OXFORD JOURNALS
OXFORD UNIVERSITY PRESS

NEWTON'S HYPOTHESES OF ETHER AND OF GRAVITATION FROM 1672 TO 1679

Author(s): Philip E. B. Jourdain

Source: *The Monist*, Vol. 25, No. 1 (JANUARY, 1915), pp. 79-106
Published by: Oxford University Press
Stable URL: https://www.jstor.org/stable/27900520
Accessed: 06-09-2018 07:24 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at https://about.jstor.org/terms

Oxford University Press is collaborating with JSTOR to digitize, preserve and extend access to *The Monist*

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

NEWTON'S HYPOTHESES OF ETHER AND OF GRAVITATION FROM 1672 TO 1679.

IN preceding articles we have considered at some length the nature and growth of Newton's conception of mass. We have now to trace the fortunes of Newton's views on ether and on the connected question as to whether or no gravitation is an essential property of matter. Writers on mechanics have often unduly neglected the theories of the ether given in Newton's optical papers, and writers on optics have often overlooked the mechanical significance —which was continually emphasized by Newton himself —of theories of the ether. Many of the books referred to are the same as in my former articles on Newton in this magazine.

I.

The first well-known author to abandon the ancient emission theory of light was Descartes.[1] Three kinds of matter have, according to him, separated, in the course of the evolution of the universe, out of the homogeneous, boundless and continuous substance which constitutes space; and the sensation of light is caused by the transmission of pressure by the small spheres of the second kind of matter. Colors are caused by different velocities of rotation of these particles which they have in virtue of oblique pressure; the most rapid rotation giving rise to the sensa-

[1] Rosenberger, *op. cit.*, pp. 21-22; cf. E. T. Whittaker, *A History of the Theories of Aether and Electricity from the Age of Descartes to the Close of the Nineteenth Century.* Dublin and London, 1910, pp. 4-9.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

tion of red, a slower one to that of yellow, and slower ones still to those of green and blue. The order of the colors was taken from the order of colors in the rainbow.

In contrast to the view of Descartes, Hooke's fundamental supposition was, according to Newton,[2] that "the parts of bodies when briskly agitated excite vibrations in the ether which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye; something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing." Again, to quote some later words of Newton, Hooke "changed Descartes's pressing or progressive motion of the medium to a vibrating one; the rotation of the globuli to the obliquation of the pulses, and the accelerating their rotation on the one hand, and retarding it on the other, by the quiescent medium to produce colors, to the like action of the medium on the two ends of his pulses for the same end."[3]

In fact, Hooke maintained that light is an actual motion and not, as Descartes did, a tendency to motion, and began the development of the undulatory theory. His hypothesis of colors (1667) was "that blue is an impression on the retina of an oblique and confused pulse of light, whose weakest part precedes and whose strongest follows."

Apart from these hypothetical considerations, it[4] had long been known to every writer on optics and to every practical optician that lenses with spherical surfaces do not give distinct images of objects. This indistinctness

[2] Letter to Oldenburg of July 11, 1672; Horsley's edition of Newton's *Opera,* Vol. IV, pp. 325-326. Cf. § III below.

[3] This view of Descartes's theory and of Hooke's opinions was given by Newton in his letter to Oldenburg dated December 21, 1675; *General Dict.,* Vol. VII. p. 783; *Macclesfield Correspondence,* Vol. II, p. 378. Cf. Brewster, *op. cit.,* Vol. I, pp. 88-89, and the end of § IV below. A very full account of that part of Hooke's work on physical optics which antedates Newton's first optical memoir was given by Rosenberger, *op. cit.,* pp. 35-42. Cf. also Whittaker, *op. cit.,* pp. 10-15.

[4] Brewster, *op. cit.,* Vol. I, pp. 37-39.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

was believed to arise solely from their spherical figure, in consequence of which the rays which passed through the marginal or outer parts of the lens were refracted to a focus nearer the lens than those which passed through its central parts. The distance between these foci was called the "spherical aberration" of the lens, and various methods were suggested for diminishing or removing this source of imperfection. Descartes had shown that hyperbolic lenses refracted the rays of light to a single focus, and accordingly we find the early volumes of the *Philosophical Transactions* filled with schemes for grinding and polishing lenses of this form. Newton had made the same attempt, but finding that a change of form produced very little change in the indistinctness of the image, he thought that the defect of lenses, and the consequent imperfection of telescopes, might arise from some other cause than the imperfect convergence of the incident rays to a single point. This conjecture was speedily confirmed by the brilliant discovery of the different refrangibilities of the rays of light. With regard to the views of the predecessors[5] of Newton respecting the nature and origin of colors, Descartes held the opinion we have already mentioned; Grimaldi, Dechales, and others regarded them as arising from different degrees of rarefaction and condensation of light; and Gregory (1663) defined color to be the hue (*tinctura*) of igneous corpuscles emerging from radiant matter. When recounting the opinions of preceding writers, Newton alleged that in all of them the color is supposed not to be innate in light, but to be produced by the action of the bodies which reflect or refract it. This, however, said Brewster,[6] is not strictly true. Isaac Voss, in a dissertation of 1662 which Newton probably never saw, distinctly maintained that all

[5] An account of the theories of light and colors of Aristotle, Kepler, De Dominis, Marci, Descartes, Voss, Grimaldi, Hooke, Boyle, Barrow, and others was given by Rosenberger, *op. cit.*, pp. 11-45, 51-52.

