ISAAC NEWTON'S
Papers & Letters
On Natural Philosophy

SIR ISAAC NEWTON

ISAAC NEWTON'S Papers & Letters On Natural Philosophy

and related documents

Second Edition

Edited, with a general introduction, by

I. BERNARD COHEN

assisted by Robert E. Schofield

Published in cooperation with the Burndy Library.

Containing Newton's contributions to the Philosophical Transactions of the Royal Society, his letter to Boyle about the æther, "De Natura Acidorum," Newton's letters to Bentley and the "Boyle Lectures" related to them, the first published biography of Newton, Halley's publications about Newton's "Principia," &c.

With explanatory prefaces by Marie Boas, Charles Coulston Gillispie, Thomas S. Kuhn, & Perry Miller.

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Preface to the Second Edition

In this new edition two changes have been made in the texts reproduced in facsimile, one of major importance, the other minor. The minor change is the removal from the bottom of our page 104 of three lines of a footnote that bear no relation to the second letter from Pardies to Newton (21 May 1672). Of far greater significance is the fact that in the previous edition Bentley's sermons were printed with their title pages reversed.

The new edition makes a few minor corrections or emendations in the editorial introductions to the several sections, but it has not proved practical to have these introductions—some of which have become classics—entirely rewritten for this new edition. A major thrust of the original General Introduction was a program for further needed research on the scientific work and influence of Isaac Newton. Written more than two decades ago, those remarks have become obsolete and irrelevant today, and accordingly have been suppressed; almost all of the General Introduction has been written anew for this second edition. In the years since 1958, when the first edition was published, there has grown up an international industry of Newtonian scholarship of such magnitude that it may be doubted whether any scholar can find time to read its literature in full while engaged in creative research. I have included a comprehensive guide to this literature in my book-length article on Newton, in volume 10 of the *Dictionary of Scientific Biography,* edited by Charles Coulston Gillispie (New York: Charles Scribner's Sons, 1974), accompanied by a guide to the Soviet literature on Newton, prepared by A. P. Youschkevitch, which should serve any reader who may wish to pursue further any of the topics that are included in the present volume. But I have given references below (pp. 498ff) to some other bibliographical guides to the Newtonian scholarly literature and I have called attention to some major scholarly studies that may bear directly on the several editorial introductions; I have additionally indicated a few instances in which recent research may require some alteration or qualification of an opinion or conclusion expressed in those introductions; and I have added some supplementary notes on the texts of Newton's papers and letters. The pagination of this edition corresponds to that of the original with the exception of the front matter (paginated in Roman numerals) and the index, so that scholarly references to either edition will be valid in the other. It may be recorded that the current academic affiliations of the contributors of introductions to the several sections are as follows: Marie Boas Hall, Imperial College (London); Charles Coulston Gillispie, Princeton University; Thomas S. Kuhn, Princeton University; Robert E. Schofield, Case Western Reserve University.

It is sad to record that death has removed from our circle one of the authors of an editorial introduction, Perry Miller, and other scholars whose help was gratefully acknowledged in the first edition: Professor A. Koyré, Professor H. W. Turnbull, Dr. A. N. L. Munby, and Professor E. N. da C. Andrade.

With pleasure I reacknowledge a debt to Bern Dibner, who has once again made possible the publication of this work, through a

grant from the Burndy Library, Norwalk, Connecticut, of which he is the founder, director, and chief moving spirit. Once again I happily record our thanks to him for his continued support and encouragement of scholarly research in the history of science and for his enthusiastic encouragement of all our endeavors.

January 1977 I. BERNARD COHEN

Preface to the First Edition

Students of intellectual history and the history of science need no reminder that the majestic figure of Isaac Newton dominates the 18th century. The "Age of Newton" must be studied in the works of Newton himself, as well as in the writings of his commentators and the scientific books and articles that either continued the investigations undertaken by Newton or ventured into new domains of knowledge which he had not explored. The intention of the present volume is to bring together for the first time Newton's scattered papers and letters on natural philosophy (excluding mathematics, pure theology, and biblical chronology) as they were actually available *in print* during most of the 18th century, that is, prior to Horsley's edition of Newton's works in 1779–1785. Newton's two major books on physical science, the *Principia* and the *Opticks,* are today readily accessible, and in print; this volume complements them by placing in the hands of students all of Newton's related publications issued during his lifetime or soon after his death. The *Principia,* the *Opticks,* and the papers collected in this volume thus represent the complete corpus of Newton's writings on physical science that actually influenced the scientists and thinking men of the "Age of Newton"; the *Optical Lectures,* however interesting, were of less importance in conditioning the advance of science or moderating the general climate of opinion on the frame of the universe or the mechanism of nature.

Since the aim of this volume is to present to the modern scholar the very works studied during Newton's life and the decades following his death, each document is reproduced in facsimile from the original publication; a facsimile is provided of a standard translation into English of those documents which are in Latin. Since many of Newton's communications are letters, as was customary in the 17th and 18th centuries, there have also been included facsimiles of the printed letters and documents written by others that were the occasion of each of Newton's communications. In every case, the page numbers of the originals have been kept, so that the scholar will have available to him in facsimile the actual pages of many rare works which are not to be found in all libraries, and certainly not on the shelves of students who wish to study the development of physical thought in the age of Newton.

In addition to Newton's own letters and papers, and documents immediately relating to them, several Newtonian productions of rarity have been included. Fontenelle's *éloge* is the first published biography of Newton and was widely read in England and abroad. Halley's review of the *Principia* and the account of the theory of the tides which he wrote for James II are as useful today as they were then, serving to orient the nonspecialist to some major aspects of Newton's monumental achievement. Finally, all students of Newton will be grateful for a "Newtonian index" to Birch's *History of the Royal Society.*

The editor wishes to acknowledge the kindness of the scholars who have aided this coöperative venture by contributing prefaces to the several sections of the volume: Marie Boas, University of California (Los Angeles); Charles Coulston Gillispie, Princeton University; Thomas S. Kuhn, University of California (Berkeley); and Perry Miller, Harvard University. Dr. Robert E. Schofield, of the University of Kansas, has helped in every stage of preparing the book and has written several contributions to it.

The editor respectfully acknowledges the stimulation to the production of this volume given by Professor A. Koyré of the Ecole

Pratique des Hautes Etudes of the University of Paris (Sorbonne), who urged upon him the necessity of producing it. The editor gratefully records the sincere interest in the history of science of Mr. Bern Dibner, the guiding spirit in the formation of the Burndy Library of the history of science in Norwalk, Connecticut, who has sponsored many important publications in the history of science as well as this one. Valuable information was provided by Professor H. W. Turnbull, editor of the projected edition of Newton's correspondence, Mr. A. N. L. Munby, Librarian of King's College, Cambridge, and Curator of the Keynes Collection of Newton Manuscripts, and Professor E. N. da C. Andrade of London, master interpreter of science in our day, whose many publications on Newton and his times have provided illumination with an elegance and charm all too rare in the current literature of the history of science.

The editor hopes that this volume may be conceived as a transatlantic tribute to the Royal Society of London, whose role was of such major importance in the development of Newton's thought. All scholars who have had the privilege of using the great library of the Royal Society are aware of the feeling of awe that arises from confronting the manuscripts that record the major progress of science in a continuous succession of almost three centuries; the remembrance of that experience is always tempered by a warm feeling of gratitude for the extreme kindness and helpfulness of the present and past librarians, Messrs. I. Kaye and H. W. Robinson, and especially of the Assistant Secretary, Dr. D. C. Martin. In saluting the Royal Society, and its two Newtonian scholars, Professors Andrade and Turnbull, a word may be said about the forthcoming Royal Society edition of Newton's correspondence, being prepared under Professor Turnbull's editorship. This task, of immense complexity and beset by extremely difficult questions at every turn, will prove to be one of the most important collections of source material for the study of 17th-century science. Based upon a careful study of the manuscripts, it will provide a complete and accurate text of each document. Hence the student who wishes to know exactly what Newton wrote, or exactly what Newton's correspondents wrote, must always turn to the Royal Society edition of Newton's correspondence. But for the student who wishes to find out what the men of the late 17th and the 18th century actually read, on which they based whatever they in turn said or wrote, there can be no substitute for the original printed versions which are gathered together in the present volume.

The publication of this volume was made possible through a grant from the Burndy Library, Norwalk, Connecticut. All readers will share our gratitude to the Burndy Library and to its scholarly director, Bern Dibner, for continued generous support of research in the history of science. Unfortunately, owing to circumstances beyond our control, there has been a delay of almost three years between the initial completion of the volume and its present publication.

1956 I. BERNARD COHEN

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I.

General Introduction

General Introduction

I. BERNARD COHEN

Scholars are unanimous in describing the 18th century as the "age of Newton," but the exact sense of this phrase requires some clarification. Pope's couplet, about nature and her laws being "hid in night" until God created Newton "and all was light," has probably misled the many historians who have quoted it and it betrays a wonderful ignorance of the nature of science. We do not understand nature by the revelation of single laws, but rather by an apparently endless sequence of discoveries and of theories invented to explain them. If any stage of this sequence seems to represent so great an advance that it marks a new era, it may appear as a revelation but the revelation is never complete. The greatest work in science is as much characterized by the creation of new questions for the next generations as by the formulation of partial answers to questions raised in the past. Newton may be esteemed as the dominating figure of the 18th century—and even, to some degree, the 19th—because the questions he raised were so fundamental that the best brains in science were hardly up to answering them.

Those who have written about the "age of Newton" have tended to concentrate their attention on the *Principia,* admittedly his masterpiece and one of the greatest productions of the human mind, and on the problems Newton solved rather than the fruitful questions his work raised. In Newton's lifetime as during the 18th century—and ever since—the *Principia* was a formidable book to read and, prior to the appearance of the second edition in 1713, it was also a difficult book to obtain, owing to the small size of the original edition, which has been estimated at somewhere between 300 and 400 copies.[1]

The difficulty in reading the *Principia* arose not merely from the technical nature of the subject matter—terrestrial and celestial mechanics, and the motion of bodies under a variety of conditions of resistance—but also from the austere mathematical style of presentation. In the last of the three "books" into which the *Principia* is divided, Newton tells the reader that he had purposely chosen to make this part of his treatise difficult, so as to make it inaccessible to a non-mathematical reader. It is in this third "book" that Newton applies to the real astronomical world of sun, planets, moons, and stars those abstract and "mathematical principles of natural philosophy"[2] that he had developed in the earlier parts. "On this subject" of the System of the World he writes:

> I composed an earlier version of Book 3 in popular form, so that it might be more widely read. But those who have not sufficiently grasped the principles set down here certainly will not perceive the force of the conclusions, nor will they lay aside preconceptions to which they have become accustomed over many years; and therefore, to avoid lengthy disputations, I have translated the substance of the earlier version into propositions in a mathematical style, so that they may be read only by those who have first mastered the principles.[3]

[1] This estimate was made by A. N. L. Munby, "The Distribution of the First Edition of Newton's *Principia*," *Notes and Records of the Royal Society of London 10*, 28–39 (1952); see, further, I. B. Cohen, *Introduction to Newton's 'Principia'* (Cambridge: at the University Press; Cambridge, Mass.: Harvard University Press, 1971), p. 138.

[2] That is, in the first two "books" (*De motu corporum*) Newton develops "mathematical principles" of motion, and then in the "third book" shows how these principles may be used in "natural philosophy."

[3] Quoted from a new translation of the *Principia* (by I. B. Cohen and Anne Whitman), opening of Book 3.

Evidently, those who became acquainted with Newton's concepts, methods, and achievements must have used sources other than the *Principia.*

In the 18th century there were three major introductions to the Newtonian natural philosophy (or the principles of Newtonian physics) available to scientists and non-scientists: the books by Pemberton, Voltaire, and Maclaurin.[4] But even before these three works had been written, Richard Bentley had produced the first general account of the Newtonian principles for the non-mathematical reader: the final two sermons in a set of eight that inaugurated the series founded by Robert Boyle.[5] This pair of sermons constituted the concluding sections of Bentley's *Confutation of Atheism,* in an argument taken "from the Origin and Frame of the World," reproduced on pages 313–394 below. Like other Boyle Lectures, these were often reprinted during the 18th century, and thus served as a convenient entry into the nontechnical aspects of the Newtonian principles. (Their initial publication was in 1693, a bare six years after the appearance of the *Principia.*)[6] As Perry Miller points out (pages 273–274 below), Bentley's two sermons have a special significance that by far transcends their having been "the first popular attempt" to make known the "sublime discoveries" of Newton; additionally they "set a precedent for the entire Enlightenment" in showing, by Newtonian principles, that "the order of

[4] Henry Pemberton, *A View of Sir Isaac Newton's Philosophy* (London: S. Palmer, 1728—facs. repr. by Johnson Reprint Corporation, New York and London, 1972); Colin MacLaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (London: printed for the Author's Children, 1748—facs. repr. by Johnson Reprint Corporation, New York and London, 1968); Voltaire's *Elements of Sir Isaac Newton's Philosophy,* an English translation by John Hanna (London: printed for Stephen Austen, 1738—facs. repr. by Frank Cass & Co., London, 1967) appeared in the same year as the original French version; the later (corrected and expanded) French edition has never been translated into English.

[5] For details see p. 272 below; and, further, the works by Margaret C. Jacob, cited in the Supplement, §IV.

[6] Among the Newtonians who gave Boyle Lecture Sermons in succession to Bentley were John Harris, Samuel Clarke, William Whiston, William Derham, Thomas Burnet; for a list of Boyle Lecturers from 1692 to 1961, see John F. Fulton, *A Bibliography of the Honourable Robert Boyle* (second ed.; Oxford: at the Clarendon Press, 1961), Appendix I, pp. 197–200. As William B. Todd observed (in his "Note on the Printing of Bentley Sermons"), there were two separate issues of the first edition in the year 1693.

the universe could not have been produced mechanically." A companion reprint (pages 279–312 below) is a set of four letters written by Newton to Bentley while the latter was getting his sermons ready for publication; when these letters were published in a pamphlet in 1756, Samuel Johnson (in the *Literary Magazine,* 1756, vol. 1, p. 89) observed that they "contain many positions of great importance," and concluded: "There can be no regular system produced but by a voluntary and meaning agent."

Also of benefit to English readers, a book review by Edmond Halley became available soon after the publication of the *Principia.* Halley's review is more than ordinarily interesting since he not only edited Newton's manuscript for the printer and saw the work through the press, but had been primarily responsible for getting Newton to write it in the first place.[7] The review, printed below on pages 405–411, appeared in the *Philosophical Transactions* of the Royal Society of London, a journal then being published by Halley himself. An "advertisement" (reprinted below on p. 411) accompanied his review, apologizing for the fact that the publication "of these *Transactions* has for some Months last past been interrupted" because Halley had had the entire "care of the Edition of this Book of Mr. *Newton*"; and he added that in the publication of the *Principia,* "he conceives he hath been more serviceable to the Commonwealth of Learning" than he would have been in merely issuing the *Philosophical Transactions* on an uninterrupted schedule. It may be observed that Halley was indirectly critical of Newton for having failed to give Kepler credit for the law of areas and the law of elliptical orbits.[8] Halley also pointed out (correctly) that one of the notable features of the *Principia* was Newton's "great skill" in using the new mathematics, by which Halley meant Newton's own "method of *infinite* Series."

In addition to these documents of Bentley and Halley,[9] this

[7] Halley also ended up paying the printing costs; see my *Introduction,* Ch. III (§§1-2), Ch. IV (§8), Ch. V (§3), and Suppl. VII. On three other reviews of the first edition of the *Principia* (one in Latin and two in French), see pp. 399, 429 below, and my *Introduction,* Ch. VI.

[8] See p. 406 below; in the *Principia,* Newton gives Kepler credit only for having found the third (or "harmonic") law.

[9] There is also reprinted (pp. 412–424 below) an account of Newton's theory of the tides, "extracted" by Halley from the *Principia* and "presented . . . to . . . King James."

volume contains yet another of the general accounts of Newton's achievements, one that was widely read on its publication: Fontenelle's *éloge* of Newton—the first biography of that great man. The text, printed (on pages 444–474 below) from one of the three almost simultaneous English translations, adds the Continental point of view, since Fontenelle's account of Newton's life and achievement was in fact the official memorial written for the Paris Académie Royale des Sciences, of which Newton had been elected an *associé étranger* in 1699. As Charles Gillispie shows in his introduction (page 436 below), Fontenelle obtained his biographical information from John Conduitt, who had married Newton's niece. Conduitt had intended to write a biography of Newton and had sought information from everyone who had known him.[10]

Fontenelle's life of Newton is a primary document in the history of the struggle to liberate French science and thought from the shackles of the Cartesian philosophy, for which Fontenelle had greater sympathy than for the Newtonian philosophy which it was his duty to expound in the official *éloge*. This feature of the *éloge* highlights the anomaly about Newton's position as *associé étranger* of the Académie Royale des Sciences (Paris), since his *Principia* was so largely devoted to an attack on the Cartesian philosophy, which was then the reigning system in France. Not only did Newton show in the *Principia* that the "hypothesis of vortices" is inconsistent with observed phenomena, but his *Opticks* also confuted the Cartesian system. We know that Newton's antagonism to Descartes was extreme, that he not only made a pointed attack on Cartesian physics again and again in the *Principia,* but in his own copy of Descartes' geometry "marked in many places with his own hand, *Error, Error, non est Geom.*"[11] So strong was this feeling on the part of Newton that

[10] The biographical materials assembled by Conduitt, and his own notes and sketches toward a full biography, are conveniently located in the library of King's College, Cambridge (Keynes MS 130), together with a copy of the memoir sent by Conduitt to Fontenelle, and various supporting documents and correspondence (Keynes MS 129), and related letters and manuscripts (Keynes MSS 130–137); these are described in Sotheby's *Catalogue of the Newton Papers, sold by order of the Viscount Lymington* (London, 1936), lot numbers 212–221, pp. 53–60.

[11] Quoted from Sir David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton* (Edinburgh: Thomas Constable and Co., 1875—facs. repr. by Johnson Reprint Corporation, New York and London, 1965, with an introduction by R. S. Westfall), vol. 1, p. 22n. Newton's own copy of the *Géométrie* has only recently turned up in the Trinity College Library; his marginal comments are now

we are led to suspect that the title of his masterpiece, *Philosophiæ Naturalis Principia Mathematica,* was intended to show its superiority over Descartes's *Principia Philosophiæ,* of which a copy of the edition of 1656 in quarto was in his library. And it is tempting to suspect further that when Newton altered the HYPOTHESES at the beginning of Book Three of the *Principia* (in the first edition of 1687) to "Hypothesis," "Phænomena," and "Regulæ Philosophandi," the latter were intended to supplant Descartes' "Regulæ ad Directionem Ingenii."[12]

Newton is generally said to be one of the first group of eight *associés étrangers* elected to the Académie Royale des Sciences, and the official list of members includes his title as *premier titulaire.* The manuscript Régistres show, however, that he was the last of the eight to be chosen, and that the choice was made only on the fourth discussion of the question. Under the new charter of 1699, there was place for eight *associés étrangers,* of whom the first three were G. G. Leibniz, E. W. v. Tschirnhaus, and Domenico Guglielmini, who were already members when named to the new title on 28 January 1699. According to the manuscript Régistres, on Saturday 14 February 1699, "On a résolu à la pluralité des voix de proposer au Roy M^{rs}. Hartsoëker, et Bernoulli, l'aîné et le cadet, pour Associez Etrangers," the Bernoullis being the brothers Jacques and Jean (I^{er}).

seen to be something quite different from the general devaluation of Descartes' book previously supposed. Rather than an all-inclusive "Error. Error. Non est geom." reported by Brewster (and previously by Conduitt), Newton's marginal annotations primarily indicate an "Error" here and there; the marginal entry "non geom." was intended to note such matters as that the Cartesian classification of curves is not really a part of geometry so much as it is of algebra.

[12] On the stages of transformation of these "Hypotheses" to "Phænomena" and "Regulæ Philosophandi," see I. B. Cohen, "Hypotheses in Newton's Philosophy," *Physis, rivista internazionale di storia della scienza 8,* 163–184 (1966), and *Introduction to Newton's 'Principia',* Ch. VI, 6.

A reader of Newton's *Principia* would never guess how important an influence Descartes had been on the formation of Newton's concepts of dynamics and even his formulation of the "Laws of Motion," and would suppose that Descartes' role had been limited to being a target for Newton's attacks. In fact, J.-L. Lagrange concluded that the main purpose for which Newton had written Book 2 was to destroy the basis of the Cartesian theory of vortices. Nevertheless, scholarly research of the last two decades has revealed how important Descartes' writings had been in the formulation of Newtonian dynamics, a fact which Newton so purposively sought to conceal.

Then, according to the Régistres, on Saturday 21 February, "M^{r}. Roëmer qui a été autrefois membre de l'Académie et qui est retourné en Dannemarc depuis longtemps, et M^{r}. Newton ont été nommés pour les deux places qui restoient d'Associés Etrangers."[13]

It is well known that Newton was deeply concerned with the subject of chemistry and the constitution and composition of matter. There is more than a hint about this in the original Preface to the *Principia,* as well as elsewhere in that treatise, and as a subject it looms large in a suppressed conclusion written out for the first edition.[14] In Newton's lifetime, and during the 18th century, his major writing on chemistry to be widely circulated in print was the tract "De Natura Acidorum," printed below (pages 255–258) in both an original Latin and English version. This is accompanied by his "Scala Graduum Caloris," published anonymously in the *Philosophical Transactions,* and which, as Marie Boas Hall points out in her Introduction (page 243 below), "is the source for Newton's Law of Cooling." The Introduction (page 243) also wisely notes that these two papers are not alchemical, and that they differ notably (as do his other "chemical papers") from the "approach to chemical problems . . . of an alchemist."[15]

In Newton's lifetime these two primary texts on chemistry were not his only publications on this subject; there were also the Queries printed at the end of his *Opticks,* which were revised and expanded in the several successive editions,[16] notably the final 31st Query. These later Queries also express Newton's final and mature views concerning the æther, which differ in several significant details from

[13] On this topic, see I. B. Cohen, "Isaac Newton, Hans Sloane and the Académie Royale des Sciences," pp. 60–117 of I. B. Cohen and René Taton, eds., *L'aventure de la science,* Mélanges Alexandre Koyré, vol. 1 (Paris: Hermann, 1964), esp. §II ("Newton & the Académie des Sciences").

[14] These two important documents (the partial draft of the preface and the suppressed conclusion) were brought to light and published by A. Rupert Hall and Marie Boas Hall in their volume of *Unpublished Scientific Papers of Isaac Newton* (Cambridge: at the University Press, 1962), part IV, §§3,7.

[15] Some other scholars—notably Betty Jo Teeter Dobbs, P. Rattansi, Richard S. Westfall—would argue for a closer link between Newton's alleged alchemy and chemistry, and even between this alchemy and the general principles of Newtonian physics (including dynamics); see the Supplement, §III.

[16] See pages 14–15 below.

his early concept of the æther.[17] Newton wrote out two separate expositions at large concerning the æther, as he had conceived it during the 1670s. One of these makes up a long "letter" (really what was then called an "epistolary discourse") addressed to Robert Boyle and dated 1678, first printed by Thomas Birch in his biography of Boyle, published in 1744, and reprinted below on pages 249–254.[18]

Newton's letter to Boyle was reprinted at least twice in the 18th century, in the second edition of Boyle's works (1772) and separately—with a commentary by Bryan Robinson—in 1745.[19] In this letter Newton attempted to show that the æther was responsible for the cohesion of bodies, played a part in the actions of acids and other chemical reactions, operated to produce and maintain the gaseous state, caused various optical phenomena, and could be considered "the cause of gravity." This letter was widely studied in the middle of the 18th century, and its influence on chemists and physicists was marked. It was quoted or cited by many students of electricity and affected the form that was taken by theories of electrical action.[20] The hypothesis of the æther also turns up in Newton's optical papers, published in the *Philosophical Transactions* during his young manhood, and was the subject of a famous long paper read at the Royal Society in 1675 and published by Birch in his *History of the Royal Society* in 1757; it is reprinted on pages 178–191 below. Finally, the possibility of an æther arises as a topic of some importance in the letters Newton wrote to Richard Bentley in 1692/3, which were published in 1756.[21]

The concept of the æther entered into, and bound together, a number of different types of phenomena studied by Newton. In the

[17] These differences have been explored by A. R. and M. B. Hall, Henry Guerlac, R. S. Westfall, and Joan L. Hawes.

[18] Birch was also responsible for the printing of Newton's other statement of the 1670s on the æther and its possible role in physical phenomena; see pp. 178–191 below.

[19] See J. F. Fulton, *Bibliography of Boyle.*

[20] See I. B. Cohen, *Franklin and Newton* (Philadelphia: American Philosophical Society 1956: Cambridge, Mass.: Harvard University Press, 1966; a new edition is forthcoming).

[21] That is, Newton argues (p. 303 below) that "Gravity must be caused by an Agent acting constantly according to certain Laws; but whether this Agent be material or immaterial, I have left to the Consideration of my Readers."

letter to Boyle (page 250 below), the æther is supposed "to stand rarer" in the "pores" of "all gross bodies" than "in free spaces, and so much the rarer as their pores are less." This "ætherial substance" is postulated to be "diffused through all places," and to be "capable of contraction and dilation, strongly elastic, and, in a word, much like air in all respects, but far more subtile." And then Newton is led to "suppose (with others)" that the æther may be "the cause" of the following phenomena: the refraction or bending of light, at the surface of bodies, toward the perpendicular; the cohesion of metal objects with flat polished surfaces, in a vessel evacuated of air; the fact that a column of mercury "stands sometimes up to the top of a glass pipe . . . higher than 30 inches" in a barometer tube; the coherence of the parts of all bodies (or at least "one of the main causes" of this phenomenon); filtration; the rise of water in capillary tubes; and, possibly, the pervasion of the "pores of bodies" by "menstruums" (or acids) which dissolve them. This same æther may cause the bending of light near the edges of opaque bodies (diffraction), as observed by Grimaldi. Finally, Newton "set down one conjecture more" (page 253 below), that in the action of this æther one might find "the cause of gravity"; this came into his mind as he "was writing this letter." In his presentation to the Royal Society in 1675 (see pages 182–183 below), Newton additionally suggested the possibility that the action of a "certain æthereal spirit included within the *dura mater*" enables the mind (soul or will) to communicate its commands to the muscles, and that the æther may explain how "the muscles are contracted and dilated to cause animal motion."

It is now generally acknowledged that Newton's views concerning an æther went through different successive stages. Following the early period up to the 1670s (including the writing of the letter to Boyle and the paper for the Royal Society), during which Newton gave serious consideration to the æther, he seems to have abandoned any firm commitment to the possibility of there being such an æther at all. Thus, the *Principia*, written in 1685–1686, during this second stage, is relatively free of references to the æther.[22] Before long,

[22] But in Book 3, Newton does write of an "aura ætherea" that fills the celestial spaces and would appear to be much like air in its physical properties, although much "rarer" and "far more subtle," since it does not impede the free motion of planets and comets. This discussion of an "aura ætherea" (or "ætherial atmos-

Newton had come back full circle to considerations of an æther, albeit a somewhat different one from that of the 1670s and earlier. In part, he was stimulated to consider the æther anew by the scheme proposed by Nicolas Fatio de Duillier, which had the particular virtue among æther theories of explaining not only how bodies near the earth or other large body would be attracted, but also how all such attractions between two bodies would be mutual—equal in magnitude but opposite in direction.[23] Newton was also greatly stimulated by the electrical experiments of Francis Hawksbee and introduced into the *Principia* a vague paragraph about a "most subtle spirit," the action of which—he then believed—was the cause of most of the phenomena said in the 1670s to have been possibly caused by the æther.[24] This "spirit" is referred to

phere") is introduced specifically to explain how the vapors exuded by comets, when heated near the sun, "rise" so as to point away from the sun—just as smoke particles are carried upwards by the heated air in a chimney. Newton evidently wrote out this discussion of comets' tails (which appears also in the preliminary version of Book 3, published posthumously as *De mundi systemate liber* [London, 1728] and in translation as *A Treatise of the System of the World* [London, 1728]) before he performed an experiment (in which he compared the oscillatory motion of a pendulum with a bob consisting of an empty wooden box with the similar motion of that same pendulum when the box was filled with metal) which allegedly proved that there can be no æther—at least of a sort that can resist motion. See, on this topic, my *Introduction to Newton's 'Principia',* pp. 103–104; also R. S. Westfall, "Uneasily Fitful Reflections on Fits of Easy Transmission," pp. 88–104 of Robert Palter, ed., *The Annus Mirabilis of Sir Isaac Newton 1666–1966* (Cambridge, Mass., and London: The M. I. T. Press, 1970), and R. S. Westfall, *Force in Newton's Physics* (London: Macdonald; New York: American Elsevier, 1971), pp. 375–377, where it is argued (without evidence) that this experiment may be "tentatively placed about 1679"; see also Henry Guerlac, "Newton's Optical Aether, " *Notes and Records of the Royal Society of London 22,* 45–57 (1967).

The experiment is described from "memory" in the concluding part of the General Scholium at the end of Book 2, Sec. 7 in the first edition of the *Principia* (1687): transferred to Book 2, Sec. 6, in the second edition (1713). The discussion of the "aura ætherea" occurs in Book 3 (p. 514 of the final edition, 1726), toward the conclusion of the lengthy "Exemplum" following Prop. 41 (in the fifth paragraph from the end.)

[23] On Fatio's æther-theory of gravitation, see Newton, *Correspondence,* vol. 3, pp. 69–70, 309; and on Newton's enthusiasm for this theory, see A. R. and M. B. Hall, *Unpublished Scientific Papers,* pp. 312–317, and my *Introduction to Newton's 'Principia',* pp. 184–185.

[24] A. R. and M. B. Hall first revealed the startling fact that this "spirit" was to be understood in relation to electricity; see their *Unpublished Scientific Papers,* pp. 207-213, 348, 357, 361-362.

in the concluding General Scholium that first appeared in the second edition of the *Principia* (1713). Here Newton stated his famous slogan, *Hypotheses non fingo* ("I frame, or feign, no hypotheses"), and asserted that it is enough to account mathematically for the motion of moon and planets and for the tides, even though the cause or mechanism of gravitational action remained unknown. But then he gave his readers a hint

> . . . concerning a certain very subtle spirit pervading gross bodies and lying hidden in them; by its force and actions, the particles of bodies mutually attract one another at very small distances and cohere when they become contiguous; and electrified bodies act at greater distances, repelling as well as attracting neighboring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the limbs of animals move at command of the will, namely, by the vibrations of this spirit being propagated through the solid fibers of the nerves from the external organs of the senses to the brain and from the brain into the muscles. But these things cannot be explained in a few words; furthermore, there is not a sufficient number of the experiments needed to determine and demonstrate accurately the laws governing the actions of this spirit.

This list of phenomena is remarkably similar to the phenomena discussed in the two documents of the 1670s, the letter to Boyle and the presentation to the Royal Society. Only gravitation is omitted.

This paragraph is printed again, without alteration, in the third and ultimate edition of the *Principia* in 1726. In the English translation published by Andrew Motte in 1729, shortly after Newton's death, the phrase "this spirit" is rendered as "this electric and elastic spirit," even though—as A. Rupert Hall and Marie Boas Hall discovered—the words "electric and elastic" are not to be found in the MS drawn up for the printer, nor in the printed second (1713) and third (1727) editions.[25] But these words do occur in Latin in an emendation made by Newton in his own hand in his personal annotated copy of the second edition, one that he evidently intended to introduce into a third edition.[26] Why he never did so, we do not know. Motte's version, therefore, accurately represents Newton's thoughts at some post-1713 date, but perhaps not his final

[25] A. Rupert Hall and Marie Boas Hall, "Newton's Electric Spirit: Four Oddities," *Isis 50,* 473–476 (1959).

[26] A. Koyré and I. B. Cohen, "Newton's 'electric & elastic' Spirit," *Isis 51,* 337 (1960).

opinion as of 1727. In the meanwhile, Newton's later version of the æther had been revealed to the public in the last Queries he put into his *Opticks.*

In the second (1717, 1718) and third (1721) English editions of the *Opticks,* the Queries at the end of Book III were enlarged from the original sixteen in the first edition (1704) to thirty-one. Query 17 takes up the problem of vibrations in the medium in which light travels, vibrations which put the rays of light into "fits" of easy reflection and easy transmission; Query 18 deals with further properties of this medium in relation to radiant heat; Queries 19 and 20 suggest that variations or differences in the "density" of the "ætherial medium" may account for refraction and inflection. In Query 21 Newton addressed himself to gravitation, the possibility that variations in the "density" of the "medium" may produce gravitation since the "medium" is much rarer within dense bodies such as the sun, planets, comets, stars, than in empty celestial space. Query 22 is devoted to the demonstration that this "æther" can offer a negligible resistance to the motion of planets and comets. Finally, in Query 23 vision is said to result chiefly from vibrations of this medium propagated through the optic nerve, and in Query 24 the vibrations of the medium are related to animal sensation being conveyed to the brain.

In 1706, two years after the first English edition and seven years before the second Latin edition of the *Principia* with the famous concluding General Scholium, a Latin version of the *Opticks* was published, prepared by Samuel Clarke at Newton's request. In this edition the number of Queries was increased from the original sixteen to twenty-three. But the new Queries in this Latin version do not correspond to Queries 17–23 in the second and third English editions of the *Opticks;* they do not deal with the æther at all. These new Queries of 1706 rather correspond to Queries 25–31 in the later English editions of the *Opticks.*

When Newton brought out the second English edition of the *Opticks* in 1717, he printed for the first time the Queries there numbered 17–24, which presented his general views on the nature, properties, and effects of a supposed æther; and these were followed by revised English versions of the Queries he had added in the Latin version of 1706, now renumbered 25–31. In respect to these new Queries about the æther, Newton said (in this English edition of the *Opticks,* 1717), that "to shew that I do not take gravity for an essential property of bodies, I have added one question concerning its cause, chusing to propose it by way of a question, because I am not yet satisfied about it for want of experiments."

For the scientists of the 18th century, there were thus three major sources for Newton's views on an æther: the letter to Boyle and Newton's "Hypothesis" of 1675 (published below on pages 178–190, 250–253) and Queries 17–24 of the later English and Latin editions of the *Opticks.* The phenomena which Isaac Newton had hoped to account for[27] are remarkably similar in the two documents of the 1670s, those Queries 17–24, and in the final paragraph about the "most subtle spirit" in the concluding General Scholium of the *Principia.* Accordingly, there is a mystery as to why Newton permitted this paragraph of the General Scholium to remain in the *Principia* in the third edition of 1726, when he apparently no longer was committed to a belief in a "spiritus electricus," and when we would accordingly have supposed that he would either have eliminated this paragraph altogether, or have substituted for it a revised version based on the concept of an "æther" or an "ætherial medium" that had been expressed in the *Opticks.* In his own interleaved copy of the second edition of the *Principia* he, in fact, did indicate that the final paragraph of the General Scholium should be cancelled, but it remained in the third edition just the same. The continuing presence of this paragraph, after the later explicit references to the æther in the *Opticks,* naturally gives rise to the question as to how closely this "electrical spirit" of the General Scholium was associated with (or even was identical to) the æther in the last Queries of the *Opticks.*[28] But it must be kept in mind that gravita-

[27] These include the reflection, refraction, and inflection (diffraction) of light; the transmission of radiant heat; gravity; the transmission of sensation to the brain via the nerves, and the control of the action of the muscles by the will, acting through the action of the nerves; and possibly capillarity; the cohering of bodies; and even some aspects of chemical action.

[28] For conflicting views on this question, compare A. R. and M. B. Hall, *Unpublished Scientific Papers,* p. 208, and R. S. Westfall, *Force in Newton's Physics,* pp. 392–393.

tion was conspicuously absent from the list of phenomena in the final paragraph of the General Scholium, although it was featured in the last Queries in the *Opticks* and had appeared also in the two statements on the æther of the 1670s.[29]

Although Newton expressed his thoughts on the æther in the guise of Queries in the *Opticks,* his fellow scientists and their successors were well aware that he was merely using the form of rhetorical questions to explain how his "hypothesis" of the æther could account for phenomena.[30] And, despite the slogan *Hypotheses non fingo* in the General Scholium, there was no lack of awareness that Newton had never really been as adverse to hypotheses as many of his commentators have supposed. Certainly after the publication of his "Hypothesis" of the 1670s in Birch's volume of 1757, no one could believe that this greatest of scientists had in fact eschewed hypotheses in relation to his scientific thought.

Newton's speculations were studied very carefully during the next two centuries, and they produced important consequences. The writings about a universal "fluid" gave sanction to the creation of other imponderable fluids, such as the electrical fluid and the fluid of caloric (or heat); but the development of the concepts of these fluids did not slavishly follow Newton's principle of "density." The scientists who tried to explain electrical phenomena by variations in density of some "subtle fluid" were not able to produce results of

[29] The striking similarity of Newton's views of the 1670s and his views in the Queries, despite the differences between the earlier "thick" æther and the later tenuous or "thin" ætherial medium, may be seen in Query 21, where Newton says: "Is not this Medium much rarer within the dense Bodies of the Sun, Stars, Planets and Comets, than in the empty celestial Spaces between them? And in passing from them to great distances, doth it not grow denser and denser perpetually, and thereby cause the gravity of those great Bodies towards one another, and of their parts towards the Bodies; every Body endeavouring to go from the denser parts of the Medium towards the rarer?"

The *Opticks* is available in a convenient reprint (New York: Dover Publications, 1952), with a foreword by Albert Einstein, an introduction by Sir Edmund Whittaker, a preface by I. B. Cohen, and an analytical table of contents prepared by Duane H. D. Roller; a new edition (in press) has a revised preface.

[30] See my *Franklin and Newton,* p. 164. Newtonians would say that Newton had "explained" something in the *Opticks* "by way of Query."

importance, while those who sought to identify the "electric fluid" or the "fluid of heat" with Newton's universal æther appear never to have advanced much beyond the stage of hypothesis and speculation. Those who really advanced the theories of heat and of electricity in the 18th century began with the concept of a particulate "elastic fluid," much like Newton's æther, composed of particles that mutually repel one another; and then went on to develop such properties of these fluids as were directly related to the outcome of experiments.[31] By the 19th century, the use of such concepts as fluid of heat or electricity lost favor, especially when it was recognized that the use of the word "fluid" in the sense of flowing or transfer from one body to another implied the existence of a material or quasi-material substance that was far from being warranted, much less required, by the phenomena.

Newton's strong stand against theories based on simple "action-at-a distance" is said to have inspired Faraday in his attempts to understand electric and magnetic phenomena. In particular, Faraday "loved to quote" a passage from one of the letters that Newton wrote to Bentley,[32] and which Maxwell also repeated with enthusiastic approval. The passage in question (see page 302 below) reads as follows:

> It is inconceivable, that inanimate brute Matter should, without the Mediation of something else, which is not material, operate upon, and affect other Matter without mutual Contact, as it must be, if Gravitation in the Sense of *Epicurus,* be essential and inherent in it. And this is one Reason why I desired you would not ascribe innate Gravity to me. That Gravity should be innate, inherent, and essential to Matter, so that one Body may act upon another at a Distance thro' a *Vacuum,* without the Mediation of any thing else, by and through which their Action and Force may be conveyed from one to another, is to me so great an Absurdity, that I believe no Man who has in philosophical Matters a competent Faculty of thinking, can ever fall into it.

Thus Newton, by concentrating attention on what happens in the space between bodies rather than on the bodies themselves, pre-

[31] *Ibid.,* Part Four (Ch. 9), and *passim.*

[32] John Tyndall, *Faraday as a Discoverer* (London: Longmans, Green, and Co., 1870), p. 82.

pared the way for the fruitful concept of "field," in particular for Faraday's version of it as a set of "strains" in the ætherial medium around charged or magnetized bodies—the famous theory of "lines of force." In turn, Faraday's research led to Clerk Maxwell's theory of "displacement currents" in the æther, and Clerk Maxwell's electromagnetic theory may be considered legitimately the high point of "classical" physics, the physics of the 19th century. To be sure, since the acceptance of Einstein's restricted theory of relativity (published in 1905), the concept of the æther, along with all Newtonian "absolute" space and time, has vanished from the discourse of physics—apparently having served a useful function for at least two centuries but needed no longer. It is not amiss, however, to note that P. A. M. Dirac, one of the most distinguished physicists of our era, has raised the question of whether the æther is completely dispensable.

Even in the early 19th century, some physicists discerned inherent difficulties in applying the concept of the æther in the fashion proposed by Newton. John Playfair put the whole problem in succinct form:

> It is very true that an elastic fluid, of which the density followed the inverse ratio of the distance from a given point, would urge the bodies immersed in it, and impervious to it, toward that point with forces inversely as the squares of the distances from it; but what could maintain an elastic fluid in this condition, or with its density varying according to this law, is a thing as inexplicable as the gravity which it was meant to explain. The nature of an elastic fluid must be, in the absence of all inequality of pressure, to become everywhere of the same density. If the causes that produce so marked and so general a deviation from this rule be not assigned, we can only be said to have substituted one difficulty for another.[33]

[33] John Playfair, "Dissertation Third: Exhibiting a General View of the Progress of Mathematical and Physical Science since the Revival of Letters in Europe," pages 433–572 of *Dissertations on the History of Metaphysical and Ethical, and Mathematical and Physical Science,* by Dugald Stuart, the Right Hon. Sir James Mackintosh, John Playfair, and Sir John Leslie (Edinburgh: Adam and Charles Black, 1835), Section IV, "Astronomy."

Another difficulty came from the fact that the æther as postulated by Newton acted as it did because the particles of which it was supposed to be made were mutually repulsive. While this very fact delights the student of Newton in affording an example of that atomism which was fundamental to the Newtonian view of nature, it raises the thorny question why a wholly inexplicable short-range repulsive force between tiny particles of æther may be considered more satisfying than an equally inexplicable long-range attraction between gross bodies.

The profound puzzle of Newton's views on the æther disturbs Pope's view that after the revelation according to the *Principia* "all was light." Newton's discussion of how nature might produce the forces whose laws he had illuminated is, therefore, essential to our understanding of the whole Newtonian natural philosophy. The development of physics in the 18th and 19th centuries cannot be studied without a clear view of Newton's own statements on what we may call the mechanism of nature's actions. To this end, the present volume reprints, with commentaries, the original documents that were the vehicles for transmitting Newton's speculations—and it does so in the exact form in which scientists and philosophers and men of learning studied them during most of the 18th century, and afterwards.

Newton's published writings on the physical sciences, as known to the 18th century, included the two treatises that were printed in his lifetime, the *Principia* and the *Opticks,* plus two posthumous works, the *Lectiones Opticæ* and *De Mundi Systemate,* a tract on the motion of the moon[34] and a series of papers or letters on optics, published in the *Philosophical Transactions.* These papers include Newton's famous presentation (pages 47–59 below) of his studies of dispersion and the composition of sunlight, and the description (pages 61–66 below) of the new reflecting telescope he had invented after becoming convinced that chromatic aberration severely limited the potential of

[34] For the history and bibliography of Newton's tract, see *Isaac Newton's Theory of the Moon's Motion,* with an Introduction by I. B. Cohen (London: Dawson, 1975); Newton's *Treatise on the System of the World* has been reprinted with an introduction by I. B. Cohen (London: Dawsons of Pall Mall, 1969); an edition of the *Lectiones Opticæ* is being readied for publication by Alan Shapiro.

telescopes using glass lenses. As Thomas S. Kuhn points out in his Introduction (page 27 below), Newton's paper on light and color is for many reasons one of the most significant papers ever published by the Royal Society; for it is the first printed work by Isaac Newton, and it is apparently the first announcement of a major scientific discovery in an "article" published in a scientific journal. These optical papers make up more than half of the Newtonian texts printed below; collecting them together was a major *raison d'être* of this volume.

Newton had evidently expected his fundamental paper on his prismatic experiments to be greeted with the plaudits of the *cognoscenti* all over the scientific world. But, instead, there was letter after letter of criticism, and Newton felt himself obliged to respond to them all. No wonder that he said, in exasperation, that "a man must either resolve to put out nothing new, or to become a slave to defend it."[35] He later expressed this same sentiment even more bitterly by declaring science to be "such an impertinently litigious Lady that a man had as good be engaged in Law suits as have to do with her."[36]

In his Introduction to Newton's optical papers, T. S. Kuhn stresses certain significant aspects of Newton's observations of the elongated spectrum produced by sunlight entering a dark room through a small round hole in the shutter and then passing through a prism (pages 31–33), and the ways in which Newton sought to destroy the "modification theory." In particular, Kuhn lays stress on the "oddity" of a discrepancy between the actual length of the spectrum and the length predicted by the older theory. But what may be even more significant to us today is the difference between Newton's "precise and detailed description of his experimental apparatus" and what Kuhn calls Newton's "imaginative idealization of his experimental results."

Newton's paper on what he called the "celebrated phænomena of colours" is, as Kuhn has noted "almost autobiographical in its development." But there has been a long-standing puzzle ever since the publication by Rupert Hall in 1948 of portions of one of

[35] Isaac Newton to Henry Oldenburg, 18 November 1676, *Correspondence,* vol. 2, p. 183.

[36] Isaac Newton to Edmond Halley, 20 June 1686, *Correspondence,* vol. 2, p. 437.

Newton's early notebooks in which he discusses the properties of light rays in terms of "globules" with differing finite velocities, interacting with themselves and other bodies according to the laws of impact.[37] Kuhn has alerted us to the discrepancy between these clearly expressed early views and the mode of presentation in that paper of Newton's. More recent studies, notably by Jos. A. Lohne, have shown how difficult it is to accept as simple truth the historical narrative proposed by Newton at the beginning of the letter read to the Royal Society on 8 February 1672, which became Newton's first published work.[38] In another analysis of Newton's narrative, A. I. Sabra has concluded that not even "the 'fortunate Newton' could have been fortunate enough to have achieved this result in such a smooth manner."[39]

The collection of all of the letters and papers by Newton in the *Philosophical Transactions* from 1672 to 1676, plus the related published communications (from M. de Bercé, Sir Robert Moray, Père Ignatius Gaston Pardies, Robert Hooke, Christiaan Huygens, Franciscus Linus, and Anthony Lucas) has been supplemented by a paper of Hooke's (pages 110–115 below) and the text of Newton's lengthy presentation to the Royal Society (including the explication of his hypothesis of the æther). These two documents had been read at meetings of the Royal Society, but were not published in the *Philosophical Transactions,* remaining unprinted until they both appeared in 1757 in Birch's *History of the Royal Society.* Since all too few readers in the 20th century are familiar with Latin, the early 19th-century translations of all Latin documents have been included.

The papers and letters assembled in this volume enable the reader to read the sources of Newtonian natural philosophy available in the age of Newton, other than his four published books, and

[37] A. Rupert Hall, "Sir Isaac Newton's Note-Book, 1661–1665," *Cambridge Historical Journal 9,* 239–250 (1948).

[38] J. A. Lohne, "Experimentum Crucis, " *Notes and Records of the Royal Society of London 23,* 169–199 (1968) and "Isaac Newton: the Rise of a Scientist, 1661–1671," *idem 20,* 125–139 (1965).

[39] A. I. Sabra, *Theories of Light from Descartes to Newton* (London: Oldbourne, 1967), p. 246.

in facsimiles of the very printed pages that were available to students of science and philosophy in the 17th and 18th centuries. The present volume thus illustrates one of two very different aspects of Newtonian scholarship, which are complementary to each other. The first of these is to understand Newton's complex personality and the nature of the creative process as illustrated by the whole range of his intellectual activity; the second, to trace the influence of what he wrote on the development of physical thought and general culture. Fully to comprehend Newton requires a knowledge of everything that he wrote, and is still difficult today because there is not yet available a complete edition of his works, say comparable to the great editions of the *Opere* of Galileo or the *Oeuvres complètes* of Christiaan Huygens. Yet in 1976, contrasted with 1958 (when the present volume appeared for the first time), the ever-growing tremendous international Newtonian "scholarly industry" has brought to light many documentary sources that illuminate aspects of his complex personality in strikingly new ways. We know much more than ever before about Newton's thoughts concerning sources of "ancient wisdom,"[40] of his actual alchemical concerns, of the philosophical and metaphysical bases of much of his scientific thinking, of the stages in growth of his great mathematical creative talent, and of his actual experiments and observations. One of the true novelties or revaluations has been the discovery that Descartes was so seminal an influence on the development of Newton's philosophy, his principles of physics, and even his mathematics.

Two great bodies of source materials concerning Newton's inner life and development are available in the Royal Society's edition of Newton's *Correspondence* (of which the first volume appeared some time after the first edition of the present work was published), and the great edition of Newton's *Mathematical Writings* edited by D. T. Whiteside, both being published by Cambridge University Press. These two collections of texts and documents, however, differ from the present volume to the degree that they are based upon private or

[40] On this topic see J. E. McGuire and P. M. Rattansi, "Newton and the 'Pipes of Pan'," *Notes and Records of the Royal Society of London 21,* 108–143 (1966) and I. B. Cohen, " 'Quantum in se est': Newton's Concept of Inertia in relation to Descartes and Lucretius," *idem 19,* 131–155 (1964).

manuscript sources, rather than the actual texts which influenced and conditioned the Age of Reason, or Age of the Enlightenment. A great edition, based upon complete manuscript sources, may even provide pitfalls for scholars who may use its texts on those occasions when recourse should have been had to the printed documents actually available in the 17th or the 18th century, as has happened more than once to scholars using the splendid editions of the complete works of Galileo and Huygens.

The differences in style[41] (chiefly spelling, punctuation, paragraphing, capitalization, italicization) between the 17th- and 18th-century versions and the contemporary one are not of very much significance. But whoever quoted portions of Newton's papers and letters in the 17th and 18th centuries (that is, when Newton's scientific writings on optics were still influential or were still providing subjects of debate within the sciences), did so from the versions which are printed in the present volume and not from the various manuscripts which have served as the basis of the scholarly edition of the *Correspondence*.[42] The texts printed here are thus the very ones read in print when the topics to which they are devoted were at the forefront of scientific discussion.

This collection of Newton's papers and letters on natural philosophy provides the exciting experience—alas! no longer possible, owing to the terse and formal style of our scientific journals—of reading how one of the world's greatest scientists said he made one of his major discoveries. We are rapidly transported backward in time through almost three centuries to the time of Newton, as we follow the reactions of the scientists of his day to that discovery and as we read Newton's answers to each objection: sometimes patient and kind, but at other times curt and even rude. We may "listen" to the long paper as it was read to the Royal Society and perhaps understand why Newton did not want to have it published. Above all, we may glimpse some of Newton's innermost thoughts about the

[41] The differences between the versions printed below and those in the *Correspondence* (edited from MS texts) have been listed in the Notes on the Texts prepared for the present edition.

[42] Some of the letters were printed in full by Samuel Horsley in his edition of Newton's *Opera quæ exstant omnia,* 5 vols. (London: John Nichols, 1779–1785).

mechanism of nature, the creation of the universe, and the need for proving by the "phænomena" about us that there was a creating God and that the universe was His handiwork. Like the scientists, philosophers, and ordinary thinking men of the 18th century, we cannot help being moved at the enormity of the fundamental questions to which Newton addressed himself and, like them, we will appreciate the ingenuity of his speculations and the often lofty and poetic rapture that was the result of his profound insight.

II.

Newton's Papers on the Improvement of the Telescope and on Physical Optics

Newton's Optical Papers

THOMAS S. KUHN

The original publication of the optical papers of Isaac Newton marked the beginning of an era in the development of the physical sciences. These papers, reprinted below, were the first public pronouncements by the man who has been to all subsequent generations the archetype of preëminent scientific creativity, and their appearance in early volumes of the *Philosophical Transactions of the Royal Society of London* constituted the first major contribution to science made through a technical journal, the medium that rapidly became the standard mode of communication among scientists.

Until the last third of the seventeenth century most original contributions to the sciences appeared in books, usually in large books: Copernicus' *De Revolutionibus* (1543), Kepler's *Astronomia Nova* (1609), Galileo's *Dialogo* (1632), Descartes's *Dioptrique* (1637), or Boyle's *Experiments and Considerations Touching Colours* (1664). In such books the author's original contributions were usually lost within a systematic exposition of a larger subject matter, so that constructive interchange of scientific experiment and hypothesis was hampered by premature systematization or, as in the case of Boyle, by the mere bulk of the experimental compilation.[1] Each scientist tended to erect his own system upon his own experiments; those experiments that could not support an entire system were frequently lost to the embryonic profession.

The first important breaches of this traditional mode of presentation occurred in the decade of 1660. The chartering of the Royal Society in 1662 and of the Académie Royale des Sciences in 1666, the first publication of the *Journal des Sçavans* and of the *Philosophical Transactions* in 1665, gave institutional expression and sanction to the new conception of science as a coöperative enterprise with utilitarian goals. The immediate objective of the individual scientist became the experimental contribution to an ultimate reconstruction of a system of nature rather than the construction of the system itself, and the journal article—an immediate report on technical experimentation or a preliminary interpretation of experiments—began to replace the book as the unit communiqué of the scientist.

Newton was the first to advance through this new medium an experimentally based proposal for the radical reform of a scientific theory, and his proposal was the first to arouse international discussion and debate within the columns of a scientific journal. Through the discussion, in which all the participants modified their positions, a consensus of scientific opinion was obtained. Within this novel pattern of public announcement, discussion, and ultimate achievement of professional consensus science has advanced ever since.

Newton's optical papers have a further importance to the student of the development of scientific thought. These brief and occasionally hasty communications to the editor of the *Philosophical Transactions* yield an insight into the personality and mental processes of their author that is obscured by the more usual approach

[1] For example, Experiments IV and V in Part III of Boyle's *Colours* are almost identical with the first and last of the three experiments that Newton employed in his first published presentation of the new theory of light and color. In Experiment IV Boyle generates a spectrum and in V he uses a lens to invert the order of the colors. But in Boyle's Baconian compilation these are but two among hundreds of experimental items. There is no evidence that they had the slightest effect on Boyle's contemporaries or successors. See *The Works of the Honourable Robert Boyle,* ed. Thomas Birch (London, 1744), vol. 2, p. 42.

to Newton through his *Principia* (1687) and *Opticks* (1704). In these later monumental creations, from which has emerged our picture of Newton the Olympian father of modern science, the creative role of the author is deliberately hidden by the superfluity of documentation and illustration and by the formality and impersonality of the organization.[2] It is primarily in his early papers, as in his letters, his notes, and his largely unpublished manuscripts, that Newton the creative scientist is to be discovered. And the shock of the discovery may be considerable, for this Newton does not always fit our ideal image.

Newton's first paper, the "New Theory about Light and Colors," is almost autobiographical in its development, and so it facilitates, more than any of Newton's other published scientific works, the search for the sources of the novel optical concepts that he drew from the "celebrated phaenomena of colours."[3] The prismatic colors to which Newton referred had been well known for centuries: white objects viewed through a triangular glass prism are seen with rainbow fringes at their edges; a beam of sunlight refracted by a prism produces all the colors of the rainbow at the screen upon which it falls. Seneca recorded the observations, which must be as old as shattered glass; Witelo, in the 13th century, employed a water-filled globe to generate rainbow colors; by the 17th century prisms, because of their striking colors, were an important item in the negotiations of the Jesuits in China.[4] Before Newton began his experiments at least four natural philosophers, Descartes, Marcus Marci, Boyle, and Grimaldi, had discussed in optical treatises the colored iris produced by a prism, and Hooke had based much of his theory of light upon the colors generated by a single refraction of sunlight at an air-water interface.[5] The "phaenomena" were

[2] The "Queries" that Newton appended to the *Opticks* are the one portion of his later published scientific works in which he allowed the fecundity of his creative imagination to appear. These speculative postscripts to his last technical work do provide a more intimate view of their author. Of course even the *Opticks* proper is a less impersonal work than the *Principia,* but, despite the frequent informality of literary style, the contents and organization are those of a treatise.

[3] The phrase is Newton's. See the beginning of the first optical paper, below.

[4] Joseph Priestley, *The History and Present State of Discoveries Relating to Vision, Light, and Colours* (London, 1722), pp. 7, 21, 169.

[5] Descartes's discussion of the prism occurs in Discours VIII of *Les météores* (1637). For Boyle's experiments see note 1, above. Marci's experiments are de-

indeed "celebrated." Newton, when he repeated them for his own edification, can have had no reason to anticipate a result that he would later describe as "the oddest, if not the most considerable detection, which hath hitherto been made in the operations of nature."[6]

But Newton's version of the experiment differed in an essential respect from that employed by most of his predecessors; furthermore, as we shall see, Newton's optical education and experience were not those of the earlier experimentalists who had employed the prism. Previously, when white light had been passed through a prism, the image of the refracted beam had normally been observed on a screen placed close to the prism.[7] With such an arrangement of the apparatus, the diverging beams of "pure" colors had little opportunity to separate before striking the screen, and the shape of the image cast on the screen was therefore identical with that produced by the unrefracted beam. But in passing through the prism the beam had acquired a red-orange fringe along one edge and a blue-violet fringe along the other.

The colored fringes on an otherwise unaltered beam of white light seemed to bear out an ancient theory of the nature of the rainbow's colors, a theory which held that a succession of modifications of sunlight by the droplets of a rain cloud produced the colors of the bow. In the century and a half preceding Newton's work such a theory was repeatedly and variously reformulated and applied to the colored iris generated by the prism. In all theories the colors were viewed as a minor perturbation restricted primarily to the edges of the homogeneous beam of sunlight. They were due

scribed in his *Thaumantias liber de arcu coelesti* . . . (Prague, 1648) and are discussed by L. Rosenfeld in *Isis 17,* 325–330 (1932). Grimaldi's *Physico-mathesis de lumine* . . . (Bologna, 1665) includes many discussions of prism experiments. Hooke's theory and experiments appear in his *Micrographia* (1665), reprinted by R. T. Gunther as vol. XIII of *Early Science in Oxford* (Oxford, 1938), pp. 47–67. There is no reason to suppose that Newton in 1672 knew of the work of either Marci or Grimaldi, but it is an index of the state of optical experimentation in the 17th century that Grimaldi, Marci, and Boyle had, among them, performed all three of the experiments that Newton employed in his first optical paper.

[6] Letter from Newton to Oldenburg, the secretary of the Royal Society, dated Cambridge, 18 January, 1671/2. Thomas Birch, *The History of the Royal Society of London* (London, 1757), vol. 3, p. 5.

[7] See particularly Descartes's diagrams and discussion, cited in note 5, above.

to a mixture of light and shade at the region of contact between the refracted beam and the dark (Descartes); or they were a consequence of the varying "condensation" and "rarefaction" produced at the edges of the beam by the variation in the angle at which rays from the finite sun were incident upon the prism (Grimaldi); or they were generated by some other mechanical modification (Hooke and the later Cartesians).

There was no consensus as to the nature of the particular modification that tinted white light, but there was agreement that there was only one such modification and that its positive or negative application (for example, condensation *or* rarefaction) to white light could produce only two *primary* colors. These two colors, usually red and blue, represented the extreme applications of the modification, so that their mixture in appropriate proportions would generate any other color by producing the corresponding intermediate degree of modification. More recent experiments have, of course, shown that two primary colors will not suffice, but color-mixing experiments performed with crude equipment are extremely deceptive, a fact that may also account for Newton's initially surprising assertion that spectral yellow and blue combine to produce a green.[8]

All of the modification theories of prismatic colors fail ultimately because of their inability to account quantitatively for the elongation of the spectrum observed when, as in Newton's version of the experiment, the screen is placed a long distance from the prism. But even with the equipment so arranged, it is not immediately apparent that the elongation of the spectrum is incompatible with the modification theories. For since the sun has a finite breadth, rays from different portions of its disk are incident upon the prism at different angles, and even in the absence of dispersion this dif-

[8] In modern terminology, blue and yellow *light* are complementary; that is, they mix to give white. The green produced when blue and yellow *pigments* are mixed is the result of subtractive color mixing, a process different from the mixing of spectral colors. But in fact a long-wavelength spectral blue and a short-wavelength spectral red can be combined to produce a light-green tint. By combining in different proportions a blue near the green region of the spectrum with a red near the yellow it is actually possible to produce a number of shades of blue, green, red, yellow, and intermediate colors. The two-color theories were not so foreign to experience as has been imagined.

ference in angle of incidence will normally produce an elongation of the refracted beam qualitatively similar to that observed by Newton. Those of Newton's predecessors who, like Grimaldi, had noted the elongation of the spectrum had employed this device to account for it, and this was the explanation given by the Jesuit Ignatius Pardies, in his first letter objecting to Newton's theory.[9] To destroy the modification theory it was necessary to notice a *quantitative* discrepancy between the elongation predicted by that theory and the elongation actually observed, and this required an experimenter with a knowledge of the mathematical law governing refraction (not announced until 1637) and with considerable experience in applying the law to optical problems. In 1666 these qualifications were uniquely Newton's. Descartes, who shared Newton's mathematical interests, had performed the experiment with the screen close to the prism, and had noted no elongation. Boyle and Hooke, whose apparatus probably generated an elongated spectrum, shared with Grimaldi a prevalent indifference to the power of mathematics in physics.

It was, then, the large elongation produced in the Newtonian version of the experiment plus the recognition that the size of the spectrum was not that predicted by Snel's new law of refraction that transformed a routine repetition of a common experiment into the "oddest . . . detection, which hath hitherto been made in the operations of nature." The oddity was not the spectrum itself, but the discrepancy between the observed length of the spectrum and the length predicted by existing theory. And this discrepancy, emphasized and investigated with far more mathematical detail in Newton's earlier oral presentations of the experiment, forced Newton to search for a new theory.[10]

[9] Ignace Gaston Pardies, S.J. (1636–1673), was born at Pau in Southern France. At the time of his dispute with Newton he was the professor of rhetoric at the Collège Louis-le-grand in Paris.

[10] Newton first presented his new theory in a series of lectures delivered at Cambridge during 1669. The lectures were not printed until 1728, after his death, when they appeared in an English translation from the Latin manuscript. A Latin edition, containing lectures for the years 1669, 1670, and 1671, appeared in 1729. Certain of the features emphasized in the present discussion emerge with even greater clarity from the lectures than from the first optical paper. The two may profitably be read together.

Newton found the clue to the new theory in the geometrical idealization that he reported as the shape of the spectrum rather than in the elongation that had caused the search. His beam of sunlight was a cylinder ¼ inch in diameter, formed by allowing sunlight to enter his chamber through a circular hole in his "window shuts." After refraction the beam fell upon the opposite wall of the room, distant 22 feet from the prism, where, according to Newton, it produced an elongated spectrum, 13¼ inches in length, bounded by parallel sides 2⅝ inches apart, and capped by semicircular ends. The shape suggested its own interpretation. For the semicircular "caps" could be viewed as the residua of the shape imposed by the circular hole in the shutter, and the spectrum could then be analyzed into an infinite series of differently colored overlapping circles whose centers lay on a straight line perpendicular to the axis of the prism. In his early lectures, as in the later *Opticks,* Newton frequently sketched the spectrum in this way, one end formed by a pure blue circular image of the original hole, the other formed by a pure red image, and the intermediate region composed of a number of variously colored circles displaced along the axis of the spectrum. By this device the existing laws of refraction, which for Newton's arrangement of the prism predicted a circular image, could be preserved. But the law now had to be applied, not to the incident beam as a whole, but to every one of the colored beams contained in the original beam. Sunlight was a mixture of all the colors of the rainbow; each of the incident colored beams obeyed the laws of optics; but each was refracted through a different angle in its passage through the prism. This was the essence of Newton's new theory, derived primarily from the reported shape of the spectrum.[11]

[11] The preceding reconstruction of Newton's research follows the essentially autobiographical narrative provided by Newton himself in the first of the optical papers. It may require important modification as a result of a recent study of Newton's manuscripts by A. R. Hall, "Sir Isaac Newton's Note-Book, 1661-1665," *Cambridge Historical Journal 9,* 239-250 (1948). On this topic, see the references to further studies in the Supplement.

Hall believes that Newton discovered the variation of refractive index with color by observing a two-colored thread through a prism, and he suggests that the experiment in which a beam of sunlight is passed through a prism was not performed until a later date. For a variety of reasons I find this portion of Hall's reconstruction implausible. The textual and historical evidence available, though

The reported shape leaves a puzzle illustrative of the nature of Newton's genius. Though the spectrum described cries aloud for the interpretation that Newton provided, it is very doubtful that he saw any such shape. Only the central 2-inch strip of his 2⅝-inch-wide spectrum was illuminated uniformly by light from the disk of the sun. The balance of the width of the spectrum consisted of a penumbral region in which the various colors gradually shaded off into the black. Since the eye can distinguish red much farther into the penumbral region than it can distinguish blue, Newton probably saw a figure appreciably narrower and more pointed at the blue end than at the red.[12] This is the shape that Newton's bitterest and least intelligent critic, Franciscus Linus, described, and this is the only one of Linus's criticisms to which Newton never responded.[13] Newton combined a precise and detailed

not decisive, persuades me that Newton had already passed a beam of sunlight through a prism when he performed the experiments that Hall has discovered in the "Note-Book."

If so, Newton's account of the development of the new theory remains autobiographical in the sense that the prism experiment did provide the initial impetus as well as an important clue for the new theory, as discussed above. But, as Hall does conclusively show, the implication of Newton's account is wrong in that Newton did not proceed so directly or so immediately from the first prism experiment to the final version of the theory as the first paper would imply. When he made the entries in his college notebook, Newton had not arrived at the final form of the new theory. So far as I can tell from the fragments reproduced by Hall, Newton then believed that different colors were refracted through different angles, but he still held that the individual colors were generated within the prism by modifications of the initially homogeneous white light. This intermediate stage of Newton's thought provides a fascinating field for further study.

[12] It is impossible to be precise about the actual shape of the spectrum viewed by Newton. The sensitivity of the human eye to short-wavelength blue varies from one individual to another, and the relative intensity of the blue in the spectrum is also a function of atmospheric conditions.

[13] Linus's description occurs midway through the first paragraph of his second letter of criticism. Although the position of Linus's prism was different from that of Newton's, the "sharp cone or pyramis" described by Linus is due to the same penumbral effects that must have caused the sides of Newton's spectrum to deviate from parallelism.

Franciscus Linus (Francis Hall or Line), S.J., was born in London in 1595. During his controversy with Newton he was a teacher of mathematics and Hebrew at the English college of Liège. He spent much of his later life attempting to reconcile the results of 17th-century experimentation with Aristotelian physics. Linus was the author of the "funiculus" hypothesis by which he claimed to explain the results of Boyle's barometer experiments without recourse to the vacuum description of his experimental apparatus with an imaginative idealization of his experimental results.

Newton's leap from the full and unintelligible complexity of the observable phenomenon to the geometrical idealization underlying it is symptomatic of the intellectual extrapolations that mark his contributions to science. And he was apparently aware of and concerned with the extrapolation, though he made it explicit in none of his communications to the Royal Society. In the optical lectures, which he delivered in Cambridge prior to the composition of his first published paper, Newton included a description of two experiments that he had designed to investigate the shape of the spectrum produced without a penumbral region. In one of these he used a lens, placed one focal length in front of the screen, to refocus the colored circular sun images of which the spectrum was composed. In a second he utilized the planet Venus, effectively a point source, instead of the sun in order to generate his spectrum. He had justified his extrapolation to himself, but, except for implicit references to the problem in his correspondence with Moray and in the *Opticks,* he did not tell his readers how to follow him.

Newton's announcement in 1672 of the discoveries made six to eight years earlier induced a great controversy within the columns of the *Philosophical Transactions.*[14] The prismatic colors that he discussed were well known, at least qualitatively, and there was

or atmospheric pressure, and experiments designed to refute him led to the discovery of Boyle's Law. Linus died in 1675, midway through the dispute with Newton, but his cause was taken up by two of his students, Gascoigne and Lucas.

Anthony Lucas (1633–1693), another British Jesuit, appears to have been a meticulous experimenter. His inability to obtain the large dispersion reported by Newton must have been due to his use of a different sort of glass. Lucas's experimental "proofs" of the inadequacy of Newton's theory are a fascinating index of the difficulties in designing unequivocal dispersion experiments. In most experiments the effects are so small that they can be fitted to any theory, so incisive documentation of a particular theory requires careful selection from the multiplicity of available phenomena. At first glance Newton's failure to answer any of Lucas's experimental criticism seems strange, particularly since Newton did respond at such length to the one remark by Lucas that did not reflect at all upon the validity of Newton's conclusions. But see the discussion, below, of Newton's attitude toward controversy.

[14] A. R. Hall, "Sir Isaac Newton's Note-Book," has pointed out that Newton probably intended to write "1665" rather than "1666" for the date of the prism experiment which opens his first paper. He also argues that Newton's work with the prism may have begun as early as 1664.

widespread conviction among 17th-century opticians that they could be adequately treated by existing optical theories. No wonder there was resentment of a newcomer who claimed that precise analysis of a well-known effect necessitated discarding established theories. Opponents could easily find grounds for rejecting the proposals. They could, for example, deny the existence of the experimental effect. The sun is an unreliable and a moving source of light; the prism generates a number of emergent beams, only one of which satisfies Newton's description; quantitative results vary with the sort of glass employed in the prism. Alternatively, they could accept Newton's experimental results, but deny the necessity or even the validity of his interpretation.

The nature and psychological sources of the controversy were typical, but the reaction was less severe than that usually produced by so radical a proposal. Newton's predecessors had all employed some form of modification theory, but, having reached no consensus on the nature of the modification, they lacked a stable base for a counterattack. And Newton's experimental documentation of his theory is a classic in its simplicity and its incisiveness. The modification theorists might finally have explained the elongation of the spectrum, but how could they have evaded the implications of the *experimentum crucis?* An innovator in the sciences has never stood on surer ground.

