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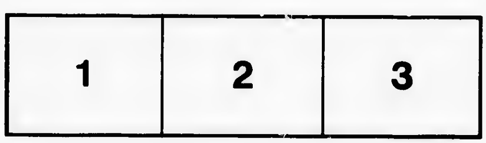

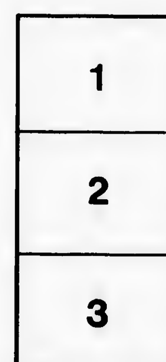

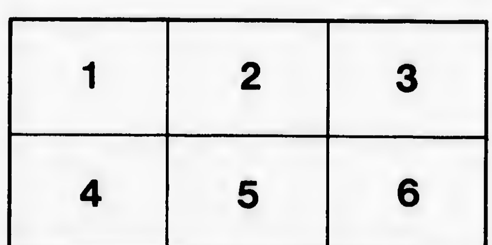

THE PLANET

JUPITER.

The diameter of Jupiter is estimated at 88,000 miles; and is, therefore, about 11 times that of the earth.

Its superficial area is accordingly about 120 times, that of the earth; and its volume (bulk) about 13,000 times that of the earth.

The density of Jupiter is estimated at 0·2415, taking that of the earth as unity. The intensity of gravitation on its surface therefore, taking that of the earth as unity, is about 1·9.

THE EARTH.

From observation of the belts, it has been concluded that the period of Jupiter's axial-rotation is about 9 hrs. 55 min., making the average length of the day less than 5 hours. The angular velocity of its rotation is, therefore, about 2½ times greater than that of the earth, which velocity can be readily appreciated by supposing the hour hand of a clock to describe the circle in 10 hours, instead of 24 hours, the angular velocity of the hour hand will then represent the angular velocity of the planet.

N.B.—We shall show that the distance (about 500 million miles) from the Sun, now assigned to Jupiter, is not supported by fact; therefore the above data, so far as dependent upon that distance, are subject to correction.

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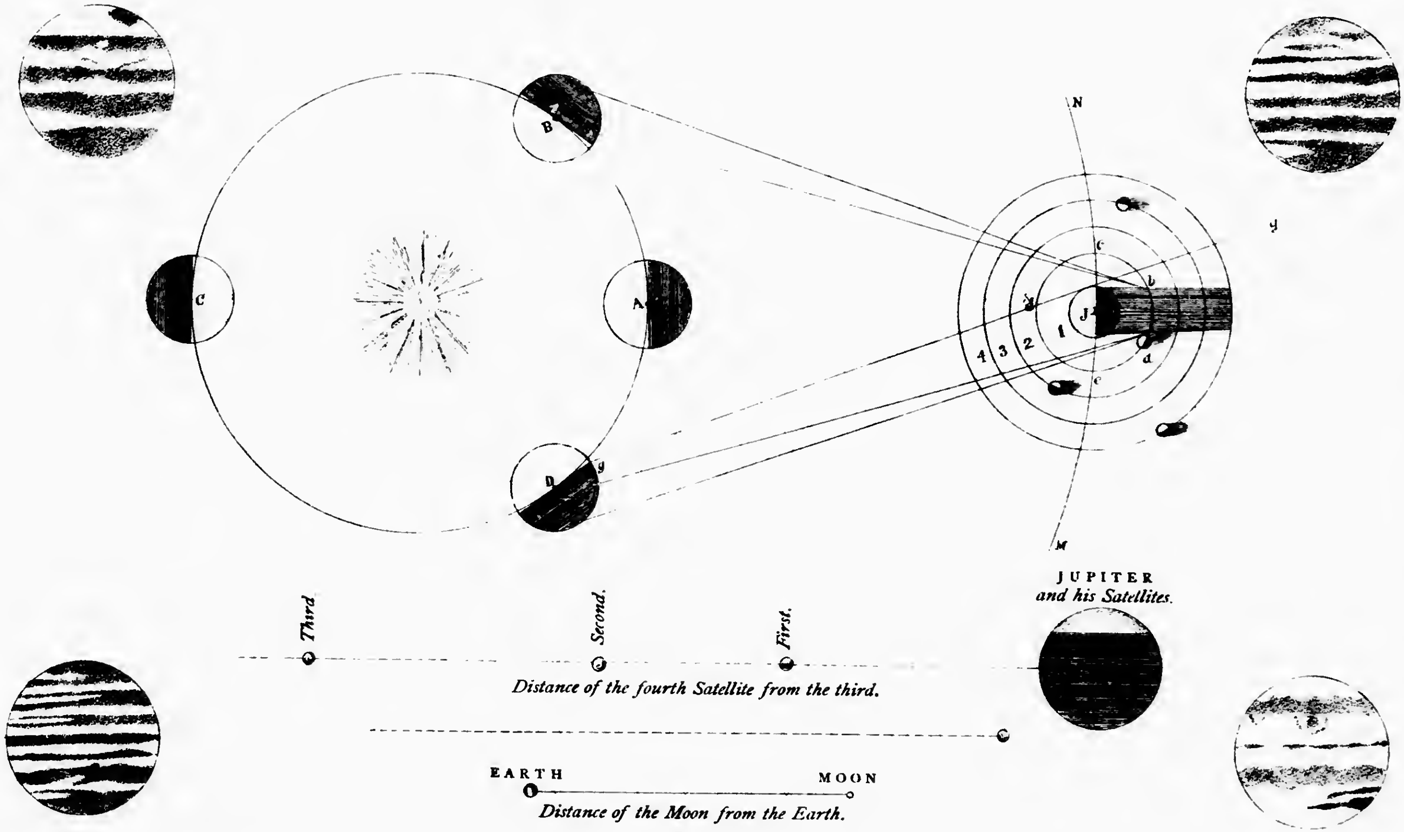


From the Encyclopedia Brittanica.

Roberts & Cº Lith Montreal

Plate 6.

Fig. 5.

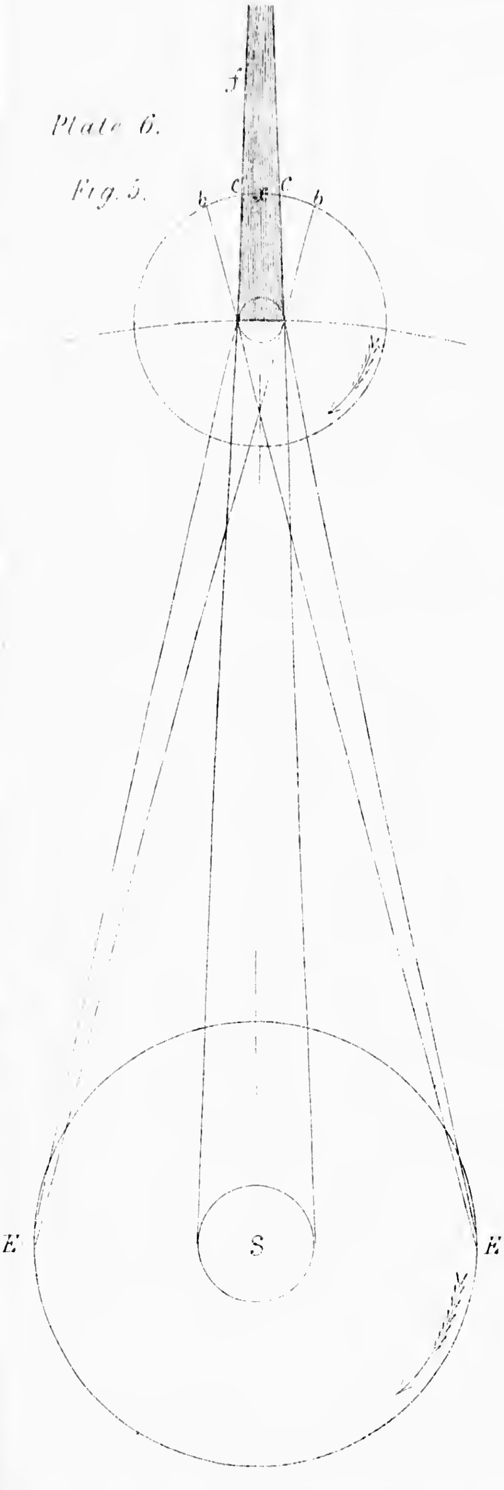
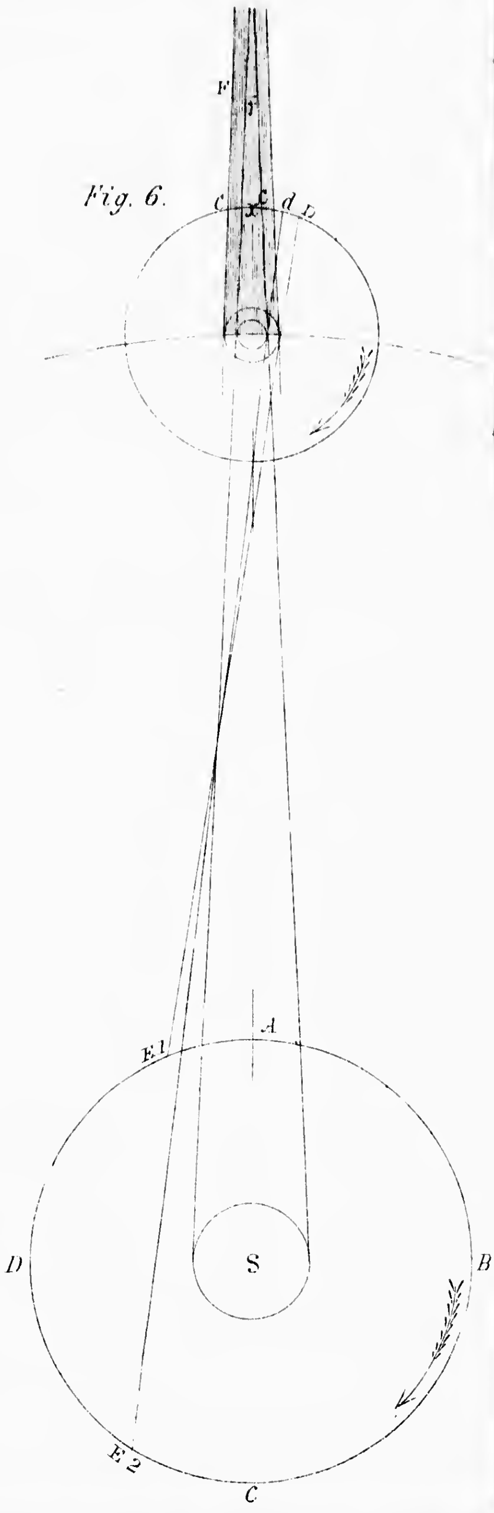


Fig. 6.




B

I

JUPITER'S SHADOW.

Plate 6, illustrates the shadow of the planet Jupiter as seen from two places in the earth's orbit; one—near the place of inferior, and the other—near the place of superior conjunction of the earth. The apparent size of the sun is shown as varying, whereas the distance of the sun from the earth is the same at both places, i. e., whether the earth be at the place E. 1., or at the place E. 2. If the earth be supposed to travel out of its orbit and to approach very near to Jupiter, it is evident that the apparent size of the sun would be diminished and the apparent size of the planet be greatly increased; nevertheless the actual relative sizes of the sun and the planet—each to the other—would remain unaltered, and the solar rays would reach the planet at the same angle as before; but the diameter of the (apparent) planet is the base of the (apparent) shadow, and the altitude, and other dimensions of the shadow are proportional to its apparent base.

For a general explanation of the illustrations in plate 6, see page 81, et seq. (The eclipse of Jupiter's satellites) to which it refers.

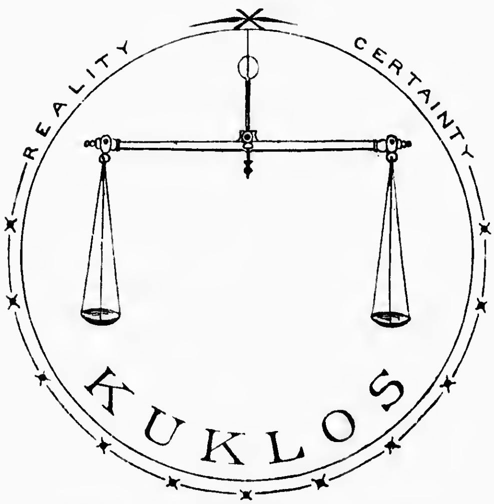
SUPPLEMENT D.

PART SECOND.

CENTRIFUGAL FORCE

AND

GRAVITATION.



