merely as they vary in distance from us. By this method of computation *Sirius*, the nearest fixed star, is supposed to be about 140,000 times† more distant than the sun, or, in round numbers, to be about 140 thousand times 91 millions of miles distant from the earth! Taking the distance of the earth to the sun (91 millions of miles) as unity, therefore, the distance of *Sirius* is as 140,000 to 1; and the distance of the bright star, *a Lyræ*, is as 800,000 to 1; *i. e.*, it is distant from us 800 thousand times 91 millions of miles! I need scarcely say that the human mind can really form no distinct conception whatever of such figures!

19. Another mode of astounding our conceptions as to the imagined distances of the fixed stars, according to current theories, is by estimating the time their light would take to reach this earth. On this point I need only say that, according to the computations of Struve and Peters, it was inferred the light of stars of the second magnitude would take twenty-

* Grant, *Hist. of Phys. Ast.*, p. 542. † *Ibid.*, pp. 546, 547.

eight years to reach us,* light being then supposed to travel at the rate of 192,000† miles per second; also that the light from the smallest stars visible to the naked eye could not have reached the earth in less than 138 years; while the light from the smallest stars visible in Herschel's 20-feet reflecting telescope must have occupied 3,541 years in reaching the earth.‡ These few figures are more than enough for my present purpose, which is utterly to discredit this notion, and all that has been deduced from it, as *ab initio* and altogether absurd. It is part of this teaching that stars of the second magnitude, that is, stars only less bright than *Sirius*, must have been shining in the firmament for twenty-eight years before they were visible on the earth; and that the smallest stars visible to the naked eye must have been invisible for 138 years. The converse absurdity (as I will venture here to call it) has also been taught, that if such stars ceased to exist, they would continue as visible stars, to earthly eyes and telescopes, for 28 years and 138 years respectively after their non-existence! It has also been gravely put forward that there may be stars so distant that their light has not yet reached the earth, though it will yet do so; and again, with converse absurdity, that stars may be visible in our telescopes, as apparently existing visible stars, thousands of years after they have ceased to exist! To enable you the better to realize the absurdity of this, I may observe that it implies that the stars forming the Milky Way, as seen by us every night, and by Hipparchus and Ptolemy 2,000 years ago, might have been equally visible, though they had ceased to exist hundreds or thousands of years before! It also implies that the twinkling of the stars, and the changes in their brilliant prismatic hues, that we gaze on with admiration any evening, are twinklings and changes that occurred many years before, and not at the moment we see them! Whether upon Newton's now abandoned corpuscular theory, or the modern undulatory theory of the transmission of light, I can only characterize all this as absurd; and (granting either theory to be true, though I believe in neither,) as being a confounding of the supposed motion of light with our mode of seeing objects. It is, I consider, refuted every evening as the stars rise and set, and indeed every time we shut and open our eyes to look upon them. We see dark objects, as well as those that are bright and which are said to "emit light," the moment they are exhibited to us, if

* Grant, p. 553.

† Now reduced to 183,470 miles per second.—*Vide* Reddie's *Current Phys. Ast.*, book iii. p. 48. (Hardwicke.) ‡ Grant, p. 553.

within the range of our vision,—or, in the expressive phrase, "in the twinkling of an eye;" and they must disappear as objects (whatever brief impression may remain on the retina of the eye), whenever they are removed from the range of our vision, or cease to exist; and, however bright they may have been with the effulgence of light, they could not possibly be seen as objects even half a minute after they ceased to exist.

20. Nay, I will venture to go one step further and ask,—But what if light is instantaneous in what is called its "transmission"? I will also add that I believe it to be so; and further that there needs no corpuscular emanation of light in order that we may see it, neither any undulation of an imagined ether; but that the moment light is, it is seen, just as instantaneously as it was "in the beginning," when "God said, Let there be light and there was light"! For, let me ask, what difference is there, or can there possibly be, in the very least, between the "transmission" of light and of darkness? Even this absurd notion that stars might remain visible as stars for years after ceasing to exist, implies merely that the blanks or dark spaces produced by the non-existence of the extinct stars, would only become perceptible to us in the same time that their light had taken to travel to us when they were formerly created! But how do we see a dark object at all? Surely there is no light to be transmitted or waved in undulations from it; and yet we do see it; and I venture to say instantaneously, the moment it comes within the range of our vision, and as quickly, if seen at all, whether at a great distance or nearer. But, in short, for I must not travel into optics, nor pursue this important and interesting subject further here, I venture to say that when we see the sun, and the dark spots upon the sun, we see them together, just as they are, and at the very time they come within the range of our eyesight or glasses (though I am not overlooking nor denying the effect of atmosphere, any more than of defective eyes or object-glasses); and so, we may be sure that the stars in the heavens, as catalogued by Hipparchus and still visible to us, are actually and most certainly existing as we gaze upon them, and also that the few occasionally variable stars do vary in their brilliancy at the very time when they appear to us to do so. In fact, I allege that there never were any optical illusions in nature so astounding and incredible, as those which have been invented and palmed off upon mankind in modern days as deductions from our current physical astronomy.

21. The origin of this notion, I must briefly observe, is based upon the theory of the velocity of light, calculated upon the

difference in the computed time for the occultation of *Jupiter's* satellites when that planet is nearest and when it is most distant from the earth. And when the earth's distance from the sun was regarded as 95 millions of miles, the velocity of light so calculated was regarded as 192,000 miles per second, but has since been reduced by 8,000 miles per second, or to 183,470 miles, when the earth's distance from the sun was reduced six years ago to 91 million miles.* I must refer to what I have pointed out elsewhere as to the supposed experimental verifications of each of these astronomical rates, by Helmholtz and Foucault respectively, with an accuracy in the former instance, it was said, "to the 77-millionth part of a second!" † And I only allude to this here that it may be kept in mind that it is to our theoretical and physical Astronomy, and not to Optics, that we owe our modern teaching as to the velocity of light, as well as those curious speculations which have been based upon it relating to the fixed stars.

22. But now I must notice that our modern astronomers, having further discovered that the so-called "fixed stars" are not literally all fixed in the sense that earned them that appellation, have further illogically deduced from what is called the "proper motions" of some such stars the theory of "solar motion in space." I ought to state, however, that the notion of the fixed stars not being really fixed, or immovable with respect to each other, was actually put forward as a speculation "before the observations of astronomers acquired a sufficient degree of precision to indicate even the slightest trace of its real existence." ‡ One of the first persons to make this guess was Jordano Bruno, formerly a Dominican monk, who seems to have renounced the religious extravagances, first of Rome and then of Geneva, only to launch into other extravagant speculations of his own. But of his sincerity there can be no doubt. He was imprisoned by the Inquisition for two years, and was burnt as a heretic and an infidel in the year 1600. But Halley is said to have been the first who adopted the notion of the proper motion of the fixed stars from actual observation. § Bradley thought the apparent motions of some of the stars might arise, either from a motion of the solar system in space, or from a real change in the position of the stars themselves. Thomas Wright, of Durham (a name little known), in a book published in 1750, concluded "that the sun with its cortege of planets, as well as all the stars of the firmament, are in continual

* *Current Phys. Ast.*, b. iii., pp. 38-48. (Hardwicke.)
† *Vide* Note A. ‡ Grant, p. 553. § Ib. 554.

motion,"* just as Bruno did. Mayer, however, in 1760, after careful observations of the proper motions of 80 stars, and comparing the observations of Roemer in 1706 with his own and Lacaille's in 1750 and 1756, came to the conclusion that the observed proper motions "of the stars did not afford evidence of motion of the solar system towards any particular region of the heavens."† But the first Herschel, in 1783, arrived at a conclusion diametrically opposed to that of Mayer;‡ and since then, till now or till very recently, our orthodox astronomers appear to have left the region of doubt upon the subject of solar motion in space, for a region of absolute and I must add of blind unreasoning faith in its certainty.

23. I have alluded (§ 2) to the Paper I sent to the British Association at Newcastle in 1863, controverting this theory. That Paper I published in September, 1863, with an appendix, in both thoroughly exhibiting the illogical reasoning and absurdities involved in the theory,—and with what result? The Members of Section A of the British Association, and Fellows of the Royal Society and of the Royal Astronomical Society, to whom I sent copies of my Paper, were, without exception, *dumb!* But I quote the following from the Annual Report of the Council of the Royal Astronomical Society, laid before the Forty-fourth Annual Meeting of the Society on the 12th of February, 1864,—the Reverend R. Main, Vice-President (the *Radcliffe Observer*), being in the chair :—

"Astronomers will regard with especial interest the Astronomer-Royal's renewed attempt to determine the magnitude and direction of the motion of the solar system in space. Sir W. Herschel, in 1783, by a graphical method of great simplicity, showed that the proper motion of a few stars might be tolerably well accounted for by assigning to the sun a motion of his own directed towards λ *Herculis*. Other astronomers, starting with this as an approximate apex of solar motion, have sought to correct it by combining together a far greater number of stars than could be taken into account by the elder Herschel. The Astronomer-Royal, by the independent method of referring all the motions to three rectangular co-ordinates, as applied to 1,167 stars, falls again very nearly upon Sir W. Herschel's original position of the solar apex. *And yet, strange to say, notwithstanding the near coincidence of all the results of the before-mentioned independent methods of investigation, the inevitable logical inference deduced by Mr. Airy is, that the whole question of solar motion in space—so far, at least, as accounting for the proper motion of the stars is concerned,—appears to remain at this moment in doubt and abeyance.*" §

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* Grant, p. 555. † Ib.; *Vide*, also, Note B. ‡ Grant, p. 555.

§ *Monthly Notices of the Roy. Ast. Soc.*, 12th Feb. 1864, vol. xxiv., No. 4, p. 104.

Such is the testimony of Professor Airy, the Astronomer-Royal of England, in 1864, although he had, in four editions of his interesting *Six Lectures on Astronomy*, and always previously, publicly taught that solar motion in space, as deduced from the apparent proper motions of the fixed stars, was believed in "by every astronomer who has examined the question carefully."* I venture to think that, after this, I was entitled to claim, as I did in 1865, that my Newcastle Paper had "already had its triumph," † and that it had, in fact, forced Professor Airy to give up the notion of solar motion in space. When my attention was called by a Fellow of the Royal Astronomical Society to the foregoing passage in the Report of its Council, I endeavoured to make this important change of conviction on the part of the Astronomer-Royal known to the general public through *The Times* and some other of the leading daily newspapers; but in vain! And the editors have, perhaps, this excuse for their deciding to keep the public in ignorance of it, that a matter so very important ought, no doubt, to have been made publicly and generally known by the Astronomer-Royal himself, or by the Royal Astronomical Society, to whom it was first officially communicated, and by whom it was merely made known to the few persons who happen to be Fellows of the Society, or who may see their *Monthly Notices* and *Transactions*. In a letter, however, addressed to Professor Airy himself in June, 1864, relating chiefly to some other astronomical questions, I claimed to have preceded him in coming to his present "logical" deduction on this point; and in replying, very courteously, to other portions of my letter, he did not gainsay that part of it.‡

24. I shall now only notice briefly two considerations, overlooked by the astronomers, that rendered the notion of solar motion in space as accounting for or deduced from the proper motion of some of the fixed stars, *ab initio* illogical and absurd. In the first place, upon the prior hypothesis that the fixed stars do not occupy the same plane or surface, but are situated at enormously varying distances behind one another in the depths of space, it ought to have been evident, that if there was solar motion onwards in space, then all the stars, and not only some of them, would necessarily vary in their relative positions, and especially all those of different magnitudes at right-angles to the direction of such solar motion; just as

* Airy's *Lects. on Ast.*, 4th ed., p. 173.

† *Current Phys. Astron. critically examined and confuted*, in three books. Introduction and Notes. (Hardwicke.)

‡ *Vide* Note C.

when a man rides through a forest, all the trees at different distances on each side of him will necessarily appear to move relatively to one another as he advances. If, however, only one or two trees here and there appeared to move to a man in the midst of a forest, while others behind them remained stationary, the man ought to be sure of two things: first, that the apparent motion of these few trees must be more or less real; and, 2nd, that at any rate, and whatever the cause of their apparent motion might be, *he is not* moving through the forest himself.

25. But in the next place, the whole speculation and all the computations in connection with it, were further vitiated, and absurd *ab initio*, from the very calculations as to the parallax of those stars having an apparent proper motion, being made upon the self-contradictory supposition that they were viewed every six months from the ends of a base line only 190 millions of miles long,—that is, from the extreme ends of the diameter of the earth's orbit round the sun, which base was only accurate upon the hypothesis that the sun is at rest and *not* moving in space! But I cannot now spare further time to point out all the absurdities connected with this ridiculous and now abandoned theory, but must refer to what I have published elsewhere on the subject.*

26. After the fixed stars the next astronomical objects that must engage our attention are the planets, with their satellites,—including our own moon, upon the current theory which regards the earth as also a planet revolving round the sun. Considering the theory of solar motion in space as now virtually given up by the Astronomer Royal, I shall not here notice further the confusion and complications and contradictions that theory necessarily introduced into the planetary theory as believed in by Kepler and Newton, but will only refer to preceding paragraphs (§§ 7, 10, 13, 15,) of this paper, and to what I have previously written elsewhere on the subject.† But the fact is, many of the difficulties and complications which the theory of solar motion in space, if accepted, would introduce into the current planetary theories, do already exist with respect to the motions of the satellites of the various planets. And this consideration obliges me to revert to the two astounding and illogical corollaries to the first and second propositions of the *Principia* already noticed (§§ 13, 16).

27. As regards the last of these, and the restoration of the plenum, I will only further observe (*vide* § 16), that even were

* *Current Phys. Ast.*, b. iii., *in loc.*

† *Current Phys. Ast.*, § 54, *et passim*; also Append., *in loc.*

all other objections got over, there is one which is very obvious as regards the motion of our own earth round the sun as a planet in a resisting medium (whether it applies to the other planets or not); and it is an objection which, so far as I am aware, is not only not obviated nor answered in any book on astronomy, but it is one which, like most if not all of those I have now adduced, appears never to have been once considered by astronomers, but, on the contrary, is simply and altogether ignored. And it is this:—That however easy it might be, apart from dynamical considerations, to accept the Newtonian theory of the earth and planets revolving round the sun in spaces void of resistance, it is impossible not to perceive insuperable difficulties to their doing so in a resisting medium, if, like the earth, they all have atmospheres. Getting rid of the vortices of Des Cartes, in and by which the planets were supposed to be carried round the sun in their orbits, indeed, necessitated a free space for these bodies to move in; for, however easy it may be to conceive that solid bodies might move with immense velocity through thin air or ether, and yet retain their form, this cannot be rationally imagined of bodies having circumambient atmospheres like the earth; for in a resisting medium the earth, with its air, would soon assume the form of a comet, and in revolving in its orbit would carry all its light atmosphere and floating vapours behind it. And so of all the planets, if they have vaporous atmospheres.

28. But the other corollary referred to (§ 13), is replete with still graver difficulties. It was, no doubt, introduced by Newton, who, as I have already said, knew nothing of solar motion in space, with the view of explaining or accounting for the motions of the satellites round their primary planets on his theory. The analogy between the motions of the sun and planets if the sun moved onwards in space, and the motions of any planet and its satellites round the sun, is perfect. In a letter which appeared in the *Astronomical Register* for February, 1864,* on "The Motion of the Solar System in Space," I wrote as follows:—

"Again, if the sun moves in space, the variation in the orbital velocities of all the bodies that revolve round it *must* differ, by the whole amount of the sun's motion, when at right angles to its path, twice every revolution they make. For instance, taking the sun's motion as 18,000 miles an hour, [the rate supposed by MM. Argelander, Struve, and Peters,] the earth's mean orbital velocity of 65,000 miles an hour must sometimes be 65,000+18,000=

* London: J. D. Potter, Poultry, and King Street, Tower Hill.

83,000; and six months afterwards 65,000 − 18,000 = 47,000 miles an hour only. Is this credible on physical principles? Moreover, if the sun's motion in space were 65,000 miles an hour (Bessel thought it twice or three times as great), then the earth's orbital motion once a year would momentarily cease; as it must then describe a perfect cycloid in going round the sun. Then its velocity once a year would be 65,000 + 65,000 = 130,000 miles an hour; and six months afterwards, 65,000 − 65,000 = 0, according to well-known mechanical principles and the necessities of the laws of space and motion. It is not so plainly obvious, but it is equally true, that if the sun's motion is only 18,000 miles an hour, the earth must yearly pause altogether for an instant, in describing the looped curve it *must* describe in going round the sun. . . . . This is a mere fact of mechanical construction; the earth's path would be what I may call a *compressed cycloid.* But is that credible?"—(*Astronomical Register*, February, 1864, p. 38.)

29. Well, to this an "orthodox" astronomer actually ventured to reply, in the following number of the *Astronomical Register*, with the initial "D."; and I am informed the writer is a Mr. Dell, of Aylesbury. He wrote as follows:—

"(1.) In the February number of the *Register*, there is a letter from Mr. Reddie on the subject of the Motion of the Solar System in Space, in which he asserts that there can be no such motion, because of some fanciful contradictions 'to well-known mechanical principles'!

"(2.) I presume it will be admitted that *Jupiter* with his satellites is strictly analogous to the solar system, on a small scale; and we can therefore bring Mr. Reddie's assertion to the test of observation.

"(3.) Substituting *Jupiter* for the *Sun* in the following paragraph of the letter referred to, we shall read,—'If *Jupiter* moves in space, the variations in the orbital velocities of all the bodies that revolve about it *must* differ by the whole amount of *Jupiter's* motion, when at right angles to its path, twice in every revolution they make.' For instance, taking *Jupiter's* motion at 29,000 miles an hour, the second satellite's mean orbital velocity of 32,000 miles an hour must sometimes be 32,000 + 29,000 = 61,000, and half a revolution afterwards, 32,000 − 29,000 = 3,000 miles an hour only.