As a result the controversy that followed the original announcement is of particular interest today for the light it sheds upon Newton's character.[15] In particular the controversial literature illuminates the genesis of Newton's relation with the Royal Society's curator, Robert Hooke, with whom he later engaged in a priority battle over the inverse-square law of gravitation.[16] Hooke's claim to the authorship of the inverse-square law almost caused Newton to omit the Third Book of the *Principia,* and it was apparently

[15] There are, however, many points of technical interest in the debate. These are discussed more fully in L. Rosenfeld, "La théorie des couleurs de Newton et ses adversaires," *Isis 9,* 44–65 (1927). A stimulating elementary account of some of the same material has been provided by M. Roberts and E. R. Thomas, *Newton and the Origin of Colours* (London: G. Bell & Sons, 1934).

[16] For bibliography and a definitive account of the gravitation controversy, see A. Koyré, "An Unpublished Letter of Robert Hooke to Isaac Newton," *Isis 43,* 312–337 (1952).

Hooke's continuing opposition to Newton's optical theories that caused Newton to delay publication of the *Opticks* until long after his own active research in the field had ended. Hooke died in 1703, and the *Opticks,* much of which had existed in manuscript for years, first appeared in the following year.

Newton's first paper was read to the assembled members of the Royal Society on February 8, 1671/2. On February 15 Hooke delivered, at the request of the Society's members, a report on and evaluation of Newton's work. Coming from a senior member of the profession, a man already established as the most original optical experimentalist of the day, the report was most judicious, though it contained important errors and displayed Hooke's typically Baconian indifference to quantitative mathematical formulations. Hooke praised and confirmed Newton's experimental results, and he conceded that the theory which Newton had derived from them was entirely adequate to explain the effects. His only major criticism (excepting the remarks on telescopes, for which see below) is that Newton's interpretation was not a *necessary* consequence of the experiments. Hooke felt that Newton had performed too few experiments to justify the theory, that another theory (his own) could equally well explain Newton's experiments, and that other experiments (particularly his own on the colors of thin films) could not be explained by Newton's theory.

Hooke's Baconian criticism is an index of the prevalent methodological emphasis upon experimentation, an emphasis that made the "experimental history" a typical scientific product of the day. Most members of the Royal Society would have concurred. But Hooke was quite wrong in thinking that his own version of the modification theory could explain Newton's results; at least he never gave a satisfactory explanation of the production of colors.[17] On the other hand, Hooke was right that Newton's theory could

[17] The difficulty in adapting a pressure-wave theory of light like Hooke's to the various color phenomena explored by Newton is well illustrated by the experience of Huygens, who brought these theories to their most perfect 17th-century form in his *Traité de la lumière* (1690). Huygens wrote Leibniz that he had "said nothing respecting colours in my *Traité de la lumière,* finding this subject very difficult, and particularly from the great number of different ways in which colours are produced." Sir David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton* (Edinburgh, 1855), vol. 1, p. 95 n.

not explain some of the experiments upon which Hooke had based his own theory. In particular, Newton's theory, as of 1672, would not explain either diffraction or the colors of thin sheets of mica, both of which Hooke had described in his *Micrographia* (1665). Nor would Newton's theory explain the colors produced by confining air between sheets of glass, an observation that Hooke reported to the Society on April 4 and June 19 in his further examination of Newton's doctrine.[18] The latter communication, incidentally, included a clear description of the phenomenon usually known as "Newton's rings," and it seems probable that Newton borrowed it from Hooke and employed it to develop a revised theory adequate to handle Hooke's experiments. For Newton, in his long letters of December and January 1675/6, did succeed in solving Hooke's problems to his own satisfaction and to that of most of his contemporaries. But to do so he had to modify his original theory by the introduction of an explicit æthereal medium which could transmit impulses as pressure waves, and this was an immense step toward Hooke's theory. Hooke, of course, did not accept even this later modification. He always felt that Newton's use of *both* corpuscles and æther impulses violated Occam's injunction against the needless multiplication of conceptual entities.[19]

In the final analysis Hooke was wrong. As Newton clearly showed in his belated reply, Hooke's pulse theory of light was incapable of accounting for linear propagation; nor could Hooke's modification theory of color account either for the *experimentum crucis* or for any of the novel color-mixing experiments that Newton apparently designed specifically to meet Hooke's objections. This much of the reply was effective, and Newton might better have begun and ended with the elaboration of these arguments, for Hooke had challenged neither Newton's experiments nor the adequacy of his theory to resolve the experiments. But this is not what Newton did. In his lengthy and gratuitously caustic response, whose incongruity with Hooke's critique has escaped attention since the two have not before been printed together,[20] Newton at-

[18] Birch, *History of the Royal Society*, vol. 3, pp. 41 & 54.

[19] *Ibid.*, p. 295.

[20] Oldenburg, the secretary of the Royal Society and editor of the *Philosophical Transactions*, is known to have hated Hooke. This may well explain his failure to

tacked Hooke on three apparently incompatible grounds: Hooke had attributed to Newton a corpuscular theory that Newton had not developed; Hooke's impulse theory was not basically incompatible with the corpuscular theory (which Newton had disowned); and Hooke's impulse theory was incapable of accounting for the phenomena. Newton might have employed any of these three lines of attack alone—though only the third seems both relevant and accurate—but it is difficult to see how anything but consuming passion could have led him to employ them concurrently.

Newton was a man of passions. It is difficult to read many of his responses to criticism without concurring in a recent judgment of Newton's personality by the late Lord Keynes. After a lengthy examination of Newton's manuscripts Keynes wrote:

> For in vulgar modern terms Newton was profoundly neurotic of a not unfamiliar type, but—I should say from the records—a most extreme example. His deepest instincts were occult, esoteric, semantic—with profound shrinking from the world, a paralyzing fear of exposing his thoughts, his beliefs, his discoveries in all nakedness to the inspection and criticism of the world. "Of the most fearful, cautious and suspicious temper that I ever knew," said Whiston, his successor in the Lucasian Chair. The too well-known conflicts and ignoble quarrels with Hooke, Flamsteed, Leibnitz are only too clear an evidence of this. Like all his type he was wholly aloof from women. He parted with and published nothing except under the extreme pressure of friends.[21]

Newton's fear of exposure and the correlated compulsion to be invariably and entirely immune to criticism show throughout the controversial writings. They are apparent in both the tone and the substance of his reply to Hooke, where they are also combined with the beginning of that tendency to deny the apparent implications of earlier writings (rather than either defending them or ad-

print Hooke's critique with Newton's reply. The omission must have seemed a gratuitous insult to Hooke, particularly in view of the tone and substance of Newton's comments.

[21]J. M. Keynes, "Newton the Man," in the Royal Society's *Newton Tercentenary Celebrations* (Cambridge, 1947), p. 28. These documents can be put to other uses, however. Examine, for an opinion of the Hooke-Newton exchange directly opposed to the one given above, the analysis provided by Brewster, *Memoirs,* vol. 1, pp. 86–92. But Brewster cannot avoid providing repeated illustrations of Newton's efforts to escape from controversy (for example, pp. 95–99).

mitting to a change of mind) which has so consistently misled subsequent students of his work. Did Hooke really misinterpret the intent of Newton's remarks on the difficulties of constructing refracting telescopes? Is Newton honest in rejecting the corpuscular hypothesis that Hooke ascribes to him? Or, to take a later and far clearer example, is not Newton convicted of an irrationally motivated lie in his reply to Huygens's remarks about the composition of the color white? In his first paper Newton had said, in discussing colors:

> But the most . . . wonderful composition is that of *Whiteness* . . . 'Tis ever compounded, and to its composition are requisite all the aforesaid primary Colours, mixed in a due proportion . . . Hence therefore it comes to pass, that *Whiteness* is the usual colour of *Light;* for Light is a confused aggregate of Rays indued with all sorts of Colours . . . if any one predominate, the Light must incline to that colour.

Yet when Huygens suggested that the combination of yellow and blue might generate white, Newton admitted the possibility but claimed that he had never meant anything else. The apparent contradiction he reconciled by saying that Huygens's white would be different from his own by virtue of its composition. Newton's position was correct in the reply, but surely he had changed his mind in reaching it.

The same defensiveness had more serious consequences in Newton's writings on telescopes. Here Newton's influence appears to have been predominantly negative. His own work on telescopes was of little practical importance, and his remarks on design were frequently wrong. Although he built the first working reflector, he was never able to perfect the model sufficiently to enable it to compete with existing refractors, and so his position was not very different from that of the contemporary and independent designers, James Gregory and Guillaume Cassegrain.[22] The reflecting telescope remained a curious toy on the shelves of the Royal Society

[22] James Gregory (1638–1675), a Scottish mathematician, described a reflecting telescope in his *Optica Promota* (1633), and Newton had studied Gregory's design when he started his own. Sieur Guillaume Cassegrain was a modeler and founder of statues in the employ of Louis XIV. His design was surely independent of Newton's and may have been independent of Gregory's. Both Gregory and Cassegrain tried to build reflectors but were unable to polish adequate mirrors.

until, in 1722, James Hadley succeeded in grinding a parabolic mirror. But as soon as the reflector could compete with the refractor, Newton's design was discarded in favor of the designs by Gregory and Cassegrain that Newton had so vehemently criticized for essentially irrelevant reasons.[23]

Far more important in the development of telescopes were Newton's mistakes in the evaluation of optical aberrations. Having been led to the reflecting telescope by the discovery of the chromatic aberration caused by the variation of refractive index with color, Newton always insisted that chromatic rather than spherical aberration imposed the major limitation upon the power of refracting telescopes. Newton's theoretical comparisons of the two were both mathematically and optically correct, but, as Huygens pointed out in his comment, Newton's interpretation of the calculations was incompatible with the observed performance of spherical lenses. Newton explained the discrepancy correctly as due to the small effect on the eye of the widely dispersed red and blue rays, but he failed to notice that in practice this made chromatic aberrations little or no more important than spherical. So Newton continued to insist upon the practical superiority of reflectors.[24]

[23] On Newton's contributions to the development of telescopes see Louis Bell, *The Telescope* (New York, 1922).

[24] The study of Newton's most important and damaging error in his writings on the telescope is complicated rather than clarified by the papers reprinted below. In his *Opticks* (Book I, Part II, Experiment 8) Newton "proved" that it was impossible to build an achromatic lens, that is, a lens compounded from two or more materials so differing in dispersive power that they will refract a ray of white light without separating the colors in it. Newton claimed to have found by experiment that when a beam of light was passed through a succession of prisms of glass and water a spectrum was invariably generated unless the emergent and incident beams were parallel. He concluded that any combination of materials which could correct dispersion would also nullify refraction, so that no achromatic lens was possible. The error may well have hindered the development of achromatic lenses.

To get the experimental result Newton must either have shut his eyes, used sugar to raise the refractive index of his water, or employed a variety of glass with unusually low dispersive power. All three of these explanations have been advanced by subsequent historians, most of whom have also expressed surprise at Newton's readiness to draw so general a conclusion from such slight experimental evidence. For a full account of the development of achromatic lenses see N. V. E. Nordenmark and J. Nordstrom, "Om uppfinningen av den akromatiska och aplanatiska linsen," *Lychnos 4,* 1–52 (1938); *5,* 313–384 (1939). The second

Subsequent history bore out the judgment expressed by Huygens in his last letter of the optics controversy that until it became possible to grind nonspherical mirrors the future of practical telescopic observations would be associated with refractors of long focal length and consequently low aberrations.[25]

But among the aspects of Newton's thought that are illuminated by recognition of his dread of controversy, the most important is his attitude toward "hypotheses." Like most of his contemporaries, Newton was guided throughout his scientific career by the conception of the universe as a gigantic machine whose components are microscopic corpuscles moving and interacting in accordance with immutable laws.[26] Most of Newton's work in physics can be viewed appropriately as a part of a consistent campaign to discover the mathematical laws governing the aggregation and motion of the corpuscles of a mechanical "clock-work universe," and many of his specifically optical, chemical, or dynamical writings are difficult to comprehend without reference to the corpuscular metaphysic which played an active role in their creation.[27] Yet from most of

portion of the article includes some appendices and an abstract in English.

It is apparent from the optical papers below that Newton's theorem concerning the relation of dispersion and refractive index was the best possible refutation for three of his early critics. It nullified the objections of Hooke and Huygens, who had urged that more attention be given to the perfection of refracting telescopes, and it made it certain that Lucas had erred in reporting the small dispersion of his prism. For this reason most historians have argued that the theorem developed in the *Opticks* was in Newton's mind, at least implicitly, from the beginning of his optical researches and that this is why he failed to consider more seriously the merits of his opponents' positions. But—and this is where the new complication enters—I can find no way of interpreting the text of Newton's response to Hooke without supposing that Newton is there proposing an achromatic lens made by compounding a water lens with two convexo-concave lenses of glass.

On this topic, see the works by D. T. Whiteside and Zev Bechler, referred to in the Supplement.

[25] The letters to and from Huygens reprinted below are only a part of a larger correspondence, most of which was not published until recently. L. T. More discusses the complete correspondence more fully in his biography, *Isaac Newton* (New York, 1934). The letters themselves will be found in volume VII of the *Oeuvres complètes de Christiaan Huygens* (The Hague, 1888–1944).

[26] M. Boas, "The Establishment of the Mechanical Philosophy," *Osiris 10,* 412–541 (1952).

[27] For the role of the metaphysic in Newton's chemistry see the next section of this book. For its role in Newton's dynamics, see A. Koyré, "The Significance of the Newtonian Synthesis," *Archives internationales d'histoires des sciences 29,* 291–311 (1950), and T. S. Kuhn, *The Copernican Revolution* (Cambridge, Mass., 1957), chap. 7.

his published writings Newton tried, never completely successfully, to eliminate just these hypothetical and therefore controversial elements.

In the notebook in which he recorded the progress of his early optical research Newton continually referred to light rays as composed of "globules," traveling with finite velocities and interacting in accordance with the known laws of impact.[28] But in his first published paper Newton omitted all explicit reference to particular corpuscular mechanisms which determine the behavior of light. He substituted geometrical entities ("rays") for physical entities (corpuscles moving in definite paths); and he contented himself with a retrospective argument showing that the experimentally determined properties of the rays must make light a substance rather than a quality. In his controversy with Hooke, who seems to have known more about the hypotheses than Newton had allowed to enter in his published discussion, he reneged on even this argument, and thus continued a retreat that had begun in his first paper and developed further in his letters to Pardies.

That this is a genuine retreat from the defense of metaphysical hypotheses which Newton believed and employed creatively is amply, if incompletely, attested by the inconsistencies in his discussions and use of hypotheses throughout the optical papers printed below. In the first paper light was a substance. In the letters to Pardies light was either a substance or a quality, but the definition of light rays in terms of "indefinitely small . . . inde-

[28] For example: "Though 2 rays be equally swift yet if one ray be lesse y^n y^e other that ray shall have so much lesse effect on y^e sensorium as it has lesse motion y^n y^e others &c.

"Whence supposing y^t there are loose particles in y^e pores of a body bearing proportion to y^e greater rays, as 9:12 & y^e less globulus is in proportion to y^e greater as 2:9, y^e greater globulus by impinging on such a particle will loose $\frac{6}{7}$ parts of its motion y^e less glob. will loose $\frac{2}{7}$ parts of its motion & y^e remaining motion of y^e glob. will have almost such a proportion to one another as their quantity have viz. $\frac{5}{7}:\frac{1}{7}::9:1\frac{4}{5}$ w^{ch} is almost 2 y^e lesse glob. & such a body may produce blews and purples. But if y^e particles on w^{ch} y^e globuli reflect are equal to y^e lesse globulus it shall loose its motion & y^e greater glob. shall loose $\frac{2}{11}$ parts of its motion and such a body may be red or yellow." Hall, "Sir Isaac Newton's Note-Book," p. 248.

pendent" parts made light again corporeal. In the same letter Newton proclaimed that his observations and *theories* could be reconciled with the pressure *hypotheses* of either Hooke or Descartes, but in the letter to Hooke he forcefully demonstrated the inadequacy of all pressure hypotheses to explain the phenomena of light and colors. Newton denied his adherence to the corpuscular hypothesis, and he stated that his credence was restricted to laws that could be proved by experiment, but he returned to the pattern of his notebook by employing implicitly the hypothetical scatterings of corpuscles at points of focus to prove the disadvantages of the Gregorian telescope.[29] In 1672 he denied the utility of hypotheses when presenting a theory which he believed could be made independent of them, but in dealing with the colors of thin films in the important letters of 1675/6 he employed explicit hypotheses, presumably because the new subject matter of these letters could not otherwise be elaborated. Significantly, it was just these later letters, from which large segments of Books II and III of the *Opticks* were transcribed, that Newton refused to publish until after Hooke's death. Of all his optical writings, these letters best reflect the procedures of Newton at work.[30]

Much of modern science inherits from Newton the admirable pragmatic aim, never completely realized, of eliminating from the final reports of scientific discovery all reference to the more speculative hypotheses that played a role in the process of discovery. The desirability of this Newtonian mode of presenting theories is well illustrated by the subsequent history of Newton's own hypoth-

[29] Brewster, *Memoirs,* p. 50 n.

[30] These critically important letters, reprinted below, deserve far more study and discussion than they here receive. But such discussion necessarily assumes the proportion of a critical analysis of the second and third books of the *Opticks* for which these letters provided a draft, and the space for such an analysis is not here available. For a discussion of the central ideas in these later letters, as they emerge in the *Opticks,* see I. B. Cohen's introduction to the recent reissue of the *Opticks* (New York, 1952).

Space limitations also prevent my discussing Newton's posthumously published design of "An instrument for observing the Moon's Distance from the fixed Stars at Sea." When written this paper contained important novelties of design, but before it was published these new features had been independently incorporated in practical navigational instruments by several designers. On these instruments see Lloyd Brown, *The Story of Maps* (Boston, 1949), pp. 191 ff.

eses. The next great step in optics, the development of an adequate wave theory, was retarded by the grip of Newton's corpuscular hypotheses upon the scientific mind. But Newton's remarks about the role of hypotheses in science were dictated by personal idiosyncrasy as often as by philosophical acumen; repeatedly he renounces hypotheses simply to avoid debate. And so he has seemed to support the further assertion that scientific research can and should be confined to the experimental pursuit of mathematical regularity—that hypotheses which transcend the immediate evidence of experiment have no place in science. Careful examination of Newton's less systematic published writings provides no evidence that Newton imposed upon himself so drastic a restriction upon scientific imagination.

The achievements initiated by Newton's own imagination are unsurpassed, and it is primarily the magnitude of his achievements that directs attention to the man. If the resulting study displays error and idiosyncrasy in Newton's complex and difficult personality, it cannot lessen his unparalleled accomplishments. It can alter only our image of the requisites for preëminent scientific achievement. But this alteration is a goal worth pursuing: a true image of the successful scientist is a first condition for understanding science.

(3075) *Numb.* 80.

PHILOSOPHICAL TRANSACTIONS.

February 19. $16\frac{71}{72}$.

The CONTENTS.

A Letter of Mr. Isaac Newton, *Professor of the Mathematicks in the University of Cambridge; containing his New Theory about* Light *and* Colors: *sent by the Author to the Publisher from Cambridge, Febr.* 6. $16\frac{71}{72}$; *in order to be communicated to the* R. Society.

SIR,

TO perform my late promise to you, I shall without further ceremony acquaint you, that in the beginning of the Year 1666 (at which time I applyed my self to the grinding of Optick glasses of other figures than *Spherical*,) I procured me a Triangular glass-Prisme, to try therewith the celebrated *Phænomena* of

Colours. And in order thereto having darkened my chamber, and made a ſmall hole in my window-ſhuts, to let in a convenient quantity of the Suns light, I placed my Priſme 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 colours produced thereby; but after a while applying my ſelf to conſider them more circumſpectly, I became ſurpriſed to ſee them in an *oblong* form; which, according to the received laws of Refraction, I expected ſhould have been *circular*.

They were terminated at the ſides with ſtreight lines, but at the ends, the decay of light was ſo gradual, that it was difficult to determine juſtly, what was their figure; yet they ſeemed *ſemicircular*.

Comparing the length of this coloured *Spectrum* with its breadth, I found it about five times greater; a diſproportion ſo extravagant, that it excited me to a more then ordinary curioſity of examining, from whence it might proceed. I could ſcarce think, that the various *Thickneſs* of the glaſs, or the termination with ſhadow or darkneſs, could have any Influence on light to produce ſuch an effect; yet I thought it not amiſs, firſt to examine thoſe circumſtances, and ſo tryed, what would happen by tranſmitting light through parts of the glaſs of divers thickneſſes, or through holes in the window of divers bigneſſes, or by ſetting the Priſme without ſo, that the light might paſs through it, and be refracted before it was terminated by the hole: But I found none of thoſe circumſtances material. The faſhion of the colours was in all theſe caſes the ſame.

Then I ſuſpected, whether by any *unevenneſs* in the glaſs, or other contingent irregularity, theſe colours might be thus dilated. And to try this, I took another Priſme like the former, and ſo placed it, that the light, paſſing through them both, might be refracted contrary ways, and ſo by the latter returned into that courſe, from which the former had diverted it. For, by this means I thought, the *regular* effects of the firſt Priſme would be deſtroyed by the ſecond Priſme, but the *irregular* ones more augmented, by the multiplicity of refractions. The event was, that the light, which by the firſt Priſme was diffuſed into an *oblong* form, was by the ſecond reduced into an *orbicular* one with as much regularity, as when it did not at all paſs through them. So that, what ever was the cauſe of that length, 'twas not any contingent irregularity.

I

I then proceeded to examin more critically, what might be effected by the difference of the incidence of Rays coming from divers parts of the Sun; and to that end, meaſured the ſeveral lines and angles, belonging to the Image. Its diſtance from the hole or Priſme was 22 foot; its utmoſt length $13\frac{1}{4}$ inches; its breadth $2\frac{5}{8}$; the diameter of the hole $\frac{1}{4}$ of an inch; the angle, with the Rays, tending towards the middle of the image, made with thoſe lines, in which they would have proceeded without refraction, was 44 deg. 56′. And the vertical Angle of the Priſme, 63 deg. 12′. Alſo the Refractions on both ſides the Priſme, that is, of the Incident, and Emergent Rays, were as near, as I could make them, equal, and conſequently about 54 deg. 4′. And the Rays fell perpendicularly upon the wall. Now ſubducting the diameter of the hole from the length and breadth of the Image, there remains 13 Inches the length, and $2\frac{3}{8}$ the breadth, comprehended by thoſe Rays, which paſſed through the center of the ſaid hole, and conſequently the angle of the hole, which that breadth ſubtended, was about 31′, anſwerable to the Suns Diameter; but the angle, which its length ſubtended, was more then five ſuch diameters, namely 2 deg. 49′.

Having made theſe obſervations, I firſt computed from them the refractive power of that glaſs, and found it meaſured by the *ratio* of the ſines, 20 to 31. And then, by that *ratio*, I computed the Refractions of two Rays flowing from oppoſite parts of the Sun's *diſcus*, ſo as to differ 31′ in their obliquity of Incidence, and found, that the emergent Rays ſhould have comprehended an angle of about 31′, as they did, before they were incident.

But becauſe this computation was founded on the Hypotheſis of the proportionality of the *ſines* of Incidence, and Refraction, which though by my own Experience I could not imagine to be ſo erroneous, as to make that Angle but 31′, which in reality was 2 deg. 49′; yet my curioſity cauſed me again to take my Priſme. And having placed it at my window, as before, I obſerved, that by turning it a little about its *axis* to and fro, ſo as to vary its obliquity to the light, more then an angle of 4 or 5 degrees, the Colours were not thereby ſenſibly tranſlated from their place on the wall, and conſequently by that variation of Incidence, the quantity of Refraction was not ſenſibly varied. By this Experiment therefore, as well as by the former computation, it was evident, that the difference of the Incidence of Rays, flowing from divers parts of the Sun, could not make them after decuſſation diverge at a ſenſibly greater angle, than that at which they before converged; which being, at moſt, but about 31 or 32 minutes, there ſtill remained ſome other cauſe to be found out, from whence it could be 2 degr. 49'.

Then I began to ſuſpect, whether the Rays, after their trajection through the Priſme, 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 ſuſpition, when I remembred 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 ſtroak, 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 Æther, on that ſide, where the motions conſpire, and thence be continually bowed to the other. But notwithſtanding this plauſible ground of ſuſpition, when I came to examine it, I could obſerve no ſuch curvity in them. And beſides (which was enough for my purpoſe) I obſerved, that the difference 'twixt the length of the Image, and diameter of the hole, through which the light was tranſmitted, was proportionable to their diſtance.

The gradual removal of theſe ſuſpitions, at length led me to the *Experimentum Crucis*, which was this: I took two boards, and placed one of them cloſe behind the Priſme at the window, ſo that the light might paſs through a ſmall hole, made in it for the purpoſe, and fall on the other board, which I placed at about 12 feet diſtance, having firſt made a ſmall hole in it alſo, for ſome of that Incident light to paſs through. Then I placed another Priſme behind this ſecond board, ſo that the light, trajected through both the boards, might paſs through that alſo, and be again refracted before it arrived at the wall. This done, I took the firſt Priſme in my hand, and turned it to and fro ſlowly about its *Axis*, ſo much as to make the ſeveral parts of the Image, caſt on the ſecond board, ſucceſſively paſs through the hole in it, that I might obſerve to what places on the wall the ſecond Priſme would refract them.

And

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And I ſaw by the variation of thoſe places, that the light, tending to that end of the Image, towards which the refraction of the firſt Priſme was made, did in the ſecond Priſme ſuffer a Refraction conſiderably greater then the light tending to the other end. And ſo the true cauſe of the length of that Image was detected to be no other, then that *Light* conſiſts of *Rays differently refrangible*, which, without any reſpect to a difference in their incidence, were, according to their degrees of refrangibility, tranſmitted towards divers parts of the wall.

When I underſtood this, I left off my aforeſaid Glaſs works; for I ſaw, that the perfection of Teleſcopes was hitherto limited, not ſo much for want of glaſſes truly figured according to the preſcriptions of Optick Authors, (which all men have hitherto imagined,) as becauſe that Light it ſelf is a *Heterogeneous mixture of differently refrangible Rays*. So that, were a glaſs ſo exactly figured, as to collect any one ſort of rays into one point, it could not collect thoſe alſo into the ſame point, which having the ſame Incidence upon the ſame Medium are apt to ſuffer a different refraction. Nay, I wondered, that ſeeing the difference of refrangibility was ſo great, as I found it, Teleſcopes ſhould arrive to that perfection they are now at. For, meaſuring the refractions in one of my Priſmes, I found, that ſuppoſing the common *ſine* of Incidence upon one of its planes was 44 parts, the *ſine* of refraction of the utmoſt Rays on the red end of the Colours, made out of the glaſs into the Air, would be 68 parts, and the *ſine* of refraction of the utmoſt rays on the other end, 69 parts: So that the difference is about a 24*th* or 25*th* part of the whole refraction. And conſequently, the object-glaſs of any Teleſcope cannot collect all the rays, which come from one point of an object ſo as to make them convene at its *focus* in leſs room then in a circular ſpace, whoſe diameter is the 50*th* part of the Diameter of its Aperture; which is an irregularity, ſome hundreds of times greater, then a circularly figured *Lens*, of ſo ſmall a ſection as the Object glaſſes of long Teleſcopes are, would cauſe by the unfitneſs of its figure, were Light *uniform*.

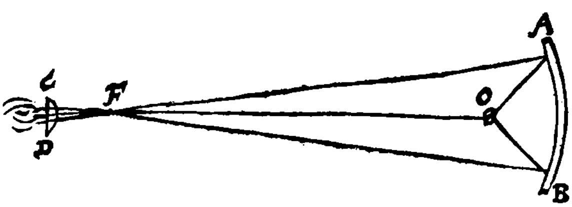
This made me take *Reflections* into conſideration, and finding them regular, ſo that the Angle of Reflection of all ſorts of Rays was equal to their Angle of Incidence; I underſtood, that by their mediation Optick inſtruments might be brought to any degree of perfection imaginable, provided a *Reflecting* ſubſtance could be found,

found, which would poliſh as finely as Glaſs, and *reflect* as much light, as glaſs *tranſmits*, and the art of communicating to it a *Parabolick* figure be alſo attained. But there ſeemed very great difficulties, and I have almoſt thought them inſuperable, when I further conſidered, that every irregularity in a reflecting ſuperficies makes the rays ſtray 5 or 6 times more out of their due courſe, than the like irregularities in a refracting one: So that a much greater curioſity would be here requiſite, than in figuring glaſſes for Refraction.

Amidſt theſe thoughts I was forced from *Cambridge* by the Intervening Plague, and it was more then two years, before I proceeded further. But then having thought on a tender way of poliſhing, proper for metall, whereby, as I imagined, the figure alſo would be corrected to the laſt; I began to try, what might be effected in this kind, and by degrees ſo far perfected an Inſtrument (in the eſſential parts of it like that I ſent to *London*,) by which I could diſcern Jupiters 4 Concomitants, and ſhewed them divers times to two others of my acquaintance. I could alſo diſcern the Moon-like phaſe of *Venus*, but not very diſtinctly, nor without ſome niceneſs in diſpoſing the Inſtrument.

From that time I was interrupted till this laſt Autumn, when I made the other. And as that was ſenſibly better then the firſt (eſpecially for Day-Objects,) ſo I doubt not, but they will be ſtill brought to a much greater perfection by their endeavours, who, as you inform me, are taking care about it at *London*.

I have ſometimes thought to make a *Microſcope*, which in like manner ſhould have, inſtead of an Object-glaſs, a Reflecting piece of metall. And this I hope they will alſo take into conſideration. For thoſe Inſtruments ſeem as capable of improvement as *Teleſcopes*, and perhaps more, becauſe but one reflective piece of metall is requiſite in them, as you may perceive by the annexed diagram, where A B repreſenteth the object metall, C D the eye glaſs, F their common Focus, and O the other focus of the metall, in which the object is placed.



But

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But to return from this digreſſion, I told you, that Light is not ſimilar, or homogeneal, but conſiſts of *difform* Rays, ſome of which are more refrangible than others: So that of thoſe, which are alike incident on the ſame medium, ſome ſhall be more refracted than others, and that not by any virtue of the glaſs, or other external cauſe, but from a prediſpoſition, which every particular Ray hath to ſuffer a particular degree of Refraction.

I ſhall now proceed to acquaint you with another more notable difformity in its Rays, wherein the *Origin of Colours* is unfolded: Concerning which I ſhall lay down the *Doctrine* firſt, and then, for its examination, give you an inſtance or two of the *Experiments*, as a ſpecimen of the reſt.

The Doctrine you will find comprehended and illuſtrated in the following propoſitions.

1. As the Rays of light differ in degrees of Refrangibility, ſo they alſo differ in their diſpoſition to exhibit this or that particular colour. Colours are not *Qualifications of Light*, derived from Refractions, or Reflections of natural Bodies (as 'tis generally believed,) but *Original* and *connate properties*, which in divers Rays are divers. Some Rays are diſpoſed to exhibit a red colour and no other; ſome a yellow and no other, ſome a green and no other, and ſo of the reſt. Nor are there only Rays proper and particular to the more eminent colours, but even to all their intermediate gradations.

2. To the ſame degree of Refrangibility ever belongs the ſame colour, and to the ſame colour ever belongs the ſame degree of Refrangibility. The *leaſt Refrangible* Rays are all diſpoſed to exhibit a *Red* colour, and contrarily thoſe Rays, which are diſpoſed to exhibit a *Red* colour, are all the leaſt refrangible: So the *moſt refrangible* Rays are all diſpoſed to exhibit a deep *Violet Colour*, and contrarily thoſe which are apt to exhibit ſuch a violet colour, are all the moſt Refrangible. And ſo to all the intermediate colours in a continued ſeries belong intermediate degrees of refrangibility. And this Analogy 'twixt colours, and refrangibility, is very preciſe and ſtrict; the Rays always either exactly agreeing in both, or proportionally diſagreeing in both.

3. The ſpecies of colour, 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

parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it. I have refracted it with Prismes, and reflected it with Bodies, which in Day-light were of other colours; I have intercepted it with the coloured film of Air interceding two compressed plates of glass; transmitted it through coloured Mediums, and through Mediums irradiated with other sorts of Rays, and diversly terminated it; and yet could never produce any new colour out of it. It would by contracting or dilating become more brisk, or faint, and by the loss of many Rays, in some cases very obscure and dark; but I could never see it changed *in specie*.

Yet seeming transmutations of Colours may be made, where there is any mixture of divers sorts of Rays. For in such mixtures, the component colours appear not, but, by their mutual allaying each other, constitute a midling colour. And therefore, if by refraction, or any other of the aforesaid causes, the difform Rays, latent in such a mixture, be separated, there shall emerge colours different from the colour of the composition. Which colours are not New generated, but only made Apparent by being parted; for if they be again intirely mix't and blended together, they will again compose that colour, which they did before separation. And for the same reason, Transmutations made by the convening of divers colours are not real; for when the difform Rays are again severed, they will exhibit the very same colours, which they did before they entered the composition; as you see, *Blew* and *Yellow* powders, when finely mixed, appear to the naked eye *Green*, and yet the Colours of the Component corpuscles are not thereby really transmuted, but only blended. For, when viewed with a good Microscope, they still appear *Blew* and *Yellow* interspersedly.

5. There are therefore two sorts of Colours. The one original and simple, the other compounded of these. The Original or primary colours are, *Red, Yellow, Green, Blew*, and a *Violet-purple*, together with Orange, Indico, and an indefinite variety of Intermediate gradations.

6. The same colours in *Specie* with these Primary ones may be also produced by composition: For, a mixture of *Yellow* and *Blew* makes *Green*; of *Red* and *Yellow* makes *Orange*; of *Orange* and *Yellowish green* makes *yellow*. And in general, if any two Colours be mixed, which in the series of those, generated by the Prisme, are not

not too far diſtant one from another, they by their mutual alloy compound that colour, which in the ſaid ſeries appeareth in the mid-way between them. But thoſe, which are ſituated at too great a diſtance, do not ſo. *Orange* and *Indico* produce not the intermediate Green, nor Scarlet and Green the intermediate yellow.

7. But the moſt ſurpriſing, and wonderful compoſition was that of *Whiteneſs*. There is no one ſort of Rays which alone can exhibit this. 'Tis ever compounded, and to its compoſition are requiſite all the aforeſaid primary Colours, mixed in a due proportion. I have often with Admiration beheld, that all the Colours of the Priſme being made to converge, and thereby to be again mixed as they were in the light before it was Incident upon the Priſme, reproduced light, intirely and perfectly white, and not at all ſenſibly differing from a *direct* Light of the Sun, unleſs when the glaſſes, I uſed, were not ſufficiently clear; for then they would a little incline it to *their* colour.

8. Hence therefore it comes to paſs, that *Whiteneſs* is the uſual colour of *Light*; for, Light is a confuſed aggregate of Rays indued with all ſorts of Colors, as they are promiſcuouſly darted from the various parts of luminous bodies. And of ſuch a confuſed aggregate, as I ſaid, is generated Whiteneſs, if there be a due proportion of the Ingredients; but if any one predominate, the Light muſt incline to that colour; as it happens in the Blew flame of Brimſtone; the yellow flame of a Candle; and the various colours of the Fixed ſtars.

9. Theſe things conſidered, the *manner*, how colours are produced by the Priſme, is evident. For, of the Rays, conſtituting the incident light, ſince thoſe which differ in Colour proportionally differ in Refrangibility, *they* by their unequall refractions muſt be ſevered and diſperſed into an oblong form in an orderly ſucceſſion from the leaſt refracted Scarlet to the moſt refracted Violet. And for the ſame reaſon it is, that objects, when looked upon through a Priſme, appear coloured. For, the difform Rays, by their unequal Refractions, are made to diverge towards ſeveral parts of the *Retina*, and there expreſs the Images of things coloured, as in the former caſe they did the Suns Image upon a wall. And by this inequality of refractions they become not only coloured, but alſo very confuſed and indiſtinct

10. Why the Colours of the *Rainbow* appear in falling drops

of Rain, is allſo from hence evident. For thoſe drops, which refract the Rays, diſpoſed to appear purple, in greateſt quantity to the Spectators eye, refract the Rays of other ſorts ſo much leſs, as to make them paſs beſide it; and ſuch are the drops on the inſide of the *Primary* Bow, and on the outſide of the *Secondary* or Exteriour one. So thoſe drops, which refract in greateſt plenty the Rays, apt to appear red, toward the Spectators eye, refract thoſe of other ſorts ſo much more, as to make them paſs beſide it; and ſuch are the drops on the exteriour part of the *Primary*, and interiour part of the *Secondary* Bow.

11. The odd Phænomena of an infuſion of *Lignum Nephriticum*, *Leaf gold*, *Fragments of coloured glaſs*, and ſome other tranſparently coloured bodies, appearing in one poſition of one colour, and of another in another, are on theſe grounds no longer riddles. For, thoſe are ſubſtances apt to reflect one ſort of light and tranſmit another; as may be ſeen in a dark room, by illuminating them with ſimilar or uncompounded light. For, then they appear of that colour only, with which they are illuminated, but yet in one poſition more vivid and luminous than in another, accordingly as they are diſpoſed more or leſs to reflect or tranſmit the incident colour.

12. From hence alſo is manifeſt the reaſon of an unexpected Experiment, which Mr. *Hook* ſomewhere in his *Micrography* relates to have made with two wedg-like tranſparent veſſels, fill'd the one with a red, the other with a blew liquor: namely, that though they were ſeverally tranſparent enough, yet both together became opake; For, if one tranſmitted only red, and the other only blew, no rays could paſs through both.

13. I might add more inſtances of this nature, but I ſhall conclude with this general one, that the Colours of all natural Bodies have no other origin than this, that they are variouſly qualified to reflect one ſort of light in greater plenty then another. And this I have experimented in a dark Room by illuminating thoſe bodies with uncompounded light of divers colours. For by that means any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light caſt upon them, but yet with this difference, that they are moſt brisk and vivid in the light of their own day-light-colour. *Minium* appeareth there of any colour indifferently, with which 'tis illuſtrated, but yet moſt luminous in red, and ſo

B ſ

Bise appeareth indifferently of any colour with which 'tis illuſtrated, but yet moſt luminous in blew. And therefore *Minium* reflecteth Rays of any colour, but moſt copiouſly thoſe indued with red; and conſequently when illuſtrated with day-light, that is, with all ſorts of Rays promiſcuouſly blended, thoſe qualified with red ſhall abound moſt in the reflected light, and by their prevalence cauſe it to appear of that colour. And for the ſame reaſon *Biſe*, reflecting blew moſt copiouſly, ſhall appear blew by the exceſs of thoſe Rays in its reflected light; and the like of other bodies. And that this is the intire and adequate cauſe of their colours, is manifeſt, becauſe they have no power to change or alter the colours of any ſort of Rays incident apart, but put on all colours indifferently, with which they are inlightned.

Theſe things being ſo, it can be no longer diſputed, whether there be colours in the dark, nor whether they be the qualities of the objects we ſee, no nor perhaps, whether Light be a Body. For, ſince Colours are the *qualities* of Light, having its Rays for their intire and immediate ſubject, how can we think thoſe Rays *qualities* alſo, unleſs one quality may be the ſubject of and ſuſtain another; which in effect is to call it *Subſtance*. We ſhould not know Bodies for ſubſtances, were it not for their ſenſible qualities, and the Principal of thoſe being now found due to ſomething elſe, we have as good reaſon to believe that to be a Subſtance alſo.

Beſides, whoever thought any quality to be a *heterogeneous* aggregate, ſuch as Light is diſcovered to be. But, to determine more abſolutely, what Light is, after what manner refracted, and by what modes or actions it produceth in our minds the Phantaſms of Colours, is not ſo eaſie. And I ſhall not mingle conjectures with certainties.

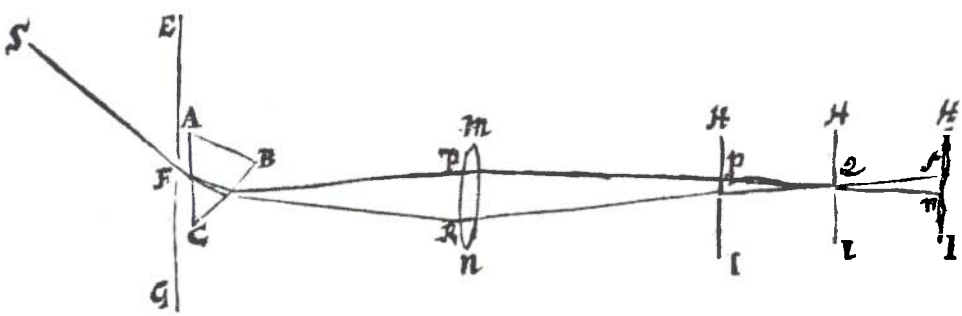
Reviewing what I have written, I ſee the diſcourſe it ſelf will lead to divers Experiments ſufficient for its examination: And therefore I ſhall not trouble you further, than to deſcribe one of thoſe, which I have already inſinuated.

In a darkened Room make a hole in the ſhut of a window, whoſe diameter may conveniently be about a third part of an inch, to admit a convenient quantity of the Suns light: And there place a clear and colourleſs Priſme, to refract the entring light towards the further part of the Room, which, as I ſaid, will thereby be diffuſed into an oblong coloured Image. Then place a *Lens* of

Hhhh 2 about

about three foot radius (ſuppoſe a broad Object-glaſs of a three foot Teleſcope,) at the diſtance of about four or five foot from thence, through which all thoſe colours may at once be tranſmitted, and made by its Refraction to convene at a further diſtance of about ten or twelve feet. If at that diſtance you intercept this light with a ſheet of white paper, you will ſee the colours converted into whiteneſs again by being mingled. But it is requiſite, that the *Priſme* and *Lens* be placed ſteddy, and that the paper, on which the colours are caſt, be moved to and fro; for, by ſuch motion, you will not only find, at what diſtance the whiteneſs is moſt perfect, but alſo ſee, how the colours gradually convene, and vaniſh into whiteneſs, and afterwards having croſſed one another in that place where they compound Whiteneſs, are again diſſipated, and ſevered, and in an inverted order retain the ſame colours, which they had before they entered the compoſition. You may alſo ſee, that, if any of the Colours at the *Lens* be intercepted, the Whiteneſs will be changed into the other colours. And therefore, that the compoſition of whiteneſs be perfect, care muſt be taken, that none of the colours fall beſides the *Lens*.

In the annexed deſign of this Experiment, A B C expreſſeth the Priſm ſet endwiſe to ſight, cloſe by the hole F of the window



E G. Its vertical Angle A C B may conveniently be about 60 degrees: *M N* deſigneth the *Lens*. Its breadth $2\frac{1}{2}$ or 3 inches. S F one of the ſtreight lines, in which difform Rays may be conceived to flow ſucceſſively from the Sun. F P, and F R two of thoſe Rays unequally refracted, which the *Lens* makes to converge towards Q, and after decuſſation to diverge again. And H I the paper, at divers diſtances, on which the colours are projected: which in Q conſtitute *Whiteneſs*, but are *Red* and *Yellow* in R, r, and ρ, and *Blew* aud *Purple* in P, p, and π.

If

(3087)

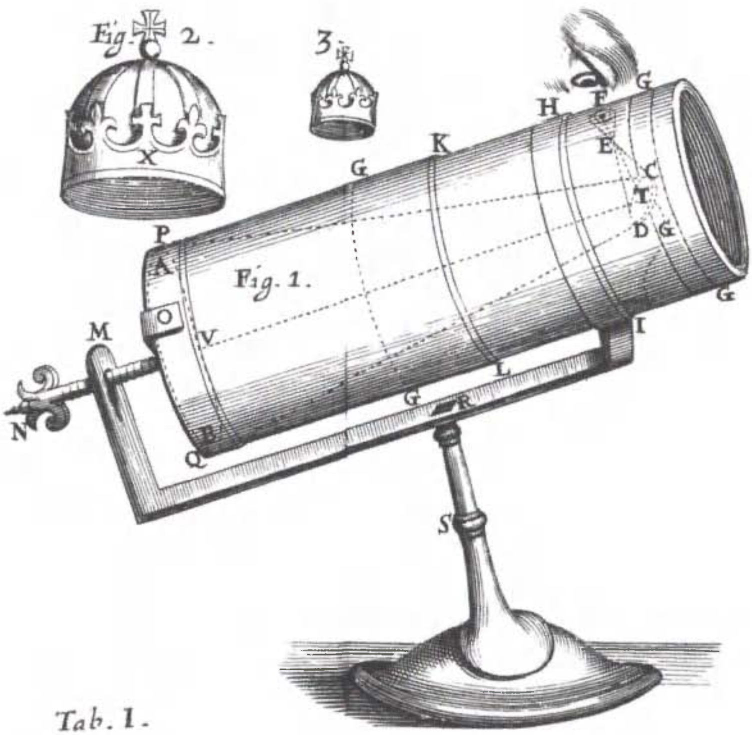
If you proceed further to try the impoſſibility of changing any uncompounded colour (which I have aſſerted in the third and thirteenth Propoſitions,) 'tis requiſite that the Room be made very dark, leaſt any ſcattering light, mixing with the colour, diſturb and allay it, and render it compound, contrary to the deſign of the Experiment. 'Tis alſo requiſite, that there be a perfecter ſeparation of the Colours, than, after the manner above deſcribed, can be made by the Refraction of one ſingle Priſme, and how to make ſuch further ſeparations, will ſcarce be difficult to them, that conſider the diſcovered laws of Refractions. But if tryal ſhall be made with colours not throughly ſeparated, there muſt be allowed changes proportionable to the mixture. Thus if compound Yellow light fall upon Blew *Biſe*, the Biſe will not appear perfectly yellow, but rather green, becauſe there are in the yellow mixture many rays indued with green, and Green being leſs remote from the uſual blew colour of Biſe than yellow, is the more copiouſly reflected by it.

In like manner, if any one of the Priſmatick colours, ſuppoſe Red, be intercepted, on deſign to try the aſſerted impoſſibility of reproducing that Colour out of the others which are pretermitted; 'tis neceſſary, either that the colours be very well parted before the red be intercepted, or that together with the red the neighbouring colours, into which any red is ſecretly diſperſed, (that is, the yellow, and perhaps green too) be intercepted, or elſe, that allowance be made for the emerging of ſo much red out of the yellow green, as may poſſibly have been diffuſed, and ſcatteringly blended in thoſe colours. And if theſe things be obſerved, the new Production of Red, or any intercepted colour will be found impoſſible.

This, I conceive, is enough for an Introduction to Experiments of this kind; which if any of the *R. Society* ſhall be ſo curious as to proſecute, I ſhould be very glad to be informed with what ſucceſs: That, if any thing ſeem to be defective, or to thwart this relation, I may have an opportunity of giving further direction about it, or of acknowledging my errors, if I have committed any.

So far this Learned and very Ingenious Letter; which having been by that *Illuſtrious Company*, before whom it was read, with much applauſe committed to the conſideration of ſome of their Fellows, well verſed in this argument, the Reader may poſſibly in an other *Tract* be informed of ſome report given in upon this Diſcourſe.

An



Tab. 1.

(4004)

An Accompt of a New Catadioptrical Telescope invented by Mr. Newton, *Fellow of the R.*Society, *and Professor of the Mathematiques in the University of Cambridge.*

THis Excellent Mathematician having given us, in the Transactions of *February* last, an account of the cause, which induced him to think upon *Reflecting* Telescopes, instead of *Refracting* ones, hath thereupon presented the Curious World with an *Essay* of what may be performed by such Telescopes; by which it is found, that Telescopical Tubes may be considerably shortned without prejudice to their magnifying effect.

This new instrument is composed of two Metallin *speculum's*, the one Concave, (instead of an Object-glass) the other Plain; and also of a small plano-convex Eye-Glass.

By *Figure* I. of *Tab.* I. the structure of it may be easily imagined; viz. *That* the Tube of this Telescope is open at the end which respects the object; *that* the other end is close, where the said Concave is laid, and *that* near the open end there is a flat oval *speculum*, made as small as may be, the less to obstruct the entrance of the rays of Light, and inclined towards the upper part of the Tube, where is a little hole furnish't with the said Eye-glass. So that the rays coming from the object, do first fall on the Concave placed at the bottome of the Tube; and are thence reflected toward the other end of it, where they meet with the flat speculum, obliquity posited, by the reflection of which they are directed to the little plano-convex Glass, and so to the spectators Eye, who looking downwards sees the Object, which the Telescope is turned to.

To understand this more distinctly and fully, the Reader may please to look upon the said *Figure*, in which

A B is the Concave *speculum*, of which the *radius* or semidiameter is $12\frac{2}{3}$ or 13 inches.

C D another metalline *speculum*, whose surface is flat, and the circumference oval.

G D

(4005)

GD an Iron wire, holding a ring of braſs, in which the ſpeculum *CD* is fixed.

F, a ſmall Eye-glaſs flat above, and convex below, of the twelfth part of an inch *radius*, if not leſs; foraſmuch as the metal collects the Sun's rays at $6\frac{1}{3}$ inches diſtance, and the Eye-glaſs at leſs than $\frac{1}{6}$ of an inch diſtance from its vertex: Beſides that the Author (as he informs us) knew their dimenſions by the tools to which they were ground, and particularly meaſuring the diameter of the hemi-ſpherical Concave, in which the Eye-glaſs was wrought, found it the *ſixth* part of an inch.

GGG, the fore part of the Tube faſtn'd to a braſs-ring H I, to keep it immoveable.

PQKL, the hind-part of the Tube, faſtn'd to another braſs-ring P Q.

O, an Iron hook faſtn'd to the Ring *PQ*, and furniſh't with a ſcrew N, thereby to advance or draw back the hind-part of the Tube, and ſo by that means to put the *ſpecula* in their due diſtance.

MQGI a crooked Iron ſuſtaining the Tube, and faſtned by the nail R to the Ball and Socket S, whereby the Tube may be turned every way.

The Center of the flat *ſpeculum* C D, muſt be placed in the ſame point of the Tube's Axe, where falls the perpendicular to this Axe, drawn to the ſame from the center of the little Eye-glaſs: which point is here marked at T.

And to give the Reader ſome ſatisfaction to underſtand, in what degree it repreſents things diſtinct, and free from colours, and to know the aperture by which it admits light; he may compare the diſtances of the *focus* E from the *vertex's* of the little Eye-glaſs and the Concave *ſpeculum*, that is, EF, $\frac{1}{6}$ of an inch, and E T V, $6\frac{1}{3}$ inches; and the *ratio* will be found as 1 to 38; whereby it appears, that the Objects will be magnified about 38 times. To which proportion is very conſentaneous, an Obſervation of the Crown on the weather-cock, about 300 feet diſtant. For the ſcheme X fig. 2. repreſents it bigger by $2\frac{1}{2}$ times in diameter, when ſeen through

through this, than through an ordinary Telescope of about 2 foot long. And so supposing this ordinary one to magnifie 13 or 14 times, as by the description it should, this new one by the Experiment must magnifie near as much as hath been assigned.

Thus far as to the structure of this Telescope. Concerning the Metalline matter, fit for these reflecting *Speculums*, the Inventor hath also considered the same, as may be seen by two of his Letters, written to the Publisher from Cambridge Jan. 18. and 29. $16\frac{71}{72}$. to this effect, *viz.*

1. That for a fit metalline substance, he would give this Caution, that whilest men seek for a white, hard and durable metallin composition, they resolve not upon such an one, as is full of small pores, only discoverable by a Microscope. For though such an one may to appearance take a good polish, yet the edges of those small pores will wear away faster in the polishing than the other parts of the metal; and so, however the Metal seem polite, yet it shall not reflect with such an accurate regularity as it ought to do. Thus Tin-glass mixt with ordinary Bell-metall makes it more white and apt to reflect a greater quantity of light; but withall its fumes, raised in the fusion, like so many aerial bubles, fill the metall full of those Microscopical pores. But white Arsenick both blanches the Metall and leaves it solid without any such pores, especially if the fusion hath not been too violent. What the *Stellate Regulus* of *Mars* (which I have sometimes used) or other such like substance will do, deserves particular examination.

To this he adds this further intimation, that *Putty* or other such like powder, with which 'tis polished, by the sharp angles of its particles fretteth the metall, if it be not very fine, and fills it full of such small holes, as he speaketh of. Wherefore care must be taken of that, before judgment be given, whether the metall be throughout the body of it porous or not.

2. He not having tried, as he saith, many proportions of the Arsenick and Metall, does not affirm, which is absolutely best, but thinks, there may conveniently be used

any

any quantity of Arsenick equalling in weight between a sixt and eight part of the Copper, a greater proportion making the Metal brittle.

The way, which he used, was this. He first melted the Copper alone, then put in the Arsenick, which being melted, he stirred them a little together, bewaring in the mean time, not to draw in breath near the pernicious fumes. After this, he put in Tin, and again so soon as that was melted (which was very suddenly) he stirred them well together, and immediately powred them off.

He saith, he knows not, whether by letting them stand longer on the fire after the Tin was melted, a higher degree of fusion would have made the metall porous; but he thought that way he proceeded to be safest.

He adds, that in that metall, which he sent to *London*, there was no Arsenick, but a small proportion of Silver; as he remembers, one shilling in three ounces of metall. But he thought withall, that the Silver did as much harm in making the metall soft, and so less fit to be polish't, as good in rendring it white and luminous.

At another time he mixed Arsenick one ounce, Copper six ounces, and Tin two ounces: And this an Acquaintance of his hath, as he intimates, polish't better, than he did the other.

As to the *objection*, that with this kind of Perspectives, objects are difficultly found, he answers in another letter of his to the Publisher, of Jan. 6. $16\frac{71}{72}$. that that is the inconvenience of all Tubes that magnifie much; and that after a little use the inconvenience will grow less, seeing that himself could readily enough find any day-Objects, by knowing which way they were posited from other objects that he accidentally saw in it; but in the night to find Stars, he acknowledges it to be more troublesome; which yet may, in his opinion, be easily remedied by two sights affixed to the Iron rod, by which the Tube is susteined; or by an ordinary perspective glass fastn'd to the same frame with the Tube, and directed towards the same object, as *Des-Cartes* in his Dioptricks hath described for remedying the same inconvenience of his best Telescopes.

So far the Inventors Letters touching this Instrument: of which having communicated the description to Monsieur *Christian Hugens de Zulichem*, we received from him an Answer to this effect, in his Letter of Febr. 13. 1672. st.n.

I see by the Description, you have sent me of Mr. *Newtons* admirable Telescope, that he hath well considered the advantage, which a *Concave speculum* hath above *Convex glasses* in collecting the parallel rays, which certainly according to the calculation, I have made thereof, is very great. Hence it is, that he can give a far greater aperture to that *speculum*, than to an Object-glass of the same distance of the *focus*, and consequently that he can much more magnifie objects this way, than by an ordinary Telescope. Besides, by it he avoids an inconvenience, which is inseparable from convex Object-Glasses, which is the Obliquity of both their surfaces, which vitiateth the refraction of the rays that pass towards the sides of the glass, and does more hurt than men are aware of. Again, by the meer reflection of the metallin *speculum* there are not so many rays lost, as in Glasses, which reflect a considerable quantity by each of their surfaces, and besides intercept many of them by the obscurity of their matter.

Mean time, the main business will be, to find a matter for this *speculum* that will bear so good and even a polish as Glasses, and a way of giving this polish without vitiating the spherical figure. Hitherto I have found no *Specula*, that had near so good a polish as Glass; and if M. *Newton* hath not already found a way to make it better, than ordinarily I apprehend, his Telescopes will not so well distinguish objects, as those with Glasses. But 'tis worth while to search for a remedy to this inconvenience, and I despair not of finding one. I believe, that M. *Newton* hath not been without considering the advantage, which a *Parabolical speculum* would have above a *Spherical* one in this construction; but that he despairs, as well as I do, of working other surfaces than spherical ones with due exactness; though else it be more easie to make a *Parabolical* than *Elliptical* or *Hyperbolical* ones, by reason of a certain propriety of the *Parabolick Conoid*, which

is,

is, that all the Sections parallel to the Axis make the same Parabola.

Thus far M. *Hugenius* his judicious Letter; to the latter part of which, concerning the grinding *Parabolical Conoids*, Mr. *Newton* saith, in his Letter to the Publisher of Feb. 20. 71. that though he with him despairs of performing that work by Geometrical rules, yet he doubts not but that the thing may in some measure be accomplished by Mechanical devises.

To all which I cannot but subjoyn an Extract of a Letter, received very lately, (March 19th) from the Inventor *of this new Telescope, from* Cambridge, *viz.*

IN my last Letter I gave you occasion to suspect, that the Instrument which I sent you, is in some respect or other indisposed, or that the metals are tarnished. And by your Letter of *March* 16. I am fully confirmed in that opinion. For, whilest I had it, it represented the Moon in some parts of it as distinctly, as other Telescopes usually do which magnifie as much as that. Yet I very well know, that that Instrument hath its imperfections both in the composition of the metall, and in its being badly cast, as you may perceive by a scabrous place near the middle of the metall of it on the polished side, and also in the figure of that metall near that scabrous place. And in all those respects that instrument is capable of further improvement.

You seem to intimate, that the proportion of 38 to 1 holds only for its magnifying Objects at small distances. But if for such distances, suppose 500 feet, it magnifie at that rate, by the rules of Opticks it must for the greatest distance imaginable magnifie more than $37\frac{3}{4}$ to 1; which is so considerable a diminishing, that it may be even then as 38 to 1.

Here is made another Instrument like the former, which does very well. Yesterday I compared it with a six foot Telescope, and found it not only to magnifie more, but also more distinctly. And to day I found, that I could read in one of the *Philosophical Transactions*, placed in the Sun's

 light,

(4010)

light, at an hundred foot diſtance, and that at an hundred and twenty foot diſtance I could diſcern ſome of the words. When I made this tryal, its Aperture (defined next the Eye) was equivalent to more than an inch and a third part of the Object-metall. This may be of ſome uſe to thoſe that ſhall endeavour any thing in *Reflexions*; for hereby they will in ſome meaſure be enabled to judge of the goodneſs of their Inſtruments, &c.

N. B. The Reader may expect in the *next Month* another Letter, which came ſomewhat too late to be here inſerted; containing a *Table*, calculated by the ſame Mr. *Newton*, about the ſeveral *Apertures* and *Charges* anſwering the ſeveral *Lengths* of theſe Teleſcopes.

(4032)

*Mr.*Newton's *Letter to the Publisher of March* 26.1672. *containing some more suggestions about his New Telescope, and a* Table *of* Apertures *and* Charges *for the several* Lengths *of that Instrument.*

SIR,

Since my last Letter I have further compared the two Telescopes, and find that of Metal to represent as well the Moon,as neerer Objects, something distincter than the other. But I must tell you also,that I am not very well assured of the goodness of that other, which I borrowed to make the Comparison;and therefore desire,that the other Experiment should be rather confided in, of reading at the distance of between a 100 and 120 foot, at which I and others could read with it in the *Transactions*, as I found by measure : At which time the aperture was $1\frac{1}{3}$ of an Inch ; which I knew by trying, that an obstacle of that breadth was requisite to intercept all the light, which came from one point of the object.

I should tell you also, that the little plain piece of metall, next the eye-glass, is not truly figured : whereby it happens, that objects are not so distinct at the middle as at the edges. And I hope, that by correcting its figure, (in which I find more difficulty than one would expect,) they will appear all over distinct, and distincter in the middle than at the edges. And I doubt not but that the performances will then be greater.

But yet I find,that there is more light lost by reflection of the metall which I have hitherto used, than by transmission through glasses : for which reason a shallower charge would probably do better for obscure objects ; suppose such an one, as would make it magnifie 34 or 32 times. But for bright objects at any distance, it seems capable of magnifying 38 or 40 times with sufficient distinctness. And for all objects, the same Charge, I believe, may with advantage be allowed, if the steely matter, imployed at *London*, be more strongly reflective than this which I have used.

The performances of one of these Instruments of any length being known, it will appear by this following *Table*,what may be be expected from thoſe of other Lengths by this way, if Art can accompliſh what is promiſed by the Theory. In the *firſt* Column is expreſſed the Length of the Teleſcope in feet; which doubled gives the ſemidiameter of the Sphere,on which the concave metall is to be ground. In the *ſecond* column are the proportions of the Apertures for thoſe ſeveral Lengths. And in the *third* column are the Proportions of the *Charges*, or diameter of the ſpheres, on which the convex ſuperficies of the eye-glaſſes are to be ground.

Lengths.	*Apertures.*	*Charges.*
$\frac{1}{2}$	100	100
1	168	119
2	283	141
3	383	157
4	476	168
5	562	178
6	645	186
8	800	200
10	946	211
12	1084	221
16	1345	238
20	1591	254
24	1824	263

The uſe of this Table will beſt appear by example: Suppoſe therefore a half foot Teleſcope may diſtinctly magnifie 30 times with an inch Aperture,and it being required to know, what ought to be the analogons conſtitution and performance of a four foot Teleſcope: By the ſecond column, as 100 to 476; ſo are the Apertures, as alſo the number of times which they magnifie. And conſequently ſince the half foot Tube hath an inch aperture and magnifieth 30 times; a four foot Tube proportionally ſhould have $4\frac{76}{100}$ inches aperture, and magnifie 143 times. And by the third column, as 100 to 168; ſo are their Charges: And therefore if the diameter of the convexity of the eye-glaſs for a half foot Teleſcope be $\frac{1}{5}$ of an inch, that for a four foot ſhould be $\frac{168}{500}$, that is, about $\frac{1}{3}$ of an inch.

In like manner, if a half foot Telescope may distinctly magnifie 36 times with $1\frac{1}{4}$ of an Inch Aperture; a four foot Telescope should with equal distinctness magnifie 171 times with 6 inches Aperture; and one of six foot should magnifie 232 times with $8\frac{1}{3}$ inches Aperture; and so of other lengths. But what the event will really be, we must wait to see determined by experience. Only this I thought fit to insinuate, that they which intend to make trials in other lengths, may more readily know how to design their Instruments. Thus for a four foot Tube, since the Aperture should be 5 or 6 inches, there will be required a piece of metal 7 or 8 inches broad at least, because the figure will scarcely be true to the edges. And the thickness of the metal must be proportional to the breadth, least it bend in the grinding. The metalls being polished, there may be tryals made with several eye-glasses, to find, what Charge may with best advantage be made use of.

An Extract of another Letter of the same to the Publisher, dated March 30. 1672. *by way of Answer to some Objections, made by an Ingenious French Philosopher to the New Reflecting Telescope.*

SIR,

I Doubt not but *M. A.* will allow the advantage of reflexion in the Theory to be very great, when he shall have informed himself of the different *Refrangibility* of the several rays of light. And for the practique part, it is in some measure manifest by the Instruments already made, to what degree of vivacity and brightness a metaline substance may be polished. Nor is it improbable but that there may be new ways of polishing found out for metal, which will far excell those that are yet in use. And when a metal is once well polished, it will be a long while preserved from tarnishing, if diligence be used to keep it dry and close, shut up from Air: For the principal cause of tarnishing seems to be, the condensing of moisture on its polished surface, which by an Acid spirit, where-

wherewith the Atmofphere is impregnated, corrodes and rufts it; or at leaft, at its exhaling, leaves it covered over with a thin skin, confifting partly of an earthly fediment of that moifture, and partly of the duft, which flying to and fro in the Air had fetled and adhered to it.

When there is not occafion to make frequent ufe of the inftrument, there may be other waies to preferve the metal for a long time; as perhaps by immerging it in Spirit of wine or fome other convenient liquor. And if they chance to tarnifh; yet their polifh may be recovered by rubbing them with a foft piece of leather, or other tender fubftance, without the affiftance of any fretting powders, unlefs they happen to be rufty: for then they muft be new polifhed.

I am very fenfible, that metal reflects lefs light than glafs tranfmits; and for that inconvenience, I gave you a remedy in my laft Letter, by affigning a fhallower charge in proportion to the Aperture, than is ufed in other Telefcopes. But, as I have found fome metaline fubftances to be more ftrongly reflective, and to polifh better, and be freer from tarnifhing than others; fo I hope there may in time be found out fome fubftance much freer from thefe inconveniences, than any yet known.

Mr. Isaac Newton's *Considerations upon part of a Letter of Monsieur de* Bercé *printed in the Eight French* Memoire, *concerning the Cata drioptrical Telescope, pretended to be improv'd and refined by M.* Cassegrain.

That the Reader may be enabled the better to Judge of the whole, by comparing together the contrivances both of Mr. Newton *and* Mr. Cassegrain; *it will be necessary, to borrow from the said French* Memoire *what is there said concerning them: which is as followes.*

I Send you (saith M. de *Bercè* to the Publisher of the *Memoire*,) the Copy of the Letter, which M. *Cassagrain* hath written to me concerning the proportions of Sr. *Samuel Morelands* Trumpet. And as for the Telescope of Mr. *Newton* it hath as much surprised me, as the same Person, that hath found out the proportions of the Trumpet. For 'tis now about three months, that that person communicated to me the figure of a Telescope, which was almost like it, and which he had invented; but which I look upon as more witty. I shall here give you the description of it in short.

ABCD. is a strong Tube, in the bottom of which there is a great concave *Speculum* CD, pierced in the midle E.

F. is a convex *Speculum*, so disposed, as to its convexity, that it reflects the *Species*, which it receives from the great *Speculum*, towards the hole E, where is an Eye-glass, which one looketh through.

The advantage, which I find in this Instrument above that of Mr. *Newton*, is first, that the mouth or aperture AB of the Tube may be of what bigness you please; and consequently you may have many more rays upon the Concave *Speculum*, than upon that, of which you have given us the description. 2. The reflexion of the rays will be very natural, since it will be made upon the *axis* it self, and therefore more vivid. 3. The vision of it will be so much the more pleasing, in that you shall not be incommoded by the great light, by reason of the bottom CD, which hideth the whole face. Besides that

you'l

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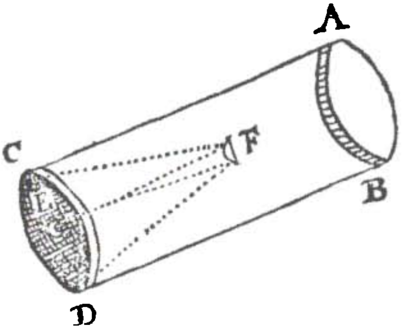
you'l have lefs difficulty in difcovering the Objects, than in that of Mr. *Newtons*.



So far this French Author. To which we shall now subjoin the Considerations of Mr. Newton, *as we received them from him in a Letter, written from* Cambridge *May* 4th 1672, *as follows.*

S I R

I Should be very glad to meet with any improvement of the Catadioptrical Telefcope; but that defign of it, which (as you informe me) Mr. *Caffegrain* hath communicated 3 months fince, and is now printed in one of the French *Memoires*, I fear will not anfwer Expectation. For, when I firft applied my-felf to try the effects of Reflexions, Mr. *Gregory's Optica Promota* (printed in the year 1663) being fallen into my hands, where there is an Inftrument (defcribed pag. 94) like that of Monfieur *Caffegrain's* with a hole in the midft of the Object-Metal to tranfmit the Light to an Eye-glafs placed behind it; I had thence an occafion of confidering that fort of conftructions, and found their difadvantages fo great, that I faw it neceffary, before I attempted any thing in the Practique, to alter the defign of them, and place the Eye glafs at the fide of the Tube rather than at the midle.

The difadvantages of it you will underftand by thefe particulars. 1. There will be more light loft in the Metal by reflexion from the little convex *fpeculum*, than from the Oval plane. For, it is an obvious obfervation, that Light is moft copioufly reflected from any fubftance when incident moft obliquely. 2 The convex *Speculum* will not reflect the rays fo truly as the oval plane, unlefs it be of an Hyperbolique figure; which is incomparably more difficult to forme than a plane; and if tru-

 ly

ly formed, yet would only reflect those rays truly, which respect the *axis*. 3 The errours of the said convex will be much augmented by the too great distance, through which the rays, reflected from it, must pass before their arrival at the Eye-glass. For which reason I find it convenient to make the Tube no wider than is necessary, that the Eye glass be placed as near to the Oval plane, as is possible, without obstructing any useful light in its passage to the object metal. 4. The errors of the object-metal will be more augmented by reflexion from the convex than from the plane, because of the inclination or deflexion of the convex on all sides from the points, on which every ray ought to be incident. 5. For these reasons there is requisite an extraordinary exactness in the figure of the little convex, whereas I find by experience, that it is much more difficult to communicate an exact figure to such small pieces of Metal, than to those that are greater. 6 Because the errors at the perimeter of the concave Object-Metal, caused by the Sphericalness of its figure, are much augmented by the convex, it will not with distinctness bear so large an aperture, as in the other construction. 7. By reason that the little convex conduces very much to the magnifying virtue of the instrument, which the Oval plane doth not, it will magnify much more in proportion to the Sphere, on which the great concave is ground, than in the other design; And so magnifying Objects much more than it ought to do in proportion to its aperture, it must represent them very obscure and dark; and not only so, but also confused by reason of its being overcharged. Nor is there any convenient remedy for this. For, if the little convex be made of a larger Sphere, that will cause a greater inconvenience by intercepting too many of the best rayes; or, if the Charge of the Eye-glass be made so much shallower as is necessary, the angle of vision will thereby become so little, that it will be very difficult and troublesome to find an object, and of that object, when found, there will be but a very small part seen at once.

By this you may perceive, that the three advantages, which Monsieur *Cassegrain* propounds to himself, are rather disadvantages. For, according to his design, the aperture of the instrument inftrument will be but fmall,the object dark and confufed,and alfo difficult to be found. Nor do I fee, why the reflexion is more upon the fame *axis*,and fo more natural in one cafe than in the other: fince the *axis* it felf is reflected towards the Eye by the Oval plain; and the Eye may be defended from external light as well at the fide, as at the bottome of the Tube.

You fee therefore, that the advantages of this defign are none, but the difadvantages fo great and unavoidable, that I fear it will never be put in practife with good effect. And when I confider, that by reafon of its refemblance with other Telefcopes it is fomething more obvious than the other conftruction; I am apt to believe, that thofe,who have attempted any thing in Catoptricks, have ever tryed it in the firft place, and that their bad fuccefs in that attempt hath been the caufe, why nothing hath been done in reflexions. For,Mr. *Gregory*, fpeaking of thefe inftruments in the aforefaid book pag 95, fayeth; *De mechanica horum fpeculorum & lentium, ab aliis fruftrà tentatâ, ego in mechanicis minus verfatus nihil dico.* So that there have been tryals made of thefe Telefcopes, but yet in vain. And I am informed,that about 7 or 8 years fince,Mr. *Gregory* himfelf, at *London*, caufed one of fix foot to be made by Mr. *Reive*, which I take to have been according to the aforefaid defign defcribed in his book; becaufe, though made by a skilful Artift, yet it was without fuccefs.