SUPPLEMENT D.

PART SECOND.

CENTRIFUGAL FORCE

AND

GRAVITATION

A LECTURE.

BY

JOHN HARRIS.

MONTREAL:
JOHN LOVELL, ST. NICHOLAS STREET.

DECEMBER, 1873.

Montreal:—John Lovell, Printer.

PREFIX.

PART II. SUPPLEMENT D.

Reasonable Theories.

Jovian Gravitation.—From observations of Jupiter's satellites, it has been ascertained that the collective gravitating influence of that planet is of about 7 times greater intensity than that of the earth operating at an equal distance. Since the intensity of gravitating influence for planets of equal density varies as the diameter, (See Lecture, page 44), and the diameter of Jupiter is about eleven times that of the earth; the density of that planet must be proportionally (*i. e.* as $7^3 : 11^3$) less than that of the earth. Assuming this to be so, the question is suggested—what is the intensity of the internal gravitating influence on the surface, or in other words, what would be the weight of a body on the surface of that planet relatively to its terrestrial weight? For example, what would 100 lbs. terrestrial weight of iron be equivalent to, if transferred to the surface of Jupiter? The question is one of considerable difficulty. We will first suppose the present volume of the planet to be so contracted as to reduce its diameter to about 7 times the diameter of the earth (*i. e.*, by rather more than one-third of its present diameter). We should then have the density equal to that of the earth, and an intensity of gravitating influence on the surface equal to about 7 times that on the surface of the earth, (*i. e.*, the 100 lbs. of iron, if transferred would weigh about 700 lbs. on the surface of the reduced globe.) We will now suppose the condensed globe to expand to its former angular (diametrical) magnitude. Since the surface is now removed to a greater radial distance from the centre, in the ratio of 11 to 7, and the collective mass has not increased, the intensity of gravitation will be therefore reduced in the inverse proportion of those quantities; but, since the area of the expanded globe is greater than that of the condensed globe in the ratio of 11^2 to 7^2, the intensity of gravitation must be accordingly further diminished in the inverse of that proportion. Therefore the computation will be.

$$\frac{7 \times 7}{11} = \frac{49}{11} \qquad \text{And } \frac{49}{11} \times \frac{49}{121} = 1{\cdot}9.$$

That is, the intensity of gravitation on the surface of Jupiter is rather less than twice the intensity of gravitation on the surface of the earth; viz. as 1·9 to 1·0. So that, the 100 lbs (terrestrial weight) of iron would weigh about 190 lbs. on the planet.

It was stated at the commencement of this work that the interest of general education in the higher sense was to be its primary purpose. Having this purpose in view, we will here notice a result of the indefinite and unsound teaching on this subject at the present time. It is scarcely too much to say that each scientific writer is considered at liberty to form an opinion (or theory) of his own: for example, we might proceed in this way:—taking again the intensity of gravity at the surface of the condensed globe, equal in density to the earth, at 7 times that of terrestrial superficial gravity, and then expanding the globe to its present size, the distance from the centre will be increased in the ratio of 11 to 7; and therefore, since the mass has not been increased, decrease of attractive force on the surface will be simply, in the inverse proportion of those numbers. . viz., as 7 to 11. (*i. e.*, reduced from 7 times to very nearly 4½ times the intensity of gravitation on the surface of the earth). Or, again, we might proceed by taking into consideration only the increased area and state the case thus:—since the angular magnitude of the globe has been increased in the ratio of 11 to 7, the area must have increased in the ratio of

11^2 to 7^2, that is, in about the proportion of $2\frac{1}{2}$ to 1, consequently the attractive force has to be divided and extended over this greater area and must diminish in the same proportion; thus we find the 7 times intensity reduced to 2·8 (times rather less than 3 (times) that of the force on the earth's surface.

Although it seems to us that the two conclusions thus arrived at, would not be more unreasonable than some which are now taught, they would, neither of them, be reasonable conclusions, because not consistent with the recognized laws belonging to the subject. They may, however, here serve a useful purpose in assisting to bring to the test of fact the opinion (as we would term it, rather than a distinct theory) that internal gravitation operates radially from the centre; now if such a hypothesis were supported by fact the first of these two conclusions might be quite reasonable, namely, that the intensity of gravity on the surface of Jupiter is actually about $4\frac{1}{2}$ times greater than on the surface of the earth.

By internal gravitation we mean the gravitating influence exerted by the mass of the planet on the matter of which it is composed, and on bodies on its surface; and we say, in reference to this, there is evidence in works considered scientific that a vague theory of this kind, viz., that of an influence distributed radially from the centre, (or of a concentrated influence operating from the centre,) is quite frequently entertained. When, however, the question is as to the influence of one planet on another at some distance, this theory is no longer countenanced. But, why? If it is concluded that light and radiant heat are distributed radially, and if the theory of an internal radial distribution of gravitation is more than half countenanced, if not quite admitted, why should it appear almost absurd* to suggest that gravitation may be exter-

---

*Observe that in a sphere the matter is distributed radially around a central point, and if the size of a given sphere be enlarged by the addition of more matter, the additional matter is also distributed nally radiated in every direction? Nevertheless, we can scarcely doubt there are at the present time not a few scientific men who would express an unhesitating belief as to the one, and who would look upon, and perhaps term the other, too preposterous for serious consideration. Leaving aside for the moment the argument as to the actual fact, it cannot but be evident that herein is a prejudice which has not only acquired strength by time, but is also in itself a result of unsound education. Observe that we do not say the theory at present held—viz., that light and heat are distributed radially, and gravitation only in the direction of a recipient, is absurd or untenable; nor do we wish to insist dogmatically (*i. e.*, by assertions unsupported by fact or by reasonable demonstration) that it is necessarily wrong; but, whether it be in fact right or wrong, *that* is just as much an unsound prejudice which, in the present state of scientific knowledge, makes it difficult to entertain a proposition that all three (influences) viz., light, radiant-heat, and gravitation, are in the same case, and that...if the two are distributed radially in every direction...so is the other one, or...if the one is only emitted in the direction of a recipient (or reciprocator)...such will be likewise the case with the other two. Admitting that such a prejudice exists, we opine that it has now become so wide-spread and so much a part of what is considered scientific education that no one is altogether free from its influence, nor can, without a certain degree of mental violence, (exertion of the will)

radially; therefore, since each portion or particle of matter possesses its proportion of gravitating influence, the radial distribution of additional matter has, as its accompaniment (consequent), an addition of radially distributed influence. But the question, to which our argument is addressed, is the relative situation and number of the definite points or places, exterior to the mass itself, at which the influence, originating in that mass, is exerted. The surface of a globe may be supposed to be entirely covered with gravitating bodies? True—but, then, it is evident that these bodies will mutually react upon each other, and, if of uniform magnitudes, they will simply constitute an addition to the mass of the globe.

free himself from its influence. It is perhaps the most difficult, and perhaps the most important, of all the duties of Science to distinguish such cases from those of propositions which it is not right to entertain, because, not only baseless in fact, but also, not in harmony with fact, *i. e.*, with the facts of creation, — and which, therefore, are unreal and monstrous. In either case, or class of cases, there is a mental difficulty in receiving the proposition ; the will has to be exerted and some resistance has to be overcome. The great and very important difference is that in the one case, there is obedience to legitimate authority, and a legitimate use of the mental faculties in accepting that which is authorized and commended by reason .. in the other, there is submission to illegitimate authority, and a wilful perversion and mis-use of the mental faculties in accepting that which is unauthorized and forbidden by reason. Where a false proposition or theory is of a very simple character, and the mind has been prepared by sound education, the rejection of it is very easy, and seems, as it may be termed, quite natural ; e.g.—if we reverse one of Euclid's axioms, and propose for acceptance : that ...the whole is, or may be, greater than its parts ; or ...of two lines each of which is equal to a third, one is greater or less than the other ; or that...two added to two may, under some peculiar circumstances, make five, or some quantity of number other than exactly four. But, if the false proposition, instead of being simple, is of a complex, or what is called, abstruse character, and instead of the mind being prepared by education to

Correction. PAGE V.

For...the whole is, or may be, 'greater than its parts;' ...*read*, 'less than its part;'...or, 'greater than the sum of its parts.'

nally radiated in every direction? Nevertheless, we can scarcely doubt there are at the present time not a few scientific men who would express an unhesitating belief as to the one, and who would look upon, and perhaps term the other, too preposterous for serious consideration. Leaving aside for the moment the argument as to the actual fact, it cannot but be evident that herein is a prejudice which has not only acquired strength by time, but is also in itself a result of unsound education. Observe that we do not say the theory at present held—viz., that light and heat are distributed radially, and gravitation only in the direction of a recipient, is absurd or untenable; nor do we wish to insist dogmatically (*i. e.*, by assertions unsupported by fact or by reasonable demonstration) that it is necessarily wrong; but, whether it be in fact right or wrong, *that* is just as much an unsound prejudice which, in the present state of scientific knowledge, makes it difficult to entertain a proposition that all three (influences) viz., light, radiant-heat, and gravitation, are in the same case, and that...if the two are distributed radially in every direction...so is the other one, or...if the one is only emitted in the direction of a recipient (or reciprocator)...such will be likewise the case with the other two. Admitting that such a prejudice exists, we opine that it has now become so wide-spread and so much a part of what is considered scientific education that no one is altogether free from its influence, nor can, without a certain degree of mental violence, (exertion of the will)

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[illegible] the radial distribution of addi-

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(*Note A.*) We would distinguish in this way between a demonstrated theorem, a sound (scientific) theory, and an unsound or false theory.

A demonstrated theorem is a theory which has been completely (*i.e.*, scientifically) established by demonstration, and which, therefore, becomes recognized as, or occupies the place of, a fact, and thenceforth forms a part of science.

A (scientific) *theory* must be reasonable; it must not be, in any part of it antagonistic to, or irreconcilable with, known and recognized facts (*i.e.*, any of the facts of Creation); but it by no means follows that a theory, being reasonable at a particular time, may not become untenable when further progress has been made in the acquisition of knowledge. On the contrary, a theory, as distinguished from a (demonstrated) theorem, is usually, if not always, incomplete and uncertain, being in a greater or less degree based upon or dependant on assumption in place of actual fact and certainty. If, when the necessary knowledge has been acquired, the assumptions can be made certain or replaced by fact, then the theory becomes established (as a theorem), but if found to have been based on assumptions which increased knowledge, or a more correct and skilful application of knowledge, shows to be unsound, then the theory becomes untenable, and must be at once given up. Therefore, a theory *belongs* to science, and is of great use, and may, indeed, be considered quite indispensable as a legitimate means of investigating the unknown and of extending the domain of science; but it should not be considered to form a part of science until absolutely established.

An unsound or false *theory* is either altogether, or contains, in some part of it, that which is inconsistent with known facts and is therefore unreal, or, it may be, is based on assumptions which are unreasonable—that is, which are untrue or inconsistent with reality in an ideal sense. Such theories belong to unsound science, which is opposed and antagonistic to science.

A hypothesis may be considered a theory in a less complete or in a less formal shape.

An *assumption* belongs to ideal philosophy. To be intelligible, the assumption must be apparently real,—*i.e.*, it must appear to be supported by fact (consistent with the facts of Creation) To be sound, the assumption must be in fact (actually) real,—*i.e.*, it must be actually supported by fact.

(*Note B.*) The use of an assumption is to supply the place of a simple fact, or to supply one or more of the elements in a compound fact (theorem), in order that combination may be carried forward. When this process is conducted in the legitimate and correct manner, it frequently (always eventually) happens that the soundness of the assumption can be tested by the reality (reasonableness) of the compounds in which it is then one of the elements. Where an assumption is suspected to be unsound it is sometimes examined in this manner, by combining with other elements (known to be of themselves sound) into a compound, of which the unreasonableness is manifest. This is termed "the *argumentum ad absurdum*." *

* The *argumentum ad absurdum*, as used by Euclid, is an application of this process to the purpose of negative demonstration. Where, for example, the case is of such a kind that only a very few assumptions are possible (and since there must be *one* correct assumption) if *all these excepting one* are shown to be unsound, it follows reasonably that the *one* is sound.

Omission.

(NOTE C.) TO REASONABLE THEORIES.

PAGE VII.