"(4.) Now when Jupiter is in opposition, these two points of maximum and minimum velocities will occur at the occultation and transit of the satellites. For instance, taking the second satellite, its velocity at its occultation should be, according to Mr. Reddie, 61,000 miles an hour, it being then moving at a right angle with the planet's path, and in the *same direction;* while at its transit it should have a velocity of 3,000 miles an hour only, as it will be moving at a right angle to its primary's path, but in an *opposite direction.* Therefore the time occupied by the transit of the satellite should be somewhere about twenty times that occupied by the occultation.

"(5.) But according to observations at previous oppositions, and to the

computation of the times which will be given in the pages of your *Register* for May next, both phenomena occupy a little over two hours, and differ only a few minutes ; and the accuracy of your computed times may be confirmed by future observation about 11th May, [1864,] at which time *Jupiter* will be again in opposition.

"(6.) It appears then, that Mr. Reddie must either deny *Jupiter's* orbital motion, for precisely the same reasons as led him to conclude the Sun to be motionless, or that he has misapplied the 'well-known mechanical principles and necessities of the laws of space and motion.'"

30. To this letter I replied in the April number of the *Astronomical Register*, as follows :—

"(1.) As regards paragraph one of D.'s letter, I think he must yet confess that I indulge in nothing 'fanciful.' 'The test of observation' (in par. 2) I accept. I agree with paragraphs three and four except the last two lines, in which D. draws a false conclusion from his own premises, as probably he has already discovered. But, since his argument stands on record, I must answer it.

"(2.) Suppose, therefore, the Sun to be at rest, and that *Jupiter* revolves round it, with a mean velocity of 29,000 miles an hour ; also that his second satellite revolves round him with a mean velocity of 32,000 miles an hour. Then, doubtless (according to well-known mechanical principles, and the necessities of the laws of space and motion), the satellite *must* move, when it is being eclipsed at the rate of $29{,}000 + 32{,}000 = 61{,}000$ miles an hour *direct*, and during a transit at the rate of $29{,}000 - 32{,}000 = 3{,}000$ miles an hour *retrograde*. So far, I beg leave to assume D. goes with me ; as I am merely quoting what he himself truly says, though he puts it forward only as an *argumentum ad hominem* against me.

"(3.) But he adds (par. 4) :—'Therefore, the time occupied by the transit of the satellite should be somewhere about twenty times that occupied by the occultation!' To this (as I have hinted) I feel it almost unnecessary to reply. D. has inadvertently overlooked the effect of *Jupiter's* own proper motion, and forgotten that *the time* of the occultation or transit only indicates the *apparent* and *relative* motions of the planet and satellite : i. e., the time in which they cross one another.

"(4.) I therefore pass over par. 5 of D.'s letter, and come to par. 6. There he says :—'It appears, then, that Mr. Reddie must either deny *Jupiter's* orbital motion, for precisely the same reasons as led him to conclude the Sun to be motionless, or [admit] that he has misapplied the well-known mechanical principles, and the necessities of the laws of space and motion.' Now, I do not take advantage of the error in reasoning already noticed, upon which the first of these alternative propositions is based ; but will frankly admit that there is a fair analogy between the solar system with a moving Sun, and the motion of *Jupiter* and his satellites. I assume, also, that D. will now give up his second proposition, taking for granted that (assuming

his own data in par. 3) he now sees that the real motions of *Jupiter's* second satellite *must* differ precisely by 58,000 miles an hour, during an occultation and transit respectively, and that these greatly varying velocities are *confirmed* by 'the test of observation.'

"(5.) But should D. not admit these assumptions, then I would beg leave to turn his own argument against himself thus:—If *Jupiter's* real motion be 29,000 miles an hour direct, and we suppose the real motion of his second satellite during an occultation to be 32,000 miles in the same direction—then the apparent and relative velocity of the satellite (i. e. the rate at which it will pass behind the planet), will be only 3,000 miles an hour direct; whereas, if during a transit, while *Jupiter* is moving at the rate of 29,000 miles an hour *direct*, we suppose his satellite really to move at 32,000 *retrograde*,—then (to apply the test of observation) the apparent speed with which they would cross one another would be 29,000 + 32,000 = 61,000 miles an hour; and, in that case, the eclipse would certainly occupy twenty times the period of the transit. The latter would be over in little more than an hour, the occultation would take more than twenty hours. But 'the test of observation' refutes these absurd suppositions and their results, and proves what D. had questioned.

"(6.) The analogy adduced by D. is nothing new to me. In a Paper on this subject, which I submitted to Section A of the British Association this year, I said:—'The motion of the moon round the earth, as it moves in its orbit round the sun, is analogous to the motion of the earth round the sun, if the sun moves in space.' . . . . In my former letter, I only noticed a few very salient points, in order to induce others *to think*.

"(7.) If D. had said that such immensely varying angular velocities as those of *Jupiter's* satellites are thus shown to be, while revolving round their primary and but slightly varying their respective distances, cannot be reconciled with the current dogmas of physical astronomy, any more than the varying velocities of the earth, if the sun moved in space, to which I have objected, I could not have gainsaid the proposition. But the *facts* as to the motions of *Jupiter's* satellites being what they are,—assuming D.'s own data, and applying his own test,—I trust that he is not prepared to say, 'so much the worse for the facts,' and to cling to irreconcileable theories.

"(8.) D. should also recollect that the motions of *Jupiter* and his satellites, like those of all the other planets and satellites, and the comets, would themselves be greatly complicated and confused by the motion of the sun as their centre. Their old aphelion and perihelion velocities would all be upset if the sun so moved; and all the elliptical orbits converted into complicated, impossible paths, that could only be characterized as *Vermicular*. For simplicity and clearness, I have chiefly argued only as to the earth and moon. To do more would be like attempting to explain the *obscurum per obscurius*. After all, we do know somewhat more of this dull earth than of *Jupiter* and his satellites. These may whirl about in looped curves, with alternate points of rest and great velocity, and yet continue, as we see they do, in the heavens. But what would happen if this massive earth were thus arrested in its orbit, or

had its velocity reduced by 36,000* miles an hour? Moreover, *what* could so reduce its velocity; or, if reduced, cause it afterwards to increase, so as to enable it to get round the flying sun?

"(9.) In conclusion, I would beg D. and others to observe that, at last, after a life-long adherence to this ill-considered theory, the Astronomer-Royal now admits it to be fraught with doubt and uncertainty and confusion. . . . . I, too, was taught, as a child, that even the earth's whole orbit is 'only as a point,' with reference to the fixed stars; but, as a man, I don't believe it.' *Credite posteri?*"

31. These arguments, as I have said, appeared in the *Astronomical Register*, which has a considerable circulation among astronomers, but no rejoinder to my last letter appeared either from Mr. Dell or any other. In fact, the arguments are unanswerable; and, of course, it is not my duty to account for the apathy, or whatever else it may be, among professed astronomers, who must be supposed to be competent to understand the bearings of such reasoning and demonstrations upon our current physical astronomy. What is thus true of *Jupiter* and his second satellite, taking the rates of motion assigned authoritatively to each, is true *mutatis mutandis* of all the planets having satellites, and so it applies to our earth and moon. It was to the actual motion of our own satellite, according to the current theory, that I endeavoured to draw the attention of the British Association at Bath in 1864, in the Paper I have already referred to, and which I afterwards placed in our Vice-President's hands (§ 2).

32. But here I will only refer to one most important point relating to the moon's motion, as bearing upon the verification, which Newton is supposed to have obtained by means of it, of the law of universal gravitation. Now, this supposed verification was obtained by calculating the amount of the moon's fall from the tangent to her orbit in a given time. Taking the moon's orbit round the earth as circular or oval, and taking the semi-diameter of this *quasi*-orbit as equal to sixty semi-diameters of the earth (*i. e.* $60 \times 4{,}000 = 240{,}000$ miles), Newton found "that the time occupied by the moon in falling through a given space was exactly sixty times greater than that occupied by a body at the earth's surface in falling through an equal space."† And so, says Mr. Grant, in his *History of Physical Astronomy*, "it thus appeared that the force which retained the moon in her orbit, as deduced

* This would be so, if the sun's motion were at the *lowest* rate assigned to it (by Struve and others) of 18,000 miles an hour.

† Grant, pp. 24, 25.

from her actual motion, was less than the force of gravity at the earth's surface, in the exact ratio of the inverse square of the distance from the centre of the earth." And Grant adds, in a note, "It is said that Newton became so much agitated as soon as he began to suspect the probable result of his calculation, that he was compelled to assign to a friend the task of bringing it to a conclusion." * Grant very fairly notices, that in making this calculation, "the force which retains the moon in her orbit is here supposed to act in the same direction during a very short space of time"; but he thinks "this supposition, though not strictly true, cannot sensibly affect the result." Now I beg to observe that the same fallacious supposition runs through all the demonstrations of the *Principia*, and is especially patent in the first proposition, which is demonstrated by an illogical application of the laws of the parallelogram of forces or velocities to the solution of a problem which relates to a central or centripetal force; and this, I say, does materially affect the result, and, in fact, entirely alters it.† But I pass over this objection now, because there are others which claim priority over it;—namely, that the moon has no such orbit in reality, as was assumed for the basis of the calculation, if the earth goes round the sun; that there was not any computation whatever "deduced from the moon's *actual* motion"; that there is no such fall from the tangent to her *actual* path; in short, that all that depends upon this famous "*experimentum crucis*" (as it has been called), "which was to decide whether Newton had penetrated into the secret of the celestial motions, or whether he had been occupying his mind with speculations of a purely mathematical nature,"‡ rests upon a series of false data and false suppositions, and upon consequent fallacious reasoning. This problem also, like that of "the three bodies" (§ 2), was only solved upon the false supposition that the moon has a nearly circular orbit round the earth, which could only be if the earth is at rest in space; the moon's *actual* path, *ex hypothesi*, if the earth goes round the sun, being an orbit, differing slightly from that of the earth, nearly in *a circle round the sun*. This is simply *a fact*, about which there can be no dispute among rational beings who understand the subject; and I need scarcely add that the force of gravitation, or any other force in the universe, can only produce or affect the actual motions of bodies; and the effects of such forces cannot possibly be truly measured by calculations based upon merely relative or apparent motions. This particular branch of this great subject, however, I have specially discussed in the Paper

* Grant, p. 25. † Vide *Vis Inertiæ Victa*, § ix. *passim*. ‡ Grant, p. 21

already alluded to (§ 31), prepared for the Bath Meeting of the British Association, and rejected;* in which I also show that if the sun does affect the moon's real motions upon the heliocentric hypothesis, its influence is *repulsive*, or the very reverse of what appears to be proved by all the fallacious demonstrations that deal with the moon's relative or apparent motions only.

33. There are still a few points connected with current physical astronomy to which I desire briefly to direct your attention. There are, especially, two dogmas of Newton's *Principia* universally accepted as true, and constantly in men's mouths, which I wish to bring before you, and ask you to consider, with the reasoning upon which they are based, in the *ipsissima verba* of Newton. One of these dogmas is embodied in what is called the Third Law of Motion, and it is as follows, viz.:—

"LAW III.—*To every action there is always opposed an equal reaction; or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.*"

Such is "the law," as laid down in Newton's "immortal work"!—Now, listen to the reasoning upon which this law is founded:—

"Whatever draws or presses another, is as much drawn or pressed by that other. If you press a stone with your finger, the finger is also pressed by the stone. If a horse draws a stone tied to a rope, the horse (if I may so speak) will be equally drawn back towards the stone." (*Prin.*, book i., sect. 1.)

Now, in direct opposition to this, I have already ventured to assert, and beg leave now to repeat—

"That mere matter, and therefore all material bodies, can only be truly regarded as perfectly passive, and without any tendency or inclination of their own; and that consequently it follows that whenever a body offers resistance to any action exerted upon it, or to any force impressed against it, such resistance is not due to the matter or body itself, or to any *vis inertiæ*, but to some previously impressed force or influence affecting the body. So, when a horse draws a stone tied to a rope on level ground, the resistance the horse has to overcome is due to the weight of the stone and the friction resulting therefrom. If the stone is small and light, the resistance may be so small as to be unfelt; or the stone may be so large and heavy, that the horse can only with the greatest difficulty move it; or it might be so heavy, that the horse could not move it at all; in which case there would be no friction, and

* *Vide* Note D.

the resistance experienced would be due to the weight of the stone exclusively. The amount of resistance, whether of weight only, or of weight and friction combined, depends on the gravity, and is only proportional to it, whatever the exertion of the horse may be. Beyond the weight and friction, there is no further resistance; and this is clearly inconsistent with the dogma that 'action and reaction are always equal and contrary.' Or, again, in pressing the hand against a stone or other rigid substance, there is no reaction whatever. According to its weight, or hardness, or strength, the substance resists. Beyond that, it yields or breaks. As long as the body resists the pressure, the resistance (*i. e.* while the body does not yield) is certainly *and therefore* greater than the pressure. As the body does not press back, but only resists, the pressure is necessarily always only equal to itself; but there is no reaction in this case, such as there would be if some elastic body were pressed in like manner." *

34. I must tell you, now, how some modern "men of science" have written upon the same subject. In the *Edinburgh New Philosophical Journal*, for April, 1864, Mr. Balfour Stewart, F.R.S. (writing anonymously), criticised my reasoning, as follows:—

"Did Mr. Reddie ever try to open a massive iron gate, or to deliver a large curling-stone? Had the weight of either body anything to do with the difficulty he experienced in handling it? Did he ever try to stop a large grindstone set in rapid rotation, or was he ever struck by a cricket-ball? We fear he has not been, or he would reverence the recollection of the *Vis Inertiæ*."

To this, of course, I could but answer:—

"That only weight in the case of the curling-stone, and weight and friction in the case of the 'massive' iron gate, could have to do with the difficulty of delivering the one, or of opening the other. And in proportion as the massiveness or weight of the stone or gate might be reduced, would the difficulty of moving them be lessened, till it might vanish altogether if the weight could vanish. The writer did not probably reflect what the word 'massive' really meant when making these interrogations. And I would suggest to him the consideration, that an empty puff-ball, almost without weight, even if thrown with the most frantic effort, will strike with no material force, and could not induce any of that 'reverence' which might doubtless follow a blow with something more substantial and solidly filled." †

35. I have, however, now to cite from a much more eminent Fellow and Vice-President of the Royal Society, namely, Mr. W. R. Grove, Q.C., who was also the President of the British Association at Nottingham in 1866; and it will, no doubt,

* Vide *Vis Inert. Victa* (in *Current Phys. Astr.*), §§ 21—26, and note.

† *Mech. of the Heav.* (*Current Phys. Astr.*), note, p. 17.

surprise some persons to find that what I quote from him is in accordance with my own "heretical" views! Well, that distinguished scientific author, in the first edition of his celebrated work on *The Correlation of Physical Forces* (1846), thus expressed himself:—

"Inertia appears to me to be a static condition of the force of gravitation, or, in other words, resistance to motion [he means force] occasioned by the force of gravitation. *Without gravitation I cannot conceive inertia.*"

And in the second edition of the same work, published in 1850, I find the following passage:—"The phenomenal effects of gravitation and inertia, *if there be such a force as inertia*, being motion and resistance to motion," &c. But I regret to be obliged to add that I cannot trace any corresponding passages in the last edition of Mr. Grove's book, published in 1862, though I am glad to find he does not shrink from repeating in it his reasoning against Black's theory of "latent heat," and opposes the more modern notion of "invisible light," as to both of which scientific dogmas I, too, have ventured to be a thorough "heretic." Why Mr. Grove now sinks his opposition to the self-contradictory notion of "such a force as *inertia*," it is not for me to say. For myself, I continue, profanely, without the least reverence for *Vis Inertiæ*, in both senses of the latter word.

36. But more popular authors also enlighten the public with their views of Newtonian dogmas. A really brilliant writer, in an article on "Force," in the *Cornhill Magazine* for 1861, put forth the following, not, indeed (he said), as the "common-sense" view of things, but as "that which arises from the thoughtful tracing of their real connection," or the scientific view:—

"All actions in nature are two equal and opposite actions. It is a law with no exception, nor possibility of exception. Nor is any change, any seeming origination or ending of an action, rightly apprehended till it is seen thus in absolute interlinking with its fellow. We are familiar with this principle in some simple instances, but the demand is that we should be sure of it in all. The very spirit of science consists in the confidence with which it is grasped, and applied to all cases, *however vast beyond the reach of our observation, or complex beyond our power to unravel*, however long the completion of the process may be deferred."

This might well be called, in my opinion, the third law of motion travestied, or action and reaction run mad! And yet I fear this writer's views are not altogether uncommon in our day. He somehow connects this "law" with the *alternate*

actions of vibration (with which it has nothing in the wide world to do), instead of the *simultaneous* "*reaction*" predicated by Newton of pressure, &c.; and the *Cornhill* writer sums up his article with a not illogical conclusion (partly quoted from some other author), which, if intended as science and not poetry, must have startled even some of the "scientific," as well as more ordinary students in natural philosophy. He says:—

"If all natural action is vibration, involving opposite and equal actions, then the sum of it all equals—none. These opposites are like *plus* and *minus*, and they make up 0. 'There never was a force in the universe for any one moment of action but there was another of equal force, acting in opposite direction. The sum total of all the forces in the universe is equal to—nothing;—and has been so at every moment.'"