[6] Cf. Rosenberger's criticism, *ibid.*, pp. 25-26.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

the colors exist in light itself, or, to use another of his expressions, that all light carries its colors along with it. This conjecture cannot however be regarded as in any way anticipating the great discovery of Newton, "that the modification of light from which colors take their origin is innate in light itself, and arises neither from reflection nor refraction, nor from the qualities or any other conditions of bodies whatever, and that it cannot be destroyed or in any way changed by them."

II.

Newton, in a letter of January 18, 1672, to Oldenburg, the secretary of the Royal Society, announced that he proposed to send, for the consideration of the Royal Society, an account of the discovery which led him to concentrate his attention on the making of reflecting[7]—and not refracting—telescopes, and which was, said he, "in my judgment the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."[8]

Newton's letter of February 6 to Oldenburg, which contained an account of his theory of light and colors, was read on February 8 and printed in the *Philosophical Transactions*[9] for February 19, both for the purpose of "having it well considered by philosophers," and for "securing the considerable notices thereof to the author against the arrogations of others." At the same time a committee, con-

[7] Newton's constructions and exhibitions of, and controversies about, his reflecting telescopes were dealt with by Brewster, *op. cit.*, Vol. I, pp. 37-53, and Rosenberger, *op. cit.*, pp. 52-59.

[8] *Isaaci Newtoni Opera quae exstant omnia*, ed. Samuel Horsley, Vol. IV, London, 1782, p. 274 (this will be referred to as *Horsley*); Thomas Birch, *The History of the Royal Society of London*,.. Vol. III, London, 1757, p. 5; Brewster, *op. cit.*, Vol. I, pp. 71-72.

[9] No. 80, February 19, 1672, pp. 3075 sqq.; *Phil. Trans., abr.*, Vol. I, pp. 134 sqq.; notice in Birch, *op. cit.*, p. 9; and Rosenberger, *op. cit.*, pp. 59-69. The letter was printed at length in *Horsley*, Vol. IV, pp. 295-308. Cf. Brewster, *op. cit.*, Vol. I, pp. 72-77; Rosenberger, *op. cit.*, pp. 59-69; Whittaker, *op. cit.*, pp. 15-16.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

sisting of Seth Ward (Bishop of Salisbury), Boyle and Hooke, was appointed to peruse and consider it, and to give a report upon it to the Society.

Newton had, in the beginning of 1666, obtained a triangular prism, "to try therewith the celebrated phenomena of colors." For this purpose, "having darkened," as he expressed himself, "my chamber and made a small hole in my window-shuts to let in a convenient quantity of the sun's light, I placed my prism at his entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement to view the vivid and intense colors produced thereby; but after a while applying myself to consider them more circumspectly, I became surprised to see them in an oblong form, which, according to the received laws of refractions, I expected should have been circular." The length of the colored spectrum was in fact about five times as great as its breadth. After more experiments, Newton found the explanation to be that ordinary white light is really a mixture of rays of every variety of color, and that the elongation of the spectrum is due to the differences in the refractive power of the glass for these different rays.

"Amidst these thoughts," said Newton,[10] "I was forced from Cambridge by the intervening plague." This was in 1666,[11] and his memoir on the subject was not presented to the Royal Society until more than five years later. In it he propounded a theory of colors directly opposed to that of Hooke. "Colors," he said,[12] "are not qualifications of light, derived from refractions or reflections of natural bodies (as is generally believed), but original and connate properties, which in divers rays are divers. Some rays are disposed to exhibit a red color and no other, some a yellow and no other, some a green and no other, and so of

[10] *Horsley,* Vol. IV, p. 300.
[11] Cf. my article in *The Monist* for April, 1914.
[12] *Horsley,* Vol. IV, pp. 301-302.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

the rest. Nor are there only rays proper and particular to the more eminent colors, but even to all their intermediate gradations. To the same degree of refrangibility ever belongs the same color, and to the same color ever belongs the same degree of refrangibility...The species of color and degree of refrangibility proper to any particular sort of rays is not mutable by refraction nor by reflection from natural bodies, nor by any other cause that I could yet observe. When any one sort of rays hath been well parted from those of other kinds, it hath afterwards obstinately retained its color, notwithstanding my utmost endeavors to change it."

Not very far from the beginning of his experiments, Newton, in his own words,[13] "began to suspect whether the rays, after their trajection through the prism, did not move in curve lines, and, according to their more or less curvity, tend to divers parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis-ball, struck with an oblique racket, describe such a curve line. For, a circular as well as a progressive motion being communicated to it by that stroke, its parts, on that side where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion; they ought to feel the greater resistance from the ambient ether on that side where the motions conspire, and thence be continually bowed to the other." But he could not observe any such "curvity."

III.

The publication of Newton's memoir gave rise to an

[13] *Horsley,* Vol. IV, pp. 297-298. Cf. Rosenberger, *op. cit.,* pp. 62-63.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

acute controversy,[14] and Hooke was among the foremost of Newton's adversaries. Indeed he was the only one who seems to have had an effect on Newton's views on the ether. It seems that this unpleasant controversy had much to do with the reluctance to publish his results which Newton ever afterwards showed. Hooke viewed Newton's discoveries through the medium of his own theory, and, when he sent in his report on February 15, 1672, he was thanked "for the pains he had taken in bringing in such ingenious reflections," but it was not "thought fit to print the two papers together, lest Mr. Newton should look upon it as a disrespect in printing so sudden a refutation of a discourse of his which had met with so much applause at the Society but a few days before."