It is of importance for the student to observe that one assumption may differ very much from another *in character.* To point the distinction we will suppose assumptions to be divided into two extreme classes. Those belonging to the first class may be termed empirical or inductive assumptions; these are more or less partially based on recognized fact or deduced therefrom, or, it may be, are, in a measure, inductive conclusions arrived at legitimately by reasoning or indirect evidence, and, although assumptions, and not wholly established or not known as a whole to be certainly true, they may contain certainty with respect to some, and perhaps to many, of their elements, and, accordingly, to some extent, and perhaps to a great extent, may partake of the nature of fact: in such a case, the value of the assumption as a basis or partial basis to the theory, into which it enters, is much dependent on whether the theory rests mainly on that part of the assumption known to be certainly true. Those belonging to the second class may be termed hypothetical or speculative assumptions; these, although, if to be considered as belonging to science, they must be, as already stated, not fundamentally unreasonable, may contain nothing more of the nature or character of fact than the possibility, and a greater or lesser measure of probability, that they may eventually be shown, when the necessary knowledge has been acquired, to be true or to contain certainty and truth.

SCIENCE

PHILOSOPHY HISTORY

I.S. AND N.S

KUKLOS

PART II.

DARKNESS AND LIGHT.

Objections to the undulatory theory of light.

The Ether. The supposed fluid thus named is usually spoken of by writers on optics as a hypothetical fluid; but such a use of the expression 'hypothetical' is apt to mislead... if the writer, who so uses the word, supposes at the same time that the undulatory theory of light is scientifically established. If the expression 'hypothetical' is merely intended to indicate that the supposed subtle fluid, the ether, cannot be directly taken cognizance of by the senses, its use is objectionable; because many natural as well as all ideal facts are in the same case; that is, they cannot be directly cognized by the senses. A belief that the undulatory theory of light is scientifically established, should include the belief that the existence of the ether is demonstrated by the observed facts and the legitimate reasoning belonging to that theory. If the *non existence* of the ether fluid were to be demonstrated, the undulatory theory of light, which is based upon its assumed existence, would necessarily have to be given up; and therefore if, or so long as, the existence of the ether is in any degree doubtful, so long must the theory itself be in doubt, and must not be considered as scientifically established—merely a theory, not a demonstrated theorem. The expression 'subsensible' as applied by Prof. Tyndall to the (supposed) ether fluid is much preferable to 'hypothetical,' *if* the theory *is accepted* as demonstrated. In Dr. Lardner's statement of the undulatory theory, quoted at page 3, a concise explanation of the supposed nature of the luminous ether will be found. Also in the remarks from Prof. Tyndall's lecture, page 47, wherein the sound and light-waves are compared, the material and gaseous nature of the subsensible fluid is very distinctly assumed: "Could you see the air through which sound-waves are passing, you would observe every individual particle of air oscillating to and fro." "Could you see the ether, you would also find every individual particle making a small excursion to and fro." The general object of this part of the lecture is to show the analogy between light and sound; but we can scarcely be incorrect in supposing that the more particular object is to demonstrate the existence of the subsensible ether-fluid by thus showing and illustrating the analogy.

A difficulty of a kind to make extreme caution necessary as to accepting the existence of the ether, until strictly demonstrated, is that the theory and the observed facts belonging to it together necessitate the assumption that the material subsensible fluid occupies all space, and that all other descriptions of matter, not absolutely opaque, must be considered porous, and as having their interior spaces all filled with ether. It appears that this undulatory theory of light became the subject of a conversation between Prof. Tyndall and Sir David Brewster, and that the latter stated his opinion as to the existence of the ether-fluid in the following words: "That his chief objection to the undulatory theory of light was that he could not think the Creator guilty of so clumsy a contrivance as the filling of (? *all*) space with ether in order to produce light." On which observation Prof. Tyndall, in his published lecture, page 40, makes this remark: "This, I may say, is very dangerous ground; and the quarrel of science with Sir David, on this point, as with many other persons on other points, is, that they profess to know too much about the mind of the Creator." These observations bring a subject directly under consideration distinct from, and of much greater importance than, the undulatory or other theory of light, and as it is also the subject to which, as stated in our introductory remarks, the purpose of our work is particularly directed, we shall include in our notice of them an examination of their significance in reference to the more generally important subject.

The subject thus brought particularly under consideration is the relation of science, or what is now considered as science, to the facts of creation and to the Creator Himself, and therefore to theology. It may be said that the observations were not particularly intended to be applied in this sense, but they are made public, and they define in a measure the position occupied at the present time by science, in this relation, according to the judgment of the speakers.

The observation of Sir David Brewster was certainly blameworthy as being expressed in irreverent language. An observation made in such terms under any circumstances cannot be reasonably regarded otherwise than as foolish and wrong; but if made deliberately and guardedly on a grave and important subject of science... and published... an observation so expressed must be considered blameworthy in a much higher degree. It should be remarked, however, on behalf of Sir David, that, in this case, his observation was probably in its form a careless off-hand expression of opinion not intended for the public. Apart from the very reprehensible form of the expression, the meaning of the objection, which Sir David may be understood as intending to convey, does not appear to us by any means unreasonable. The supposition of all space being filled with a material fluid for the purpose of producing effects at certain distant points, or in other words an omnipresent material fluid filling and pervading the universe for the production of one class or kind of effect only, does not seem to harmonize with what we do know of the Creator's work; but, on the contrary, it presents itself to the educated mind as a contrast to the directness and simplicity of the methods employed in other parts of creation. Notwithstanding the supposed attenuated subsensible characteristics of the fluid, the inconceivably enormous quantity of material required by the theory at once suggests improbability. If, however, the objection went no further than this, it might perhaps be answered with some degree of force by supposing that the ether fulfilled other important purpose or purposes with which we are as yet altogether unacquainted, but the objection does go much further, because, the ether fluid is, by the assumption, material, and upon this assumption the theory rests. Why, then, does not the ether, in obedience to the general law known to govern, and recognized as universally governing matter, collect around the centres of gravitating influence? Are we asked to suppose the ether to differ from all other varieties of matter, to be exempt from the influence of gravitation and at the same time to have other properties in common with the other descriptions, and to be controlled by some of the laws governing other kinds of matter; as, for example, to possess the property of elasticity, and to be capable of propagating an impulse by undulations of its particles? To admit such a proposition even as an assumption is extremely dangerous. It is to take leave of all certainty; to bid farewell to science, and to set sail without rudder or compass on the dark and treacherous ocean of metaphysics. Very much of the sound natural science now possessed by us is based on the certain knowledge that *gravitation* is a general law governing all matter. If this is uncertain, or is to be considered uncertain and open to controversy, where, then, are we to find scientific certainty in respect to the material world? If any one variety of matter may be exempt from such a general law, so also may other varieties. If the reply to this should be . . well, then, the ether in that sense is not material; it is evident that the undulatory theory of light must at once fall to the ground, because it rests upon the assumption that the ether is a material fluid, possessing (some of) the properties belonging essentially and distinctively to all matter.

Prof. Tyndall does not, however, make any direct reply to the objection of Sir David Brewster, but thereupon makes a statement in the name of science, against which statement we feel obliged, also in the name of science, to protest. The statement or remark is perhaps not quite so definite as to preclude the possibility of misapprehension as to its full meaning; but we are afraid that the meaning which it will be generally understood to convey is that the mind of the Creator, as displayed in, and made known to us by and through, the facts of creation, is not the proper and legitimate subject for science to occupy itself about. Moreover, a meaning may be understood to be indirectly included to the effect that the 'reason' or 'reasonableness' of the Creator is different *in kind* from the 'reason' or 'reasonableness' of human science. Now, assuming that either or both of these meanings are to be understood, we have to state, directly to the contrary, that the express object and purpose of science, in any high sense, is to obtain knowledge, a better and more perfect knowledge, and always more knowledge of the mind of the Creator; and that having obtained such knowledge, the legitimate and best employment of those possessed thereof is in teaching and making it known for the guidance of those who have not the opportunity to seek this higher kind of knowledge for themselves.

In reference to the (inferential) secondary meaning mentioned above, we would say that, if we had not an assured belief as to human reason being in harmony with, based upon, and the same *in kind* as the Reason of the Creator (*divine-reason*), science, in any higher sense, would lose its interest for us. A merely human science —which is not a part of universal science, and which, so far as any one man is concerned, is confined to the few years of a man's terrestrial existence—does not appear to us a very desirable or interesting pursuit, merely for its own sake. A man crammed full of scientific knowledge is not, therefore, necessarily, in a merely terrestrial sense, happier than a man who possesses but very little, nor is he likely to be physically stronger or better developed in consequence. Prof. Tyndall highly values scientific knowledge, so do we; he also expresses a sort of compassion for those who do not know, and a certain degree of contempt for those who contemn, science in the higher sense—and which feeling we also share; but it is *because* we believe that in acquiring scientific knowledge we are acquiring, and in communicating scientific knowledge we are communicating, that which belongs, not merely to a brief terrestrial, but, also, to a higher state of existence, and which may be considered as belonging to, and forming a part of, immortality. If we did not so believe, we should incline to the opinion that the practical-sense men who say, 'cui bono' when invited to engage in scientific pursuits, and who can see no use in science except as a means, or as furnishing the means of ameliorating the hardship of human existence and of increasing the amount of sensual ease and enjoyment—the practical-sense and common-sense men, who so argue, would, in that case, as it seems to us, have much the strongest argument; for other purpose or in other sense—for its own sake, for instance, or for a meaningless desire of being intellectually developed—the man ardently devoted to science may be justly regarded as indulging a taste for a foolish, and a not altogether harmless, hobby.

The objection which we have just stated, in the foregoing amplification of Sir David Brewster's objection, viz., the assumption of ether as a form of matter exempt from the influence of gravitation, appears to us to be, of itself, altogether fatal to the undulatory theory of light, and to render the acceptance of that theory scientifically inadmissable; but even if the supposition were to be entertained that this objection might be, in some way or other, surmounted, and the actual existence of the ether shown to be theoretically possible, there would yet remain, at least one serious difficulty which has been, as it seems to us, put aside, rather than dealt with and surmounted by the supporters of the theory. We allude to the kindred phenomena of radiant heat. The nature of this particular difficulty may be thus briefly stated:—the phenomena of light and of radiant heat are so analogous, so evidently allied and similar to each other in many respects that it is almost, if not quite, impossible in a reasonable sense to suppose the one effect (or class of effects) to result from the undulation of an elastic material fluid and not to suppose the other effect (or class of effects) to be produced in the like manner; but although there are very close analogies between the two kinds of effect (or classes of phenomena) in some respects, there are also differences of an essential and distinctive character, such that we should feel at least a very grave difficulty as to admitting even a theoretical supposition that a mere variation in the velocities of the undulations of the same fluid can occasion them. We say that the difference in the characteristics of light and radiant heat are too great, and of essentially too distinctive a kind, to allow the supposition that a certain number of undulations, or vibratory pulses of ether, taking place in a second of time may produce light, and that a certain lesser (or greater) number of vibratory pulses, in a second, may be productive of radiant heat. What is the alternative? To suppose the existence of two different omnipresent ethers?

Before leaving for the present the important question as to the position which science, in its higher signification, does or ought to occupy, we will make some remarks as to the meaning which correctly belongs to the term metaphysics, in its relation to science. The indefiniteness which constitutes one of the leading characteristics of metaphysics attaches in some degree even to the name or expression by which it is denoted, in such wise that probably no two scientifically educated persons could be found at the present time to agree as to what ought or ought not to be included in a strict sense under that title.

To make our remarks as brief and subjective as the nature of the subject will permit, we will give here one example of the confusion of thought, and inferential mystification instead of increased knowledge, which results from that indefiniteness.

In the work of Sir John Herschel, *Outlines of Astronomy*, from which we have made many quotations, we find a note, at the foot of page 212 as follows: "This condition is indispensable. Without it we fall into all those difficulties which M. Doppler has so well pointed out in his paper on Aberration. (Abhandlungen der k. boemischen Gesellschaft der Wissenschaften. Folge v. Vol. iii.) If light itself, or the luminiferous ether, be corporeal, the condition insisted on amounts to a formal surrender of the dogma, either of the extension or of the impenetrability of matter; at least in the sense in which those terms have been hitherto used by metaphysicians. At the point to which science is arrived, probably few will be found to maintain either the one or the other." The indispensable condition referred to is stated in the text above. "In whatever manner we consider light, whether as an advancing wave in a motionless ether, or a shower of atoms traversing space, (provided that in both cases we regard it as absolutely incapable of suffering resistance or corporeal obstruction from the particles of transparent media traversed by it.)" The words within the brackets expressing the condition insisted upon.