With such ridiculous nonsense passing current amongst us as "the very spirit of science," need we be surprised that the "Positive philosophers" of our day, in like manner, profess to have found, that the great First Cause of all the Phenomena of this world is, also, only equal to 0? But to revert to *the* "action and reaction" referred to in the third law of motion, it is not unnatural to ask,—How, if every action were always opposed by *an equal reaction*, could the impression of force ever produce motion? These opposites would really be "like *plus* and *minus*"; and "all the forces in the universe would indeed be = 0"! Surely science and common sense must alike agree, that when bodies resist pressure, the degree of resistance (call it "reaction" if you please) depends upon the weight or quality of the body, and not upon the amount of pressure. When we press with the finger against marble, quicksilver, or water, with equally great force, we experience three different degrees of resistance from these three different material substances, arising from their different qualities of hardness or softness, solidity or fluidity, but having no dependence upon, nor equality with, the degree of force or pressure (or "action") exerted upon them.

37. But I must now pass on to notice, as I promised (§ 12), what appears in the 1st section of the *Principia* relating to a centripetal force:—

"DEFINITION V.—*A centripetal force is that by which bodies are drawn or impressed, or any way tend towards a point as a centre.*

"Of this sort is gravity, by which bodies tend to the surface of the earth; magnetism, by which iron tends to the loadstone; and that force, whatever it is, by which the planets are perpetually drawn aside from the rectilinear motions which otherwise they would pursue and made to revolve in curvi-

linear orbits. A stone, whirled about in a sling, endeavours to recede from the hand that turns it, and by that endeavour distends the sling, and that with so much the greater force, as it is revolved with the greater velocity, and as soon as ever it is let go, flies away. That force which opposes itself to this endeavour, and by which the string perpetually draws back the stone towards the hand, as the centre of the orbit, I call the centripetal force. And the same thing is to be understood of all bodies revolved in any orbits," &c.

"If a leaden ball projected from the top of a mountain by the force of gunpowder with a given velocity, and in a direction parallel to the horizon, is carried in a curve-line to the distance of two miles before it falls to the ground, the same, if the resistance of the air was taken away, with a double or decuple velocity, would fly twice or ten times as far. And by increasing the velocity we may at pleasure increase the distance to which it might be projected, and diminish the curvature of the line which it might describe, till at last it should fall at the distance of 10, 30, or 90 degrees, or even might go quite round the whole earth before it falls; or, lastly, so that it might never fall to the earth, but go forward into the Celestial Spheres, and proceed in its motion *ad infinitum*."

Now, in these two brief citations you have, in Sir Isaac Newton's own words, the sum and substance of his arguments in support of the theory that the heavenly bodies could be held in their orbits and made to revolve by gravitation. As regards the first illustration of "a stone whirled about in a sling," I can only ask (as I did in my Cambridge paper),—Does the string, in the case supposed, "draw back" the stone towards the hand, or merely restrain it, or hold it, at a certain distance from the centre? And, could a force like gravity act as the string does? Let a rod of wood or iron be substituted for the string, and it must be self-evident that the rod does not and cannot "draw back" the stone attached to it. But I equally maintain that the string does not draw back the stone, but only holds or restrains it; and that a positive and, if I may so say, elastic force like gravity could not act as the string or rod does.* Now this illustration is of more consequence than might at first be thought possible; for, when well considered, and when you once fully realize the fact that the string does not in the least draw back the revolving stone, but only holds or restrains it from flying away, you will find it impossible to accept any kind of *quasi*-demonstration that might seem to prove the contrary.

38. But the popular notion is, that there can be a kind of balance between the force of gravity and a projectile force, that would enable bodies to revolve in a perfect circle; or, in

* Vide *Mech. of the Heavens*, §§ 41—44.

other words, it is held, that "when a body revolves in a circular orbit, by means of a force directed to the centre of the circle, the centripetal and centrifugal forces will be equal." * Now, if you will only keep in mind the fact, that when a stone is made to revolve at the end of a string, the string does nothing but *hold* or *restrain* the stone, and is *not* "a centripetal force" that "draws back," according to the *Principia*, you will reject this popular notion, or at least see that it requires some better proof before acceptance. This imagined "balance" between a force of projection and gravity, reminds me of the mythical balance of Mahomet's coffin between heaven and earth,—only it is less rational. Granting that it might be just possible to balance an iron box half-way between a magnet and the earth, (which I do not, however, admit,) still the balance would necessarily be so fine, that the slightest breath, or anything producing the slightest vibration, would destroy the balance, and down the box would fall. Mahomet's body would then certainly "go to the mountain" of the earth, and that with a vengeance! But however conceivable such a "balance" might be as a *statical* problem, or (perhaps I should rather say) puzzle, I venture to add that as a *dynamical* conception, when carefully considered, it is well-nigh foolish. A constant force like gravitation must needs always overcome any single impulse once given to a gravitating body, however great the impulse might be. And Sir Isaac Newton's other illustration, with his reasoning upon it, is extremely weak and faulty. For small distances on the surface of the earth, such as for two or even twenty miles, when the earth may be regarded as a level plain, and gravity as acting in parallel directions downwards, what he says as regards the distance a ball might be projected is approximately true. The parabolic theory of projectiles is, in fact, based upon these two assumptions. But when he goes on to apply the same reasoning to a projectile supposed to be made to "fall at the distance of 10, 30, or 90 degrees," the curvature of the earth's surface, and the converging of the lines of direction in which gravitation really acts, ought not to have been disregarded. I know not whether to consider it as amusing or sad, to find such an instance as this of "absence of mind" on the part of the great Newton. His whole reasoning, to prove that gravitating bodies might be projected so as to go forward and revolve in the celestial spheres, is really based upon results previously arrived at upon the supposition that the earth is a level plain, and that gravity acts in parallel lines, and not in lines con-

* Grant, *Hist. of Phys. Astr.*, p. 23, note.

verging to a centre! But this is, after all, quite in keeping with the extent of "the fall of the moon from the tangent to her orbit," being computed from an *unreal* fall from an *imaginary* tangent to an orbit that could have *no actual* existence (unless the earth were *at rest*), and with gravity acting in *parallel directions*, instead of towards a centre! (§ 32.)

39. But I feel that it is now time to bring this paper to a close. I must apologise for its great length, and yet observe that it is far too brief to do full justice to so large and complicated a subject. My remaining words, also, like those with which I commenced, must further partake of an apologetic character. I know very well from experience, that two remarks are likely to be made off-hand, both about this paper and what will be called "my peculiar views." Some of your "scientific" friends may tell you very plainly, that "*they* know I am all wrong"; and others may ask, what may seem to be a very pertinent question, namely, "How the astronomers can make their accurate calculations of the positions of the planets and of the periods of eclipses, if *all their astronomy* is as wrong as I wish to make out?" Now, I must reply, that this question could not be put by any one, however "scientific," who understands the subject, and knows the difference between theoretical and practical astronomy. And I venture to say that neither such questioners, nor those who would fain decide with a word of authority that I am wrong, are likely to enter the lists in order to exhibit to you my errors. If they do, however—and they are at least fairly challenged,—I shall be agreeably surprised, and will feel greatly indebted to them. My delusion, if I am wrong, must be even greater than theirs; for they can plead great names, a long tradition, and that most powerful corrupter of the human intellect, inveterate prejudice, as all on their side; while I—unfortunately, I must admit, with seeming presumption—stand almost alone, and *contra mundum!* Let me then plead, in these circumstances, at least,—for refutation and enlightenment and unsparing criticism. I beg for this, much rather than for observations which may be confirmatory of any of my arguments, on the present occasion. Not that I despise the latter; for I am about to cite a few words from a recent pamphlet "by a Wrangler,"* which may serve as the best answer to the question, which I have anticipated might be put by some, as to the calculations of astronomers. The "Wrangler" says:—

* *The Theories of Copernicus and Ptolemy.* (Lond.: Longmans, 1867.)

"It is a common notion, and one popularly believed to be unanswerable, that the calculations of the positions of the planets, the periods of the comets, the times of eclipses, and other astronomical problems solved by the application of Newton's beautiful theory of gravitation to orbital motion, and so marvellously confirmed by the actual observation of the phenomena in the heavens themselves, essentially require, as their starting-point, the supposition of the earth's *absolute* motion round the sun.

"That this is not a true notion will appear evident, simply from this, that astronomers actually make some of these calculations on other hypotheses.

"The mathematician, before commencing his calculation of the motion of a heavenly body, is obliged to seek for some point either really fixed in space, or, if that be impossible, to suppose some point to be fixed: such a point is commonly called the *origin of co-ordinates*. . . . .

"Practically, indeed, the astronomer chooses the origin of co-ordinates quite arbitrarily, placing it where he will be able most easily to simplify the analytical process which any particular investigation requires.

"Thus, for example, in the *planetary theory* . . . the centre of the sun is taken as the fixed point, and the earth, together with all the other planets, are supposed to revolve round it.

"On the contrary, in the *lunar theory* the centre of the earth is chosen as the fixed point, &c. . . . .

"Again, in Goodwin's 'Mathematical Course,' art. 12 of the section on astronomy [this passage occurs]: 'According to observation, the sun appears to move round the earth; but the phenomena will be exactly the same whether the earth moves round the sun, or the sun round the earth.' . . . .

"The practice, then, of astronomers favours neither theory, and ignores the question of *absolute motion* altogether, recognizing merely that which is *relative*." (Pp. 3—6.)

40. Afterwards, the "Wrangler" goes on to ask and to answer a question, which will probably astonish all who hear or read this paper much more than anything I have yet said. He asks:—

"Has it ever been demonstrated that the earth revolves round its own axis?" [And his answer is] "I must reply in the negative, and assert, moreover, that we shall not find that its demonstration is claimed in any truly scientific treatise, although by every one its revolution [he means rotation] is assumed to be a most probable truth." (P. 26.)

Again, he goes on:—

"Should, then, the earth be at rest on its own axis, the only alternative we have is to suppose a revolution of the whole heavens in the short space of twenty-four hours.

"Startling as this is, we have seen that it has not been demonstrated to be mechanically impossible, as far as the terrestrial phenomena are concerned;

and now I shall proceed to show that, so far as our knowledge goes, we cannot consider it to be even improbable." (P. 31.)

I must make two further brief citations from the "Wrangler's" pamphlet. He comes to this conclusion, among others:—

"(2.) That the law of mutual attraction is not universal; some constellations attract while others repel." (P. 32.)

And he goes on—

"As [this] answer implies that Newton's law of gravity is not universally true, and drives us to the assumption of some conflicting law of repulsion, there must be a more general law, comprehending these two, which shall determine under what circumstances each of these opposite forces is to act; but of this law we know, as yet, nothing." (P. 33.)

41. I have made these citations from this remarkable pamphlet—chiefly remarkable because it comes from a Cambridge wrangler—for the sake of its facts and mathematical testimony, but not as always agreeing with the candid author's arguments. He takes, in fact, "a too simply mathematical view of the case,"—in that respect being thoroughly Newtonian! (*vide* § 15.) Mathematically, no doubt, and as far as practical astronomy is concerned, it may not signify whether the earth or the sun is regarded as in the centre; but theoretically and physically it makes all the difference in the world. If the sun is regarded as the centre, with the earth describing an orbit round it at a distance of 91 million miles, and the exterior planets are all still further and further off, then the fixed stars are necessarily banished far away to the inconceivably immense distances that current physical astronomy assigns to them. But if the earth is in the centre, whether at perfect rest or only rotating on its axis, then all these enormous distances would be reduced, either on the Ptolemaic theory or on that of Tycho Brahe. And this brings me naturally to another question, which I have frequently been asked, namely, What theory have I of my own, to substitute for that which I have claimed to upset? To that question I beg leave to reply, that twenty-seven years ago I should have been much more likely to propound a fresh theory than I am now. In fact, I then had a theory, and at that time it was *not* anti-Copernican, but proceeded upon the hypothesis which I had been taught, like you, to believe in from childhood, namely, that the sun is at rest and the centre of our system. I frankly confess, however, that the more I have studied this subject, only the more inclined have I become to depart from all the teachings of our current physical astronomy! And I must observe that it is a popular delusion to suppose, that a helio-

centric hypothesis was never heard of before it was propounded by Copernicus, and is so rational that everybody ought to accept it the moment it is advanced. Pythagoras, "the first philosopher," taught a heliocentric theory 2,000 years before Copernicus, and there was also the Egyptian system and the theories of Apollonius and Heraclides. The idea of physical astronomy ever again becoming completely revolutionized may seem monstrous to those who have not gone deeply into astronomical problems; but it should be remembered, that, even when the subtile forces of nature were very imperfectly known, and when the heavens were supposed to be regulated by geometry and mechanical arrangements of various circular movements, even then the two greatest mathematicians who ever lived, Euclid and Archimedes, as well as Eudoxus, Hipparchus, and Aristotle, all "deliberately preferred the geocentric solution of the astronomical phenomena." * And since even a Cambridge wrangler has put forth a plea in favour of the Ptolemaic system, and acknowledges the necessity for some law of repulsion to counterbalance that of gravity, I may now perhaps venture to conclude this paper with a few passages from the first tractate† that I published on this subject seven years ago, which will express nearly my present views, and at least as much of fresh theory as I have any inclination now to indulge in :—

"Supposing cold to be the cause of gravitation, acting as it were externally, and pressing all bodies, in proportion to their matter, towards a centre, from every side. And suppose the sun to be in such a centre of the solar system, and the effect of its heat to be repulsive, and contrary to the cold causing gravitation. Then, the fatal defect in the theory of universal gravitation would be supplied; for when bodies approached the sun they would thus again be repelled; and the more directly and with the greater velocity they approached it in their revolutions, the more violent would be their repulsion, as, for instance, in the case of comets."

"It also follows from what has been laid down, strange as it now may sound, that the heavenly bodies *might* revolve in crystalline spheres, either perfectly round or elliptical, that is, if *not* attracted towards the centre, as was supposed to be the case by a very ancient system of astronomy; or that they could be carried round their centres in circular vortices of ether, or some other element, as was held by the Cartesians; or they could revolve if held by some balancing and opposite powers or forces of nature, that could really act as centripetal and centrifugal forces of attraction and repulsion, both equally constant, and alternately increasing and decreasing, so as to

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* *The Astronomy of the Ancients.* By Sir G. C. Lewis, Bart., *in loc.*

† *Vis Inertiæ Victa*, § 137, note, and §§ 142—146.

counterbalance one another, and produce the elliptical motions of the planets round the sun, if their orbits are elliptical. But it follows also, that the theory of mechanical laws and gravity which have been assigned to them for producing these effects for the last 150 years, are inadequate for the purpose, and that this hypothesis is perhaps, of all others, the only one demonstrably impossible.

"The theory herein hinted at as a substitute, namely, that the laws of the universe are chemical rather than mechanical; and that an equally constant and universal repulsive influence must operate to counterbalance any force of gravitation, if gravitation be universal, is equally tenable, whether the Copernican or Ptolemaic systems of astronomy be true. The central heat of the sun, probably the source of electricity also, as well as of light, with the exterior cold, whence may come the principle and currents of magnetism, as opposed to those of the electric fluid, obviously suggest themselves to all who have made the chemistry of creation their study, as such universal and opposing forces in nature.

"On the other hand, it would be vain to deny that, when full regard is had to the contrast between light and heavy substances, between heat and cold, between terrestrial and celestial bodies as they appear; judging from all the analogies of substances of which we have experience, and apart from the preconceived notions as to the physical laws and mechanical theory of astronomy which have been instilled into us from the cradle; there is much to be said in favour of reverting to the old notion of a central earth, surrounded by its glorious canopy and hosts of revolving lights, as after all most probable. That such a central globe would, however, most probably revolve on its axis, and only be at perfect rest at its poles, is also perfectly consistent with the notion above hinted, as to the effects of heat and cold, or electricity and its opposite, in regulating the motions of the earth and heavenly bodies. The expansion of the air and elements from the heat and light of the sun, and its electrical influence (if it have such influence), acting obliquely, and upon one side of the earth at a time, might have originally produced, and may now continue to cause, its rotation.

"Certainly, from all we know of fire and light, it seems more natural to conclude that the heavenly bodies are formed of some such imponderable substances, as it were embodied, and in continual motion, rather than to regard them as formed of dull and heavy matter like the earth. If so—lighter than air of whatever tenuity, lighter than the imagined ether—they might float on the surface of such a surrounding extension of the earth's atmosphere, like the balloon that floats majestically in the air. And, perhaps, now, one may be allowed to speculate thus, as to *how* the universe may be arranged by its all-wise Creator, upon the hypothesis that there are *not* 'more worlds than one.'

"As for the fixed stars, it is not a little surprising that so little modification of former theories has followed the revelations made of late years by our largest telescopes, inconsistent with those theories, which were previously as firmly believed in as universal gravitation and the mechanical laws which

are now supposed to govern the solar system. I confess that to me there have been even prior difficulties to get over in the theories respecting the stars. I have never read anything like a rational attempt to reconcile the apparent alternate advancing and retreating, or increase and decrease in brilliancy, of some fixed stars, with their theoretical position as suns and centres of systems like our own, in the illimitable depths of space. Nor could I ever satisfy my mind that, whatever their distance from us, their own relative distances from one another—as stars of different magnitudes—and especially when a star of the first magnitude is apparently near to one of the smallest—the one being millions of millions of miles further off than the other—would not be apparently different when viewed from one side of the earth's orbit and from another. That that orbit is 'as a point' merely, compared to the distance of those stars (which is necessary to be believed in order to be satisfied that their relative distances would not alter in the least to our view), is also to my mind one of the mere mysteries of science which seem to be professed rather than believed, or professed to be believed, as a kind of poetry, rather than a rational faith, contrary to understanding. Indeed, both the magnitude and magnificence of the earth, as well as of its orbit, appear too little considered, and altogether underestimated, in contemplating the imagined, but scarcely more than arithmetical, sublimity of the system of the universe, according to our current philosophy. And yet there is a meagreness in the solar system upon that hypothesis, compared with that which regards the earth as a centre, placed there as the habitation of man, God's chief creation, and surrounded by sun, moon, planets, comets, and stars, all to serve for the adornment and use of earth alone, as man's temporary abode, and for the glory of the Great Creator; while all beyond may be imagined the heaven of heavens, illumined with the everlasting light and presence of the Eternal God, surrounded with angels and beings of a higher order than man is now, and with the glorified spirits of men raised to a state of superior existence; where there is and can be no more death, or any of those moral or physical evils which are alike the curse and paradox of man's present state of existence, and which mar the fair face of creation."