In Hooke's report[15] on Newton's communication, criticism was directed solely at the hypothetical part. He expressed his ready agreement with Newton's experiments and thought his hypothesis "very subtle and ingenious," but could not "think it to be the only hypothesis, nor so certain as mathematical demonstration." In the course of the report Hooke said: "But grant his first supposition, that light is a body and that as many colors as degrees thereof as there may be, so many sorts of bodies there may be, all which compounded together would make white, ...";[16] thus indicating that Hooke considered Newton's corpuscular hypothesis to be of some importance in Newton's doctrine of light and colors.

Newton replied to Hooke's criticism in a letter[17] addressed to Oldenburg and dated July 11, 1672. As to the supposition attributed to him by Hooke, he replied:[18] "It is

[14] Cf. Brewster, *op. cit.*, Vol. I, pp. 77-86, 89-96; Rosenberger, *op. cit.*, pp. 73-101.

[15] Birch, *op. cit.*, Vol. III, pp. 10-15; Rosenberger, *op. cit.*, pp. 73-75.

[16] Birch, *op. cit.*, Vol. III, p. 14; Rosenberger, *op. cit.*, p. 74.

[17] *Horsley*, Vol. IV, pp. 322-342; Rosenberger, *op. cit.*, pp. 75-82. Cf. Brewster, *op. cit.*, Vol. I, pp. 90-91.

[18] *Horsley*, Vol. IV, pp. 324-326.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

true that from my theory I argue the corporeity of light, but I do it without any absolute positiveness, as the word *perhaps* intimates, and make it at most but a very plausible consequence of the doctrine, and not a fundamental supposition, nor so much as any part of it, which was wholly comprehended in the precedent propositions. And I wonder how Mr. Hooke could imagine that, when I had asserted the theory with the greatest rigor, I should be so forgetful as afterwards to assert the fundamental supposition itself with no more than a *perhaps*. Had I intended any such hypothesis, I should somewhere have explained it. But I knew that the properties which I declared of light were in some measure capable of being explicated not only by that, but by many other mechanical hypotheses; and therefore I chose to decline them all, and speak of light in general terms, considering it abstractedly as something or other propagated every way in straight lines from luminous bodies, without determining what that thing is; whether a confused mixture of difform qualities, or modes or bodies, or of bodies themselves, or of any virtues, powers, or beings whatsoever. And for the same reason I choose to speak of colors according to the information of [our] senses, as if they were qualities of light without us. Whereas, by that hypothesis, I must have considered them rather as modes of sensation, excited in the mind by various motions, figures, or sizes of the corpuscles of the light, making various mechanical impressions on the organs of sense, as I expressed it in that place where I spoke of the corporeity of light.

"But supposing I had propounded this hypothesis, I understand not why Mr. Hooke should so much endeavor to oppose it. For certainly it has a much greater affinity with his own hypothesis than he seems to be aware of; the vibrations of ether being as useful and necessary in this as in his own. For, assuming the rays of light to be small

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

bodies, emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the ether as stones do in water when thrown into it. And supposing these vibrations to be of several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities,—of what use they will be for explicating the manner of reflection and refraction, the production of heat by the sun's beams, the emission of light from burning, putrefying, or other substances whose parts are vehemently agitated, the phenomena of thin transparent plates and bubbles and of all natural bodies, the manner of vision, and the difference of colors, as also their harmony and discord,—I shall leave to the consideration of those who may think it worth their endeavor to apply this hypothesis to the solution of phenomena.

"In the second place, I told you that Mr. Hooke's hypothesis, as to the fundamental part of it, is not against me. The fundamental supposition is that the parts of bodies, when briskly agitated, excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light, by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing. Now the most free and natural application of this hypothesis to the solution of phenomena I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, excite vibrations in the ether of various depths or bignesses, and which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white color: but if by any means those of unequal bignesses be separated from one another, the largest, a sensation of red color; the least, or shortest,

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

of a deep violet; and the intermediate ones, of intermediate colors."

In this reply to Hooke, Newton pointed out the character of experimental science, the duties of scientific men, and the unquestionableness of experiment and observation. But it is not necessary to quote this part of the reply. I have quoted at length the parts referring to the ether and to the connected question of hypotheses as to the nature of light, and in what follows I shall continue to do this for Newton's other memoirs. The connection of the ether with the principle of gravitation will gradually appear; at present the most noticeable thing is that in 1672 Newton granted as a matter of course the existence of an ether.

"It may be," said Rosenberger,[19] "that Newton, in his first memoir only adopted the emission-theory of light because it was the simplest and most convenient, and did not make a further study of other theories. But before his reply to Hooke's report, he certainly examined the undulatory hypothesis very carefully, and the supporters of this hypothesis were greatly indebted to him, for without question he was the first to show how it was possible to arrive, starting from the undulatory hypothesis, at a definition of colors and at an explanation of the dispersion of light on refraction."

IV.

At the end[20] of a letter written by Newton to Oldenburg on November 13, 1675,[21] Newton wrote: "I had some thoughts of writing a further discourse about colors to be read at one of your assemblies; but find it yet against the grain to put pen to paper any more on that subject. But, however, I have one discourse by me on that subject, writ-

[19] *Op. cit.*, p. 82.