The doctrine of the impenetrability of matter is thus stated by *Lardner*, in his *Natural Philosophy*.*

(22) *All matter impenetrable.* Impenetrability is the

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* Extension is not distinguished by Lardner as one of the properties of matter; probably he considered it synonymous with magnitude, or, perhaps, as merely expressing the existence and natural reality of matter.

"quality in virtue of which a body occupies a certain space, to the exclusion of all other bodies. This idea is so inseparable from matter, that some writers affirm that it is nothing but matter itself: that is, that when we say that a body is impenetrable, we merely say that it is a body.

However this be, the existence of this quality of impenetrability is so evident as to admit of no other proof than an appeal to the senses and the understanding. No one can conceive two globes of lead, each a foot in diameter, to occupy precisely the same place at the same time. Such a statement would imply an absurdity, manifest to every understanding."

23. "*Gaseous bodies impenetrable.* Even bodies in the gaseous form, the most attenuated state in which matter can exist, possess this quality of impenetrability as positively as the most hard and dense substances."

Now this doctrine, or teaching, as to one of the recognized properties of matter, is, if we understand Herschel's remarks aright, considered by him to be a metaphysical dogma, and no longer tenable in a strictly scientific sense; and, moreover, he supposes, it would seem, there are now but few persons of scientific education who think differently.

Two distinct questions are herein involved, both of them of much importance. The one is, whether that science, which has and professes a definite belief in the existence of matter, such as defined by Lardner, is sound science or the reverse. The other question is, the correct use of the expression 'metaphysics.' It is with respect to the last of these questions that we think it desirable to make here a few observations. The particular word 'metaphysics' may, of course, be used, as any word in nomenclature may be used, in whatever sense it may be generally agreed to use and understand it. But the important question here at issue is, (1st)—Whether all knowledge, all that is supposed to be, or goes by the name of, knowledge, of every kind and description, if only it be classified and systemized, is to be called and considered 'science;' or, (2nd)—Whether it is necessary there should be a distinction and separation of the certain and sound knowledge, from the uncertain and unsound.

Now the necessity of a division has been for a long time past generally recognized; for instance, those descriptions of classified knowledge which at an earlier period were admitted and highly reputed under the names 'astrology' and 'magic,' have, since the time of Francis Bacon, ceased to be considered a part of the classified knowledge belonging to science. If, therefore, it is right and proper to have a division and to have an expression 'science,' to denote collectively all the descriptions of knowledge considered to be beneficial and worth preserving and cultivating, it is likewise desirable to have a general expression to denote collectively those descriptions of knowledge, systematically classified or otherwise, which are of the opposite character from the former—that is, which are, or ought to be, under a strictly correct division, excluded from science. It is in this sense that we understand and purpose to use the expression 'metaphysics,' because certain classified descriptions of knowledge which ought, according to our judgment, to be excluded from science, are already particularly denoted by that expression, and, moreover, the indefinite and mystical meaning which the expression suggests to the generality of people makes it the more suitable as a collective expression for all kinds of knowledge which are indefinite, uncertain and unsound.

The mode of its use by Sir John Herschel, in the preceding quotation is, therefore, according to our view, an inversion of the correct or desirable application of the term, 'metaphysics;' for, therein it is used to denote the *definite* teaching—*i. e.*, the recognition and intelligible definition of the natural reality; and, by inference, the expression 'science' may be understood to apply to the *indefinite* teaching—viz., to that which is indefinite and unintelligible.

We find in the quotation already given from Prof. Tyndall * the indirect statement of, what appears to us to constitute, another difficulty in the way of accepting the undulatory theory. That statement reads: "All space is filled with matter oscillating at such rates. From every star waves of these dimensions move with the velocity of light, like spherical shells, outwards. And in the ether, just as in the water, the motion of every particle is the algebraic sum of all the separate motions imparted to it. Still, one motion does not blot the other out; or, if extinction occur at one point, it is atoned for at some other point. Every star declares by its light its undamaged individuality, as if it alone sent its thrills through space."

It is an observed fact and unquestionably true that every star does so declare its undamaged individuality; but how can these undulations, which are defined by the theory to be of the nature of waves or of progressive oscillations resulting from motion in the particles of a material fluid; . . . we ask, how can these undulations reach us in innumerable quantity at the same time and from every direction, and yet not damage, modify, interrupt, or, in any way, interfere with each other? It is not only from every star and every luminous body that these undulations have to reach the eye in undamaged individuality, but, if we apprehend the explanations of the theory aright, also from every visible object. At the rate of only one undulation in a second it would be embarrassing even to imagine these undulations crossing each other in every direction without mutual interruption, but what is the estimate of the number by those who support the theory? "All these waves enter the

* Lectures on Light, page 55.

eye, and hit the retina at the back of the eye, in one second. The number of shocks per second necessary to the production of the impression of red is, therefore, four hundred and fifty one millions of millions. In a similar manner, it may be found that the number of shocks corresponding to the impression of violet is seven hundred and eighty-nine millions of millions. All space is filled with matter oscillating at such rates." *

We do not find any explanation of this difficulty even attempted; an occasional or possible interference is alluded to by the remark that "if extinction occurs at one point, it is atoned for at some other point;" but, with the various effects, classed as the phenomena of interference and belonging to Optics and Acoustics respectively, in mind, we feel only that this last remark increases our inability to accept the proposition by making the impossibility of reconciling the theory with the known facts of science still more apparent.

In our objections to the *undulatory theory* of light we have not directly included the *emission* or *corpuscular* theory of Newton. Although that theory (the corpuscular) does not necessitate the supposition of an omnipresent universal ether, it is nevertheless open to obvious and fatal objection.

The explanation of the theory furnished by *Lardner* has been already quoted. "In the corpuscular theory, which was adopted by Newton as the basis of his optical inquiries, light is considered as a material substance, consisting of infinitely minute molecules which issue from luminous bodies and pass through space with prodigious velocities." It would necessarily follow, as a corollary to this theory that, when the sun has continued to shine for any length of time on a body which absorbs the light, a certain appreciable amount of the material projected from the sun, as light, will remain in that body, by which the gravity and mass of the body

* Lectures on Light, page 55.

will be increased. Now if this actually happened it could not long remain unnoticed. Lardner himself remarks, in reference to the hypothetical particles which, according to the theory, issue from luminous bodies, it is necessary to suppose that they are so minute as to be altogether destitute of inertia or gravity. "The strongest beam of sunlight acting upon the most delicate substance, upon the fibres of silk, or the web of the spider, or upon gold-leaf, does not impress upon them the slightest perceptible motion. Now, in order that a particle of matter endued with a velocity so great should have no perceptible momentum, it is necessary to suppose it to be almost infinitely minute." It is evident that Dr. Lardner, in writing this, must have been misled by the theory itself and the authority of Newton into stating a supposition which it is not scientifically permissible to entertain. By the expression particles almost infinitely minute is meant particles extremely small, *i. e.*, particles of very small size. But the gravity of a material substance, whatever its size may be, cannot be got rid of by dividing and sub-dividing it into very small or into extremely minute parts; its gravity cannot in such a manner be even lessened. The sum of the gravities of the very small, or of the extremely minute parts will exactly make up the gravity which the entire body possessed, previously to its division. Take, for instance, a pound by weight of any substance, and suppose it to be divided into a million parts, each of the parts being exactly similar and of the same size; then, each of those parts will weigh the one millionth of a pound, and, if one of them were to be again divided into a thousand parts, then one of those products of the sub-division would weigh the $\frac{\text{one millionth of a pound}}{\text{one thousand}}$ *i. e.* $\frac{1}{1000,000,000}$. The last particles would be very small, but nevertheless, if a thousand millions of them were projected by the sun in the course of an hour onto any one particular spot, a quantity of the material amounting to a pound in weight of the material would be the aggregate product at the end of that time. And again, how is matter, whether the particles be large or small, to move with an enormous velocity without having or acquiring momentum? *Gravity*, when motionless, [*i. e.*, when restrained from moving] and *momentum*, when in motion, are two of the characteristic properties of *matter*, by which is meant *some material thing* whether it be an aggregated mass of enormous bulk such as the planet Jupiter or the most minute particle that can be imagined. Dr. Lardner also states that: "The law of refraction is explained by supposing that such molecules are subject to an attraction towards the perpendicular when they enter a denser, and by a repulsion from it when they enter a rarer medium." Now this is no explanation in a scientific sense; so far from it, such a supposition is inadmissible unless supported by some proof or evidence outside the theory. There is no support in this case, but on the contrary the suggestion is quite gratuitous and altogether improbable. Why should a molecule of matter be attracted by a perpendicular to a denser, or be repelled by a perpendicular to a rarer medium?

It has been long since established as a fact, by the results of numerous careful experiments and observation, that a ray of light, on entering a denser from a rarer medium is refracted towards a perpendicular to the surface at which it enters, and, on entering a rarer from a denser medium, is refracted from a perpendicular to the surface of the rarer medium, at which it enters. When asked to give a reason, it is scientifically correct to say in reply that it is according to, or in obedience to, the law of the refraction of light, which is recognised as an established law belonging to the science of Optics, because demonstrated by the observed facts of which, or from which, it may be said to be a generalization. But when we wish to proceed further, and to explain the particular nature and properties of the ray of light which is so refracted, and to refer the law of the refraction of light to a more general or primary law; and thus to explain *particularly* the cause of the ray being refracted according to the law; it will not be then in accordance with the rules of sound science to invent a cause expressly for the purposes of the explanation; namely, to suppose a unique cause unsupported by experimental evidence or by analogy; such, for example, as a force elsewhere unknown or unrecognized, or a known force as acting in a manner unprecedented and elsewhere unobserved. To do this would be not to explain, but to build up prejudice in the way of scientific explanation. If more sound and certain knowledge cannot be obtained on a particular subject, it is unadvisable to dilute with uncertain, and worse then useless to vitiate with false and unsound knowledge, that which we do already possess.

Both of these theories, the undulatory and corpuscular theories of light, which have been successively accepted and adopted as forming a part of optical science, have one primary basis in common, namely, the velocity of light, which it is assumed was antecedently established as an observed fact.

Since we have objected, for reasons more or less fully stated, to the acceptance of either of these theories as belonging to sound science in the sense of demonstrated theorems, we propose now to examine the evidence as to the alleged velocity of light which it is assumed was antecedently established.

The history of this (assumed) discovery or observation, we find from the general record to be briefly as follows;

Lardner's Astronomy.

(2959). "*Motion of light discovered and its velocity measured.*

Soon after the invention of the telescope, Rœmer, an eminent Danish astronomer, engaged in a series of ob-"

"servations, the object of which was the discovery of the exact time of the revolution of one of these bodies around Jupiter. The mode in which he proposed to investigate this was by observing the successive eclipses of the satellite, and noticing the time between them.

Now if it were possible to observe accurately the moment at which the satellite would, after each revolution, either enter the shadow or emerge from it, the interval of time between these events would enable us to calculate exactly the velocity and motion of the satellite. It was, then, in this manner that Rœmer proposed to ascertain the motion of the satellite. But, in order to obtain this estimate with the greatest possible precision, he proposed to continue his observations for several months.

Let us, then, suppose that we have observed the time which has elapsed between two successive eclipses, and that this time is, for example, forty-three hours. We ought to expect that the eclipse would recur after the lapse of every successive period of forty-three hours.

Imagine, then, a table to be computed in which we shall calculate and register before hand the moment at which every successive eclipse of the satellite for twelve months to come shall occur, and let us conceive that the earth is at *A*, at the commencement of our observations; we shall then, as Rœmer did, observe the moment at which the eclipses occur, and compare them with the moments registered in the table.

Let the earth, at the commencement of these observations, be supposed at *E*. fig. 756, where it is nearest to Jupiter. When the earth has moved to *E*," it will be found that the occurence of the eclipse is *a little later* than the time registered in the table. As the earth moves, from *E*." towards *E*.‴, the actual occurrence of the eclipse is more and more retarded beyond the computed occurrence, until at *E*.‴ in conjunction, it is found to occur about sixteen minutes later than the calculated time.'