The CHAIRMAN.—I am sure we must all give Mr. Reddie our thanks for his paper, whatever may be our opinion as to the truth or validity of his arguments. At the same time, this is an institution where we wish these subjects to be freely discussed; and at a time when all we believe to be holy and all that is dear to us is met with the utmost degree of scepticism, it is only right that there should be some who will show that there are some reasons for regarding with scepticism even that which is supposed to be founded on what is believed to be the highest kind of demonstration—mathematical demonstration. I shall be happy to hear what any gentleman has to say on the subject, and invite the fullest and freest discussion.

CHARLES BROOKE, Esq., F.R.S., V.P.—After the very long period of time

which the reading of this paper has occupied, it is utterly impossible for me to follow out all the numerous points which Mr. Reddie has gone into; indeed, if I were to attempt to do so, I should keep you here until to-morrow morning. I will therefore beg your attention to some few remarks which I wish to make, and the rest must be left; premising, however, that whatever is omitted to be answered, is not omitted because I think it unanswerable, but because it is impossible to take up your time with the answer. In the first place, I would beg leave to suggest to Mr. Reddie that scientific conviction and scientific prejudice are two different things. I have the fullest conviction of the truth of the astronomical theory and the law of gravitation as commonly accepted. My judgment, applied in the best way in which I can apply it to the facts that are capable of observation, has been convinced that these theories are true; and I trust that before I have concluded I shall have, in some degree, led you to suspect that scientific conviction is on our side, and scientific prejudice is monopolized by Mr. Reddie. (Laughter.) I dare say, ladies and gentlemen, you have often heard the story of the juryman who was never placed upon a jury but he invariably found that he had eleven obstinate men to contend with. (Laughter.) With regard to some of the preliminary observations of Mr. Reddie, I may say that I happened to be at the meeting of the British Association to which he alluded—and I have always been rather an active attendant at section A—and I beg leave to inform him, with all due deference, that his paper in 1862 was not declined because the Newtonian theory was attacked, but because it was the opinion of the committee of that section that the attack was really not worth defending, and because we did not feel disposed to be accessories before the fact to Mr. Reddie's following the plan of the "Derby Ram," in *Punch*, running his head against a wall. Now, with regard to the problem of the three bodies, Mr. Reddie has alluded to the strange assumption—that is the lunar theory—that the calculation is based on the supposition of the earth being at rest, and the moon moving round the earth, and the apparent motion of the sun round the earth. With regard to all such points, it may be said that there are many physical facts, amongst others, the actual motion of the moon in space, that are beyond the reach of mathematical analysis. In order to reduce the lunar theory to a differential equation it is necessary to assume that the earth is at rest (hear hear), and that the apparent motion of the sun round the earth is a real motion, and that the apparent motion of the moon is a real motion also. And I maintain that that or any other hypothesis is legitimate unless it can be shown that the effect of that hypothesis invalidates the results which are ultimately arrived at. Because, with regard to the motions of the moon, you are first obliged to suppose that the moon is influenced by the attractions of the planets, and an immense variety of mechanical circumstances, which you cannot put into the calculations all at once. You are obliged to assume that some of them do not exist; and having attained the result which the analysis can bring you to, it is then necessary to ascertain the alterations which it is necessary to introduce, in order to take in the

other considerations which have been neglected. All this is somewhat tentative, but the actual process is the only one by which, with our present mathematical resources, the results which have been obtained can be arrived at. And that is the reason why in the problems connected with the lunar theory it is necessary to assume in the first instance that the earth is at rest. Now I believe that a great many of the misconceptions—as I believe them to be—which Mr. Reddie has entered into, would be entirely removed if he could only satisfy his own mind of the fact, of which my own mind is perfectly satisfied, that in considering the motion of bodies, it is immaterial whether any other motion which they may have at the same time is taken into consideration or not. I can explain myself better by an instance. The motion of the moon round the earth would be the same whether the earth is supposed to be in motion or at rest; and the action of gravitation takes place upon a body just in the same way whether that body is at rest or in motion. I could give experimental illustrations of this *ad nauseam;* but I will confine myself to one simple illustration. Suppose I have two similar balls, and I project one forward horizontally, while at the same time I let the other drop; it will be found that the action of gravitation upon these two balls is precisely the same: they will reach the floor precisely at the same instant. That simple experiment proves that the action of gravitation is the same in both cases——

Rev. Dr. IRONS.—Will you explain whether you mean that this would be the result whatever amount of force was used to project the ball?

Mr. BROOKE.—Whatever the amount of force—if it were projected a thousand miles.

Mr. REDDIE.—There is no issue between Mr. Brooke and me on that point, though I don't admit that his reasoning from that fact is valid.

Mr. BROOKE.—I merely adduce that as evidence that the obedience of a body to the law of gravitation is not affected by its having any other motion at the same time. So in the same way the obedience of the moon to the law of gravitation, which brings it towards the earth, is not affected by the consideration whether the earth be in motion or at rest at the same time. But the necessity for simplifying the considerations of motion might be illustrated in a variety of different ways. For instance, suppose that we take the case of a steam-vessel. It may at the same time be going under steam in a certain direction, and it may have a wind, constant or variable, blowing upon it, which would tend to drive it in another direction. If we wished to investigate the circumstances in any particular part of the machinery or point in circumference in the wheel or screw, how should we proceed? We should not begin by taking into consideration the path in space in which this point we wish to consider is travelling in obedience to the propulsion of the vessel itself, and also in obedience to the wind, and in obedience to the impulse of the steam-engine; but we should simplify our considerations by supposing the vessel to be at rest, and by considering simply the relation of the motion of the point in the wheel to the direction in which it is driven, and afterwards we might add the compound motions to

the result. Then we may suppose a watch placed upon a table, and suppose it to be placed on a revolving table which happened to revolve in a contrary direction to the motion of the hand of the watch. What would be the result? The hand of the watch would remain in the same direction. We should not, therefore, interfere with the motion of the watch in going. And if we wanted to consider the relation of the motion of the hand to the machinery that drives it, we should not take into consideration the compound motions by which the hand appears to remain at rest; but we should simply consider the motion of the hand in obedience to the mechanical force of the watch which drives it. With regard to Newton's principle of circular or elliptic orbits, the same observation will apply. It is perfectly true that the moon does not describe an orbit, circular or elliptic, but describes a wavy line round the earth's orbit round the sun; but in considering that orbit the difficulties are simplified by considering the earth to be at rest. And it appears as the result of observation that the results deduced from such a supposition are not vitiated by the circumstance of the earth being in motion. Mr. Reddie alluded in his paper to motion in a resisting medium, and he referred to the confusion which would arise from such a supposition. Now the fact is, that the resistance of the medium which is supposed to pervade all space is the means of transferring the vibrations of light and heat from the sun and other sources to us, more especially from the sun to the earth. That it is a resisting medium has been proved by the retardation of Encke's comet; but the reason why it affected Encke's comet was that it consisted only of a mass of vapour, and was so light and attenuated as to feel the least resistance. The earth or planets, being immeasurably heavier, are not affected at all. The motion of the earth in its orbit I believe to be wholly unaffected by the existence of ether, that elastic medium which pervades all space. Not because there is no resistance, but because it is so minute in comparison to the magnitude and weight of the body in motion. I might illustrate it in this way. If you exercise the slightest puff of breath upon an air-ball that a child plays with, you alter its course; but would that afford any ground for supposing that, if you had the courage to stand near the mouth of a cannon, say a six-hundred pounder, from which a shot was about to be fired, and the moment the shot was passing out you gave it a puff, you would have any effect in altering the point at which the ball would strike? Certainly not; and the reason is precisely the same: the force of your breath in the one case and the resistance of the assumed medium in the other were so minute in relation to the mass in motion as to have no sensible effect——

Mr. Reddie.—I have granted quite as much as that in my paper, and reduced my objections to the effect of a resisting medium upon the earth's atmosphere, (§ 27, last 18 lines.)

Mr. Brooke.—I certainly understood that the gist of Mr. Reddie's argument was the assumption that there was a contradiction. He quotes the letter of Voltaire:—"I left the world *full* in Paris, but found it empty in London. In France the earth is believed to be shaped like a melon, but

here it is flat like an orange." That means that in one case the resisting medium was supposed to exist, and in the other was not; and therefore there was confusion and discrepancy. But as regards the motion of the earth or planets, I mean simply that the influence of the resisting medium is inappreciable, and that it only becomes appreciable when it affects the orbit of a body so attenuated as a comet. And therefore the question of the existence of a resisting medium does not invalidate the conclusions drawn with regard to the earth, the moon, the sun, and the planets——

Dr. IRONS.—Would you apply the same remarks even to motion in the *plenum?* Supposing the motion to get more and more intense, would it never be affected: is it so far attenuated that no amount of velocity would beat it?

Mr. BROOKE.—That appears to be another question. I am speaking of the existing velocities in relation to the moon and sun; but probably the attenuation of the medium is such that no velocity which has hitherto been imagined would be in the slightest degree affected by it. Mr. Reddie goes on to say, "If there be really solar motion in space, and if there be a resisting medium, through which all the heavenly bodies must move, there is not a single demonstration in the *Principia*, whether sound or fallacious, which is in accordance with our 'current physical astronomy'; and no conclusion at which Newton arrived by 'demonstration' in his 'immortal work' is now really accepted by modern astronomers." There I entirely join issue with Mr. Reddie, because, as I have already said, I believe that the resistance of the assumed medium is so minute that it will not affect any of the deductions of modern physical astronomy, and therefore will not affect their relation to the demonstrations or anything else in the *Principia*. Mr. Reddie then says, "The 'revolving body' is supposed to move in free space, 'void of resistance,' and the areas are described 'in one immovable plane'; and it is to these two points I especially desire to direct attention." And, again, "In the last of the corollaries it is said, 'The same thing holds good when the planes in which the bodies are moved, together with the centres of force, which are placed in those planes, are not at rest, but move uniformly in a right line.'" And this, he then remarks, is "an astounding corollary." But it is not astounding at all. Unless it can be shown that the results deduced from this hypothesis lead to conclusions which are at variance with the fact, there certainly is nothing astounding in the hypothesis as at present assumed——

Mr. REDDIE.—A corollary is generally something obviously deduced from what has been previously demonstrated; and I say there never was such a corollary as this in any strictly mathematical work. You will find no such contradictary corollary in "Euclid": first proving that the thing is true in one way, and then assuming that it is all the same if it is supposed to be quite another way!

Mr. BROOKE.—The object of omitting a consideration in the first instance is to simplify the matter to be examined, and unless it can be shown that the neglect of that consideration would lead to an erroneous conclusion it appears to me to be perfectly legitimate. Mr. Reddie says, "So, then, we are to

believe that it would be 'indifferent' if we were to start the sun off in a right line at the rate of 65,000 miles an hour." No; by no means. Nobody supposes it would be indifferent if, the sun being now at rest, and the existing motions of the heavenly bodies being what they are, we were now to start the sun off at the rate of 65,000 miles an hour. That is a very different question. But it is of no consequence, and will not affect the results obtained, if we suppose that the whole solar system is moving conjointly at the rate of 65,000 miles an hour, or at any other rate. (Hear, hear.) It would make all the difference, however, if you were now to start the sun at that velocity, and not start the other bodies in the same direction——

Mr. REDDIE.—I am letting you suppose that they have all been going together, but say that they couldn't do so by the same forces as when the sun is at rest, which was what Newton supposed. (See § 15, line 24.)

Mr. BROOKE.—Then if they are all supposed to go together, I will simply say that I believe firmly that it would be quite indifferent. I am obliged, of course, to omit a great many points; but in § 19 he says, "It is part of this teaching that stars of the second magnitude, that is, stars only less bright than Sirius, must have been shining in the firmament for twenty-eight years before they were visible on the earth; and that the smallest stars visible to the naked eye must have been invisible for 138 years. The converse absurdity has also been taught, that if such stars ceased to exist, they would continue as visible stars to earthly eyes and telescopes for twenty-eight years and 138 years respectively after their non-existence!" Now, I simply mean to say that there is no absurdity in this at all; and I will in a few words reduce it to his comprehension. Did any one of you ever see a stone dropped into the surface of a still pond and notice the effect? You see some little waves—some little undulations travelling off; but after a very short time the point where the stone was thrown in becomes absolutely at rest. But there the little batch of waves goes travelling on and on to an indefinite extent according to the extent of the lake or sheet of water. It may become less and less visible, but it is still visible to a great distance. Again, I would ask Mr. Reddie, did he ever hear an echo? If a short sudden sound is made, as by a whistle or the blowing of a horn——

Mr. REDDIE.—I was discussing not sound, nor even light, but sight. Sound or light can go round a corner; but you cannot *see* objects so. (Hear, hear.)

Mr. BROOKE.—Wait for my point. What takes place in that case? A batch of waves is sent off through the air; these waves strike an object at a distance; they are reflected at that distance and come back again to you, and that batch of waves then produces upon the ear the impression of sound. Now the case is precisely analogous with regard to light. No one that I am aware of doubts in the present day that light consists of undulations, of vibratory motion of matter of some kind. If that be the case, it is just the same with regard to sound or waves on the surface of a pond. If a body is luminous it has the power of setting in motion these undulations; and,

supposing that power to cease, you will then have a batch of these undulatory motions travelling on and on from the source of light until they reach the eye, and produce upon it the sensation of light. But it is a matter of perfect indifference whether the cause of those undulations has in the mean time ceased to exist; for, the undulations having once been excited, will travel through space until they reach the eye, just as the sound undulations will travel through the air, or the waves through water. I therefore say that there is no absurdity at all in the supposition that light may reach the eye after the star or heavenly body that emitted it has ceased to emit light—or, I will say, ceased to exist; but we know nothing of its existence except by the light——

Dr. IRONS.—Light is a vague word.

Mr. BROOKE.—The impression we derive from seeing a star at any particular moment is just the same whether the star emits light at that moment or not. The star cannot affect the undulations after they are emitted——

Mr. REDDIE.—May I ask this question: If you are right, how is it that the most distant stars dip below the horizon, just as the moon does, and do not continue to exhibit themselves long afterwards?

Mr. BROOKE.—It is simply this. The stars dipped below the horizon long before we cease to see them. They may have dipped below the horizon days or weeks before——

Mr. REDDIE.—Days or weeks! If a star, say of the sixth magnitude, sank now, should we not cease at once to see it?

Mr. BROOKE.—Certainly. The undulations were travelling from the star to us, and at length the star is in such a position that the undulations in that line of light no longer reach our eye, and therefore we cease to see the star. (Mr. REDDIE: Hear, hear.) The star itself will have gone below the horizon long before. That light travels at a certain known rate is established by facts which we know astronomically, and the results which have been obtained with inevitable certainty appear to me to be *post facto* demonstrations of the truth of the theory; because, if light had not travelled at that velocity many ascertained astronomical results which have followed from the assumption of that velocity would not have been obtained——

The CHAIRMAN.—I know that Mr. Brooke is so well acquainted with the subject that he can inform us whether means independently of astronomical observation have not been employed to prove experimentally that light does take a definite time to travel?

Mr. BROOKE.—Oh, yes, there are many other means——

Mr. REDDIE.—I have read all about the experiments you refer to; but formerly they were said to "prove" that the velocity of light was 192,000 miles per second, and now it is said they prove it to be 185,000 miles only per second. (Hear, hear.)

Mr. BROOKE.—I will now pass on to the last point to which I wish to allude. An observation was made with regard to the third law of motion. Now it is quite true that in my little work I have expressed the third law of motion in different terms from those used in the *Principia* of Newton; but

I did that simply because I thought the terms I used were not open to misconception. I think the terms in which it is expressed by Newton are capable of some misconception; but the drift of the law is precisely the same in either case. Now, if I have rightly understood him, I will just make a few observations in order to convince Mr. Reddie that he is making a distinction without a difference. Suppose I tie a string to a wall, and pull it with a force of twenty pounds with my hand, the wall pulls my hand backwards just with the same force that I pull the string from the wall; but that proposition Mr. Reddie denies. (Laughter.) Well, Mr. Reddie, no doubt, will allow that if, instead of tying my string to the wall, I pass it over a pulley with the weight of twenty pounds suspended, and I pull the string, the weight will pull my hand back——

Mr. REDDIE.—In that case, I say there would be a distinct reaction from the weight; and if you ceased to pull, the weight would descend.

Mr. BROOKE.—My object is to show that the reaction is the same in either case. Suppose that, iustead of tying the string to the wall, I attach it to a spring, and with a force of twenty pounds draw it from the wall. Mr. Reddie, I assume, would admit that it pulled my hand back?——

Mr. REDDIE.—I have noticed that kind of reaction, which is perfectly real. The spring acts like the weight.

Mr. BROOKE.—If I double my force of pull I shall only pull the spring out half the distance, and as I increase it the spring will become so strong that I cannot pull it out at all, until at last it becomes a part of the wall. In that case the reaction is just the same as in the other case; and I would ask Mr. Reddie to say where he would stop. He admits that a suspended weight reacts upon my hand, and a spring also; I therefore ask where the reaction ceases?——

Mr. REDDIE.—Wherever there is no elasticity, or pull, or spring, in the opposing force; wherever you have rigidity. Take, for instance, a horse drawing a stone. If you brought the string over a pulley, of course the weight of the stone would pull back the horse, if the horse did not keep up the tension. There is then a distinct reaction, but you know the cause of it.