[20] *Horsley*, Vol. IV, p. 355; Brewster, *op. cit.*, Vol. I, p. 132; Rosenberger, *op. cit.*, p. 101.

[21] The date was not given by Horsley.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

ten when I sent my first letters to you about colors, and of which I then gave you notice. This you may command, when you think it will be convenient, if the custom of reading weekly discourses still continue."

Newton again wrote to Oldenburg on November 30, 1675,[22] that he intended to have sent the papers that week, but that upon reviewing them it came into his mind to write another "little scribble" to accompany them. This "little scribble" was his "hypothesis" to which we shall presently refer. The whole discourse was produced in manuscript on December 9, 1675, with the title of: "A Theory of Light and Colours, containing partly an Hypothesis to explain the properties of light discoursed of by me in my former papers, partly the principal phenomena of the various colours exhibited by thin plates or bubbles, esteemed to be of a more difficult consideration, yet to depend also on the said properties of light."[23] The "scribble" in particular, was entitled "An Hypothesis explaining the Properties of light discoursed of in my several Papers," and was stated by Brewster[24] to have been contained in a letter to Oldenburg dated January 25, 1676,—a date which seems to be a mistaken one.

In the letter[25] to Oldenburg which accompanied the papers forming his "discourse," Newton wrote: "I had formerly purposed never to write any hypothesis of light and colors, fearing it might be a means to engage me in

[22] Brewster, *op. cit.*, Vol. I, p. 132; Rosenberger, *op. cit.*, pp. 101-102.

[23] This has been referred to in § IX of my paper on "The Principles of Mechanics with Newton from 1666 to 1679" in *The Monist* for April, 1914.

[24] *Op. cit.*, Vol. I, p. 390.

[25] Birch, *op. cit.*, Vol. III, pp. 247-248; Brewster, *op. cit.*, Vol. I, pp. 132-134; Rosenberger, *op. cit.*, pp. 101-102. This letter, and the "Hypothesis" introduced by it were read to the Royal Society, and the "Hypothesis" was printed in Birch, *op. cit.*, pp. 248-260, 261-269, and Brewster, *op. cit.*, Vol. I, pp. 390-409 (cf. Rosenberger, *op. cit.*, pp. 102-111), but not in Horsley's edition of Newton's works. The experimental part of the paper (Birch, *op. cit.*, pp. 272-305) was included without alteration in the first and second parts of the second book, and the first eight propositions of the third part of that book, of the *Opticks* of 1704. On the "Hypothesis" the conclusions from it which Newton drew, and the controversy with Hooke to which it gave rise, see also Brewster, *op. cit.*, Vol. I, pp. 136-145, 151-161.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

vain disputes; but I hope a declared resolution to answer nothing that looks like a controversy, unless possibly at my own time upon some by-occasion, may defend me from that fear. And therefore, considering that such a hypothesis would much illustrate the papers I promised to send you, and having a little time this last week to spare. I have not scrupled to describe one, so far as I could on a sudden recollect my thoughts about it; not concerning myself whether it should be thought probable or improbable, so it do but render the paper I send you and others sent formerly more intelligible. You may see by the scratching and interlining it was done in haste; and I have not had time to get it transcribed, which makes me say I reserve a liberty of adding to it, and desire that you would return these and the other papers when you have done with them. I doubt there is too much to be read at one time, but you will soon see how to order that. At the end of the hypothesis you will see a paragraph to be inserted as is there directed. I should have added another or two, but I had not time, and such as it is I hope you will accept it."

In his reply to Hooke of July 11, 1672, Newton had stated that what he called the fundamental supposition in Hooke's hypothesis—namely, that the waves or vibrations of the ether could, like the rays of light, be propagated in straight lines without a very extravagant spreading and bending every way into the quiescent medium by which they are bounded—seemed impossible, and added: "I am mistaken if there be not both experiment and demonstration to the contrary." However, Newton at once suggested a modification of Hooke's hypothesis, so that it could better account for the phenomena, as we have seen towards the end of § III above.[26] But it is certain that Newton did not regard either this hypothesis or that of 1675 as expressing his

[26] Cf. *Phil. Trans.*, 1672, No. 88, p. 5088. Cf. also Brewster, *op. cit.*, Vol. I, pp. 135-136, 390.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

own convictions, or as of having any more than an illustrative value. "Because I have observed," he said, "the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted a hypothesis to explain them by, and found that some, when I could not make them take my meaning when I spoke of the nature of light and colors abstractly, have readily apprehended it when I illustrated my discourse by a hypothesis; for this reason I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the illustration of the papers I herewith send you." And he added that he would not assume either this or any other hypothesis; yet that he would, while describing this hypothesis, "sometimes, to avoid circumlocution, and to represent it more conveniently," speak of it as if he assumed it and propounded it to be believed. "This," he said, "I thought fit to express, that no man may confound this with my other discourses, or measure the certainty of one by the other, or think me obliged to answer objections against this script; for I desire to decline being involved in such troublesome, insignificant disputes."[27] Newton, however, confessed that he did not see how the colors of thin transparent plates could be well explained without having recourse to ethereal pulses;[28] and shortly afterwards remarked: "Were I to assume an hypothesis, it should be this [that mentioned in § III above], if propounded more generally so as not to determine what light is, further than that it is something or other capable of exciting vibrations in the ether."[29]