" By observations such as these, Rœmer was struck with the fact that his predictions of the eclipses proved in "

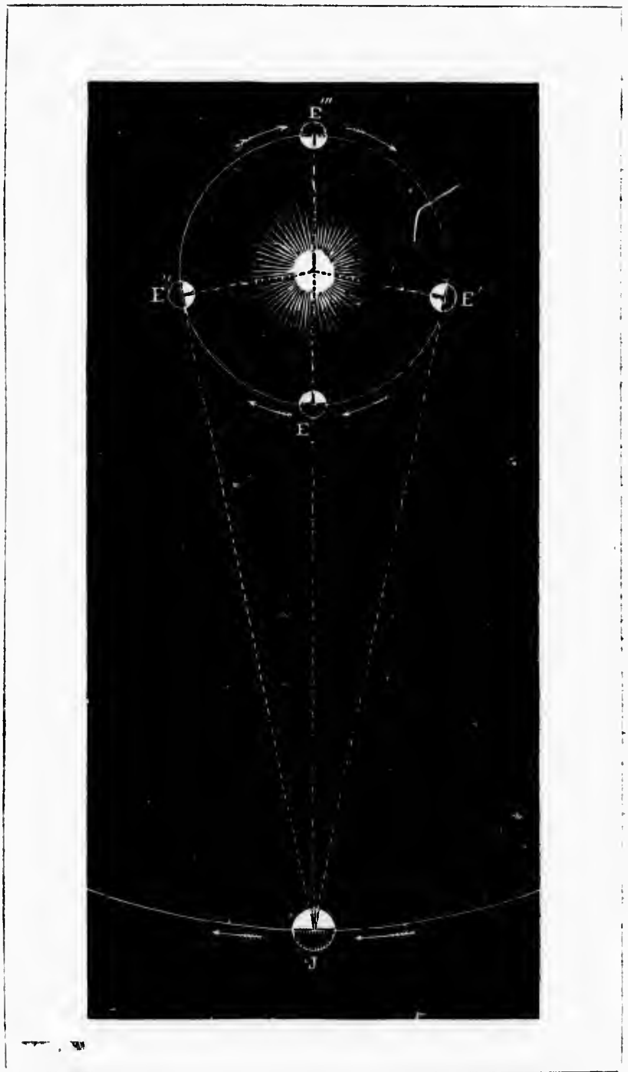

Fig. 756.

" every case to be wrong. It would at first occur to him that this discrepancy might arise from some errors of his observations; but, if such were the case, it might be expected that the result would betray that kind of irregularity which is always the character of such errors. Thus, it would be expected that the predicted time would sometimes be later, and sometimes earlier, than the observed time, and that it would be later and earlier to an irregular extent. On the contrary, it was observed, that while the earth moved from *E.* to *E.'''*, the observed time was continually later than the predicted time, and moreover, that the interval by which it was later continually and regularly increased. This was an effect, then, too regular and consistent to be supposed to arise from the casual errors of observation, it must have its origin in some physical cause of a regular kind. The attention of Rœmer being thus attracted to the question, he determined to pursue the investigation by continuing to observe the eclipses. Time accordingly rolled on, and the earth, transporting the astronomer with it, moved from *E.'''* to *E.'* It was now found, that though the time observed was later than the computed time, it was not so much so as at *E.'''*, and, as the earth again approached opposition, the difference became less and less, until, on arriving at *E.*, the position of opposition, the observed eclipse agreed in time exactly with the computation. From this course of observation it became apparent that the lateness of the eclipse depended altogether on the increased distance of the earth from Jupiter. The greater that distance, the later was the occurrence of the eclipse as apparent to the observers, and on calculating the change of distance, it was found that the delay of the eclipse was exactly proportional to the increase of the earth's distance from the place where the eclipse occurred. Thus, when the earth was at *E.'''* the eclipse was observed sixteen minutes, or about 1000 seconds, later than when the earth was at *E.* The diameter of the orbit of the " "earth *E. E.*'" measuring about two hundred millions of miles, it appeared that that distance produced a delay of a thousand seconds, which was at the rate of two hundred thousand miles per second. It appeared, then, that for every two hundred thousand miles that the earth's distance from Jupiter was increased, the observation of the eclipse was delayed one second.

Such were the facts which presented themselves to Rœmer. How were they to be explained? It would be absurd to suppose that the actual occurrence of the eclipse was delayed by the increased distance of the earth from Jupiter. These phenomena depend only on the motion of the satellite and the position of Jupiter's shadow, and have nothing to do with, and can have no dependence on, the position or motion of the earth, yet unquestionably the time they *appear* to occur to an observer upon the earth, has a dependence on the distance of the earth from Jupiter.

To solve this difficulty, the happy idea occurred to Rœmer that the moment at which we see the extinction of the satellite by its entrance into the shadow is not, in any case, the very moment at which that event takes place, but sometime afterward, viz., such an interval as is sufficient for the light, which left the satellite just before its extinction, to reach the eye. Viewing the matter thus, it will be apparent that the more distant the earth is from the satellite, the longer will be the interval between the extinction of the satellite and the arrival of the last portion of light, which left it, at the earth; but the moment of the extinction of the satellite is that of the commencement of the eclipse, and the moment of the arrival of the light at the earth is the moment the commencement of the eclipse is observed.

Thus Rœmer, with the greatest felicity and success, explained the discrepancy between the calculated and the observed times of the eclipses; but he saw that these circumstances placed a great discovery at his hand. In " "short, it was apparent that light is propagated through space with a certain definite speed, and that the circumstances we have just explained supply the means of measuring that velocity.

We have shown that the eclipse of the satellite is delayed one second more for every two hundred thousand miles that the earth's distance from Jupiter is increased, the reason of which obviously is, that light takes one second to move over that space; hence it is apparent that the velocity of light is at the rate, in round numbers, of two hundred thousand miles per second.

By more exact observation and calculation the velocity is found to be 192,000 miles per second, the time taken in crossing the earth's orbit being 16*m*, 26'.6*s*."

Having herein the history and particular definition of the observed facts upon which the theory of the velocity of light is supposed to be directly based, we will now take, also from the same work (*Lardner's Astronomy*), the more general explanation of the phenomena belonging to these facts, in order that the whole of the case may be clearly understood.

Eclipses, Transits, and occultations of the Jovian System.

(2950) "The motions of Jupiter and his satellites, as seen from the earth, exhibit, from time to time, all the effects of interposition. Let *J. J.'* fig. 810, represent the planet, *J. f. J.'* its conical shadow, *S. S* the sun, *E.* and *E.'* the positions of the earth when the planet is in quadrature, in which position the shadow *J. f. J.'* is presented with least obliquity to the visual line, and therefore least foreshortened, and most distinctly seen. Let *b. b.' d.' d.* represent the orbit of one of the satellites, the plane of which coincides nearly with that of the planet's orbit, and, for the purposes of the present illustration, the latter may be considered as coinciding with the ecliptic without producing sensible error. From *E.* suppose the visual lines *E. J.* and *E. J'*, to be drawn, meeting the path of the satelite at *d.* and *g.*, and at *a.'* and *b.'*, and in like manner, let the corresponding visual lines from *E.'* meet it at *d.'* and *g.'*, and at *a.* and *b.* Let *c.* and

Fig. 810.

c.' be the points where the path of the satellite crosses the limits of the shadow, and *h.* and *h.'*, the points where it crosses the extreme solar rays which pass along those limits.

If *l.* express the length *J.f.* of the shadow, *d.* the distance of the planet from the sun in semi-diameters of the "planet, and r. and r'. the semi-diameters of the sun and the planet respectively,

$$\text{we shall have (2917)} \quad l = d \times \frac{r'}{r - r'}$$

$$\text{But } d = 11227 \qquad r = 441000 \qquad r' = 44000$$

$$\text{and therefore } l = 11227 \times \frac{44}{441 - 44} = 1247:$$

that is to say, the length of the shadow is 1247 semi-diameters of the planet. Now, since the distance of the most remote satellite is not so much as 27 semi-diameters of the planet (2760), and since the orbits of the satellites are almost exactly in the plane of the orbit of the planet, it is evident that this will necessarily pass through the shadow, and almost through its axis, every revolution, and the lengths of their paths in the shadow will be very little less than the diameter of the planet.

The fourth satellite, in extremely rare cases, presents an exception to this, passing through opposition without entering the shadow. In general, however, it may be considered that all the satellites in opposition pass through."

(*Note. This last statement about the fourth satellite appears very remarkable in connection with that which precedes it, and with the great breadth of the shadow. But if we assume a moderate amount of vertical deviation above and below the orbital plane of the planet's equator, it becomes intelligible that the fourth satellite may sometimes pass through opposition without entering the shadow.*)

(2951) "*Effects of interposition.*—The planet and satellites exhibit, from time to time, four different effects of interposition."

(2952) "1st. *Eclipses of the satellites.*—These take place when the satellites pass behind the planet. Their entrance into the shadow, called the *immersion*, is marked by their sudden extinction. Their passage out of the shadow, called their *emersion*, is manifested by their being suddenly relighted."

(2953). 2nd. "*Eclipses of the planet by the satellites.*—When the satellites, at the periods of their conjunctions, pass between the lines *S J*, and *S′ J′*, their shadows are projected on the surface of the planet in the same manner as the shadow of the moon is projected on the earth in a solar eclipse, and, in this case, the shadow may be seen moving across the disk of the planet, in , direction parallel to its belts, as a small round and intensely black spot."

(2954). 3rd. "*Occultations of the satellites by the planet.*—When a satellite, passing behind the planet, is between the tangents *E.J.a′.*, and *E.J′.b′*, drawn from the earth, it is concealed from the observer on the earth by the interposition of the body of the planet. It suddenly disappears on one side of the planet's disk, and as suddenly reappears on the other side, having passed over that part of its orbit which is included between the tangents. This phenomena is called an occultation of the satellite."

(2955). "*Transit of the satellites over the planet.*—When a satellite, being between the earth and planet, passes between the tangents *E.J.* and *E.J′.*, drawn from the earth to the planet, its disk is projected on that of the planet, and it may be seen passing across, as a small brown spot, brighter or darker than the ground on which it is projected, according as it is projected on a dark or bright belt. The entrance of the satellite upon the disk, and its departure from it, are denominated its *ingress* and *egress*."

(2956). "*All these phenomena manifested at quadrature.* When the planet is in quadrature, and the shadow therefore presented to the visual ray with least effect of foreshortening, all these several phenomena may be witnessed in the revolution of each satellite.

The earth being at *E.* or *E.′*, the visual line *E.J.* or *E.′ J.′* crosses the boundary *x.′* or *x.* of the shadow at a distance *x.′ J.′* or *x. J.*, from the planet, which bears the same ratio to its diameter as the distance of Jupiter from "

" the sun bears to the distance of the earth from the sun, as is evident from the figure. But Jupiter's distance from the sun being five times that of the earth, it follows that the distance *x.J.* is five diameters, or ten semi-diameters, of the planet. But since the distance of the first satellite is only six, and that of the second somewhat less than ten, semi-diameters of the planet, it follows that the paths of these two will lie within the distance *x.J.* or *x.'J.'*

The planet being in quadrature 90° behind the sun, the earth will be at *E.* and the entire section *c. c'.* of the shadow, at the distances of the third and fourth satellites (which are 15 and 27 semi-diameters of the planet respectively), will be visible to the west of the planet, so that when these satellites, moving from *b*, as indicated by the arrow, pass through the shadows, their immersion and emersion will be both manifested on the west of the planet, by their sudden disappearance and reappearance on entering and emerging from the shadow at *c.* and *c.'* But the section of the shadow, at the distances of the first and second satellites, being nearer to the planet than *x.x.'* will be visible only at its western edge, the planet intercepting the visual ray directed to the eastern edge. The immersion, therefore, of these will be manifested by their sudden disappearance on the west of the planet, at the moment of their immersion; but the view of their emersion will be intercepted by the body of the planet, and they will only reappear after having passed behind the planet.

The third and fourth satellites, after emerging from the shadow at *c'.* and appearing to be re-lighted, will again be extinguished when they come to the visual ray *E. J. a'.* which touches the planet. The moment of passing this ray is that of the commencement of their occultation by the planet. They will continue invisible until they arrive at the other tangential visual ray *E. J.' b.'*, when they will suddenly reappear to the east of the planet, the occultation ceasing."

"In the cases of the first and second satellites, the commencement of the occultation preceding the termination of the eclipse, it is not perceived, the satellite at the moment from the interposition of the edge of the planet not having yet emerged from the shadow. In these cases, therefore, the disappearance of the satellite at the commencement of the eclipse, and its reappearance at the termination of the occultation, alone are perceived, the emersion from the shadow being concealed by the occultation, which has already commenced, and the disappearance at the commencement of the occultation being prevented by the eclipse not yet terminated.