I could wifh therefore, Mr. *Caffegrain* had tryed his defign before he divulged it: But if,for further fatisfaction, he pleafe hereafter to try it, I believe the fuccefs will inform him, that fuch projects are of little moment till they be put in practife.

Some Experiments propos'd in relation to Mr. Newtons *Theory of light, printed in* Numb. 80; *together with the Obfervations made thereupon by the Author of that Theory; communicated in a Letter of his from* Cambridge, *April* 13. 1672.

I. TO contract the beams of the Sun without the hole of the window, and to place the prifm between the focus of the *Lens* and the hole,fpoken of in M.*Newtons* theory of light,

II. To

II. To cover over both Ends of the Prifm with paper at feveral diftances from the middle ; or with moveable rings,to fee,how that will vary or divide the length of the figure, infifted upon in the faid Theory.

III. To move the Prifm fo, as the End may turn about the middle being fteady.

IV. To move the prifm by fhoving it,till firft the one fide,than the midle, than the other fide pafs over the hole, obferving the fame Parallelifm.

The Obfervations, made upon thefe propofals.

I Suppofe the defign of the Propofer of thefe Experiments is, to have their events expreffed, with fuch obfervations as may occur concerning them. 1. Touching the *firft*, I have obferv'd, that the Solar image falling on a paper placed at the *focus* of the *Lens*, was by the interpofed Prifm drawn out in length proportional to the Prifms reflexion or diftance from that *focus*. And the chief obfervable here, which I remember, was, that the Streight edges of the oblong image were diftincter than they would have been without the *Lens*.

Confidering that the rays coming from the Planet *Venus* are much lefs inclined one to another, than thofe, which come from the oppofite parts of the Suns difque; I once tryed an experiment or two with *her* light. And to make it fufficiently ftrong, I found it neceffary to collect it firft by a broad *lens*, and then interpofing a Prifm between the *lens* and its *focus* at fuch diftance, that all the light might pafs through the Prifm; I found the *focus*, which before appeared like a lucid point, to be drawn out into a long fplendid line by the Prifms reflexion. I have fometimes defigned to try,how a fixt *Star*,feen through a long Telefcope, would appear by interpofing a Prifm between the Telefcope and my eye. But by the appearance of *Venus*, viewed with my naked eye through a Prifm, I prefage the event.

2. Concerning the *fecond* experiment, I have occafionally obferved, that by covering both ends of the Prifm with Paper at feveral diftances from the midle, the breadth of the Solar image will be increafed or diminifhed as much, as is the aper-

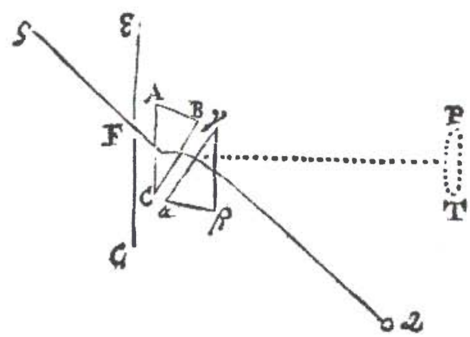
ture

ture of the Prism without any variation of the length: Or, if the aperture be augmented on all sides, the image on all sides will be so much and no more augmented.

3. Of the *third* experiment I have occasion to speak in my answer to another person; where you'l find the effects of two Prisms in all cross positions of one to another described. But if one Prism alone be turned about, the coloured image will only be translated from place to place, describing a circle or some other Conick Section on the wall, on which it is projected, without suffering any alteration in its shape, unless such as may arise from the obliquity of the wall or casual change of the Prisms obliquity to the Suns rays.

4. The effect of the *fourth* experiment I have already insinuated telling you (in pag. 3076 of the *Transactions*) that Light, passing through parts of the Prism of divers thicknesses, did still exhibit the same Phænomena.

Note, that the long *axes* of the two Prisms in the experiment described in the said pag. 3076 of the *Transactions*, were parallel one to another. And for the rest of their position, you will best apprehend it by this Scheme; where let EG design the window; F the hole in it, through which the light arrives at the Prisms; ABC the *first* Prism, which refracts the light towards PT, painting there the colour in an oblong form; and $\alpha\beta\gamma$ the *second* Prism, which refracts back again the rays to Q, where the long image PT is contracted into a round one.



The plane $\alpha\gamma$ to BC, and $\beta\gamma$ to AC, I suppose parallel, that the rays may be equally refracted contrary ways in both Prisms. And the Prisms must be placed very near to one another: For if

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if their diſtance be ſo great,that colours begin to appear in the light before its incidence on the ſecond Priſm, thoſe colours will not be deſtroyed by the contrary refractions of that Priſm.

Theſe things being obſerved,the round image Q will appear of the ſame bigneſs, which it doth when both the Priſms are taken away,that the light may paſs directly towards Q from the hole without any refraction at all. And its diameter will equal the breadth of the long image PT, if thoſe images be equally diſtant from the Priſms.

If an accurate conſideration of theſe refractions be deſigned, it is convenient,that a *Lens* be placed in the hole F,or immediately after the Priſms,ſo that its *focus* be at the image Q or PT. For,thereby the Perimeter of the image Q and the ſtraight ſides of the image PT will become much better defined than otherwiſe.

A Latin Letter written to the Publisher *April* 9. 1672. n. st. by *Ignatius Gaston Pardies* P. Prof. of the Mathematicks in the Parisian Colledge of *Clermont*; containing some Animadversions upon Mr. *Isaac Newton*, Prof. of the Mathematicks in the University of *Cambridge*, his *Theory of Light*, printed in N°. 80.

LEgi ingeniosissimam Hypothesin de* Lumine & Coloribus *Clarissimi* Newtoni. *Et quia nonnullam Ego operam dedi in ista contemplatione atque Experimentis peragendis, perscribam ad Te pauca, quæ mihi circa novam istam doctrinam occurrerunt.*

Circa ipsam Luminis naturam illud profectò extraordinarium videtur, quòd ait vir eruditissimus, Lumen constare ex aggregatione infinitorum propemodum radiorum, qui suâpte indole suum quisque colorem referant retineantque, atque adeò nati apti sint certâ quadam & peculiari ratione, plus alij, alij minus, refringi: Radios ejusmodi, dum promiscui in aperto lumine confunduntur, nullatenus discerni, sed candorem potiùs referre; in refractione verò singulos unius coloris ab aliis alterius coloris secerni, & hoc modo secretos, sub proprio & nativo colore apparere: Ea corpora sub aliquo colore, v. g. rubro, videri, quæ apta sint reflectere aut transmittere radios solummodò rubros, &c.

Istæc tam extraordinaria Hypothesis, quæ, ut ipse observat, Dioptricæ fundamenta evertit, praxêsque hactenus institutas inutiles reddit, tota nititur illo Experimento Prismatis Crystallini, ubi radij per foramen fenestræ intra obscurum cubiculum ingressi, ac deinde in parietem impacti, aut in charta recepti, non in rotundum conformati, ut ipsi, ad regulas refractionum receptas attendenti, expectandum videbatur, *sed in oblongam figuram extensi apparuerunt: Unde conclusit, oblongam ejusmodi figuram ex eo esse, quòd nonnulli radij minus, nonnulli magis refringerentur.*

Sed mihi quidem videtur juxta communes *& receptas Dioptricæ* leges *figuram illam, non rotundam, sed oblongam esse oportere. Cùm enim radij ex oppositis disci Solaris partibus procedentes, variam habeant in ipso transitu Prismatis inclinationem, variè quoque refringi debent; ut cùm unorum inclinatio* 30 *saltem minutis major sit inclinatione aliorum, major quoque evadat illorum Refractio.*

Xxxx *Igitur*

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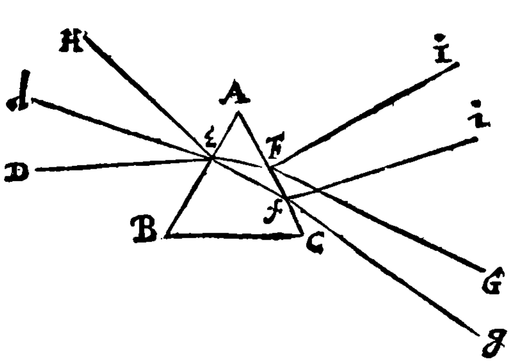
Igitur Radii oppositi, ex altera superficie Prismatis emergentes magis divergunt & divaricantur, quàm si nullatenus, aut saltem æqualiter, omnes infracti processissent. Refractio autem ista radiorum fit solummodo versùs eas partes quæ fingi possunt in planis ad axem Prismatis rectis; nulla autem refractionis inæqualitas contingit versùs eas partes, quæ intelliguntur in planis axi parallelis; ut facilè demonstrari potest: superficies enim duæ Prismatis censeri possunt inter se parallelæ, ratione habita ad inclinationem axis, cùm singulæ ipsi axi parallelæ sint. Refractio autem per duas parallelas planas superficies nulla computatur, quia quantùm à prima superficie radius in unam partem torquetur, tantum ab altera in oppositam partem detorquetur. Igitur cùm radij solares è foramine per Prisma transmissi ad latera quidem non frangantur, procedunt ulterius, perinde ac si nulla Prismatis superficies obstitisset, (habitâ, inquam, ratione solùm ad lateralem illam divaricationem;) at verò cùm iidem radij ad superiores seu inferiores partes, alij quidem magis, alij verò minus, utpote inæqualiter inclinati, infringantur; necesse est eos magis inter se divaricari, adeóque & in longiorem figuram extendi.

Quin si calculus ritè obeatur; ut radij laterales inventi sunt à Cl. Newtono *in ea latitudine quæ subtendit arcum 31', qui arcus respondet diametro Solis; ita nullus dubito, quin illa inventa quoque altitudo imaginis, quæ 2 gradus & 49' subtendit, sit illa ipsa quæ eidem diametro Solis post inæquales refractiones in illo ipso casu respondeat.*



Et reverâ, posito Prismate A B C, *cujus angulus* A *sit* 60 *grad. Radio* D E, *qui faciat cum perpendiculari* E H *angulum* 30 *grad. Invenio illum, dum emergit per* F G, *facere cum perpendiculari* F I *angulum* 76 *gr.* 22'. *At verò posito alio radio* d E, *qui cum perpendiculari*

culari EH *faciat angulum* 29°. 30', *invenio illum, dum emergit per* fg, *facere cum perpendiculari* fi, *angulum* 78°. 45'. *Unde isti duo radij* DE, dE, *qui procedere supponuntur ex oppositis partibus disci Solaris, faciuntque inter se angulum* 30', *iidem dum emergunt per lineas* Fg, fg, *ita divergunt ut constituant angulum inter se* 2 *gr.* 23'. *Quod si duo alij radij assumerentur magis accedentes ad perpendicularem* EH, (*v.g. qui cum eadem perpendiculari facerent, unus quidem, angulum* 29°. 30', *alter verò*, 29°. 0';) *tunc iidem radij emergentes magis adhuc divergerent, constituerentque angulum majorem etiam aliquando plus quàm trium graduum. Et praeterea augetur ulterius ista intercapedo refractorum radiorum ex eo, quòd duo radij* D E, d E, *concurrentes in* E, *illico incipiunt divaricari, atque impingunt in duo puncta disjuncta alterius superficiei, nempe in* F *& in* f. *Quapropter non sufficit ad obeundum ritè calculum, ex longitudine imaginis impactae in chartam subtrahere magnitudinem foraminis fenestrae; quandoquidem etiam posito foramine indivisibili* E, *adhuc fieret aliud veluti foramen latum in alia superficie, nempe* F f.

Quod etiam vocat Experimentum crucis, *mihi quidem videtur quadrare cum vulgaribus & receptis Refractionum regulis. Nam, ut modo ostendi, radij solares, qui accedentes & convergentes faciunt angulum* 30', *egredientes deinde etiam post indivisibile foramen divergunt in angulum duorum & trium grad. Quapropter non mirum, si isti radij, sigillatim impingentes in alterum Prisma, perexiguo item apertum foramine, inaequaliter infringantur, cùm sit inaequalis illorum inclinatio. Neque refert, quòd isti radij attollantur aut deprimantur per conversionem primi Prismatis, manente immoto secundo Prismate, (quod tamen in omni casu fieri non potest) vel quòd manente primo immobili, secundum moveatur, ut successivè radios coloratos totius imaginis excipiat & per proprium foramen transmittat; utrolibet enim modo necesse est radios illos extremos, hoc est*, Rubrum *&* Violaceum, *incidere in secundum Prisma sub inaequali angulo, adeoque eorundem refractionem esse inaequalem, ut* Violaceorum *sit major.*

Cùm igitur manifesta causa appareat oblongae ejusmodi figurae radiorum, causaque illa ex ipsa natura Refractionis oriatur; non videtur necesse recurrere ad aliam Hypothesin, aut admittere diversam illam radiorum frangibilitatem.

Xxxx 2 *Quod*

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Quod deinde excogitavit de Coloribus, *illud quidem egregiè consequitur ex precedente Hypothesi; veruntamen nonnullas & ipsum patitur difficultates. Nam quod ait, nullum colorem, sed potius* candorem *apparere, ubi omnes omnium colorum radij promiscuè confunduntur, id verò non videtur conforme omnibus phænomenis. Certè quæ variationes cernuntur in permistione diversorum corporum, diversis coloribus imbutorum, eædem omninò observantur in permistione diversorum radiorum diversis item coloribus imbutorum: Atque optimè ipse advertit, quòd quemadmodum ex flavo & cæruleo corpore exsurgit viridis color; ita ex flavo & cæruleo radio viridis item color efficitur. Quare si omnes omnium colorum radii simul confunderentur, necesse esset in ista hypothesi, ut ille color appareret, qui revera apparet in permixtione omnium pigmentorum. Atqui si ista, hoc est, rubrum simul & flavum unà cum cæruleo & purpureo aliisque omnibus, si quæ sint, conterantur & confundantur, non jam candidus, sed obscurus & satur color exsurget. Ergo similis color appareret in lumine ordinario, quod constaret ex aggregatione omnium colorum.*

Præterea nihil primo aspectu magis ingeniosum magisque aptum videtur, quàm quod ait circa experimentum acutissimi Hookii, *quo duo diversi liquores, quorum alter rubeus, alter cæruleus, uterque sigillatim pellucidus, simul permixti, opaci evadunt. Id autem ait Clarissimus* Newtonus *ex eo oriri, quòd unus liquor solos rubeos natus sit transmittere, alter verò solos flavos; unde permisti nullos transmittent. Hoc, inquam, videtur statim valdè appositum; nihilominus tamen ex eo conficeretur, quòd similis opacitas fieret in permistione quorumcunque liquorum qui essent diversi coloris; quod tamen verum non est.*

Mr. *Newtons*

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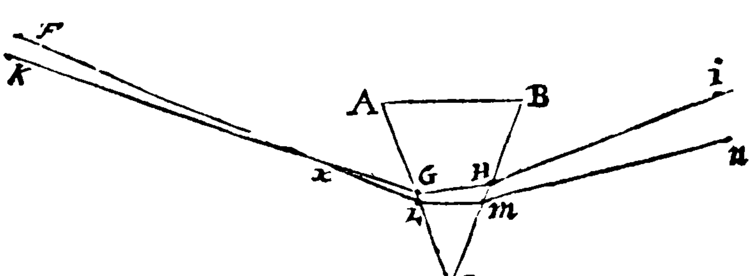
Mr. *Newtons* Letter of *April* 13. 1672. st. v. written to the Publisher, being an Answer to the fore-going Letter of *P. Pardies*.

Accepi Observationes Reverendi Patris Ignatii Pardies *in Epistolam meam de Lucis Refractionibus & Coloribus ad Te conscriptam: quo nomine me illi valde devinctum agnosco; atque hoc difficultatibus, quas proposuit, eluendis rescribo. Imprimis ait, longitudinem solaris Imaginis à refractione Prismatis effectam non aliâ indigere causâ, quàm diversâ radiorum ab oppositis partibus solaris disci profluentium incidentiâ, adeoque non probare diversam refrangibilitatem diversorum radiorum. Et, quò assertionis ejus veritatem confirmet, ostendit casum, in quo ex* diversa *incidentia* 30 *minutorum, differentia refractionis potest esse* 2 *grad.* 23. *min. vel etiam paulo major, prout exigit meum experimentum. Sed hallucinatus est R. P. Nam refractiones à diversa parte Prismatis quantum potest inæquales statuit, cùm tamen ego tum in experimentis, tum in calculo de experimentis istis inito, æquales adhibuerim, ut in Epistola præfata videre est. Sit ergo A B C Prismatis sectio ad axem ejus perpendicularis, F L & K G radii duo in x (medio foraminis) decussantes & in Prisma illud incidentes ad G & L; sintque eorum refracti G H & L m, ac denuò H I & m n. Et*



cùm refractiones ad latus A C æquales esse refractionibus ad latus B C quam proximè supposuerim; Si A C & B C statuantur æqualia, similis erit radiorum G H & L m ad AB basin Prismatis inclinatio; adeoque ang. C L m = ang. C H G & ang. C m L = ang. C G H. Quare etiam refractiones in G & m æquales erunt, ut & in L & H;

atque

atque adeò ang. KGA = ang. n m B, & ang. F L A = ang. B H I ; & proinde refractorum H I & m n eadem erit ad invicem inclinatio ac est incidentium radiorum F L & K G. Sit ergo angulus F x K 30 min. æqualis nempe solari diametro, & erit angulus, quem H I & m n comprehendunt, etiam 30 min. si modò radii F L & K G æqualiter refrangibiles statuantur. At mihi experienti prodiit angulus ille circiter 2 grad. 49. min. quem radius H I, extremum violaceum colorem, & m n, cæruleum exhibens, constituêre; ac proinde radios illos diversimodè refrangibiles esse, sive refractiones secundùm disparem sinuum incidentiæ & refractionis rationem peragi necessariò concedendum est.

Addit præterea R. P. quòd non sufficit ad obeundum ritè calculum, ex longitudine imaginis impactæ in Chartam subtrahere magnitudinem foraminis fenestræ ; quandoquidem etiam posito foramine indivisibili, adhuc fieret aliud veluti foramen latum in posteriori superficie prismatis. Mihi tamen videtur, his non obstantibus, quòd refractiones radiorum, in anteriori æquè ac in posteriori superficie Prismatis accussantium, ex adhibitis principiis possint ritè computari. Sed si res secùs esset, latitudo hiatûs in posteriori superficie, quod ad instar foraminis est, haud efficeret errorem duorum minutorum secundorum; & in rebus practicis non operæ pretium duco ad minutias istas attendere.

Illi insuper experimento, quod Crucis *vocaveram, nihil adversatur R. P, dum contendit, inæquales radiorum, diversis coloribus imbutorum, refractiones ex inæqualibus incidentiis effectas fuisse. Nam radiis per duo admodum parva, ab invicem distantia & immota foramina, transeuntibus, incidentiæ illæ, prout ego experimentum institui, omninò æquales erant, & tamen refractiones liquidò inæquales. Sin ille de experimentis nostris dubitet, oro, ut radiorum diversis coloribus præditorum refractiones ex incidentiis paribus mensuret, & sentiet inæquales esse. Si modus ille, quem ego ad hoc negotium adhibui, minùs placeat (quo tamen nullus potest esse luculentior,) facile est alios excogitare ; sicut & alios ipse haud paucos cum fructu expertus sum.*

Contra Theoriam de Coloribus *obijcitur, quòd pulveres diversorum colorum permisti non candidum sed subobscurum & fuscum colorem exhibent. Mihi verò albus, niger, & omnes intermedii fusci, qui ab albo & nigro permistis componi possunt, non specie coloris sed quantitate lucis tantùm differre videntur. Et cùm in mistione pigmentorum, singula corpuscula non nisi proprium colorem reflectant, adeoq; maxima*

pars

(4093)

pars lucis incidentis supprimatur & retineatur; lux reflexa subobscura evadet, & quasi cum tenebris permista, adeò ut non intensum alborem, sed qualem nigredinis permistio conficit, hoc est, fuscum, exhibere debeat.

Objicitur deinde, quòd à liquoribus quibuscunque diversi coloris in eodem vase commistis, æquè ac in diversis vasis contentis, opacitas oriri debet; quod tamen, ait, verum non esse. Sed non video consequentiam. Nam plurimi liquores agunt in se invicem, & novam sibi mutuò partium contexturam secretò inducunt; unde opaci, diaphani, vel variis coloribus, ex coloribus permistorum nullo modo oriundis, prædi-ti evadere possunt. Et hâc de causâ experimenta hujusmodi minùs apta semper existimavi, à quibus conclusiones deduci possint. Subnoto tamen, quòd ad hoc experimentum requiruntur liquores saturis & intensis coloribus præditi, qui perpaucos nisi proprii coloris radios transmittant; quales rarò occurrunt, ut videbitur illuminando liquores cum diversis coloribus Prismaticis in obscurato cubiculo. Nam pauci reperientur, qui in propriis coloribus satis diaphani appareant, inque alienis opaci. Convenit præterea, ut adhibiti colores sint inter se oppositi, quales existimo fore rubrum & cæruleum, vel flavum & violaceum, vel etiam viridem & purpureum illum qui coccineo affinis est. Et ex hujusmodi liquoribus nonnulli (quorum partes tingentes non congredientur) fortaße permisti evadent opaciores. Sed de eventu nihil sum sollicitus, tum *quod luculentius est experimentum in liquoribus seorsim existentibus,* tum *quod experimentum illud (sicut & Iridis, Tincturæ* Nephriticæ, *& aliorum corporum naturalium phænomena) non ad probandam sed ad illustrandam tantùm doctrinam proposui.*

Quod R. P. Theoriam nostram Hypothesin *vocat, amicè habeo, siquidem ipsi nondum constet. Sed alio tamen consilio proposueram, & nihil aliud continere videtur quàm* proprietates quasdem Lucis, *quas jam inventas probare haud difficile existimo, & quas si non veras eße cognoscerem, pro futili & inani speculatione mallem repudiare, quàm pro mea Hypothesi agnoscere. Quid verò censeri mereatur, ex responsionibus ad animadversiones Domini* N.N. fortaße statim prodituris *clariùs patebit. Interea vale, & perge amare*

Tibi devinctissimum

J Newton

Some Animadversions on the Theory of Light of Mr. ISAAC NEWTON, *Prof. of Mathematics in the University of Cambridge, printed in N°.* 80. *In a Letter of April* 9, 1672, *N. S. from* IGNATIUS GASTON PARDIES,* *P. Prof. of Mathematics in the Parisian College of Clermont. Translated from the Latin. N°* 84, *p.* 4087.

I have read Mr. Newton's very ingenious hypothesis of light and colours.

* Ignatius Gaston Pardies, a French Jesuit, and professor of mathematics in the Parisian college of Clermont, was born in 1636. He entered the Jesuits order at 16, and after some time he devoted himself entirely to mathematics and natural philosophy. In this latter branch he followed the opinions of Descartes, though he feebly affected the contrary. He died at Paris in 1673, aged only 37, of a contagious disorder caught at the Bicêtre, where he officiated as a preacher and a confessor. He

And as I have given some attention to that subject, and also made experiments, I shall here inform you of what has occurred to me on that new doctrine.

It seems very extraordinary that the learned author should make light to consist of an almost infinite number of rays, endued with a natural disposition of retaining and exhibiting their own proper colours, and that are disposed in a certain peculiar way to be refracted, some in a greater, and others in a less degree: that these rays which, while promiscuously blended together in open daylight, are undiscernible, and exhibit only the colour of whiteness, should notwithstanding in refraction have rays of one colour separated from those of all others, and, thus separated, appear in their proper and native colours: and that bodies should appear of a certain colour, red for instance, which are adapted to reflect or transmit rays of that colour only.

This extraordinary hypothesis, which, as he observes, overturns the very basis of dioptrics, and renders useless the practice hitherto known, is founded entirely on the experiment of the prism, in which rays entering into a dark room through a hole in the window-shutter, and then falling on the wall, or received on a paper, did not form a round figure, as he expected according to the received rules of refraction, but appeared extended into an oblong form: whence he concluded, that this oblong figure was owing to the different refrangibility of the rays of light.

But it appears to me that, according to the common and received laws of dioptrics, the figure ought to be, not round but oblong. For since the rays proceeding from the opposite parts of the sun's disk, are variously inclined in their passage to the prism, they ought also to be variously refracted; that since the inclination of some rays is at least 30′ more than that of others, their refraction must also be greater. Therefore the opposite rays, emerging from the other surface of the prism, become more diverging, than if they had proceeded without any refraction, or at least with an equal one. Now that refraction of the rays is made only towards those parts, which may be supposed to be in the planes perpendicular to the axis of the prism; for there is no inequality of refraction towards those parts which are conceived to be in planes parallel to the axis, as may easily be demonstrated: for the two surfaces of the prism may be

was author of several ingenious works, which are written in a manner remarkably neat and clear, by which he acquired considerable credit, and by his talent as a teacher; but, unfortunately for him, lost himself by the above imprudent attack on Sir I. Newton's theory of light and colours His works were chiefly, 1. Elements of Geometry, translated into English by Dr. John Harris, secretary of the Royal Society. 2. Discourse on the Knowledge of Beasts. 3. Statics, or the science of Moving Forces. 4. Two machines for drawing dials. 5. Discourse on Local motion. 6. Horologium Thaumanticum Duplex. 7. Dissertation on the Nature and Motion of Comets.

considered as parallel, with respect to the inclination to the axis, since they are both parallel to it. But the refraction through two parallel plane surfaces is accounted none, because by how much a ray is refracted one way by the first surface, by just so much is it refracted the contrary way by the other surface. Therefore since the solar rays, transmitted by a hole through a prism, are not refracted sideways, they proceed in that respect as if no prism at all stood in their way, that is with regard to the lateral divarication; but when the same rays on the superior and inferior parts, are refracted, some more, some less, as being unequally inclined, they must needs diverge more, and consequently be extended in an oblong figure.

But when a calculation is rightly made, as the lateral rays were found by Mr. Newton, of a breadth that subtended an arc of 31′, which answers to the sun's diameter; so there is no doubt but the length of the image, which subtended 2° 49′, would correspond with the same diameter after the unequal refractions. Thus, supposing the prism at ABC, (fig. 7, pl. 15,) having the angle A of 60°; and a ray DE making with the perpendicular EH an angle of 30°; after emerging in the line FG, I find it makes with the perpendicular FI an angle of 76° 22′. But taking another ray dE, which makes with the perpendicular EH an angle of 29° 30., I find that, when it emerges by fg, it makes with the perpendicular fi, an angle of 78° 45′. Hence those two rays DE, dE, which are supposed to proceed from opposite parts of the solar disk, and forming between them an angle of 30′, where they emerge by the lines FG, fg, they diverge so as to form between them an angle of 2° 23′. And if two other rays were assumed approaching nearer the perpendicular EH, as suppose one of them forming with it an angle of 29° 30′, and the other 29°; these rays, after emerging, would diverge still more, and form a greater angle, even sometimes more than 3°. And besides, this distance between the refracted rays is further increased, on this account, that the two rays DE, dE, meeting in E, begin immediately to diverge, and then fall on two distant points of the second surface, viz. in F and f. Therefore, in order to render the calculation just, it is not sufficient barely to subduct the diameter of the hole from the length of the image; for supposing the hole E to be invisible, or almost nothing, yet there would be formed a great hole as it were, in Ff, in the second surface of the prism.

What the author calls the Experimentum Crucis, seems also to agree with the commonly received laws of refraction. For, as was just now shown, the sun's rays, which approaching and converging from an angle of 30′, coming from an invisible hole, do afterwards diverge in an angle of two or three degrees. It is not then to be wondered at, if these rays falling severally on a second prism, and having a very small hole in it, be unequally refracted, since their inclination is unequal. Nor does it alter the case, that those rays are raised or depressed by the rotation of the first prism, the second remaining immoveable, (which however cannot be done in all cases, or contrarywise, the second being turned while the first is fixed, that it may successively receive the coloured rays of the whole image, and transmit them through its proper hole; for in either case it is necessary that the extreme rays, viz. the red and the violet, should fall on the second prism under unequal angles, and consequently that their refraction be unequal, that of the violet being the greater.

Since then, here is an evident cause of that oblong figure of the rays, and that cause such as arises from the very nature of refraction; it seems needless to have recourse to another hypothesis, or to admit of that diverse refrangibility of the rays.

The author's notion of colours indeed follows very well from the preceding hypothesis; yet it is not without its difficulties. For when he says, that all the rays being promiscuously blended together, yield no colour, but rather a whiteness, this does not seem conformable to all the phænomena. Doubtless the same variations that are seen in the mixture of divers bodies of different colours, are also observed in the mixture of different rays of various colours: and the author himself has well observed, that as a green colour arises from a yellow and a blue body, so likewise a green colour is produced from a yellow and a blue ray. Therefore, if all the rays of the several colours be blended together, it is necessary in that hypothesis, that that colour should appear, which in reality arises on mixing together the several sorts of painters colours. That is, as the red, yellow, blue, purple, and all the others, when mixed together, produce, not a white, but an obscure sated colour. So also ordinary light should appear of the same colour, being a like aggregate of all the colours.

Indeed nothing can be more ingenious and proper, than what he says about Mr. Hook's experiment, in which are two different liquors, the one red, the other blue, and each apart transparent, yet when mixed together they become opaque: this the ingenious author thus explains: that the one liquor is disposed to transmit only the red rays, the other only the yellow; hence, both being mixed together, they transmit none at all. But it should seem that the like opacity should take place on the mixture of liquors of any other different colours: which however is far enough from the truth.

Mr. NEWTON's *Letter of April* 13, 1672, *O. S. written to the Editor, being an Answer to the foregoing Letter of F.* PARDIES. *Translated from the Latin.* *N°* 84, *p.* 4091.

I received, Sir, the observations of the Rev. Father Ignatius Pardies, on my letter concerning the refractions and colours of light: for which I acknowledge myself much obliged to him; and shall here clear up the difficulties he complains of. In the first place, he says that the length of the solar image produced by the refraction of the prism, requires no other cause to account for it, than the different incidence of the rays from opposite parts of the sun's disk; and that therefore it does not prove a different refrangibility in the different rays. And, to prove the truth of his assertion, he states a case, in which from a difference of 30′ in the incidence, the difference of the refraction may be 2° 23′, or rather more, as my experiment requires. But the Rev. Father is under a mistake. For he has made the refractions by the different parts of the prism to be as unequal as possible, whereas in the experiments, and in the calculation from them, I employed equal refractions. Thus, let ABC (fig. 8, pl. 15,) be a section of the prism perpendicular to its axis; FL and KG two rays crossing each other in x, the middle of the hole, and incident on the prism at G and L; which let be first refracted into GH and Lm, and then into HI and mn. And since I supposed the refractions at the side AC are nearly equal to those at the side BC; if AC and BC be equal, the inclination of the rays GH and Lm, to the base AB of the prism, will be similar; and therefore the angle CLm=the angle CHG, and the angle CmL=the angle CGH. Therefore the refractions in G and m will be also equal, as well as those at L and H; consequently the angle KGA=the angle nmB, and the angle FLA=the angle BHI; and hence the inclination of the refracted rays HI and mn will be the same with that of the incident rays FL and KG. Therefore let the angle FxK of 30′ be equal to the sun's diameter, then the angle made by HI and mn will be also of 30′, provided the rays FL and KG be equally refrangible. But my experiment gave that angle about 2° 49′, which is constituted by the ray HI of the extreme violet colour, and by the ray mn which gives the blue; and therefore those rays were differently refrangible, or the refractions were necessarily produced according to the unequal ratio of the sines of incidence and refraction.

The Rev. Father further adds, that to make a just calculation, it is sufficient to subtract the magnitude of the window hole from the length of the image on the paper; since, even supposing the hole indivisible, yet there would be formed as it were a broad hole in the posterior surface of the prism. But yet it seems to me, that the refractions of rays crossing each other, both in the anterior and posterior surface of the prism, may be justly calculated from my principles. But if the case were otherwise, the breadth of the hole in the posterior surface, if such there be, would hardly produce an error of two seconds; and in practice such niceties may well be neglected.

What the Rev. Father contends is not inconsistent with what I called the Experimentum Crucis, viz. that the unequal refractions of rays endued with different colours, were produced by unequal incidences: for transmitting rays through two very small immoveable holes, and at a distance from each other, the incidences, as I made the experiment, were always equal, and yet the refractions were manifestly unequal. If he has any doubt of our experiment, I request that he may measure the refractions of the said rays of divers colours from equal incidences, and he will then see that they are unequal. But if he dislikes the manner in which I have performed this matter (than which however nothing can be clearer) it is easy to devise other ways; as indeed I myself have tried several other methods with advantage.

Against the theory of colours it is objected, that powders of divers colours mixed together, do not yield a white, but an obscure and dusky colour. But to me, white, black, and all the intermediate dusky colours, which can be compounded of mixtures of white and black, do not differ as to their species, but only as to their quantity of light. And since in the mixture of painters' colours, each corpuscle reflects only its own proper colour, and therefore the greatest part of the incident light is suppressed and retained; the reflected light will become obscure, and as if mixed with darkness, so that it exhibits not an intense whiteness, but an obscure dusky colour.

Again it is objected that an opacity ought equally to arise from a mixture of any liquors of different colours in the same vessel, as from the same liquors contained in different vessels; which however he says is not true. But I see no consequence in this. For many liquors act mutually on each other, and acquire a new texture of parts; hence they may become opaque, or diaphanous, or of various colours, in no manner owing to the colours of the compound. And on that account I have always esteemed experiments of this kind not so proper to draw conclusions from. It must also be noted that this experiment requires liquors of full and intense colours, which transmit very few rays besides those of their own colours; such as rarely occur, as will be seen by illuminating liquors with different prismatic colours in a dark room. For few will be found diaphanous enough in their own proper colours, and opaque in the others. Besides, it is proper that the colours employed be opposites, such as I count red and blue to be, or yellow and violet, or green, and that purple which approaches to scarlet. And perhaps some of these liquors mixed together, whose tinging parts do not coalesce, will become more opaque. But I am not solicitous about the event, both as the experiment is clearer in liquors apart, and as the experiment (like the phænomena of the iris, and the tincture of lignum nephriticum, and of other natural bodies) I proposed not to prove but only to illustrate the doctrine.

I do not take it amiss that the Rev. Father calls my theory an hypothesis, inasmuch as he was not acquainted with it. But my design was quite different, for it seems to contain only certain properties of light, which, now discovered, I think easy to be proved, and which if I had not considered them as true, I would rather have them rejected as vain and empty speculation, than acknowledged even as an hypothesis.

(4004)

A Serie's of Quere's *propounded by Mr.* Isaac Newton, *to be determin'd by Experiments, positively and directly concluding his new Theory of Light and Colours; and here recommended to the Industry of the Lovers of Experimental Philosophy, as they were generously imparted to the Publisher in a Letter of the said Mr.* Newtons *of* July 8. 1672.

IN the mean while give me leave, Sir, to insinuate, that I cannot think it effectual for determining truth, to examin the several waies by which Phænomena may be explained, unless where there can be a perfect enumeration of all those waies. You know, the proper Method for *inquiring* after the properties of things is, to deduce them from Experiments. And I told you, that the Theory, which I propounded, was evinced to me, not by inferring 'tis thus because not otherwise, that is, not by deducing it only from a confutation of contrary suppositions, but by deriving it from Experiments concluding positively and directly. The way therefore to examin it is, by considering, whether the Experiments which I propound do prove those parts of the Theory, to which they are applyed; or by prosecuting other Experiments which the Theory may suggest for its examination. And this I would have done in a due Method; the Laws of *Refraction* being throughly inquired into and determined before the nature of *Colours* be taken into consideration. It may not be amiss to proceed according to the *Series* of these *Quæries*; which I could wish were determined by the Event of proper Experiments; declared by those that may have the curiosity to examin them.

1. Whether rays, that are *alike* incident on the same *Medium*, have *unequal* refractions; and how great are the inequalities of their refractions at any incidence?

2. What is the Law according to which each ray is more or less refracted; whether it be that the same ray is ever refracted according to the same *ratio* of the sines of incidence and refraction; and divers rays, according to divers *ratio's*; or that the refraction of each ray is greater or less without any certain rule? *That is*, whether each ray have a certain degree of refrangibility according to which its refraction is performed; or is refracted without that regularity?

3. Where-

3. Whether rays, which are endued with particular degrees of refrangibility, when they are by any means ſeparated, have particular colours conſtantly belonging to them; *viz.* the leaſt refrangible, *Scarlet*; the moſt refrangible, *deep Violet*; the middle, *Sea-green*; and others, other colours? And on the contrary?

4. Whether the colour of any ſort of rays apart may be changed by refraction?

5. Whether colours by coaleſcing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, indued with all variety of colours, produces Light perfectly like that of the Sun, and which hath all the ſame properties, and exhibits the ſame *Phænomena*?

7. Whether the component colours of each mixture be really changed; or be only ſeparated when from that mixture various colours are produced again by Refraction?

8. Whether there be any other colours produced by refraction than ſuch, as ought to reſult from the colours belonging to the diverſly refrangible rays by their being ſeparated or mixed by that refraction?

To determine by Experiments theſe and ſuch like *Quære's* which involve the propounded Theory, ſeems the moſt proper and direct way to a concluſion. And therefore I could wiſh all objections were ſuſpended, taken from *Hypotheſes* or any other heads than theſe two; Of ſhewing the inſufficiency of Experiments to determine theſe *Quære's* or prove any other parts of my Theory, by aſſigning the flaws and defects in my concluſions drawn from them; Or of producing other Experiments which directly contradict me, if any ſuch may ſeem to occur. For if the Experiments, which I urge, be defective, it cannot be difficult to ſhow the defects; but if valid, then by proving the Theory they muſt render all Objections invalid.

So far this accurate Propoſer; whoſe Method appearing to be moſt genuine and proper to the purpoſe it is propounded for, and deſerving therefore to be conſidered and put to trial by Philoſophers, abroad as well as at home; the Publiſher, to invite and gratify Forraigners, was willing to deliver the above recited Extract of Mr. *Newtons* Letter in the language alſo of the Learned, as followeth; Zzzz 2 Ex-

(5006)

Excerptum ex *Isaaci Newtoni* Epistola, nuper ad Editorem script, quâ ipse genuinam suggerit Methodum, doctrinam suam de *Luc* & *Coloribus*, antehac propositam, evincendi, subjectâ certorum *Quæsitorum*, debitis Experimentis solvendorum, serie.

L*Iceat mihi hac occasione tibi significare, nequaquam censere me, efficacem eam esse determinandæ veritatis rationem, quâ diversi examinantur modi, quibus Phænomena explicari possunt, nisi ubi perfecta fuerit omnium istorum modorum Enumeratio. Nosti, genuinam proprietates rerum investigandi Methodum esse, quâ illæ ab Experimentis deducuntur. Ac jam antè tibi dixeram; Theoriam à me propositam evictam mihi fuisse,* non quidem inferendo rem ita se habere quia haud se habeat aliter, *i. e. non eam deducendo duntaxat à contrariarum suppositionum confutatione;* sed ipsam ab Experimentis, positivè & directè concludentibus, derivando. *Vera itaque ratio eam examinandi hæc erit, si consideremus scilicet, num Experimenta à me proposita illas Theoriæ partes, quibus accommodantur, reverà probent; vel si alia prosequamur Experimenta, quæ ab ipsa Theoria ad examinandam eam suggerantur. Atque hoc ipsum Methodo genuinâ fieri velim; pervestigatis primum ac determinatis Legibus* Refractionis, *priusquam* Colorum *natura disquiratur. Præter rem itaque haud fore crediderim, disquisitionem hanc ex sequentium* Quæsitorum *serie instituere; quæ quidem ut à solertibus sagacibusque naturæ Mystis, pronunciatis Experimentorum Eventibus, dirimantur, in votis quàm maximè habeo. Ea sunt;*

Primò, *Num radii, qui* æquali *incidentiâ in idem medium incidunt, Refractiones habeant* inæquales; *quantæque sint refractionum, quas illi subeunt, inæqualitates in quavis incidentia?*

Secundò, *Quænam ea Lex sit, juxta quam radius quilibet magis minúsve refringitur? sitnè, quòd idem radius semper refringatur secundùm eandem rationem Sinuum Incidentiæ & Refractionis; diversi autem radii, secundùm rationes diversas? An verò, quòd cujuslibet radii refractio major minórve sit absque ulla regula certa? Hoc est, Utrum unusquisque radius certum habeat gradum Refrangibilitatis, juxta quem fiat ipsius refractio; an verò refringatur sine ista regularitate?*

Tertiò, *Num radii, certis gradibus refrangibilitatis præditi, quando, quodemum cumque modo, secernuntur, certos obtineant colores ipsis proprios; puta radii minimè omnium refrangibiles, Coccineum; maximè refrangibiles, saturum Violaceum; intermedii, sub-Viridem; alii, alios? Et è contrà.*

Quartò, *Num color cujusvis generis radiorum seorsim existentium mutari possint Refractione?*

Quintò, *Utrum colores coalescendo reverà se invicem mutent ad producendum colorem novum; an verò eum producant nonnisi se invicem commiscendo?*

Sextò, *Num debita radiorum miscela, omnigenâ colorum varietate prædita, Lucem producat Solari luci simillimam; quæque easdem omninò proprietates obtineat, eademque Phænomena exhibeat?*

Septimò

(5007)

Septimò, *Utrum componentes cujusvis miscela colores reverà mutentur; an verò secernantur duntaxat, quando ex mixtura illa varii colores rursum producuntur per Refractionem?*

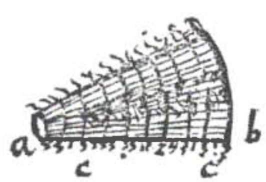
Octavò, *Denturne ulli alii colores Refractione producti præter eos, quos oriri oportet á Coloribus, ad radios diversimodè refrangibiles pertinentibus, dum illi refractione istâ secernuntur vel miscentur?*

Per Experimenta determinare hæc similiave Quæsita, quæ propositam Theoriam involvunt, maximè genuina directaque videtur ad Conclusionem via: Proindéque omnes velim Objectiones suspendi, quæ ab Hypothesibus desumuntur ullisve Fontibus aliis, quàm his duobus; quibus nempe vel *ostendatur Experimentorum ad determinanda hæc ζητήματα probandasve ullas alias Theoriæ meæ partes insufficientia, hallucinationes defectusque in Conclusionibus meis inde deductis indigitando;* vel *alia producantur Experimenta, é diametro mihi opposita, si quæ talia occurrere videantur. Si enim Experimenta, quæ á me urgentur, laborant defectibus, difficile haud fuerit eos ostendere; si veró valida fuerint, eo ipso dum Theoriam meam asserunt probantque omnes Objectiones convellunt.*

(5012)

A Second Letter of P. *Pardies*, written to the Publisher from *Paris May* 21. 1672. to Mr.*Newtons* Answer, made to his first Letter, printed in *Numb.* 84.

R*Edditæ mihi sunt tuæ literæ cum Observationibus Clarissimi atque Ingeniosissimi* Newtoni, *quibus ad meas difficultates respondit. Eas ego legi non sine maxima voluptate: Et primùm, quod attinet ad ipsum Experimentum majoris Latitudinis colorum quàm exigeret vulgaris Theoria Refractionum; fateor, me* inæquales *refractiones in oppositis Prismatis faciebus supposuisse, nec ulla tenus advertisse in literis relatis in* Transactionibus, *observatam fuisse à* Newtono *majorem illam latitudinem in eo casu in quo refractiones ponerentur reciprocè* æquales, *eo modo quo hîc in istis observationibus dicitur. Sed nec ab eo tempore in iisdem* Transactionibus *videre licuit, cùm eas non potuerim recuperare. Cùm igitur nunc videam, etiam in eo Casu observatam majorem illam Colorum latitudinem; certè ex hoc capite nihil mihi ulterius restat difficultatis: Ex hoc, inquam, capite; nam aliundè videtur posse reddi ratio illius Phænomeni absque ista varia Radiorum Refrangibilitate. Etenim in ea Hypothesi, quam fusè explicat noster* Grimaldus, *in qua supponitur Lumen esse substantia quadam rapidissimè mota, posset fieri aliqua diffusio luminis post transitum foraminis & decussationem radiorum.* Item *in ea Hypothesi, qua lumen ponitur progredi per certas quasdam materiæ subtilis Undulationes, ut explicat subtilissimus* Hookius, *possunt explicari colores per certam quandam diffusionem atque expansionem Undulationum, qua fiat ad latera radiorum ultra foramen, ipso contagio ipsaque materiæ continuatione. Certe ego talem adhibeo hypothesin in* Dissertatione de motu undulationis, *quæ est sexta pars meorum Mechanicorum; ut ponam, colores istos apparentes fieri ex sola illa Communicatione motionis, quæ ab Undulationibus directè procedentibus ad latera effundatur: Ut, si radii intrantes per foramen* a *progrediantur versùs* b, *undulationes quidem directè terminari deberent (habendo rationem ad motum rectum & naturalem) ad lineam rectam* a b; *nihilominus tamen, propter continuitatem materiæ, fit aliqua communicatio commotionis versus latera* c c, *ubi tremula quadam & crispans succussio excitatur: Atque si in illa laterali crispatione consistere colores supponatur, existimo omnia phænomena colorum explicari posse, ut fusius in ea, quam dixi, Dissertatione expono. Quibus item positis apparet etiam, cur ultra quàm ferat radiorum ipsorum divaricatio, expandi colorum latitudinem necesse sit. Verùm ista obiter hîc tantùm adnotasse sufficiat.*



Quod

(5013)

Quod annotat, errorem, qui oriri posset in calculo, ex eo, quod dixeram, veluti foramine facto in posteriori facie prismatis; errorem, inquam, illum non posse inducere sensibilem varietatem: id optimè annotatum est; neque ego existimavi, inde multum augeri colorum latitudinem, sed tantummodò accuratam calculi rationem indicare volui: Quapropter etiam & ego in praxi negligendam hanc cautionem censeo.

Circa Experimentum crucis, *nequaquam dubito, quo minus in suo experimento talem situm adhibuerit, in quo* æqualis *inclinatio fuerit Radiorum incidentium; quandoquidem id ita à se præstitum expressè affirmat. Verùm id non ego poteram conijcere ex iis quæ in* Transactionibus *legeram; ubi ponuntur duo exigua & maximè distantia foramina, & unum Prisma prope* primum *foramen quod est in fenestra; per quod Prisma radij colorati erumpentes incidunt in alterum distans foramen. Addebatur autem, quòd ad hoc ut omnes illi radii successivè inciderent in* secundum *illud foramen, convertebatur primum Prisma supra axem: Atqui hoc modo necesse est mutari inclinationem radiorum qui incidunt in secundum foramen: atque indicavi ego in literis, quòd perinde se se res haberet, sive manente primo Prismate immobili, secundum foramen attolleretur aut deprimeretur, ut posset successivè radios omnes depictæ imaginis Solaris excipere; sive manente isto secundo foramine immobili, primum prisma converteretur, ut ita eadem imago situm mutaret, atque in foramen impingere secundùm omnes successivè partes posset. Sed alias sine dubio adhibuit cautiones solertissimus* Newtonus.

Quæ circa Colores *objeceram, optimè soluta existimo. Quod autem* Theoriam *istam, appellarim* Hypothesin, *id certè ego nullo adhibito consilio feci; atque nomen usurpavi quod primum occurrit: quapropter velim ut ne per contemptum adhibitam vocem ejusmodi existimet. Præclara sanè inventa semper ego magni feci, Clarissimum verò* Newtonum *imprimis suspicio ac veneror.*

Aaaaa 2 Mr. *Newtons*

Mr. *Newtons* Answer to the foregoing Letter.

IN *Observationibus R. Patris* J. Pardies, *quas ad te denuò conscripsit, an majus sit Humanitatis argumentum quòd meis responsionibus vim omnem attribuit; an Ingenii, quòd Objectiones proponit, quæ, si non probe tollantur, Doctrinam nostram frustrari possint, vix dixerim. Utrumque sanè ad determinandam veritatem optimè conducit, efficitque ut acceptis quàm lubentissimè respondeam.*

Ait R. P. quòd absque *varia diversorum radiorum refrangibilitate possibile sit explicare longitudinem colorum; puta ex Hypothesi P.* Grimaldi, *per diffusionem luminis, quod supponitur esse substantia quadam rapidissimè mota; vel ex Hypothesi* Hookii *nostri, per diffusionem vel expansionem Undulationum, quas statuit in æthere à lucidis corporibus excitatas quaquaversum propagari. Addo, quòd ex Hypothesi* Carteſiana *potest etiam effingi consimilis diffusio conatus vel pressionis globulorum, perinde ut in explicatione* Caudæ Cometæ *supponitur. Et eadem diffusio vel expansio juxta aliam quamvis Hypothesin, in qua lumen statuitur esse vis, actio, qualitas, vel substantia qualibet à luminosis corporibus undique emissa, effingi potest.*

Ut his respondeam, animadvertendum est, quòd Doctrina illa, quam de Refractione & Coloribus explicui, in quibusdam Lucis Proprietatibus *solummodo constitit, neglectis* Hypothesibus *per quas Proprietates illæ explicari debent. Optimus enim & tutissimus philosophandi modus videtur, ut imprimis rerum proprietates diligenter inquiramus, & per experimenta stabiliamus; ac dein tardius contendamus ad Hypotheses pro earum explicatione. Nam* Hypotheses *ad explicandas rerum proprietates tantùm accommodari debent, & non ad determinandas usurpari, nisi quatenus experimenta subministrare possint. Et siquis ex sola* Hypothesium *possibilitate de veritate rerum conjecturam faciat, non video quo pacto quicquam certi in ulla scientia determinare possit; siquidem alias atque alias Hypotheses semper liceat excogitare, quæ novas difficultates suppeditare videbuntur. Quamobrem ab* Hypothesium *contemplatione, tanquam improprio argumentandi loco, hîc abstinendum esse censui, & vim Objectionis abstrahendam, ut pleniorem & magis generalem responsionem accipiat.*

Itaque per Lumen *intelligo quodlibet Ens vel entis potestatem (sive sit substantia, sive quævis ejus vis, actio, vel qualitas) quod à corpore lucido rectà pergens aptum sit ad excitandam visionem; & per* radios *Luminis intelligo minimas vel quaslibet indefinitè parvas ejus partes, quæ ab invicem non dependent; quales sunt illi omnes radii, quos lucentia corpora vel simul vel successivè secundùm rectas lineas emittunt. Nam illæ tum collaterales tum successivæ partes luminis sunt independentes; siquidem unæ absque aliis intercipi possint, & in quaslibet plagas seorsim reflecti vel refringi. Et hoc præcognito, Objectionis vis omnis in eo sita erit; Quòd colores per aliquam Luminis ultra foramen diffusionem, quæ non oritur ab inæ-*

quali

quali diversorum radiorum (seu luminis independentium partium) refrangibilitate, in longum diduci possint.

Quòd autem non aliunde oblongentur, monstravi in Literis relatis in Phil. Transactionibus, Num. 80. *Et ut rationes facilius percipiantur, non gravabor jam fusius explicare.*

Scilicet ex observatione, quòd radii post refractionem non incurvabantur, sed rectâ ad parietem progressi fuêre, patuit, eandem fuisse eorum ad se mutuò inclinationem cùm modò exiêrunt Prismate, atque cùm impegerunt in parietem; & proinde Longitudo colorum ex inclinatione radiorum emersit quam inter refringendum obtinuêre, hoc est, ex quantitate refractionis quam singuli radii in Prismate patiebantur: Adeóque cùm colorum longitudo latitudinem aliquot vicibus ex observatione superavit, sequitur, majorem fuisse inaequalitem refractionum quàm potuit oriri ex inaequalitate incidentiarum. Quin imò ex figura imaginis coloratae, quòd nempe non fuit Ovalis, sed ad latera duabus parallelis rectis lineis terminata, patuit, eam ex indefinitè multis imaginibus Solis, per inaequalem refractionem in longum distractis, & serie continuâ dispositis, constitui; adeoque radios à singulis partibus solaris Disci provenientes per totam ferè longitudinem colorum dispergi; & proinde similiter incidentium inaequales esse refractiones. Id quod aliis etiam indiciis ostendi posset.

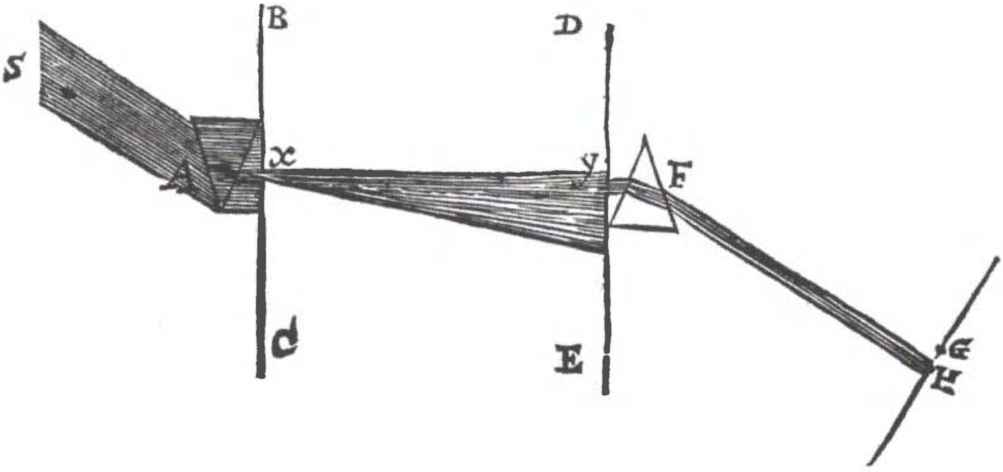
Constat itaque diversas esse refractiones, ubi pares sunt incidentiae. Sed amplius inquirendum est, Unde oriatur illa diversitas; An sit à causa aliqua incerta & irregulari, vel certâ lege, secundùm quam radius quilibet aptus est determinatam aliquam refractionem pati. Per incertas & irregulares causas intellige asperitates in superficie, vel venas diversae densitatis in interiori parte vitri ex quo Prisma conflatur; item irregularem situm pororum, quos nonnulli ob luminis transmissionem directo tramite per vitrum omnifariam traijci statuunt; nec non tremores & inaequales commotiones partium aetheris, aëris, vel vitri; radiorum in refringente superficie se mutuò fortasse comprimentium resulturam ab invicem; ejusdem cujusque radii divisionem ac dissipationem in partes divergentes, quas vel numero finitas vel indefinitè multas in superficie aliquâ continuatim jacentes imaginari liceat; vel quamvis aliam diffusionem & dilatationem Luminis quam possumus excogitare, non ortam ex diversa praedispositione cujusque radii ad refractionem, in certo aliquo & constanti gradu patiendam.

Quod autem diversa refractio non orta sit ex ullis ejusmodi causis incertis & irregularibus, probavi per Experimentum duorum consimilium Prismatum in contrario situ juxta-positorum, ita ut posterius contrariâ suâ refractione retro-flecteret radios, & sic regulares effectus prioris destrueret, sed per iteratas refractiones augeret irregulares. Utpote si prius Prisma diffunderet ac divergere faceret parallelos radios; e.g. per asperam polituram, inaequabilem densitatem, aut irregularem situm pororum Prismatis; vel per tremulos motus partium aetheris, aëris aut vitri; vel per dilatationem luminis propter partium ejus (i.e. radiorum) se mutuò comprimentium relaxationem versùs adjacentia spatia, quae vel nullo vel minùs constipato lumine irradiantur;

vel

vel denique per cujusque radii dilatationem aut diffractionem in complures divergentes radios: tum sanè posterius Prisma magis diffunderet ac dissiparet radios per dictas irregularitates ætheris, aëris, aut vitri, vel per iteratam dilatationem luminis a refringentis superficiei resistentia denuò constipati ac diffusi, vel etiam per cujusque radii a priori diffractione orti iteratam diffractionem ac divisionem in longè plures divergentes radios. Et sic Lumen magis dispergeretur per refractionem secundi Prismatis, & in parietem projectam Imaginem duplo longiorem minimùm exhiberet, quàm per solam refractionem prioris Prismatis exhiberi potuisset. Quamobrem cùm, experientiâ teste, refractio secundi Prismatis adeò non dispergat lumen ut contrahat & in pristinum statum reducat, efficiatque ut in forma Coni postea progrediatur, perinde ac si nullam omnino refractionem passum fuisset; concedendum est, Diffusionem Luminis, à refractione anterioris Prismatis effectam, non oriri ab aliqua præfatarum causarum, aut alia quavis irregularitate, sed diversæ refrangibilitati diversorum radiorum solummodo tribuendam esse; utpote quâ radius unusquisque, ex insita dispositione tantam refractionem in posteriori Prismate ac in priori passus, reducitur in parallelismum cum seipso; & sic omnes radii ad se mutuò easdem inclinationes resumunt quas ante refractiones habuêre.

Demum, ut hæc omnia summè confirmarem, adjeci Experimentum illud quod jam nomine Crucis *passim insignitur: de cujus conditionibus cum R. P. dubitaverit, placuit jam designare Schemate. Sit B C anterior tabula, cui Prisma A immediate præfigitur, sitque D E altera tabula, quasi duodecim pedibus abinde distans, cui suffigitur alterum Prisma F. Tabulæ autem ad* x *&* y *ita perforentur, ut aliquantulum lucis ab anteriori Prismate refractæ,*



traijci possit per utrumque foramen ad secundum Prisma, inque eo denuò refringi. Jam Prisma anterius circa axem reciproco motu convertatur, & colores in Tabulam posteriorem D E procidentes, per vices attollentur ac deprimentur, eoque pacto alius atque alius color successivè pro arbitrio traijci potest per foramen ejus y *ad posterius Prisma, dum cæteri colores in Tabulam impingunt: Et videbis, radios diversis coloribus præditos diversam pati refractionem*

(5017)

nem in illo posteriori Prismate, ex eo quòd ad diversa loca parietis vel cujusvis obstaculi G H, pedibus aliquot alterius remoti, allabentur; puta violacei *radii ad H,* rubri *ad G, &* intermedii *ad loca intermedia: & tamen propter determinatam positionem foraminum necesse est ut similis sit incidentia radiorum cujusque coloris per utrumque trajecti. Atque ita ex mensura constat radios, diversis coloribus affectos, habere diversas leges refractionum.*

Sed suspicor unde adductus sit R.P. in dubitationem; nempe videtur collocasse primum *Prisma A* post *Tabulam B C, atque ita convertendo circa Axem, verisimile est inclinationem radiorum qui interjacent foramina propter intermediam refractionem fuisse mutatam. At ex descriptione expositâ in* Phil. Transactionibus *debuit Tabula illa collocari* post *Prisma, ut radii inter foramina in directum jacerent, quemadmodum ex verbis;* I took two Boards and placed one of them close behind the Prism at the Window *, *constare potest. Et usus Experimenti idem innuit.*

* *Vid. Num.*80. *p.*3078. *quæ verba Latinè ita sonant;* Capiebam duas Tabulas ligneas, unamque earum immediatè collocabam *post* Prisma ad fenestram.

Ex abundanti placet observare, quòd in hoc Experimento colorata Lux ob refractionem secundi Prismatis longe minùs diffunditur ac divaricat, quàm cùm alba existit, adeò ut imago ad G vel H sit penè circularis; præsertim si Prismata statuantur parallela & in contrario situ angulorum, prout in Schemate designantur. Quinetiam, si præterea diameter foraminis y *adæquet latitudinem colorum, nulla erit ejusdem coloratæ lucis in longum diffusio; sed imago, quæ à quopiam colore ad G vel H effingitur, (positis circularibus foraminibus, & refractione posterioris Prismatis non majori quàm prioris, radiisque ad obstaculum quàm proximè perpendicularibus,) erit planè circularis. Id quod arguit diffusionem, de qua supra egimus, non ex contagione vel continuitate materiæ undulantis aut celerrimè motæ vel similibus causis ortam esse, sed ex certa refractionum cujusque generis radiorum lege. Cur autem Imago illa in uno casu sit circularis, & in aliis nonnihil oblongata, & quomodo diffusio lucis in longitudinem in quolibet casu pro arbitrio minui possit, à Geometris determinandum & cum experientia conferendum relinquo.*

Postquam Proprietates Lucis *his & similibus experimentis satis exploratæ fuerint, spectando radios tanquam ejus sive collaterales sive successivas partes, de quibus experti simus per independentiam quod sint ab invicem distinctæ; Hypotheses exinde dijudicandæ sunt, & quæ non possunt conciliari reijciendæ. Sed levissimi negotii est, accommodare Hypotheses ad hanc Doctrinam. Nam siquis Hypothesin* Cartesianam *defendere velit, dicendum est, globulos esse inæquales; vel pressiones globulorum esse alias aliis fortiores, & inde diversimodè refrangibiles, & aptas ad excitandam sensationem diversorum colorum. Et sic juxta Hypothesin* Cl. Hookii *dicendum est, Undulationes ætheris esse alias majores sive crassiores aliis. Atque ita in cæteris. Hæc enim videtur esse summè necessaria Lex & Conditio* Hypothesium, *in quibus Naturalia corpora ponuntur constare ex quàm plurimis corpusculis acervatim contextis, ut à diversis lucentium corpusculis, vel ejusdem corpusculi diversis partibus (prout motu, figurâ, mole, aut aliis qualitatibus differunt) inæquales pressiones, motiones*

aut

(5018)

aut mota corpuscula per æthera quaquaversum traijciantur, ex quibus, confusè mistis, lux constitui supponetur. Et nihil durius esse potest in istis Hypothesibus quàm contraria suppositio.

Ex apertura sive dilatatione Lucis in posteriori facie Prismatis, quam R. P. dixit esse veluti foramen, sufficit, quod error non emerget sensibilis si modò aliquis emergeret. Quòd si calculus juxta Observationes præcisè ineatur, error erit nullus. Nam diametro foraminis à longitudine Imaginis subductâ, restabit longitudo quam Imago haberet si modò foramen ante Prisma esset indivisibile, idque non obstante præfatâ lucis dilatatione in posteriori facie Prismatis; ut facile ostenditur. Deinde ex data illa longitudine Imaginis, ac distantia à foramine indivisibili, ut & positione & forma Prismatis, & ad id inclinatione incidentium radiorum, ac angulo, quem refracti radii, ad medium Imaginis tendentes, cum à centro Solis incidentibus constituunt, cætera omnia determinantur. Et quæ determinant refractiones & positiones radiorum, sufficiunt ad calculum istarum refractionum ritè ineundum. Sed res non tanti esse videtur ut moram inferat.

Quòd R.P. Doctrinam nostram Hypothesin *vocaverit, non aliunde factum esse credo quàm quòd vocabulum usurpavit quod primùm occurrit; siquidem mos obtinuit ut quicquid exponitur in Philosophia dicatur Hypothesis. Et ego sane non alio consilio vocabulum istud reprehendi quàm ut nè invalesceret appellatio quæ rectè Philosophantibus præjudicio esse posset. R. Patris verò candor in omnibus conspicitur; indeque modus efferendi Benevolentiam, qui mihi minimè convenit. Quod tamen nostra non displicent, vehementer gaudeo. Vale. Dab.* Cantabrig. 11mo Junii 1672.

Hæc responsio ad R.P. Ignatium Pardies *mox transmissa id effecit, ut ille die* 9. Julii 1672. *rescriberet Gallicè in hunc sensum*;

Omnino mihi satisfecit novissima responsio, à Dn. *Newtono* ad meas Instantias data. Novissimus scrupulus, qui mihi hærebat circa *Experimentum Crucis*, penitus fuit exemptus. Atque nunc planè ex Figura ipsius intelligo quod non intellexeram ante. Experimentum peractum cùm fuerit isto modo, nil habeo quod in eo desiderem ampliùs. Rem mihi pergratam feceris, si ipsi singularem meum ingenii & doctrinæ ejus cultum contesteris, & pro illo studio maximas gratias agas, quo voluit Annotationes meas examinare iisque respondere. Præter existimationem illam, quam jam ante de acumine ejus conceperam, affectus hic officiosus magnopere me ipsi devinxit.

A second Letter of P. Pardies, written to the Editor from Paris, May 21, 1672, to Mr. Newton's Answer made to his first Letter, printed in N°. 84. N°. 85, p. 5012. Translated from the Latin.

I have received your letter, with the observations of the very ingenious Mr. Newton, in which he answers my difficulties, which I have read with great pleasure. And first, with respect to that experiment of the greater breadth of the colours than what is required by the common theory of refractions; I confess that I supposed the refractions at the opposite sides of the prism unequal, till informed by the letter in the Transactions, that the greater breadth was observed by Newton in that case in which the refractions are supposed reciprocally equal, in the manner mentioned in those observations. But since I now see that it was in that case that the greater breadth of the colours was observed, on that head I find no further difficulty. I say on that head; for the greater length of the image may be otherwise accounted for, than by the different refrangibility of the rays. For according to that hypothesis, which is explained at large by Grimaldi, and in which it is supposed that light is a certain substance very rapidly moved, there may take place some diffusion of the rays of light after their passage and decussation in the hole. Also on that other hypothesis, in which light is made to proceed by certain undulations of a subtile matter, as explained by Mr. Hook, colours may be explained by a certain diffusion and expansion of the undulations, made on the sides of the rays beyond the hole by

that there is no other channel by which the chyle is conveyed into the blood than that of the thoracic duct, which generally opens into the left subclavian vein at the angle formed by it and the internal jugular vein. Sometimes however it is inserted directly into the internal jugular.]

the influence and continuation of the subtile matter. Indeed I admit such an hypothesis in "the Dissertation on the Motion of Undulation," which is the sixth part of my mechanics, as I suppose that those apparent colours are the sole effect of that communication of motion which is diffused laterally by the direct undulations. As if the rays entering by the hole *a*, (fig. 9, pl. 15) should proceed towards *b*, the undulations ought indeed to terminate directly, with regard to their direct and natural motion, at the right line *ab*; yet nevertheless, because of the continuity of the matter, there is some communication of the motion towards the sides *cc*, where it becomes tremulous and undulatory. And if colours be supposed to consist in the lateral undulation, all their phænomena may be explained in this manner, as I have shown in the dissertation beforementioned; by which also the reason will appear, why the breadth of the colours must be expanded beyond the divergency of the rays themselves.

As to what he says of the error, which might arise in the calculation, from what I mentioned like a hole made in the posterior face of the prism, that that error could not cause any sensible variation; his remark is very proper: neither have I judged that hence the breadth of the colours would be much increased, but I wished only to indicate an accurate mode of calculation: and therefore I also think this caution may be neglected in practice.

As to the Experimentum Crucis, I make no doubt that the incident rays had an equal inclination, since the author expressly affirms it. But that is what I could not gather from what I read in the Transactions; where it is stated, that there are two small and very distant holes, and one prism near the first hole in the window; through which prism the coloured rays escaping, fall on the other distant hole. And it is added, that the first prism was turned round its axis, to cause all the rays to fall successively on the second hole. Now in this case the inclination of the rays which fall on the second hole, must necessarily be changed: and I hinted in my letter, that it would be the same thing, whether the second hole were raised or depressed, for all the rays pointing to the sun's image, to fall successively on it, while the first prism was invariable; or whether, the second hole being immoveable, the first prism were turned round, so that the same image might change its situation, and all its parts successively fall on the second hole. But no doubt the sagacious Newton used other precautions.

As to what I objected about colours, I am well satisfied with the solutions. And as to my calling the author's theory an hypothesis, that was done without any design, having only used that word as first occurring to me; and therefore request it may not be thought as done out of any disrespect. I have always esteemed ingenious discoveries, and the excellent Newton I very highly admire and honour.

Mr. NEWTON'S *Answer to the foregoing Letter.* Nº 85, *p.* 5014. *Translated from the Latin.*

In the observations of the Rev. F. Pardies, one can hardly determine whether there is more of humanity and candour, in allowing my arguments their due weight, or penetration and genius in starting objections. And doubtless these are very proper qualifications in researches after truth. But to proceed, F. Pardies says, that the length of the coloured image can be explained, without having recourse to the divers refrangibility of the rays of light; as suppose by the hypothesis of F. Grimaldi, viz. by a diffusion of light, which is supposed to be a certain substance put into very rapid motion; or by Mr. Hook's hypothesis, by a diffusion and expansion of undulations; which, being formed in the æther by lucid bodies, is propagated every way. To which may be added the hypothesis of Descartes, in which a similar diffusion of *conatus*, or pression of the globules, may be conceived, like as is supposed in accounting for the tails of comets. And the same diffusion or expansion may be devised according to any other hypotheses, in which light is supposed to be a power, action, quality, or certain substance emitted every way from luminous bodies.

In answer to this, it is to be observed that the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypotheses, by which those properties might be explained. For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose, that the force of the objection should be abstractedly considered, and receive a more full and general answer.

By light therefore I understand, any being or power of a being, (whether a substance or any power, action, or quality of it, which proceeding directly from a lucid body, is apt to excite vision. And by the rays of light I understand its least or indefinitely small parts, which are independent of each other; such as are all those rays which lucid bodies emit in right lines, either successively or all together. For the collateral as well as the successive parts of light are independent; since some of the parts may be intercepted without the others, and be separately reflected or refracted towards different sides. This being premised, the whole force of the objection will lie in this, that colours may be lengthened out by some certain diffusion of light beyond the hole, which does not arise from the unequal refraction of the different rays, or of the independent parts of light. And that the image is no otherwise lengthened, was shown in my letter in Numb. 80 of the Transactions; and to confirm the whole in the strictest manner, I added that experiment now known by the name Experimentum Crucis; of the conditions of which, since the Rev. Father has some doubt, I have thought fit to represent it by a scheme. Let BC (fig. 10, pl. 15) then be the anterior board, to which the prism A is immediately prefixed, and let DE be the other board, at the distance of about 12 feet from the former, to which the other prism F is affixed. And let the boards be perforated at x and y in such a manner, that a little of the light refracted by the former prism may pass through both the holes to the second prism, and be there refracted again. Now let the former prism be turned about its axis with a reciprocal motion; then the colours falling on the latter board DE will be raised and depressed by turns: and thus the several colours may at pleasure be made to pass successively through the hole y to the latter prism, while all the other colours fall on the board. Then you will see that the said rays of different colours will be differently refracted at the latter prism, as they will be seen on different places of the opposite wall. or of any obstacle GH, at the distance of some feet from it; as suppose the violet rays at H, the red at G, and the intermediate rays at the intermediate places: and yet, because of the determinate position of the holes, the incidence of the rays of each colour through both must be similar. And thus it appears, by measuring, that the rays of different colours have different laws of refractions.