Mr. BROOKE.—As the spring becomes stronger and stronger the hand will be drawn out less, until at last it is not drawn out to an appreciable degree. Again, I will go further, and assume that I did pull the wall out—that the wall bends to some indefinitely small extent—a millionth part of an inch, say. I dare say Mr. Reddie may dispute the possibility of that, but I have no doubt that if one could only put a rope round the top of Eddystone lighthouse, and pull it out with sufficient force horizontally, one would be able to sensibly bend the whole lighthouse; and when you released the rope it would go back again. Where, then, does the reaction cease?——

Mr. REDDIE.—It is not equal to the action. That is my point. I have never denied resistance in such a case; but reaction and "equal reaction."

Mr. BROOKE.—But I think I have shown that it is always equal. When I pull the spring out, the reaction of the spring is equal to the pull I put upon it——

Mr. REDDIE.—No, no; if equal, you could not have pulled it out; and if the wall continued rigid, there would be no spring or reaction.

Mr. BROOKE.—But if there is no reaction when the string is fixed to the wall, I want to know where the reaction ceases. It is a distinction without a difference. (No, no.) I would only say, as a last remark, that nothing has ever impressed my mind with the conviction of the truth of the law of gravitation more strongly than the projection of eclipses; in which, basing your calculations upon the law of gravitation, you can, months beforehand, state the time to a second, and the spot, within a small space, where the eclipse will occur. But the discovery of the planet Neptune from the disturbances of Uranus was a still stronger proof. I may mention that the planet Uranus was observed to have certain disturbances in its orbit motion in an unaccountable manner. M. Le Verrier, of the Paris observatory, and Mr. Adams, in this country, set themselves to discover where and of what magnitude a body must be which could, by its attraction, affect this disturbance, and they both came to very nearly the same conclusion as to place and magnitude of this body. M. Le Verrier communicated to another French astronomer where he supposed some body must be, and he looked for it in his telescope and found it. The result arrived at by our own astronomer, Mr. Adams, were unfortunately for a time laid by, and we in this country lost the merit of the discovery of the planet Neptune. But it was described inductively from assuming the law of gravitation to be correct, and finding where the body according to it must and ought to be placed in order to produce such disturbances; and there the body was found. Nothing can convey a stronger conviction to my mind than such facts as these that the theory of the law of gravitation is substantially true, and that the principles advocated by Newton are also substantially true; and that some of the difficulties which Mr. Reddie has laid hold of are only difficulties which have been necessarily introduced into calculations founded on these grand principles in order to bring the facts within the scope of exact analysis. (Applause.)

Admiral FISHBOURNE.—I will detain the meeting only a few minutes while I refer to what Mr. Brooke has said about the effect of a breath of wind on the direction of a ball fired from a cannon——

Mr. BROOKE.—Excuse me; a man's breath, not a breath of wind.

Admiral FISHBOURNE.—That is only a difference of degree. Now, it has been established, by means of a very elegant instrument, that a round ball projected with only a limited velocity as compared with the motion of a heavenly body, rotates, and, because of its rotating in its progress, one side of the ball is receding from the wind and the other is approaching it; and the result of that is that the difference between the action of the air on one side and on the other deflects the ball in its course. If that is the case even in our atmosphere, though it is not so attenuated as the medium the heavenly bodies traverse, there can be no doubt there must be a difference in their velocities, when moving in a plenum and a vacuum——

Mr. BROOKE.—Of course, every rifleman knows that it is necessary to

allow in taking aim for the effect of the wind upon the ball; it is a question of resistance.

The CHAIRMAN.—No, it is not a question of resistance; it is the very reverse.

Mr. BROOKE.—It is, indeed.

The CHAIRMAN.—It is a different thing altogether. I think Mr. Brooke has mistaken Admiral Fishbourne's point. It is this. We are not dealing with a circular ball, but with a bolt. The wind acts upon it more on one side than on the other as it is revolving on its own axis, and the consequence is that it is deflected from a straight line, not by the resistance of the air, but by the effect of the wind upon it. It is not a case of resistance, but of deflection, which is a totally distinct thing. (Hear, hear.)

Admiral HALSTED.—It is shown by the experiments of Mr. Glaisher that at the extreme point of the atmosphere from the surface of the earth it is very attenuated; so that, upon the upper surface of the atmosphere, we should get a medium scarcely more dense than Encke's comet itself. I merely mention the point with regard to the effect it would produce.

Mr. BROOKE.—The fact is unquestionable that the density of our rarest atmosphere is so great compared with the density of the ether, that the moment one of those shooting stars enters the confines of our atmosphere it becomes red hot, and is very soon ignited and burns away; whereas it has travelled indefinitely through ether without being sensibly warmed.

The CHAIRMAN.—That I doubt altogether. (Laughter.)

Mr. BROOKE.—That is my belief.

Mr. REDDIE.—There is no proof of that, of course?

Mr. BROOKE.—No proof at all, but strong inference. But there is abundant proof that aërolites——

The CHAIRMAN.—But that is a totally different thing. I read a paper in which I endeavoured to show that there was no analogy between aërolites and falling stars.

Mr. BROOKE.—I think I have read the paper. (Laughter.)

The CHAIRMAN.—I have few observations to make, except to say that I doubt whether Mr. Reddie is altogether right in the title of the paper he has read, as being in opposition to "current *physical* astronomy." I do not think his paper really touches current physical astronomy at all. A great part of the paper is directed against arguments contained in Newton's *Principia*, but more against mathematical methods made use of by him than those current amongst physical astronomers of the present day. I believe that, in order to attack current physical astronomy, you will have to attack, not the mathematical processes of Newton, but those mathematical processes which have been introduced by astronomers into the present system, which is in the main very different from Newton's. (Mr. REDDIE.—Hear, hear.) His very peculiar kind of geometrical analysis enabled him to solve the problem of the three bodies, but only to a certain limited extent; and it has been conceived by some that had Newton lived, and had more facts of physical astronomy been brought within the range of his vision, probably his powerful

mathematical mind would have enabled him to devise processes by means of which that mathematical system of reasoning could have been improved, and made to bring in facts which are now supposed to be brought into physical astronomy——

Mr. REDDIE.—Do you mean theoretical or practical astronomy?

The CHAIRMAN.—What I mean is this. There is a difference between what is technically called plain astronomy, which deals with the actual, visible motions of the planets, and that which accounts for motions that are matter of theory. This is called physical astronomy, and it consists in the main of two parts—namely, what is called the lunar theory, and which accounts for the exceedingly complicated apparent motions of the moon with respect to the fixed stars; and the planetary theory, which accounts for the equally complicated motions of the planets. Physical astronomy does not go much beyond these two theorems: the apparent motions of the moon amongst the stars, and the apparent motions of the planets amongst the stars; and extremely complicated motions they are. If you traced them on the celestial globe, you would find that they described curves of the most complicated character. It is the business of physical astronomy, on the hypothesis of gravitation, not only to account for these extremely complicated motions but to do more than this: to predict the position of these bodies, and tell where they will be at any future time. The mathematical astronomers were for a long time bigoted to the processes of Newton, and while they were physical astronomy made no progress in this country. (Hear, hear.) Astronomers who were not prejudiced, however, took up the methods of Leibnitz, and the consequence was that they were able to predict the motions of the moon amongst the stars. And therefore, supposing that the whole *Principia* was abolished at once—if it were given up, you have not attacked current physical astronomy, because it does not the slightest degree depend upon Newton's *Principia*, or any proposition in that *Principia*, except the assumption of the three laws of motion. Current physical astronomy is based upon the assumption of the law that Newton determined—namely, that of gravitating bodies attracting one another directly as their masses and inversely as the square of their distances. Take that hypothesis for granted, and combine it with the three laws of motion—not one of which can be obtained from direct experiment, but which are derived incidentally from thousands of experiments, and deduced rather than proved,—and the physical astronomer maintains that he can predict these exceedingly complicated motions, so as to account for the positions of the heavenly bodies with an extreme degree of accuracy. The question is, can the physical astronomer do so? You can attack him in two ways. You can show that his mathematical analysis is unsafe, and not fit to be trusted; or you can show, which is still more important, that he cannot calculate these things beforehand—that observation does not agree with his theory. I think that is the way in which current physical astronomy is to be opposed, and not in the manner Mr. Reddie has done. For if he has done anything at all, all that he has done is to oppose Newton's mode of demonstration and mode of reasoning in the *Principia*.

I do not think that it has at all interfered with the facts of physical astronomy; but at the same time one cannot help feeling that there is a great deal to be said on the other side. I think considerable light is thrown upon it by the well-written and cautious paper published in the name of a Cambridge "Wrangler." The question is well put, not whether the law of gravitation is true or false, but whether we are to adopt the Copernican or the Ptolemaic theory. It is generally assumed that the theory of gravitation can only be supported on the Copernican theory. What do physical astronomers do in the celebrated problem of the three bodies? As to the lunar motion, they assume that the earth is in the centre, with the moon moving round the earth nearly in a circle, and the sun also moving round the earth at a certain distance nearly in a circle (Mr. REDDIE.—Hear, hear); and then they apply the differential calculus to get a differential equation, which assumes the three laws of motion and the law of attraction —of gravity. Thus they get a differential equation, which they cannot solve (hear, hear), and then, by various extremely clever devices, and a successful series of mathematical dodges, they get at—not the real motion of the moon, because they take the earth as the centre — but the apparent motions of the moon, as seen from the earth. (Hear, hear.) But, supposing the earth to be perfectly still, and the moon moving round it, the theoretical path of the moon is not an ellipsis, and not any known curve; and, moreover, it is not in any one plane, but in a plane which is constantly in a state of oscillation. Thus you get for the motion of the moon one of the most complicated curves that the mind can conceive. But why did astronomers reject the Ptolemaic theory and accept the Copernican? Because the latter was supposed to give the simplest possible motions. But modern physical astronomy gives us motions of such an exceedingly complicated character, that the argument of simplicity does not apply to the present system any more than to the Ptolemaic. Then Mr. Brooke very pertinently said that one of the greatest proofs of current physical astronomy was its power of predicting eclipses and the moon's motions. To my mind, one of the most astounding things is that little nautical almanac, in which you have the moon's position calculated years before. Now, does that agree with the theory, or does it not? You will say that if it does agree with the theory it will prove the modern theory to be true, and not the Ptolemaic. The Cambridge "Wrangler" says it does not do anything of the kind. Your mathematical analysis has been based upon the assumption that the earth is standing still. (Mr. REDDIE.—Hear, hear: that is just my argument.) You have calculated all these motions upon an hypothesis which is as likely to be true as the converse; so that anything that you prove with regard to the motions of the moon, or with regard to eclipses, can be held to be equally true, whether you take the current system of belief of the sun being fixed in the centre, with the moon rotating round the earth, and the earth round the sun; or, suppose the earth to be fixed, and the moon and sun rotating round it. (Hear, hear.) Therefore you get no direct proof from the lunar theory of the present system of current physical astronomy. But the Cambridge

"Wrangler" has altogether left us in the dark as to the mode of accounting upon his own system for the exceedingly complicated motions of the planets; and I think that the strongest possible confirmation of current physical astronomy is that the planets are not only moving round the sun, but occupying those positions in the atmosphere which they would do according to the current hypothesis. I do not see that Mr. Reddie has refuted in any way the differential equation, or the solution of it, which gives you these motions. There are two ways of attacking this theory. I do not think we have anything to do with what Newton said. The way to attack it is to show first that the mathematical analysis is not true; and you may possibly be able to do that, for I do not know that it is impregnable. It requires an enormous amount of faith to digest the differential calculus; but, when you have digested it, it will account for myriads of phenomena amongst the heavenly bodies. But, then, it is fair to state beforehand that the whole of this is not so much matter of demonstration as it is supposed to be. (Hear, hear.) After all, you put into it all sorts of disturbing calculations. You say you will begin with the three bodies, but by-and-by you take one out to put another in, so that there is always a little "tinkering" and a little finding out that something has been neglected which ought to be taken into consideration. (Hear, hear.) Then, with regard to the greatest triumph of the mathematical planetary theory—the discovery of Neptune,—Mr. Reddie brought the matter before this society, supported by the authority of astronomers of eminence in America, and said that there was the greatest possible discrepancy between the elements of Neptune as calculated by Le Verrier and Adams, on the perturbation theory, and the elements as calculated from observation since, by Mr. Walker. Therefore I think that, as a matter of abstract science, we cannot assert that the discovery of Neptune has demonstrated the theory of gravitation.

Mr. Brooke.—I cannot argue that matter without the data. The calculated orbits might or might not correspond, but that would not invalidate the fact that the position of the body causing the disturbing influence was first assigned and then found to be in the place assigned to it. That fact is not impugned.

Admiral Halsted.—I wish to ask Mr. Brooke a question of professional interest as to the length of time it takes light to travel to the earth. For instance, I get the meridian of the sun at noon. Now, is the light which I get into my sextant actually then proceeding from the sun, or has it proceeded from the sun long before?

Mr. Brooke.—Eight minutes previously.

Admiral Halsted.—With regard to the question of the stars going out when they go down, say I have been taking my observations of a particular very distant star, and it has gone down. On the following night I pick it up for the same purpose. I look out for that light again. Is there a special law with regard to that? What is the distance of time by which I ascertain exactly the variation between the light which I use and that which has left the star?

Mr. BROOKE.—It is easy to calculate the time which the undulations of light emitted from a particular star take to reach the earth.

Admiral FISHBOURNE.—Supposing the light has been travelling in space for one hundred years, and he wants to get the position of the star at the moment when he takes the observation?

Mr. BROOKE.—The position of the star bears the same relation to the earth and the moon and surrounding objects as it did yesterday or a hundred years before.

Dr. IRONS.—I think it is important that we should know whether the calculations with which the public mind is familiar—those which produce a knowledge of eclipses—are really to be made on the old Ptolemaic theory.

The CHAIRMAN.—I do not think Mr. Brooke or any other gentleman can contradict my assertion. Our books on physical astronomy are open to everybody; and you will find that, so far as the lunar theory is concerned, it is calculated according to the Ptolemaic theory. (Mr. REDDIE,—Hear, hear.) All our mathematical demonstrations of the lunar theory go upon the assumption—the convenient assumption—of the Ptolemaic theory. The planetary theory, however, assumes the sun as the centre of the system, and gives the strongest probability to the Copernican.—I now call on Mr. Reddie to reply.

Mr. REDDIE.—In the first place, I must observe, with reference to the criticism upon the title of my paper, that I differ, of course, from you, sir, with great deference, and very unwillingly. But still I must defend the title of my paper. According to all the books on astronomy with which I am acquainted, what you have been speaking of as physical astronomy is usually called "practical astronomy." Leaving this, however, I am extremely obliged to you for what you have said in answer to some of the remarks of Mr. Brooke, especially as to the calculation of eclipses. But you have not answered him completely. Eclipses were calculated not only long before Newton's time, but before Copernicus, and I might even say before Ptolemy, in Egypt, India, and China. Long before they were known to the observations of astronomers in this country or in Europe, they were known to the astronomers among the ancients; and eclipses were not only accurately calculated, but critical chronology actually rests upon those calculations and observations. As to the modern mode of making such calculations, of course I am extremely obliged to the Chairman for so completely answering the first part of his speech by his later remarks. (Hear, hear.) He has told us of the extremely convenient "devices" or "tinkering" which are had recourse to, and it is no doubt extremely convenient, when you are out a little in your theoretical calculations, to be able to add something to make you right. And, in fact, this is an admission that these calculations prove nothing, being vulgarly what we call "dodged." But I venture to say that the main points I have thrown out in my paper for discussion have not been really met. (Hear, hear.) Mr. Brooke has chiefly noticed what may be regarded as merely incidental points, which, for that reason, I almost now regret I introduced into my paper. And yet they are of importance in their proper place. As regards action and reaction——

[Mr. Reddie here proceeded, with the aid of diagrams on the black board, to give illustrations in reply to Mr. Brooke. He afterwards continued]—
I must, however, give a still better answer than this to Mr. Brooke's arguments in defence of action and reaction this evening, by quoting from the 5th edition of his own very valuable work on *Natural Philosophy*, where you will find he has said almost as much against it as I have myself. He says :—

"The Third Law of Motion has sometimes been expressed by the terms 'action and reaction are equal, and in opposite directions;' which have been abandoned, from the difficulty of assigning any definite meaning to the terms action and reaction" (§ 200).