When the satellite, proceeding in its orbit, arrives at *h.'* its shadow falls upon the planet, and is seen from the earth, at *E*, to move across its disc as a small black spot, while the planet moves from *h.'* to *h*.

When the planet arrives at *g.* it passes the visual ray *E. J.'* and while it moves from *g.* to *d.* its disc is projected on that of the planet, and a transit takes place, as already described.

Thus, at quadrature, the third and fourth satellites present successively all the phenomena of interposition: 1st, an eclipse of the satellite to the west of the planet shows both immersion and emersion; 2nd, an occultation of the satellite by the planet, the disappearance and reappearance being both manifested; 3rd, the eclipse of the planet by the satellite; and 4th, the transit of the satellite over the planet."

(2957) "*Effects modified at other elongations.*—There is a certain limit, such as *e*, at which the emersion of the third and fourth satellites is intercepted, like that of the first, by the body of the planet. This is determined by the place of the earth from which the visual ray *e. J. e.'* is directed to the eastern edge of the section of the shadow at the planet's distance. Within this limit the phenomena for the third and fourth satellites are altogether similar to those already explained in the case of the first and second satellites seen from *E*."

"When the earth is between *s*., and *s'*. no eclipses can be witnessed. Those of the satellites are rendered invisible by the interposition of the planet, and those of the planet by the interposition of the satellites. When the earth is at *e*.' and *E*.', the phenomena are similar to those manifested at *e*. and *E*., but they are exhibited in a different order and direction. The occultation of the satellite precedes its eclipse, and the latter takes place to the east of the planet. In like manner, the transit of the satellite precedes the eclipse of the planet." *

In carefully examining the record of the phenomena together with the explanation contained in the foregoing, we particularly note the very positive assumption that "these phenomena depend only on the motion of the satellites and the position of Jupiter's shadow, and *have nothing to do with*, and *can have no dependence on the position or motion of the earth*." Attentive consideration makes it apparent that this assumption is not supported; but that, on the contrary, it is contradicted by the recorded facts of the phenomena.

If we first suppose the earth's place to be at that part of its orbit nearest to Jupiter, and, having there noted the apparent magnitude (angular magnitude) of that planet, we then suppose the earth removed to the opposite extremity of the orbit to the place most distant from Jupiter, and again note the apparent magnitude of that planet, it is manifest that, the distance of the earth from the planet having increased by about 190 million miles, the apparent magnitude (angular magnitude) of the planet, as seen from the earth, must have decreased proportionally. Has this no particular relation to the phenomena, such as Lardner assumes that it has not? If the apparent magnitude of the planet is thus increased

* As the subject is of very great interest, we will also reproduce in the appendix (at the end of this supplement), Herschel's record and explanation of the same phenomena. To which the reader is accordingly referred.

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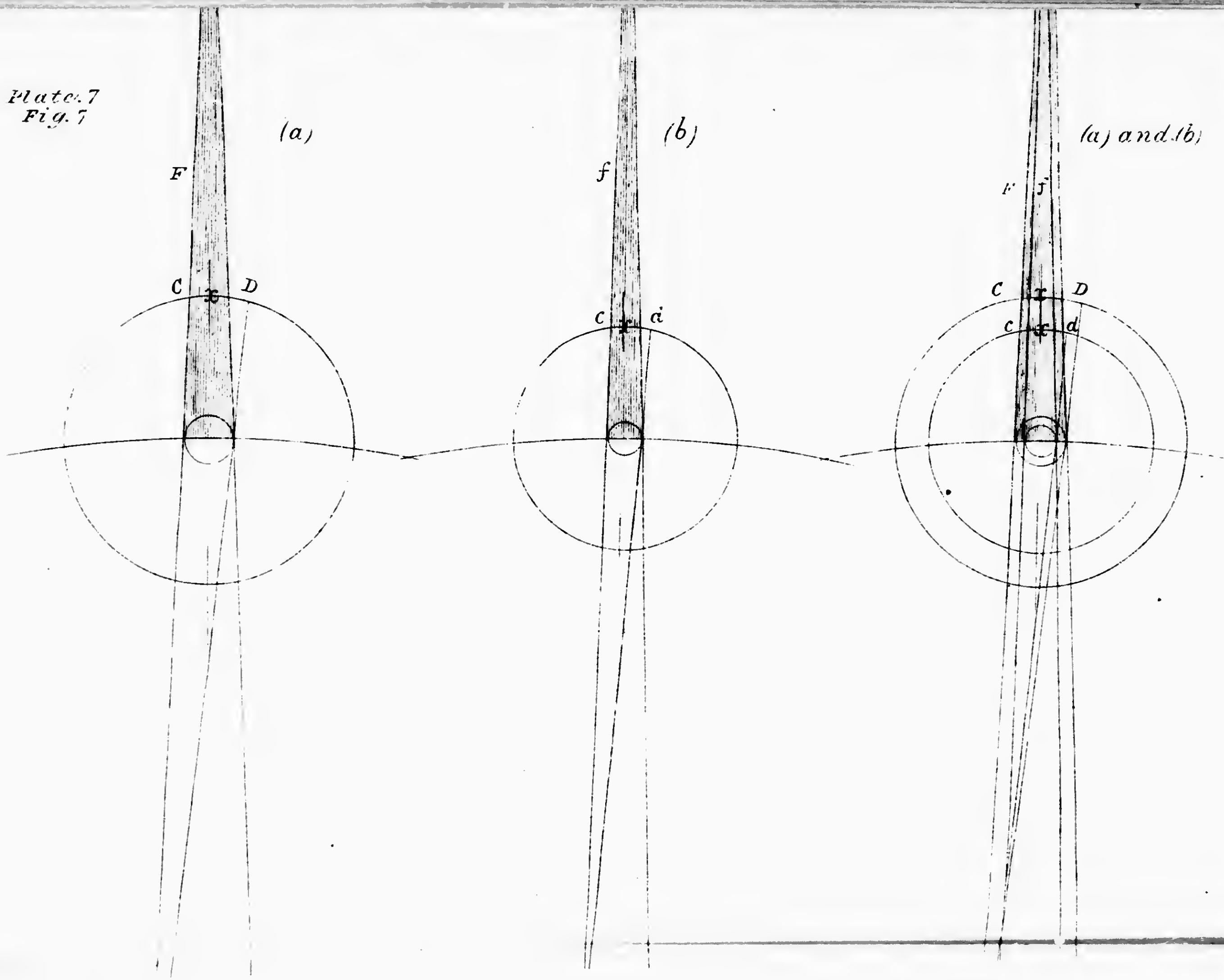
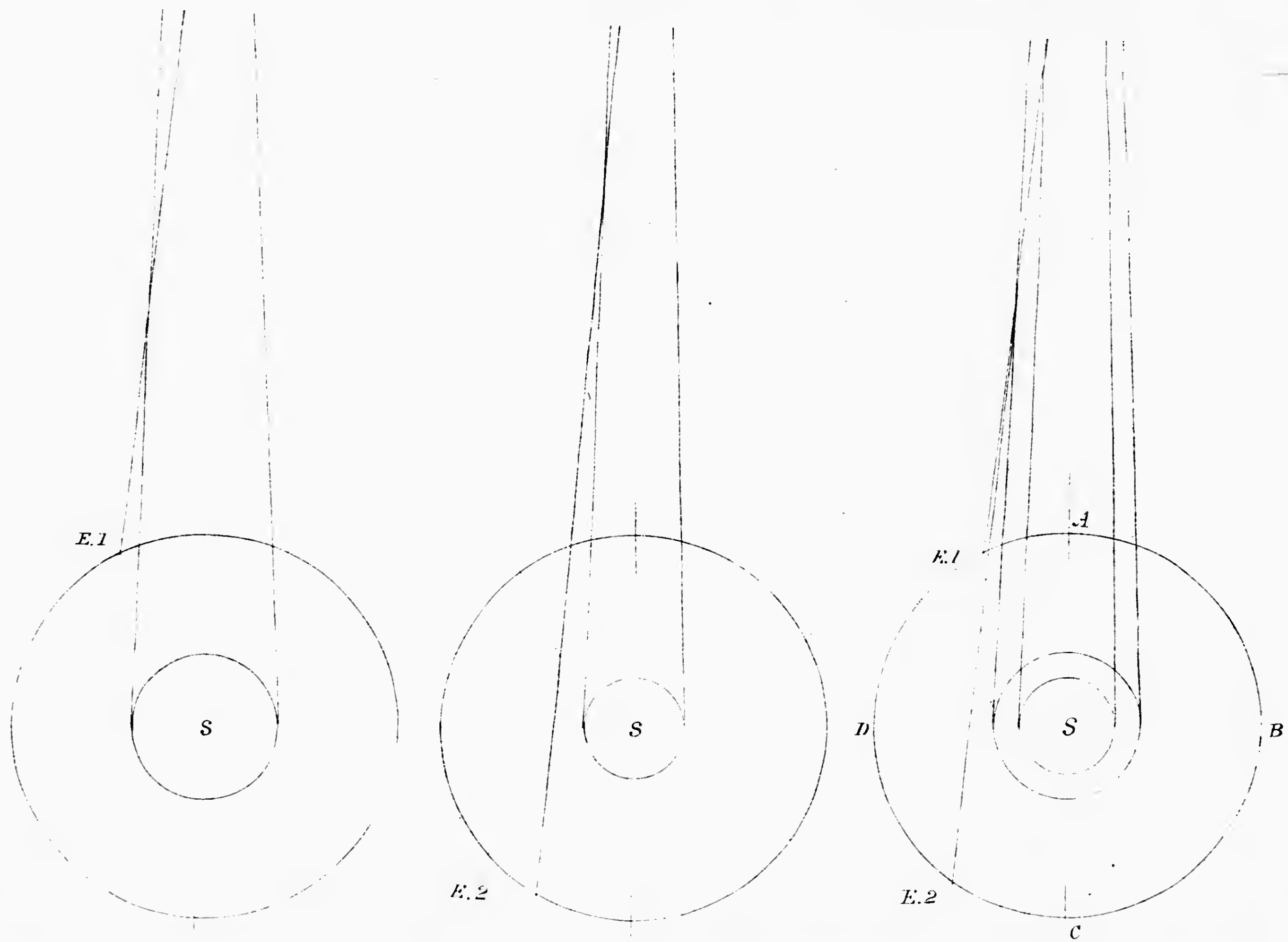
Plate 7
Fig. 7





Jupiters Shadow

and diminished by the variation of the relative place of the earth from which it is observed, so must likewise the apparent magnitude of the planet's shadow be increased and diminished. Is, then, the apparent time occupied by the satellite of Jupiter in making a complete revolution around that planet, as observed from the earth, subject to a similar increase and diminution, as a consequence of the variation in the distance? Certainly not. Then, the time occupied by the revolution of the satellite is absolute or constant, and the apparent magnitude of the shadow varies? Yes. Does it appear that this circumstance has been taken into consideration in explaining the phenomena? No: it appears to have been entirely overlooked or neglected. Will this circumstance, when taken into consideration, alone account for the variation in the time of the eclipse when observed from different places in the earth's orbit? Yes. For the whole difference in the time, viz., the 16 minutes? Yes.

In order to illustrate this, we will now take Figs. 5, and 6, (Plate 6) and Fig. 7, (Plate 7) in which *S.* represents the sun; the circle *A.B.C.D.* the earth's orbit; *E.*1 the earth's place in its orbit near to the place of least distance from the planet; *E.*2. the earth in the same angular direction from the planet, and not far from the place of greatest distance from Jupiter; *F.* the apparently greater shadow of the planet as seen from *E.*1.; *f* (inside *F.*) the (apparently) lesser shadow as seen from *E.*2.; *C.*', the point of immersion at the commencement of the eclipse, as seen from *E.*1; *c.* the similar point of immersion as seen from *E.*2.; *D.* the point of egress from the occultation of the satellite as seen from *E.*1; *d.* the similar point of egress (from the occultation) as seen from *E.*2; (*B.* would be the point of egress from the eclipse when observed from *E.*1, and *b.* the similar point of egress from the eclipse, when observed from *E.*2, but the emergence of the satellite from the eclipse cannot be visible at the earth, the planet itself being interposed, as explained by Lardner;) *x.* is the central point in the breadth of the shadow (of both the shadows.) Fig. 8, (Plate 8,) repeats a part of Fig. 7, on a larger scale, illustrating the phenomena of the satellite's eclipse, and making more distinct the relative points.