Newton then proceeded to describe the hypothesis. "(1) It is to be supposed therein that there is an ethereal medium, much of the same constitution with air, but far rarer, subtler, and more strongly elastic. Of the existence of this

[27] Brewster, *op. cit.*, Vol. I, pp. 136, 391-392.
[28] *Ibid.*, p. 391.
[29] Ibid.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

medium the motion of a pendulum in a glass exhausted of air almost as quickly as in the open air is no inconsiderable argument. But it is not to be supposed that this medium is one uniform matter, but [that it is] composed partly of the main phlegmatic body of ether, partly of other various ethereal spirits, much after the manner that air is compounded of the phlegmatic body of air intermixed with various vapors and exhalations. For the electric and magnetic effluvia and the gravitating principle seem to argue such variety. Perhaps the whole frame of nature may be nothing but various contextures of some certain ethereal spirits or vapors, condensed as it were by precipitation, much after the manner that vapors are condensed into water, or exhalations into grosser substances, though not so easily condensable; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, which, by virtue of the command, increase and multiply, became a complete imitator of the copy set her by the Protoplast. Thus perhaps may all things be originated from ether.

"At least the electric effluvia seem to instruct us that there is something of an ethereal nature condensed in bodies. I have sometimes laid upon a table a round piece of glass about two inches broad, set in a brass ring, so that the glass might be about one-eighth or one sixth of an inch from the table and the air between them enclosed on all sides by the ring, after the manner as if I had whelmed a little sieve upon the table. And then rubbing a pretty while the glass briskly with some rough and raking stuff, till some very little fragments of very thin paper laid on the table under the glass began to be attracted and move nimbly to and fro; after I had done rubbing the glass, the papers would continue a pretty while in various motions, sometimes leaping up to the glass and resting there a while, then leaping down and resting there, then leaping up, and

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

perhaps down and up again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also they would leap up in one arc and down in another divers times together, without sensible resting between; sometimes skip in a bow from one part of the glass to another without touching the table, and sometimes hang by a corner and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind, and be otherwise variously moved,—every paper with a divers motion. And upon sliding my finger on the upper side of the glass, though neither the glass nor the enclosed air below were moved thereby, yet would the papers as they hung under the glass receive some new motion, inclining this way or that way, accordingly as I moved my finger. Now whence all these irregular motions should spring I cannot imagine, unless from some kind of subtle matter lying condensed in the glass, and rarefied by rubbing, as water is rarefied into vapor by heat, and in that rarefaction diffused through the space round the glass to a great distance, and made to move and circulate variously, and accordingly to actuate the papers, till it returns into the glass again, and be recondensed there. And as this condensed matter by rarefaction into an ethereal wind (for by its easy penetrating and circulating through glass I esteem it ethereal) may cause these odd motions, and by condensing again may cause electrical attraction with its returning to the glass to succeed in the place of what is there continually recondensed; so may the gravitating attraction of the earth be caused by the continual condensation of some other such like ethereal spirit, not of the main body of phlegmatic ether, but of something very thinly and subtly diffused through it, perhaps of an unctuous or gummy tenacious and springy nature, and bearing much the same relation to ether which the vital aëreal spirit requisite for the conservation of flame and vital motions does

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

to air. For if such an ethereal spirit may be condensed in fermenting or burning bodies, or otherwise coagulating in the pores of the earth and water into some kind of humid active matter for the continual uses of nature, (adhering to the sides of those pores after the manner that vapors condense on the sides of a vessel,) the vast body of the earth, which may be everywhere to the very center in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with great celerity for a supply. In this descent it may bear down with it the bodies it pervades, with force proportional to the superficies of all their parts it acts upon, nature making a circulation by the slow ascent of as much matter out of the bowels of the earth in an aëreal form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapors rising underneath, at length (some part of the vapors which return in rain excepted) vanishes again into the ethereal spaces, and there perhaps in time relents and is attenuated into its first principle. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtle out of gross, and gross out of subtle, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him: and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of light, and that the vast ethereal spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of ethereal natures by the bye.

"In the second place, it is to be supposed that the ether

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

is a vibrating medium like air, only the vibrations far more swift and minute; those of air made by a man's ordinary voice succeeding one another at more than half a foot or a foot distance, but those of ether at a less distance than the hundred-thousandth part of an inch. And as in air the vibrations are some larger than others, but yet all equally swift, (for in a ring of bells the sound of every tone is heard at two or three miles' distance in the same order that the bells are struck,) so I suppose the ethereal vibrations differ in bigness, but not in swiftness. Now these vibrations, besides their use in reflection and refraction, may be supposed the chief means by which the parts of fermenting or putrefying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion, are shaken asunder like a ship by waves, and dissipated into vapors, exhalations, or smoke, and light loosed or excited in those bodies, and consequently by which a body becomes a burning coal, and smoke flame; and I suppose flame is nothing but the particles of smoke turned by the access of light and heat to burning coals, little and innumerable.