Well, then, if this is Mr. Brooke's own deliberate verdict, or rather testimony, against these terms, you need not be surprised if he failed to give us a very distinct explanation in defence of them now. Of course, as the preamble to my paper itself will show, I am quite aware that it was not to be expected that views so "heretical" and opposed to current opinion could be at once accepted: I was not even unprepared for a few jokes; and I am really only sorry that my arguments have been so vaguely met. I beg to assure Mr. Brooke that I did not mean that the prejudices which I know are opposed to me, are not supposed to be based on conviction. In my paper I say they are not only supposed to rest upon the demonstrations of the *Principia*, but to have the "decision of time"—meaning experimental verification—in their favour. One, and the grand illustration of this, was the discovery of *Neptune*. Well, as to this, the facts are on record in our *Journal*.* I appeal to those facts, when properly understood and weighed. I may also say as regards the rejection of my paper at Cambridge in 1862, that in my account I give the reason for its rejection given to me by Professor Clifton, Secretary to Section A, the first morning the Committee sat, when, perhaps, Mr. Brooke was not present. I published that account immediately, and it was never questioned by Professor Clifton, nor till now; and I can only conclude that at this interval Mr. Brooke has forgotten what really occurred. As regards the transmission of light from the stars, and Mr. Brooke's replies to Admiral Halsted's very pertinent and important queries, I can only say I am content to leave what has been said to-night, and what I have said in my paper, for further reflection. What I have said I know is startling; but it is only so because, unfortunately, we have got accustomed to the much more startling ideas put forward to us in the name of science, which we have too credulously believed, but which I venture to denounce as merely and grossly absurd. With regard to Mr. Brooke's illustrations of a watch going round while carried along, all that he said is perfectly true of motions when bodies are attached mechanically to one another. Mr. Brooke will find that fully admitted and dwelt upon in the paper "On the Motion of the Moon," appended to that which I read this evening. There I fairly meet Newton's

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* Vol. II. pp. 69—71.

and Ferguson's mechanical illustrations, and show their inapplicability, by supposing (instead of motions in the cabin of a vessel) you have two separate steam-vessels attached to one another by a rope; and first suppose one of them to be at rest, and the other to steam round it at the rate of two knots an hour; that will give the string a certain tension which may represent gravity; but if you start off the stationary vessel at ten knots, what would be the effect? It would then drag the other after it, the tension on the rope only being lessened *pro tanto* by the two knots at which the other was steaming. But in order that the latter should now steam round the former as before, it must sometimes steam twelve knots an hour in the same direction; and even when *appearing* to go the other way, it must be steaming eight knots, and still in the same direction. But I say that you could not have two free bodies thus held together by attraction—one going steadily at ten knots, and the other sometimes at twelve knots, sometimes at eight knots an hour,—and that there is no attempt at demonstrating anywhere that such a thing is dynamically possible. If the bodies were mechanically attached by ropes or rods, that would be another matter, though even then you would require a "law" other than attraction to explain these greatly varying velocities. Therefore I say the moment you adopt the theory of solar motion in space you upset Newton's *Principia*. But Mr. Brooke has not alluded to the fact that the Astronomer Royal himself has now given up this theory. And it is no answer to the objections I have urged against current physical astronomy to say that I furnish you with no theory to take its place. I might rather take credit for that. And, at any rate, you cannot believe a thing which is proved to be untenable, merely because you cannot properly account for the phenomena in some other way. It is better and simply honest in such circumstances to say that we do not know, when in sober truth we are in ignorance.

The CHAIRMAN.—So far as I have read Mr. Reddie's works, he has answered the popular explanations of such men as Airy, Herschel, and others, rather than the purely scientific part of the question. The interpretation of the differential equation is of that kind that it is impossible to bring it before the popular mind except by rough illustrations. The popular lectures on gravitation by Airy, are just an attempt, by a rough kind of illustration, to give some kind of idea of what would be the motion of the heavenly bodies according to Newton's system of gravitation. There is no rigid demonstration in them. Men like Michell have simply copied what was written by Airy and Herschel. As I have already said, the way in which physical astronomy is to be attacked, is either by showing that the differential equations depend on unsound assumptions, and that the calculations made by their aid, of a series of complicated phenomena, are not to be relied on; or else that those complicated phenomena do not agree with mathematical demonstration, or that they can be explained in some other way.

Mr. REDDIE.—It is too late now to renew the discussion, and I was not prepared for a second attack after having made my reply. I beg to be

allowed to say, that I have pointed out "unsound assumptions," such as assuming the earth to be at rest, in the lunar theory, and the sun in the planetary theory; and I appeal to my paper to show that I have scarcely quoted Professor Airy's lectures at all, and only to show that in them he declared that every astronomer did then believe in solar motion in space, which is now given up, or considered as in "doubt and abeyance." And I refrained in my paper from quoting from Michell's somewhat sensational work,—though in such works you really get the frankest confessions of such extreme absurdities as I have noticed, as to stars taking hundreds or thousands of years to become visible, or remaining equally long visible after extinction,—but I know that the same things are really to be found in books of men such as Herschel and Airy, who are properly responsible for them.

The discussion then terminated, and the Ordinary Meetings were declared adjourned till next Session.

[Having cited the pamphlet of a "Wrangler" (pp. 406, 407, and 422), it is only fair to publish the following letter from him, which will, no doubt, be read with interest:—

"*March* 21*st*, 1870.

"SIR,—I find from Messrs. Longmans that I have to thank you for a copy of your Paper read June 21st, 1869, at the Victoria Institute.

"I have already read it through, and find it extremely interesting; and I see that there is much which deserves *very* careful consideration. If after reading it *thoughtfully*, I find anything suggested to my mind which would help to develop the ideas contained in it, I shall take the liberty of sending you a few lines.

"As you do me the honour to notice my pamphlet favourably (*The Theories of Copernicus and Ptolemy*), I would add that I see reason for modifying some of the views as they are expressed in it; but I am quite convinced that modern astronomy is tottering, and is based upon many groundless assumptions. My experience of Cambridge is that sound mathematicians, who have considered this particular branch of science, are inclined to admit this,—at least those who are free from the trammels of certain modern societies, whose object now-a-days seems to be, not to elicit truth, but to keep people's minds in darkness.

"Renewing my thanks, I have the honour to remain, &c."]

NOTE A. (§ 21.)

THE VELOCITY OF LIGHT: ITS "ASTRONOMICAL DATA" AND "EXPERIMENTAL PROOF."

The following correspondence may be read with interest, especially considering that another edition of Mr. Chambers's *Handbook* has recently issued from the Clarendon Press, Oxford :—

"Royal Institution of Great Britain,
"*November* 21*st*, 1863.

"SIR,—My attention has this day been directed to your *Victoria Toto Cœlo*, in which (p. 48) you do me the honour of a reference to my *Handbook of Astronomy*:—

"'How often have we been assured of the "certainty" and experimental confirmation of the old 192,000 miles per second as the velocity of light. (*Vide* Airy's *Lectures*, Worms's *Earth and its Mechanism*, Chambers's *Handbook of Astronomy*, &c., *in loc.*)'

"Two courses of comment suggest themselves on reading this passage —a personal and a general one. As regards the former, I think that in citing my remark as an exemplification of your own, you have unwarrantably laboured to make me the object of a gratuitous sneer, which I hereby complain of.

"If you will read again the passage in p. 166 of my book, you will find that I have done nothing but *casually and incidentally* advert to a statement which (though I believed it) it was no part of my province as an astronomer to discuss critically. Any person reading what you have said, without being acquainted with the original, could scarcely fail to infer that I was a dogmatic pleader for the indisputable accuracy of the aforesaid figures; whereas, so far as my opinion was concerned, I said next to nothing on the subject. I shall trust to your candour in a future edition either to modify the passage or to append a copy of this note.

"The general question is one which I can scarcely believe ought to be argued. Surely a physicist may make a mistake as well as any other man, and is entitled to a rehearing when he becomes possessed of more reliable results. For my own part, I entertain a high opinion of the value of Foucault's discovery, and you will find it adopted in my second edition, now in a forward state for issue early next year, the first being all but exhausted.

"I am, Sir, your obedient servant,

"J. REDDIE, Esq. "G. F. CHAMBERS.

"P.S.—If you will point out any *real* errors in my book, you will be conferring a favour equally on the public as on myself."

[*Answer to the above Letter.*]

"Bridge House, Hammersmith, W.
"*November* 26*th*, 1863.

"SIR,—I have delayed answering your letter of the 21st inst, in order to ask some friends whether they could discover anything either 'unwarrantable'

or 'sneering' in the few words you quote from *Victoria Toto Cœlo; or, Modern Astronomy Recast*, where I refer to your *Handbook*. I am glad to say they do not; nor can I. I regret, of course, that you think yourself aggrieved, and shall cheerfully comply with your request to append a copy of your letter in a future edition. I shall also, so far as I can, make the contents of your letter public where my own book is likely to be known.

"I may venture to observe, however, that there is nothing in your work to show that you were not prepared to maintain the accuracy of the so-called 'experimental proof' of the old velocity of light to which you made allusion; and I profess I do not understand on what grounds you can now 'entertain a high opinion' of those experiments—which, as I have shown (pp. 38 and 48 of *Vict. Tot. Cœlo*), have been cited as *proving*, till lately, 'within the 77-millionth part of a second,' that the velocity of light was 192,000 miles per second, and more recently (by Mr. Hind) as *proving* it now to be only 185,170 miles per second! Perhaps you will afford the public some explanation of this in the second edition of your own work.

"As regards your remark that 'the general question is one which you can scarcely believe ought to be argued'! I confess it puzzles me; especially when read in connection with your P.S. In my opinion the greatest injury is done to science, in the present day, by what deserves no other name than scientific credulity; and the general public are, in fact, imposed upon by *quasi* facts in science being readily taken for granted and repeated (merely because 'believed') in books of scientific pretension, by one author after another, without the question of their real accuracy or error being ever argued.

"You could scarcely have read through *Victoria Toto Cœlo* when you wrote, or you would scarcely have asked me 'to point out any *real* errors in your work'! Some months ago I sent you a copy of *The Mechanics of the Heavens* (which, though not acknowledged, was not returned through the Post-office), and it draws attention to some facts, bearing on the general question, which affect the whole basis of your *Handbook*, so far as Physical Astronomy is concerned. When you have read, also, my *Victoria Toto Cœlo*, you will find I have not shrunk from pointing out to the public the innumerable errors and absurd contradictions that are now professed to be believed by astronomers, and which are to be found repeated in your *Handbook*, as in other popular works.

"If, however, you will direct my attention to anything in your book not to be found in other works, and which you *are* prepared to defend as accurate, I shall certainly devote special attention to it publicly, whether it agrees or disagrees with anything I have publicly advanced.

"I am, Sir, your obedient servant,

"GEORGE F. CHAMBERS, Esq. "J. REDDIE.
&c. &c. &c."

"The passage quoted by Mr. Chambers is a foot-note having reference to the words in italics contained in the following extract from Mr. J. R. Hind's letter to the *Times* of 17th September, 1863, in which he states some of the consequences of the sun's mean distance from the earth having been recently reduced from 95 millions to 91 millions of miles;—the principal parts of which letter are given in *Victoria Toto Cœlo;* viz. :—

"'The earth's mean distance becomes 91,328,000 miles, being a reduction of 4,036,000. The circumference of her orbit 599,194,000 miles, being a diminution of 25,360,000. Her mean hourly velocity 65,460 miles [instead of 68,000]. The diameter of the sun 850,100 miles, which is smaller by nearly 38,000. The distances, velocities, and dimensions of all the members

of he planetary system of course require similar correction . . . . in the case of Neptune . . . . about 122 millions of miles. *The velocity of light is decreased by nearly* 8,000 *miles per second, and becomes* 183,470 *if based upon astronomical data alone'* (p. 48).

"The 'astronomical data' upon which the velocity of light has been long given out by astronomers as 192,000 miles per second, are (1st) the diameter of the earth's orbit, depending upon its distance from the sun; which distance is stated in Mr. Chambers's Handbook (as in other astronomical works) to 'have been ascertained *with great accuracy* from the transit of Venus in 1769'; and (2nd) 'the difference in the time of the eclipses of Jupiter's satellites when the earth was at its greatest [and least] distance from Jupiter, namely, 16′ 26″=190 millions miles (diam. of earth's orbit)= 192,000 miles per second.'—(Handb. of Ast. *in loc.*)

"On page 38 of *Victoria Toto Cœlo,* referring to the *instantaneous coincidence* of some bright appearances on the sun's disk with certain magnetic disturbances on the earth (alluded to in Sir William Armstrong's address to the British Association), the following passage occurs:—

"'I would first beg to observe, that we seem to have an indication here, that electric or magnetic forces and light, probably travel with identical velocity. This is important in connection with Professor Wheatstone's interesting experiments with the "rotating mirror" as to the velocity of electricity, afterwards applied by Foucault to measure the velocity of light. Mr. Hind has quoted, in his letter to the *Times* of 17th September, 1863, the results of M. Foucault's experiments as confirming the *reduced* velocity of light, following from the newly reduced diameter of the earth's orbit. He says:-

"'M. Léon Foucault, of Paris, has succeeded in measuring the absolute velocity of light by means of the 'turning mirror,' an experimental determination of no little interest and significance. He concludes that it cannot differ much from 298,000,000 of French metres per second, or 185,170 English miles, which is a *notable diminution* upon the velocity previously derived from astronomical data alone.'

"But some years ago, Professor Helmholtz wrote of these same experiments, *when the velocity of light was believed to be* 192,000 *miles a second,*—

"'We have thus determined in a distance of twelve feet no less than the velocity with which light is propagated, *which is known to be nearly* 200,000 *miles a second;—the distance mentioned corresponds, therefore, to the 77-millionth part of a second.*'

"'At that time, it will be observed, the experiments with the rotating mirror were said to accord with the velocity derived from the then existing 'astronomical data,' *without* any 'notable diminution!'" (p. 38).

"I will only add that there has been a significantly marked silence, on the part of some journals that call themselves 'scientific,' as regards Mr. Hind's important letter to the *Times.* They almost entirely ignored it; and it is whispered that its publication has given offence in 'scientific' quarters. Heaven help us, if 'science' is thus to demean itself in the nineteenth century, in England! But, to quote once more, 'Neither the British press nor public have any vested interest in error.' . . . . 'Unquestionably, science is honoured and credited in the present day, as perhaps no religion, even, ever was. But it should be remembered by 'men of science' that the worship is sincere. What is thus credited is credited *as truth;* and if that is suppressed, ignored, or tampered with, the injury done to true science by those who ought to have been its guardians, will never be forgiven.' (*Vict. Tot. Cœl.*, pp. 41, 51.) "J. R.

"*November* 30*th*, 1863."

NOTE B. (§ 22.)

The Direction of "Solar Motion in Space."

Mayer, it will be observed, could not find that the proper motions of the stars afforded evidence of the motion of the solar system towards any particular region of the heavens; and he therefore rationally disbelieved in such solar motion. The advancement of science in our day has enabled some people to get over such a difficulty with ease. A clever correspondent of the *Times*, who frequently writes on scientific matters, with the initial "Y.," thus wrote in that journal on 15th September, 1863:—

"The whole of the solar system seems to be travelling—some report at the *slow rate* [*sic*] of 47,000 miles an hour—towards an *unknown region* of infinite space."

But the most curious thing that has ever appeared on the subject, has been put out by Lieutenant Morrison, R.N., in his *Astronomy in a Nutshell*, in which he claims to have demonstrated the sun's velocity to be "1,665 miles in a minute, or very nearly 100,000 miles an hour." But he has made even a stranger discovery than "Y." in the *Times;* namely, that this motion is neither towards the left arm of Hercules (as the "orthodox" believe), nor to the "unknown region" of "Y.," but precisely to the west! not considering that *the west*, as a point in space, is even more mythical than an unknown region, inasmuch as it is a direction that is *known to be ever varying!* The direction that is *west* to us at mid-day is—if the earth goes round, towards precisely the opposite point in space at midnight! And, whether the earth rotates or not, when we and our antipodes look towards the conventional *west* at the same time—at this present moment, for instance—we are looking in as directly opposite directions in space, as if we stood back to back! If Lieutenant Morrison's discovery were a real one, it would only be the discovery of the true rate of velocity with which the sun moves westwards *round the earth!*

NOTE C. (§ 23.)

Correspondence with Professor Airy.

Bridge House, Hammersmith, W.
June 6th, 1864.

Sir,—I beg leave, with the greatest respect, to call your attention to §§ 6, 57-63, and pp. 41-45, of the accompanying book (*Victoria Toto Cœlo; or, Modern Astronomy Recast*), relating to the motion of the moon. I venture to do so on three grounds: (1) In the interest of scientific truth, because of your eminent position as Astronomer Royal of England; (2) Because in the book referred to,—the text of which is a paper submitted by me to section A of the British Association last August,—I especially assailed as untenable the notion of the solar system in space; and having been the first and I believe only person who has done so, since the speculation was originally put forth by Sir William Herschel eighty years ago, I hailed with extreme satisfaction, and as a triumphant comment upon the dumb contempt with which my paper was treated by Professor Rankine and the committee of Section A, at Newcastle, the subsequent announcement contained in the last

Annual Report of the Council of the Royal Astronomical Society, and published in the *Monthly Notices* for February last, that—"strange as it may appear," and notwithstanding the recent re-verification at the Royal Observatory of all the parallactic calculations hitherto supposed to justify the theory of the sun's motion,—you, Sir, had arrived at the conclusion, "that the whole question of solar motion in space, so far at least as accounting for the proper motion of the stars is concerned, appears to remain at this moment in doubt and abeyance"; but (3) I now venture mainly to address you, because I am about to write another paper intended to be hereafter published, elaborating more minutely and discussing more rigidly than before, the glaring fallacies, dating from the time of Newton, relating to the motion of the moon, which are briefly alluded to in the passages of *Victoria Toto Cœlo* to which I have ventured to direct your attention; where you may observe I have frequently cited your admirably lucid *Six Lectures on Astronomy*, in justification of what I have advanced as to current views.

I have taken the liberty to refer you to the printed matter in my book (the citations from which only occupy a few pages and will be more easily read than MS.) that I may thus be enabled to shorten this letter; having now only further to acquaint you,—which I do as a duty and an act of courtesy towards you,—that finding nothing so distinct and clearly enunciated elsewhere on this subject, as in your *Six Lectures*, I shall write with special reference to one or two passages in them; and these I will now point out, with a brief indication of the nature of the issues I intend to raise.

I think this course the more proper on my part, as I am not unmindful that these lectures were originally delivered to a mixed audience in the country, though they appear to have been subsequently revised for publication—the preface to the 4th edition, which I shall cite, being dated from the Royal Observatory.