The time of a complete revolution of the satellite around the planet, whether observed from *E.*1 or from *E.*2, is the time occupied by the satellite in travelling *from* the point *x. to* the (same) point *x.* Now *c* is the boundary of the shadow as seen from *E.*2; *C.'* is the boundary of the shadow as seen from *E.*1; and the distance *cx.* is manifestly less than the distance *C'x.* Wherefore the satellite, travelling in the direction of the arrows, arrives at *C'* sooner than it arrives at c., and consequently the commencement of the eclipse, seen from *E.*1, is just so much earlier than is the commencement of the eclipse when seen from *E.*2.

It will be observed that in Fig. 6 we have omitted to increase the magnitude of the satellite's orbital circle; this is done for the purpose of furnishing in the first place a close comparison with the illustration given by Lardner* (as in fig. 5). In fig. 7. and fig. 8. the apparent orbital distance of the satellite is increased proportionally to the increase in the apparent magnitude of the planet.

It is quite true that in fact nothing is altered in respect to the satellite's eclipse; nor is there any actual alteration in the size of the planet. It is an apparent alteration only. The argument may, therefore, suggest itself that, since the apparent orbital distance of the satellite increases proportionally, so must likewise its apparent velocity; and consequently, the satellite will only occupy the same time to pass through the (apparent) greater breadth of shadow in the one case, which in the other case it takes to pass through the (apparent) lesser breadth. This is, likewise, quite correct. But attentive

* And also with that of Herschel which, as already stated, will be found in the Appendix.

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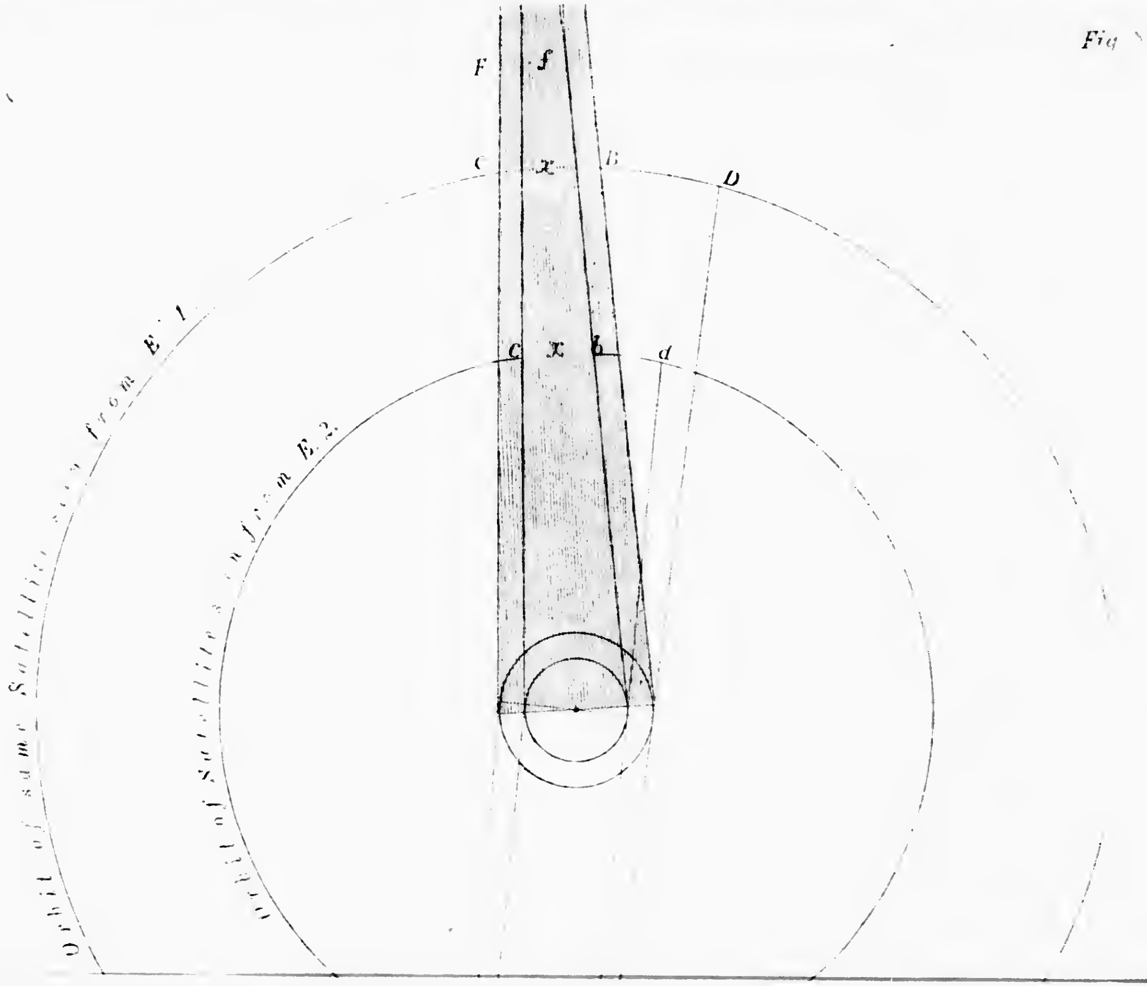
Plate

Fig



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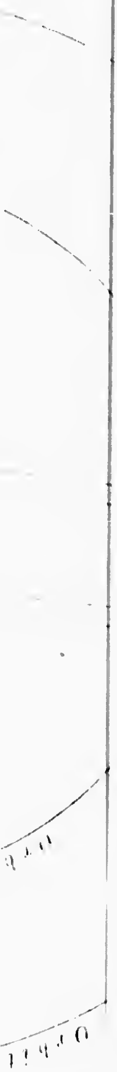
Orbit of same Satellite seen from E 1
Orbit of Satellite seen from E 2.



Fig

consideration will show that, in consequence of the (apparent) variation in the magnitude of the shadow having been overlooked or neglected, a variation has been unwittingly attributed to the place of x. Whether the place of x. be determined by the immersion and emersion of the satellite at the eclipse, or at the occultation, or, as it most probably was determined, from the immersion at the commencement of the eclipse and the egress at the termination of the occultation, in whichever way the place of x. was determined, it is dependent upon the observed time of the satellite's immersion and egress in such wise that if the theory of the velocity of light be entertained in respect to the satellite at its immersion or ingress, it must also apply to the satellite at its emersion and egress; and, consequently, also apply to the place of x. (The effect of this would be to shift the place of x. towards c. to the left, or the reverse), and thus occasion the inference that the commencement of the eclipse was so much later (or earlier) accordingly).

(*Note.*—It may be here remarked that even if the theory were in itself sound, it could not have the effect attributed to it. If it be allowed, for the sake of illustration, that, during one revolution of the satellite whilst the earth is receding from the planet, an apparent loss of time may take place in the manner and for the cause alleged, it is not true that during the next revolution of the satellite an increase in the apparent lateness of the eclipse would take place. Notwithstanding that the earth continued to recede, there could be no such increase in the apparent retardation, because the second revolution would be in precisely the same case as the first. If the first revolution terminates late, the second revolution must commence just so much late, and, therefore, if the earth did not continue to recede, the second revolution would, by the theory, appear to be more rapid than the first; but, since the earth is supposed to continue receding uniformly, so must the apparent times of the satellite's successive revolutions be equal under the circumstances supposed.

Assume that the velocity of light effects the commencement of any one eclipse,.. then must the assumption also apply to the termination of the revolution (of the satellite) to which that eclipse belongs, and to the commencement of the next revolution and to the termination of the eclipse.

It will perhaps make the manner in which the imperfect appreciation of the circumstances takes effect more readily understood, to consider in which way the practical observer would determine or endeavour to determine the starting (or terminal) point of the satellite's orbital revolution so as to coincide with the centre of the planet's shadow, viz., the point *x*. It has been already explained that both the immersion and emersion of the satellite can be only seen when the earth, as viewed from the planet, is near its greatest angle of elongation. When the earth is very close to its point of inferior conjunction no eclipse is visible, but both the ingress and egress of the occultation can be seen. In all other positions either the emersion of the eclipse is hidden by the occultation, and the ingress of the occultation hidden by the eclipse, or *vice versa*. Therefore to determine the point *x*., the commencement of the eclipse and termination of the occultation (or *vice versa*) must be taken together, or, what is more simple, the commencement of the eclipse, and the centre of the planet; that is to say, the relative angular direction of the centre of the planet and that of the side of the shadow where the satellite is immersed in entering it. Now, an observer who, having made his first observation when the earth was at the side of its orbit nearest the planet, on making a second observation when the earth had removed to the more distant side of its orbit, proceeded thus to determine the position, *i. e.*, by comparison of the angles, would evidently not succeed in doing so correctly unless he distinctly appreciated the variation in the apparent magnitude of the shadow together with the other circumstances of the case.* If he did not so appreciate it, he would reason thus :—everything varies proportionally and therefore the angles must remain relatively the same as before. The effect of this would be that finding he occupied the same angular position relatively to the centre of the planet as at the first observation, he would expect the eclipse to commence at the same relative angle as before, and the eclipse would appear to him as it appeared to Roemer, to commence late proportionally to the increased distance of the earth, although the communication of light from the satellite to his eye be in fact, or be assumed to be, instantaneous.

Since, therefore, we have to conclude that the theory of the velocity of light, upon which both the corpuscular and undulatory theories are primarily based, is not supported by the observed facts (phenomena) belonging to the eclipses of Jupiter's satellites, can these observed facts be utilized for any other purpose?

Let us consider what effects would be necessarily consequent upon some very small quantity of time being occupied by light, in its communication from the sun to Jupiter, and from Jupiter to the earth. The quantity of *time* attributed by the theory (of the velocity of light) to a certain (definite) quantity of *motion*, seems to us less than reason authorizes the mind to accept as a (reasonable) possibility; or, in other words, the velocity attributed to light, by that theory, seems to us greater than is scientifically conceivable, keeping in mind that, by the theory, this velocity represents the actual progressive motion of a variety or form of matter (*i. e.*, of a material substance). To simplify the consideration of the subject, however, we will assume, for the moment, the possibility of such velocity, and suppose it to be 8 minutes for a distance of

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* That is, he would not succeed in determining correctly the alteration in the position, or the position relatively to the former position at the time of the first observation, unless he appreciated the apparent variation.

100 million miles. We will take Jupiter's distance roughly at 500 million miles, and thus we obtain at once a more distinct estimate of what the hypothesis involves, for instance, in respect to the entrance of the satellite into the shadow of the planet, as described by Lardner, the assumption of the theory is that the satellite enters the shadow of the planet *in fact* about 40 minutes (on an average) before it appears to us, viewing it from the earth, to do so; and, hence, the eclipse must have nearly terminated and the occultation be far advanced, before the eclipse *appears* to *us* to have commenced.