But I suspect what it was that caused the Rev. Father to doubt; viz. it seems he placed his first prism A behind the board BC, and thus by turning it about its axis, it is probable that the inclination of the rays intercepted between the two holes may have suffered some change by the intermediate refraction. But by the description before given in the Transactions, the first board ought to be placed after the prism, that the rays may pass in a straight direction between the holes, agreeably to my words; "I took two boards, and placed one of them close behind the prism at the window." And the design of the experiment requires the same thing.

It may be further observed, that in this experiment, because of the refraction of the second prism, the coloured light is much less diffused and less divergent, than when it is quite white, so that the image at G or H is nearly circular; especially if the prisms be placed parallel, and their angles in a contrary position, as in the present figure. And besides, if the diameter of the hole y be equal to the breadth of the colours, the coloured light will not be diffused lengthwise; but the image, which is formed by any colour at G or H, will be manifestly circular; supposing the holes to be circular, and the refraction of the latter prism not to be greater than that of the former, and the rays to be nearly perpendicular to the obstacle. This shows that the diffusion, above-mentioned, does not arise from the influence or continuity of the undulating matter, or matter put into a rapid motion, or any such like causes, but from a certain law of refractions for every species of rays. But why the image is in one case circular, and in others a little oblong, and how the diffusion of light lengthwise may in any case be diminished at pleasure, I leave to be determined by geometricians, and compared with experiments.

After the properties of light shall, by these and such like experiments, have been sufficiently explored, by considering its rays either as collateral or successive parts of it, of which we have found by their independence that they are distinct from one another; hypotheses are thence to be judged of, and those to be rejected which cannot be reconciled with the phænomena. But it is an easy matter to accommodate hypotheses to this doctrine. For if any one wish to defend the Cartesian hypothesis, he need only say that the globules are unequal, or that the pressures of some of the globules are stronger than others, and that hence they become differently refrangible, and proper to excite the sensation of different colours. And thus also according to Hook's hypothesis, it may be said, that some undulations of the æther are larger or denser than others. And so of the rest. For this seems to be the most necessary law and condition of hypotheses, in which natural bodies are supposed to consist of a multitude of corpuscules cohering together, and that from the different particles of lucid bodies, or from the different parts of the same corpuscule, (as they may happen to differ in motion, figure, bulk, or other qualities) unequal pressions, motions, or moved corpuscules, may be propagated every way through the æther, of the confused mixture of which light may be supposed to be constituted. And there can be nothing more difficult in these hypotheses than the contrary supposition.

As to that aperture or dilatation of the light in the posterior face of the prism, which the Rev. Father supposes to resemble a hole, it is sufficient that no sensible error can arise from it, if any at all. For if a calculation be made precisely according to the observations, the error will be found nothing. For by subtracting the diameter of the hole from the length of the image, there will remain that length which the image would have, if the hole before the prism were an indivisible point, and that notwithstanding the aforesaid dilatation of the light in the posterior face of the prism; as is easily shown. Then from that given length of the image, and its distance from the indivisible hole, as also from the position and form of the prism, and besides from the inclination of the incident rays, and from the angle which the refracted rays bending to the middle of the image make with those that are incident from the sun's centre, all other things may be determined. And the same data that determined the refractions and positions of the rays, are sufficient for an accurate calculation of these refractions. But this matter seems not to be of importance enough to be much regarded.

As to the Rev. Father's calling our doctrine an hypothesis, I believe it only proceeded from his using the word which first occurred to him, as a practice has arisen of calling by the name hypothesis whatever is explained in philosophy: and the reason of my making exception to the word, was to prevent the prevalence of a term, which might be prejudicial to true philosophy.

The above answer being sent to the Rev. Father Ig. Pardies, he returned his acknowledgement in a note as below.

I am quite satisfied with Mr. Newton's new answer to me. The last scruple which I had, about the Experimentum Crucis, is fully removed. And I now clearly perceive by his figure what I did not before understand. When the experiment was performed after his manner, every thing succeeded, and I have nothing further to desire.

Mr. HOOKE's confiderations upon Mr. NEWTON's difcourfe on light and colours were read. Mr. HOOKE was thanked for the pains taken in bringing in fuch ingenious reflections; and it was ordered, that this paper fhould be regiftred [a], and a copy of it immediately fent to Mr. NEWTON: and that in the mean time the printing of Mr. NEWTON's difcourfe by itfelf might go on, if he did not contradict it; and that Mr. HOOKE's paper might be printed afterwards, it not being thought fit to print them together, left Mr. NEWTON fhould look upon it as a difrefpect, in printing fo fudden a refutation of a difcourfe of his, which had met with fo much applaufe at the Society but a few days before.

Mr. HOOKE's paper was as follows:

" I have perufed the difcourfe of Mr. NEWTON about colours and refractions,
" and I was not a little pleafed with the niceness and curiofity of his obfervations.
" But, tho' I wholly agree with him as to the truth of thofe he hath alledged,

[r] Letter-book, vol. v. p. 155. [a] Regifter, vol. iv. p. 148.

" as

" as having, by many hundreds of trials, found them so; yet as to his hypo-" thesis of solving the phenomæna of colours thereby, I confess, I cannot see yet " any undeniable argument to convince me of the certainty thereof. For all " the experiments and observations I have hitherto made, nay, and even those " very experiments, which he alledgeth, do seem to me to prove, that *white* " is nothing but a pulse or motion, propagated through an homogeneous, uni-" form, and transparent medium: and that colour is nothing but the disturb-" ance of that light, by the communication of that pulse to other transparent me-" diums, that is, by the refraction thereof: that *whiteness* and *blackness* are no-" thing but the plenty or scarcity of the undisturbed rays of light: and that " the two colours (than the which there are not more uncompounded in nature) " are nothing but the effects of a compounded pulse, or disturbed propagation " of motion caused by refraction.

" But, how certain soever I think myself of my hypothesis (which I did not take " up without first trying some hundreds of experiments) yet I should be very glad " to meet with one *experimentum crucis* from Mr. NEWTON, that should divorce me " from it. But it is not that, which he so calls, will do the turn; for the same phæ-" nomenon will be solved by my hypothesis, as well as by his, without any man-" ner of difficulty or straining: nay, I will undertake to shew another hypothesis, " differing from both his and mine, that shall do the same thing.

" That the ray of light is as it were split or rarified by refraction, is most cer-" tain; and that thereby a differing pulse is propagated, both on those sides, and " in all the middle parts of the ray, is easy to be conceived: and also, that differ-" ing pulses or compound motions should make differing impressions on the eye, " brain, or sense, is also easy to be conceived: and that, whatever refracting me-" dium does again reduce it to its primitive simple motion by destroying the ad-" ventitious, does likewise restore it to its primitive whiteness and simplicity.

" But why there is a necessity, that all those motions, or whatever else it be " that makes colours, should be originally in the simple rays of light, I do not " yet understand the necessity of, no more than that all those sounds must be in " the air of the bellows, which are afterwards heard to issue from the organ-" pipes; or in the string, which are afterwards, by different stoppings and strik-" ings produced; which string (by the way) is a pretty representation of the shape " of a refracted ray to the eye; and the manner of it may be somewhat imagined " by the similitude thereof: for the ray is like the string, strained between the " luminous object and the eye, and the stop or fingers is like the refracting sur-" face, on the one side of which the string hath no motion, on the other a vi-" brating one. Now we may say indeed and imagine, that the rest or streight-" ness of the string is caused by the cessation of motions, or coalition of all vi-" brations; and that all the vibrations are dormant in it: but yet it seems more " natural to me to imagine it the other way.

" And I am a little troubled, that this ſuppoſition ſhould make Mr. NEWTON " wholly lay aſide the thoughts of improving teleſcopes and microſcopes by re- " fractions; ſince it is not improbable, but that he, that hath made ſo very good an " improvement of teleſcopes by his own trials upon reflection, would, if he had " proſecuted it, have done more by refraction. And that reflection is not the " only way of improving teleſcopes, I may poſſibly hereafter ſhew ſome proof " of. The truth is, the difficulty of removing that inconvenience of the ſplit- " ting of the ray, and conſequently of the effect of colours, is very great; but " yet not inſuperable. I have made many trials, both for teleſcopes and mi- " croſcopes by reflection, which I have mentioned in my Micrographia, but de- " ſerted it as to teleſcopes, when I conſidered, that the focus of the ſpherical con- " cave is not a point but a line, and that the rays are leſs true reflected to a " point by a concave, than refracted by a convex; which made me ſeek that by " refraction, which I found could not rationally be expected by reflection: nor " indeed could I find any effect of it by one of ſix foot radius, which, about ſe- " ven or eight years ſince, Mr. REEVE made for Mr. GREGORY, with which I " made ſeveral trials; but it now appears it was for want of a good encheiria " (from which cauſe many good experiments have been loſt) both which conſi- " derations diſcouraged me from attempting further that way; eſpecially ſince I " found the parabola much more difficult to deſcribe, than the hyperbola or el- " lipſis. And I was wholly taken from the thoughts of it, by lighting on divers " ways, which in theory anſwered all I could wiſh for; tho' having much more " buſineſs, I could not attend to bring them into uſe for teleſcopes; tho' for mi- " croſcopes I have for a good while uſed it. Thus much as to the preamble; I " ſhall now conſider the propoſitions themſelves.

" Firſt then, Mr. NEWTON alledgeth, that as the rays of light differ in re- " frangibility, ſo they differ in their diſpoſition to exibit this or that colour: " with which I do in the main agree; that is, that the ray by refraction is, as it " were, ſplit or rarified, and that the one ſide, namely that which is moſt refracted, " gives a *blue*, and that which is leaſt a *red*: the intermediate are the dilutings " and intermixtures of thoſe two, which I thus explain. The motion of light in " an uniform medium, in which it is generated, is propagated by ſimple and " uniform pulſes or waves, which are at right angles with the line of direction; " but falling obliquely on the refracting medium, it receives another impreſſion " or motion, which diſturbs the former motion, ſomewhat like the vibration of a " ſtring: and that, which was before a line, now becomes a triangular ſuperfi- " cies, in which the pulſe is not propagated at right angles with its line of direc- " tion, but aſcew, as I have more at large explained in my Micrographia; and " that, which makes excurſions on the one ſide, impreſſes a compound motion on " the bottom of the eye, of which we have the imagination of *red*; and that, " which makes excurſions on the other, cauſes a ſenſation, which we imagine a " *blue*; and ſo of all the intermediate dilutings of thoſe colours. Now, that the " intermediate are nothing but the dilutings of thoſe two primary, I hope I have " ſufficiently proved by the experiment of the two wedge-like boxes, deſcribed " in my Micrographia. Upon this account I cannot aſſent to the latter part of

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" the propofition, that colours are not qualifications of light, derived from refrac-" tions, or refections of natural bodies, but original and connate properties, &c.

" The fecond propofition I wholly allow, not exactly in the fenfe there meant, " but with my manner of expreffing it; that is, that part of the fplit ray, which is " moft bent, exhibits a blue, that which is leaft, a red, and the middle parts midling " colours; and that thofe parts will always exhibit thofe colours till the com-" pound motions are deftroyed, and reduced by other motions to one fimple and " uniform pulfe as it was at firft.

" And this will eafily explain and give a reafon of the phænomena of the third " propofition, to which I do readily affent in all cafes, except where the fplit ray " is made by another refraction, to become intire and uniform, again to diverge " and feparate, which explains his fourth propofition.

" But as to the fifth, that there are an indefinite variety of primary or original " colours, amongft which are yellow, green, violet, purple, orange, &c. and " an infinite number of intermediate gradations, I cannot affent thereunto, as " fuppofing it wholly ufelefs to multiply entities without neceffity, fince I have " elfewhere fhewn, that all the varieties of colours in the world may be made " of two. I agree in the fixth, but cannot approve of his way of explicating " the feventh. How the fplit ray being made doth produce a clear and uniform " light, I have before fhewed; that is, by being united thereby from a fuperfi-" cial motion, which is fufceptible of two, to a lineary, which is fufceptible of " one only motion; and it is as eafy to conceive how all thofe motions again ap-" pear after the rays are again fplit or rarified. He, that fhall but a little confider " the undulations on the furface of a fmall river of water, in a gutter, or the " like, will eafily fee the whole manner curioufly exemplified.

" The eighth propofition I cannot at all affent to, for the reafons above; and " the reafons of the blue flame of brimftone, of the yellow of a candle, the " green of copper, and the various colours of the ftars, and other luminous bo-" dies, I take to proceed from quite another caufe, eafily explained by my for-" mer hypothefis.

" I agree with the obfervations of the ninth, tenth, and eleventh, though not " with his theory, as finding it not abfolutely neceffary, being as eafily and na-" turally explained and folved by my hypothefis.

" The reafon of the phænomena of my experiment, which he alledgeth, is " as eafily folvable by my hypothefis as by his; as are alfo thofe, which are men-" tioned in the thirteenth. I do not therefore fee any abfolute neceffity to be-" lieve his theory demonftrated, fince I can affure Mr. Newton, I cannot only " folve all the phænomena of light and colours by the hypothefis I have for-" merly printed, and now explicate them by, but by two or three other very dif-

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“ ſering from it, and from this, which he hath deſcribed in his ingenious diſ-
“ courſe.

“ Nor would I be underſtood to have ſaid all this againſt his theory, as it is
“ an hypotheſis; for I do moſt readily agree with them in every part thereof, and
“ eſteem it very ſubtil and ingenious, and capable of ſolving all the phænomena
“ of colours: but I cannot think it to be the only hypotheſis, nor ſo certain as
“ mathematical demonſtrations.

“ But grant his firſt propoſition, that light is a body, and that as many co-
“ lours as degrees thereof as there may be, ſo many ſorts of bodies there may
“ be, all which compounded together would make white; and grant further,
“ that all luminous bodies are compounded of ſuch ſubſtances condenſed, and
“ that whilſt they ſhine, they do continually ſend out an indefinite quantity there-
“ of, every way in orbem, which in a moment of time doth diſperſe itſelf to the
“ utmoſt and moſt indefinite bounds of the univerſe; granting theſe, I ſay, I
“ do ſuppoſe there will be no great difficulty to demonſtrate all the reſt of his
“ curious theory: though yet, methinks, all the coloured bodies in the world
“ compounded together ſhould not make a white body, and I ſhould be glad
“ to ſee an experiment of that kind done on the other ſide. If my ſuppoſition
“ be granted, that light is nothing but a ſimple and uniform motion, or pulſe
“ of a homogeneous and adopted (that is a tranſparent) medium, propagated from
“ the luminous body in orbem, to all imaginable diſtances in a moment of time,
“ and that that motion is firſt begun by ſome other kind of motion in the lu-
“ minous body; ſuch as by the diſſolution of ſulphureous bodies by the air, or
“ by the working of the air, or the ſeveral component parts one upon another,
“ in rotten wood, or putrifying fiſh, or by an external ſtroke, as in diamond, ſu-
“ gar, the ſea-water, or two flints or cryſtal rubbed together; and that this
“ motion is propagated through all bodies ſuſceptible thereof, but is blended or
“ mixt with other adventitious motions, generated by the obliquity of the ſtroke
“ upon a refracting body; and that, ſo long as thoſe motions remain diſtinct in
“ the ſame part of the medium or propagated ray, ſo long they produce the ſame
“ effect, but when blended by other motions, they produce other effects: and
“ ſuppoſing, that by a direct contrary motion to the newly impreſſed, that ad-
“ ventitious one be deſtroyed and reduced to the firſt ſimple motion; I believe
“ Mr. NEWTON will think it no difficult matter, by my hypotheſis, to ſolve all the
“ phænomena, not only of the priſm, tinged liquors, and ſolid bodies, but of
“ the colours of plated bodies, which ſeem to have the greateſt difficulty. It
“ is true, I can, in my ſuppoſition, conceive the white or uniform motion of
“ light to be compounded of the compound motions of all the other colours,
“ as any one ſtrait and uniform motion may be compounded of thouſands of
“ compound motions, in the ſame manner as DESCARTES explicates the reaſon
“ of the refraction; but I ſee no neceſſity of it. If Mr. NEWTON hath any
“ argument, that he ſuppoſes an abſolute demonſtration of his theory, I ſhould be

" very glad to be convinced by it, the phænomena of light and colours being, in
" my opinion, as well worthy of contemplation, as any thing elfe in the world."

(5084)

Mr. Isaac Newtons *Answer to some Considerations upon his Doctrine of* Light *and* Colors; *which Doctrine was printed in* Numb. 80. *of these Tracts.*

SIR, I have already told you, that at the perusal of the considerations, you sent me, on my Letter concerning *Refractions* and *Colors*, I found nothing, that, as I conceived, might not without difficulty be answer'd. And though I find the *Considerer* somewhat more concern'd for an *Hypothesis*, than I expected; yet I doubt not, but we have one common design; I mean, a sincere endeavour after knowledge, without valuing uncertain speculations for their subtleties, or despising certainties for their plainness: And on confidence of this it is, that I make this return to his discourse.*

* *Which* Discourse *was thought needless to be here printed at length, because in the body of this Answer are to be met with the chief particulars, wherein the* Answerer *was concern'd.*

1. *Of the Practique part of Optiques.*

The *first* thing that offers it self is less agreeable to me, and I begin with it because it is so. The considerer is pleased to reprehend me for laying aside the thoughts of improving Optiques by *Refractions.* If he had obliged me by a private Letter on this occasion, I would have acquainted him with my successes on the Tryals I have made of that kind, which I shall now say have been less than I sometimes expected, and perhaps than he at present hopes for. But since he is pleased to take it for granted, that I have let this subject pass without due examination, I shall refer him to my former Letter, * by which that conjecture will appear to be un-grounded. For, what I said there, was in respect of Telescopes of the ordinary construction, signifying, that their improvement is not to be expected from the *well-figuring* of Glasses, as Opticians have imagin'd; but I despaired not of their improvement by other constructions; which made me cautious to insert nothing that might intimate the contrary. For, although successive refractions that are all made the same way, do necessarily more and more augment the errors of the first refraction; yet it seem'd not impossible for *contrary* refractions so to correct each others inequalities, as to make their difference regular; and, if that could

* *Printed in* Numb. 80. *of these Tracts.*

could be conveniently effected, there would be no further difficulty. Now to this end I examin'd, what may be done not only by *Glasses alone*, but more especially by a Complication of divers successive *Mediums*, as by two or more Glasses or Crystals with Water or some other fluid between them; all which together may perform the office of *one Glass*, especially of the Object-glass, on whose construction the perfection of the instrument chiefly depends. But what the results in Theory or by Tryals have been, I may possibly find a more proper occasion to declare.

To the Assertion, that Rays are less true *reflected* to a point by a *Concave*, than *refracted* by a Convex, I cannot assent; nor do I understand, that the *focus* of the latter is less a line than that of the former. The truth of the contrary you will rather perceive by this following Table, computed for such a *Reflecting Concave*, and *Refracting convex*, on supposition that they have equal Apertures, and collect parallel rays at an equal distance from their *vertex*; which distance being divided into 15000 parts, the Diameter of the Concave Sphere will be 60000 of those parts, and of the Convex, 10000; supposing the *Sines* of Incidence and Refraction to be, in round numbers, as 2 to 3. And this Table shews, how much the exterior rays, at several Apertures, fall short of their principal *focus*.

The Diameter of the Aperture.	*The parts of the Axis intercepted between the* vertex *and the rays.* *Reflected.*	*Refracted.*	*The Error by* *Reflexion.*	*Refraction.*
2000	$14991\frac{2}{3}$	14865	$8\frac{1}{3}$	135 .
4000	14966	14449	33	551 .
6000	14924	13699	76	1301 .
8000	14865	12475	135	2575 :
10000	14787	9472	213	5528 .

By this you may perceive, that the Errors of the *Refracting convex* are so far from being *less*, that they are more than sixteen times greater than the like errors of the *Reflecting Concave*, especially in great Apertures; and that without respect to the Heterogeneous constitution of light. So that, however the contrary supposition might make the Author of these Animadversions reject *Reflections* as useless for the promoting of Op-

 tiques

tiques ; yet I muſt for this as well as other conſiderations pre fer them in the Theory before *Refractions.*

Whether the *Parabola* be more difficult to deſcribe than the *Hyperbola* or *Ellipſis*, may be a *Quære* : But I ſee no abſolute neceſſity of endeavouring after any of their deſcriptions. For, if Metals can be ground truly Spherical, they will bear as great Apertures, as I believe men will be well able to communicate an *exact* poliſh to. And for Dioptrique Teleſcopes, I told you, that the difficulty conſiſted not in the Figure of the glaſs, but in the Difformity of Refractions : Which if it did not, I could tell you a better and more eaſie remedy than the uſe of the *Conic Sections*.

2. Of the Theorique part.

Thus much concerning the *Practique* part of Optiques. I ſhall now take a view of the Conſiderations on my *Theories*. And thoſe conſiſt in aſcribing an *Hypotheſis* to me, which is not mine; in Aſſerting an *Hypotheſis*, which, as to the principal parts, is not againſt me ; in Granting the greateſt part of my diſcourſe if explicated by that *Hypotheſis* ; and in Denying ſome things, the truth of which would have appear'd by an experimental examination.

3. Of an Hypotheſis miſtaken to be mine.

Of theſe Particulars I ſhall diſcourſe in order. And firſt of the *Hypotheſis*, which is aſcribed to me in theſe words : *But grant his firſt ſuppoſition, that light is a body, and that as many colours or degrees as there may be, ſo many bodies there may be ; all which componnded together would make White*, &c. This, it ſeems, is taken for my *Hypotheſis*. 'Tis 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 ſomewhat wonder, how the *Objector* could imagine, that, when I had aſſerted the Theory with the greateſt rigour, I ſhould be ſo forgetful as afterwards to aſſert the fundamental ſuppoſition it ſelf with no more than a *perhaps*. Had I intended any ſuch *Hypotheſis*, I ſhould ſomewhere have explain'd it. But I knew, that the *Properties*, which I declar'd of *Light*, were in

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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 to ſpeak of *Light* in *general* terms, conſidering it abſtractly, as ſomething or other propagated every way in ſtreight lines from luminous bodies, without determining, what that Thing is; whether a confuſed Mixture of difform qualities, or Modes of bodies, or of Bodies themſelves, or of any Virtues, Powers, or Beings whatſoever. And for the ſame reaſon I choſe to ſpeak of *Colours* 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 Light, making various Mechanical impreſſions on the Organ of Senſe; as I expreſſed it in that place, where I ſpake of the Corporeity of Light.

But ſuppoſing I had propounded that *Hypotheſis*, I underſtand not, why the Objector ſhould ſo much endeavour 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 the *Æther* being as uſeful and neceſſary in *this*, as in *his*. For, aſſuming the Rays of Light to be ſmall bodies, emitted every way from Shining ſubſtances, thoſe, when they impinge on any Refracting or Reflecting ſuperficies, muſt as neceſſarily excite Vibrations in the *æther*, 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-beams, the Emiſſion of Light from burning putrifying, or other ſubſtances, whoſe parts are vehemently agitated, the *Phænomena* of thin tranſparent Plates and Bubles, and of all Natural bodies, the Manner of Viſion, and the Difference of Colors, as alſo their Harmony and Diſcord; I ſhall leave to their conſideration, who may think it worth their endeavor to apply this *Hypotheſis* to the ſolution of *phænomena*.

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4. Of the Objector's Hypothesis, *and that the most free and genuine Constitution of that and all other Mechanical Hypotheses is conformable to my Doctrine.*

In the second place, I told you, that the Objectors *Hypothesis*, as to the fundamental part of it, is not against me. That fundamental Supposition is: *That the parts of bodies, when briskly agitated, do excite Vibrations in the Æther, which are propagated every way from those bodies in streight 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 *phænomena* I take to be this: *That* the agitated parts of bodies, according to their several sizes, figures, and motions, do excite Vibrations in the *æther* of various depths or bignesses, which being promiscuously propagated through that *Medium* to our Eyes, effect in us a Sensation of Light of a *White* colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a Sensation of a *Red* colour, the least or shortest, of a deep *Violet*, and the intermediat ones, of intermediat colors; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the Air of various bignesses, which, according to those bignesses, make several Tones in Sound: *That* the largest Vibrations are best able to overcome the resistance of a Refracting superficies, and so break through it with least Refraction; whence the Vibrations of several bignesses, that is, the Rays of several Colors, which are blended together in Light, must be parted from one another by Refraction, and so cause the *Phænomena* of *Prismes* and other refracting substances: And *that* it depends on the thickness of a thin transparent Plate or Buble, whether a Vibration shall be *reflected* at its further superficies, or *transmitted*; so that, according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And since the Vibrations which make *Blew* and *Violet*, are supposed shorter than those which make *Red* and *Yellow*, they must be reflected at a less thickness of the Plate: Which is sufficient to explicate all the ordinary *phænomena* of those Plates or Bubles, and also of all natural bodies, whose

whose parts are like so many fragments of such Plates.

These seem to be the most plain, genuine and necessary conditions of this *Hypothesis*: And they agree so justly with my Theory, that if the *Animadversor* think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet how he will defend it from other difficulties, I know not. For, to me, the Fundamental Supposition it self seems impossible; namely, That the *Waves* or Vibrations of any Fluid, can, like the Rays of Light, be propagated in *Streight* lines, without a continual and very extravagant spreading and bending every way into the quiescent Medium, where they are terminated by it. I mistake, if there be not both Experiment and Demonstration to the contrary. And as to the other two or three *Hypotheses*, which he mentions, I had rather believe them subject to the like difficulties, than suspect the *Animadversor* should select the worst for his own.

What I have said of this, may be easily applied to all other *Mechanical Hypotheses*, in which Light is supposed to be caused by any Pression or Motion whatsoever, excited in the *æther* by the agitated parts of Luminous bodies. For, it seems impossible, that any of those Motions or Pressions can be propagated in *Streight* lines without the like spreading every way into the shadow'd Medium, on which they border. But yet, if any man can think it possible, he must at least allow, that those Motions or Endeavors to motion, caused in the *æther* by the several parts of any Lucid body that differ in size, figure, and agitation, must necessarily be unequal: Which is enough to denominate Light an Aggregat of *difform* rays, according to any of those Hypotheses. And if those Original inequalities may suffice to difference the Rays in Colour and Refrangibility, I see no reason, why they, that adhere to any of those *hypotheses*, should seek for other Causes of these Effects, unless (to use the *Objectors* argument) they will multiply entities without necessity.

5. Of the Animadversor's *Concessions, and their limitation to his Hypothesis.*

The *third* thing to be considered is, the Condition of the *Animadversor's* Concessions, which is, that I would explicate my *Theories* by his *Hypothesis*: And if I could comply with him in that point, there

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there would be little or no difference between Us. For he grants, that without any refpect to a different Incidence of rays there are different Refractions; but he would have it explicated, not by the different Refrangibility of feveral Rays, but by the Splitting and Rarefying of æthereal pulfes. He grants my *third*, *fourth* and *fixth* Propofitions; the fenfe of which is, That Un-compounded Colors are unchangeable, and that Compounded ones are changeable only by refolving them into the colors, of which they are compounded; and that all the Changes, which can be wrought in Colours, are effected only by varioufly mixing or parting them: But he grants them on condition that I will explicate Colors by the two fides of a fplit pulfe, and fo make but two *fpecies* of them, accounting all other Colors in the world to be but various degrees and dilutings of thofe two. And he further grants, that *Whitenesse* is produced by the Convention of all Colors; but then I muft allow it to be not only by Mixture of thofe Colors, but by a farther Uniting of the parts of the Ray fuppofed to be formerly fplit.

If I would proceed to examine thefe his Explications, I think it would be no difficult matter to fhew, that they are not only *infufficient*, but in fome refpects to me (at leaft) *un-intelligible*. For, though it be eafie to conceive, how *M*otion may be dilated and fpread, or how parallel motions may become diverging; yet I underftand not, by what artifice any *Linear* motion can by a refracting fuperficies be *infinitely* dilated and rarefied, fo as to become *Superficial*: Or, if that be fuppofed, yet I underftand as little, why it fhould be fplit at fo fmall an angle only, and not rather fpread and difperfed through the whole angle of Refraction. And further, though I can eafily imagine, how Unlike motions may crofs one another; yet I cannot well conceive, how they fhould coalefce into one *uniform* motion, and then part again, and recover their former Unlikenefs; notwithftanding that I conjecture the ways, by which the *Animadverfor* may endeavour to explain it. So that the Direct, uniform and undifturbed Pulfes fhould be fplit and difturbed by Refraction; and yet the Oblique and difturbed Pulfes perfift without fplitting or further difturbance by following Refractions, is (to me) as unintelligible. And there is

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as great a difficulty in the Number of Colours; as you will see hereafter.

6. *That it is not necessary, to limit or explain my Doctrine by any* Hypothesis.

But whatever be the advantages or disadvantages of this *Hypothesis*, I hope I may be excused from taking it up, since I do not think it needful to explicate my Doctrine by any *Hypothesis* at all. For if *Light* be consider'd abstractedly without respect to any *Hypothesis*, I can *as* easily conceive, that the several parts of a shining body may emit rays of differing colours and other qualities, of all which Light is constituted, *as* that the several parts of a false or uneven string, or of uneavenly agitated water in a Brook or Cataract, or the several Pipes of an Organ inspired all at once, or all the variety of Sounding bodies in the world together, should produce sounds of several Tones, and propagate them through the Air confusedly intermixt. And, if there were any natural bodies that could *reflect* sounds of one tone, and stifle or *transmit* those of another; then, as the *Echo* of a confused Aggregat of all Tones would be that particular Tone, which the Echoing body is disposed to reflect; so, since (even by the *Animadversor's* concessions) there are bodies apt to *reflect* rays of one colour, and stifle or *transmit* those of another; I can as easily conceive, that those bodies, when illuminated by a mixture of all colours, must appear of that colour only which they reflect.

But when the *Objector* would insinuate a difficulty in these things, by alluding to Sounds in the string of a *Musical* instrument before percussion, or in the Air of an Organ Bellowes before its arrival at the Pipes; I must confess, I understand it as little, as if one had spoken of Light in a piece of Wood before it be set on fire, or in the oyl of a Lamp before it ascend up the match to feed the flame.

7. *The difficulties of the* Animadversors *discourse abstracted from* Hypotheses, *and consider'd more generally.*

You see therefore, how much it is besides the business in hand, to dispute about *Hypotheses*. For which reason I shall now in the last place, proceed to abstract the difficulties in the *Animadversor's* discourse, and, without having regard to any *Hypothesis*, consider them in general terms. And they may be reduced to these 3 *Queres*: L llll 1. Whe-

1. Whether the unequal Refractions, made without respect to any inequality of incidence, be caused by the different Refrangibility of several Rays; or by the splitting, breaking or dissipating the same Ray into diverging parts?

2. Whether there be more than two sorts of Colours?

3. Whether Whiteness be a mixture of all Colours?

8. *That the Ray is not split, or any otherwise dilated.*

The *First* of these *Quæres* you may find already determin'd by an Experiment in my former Letter; the design of which was to shew, That the length of the colour'd Image proceeded not from any unevenness in the Glass, or any other *contingent* Irregularity in the Refractions. Amongst other Irregularities I know not, what is more obvious to suspect, than a fortuitous dilating and spreading of Light after some such manner, as *Des-Cartes* hath described in his Æthereal Refractions for explicating the *Tayle* of a *Comet*; or as the *Animadversor* now supposes to be effected by the Splitting and Rarifying of his Æthereal Ipulses. And to prevent the suspicion of any such Irregularities, I told you, that I refracted the Light contrary ways with two Prismes successively, to destroy thereby the *Regular* effects of the *first* Prisme by the *second*, and to discover the *Irregular* effects by augmenting them with iterated refractions. Now, amongst other Irregularities, if the *first* Prisme had spread and dissipated every ray into an indefinit number of diverging parts, the *second* should in like manner have spread and dissipated every one of those parts into a further indefinite number, whereby the Image would have been still more dilated, contrary to the event. And this ought to have hapned, because those Linear diverging parts depend not on one another for the manner of their Refraction, but are every one of them as truly and compleatly Rays as the whole was before its Incidence; as may appear by intercepting them severally.

The reasonableness of this proceeding will perhaps better appear by acquainting you with this further circumstance. I sometimes placed the *second* Prisme in a position Transverse to the *first*, on design to try, if it would make the long Image become four-square by refractions crossing those that had drawn the round Image into a long one. For, if amongst other Irregularities the Refraction of the *first* Prisme, did by Splitting dilate

dilate a Linear ray into a Superficial, the Cross refractions of that *second* Prisme ought by further splitting to dilate and draw that Superficial ray into a Pyramidal solid. But, upon tryal, I found it otherwise; the Image being as regularly Oblong as before, and inclin'd to both the Prismes at an angle of 45. degrees.

I tryed also all other Positions of the second Prisme, by turning the Ends about its middle part; and in no case could observe any such Irregularity. The Image was ever alike inclined to both Prismes, its Breadth answering to the Suns Diameter, and its length being greater or less accordingly as the Refractions more or less agreed, or contradicted one another.

And by these Observations, since the Breadth of the Image was not augmented by the Cross refraction of the *second* Prisme, that refraction must have been perform'd *without* any splitting or dilating of the ray; and therefore at least the Light incident on that Prisme must be granted an Aggregat of Rays *unequally refrangible* in my sense. And since the Image was equally inclin'd to both *Prismes*, and consequently the Refractions alike in both, it argues, that they were perform'd according to some *Constant Law* without any irregularity.

9. That there are more than two Original Colors.

To determine the *second* Quære, the *Animadversor* referrs to an Experiment made with two *Wedge-like boxes*, recited in the *Micrography* of the Ingenious Mr. *Hook* Observ. 10. pag. 73. the design of which was to produce *all* Colours out of a mixture of *two*. But there is, I conceive, a double defect in this instance. For, it appears not, that by this Experiment all colours can be produced out of two; and, if they could, yet the Inference would not follow.

That *all* Colours cannot by that Experiment be produced out of two, will appear by considering, that the Tincture of *Aloes*, which afforded one of those Colours, was not all over of one uniform colour, but appear'd *yellow* near the edge of the Box, and *red* at other places where it was thicker: affording all variety of colours from a *pale yellow* to a *deep red* or Scarlet, according to the various thickness of the liquor. And so the ſolution of *Copper*, which afforded the other colour, was of various *Blews* and *Indigo's*. So that inſtead of *two* colours, here is a great variety made uſe of for the production of all others. Thus, *for inſtance*, to produce all ſorts of *Greens*, the ſeveral degrees of *Yellow* and *pale Blew* muſt be mixed ; but to compound *Purples*, the *Scarlet* and *deep Blew* are to be the Ingredients.

Now, if the *Animadverſor* contend, that all the *Reds* and *Yellows* of the one Liquor, or *Blews* and *Indigo's* of the other, are only various degrees and dilutings of the ſame Colour, and not divers colours, that is a Begging of the Queſtion : And I ſhould as ſoon grant, that the two *Thirds* or *Sixths* in Muſick are but ſeveral degrees of the ſame ſound, and not divers ſounds. Certainly it is much better to believe our Senſes, informing us, that *Red* and *yellow* are divers colours, and to make it a Philoſophical *Quære*, Why the ſame Liquor doth, according to its various thickneſs, appear of thoſe divers colours, than to ſuppoſe them to be the ſame colour becauſe exhibited by the ſame liquor ? For, if that were a ſufficient reaſon, then *Blew* and *Yellow* muſt alſo be the ſame colour, ſince they are both exhibited by the ſame Tincture of *Nephritick Wood*. But that they are *divers* colours, you will more fully underſtand by the reaſon, which, in my Judgment, is this : The Tincture of *Aloes* is qualified to tranſmit *moſt eaſily* the rays indued with *red*, *moſt difficultly* the rays indued with *violet*, and with *intermediat degrees of facility* the rays indued with *intermediat* colours. So that where the liquor is very thin, it may ſuffice to intercept moſt of the *violet*, and yet tranſmit moſt of the other colours ; all which together muſt compound a middle Colour, that is, a *faint yellow*. And where it is ſo much thicker as alſo to intercept moſt of the *Blew* and *Green*, the remaining *Green*, *Yellow*, and *Red*, it muſt compound an *Orenge*. And where the thickneſs is ſo great, that ſcarce any rays can paſs through it beſides thoſe indued with *Red*, muſt appear of that colour, and that ſo much the deeper and obſcurer, by how the liquor is thicker. And the ſame may be underſtood of the various degrees of *Blew*, exhibited by the Solution of *Copper*, by reaſon of its diſpoſition to intercept *Red* moſt eaſily, and tranſmit a *deep Blew* or *Indigo*-Colour moſt freely.

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But, ſuppoſing that *all* Colours might, according to this experiment, be produced out of *two* by mixture; yet it follows not, that thoſe two are the only *Original* colours, and that for a double reaſon. *Firſt*, becauſe thoſe two are not themſelves Original colours, but compounded of others; there being no liquor nor any other body in nature, whoſe colour in Day-light is wholly un-compounded. And *then*, becauſe, though thoſe two were Original, and all others might be compounded of them, yet it follows not, that they cannot be otherwiſe produced. For I ſaid, that they had a double Origin, the ſame Colours to ſenſe being in ſome caſes compounded and in others un compounded; and ſufficiently declar'd in my *third* and *fourth* Propoſitions, and in the Concluſion, by what Properties the one might be known and diſtinguiſh't from the other. But, becauſe I ſuſpect by ſome Circumſtances, that the *Diſtinction* might not be rightly apprehended, I ſhall once more declare it, and further explain it by Examples.

That Colour is *Primary* or *Original*, which cannot by any Art be changed, and whoſe Rays are not *alike* refrangible: And that *Compounded*, which is changeable into other colours, and whoſe Rays are *not alike* refrangible. *For inſtance*, to know, whether the colour of any *Green* object be compounded or not, view it through a Prime, and if it appear *confuſed*, and the edges tinged with *Blew*, *Yellow*, or any variety of other colours, then is that *Green* compounded of ſuch colours as at its edges emerge out of it: But if it appear *diſtinct*, and well defin'd, and entirely Green to the very edges, without any other colours emerging, it is of an Original and un-compounded Green. In like manner, if a refracted beam of light, being caſt on a white wall, exhibit a *Green* colour, to know whether that be compounded, refract the beam with an interpoſed Priſme; and if you find any Difformity in the refractions, and the *Green* be transform'd into *Blew*, *Yellow*, or any variety of other colours, you may conclude, that it was compounded of thoſe which emerge: But if the Refractions be uniform, and the *Green* perſiſt without any change of colour, then is it Original and un compounded. And the reaſon why I call it ſo, is, becauſe a *Green* indued with ſuch properties cannot be produced by any mixing of other colours.

Now

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Now, if two *Green* Objects may to the naked eye appear of the same colour, and yet one of them through a prisme seem *confused* and variegated with other colours at the edges, and the other *distinct* and entirely Green; or, if there may be two Beams of Light, which falling on a white wall do to the naked eye exhibit the same *Green* colour, and yet one of them, when transmitted through a Prisme, be uniformly and *regularly* refracted, and retain its colour unchanged, and the other be *irregularly* refracted and to divaricate into a multitude of other colours; I suppose, these two *greens* will in both cases be granted of a different Origin and constitution. And if by mixing colours, a *green* cannot be compounded with the properties of the *Unchangeable* Green, I think, I may call *that* an *Un-compounded* colour, especially since its rays are alike refrangible, and uniform in all respects.

The same rule is to be observ'd in examining, whether *Red, Orenge, Yellow*, B*lew*, or any other colour be compounded or not. And, by the way, since all *White* objects through the Prisme appear confus'd and terminated with colours, *Whiteness* must, according to this distinction, be ever compounded, and that the most of all colours, because it is the most confus'd and changed by Refractions.

From hence I may take occasion to communicate a way for the improvement of *Microscopes* by Refraction. The way is, by illuminating the Object in a darkned room with Light of any convenient colour not too much compounded: for by that means the Microscope will with distinctness bear a deeper Charge and larger Aperture, especially if its construction be such, as I may hereafter describe; for, the advantage in Ordinary Microscopes will not be so sensible.

10. *That Whiteness is a mixture of all Colours.*

There remains now the *third* Quære to be consider'd, which is, Whether *Whiteness* be an Uniform Colour, or a dissimilar Mixture of all colours? The Experiment which I brought to decide it, the *Animadversor* thinks may be otherwise explain'd, and so concludes nothing. But he might easily have satisfied himself by trying, what would be the result of a Mixture of all colours. And that very Experiment might have satisfied him, if he had pleased to examine it by the

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the various circumſtances. One circumſtance I there declared, of which I ſee no notice taken; and it is, That if any colour at the *Lens* be intercepted, the *Whiteneſs* will be changed into the other colours: If all the colours but *red* be intercepted, that Red alone in the concourſe or croſſing of the Rays will not conſtitute Whiteneſs, but continues as much Red as before; and ſo of the other colours. So that the buſineſs is not only to ſhew, how rays, which before the concourſe exhibit colours, do in the concourſe exhibit *White*; but to ſhew, How in the ſame place, where the ſeveral ſorts of rays apart exhibit ſeveral colours, a Confuſion of all together make White. For inſtance, if red alone be firſt tranſmitted to the paper at the place of concourſe, and then the other colours be let fall on that Red, the *Queſtion* will be, Whether they convert it into White, by mixing with it only, as Blew falling on Yellow light is ſuppos'd to compound Green; or, Whether there be ſome further change wrought in the colours by their mutual acting on one another, untill, like contrary *Peripatetic* qualities, they become aſſimilated. And he that ſhall explicate this laſt Caſe *mechanically*, muſt conquer a double impoſſibility. He muſt *firſt* ſhew, that many unlike motions in a Fluid can by claſhing ſo act on one another, and change each other, as to become one Uniform motion; and *then*, that an Uniform motion can of it ſelf, without any new unequal impreſſions, depart into a great variety of motions regularly un-equal. And after this he muſt further tell me, Why all Objects appear not of the ſame colour, that is, why their colours in the Air, where the rays that convey them every way are confuſedly mixt, do not aſſimilate one another and become Uniform before they arrive at the Spectators eye?

But if there be yet any doubting, 'tis better to put the Event on further Circumſtances of the *Experiment*, than to acquieſce in the poſſibility of any *Hypothetical* Explication. As, for inſtance, by trying, What will be the apparition of theſe colours in a very quick Conſecution of one another. And this may be eaſily perform'd by the rapid gyration of a Wheel with many Spoaks or coggs in its perimeter, whoſe Interſtices and thickneſſes may be equal and of ſuch a largeneſs, that, if the Wheel be interpoſed between the Priſme and the white concourſe

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of the colours, one half of the Colours may be intercepted by a ſpoake or cogg, and the other half paſs through an interſtice. The Wheel being in this poſture, you may firſt turn it ſlowly about, to ſee all the colours fall ſucceſſively on the ſame place of the paper, held at their aforeſaid concourſe; and if you then accelerate its gyration, until the Conſecution of thoſe colours be ſo quick, that you cannot diſtinguiſh them ſeverally, the reſulting colour will be a Whiteneſs perfectly like that, which an un-refracted beam of Light exhibits, when in like manner ſucceſſively interrupted by the ſpoaks or coggs of that circulating Wheel. And that this *Whiteneſs* is produced by a ſucceſſive Intermixture of the Colours, without their being aſſimilated, or reduc'd to any Uniformity, is certainly beyond all doubt, unleſs things that exiſt not at the ſame time may notwithſtanding act on one another.

There are yet other Circumſtances, by which the Truth might have been decided; as by viewing the White concourſe of the Colours through another Priſme plac'd cloſe to the eye, by whoſe Refraction that whiteneſs may appear again transform'd into Colours: And then, to examine their Origin, if an Aſſiſtant intercept any of the colours at the *Lens* before their arrival at the Whiteneſs, the ſame colours will vaniſh from amongſt thoſe, into which that Whiteneſs is converted by the *ſecond* Priſme. Now, if the rays which diſappear be the ſame with thoſe that are intercepted, then it muſt be acknowledged, that the *ſecond* Priſme makes no new colours in any rays, which were not in them *before* their concourſe at the paper. Which is a plain indication, that the rays of ſeveral colours remain diſtinct from one another in the Whiteneſs, and that from their *previous* diſpoſitions are deriv'd the Colours of the *ſecond* Priſme. And, by the way, what is ſaid of their Colors may be applied to their Refrangibility.

The aforeſaid *Wheel* may be alſo here made uſe of; and, if its gyration be neither too quick nor two ſlow, the ſucceſsion of the colours may be diſcern'd through the Priſme, whilſt to the naked eye of a Byſtander they exhibit whiteneſs.

There is ſomething ſtill remaining to be ſaid of this Experiment

ment. **But this, I conceive, is enough to enforce it, and** ſo to de- cide the controverſy. How-ever, I ſhall now proceed to ſhew ſome other ways of producing *Whiteneſs by mixtures*, ſince I perſwade my ſelf, that this Aſſertion above the reſt appears Paradoxical, and is with moſt difficulty admitted. And becauſe the *Animadverſor* deſires an inſtance of it in Bodies of divers colours, I ſhall begin with that. But in order thereto it muſt be conſider'd, that ſuch colour'd Bodies reflect but ſome part of the Light incident on them ; as is evident by the 13 *Propoſition* : And therefore the Light reflected from an Aggregat of them will be much weakned by the loſs of many rays. Whence a perfect and *intenſe* Whiteneſs is not to be expected, but rather a Colour between thoſe of Light and Shadow, or ſuch a Gray or Dirty colour as may be made by mixing White and Black together.

And that ſuch a Colour will reſult, may be collected from the colour of *Duſt* found in every corner of an houſe, which hath been obſerv'd to conſiſt of many colour'd particles. There may be alſo produced the like Dirty colour by mixing ſeveral *Painters colours* together. And the ſame may be effected by Painting a *Top* (ſuch as Boys play with) of divers colours. For, when it is made to circulate by whipping it, it will appear of ſuch a dirty colour.

Now, the Compounding of theſe colours is proper to my purpoſe, becauſe they differ not from Whiteneſs in the *Species* of colour, but only in *degree* of Luminouſneſs : which (did not the *Animadverſor* concede it) I might thus evince. A beam of the Suns Light being tranſmitted into a darkned room, if you illuminate a ſheet of White Paper by that Light, reflected from a body of any colour, the paper will always appear of the colour of that body, by whoſe reflected light it is illuminated. If it be a red body, the paper will be red ; if a green body, it will be green ; and ſo of the other colours. The reaſon is, that the fibers or threds, of which the paper conſiſts, are all tranſparent and ſpecular ; and ſuch ſubſtances are known to reflect colours without changing them. To know therefore, to what *Species* of colour a *Grey* belongs, place any Gray body (ſuppoſe a Mixture of *Painters colours*,) in the ſaid Light, and the paper, being illuminated by its reflexion, ſhall appear White. And the ſame thing will happen, if it be illuminated by reflexion from a *black* ſubſtance.

Theſe therefore are all of one *Species* ; but yet they ſeem diſtinguiſht not only by *degrees* of Luminouſneſs, but alſo by ſome other Inequalities, whereby they become more harſh or pleaſant. And the diſtinction ſeems to be, that *Greys* and perhaps *Blacks* are made by an uneven defect of Light, conſiſting as it were of many little veins or ſtreams, which differ either in Luminouſneſs or in the Unequal di-

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ſtribution of diverſly colour'd rays; ſuch as ought to be caus'd by Reflexion from a Mixture of white and black, or of diverſly co. lour'd corpuſcles. But when ſuch imperfectly mixt Light is by a *ſecond* Reflexion from the paper more evenly and uniformly blended, it becomes more pleaſant, and exhibits a *faint* or ſhadow'd Whiteneſs. And that ſuch little irregularities as theſe may cauſe theſe differences, is not improbable, if we conſider, how much variety may be cauſed in *Sounds* of the ſame tone by irregular and uneven jarrings. And beſides, theſe differences are ſo little, that I have ſometimes doubted, whether they be any at all, when I have conſider'd that a Black and White Body being plac'd together, the one in a ſtrong light, and the other in a very faint light, ſo proportion'd that they might appear equally luminous; it has been difficult to diſtinguiſh them, when view'd at diſtance, unleſs when the Black ſeem'd more blewiſh; and the White body in a light ſtill fainter, hath, in compariſon of the Black body, it ſelf appear'd Black.

This leads me to another way of *Compounding Whiteneſs*; which is, That, if four or five Bodies of the more eminent colours, or a Paper painted all over, in ſeveral parts of it, with thoſe ſeveral colours in a due proportion, be placed in the ſaid Beam of Light; the Light, reflected from thoſe Colours to another White paper, held at a convenient diſtance, ſhall make that paper appear White. If it be held too near the Colours, its parts will ſeem of thoſe colours that are neareſt them; but by removing it further, that all its parts may be equally illuminated by all the colours, they will be more and more diluted, until they become perfectly White. And you may further obſerve, that if any of the colours be intercepted, the Paper will no longer appear White, but of the other colours which are not intercepted. Now, that this *Whiteneſs* is a Mixture of the ſeverally colour'd rays, falling confuſedly on the paper, I ſee no reaſon to doubt of; becauſe, if the Light became Uniform and Similar before it fell confuſedly on the paper, it muſt much more be Uniform, when at a greater diſtance it falls on the Spectators eye, and ſo the rays, which come from ſeveral colours, would in no qualities differ from one another, but all of them exhibit the ſame colour to the Spectator, contrary to what he ſees.

Not much unlike this Inſtance it is, That, if a poliſht piece of Metal be ſo placed, that the colours appear in it as in a Looking-glaſs, and then the Metal be made rough, that by a confus'd reflexion thoſe apparent colours may be blended together, they ſhall diſappear, and by their mixture cauſe the Metall to look White.

But

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But further to enforce this *Experiment*; if, inftead of the Paper, any White *Froth*, confifting of fmall bubles, be illuminated by reflexion from the aforefaid Colours, it fhall to the naked eye feem White, and yet through a good Microfcope the feveral Colours will appear diftinct on the bubles, as if feen by reflexion from fo many fpherical furfaces. With my naked eye, being very near, I have alfo difcern'd the feveral colours on each buble; and yet at a greater diftance, where I could not diftinguifh them apart, the Froth hath appear'd entirely White. And at the fame diftance, when I look'd intently, I have feen the colours diftinctly on each buble; and yet, by ftraining my eyes as if I would look at fomething far off beyond them, thereby to render the Vifion confus'd, the Froth has appear'd without any other colour than Whitenefs. And what is here faid of Froths, may eafily be underftood of the Paper or Metal in the foregoing Experiments. For, their parts are fpecular bodies, like thefe Bubles: And perhaps with an excellent Microfcope the Colours may be alfo feen intermixedly reflected from them.

In proportioning the feverally Colour'd bodies to produce thefe effects, there may be fome nicenefs; and it will be more convenient, to make ufe of the colours of the Prifme, caft on a Wall, by whofe reflexion the Paper, Metal, Froth, and other White fubftances may be illuminated. And I ufually made my Tryals this way, becaufe I could better exclude any fcattering Light from mixing with the colours to dilate them.

To this way of Compounding Whitenefs may be referr'd that other, by Mixing light after it hath been trajected through tranfparently colour'd fubftances. *For inftance*, if no Light be admitted into a room but only through Colour'd glafs, whofe feveral parts are of feveral colours in a pretty equal proportion; all White things in the room fhall appear White, if they be not held too near the Glafs. And yet this light, with which they are illuminated, cannot poffibly be uniform, becaufe, if the Rays, which at their entrance are of divers colours, do in their progrefs through the room fuffer any alteration to be reduced to an Uniformity; the Glafs would not in the remoteft parts of the room appear of the very fame colour, which it doth when the Spectators eye is very near it: Nor would the rays, when tranfmitted into another dark room through a little hole in an oppofite door or partition-wall, project on a Paper the *Species* or reprefentation of the glafs in its proper colours.

And, by the by, this feems a very fit and cogent Inftance of fome other parts of my *Theory*, and particularly of the 13 *Propofition*. For, in this room all natural Bodies whatever appear in their proper colours. And all the *Phænomena* of colours in nature, made either by Refraction or without it, are here the fame as in the Open Air. Now, the Light in this room being fuch a Diffimilar mixture, as

I have deſcrib'd in my *Theory*, the Cauſes of all theſe *Phænomena* muſt be the ſame that I have there aſſign'd. And I ſee no reaſon to ſuſpect, that the ſame *Phænomena* ſhould have other cauſes in the Open Air.

The ſucceſs of this Experiment may be eaſily conjectur'd by the appearances of things in a Church or Chappel, whoſe windores are of colour'd glaſs; or in the Open Air, when it is illuſtrated with Clouds of various colours.

There are yet other ways, by which I have produced *Whiteneſs*; as *by* caſting ſeveral Colours from two or more Priſmes upon the ſame place; *by* Refracting a Beam of Light with two or three Priſmes ſucceſſively, to make the diverging colours converge again; *by* Reflecting one colour to another; and *by* looking through a Priſme on an Object of many colours; and, (which is equivalent to the above mention'd way of mixing colours by concave *Wedges* fill'd with colour'd liquors,) I have obſerv'd the ſhadows of a painted Glaſs-window to become White, where thoſe of many colours have at a great diſtance interfered. But yet, for further ſatisfaction, the *Animadverſor* may try, if he pleaſe, the effects of four or five of ſuch *Wedges* filled with liquors of as many ſeveral colours.

Beſides all theſe, the Colours of *Water-bubbles* and other thin pellucid ſubſtances afford ſeveral inſtances of Whiteneſs produced by their mixture; with *one* of which I ſhall conclude this particular. Let ſome Water, in which a convenient quantity of Soap or waſh-ball is diſſolv'd, be agitated into Froth, and, after that froth has ſtood a while without further agitation, till you ſee the bubbles, of which it conſiſts, begin to break, there will appear a great variety of colours all over the top of every bubble, if you view them near at hand; but, if you view them at ſo great a diſtance that you cannot diſtinguiſh the colours one from another, the Froth will appear perfectly White.

11. *That the* Experimentum crucis *is ſuch.*

Thus much concerning the deſign and ſubſtance of the *Animadverſor's* Conſiderations. There are yet ſome particulars to be taken notice of, before I conclude; as the denyal of the *Experimentum Crucis*. On *this* I choſe to lay the whole ſtreſs of my diſcourſe; which therefore was the principal thing to have been objected againſt. But I cannot be convinced of its inſufficiency by a bare denyal without aſſigning a Reaſon for it. I am apt to believe, it has been miſunderſtood; for otherwiſe it would have prevented the diſcourſes about Rarifying and Splitting of rays; becauſe the deſign of it is, to ſhew, that Rays of divers colours, conſider'd a part, do at *Equal* Incidences ſuffer *Unequal* Refractions, without being ſplit, rarified, or any ways diſ-lated.

In

In the Considerations of my first and second Propositions, the *Animadversor* hath rendred my Doctrine of *Un-equal Refrangibility* very imperfect and maimed, by explicating it wholly by the Splitting of rays; whereas I chiefly intended it in those Refractions that are perform'd without that suppos'd Irregularity; such as the *Experimentum Crucis* might have inform'd him of. And, in general I find, that, whilst he hath endeavour'd to explicate my Propositions *Hypothetically*, the more material suggestions, by which I design'd to recommend them, have escap'd his consideration; such as are, The Unchangeableness of the degree of Refrangibility peculiar to any sort of rays; the strict Analogy between the degrees of Refrangibility and Colours; the Distinction between compounded and un-compounded colours; the Unchangeableness of un-compounded colours; and the Assertion, that if any one of the Prismatique colours be wholly intercepted, that colour cannot be new produced out of the remaining Light by any further Refraction or Reflexion whatsoever. And of what strength and efficacy these Particulars are for enforcing the *Theory*, I desire therefore may be now consider'd.

12. Some particulars recommended to further consideration.

An Extract of a Letter lately written by an ingenious person from Paris, *containing some Considerations upon* Mr. Newtons *doctrine of* Colors, *as also upon the effects of the different Refractions of the Rays in Telescopical Glasses.*

I Have seen, how Mr. *Newton* endeavours to maintain his new Theory concerning *Colours*. Me thinks, that the most important Objection, which is made against him by way of *Quære*, is that, Whether there be more than two sorts of Colours. For my part, I believe, that an *Hypothesis*, that should explain mechanically and by the nature of motion the Colors *Yellow* and *Blew*, would be sufficient for all the rest, in regard that those others, being only more deeply charged (as appears by the Prismes of Mr. *Hook*,) do produce the dark or deep-Red and Blew; and that of these four all the other colors may be compounded. Neither do I see, why Mr. *Newton* doth not content himself with the two Colors, Yellow and Blew; for it will be much more easy to find an *Hypothesis* by Motion, that may explicate these two differences, than for so many diversities as there are of others Colors. And till he hath found this *Hypothesis*, he hath not taught us, what it is wherein consists the nature and difference of Colours, but only this accident (which certainly is very considerable,) of their *different Refrangibility*.

As for the composition of *White* made by all the Colors together, it may possibly be, that *Yellow* and *Blew* might also be sufficient for that: Which is worth while to try; and it may be done by the Experiment, which Mr. *Newton* proposeth, by receiving against a wall of a darkn'd room the Colours of the Prisme, and to cast their reflected light upon white paper. Here you must hinder the Colors of the extremities, *viz.* the Red and Purple, from striking against the wall, and leave only the intermediate Colors, yellow, green and blew, to see, whether the light of these alone would not make the paper appear white, as well as when they all give light. I even doubt, whether the lightest place of the yellow color may not all alone produce that effect, and I mean to try it at the first conveniency; for this thought never came into my mind but just

now.

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now. Mean time you may ſee, that if theſe Experiments do ſucceed, it can no more be ſaid, that all the Colors are neceſſary to compound White, and that 'tis very probable, that all the reſt are nothing but degrees of *Yellow* and *Blew*, more or leſs charged.

Laſtly, touching the Effect of the different Refractions of the Rays in Teleſcopical Glaſſes, 'tis certain, that Experience agrees not with what Mr. *Newton* holds. For to conſider only a picture, which is made by an object-glaſs of 12 feet in a dark room, we ſee, it is too diſtinct and too well defined to be produced by rayes, that ſhould ſtray the 50*th.* part * of the Aperture. So that, (as I believe I have told you heretofore) the difference of the Refrangibility doth not, it may be, alwayes follow the ſame proportion in the great and ſmall inclinations of the Rayes upon the ſurface of the Glaſs.

* *Compare herewith what Mr.* Newton, *ſaith in* Numb. 80. *of theſe Tracts*, pag. 3079.

Mr. Newtons *Anſwer to the foregoing Letter further explaining his Theory of Light and Colors, and particularly that of Whiteneſs; together with his continued hopes of perfecting Teleſcopes by Reflections rather than Refractions.*

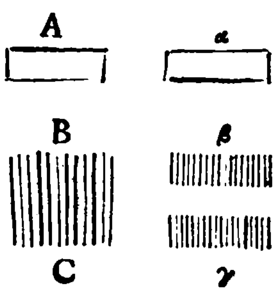
Concerning the buſineſs of Colors; in my ſaying that when Monſieur N. hath ſhewn how *White* may be produced out of two uncompounded colors, I will tell him, why he can conclude nothing from *that*; my meaning was, that ſuch a White, (were there any ſuch,) would have different properties from the White, which I had reſpect to, when I deſcribed my Theory, that is, from the White of the Sun's immediate light, of the ordinary objects of our ſenſes, and of all white *Phænomena* that have hitherto faln under my obſervation. And thoſe different properties would evince it to be of a different conſtitution: Inſomuch that ſuch a production of white would be ſo far from contradicting, that it would rather illuſtrate and confirm my Theory; becauſe by the difference of that from other whites it would appear, that other Whites are not compounded of only two colours like that. And therefore if Monſieur N. would prove any thing, it is requiſite that he do not only produce out of two primitive Colors,

lors a white which to the naked eye ſhall appear like other whites, but alſo ſhall agree with them in all other properties.

But to let you underſtand wherein ſuch a white would differ from other whites and why from thence it would follow that other whites are otherwiſe compounded, I ſhall lay down this poſition.

That a compounded color can be reſolved into no more ſimple colors then thoſe of which it is compounded.

This ſeems to be ſelf evident, and I have alſo tryed it ſeveral ways, and particularly by this which follows. Let α repreſent an oblong piece of white-paper about $\frac{1}{3}$ or $\frac{1}{4}$ of an inch broad, and illuminated in a dark room with a mixture of two colours caſt upon it from two Priſms, ſuppoſe a deep blew and ſcarlet, which muſt ſeverally be as uncompounded as they can conveniently be made. Then at a convenient diſtance, ſuppoſe of ſix or eight yards, view



it through a clear triangular glaſs or cryſtal Priſm held parallel to the paper, and you ſhall ſee the two colors parted from one another in the faſhion of two images of the paper, as they are repreſented at ϐ and γ, where ſuppoſe β the ſcarlet and γ the blew, without green or any other color between them.

Now from the aforeſaid Poſition I deduce theſe two concluſions. 1. That if there were found out a way to compound white of two ſimple colors only, that white would be again reſolvable into no more than two. 2. That if other whites (as that of the Suns light, &c. be reſolvable into more than two ſimple colours (as I find by Experiment that they are) then they muſt be compounded of more than two.

To make this plainer, ſuppoſe that A repreſents a white body illuminated by a direct beam of the Sun tranſmitted through a ſmall hole into a dark room, and α ſuch another body illuminated by a mixture of two ſimple colors, which if poſſible may make it alſo appear of a white color exactly like A. Then at a convenient diſtance view theſe two whites through a Priſm, and A will be changed into a ſeries of all colors, Red, Yellow, Green, Blew, Purple, with their intermediate degrees ſucceeding in order from B to C. But α, according to the aforeſaid Experiment, will only yield thoſe two colors of which 'twas compounded, and thoſe not conterminate like the colors at BC, but ſeparate from one another as at ϵ and γ, by means of the different refrangibility of the rays to which they belong. And thus by comparing theſe two whites, they would appear to be of a different conſtitution, and A to conſiſt of more colors then α. So that what Monſieur *N.* contends for, would rather advance my Theory by the acceſs of a new kind of white than conclude againſt it. But I ſee no hopes of compounding ſuch a white.

As for Monſieur *N.* his expreſſion, that I maintain my doctrine with ſome concern, I confeſs it was a little ungrateful to me to meet with objections which had been anſwered before, without having the leaſt reaſon given me why thoſe anſwers were inſufficient. The anſwers which I ſpeak of are in the Tranſactions from *pag.* 5093 to *pag.* 5102. And particularly in *pag.* 5095; to ſhew that there are other ſimple colors beſides blew and yellow, I inſtance in a ſimple or homogeneal Green, ſuch as cannot be made by mixing blew and yellow or any other colours. And there alſo I ſhew why, ſuppoſing that all colors might be produced out of two, yet it would not follow that thoſe two are the only Original colors. The reaſons I deſire you would compare with what hath been now ſaid of White. And ſo the neceſſity of all colors to produce white might have appear'd by the Experiment *pag.* 5097, where I ſay, that if any color at the *Lens* be intercepted, the whiteneſs (which is compounded of them all) will be changed into (the reſult of) the other colors.

However, ſince there ſeems to have happened ſome miſunderſtanding between us, I ſhall endeavor to explain myſelf a little further in theſe things according to the following method.

Defini-

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Definitions.

1 I call that Light homogeneal, ſimilar or uniform, whoſe rays are equally refrangible.

2. And that heterogeneal, whoſe rays are unequally refrangible.

Note. There are but three affections of Light in which I have obſerved its rays to differ, *viz.* Refrangibility, Reflexibility, and Color; and thoſe rays which agree in refrangibility agree alſo in the other two, and therefore may well be defined homogeneal, eſpecially ſince men uſually call thoſe things homogeneal, which are ſo in all qualities that come under their knowledg, though in other qualities that their knowledg extends not to there may poſſibly be ſome heterogeneity.

3. Thoſe colors I call ſimple, or homogeneal, which are exhibited by homogeneal light.

4. And thoſe compound or heterogeneal, which are exhibited by heterogeneal light.

5. Different colors I call not only the more eminent ſpecies, red, yellow, green, blew, purple, but all other the minuteſt gradations; much after the ſame manner that not only the more eminent degrees in Muſick, but all the leaſt gradations are eſteemed different ſounds.

Propoſitions.

1. The Sun's light conſiſts of rays differing by indefinite degrees of Refrangibility.

2. Rays which differ in refrangibility, when parted from one another do proportionally differ in the colors which they exhibit. Theſe two Propoſitions are matter of fact.

3. There are as many ſimple or homogeneal colors as degrees of refrangibility. For, to every degree of refrangibility belongs a different color, by *Prop.* 2. And that color is ſimple by *Def.* 1, and 3.

4. Whiteneſs in all reſpects like that of the Sun's immediate light and of all the uſual objects of our ſenſes cannot be compounded of two ſimple colors alone. For ſuch a compoſition muſt be made by rays that have only two degrees of refrangibility, by *Def.* 1. and 3; and therefore it cannot be like that of the Suns light, by *Prop.* 1; Nor, for the ſame reaſon, like that of ordinary white objects.

5. Whiteneſs

5. Whiteneſs in all reſpects like that of the Sun's immediate light cannot be compounded of ſimple colors without an indefinite variety of them. For to ſuch a compoſition there are requiſite rays indued with all the indefinite degrees of refrangibility, by *Prop.* 1. And thoſe infer as many ſimple colors, by *Def.* 1. and 3. and *Prop.* 2. and 3.

To make theſe a little plainer, I have added alſo the Propoſitions that follow.

6. The rays of light do not act on one another in paſſing through the ſame Medium. This appears by ſeveral paſſages in the *Tranſactions pag.* 5097, 5098, 5100, and 5101. and is capable of further proof.

7. The rays of light ſuffer not any change of their qualities from refraction.

8. Nor afterwards from the adjacent quiet *Medium*. Theſe two Propoſitions are manifeſt *de facto* in homogeneal light, whoſe color and refrangibility is not at all changeable either by refraction or by the contermination of a quiet *Medium*. And as for heterogeneal light, it is but an aggregate of ſeveral ſorts of homogeneal light, no one ſort of which ſuffers any more alteration than if it were alone, becauſe the rays act not on one another, by *Prop.* 6. And therefore the aggregate can ſuffer none. Theſe two *Propoſitions* alſo might be further proved apart by Experiments, too long to be here deſcribed.

9. There can no homogeneal colors be educed out of light by refraction which were not commixt in it before: Becauſe, by *Prop.* 7, and 8, Refraction changeth not the qualities of the rays, but only ſeparates thoſe which have divers qualities, by meanes of their different Refrangibility.

10. The Sun's light is an aggregate of an indefinite variety of homogeneal colors; by *Prop.* 1, 3, and 9. And hence it is, that I call homogeneal colors alſo primitive or original. And thus much concerning Colors.

Monſieur *N.* has thought fit to inſinuate, that the aberration of rays (by their different refrangibility) is not ſo conſiderable a diſadvantage in glaſſes as I ſeemed to be willing to make men believe, when I propounded concave mirrors as the only hopes of perfecting Teleſcopes. But if he pleaſe to take his pen and compute the errors of a Glaſs and Speculum that collect rays at equal diſtances, he will find how much he is miſtaken, and that I have not been extravagant, as he imagins, in preferring Reflexions. And as for what he ſays of the difficulty of the praxis, I know it is very difficult, and by thoſe ways which he attempted it I believe it unpracticable. But there is a way inſinuated in the *Tranſactions pag.* 3080. by which it is nct improbable but that as much may be done in large Teleſcopes, as I have thereby done in ſhort ones, but yet not without more then ordinary diligence and curioſity.

An Extract of Mr. Isaac Newton's *Letter, written to the Publisher from* Cambridge April 3. 1673. *concerning the Number of* Colors, *and the Necessity of mixing them all for the production of* White; *as also touching the Cause why a Picture cast by Glasses into a darkned room appears so distinct notwithstanding its Irregular refraction: (Which Letter, being an Immediat answer to that from* Paris, *printed* N°.96.p.6086. *of these* Tracts, *should also, if it had not been mis-laid, have immediately followed the same.)*

IT seems to me, that *N.* takes an improper way of examining the nature of *Colors*, whilst he proceeds upon compounding those that are already compounded; as he doth in the former part of his Letter. Perhaps he would sooner satisfie himself by resolving Light into Colors, as far as may be done by Art, and then by examining the properties of those colors apart, and afterwards by trying the effects of re-conjoining two or more or all of those; and lastly, by separating them again to examine, what changes that re-conjunction had wrought in them. This, I confess, will prove a tedious and difficult task to do it as it ought to be done; but I could not be satisfied, till I had gone through it. However, I only propound it, and leave every man to his own method.