In p. 176 of the lectures (fig. 56), it may be considered we have the working out of Prop. iv., Theor. iv., of Newton's *Principia*, b. iii., and what constitutes the unfortunately false basis upon which the famous "Problem of the Three Bodies" has invariably been solved. I may briefly observe, that my primary argument against this, and the main principle of all my reasoning, will be that the physical or dynamical laws of astronomy can only deal with the real or absolute motions of the heavenly bodies,—*not* with mere relative or apparent motions,—and that the real motions of the moon, both as regards velocity and path, are utterly disregarded in these propositions.

In p. 177 of the Lectures, the real motions of the moon being thus disregarded, her velocity is represented as only equal to 0·6356 of a mile in $1'' = 2{,}288$ miles an hour (or 2,290 miles, as given in Ferguson's Astronomy). I object, that on the heliocentric hypothesis, taking the radius of the earth's orbit as = 95 million miles, and its mean motion as 68,000 miles an hour (as in the Lectures), then the moon's motion is thirty times greater than above represented; the motion of the moon being, in fact, upon the whole, greater than that of the earth.

The "circumference of the moon's orbit" is in the same place spoken of (as if it described a circular or oval path each lunation) and represented as only 1,500,450 miles in a month; whereas the moon's real path in a month is only an undulatory curve, crossing and re-crossing an arc of between one-twelfth and one-thirteenth part of the orbit of the earth, and, in round figures, is thirty times greater than represented, or equal to more than 45,000,000 miles in a month. Every part of the reasoning based upon the moon's fictitious "orbit" round the earth as a fixed centre, both as to the moon's angular velocity, the direction of its motion, and its fall from the tangent (as well as the force of gravity thence deduced), is consequently

fallacious, unless the hypothesis that the earth goes round the sun is abandoned.

Again, on p. 184 of the Lectures, and in fig. 59, the whole argument is only tenable if based upon the hypothesis that the earth is stationary, and the moon moving in an oval "orbit" round it every month. In §§ 60–63 of *Victoria Toto Cœlo* will be found a sketch of the line of reasoning to be adduced against this.

On p. 185 of the Lectures an allusion is made to what is previously advanced at pp. 85-87, to which I should not at present have cared otherwise to advert; but I cannot help considering that what is there stated can scarcely have been stated intentionally, and I have no wish to take advantage in argument of what it would appear may have been an oversight. In *Vict. Toto Cœlo*, § 11, I have pointed out that the motion of the moon in the *quasi*-ellipse, in which she has thus been represented to move, is in certain respects unlike the elliptical motions of the other heavenly bodies; the moon's motion being described as least at the apsides of her orbit, where the curvature is greatest, and greatest when in syzygy where the curvature is least. This -- which is *not* the case, however, as regards the hypothetical ellipses described by the planets and comets round the sun—is nevertheless stated to be so (and it is even repeated) in the Lectures. For instance (p. 86) it is stated, "The greater its (the planet's) speed, the less its path is curved," referring to K in fig. 30, where the curvature is obviously greatest, the planet being then in perihelion, and moving round the lower focus of the ellipse with its greatest velocity.

In p. 85 of the Lectures, and the same figure (30), I regret that I may also be obliged to point out, that the tangential velocity or "force," "that part" ["of the force"] "which acts in the direction OM parallel to the orbit," is said to "accelerate the planet's motion in its orbit." But in "resolving the force MS into two, NM and OM," an unusual and unreal element is introduced into the demonstration. According to the first and second propositions of the *Principia*, and the ordinary methods of exhibiting the effects of centripetal forces, MS, the central force, is—besides OM, the tangential velocity—the only force affecting the body *ex hypothesi*. NM is therefore purely fictitious, and could only have been real, had the orbit (instead of an ellipse) been a perfect circle, when NM would have been merely SM, the *radius vector*, produced beyond the circumference of the orbit; in which case, also, there would be no "accelerative force," as the circle would be described with a uniform velocity. I point out this for the sake of accuracy and *ad hominem* only, not as myself adopting *any* mode of demonstration that would seem to prove that gravitating bodies could ever revolve either in circles or ellipses round centres of attraction; which I affirm, and claim to have proved elsewhere, to be demonstrably impossible.

To revert to the motion of the moon. I will only further trespass upon your time by observing that when the moon is in conjunction, and when (as stated in the Lectures) the sun's attraction upon it is greatest, it is precisely then (the moon's real path being regarded) that the moon begins to move away from the sun *with increasing velocity, as if repelled.* It is also when the moon as it were has dipped within the earth's orbit, between her last and first quarter, and when nearest the sun in conjunction, that her real motion is necessarily slowest, for then she ultimately falls behind the earth's motion in its orbit; and it is only when she rises beyond the earth's path, between her first and last quarter, and when her distance from the sun is greatest in opposition, that her motion is greatest; in other words, the reverse of what is stated in the Lectures, and of what may appear when a fictitious elliptical path is constructed for her, as with the earth at rest in its centre; also the reverse of what would result were there really an attractive influence exer-

cised upon her by the sun. And, not only so, but the very direction of her motion is also reversed by this fictitious hypothesis, as exhibited in fig. 59 of the Lectures. Every astronomer must know that the moon's real motion is always *direct*. (Vide *Ferguson's Astr., in loco.*) In fig. 59 of the Lectures her path is represented as *retrograde*, when in conjunction and between her last and first quarter; as going, in short, at the rate of 2,288 miles an hour to the right, in a path greatly curved and convex to the sun, when in reality she is moving to the left, in a totally different curve, which is concave to the sun, and then she is so moving with thirty times greater speed than the 2,288 miles an hour assigned to her in the Lectures.

I had the honour to forward to you, in 1862, a paper entitled *The Mechanics of the Heavens*, which I had that year laid before the British Association at Cambridge; in § 11 of which paper, and the foot-note p. 6, I have expressed all I care to say as regards the difficulty, which I am quite aware there often is, in obtaining a hearing from eminent men like yourself for communications such as this. I beg leave only to add that I, nevertheless, think it a duty, from which I will not shrink, and also an act of courtesy on my part, to send you this letter, whatever may be its reception. But should I be favoured with any answer, I would beg that, whatever may be its nature, I may be permitted to publish it along with what I have now, most respectfully though freely, ventured to address to you as the Astronomer Royal of England.

I have the honour to be, Sir,

Your very faithful, humble Servant,

J. REDDIE.

To GEORGE BIDDELL AIRY, Esq., F.R.S., F.R.A.S., &c. &c.,
Astronomer Royal, Greenwich.

P.S.—To save you all unnecessary trouble, should you now be pleased to bestow any attention on this matter, I shall forward by book post, along with *Victoria Toto Cœlo*, another copy of *The Mechanics of the Heavens*, and also of *Vis Inertiæ Victa; or, Fallacies affecting Science*, which is frequently referred to in both the others, as it is probable you may not have cared to preserve those previously forwarded to you.

[*Answer to the above Letter.*]

Royal Observatory, Greenwich, London, S.E.,
June 7th, 1864.

SIR,—I am obliged by your courtesy in sending me three pamphlets, and by the trouble which you have taken in your letter of June 6, in indicating certain points to which you wish to call my attention.

I cannot at any length enter into the matter; but I will merely observe that much of what you say is quite correct, but that the difficulties which you have founded thereon are incorrect. It *is* true that the earth and the moon are two independent planets circulating round the sun, but under circumstances which make their perturbations excessively large, so large as to give the appearance or relative fact of the moon circulating round the earth. It *is* true that the moon as a planet has the large velocity round the sun of which you speak. But it is *also* true that, inferring relative forces from the difference of absolute forces (which on mechanical principles is perfectly correct), and inferring relative motions from the difference of

absolute motions (which on geometrical principles is necessarily correct) there is no error in treating the moon as describing an ellipse round the earth, perturbed by the difference of sun's force on earth and on moon ; and there is no error in speaking of the moon's relative velocity round the earth as the small velocity in such an ellipse.

The failure in your reasoning is simply the want of the steps for inferring relative force and relative motion from absolute force and absolute motion ; and this seems to go through the whole.

You can perhaps understand that, as a very closely occupied man, I cannot enter further into this matter.

I am, Sir,
Your obedient Servant,
G. B. AIRY.

To JAMES REDDIE, Esq.

[The Reply to this letter is not inserted, as its substance will be found in the paper *On the Motion of the Moon*, Note D.]

NOTE D. (§§ 2, 31, 33.)

The Paper alluded to in the text as submitted to Section A of the British Association at Bath, in August, 1864, having been referred to two of our Vice-Presidents, Mr. Mitchell and Dr. Thornton, is now here printed upon their recommendation, with the approval of the Council of the Victoria Institute, that it may be discussed along with the foregoing Paper, should any prefer doing so. It is as follows :—

ON THE MOTION OF THE MOON, AND THE SUN'S REPULSIVE INFLUENCE, AS THE PROBABLE CAUSE OF THE VARIATIONS OF THE MOON'S MOTIONS, &c.

1. THE time which the moon occupies in passing through the shadow of the earth during an eclipse is, roughly speaking, four hours ; and—taking the earth's diameter at 8,000 miles, and assuming the breadth of the earth's shadow, plus that of the moon's disk, to be the same as the breadth of the earth itself,—it has hence been deduced, that the moon in passing through the earth's shadow is moving at the rate of 2,000 miles an hour ; so, calculating her path for a day or for a month of thirty days at the same rate, we have 48,000 miles as the extent of her daily path, and 1,440,000 miles as her path during each lunation. These figures and calculations, however, are only approximative. The moon's velocity is stated by the Astronomer Royal of England, in his well-known *Six Lectures on Astronomy*, to be more precisely 2,288 miles an hour, and her path each lunation 1,500,450 miles ; and even in old works on Astronomy, such as Ferguson's, it will be found that the velocity of the moon is given as "about 2,290 miles an hour."

2. From the same simple data, the moon's mean distance from the earth has been deduced. Assuming her path in a month to be a circle of 1,500,450 miles in circumference, we have only to divide these figures by 3·1415...(the well-known ratio of the diameter to the circumference of

a circle), and again by 2,—which will give us a semi-diameter or radius of 238,800 miles,—in other words, the moon's mean distance,—as we find it stated in Professor Airy's *Six Lectures*.

3. It is and has long been usual, however, to speak of the moon's distance from us, in round figures, as about 240,000 miles, or as 60 semi-diameters of the earth; which is thus arrived at:—She passes through the earth's shadow when eclipsed in four hours, and is therefore considered as describing the breadth of the earth or 8,000 miles in that time. Consequently in one day (or six times four hours) she describes six times the breadth of the earth; and taking thirty days as representing the period of each lunation, the moon will describe 6 times 30, or 180 times, the breadth of the earth in a month. One-third of this will be the diameter of her orbit, namely, 60 diameters of the earth, and she is consequently distant from us 60 semi-diameters of the earth, or 240,000 miles.

4. We find this mode of computing and speaking of the motion, and path, and distance of the moon, in the most modern astronomical works. I have made use of the *ipsissima verba* of the present Astronomer Royal, taken from the fourth edition of his Lectures. But it is by no means a merely modern view. It dates back far beyond our own day or even the time of Newton, Kepler, or Copernicus. In fact, it really belongs to the Ptolemaic system; and it rightly belongs to it; for it will be found, upon due consideration, that in all respects the deductions which have been drawn from the one initial fact of observation, that a lunar eclipse lasts about four hours, depend for their approximate accuracy upon a geocentric hypothesis, with the earth at rest in the centre of the moon's orbit.

5. According to Ptolemy and other astronomers about his time, the moon was regarded when *in syzygiis*, that is, when in conjunction with and in opposition to the sun, or when dark and full, as distant from us 59 semi-diameters of the earth. Huygens regarded its distance as 60 semi-diameters, Copernicus as $60\frac{1}{3}$, Street as $60\frac{2}{3}$, and Tycho-Brahe (if we correct the error due to his peculiar theory of Refractions) as $60\frac{1}{2}$. In the *Principia*, B. III., Prop. IV., Theor. IV., the distance is taken as 60; which is the basis of Newton's original calculations of the force of the moon's gravitation towards the earth, measured by the fall from a tangent to the moon's circular orbit, described with this radius.

6. As regards Ptolemy and others, who believed the earth to be at rest, their deductions as to the path of the moon in a month, in an orbit nearly circular round the earth, and consequently as to the extent of the moon's radius or mean distance, based upon the duration of a lunar eclipse, and the moon's consequent rate of motion, were necessarily very nearly accurate, if they were correct in the primary assumption that the breadth of the earth's shadow is nearly three times the breadth of the moon. To them, and upon the geocentric hypothesis, the velocity or rate of motion, and the monthly orbit of the moon in a nearly circular path, were real and actual. Not so, upon the Copernican system.

7. It is obvious, upon a moment's consideration—if we regard the earth as a planet in rapid motion round the sun, flying from west to east, or from right to left, with a velocity of 65,000 miles an hour, while the moon, when at the full, is moving in the same direction so swiftly that she passes through and beyond the earth's rapidly-moving shadow in the course of four hours—that the moon is really moving not at the comparatively slow rate of merely 2,000 miles an hour, but with an enormous velocity, 2,000 miles an hour swifter than the earth itself, that is, with a speed of no less than 67,000 miles an hour, during a lunar eclipse.

8. But the whole problem of the moon's motion and path is otherwise

changed and complicated, by the hypothesis of the earth's revolution round the sun. On that hypothesis we may not longer simply take this rate of the moon's motion during an eclipse, namely of 67,000 miles an hour, and multiply it by 24 to give the moon's path in a day, and again by 30 to obtain her path approximately in a month; because although, upon the data assumed, 67,000 miles is truly the velocity per hour of the moon when in opposition, it is by no means or approximately the rate of her whole motion during a lunation, as the rate of 2,000 miles an hour almost truly was upon the simpler hypothesis that the earth is at rest. Upon the heliocentric hypothesis, with the earth in rapid motion, and the moon passing round it while it thus moves, the moon must indeed travel 2,000 miles an hour more swiftly than the earth when at the full, and she must retain a greater velocity than the earth in order to get before it and arrive at her place in her last quadrature; but it is equally a necessity of the hypothesis, that when there her velocity must diminish to less than that of the earth, that she may fall back to her place in conjunction between the earth and the sun, and that she must continue to move with a velocity less than the earth till she falls behind the earth in its orbit, and so reaches her place in her first quarter; so that, just as the moon required to travel at the rate of 67,000 miles an hour, or 2,000 miles faster than the earth, in order to pass through its shadow in four hours when in opposition;—so when she is in conjunction, and falling behind the earth as much as before she exceeded it in velocity, her rate of motion must become reduced to 63,000 miles an hour, or 2,000 miles an hour less than that of the earth.

9. Thus we see, that upon the geocentric system, the moon's motion, computed from the duration of a lunar eclipse, was very nearly at a uniform rate of about 2,000 miles an hour; but, from precisely the same data, when we change the hypothesis, and assign to the earth a mean orbital motion of 65,000 miles an hour, then the moon's velocity must of necessity vary during each lunation no less than 4,000 miles an hour, her speed, when she is full or in opposition to the sun, being 67,000 miles, and when she is dark or in conjunction with the sun, 63,000 miles an hour only. Reasoning from the same one initial fact of observation, namely, that during a lunar eclipse the moon traverses the earth's shadow in about four hours, I repeat, that upon the geocentric hypothesis the moon's real motion is very little more than 2,000 miles an hour throughout, and is nearly the same in every part of her orbit, the variation being comparatively slight; while upon the heliocentric hypothesis her mean velocity is not only increased by the whole velocity of the earth in its orbit, but it actually becomes 4,000 miles an hour greater and less at one time than another. The moon's real velocity during a lunar eclipse, and always when she is full and furthest from the sun, upon the heliocentric hypothesis, is no less than 4,000 miles an hour greater than it is at the time of a solar eclipse, and always when she is nearest the sun immediately before new moon. This great variation in her velocity also occurs, though her distance from the earth is supposed to be nearly the same at these two times.

10. But not only is the rate of the moon's real motion thus altered, and its comparatively uniform motion changed, so materially as to differ by no less than 4,000 miles an hour at one time and another each lunation, when we abandon the Ptolemaic system, but the actual path of the moon is also entirely altered, and the very direction in which she moves is thereby changed, and even at times reversed. She no longer describes a nearly circular or oval path both in space and round the earth every month, at a radial distance of less than 240,000 miles, but she moves in an enormously larger orbit with a radius some 380 times greater; and this nearly circular orbit she now describes, not monthly round the earth, but round the sun once a year. Then her path during each lunation, though she still

appears to move in a circle round the earth, is no longer really a circle, but a slightly irregular arc, crossing and re-crossing, and nearly corresponding with an arc of about 30° of the earth's annual orbit round the sun. The moon's apparently circular monthly orbit round the earth is now but a mere appearance, resulting from her varying velocities as she thus crosses and re-crosses the path of the earth, always moving with decreasing speed as she approaches the sun from full moon till she is in conjunction, and always increasing in velocity as she recedes from the sun between new moon and till she is full; her velocity being always least while she dips within the orbit of the earth, and greatest when she is moving outside, or beyond the earth's orbital path.

11. I call attention to these details and dwell upon them, not as advancing anything that is absolutely new,—though I know they will appear as such to many, but because they have been too much or altogether disregarded, and have not been duly weighed, nor truly represented, in the explanations or interpretations of the phenomena of the moon's motion hitherto put forth, and now generally accepted.