Let us now merely note that this case is necessarily included in the assumption, and consider other consequents; we will suppose the earth in its orbit, as shewn at *A. a.*, Fig. 9, (Plate 9) with the planet Jupiter in opposition, that is at the orbital place nearest to Jupiter. The earth then travels round to the opposite extremity of the orbit, into conjunction of that planet. If the planet were to remain motionless, this place in the earth's orbit would be *B.*, in the Fig.; but, since Jupiter's angular velocity is about one twelfth that of the earth, the planet will have moved through about 15° to *M.*; and the earth's orbital place of superior conjunction will be *N.* For the earth again to arrive at the place of opposition of the planet, half the earth's solar orbit together with an additional 15° will be the distance required, and *O. o.* will be the place of opposition; *M. O.* being equal to *A. M.*, and *a. o.* equal to twice *B. N.*, and in like manner *P. p.* will be the next place of conjunction, *O. P.* being also equal to *A. M.* So that the distance from opposition to conjunction is in fact equal to the distance from conjunction to opposition; but, on the assumption of the truth of the theory, will this actual equality in the distances also hold good when the motions are observed from the earth? Taking the earth at *a.*, with the planet in opposition, and considering that, as the earth travels in its orbit towards *N.*, the distance of the earth from the planet

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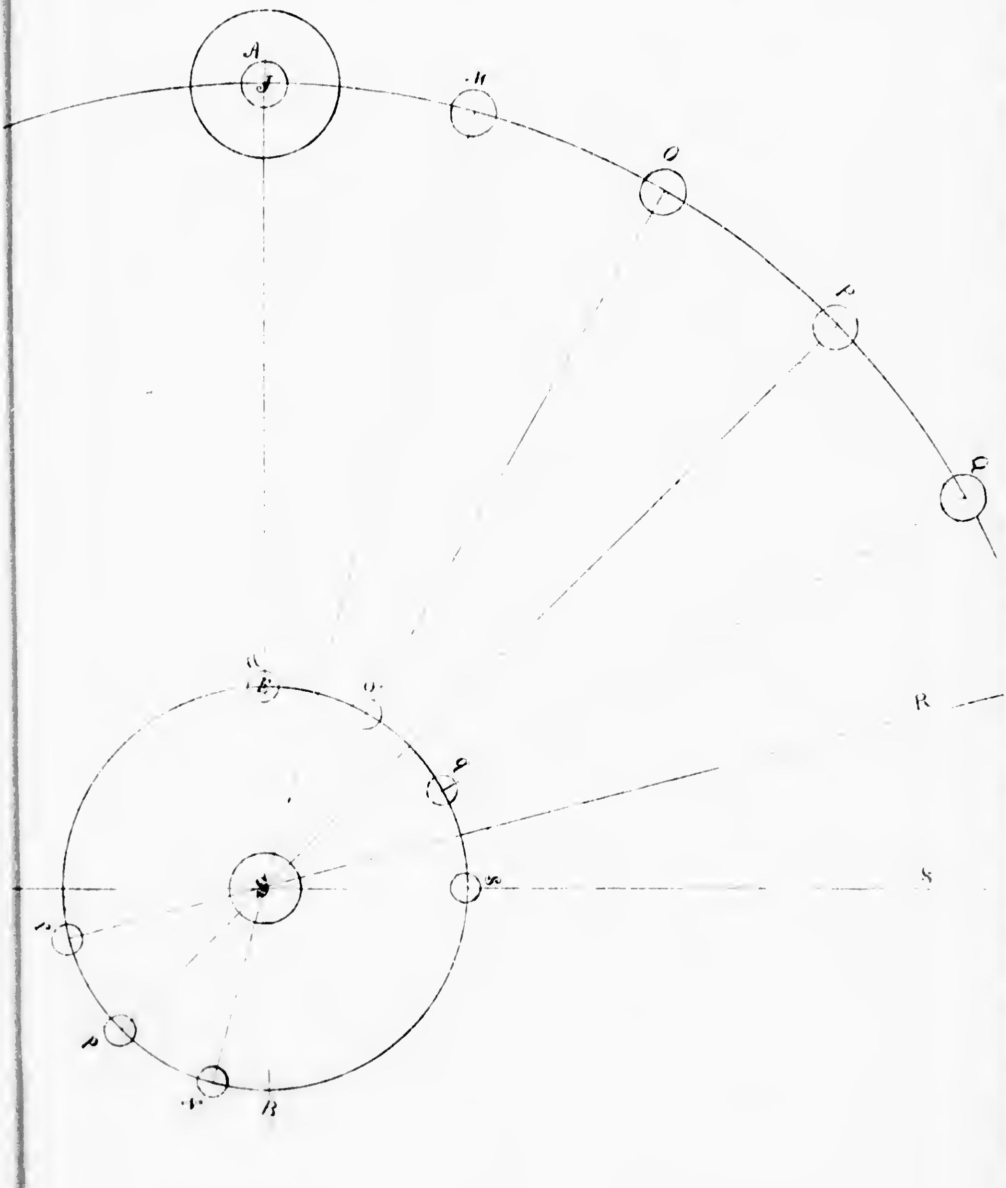
Plate 9. Fig 9.



continually becomes greater and, consequently, an increasing quantity of time is required for the light from the planet to reach the earth, we find that when the earth has arrived at *N.*, this apparent increase in the time actually occupied will, by the theory, amount to 16 minutes. But now as the earth continues its progress, and returns towards opposition, the contrary effect must take place, and the like apparent quantity of time be gained. The result must, therefore, be, if we compare the two halves of the entire synodic revolution of the earth, a difference of 32 minutes. But, moreover, this semi-orbital difference as measured by time, which belongs to the theory, is not peculiar to the planet Jupiter: it is equally applicable to any other superior planet, because the distance we are here considering is that of the diameter of the earth's orbit. Therefore we have to ask whether there can be a difference of 32 minutes between the two halves of the earth's synodic revolution in the case of each superior planet which has never yet been observed, or which, in other words, has hitherto escaped the observation of all astronomers?

(*Note.*—It is unnecessary to complicate the subject by investigating the additional effect which would arise under the hypothetical conditions of the case in consequence of the reversed direction of the earth's orbital motion from inferior to superior conjunction. It will be sufficient to observe that, at inferior conjunction, light from the planet would, according to the theory, require about 36 minutes to reach the earth which would be then moving from east to west; and, at superior conjunction, about 52 minutes, when the earth would be moving from west to east.)

But let us consider the case of an inferior planet; take, for example, the planet Venus. Now, there is this difference between the case of an inferior and that of a superior planet; that, when the former is in inferior conjunction, the solar light passes the planet and comes directly on to the earth; whereas, in the latter, the light of the sun goes first to the planet and is then reflected from the planet to the earth. In the case of a superior planet therefore it does not appear practicable, if we entertain the hypothesis that light may have a velocity, to ascertain, by direct observation, the precise moment of the coincidence of the centres of the earth and of the planet in a verticle plane joining also the centre of the sun, independently of that hypothesis.* But, will this difficulty apply equally to the case of an inferior planet; will it, for example, apply to the case of the planet Venus? Have we, in this case, the means of determining the actual moment of coincidence, and, thereby of obtaining a direct and decisive solution of the question?

Referring to authoritative works on Astronomy to obtain the record of the observed facts, we find evidence of apparent error or oversight of a very serious nature, with which the record itself is so involved, that in order to separate the observed facts from the ass' mptions and conclusions, which we suspect to be unsound, it becomes necessary to examine that general explanation which, at the present time, is accepted as the correct teaching, throughout. To do this, it will suffice to take the works of the same two teachers, from whom we have hitherto mainly taken our quotations. We will, therefore, in the first place, examine the explanation given by Lardner, and, then compare Herschel's explanation therewith, in order to ascertain whether any error or apparent error occurring in the one is attributable to the one teacher only, or, whether it is of a general character and belongs to what is considered the approved scientific teaching on the subject.

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* Because the supporters of the theory expressly reject the testimony of sight as evidencing that what appears to take place at a certain time does actually take place at that time.

We are told..No: your sight deceives you; you are reading only the record of the past; what appears to you to be now taking place has *in fact* taken place some time since.

SCIENCE

PHILOSOPHY

HISTORY

I.S. AND N.S

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APPENDIX TO SUPPLEMENT D.

PART SECOND.

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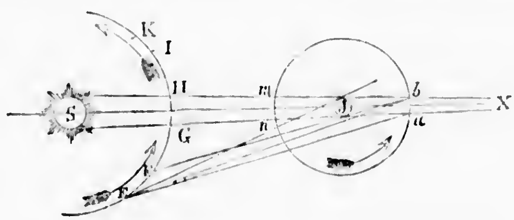
Herschel's Outlines of Astronomy.

(537) "These eclipses (of Jupiter's satellites) moreover, are not seen, as is the case with those of the moon, from the centre of their motion, but from a remote station, and one whose situation with respect to the line of shadow is variable. This, of course, makes no difference in the *times* of the eclipses, but a very great one in their visibility, and in their apparent situations with respect to the planet at the moments of their entering and quitting the shadow."

(538) "Suppose *S.* to be the sun, *E.* the earth in its orbit, *E.F.G.K.*, *J.* Jupiter, and *a.b.* the orbit of one of its satellites. The cone of the shadow, then, will have its vertex at *X.* a point far beyond the orbits of all the satellites; and the penumbra, owing to the great distance



of the sun, and the consequent smallness of the angle (about 6′ only) its disc subtends at Jupiter, will hardly extend, within the limits of the satellites' orbits, to any perceptible distance beyond the shadow—for which reason it is not represented in the figure. A satellite revolving from west to east (in the direction of the arrows) will be eclipsed when it enters the shadow at *a.*, but not suddenly, because, like the moon, it has a considerable diameter seen from the planet; so that the time elapsing from the first perceptible loss of light to its total extinction will be that which it occupies in describing about Jupiter an angle equal to its apparent diameter as seen from the centre of the planet, or rather somewhat more, by reason of the penumbra; and the same remark applies to its emergence at *b.* Now, owing to the difference of telescopes and of eyes, it is not possible to assign the *precise* moment of incipient obscuration, or of total extinction at *a.*, or that of the first glimpse of light falling on the satellite at *b.*, or the complete recovery of its light. The observation of an eclipse, then, in which only the immersion, or only the emersion, is seen, is incomplete, and inadequate to afford any precise information, theoretical or practical. But, if both the immersion and emersion can be observed with *the same telescope* and *by the same person*, the interval of the times will give the duration, and their mean the exact middle of the eclipse, when the satellite is in the line *S.J.X.*, *i.e.*, the true moment of its opposition to the sun. Such observations, and such only, are of use for determining the periods and other particulars of the motions of the satellites, and for the calculation of terrestrial longitudes. The intervals of the eclipses, it will be observed, give the synodic periods of the satellites' revolution; from which their siderial periods must be concluded by the method in art. 418."

(539) "It is evident, from a mere inspection of our figure, that the eclipses take place to the west of the planet, when the earth is situated to the west of the line *SJ*, *i.e.*, before the opposition of Jupiter; and to the east, when in the other half of its orbit, or after the opposition. When the earth approaches the opposition, the visual line becomes more and more nearly coincident with the direction of the shadow, and the apparent place where the eclipses happen will be continually nearer and nearer to the body of the planet. When the earth comes to *F.*,

a point determined by drawing *b.F.*, to touch the body of the planet, the *emersions* will cease to be visible, and will thenceforth, up to the time of the opposition, happen *behind* the disc of the planet. Similarly, from the opposition till the time when the earth arrives at *I*, a point determined by drawing *a. I.* tangent to the eastern limb of Jupiter, the *emersions* will be concealed from our view. When the earth arrives at *G.*, [or *H*] the immersion [or emersion] will happen at the very edge of the visible disc, and when between *G.* and *H* [a very small space] the satellites will *pass uneclipsed behind the limb* of the planet."

(540) "Both the satellites and their shadows are frequently observed to *transit* or pass across the disc of the planet. When a satellite comes to *m.*, its shadow will be thrown on Jupiter, and will appear to move across it as a black spot till the satellite comes to *n.* But the satellite itself will not appear to enter on the disc till it comes up to the line drawn from *E.* to the eastern edge of the disc, and will not leave it till it attains a similar line drawn to the western edge. It appears then that the shadow will *precede* the satellite in its progress over the disc *before* the opposition of Jupiter, and *vice versa.*"

(541) "Besides the eclipses and the transits of the satellites across the disc, they may also disappear to us when not eclipsed, by passing behind the body of the planet. Thus, when the earth is at *E.*, the immersion of the satellite will be seen at *a.*, and its emersion at *b.* both to the west of the planet, after which the satellite, still continuing its course in the direction *b.*, will pass behind the body and again emerge on the opposite side, after an interval of occultation greater or less according to the distance of the satellite. This interval, (on account of the great distance of the earth compared with the radii of the orbits of the satellites,) varies but little in the case of each satellite, being nearly equal to the time which the satellite requires to describe an arc of its orbit, equal to the angular diameter of Jupiter as seen from its centre.

which time, for the several satellites, is as follows: viz., for the first, 2h. 20m.; for the second, 2h. 56m.; for the third, 3h. 43m.; and for the fourth, 4h. 56m.; the corresponding diameter of the planets as seen from these respective satellites being, 19° 49′; 12° 25′; 7° 47′; and 4° 25′. Before the opposition of Jupiter, these occultations of the satellites happen *after* the eclipses: after the opposition when, for instance, the earth is in the situation *K.*, the occultations take place before the eclipses. It is to be observed, that, owing to the proximity of the orbits of the first and second satellites to the planet, *both* the immersion and emersion of either of them can never be observed in any single eclipse, the immersion being concealed by the body, if the planet be past its opposition, the emersion, if not yet arrived at it. So also of the occultation. The commencement of the occultation, or the passage of the satellite behind the disc, takes place while obscured by the shadow, before opposition, and its re-emergence after. All these particulars will be easily apparent on mere inspection of the Figure, Art. 536. It is only during the short time that the earth is in the arc *G. H.*, *i. e.* between the sun and Jupiter, that the cone of the shadow converging (while that of the visual rays diverges) behind the planet, permits their occultations to be completely observed both at ingress and egress, unobscured, the eclipses being then invisible."

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