As to the Contents of his Letter, I conceive, my former Answer to the *Quære* about the *Number* of *Colors* is sufficient, which was to this effect; That all Colors cannot practically be derived out of the *Yellow* and *Blew*, and consequently that those *Hypotheses* are groundless which imply they may. If you ask, What colors cannot be derived out of *yellow* and *blew*? I answer, none of all those which I defin'd to be Original; and if he can shew by experiment, how they may, I will acknowledge my self in an error. Nor is it easier to frame an *Hypothesis* by assuming only two Original colors rather than an indefinit variety; unless it be easier to suppose, that there are but two figures, sizes and degrees of velocity or force of the Æthereal corpuscles or pulses, rather than indefinit variety; which certainly would be a harsh supposition. No man wonders at the indefinit variety of Waves of the Sea, or of sands on the shore; but,

but, were they all but two fizes, it would be a very puzling *phænomenon*. And I ſhould think it as unaccountable, if the ſeveral parts or corpuſcles, of which a ſhining body conſiſts, which muſt be ſuppos'd of various figures, ſizes and motions, ſhould impreſs but two ſorts of motion on the adjacent Æthereal *medium*, or any other way beget but two ſorts of Rays. But to examine, how Colors may be explain'd *hypothetically*, is beſides my purpoſe. I never intended to ſhew, wherein conſiſts the Nature and Difference of colors, but only to ſhew, that *de facto* they are Original and Immutable qualities of the Rays which exhibit them; and to leave it to others to explicate by Mechanical *Hypotheſes* the Nature and Difference of thoſe qualities: which I take to be no difficult matter. But I would not be underſtood, as if their Difference conſiſted in the Different Refrangibility of thoſe rays; for, that different Refrangibility conduces to their production no otherwiſe, than by ſeparating the Rays whoſe qualities they are. Whence it is, that the ſame Rays exhibit the ſame Colors when ſeparated by any other means; as by their different *Reflexibility*, a quality not yet diſcourſed of.

In the next particular, where *N.* would ſhew, that it is not neceſſary to mix all Colors for the production of *White*; the mixture of *Yellow*, *Green* and *Blew*, without *Red* and *Violet*, which he propounds for that end, will not produce *White*, but *Green*; and the brighteſt part of the Yellow will afford no other colour but Yellow, if the Experiment be made in a room well darkn'd, as it ought; becauſe the Colour'd light is much weaken'd by the Reflexion, and ſo apt to be diluted by the mixing of any other ſcattering light. But yet there is an Experiment or two mention'd in my Letter in the *Tranſactions* *Numb.* 88, by which I have produced *White* out of two colors alone, and that variouſly, as out of *Orange* and a *full Blew*, and out of *Red* and *pale Blew*, and out of *Yellow* and *Violet*, as alſo out of other pairs of Intermediat colors. The moſt convenient Experiment for performing this, was that of caſting the colors of one Priſme upon thoſe of another, after a due manner. But what *N.* can deduce from hence, I ſee not. For the two colors were compounded of all others, and ſo the reſulting White, (to ſpeak properly,) was compounded of them all,

 and

and only de-compounded of those two. For *instance*, the *Orange* was compounded of Red, Orange, Yellow and some Green; and the *Blew*, of Violet, full Blew, light Blew, and some Green, with all their Intermediat degrees; and consequently the Orange and Blew together made an Aggregate of all colors to constitute the White. Thus, if one mix red, orange and yellow Powders to make an Orange; and green, blew and violet colors to make a Blew; and lastly, the two mixtures, to make a Grey; that Grey, though de-compounded of no more than two Mixtures, is yet compounded of all the six Powders, as truly as if the powders had been all mixt at once.

This is so plain, that I conceive there can be no further scruple; especially to them who know how to examine, whether a colour be simple or compounded, and of what colors it is compounded; which having explained in another place, I need not now repeat. If therefore *N.* would conclude any thing, he must shew, how White may be produced out of two *Un-compounded* colors; which when he hath done, I will further tell him, why he can conclude nothing from that. But I believe, there cannot be found an Experiment of that kind; because, as I remember, I once tryed, by gradual succession, the mixture of all pairs of Un-compounded colors; and, though some of them were paler, and nearer to White, than others, yet none could be truly call'd White. But it being some years since this tryal was made, I remember not well the circumstances, and therefore recommend it to others to be tryed again.

In the last place, had I thought, the Distinctness of the Picture, which (for *instance*) a Twelf foot Object glass casts into a darken'd room, to be so contrary to me as *N.* is pleased to affirm, I should have waved my Theory in that point before I propounded it. For, that I had thought on that difficulty, you may easily guess by an expression, somewhere in my first Letter *, to this purpose; That I wonder'd, how Telescopes could be brought to so great perfection by Refractions which were so Irregular. But, to take away the difficulty, I must acquaint you *first*, That, though I put the greatest Lateral error of the rays from one another to be about $\frac{1}{50}$ of the Glasses diameter; yet their greater error from the Points on which they ought to fall, will be

* *See* Numb. 80. p. 3079.

but

but $\frac{1}{100}$ of that diameter : And *then*, that the rays, whose error is so great, are but very few in comparison to those, which are refracted more Justly ; for, the rays which fall upon the middle-parts of the Glass, are refracted with sufficient exactness, as also are those that fall near the *perimeter* and have a *mean* degree of Refrangibility ; So that there remain only the rays, which fall near the perimeter and are *most* or *least* refrangible to cause any sensible confusion in the Picture. And these are yet so much further weaken'd by the greater space, through which they are scatter'd, that the Light which falls on the due point, is infinitely more dense than that which falls on any other point round about it. Which though it may seem a *Paradox*, yet is certainly demonstrable. Yea, although the Light, which passes through the middle parts of the Glass, were wholly intercepted, yet would the remaining light convene infinitely more dense at the due points, than at other places. And by this excess of Density, the Light, which falls *in* or invisibly *near* the just point, may, I conceive, strike the *sensorium* so vigorously, that the impress of the weak light, which errs round about it, shall, in comparison, not be strong enough to be animadverted, or to cause any more sensible confusion in the Picture than is found by Experience.

This, I conceive, is enough to shew, Why the Picture appears so distinct, notwithstanding the Irregular refraction. But, if this satisfie not, *N.* may try, if he please, how distinct the Picture will appear, when all the *Lens* is cover'd excepting a little hole next its edge on one side only : And, if in this case he please to measure the breadth of the colors thus made at the edge of the Suns picture, he will perhaps find it to approach nearer to my proportion than he expects.

An

An Anſwer (to the former Letter,) written to the Publiſher June 10.1673. *by the ſame Pariſian Philoſopher, that was lately ſaid to have written the Letter already extant in* N°. 96.p.6086.

TOuching the Solutions, given by M. *Newton* to the ſcruples by me propos'd about his Theory of Colors, there were matter to anſwer them, and to form new difficulties; but ſeeing that he maintains his opinion with ſo much concern, I liſt not to diſpute. But what means it, I pray, that he ſaith; *Though I ſhould ſhew him, that the White could be produced of only two Un-compounded colors, yet I could conclude nothing from that.* And yet he hath affirm'd in *p.* 3083. of the *Tranſactions*, that to compoſe the White, all primitive colors are neceſſary.

As to the manner, whereby he reconciles the effect of Convex glaſſes for ſo well aſſembling the rays, with what he eſtabliſhes concerning the different refrangibility, I am ſatisfied with it; but then he is alſo to acknowledge, that this aberration of the rays is not ſo diſadvantagious to Optic glaſſes as he ſeems to have been willing to make us believe, when he propoſed *Concave ſpeculums* as the only hopes of perfecting Teleſcopes. His invention certainly was very good; but, as far as I could perceive by experience, the defect of the Matter renders it as impoſſible to execute, as the difficulty of the Form obſtructs the uſe of the *Hyperbole* of M. *Des-Cartes*: So that, in my opinion, we muſt ſtick to our Spheric Glaſſes, whom we are already ſo much obliged to, and that are yet capable of greater perfection, as well by increaſing the length of Teleſcopes, as by correcting the nature of Glaſs it ſelf. *So far He.*

To this Letter is to be referr'd that, which is already extant in N.96.p.6087. *as being an Anſwer thereto.*

(217) *Numb.* 110.

PHILOSOPHICAL TRANSACTIONS.

Januar. 25. $16\frac{74}{75}$.

The CONTENTS.

A Letter of the Learn'd Franc. Linus, *to a Friend of his in* London, *animadverting upon Mr.* Isaac Newton's *Theory of Light and Colors, formerly printed in these Tracts.*

Honoured Sir,

UNderstanding, that things of the nature I now write, are always welcom unto you, from what hand soever they come, I thought good, though unknown to you, to give you notice, That perusing lately the *Philosophical Transactions,* to see what I could find therein, in order to a little Treatise of Opticks I have in hand; I lighted in page 3075. upon a Letter of Mr. *Isaac Newton,* Professor of Mathematicks in the University of *Cambridge,* wherein he speaks of an

Experiment he tryed, by letting the Sun-beams through a little hole into a dark chamber; which passing through a glass Prism to the opposite wall, exhibited there a *Spectrum* of divers colours, but in a form much more long then broad: whereas according to the received Laws of Refraction, it should rather have appeared in a circular form. Whereupon conceiving a defect in those usual Laws of Refraction, he frames his new Theory of Light, giving to several rays, several refrangibilities, without respect to their Angles of Incidence, *&c.*

Truly, Sir, I doubt not of what this learned Author here affirms; and have my self sometimes in like circumstances observed the like difference between the length and breadth of this coloured *Spectrum*; but never found it so when the sky was clear and free from clouds, near the Sun: but then only appeared this difference of length and breadth, when the Sun either shined through a white cloud, or enlightned some such clouds near unto it. And then indeed it was no marvel, the said *Spectrum* should be longer then broad; since the cloud or clouds, so enlightned, were in order to those colours like to a great Sun, making a far greater Angle of Intersection in the said hole, then the true rays of the Sun do make; and therefore are able to enlighten the whole length of the Prism, and not only some small part thereof, as we see enlightned by the true Sun-beams coming through the same little hole. And this we behold also in the true Sun-beams, when they enlighten the whole Prism: for, although in a clear Heaven, the rays of the Sun, passing through the said hole, never make a *Spectrum* longer then broad, because they then occupy but a small part of the Prism; yet if the hole be so much bigger as to enlighten the whole Prism, you shall presently see the length of the *Spectrum* much exceed its breadth; which excess will be always so much the greater, as the length of the Prism exceeds its breadth. From whence I conclude, that the *Spectrum*, this learned Author saw much longer then broad, was not effected by the true Sun-beams, but by rays proceeding from some bright cloud, as is said; and by consequence, that the Theory of Light grounded upon that Experiment cannot subsist.

What I have here said, needs no other confirmation than meer experience, which any one may quickly try; neither have I only tryed the same upon this occasion, but near 30 years ago shewed the same, together with divers other Experiments of Light, to that worthy Promoter of Experimental Philosophy, Sr. *Kenelm Digby*, who coming into these parts to take the Spaw-Waters, resorted oftentimes

to

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to my darkned Chamber, to ſee thoſe various Phænomena of Light made by divers Refractions and Reflexions, and took Notes upon them; which induſtry if they alſo had uſed, who endeavour to explicate the aforeſaid difference between the length and breadth of this coloured *Spectrum*, by the received Laws of Refraction, would never have taken ſo impoſſible a task in hand.

The reſt is, Honoured Sir, that it is far from my intent, that the miſtake here mentioned do any way derogate from that learned perſon: Which truly might have happened to my ſelf, if at my firſt tryal thereof, the Sun had been in a white cloud, as it ſeems, it happened to him. Wherefore ceaſing further to trouble you, I reſt,

Yours to command, *Francis Linus.*

6 Octob. 1674.

Sir, *An Anſwer to this Letter.*

THE Letter you thought fit to write by way of Animadverſion upon Mr. *Newton*'s new Theory of Light and Colors, grounded upon an Experiment of letting the Sun-beams through a little hole into a dark chamber, ſeems to need no other Anſwer but this, That you would be pleaſed to look upon and conſider the Scheme in Mr. *Newton*'s 2d Anſwer to *P. Pardies* in *Numb.* 85. of the *Ph. Tranſactions*; and reſt aſſured, that the Experiment, as it is repreſented, was tryed in clear days, and the Priſm placed cloſe to the hole in the window, ſo that the Light had no room to diverge, and the colour'd Image made not parallel (as in that conjecture) but tranſverſe to the axis of the Priſm.

London, Decemb. 17. 1674.

A Letter of Mr. Franc. Linus, *written to the Publisher from* Liege *the* 25th *of* Febr. 1675. st.n. *being a Reply to the Letter printed in* Numb. 110. *by way of Answer to a former Letter of the same Mr.* Linus, *concerning Mr.* Isaac Newton's *Theory of Light and Colours.*

Honoured Sir,

IN yours of *Dec.* 17. which I received about the end of *Jan.* you say, I may rest assured, *First*, that the Experiment was made in clear days. *Secondly*, that the Prism was placed close to the hole, so that the light had no room to diverge: And *thirdly*, that the Image was not Parallel (as I conjectured) but Transverse to the Axis of the Prism. Truly, Sir, if these Assertions be admitted, they do indeed directly cut off what I said of Mr. *Newton*'s being deceived by a bright cloud. But if we compare them with Mr. *Newton*'s Relation of the Experiment in the *Phil. Transactions, N.* 80 *p.* 3076. it will evidently appear, they cannot be admitted as being directly contrary to what is there delivered. For there he tells us, the ends of the coloured Image, he saw on the opposit wall, near five times as long as broad, *seemed to be Semicircular.* Now these Semicircular Ends are never seen in a clear day, as Experience shews. From whence follows against the *first* Assertion, That the Experiment was not made in a clear day. Neither are those Semicircular Ends ever seen, when the Prism is placed close to the Hole; which contradicts the *second* Assertion. Neither are they ever seen, when the Image is Transverse to the length or Axis of the Prism; which directly opposes the *third* Assertion. But if in any of these three Cases, the Image be made so much longer than broad (as easily it may, by turning the Prism a little about its Axis) near five times as long as broad, than the one End thereof will run out into a sharp Cone or Pyramis like the flame of a Candle, and the other into a Cone somewhat more blunt; both which are far from seeming Semicircular: Whereas, if the Image be made not in a clear day, but with a bright cloud, and the Prism not placed close to the Hole, but in a competent distance from the same (as you see it placed in the Scheme of the Experiment in *N.* 84. *p.* 4091.) then these Semicircular Ends always appear with the sides thereof straight lines just as Mr. *Newton* there describes them. Neither is the length of the Image Tranfverfe, but Parallel to the length of the Prifm. Out of all which evidently follows, that the Experiment was not made in a clear day; nor with the Prifm clofe to the Hole; nor yet with the Image Tranfverfe(as is now affirmed,) but by a bright Cloud, and a Parallel Image (as I conjectured;) and I hope you will alfo now fay, I had good reafon fo to conjecture, fince it fo well agrees with the Relation. And Experience will alfo fhew you, if you pleafe to make tryal, as it was made, in a dark Chamber, and obferve the difference between fuch an Image made by a bright Cloud, and another made by the immediate rayes of the Sun: For, the former you fhall always find Parallel, with the Ends Semicircular; but the latter you fhall find Tranfverfe, with the Ends Pyramidical, as aforefaid, whenfoever it appears fo much longer than broad.

More might be faid out of the fame Relation, to fhew that the Image was not Tranfverfe. For, if it had been Tranfverfe, Mr. *Newton*, fo well skilled in Opticks, could not have been furprifed (as he fays he was) to fee the length thereof fo much to exceed the breadth; it being a thing fo obvious and eafie to be explicated by the ordinary Rules of Refraction. That other place alfo, in the next *page* 3077.(where he fays, the Incident Refractions were made in the Experiment equal to the Emergent,) proves again that the faid oblong Image was not Tranfverfe, but Parallel. For it is impoffible, the Tranfverfe Image fhould be fo much longer than broad, unlefs thofe two Refractions be made very unequal, as both the computation according to the common Rules of Refraction, and Experience teftifie. Wherefore Mr. *Newton* had no reafon to tax (in *pag*. 4091.) P. *Pardies* of Hallucination, for making in *page* 4088. thofe two Refractions very unequal: For, that learned Optike very well faw, that in a clear day fo great an inequality of length and breadth could not be made, unlefs thofe two Refractions were alfo made very unequal. Thefe places, I fay, might be added to the former, and further here explicated if need were; but there being no need, I ceafe to detain you any longer herein.

Mr.

(500)

Mr. Isaac Newton's *Considerations on the former Reply; together with further Directions, how to make the Experiments controverted aright: Written to the Publisher from* Cambridge, Novemb. 13: 1675.

SIR,

WHen you shew'd me Mr. *Line*'s second Letter, I remember I told you, that I thought an answer in writing would be insignificant, because the dispute was not about any Ratiocination, but my veracity in relating an Experiment, which he denies will succeed as it is described in my printed Letters: For this is to be decided not by discourse, but new tryal of the Experiment. What it is that imposes upon Mr. *Line* I cannot imagin; but I suspect he has not tryed the Experiment since he acquainted himself with my Theory, but depends upon his old notions taken up before he had any hint given to observe the figure of the coloured Image. I shall desire him therefore, before he returns any answer, to try it once more for his satisfaction, and that according to this manner.

Let him take any Prisme, and hold it so that its Axis may be perpendicular to the Sun's rays, and in this posture let it be placed as close as may be to the hole through which the Sun shines into a dark room, which hole may be about the bigness of a Pease. Then let him turn the Prism slowly about its Axis, and he shall see the colours move upon the opposite wall first towards that place to which the Sun's direct light would pass, if the Prism were taken away, and then back again. When they are in the middle of these two contrary motions, that is, when they are nearest that place to which the Sun's direct ray tends, there let him stop; for then are the rays equally refracted on both sides the Prism. In this posture of the Prism let him observe the figure of the colours, and he shall find it not round as he contends, but oblong, and so much the more oblong as the Angle of the Prism, comprehended by the refracting plains, is bigger, and the wall, on which the colours are cast, more distant from the Prism; the colours red, yellow, green, blew, purple, succeeding in order not from one side of the figure to the other, as in Mr. *Line*'s conjecture, but from one end to the other; and the length of the Figure being not parallel but tranverse to the Axis of the Prism. After this manner I used to try the Experi-

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ment: For I have try'd it often; ſometimes to obſerve the circumſtances of it, ſometimes in order to further Experiments, and ſometimes to ſhow it to others, and in all my tryals the ſucceſs was the ſame. But whereas Mr. *Line* thinks, I tryed it in a cloudy day, and placed the Priſm at a great diſtance from the hole of the window; the Experiment will not ſucceed well if the day be not clear, and the Priſm placed cloſe to the hole, or ſo near at leaſt, that all the Sun's light that comes from the hole may paſs through the Priſm alſo, ſo as to appear in a round form if intercepted by a paper immediately after it has paſt the Priſm.

When Mr. *Line* has tryed this, I could wiſh, he would proceed a little further to try that which I call'd the *Experimentum Crucis*, ſeeing (if I miſ-remember not) he denies that as well as the other. For when he has tryed them (which by his denying them, I know he has not done yet as they ſhould be tryed) I preſume he will reſt ſatisfied.

Three or four days after you gave me a ſight of Mr. *Line*'s ſecond Letter, I remember I thereupon ſhow'd the firſt of theſe two Experiments to that Gentleman whom you found with me, when you gave me that viſit, and whilſt I was ſhewing it to him, *A.H.* (a member of the *R. Society*) came in and I ſhewed it to him alſo. And you may remember, that *R.H.* two or three years agoe in a Letter read before the *R. Society*, and tranſmitted to me, gave teſtimony not only to the Experiments queſtioned by Mr. *Line*, but to all thoſe ſet down in my firſt Letter about Colours, as having tryed them himſelf; and when you read Mr. *Line*'s Letter at a meeting of the ſaid *Society*; and was pleaſed to do me the favour to propound the Experiment to be tryed in their preſence, *R.H.* ſpake of it to them as a thing not to be queſtioned. But if it have not yet been tryed before them, and any of them, upon Mr. *Line*'s confidence, doubt of it, I promiſe when I ſhall have the happineſs to be at any more of their Aſſemblies, upon the leaſt hint, to ſhew 'em the tryal of it; and I hope, I ſhall not be troubleſome, becauſe it may be tryed (though not ſo perfectly) even without darkning a room, or the expence of any more time than half a quarter of an hour; although, if Mr. *Line* perſiſt in his denyal of it, I could wiſh it might be tryed ſooner there, than I ſhall have an opportunity to be among them.

An

An Extract of another Letter of Mr.* Newton, *written to the Publisher the* 10th *of* January 167$\frac{5}{6}$, *relating to the same Argument.

——BY Mr. *Gascoin*'s Letter * one might suspect, that Mr. *Linus* tryed the Experiment some other way than I did; and therefore I shall expect, till his friends have tryed it according to my late Directions. In which tryal it may possibly be a further guidance to them, to acquaint them, that the Prism casts from it several Images: *One* is, that *Oblong* one of *Colours* which I mean; and this is made by two Refractions only. *Another* there is, made by two Refractions and an intervening Reflexion; and this is *Round* and *Colourless*, if the Angles of the Prism be exactly equal; but if the Angles at the Reflecting base be not equal, it will be *colour'd*, and that so much the more, by how much unequaller the Angles are, but yet not much *unround*, unless the angles be very unequal. A *third* Image there is, made by one single Reflexion, and this is always *round* and *colourless*. The only danger is in mistaking the *second* for the *first*. But they are distinguishable not only by the Length and Lively colors of the *first*, but by it's different Motion too: For, whilst the Prism is turned continually the same way about it's *axis*, the *second* and *third* move swiftly, and go always on the same way till they disappear; but the *first* moves slow, and grows continually slower till it be stationary, and then turns back again, and goes back faster and faster, till it vanish in the place where it began to appear.

* This Letter was written to the Publisher, *Decemb.* 15. 1675. from *Leige*, where Mr. *Gascoines*, having been a Scholar of Mr. *Linus*, now deceased, resides. In it are contained these words, to which Mr. *Newton*, to whom it was communicated, seems here to have respect; *viz.* Mr. *Linus tryed the Experiment again and again, and called divers on purpose to see it, nor ever made difficulty to shew it to any one, who either by chance came to his chamber as he was doing it, or shewed the least desire to see the same. So that, for point of Experience, Mr.* Newton *cannot be more confident on his side, than we are here on the other; who are fully persuaded, that, unless the diversity of placing the Prism, or the bigness of the Hole, or some other such circumstance, be the cause of the difference betwixt them, Mr.* Newton's *Experiment will hardly stand.*

If without darkning their Room they hold the Prism at their window in the Sun's open Light, in such a posture that it's *axis* be perpen perpendicular to the Sun-beams,and then turn it about its *axis*,they cannot miſs of ſeeing the *firſt* Image; which having found,they may double up a paper once or twice, and make a round hole in the middle of it about $\frac{1}{2}$ or $\frac{3}{4}$ of an inch broad, and hold the paper immediately before the Priſm, that the Sun may ſhine on the Priſm through that hole ; and the Priſm being ſtay'd, and held ſteddy in that poſture which makes the Image Stationary ; if the Image then fall directly on an oppoſite wall, or on a ſheet of paper placed at the wall, ſuppoſe 15 or 20 foot from the Priſm, or further off;they will ſee the Image in ſuch an *Oblong* figure as I have deſcribed,with the *Red* at one end, the *Violet* at the other, and a *Blewiſh green* in the middle : And if they obſcure their Room, as much as they can, by drawing curtains or otherwiſe, it will make the Colours the more conſpicuous.

This direction I have ſet down, that no body, into whoſe hands a Priſm ſhall happen,may find difficulty or trouble in trying it.But when Mr. *Linus*'s friends have tryed it thus, they may proceed to repeat it in a dark Room with a *leſs* hole made in their window ſhut. And then I ſhall deſire, that they will ſend you a full and clear deſcription, How they tryed it, expreſſing the length, breadth and angles of the Priſm; its poſition to the Incident rays and to the window ſhut; the bigneſs of the hole in the window ſhut through which the Sun ſhined on the Priſm; what ſide of the Priſm the Sun ſhin'd on; and at what ſide the light came out of it again; the diſtance of the Priſm from the oppoſite paper or wall on which the Refracted light was caſt perpendicularly; and the length,breadth, and figure of the ſpace there illuminated by that light,and the ſcituation of each colour within that figure. And, if they pleaſe to illuſtrate their deſcription with a Scheme or two, it will make the buſineſs plainer. By this means, if there be any difference in our way of experimenting, I ſhall be the better enabled to diſcern it, and give them notice, where the failure is, and how to rectifie it. I ſhould be glad too, if they would favour me with a deſcription of the Experiment,as it hath been hitherto tryed by Mr. *Linus*, that I may have an opportunity to conſider, what there is in that which makes againſt me.

So far Mr. Newton;which was thought fit to make publick with the reſt,that ſo the Curious every where, who have a mind to try the Experiment, may find the fuller directions for their tryal.

An

A particular Anſwer of Mr. Iſaak Newton *to Mr.* Linus *his Letter, printed in* Numb. 121. p.499. *about an Experiment relating to the New Doctrine of Light and Colours: This Anſwer ſent from Cambridge in a Letter to the Publiſher Febr.* 29. 167$\frac{5}{6}$.

Sir,

BY reading Mr. *Linus*'s Letter when you ſhew'd it to me at *London*, I retained only a general remembrance, that Mr. *Linus* deny'd what I affirmed, and ſo could lately ſay nothing in particular to it; but having the opportunity to read it again in *Numb.* 121. of the *Tranſactions*, I perceive he would perſwade you, that the information you gave him about the Experiment is as inconſiſtent with my printed Letters as with experience; and therefore, leſt any who have not read thoſe Letters ſhould take my ſilence in this point for an acknowledgment, I thought it not amiſs, to ſend you ſomething in anſwer to this alſo.

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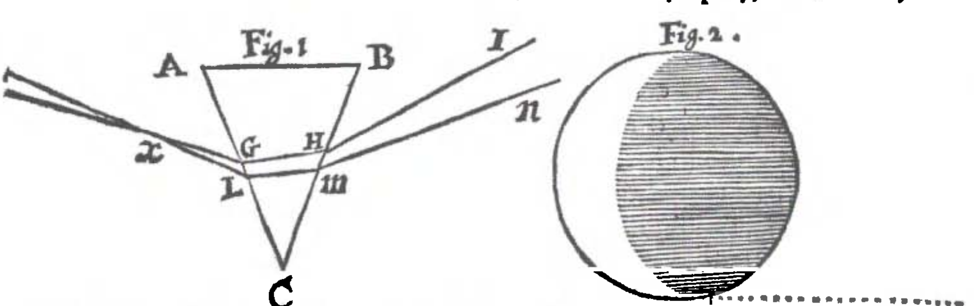
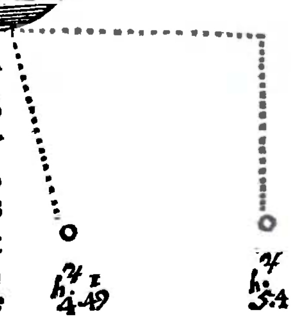
He tells you that, *Whereas you assure him, First, that the Experiment was made in clear days; secondly, that the Prism was placed close to the hole, so that the light had no room to diverge; and thirdly, that the Image was not parallel but transverse to the axis of the Prism: If these Assertions be compared with my Relation of the Experiment in the Phil. Transaction N. 80. p. 3076. it will evidently appear, they cannot be admitted as being directly contrary to what is there delivered.* His reasons are these:

First, that I said, *the ends of the long Image seemed semicircular, which*, saies he, *never happens in any of the three cases above-said.* But this is not to set me at odds with my self, but with the experiment; for it is there described to happen in them all; and I still say, it doth happen in them. Let others try the Experiment, and judge.

Further he saies, that *the Prism is placed at a distance from the hole in the Scheme of the Experiment in* N. 84. p 4091. But, what if it were so *there?* For, that is the Scheme of a *demonstration*, not of the *experiment*, and would have served for the demonstration, had the distance been put twenty times greater than it is. In the Schemes of the Experiment *N.* 80. *p.* 3086, and *N.* 82. *p.* 5016. it is represented close, and close enough in the Scheme, *N.* 83. *p.* 4061: But Mr. *Linus* thought fit to wink at these, and pitch upon the Scheme of a Demonstration, and such a Scheme too as hath no hole at all represented in it. For, the Scheme † *Numb.* 84. *p.* 491 is this;



in which the rays are not so far distant from one another at *GL*, but that the hole, had I exprest it, might have been put there, and yet have comprehended them. But if we should put the hole at *x*, their decussation; yet will it not be any thing to his purpose; the distance *x G* or *x L* being but about half the breadth of a side of the Prism ($\frac{1}{2}$AC) which I conceive is not the twentieth part of the distance requisite in his conjecture.



† *See Fig.* 1.

Thirdly,

3. He says, that *more might be said out of my relation to shew, that the Image was not transvers, for if it had been transvers, I could not have been surprized (as I said I was) to see the length thereof so much exceed the breadth, it being a thing so obvious & easie to be explicated by the ordinary rules of Refraction.* But on the contrary, it may rather be said, that if the Image had been parallel, I could not have been surprized to see the length thereof so much exceed the breadth, it being a thing so extreamly obvious as not to need any explication. For who that had but common sense, and saw the whole Prism or a good part of it illuminated, could not expect the light should have the same long figure upon the wall that it had when it came out of the Prism? Mr. *Linus* therefore, while he would strengthen his argument by representing me well skilled in Opticks, does but overthrow it. But whereas he sayes, *I could not have been surprized at the length, had the Image been parallel, it being a thing so obvious and easy to be explicated by the ordinary rules of refraction:* Let any Man take the Experiment intire as I have there delivered it, that is, with this condition, that *the refractions on both sides the Prism were equal*, and try if he can reconcile it with the ordinary rules of refraction. On the contrary, he may find the impossibility of such a reconciliation, demonstrated in my Answer to *P. Pardies N.* 84, *p.* 4091.

In the last place, he objects, that my saying in *N.* 80, *p.* 3077, *that the incident refractions were in the Experiment equal to the emergent*, proves again, that the long Image was parallel. And yet that very saying is a sufficient argument, that I meant the contrary, because it becomes wholly impertinent, if apply'd to a parallel image; but in the other case is a very necessary circumstance. What is added therefore of *P. Pardies*, might have been spared, especially since that Learned Person understood my discourse to be meant of a transvers Image, and acquiesced in my Answers.

This in answer to Mr. *Linus*'s Letter: And now to take away the like suspicions from his Friends, if my declaration of my meaning satisfie not, I shall note some further passages in my Letters, whereby they may see, how I was to be understood from the beginning, as to the aforesaid three circumstances.

For the *Day*; I express every where that the Experiment was tried in the Sun's light, and in *N.* 80. *p.* 3077, that the breadth of the Image by measure answered to the Sun's diameter: But because it is pretended, I was imposed upon, I would ask, what the Experiment as it is advanced to that which I called the *Experimentum*

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tum Crucis, can have to do with a cloudy day? For, if the *Experimentum Crucis* (which is that which I depend on) can have nothing to do with a cloudy day, then is it to no purpofe to talk of a cloudy day in the firft Experiment, which does but lead on to that. But if this fatisfie not, let the *Tranfactions N.* 83. *p.* 4060, be confulted: For. there I tell you, how *by* applying a *Lens* to the Prifm, the ftreight edges of the oblong Image became diftincter than they would have been *without* the *Lens*: A circumftance which cannot happen in Mr. *Linus*'s cafe of a bright Cloud.

For the *Pofition of the Prifm*; I tell you *N.* 80. *p.* 3076, that it was placed *at the Sun's entrance* into the Chamber, and in *p.* 3085. I bad to make a hole in the fhut, and *there* place the Prifm, and in the next page I fay again, that the Prifm ABC is to be fet *clofe* by the hole F of the window EG; and accordingly reprefent it clofe in the Figure. Alfo in *pag.* 3077 I tell you, that the diftance of the *Image* from the *hole* or *prifm* was 22 foot; which is as much as to fay, that the Prifm (fuppofe that fide of it next the hole) was as far from the Image as the hole it felf was, and confequently that the Prifm and Hole were contiguous. Alfo in *p.* 3078, where inftead of the Window fhut I made ufe of a hole in a loofe board, I tell you expreffly, that I placed the board *clofe* behind the Prifm. All thefe paffages are in my very firft Letter about Colours; and who therefore would imagine, that any one that had read that Letter fhould fo much as fufpect, that I placed the Prifm, I fay not at fo great a diftance as Mr. *Linus* fuppofes, but at any diftance worth confidering?

Laftly, for the *Pofition of the Image*, it is reprefented tranfvers to the axis of the Prifm in the figures *N.* 80. *p.* 3086. *N.* 83. *p.* 4061, and *N.* 85. *p.* 5016. And in *N.* 88. *p.* 5093, where I made ufe of two crofs Prifms, I tell you expreffly, that the Image was crofs to both of them at an angle of 45 degrees. The calculations alfo *N.* 80. *p.* 3077. are not to be underftood without fuppofing the Image crofs. Nor are my notions about different Refrangibility otherwife intelligible: For in Mr. *Linus*'s fuppofition, the rays that go to the two ends of the Image, are equally refracted. So for colours, the *red*, according to my defcription, falls at one end of the Image, and the *blew* at the other; which cannot happen but in a tranfvers Image. The fame pofition is alfo demonftrable from what I faid in *N.* 80. *p.* 3076, about turning the long Image into a round one, by the contrary refraction of a second Prism, further explained in *Num.* 83. *p.* 4061. For this is not to be done in Mr. *Linus* surmise of a parallel Image, and therefore had Mr. *Linus* considered it, he could never have run into that surmise.

This I suppose is enough to manifest the three particulars; any one of which being evidenced, is sufficient to take away the scruple. And therefore Mr. *Linus* Friends need not fear but that the further directions I sent them lately for trying the Experiment are the same with those I have follow'd from the beginning; nor trouble themselves about any thing but to try the Experiment right. But yet, because Mr. *Gascoin* has been pleased to insinuate his suspicion that I do differ from himself in those directions, I shall not scruple here to reduce them into particulars, and shew where each particular is to be found.

1. Then, he is to get a Prism with an angle about 60 or 65 degrees, *N.* 80, *p.* 3077, and *p.* 3086. If the angle be about 63 degrees, as that was which I made use of *N.* 80. *p.* 3077, he will find all things succeed exactly as I described them there. But if it be bigger or less, as 30, 40, 50, or 70 degrees, the Refraction will be accordingly bigger or less, and consequently the Image longer or shorter. If his Prism be pretty nearly equilateral (such as I suppose are usually sold in other places as well as in *England*) he may make use of the biggest angle. But he must be sure to place the Prism so, that the Refraction be made by the two planes which comprehend this angle. I could almost suspect, by considering some circumstances in Mr. *Linus*'s Letter, that his error was in this point, he expecting the Image should become as long by a little refraction as by a great one; which yet being too gross an error to be suspected of any Optician, I say nothing of it, but only hint this to Mr. *Gascoin*, that he may examine all things.

2. Having such a Prism, he must place it so, that its Axis be perpendicular to the rays *N.* 84, *p.* 4091, *lin.* 18, 19. A little error in this point makes no sensible variation of the effect.

3. The Prism must be so placed, that the Refractions on both sides be equal *N.* 80, *p.* 3077: which how it was to be readily done by turning it about its Axis, and staying it when you see the Image rest between too contrary motions, as I explained in my late Descriptions, so I hinted before *N.* 80. *p.* 3077, *lin.* 34, 35, 36. If there should be a little error in this point also, it can do no hurt.

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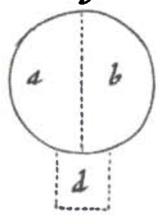
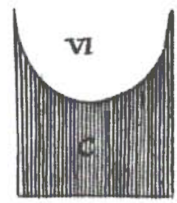
4. The Diameter of the hole I put $\frac{1}{4}$ of an inch *N.* 80, *p.* 3077, and placed the Prifm clofe to it, even fo clofe as to be contiguous, *N.* 80, *p.* 3077, *lin.* 4, 5. But yet there needs no curiofity in thefe circumftances. The hole may be of any other bignefs, and the Prifm at a diftance from the hole, , provided things be fo ordered, that the light appear of a round form, if intercepted perpendicularly at its coming out of the Prifm. Nor needs there any curiofity in the *day*. The clearer it is the better ; but if it be a little cloudy, that cannot much prejudice the Experiment, fo the Sun do but fhine diftinctly through the cloud.

Thefe things being thus ordered, if the refracted light fall perpendicularly on a wall or paper at 20 foot or more from the Prifm, it will appear in an oblong form, crofs to the axis of the Prifm, *red* at one end, and *violet* at the other; the length five times the breadth (more or lefs according to the quantity of the refraction,) the fides, ftreight lines, parallel to one another, and the ends confufed, but yet feeming femi-circular.

I hope therefore, Mr. *Linu*'s Friends will not entertain themfelves any further about incongruous *furmifes*, but try the Experiment as Mr. *Gafcoin* has promifed. And then, fince Mr. *Gafcoin* tells you, That *the Experiment being of it felf extraordinary and furprizing, and befides ufhering in new Principles into Opticks, quite contrary to the common and received, it will be hard to perfwade it as a truth, till it be made fo vifible to all as it were a fhame to deny it*: if he efteem it fo extraordinary, he may have the priviledg of making it fo vifible to all, that it will be a fhame to deny it. For, I dare fay, after his teftimony no body elfe will fcruple it. And I make no queftion but he will hit of it, it being fo plain and eafy, that I am very much at a lofs to imagine what way Mr. *Linus* took to mifs. Dat. *Cambridge Feb.* 29. $167\frac{5}{6}$.

Fig. V.





A Letter from Liege *concerning Mr.* Newton'*s Experiment of the coloured* Spectrum ; *together with ſome Exceptions againſt his Theory of* Light *and* Colours.

Hon[rd] Sir,

MR *Gaſcoigne* having received your obliging Letter of *Jan.* 18, with freſh directions from Mr. *Newton*; but wanting convenience to make the Experiment according to the ſaid inſtructions, he has requeſted me to ſupply his want. In compliance with his requeſt I have made many Trials; the iſſue whereof I here acquaint you with: next, with ſome exceptions, grounded on Experiments, againſt Mr. *Newton*'s new Theory of *Light* and *Colours*.

The vertical angle of my Priſm was 60 *deg*; the diſtance of the Wall, whereon the coloured *Spectrum* appeared, from the Window, about 18 foot: The diameter of the Hole in the Window-ſhuts in length the line *a*, which upon occaſions I contracted to half the ſaid diameter; but ſtill with equal ſucceſs as to the main of the Experiment. The refractions on both ſides the Priſm, were as near as I could make them,

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equal, and confequently about 48 *deg.*40′, the refractive power of Glafs being computed according to the *Ratio* of the *Sines* 2 to 3. The diftance of the Prifm from the hole in the Shuts was about 2 inches: The Room darkned to that degree as to equal the darkeft night, while the hole in the Shuts was covered.

Now as to the iffue of my Trials; I conftantly found the length of the coloured image (tranfverfe to the axis of the Prifm) confiderably greater than its breadth, as often as the Experiment was made on a clear day; but if a bright Cloud were near the Sun, I found it fometimes exactly as Mr. *Line* wrote you, namely broader than long, efpecially while the Prifm was placed at a great diftance from the hole. Which Experiment will not, I conceive, be queftioned by Mr. *Newton*, it being fo agreeable to the received laws of Refractions. And indeed the Obfervations of thefe two Learned perfons, as to this particular, are eafily reconcileable to each other, and both to truth; Mr *Newton* (as appears by his Letter of *Nov.* laft, wherein more fully he delivers his mind) contending only for the length of the Image (tranfverfe to the axis of the Prifm) in a very clear day; whereas Mr. *Line* only maintain'd the excefs of breadth, parallel to the fame axis, while the Sun is in a bright cloud. Though as to what is further delivered by Mr. *Newton* (*Phil.Tranfact. N.* 80. *p.* 3077; and oppofed by Mr. *Line*, *N.* 129. *p* 501.) namely that the length of the coloured Image was five times the diameter of its breadth; I never yet have found the excefs above thrice the diameter, or at moft $3\frac{1}{2}$, while the refractions on both fides the Prifm were equal. So much as to the matter of fact.

Now as to Mr. *Newton*'s Theory of *Light* and *Colours*, I confefs, his neat Sett of very ingenious and natural inferences, was to me upon the firft perufal a ftrong conjecture in favour of his new doctrine; I having formerly obferv'd the like chain of Inferences upon fearch into Natural truths. But fince feveral experiments of Refractions remain ftill untouch'd by him, I conceived, a further fearch into them would be very proper in order to a further difcovery of the truth of his Affertion. For, accordingly as they are found either agreeing with, or difagreeing from, his new Theory, they muft needs much ftrengthen

then,or wholly overthrow the fame. The Experiments I pitched upon for this purpofe, are as follow:

1. Having frequently obferved, that the form of Objects viewed in the Microfcope (or rather of the Microfcope it felf) confifts almoft in an indivifible point, I concluded, two very fmall pieces of Silk, the one fcarlet, the other violet colour, placed near together, fhould, according to Mr. *Newton*'s Theory, appear in the Microfcope in a very different degree of clarity, in regard their unequal refrangibility muft caufe the fcarlet rays or fpecies to over-reach the *Retina*, while placed in the due focus of the violet ones, and confequently muft occafion a fenfible confufion in the vifion of the former, one and the fame point of the Scarlet object affecting feveral nerves in the *Retina*. Yet upon frequent trials I have not been able to perceive any inequality in this point.

2. The fecond Experiment I made in Water. I took a brafs Ruler, and faftening thereunto feveral pieces of Silk, red, yellow, green, blew and violet, I placed it at the bottom of a fquare veffel of Water: then I retired from the Veffel fo far as not to be able to fee the aforefaid Ruler and coloured Silks otherwife than by help of the refracted Ray. Now, did Mr. *Newton*'s doctrine hold, I conceiv'd, I fhould not fee all the mentioned Colours in a ftreight line with the Ruler, in regard the unequal refrangibility of different Rays muft needs difplace fome more than others. Yet in effect, upon many Trials, I conftantly found them in as ftreight a line as the bare Ruler had appeared in.

3. To advance this Experiment, I adjoyned a fecond refraction to the former of the Water, by placing my Prifm fo as to receive *perpendicularly* the refracted *fpecies* of the Silk and Ruler; whereby only the emergent *fpecies* fuffered a fecond refraction. But ftill with equal fuccefs, as to their appearing in a ftraight line, to the eye placed behind the Prifm.

4. To thefe two Refractions I further added a third, by receiving the coloured fpecies *obliquely* upon the Prifm; whereby both incident and emergent *fpecies* fuffered their refpective refractions. But ftill with the fame fuccefs as formerly, as to the ftreight line they appeared in.

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For further aſſurance in this Experiment, leſt prepoſſeſſion, occaſioned from previous knowledge of the Silks ſcituation in a ſtreight line, might poſſibly prejudice the judgment of the eye (as ſometimes I have obſerved to happen to the judgment the Eye paſſeth upon the diſtance of Objects) I called into the room ſome unconcerned perſons, wholly ignorant what the Experiment aimed at; and demanding whether they ſaw not the coloured Silks and Ruler in a crooked line? they anſwered in the negative.

5. The next Experiment I made in uncompounded Colours (as Mr. *Newton* terms them, *Prop.* 5 *&* 13) as follows. Having caſt two coloured Images upon the Wall, ſo as the Scarlet colour of the one did fall in a ſtreight line (parallel to the Horizon) with the Violet of the other: I then looked upon both through another Priſm, and found them ſtill appear in a ſtreight line parallel to the Horizon, as they had formerly done to the naked eye. Now according to Mr. *Newton*'s Aſſertion of different refrangibility in different Rays, I conceive the Violet rays ſhould ſuffer a greater refraction in the Priſm at the eye, than the Scarlet ones, and conſequently both colours ſhould not appear in a ſtreight line parallel to the Horizon.

6. Another Experiment I made in order to ſome further diſcovery of that ſurprizing *Phænomenon* of the coloured Image, which occaſioned Mr. *Newtons* ingenious Theory of *Light* and *Colours*, as alſo his excellent invention of the reflecting *Teleſcope* and *Microſcope*. Having then ſometimes ſuſpected, that not only the direct Sun-beams, but alſo other extraneous light might poſſibly influence the coloured *Spectrum*, I hoped to diſcover the truth of this ſuſpicion by means of the Sun-ſpots, made to appear in the coloured Image by placing a Teleſcope behind the Priſm. But my endeavours proving ineffectual herein by reaſon of ſome intervening difficulties, I thought at length of a more feaſible method in order to the deſigned diſcovery, as in the following Experiment.

I faſtened a very white Paper-circle (about an inch in diameter) upon my Window-ſhuts; and beholding it through my Priſm, I found a Coloured image painted thereby upon my *Retina*, anſwerable in almoſt all reſpects to the former of the

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Sun-beams upon the Wall, eſpecially when the Paper-circle was indifferently well illuminated. This Image indeed appeared contrary to the former as to the ſcituation of Colours, that is, the Scarlet appearing above, the Violet below, though but faint. But this I was not ſurprized at, having obſerv'd upon diſſecting the eye, that objects are painted on the *Retina* after a contrary poſture to what they appear to Sight. Having thus rendred the Coloured image much more tractable than formerly it was, I conceived good hopes of ſome further diſcovery in the point mentioned.

In purſuance then of my former ſuſpicion, having fixed my Priſm in a ſteady poſture, I cauſed the paper *C* to be applied cloſe up to the Paper-circle *a b d*: whereupon the former Violet *d*, and Scarlet colour of *C* vaniſhed into whitenefs. Next, I removed the mentioned Circle from the Shuts, and placed it in the open window, ſupported only by the edge *d*: whereupon, to my aſtoniſhment, all the former Colours exchanged poſtures in the *Retina*, the Scarlet now appearing below, the Violet above; the intermediate Colours ſcarce diſcernible. And here, on the by, 'tis very remarkable, that, during this Obſervation, I clearly perceived both Blew and Scarlet-light to be tranſparent, I being able to diſcern ſeveral objects through both, namely Steeples oppoſit to my window. Whence it follows, that theſe Colours do in great part ariſe from the neighbouring light. Laſtly, I placed the Paper-circle anew, ſo as the one half *b* was faſtened to the Shuts, the other ſemicircle *a* being expoſed to the open Air. Whereupon the ſemicircle *a* became bordered with Violet above, Scarlet below; but the other ſemicircle *b* quite contrary. Hence I make the following Inferences.

V. Tab.II. Fig.5 & 6.

Firſt, That not only the Light reflected from the Paper-circle, but alſo from the ambient Air, hath great influence upon the Coloured image, eſpecially as to the Violet and Scarlet colours. Whence perchance it will not hereafter ſeem ſtrange, that the coloured *Spectrum* on the Wall is ſo long, but only that the breadth is not greater. *Secondly*, Were there a more luminous body behind the Sun, we ſhould in all likelyhood have the colours of the *Spectrum* in a contrary ſcituation to what they appear in at preſent: Whence (*thirdly*) it ſeems to follow, that the

the present scituation and order of Colours, ariseth not from any intrinsecal property of refrangibility (as maintained by Mr. *Newton*) but from contingent and extrinsecal circumstances of neighbouring objects. For accordingly as the body behind the Paper-circle was more or less illuminated than the Circle it self, all the several Colours changed their scituation.

8. The next Experiment was made in order to Mr. *Newtons* doctrine of primary Colours, as *Prop.*5. Having covered the Hole in the Window-shuts with a thin slice of *Ivory*, the transmitted light appeared yellow; but upon adding three, four, and more slices, it became red. Whence it seems to follow, that Yellowness of light is not a primary colour, but a compound of Red, &c.

9. The last Experiment was made in reference to Mr. *Newton*'s 12 *Prop.* where from his own principles he renders a very plausible Reason of a surprizing *Phænomenon*, related by Mr. *Hooke*; namely of two liquors, the one Blew, the other Red, both severally transparent, yet both, if placed together, became opake. The reason whereof, saith Mr. *Newton*, is, because if one liquor transmitted only Red, the other only Blew, no rays could pass through both.

In reference then to this point, I filled two small Glasses with flat polished bottoms, the one with *Aqua fortis*, deeply died Blew; the other with Oyl of *Turpentine*, died Red; both to that degree, as to represent all objects through them respectively Blew or Red. Then placing the one upon the other, I was able to discern several bodies through both: whereas according to Mr. *Newtons* Theory, no object should appear through both Liquors; because if one transmit only Red, the other only Blew, no rays can pass through both.

These Experimental Exceptions will not, I hope, be unwelcome to Mr. *Newton*, his only aim being the improvement of Natural knowledge, as it is also of,

Sir,

Your humble Servant,

Anthony Lucas.

Poſtſcript.

Juſt upon the cloſe of the adjoyned Letter, I received from Mr. Gaſcoine, *yours of* May *the fourth; wherein you are pleaſed to favour us with an exact account of the famous Experiment of the coloured* Spectrum, *lately exhibited before the* Royal Society. *I was much rejoyced to see the Trials of that Illuſtrious Company, agree ſo exactly with ours here, though in ſomewhat ours diſagree from Mr.* Newton, *as you will underſtand by the incloſed impartial account from,*

Sir, &c.

Mr. Newton's *Anſwer to the precedent Letter, ſent to the Publiſher.*

Sir,

THe things oppoſed by Mr. *Line* being upon Trials found true and granted me; I begin with the new queſtion about the proportion of the length of the Image to its breadth. This I call a *new one*; for, though Mr. *Line* in his laſt Letter ſpake againſt ſo great a length as I aſſign, yet, as it ſeems to me, it was not to grant any tranſverſe length ſhorter than that aſſigned by me, (for in his firſt Letter he abſolutely denied that there would be any ſuch length;) but to lay the greater emphaſis upon his diſcourſe whilſt in defence of common Optiques he was diſputing in general againſt a tranſverſe Image: And therefore in my Anſwer I did not preſcribe the juſt quantity of the refracting Angle with which I would have the Experiment repeated: which would have been a neceſſary circumſtance, had the diſpute been about the juſt proportion of the length to the breadth. Yet I added * this Note, that the bigger the angle of the Priſm is, the greater will be the length in proportion to the breadth: not imagining but that when he had found in any Priſm the length of the Image tranſverſe to the axis, he would eaſily thence conclude, that a Priſm with a greater angle would make the Image longer, and conſequently that by uſing an angle great enough he might bring it to equal or exceed the length aſſigned by me; as indeed he might: for, by taking an Angle of 70 or 75 degrees, or a little greater,

* *In my firſt Letter in* Phil. Tranſ. N. 121. p. 500.

greater, he might have made the length not only five, but ſix or eight times the breadth and more. No wonder therefore, that Mr. *Lucas* found the Image ſhorter than I did, ſeeing he tried the Experiment with a leſs Angle.

The Angle indeed which I uſed was but about 63 degrees 12 minutes, and his is ſet down 60 degrees: the difference of which from mine, being but 3 degrees 12 minutes, is too little to reconcile us, but yet it will bring us conſiderably nearer together. And if his Angle was not exactly meaſured, but the round number of 60 degrees ſet down by gueſs or by a leſs accurate meaſure (as I ſuſpect by the conjectural meaſure of the refraction of his Priſm by the *ratio* of the ſigns 2 to 3, ſet down at the ſame time, inſtead of an Experimental one,) then might it be two or three degrees leſs than 60, if not ſtill leſs: and all this, if it ſhould be ſo, would take away the greateſt part of the difference between us.

But however it be, I am well aſſured, my own obſervation was exact enough. For I have repeated it divers times ſince the receipt of Mr. *Lucas*'s Letter, and that without any conſiderable difference of my Obſervations either from one another, or from what I wrote before. And that it might appear experimentally, how the increaſe of the Angle increaſes the length of the Image, and alſo that no body who has a mind to try the Experiment exactly, might be troubled to procure a Priſm which has an angle juſt of the bigneſs aſſigned by me; I tried the Experiment with divers Angles, and have ſet down my Trials in the following Table; where the firſt column expreſſes the ſix Angles of two Priſms which I uſed, which were meaſured as exactly as I could by applying them to the angle of a Sector; and the ſecond column expreſſes in inches the length of the Image made by each of thoſe Angles; its breadth being two inches, its diſtance from the Priſm 18 feet and four inches, and the breadth of the hole in the Window-ſhut $\frac{1}{4}$ of an inch.

	The Angles of degr.	*min.*	*The Lengths of the Image.*
The firſt Priſm	56	10	$7\frac{3}{4}$
	60	24	$9\frac{1}{2}$
	63	26	$10\frac{1}{3}$

The

	The Angles of degr.	*min.*	*The Lengths of the Image.*
the second Prism.	54	0	$7\frac{1}{3}$
	62	12	$10\frac{1}{8}$
	63	48	$10\frac{3}{4}$

You may perceive, that the length of the Images in respect of the angles that made them, are something greater in the second Prism than in the first; but that was because the glass, of which the second Prism was made, had the greater refractive power.

The days in which I made these Trials were pretty clear, but not so clear as I desired, and therefore afterwards meeting with a day as clear as I desired, I repeated the Experiment with the second Prism, and found the lengths of the Image made by its several angles to be about $\frac{1}{4}$ of an inch greater than before, the measures being those set down in this Table.

	The Angles of degr.	*min.*	*The Lengths of the Image.*
the second Prism	54	0	$7\frac{2}{3}$
	62	12	$10\frac{1}{2}$
	63	48	11

The reason of this difference I apprehend was, that in the clearest days the light of the white skies, which dilutes and renders invisible the faintest Colours at the ends of the Image, is a little diminished in a clear day, and so gives leave to the Colours to appear to a greater length; the Suns light at the same time becoming brisker, and so strengthning the Colours and making the faint ones at the two ends more conspicuous. For I have observed, that in days something cloudy, whilst the Prism has stood unmoved at the window, the Image would grow a little longer or a little shorter, accordingly as the Sun was more or less obscured by thin Clouds which passed over it; the Image being shortest when the Cloud was brightest and the Suns light faintest. Whence it is easie to apprehend, that, if the light of the Clouds could be quite taken away, so that the

Sun

Sun might appear ſurrounded with darkneſs, or if the Suns light were much ſtronger than it is, the colours would ſtill appear to a greater length.

In all theſe Obſervations the breadth of the Image was juſt two inches. But obſerving, that the ſides of the two Priſms, I uſed, were not exactly plain, but a little convex, (the convexity being about ſo much as that of a double Convex-glaſs of a ſixteen or eighteen foot *Teleſcope*) I took a third Priſm, whoſe ſides were as much concave as thoſe of the other were convex; and this made the breadth of the Image to be two inches and a third part of an inch; the angles of this Priſm, and the lengths of the Image made by each of thoſe Angles being thoſe expreſt in this Table.

The Angles of the Priſm. degr.	*The Lengths of the Image in inches.*
58	$8\frac{1}{2}$
$59\frac{1}{2}$	9
$62\frac{1}{2}$	$10\frac{1}{3}$

In this caſe you ſee, the concave figure of the ſides of the Priſm by making the rays diverge a little, cauſes the breadth of the Image to be greater in proportion to its length than it would be otherwiſe. And this I thought fit to give you notice of, that Mr. *Lucas* may examine, whether his Priſm have not this fault. If a Priſm may be had with ſides exactly plain, it may do well to try the Experiment with that; but its better, if the ſides be about ſo much convex as thoſe of mine are, becauſe the Image will thereby become much better defined. For this convexity of the ſides does the ſame effect, as if you ſhould uſe a Priſm with ſides exactly plain, and between it and the hole in the Window-ſhut, place an Object-glaſs of an 18 foot *Teleſcope*, to make the round Image of the Sun appear diſtinctly defined on the wall when the Priſm is taken away, and conſequently the long Image made by the Priſm to be much more diſtinctly defined (eſpecially at its ſtreight ſides) than it would be otherwiſe.

One thing more I ſhall add: That the utmoſt length of the Image from the fainteſt Red at one end to the fainteſt Blew at the

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the other, must be measured. For in my first Letter about Colours, where I set down the length to be five times the breadth, I called that length the utmost length of the image; and I measured the utmost length, because I account all that length to be caused by the immediate light of the Sun, seeing the Colours (as I noted above) become visible to the greatest length in the clearest days, that is, when the light of the Sun transcends most the light of the Clouds. Sometimes there will happen to shoot out from both ends of the Image a glaring light a good way beyond these colours, but this is not to be regarded, as not appertaining to the Image. If the measures be taken right, the whole length will exceed the length of the streight sides by about the breadth of the Image.

By these things set down thus circumstantially, I presume Mr. *Lucas* will be enabled to accord his tryals of the Experiment with mine; so nearly, at least, that there shall not remain any very considerable difference between us. For, if some little difference should still remain, that need not trouble us any further, seeing there may be many various circumstances which may conduce to it; such as are not only the different figures of prisms, but also the different refractive power of Glasses, the different diameters of the Sun at divers times of the year, and the little errors that may happen in measuring lines and angles, or in placing the prism at the window; though, for my part, I took care to do these things as exactly as I could. However Mr. *Lucas* may make sure to find the Image as long or longer than I have set down, if he take a prism whose sides are not hollow ground, but plain, or (which is better) a very little convex, and whose refracting angle is as much greater than that I used, as that he has hitherto tryed it with, is less; that is, whose angle is about 66 or 67 degrees, or (if he will) a little greater.

Concerning Mr. *Lucas*'s other Experiments, I am much obliged to him that he would take these things so far into consideration, and be at so much pains for examining them; and I thank him so much the more, because he is the first that has sent me an experimental examination of them. By this I may presume he really desires to know what truth there is in these matters. But yet it will conduce to his more speedy and full

satis-

ſatisfaction if he a little change the method which he has propounded, and inſtead of a multitude of things try only the *Experimentum Cancis*. For it is not number of Experiments, but weight to be regarded; and where one will do, what need many?

Had I thought more requiſite, I could have added more: For before I wrote my firſt Letter to you about Colours, I had taken much pains in trying Experiments about them, and written a Tractate on that ſubject, wherin I had ſet down at large the principal of the Experiments I had tried; amongſt which there happened to be the principal of thoſe Experiments which Mr. *Lucas* has now ſent me. And as for the Experimeuts ſet down in my firſt Letter to you, they were only ſuch as I thought convenient to ſelect out of that Tractate.

But ſuppoſe thoſe had been my whole ſtore, yet Mr. *Lucas* ſhould not have grounded his diſcourſe upon a ſuppoſition of my want of Experiments, till he had examined thoſe few. For if any of thoſe be demonſtrative, they will need no aſſiſtants, nor leave room for further diſputing about what they demonſtrate.

The main thing he goes about to examine is, *the different refrangibility* of Light. And this I demonſtrated by the *Experimentum Crucis*. Now if this demonſtration be good, there needs no further examination of the thing; if not good, the fault of it is to be ſhewn: for the only way to examine a demonſtrated propoſition is, to examine the demonſtration. Let that Experiment therefore be examined in the firſt place, and that which it proves be acknowledged, and then if Mr. *Lucas* want my aſſiſtance to unfold the difficulties which he fancies to be in the Experiments he has propounded, he ſhall freely have it; for then I ſuppoſe a few words may make them plain to him: whereas, ſhould I be drawn from demonſtrative Experiment to begin with thoſe, it might create us both the trouble of a long diſpute, and by the multitude of words, cloud rather than clear up the truth. For if it has already coſt us ſo much trouble to agree upon the matter of fact in the firſt and plaineſt Experiment, and yet we are not fully agreed; what an endleſs trouble might it create us, if we ſhould give our ſelves up to diſpute upon every Argument that occurs, and what would become of Truth in ſuch a tedious diſpute?

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The way therefore that I propound, being the ſhorteſt and cleareſt (not to ſay, the only proper way,) I queſtion not but Mr. *Lucas* will be glad that I have recommended it, ſeeing he profeſſes, that it is the knowledge of *truth* that he ſeeks after. And therefore at preſent I ſhall ſay nothing in anſwer to his Experimental diſcourſe, but this in general; that it has proceeded partly from ſome miſunderſtanding of what he writes againſt, and partly from want of due caution in trying Experiments; and that amongſt his Experiments there is one, which when duly tried, is, next to the *Experimentum Crucis*, the moſt conſpicuous Experiment, I know, for proving the different refrangibility of Light, which he brings it to prove againſt.

By the *Poſt-ſcript* of Mr. *Lucas*'s Letter, one not acquainted with what has paſſed, might think, that he quotes the Obſervation of the *R. Society* againſt me; whereas the relation of their Obſervation, which you ſent to *Liege*, contained nothing at all about the juſt proportion of the Length of the Image to its Breath according to the angle of the Priſm, nor any thing more (ſo far as I can perceive by your laſt) than what was pertinent to the things then in diſpute, *viz.* that they found them ſucceed as I had affirmed. And therefore ſince Mr. *Lucas* has found the ſame ſucceſs, I ſuppoſe, that when he expreſſed, that *he much rejoyced to ſee the Trials of the* R. Society *agree ſo exactly with his*, he meant only ſo far as his agreed with mine.

And becauſe I am again upon this firſt Experiment, I ſhall deſire, that Mr. *Lucas* will repeat it with all the exactneſs and caution that may be, regard being had to the information about it, ſet down in this Letter; and then I deſire to have the *length* and *breadth* of the Image with its *diſtance* from the Priſm, ſet down exactly in feet and inches, and parts of an inch, that I may have an opportunity to conſider what relation its length and breadth have to the Suns diameter. For I know, that Mr. *Lucas* Obſervation cannot hold where the refracting angle of the Priſm is full 60 degrees, and the day is clear, and the full length of the Colours is meaſured, and the breadth of the Image anſwers to the Sun's diameter: And ſeeing I am well aſſured of the truth and exactneſs of my own Obſervations, I ſhall be unwilling to be diverted by any other Experiments, from having a fair end made of this in the firſt place. *Sir, I am, &c.*

Poſt-

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Poſtſcript.

I *Had like to have forgotten to adviſe, that the* Experimentum Crucis, *and ſuch others as ſhall be made for knowing the nature of Colours, be made with Priſms which refract ſo much, as to make the length of the Image five times its breadth, and rather more than leſs; for, otherwiſe Experiments will not ſucceed ſo plainly with others as they have done with me.*

December 9. There was produced a manuſcript of Mr. NEWTON, touching his theory of light and colours, containing partly an hypotheſis to explain the properties of light diſcourſed of by him in his former papers, partly the principal phænomena of the various colours exhibited by thin plates or bubbles, eſteemed by him to be of a more difficult conſideration; yet to depend alſo on the ſaid properties of light.

Of the hypotheſis only the firſt part was read, giving an account of refraction, reflection, tranſparency, and opacity; the ſecond part explaining colours being referred to the next meeting.

The firſt was as follows [h]:

" Sir,

" I have ſent you the papers I mentioned, by JOHN STILES. Upon reviewing " them, I find ſome things ſo obſcure, as might have deſerved a further explication " by ſchemes; and ſome other things, I gueſs, will not be new to you, though al- " moſt all was new to me when I wrote them. But as they are, I hope you will accept " of them, though not worth the ample thanks you ſent. I remember, in ſome " diſcourſe with Mr. HOOKE, I happened to ſay, that I thought light was re- " flected, not by the parts of glaſs, water, air, or other ſenſible bodies; but by " the ſame confine or ſuperficies of the æthereal mediums, which refracts it, the " rays finding ſome difficulty to get through it in paſſing out of the denſer into " the rarer medium, and a greater difficulty in paſſing out of the rarer into the " denſer; and ſo being either refracted or reflected by that ſuperficies, as the " circumſtances they happened to be in at their incidence make them able or " unable to get through it. And, for confirmation of this, I ſaid further, that " I thought the reflection of light, at its tending out of glaſs into air, would not " be diminiſhed or weakened by drawing away the air in an air-pump, as it ought " to be, if they were the parts of air that reflected: and added, that I had not " tried this experiment, but thought he was not unacquainted with notions of " this kind. To which he replied, that the notion was new, and he would the " firſt opportunity try the experiment I propounded. But upon reviewing the " papers I ſend you, I found it there ſet down for tried; which makes me recol- " lect, that about the time I was writing theſe papers, I had occaſionally obſerved " in an air-pump here at Chriſt's College, that I could not perceive the reflection " of the inſide of the glaſs diminiſhed in drawing out the air. This I thought " fit to mention, leaſt my former forgetfulneſs, through having long laid aſide " my thoughts on theſe things, ſhould make me ſeem to have ſet down for cer- " tain what I never tried.

[h] Regiſter, vol. v. p. 65.

" Sir,

" Sir, I had formerly purposed never to write any hypothesis of light and " colours, fearing it might be a means to engage me in 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 an 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 recol- " lect my thoughts about it; not concerning myself, whether it shall be thought " probable or improbable, so it do but render the papers I send you, and others " sent formerly, more intelligible. You may see, by the scratching and inter- " lining, 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 it; and desire, that you would " return those 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 know how to " order that. At the end of the hypothesis you will see a paragraph to be in- " serted as is there directed: I should have added another or two, but I had not " time, but such as it is, I hope you will accept it. Sir, I am, &c.

Is. Newton.

" An Hypothesis explaining the Properties of Light, discoursed of in my se- " veral Papers.

" Sir,

" In my answer to Mr. Hooke, you may remember, I had occasion to say " something of hypotheses, where I gave a reason, why all allowable hypotheses " in their genuine constitution should be conformable to my theories; and said " of Mr. Hooke's hypothesis, that I took the most free and natural application " of it to phænomena to be this [1]: that the agitated parts of bodies, according " to their several sizes, figure, and motions, do excite vibrations in the æther of " various depths or bignesses, which being promiscuously propagated through that " medium to our eyes, effect in us a sensation of light of a white colour; but, " if by any means those of unequal bignesses be separated from one another, the " largest beget a sensation of a red colour; the least, or shortest, of a deep " violet; and the intermediate ones, of intermediate colours: much after the " manner that bodies, according to their several sizes, shapes, and motions, ex- " cite vibrations in the air of various bignesses, which, according to those big- " nesses, make several tones in sound, &c. I was glad to understand, as I ap- " prehend, from Mr. Hooke's discouse at my last being at one of your assem- " blies, that he had changed his former notion of all colours being compounded " of only two original ones, made by the two sides of an oblique pulse; and " accommodated his hypothesis to this my suggestion of colours, like sounds, " being various, according to the various bigness of the pulses. For this I take " to be a more plausible hypothesis than any other described by former authors, " because I see not how the colours of thin transparent plates or skins can be " handsomely explained, without having recourse to æthereal pulses: but yet I

[1] Transact. n° 88. p. 5088.

" like-

" like another hypothesis better, which I had occasion to hint something of in the " same letter in these words [k]:

" *The hypothesis of light's being a body, had I propounded it, has a much greater " affinity with the objector's own hypothesis, than he seems to be aware of; the vibra- " tions of the æther being as useful and necessary in this as in his. For, assuming the " rays of light to be small bodies emitted every way from shining substances, those, " when they impinge on any refracting or reflecting superficies, must as necessarily ex- " cite vibrations in the æther, as stones do in water when thrown into it. And, sup- " posing 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 reflexion and refraction; the production of " heat by the sun-beams; the emission of light from burning, putrifying, or other sub- " stances, whose parts are vehemently agitated; the phænomena of thin transparent " plates, and bubbles, and of all natural bodies; the manner of vision, and the dif- " ference of colours; as also their harmony and discord; I shall leave to their consi- " deration, who may think it worth their endeavour to apply this hypothesis to the " solution of phænomena.*

" Were I to assume an hypothesis, it should be this, if propounded more ge- " nerally, so as not to determine what light is, farther than that it is something " or other capable of exciting vibrations in the æther: for thus it will become " so general and comprehensive of other hypotheses, as to leave little room for " new ones to be invented. And therefore, because I have observed the heads " of some great virtuosos to run much upon hypotheses, as if my discourses want- " ed an hypothesis to explain them by, and found, that some, when I could not " make them take my meaning, when I spake of the nature of light and colours " abstractedly, have readily apprehended it, when I illustrated my discourse by " an hypothesis; for this reason I have here thought fit to send you a descrip- " tion of the circumstances of this hypothesis as much tending to the illustration " of the papers I herewith send you. And though I shall not assume either this or " any other hypothesis, not thinking it necessary to concern myself, whether the " properties of light, discovered by me, be explained by this, or Mr. HOOKE's, " or any other hypothesis capable of explaining them; yet while I am describ- " ing this, I shall sometimes, to avoid circumlocution, and to represent it more " conveniently, speak of it, as if I assumed it, and propounded it to be believed. " This 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 and insignificant disputes.

" But to proceed to the hypothesis: First, it is to be supposed therein, that " there is an æthereal medium much of the same constitution with air, but far " rarer, subtler, and more strongly elastic. Of the existence of this medium " the motion of a pendulum in a glass exhausted of air almost as quickly as in

[k] Transact. n° 88. p. 5087.

" the open air, is no inconſiderable argument. But it is not to be ſuppoſed, " that this medium is one uniform matter, but compounded, partly of the main " phlegmatic body of æther, partly of other various æthereal ſpirits, much after " the manner, that air is compounded of the phlegmatic body of air intermixed " with various vapours and exhalations: for the electric and magnetic effluvia, " and gravitating principle, ſeem to argue ſuch variety. Perhaps the whole " frame of nature may be nothing but various contextures of ſome certain æthe- " real ſpirits, or vapours, condenſed as it were by precipitation, much after the " manner, that vapours are condenſed into water, or exhalations into groſſer ſub- " ſtances, though not ſo eaſily condenſible; 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 copies ſet her by the protoplaſt. " Thus perhaps may all things be originated from æther.