12. Some, at least, of these facts as to the moon's real path and varying velocities will be found recognized in the following passages, which I cite from the ninth edition of Ferguson's well-known work on Astronomy. Having drawn a diagram to scale of the earth's and moon's relative paths in their respective orbits round the sun, he says :—

"Thus we see that, although the moon goes round the earth in a circle with respect to the earth's centre, her real path in the heavens is not very different in appearance from the earth's path. . . . The moon's absolute motion from her change to her first quarter is so much slower than the earth's, that she falls 240,000 miles (equal to the semi-diameter of her orbit) behind the earth at her first quarter; that is, she falls back a space equal to her distance from the earth. From that time her motion is gradually accelerated to her opposition or full, and then she is come up as far as the earth, having regained what she lost in her first quarter. From the full to the last quarter her motion continues accelerated, so as to be just as far before the earth as she was behind it at her first quarter. Afterwards her motion is retarded, so that she loses as much with respect to the earth as is equal to her distance from it. . . . Hence we find that the moon's absolute motion is slower than the earth's from her third quarter to her first, and swifter than the earth's from her first quarter to her third, her path being less curved than the earth's in the former case, and more in the latter. Yet it is still bent the same way towards the sun," or (as he again shows by the diagram drawn to scale) "is concave to the sun throughout." (§§ 266, 267.)

13. These brief citations from Ferguson's Astronomy show, that the hypothetical facts to which I appeal, have been substantially recognized by astronomers, and are not really new, though they have been too much or almost altogether disregarded, and although what flows from them has been overlooked. As an instance of this, I beg leave to refer once more to the Astronomer Royal's *Six Lectures on Astronomy*. The author is speaking of the deceptiveness and frequent unreality of mere appearances, as regards rest and motion; and, arguing in favour of the heliocentric theory, he says :—

"The argument is precisely the same as applied to the heavens. If we had nothing but the sun and moon turning about in various ways; even then, remarking their great size and their great distance, and the great speed with which they must be supposed to turn *(for the moon must be supposed to move at the rate of* 60,000 *miles an hour*, and the sun very much quicker), their daily revolution round the earth would be very unlikely." (4th ed, p. 54.)

Here we see that the actual motion of the moon, which has, of necessity, upon the received hypothesis, a velocity of more than 65,000 miles an hour, is not only disregarded or forgotten; but, that the moon should require to move with any such great velocity, is even made an argument against the probability of the Ptolemaic system,—though the facts, and consequently the argument as regards the moon's motion, computed from the duration of an eclipse, upon the two rival hypotheses, are precisely the other way. It is only, as we have already seen, if the earth be at rest that the moon can be regarded as passing through the earth's shadow at the approximate rate of 2,000 miles an hour;* whereas, upon the Copernican hypothesis, and regarding the earth's velocity in its orbit as 65,000 miles an hour, the rate of the moon's motion is actually, of necessity and *ex hypothesi*, even greater than the rate of 60,000 miles an hour, which was urged as so "very unlikely" as to amount to an argument against the Ptolemaic system!

14. But, not only has the real velocity of the moon been thus disregarded,—and in fact it is not only disregarded, but apparently denied, by the argument employed in the passage above cited,—but in no other part of these lectures is the moon's real motion or path even once mentioned. Its motion is exclusively spoken of as only about 2,000 miles, or more precisely as 2,288 miles an hour, in a nearly circular monthly orbit.

15. But, since in Ferguson's Astronomy the real path and rapid motions of the moon as she accompanies the earth round the sun, and also the great variations in her velocities at one time and another, are recognized; let us examine by what kind of reasoning or argument her actual velocities are practically set aside and become resolved into the small mean motion of only 2,290 miles an hour. It can scarcely be said that anything like adequate argument is attempted. What Ferguson says is solely directed to meet a single "difficulty," which alone appears to him to require to be removed. His words are:—

"The moon's path being concave to the sun throughout, demonstrates that her gravity towards the sun at her conjunction, exceeds her gravity toward the earth. And if we consider that the quantity of matter in the sun is almost 230,000† times as great as the quantity of matter in the earth, and that the attraction of each body diminishes as the square of the distance from it increases, we shall soon find that the point of equal attraction between the earth and the sun is about 70,000 miles nearer the earth than the moon is at her change. It may then appear surprising that the moon does not abandon the earth when she is between it and the sun, because she is considerably more attracted by the sun than by the earth at that time. But this difficulty vanishes when we consider, that

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* But, even upon a geocentric hypothesis, the rate of the moon's motion cannot be so very simply ascertained. The true solution of the problem will depend upon the breadth of the earth's shadow, the distance and size of the sun, and the motion of the earth's shadow in one direction or another; which will again depend upon whether the earth, if supposed to be at rest in space, has an axial rotation or not, and whether the moon is moving faster than the sun in one direction, or slower than the sun in another. In fact, unless both the earth and the sun were at rest in space, the duration of a lunar eclipse, on either hypothesis, could not give precisely the rate of the moon's motion, even if we knew the precise breadth of the earth's shadow. If only the earth were at rest and in the centre, the duration of eclipses could only indicate the difference between the velocity of the sun and moon.

† Increased to 352,280 times, taking the earth's radius as 95,000,000 miles. (*Airy's Lects.*, p. 215.)

a common impulse on any system of bodies affects not their relative motions; but that they will continue to attract, impel, or circulate round one another, in the same manner as if there was no such impulse. The moon is so near the earth and both of them so far from the sun, that the attractive power of the sun may be considered as equal on both; and therefore the moon will continue to circulate round the earth in the same manner as if the sun did not attract them at all. For bodies in the cabin of a ship may move round, or impel one another, in the same manner when the ship is under sail, as when it is at rest; because they are all equally affected by the common motion of the ship." (§ 268.)

16. Now, had I anything better to produce on that side of the question, I should have been glad to do so. Unfortunately the point is not discussed at all in the Astronomer Royal's lectures. Ferguson's reasoning is not only very poor and inadequate, but it is self-contradictory. He first says, that when the moon is nearest the sun, "her gravity towards the sun exceeds her gravity towards the earth"; and he even repeats, that "she is considerably more attracted by the sun than by the earth at that time"! But he afterwards says, that "the moon is so near the earth, and both of them so far from the sun, that the attractive power of the sun may be considered as equal on both; and therefore that the moon will continue to circulate round the earth as if the sun did not attract them at all"! The best answer I can give to this is the following words of the Astronomer Royal. He says:—"The sun by the law of gravitation attracts bodies which are near with greater force than those which are far distant from it. Therefore, when the moon is nearest the sun, the sun attracts the moon more than the earth, and tends to pull the moon away from the earth" (p. 184). He afterwards clearly points out that it is only when the moon is in quadrature that the sun's attraction upon her and the earth is nearly the same, as they are then both equidistant from it.

17. But, unfortunately, while Ferguson is arguing upon these false notions as to the force of gravity, and with questionable logic throughout, though very properly with reference to the actual path of the moon on the heliocentric hypothesis, Professor Airy does not apply his sounder reasoning as to the sun's attraction to the real motion of the moon at all, but only to her *quasi* "motion" in an unreal circular path round the earth as a centre at rest. As he thus, when the moon moves slowest, has reversed the real direction of her motion between her quadratures, he by that means shows that the sun's increasing attraction increases the moon's velocity from her last quadrature as she is approaching nearer the sun to her place in conjunction; which is directly contrary to the fact that the moon then really decreases her speed till nearest the sun, where she moves with her least velocity. It is in this manner alone he arrives at the following conclusion:—"Therefore, when the moon is *nearest the sun*, and furthest from the sun, she is moving with the greatest velocity"; which could only possibly be true were the earth at rest.

18. Ferguson's other argument is as follows:—"But this difficulty vanishes when we consider that a common impulse on any system of bodies affects not their relative motions, but that they will continue to attract, impel, or circulate round one another, in the same manner as if there was no such impulse. For [he argues] bodies in the cabin of a ship may move round, or impel one another in the same manner when the ship is under sail and when it is at rest, because they are all equally affected by the common motion of the ship."

19. If Ferguson, or any other persons who have made use of this illustration, had only carefully considered what is the cause or reason why all bodies in the cabin of a ship are necessarily affected by the motion which is truly

described as being "common to all," while they may move among one another from other causes, they could scarcely have relied upon it as furnishing an argument applicable to the relative motions of detached and independent bodies like the earth and moon. All bodies in the ship are somehow attached to it, whether they stand or move about, while they are supported from below, or whether they hang and are swung about while they are supported or suspended from above. They partake of the common motion of the ship, because they are attached to it mechanically. Therefore, let us vary the illustration; for it is a fact that men have been influenced by popular arguments upon this question much more than at first may be supposed.

20. Let us suppose, then, that we are in one of the carriages of a railway-train, travelling eastward at the rate of 40 miles an hour, and that we overtake another train on a parallel line of rail moving also eastward at 35 miles an hour. When we pass that train, it will lag behind ours, and so it will appear to move away in an opposite direction. But would we, therefore, be entitled to reason as if the other train were really moving westward at the rate of five miles an hour; and—which follows as a necessary consequence of our doing so—to speak as if our own train were at rest, though we know the facts to be that both are travelling eastward, only that ours is moving quickest, with a velocity of 5 miles an hour greater than the other? Observe the result if we do. The train that is moving fastest must be regarded as not moving at all, and the other as moving in a direction opposite to its actual motion; and then, also, the slower it moves, the greater its velocity will appear: the direction of the motion and the rate of velocity being both reversed and made contrary to reality.

21. Or, again, were we in the slower train, would it be rational to speak of the greater velocity of the passing train as only a speed of five miles an hour, and regard our own train as at rest? In that case, it will be observed, the mere appearance is not so utterly contrary to fact as in the other. As it is now the slowest train that is considered at rest, the direction of the motion of the other is not reversed; the delusion is limited to the rate of the velocity.

22. But, if it would be absurd to do this, when we merely consider the directions of the motions and the relative rates of velocity of the two trains, it would, if possible, be even more absurd, if we were further to reason from the mere appearances, as to the probable motive power, or force, which had produced the motions and respective velocities of the two trains. If the conversion, for instance, of one ton of fuel into heat represented the force that would produce a speed of one mile an hour for a given time; and in like manner thirty-five tons a speed of thirty-five miles, and forty tons of forty miles an hour; then, founding our calculations upon the mere appearances, instead of the real motions, would lead us to astounding conclusions.

23. One other illustration will suffice. Suppose there is a steam-vessel with a single mast, floating at rest on a placid sheet of water, without any current and with no wind; and at a little distance, that another smaller steamer is lying parallel and attached to the larger vessel by a rope looped round its mast. Then let us suppose the small steamer gets up steam, and begins to move eastward with a horse-power equal to propel it two knots an hour. The result will be that the small vessel will not steam forward in a straight line, but it will move round the larger vessel to which it is held attached by the rope. Although the rope does not draw the smaller vessel towards the larger, yet as it holds it at the distance of the rope's length, and so causes it to move round,—an illustration like this has been often used as representing the revolving of a body held to a centre by gravity. Ferguson, among others, does so, in § 107 of his Astronomy: his illustration being a boat rowed by a man while attached to a stationary ship by a rope. But he

only applies the illustration to the case of the earth and planets, as revolving round the sun at rest. Adopting this illustration *quantum valeat*, let us now apply it to the motion of the moon round the earth as a moving centre. We have now only to suppose that our larger steamer also gets up steam, and begins to move eastward, say with a speed of twelve knots an hour, and watch the result. The little steamer being detached in one sense from the other—though it is attached to it in a different sense, *i.e.*, held by a rope at a certain distance detached from it—does *not* partake of the motion of the larger vessel, as all bodies in its cabin and on its deck do, as in Ferguson's previous illustration (§ 18, *ante*). The little vessel, therefore, now falls behind, where it will be towed along; the only effect of its exerting its steam power of two knots an hour being to lessen, *pro tanto*, the tension upon the rope that holds it. In order that the small vessel may now go round the other as before, and keep the rope always stretched out with the same tension, while the larger vessel now steams along at twelve knots an hour, it will require a horse-power sufficient to give it a speed of fourteen knots an hour in moving eastward, and when it has passed before and round to the other side of the larger vessel, it must then have reduced its speed to ten knots an hour, still however steaming in an easterly direction, or it could not make its apparent revolution round the other.

24. This illustration, however, would only be strictly analogous if the moon's motion in its apparent circular orbit were always the same; which is not the case. If that were so, then the influence of the sun upon the moon's motion would be omitted as imperceptible or *nil*, according to the usual methods of dealing with this problem; for it should be remembered that it is what is called "the *moon's variation*" (that is, *the variation* of her motions in her apparent orbit) that is attributed to the influence of the sun's attraction. Let us, therefore, leave illustrations, to reason from the actual facts of the case that is under discussion, which are perfectly clear of themselves, and really not in dispute. I admit the *apparent* increase of velocity in the motion of the moon as she approaches the sun; and, were this apparent increase of velocity real, instead of merely apparent, I would further admit that it might be caused by the sun's attraction; but what I maintain is, that if we believe in the Copernican theory, we also know quite well that this apparent increase of velocity as the moon approaches the sun is only apparent and unreal, being, in fact, the result of the moon's decreasing velocity when viewed from the earth as a stand-point. As the earth is, then, *ex hypothesi*, moving quicker than the moon, the moon merely appears to move quicker, and also to move in an opposite direction, contrary to reality, as we have seen is also the case in the simple illustration of the passing railway-trains. If we really believe the earth to be in motion, then we have only to take into account its velocity eastward, in order that the apparently increasing motion of the moon the other way may be known to be, in fact, a decreasing velocity in the same eastward direction; and, consequently, if this variation of the moon's velocity is attributable to the influence of the sun, it follows that that influence must be *repulsive*, since it has really retarded the moon's velocity in approaching the sun. In like manner also, therefore, as the moon's motion, which is apparently retarded and decreasing from her place in conjunction till in her first quarter, is really increasing during that time, and goes on increasing more and more as she recedes from the sun till she reaches her greatest distance in opposition, the real influence of the sun upon the moon must be *repulsive*, or, the reverse of that, attributed to the sun, when only the apparent variations of her motion are considered, instead of the real variations, upon the Copernican hypothesis.

25. How this obvious oversight can have occurred is not the question. To say the least, it is certainly remarkable, when we consider that the very

watchword of Copernicus and Galileo was virtually that appearances are deceptive. It seems to have been forgotten that if appearances are deceptive as regards the motion of the earth, they may be equally deceptive as regards the motions of the moon. The moon's variation, in fact, was first discovered by Tycho Brahe, who held a geocentric hypothesis, and who would naturally therefore speak of the moon's apparent increase and decrease of velocity as not only apparent but real, for so he believed them to be; and so they would have been, if his geocentric hypothesis were true.

26. It is for us, however, now, whatever others may have done, to get rid of all unrealities and deceptive appearances in science. We are bound, as rational beings, if we accept a heliocentric system, to look at all its consequences. In some respects the puzzling motions of the moon may probably be better understood if we regard her actual path on that system. The moon's variation, the alteration of the place of her nodes, and the progression of the apse—probably, also, her annual equation, would, perhaps, all be more simply explained and better understood, by dealing with her actual motions and velocities, instead of fictions.* I do not say there will not be found other difficulties of another kind. But that is nothing to the purpose. We may not get over difficulties in science by having recourse to mere false appearances. For instance, there may be a difficulty from the non-coincidence of the moon's path with the plane of the ecliptic, as this will make her path not a simple undulatory wave-line crossing and recrossing the earth's orbit in the same plane, but a kind of drawn-out spiral path round the orbit of the earth. On the other hand, even this may be found a simplification that may serve to explain the apparent librations and some of the other various perturbations of the moon, so far as they may not be mere optical effects of changed position and varying refractions.

27. At all events we must not flinch from the consequences that flow from our adopted hypothesis. The opposite course has been far from satisfactory. With a heterogeneous mixture of effects which are only apparent in the moon's motions, explained by a physical cause believed to be real; with a fictitious orbit never described by the moon if the earth revolves, in which, also, the very direction of her real motions is sometimes reversed, and, as a natural consequence, is accounted for by an influence which must really repel, mistaken for a force that attracts, we need not be astonished that the result has been perplexities and complications. "Of these applications of the theory of gravitation to explain the different perturbations of the moon" (says Professor Airy), "a great deal might be said. It is a subject involved in mathematical perplexity beyond anything else that I know" (p. 183). One of the latest of these perplexities is the famous dispute as to the acceleration of the moon's mean motion, described in Lord Wrottesley's Address to the British Association at Oxford in 1860. I allude to it now, because it certainly is one of those difficulties from which all that has been in dispute between several eminent analysts of England and the Continent, is entirely cleared away, when we have regard to the moon's real path round the sun instead of to a fictitious path round the earth. His Lordship said: "Professor Adams asserts that his predecessors have improperly omitted the consideration of the effect produced by the action of that part of the sun's disturbing force which acts in the direction of a tangent to

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* So also, the various phenomena of the tides may be more simply explained by the hypothesis of a repulsive influence than they are by the theory of the attractions of the sun and moon; especially considering that there are *no tides* at the Equator, where the theory of attraction requires them (and Newton and his followers actually represent them) to be *greatest*!

the moon's orbit, and which increases its velocity. His opponents deny that it is necessary to take this into account at all," and probably they did so with very good reason; for, at the opposite side of the moon's orbit, when represented as nearly a circle round the earth, there of course would be the same disturbing influence to act against the now precisely opposite direction of the moon's motion. But, if it had only been kept in mind that when the earth is regarded as in motion, the moon's real path is always concave to the sun, this dispute could never have been raised, for the simple reason that no tangent to the moon's orbit could then possibly be imagined in the direction of the sun!

28. It may be from forgetfulness such as this, and the inadvertent confounding together of real and relative and apparent motions, that (in the words of Professor Whewell) "the Copernican system itself is very complex, when it undertakes to account, as the Ptolemaic did, for the inequalities of the motions of the sun, moon, and planets; for," he adds, "even the moon's motion cannot be conceived without comprehending a scheme more complex than the Ptolemaic epicycles and eccentrics in their worst form." But, be that as it may, I now appeal to the actual facts of the moon's real path and her greatly varying velocities, upon the Copernican hypothesis, to establish one obvious truth, namely, that if these variations of motion are caused by the influence of the sun, the sun's influence upon the moon is inevitably *repulsive*.

August 12*th*, 1864.