"Thirdly, the air can pervade the bores of small glass pipes, but yet not so easily as if they were wider, and therefore stands at a greater degree of rarity than in the free aëreal spaces, and at so much greater a degree of rarity as the pipe is smaller, as is known by the rising of water in such pipes to a much greater height than the surface of the stagnating water into which they are dipped. So I suppose ether, though it pervades the pores of crystal, glass, water, and other natural bodies, yet it stands at a greater degree of rarity in those pores than in the free ethereal spaces, and at so much a greater degree of rarity as the pores of the body are smaller. Whence it may be that spirit of wine, for instance, though a lighter body, yet having subtler parts, and consequently smaller pores than

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

water, is the more strongly refracting liquor. This also may be the principal cause of the cohesion of the parts of solids and fluids, of the springiness of glass and other bodies whose parts slide not one upon another in bending, and of the standing of the mercury in the Torricellian experiment, sometimes to the top of the glass, though a much greater height than twenty-nine inches. For the denser ether which surrounds these bodies must crowd and press their parts together, much after the manner that air surrounding two marbles presses them together if there be little or no air between them. Yea, and that puzzling problem *by what means the muscles are contracted and dilated to cause animal motion, may receive greater light from hence than from any other means men have hitherto been thinking on.* For if there be any power in man to condense and dilate at will the ether that pervades the muscle, that condensation or dilatation must vary the compression of the muscle made by the ambient ether, and cause it to swell or shrink, accordingly. For though common water will scarce shrink by compression and swell by relaxation, yet (so far as my observation reaches) spirit of wine and oil will; and Mr. Boyle's experiment of a tadpole shrinking very much by hard compressing the water in which it swam, is an argument that animal juices do the same: and as for their various pression by the ambient ether, it is plain that that must be more or less, accordingly as there is more or less ether within to sustain and counterpoise the pressure of that without. If both ethers were equally dense, the muscle would be at liberty as if pressed by neither: if there were no ether within, the ambient would compress it with the whole force of its spring. If the ether within were twice as much dilated as that without, so as to have but half as much springiness, the ambient would have half the force of its springiness counterpoised thereby, and exercise but the other half upon the muscle; and so in all other cases

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

the ambient compresses the muscle by the excess of the force of its springiness above that of the springiness of the included. To vary the compression of the muscle therefore, and so to swell and shrink it, there needs nothing but to change the consistence of the included ether; and a very little change may suffice, if the spring of ether be supposed very strong, as I take it to be many degrees stronger than that of air.

"Now for the changing the consistence of the ether, some may be ready to grant that the soul may have an immediate power over the whole ether in any part of the body, to swell or shrink it at will; but then how depends the muscular motion on the nerves? Others therefore may be more apt to think it done by some certain ethereal spirit included within the *dura mater,* which the soul may have power to contract or dilate at will in any muscle, and so cause it to flow thither through the nerves; but still there is a difficulty why this force of the soul upon it does not take off the power of springiness, whereby it should sustain more or less the force of the outward ether. A third supposition may be that the soul has a power to inspire any muscle with this spirit by impelling it thither through the nerves; but this too has its difficulties, for it requires a forcible intruding the spring of the ether in the muscles by pressure exerted from the parts of the brain; and it is hard to conceive how so great force can be exercised amidst so tender matter as the brain is; and besides, why does not this ethereal spirit, being subtle enough, and urged with so great force, go away through the *dura mater* and skins of the muscle, or at least so much of the other ether go out to make way for this which is crowded in? To take away these difficulties is a digression, but seeing the subject is a deserving one, I shall not stick to tell you how I think it may be done.

"First, then, I suppose there *is* such a spirit; that is,

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

that the animal spirits are neither like the liquor, vapor, or gas, or spirits of wine; but of an ethereal nature, subtle enough to pervade the animal juices as freely as the electric, or perhaps magnetic, effluvia do glass. And to know how the coats of the brain, nerves, and muscles, may become a convenient vessel to hold so subtle a spirit, you may consider how liquors and spirits are disposed to pervade, or not pervade, things on other accounts than their subtlety; water and oil pervade wood and stone, which quicksilver does not; and quicksilver, metals, which water and oil do not; water and acid spirits pervade salts, which oil and spirit of wine to not; and oil and spirit of wine pervade sulphur, which water and acid spirits do not; so some fluids, (as oil and water,) though their parts are in freedom enough to mix with one another, yet by some secret principle of *unsociableness* they keep asunder; and some that are *sociable* may become *unsociable* by adding a third thing to one of them, as water to spirit of wine by dissolving salt of tartar in it. The like *unsociableness* may be in ethereal natures, as perhaps between the ethers in the vortices of the sun and planets; and the reason why air stands rarer in the bores of small glass pipes, and ether in the pores of bodies may be, not want of subtlety, but *sociableness*; and on this ground, if the ethereal vital spirit in a man be very *sociable* to the marrow and juices, and *unsociable* to the coats of the brain, nerves, and muscles, or to any thing lodged in the pores of those coats, it may be contained thereby, notwithstanding its subtlety; especially if we suppose no great violence done to it to squeeze it out, and that it may not be altogether so subtle as the main body of ether, though subtle enough to pervade readily the animal juices, and that as any of it is spent, it is continually supplied by new spirit from the heart.