" At leaſt, the elaſtic effluvia ſeem to inſtruct us, that there is ſomething of " an æthereal 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 incloſed 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 perhaps down and up again, and this ſometimes in lines ſeeming per- " pendicular to the table; ſometimes in oblique ones; ſometimes alſo they would " leap up in one arch and down in another, divers times together, without " ſenſibly 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 diverſe " motion. And upon ſliding my finger on the upper ſide of the glaſs, though " neither the glaſs, nor incloſed 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 " ſubtil matter lying condenſed in the glaſs, and rarefied by rubbing, as water is " rarefied into vapour 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 return into the glaſs again, and be " recondenſed there. And as this condenſed matter by rarefaction into an æthe- " real wind (for by its eaſy penetrating and circulating through glaſs I eſteem it " æthereal) 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

" earth be caufed by the continual condenfation of fome other fuch like æthereal " fpirit, not of the main body of phlegmatic æther, but of fomething very " thinly and fubtilly diffufed through it, perhaps of an unctuous or gummy, " tenacious, and fpringy nature, and bearing much the fame relation to æther, " which the vital aereal fpirit, requifite for the confervation of flame and vital " motions, does to air. For, if fuch an æthereal fpirit may be condenfed in " fermenting or burning bodies, or otherwife coagulated in the pores of the earth " and water into fome kind of humid active matter, for the continual ufes of " nature, adhering to the fides of thofe pores, after the manner that vapours " condenfe on the fides of a veffel; the vaft body of the earth, which may be " every where to the very center in perpetual working, may continually condenfe " fo much of this fpirit, as to caufe it from above to defcend with great celerity " for a fupply; in which defcent it may bear down with it the bodies it pervades " with force proportional to the fuperficies of all their parts it acts upon; nature " making a circulation by the flow afcent of as much matter out of the bowels " of the earth in an aereal form, which, for a time, conftitutes the atmofphere; " but being continually buoyed up by the new air; exhalations and vapours rifing " underneath, at length (fome part of the vapours, which return in rain, excepted) " vanifhes again into the æthereal fpaces, and there perhaps in time relents, and is " attenuated into its firft principle: for nature is a perpetual worker, generating " fluids out of folids, and folids out of fluids, fixed things out of volatile, and " volatile out of fixed, fubtil out of grofs and grofs out of fubtil; fome things " to afcend, and make the upper terreftrial juices, rivers, and the atmofphere; and " by confequence, others to defcend for a requital to the former. And, as the " earth, fo perhaps may the fun imbibe this fpirit copioufly, to conferve his fhin- " ing, and keep the planets from receding further from him. And they, that " will, may alfo fuppofe, that this fpirit affords or carries with it thither the folary " fewel and material principle of light: and that the vaft æthereal fpaces between " us and the ftars are for a fufficient repofitory for this food of the fun and " planets. But this of the conftitution of æthereal natures by the by.

" In the *fecond* place, it is to be fuppofed, that the æther is a vibrating medium " like air, only the vibrations far more fwift and minute; thofe of air, made by " a man's ordinary voice, fucceeding one another at more than half a foot or a " foot diftance; but thofe of æther at a lefs diftance than the hundred thoufandth " part of an inch. And, as in air the vibrations are fome larger than others, " but yet all equally fwift (for in a ring of bells the found of every tone is heard " at two or three miles diftance, in the fame order that the bells are ftruck;) fo, " I fuppofe, the æthereal vibrations differ in bignefs, but not in fwiftnefs. Now, " thefe vibrations, befide their ufe in reflexion and refraction, may be fuppofed " the chief means, by which the parts of fermenting or putrifying fubftances, " fluid liquors, or melted, burning, or other hot bodies, continue in motion, are " fhaken afunder like a fhip by waves, and diffipated into vapours, exhalations, " or fmoke, and light loofed or excited in thofe bodies, and confequently by " which a body becomes a burning coal, and fmoke, flame; and, I fuppofe, " flame is nothing but the particles of fmoke turned by the accefs of light and " heat to burning coals, little and innumerable.

" *Thirdly*, as 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 aereal ſpaces, and at ſo much a greater degree of rarity as the " pipe is ſmaller, as is known by the riſing of water in ſuch pipes to a much " greater hight than the ſurface of the ſtagnating water, into which they are " dipped; ſo I ſuppoſe æther, 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 æthereal ſpaces, and at ſo much a greater degree of " rarity, as the pores of the body are ſmaller. Whence it may be, that the ſpirit " of wine, for inſtance, though a lighter body, yet having ſubtiler parts, and " conſequently ſmaller pores, than 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 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 hight " than twenty-nine inches. For the denſer æther, which ſurrounds theſe bodies, " muſt croud and preſs their parts together, much after the manner that air " ſurrounding two marbles preſſes them together, if there be little or no air be- " tween 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 means men have hitherto been thinking on.* For, if there be any " power in man to condenſe and dilate at will the æther, that pervades the " muſcle, that condenſation or dilation muſt vary the compreſſion of the muſcle, " made by the ambient æther, and cauſe it to ſwell or ſhrink accordingly. For " though common water will ſcarce ſhrink by compreſſion, and ſwell by relax- " ation, 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 compreſ- " ſing the water, in which it ſwam, is an argument, that animal juices do the " ſame. And as for their various preſſion by the ambient æther, it is plain, " that that muſt be more or leſs accordingly as there is more or leſs æther with- " in, to ſuſtain and counterpoiſe the preſſure of that without. If both æthers " were equally denſe, the muſcle would be at liberty, as if preſſed by neither: " if there were no æther within, the ambient would compreſs it with the whole " force of its ſpring. If the æther 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 the ambient compreſſes " the muſcle by the exceſs of the force of its ſpringineſs above that of the ſpring- " ineſs 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 æther; and a very little change may ſuffice, if the ſpring of " æther 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 æther; ſome may be ready to " grant, that the ſoul may have an immediate power over the whole æther in " any part of the body, to ſwell or ſhrink it at will: but then how depends the

" muſcular

" mufcular motion on the nerves? Others therefore may be more apt to think " it done by fome certain æthereal fpirit included within the *dura mater*, which " the foul may have power to contract or dilate at will in any mufcle, and fo " caufe it to flow thither through the nerves. But ftill there is a difficulty, why " this force of the foul upon it does not take off the power of its fpringinefs, " whereby it fhould fuftain, more or lefs, the force of the outward æther. A " third fuppofition may be, that the foul has a power to infpire any mufcle with " this fpirit, by impelling it thither through the nerves. But this too has its " difficulties, for it requires a forcible intending the fpring of the æther in the " mufcles, by preffure exerted from the parts of the brain: and it is hard to " conceive, how fo great force can be exercifed amidft fo tender matter as the " brain is. And befides, why does not this æthereal fpirit, being fubtil enough, " and urged with fo great force, go away through the dura mater and fkins of " the mufcle; or at leaft fo much of the other æther go out to make way for " this, which is crouded in? To take away thefe difficulties is a digreffion; but " feeing the fubject is a deferving one, I fhall not ftick to tell you how I think " it may be done.

" Firft then, I fuppofe, there is fuch a fpirit; that is, that the animal fpirits " are neither like the liquor, vapour, or gas of fpirit of wine; but of an æthereal " nature, fubtil enough to pervade the animal juices, as freely as the electric, or " perhaps magnetic, effluvia do glafs. And to know, how the coats of the " brain, nerves, and mufcles, may become a convenient veffel to hold fo fubtil " a fpirit, you may confider, how liquors and fpirits are difpofed to pervade or " not pervade things on other accounts than their fubtilty. Water and oil per- " vade wood and ftone, which quickfilver does not; and quickfilver metals, " which water and oil do not: water and acid fpirits pervade falts, which oil " and fpirit of wine do not; and oil and fpirit of wine pervade fulphur, which " water and acid fpirits do not. So fome fluids, as oil and water, though their " parts are in freedom enough to mix with one another, yet by fome fecret " principle of unfociablenefs they keep afunder; and fome, that are fociable, may " become unfociable, by adding a third thing to one of them, as water to fpirit " of wine, by diffolving falt of tartar in it. The like unfociablenefs may be in " æthereal natures, as perhaps between the æthers in the vortices of the fun and " planets; and the reafon, why air ftands rarer in the boxes of fmall glafs-pipes, " and æther in the pores of bodies, than elfewhere, may be, not want of fub- " tilty, but fociablenefs. And on this ground, if the æthereal vital fpirit in a " man be very fociable to the marrow and juices, and unfociable to the coats of " the brain, nerves, and mufcles, or to any thing lodged in the pores of thofe " coats, it may be contained thereby, notwithftanding its fubtilty; efpecially if " we fuppofe no great violence done to it to fqueeze it out; and that it may not " be altogether fo fubtil as the main body of æther, though fubtil enough to " pervade readily the animal juices, and that, as any of it is fpent, it is continu- " ally fupplied by new fpirit from the heart.

" In the next place, for knowing how this fpirit may be ufed for animal mo- " tion, you may confider, how fome things unfociable are made fociable by the

" mediation

"mediation of a third. Water, which will not diffolve copper, will do it, if "the copper be melted with fulphur: aqua fortis, which will not pervade gold, "will do it by addition of a little fal armoniac, or fpirit of falt: 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 wafted by "throwing faltpeter or otherwife: and fo lead melted with filver quickly per-"vades and liquefies the filver in a much lefs heat than is requifite to melt the "filver alone; but, if they be kept in the teft till that little fubftance, that re-"conciled them, be wafted or altered, they part again of their own accord. And, "in like manner, the æthereal animal fpirit in a man may be a mediator between "the common æther and the mufcular juices, to make them mix more freely; "and fo, by fending a little of this fpirit into any mufcle, though fo little as to "caufe no fenfible tenfion of the mufcle by its own force; yet, by rendering the "juices more fociable to the common external æther, it may caufe that æther to "pervade the mufcle of its own accord in a moment more freely and copioufly "than it would otherwife do, and to recede again as freely, fo foon as this medi-"ator of fociablenefs is retracted. Whence, according to what I faid above, "will proceed the fwelling or fhrinking of the mufcle, and confequently the ani-"mal motion depending thereon.

"Thus may therefore the foul, by determining this æthereal animal fpirit or "wind into this or that nerve, perhaps with as much eafe as air is moved in open "fpaces, caufe all the motions we fee in animals: for the making which motions "ftrong, it is not neceffary, that we fhould fuppofe the æther within the mufcle "very much condenfed or rarified by this means, but only that its fpring is fo "very great, that a little alteration of its denfity fhall caufe a great alteration in "the preffure. And what is faid of mufcular motion, may be applied to the mo-"tion of the heart, only with this difference, that the fpirit is not fent thither, "as into other mufcles, but continually generated there by the fermentation of "the juices, with which its flefh is replenifhed, and as it is generated, let out by "ftarts into the brain through fome convenient ductus to perform thofe motions "in other mufcles by impreffion, which it did in the heart by its generation. "For I fee not, why the ferment in the heart may not raife as fubtil a fpirit out "of its juices, to caufe thefe motions, as rubbing does out of a glafs, to caufe "electric attraction, or burning out of fewel, to penetrate glafs, as Mr. Boyle "has fhewn, and calcine by corrofion metals melted therein.

"Hitherto I have been contemplating the nature of æther and æthereal fub-"ftances by their effects and ufes; and now I come to join therewith the confi-"deration of light.

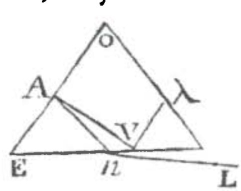
"In the fourth place therefore, I fuppofe light is neither æther, nor its vibrating "motion, but fomething of a different kind propagated from lucid bodies. They, "that will, may fuppofe it an aggregate of various peripatetic qualities. Others "may fuppofe it multitudes of unimaginable fmall and fwift corpufcles of various "fizes, fpringing from fhining bodies at great diftances one after another; but "yet without any fenfible interval of time, and continually urged forward by a "principle

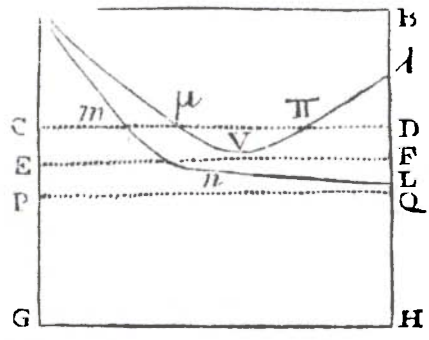
" principle of motion, which in the beginning accelerates them, till the refiftence " of the æthereal medium equal the force of that principle, much after the " manner that bodies let fall in water are accelerated till the refiftance of the wa- " ter equals the force of gravity. God, who gave animals felf-motion beyond " our underftanding, is, without doubt, able to implant other principles of mo- " tion in bodies, which we may underftand as little. Some would readily grant " this may be a fpiritual one; yet a mechanical one might be fhewn, did not I " think it better to pafs it by. But they, that like not this, may fuppofe light " any other corporeal emanation, or any impulfe or motion of any other medium " or æthereal fpirit diffufed through the main body of æther, or what elfe they " can imagine proper for this purpofe. To avoid difpute, and make this hypo- " thefis general, let every man here take his fancy: only, whatever light be, I " fuppofe, it confifts of rays differing from one another in contingent circum- " ftances, as bignefs, form, or vigour; like as the fands on the fhore, the waves " of the fea, the faces of men, and all other natural things of the fame kind " differ; it being almoft impoffible for any fort of things to be found without " fome contingent variety. And further, I would fuppofe it diverfe, from the " vibrations of the æther, becaufe (befides, that were it thefe vibrations, it " ought always to verge copioufly in crooked lines into the dark or quiefcent " medium, deftroying all fhadows; and to comply readily with any crooked pores " or paffages, as founds do,) I fee not how any fuperficies (as the fide of a glafs " prifm, on which the rays within are incident at an angle of above forty de- " grees) can be totally opake. For the vibrations beating againft the refract- " ing confine of the rarer and denfer æther muft needs make that pliant fuper- " ficies undulate, and thofe undulations will ftir up and propagate vibrations on " the other fide. And further, how light, incident on very thin fkins or plates " of any tranfparent body, fhould, for many fucceffive thickneffes of the plate " in arithmetical progreffion, be alternately reflected and tranfmitted, as I find " it is, puzzles me as much. For, though the arithmetical progreffion of thofe " thickneffes, which reflect and tranfmit the rays alternately, argues, that it de- " pends upon the number of vibrations between the two fuperficies of the plate, " whether the ray fhall be reflected or tranfmitted: yet I cannot fee, how the " number fhould vary the cafe, be it greater or lefs, whole or broken, unlefs " light be fuppofed fomething elfe than thefe vibrations. Something indeed " I could fancy towards helping the two laft difficulties, but nothing which I fee " not infufficient.

" Fifthly, it is to be fuppofed, that light and æther mutually act upon one " another, æther in refracting light, and light in warming æther; and that the " denfeft æther acts moft ftrongly. When a ray therefore moves through æther " of uneven denfity, I fuppofe it moft preffed, urged, or acted upon by the me- " dium on that fide towards the denfer æther, and receives a continual impulfe or " ply from that fide to recede towards the rarer, and fo is accelerated, if it move " that way, or retarded, if the contrary. On this ground, if a ray move " obliquely through fuch an unevenly denfe medium (that is, obliquely to thofe " imaginary fuperficies, which run through the equally denfe parts of the me- " dium, and may be called the refracting fuperficies) it muft be incurved, as it " is

" is found to be, by obſervation in water [1], whoſe lower parts were made gradu-
" ally more ſalt, and ſo more denſe than the upper. And this may be the ground
" of all refraction and reflexion: for as the rarer air within a ſmall glaſs-pipe,
" and the denſer without, are not diſtinguiſhed by a meer mathematical ſuper-
" ficies, 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
" æther, between unequally denſe mediums, to be not a mathematical one; but
" of ſome breadth, the æther therein, at the orifices of the pores of the ſolid body,
" being of all intermediate degrees of denſity between the rarer and denſer æthe-
" real 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 be increaſed or dimi-
" niſhed in a certain proportion, according to the difference of the denſities of the
" æthereal 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 Des Cartes
" has demonſtrated.

" The ray therefore, in paſſing out of the rarer medium into the denſer,
" inclines continually more and more towards parallelifm with the refracting ſu-
" perficies; and if the differing denſities of the mediums be not ſo great, nor the
" incidence of the ray ſo oblique, as to make it parallel to that ſuperficies before
" it gets through, then it goes through and is refracted; but if, through the afore-
" ſaid cauſes, the ray become parallel to that ſuperficies before it can get through,
" then it muſt turn back and be reflected. Thus, for inſtance, may be obſerved
" in a triangular glaſs-priſm O E F, that the rays A *n*,
" that ſend out of the glaſs into air, do, by inclining
" them more and more to the refracting ſuperficies, emerge
" more and more obliquely till they be infinitely oblique;
" that is, in a manner parallel to the ſuperficies, which hap-
" pens when the angle of incidence is about forty degrees;
" and then, if they be a little more inclined are all reflected,
" as at A V λ, becoming, I ſuppoſe, parallel to the ſuperficies before they can get
" through it. Let A B D C repreſent the rarer medium; E F H G the denſer,
" C D F E the ſpace between them, or re-
" fracting phyſical ſuperficies, in which the
" æther is of all intermediate degrees of
" denſity, from the rareſt æther at C D,
" to the denſeſt, at E F; A *m n* L a ray,
" A *m* its incident part, *m n* its incurvation
" by the refracting ſuperficies, and *n* L its
" emergent part. Now, if the ray A *m* be
" ſo much incurved as to become at its
" emergence *n*, as nearly as may be, paral-
" lel to C D, it is plain, that if that ray
" had been incident a little more obliquely,





[1] See Mr. Hooke's Micrographia, where he ſpeaks of the inflexion of rays.

" it

"it muſt have become parallel to C D, before it had arrived at E F, the further "ſide of the refracting ſuperficies; and ſo could have got no nearer to E F, but "muſt have turned back by further incurvation, and been reflected, as it is re-"preſented at A μ V λ. And the like would have happened, if the denſity "of the æther had further increaſed from E F to P Q; ſo that P Q H G might "be a denſer medium than E F H G was ſuppoſed; for then the ray, in paſſ-"ing from *m* to *n*, being ſo much incurved, as at *n* to become parallel to C D "or P Q, it is impoſſible it ſhould ever get nearer to P Q, but muſt at *n* be-"gin by further incurvation to turn back, and ſo be reflected. And becauſe, if "a refracted ray, as *n* L, be made incident, the incident, A *m*, ſhall become the "refracted; and therefore, if the ray A μ V, after it is arrived at V, where I "ſuppoſe it parallel to the refracting ſuperficies, ſhould be reflected perpendicu-"larly back, it would return back in the line of incidence V μ A. Therefore "going forward, it muſt go forward in ſuch another line, V π λ, both caſes be-"ing alike, and ſo be reflected at an angle, equal to that of incidence.

"This may be the cauſe and manner of reflection, when light tends from the "rarer towards the denſer æther: but to know, how it ſhould be reflected, "when it ſtands from the denſer towards the rarer, you are further to conſider, "how fluids near their ſuperficies are leſs pliant and yielding than in their more "inward parts; and, if formed into thin plates, or ſhells, they become much "more ſtiff and tenacious than otherwiſe. Thus, things, which readily fall in "water, if let fall upon a bubble of water, they do not eaſily break through it, "but are apt to ſlide down by the ſides of it, if they be not too big and heavy. "So, if two well poliſhed convex glaſſes, ground on very large ſpheres, be laid "one upon another, the air between them eaſily recedes, till they almoſt touch; "but then begins to reſiſt ſo much, that the weight of the upper glaſs is too "little to bring them together ſo as to make the black, mentioned in the other "papers I ſend you, appear in the midſt of the rings of colours: and, if the "glaſſes be plain, though no broader than a two-pence, a man with his whole "ſtrength is not able to preſs all the air out from between them, ſo as to make "them fully touch. You may obſerve alſo, that inſects will walk upon water "without wetting their feet, and the water bearing them up; alſo motes fal-"ling upon water will often lie long upon it without being wetted: and ſo, "I ſuppoſe, æther in the confine of two mediums is leſs pliant and yielding "than in other places, and ſo much the leſs pliant by how much the mediums "differ in denſity: ſo that in paſſing out of denſer æther into rarer, when there "remains but a very little of the denſer æther to be paſt through, a ray finds "more than ordinary difficulty to get through; and ſo great difficulty, where the "mediums are of very differing denſity, as to be reflected by incurvation, after "the manner deſcribed above; the parts of æther on that ſide, where they are "leſs pliant and yielding, acting upon the ray much after the manner that they "would do were they denſer there than on the other ſide: for the reſiſtance of "the medium ought to have the ſame effect on the ray, from what cauſe ſoever "it ariſes. And this, I ſuppoſe, may be the cauſe of the reflection of quick-"ſilver, and other metalline bodies. It muſt alſo concur to increaſe the reflective "virtue of the ſuperficies, when rays tend out of the rarer medium into the "denſer:

" denfer: and, in that cafe therefore, the reflection having a double caufe, ought " to be ftronger than in the æther, as it is apparently. But in refraction, this ri- " gid tenacity or unpliablenefs of the fuperficies need not be confidered, becaufe " fo much as the ray is thereby bent in paffing to the moft tenacious and rigid " part of the fuperficies, fo much it is thereby unbent again in paffing on from " thence through the next parts gradually lefs tenacious.

" Thus may rays be refracted by fome fuperficies, and reflected by others, be " the medium they tend into, denfer or rarer. But it remains further to be ex- " plained, how rays alike incident on the fame fuperficies (fuppofe of cryftal, glafs, " or water) may be at the fame time fome refracted, others reflected. And for ex- " plaining this, I fuppofe, that the rays, when they impinge on the rigid refift- " ing æthereal fuperficies, as they are acted upon by it, fo they react upon it and " caufe vibrations in it, as ftones thrown into water do in its furface; and that " thefe vibrat ons are propagated every way into both the rarer and denfer me- " diums; as the vibrations of air, which caufe found, are from a ftroke, but yet " continue ftrongeft where they began, and alternately contract and dilate the æther " in that phyfical fuperficies. For it is plain by the heat, which light produces in " bodies, that it is able to put their parts in motion, and much more to heat and " put in motion the more tender æther; and it is more probable, that it com- " municates motion to the grofs parts of bodies by the mediation of æther than " immediately; as for inftance, in the inward parts of quickfilver, tin, filver, " and other very opake bodies, by generating vibrations, that run through them, " than by ftriking the outward parts only, without entering the body. The fhock " of every fingle ray may generate many thoufand vibrations, and by fending " them all over the body, move all the parts, and that perhaps with more mo- " tion than it could move one fingle part by an immediate ftroke; for the vi- " brations, by fhaking each particle backward and forward, may every time " increafe its motion, as a ringer does a bell by often pulling it, and fo at length " move the particles to a very great degree of agitation, which neither the fimple " fhock of a ray, nor any other motion in the æther, befides a vibrating one could " do. Thus in air fhut up in a veffel, the motion of its parts caufed by heat, " how violent foever, is unable to move the bodies hung in it, with either a trem- " bling or progreffive motion: but if air be put into a vibrating motion by beat- " ing a drum or two, it fhakes glafs-windows, the whole body of a man, and " other maffy things, efpecially thofe of a congruous tone: yea I have obferved it " manifeftly fhake under my feet a cellared free-ftone floor of a large hall, fo as, " I believe, the immediate ftroke of five hundred drumfticks could not have done, " unlefs perhaps quickly fucceeding one another at equal intervals of time. Æthe- " real vibrations are therefore the beft means by which fuch a fubtile agent as " light can fhake the grofs particles of folid bodies to heat them: and fo fup- " pofing that light, impinging on a refracting or reflecting æthereal fuperficies, puts " it into a vibrating motion, that phyfical fuperficies being by the perpetual ap- " pulfe of rays always kept in a vibrating motion, and the æther therein conti- " nually expanded and compreffed by turns; if a ray of light impinge upon it, " while it is much compreffed, I fuppofe it is then too denfe and ftiff to let the ray

" pafs

" paſs through, and ſo reflects it; but the rays, that impinge on it at other times, " when it is either expanded by the interval of two vibrations, or not too much " compreſſed and condenſed, go through and are refracted.

" Theſe may be the cauſes of refractions and reflections in all caſes; but, for " underſtanding how they come to be ſo regular, it is further to be conſidered, " that in a heap of ſand, although the ſurface be rugged, yet if water be poured " on it to fill its pores, the water, ſo ſoon as its pores are filled, will evenly over- " ſpread the ſurface, and ſo much the more evenly, as the ſand is finer: ſo, al- " though the ſurface of all bodies, even the moſt poliſhed, be rugged, as I con- " ceive, yet where that ruggedneſs is not too groſs and coarſe, the refracting æthe- " real ſuperficies may evenly overſpread it. In poliſhing glaſs or metal, it is not " to be imagined, that ſand, putty, or other fretting powders, ſhould wear the " ſurface ſo regularly, as to make the front of every particle exactly plain, and " all thoſe plains look the ſame way, as they ought to do in well poliſhed bodies, " were reflection performed by their parts: but that thoſe fretting powders ſhould " wear the bodies firſt to a coarſe ruggedneſs, ſuch as is ſenſible, and then to a finer " and finer ruggedneſs, till it be ſo fine that the æthereal ſuperficies evenly over- " ſpreads it, and ſo makes the body put on the appearance of a poliſh, is a very na- " tural and intelligible ſuppoſition. So in fluids, it is not well to be conceived, that " the ſurfaces of their parts ſhould be all plain, and the plains of the ſuperficial parts " always kept looking all the ſame way, notwithſtanding that they are in perpetual " motion. And yet without theſe two ſuppoſitions, the ſuperficies of fluids could " not be ſo regularly reflexive as they are, were the reflexion done by the parts them- " ſelves, and not by an æthereal ſuperficies evenly overſpreading the fluid.

" Further, concerning the regular motion of light, it might be ſuſpected, whe- " ther the various vibrations of the fluid, through which it paſſes, may not much " diſturb it: but that ſuſpicion, I ſuppoſe, will vaniſh, by conſidering, that " if at any time the foremoſt part of an oblique wave begin to turn it awry, " the hindermoſt part, by a contrary action, muſt ſoon ſet it ſtraight again.

" Laſtly, becauſe without doubt there are, in every tranſparent body, pores of " various ſizes, and I ſaid, that æther ſtands at the greateſt rarity in the ſmalleſt " pores; hence the æther in every pore ſhould be of a differing rarity, and ſo " light be refracted in its paſſage out of every pore into the next, which would " cauſe a great confuſion, and ſpoil the body's tranſparency. But conſidering that " the æther, in all denſe bodies, is agitated by continual vibrations, and theſe vi- " brations cannot be performed without forcing the parts of æther forward and " backward, from one pore to another, by a kind of tremor, ſo that the æther, " which one moment is in a greater pore, is the next moment forced into a leſs; " and on the contrary, this muſt evenly ſpread the æther into all the pores not " exceeding ſome certain bigneſs, ſuppoſe the breadth of a vibration, and ſo make " it of an even denſity throughout the tranſparent body, agreeable to the middle " ſort of pores. But where the pores exceed a certain bigneſs, I ſuppoſe " the æther ſuits its denſity to the bigneſs of the pore, or to the medium within " it; and ſo being of a diverſe denſity from the æther that ſurrounds it, refracts "or reflects light in its superficies, and so make the body, where many such interstices are, appear opake."

Some of the members taking particular notice, among other things, of an experiment mentioned in this hypothesis, desired, that it might be tried; viz. that having laid upon a table a round piece of glass, about two inches broad, in a brass ring; so that the glass might be one third part of an inch from the table; and then rubbing the glass briskly, till some little fragments of paper laid on the table under the glass began to be attracted, and move nimbly to and fro; after he 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, and then leaping up and down again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also leaping up in one arch, and leaping down in another divers times together, without sensibly resting between; sometimes skipping in a bow from one part of the glass to another, without touching the table, and sometimes hanging by a corner, and turning often about very nimbly, as if they had been carried about in the middle of a whirlwind; and being otherwise variously moved, every paper with a different motion. And upon sliding his finger upon the upper side of the glass, though neither the glass nor the inclosed air below were moved thereby, yet would the papers, as they hung under the glass, receive some new motion, inclining this or that way, according as he moved his finger.

This experiment Mr. Newton proposed to be varied with a larger glass placed farther from the table, and to make use of bits of leaf gold instead of papers; thinking, that this would succeed much better, so as perhaps to make the leaf gold rise and fall in spiral lines, or whirl for a time in the air, without touching either the table or glass.

It was ordered, that this experiment should be tried at the next meeting; and Mr. Hooke promised to prepare it for that meeting.

Mr. Oldenburg was desired to enquire by letter of Mr. Newton, whether he would consent, that a copy might be taken of his papers, for the better consideration of their contents.

Mr. Oldenburg presented from Mr. Martyn, the printer to the Society, Mr. Willughby's *Ornithologia*, printed at London, 1676, in fol.

December 16. Mr. Newton's experiment of glass rubbed to cause various motions in bits of paper underneath, was tried, but did not succeed in those circumstances, with which it was tried. This trial was made upon the reading of a letter of his to Mr. Oldenburg, dated at Cambridge, 14th December, 1675 [a], in which he gives some more particular directions about that experiment.

The letter was as follows:

[a] Letter-book, vol. vii. p. 280.

The

" The notice you gave me of the Royal Society's intending to fee the experi-
" ment of glafs rubbed, to caufe various motions in bits of paper underneath,
" put me upon recollecting myfelf a little further about it; and then remembring,
" that, if one edge of the brafs hoop was laid downward, the glafs was as near
" again to the table as it was when the other edge was laid downward, and that
" the papers played beft when the glafs was neareft to the table; I began to fuf-
" pect, that I had fet down a greater diftance of the glafs from the table than I
" fhould have done; for in fetting down that experiment, I trufted to the idea I
" had of the bignefs of the hoop, in which I might eafily be miftaken, having
" not feen it of a long time. And this fufpicion was increafed by trying the ex-
" periment with an object glafs of a telefcope, placed about the third part of an
" inch from the table; for I could not fee the papers play any thing near fo well
" as I had feen them formerly. Whereupon I looked for the old hoop with its
" glafs, and at length found the hoop, the glafs being gone; but by the hoop I
" perceived, that, when one edge was turned down, the glafs was almoft the
" third part of an inch from the table, and when the other edge was down,
" which made the papers play fo well, the glafs was fcarce the eighth part of an
" inch from the table. This I thought fit to fignify to you, that, if the expe-
" riment fucceed not well at the diftance I fet down, it may be tried at a lefs
" diftance, and that you may alter my paper, and write in it the eighth part of an
" inch inftead of $\frac{1}{4}$ or $\frac{1}{3}$ of an inch. The bits of paper ought to be very little,
" and of thin paper; perhaps little bits of the wings of a fly, or other light fub-
" ftances, may do better than paper. Some of the motions, as that of hanging
" by a corner and twirling about, and that of leaping from one part of the glafs
" to another, without touching the table, happen but feldom; but it made me take
" the more notice of them.

" Pray prefent my humble fervice to Mr. Boyle, when you fee him, and thanks
" for the favour of the converfe I had with him at Spring. My conceit of tre-
" paning the common æther, as he was pleafed to exprefs it, makes me begin
" to have the better thoughts on that he was pleafed to entertain it with a fmile.
" I am apt to think, that when he has a fet of experiments to try in his air-pump,
" he will make that one, to fee how the compreffion or relaxation of a mufcle will
" fhrink or fwell, foften or harden, lengthen or fhorten it..

" As for regiftring the two difcourfes, you may do it; only I defire you would
" fufpend till my next letter, in which I intend to fet down fomething to be al-
" tered, and fomething to be added in the hypothefis."

It was ordered, that Mr. Oldenburg fhould again write to Mr. Newton, and acquaint him with the want of fuccefs of his experiment, and defire him to fend his own apparatus, with which he had made it: as alfo to enquire, whether he had fecured the papers being moved from the air, that might fomewhere fteal in.

Hereupon the fequel of his hypothefis, the firft part of which was read at the preceding meetings, was read to the end.

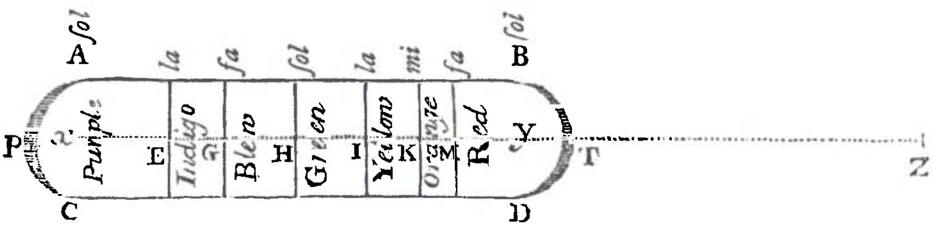
2

" Thus

" Thus much of refraction, reflection, transparency, and opacity; and now to " explain colours; I suppose, that as bodies of various sizes, densities, or sensa- " tions, do by percussion or other action excite sounds of various tones, and " consequently vibrations in the air of various bigness; so when the rays of " light, by impinging on the stiff refracting superficies, excite vibrations in the " æther, those rays, whatever they be, as they happen to differ in magnitude, " strength or vigour, excite vibrations of various bigness; the biggest, strongest, " or most potent rays, the largest vibrations; and others shorter, according to " their bigness, strength, or power: and therefore the ends of the capillamenta of " the optic nerve, which pave or face the retina, being such refracting superfi- " cies, when the rays impinge upon them, they must there excite these vibra- " tions, which vibrations (like those of sound in a trunk or trumpet) will run " along the aqueous pores or crystalline pith of the capillamenta through the " optic nerves into the sensorium (which light itself cannot do) and there, I sup- " pose, affect the sense with various colours, according to their bigness and mix- " ture; the biggest with the strongest colours, reds and yellows; the least with " the weakest, blues and violets; the middle with green, and a confusion of " all with white, much after the manner, that in the sense of hearing, nature " makes use of aereal vibrations of several bignesses to generate sounds of divers " tones; for the analogy of nature is to be observed. And further, as the " harmony and discord of sounds proceed from the proportions of the aereal vi- " brations, so may the harmony of some colours, as of golden and blue, and the " discord of others, as of red and blue, proceed from the proportions of the æthe- " real. And possibly colour may be distinguished into its principal degrees, red, " orange, yellow, green, blue, indigo, and deep violet, on the same ground, " that sound within an eighth is graduated into tones. For, some years past, the " prismatic colours being in a well darkened room cast perpendicularly upon " a paper about two and twenty foot distant from the prism, I desired a friend " to draw with a pencil lines cross the image, or pillar of colours, where every " one of the seven aforenamed colours was most full and brisk, and also where he " judged the truest confines of them to be, whilst I held the paper so, that the said " image might fall within a certain compass marked on it. And this I did, partly " because my own eyes are not very critical in distinguishing colours, partly be- " cause another, to whom I had not communicated my thoughts about this mat- " ter, could have nothing but his eyes to determine his fancy in making those " marks. This observation we repeated divers times, both in the same and di- " vers days, to see how the marks on several papers would agree; and comparing " the observations, though the just confines of the colours are hard to be assigned, " because they pass into one another by insensible gradation; yet the *differences* " of the observations were but little, especially towards the red end, and taking " means between those differences, that were, the length of the image (reckoned " not by the distance of the verges of the semicircular ends, but by the distance of " the centres of those semicircles, or length of the strait sides as it ought to be) " was divided in about the same proportion that a string is, between the end and " the middle, to sound the tones in the eighth. You will understand me best " by viewing the annexed figure, in which A B and C D represent the strait " sides, about ten inches long, A B C and B T D the semicircular ends, X and

" Y

" Y the centres of thofe femicircles, X Z the length of a mufical ftring double to



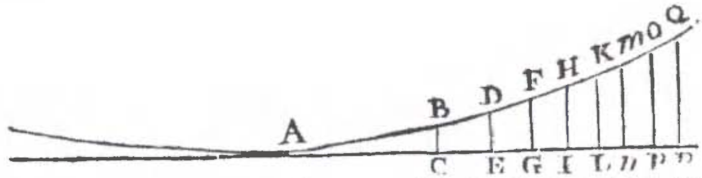
" X Y, and divided between X and Y, fo as to found the tones expreffed at the " fide (that is X H the half, X G and G I the third part, Y K the fifth parr, " Y M the eighth part, and G E the ninth part of X Y) and the intervals between " thefe divifions exprefs the fpaces which the colours written there took up, every " colour being moft brifkly fpecific in the middle of thofe fpaces.

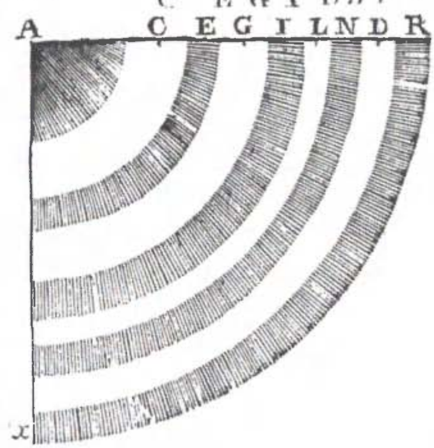
" Now for the caufe of thefe and fuch like colours made by refraction, the " biggeft or ftrongeft rays muft penetrate the refracting fuperficies more freely " and eafily than the weaker, and fo be lefs turned awry by it, that is, lefs re- " fracted; which is as much as to fay, the rays, which make red, are leaft refran- " gible, thofe, which make blue and violet, moft refrangible, and others otherwife " refrangible according to their colour: whence, if the rays, which come promif- " cuoufly from the fun, be refracted by a prifm, as in the aforefaid experiment, " thefe of feveral forts being varioufly refracted, muft go to feveral places on an " oppofite paper or wall, and fo parted, exhibit every one their own colours, " which they could not do while blended together. And, becaufe refraction only " fevers them, and changes not the bignefs or ftrength of the ray, thence it is, " that after they are once well fevered, refraction cannot make any further changes " in their colour.

" On this ground may all the phænomena of refractions be underftood: but to " explain the colours made by reflections, I muft further fuppofe, that, though " light be unimaginably fwift, yet the æthereal vibrations, excited by a ray, move " fafter than the ray itfelf, and fo overtake and outrun it one after another. And " this, I fuppofe, they will think an allowable fuppofition, who have been in- " clined to fufpect, that thefe vibrations themfelves might be light. But to make " it the more allowable, it is poffible light itfelf may not be fo fwift, as fome are " apt to think; for, notwithftanding any argument, that I know yet to the con- " trary, it may be an hour or two, if not more, in moving from the fun to us. " This celerity of the vibrations therefore fuppofed, if light be incident on a thin " fkin or plate of any tranfparent body, the waves, excited by its paffage through " the firft fuperficies, overtaking it one after another, till it arrive at the fecond " fuperficies, will caufe it to be there reflected or refracted accordingly as the con- " denfed or expanded part of the wave overtakes it there. If the plate be of fuch " a thicknefs, that the condenfed part of the firft wave overtake the ray at the fe " cond fuperficies, it muft be reflected there; if double that thicknefs, that the " following rarified part of the wave, that is, the fpace between that and the next

" wave,

" wave, overtake it, there it muſt be tranſmitted; if triple the thickneſs, that the
" condenſed part of the ſecond wave overtake it, there it muſt be reflected, and
" ſo where the plate is five, ſeven, or nine times that thickneſs, it muſt be *reflected*
" by reaſon of the third, fourth, or fifth wave, overtaking it at the ſecond ſuper-
" ficies; but when it is four, ſix, or eight times that thickneſs, ſo that the ray
" may be overtaken there by the dilated interval of thoſe waves, it ſhall be *tranſ-*
" *mitted*, and ſo on; the ſecond ſuperficies being made able or unable to reflect
" accordingly as it is condenſed or expanded by the waves. For inſtance, let
" A H Q repreſent the ſuperficies of a ſpherically convex glaſs laid upon a plain
" glaſs A I R, and A I R Q H the thin plane-concave plate of air between them,
" and B C, D E, F G, H I, &c. thickneſſes of that plate, or diſtances of the
" glaſſes in the arithmetical progreſſion of the numbers 1. 2. 3. 4. &c. whereof





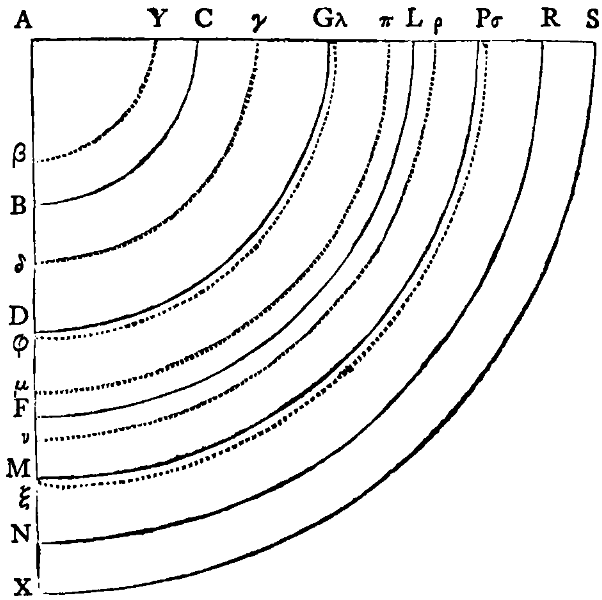
" B C is the diſtance, at which the ray is over-
" taken by the moſt condenſed part of the firſt
" wave: I ſay, the rays incident at B, F, K,
" and O, ought to be *reflected* at C, G, L,
" and P, and thoſe incident at D, H, M, and
" Q, ought to be *tranſmitted* at E, I, N, and
" R; and this, becauſe the ray B C arrives
" at the ſuperficies A C, when it is condenſed,
" by the firſt wave that overtakes it; D E,
" when rarified by the interval of the firſt and
" ſecond; F G, when condenſed by the ſe-
" cond wave; H I, when rarified by the in-
" terval of the ſecond and third; and ſo on
" for an indeterminate number of ſucceſſions; and at A, the center or contact of
" the glaſſes, the light muſt be *tranſmitted*, becauſe there the æthereal mediums
" in both glaſſes are continued as if but one uniform medium. Whence, if the
" glaſſes in this poſture be looked upon, there ought to appear at A, the contact
" of the glaſſes, a black ſpot, and about that many concentric circles of light and
" darkneſs, the ſquares of whoſe ſemidiameters are to ſenſe and arithmetical pro-
" greſſion. Yet all the rays, without exception, ought not to be thus reflected or
" tranſmitted: for ſometimes a ray may be overtaken at the ſecond ſuperficies,
" by the vibrations raiſed by another collateral or immediately ſucceeding ray;
" which vibration, being as ſtrong or ſtronger than its own, may cauſe it to be
" reflected or tranſmitted when its own vibration alone would do the contrary.
" And hence ſome little light will be reflected from the black rings, which makes
" them

" them rather black than totally dark; and fome tranfmitted at the lucid rings, " which makes the black rings, appearing on the other fide of the glaffes, not fo " black as they would otherwife be. And fo at the central black fpot, where the " glaffes do not abfolutely touch, a little light will be reflected, which makes the " fpot darkeft in the middle, and only black at the verges. For thus I have ob- " ferved it to be, by tying very hard together two glafs prifms, which were ac- " cidentally (one of them at leaft) a very little convex, and viewing by divers " lights this black fpot at their contact. If a white paper was placed at a little " diftance behind a candle, and the candle and paper viewed alternately by re- " flection from the fpot, the verges of the fpot, which looked by the light of the " paper as black as the middle part, appeared by the ftronger light of the candle " lucid enough, fo as to make the fpot feem lefs than before; but the middle part " continued as abfolutely black in one cafe as in the other, fome fpecks and ftreaks " in it only excepted, where I fuppofe the glaffes, through fome unevennefs in " the polifh, did not fully touch. The fame I have obferved by viewing the fpot " by the like reflection of the fun and clouds alternately.

" But to return to the lucid and black rings, thofe rings ought always to ap- " pear after the manner defcribed, were light uniform. And after that manner, " when the two contiguous glaffes A Q and A R have been illuftrated, in a dark " room, by light of any uniform colour made by a prifm, I have feen the lucid " circles appear to about twenty in number, with many dark ones between them, " the colour of the lucid ones being that of the light, with which the glaffes were " illuftrated. And if the glaffes were held between the eye and prifmatic colours, " caft on a fheet of white-paper, or if any prifmatic colour was directly trajected " through the glaffes to a fheet of paper placed a little way behind, there would " appear fuch other rings of colour and darknefs (in the firft cafe between the " glaffes, in the fecond, on the paper) oppofitely correfponding to thofe, which " appeared by reflection: I mean, that, whereas by reflected light there appeared " a black fpot in the middle, and then a coloured circle; on the contrary, by tranf- " mitted light there appeared a coloured fpot in the middle, and then a black circle, " and fo on; the diameters of the coloured circles, made by tranfmiffion, equall- " ing the diameters of the black ones made by reflection.

" Thus, I fay, the rings do and ought to appear when made by uniform light; " but in compound light it is otherwife. For the rays, which exhibit red and " yellow, exciting, as I faid, larger pulfes in the æther than thofe, which make " blue and violet, and confequently making bigger circles in a certain propor- " tion, as I have manifeftly found they do, by illuminating the glaffes fucceffively " by the aforefaid colours of prifm in a well darkened room, without changing " the pofition of my eye or of the glaffes; hence the circles, made by illuftrating " the glaffes with white light, ought not to appear black and white by turns, as " the circles made by illuftrating the glaffes; for inftance, with red light, appear " red and black; but the colours, which compound the white light, muft difplay " themfelves by being reflected, the blue and violet nearer to the center than the " red and yellow, whereby every lucid circle muft become violet in the inward " verge, red in the outward, and of intermediate colours in the intermediate

“ parts, and be made broader than before, ſpreading the colours both ways into “ thoſe ſpaces, which I call the black rings, and which would here appear black, “ were the red, yellow, blue, and violet, which make the verge of the rings, taken “ out of the incident white light, which illuſtrates the glaſſes, and the green only “ left to make the lucid rings. Suppoſe C B, G D, L F, P M, R N, S X, re- “ preſent quadrants of the circles made in a dark room by the very deepeſt priſ-



“ matic *red* alone; and Y β, γ δ, “ λ φ, π μ, ρ ν, σ ξ, the qua- “ drants of like circles made “ alſo in a dark room, by the “ very deepeſt priſmatic *violet* “ alone: and then, if the glaſ- “ ſes be illuminated by open “ day light, in which all ſorts “ of rays are blended, it is ma- “ nifeſt, that the firſt lucid “ ring will be Y β B C; the ſe- “ cond γ δ D G, the third, “ λ φ F L, the fourth π μ M P, “ the fifth ρ ν N R, the ſixth “ σ ξ XS, &c. in all which “ the deepeſt violet muſt be “ reflected at the inward edges “ repreſented by the pricked “ lines, where it would be re- “ flected were it alone, and the deepeſt *red* at the outward edges repreſented by “ the black lines, where it would be reflected, were it alone; and all intermediate “ colours at thoſe places, in order, between theſe edges, at which they would be re- “ flected were they alone; each of them in a dark room, parted from all other “ colours by the refraction of a priſm. And becauſe the ſquares of the ſemidia- “ meters of the outward verges A C, A G, A L, &c. as alſo of A Y, A γ, A λ, “ &c. the ſemidiameters of the inward are in arithmetical progreſſion of the num- “ bers 1, 3, 5, 7, 9, 11, &c. and the ſquares of the inward are to the ſquares “ of the outward (A Y^{9} to A C^{9}, A γ^{9} to A G^{9}, A λ^{9} to A L^{9}, &c.) as 9 to 14, “ (as I have found by meaſuring them carefully and often, and comparing the “ obſervations:) therefore the outward *red* verge of the ſecond ring, and inward “ *violet* one of the third, ſhall border upon one another (as you may know by com- “ putation, and ſee them repreſented in the figure) and the like edges of the third “ and fourth rings ſhall interfere, and thoſe of the fourth and fifth interfere more, “ and ſo on. Yea, the colours of every ring muſt ſpread themſelves ſomething “ more both ways than is here repreſented, becauſe the quadrantal arcs here de- “ ſcribed repreſent not the verges, but the middle of the rings made in a dark “ room by the extreme violet and red; the *violet* falling on both ſides the pricked “ arches, and *red* on both ſides the black line arches. And hence it is, that “ theſe rings or circuits of colours ſucceed one another continually, without any

" intervening black, and that the colours are pure only in the three or four firſt " rings, and then intervening and mixing more and more, dilute one another ſo " much, that after eight or nine rings they are no more to be diſtinguiſhed, but " ſeem to conſtitute an even whiteneſs; whereas, when they were made in a dark " room by one of the priſmatic colours alone, I have, as I ſaid, ſeen above twenty " of them, and without doubt could have ſeen them to a greater number, had I " taken the pains to make the priſmatic colour more uncompounded. For by " unfolding theſe rings from one another, by certain refractions expreſſed in the " other [a] papers I ſend you, I have, even in day-light, diſcovered them to above " an hundred; and perhaps they would have appeared innumerable, had the light " or colour illuſtrating the glaſſes been abſolutely uncompounded, and the pupil " of my eye but a mathematical point; ſo that all the rays, which came from " the ſame point of the glaſs might have gone into my eye at the ſame obliquity " to the glaſs.

" What has been hitherto ſaid of the rings, is to be underſtood of their appear- " ance to an unmoved eye: but if you vary the poſition of the eye, the more " obliquely you look on the glaſs, the larger the rings appear. And of this the " reaſon may be, partly that an oblique ray is longer in paſſing through the " firſt ſuperficies, and ſo there is more time between the waving forward and back- " ward of that ſuperficies, and conſequently a larger wave generated, and partly, " that the wave in creeping along between the two ſuperficies may be impeded and " retarded by the rigidneſs of thoſe ſuperficies, bounding it at either end, and ſo " not overtake the ray ſo ſoon as a wave, that moves perpendicularly croſs.

" The bigneſs of the circles made by every colour, and at all obliquities of the " eye to the glaſſes, and the thickneſs of the air, or intervals of the glaſſes, " where each circle is made, you will find expreſſed in the other papers I ſend " you; where alſo I have more at large deſcribed, how much theſe rings inter- " fere, or ſpread into one another; what colours appear in every ring, where " they are moſt lively, where and how much diluted by mixing with the colours of " other rings; and how the contrary colours appear on the back ſide of the glaſſes " by the tranſmitted light, the glaſſes tranſmitting light of one colour at the ſame " place, where they reflect that of another. Nor need I add any thing further of " the colours of other thinly plated mediums, as of water between the aforeſaid " glaſſes, or formed into bubbles, and ſo encompaſſed with air, or of glaſs blown " into very thin bubbles at a lamp furnace, &c. the caſe being the ſame in all theſe, " excepting that, where the thickneſs of the plate is not regular, the rings will not " be ſo; that in plates of denſer tranſparent bodies, the rings are made at a leſs " thickneſs of the plate (the vibrations, I ſuppoſe, being ſhorter in rarer æther than " in denſer) and that in a denſer plate, ſurrounded with a rarer body, the colours " are more vivid than in the rarer ſurrounded with the denſer; as, for inſtance, " more vivid in a plate of glaſs ſurrounded with air, than in a plate of air ſur- " rounded with glaſs; of which the reaſon is, that the reflection of the ſecond ſu- " perficies, which cauſes the colours, is, as was ſaid above, ſtronger in the for-

[a] Obſ. 24.

" mer caſe than in the latter: for which reaſon alſo the colours are moſt vivid, " when the difference of the denſity of the medium is greateſt.

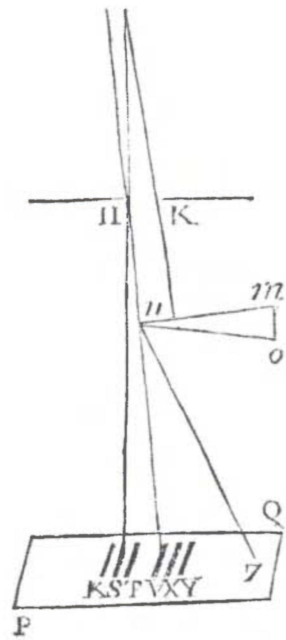
" Of the colours of natural bodies alſo I have ſaid enough in thoſe papers, ſhew- " ing how the various ſizes of the tranſparent particles, of which they conſiſt, is " ſufficient to produce them all, thoſe particles reflecting or tranſmitting this or " that ſort of rays, according to their thickneſs, like the aforeſaid plates, as if they " were fragments thereof. For, I ſuppoſe, if a plate of an even thickneſs, and " conſequently of an uniform colour, were broken into fragments of the ſame thick- " neſs with the plate, a heap of thoſe fragments would be a powder much of the " ſame colour with the plates. And ſo, if the parts be of the thickneſs of the " water in the black ſpot at the top of a bubble deſcribed in the ſeventeenth of " the obſervations I ſend you, I ſuppoſe the body muſt be black. In the pro- " duction of which blackneſs, I ſuppoſe, that the particles of that ſize being diſ- " poſed to reflect almoſt no light outward, but to refract it continually in its paſ- " ſage from every part to the next; by this multitude of refractions, the rays " are kept ſo long ſtraggling to and fro within the body, till at laſt almoſt all " impinge on the ſolid parts of the body, and ſo are ſtopped and ſtifled; thoſe " parts having no ſufficient elaſticity, or other diſpoſition to return nimbly enough " the ſmart ſhock of the ray back upon it.

" I ſhould here conclude, but that there is another ſtrange phænomenon of " colours, which may deſerve to be taken notice of. Mr. HOOKE, you may re- " member, was ſpeaking of an odd ſtraying of light, cauſed in its paſſage near the " edge of a razor, knife, or other opake body in a dark room; the rays, which " paſs very near the edge, being thereby made to ſtray at all angles into the " ſhadow of the knife.

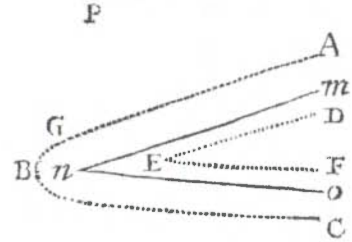
" To this Sir WILLIAM PETTY, then preſident, returned a very pertinent query, " Whether that ſtraying was in curve lines? and that made me, having heard " Mr. HOOKE ſome days before compare it to the ſtraying of ſound into the qui- " eſcent medium, ſay, that I took it to be only a new kind of refraction, cauſed " perhaps by the external æther's beginning to grow rarer a little before it " came at the opake body, than it was in free ſpaces; the denſer æther without " the body, and the rarer within it, being terminated not in a mathematical " ſuperficies, but paſſing into one another through all intermediate degrees of " denſity: whence the rays, that paſs ſo near the body, as to come within that " compaſs, where the outward æther begins to grow rarer, muſt be refracted by " the uneven denſeneſs thereof, and blended inwards toward the rarer medium of " the body. To this Mr. HOOKE was then pleaſed to anſwer, that though it " ſhould be but a new kind of refraction, yet it was a new one. What to make " of this unexpected reply, I knew not; having no other thoughts, but that a " new kind of refraction might be as noble an invention as any thing elſe about " light; but it made me afterwards, I know not upon what occaſion, happen to " ſay, among ſome that were preſent to what paſſed before, that I thought I had " ſeen the experiment before in ſome Italian author. And the author is HONO- " RATUS FABER, in his dialogue De Lumine, who had it from GRIMALDO;

" whom

“ whom I mention, becauſe I am to deſcribe ſomething further out of him, which
“ you will apprehend by this figure : ſuppoſe the ſun ſhine through the little hole
“ H K into a dark room upon the paper P Q, and with a wedge M N O intercept
“ all but a little of that beam, and you will ſee
“ upon the paper ſix rows of colours, R, S, T,
“ V, X, Y, and beyond them a very faint light
“ ſpreading either way, ſuch as rays broken, like
“ H N Z, muſt make. The author deſcribes it
“ more largely in divers ſchemes. I have time
“ only to hint the ſum of what he ſays.



“ Now for the breaking of the ray H N Z, ſup-
“ poſe, in the next figure M N O be the ſolid
“ wedge, A B C the inward bound of the uniform
“ rarer æther within, between which bounds the
“ æther runs through all the intermediate degrees;
“ and it is manifeſt, that, if a ray come between
“ B and N, it muſt in its paſſage there bend from
“ the denſer medium towards C, and that ſo much
“ the more, by how much it comes nearer N. Fur-
“ ther, for the three rows of colours V X Y, thoſe
“ may perhaps proceed from the number of vibra-
“ tions (whether one, two, or three) which over-
“ take the ray in its paſſage from G, till it be about
“ the mid-way between G and H; that is, at its
“ neareſt diſtance to N, ſo as to touch the circle
“ deſcribed about N, with that diſtance; by the
“ laſt of which vibrations, expanding or con-
“ tracting the medium there, the ray is licenſed
“ to recede again from N, and go on to make the
“ colours; or further bent about N, till the inter-
“ val of the next wave overtake it, and give it li-
“ berty to go from N, very nearly in the line it is
“ then moving, ſuppoſe toward Z, to cauſe the faint light ſpoken of above, you
“ will underſtand me a little better, by comparing this with what was ſaid of the
“ colours of thin tranſparent plates, comparing the greateſt diſtance that the ray
“ goes from G B H towards N, to the thickneſs of one of thoſe plates. Some-
“ thing too there is in DES CARTES's explication of the rainbow's colours, which
“ would give further light in this. But I have no time left to inſiſt further upon
“ particulars; nor do I propound this without diffidence, having not made ſuffici-
“ ent obſervation about it.”



After reading this diſcourſe, Mr. HOOKE ſaid, that the main of it was contained in his *Micrographia*, which Mr. NEWTON had only carried farther in ſome particulars.

The Society adjourned till December 30.

Decem-

December 30. There was read a letter to Mr. OLDENBURG from Mr. NEWTON, dated at Cambridge 21ſt December, 1675 [1], in anſwer to what had been written to him by Mr. OLDENBURG concerning the want of ſuccefs of his experiment made with a glaſs rubbed, &c. This letter was as follows :

" Upon your letter I took another glaſs four inches broad, and one fourth of " an inch thick, of ſuch glaſs as teleſcopes are made of, and placed it a one ſixth " part of an inch from the table. It was ſet in ſuch a piece of wood, as the ob- " ject-glaſſes of teleſcopes uſe to be ſet in : and the experiment ſucceeded well. " After the rubbing was ſtill, and all was ſtill, the motion of the papers would " continue ſometimes while I counted a hundred, every paper leaping up about " twenty times more or leſs, and down as often. I tried it alſo with two other glaſſes " that belong to a teleſcope, and it ſucceeded with both; and I make no queſtion " but any glaſs will do that, if it be excited to electric virtue, as I think any may. " If you have a mind to any of theſe glaſſes, you may have them; but I ſup- " poſe, if you cannot make it do in other glaſs, you will fail in any I can ſend " you. I am apt to ſuſpect the failure was in the manner of rubbing; for I have " obſerved, that the rubbing variouſly, or with various things, alters the caſe. At " one time I rubbed the aforeſaid great glaſs with a napkin, twice as much as I " uſed to do with my gown, and nothing would ſtir; and yet preſently rubbing " it with ſomething elſe, the motions ſoon began. After the glaſs has been " much rubbed too, the motions are not ſo laſting; and the next day I found the " motions fainter and difficulter to excite than the firſt. If the Society have a " mind to attempt it any more, I can give no better advice than this: to take a " new glaſs not yet rubbed (perhaps one of the old ones may do well enough after " it has lain ſtill a while) and let this be rubbed, not with linen, nor ſoft nappy " woolen, but with ſtuff, whoſe threads may rake the ſurface of the glaſs, ſup- " poſe tamerine, or the like, doubled up in the hand, and this with a briſk mo- " tion as may be, till an hundred or an hundred and fifty may be counted, the " glaſs lying all the while over the papers. Then, if nothing ſtir, rub the glaſs " with the finger ends half a ſcore of times to and fro, or knock your finger- " ends as often upon the glaſs; for this rubbing or knocking with your fingers, " after the former rubbing, conduces moſt to excite the papers. If nothing ſtir " yet, rub again with the cloth till ſixty or eighty may be counted, and then " rub or knock again with your fingers, and repeat this till the electric virtue of " the glaſs be ſo far excited as to take up the papers, and then a very little rubb- " ing or knocking now and then will revive the motions. In doing all this, let " the rubbing be always done as nimbly as may be; and if the motion be circu- " lars, like that of glaſs-grinding, it may do better. But if you cannot make it " yet ſucceed, it muſt be let alone till I have ſome opportunity of trying it be- " fore you. As for the ſuſpicion of the papers being moved by the air, I am ſe- " cure from that; yet in the other, of drawing leaf-gold to above a foot diſtance, " which I never went about to try myſelf till the laſt week, I ſuſpect the air might " raiſe the gold, and then a ſmall attraction might determine it towards the glaſs; " for I could not make it ſucceed."

[1] Letter-book, vol. vii. p. 284.

It

It was ordered, that Mr. NEWTON's directions in this letter should be observed in the experiment to be made at the next meeting of the Society.

Mr. OLDENBURG read a letter to himself from Mr. JOHN GASCOIGNE, dated at Liege, 15th December, 1675 [u], acquainting him with the death of Mr. LINUS of the epidemical disease, which then raged through so many countries, and with the resolution of Mr. LINUS's disciples, to try Mr. NEWTON's experiment concerning light and colours more clearly and carefully, and before more witnesses, according to the directions given them by Mr. NEWTON's last letter: intimating withal, that if the said experiment be made before the Royal Society, and be attested by them to succeed, as Mr. NEWTON affirmed, they would rest satisfied.

It was ordered, that when the sun should serve, the experiment should be made before the Society.

Mr. AUBREY presented the Society with his observations made in Wiltshire, which being read, he was desired to endeavour to procure some of the iron-ore of Sein in that county, said to be so rich, that the smith could melt it in his forge: as also to procure from Easton-Peires in Malmesbury hundred, some of the blue clay, free from sand, and almost of the colour of ultramarine; which clay Mr. DOIGHT supposed to be very fit for porcelane.

The Society adjourned till the 13th of January following.

January 13. Captain HENRY SHEERES, JOHN MAPLETOFT, M. D. [x], and Signor FRANCISCO TRAVAGINI were proposed candidates, the first in the name of Sir JOSEPH WILLIAMSON, the second by Mr. HOOKE, and the third by Mr. OLDENBURG.

Mr. NEWTON's experiment of glass rubbed, to cause various motions in bits of paper underneath, being made according to his more particular directions, succeeded very well. The rubbing was made both with a scrubbing brush, made of short hog's bristles, with a knife, the haft of the knife made of whalebone, and with the nail of one's finger. It appeared, that touching many parts at once with a hard and rough body, produced the effect expected.

It was ordered, that Mr. NEWTON should have the thanks of the Society, for giving himself the trouble of imparting to them such full instructions for making the experiment.

Mr. OLDENBURG produced and read a Latin letter of Mr. FLAMSTEAD to Sir JONAS MOORE, dated at Greenwich, 24th December, 1675 [y], containing an account of his observations made of the late eclipse of the moon on the 21st December, *p. m.*

[u] Letter-book, vol. vii. p. 282.

[x] Professor of physic at Gresham College.

[y] It is printed in the Philosoph. Transact. vol. x. n° 121. p. 495.

It

It was ordered, that Mr. OLDENBURG should be desired, according to the motion made by Mr. FLAMSTEAD, to impart these observations to Signor CASSINI at Paris, and to desire him to communicate to the Society his observations on the same eclipse.

Mr. OLDENBURG produced likewise some papers of Mr. AUBREY, containing his observations of the county of Surry. But the time being elapsed, these papers were referred to the next meeting.

January 20. Mr. AUBREY's papers of observations on Surrey were read.

There was also read the beginning of Mr. NEWTON's discourse, containing such observations, as conduce to further discoveries for completing his theory of light and colours, especially as to the constitution of natural bodies, on which their colours or transparency depend: in which he describes first the principal of his observations, and then considers and makes use of them.

At this time there were read the first fifteen of those observations as follow [z]:

" I suppose you understand, that all transparent substances (as glass, water, " air, &c.) when made very thin by being blown into bubbles, or otherwise " formed into plates, do exhibit various colours, according to their various thin- " ness, although at a greater thickness they appear very clear and colourless. In " my former discourse about the constitution of light, I omitted these colours, " because they seemed of a more difficult consideration, and were not necessary for " the establishing of the doctrine, which I propounded; but because they may con- " duce to further discoveries for compleating that theory, especially as to the " constitution of the parts of natural bodies, on which their colours or transpa- " rency depend, I have now sent you an account of them. To render this dis- " course short and distinct, I have first described the principal of my observations, " and then considered and made use of them. The observations are these:

" Obs. 1. Compressing two prisms hard together, that their sides (which by " chance were a very little convex) might somewhere touch one another, I found " the place, in which they touched, to become [a] absolutely transparent, as if they " had been there one continued piece of glass; for when the light fell so ob- " liquely on the air, which in other places was between them, as to be all re- " flected, in that place of contact it seemed wholly transmitted; insomuch that " when looked upon, it appeared like a black or dark spot, by reason of no sen- " sible light was reflected from thence, as from other places; and when looked " through, it seemed, as it were, a hole in that air, that was formed into a thin

[z] Register, vol. v. p. 89.

[a] " Note, that there is some light reflected from " those parts of this black spot, where the glasses, " by reason of their convexity, and some little un- " evenness of their surfaces, do not come to abso- " lute contact. For by viewing the sun, by re- " flection from this spot, not only the verges of " it became lucid, but divers lucid veins, as specks, " appeared in the midst of the blackness: but yet " some parts of the spot seemed still as black as " before, which parts I take to be those, where " the glasses touched.

" plate by being compreſſed between the glaſſes; and through this hole objects" that were beyond, might be ſeen diſtinctly, which could not at all be ſeen through " other parts of the glaſſes, where the air was interjacent. Although the glaſſes " were a little convex, yet this tranſparent ſpot was of a conſiderable breadth, " which breadth ſeemed principally to proceed from the yielding inwards of the " parts of the glaſſes by reaſon of their mutual preſſure; for by preſſing them very " hard together, it would become much broader than otherwiſe.

" Obſ. 2. When the plate of air, by turning the priſms about their common " axis, became ſo little inclined to the incident rays, that ſome of them began to " be tranſmitted, there aroſe in it many ſlen- " der arcs of colours, which at firſt were " ſhaped almoſt like the conchoid, as you ſee " them here delineated. And by continuing " the motion of the priſms, theſe arcs in- " creaſed and bended more and more about " the ſaid tranſparent ſpot, till they were " compleated into circles or rings incompaſſing it, and afterwards continually " grew more and more contracted.

Fig. I.

" Theſe arcs, at their firſt appearance, were of a violet and blue colour, and " between them were white arcs of circles, which preſently became a little tinged " in their inward limbs with red and yellow, and to their outward limbs the blue " was adjacent; ſo that the order of theſe colours from the central dark ſpot, " was at that time white, blue, violet, black, red, orange, yellow, white, blue, " violet, &c. but the yellow and red were much fainter than the blue and vio- " let.

" The motion of the priſms about their axis being continued, theſe colours " contracted more and more, ſhrinking towards the whiteneſs on either ſide of " it, until they totally vaniſhed into it; and then the circles in thoſe parts ap- " peared black, and white, without any other colours intermixed; but by fur- " ther moving the priſms about, the colours again emerged out of the whiteneſs, " the violet and blue at its inward limb, and at its outward limb the red and yel- " low; ſo that now their order from the central ſpot was white, yellow, red, " black, violet, blue, white, yellow, red, &c. contrary to what it was before.

" Obſ. 3. When the rings or ſome parts appeared only black and white, they " were very diſtinct and well defined, and the blackneſs ſeemed as intenſe as that " of the central ſpot; alſo, in the borders of theſe rings, where the colours began " to emerge out of the whiteneſs, they were pretty diſtinct, which made them vi- " ſible to a very great multitude. I have ſometimes numbered above thirty ſuc- " ceſſions (reckoning every black and white ring for one ſucceſſion) and ſeen more " of them, which by reaſon of their ſmallneſs I could not number. But in other " poſitions of the priſms, at which the rings appeared of many colours, I could " not diſtinguiſh above eight or nine of them, and the exterior of thoſe too " were confuſed and dilute.

" In theſe two obſervations, to ſee the rings diſtinct, and without any other colour but black and white, I found it neceſſary that I held my eye at a good diſtance from them. For by approaching nearer, although in the ſame inclination of my eye, yet there emerged a bluiſh colour out of the white, which by dilating itſelf more and more into the black, rendered the circles leſs diſtinct, and left the white a little tinged with red and yellow. I found alſo, that by looking through a ſlit or oblong hole, which was narrower than the pupil of my eye, and held cloſe to it parallel to the priſms, I could ſee the circles much diſtincter and viſible to a far greater number than otherwiſe.

" Obſ. 4. To obſerve more nicely the order of the colours, which aroſe out of the white circles, as the rays became leſs and leſs inclined to the plate of air; I took two object-glaſſes, the one a plane-convex for a fourteen foot teleſcope, and the other a large double convex for one of fifty foot; and upon this laying the other with its plane ſide downwards, I preſſed them ſlowly together, to make the colours ſucceſſively emerge in the middle of the circles, and then ſlowly lifted the upper glaſs from the lower, to make them ſucceſſively vaniſh again in the ſame place, where being of a conſiderable breadth, I could more eaſily diſcern them. And by this means I obſerved their ſucceſſion and quantity to be as followeth.

" Next to the pellucid central ſpot made by the contact of the glaſſes, ſucceeded violet, blue, white, yellow, and red. The violet and blue were ſo very little in quantity, that I could not diſcern them in the circles made by the priſms; but the yellow and red were pretty copious, and ſeemed about as much in extent as the white, and four or five times more than the blue and violet. The next circuit or order of colours immediately encompaſſing theſe was violet, blue, green, yellow, and red. And theſe were all of them copious and vivid, excepting the green, which was very little in quantity, and ſeemed much more faint and dilute than the other colours. Of the other four the violet was leaſt, and the blue leſs than the yellow or red. The third circuit or order was alſo purple, blue, green, yellow, and red, in which the purple ſeemed more reddiſh than the violet in the former circuit, and the green was much more conſpicuous, being as briſk and copious as any of the other colours except the yellow; but the red began to be a little faded, inclining very much to purple. After theſe ſucceeded green and red: the green was very copious and lively, inclining on the one ſide to blue, and the other to yellow. But in this fourth circuit there was neither violet, blue, nor yellow, and the red was very imperfect and dirty. Alſo the ſucceeding colours became more and more imperfect and dilute, till after three or four more revolutions they ended in perfect whiteneſs.

" Obſ. 5. To determine the interval of the glaſſes, or thickneſs of the interjacent air, by which each colour was produced; I meaſured the diameter of the firſt ſix rings at the moſt lucid part of their orbits, and ſquaring them I found their ſquares to be in arithmetical progreſſion of the odd numbers, 1. 3. 5. 7. 9. 11. And ſince one of the glaſſes was plane and the other ſpherical, their

" intervals at thofe rings muft be in the fame progreffion. I meafured alfo the " diameters of the dark or faint rings between the more lucid colours, and found " their fquares to be in arithmetical progreffion, of the even numbers 2, 4, 6, " 8, 10, 12; and it being very nice and difficult to take thefe meafures exactly, " I repeated them divers times, at divers parts of the glaffes, that by their agree- " ment I might be confirmed in them; and the fame method I ufed in deter- " mining fome others of the following obfervations.

" Obf. 6. The diameter of the firft ring, at the moft lucid part of its orbit, " was $\frac{58}{100}$ parts of an inch, and the diameter of the fphere, on which the double " convex object-glafs was ground, was an hundred and two foot, as I found by " meafuring it; and confequently the thicknefs of the air, or aereal interval of the " glaffes at that ring, was $\frac{1}{14554}$ of an inch. For as the diameter of the faid fphere " (an hundred and two foot, or twelve hundred and twenty-four inches) is to " the femidiameter of the ring $\frac{29}{100}$, fo very nearly is that femidiameter to $\frac{1}{14554}$, " the faid diftance of the glaffes. Now, by the precedent obfervations, the " eleventh part of this diftance ($\frac{1}{160094}$) is the thicknefs of the air at that part " of the firft ring, where the yellow would be moft vivid, were it not mixed " with other colours in the white; and this doubled gives the difference of its " thicknefs at the yellow in all the other rings, viz. $\frac{1}{80047}$, or, to ufe a round " number, the eighty thoufand part of an inch.

" Obf. 7. Thefe dimenfions were taken, when my eye was placed perpendicu- " larly over the glaffes, in or near the axis of the rings; but when I viewed " them obliquely, they became bigger, continually fwelling as I removed my eye " farther from their axis; and partly by meafuring the diameter of the fame " circle at feveral obliquities of my eye, partly by other means; as alfo by mak- " ing ufe of the two prifms for very great obliquities, I found its diameter, and " confequently the thicknefs of the air at its perimeter in all thofe obliquities, to " be very nearly in the proportions expreffed in this table.

Incidence on the air.		Refraction into the air.		Diameter of the ring.	Thicknefs of the air.
gr.	min.	gr.	min.		
00	00	00	00	. 10	. 10
6	26	10	00	. 10$\frac{1}{13}$	. 10$\frac{2}{16}$
12	45	20	00	. 10$\frac{1}{3}$	. 10$\frac{2}{3}$
18	49	30	00	. 10$\frac{3}{4}$	. 11$\frac{1}{2}$
24	30	40	00	. 11$\frac{2}{5}$	. 13
29	37	50	00	. 12$\frac{1}{2}$	. 15$\frac{1}{2}$
33	58	60	00	. 14	. 20
35	47	65	00	. 15$\frac{1}{4}$	. 23$\frac{1}{5}$
37	19	70	00	. 16$\frac{4}{5}$	. 28$\frac{1}{2}$
38	33	75	00	. 19$\frac{1}{4}$	. 37
39	27	80	00	. 22$\frac{6}{7}$	. 52$\frac{1}{4}$
40	00	85	00	. 29	. 84
40	11	90	00	. 35	. 122$\frac{1}{2}$

" In the two first columns are expressed the obliquities of the rays to the plate " of air; that is, their angles of incidence and refraction. In the third column, " the diameter of any coloured ring of those obliquities is expressed in parts, of " which ten constitute that diameter, when the rays are perpendicular. And [in " the fourth column the thickness of the air at the circumference of that ring is " expressed in parts, of which also ten constitute that thickness, when the rays " are perpendicular.

" Obs. 8. The dark spot in the middle of the rings increased also by that " obliquation of the eye, although almost insensibly. But, if instead of the " object-glasses, the prisms were made use of, its increase was more manifest, when " viewed so obliquely, that no colours appeared about it. It was least, when the " rays were incident most obliquely on the interjacent air, and increased more and " more, until the coloured rings appeared, and then decreased again, but not so " much as it increased before. And hence it is evident, that the transparency " was not only at the absolute contact of the glasses, but also where they had some " little interval. I have sometimes observed the diameter of that spot to be be- " tween half and two fifth parts of the diameter of the exterior circumference of " the red in the first circuit or revolution of colours, when viewed almost per- " pendicularly; whereas, when viewed obliquely, it hath wholly vanished, and " become opake and white, like the other parts of the glass. Whence it may " be collected, that the glasses did then scarcely, or not at all, touch one ano- " ther; and that their interval of the perimeter of that spot, when viewed per- " pendicularly, was about a fifth or sixth part of their interval at the circum- " ference of the said red.

" Obs. 9. By looking through the two contiguous object-glasses, I found, that " the interjacent air exhibited rings of colours, as well by transmitting light as " by reflecting it. The central spot was now white, and from it the order of " the colours were yellowish, red, black, violet, blue, white, yellow, red; " violet, blue, green, yellow, red, &c. but these colours were very faint and " dilute, unless when the light was trajected very obliquely through the glasses; " for by that means they became pretty vivid, only the first yellowish red, like " the blue in the fourth observation, was so little and faint as scarcely to be dis- " cerned. Comparing the coloured rings made by reflection with these made by " transmission of the light, I found, that white was opposite to black, red to blue, " yellow to violet, and green to a compound of red and violet; that is, those " parts of the glass were black when, looked through, which when looked upon " appeared white, and on the contrary; and so those, which in one case exhibited " blue, did in the other case exhibit red; and the like of the other colours.

" Obs. 10. Wetting the object-glass a little at their edges, the water crept in " slowly between them, and the circles thereby became less, and the colours " more faint; insomuch that, as the water crept along, one half of them, at which " it first arrived, would appear broken off from the other half, and contracted " into a less room. By measuring them I found the proportion of their diameters " to the diameters of the like circles made by air, to be about seven to eight;

" and

" and confequently the intervals of the glaffes at like circles, caufed by thefe two " mediums, water and air, are as about three to four. Perhaps it may be a general " rule, that if any other medium, more or lefs denfe than water, be compreffed " between the glaffes, their interval at the rings, caufed thereby, will be to their " interval, caufed by interjacent air, as the fines are, which meafure the refrac- " tion made out of that medium into air.

" Obf. 11. When the water was between the glaffes, if I preffed the upper " glafs varioufly at its edges to make the rings move nimbly from one place to " another, a little bright fpot would immediately follow the center of them, " which, upon creeping in of the ambient water into that place, would prefently " vanifh. Its appearance was fuch, as interjacent air would have caufed, and it " exhibited the fame colours; but it was not air, for where any aereal bubbles " were in the water they would not vanifh. The reflection muft rather have been " caufed by a fubtiler medium, which could recede through the glafs at the " creeping in of the water.

" Obf. 12. Thefe obfervations were made in the open air. But further, to " examine the effects of coloured light falling on the glaffes, I darkened the " room, and viewed them by reflection of the colours of a prifm caft on a fheet " of white paper; and by this means the rings became diftincter, and vifible to " a far greater number than in the open air.

" I have feen more than twenty of them, whereas in the open air I could not " difcern above eight or nine.

" Obf. 13. Appointing an affiftant to move the prifm to and fro about its " axis, that all its colours might fucceffively fall on the fame place of the paper, " and be reflected from the circles to my eye whilft I held it immoveable; I " found the circles, which the red light made, to be manifeftly bigger than " thofe, which were made by the blue and violet; and it was very pleafant to fee " them gradually fwell or contract, accordingly as the colour of the light was " changed. The interval of the glafs at any of the rings, when they were made " by the utmoft red light, was to their interval at the fame ring, when made " by the utmoft violet, greater than three to two, and lefs than thirteen to eight. " By the moft of my obfervations it was as nine to fourteen. And this pro- " portion feemed very nearly the fame in all obliquities of my eye, unlefs when " two prifms were made ufe of inftead of the object-glaffes: for then, at a " certain great obliquity, the rings made by the feveral colours feemed equal; " and, at a greater obliquity, thofe made by the violet would be greater than the " fame rings made by the red.

" Obf. 14. While the prifm was turned about uniformly, the contraction or " dilatation of a ring made by all the feveral colours of the prifm fucceffively " reflected from the object-glaffes, was fwifteft in the red, floweft in the violet, " and in intermediate colours it had intermediate degrees of celerity. Comparing the extent, which each colour obtained by this contraction or dila[illegible] I found,

" that

"that the blue was ſenſibly more extended than the violet, the yellow than the "blue, and the red than the yellow. And, to make a juſter eſtimation of their "proportions, I obſerved, that the extent of the red was almoſt double to that "of the violet, and that the light was of a middle colour between yellow and "green at that interval of the glaſſes, which was an arithmetical mean between "the two extremes; contrary to what happens in the colours made by the re- "fraction of a priſm, where the red is moſt contracted, the violet moſt expanded, "and in the midſt of them is the confine of green and blue.

"Obſ. 15. Theſe rings were not of various colours, like thoſe in the open "air, but appeared all over of that priſmatic colour only, with which they were "illuminated: and, by projecting the priſmatic colours immediately upon the "glaſſes, I found, that the light, which fell on the dark ſpaces, which were be- "tween the coloured rings, was tranſmitted through the glaſſes without any va- "riation of colour. For, on a white paper placed behind, it would paint rings "of the ſame colour with thoſe, which were reflected, and of the bigneſs of their "intermediate ſpaces. And from hence the origin of theſe rings is manifeſt, "namely, that the aereal interval of the glaſſes, according to its various thick- "neſs, is diſpoſed in ſome places to reflect, and in others to tranſmit, the light "of any colour; and, in the ſame place to reflect one colour, where it tranſmits "another.

Theſe obſervations ſo well pleaſed the Society, that they ordered Mr. Oldenburg to deſire Mr. Newton to permit them to be publiſhed, together with the reſt; which, they preſumed, did correſpond with thoſe, that had been now read to them.

Beſides, there was read a paſſage of Mr. Newton's letter to Mr. Oldenburg, of 21 December, 1675, ſtating the difference between his hypotheſis and that of Mr. Hooke. Which paſſage was as follows:

"As for Mr. Hooke's inſinuation, that the ſum of the hypotheſis I ſent you "had been delivered by him in his Micrography, I need not be much concerned "at the liberty he takes in that kind: yet, becauſe you think it may do well, "if I ſtate the difference I take to be between them, I ſhall do it as briefly as I "can, and that the rather, that I may avoid the ſavour of having done any "thing unjuſtifiable or unhandſome towards Mr. Hooke. But, for this end, I "muſt firſt (to ſee what is his) caſt out what he has borrowed from Des Cartes, or others, viz. that there is an æthereal 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 cer- "tain proportion; that refraction ariſes from this acceleration, and has ſines "proportional; that light is at firſt uniform; that its colours are ſome diſtur- "bance or new modification of its rays by refraction or reflection; that the co- "lours 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

" the other fide, where blue appears; and, that there are but thefe two original " colours, or colour-making modifications of light, which by their various de- " grees, or, as Mr. HOOKE calls it, dilutings, produce all intermediate ones. " This rejected, the remainder of his hypothefis is, that he has changed DES " CARTES's preffing or progreffive motion of the medium to a vibrating one, the " rotation of the globuli to the obligation of pulfes, and the accelerating their " rotation on the one hand, and retarding it on the other, by the quiefcent me- " dium, to produce colours, to the like action of the medium on the two ends of " his pulfes for the fame end. And having thus far modified his by the Carte- " fian hypothefis, he has extended it further, to explicate the phænomena of thin " plates, and added another explication of the colours of natural bodies, fluid " and folid.

" This, I think, is in fhort the fum of his hypothefis; and in all this I have " nothing common with him, but the fuppofition, that æther is a fufcep- " tible medium of vibrations, of which fuppofition I make a very different ufe; " he fuppofing it a light itfelf, which I fuppofe it is not. This is as great a dif- " ference as is between him and DES CARTES. But befides this, the manner of " refraction and reflection, and the nature and production of colours in all cafes " (which takes up the body of my difcourfe) I explain very differently from " him; and even in the colours of thin tranfparent fubftances, I explain every " thing after a way fo differing from him, that the experiments I ground my " difcourfe on, deftroy all he has faid about them; and the two main experi- " ments, without which the manner of the production of thofe colours is not to " be found out, were not only unknown to him, when he wrote his Microgra- " phy, but even laft fpring, as I underftood, in mentioning them to him. This " therefore is the fum of what is common to us, that æther may vibrate; and " fo, if he thinks fit to ufe that notion of colours, arifing from the various big- " nefs of pulfes (without which his hypothefis will do nothing) *his* will borrow " as much from my anfwer to his objections, as that I fend you does from his " Micrography.

" But, it may be, he means, that I have made ufe of his obfervations, and of " fome I did; as, that of the inflection of rays, for which I quoted him; that " of opacity, arifing from the interftices of the parts of bodies, which I infift " not on; and that of plated bodies exhibiting colours, a phænomenon, for the " notice of which I thank him. But he left me to find out and make fuch ex- " periments about it, as might inform me of the manner of the production of thofe " colours, to ground an hypothefis on; he having given no further infight to it " than this, that the colour depended on fome certain thicknefs of the plate; " though what that thicknefs was at every colour, he confeffes in his Microgra- " phy, he had attempted in vain to learn; and therefore, feeing I was left to " meafure it myfelf, I fuppofe he will allow me to make ufe of what I took " the pains to find out. And this I hope may vindicate me from what Mr. " HOOKE has been pleafed to charge me with."

The reading of the reft of Mr. NEWTON's difcourfe was referred to the next meeting.

January

January 27. Mr. OLDENBURG produced from his highneſs prince RUPERT a piece of marble, having ſeveral pictures of boys and trees painted upon it in ſuch a manner, that all the out-lines of the pictures were exactly defined without any flowing of the colours abroad, and the colours fixed by the fire, and afterwards ſo poliſhed, that they would be permanent, and laſt as long as the marble.

This was acknowledged by the members to be a very great improvement of what had been done at Oxford by a certain ſtone-cutter there; and that all, that had been performed before in this art, was not comparable to this degree of improvement.

Mr. HOOKE remarked, that he conceived, that there were but two colours in this piece; and that he had a method of doing it with moſt colours, and to paint with them upon marble almoſt as curiouſly as with a pencil.

Mr. NEWTON's letter of January 25, $167\frac{5}{6}$ [c], in which he acknowledged the favour of the Society in their kind acceptance of his late papers; and declared, that he knew not how to deny any thing, which they deſired ſhould be done: but he requeſted, that the printing of his obſervations about colours might be ſuſpended for a time, becauſe he had ſome thoughts of writing ſuch another ſet of obſervations for determining the manner of the production of colours by the priſm: which obſervations, he ſaid, ought to precede thoſe now in the Society's poſſeſſion, and would be moſt proper to be joined with them.

There was alſo read a letter of Mr. PASCALL of Somerſetſhire to Mr. AUBREY, dated 18 January, $167\frac{5}{6}$, containing ſome natural obſervations of that county, viz. concerning the nature of the lead-mines in Mendip-Hills; a well reſembling the ſulphur-well near the Spaw in Yorkſhire; a ſpring petrifying far more than the dropping-well at Knareſborough in the north; the motion of ſome underground waters in the pariſhes of ZOLANDE, formerly recovered from the ſea, &c.

It was ordered, that the reading of Mr. NEWTON's obſervations about colours be continued at the next meeting.

February 3. There was preſented from Dr. WALLIS his edition of ARCHIMEDES's *Arenarius*, with a new tranſlation of his and notes, printed at Oxford, in 1676.

The reading of Mr. NEWTON's obſervations on colours was continued, viz. that part, wherein he explains by the ſimpleſt of colours the moſt recompounded; as follows:

" Obſ. 16. The ſquares of the diameters of theſe rings, made by priſmatic
" colour, were in arithmetical progreſſion, as in the fifth obſervation. And the
" diameter of the ſixth circle, when made by the yellow, and viewed almoſt
perpendicularly, was obout $\frac{58}{100}$ parts of an inch, agreeable to the ſixth obſer-
" vation.

[c] There are no letters entered from the beginning of the year $167\frac{5}{6}$ till July 1677.

" perpen-

" The precedent obfervations were made with a rarer thin medium terminated " by a denfer, fuch as was air or water compreffed betwixt two glaffes. In " thofe, that follow, are fet down the appearances of a denfer medium thinned " within a rarer; fuch as are plates of Mufcovy-glafs, bubbles of water, and " fome others thin fuftances terminated on all fides with air.

" Obf. 17. If a bubble be blown with water, firft made tenacious by diffolv- " ing a little foap in it, it is a common obfervation, that after a while it will " appear tinged with a great variety of colours. To defend thefe bubbles from " being agitated by the external air (whereby their colours are irregularly moved " one among another, fo that no accurate obfervation can be made of them) as " foon as I had blown any of them, I covered it with a clear glafs, and by that " means its colours emerged in a very regular order, like fo many concentric " rings incompaffing the top of the bubble. And as the bubble grew thinner " by the continual fubfiding of the water, thefe rings dilated flowly, and over- " fpread the whole bubble, defcending in order to the bottom of it, where they " vanifhed fucceffively. In the mean while, after all the colours were emerged " at the top, there grew in the center of the rings a fmall, round, black fpot, " like that in the firft obfervation, which continually dilated itfelf, till it became " fometimes more than one half or three fourths of an inch in breadth, before the " bubble broke. At firft I thought there had been no light reflected from the water " in that place; but obferving it more curioufly, I faw within it feveral fmaller, " round fpots, which appeared much blacker and darker than the reft, whereby " I knew, that there was fome reflection at the other places, which were not fo " dark as thofe fpots. And by further trial I found, that I could fee the images " (as of a candle or the fun) very faintly reflected, not only from the great black " fpot, but alfo from the little darker fpots, which were within it.

" Befides the aforefaid coloured rings, there would often appear fmall fpots of " colours afcending and defcending up and down the fide of the bubble, by rea- " fon of fome inequalities in the fubfiding of the water; and fometimes fmall black " fpots generated at the fides, would afcend up to the larger black fpot at the " top of the bubble, and unite with it.

" Obf. 18. Becaufe the colours of thefe bubbles were more extended and " lively than thofe of air thinned between two glaffes, and fo more eafy to be " diftinguifhed, I fhall here give you a further defcription of their order, as they " were obferved in viewing them by reflection of the fkies, when of a white " colour, whilft a black fubftance was placed behind the bubble: and they were " thefe; red, blue, red, blue; red, blue; red, green; red, yellow; green, blue, " purple; red, yellow, green, blue, violet; red, yellow, white, blue, black.

" The three firft fucceffions of red and blue were very dilute and dirty, efpe- " cially the firft, where the red feemed in a manner to be white. Amongft thefe " there was fcarcely any other colour fenfible, only the blues (and principally the " fecond blue) inclined a little to green.

" The fourth red was alfo dilute and dirty, but not fo much as the former " three: after that fucceeded little or no yellow, but a copious green, which at " firft was inclined a little to yellow, and then became a pretty brifk and good " willow green, and afterwards changed to a blueifh colour; but there fucceded " neither blue nor violet.

" The fifth red at firft was very much inclined to purple, and afterwards be- " came more bright and brifk, but yet not very pure. This was fucceeded with " a very bright and intenfe yellow, which was but little in quantity, and foon " changed to green; but that green was copious, and fomething more pure, " deep, and lively, than the former green. After that followed an excellent blue " of a bright fky colour; and then a purple, which was lefs in quantity than the " blue, and much inclined to red.

" The fixth red was at firft of a very fair and lively fcarlet, and foon after " of a brighter colour, being very pure and brifk, and the beft of all the reds. " Then, after a lively orange, fol owed an intenfe, bright, and copious yellow, " which was alfo the beft of all the yellows; and this changed, firft to a greenifh " yellow, and then to a greenifh blue; but the green between the yellow and " blue was very little and dilute, feeming rather a greenifh white than a green. " The blue, which fucceeded, became very good, and of a fair, bright, fky-colour; " but yet fomething inferior to the former blue: and the violet was intenfe and " deep, with little or no rednefs in it, and lefs in quantity than the blue.

" In the laft red appeared a tincture of fcarlet next the violet, which foon " changed to a brighter colour, inclining to an orange: and the yellow, which " followed, was at firft pretty good and lively, but afterwards it grew more and " more dilute, until by degrees it ended in perfect whitenefs: and this whitenefs, " if the water was very tenacious and well tempered, would flowly fpread and " dilate itfelf over the greateft part of the bubble, continually growing paler at " the top, where at length it would crack, and thofe cracks, as they dilated, " would appear of a pretty good, but yet obfcure and dark, fky-colour; the " white between the blue fpots diminifhing, until it refembled the threads of an " irregular net-work, and foon after vanifhed and left all the upper part of the " bubble of the faid dark blue colour; and this colour, after the aforefaid man- " ner, dilated itfelf downwards, until fometimes it hath overfpread the whole " bubble. In the mean while, at the top, which was of a darker blue than the " bottom, and appeared alfo of many round blue fpots, fomething darker than " the reft, there would emerge one or more very black fpots, and within thofe, " other fpots of an intenfer blacknefs, which I mentioned in the former obferva- " tion; and thofe continually dilated themfelves until the bubble broke.

" If the water was not very tenacious, the black fpots would break forth in " the white, without any fenfible intervention of the blue: and fometimes they " would break forth within the precedent yellow, or red, or perhaps within " the blue of the fecond order, before the intermediate colours had time to dif- " play themfelves.

" By

" By this defcription you may perceive, how great an affinity thefe colours " have with thofe of air, defcribed in the fourth obfervation, although fet down " in a contrary order, by reafon that they begin to appear, when the bubble is " thickeft, and are moft conveniently reckoned from the loweft and thickeft part " of the bubble upwards.

" Obf. 19. Viewing, at feveral oblique pofitions of my eye, the rings of " colours emerging on the top of the bubble, I found, that they were fenfibly " dilated by increafing the obliquity, but yet not fo much by far, as thofe made " by thinned air in the feventh obfervation. For there they diftended fo much, " as, when viewed moft obliquely, to arrive at a part of the plate more than " twelve lines thicker than that where they appeared, when viewed perpendicu- " larly; whereas in this cafe the thicknefs of the water, at which they arrived " when viewed moft obliquely, was, to that thicknefs, which exhibited them by " perpendicular rays, fomething lefs than eight to five. By the beft of my ob- " fervations, it was between fifteen and fifteen and a half to ten, an increafe " about twenty-four times lefs than in the other cafe.

" Sometimes the bubble would become of an uniform thicknefs all over, ex- " cept at the top of it near the black fpot, as I knew, becaufe it would exhibit " the fame appearance of colours in all pofitions of the eye; and then the co- " lours, which were feen at its apparent circumference by the obliqueft rays, " would be different from thofe, that were feen in other places by rays lefs " oblique to it. And divers fpectators might fee the fame part of it of differing " colours, by viewing it at very differing obliquities. Now, obferving how " much the colours at the fame place of the bubble, or at divers places of equal " thicknefs, were varied by the feveral obliquities of the rays, by affiftance of " the fourth, fourteenth, fixteenth, and eighteenth obfervations, as they are " hereafter explained, I collected the thicknefs of the water, requifite to exhibit " any one the fame colour at feveral obliquities, to be very nearly in the propor- " portion expreffed in this table.

Incidence on the water.		Refraction into the water.		Thicknefs of the water.
degr.	min.	degr.	min.	
00	00	00	00	10
15	00	11	11	$10\frac{1}{4}$
30	00	22	1	$10\frac{4}{5}$
45	00	32	2	$11\frac{4}{5}$
60	00	40	30	13
75	00	46	25	$14\frac{1}{2}$
90	00	48	35	$15\frac{1}{2}$

" In the two firft columns are expreffed the obliquities of the rays to the " fuperficies of the water; that is, their angles of incidence and refraction; " where, I fuppofe, that the lines, which meafure them, are in round numbers, " as three to four, though probably the diffolution of foap in the water may a " little alter its refractive virtue. In the third column the thicknefs of the bubble,

" at which any one colour is exhibited in those several obliquities, is exprest in " parts, of which ten constitute that thickness, when the rays are perpendicular.

" I have sometimes observed of the colours, which arise on polished steel by " heating it, or on bell metal and some other metalline substances, when melted " and poured on the ground, where it may cool in the open air, that they have, " like those of water-bubbles, been a little changed by viewing them at divers " obliquities; and particularly, that a deep blue or violet, when viewed very " obliquely, hath been changed to a deep red. But the changes of these colours " are not so sensible as of those made by water; for the scoria, or vitrified part " of the metal, which most metals, when heated or melted, continually protrude " to their surface, where, by covering them in form of a thin glassy skin, it " causes these colours, is much denser than water, and I find, that the change " made by the obliquation of the eye, is least in colours of the densest thin sub- " stances.

" Obs. 20. As in the ninth observation, so here, the bubble, by transmitted " light appeared of a contrary colour to that, which it exhibited by reflection. " Thus, when the bubbles, being looked on by the light of the clouds reflected " from it, seemed red at its apparent circumference, if the clouds at the same " time, or very suddenly, were viewed through it, the colour at its circumfe- " rence would be blue. And, on the contrary, when by reflected light it ap- " peared blue, it would appear red by transmitted light.

" Obs. 21. By wetting plates of Muscovy-glass, whose thinness made the like " colours appear, the colours became more faint, especially by wetting the plates " on that side opposite the eye; but I could not perceive any variation of their " species. So that the thickness of a plate requisite to produce any colour, de- " pends only on the density of the plate, and not of the ambient medium. " And hence, by the tenth and sixteenth observations, may be known the thick- " ness of bubbles of water or plates of Muscovy-glass, or of any other substan- " ces, which they have at any colour produced by them.

" Obs. 22. A thin transparent body, which is denser than its ambient me- " dium, exhibits more brisk and vivid colours than that, which is so much " rarer; as I have particularly observed in air and glass: for, blowing glass " very thin at a lamp furnace, those plates encompassed with air did exhibit co- " lours much more vivid than those of air made thin between two glasses.

" Obs. 23. Comparing the quantity of light reflected from the several rings, " I found it was most copious from the first or inmost, and in the exterior rings be- " came gradually less and less. Also the whiteness of the first ring was stronger than " that reflected from those parts of the thinned medium, which were without the " rings, as I could manifestly perceive by viewing at distance the rings made by " the two object glasses; or by comparing two bubbles of water blown at distant " times, in the first of which the whiteness appeared, which succeeded the colours, " and the whiteness, which preceded them, in the other.

" Obſ. 24. When the two object-glaſſes were laid upon one another, ſo as to
" make the rings of colours appear, though with my naked eye I could not
" diſcern above eight or nine of thoſe rings, yet, by viewing them through a
" priſm, I have ſeen a far greater multitude, inſomuch, that I could number
" more than forty, beſides many others, that were ſo very ſmall and cloſe toge-
" ther, that I could not keep my eye ſo ſteady on them ſeverally as to number
" them : but by their extent I have ſometimes eſtimated them to be more than
" a hundred. And, I believe, the experiment may be improved to the diſcovery
" of far greater numbers; for they ſeem to be really unlimited, though viſible
" only ſo far as they can be ſeparated by the refraction, as I ſhall hereafter
" explain.

" But it was but one ſide of theſe rings, namely, that, towards which the re-
" fraction was made, which by that refraction was rendered diſtinct; and the
" other ſide became more confuſed than to the naked eye, inſomuch that there I
" could not diſcern above one or two, and ſometimes none of thoſe rings, of
" which I could diſcern eight or nine with my naked eye. And their ſegments,
" or arcs, which on the other ſide appeared ſo numerous, for the moſt part ex-
" ceeded not the third part of a circle. If the refraction was very great, or the
" priſms very diſtant from the object-glaſſes, the middle part of thoſe arcs be-
" came alſo confuſed, ſo as to diſappear and conſtitute an even whiteneſs, whilſt
" on either ſide their ends, as alſo the whole arcs fartheſt
" from the center, became diſtincter than before, appearing
" in the form you ſee them here deſigned.

Fig. II.

" The arcs, where they ſeemed diſtincteſt, were only white
" and black ſucceſſively, without any other colours in-
" termixed. But in other places there appeared colours
" whoſe order was inverted by the refraction, in ſuch man-
" ner, that, if I firſt held the priſm very near the object-
" glaſſes, and then gradually removed it farther off towards
" my eye, the colours of the ſecond, third, fourth, and following rings ſhrunk
" towards the white, that emerged between them, until they wholly vaniſhed into
" it at the middle of the arcs, and afterwards emerged again in a contrary or-
" der : but at the end of the arcs they retained their order unchanged.

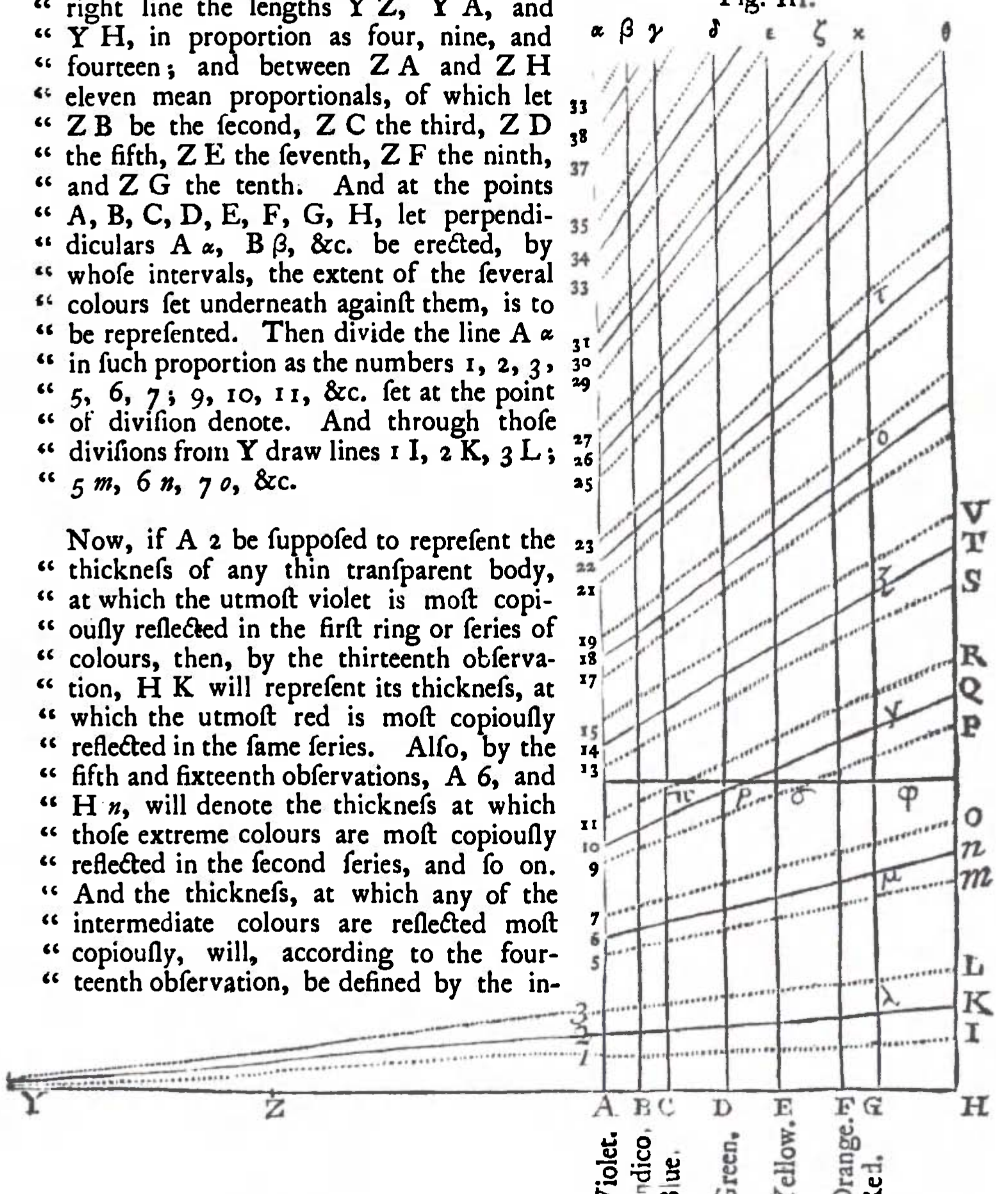
" I have ſometimes ſo laid one object-glaſs upon the other, that, to the naked
" eye, they have all over ſeemed uniformly white, without the leaſt appearance
" of any of the coloured rings; and yet, by viewing them through a priſm, great
" multitudes of thoſe rings have diſcovered themſelves. And, in like manner,
" plates of Muſcovy glaſs, and bubbles of glaſs blown at a lamp furnace,
" which were not ſo thin, as to exhibit any colours to the naked eye, have
" through the priſm exhibited a great variety of them, ranged irregularly up
" and down, in the form of waves. And ſo bubbles of water, before they be-
" gan to exhibit their colours to the naked eye of a by-ſtander, have appeared,
" through a priſm, girded about with many parallel and horizontal rings; to pro-
" duce which effect, it was neceſſary to hold the priſm parallel, or very nearly paral-
" lel, to the horizon, and to diſpoſe it ſo, that the rays might be refracted upwards.
" Having

" Having given my obfervations of thefe colours, before I make ufe of them " to unfold the caufes of the colours of natural bodies, it is convenient, that, by " the fimpleft of them, I firft explain the more compounded; fuch as are the " fecond, third, fourth, ninth, twelfth, eighteenth, twentieth, and twenty-fourth.

" And firft, to fhow how the colours in the fourth and eighteenth obfervations " are produced, let there be taken in any " right line the lengths Y Z, Y A, and " Y H, in proportion as four, nine, and " fourteen; and between Z A and Z H " eleven mean proportionals, of which let " Z B be the fecond, Z C the third, Z D " the fifth, Z E the feventh, Z F the ninth, " and Z G the tenth. And at the points " A, B, C, D, E, F, G, H, let perpendi- " diculars A α, B β, &c. be erected, by " whofe intervals, the extent of the feveral " colours fet underneath againft them, is to " be reprefented. Then divide the line A α " in fuch proportion as the numbers 1, 2, 3, " 5, 6, 7; 9, 10, 11, &c. fet at the point " of divifion denote. And through thofe " divifions from Y draw lines 1 I, 2 K, 3 L; " 5 *m*, 6 *n*, 7 *o*, &c.

Now, if A 2 be fuppofed to reprefent the " thicknefs of any thin tranfparent body, " at which the utmoft violet is moft copi- " oufly reflected in the firft ring or feries of " colours, then, by the thirteenth obferva- " tion, H K will reprefent its thicknefs, at " which the utmoft red is moft copioufly " reflected in the fame feries. Alfo, by the " fifth and fixteenth obfervations, A 6, and " H *n*, will denote the thicknefs at which " thofe extreme colours are moft copioufly " reflected in the fecond feries, and fo on. " And the thicknefs, at which any of the " intermediate colours are reflected moft " copioufly, will, according to the four- " teenth obfervation, be defined by the in-

Fig. III.



1

" termediate

" termediate parts of the lines 2 K, 6 *n*, &c. againſt which the names of thoſe
" colours are written below.

" But farther, to define the latitude of theſe colours in each ring or ſeries, let
" A 1 deſign the leaſt thickneſs, and A 3 the greateſt thickneſs, at which the
" extreme violet in the firſt ſeries is reflected; and let H I and H L deſign the
" like limit for the extreme red, and the intermediate colours be limited by the
" intermediate parts of the lines, 1 I and 3 L; againſt which the names of thoſe
" colours are written. And in the ſecond ſeries, let thoſe limits be the lines
" 5 M and 7 O; and ſo on: but yet with this caution, that the reflections be
" ſuppoſed ſtrongeſt at the intermediate ſpaces, 2 K, 6 N, 10 R, &c. and to
" decreaſe gradually towards theſe limits, 1 I, 3 L; 5 M, 7 O, &c. on either
" ſide, where you muſt not conceive them to be preciſely limited, but to decay
" indefinitely. And whereas I have deſigned the ſame latitude to every ſeries, I
" did it, becauſe, although the colours in the firſt ſeries ſeem to be a little broader
" than the reſt, by reaſon of a ſtronger reflection there; yet that inequality is ſo
" inſenſible as ſcarcely to be determined by obſervation.

" Now, according to this deſcription, conceiving, that the rays, in which ſeve-
" ral colours in here, are by turns reflected at the ſpace 1 K, 3 L, 5 M, O 7,
" 9 P, R 11, &c. and tranſmitted at the ſpaces A H I 1, 3 L, M 5, 7 O,
" P 9, &c. it is eaſy to know what colour in the open air muſt be exhibited
" at any thickneſs of a tranſparent thin body. For, if a ruler be applied paral-
" lel to A H, at that diſtance from it by which the thickneſs of the body is
" repreſented, the alternate ſpaces 1 I, L 3, 5 M, O 7, &c. which it croſſeth,
" will denote the reflected original colours, of which the colour exhibited in the
" open air is compounded. Thus, if the conſtitution of the green in the third
" ſeries of colours be deſired; apply the ruler, as you ſee, at $\pi \rho \sigma \varphi$, and by its
" paſſing through ſome of the blue at π, and yellow at σ, as well as through the
" green ρ, you may conclude, that green, exhibited at that thickneſs of the
" body, is principally conſtituted of original green, but not without a mixture
" of ſome blue and yellow. By this means you may know, how the colours
" from the center of the rings outwards ought to ſucceed in order, as they were
" deſcribed in the fourth and eighteenth obſervations: for, if you move the ruler
" gradually from A H through all diſtances, having paſt over the firſt ſpace,
" which denotes little or no reflection to be made by thinneſt ſubſtances, it will firſt
" arrive at 1, the violet, and then very quickly at the blue and green, which, to-
" gether with that violet compounded blue, and then at the yellow and red, by
" whoſe further addition, that blue is converted into whiteneſs, which white-
" neſs continues during the tranſit from I to 3; and after that, by the ſucceſſive
" deficience of its component colours, turns firſt to compound yellow, and then
" to red, and laſt of all the red ceaſeth at L. Then begin the colours of the ſecond
" ſeries, which ſucceed in order between 5 and O, and are more lively than be-
" fore, becauſe more expanded and ſevered. And, for the ſame reaſon, inſtead of
" the former white, there intercedes between the blue and yellow a mixture of
" orange, yellow, green, blue and indico, all which together ought to exhibit
" a dilute an imperfect green. So the colours of the third ſeries all ſucceed in
" order

" order; firſt the violet, which a little interferes with the red of the ſecond or- " der, and is thereby inclined to a rediſh purple; then the blue and green, which " are leſs mixed with other colours, and conſequently more lively than before, " eſpecially the green. Then follows the yellow, ſome of which towards the " green is diſtinct and good; but that part of it towards the ſucceeding red, as " alſo that red, is mixed with the violet and blue of the fourth ſeries, whereby va- " rious degrees of red, very much inclining to purple, are compounded. The " violet and blue, which ſhould ſucceed this red, being mixed with, and hidden " in it, there ſucceeds a green; and this at firſt is much inclined to blue, but " ſoon becomes a good green; the only unmixed and lively colour in this fourth " ſeries: for as it verges towards the yellow, it begins to interfere with the " colours of the fifth ſeries, by whoſe mixture the ſucceeding yellow and red are " very much diluted, and made dirty, eſpecially the yellow, which being the " weaker colour, is ſcarce able to ſhew itſelf. After this the ſeveral ſeries inter- " fere more and more, and their colours become more and more intermixed, till " after three or four revolutions (in which the red and blue predominate by " turns) all ſorts of colours are in all places pretty equally blended, and com- " pound one even whiteneſs.

" And ſince, by the fifteenth obſervation, the rays indued with one colour are " tranſmitted, where thoſe of another colour are reflected, the reaſon of the co- " lours made by the tranſmitted light, in the ninth and twentieth obſervations, is " alſo from hence evident.

" If not only the order and ſpecies of theſe colours, but alſo the preciſe thick- " neſs of the plate, or thin body, at which they are exhibited, be deſired in parts " of an inch, that may be alſo performed by aſſiſtance of the ſixth or ſixteenth " obſervation. For, according to thoſe obſervations, the thickneſs of the thinned " air, which, between two glaſſes, exhibited the orange or bright red of the " ſixth order, was $\frac{1}{14354}$ parts of an inch. Now, ſuppoſe this thickneſs be " repreſented by G τ, and the eleventh part of it, G λ, will be about $\frac{1}{160000}$ of " an inch. And ſo G μ, G V, G ξ, G o, will be $\frac{3}{160000}$, $\frac{5}{160000}$, $\frac{7}{160000}$, " and $\frac{9}{160000}$. And this being known, it is eaſy to determine what thickneſs " of air is repreſented by G φ, or any other diſtance of the ruler from A H.

" But further, ſince, by the tenth obſervation, the thickneſs of air was to the " thickneſs of water, which between the ſame glaſſes exhibited the ſame colour, " as four to three; and, by the twenty-firſt obſervation, the colours of thin " bodies are not varied by varying the ambient medium; the thickneſs of a " bubble of water exhibiting any colour will be three fourths of the thickneſs of " air producing the ſame colour. And ſo, according to the ſame tenth and twenty- " firſt obſervations, the thickneſs of a plate of glaſs, whoſe refraction is meaſured " by the proportion of the ſines thirty-one to twenty, may be $\frac{20}{31}$ of the thickneſs " of air producing the ſame colours: and the like of other mediums. On theſe " grounds I have compoſed the following table; wherein the thickneſs of air, " water, and glaſs, at which each colour is moſt intenſe and ſpecific, is expreſſed " in parts of an inch divided into ten hundred thouſand equal parts.

" The

		The thickness of Air	The thickness of Water	The thickness of Glass
The colours of the first order	Black	2	$1\frac{1}{2}$	$1\frac{1}{4}$ or less.
	Blue	$2\frac{2}{5}$	2	$1\frac{3}{4}$
	White	$5\frac{1}{5}$	4	$3\frac{1}{2}$
	Yellow	8	6	$5\frac{1}{4}$
	Orange	9	$6\frac{3}{4}$	$5\frac{4}{5}$
	Red	10	$7\frac{1}{2}$	$6\frac{1}{2}$
Of the second order	Violet	12	9	$7\frac{3}{4}$
	Indico	$13\frac{1}{4}$	$9\frac{11}{12}$	$8\frac{1}{2}$
	Blue	$14\frac{3}{4}$	11	$9\frac{1}{2}$
	Green	16	12	$10\frac{1}{3}$
	Yellow	$17\frac{1}{2}$	$13\frac{1}{8}$	$11\frac{1}{3}$
	Orange	$19\frac{1}{4}$	$14\frac{1}{2}$	$12\frac{2}{5}$
	Bright red	20	15	13
	Scarlet	$21\frac{1}{4}$	16	$13\frac{2}{3}$
Of the third order	Purple	23	$17^{\frac{1}{4}}$	$14\frac{4}{5}$
	Indico	24	18	$15\frac{1}{2}$
	Blue	$25\frac{1}{5}$	19	$16\frac{1}{4}$
	Green	$27\frac{1}{5}$	$20\frac{2}{5}$	$17\frac{1}{2}$
	Yellow	$29\frac{1}{4}$	22	19
	Red	31	$23\frac{1}{4}$	20
	Bluish red	$33\frac{1}{2}$	25	$21\frac{2}{5}$
Fourth order	Bluish	36	27	$23\frac{1}{4}$
	Green	$37\frac{2}{3}$	$28\frac{1}{4}$	$24\frac{1}{3}$
	Yellowish green	$39\frac{1}{3}$	$29\frac{1}{2}$	$25\frac{1}{3}$
	Red	44	33	$28\frac{1}{3}$
Fifth order	Greenish blue	$50\frac{2}{3}$	38	$32\frac{2}{3}$
	Red	$57\frac{1}{3}$	43	37
Sixth order	Greenish blue	64	48	$41\frac{1}{3}$
	Red	$70\frac{2}{3}$	53	$45\frac{2}{3}$
Seventh order	Greenish blue	$77\frac{1}{3}$	58	50
	Red or White	84	63	$54\frac{1}{2}$

" Now, if this table be compared with the third scheme, you will there see " the constitution of each colour, as to its ingredients, or the original colours, " of which it is compounded, and thence be enabled to judge of its intenseness " or imperfection, which may suffice in explication of the fourth and eighteenth

" observations, unless it be further desired to delineate the manner, how the
" colours appear, when the two object-glasses are laid upon one another: to do
" which let there be described a large arc of a circle and a strait line, which
" may touch that arc; and parallel to that tangent several occult lines at such
" distances from it, as the numbers set against the several colours in the table
" denote. For the arc and its tangent will represent the superficies of the
" glasses, terminating the interjacent air, and the places, where the occult lines
" cut the arc, will show at what distances from the center, or point of the con-
" tact, each colour is reflected.

" There are also other uses for this table; for by its assistance the thickness
" of the bubble, in the nineteenth observation, was determined by the colours,
" which it exhibited. And so the bigness of the parts of natural bodies may be
" conjectured at by their colours, as shall be hereafter shown. Also, if two
" or more very thin plates be laid one upon another, so as to compose one plate,
" equalling them all in thickness, the resulting colour may be hereby determined.
" For instance, Mr. Hooke, in his Micrographia, observes, that a faint yellow
" plate of Muscovy glass, laid upon a blue one, constituted a very deep purple.
" The yellow of the first order is a faint one, and the thickness of the plate ex-
" hibiting it, according to the table, is $5\frac{1}{4}$, to which add $9\frac{1}{2}$, the thickness ex-
" hibiting blue of the second order, and the sum will be $14\frac{3}{4}$, which most
" nearly approaches $14\frac{4}{5}$, the thickness exhibiting the purple of the third
" order.

" To explain, in the next place, the circumstances of the second and third
" observations, that is, how the colours (by turning the prisms about their com-
" mon axis the contrary way to that expressed in those observations) may be con-
" verted into white and black rings, and afterwards into colours again in an
" inverted order; it must be remembered, that those colours are dilated by obli-
" quation of rays to the air, which intercedes the glasses; and that, according
" to the table in the seventh observation, their dilatation or reflection from the
" common center is most manifest and speedy when they are obliquest. Now,
" the rays of yellow being more refracted by the first superficies of the said air
" than those of red, are thereby made more oblique to the second superficies,
" at which they are reflected, to produce the coloured rings; and consequently,
" the yellow in each ring will be more dilated than the red; and the excess of
" its dilatation will be so much the greater, by how much the greater is the obli-
" quity of the rays, until at last it become of equal extent with the red of the
" same ring. And, for the same reason, the green, blue, and violet, will be
" also so much dilated by the still greater obliquity of their rays, as to become
" all very nearly of equal extent with the red; that is, equally distant from the
" center of the rings. And then all the colours of the same series must be coinci-
" dent, and by their mixture exhibit a white ring; and these white rings must
" have black or dark rings between them, because they do not spread and inter-
" fere with one another as before; and, for that reason also, they must become
" distincter, and visible to far greater numbers. But yet the violet, being
" obliquest,

" obliqueſt, will be ſomething more dilated in proportion than the other colours; " and ſo very apt to appear at the exterior verges of the white.

" Afterwards, by a greater obliquity of the rays, the violet and the blue become ſenſibly more dilated than the red and yellow; and ſo being further " removed from the center of the rings, the colours muſt emerge out of the white " in an order contrary to that which they had before, the violet and blue at the " exterior limbs, and the red and yellow at the interior. And the violet, by " reaſon of the greateſt obliquity of its rays, being, in proportion, moſt of all " expanded, will ſooneſt appear at the exterior limb of each white ring, and " become more conſpicuous than the reſt. And the ſeveral ſeries of colours, by " their unfolding and ſpreading, will begin again to interfere, and thereby render " the rings leſs diſtinct, and not viſible to ſo great numbers.

" If, inſtead of the priſms, the object-glaſſes be made uſe of, the rings, which " they exhibit, become not white and diſtinct by the obliquity of the eye, by " reaſon, that the rays, in their paſſage through that air, which interceded the " glaſſes, are very nearly parallel to themſelves, when firſt incident on the glaſſes; " and conſequently, thoſe indued with ſeveral colours are not inclined one more " than another to that air, as it happens in the priſms.

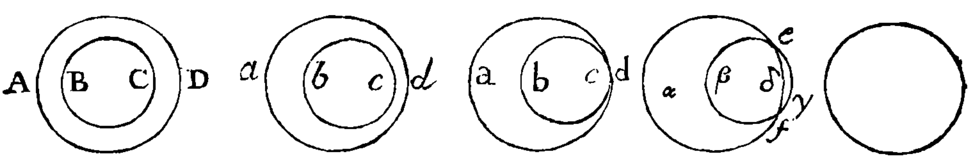
" There is yet another circumſtance of theſe experiments to be conſidered; " and that is, why the black and white rings, which, when viewed at a diſtance, " appear diſtinct, ſhould not only become confuſed by viewing them near at " hand, but alſo yield a violet colour at both the edges of every white ring: " and the reaſon is, that the rays, which enter the eye at ſeveral parts of the " pupil, have ſeveral obliquities to the glaſſes, and thoſe, which are moſt oblique, " if conſidered apart, would repreſent the rings bigger than thoſe, which are the " leaſt oblique. Whence the breadth of the perimeter of every white ring is expanded outwards by the obliqueſt rays, and inwards by the leaſt oblique. And " this expanſion is ſo much the greater, by how much the greater is the difference " of the obliquity; that is, by how much the pupil is wider, or the eye nearer " to the glaſſes: and the breadth of the violet muſt be moſt expanded, becauſe " the rays, apt to excite a ſenſation of that colour, are moſt oblique to the " ſecond or further ſuperficies of the thinned air, at which they are reflected; " and have alſo the greateſt variation of obliquity, which makes that colour " ſooneſt emerge out of the edges of the white. And, as the breadth of every " ring is thus augmented, the dark intervals muſt be diminiſhed, until the neighbouring rings become continuous, and are blended, the exterior firſt, and " then thoſe nearer the center; ſo that they can no longer be diſtinguiſhed a-part, " but ſeem to conſtitute an even and uniform whiteneſs.

" Amongſt all the obſervations there is none accompanied with ſo odd circumſtances as the twenty-fourth. Of thoſe the principal are, that in thin plates, " which, to the naked eye, ſeem of an even and uniform tranſparent whiteneſs, " the refraction of a priſm ſhould make the rings of colours appear; whereas it " uſually makes objects to appear coloured only, where they are terminated with

 " ſhadows,

" ſhadows, or have parts unequally luminous; and that it ſhould make thoſe " rings exceedingly diſtinct and white, although it uſually renders thoſe objects " confuſed and coloured. The cauſe of theſe things you will underſtand by " conſidering, that all the rings of colours are really in the plate, when viewed " by the naked eye, although, by reaſon of the great breadth of their circum- " ferences, they ſo much interfere, and are blended together, that they ſeem to " conſtitute an even whiteneſs. But, when the rays paſs through the priſm to " the eye, the orbits of the ſeveral colours in every ring are refracted, ſome more " than others, according to their degree of refrangibility; by which means the " colours on one ſide of the ring become more unfolded and dilated, and on the " other ſide more complicated and contracted. And where, by a due refrac- " tion, they are ſo much contracted, that the ſeveral rings become narrower " than to interfere with one another, they muſt appear diſtinct, and alſo white, " if the conſtituent colours be ſo much contracted as to be wholly coinci- " dent: but on the other ſide, where every ring is made broader by the further " unfolding its colours, it muſt interfere more with other rings than before, and " ſo become leſs diſtinct.

" To explain this a little further; ſuppoſe the concentric circles, A B and " C D, repreſent the red and violet of any order, which, together with the in " termediate colours, conſtitute any one of theſe rings. Now, theſe being " viewed through a priſm, the violet circle, B C, will, by a greater refraction, be " further tranſlated from its place than the red, A D, and ſo approach nearer



" to it on that ſide towards which the refractions are made. For inſtance, if " the red be tranſlated to *a d*, the violet may be tranſlated to *b c*, ſo as to ap- " proach nearer to it at *c* than before; and, if the red be further tranſlated to " a d, the violet may be ſo much further tranſlated to b c, as to convene with " it at c, and, if the red be yet further tranſlated to $\alpha\,\delta$, the violet may be ſtill " ſo much further tranſlated to $\beta\,\gamma$, as to paſs beyond it at γ, and convene with it " at *e* and *f*. And this being underſtood, not only of the red and violet, but of " all the other intermediate colours; and alſo of every revolution of thoſe co- " lours, you will eaſily perceive, how theſe of the ſame revolution or order, by " their narrowneſs at *c d*, and $\delta\,\gamma$, and their coincidence at *c d*, *e* and *f*, ought " to conſtitute pretty diſtinct arcs of circles, eſpecially at *c d*, or at *e* and *f*, and " that they will appear ſeveral at *c d*, at c d exhibit whiteneſs by their coinci- " dence, and again appear ſeveral at $\delta\,\gamma$, but yet in a contrary order to that " which they had before, and ſtill retain beyond *e* and *f*. But, on the other " ſide, at *a b*, a b, or $\alpha\,\beta$, theſe colours muſt become much more confuſed by " being dilated, and ſpread ſo as to interfere with thoſe of other orders. And " the ſame confuſion will happen at $\delta\,\gamma$ between *e* and *f*, if the refraction be

" very

" very great, or the prifm very diftant from the object-glaffes; in which cafe no " parts of the ring will be feen, fave only two little arcs at *e* and *f*, whofe diftance " from one another will be augmented by removing the prifm ftill further from " the object-glaffes. And thefe little arcs muft be diftincteft and whiteft at their " middle; and at their ends, where they grow confufed, they muft be coloured; " and the colours at one end of every arc muft be in a contrary order to thofe " at the other end, by reafon that they crofs in the intermediate white; namely, " their ends, which verge towards $\delta\ \gamma$, will be red, and yellow on that fide next " the center, and blue and violet on the other fide. But their other ends, which " verge from $\delta\ \gamma$, will, on the contrary, be blue and violet on that fide towards " the center, and on the other fide red and yellow.

" For confirmation of all this, I need alledge no more, than that it is mathe- " matically demonftrable from my former principles. But I fhall add, that they, " which pleafe to take the pains, may by the teftimony of their fenfes be affured, " that thefe explications are not hypothetical, but infallibly true and genuine: " for in a dark room, by viewing thefe rings through a prifm, by reflection of " the feveral prifmatic colours, which an affiftant caufes to move to and fro " upon a wall or paper, from whence they are reflected, whilft the fpectator's " eye, the prifm, and object-glaffes (as in the thirteenth obfervation) are placed " fteddy, the pofition of the circles, made fucceffively by the feveral colours, " will be found fuch, in refpect of one another, as I have defcribed at *a b c d*, or " a b c d, or $\alpha\ \beta\ \gamma\ \delta$. And by the fame method the truth of the explications of " the other obfervations is to be examined.

" By what hath been faid, the like phænomena of water-bubbles and thin " plates of glafs may be underftood. But in fmall fragments of thofe plates, " there is this further obfervable, that, if they, lying flat upon a table, be turned " about their center, whilft they are viewed through a prifm, fome of them ex- " hibit waves in one or two pofitions only; but the moft of them do in all pofi- " tions exhibit thofe waves, and that for the moft part appearing almoft all over " the glafs. The reafon is, that the fuperficies of fuch plates are not even, but " have many cavities and fwellings, which, how fhallow foever, do a little vary " the thicknefs of the plate; and by the feveral fides of thofe cavities there " muft be produced waves in feveral poftures of the prifm. Now, though it " be but fome very fmall and narrow parts of the glafs, by which thefe waves " for the moft part are caufed, yet they may feem to extend themfelves over the " whole glafs, becaufe from the narroweft of thofe parts there are colours of feveral " orders confufedly reflected, which by refraction of the prifm are unfolded, and " difperfed to feveral places, fo as to conftitute fo many feveral waves as there " were divers orders of the colours promifcuoufly reflected from that part of the " glafs.

" Thefe are the principal phænomena of thin plates or bubbles, whofe expli- " cations depend on the properties of light, that I have heretofore delivered: " and thefe, you fee, do neceffarily follow from them, and agree with them even " to their very leaft circumftances; and not only fo, but do very much tend to

" their

" their proof. Thus, by the twenty-fourth obfervation, it appears, that the " rays of feveral colours, made, as well by thin plates or bubbles, as by the re- " fractions of a prifm, have feveral degrees of refrangibility, whereby thofe of " each order, which, at their reflection from the plate or bubble, are intermixed " with thofe of other orders, are feparated from them by refraction, and affoci- " ated together, fo as to become vifible by themfelves, like arcs of circles. For, " if the rays were all alike refrangible, it is impoffible, that the whitenefs, which " to the naked fenfe appears uniform, fhould by refraction have its parts tranf- " pofed, and ranged into thofe black and white arcs.

" It appears alfo, that the unequal refractions of difform rays proceed not " from any contingent irregularities, fuch as are veins, an uneven polifh, or for- " tuitous pofition of the pores of glafs, unequal motions in the air or æther, " fpreading, breaking, or dividing the fame ray into many diverging parts, or " the like. For, admitting any fuch irregularities, it would be impoffible for " refractions to render thofe rings fo very diftinct and well defined, as they do " in the twenty-fourth obfervation. It is neceffary therefore, that every ray have " its proper and conftant degree of refrangibility connate with it; according to " which its refraction is ever juftly and regularly performed, and that feveral " rays have feveral of thofe degrees.

" And what is faid of their refrangibility may be underftood of their reflexi- " bility; that is, of their difpofitions to be reflected, fome at a greater, and others " at a lefs thicknefs of thin plates or bubbles, namely, that thofe difpofitions are " alfo connate with the rays, and immutable, as may appear by the thirteenth, " fourteenth, and fifteenth obfervations, compared with the fourth and eigh- " teenth.

" By the precedent obfervations it appears alfo, that whitenefs is a diffimilar " mixture of all colours, and that light is a mixture of rays endowed with all " thofe colours. For, confidering the multitude of the rings of colours in the " third, twelfth, and twenty-fourth obfervations, it is manifeft, that, although " in the fourth and eighteenth obfervations there appear more than eight or nine " of thofe rings, yet there are really a far greater number, which fo much inter- " fere and mingle with one another, as, after thofe eight or nine revolutions, to " dilute one another wholly, and conftitute an even and fenfible uniform white- " nefs. And confequently, that whitenefs muft be allowed a mixture of all co- " lours, and the light, which conveys it to the eye, muft be a mixture of rays " endued with all thofe colours.

" But further, by the twenty-fourth obfervation it appears, that there is a con- " ftant relation between colours and refrangibility, the moft refrangible rays being " violet, the leaft refrangible red, and thofe of intermediate colours having pro- " portionally intermediate degrees of refrangibility. And, by the thirteenth, " fourteenth, and fifteenth obfervations, compared with the fourth or eighteenth, " there appears to be the fame conftant relation between colour and refrangi- " bility; the violet being on equal terms reflected at leaft thicknefs of any thin

" plate

7

" plate or bubble; the red at greateſt thickneſs, and the intermediate colours at
" intermediate thickneſſes: whence it} follows, that the colorific diſpoſitions of
" rays are alſo connate with them, and immutable; and by conſequence, that all
" the productions and appearances of colours in the world are derived, not from
" any phyſical change cauſed in light by refraction or reflection, but only from
" the various mixtures or ſeparations of rays, by virtue of their different refran-
" gibility or reflexibility. And, in this reſpect it is, that the ſcience of colours
" becomes a ſpeculation more proper for mathematicians than naturaliſts.

This being read, occaſion was taken to diſcourſe of Mr. NEWTON's theory itſelf, and to debate, whether the rays of light, which, though alike incident in the ſame medium, yet exhibit different colours, may not reaſonably be ſaid to owe that exhibition of different colours to the ſeveral degrees of the velocity of pulſes, rather than, as Mr. NEWTON thought, to the ſeveral connate degrees of refrangibility in the rays themſelves?

Mr. HOOKE was of opinion, that the former of theſe ways was ſufficient to give a good account of the diverſity of colours.

February 10. Dr. MAPLETOFT was elected and admitted.

Capt. SHEERES, Mr. HALL, and Signor TRAVAGINO were elected.

Mr. BERCHENSHAW preſented himſelf to the Society, and ſhewed them his ſcale of muſic, wherein were contained,

1. A table of all conſonant and diſſonant intervals ſuitable to muſical harmony, which are practicable, and may be expreſſed by the voice and other inſtruments. To theſe reſpective intervals apt and proper numbers were aſſigned, by which their ratio's and proportions were demonſtrated.

2. A ſyſtem of all the keys, by which the aforeſaid intervals were completed; of which keys ſome were natural; ſome intended to the firſt degree of acuteneſs; ſome remitted to the firſt degree of gravity; ſome twice ſpiſſated; ſome twice aſperated.

3. In this ſcale the magnitude, dimenſion, and proportion of the ſaid keys were exactly demonſtrated according to the proportional parts of a chord, the chord being ſuppoſed thirty-ſix inches long.

If it were demanded, whether there was any thing in this table and ſyſtem, that was not to be found in the ſcales and writings of other muſicians? he anſwered,

1. That the intervals in this table were perfect and complete. There was not one too many, nor one wanting, which might conduce to the making of harmony.

2. That

2. That the ſounds or muſical numbers contained in this ſyſtem aroſe out of the uniſon, and from one another, according to the reaſon of figurate, not ſimple numbers, (as, he ſaid, he could demonſtrate by numbers aſſigned to the reſpective intervals in the table) for that ſo the reaſon of the ſtate of muſic required.

3. That there are neither more or leſs keys in this ſyſtem, than would complete the aforeſaid intervals.

4. That in this ſcale all the tones are of the ſame ratio, and that ſo are all the ſemitones, ſemiditones, ditones, and other intervals.

5. That the true magnitude and dimenſion of every one of the ſaid keys are demonſtrated according to the proportional parts of a chord.

6. That the natural, genuine, and true reaſon of the excellency and fullneſs of the harmony of three, four, five, fix, and ſeven parts, may clearly be diſcerned by the ſyſtem of ſeven parts.

He added, that many other things were to be found in this table and ſcale, of which little or no mention is made in the ſcales and writings of either modern or antient muſical authors; which, he ſaid, he intended to diſcover, and to write of them at large, as he ſhould be enabled thereunto.

He was exhorted to finiſh this work, or at leaſt to publiſh this ſyſtem with an explanation thereof.

After this was read the laſt part of Mr. Newton's *obſervations*, wherein he conſidered in nine propoſitions, how the phænomena of thin tranſparent plates ſtand related to thoſe of all other natural bodies: of which bodies having before mentioned, that they appear of divers colours, according as they are diſpoſed to reflect moſt copiouſly the rays indued with theſe colours, he now inquires after their conſtitutions.

Here, among many other conſiderable things, he ſhews, how the bigneſs of the component parts of natural bodies may be conjectured by their colours: as alſo, that the cauſe of reflexion is not the impinging of light on the ſolid and impervious parts of bodies, as was commonly ſuppoſed.

This laſt part was as follows:

" I am now come to the laſt part of this deſign; which is, to conſider, how
" the phænomena of thin tranſparent plates ſtand related to thoſe of all other na-
" tural bodies. Of theſe bodies I have already told you, that they appear of di-
" vers colours, accordingly as they are diſpoſed to reflect moſt copiouſly the rays
" endued with thoſe colours. But their conſtitutions, whereby they reflect ſome rays

" more copiously than others, remains to be inquired after. And this I shall en-" deavour in the following propositions.

" Prop. 1. Those superficies reflect the greatest quantity of light, which have " the greatest refracting power; that is, which interceeds mediums, that differ most " in their refracting densities; and in the confines of equally dense mediums there " is no reflection.

" The analogy between reflection and refraction will appear by considering, that " when light passeth obliquely out of one medium into another, which refracts " from the perpendicular, the greater is the difference of their density, the less " obliquity is requisite to cause a total reflection; because as the sines are, which " measure the refraction, so is the sine of incidence, at which the total reflection " begins, to the radius of the circle; and consequently that incidence is least, " where there is the great difference of the sines. Thus in the passing of light out " of water into air, where the refraction is measured by the ratio of the sines, 3 to " 4, the total reflection begins, when the angle of the incidence is about forty-" eight degrees and thirty-five minutes. In passing out of glass into air, where " the refraction is measured by the ratio of the sines 20 to 31, the total reflection " begins, when the angle of incidence is forty degrees and ten minutes: and so, " in passing out of crystal, or more strongly refracting mediums, into air, there " is still a less obliquity requisite to cause a total reflection. Superficies therefore, " which refract most, do soonest reflect all the light, which is incident on them, " and so must be allowed most strongly reflective.

" But the truth of this proposition will further appear, by observing, that in " the superficies, interceeding any two of those mediums, air or water, or other " liquors, common glass, crystal, and metalline glasses, the reflection is stronger " or weaker accordingly as the superficies hath a greater or less refracting power. " Thus, when other mediums are contiguous to air, the reflection is stronger " in the superficies of glass than of water, still stronger in the superficies of crys-" tal, and strongest in the superficies of metalline glass. So, in the confine of " water and common glass, the reflection is very weak, but yet stronger than in " the confine of water and oil, or almost any other two liquors, and still stronger " in the confine of water and crystal, or metalline glass: accordingly as those " mediums differ more or less in density, so in the confine of common glass and " crystal there is a weak reflection, and a stronger reflection in the confine of " common and metalline glass: but in the confine of two glasses of equal den-" sity, there is not any sensible reflection, as was shewn in the first observation. " And the same may be understood of the superficies of two crystals or liquors, " or any other substances, in which no refraction is caused: whence it comes to " pass, that uniform mediums have no sensible reflexion but in their external su-" perficies, where they are adjacent to their mediums of a different density.

" Prop. 2. The least parts of natural bodies are in some measure transparent; " and the opacities of those bodies arise from the multitude of reflections caused " in their internal parts.

“ That this is ſo, will eaſily be granted by them, that have been converſant with
“ microſcopes: and it may be alſo tried by applying any ſubſtance to a hole, through
“ which the light is emitted into a dark room; for how opake ſoever that ſub-
“ ſtance may ſeem in the open air, it will, by that means, appear very manifeſtly
“ tranſparent, if it be of a ſufficient thickneſs: only metalline bodies muſt be ex-
“ empted, which, by reaſon of their exceſſive denſity ſeem to reflect almoſt all the
“ light incident on their firſt ſuperficies.

“ Prop. 3. Between the parts of opake or coloured bodies are many interſtices,
“ repleniſhed with mediums of other denſities, as water between the tinging cor-
“ puſcles, wherewith any liquor is impregnated; air between the aqueous globules
“ that conſtitute clouds or miſts; and for the moſt part ſpaces void of both air
“ and water; but yet perhaps repleniſhed with ſome ſubtiler medium between
“ the parts of hard bodies.

“ The truth of this is evinced by the two precedent propoſitions: for by the
“ ſecond propoſition there are many reflections from the internal part of bodies,
“ which by the firſt propoſition would not happen, if the parts of thoſe bodies
“ were continued without any ſuch interſtices between them, becauſe reflections
“ are cauſed only in ſuperficies, which interceed mediums of a different denſity.

“ But further, that this diſcontinuity of parts is the principal cauſe of the opa-
“ city of bodies, will appear by conſidering, that opake ſubſtances become tranſ-
“ parent by filling their pores with any ſubſtance of equal, or almoſt equal denſity
“ with their parts. Thus paper dipped in water or oil, the oculus mundi ſtone
“ ſteeped in water, linen-cloth oiled or varniſhed, and many other ſubſtances ſoaked
“ in ſuch liquors, as will intimately pervade their little pores, become by that
“ means more tranſparent than otherwiſe. So, on the contrary, the moſt tranſ-
“ parent ſubſtances may, by ſeparating their parts, be rendered ſufficiently opake;
“ as glaſs, by being reduced to powder, or otherwiſe flawed, water by being form-
“ ed into many ſmall bubbles, either alone in the form of froth, or by ſhaking
“ it together with oil of turpentine, or ſome other convenient liquor, with which
“ it will not incorporate, and horn by being ſcraped.

“ To the increaſe of the opacity of theſe bodies it conduces ſomething, that by
“ the twenty third obſervation, the reflections of very thin tranſparent ſubſtances
“ are conſiderably ſtronger than thoſe made by the ſame ſubſtances of a greater
“ thickneſs. And to the reflection of ſolid bodies it may be further added, that
“ the interſtices of their parts are void of air. For that for the moſt part they
“ are ſo, is reaſonable to believe, conſidering the ineptitude, which air hath to
“ pervade ſmall cavities, as appears by the aſcenſion of water in ſlender glaſs-
“ pipes, paper, cloth, and other ſuch like ſubſtances, whoſe pores are found too
“ ſmall to be repleniſhed with air, and yet large enough to admit water; and by
“ the difficulty, wherewith air pervades the pores of a bladder, through which
“ water find ready paſſage. And according to the eleventh obſervation, the ca-
“ vities thus void of air will cauſe the ſame kind of effects as to reflection, which
“ thoſe do, that are repleniſhed with it; but yet ſomething more manifeſtly, be-
“ cauſe

" cause the medium in relation to refractions is rarest, when most empty of air, " as Mr. HOOKE hath proved in his Micrographia; in which book he hath also " largely discoursed of this and the precedent proposition, and delivered many " other very excellent things concerning the colours of thin plates, and other na- " tural bodies, which I have not scrupled to make use of so far as they were for " my purpose.

" Prop. 4. The parts of bodies and their interstices must not be less than " of some definitive bigness, to render them opake and coloured; for the opakest " bodies, if their parts be subtilly divided (as metals by being dissolved in acid " menstruums, &c.) become perfectly transparent. And you may also remem- " ber, that in the eighth observation there was no reflection at the superficies of " the object-glasses, where they were very near one another, though they did not " absolutely touch. And in the seventeenth observation, the reflection of the " water-bubble, where it became thinnest, was almost insensible, so as to cause the " apparitions of very black spots.

" On these grounds I conceive it is, that water, salt, glass, stones, and such " like substances, are transparent; for, upon divers considerations, they seem to " be as porous as other bodies, but yet their pores and parts too small to cause " any opacity.

" Prop. 5. The transparent parts of bodies, according to their several sizes, " must *reflect* rays of one colour, and *transmit* those of another, on the same " grounds, that thin plates or bubbles do reflect or transmit those rays: and this " I take to be the ground of all their colours.

" For, if a thinned or plated body, which being of an even thickness appears " all over of one uniform colour, should be broken into fragments of the same " thickness with the plate, I see no reason, why a heap of those fragments should " not constitute a powder of the same colour, which the plate exhibited before it " was broken. And the parts of all natural bodies, being like so many fragments " of a plate, must on the same grounds exhibit the same colours.