"In the next place, for knowing how this spirit may be used for animal motion, you may consider how some things

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

unsociable are made sociable by the mediation of a third. Water, which will not dissolve copper, will do it if the copper be melted with sulphur. *Aquafortis,* which will not pervade gold, will do it by addition of a little sal-ammoniac or spirit of salt. Lead will not mix in melting with copper; but if a little tin, or antimony, be added, they mix readily, and part again of their own accord, if the antimony be wasted by throwing saltpetre, or otherwise. And so lead melted with silver quickly pervades and liquefies the silver in a much less heat than is required to melt the silver alone; but if they be kept in the test till that little substance that reconciled them be wasted or altered, they part again of their own accord. And in like manner the ethereal animal spirit in a man may be a mediator between the common ether and the muscular juices to make them mix more freely, and so by sending a little of this spirit into any muscle, though so little as to cause no sensible tension of the muscle by its own force, yet by rendering the juices more sociable to the common external ether, it may cause that ether to pervade the muscle of its own accord in a moment more freely and more copiously than it would otherwise do, and to recede again as freely, so soon as this mediator of sociableness is retracted; whence, according to what I said above, will proceed the swelling or shrinking of the muscle, and consequently the animal motion depending thereon.

"Thus may therefore the soul, by determining this ethereal animal spirit or wind into this or that nerve, perhaps with as much ease as air is moved in open spaces, cause all the motions we see in animals; for the making which motions strong, it is not necessary that we should suppose the ether within the muscle very much condensed, or rarefied, by this means, but only that its spring is so very great that a little alteration of its density shall cause a great alteration in the pressure. And what is said of

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

muscular motion may be applied to the motion of the heart, only with this difference: that the spirit is not sent thither as into other muscles, but continually generated there by the fermentation of the juices with which its flesh is replenished, and as it is generated, let out by starts into the brain, through some convenient *ductus,* to perform those motions in other muscles by inspiration, which it did in the heart by its generation. For I see not why the ferment in the heart may not raise as subtle a spirit out of its juices, to cause those motions, as rubbing does out of a glass to cause electric attraction, or burning out of fuel to penetrate glass, as Mr. Boyle has shown, and calcine by corrosion metals melted therein.[30]

"Hitherto I have been contemplating the nature of ether and ethereal substances by their effects and uses, and now I come to join therewith the consideration of light.

"In the fourth place, therefore, I suppose light is neither ether, nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after another, but yet without any sensible interval of time, and continually urged forward by a principle of motion, which in the beginning accelerates them, till the resistance of the ethereal medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance of the water equal the force of gravity. God, who gave animals motion beyond our understanding, is, without doubt, able to implant other principles of motion in bodies which we may understand as little. Some would readily

[30] Boyle's *Essays of the strange subtlety, etc. of effluviums, etc., together with a discovery of the perviousness of glass to ponderable parts of flame.*—Note of Newton.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

grant this may be a spiritual one; yet a mechanical one might be shown, did not I think it better to pass it by.[31] But they that like not this may suppose light any other corporeal emanation, or an impulse or motion of any other medium or ethereal spirit diffused through the main body of ether, or what else they imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy; only, whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigor, like as the sands on the shore, the waves of the sea, the faces of men, and all other natural things of the same kind differ, it being almost impossible for any sort of things to be found without some contingent variety. And further, I would suppose it diverse from the vibrations of the ether, because, (besides that were it those vibrations, it ought always to verge copiously in crooked lines into the dark or quiescent medium, destroying all shadows, and to comply readily with any crooked pores or passages as sounds do,) I see not how any superficies, (as the side of a glass prism on which the rays within are incident at an angle of about forty degrees,) can be totally opake. For the vibrations beating against the refracting confine of the rarer and denser ether must needs make that pliant superficies undulate, and those undulations will stir up and propagate vibrations on the other side. And further, how light, incident on very thin skins or plates of any transparent body, should for many successive thicknesses of the plate in arithmetical progression, be alternately reflected and transmitted, as I find it is, puzzles me as much. For though the arithmetical progression of those thicknesses, which reflect and transmit the rays alternately, argues that it depends upon the number of vibrations between the two superficies of the plate whether the ray shall be reflected or trans-

[31] Rosenberger (*op. cit.*, p. 106) remarked that this is the basis on which, later, gravitation was maintained to be a primitive force of matter.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

mitted, yet I cannot see how the number should vary the case, be it greater or less, whole or broken, unless light be supposed something else than these vibrations. Something indeed I could fancy towards helping the two last difficulties, but nothing which I see not insufficient.

"Fifthly, it is to be supposed that light and ether mutually act upon one another, ether in refracting light, and light in warming ether, and that the densest ether acts most strongly. When a ray therefore moves through ether of uneven density, I suppose it most pressed, urged, or acted upon by the medium on that side towards the denser ether, and receives a continual impulse or ply from that side to recede towards the rarer, and so is accelerated if it move that way, or retarded if the contrary. On this ground, if a ray move obliquely through such an unevenly dense medium, (that is, obliquely to those imaginary superficies which run through the equally dense parts of the medium, and may be called the refracting superficies,) it must be incurved, as it is found to be by observation in water,[32] whose lower parts were made gradually more salt, and so more dense than the upper. And this may be the ground of all refraction and reflection. For as the rarer air within a small glass pipe, and the denser without, are not distinguished by a mere mathematical superficies, but have air between them at the orifice of the pipe running through all intermediate degrees of density, so I suppose the refracting superficies of ether between unequally dense mediums to be not a mathematical one, but of some breadth, the ether therein at the orifices of the pores of the solid body being of all intermediate degrees of density between the rarer and denser ethereal mediums; and the refraction I conceive to proceed from the continual incurvation of the ray all the while it is passing the physical superficies. Now if the motion of the ray be supposed in this passage to

[32] Mr. Hooke's *Micrographia* where he speaks of the inflection of rays.—Note of Newton.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

be increased or diminished in a certain proportion, according to the difference of the densities of the ethereal mediums, and the addition or detraction of the motion be reckoned in the perpendicular from the refracting superficies, as it ought to be, the sines of incidence and refraction will be proportional according to what Descartes has demonstrated."

After some further discussion of refraction and reflection on this hypothesis, and the causes of transparency, opacity, and color, the "scribble" concluded with an application of the hypothesis to the colors of thin plates, to the inflection of light, and to the colors of natural bodies.

After the reading of the first part of this discourse on December 9, Hooke[33] said that the main part of it was contained in his *Micrographia,* and that Newton had only carried what that work taught farther in some particulars. When this remark was communicated to Newton, he seems to have been greatly offended, and on December 21 he wrote a letter[34] to Oldenburg, pointing out the difference between his hypothesis and that of Hooke. Although he was "not much concerned at the liberty of Mr. Hooke's insinuation," yet he wished to "avoid the savor of having done anything unjustifiable or unhandsome" to him. He therefore separated out the part of the hypothesis that belongs to Descartes and others. This part was as follows: "That there is an ethereal medium; that light is the action of this medium; that this medium is less implicated in the parts of solid bodies, and so moves more freely in them, and transmits light more readily through them; and that after such a manner as to accelerate the rays in a certain proportion; that refraction arises from this acceleration, and has sines proportional; that light is at first uniform; that colors are some disturbance or modification of its rays

[33] Brewster, *op. cit.,* Vol. I, pp. 138-139.

[34] *Horsley,* Vol. IV, pp. 379-381; Birch, *op. cit.,* Vol. III, p. 278; Rosenberger, *op. cit.,* pp. 111-114.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

by refraction or reflection; that the colors of a prism are made by means of the quiescent medium accelerating some motion of the rays on one side where red appears, and retarding it on the other side where blue appears; that there are but these two original colors or color making modifications of light, which by their various degrees, or, as Mr. Hooke calls it, dilutinga, produce all intermediate ones."

When we have put this on one side, continued Newton, we find that:

"The remainder of his hypothesis is, that he has changed Descartes's pressing or progressive motion of the medium to a vibrating one; the rotation of the globuli to the obliquation of the pulses; and the accelerating their rotation on the one hand, and retarding it on the other, by the quiescent medium, to produce colors, to the like action of the medium on the two ends of his pulses for the same end. And having thus far modified his by the Cartesian hypothesis, he has extended it farther to explicate the phenomena of thin plates; and added another explication of the colors of natural bodies fluid and solid.

"This, I think, is in short the sum of his hypothesis. And in all this, I have nothing common with him but the supposition that ether is a medium susceptible of vibrations. Of which supposition I make a very different use; he supposing it light itself; which I suppose it is not. This is as great a difference, as is between him and Descartes. But besides this, the manner of refraction and reflection, and the nature and production of colors in all cases, which take up the body of my discourse, I explain very differently from him; and even in the colors of thin transparent substances, I explain everything after a way so different from him that the experiments on which I ground my discourse destroy all he has said about them. And the two main experiments, without which the manner of the production of those colors is not to be found out, were not only un-

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

known to him when he wrote his *Micrographia* but even last spring, as I understood in mentioning them to him. This therefore is the sum of what is common to us: that ether may vibrate. And so if he thinks fit to use that notion of colors arising from the various bignesses of pulses, without which his hypothesis will do nothing, his will borrow as much from my answer to his objections as that I send you does from his *Micrographia*.

"But it may be that I have made use of his observations. And of some I did: as that of the inflection of rays, for which I quoted him: that of opacity arising from the interstices of the parts of bodies, which I insist not on; and that of plated bodies exhibiting colors; a phenomenon for the notice of which I thank him. But he left me to find out and make such experiments about it as might inform me of the manner of the production of those colors to ground a hypothesis on: he having given no farther insight into it than this, that the color depended on some certain thickness of the plate. Though what that thickness was at every color, he confesses in his *Micrographia* he had attempted in vain to learn. And therefore seeing I was left to measure it myself, I suppose he will allow me to make use of what I took the pains to find out. And this I hope may vindicate me from what Mr. Hooke has been pleased to charge me with."[35]

The friendly letters which passed between Newton and Hooke shortly after this, and which were discovered by Brewster,[36] seem to show that both Hooke and Newton disliked controversy and held those just views on the proper attitude of one's mind to "natural philosophy" which are not very difficult to put into noble language,

[35] A paper entitled "Observations" accompanied this letter but was not printed. In it, according to Brewster (*op. cit.*, Vol. I, p. 139), Newton said that Hooke in his *Micrographia* had "delivered many very excellent things concerning the colors of thin plates and other natural objects," of which he had not scrupled to make use in so far as they were relevant to his purpose.

[36] *Op. cit.*, Vol. I, pp. 139-145.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms

but which are not so easy to express with such good feeling as these letters show.

After the publication of the "Hypothesis," Newton seems[37] to have conversed with Robert Boyle on the subject of its application of chemistry, and in 1679 wrote him a long letter which we must next examine.

PHILIP E. B. JOURDAIN.

CAMBRIDGE, ENGLAND.

[37] *Ibid.*, p. 145.

This content downloaded from 79.12.34.189 on Thu, 06 Sep 2018 07:24:37 UTC
All use subject to https://about.jstor.org/terms