" Now, that they do so, will further appear by the affinity of their proper- " ties: as that the infusion of nephritic-wood, and many other substances reflect " one colour, and transmit another, like thin bodies in the ninth and twentieth " observations. That the colours of silks, cloaths, and others substances, which " water or oil can intimately penetrate, become more faint and obscure by being " emerged in those liquors, and recover their vigour again by being dried, much " after the manner declared of thin bodies, in the tenth and twenty first obser- " vations: and that some of those coloured powders, which painters use, may have " their colours a little changed, by being very elaborately and finely ground. " Where I see not, what can be justly pretended for those changes, besides the " breaking of their parts into less parts by that contrition, after the same manner " that the colour of a plate is changed by varying its thickness. For which rea- " son also it is, that many flowers, by being bruised, become more transparent

" than before, or, at leaſt, in ſome degree or other, change their colours. Nor " is it much leſs to my purpoſe, that, by mixing divers liquors, very odd and " remarkable productions and changes of colours may be effected, of which no " cauſe can be more obvious and natural, than that the ſaline corpuſcles of one " liquor do variouſly act upon, or unite with, the tinging corpuſcles of another; " ſo as to make them ſwell or ſhrink (whereby not only their bulk, but their " denſity alſo may be changed) or to divide them into ſmaller corpuſcles, or make " many of them aſſociate into one cluſter; for we ſee how apt thoſe ſaline men- " ſtruums are to penetrate and diſſolve ſubſtances, to which they are applied; and " ſome of them to precipitate what others diſſolve. In like manner, if we con- " ſider the various phænomena of the atmoſphere, we may obſerve, that when " vapours are firſt raiſed, they hinder not the tranſparency of the air, being di- " vided into parts too ſmall to cauſe any reflection in their ſuperficies: but when, " in order to compoſe drops of rain, they began to coaleſce and conſtitute glo- " bules of all intermediate ſizes; thoſe globules, when they become of a conveni- " ent ſize to reflect ſome colours, and tranſmit others, may conſtitute clouds of " various colours, according to their ſizes. And I ſee not what can be rationally " conceived, in ſo tranſparent a ſubſtance as water for the production of theſe " colours, beſides the various ſizes of its parcels, which ſeem to affect a globular " figure moſt; but yet perhaps not without ſome inſtability in the ſmalleſt of " them, by reaſon that thoſe are moſt eaſily agitated by heat or any trembling mo- " tions in the air.

" Prop. 6. The parts of bodies, on which their colours depend, are denſer than " the medium, which pervades their interſtices.

" This will appear by conſidering, that the colour of a body depends not only " on the rays, which are incident perpendicularly or its parts, but on thoſe alſo, " which are incident at all other angles. And that, according to the ſeventh " obſervation, a very little variation of obliquity will change the reflected colour, " where the thin body or ſmall particle is rarer than the ambient medium, in " ſomuch that ſuch a ſmall particle will, at diverſly oblique incidents, reflect all " ſorts of colours, in ſo great a variety, that the colour, reſulting from them all " confuſedly reflected from a heap of ſuch particles, muſt rather be a white or " grey, than any other colour, or at beſt it muſt be but a very imperfect and " dirty colour; whereas, if the thin body or ſmall particle be much denſer than " the ambient medium, the colours, according to the nineteenth obſervation, are " ſo little changed by the variation of obliquity, that the rays, which are re- " flected leaſt obliquely, may predominate over the reſt ſo much, as to cauſe a " heap of ſuch particles to appear very intenſly of their colour.

" It conduces alſo ſomething to this propoſition, that, according to the twenty- " ſecond obſervation, the colours exhibited by the denſer thin body within the " rarer are more briſk than thoſe exhibited by the rarer within the denſer.

" Prop. 7. The bigneſs of the component parts of natural bodies may be " conjectured by their colours.

" For ſince the parts of theſe bodies, by propoſition 5. do moſt probably ex- " hibit the ſame colours with a plate of equal thickneſs, provided they have the " ſame refractive denſity; and ſince their parts ſeem for the moſt part to have " much the ſame denſity with water or glaſs, as by many circumſtances is obvious " to collect: to determine the ſizes of theſe parts, you need only have recourſe " to the precedent tables, in which the thickneſs of water or glaſs exhibiting any " colour is expreſſed. Thus, if it be deſired to know the diameter of a cor- " puſcle, which being of equal denſity with glaſs, ſhall reflect green of the third " order; the number $17\frac{1}{2}$ ſhows it to be about $\frac{17\frac{1}{2}}{1000000}$ parts of an inch.

" The greateſt difficulty is here to know, of what order the colour of any " body is; and for this end we muſt have recourſe to the fourth and eighteenth " obſervations, from whence may be collected theſe particulars.

" *Scarlets*, and other *reds*, *oranges* and *yellows*, if they be pure and intenſe, are " moſt probably of the ſecond order. Thoſe of the firſt and third order alſo may " be pretty good; only the orange and red of the third order have too great a " mixture of violet and blue.

" There may be good *greens* of the fourth order, but the pureſt are of the third: " and of this order the green of all vegetables ſeems to be, partly by reaſon of " the intenſeneſs of their colours, and partly becauſe when they wither, ſome of " them turn to a greeniſh yellow, and others to a more perfect yellow or orange, " or perhaps to red; paſſing firſt through all the aforeſaid intermediate colours, " which changes ſeem to be effected by the exhaling of the moiſture, which may " leave the tinging corpuſcles more denſe, and ſomething augmented by the ac- " cretion of the oily and earthy part of that moiſture. Now the green, without " doubt, is of the ſame order with thoſe colours, into which it changeth, becauſe " the changes are gradual, and thoſe colours, though uſually not very pure, yet " for the moſt part are too pure and lively to be of the fourth order.

" *Blues* and *purples* may be either of the ſecond or third order; but the beſt are " of the third. Thus the colour of *violet* ſeems to be of that order; becauſe " their ſyrup, by acid liquors, turns red, and by urinous and alkalazite turns " green. For ſince it is of the nature of acids to diſſolve or attenuate, and of " alcalis to precipitate or incraſſate, if the purple colour of the ſyrup was of " the ſecond order, an acid liquor by attenuating its tinging corpuſcles would tinge " it to a red of the firſt order, and an alcali, by incraſſating them, would change " it to a green of the ſecond order; which red and green, eſpecially the green, " ſeem too imperfect to be the colours produced by theſe changes. But if the " ſaid purple be ſuppoſed of the third order, its change to red of the ſecond " and green of the third may, without any inconvenience, be allowed.

" If there be found any body of a deeper and leſs reddiſh purple than that of " violets, its colour moſt probably is of the ſecond order. But yet there being " no body commonly known, whoſe colour is conſtantly more deep than theirs,

" I have

" I have made use of their name to denote the deepest and least reddish purples, " such as manifestly transcend their colour in purity.

" The *blue* of the first order, though very faint and little, may possibly be the " colour of some substances; and particularly the azure colour of the skies " seems to be of this order. For all vapours, when they begin to condense and " coalesce into small parcels, become first of that bigness, whereby such an azure " must be reflected, before they can constitute clouds of other colours. And so " this being the first colour, which vapours begin to reflect, it ought to be the " colour of the finest and most transparent skies, in which vapours are not ar- " rived to that grossness requisite to reflect other colours, as we find it is by ex- " perience.

" Whiteness, if it be intense, is either that in the first order of colours, of " which sort perhaps is the colour of white lead; or else it is a mixture of " those succeeding the third or fourth order, such as is the colour of paper, " linen, and most white substances. If corpuscles of various sizes, exhibiting the " colours of the second and third order, be mixed, they should rather constitute " an imperfect whiteness or grey, of which I have already spoken: but yet it seems " not impossible for them to exhibit an intense whiteness, if they be disposed to " transmit all the light, which they reflect not, and do not retain and stifle much " of it. For thus I told you, that froth at a distance hath appeared very white, " and yet, near at hand, the several bubbles, of which it was constituted, were " seen tinged all over with rings of colours of the four or five first orders.

" Lastly, for the production of *black*, the corpuscles must be less than any of " those, which exhibit colours. For at all greater sizes there is too much light re- " flected to constitute this colour. But if they be supposed a little less than is re- " quisite to reflect the blue of the first order, they will, according to the fourth, " eight, seventeenth, and eighteenth observations, reflect so very little light as " to appear intensely black, and yet may perhaps variously refract it to and fro " within themselves so long, until it happen to be stifled and lost; by which " means they will appear black in all positions of the eye without any transpa- " rency. And from hence may be understood, why fire, and the more subtil " dissolver, putrefaction, turn substances to black; why small quantities of black " substances impart their colour very freely and intensely to other substances, to " which they are applied; why glass ground very elaborately, on a copper-plate, " till it be well polished, makes the sand, together with what is worn off from " the glass, and copper, become very black; why black substances do soonest of " all others become hot and burn, which effect may proceed, partly from the " multitude of refractions in a little room, and partly from the easy commo- " tion of so very small corpuscles; and why blacks are usually a little inclined to " a bluish colour. For that they are so, may be seen by illuminating white " paper by reflection from black substances, which will usually appear of a bluish " white. And the reason is, that black borders on the obscure blue of the first " order, described in the eighteenth observation, whence the corpuscles of black " substances are most apt to reflect that colour.

" In

" In thefe defcriptions I have been the more particular, becaufe it is not impof-" fible, but that microfcopes may at length be improved to the difcovery of " corpufcles of bodies, on which their colours depend. For if thofe inftruments " could be fo far improved, as with fufficient diftinctnefs to reprefent objects five " or fix hundred times bigger than at a foot diftance they appear to our naked eyes. " I fhould hope, that we might be able to difcover fome of the greateft of thofe " corpufcles. And by one, that would magnify three or four thoufand times, per-" haps they might all be difcovered but thofe, which produce blacknefs. In the " mean while, I fee nothing material, that rationally can be doubted of, except-" ing this pofition, that tranfparent corpufcles of the fame thicknefs and denfity " with a plate do exhibit the fame colour. And this I would have underftood " not without fome latitude, as well becaufe thofe corpufcles may be of irregular " figures, and many rays muft be obliquely incident, and fo have a fhorter way " through them than the length of their diameter; as becaufe the ftraitnefs of " the medium, pent in on all fides, may a little alter its motions, or other qua-" lities, on which the reflexion depends. But yet I cannot much fufpect the laft, " becaufe I have obferved of fome fmall plates of Mufcovy-glafs, which were of " an even thicknefs, that through a microfcope they have appeared of the fame " colour at their edges and corners, where the included medium was terminated, " which they appeared of in other places. However, it would add much to our " fatisfaction, if thofe corpufcles could be difcovered with microfcopes, which if " we fhall ever attain to, I fear it will be the utmoft improvement of this fenfe; " for it feems impoffible to fee the more fecret and noble works of nature within " thofe corpufcles, by reafon of their tranfparency.

" This may fuffice concerning the conftitution of natural bodies, on which their " colours depend. But for further underftanding the nature of reflections, I " fhall add thefe two following propofitions.

" Prop. 8. The caufe of the reflection is not the impinging of light on the " folid and impervious parts of bodies, as is commonly fuppofed.

" This will appear by the following confiderations: firft, that in the paffage of " light out of glafs into air, there is a reflection as ftrong or ftronger than in its " paffage out of air into glafs, and by many degrees ftronger than in its paffage " out of glafs into water. And it feems not probable, that air fhould have more " reflecting parts than water or glafs. But if that fhould poffibly be fuppofed, it " will avail nothing; for the reflection is as ftrong, if not ftronger, when the air " is drawn away from the glafs (fuppofe in the air-pump invented by Mr. Boyle) " as when it is adjacent to it. Secondly, if light in its paffage out of glafs into " air be incident more obliquely than at an angle of forty or forty-one degrees, " it is wholly *reflected*; if lefs obliquely, it is in great meafure *tranfmitted*. Now " it is not to be imagined, that light at one degree of obliquity fhould meet with " pores enough in the air to tranfmit the greater part of it, and at another degree " of obliquity meet with nothing but parts to reflect it wholly; efpecially confi-" dering, that in its paffage out of air into glafs, how oblique foever be its " incidence, it finds pores enough in the glafs to tranfmit the greateft part of it.

" If

" If any man ſuppoſe, that it is not reflected by the air, but by the utmoſt ſu-
" perficial parts of the glaſs, there is ſtill the ſame difficulty; beſides, that ſuch
" a ſuppoſition is unintelligible; and will alſo appear to be falſe, by applying wa-
" ter behind ſome part of the glaſs inſtead of air. For ſo in a convenient obli-
" quity of the rays, ſuppoſe of forty-five or forty-ſix degrees, at which they are
" all *reflected*, where the air is adjacent to the glaſs, they ſhall be in great meaſure
" *tranſmitted*, where the water is adjacent to it; which argues, that their reflection
" or tranſmiſſion depends on the conſtitution of the air and water behind the
" glaſs, and not on the parts of the glaſs.

" Thirdly, if the colours made by a priſm, placed at the entrance of a beam
" of light into a darkened room, be ſucceſſively caſt on a ſecond priſm placed
" at a great diſtance from the former, in ſuch manner that they are all alike in-
" cident upon it; the ſecond priſm may be ſo inclined to the incident rays, that
" thoſe, which are of a blue colour, ſhall be all reflected by it; and yet thoſe of a
" red colour pretty copiouſly tranſmitted. Now if the reflection be cauſed by
" the parts of air or glaſs, I would aſk, why at the ſame obliquity of incidence
" the blue ſhould wholly impinge on thoſe parts ſo as to be all reflected, and yet
" the red find pores enough to be in great meaſure tranſmitted. Fourthly,
" where two glaſſes touch one another, there is no ſenſible reflection, as was de-
" clared in the firſt obſervation; and yet I ſee no reaſon, why the rays ſhould not
" impinge on the parts of glaſs, when contiguous to another glaſs, a ſmuch as
" when contiguous to air. Fifthly, when the top of a water-bubble (in the ſe-
" venteenth obſervation) by the continual ſubſiding and exhaling of the water
" grew very thin, there was ſuch a little and almoſt inſenſible quantity of light
" reflected from it, that it appeared intenſely black; whereas, round about that
" black ſpot, where the water was thicker, the reflection was ſo ſtrong as to make
" the water ſeem very white. Nor is it only at the leaſt thickneſs of thin plates
" or bubbles that there is no manifeſt reflection, but at many other thickneſſes
" continually greater and greater. For in the fifteenth obſervation, the rays of the
" ſame colour were by turns tranſmitted at one thickneſs, and reflected at another
" thickneſs, for an intermediate number of ſucceſſions. And yet in the ſuperfi-
" ficies of the third body, where it is of any one thickneſs, there are as many
" parts for the rays to impinge on, as where it is of any other thickneſs.

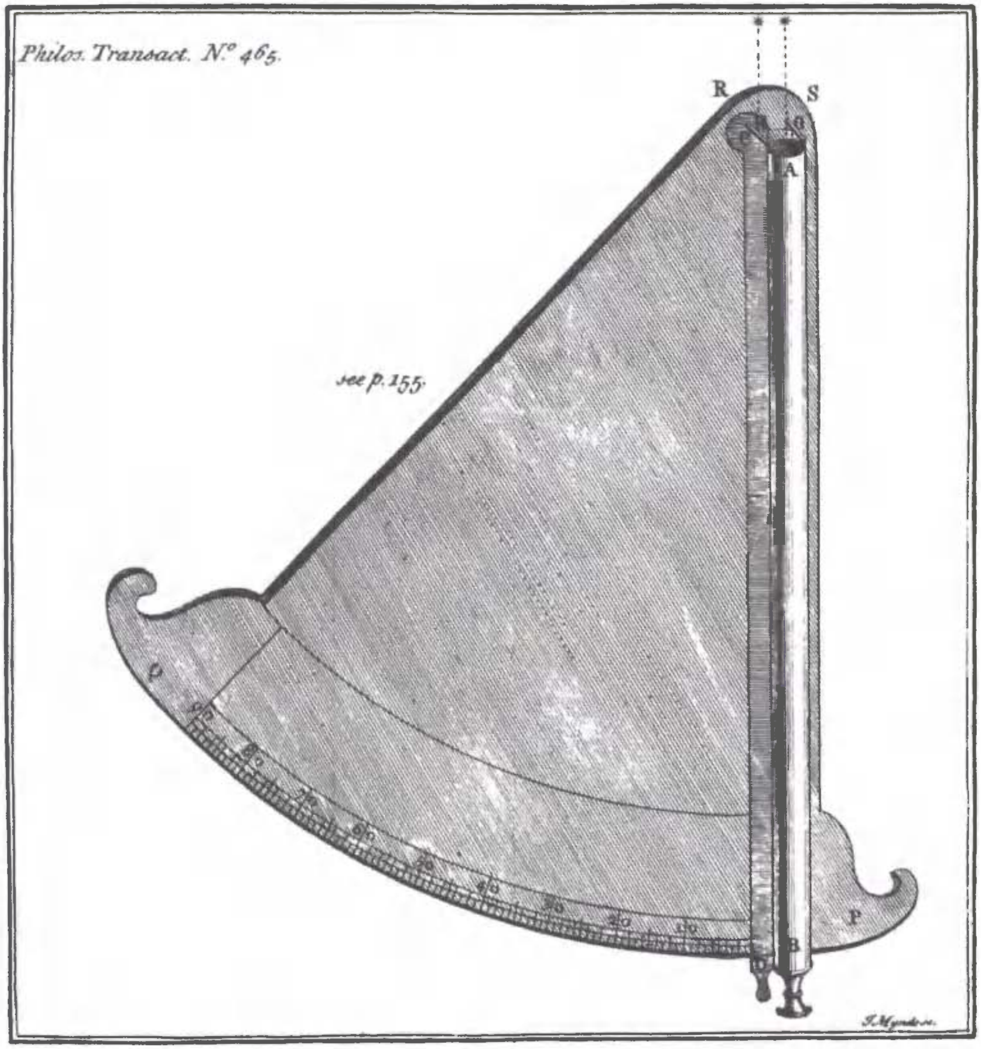
" Laſtly, if reflection were cauſed by the parts of reflecting bodies, it would
" be impoſſible for thin plates or bubbles, at the ſame place to reflect the rays of
" one colour, and tranſmit thoſe of another, as they do according to the thirteenth
" and fifteenth obſervations. For is is not to be imagined, that at one place the
" rays, which, for inſtance, exhibit a blue colour, ſhould have the fortune to daſh
" upon the *parts*, and thoſe, which exhibit a red, to hit upon the pores of the
" body; and then at another place, where the body is either a little thicker, or a
" little thinner, that on the contrary the blue ſhould hit upon its *pores*, and the
" *red* upon its *parts*.

" Prop. 9. It is moſt probable, that the rays, which impinge on the ſolid
" parts of any body, are not reflected but ſtifled and loſt in that body.

" This

" This is confentaneous to the precedent propofition, and will further appear " by confidering, that if all the rays fhould be reflected, which impinge on the in- " ternal parts of clear water or cryftal, thofe fubftances fhould rather have a cloudy " than fo very clear tranfparency.

" And further, there would be no principle of the obfcurity or blacknefs, which " fome bodies have in all pofitions of the eye. For to produce this effect, it is ne- " ceffary, that many rays be retained and loft in the body, and it feems not pro- " bable, that any rays can be ftopped and retained in it, which do not impinge on " its parts."



Philos. Transact. N° 465.
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see p. 155.
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J. Mynde sc.

I. *A true Copy of a Paper found, in the Hand Writing of Sir* Isaac Newton, *among the Papers of the late Dr.* Halley, *containing a Description of an* Instrument *for observing the* Moon's Distance *from the* Fixt Stars *at* Sea.

Read at a Meeting of the Royal Society, October 28. 1742.

IN the annexed Scheme, *PQRS* denotes a Plate of Brass, accurately divided in the Limb *DQ*, into $\frac{1}{2}$ Degrees, $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, by a Diagonal Scale; and the $\frac{1}{2}$ Degrees, and $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, counted for Degrees, Minutes, and $\frac{1}{6}$ Minutes.

AB, is a Telescope, three or four Feet long, fixt on the Edge of that Brass Plate.

G, is a *Speculum*, fixt on the said Brass Plate perpendicularly, as near as may be to the Object-glass of the Telescope, so as to be inclined 45 Degrees to the Axis of the Telescope, and intercept half the Light which would otherwise come through the Telescope to the Eye.

CD, is a moveable Index, turning about the Centre *C*, and, with its fiducial Edge, shewing the Degrees, Minutes, and $\frac{1}{6}$ Minutes, on the Limb of the Brass Plate *PQ*; the Centre *C*, must be over-against the Middle of the *Speculum G*.

H, is another *Speculum*, parallel to the former, when the fiducial Edge of the Index falls on ood oo′ oo″; so that the same Star may then appear through

X the

the Telefcope, in one and the fame Place, both by the direct Rays and by the reflex'd ones; but if the Index be turned, the Star fhall appear in two Places, whofe Diftance is fhewed, on the Brafs Limb, by the Index.

By this Inftrument, the Diftance of the Moon from any Fixt Star is thus obferved: View the Star through the Perfpicil by the direct Light, and the Moon by the Reflext (or on the contrary); and turn the Index till the Star touch the Limb of the Moon, and the Index fhall fhew upon the Brafs Limb of the Inftrument, the Diftance of the Star from the Limb of the Moon; and though the Inftrument fhake, by the Motion of your Ship at Sea, yet the Moon and Star will move together, as if they did really touch one another in the Heavens; fo that an Obfervation may be made as exactly at Sea as at Land.

And by the fame Inftrument, may be obferved, exactly, the Altitudes of the Moon and Stars, by bringing them to the Horizon; and thereby the Latitude, and Times of Obfervations, may be determined more exactly than by the Ways now in Ufe.

In the Time of the Obfervation, if the Inftrument move angularly about the Axis of the Telefcope, the Star will move in a Tangent of the Moon's Limb, or of the Horizon; but the Obfervation may notwithftanding be made exactly, by noting when the Line, defcribed by the Star, is a Tangent to the Moon's Limb, or to the Horizon.

To make the Inftrument ufeful, the Telefcope ought to take in a large Angle: And to make the Obfervation true, let the Star touch the Moon's Limb, not on the Outfide of the Limb, but on the Infide.

II. *The*

III.

Newton on Chemistry, Atomism, the Æther, and Heat

Newton's Chemical Papers

MARIE BOAS

Newton's extraordinary achievements in physics have understandably overshadowed his chemical work; it is fortunate that the sale some years ago of his large collection of alchemical books and notes forced a renewed consideration of his overt preoccupation with alchemy, which in turn has led to the study of his place in the history of chemistry. He has been found to have been a skilled, original, and painstaking chemist with a wide and profound influence.

A full understanding of Newton's chemical thinking and of the experimental basis of his conclusions will be reached only after a careful analysis of his extant chemical notebooks, now in the University Library, Cambridge.[1] The notebooks have not yet been fully studied. They were summarized by the group who compiled the *Cata-*

[1] See *Catalogue of the Newton Papers Sold by Order of the Viscount Lymington* (London, 1936). An early general account is Douglas McKie, "Some Notes on Newton's Chemical Philosophy Written Upon the Occasion of the Tercentenary of his Birth," *Philosophical Magazine* [7] *33,* 847–70 (1942). These notebooks have been discussed and partially printed in A. R. and M. B. Hall, "Newton's Chemical Experiments," *Archives internationales d'histoire des sciences 11,* 113–52 (1958).

logue of the Portsmouth Collection in 1888.[2] There are three notebooks of considerable chemical interest containing chaotic records of experiments, proposed experiments, notes from books, recipes, topics for possible investigation—a fascinating laboratory record. But, important as these sources are for Newton's chemical development, they can safely be ignored in evaluating his chemical influence, since 18th-century Newtonians read the published works, not the manuscripts. Such Newtonian scientists were, as Mme. Metzger showed in her brilliant *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris, 1930), profoundly influenced by the chemical implications of the theory of universal gravitation. More than that, they read and absorbed those of Newton's works that were wholly or partly chemical in nature.

Of the papers reprinted here, the "Letter to Boyle," dated 1678, was first published in Thomas Birch's "Life of Boyle" prefixed to the first edition, in 1744, of *The Works of the Honourable Robert Boyle,* which Birch edited. Though it can have had wide circulation only after Newton's death, it may have been known earlier, since Boyle or his executors could easily have shown it to interested scientists. Once printed, the letter was immediately reprinted in Bryan Robinson's *Sir Isaac Newton's Account of the Æther, with some additions by way of an appendix.*[3] Inevitably of greater influence was the "De natura acidorum," written in 1692 and first published in 1710 in the "Introduction" to volume II of the *Lexicon Technicum* of John Harris, F.R.S.[4] Harris is the authority for the date of composition; he stated that he printed the paper with Newton's permission and that the translation into English had been read and approved by Newton. The paper was subsequently printed, in a slightly different Latin version, in volume II of Newton's *Opuscula mathematica, philosophica et philologica* (Lausanne, 1744). This version, and that

[2] *A Catalogue of the Portsmouth Collection of Books and Papers Written by or Belonging to Sir Isaac Newton* (Cambridge, 1888). I have to thank the authorities of the University Library, Cambridge, who kindly allowed me access to the chemical notebooks mentioned.

[3] Dublin, 1745. This reprints not only the "Letter," but Queries 16–23 of the *Opticks.* Robinson had already published in 1743 *A Dissertation on the Æther of Sir Isaac Newton,* based on the *Opticks.*

[4] Cf. Douglas McKie: "John Harris and his *Lexicon Technicum* (1704)," *Endeavour 4,* 53–57 (1945).

later published by Samuel Horsley in *Isaaci Newtoni Opera quae exstant omnia* (1782), confirm the statement by Harris that more than one version of the paper was known to him; fragments, both in Newton's hand and in that of an amanuensis, are tucked into one of Newton's chemical notebooks.[5] The paper on heat was first published anonymously in the *Philosophical Transactions* for March-April 1701, and is the source for Newton's Law of Cooling. It is essentially a chemical paper, not only because of its interesting use of the melting points of mixtures of metals, but also because the related problems of heat and fire were considered to be a part of chemistry, rather than of physics, in the eighteenth century. There are a number of references to both chemical experiment and theory, particularly the nature of solubility and solution, in the paper on optics printed in this volume on page 177. Finally, there is one very important source for Newton's chemistry not reprinted here: the Queries, and more particularly the 31st Query, of the *Opticks.*

Even a cursory glance at Newton's chemical papers indicates that his approach to chemical problems was not that of an alchemist. His explanations are in the language and spirit of experimental natural philosophy, quite different from the usually cloudy and often mystic views of the alchemists whose works he bought or borrowed so avidly, as he did all books that had any pretensions to dealing with chemical theory or practice.[6] His library included scores of alchemical works; he read and was influenced by Van Helmont and his English follower George Starkey; but equally he read and was influenced by such natural philosophers as Robert Boyle who despised all mysticism in science. Actually Newton's chemical approach was far nearer to Boyle's than to Van Helmont's. Many of Newton's experiments on the colors of chemical solutions appear to be extensions of Boyle's experiments. And Newton always followed Boyle in treating chemistry

[5] S. I. Vavilov, "Newton and the Atomic Theory," in The Royal Society, *Newton Tercentenary Celebrations 15–19 July 1946* (Cambridge, 1947), 43–55, is in error in believing that Harris mistranslated the Latin; the divergence is so great that he can only have translated from another version.

[6] For a list of books owned by Newton, see R. de Villamil, *Newton: The Man* (London, n.d.).

as a physical science, rather than as a mystic art, and in using chemistry to suggest and confirm a molecular physics.

The "Letter to Boyle," the "De natura acidorum," and the 31st Query of the *Opticks* have much in common, although the exposition varies decidedly in works written over a period of more than twenty years. Basically, all three are concerned with the problems of chemical reactivity and the action of solvents as explicable in terms of a particulate theory of matter, which assumes a theory of universal attraction.

Underlying the whole of Newton's chemical (and physical) theory is the concept of matter as particulate. Almost all scientists of the later 17th century agreed that matter was composed of small, discrete particles, corpuscles, or atoms, and that the chemical and physical properties of bodies could be accounted for by means of the size, shape, *and motion* of the constituent particles. This is the so-called mechanical philosophy which rejected all "occult forces" such as sympathy, antipathy, congruity, incongruity, attraction, and hostility, and instead explained all the properties of matter in terms of the new science of dynamics.[7] One kind of mechanical explanation was the Cartesian: Descartes and his followers believed in an æther, a material substance composed of specially small, mobile particles which imparted motion and impulse to the naturally inert and gross particles of ordinary matter. Boyle on the other hand rejected even the æther, assuming random but constant motion of all particles to explain impulse; even chemical reaction he believed to be caused not by an æther nor by any attraction of one particle toward another, but by the fact that the size and shape of the particles of one substance happened to correspond to the size and shape of the pores between the particles of another substance.

Newton's addition to the mechanical philosophy was the assumption that particles moved mainly under the influence of what he at first called sociability and later called attraction. Attraction is, of course, the concept that made the *Principia* possible; the theory of universal gravitation is that all bodies in the universe, large or small, are mutually attracted to one another, and this theory Newton extended to both the physical and the chemical worlds. In using

[7] For a detailed account, see M. Boas, "The Establishment of the Mechanical Philosophy," *Osiris, 10,* 412–541 (1952).

a force like attraction Newton was, as Cartesian critics tirelessly insisted, something of a scientific reactionary, for the great prestige of the mechanical philosophers had been based chiefly upon the determined banishing of all such "occult" forces. But Newton found the concept uniquely useful, and made it as little occult as possible by treating it from the mechanical point of view.

Newton's ideas on the possible mechanism of attraction were never definitely worked out. When he spoke of attraction he sometimes, as in the "Letter to Boyle," immediately explained it in terms of impulse by an æther—sometimes, but by no means always. The *Principia* loftily and expressly avoided any explanation of the mechanism of gravitational attraction except for a suggestion in the final scholium added only in the second edition. In other works, especially in Queries 16–24 of the *Opticks,* Newton discussed ways in which the æther might account for chemical and gravitational attraction, but never did he offer a developed hypothesis. One is left with the feeling that Newton preferred a mechanical explanation in terms of an æther to the "action-at-a-distance" concept of pure attraction; this indeed is what he wrote Bentley. His avoidance of any decision in the *Principia* must have come chiefly from the absence of genuine experimental evidence. In fact, Boyle had published experiments that showed it unlikely that the æther as postulated by Descartes could exist; and Newton had demolished the Cartesian æther and its vortex action theoretically in the second book of the *Principia.* Any satisfactory æther had to be so different from that of Descartes as to be, essentially, experimentally undetectable, a most uncomfortable position for an empiricist to maintain.[8]

There is one further aspect of Newton's theory of matter that deserves mention, an aspect contained in the random notes at the end of the "De natura acidorum." Here—and it is the only place where he discussed the matter—Newton suggested that particles associate to form aggregates "of the first composition," that these associate to form aggregates "of the second composition," and so on. This led to a method of differentiating between reaction and transmutation. When gold reacts with mercury to form mercury amalgam

[8] Boyle, *The Spring and Weight of the Air, First Continuation,* in Thomas Birch, *The Works of the Honourable Robert Boyle* (second ed., London, 1772), III, 250 ff.; *Rarefaction of the Air; Works,* III, 495 ff.

the gold is recoverable, so presumably the mercury particles penetrated only to the particles of the "last" composition. But if mercury could get between the particles of the first composition, then and only then would gold be transmuted into some other substance. This is an intriguing suggestion, but analogies with modern atomic physics are not valid. The fact that this theory is not referred to in the *Opticks* must mean that Newton found this concept (which incidentally is not entirely original with him, for the notion of aggregates of particles is to be found in the work of many early 17th-century chemists writing on the nature of matter—for example, Sennert) not to be a useful enough hypothesis to be pursued; but it did convince him that transmutation was too difficult to be probable.

Unlike gravitational attraction, which was a universal force varying only objectively with mass and distance, chemical attraction was selective and varied subjectively with each pair of chemical compounds. So complex was the action of this kind of attraction that the addition of one chemical to another could alter the attraction or sociability of the latter to a third compound. This Newton pointed out as early as 1675 in one of the optical papers sent to the Royal Society, a letter which incidentally indicates Newton's current chemical interest.

Almost always Newton's discussion of sociability is a part of a search for a general theory of solution. The solvents that most interested him were the common strong acids and he repeatedly grappled with the difficult reactions between acids and metals. The most interesting of these he thought were the reactions of acid mixtures with gold and silver and he several times mentioned the specific nature of the ability of aqua regia to dissolve gold but not silver, and of aqua fortis to dissolve silver but not gold; this problem he tried to resolve by combining attraction and the relative size of the particles of acids and the pores between the particles of the metal as criteria of solubility.[9] His difficulties in this regard were not made easier by the necessity of defining clearly what substances should be classed as acids. In the mid 17th century there had been developed

[9] See Thomas S. Kuhn, "Newton's '31st Query' and the Degradation of Gold," *Isis 42,* 296-298 (1951), and M. Boas and T. S. Kuhn, "Newton and the Theory of Chemical Solution," *Isis 43,* 123-124 (1952).

a chemical theory based on the notion that all substances contained either acids or alkalies, so that all chemical reactions could be regarded as the combination of an acid and an alkali. Robert Boyle repeatedly attacked this view, in works which now have only the interest of controversy; but of lasting importance was his classification of acids and alkalies on the basis of their characteristic reactions: thus all acids turned syrup of violets red, all alkalies turned syrup of violets green, and some substances did neither.[10] Eighteenth-century chemists very frequently accepted this useful empirical classification. Newton used these tests in his own work; but he preferred to define acids theoretically, rather than empirically, as substances "endued with a great Attractive Force; in which Force their Activity consists." This definition, in the "De natura acidorum," was still assumed in the 31st Query of the *Opticks;* it was the chief explanation, for Newton, of the great solvent activity habitually displayed by acids. Another reaction involving acids which interested Newton, and which he also discussed in the 31st Query, was the replacement of one metal by another in an acid solution. He went so far as to list the six common metals in the order in which they would displace one another from a solution of aqua fortis (strong nitric acid). This is perhaps a forerunner of the tables of affinity so common in the eighteenth century, by which chemists tried to predict the course of a reaction.

Newton built no great chemical system comparable to his physical system of the universe, but by combining a particulate theory of matter with a profound experimental knowledge of chemistry he helped push chemistry one step nearer its acceptance as a true physical science. Newton is not less of a chemist because there is no positive chemical discovery associated with his name. Boerhaave, the great eighteenth-century physician and Newtonian chemist, more than once underlined the importance of chemistry as a part of natural philosophy with such remarks as, "*Sir Isaac Newton* gives us many chymical Experiments of the Attraction of Bodies" and "Isaac Newton . . . when he demonstrates by manifest effects the laws, actions, and forces of bodies does so not otherwise than by chem-

[10] *Reflections on the Hypothesis of Alkali and Acidum; Works,* IV, 284–292; cf. H. Metzger, *Les doctrines chimiques en France* (Paris, 1923), pp. 205–210. For Boyle's classification, see *The Experimental History of Colours; Works,* I, 744, 765–767.

istry."[11] Henry Pemberton, editor of the third edition of the *Principia* and author of a widely read popularization of Newtonian physics, in his chemical lectures, read at Gresham College about 1730, cited Newton's chemical achievements as laying the groundwork for greater discoveries: "Not only his general proofs, drawn from chemical experiments, of some active principles existing in nature, by which all natural effects are caused, but his more particular thoughts, concerning the nature of acids, cannot be sufficiently admired."[12] Many more examples could be cited; and a modern historian must agree that Newton's approach to chemical problems and his attempt to interpret and analyze chemical reactivity are very nearly as full of insight, as interesting, and as influential as the 18th-century chemists thought them to be. Or, as John Harris said in the *Lexicon Technicum,* introducing the "De natura acidorum," "The following Paper of Sir *Isaac Newton's* is excellently well worth the Philosophical Reader's most serious and repeated Perusal; for it containes in it the Reason of the Ways and Manner of all Chymical Operations, and indeed of almost all the Physical Qualities, by which Natural Bodies, by their small Particles, act one upon another."

[11] H. Boerhaave, *A Method of Studying Physick* (London, 1719), p. 101, and *Sermo academicus de chemia* (Leyden, 1718).

[12] *A Course of Chymistry . . . now first published from the Author's Manuscript by James Wilson* (London, 1771), pp. 13–14.

THE

WORKS

OF THE HONOURABLE

ROBERT BOYLE.

In FIVE VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME I.

LONDON:

Printed for A. MILLAR, opposite *Catharine-Street*, in the *Strand.*

MDCCXLIV.

THE regard, which the great *Newton* had for Mr. *Boyle*, will appear from a very curious letter, which the former wrote to him, explaining his ſentiments upon one of the moſt abſtruſe points of philoſophy, with reſpect to the ætherial medium, which in his *Optics* he propoſes as the mechanical cauſe of gravitation. This letter having never before ſeen the light, will be proper to be inſerted here.

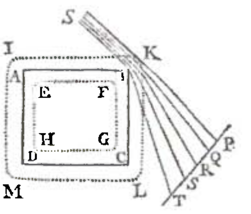
“ Honoured Sir,

“ I HAVE ſo long deferred to ſend you my thoughts about the phyſical qualities we ſpake of, that did I not eſteem myſelf obliged by promiſe, I think I ſhould be aſhamed to ſend them at all. The truth is, my notions about things of this kind are ſo indigeſted, that I am not well ſatisfied my ſelf in them; and what I am not ſatisfied in, I can ſcarce eſteem fit to be communicated to others; eſpecially in natural philoſophy, where there is no end of fancying. But becauſe I am indebted to you, and yeſterday met with a friend, Mr. *Maulyverer*, who told me he was going to *London*, and intended to give you the trouble of a viſit, I could not forbear to take the opportunity of conveying this to you by him.

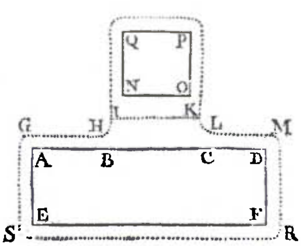
“ IT being only an explication of qualities, which you deſire of me, I ſhall ſet down my apprehenſions in the form of ſuppoſitions, as follows. And firſt, I ſuppoſe, that there is diffuſed through all places an æthereal ſubſtance, capable of contraction and dilatation, ſtrongly elaſtic, and, in a word, much like air in all reſpects, but far more ſubtile.

“ 2. I SUPPOSE this æther pervades all groſs bodies, but yet ſo as to ſtand rarer in their pores than in free ſpaces, and ſo much the rarer, as their pores are leſs. And this I ſuppoſe (with others) to be the cauſe, why light incident on thoſe bodies is refracted towards the perpendicular; why two well poliſhed metals cohere in a receiver exhauſted of air; why ☿ ſtands ſometimes up to the top of a glaſs pipe, though much higher than 30 inches; and one of the main cauſes, why the parts of all bodies cohere; alſo the cauſe of filtration, and of the riſing of water in ſmall glaſs pipes above the ſurface of the ſtagnating water they are dipped into: for I ſuſpect the æther may ſtand rarer, not only in the inſenſible pores of bodies, but even in the very ſenſible cavities of thoſe pipes. And the ſame principle may cauſe menſtruums to pervade with violence the pores of the bodies they diſſolve, the ſurrounding æther, as well as the atmoſphere, preſſing them together.

“ 3. I ſuppoſe the rarer æther within bodies, and the denſer without them, not to be terminated in a mathematical ſuperficies, but to grow gradually into one another; the external æther beginning to grow rarer, and the internal to grow denſer, at ſome little diſtance from the ſuperficies of the body, and running through all intermediate degrees of denſity in the intermediate ſpaces: And this may be the cauſe, why light, in *Grimaldo*'s experiment, paſſing by the edge of a knife, or other opake body, is turned aſide, and as it were refracted, and by that refraction makes ſeveral colours. Let ABCD be a denſe body, whether opake, or tranſparent, EFGH the outſide of the uniform æther, which is within it, IKLM the inſide of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of denſity between that of the two uniform æthers on either ſide. This being ſuppoſed, the rays of the ſun SB, SK, which paſs by the edge of this body between B and K, ought in their paſſage through the unequally denſe æther there, to receive a ply from the denſer æther, which is on that ſide towards K, and that the more, by how much they paſs nearer to the body, and thereby to be ſcattered through the ſpace PQRST, as by experience they are found to be. Now the ſpace between the limits EFGH and IKLM I ſhall call the ſpace of the æther's graduated rarity.



“ 4. When two bodies moving towards one another come near together, I ſuppoſe the æther between them to grow rarer than before, and the ſpaces of its graduated rarity to extend further from the ſuperficies of the bodies towards one another; and this, by reaſon, that the æther cannot move and play up and down ſo freely in the ſtrait paſſage between the bodies, as it could before they came ſo near together. Thus, if the ſpace of the æther's graduated rarity reach from the body ABCDFE only to the diſtance GHLMRS, when no other body is near it, yet may it reach farther, as to IK, when another body NOPQ approaches: and as the other body approaches more and more, I ſuppoſe the æther between them will grow rarer and rarer.



“ Theſe

" THESE ſuppoſitions I have ſo deſcribed, as if I thought the ſpaces of graduated æther " had preciſe limits, as is expreſſed at IKLM in the firſt figure, and GMRS in the " ſecond: for thus I thought I could better expreſs my ſelf. But really I do not think they " have ſuch preciſe limits, but rather decay inſenſibly, and, in ſo decaying, extend to a much " greater diſtance, than can eaſily be believed, or need be ſuppoſed.

" 5. Now from the fourth ſuppoſition it follows, that when two bodies approaching one " another, come ſo near together, as to make the æther between them begin to rarefy, they " will begin to have a reluctance from being brought nearer together, and an endeavour to " recede from one another: which reluctance and endeavour will encreaſe, as they come " nearer together, becauſe thereby they cauſe the interjacent æther to rarefy more and more. " But at length, when they come ſo near together, that the exceſs of preſſure of the exter- " nal æther, which ſurrounds the bodies, above that of the rarefied æther, which is between " them, is ſo great, as to overcome the reluctance, which the bodies have from being brought " together; then will that exceſs of preſſure drive them with violence together, and make " them adhere ſtrongly to one another, as was ſaid in the ſecond ſuppoſition. For inſtance, " in the ſecond figure, when the bodies ED and NP are ſo near together, that the ſpaces " of the æther's graduated rarity begin to reach to one another, and meet in the line IK; " the æther between them will have ſuffered much rarefaction, which rarefaction requires " much force, that is, much preſſing of the bodies together: and the endeavour, which the " æther between them has to return to its former natural ſtate of condenſation, will cauſe the " bodies to have an endeavour of receding from one another. But on the other hand, to " counterpoiſe this endeavour, there will not yet be any exceſs of denſity of the æther, which " ſurrounds the bodies, above that of the æther, which is between them at the line IK. But " if the bodies come nearer together, ſo as to make the æther in the mid-way-line IK grow " rarer than the ſurrounding æther, there will ariſe from the exceſs of denſity of the ſur- " rounding æther a compreſſure of the bodies towards one another: which when by the " nearer approach of the bodies it becomes ſo great, as to overcome the aforeſaid endeavour " the bodies have to recede from one another, they will then go towards one another, and " adhere together. And, on the contrary, if any power force them aſunder to that diſtance, " where the endeavour to recede begins to overcome the endeavour to accede, they will again " leap from one another. Now hence I conceive it is chiefly, that a fly walks on water " without wetting her feet, and conſequently without touching the water; that two poliſhed " pieces of glaſs are not without preſſure brought to contact, no, not though the one be plain, " the other a little convex; that the particles of duſt cannot by preſſing be made to cohere, " as they would do, if they did but fully touch; that the particles of tinging ſubſtances and " ſalts diſſolved in water do not of their own accord concrete and fall to the bottom, but " diffuſe themſelves all over the liquor, and expand ſtill more, if you add more liquor to " them. Alſo, that the particles of vapours, exhalations, and air, do ſtand at a diſtance from " one another, and endeavour to recede as far from one another, as the preſſure of the in- " cumbent atmoſphere will let them: for I conceive the confuſed maſs of vapours, air, and " exhalations, which we call the atmoſphere, to be nothing elſe but the particles of all ſorts " of bodies, of which the earth conſiſts, ſeparated from one another, and kept at a diſtance, " by the ſaid principle.

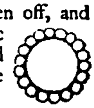
" FROM theſe principles the actions of menſtruums upon bodies may be thus explained. " Suppoſe any tinging body, as cochineal, or logwood, be put into water; ſo ſoon as the " water ſinks into its pores and wets on all ſides any particle, which adheres to the body " only by the principle in the ſecond ſuppoſition, it takes off, or at leaſt much diminiſhes " the efficacy of that principle to hold the particle to the body, becauſe it makes the æther " on all ſides the particle to be of a more uniform denſity than before. And then the particle " being ſhaken off, by any little motion, floats in the water, and with many ſuch others makes " a tincture; which tincture will be of ſome lively colour, if the particles be all of the " ſame ſize and denſity; otherwiſe of a dirty one. For the colours of all natural bodies " whatever ſeem to depend on nothing but the various ſizes and denſities of their particles; " as I think you have ſeen deſcribed by me more at large in another paper. If the particles " be very ſmall (as are thoſe of ſalts, vitriols, and gums) they are tranſparent; and as they " are ſuppoſed bigger and bigger, they put on theſe colours in order, black, white, yellow, " red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, " red, *&c.* as is diſcerned by the colours, which appear at the ſeveral thickneſſes of very thin " plates of tranſparent bodies. Whence, to know the cauſes of the changes of colours, " which are often made by the mixtures of ſeveral liquors, it is to be conſidered, how the " particles of any tincture may have their ſize or denſity altered by the infuſion of another " liquor.

" WHEN any metal is put into common water, the water cannot enter into its pores, to act " on it and diſſolve it. Not that water conſiſts of too groſs parts for this purpoſe, but be- " cauſe it is unſociable to metal. For there is a certain ſecret principle in nature, by which " liquors are ſociable to ſome things, and unſociable to others. Thus water will not mix " with oil, but readily with ſpirit of wine, or with ſalts. It ſinks alſo into wood, which " quickſilver will not; but quickſilver ſinks into metals, which, as I ſaid, water will not. " So aqua fortis diſſolves ☽, not ☉, aqua regis ☉, not ☽, *&c.* But a liquor, which is of itſelf

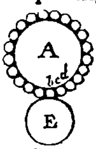
" unſociable

3

“ unſociable to a body, may, by the mixture of a convenient mediator, be made ſociable. So molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of ſaline ſpirits, will mix with metal. Now when any metal is put in water impregnated with ſuch ſpirits, as into aqua fortis, aqua regis, ſpirit of vitriol, or the like, the particles of the ſpirits, as they, in floating in the water, ſtrike on the metal, will by their ſociableneſs enter into its pores, and gather round its outſide particles, and, by advantage of the continual tremor the particles of the metal are in, hitch themſelves in by degrees between thoſe particles and the body, and looſen them from it; and the water entering into the pores together with the ſaline ſpirits, the particles of the metal will be thereby ſtill more looſed, ſo as, by that motion the ſolution puts them into, to be eaſily ſhaken off, and made to float in the water: the ſaline particles ſtill encompaſſing the metallic ones as a coat or ſhell does a kernel, after the manner expreſſed in the annexed figure. In which figure I have made the particles round, though they may be cubical, or of any other ſhape.



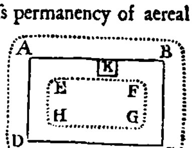
“ If into a ſolution of metal thus made be poured a liquor, abounding with particles, to which the former ſaline particles are more ſociable than to the particles of the metal (ſuppoſe with particles of ſalt of tartar) then ſo ſoon as they ſtrike on one another in the liquor, the ſaline particles will adhere to thoſe more firmly than to the metalline ones, and by degrees be wrought off from thoſe to encloſe theſe. Suppoſe A a metalline particle, encloſed with ſaline ones of ſpirit of nitre, E a particle of ſalt of tartar, contiguous to two of the particles of ſpirit of nitre b and c, and ſuppoſe the particle E is impelled by any motion towards d, ſo as to roll about the particle c, till it touch the particle d, the particle b adhering more firmly to E than to A, will be forced off from A. And by the ſame means the particle E, as it rolls about A, will tear off the reſt of the ſaline particles from A, one after another, till it has got them all, or almoſt all, about itſelf. And when the metallic particles are thus diveſted of the nitrous ones, which, as a mediator between them and the water, held them floating in it; the alcalizate ones crouding for the room the metallic ones took up before, will preſs theſe towards one another, and make them come more eaſily together: ſo that by the motion they continually have in the water, they ſhall be made to ſtrike on one another, and then, by means of the principle in the ſecond ſuppoſition, they will cohere and grow into cluſters, and fall down by their weight to the bottom, which is called precipitation.



“ In the ſolution of metals, when a particle is looſing from the body, ſo ſoon as it gets to that diſtance from it, where the principle of receding deſcribed in the fourth and fifth ſuppoſitions begins to overcome the principle of acceding, deſcribed in the ſecond ſuppoſition, the receding of the particle will be thereby accelerated; ſo that the particle ſhall as it were with violence leap from the body, and putting the liquor into a briſk agitation, beget and promote that heat we often find to be cauſed in ſolutions of metals. And if any particle happen to leap off thus from the body, before it be ſurrounded with water, or to leap off with that ſmartneſs, as to get looſe from the water; the water, by the principle in the fourth and fifth ſuppoſitions, will be kept off from the particle, and ſtand round about it, like a ſpherically hollow arch, not being able to come to a full contact with it any more. And ſeveral of theſe particles afterwards gathering into a cluſter, ſo as by the ſame principle to ſtand at a diſtance from one another, without any water between them, will compoſe a bubble. Whence I ſuppoſe it is, that in briſk ſolutions there uſually happens an ebullition.

“ This is one way of tranſmuting groſs compact ſubſtances into aereal ones. Another way is, by heat. For as faſt as the motion of heat can ſhake off the particles of water from the ſurface of it, thoſe particles, by the ſaid principle, will float up and down in the air, at a diſtance both from one another, and from the particles of air, and make that ſubſtance we call vapour. Thus I ſuppoſe it is, when the particles of a body are very ſmall (as I ſuppoſe thoſe of water are) ſo that the action of heat alone may be ſufficient to ſhake them aſunder. But if the particles be much larger, they then require the greater force of diſſolving menſtruums, to ſeparate them, unleſs by any means the particles can be firſt broken into ſmaller ones. For the moſt fixed bodies, even gold itſelf, ſome have ſaid will become volatile, only by breaking their parts ſmaller. Thus may the volatility and fixedneſs of bodies depend on the different ſizes of their parts.

“ And on the ſame difference of ſize may depend the more or leſs permanency of aereal ſubſtances, in their ſtate of rarefaction. To underſtand this, let us ſuppoſe ABCD to be a large piece of any metal, EFGH the limit of the interior uniform æther, and K a part of the metal at the ſuperficies AB. If this part or particle K be ſo little, that it reaches not to the limit EF, it is plain, that the æther at its centre muſt be leſs rare, than if the particle were greater; for were it greater, its centre would be further from the ſuperficies AB, that is, in a place, where the æther (by ſuppoſition) is rarer. The leſs the particle K therefore, the denſer the æther at its centre, becauſe its centre comes nearer to the edge AB, where



the

" the æther is denser than within the limit EFGH. And if the particle were divided from " the body, and removed to a distance from it, where the æther is still denser, the æther " within it must proportionally grow denser. If you consider this, you may apprehend, " how by diminishing the particle, the rarity of the æther within it will be diminished, till " between the density of the æther without, and the density of the æther within it, there be " little difference; that is, till the cause be almost taken away, which should keep this and " other such particles at a distance from one another. For that cause, explained in the fourth " and fifth suppositions, was the excess of density of the external æther above that of the " internal. This may be the reason then, why the small particles of vapours easily come to- " gether, and are reduced back into water, unless the heat, which keeps them in agitation, be " so great as to dissipate them as fast as they come together: but the grosser particles of ex- " halations raised by fermentation keep their aerial form more obstinately, because the æther " within them is rarer.

" NOR does the size only, but the density of the particles also, conduce to the permanency " of aerial substances. For the excess of density of the æther without such particles above " that of the æther within them is still greater. Which has made me sometimes think, " that the true permanent air may be of a metallic original; the particles of no substances " being more dense than those of metals. This, I think, is also favoured by experience, for I " remember I once read in the Philosophical Transactions, how M. *Huygens* at *Paris* found, " that the air made by dissolving salt of tartar would in two or three days time condense " and fall down again, but the air made by dissolving a metal continued without con- " densing or relenting in the least. If you consider then, how by the continual fermentations " made in the bowels of the earth there are aerial substances raised out of all kinds of bodies, " all which together make the atmosphere, and that of all these the metallic are the most " permanent, you will not, perhaps, think it absurd, that the most permanent part of the " atmosphere, which is the true air, should be constituted of these; especially since they are " the heaviest of all other, and so must subside to the lower parts of the atmosphere, and " float upon the surface of the earth, and buoy up the lighter exhalation and vapours to float " in greatest plenty above them. Thus, I say, it ought to be with the metallic exhalations " raised in the bowels of the earth by the action of acid menstruums, and thus it is with " the true permanent air; for this, as in reason it ought to be esteemed the most pon- " derous part of the atmosphere, because the lowest, so it betrays its ponderosity, by mak- " ing vapours ascend readily in it, by sustaining mists and clouds of snow, and by buoying " up gross and ponderous smoke. The air also is the most gross unactive part of the at- " mosphere, affording living things no nourishment, if deprived of the more tender exha- " lations and spirits, that float in it: and what more unactive and remote from nourishment " than metallic bodies?

" I SHALL set down one conjecture more, which came into my mind now as I was writ- " ing this letter. It is about the cause of gravity. For this end I will suppose æther to " consist of parts differing from one another in subtilty by indefinite degrees: that in the " pores of bodies there is less of the grosser æther, in proportion to the finer, than in open " spaces; and consequently, that in the great body of the earth there is much less of the " grosser æther, in proportion to the finer, than in the regions of the air: and that yet the " grosser æther in the air affects the upper regions of the earth, and the finer æther in the " earth the lower regions of the air, in such a manner, that from the top of the air to the " surface of the earth, and again from the surface of the earth to the centre thereof, the " æther is insensibly finer and finer. Imagine now any body suspended in the air, or lying " on the earth: and the æther being by the hypothesis grosser in the pores, which are in the " upper parts of the body, than in those which are in its lower parts, and that grosser æther " being less apt to be lodged in those pores, than the finer æther below, it will endeavour to " get out and give way to the finer æther below, which cannot be without the bodies " descending to make room above for it to go out into.

" FROM this supposed gradual subtilty of the parts of æther some things above might " be further illustrated, and made more intelligible; but by what has been said, you will " easily discern, whether in these conjectures there be any degree of probability, which is all " I aim at. For my own part, I have so little fancy to things of this nature, that, had not " your encouragement moved me to it, I should never, I think, have thus far set pen to " paper about them. What is amiss therefore, I hope, you will the more easily pardon in

Cambridge, Feb. 28, 1678-9. " Your most humble servant,

" and honourer,

" ISAAC NEWTON.

THIS letter of our incomparable *Newton* may perhaps receive some illustration from another [*], which he wrote a few years before to Mr. *Oldenburg*, and was as follows.

[*] In the possession of *William Jones*, Esq.

" SIR,

" I RECEIVED both yours, and thank you for your care in disposing those things between me and Mr. *Linus*. I suppose his friends cannot blame you at all for printing his first letter, it being written, I believe, for that end, and they never complaining of the printing of that, but of the not printing that, which followed, which I take myself to have been *per accidens* the occasion of, by refusing to answer him. And though I think I may truly say, I was very little concerned about it, yet I must look upon it as the result of your kindness to me, that you was unwilling to print it without an answer.

" As to the paper of Observations, which you move in the name of the Society to have printed, I cannot but return them my hearty thanks for the kind acceptance they meet with there, and know not how to deny any thing, which they desire should be done. Only I think it will be best to suspend the printing of them for a while, because I have some thoughts of writing such another set of Observations for determining the manner of the productions of colours by the prism, which, if done at all, ought to precede that now in your hands, and will do best to be joined with it. But this I cannot do presently, by reason of some incumbrances lately put upon me by some friends, and some other business of my own, which at present almost take up my time and thoughts.

" THE additions, that I intended, I think I must, after putting you to so long expectations, disappoint you in; for it puzzles me how to connect them with what I sent you; and if I had those papers, yet I doubt the things I intended will not come in so freely as I thought they might have done. I could send them described without dependance on those papers; but I fear I have already troubled your Society and yourself too much with my scribbling, and so suppose it may do better to defer them till another season. I have therefore at present only sent you two or three alterations, though not of so great moment, that I need have staid you for them; and they are these:

" WHERE I say, that *the frame of nature may be nothing but æther condensed by a fermental principle*, instead of these words write, that it may be nothing but various contextures of some certain ætherial spirits or vapours condensed, as it were, by præcipitation, much after the manner, that vapours 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, who, by virtue of the command, *Increase and multiply*, became a complete imitator of the copies set her by the Protoplast. Thus perhaps may all things be originated from æther, *&c.*

A LITTLE after, when I say, the ætherial spirit may be *condensed in fermenting or burning bodies, or otherwise inspissated in the pores of the earth to a tender matter, which may be, as it were, the* succus nutritius *of the earth, or primary substance, out of which things generable grow:* instead of this you may write, that that spirit may be condensed in fermenting or burning bodies, or otherwise coagulated 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 vapours condense on the sides of a vessel.

" IN the same paragraph there is, I think, a parenthesis, in which I mention volatile saltpetre. Pray strike out that parenthesis, lest it should give offence to somebody.

" Also where I relate the experiment of little papers made to move variously with a glass rubbed, I would have all that struck out, which follows about trying the experiment with leaf-gold.

" SIR, I am interrupted by a visit, and so must in haste break off.

" Yours

Jan. 25, 1675-6. " IS. NEWTON."

BUT to return to Mr. *Boyle*, in the year 1680, he gave the world the following tracts, *viz. The Aerial Noctiluca: or some new Phænomena, and a process of a factitious self-shining substance*; London, in 8vo. *A new Lamp*, printed in Mr. *Hooke*'s *Philosophical Collections*, No. II. p. 33. and *Divers Experiments and Notes about the produciblenefs of chemical Principles*, subjoined to the second edition of his *Sceptical Chemist*, at *Oxford* 1680, in 8vo.

THE Royal Society, of which he had been so long one of the greatest ornaments, now thought proper at their annual election on St. *Andrew*'s day, November 30, this year, to choose him for their president. But after a mature consideration he excused himself from accepting that post, for reasons, which shew his extreme tenderness and delicacy in all matters of conscience, and were represented by him in the following letter to Mr. *Hooke*.

" *Pall-Mall*, Dec. 18, 1680.

SIR,

" THOUGH since I last saw you, I met with a lawyer, who has been a member of several parliaments, and found him of the same opinion with my council in reference to the obligation to take the test and oaths you and I discoursed of; yet not content with

" this,

Lexicon Technicum:

Or, An UNIVERSAL

English Dictionary

OF

ARTS and SCIENCES:

EXPLAINING

Not only the TERMS of ART, but the ARTS Themſelves.

VOL. II.

BY

JOHN HARRIS, D. D. Secretary to the *Royal-Society*, and Chaplain to the *Lord High-Chancellor* of GREAT-BRITAIN.

LONDON:

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INTRODUCTION.

DE
NATURA ACIDORUM.
Is. NEWTON. 1692.

Acidorum particulæ ſunt Aqueis Craſſiores, & propterea minus Volatiles, at Terreſtribus multo ſubtiliores & propterea multo minus fixæ. Vi magna Attractivâ pollent, & in hac vi conſiſtit earum Activitas, quâ & Corpora diſſolvunt & Organa Senſuum agitant & pungunt. Mediæ ſunt Naturæ inter Aquam & Corpora, & Utraque attrahunt. Per vim ſuam attractivam congregantur circum particulas corporum ſeu Lapideas ſeu Metallicas iiſq; undiq; adhærent arctiſſimè, ut ab iiſdem deinceps per Diſtillationem vel Sublimationem vix poſſint ſeparari, Attractæ vero & undique congregatæ, elevant, disjungunt & diſcutiunt particulas corporum ab invicem, id eſt corpora diſſolvunt ; & per vim Attractionis quâ ruunt in particulas commovent fluidum & ſic calorem excitant, particulaſq; nonnullas adeo diſcutiunt ut in Aerem convertant & ſic Bullas generant. Et hæc eſt Ratio Diſſolutionis & Fermentationis ; Acidum verò attrahendo Aquam æquè ac Terram efficit ut particulæ diſſolutæ prompte miſceantur cum Aquâ eique innatent ad modum ſalium. Et quem admodum Globus Terræ per vim Gravitatis attrahendo aquam fortiùs quam Corpora leviora, efficit ut leviora aſcendant in Aquâ, & fugiant de Terrâ. Sic particulæ Salium attrahendo Aquam fugant ſe mutuò & ab invicem quam maxime recedendo, per Aquam totam expanduntur.

Particulæ Salis Alkali ex Terreis & Acidis ſimilitèr Unitis conſtant ; ſed hæ Acidæ vi maxima Attractivâ pollent ut per ignem non ſeparentur à Sale ; utq; Metalla diſſoluta præcipitant attrahendo ab ipſis particulas Acidas quibus diſſolvebantur.

Si particulæ Acidæ in minori proportione cum Terreſtribus jungantur, hæ tam arcte retinentur à Terreſtribus, ut ab iis ſupprimi ac occultari videantur. Neq; enim ſenſum jam pungunt neq; attrahunt aquam, ſed corpora dulcia & quæ cum aquâ ægre miſcentur, hoc eſt pinguia, *componunt ; ut fit in Mercurio dulci, Sulphure communi, Luna* Cornea *& Cupro quod Mercurius Sublimatus corroſit. Ab Acidi vero ſic ſuppreſſi vi attractivâ fit ut pinguia Corporibus prope Univerſis adhæreant & flammam facilè concipiant, ſi modo Acidum calefactum inveniat alia Corpora in fumo accenſorum quæ fortius attrahat quam propria. Sed & Acidum in Sulphureis ſuppreſſum fortius attrahendo particulas aliorum Corporum (ſcilicet Terreas) quam proprias, Fermentationem lentam & Naturalem ciet & fovet uſq; ad Putrefactionem Compoſiti.*

Quæ Putrefactio ſita eſt in eo quod Acidæ Fermentationem diu foventes tandem in interſtitia minima & primæ Compoſitionis *partes interjacentia ſeſe inſinuant, intiméq; iis partibus Unitæ mixtionem Novam efficiunt non amovendam nec cum priore commutandam.*

Cogitationes Variæ ejuſdem.

Flamma eſt Fumus Candens ; differtque à Fumo ut Ferrum rubens ab ignito ſed non rubente.

Calor eſt Agitatio Partium quaqua verſum.

Nihil eſt abſolutè quieſcens ſecundum partes ſuas & ideo frigidum, præter atomos, vacui ſcilicet expertes.

Terra augetur, Aquâ in eam converſâ, & omnia in aquam [vi ignis] reduci poſſunt.

Nitrum abit diſtillatione magnam partem in Spiritum Acidum, relictâ terrâ, quia Acidum Nitri attrahit Phlegma ; & idcirco ſimul aſcendunt conſtituuntq; Spiritum : at Nitrum Carbone accenſum magnam partem abit in Sal Tartari, quia ignis eo modo applicatus partes Acidi & Terræ in ſeſe impingit fortiuſq; unit.

Spiritus ardentes ſunt Olea cum Phlegmate per Fermentationem Unita.

Tinctura Cochinellæ cum Spiritu Vini facta in aquæ magnam molem immiſſa, parva licet doſi, totam aquam inficit : Sc. quia particulæ Cochinellæ magis attrahuntur ab aquâ quam a ſe mutuo.

Aqua non habet magnum vim diſſolvendi quia pauco Acido gaudet. Acidum enim dicimus quod multum attrahit & attrahitur, videmus nempe ea quæ in aquâ ſolvuntur lente & ſine Effervescentiâ ſolvi, at ubi eſt attractio fortis & particulæ menſtrui undiq; attrahuntur à particulâ Metalli, vel potius particula metalli undiq; attrahitur a particulis menſtrui, hæ illam abripiunt & circumſiſtunt, hoc eſt Metallum corrodunt : Hæ eadem particulæ ſenſorio applicatæ ejus partes eodem modo divellunt doloremq; inferunt ; à quo Acidæ appellantur, relictâ ſcilicet terrâ Subtili cui adhærebant ob majorem attractionem ad liquidum linguæ, &c.

In

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In omni Solutione per Menstruum particulæ solvendæ magis attrahuntur a partibus Menstrui quam à se mutuo.

In omni Fermentatione est Acidum suppressum quod coagulat præcipitando.

Oleum cum nimis magnâ mole phlegmatis intime mixtum, fit Salinum quiddam & sic Acetum constituit, hic etiam Tartari seu Terræ admistæ habenda est ratio.

Mercurius attrahitur id est corroditur ab Acidis & sicut pondere Obstructiones tollit ita vi attractrice Acida infringit.

Mercurius est Volatilis & facile elevatur calore quia ejus particulæ ultimæ Compositionis *sunt parvæ & facile separantur separatæq; sese fugant; ut fit in particulis Vaporis, fluidorumq; rarefactorum.*

Aqua comprimi non potest quia ejus particulæ jamjam se tangunt. Et si se tangerent particulæ Aeris (nam Aer comprimi potest, quia ipsius particulæ nondum se tangunt) Aer evaderet in Marmor. Seq. ex Prop. 23. Lib. 2. Princ. Philosoph.

Aurum particulas habet se mutuo trahentes; minimarum summæ vocentur primæ Compositionis, *harum summarum summæ* secundæ Compositionis, *&c.*

Potest Mercurius, potest Aqua Regia poros pervadere, qui particulas ultimæ Compositionis *interjacent at non alios.*

Si posset Menstruum alios illos pervadere vel si auri partes primæ & secundæ Compositionis *possent separari fieret Aurum, vel Fluidum, vel saltem magis malleabile. Si Aurum fermentescere posset in aliud quodvis corpus posset transformari.*

Viscid tas est vel solum defectus fluiditatis, quæ sita est in partium parvitate & separabilitate (intellige partes ultimæ Compositionis) vel defectus lubricitatis seu lævioris partes unius supra alias labi impediens. Hujus viscididatis Acidum sæpe causa est; sæpe Spiritus alius lubricus, terræ junctus, ut oleum Terebinthinæ capiti suo Mortuo redditum fit tenax.

Ratio cur Charta Oleo in uncta Transitum Oleo non Aquæ concedat est quia Aqua Oleo non miscetur sed fugatur ab eo.

Cum Acidæ partes, minores scilicet, aliquid dissolvunt, id faciunt, quia partem rei solvendæ includunt undiq; utpote Majorem quâlibet Acidi partium.

Some Thoughts about the NATURE *of* ACIDS; *By Sir* ISAAC NEWTON.

THE Particles of Acids are of a Size grosser than those of Water, and therefore less volatile; but much smaller than those of Earth, and therefore much less fix'd than they. They are endued with a great Attractive Force; in which Force their Activity consists; and thereby also they affect and stimulate the Organ of Taste, and dissolve such Bodies as they can come at. They are of a middle Nature between Water and Terrestrial Bodies, and attract the Particles of both.

By this Attractive Force they get about the Particles of Bodies, whether they be of a metallick or stony Nature, and adhere to them most closely on all sides; so that they can scarce be separated from them by Distillation or Sublimation. When they are attracted and gather'd together about the Particles of Bodies, they raise, disjoyn and shake them one from another; that is, they dissolve those Bodies.

By their Attractive Force also, by which they rush towards the Particles of Bodies, they move the Fluid, and excite Heat; and they shake asunder some Particles, so much as to turn them into Air, and generate Bubbles: And this is the Reason of Dissolution, and all violent Fermentation; and in all Fermentation there is an Acid latent or suppress'd, which coagulates in Precipitation.

Acids also, by attracting Water as much as they do the Particles of Bodies, occasion that the dissolved Particles do readily mingle with Water, or swim or float in it, after the manner of Salts.

And as this Globe of Earth, by the Force of Gravity, attracting Water more strongly than it doth lighter Bodies, causes those lighter Bodies to ascend in the Water, and to go upwards from the Earth: So the Particles of Salts, by attracting the Water, do mutually avoid and recede from one another as far as they can, and so are diffused throughout the whole Water.

The Particles of *Sal Alkali*, do consist of *Earthy* and *Acid* united together, after the same manner: But these Acids have so great an Attractive Force, that they can't be separated from the Salt by Fire; they do also precipitate the Particles of Metals dissolv'd

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diſſolv'd in *Menſtrua*, by attracting from them the Acid Particles, which before had diſſolved them, and kept them ſuſpended in the *Menſtruum*.

If theſe Acid Particles be joyn'd with Earthy ones, in but a ſmall Quantity, they are ſo cloſely retain'd by them, as to be quite ſuppreſs'd and hidden as it were by them; ſo that they neither ſtimulate the Organ of Senſe, nor attract Water, but compoſe Bodies which are not Acid, *i. e.* Fat and Fuſible Bodies, ſuch as are *Mercurius dulcis*, *Common Brimſtone*, *Luna Cornea*, and *Copper* corroded by *Mercury Sublimate*.

From the Attractive Force in theſe Acid Particles thus ſuppreſs'd, ariſes that univerſal Property of almoſt all Fat Bodies, that they adhere or ſtick to others, and are eaſily inflammable, if the heated Acid Particles meet with other Particles of Bodies in Fume, which the Acid attracts more ſtrongly, than it doth the Particles to which it is united. And thus the Acid that lies ſuppreſs'd in ſulphureous Bodies, by more ſtrongly attracting the Particles of other Bodies (Earthy ones for Inſtance) than its own, promotes a gentle Fermentation, produces and cheriſhes Natural Heat, and carries it on ſo far ſometimes, as to the Putrefaction of the Compound: Which Putrefaction ariſes hence, That the Acid Particles which have a long while kept up the Fermentation, do at long run inſinuate themſelves into the leaſt Interſtices that lie between the Particles of the *firſt Compoſition*, and ſo intimately uniting with thoſe very Particles do produce a new Mixture or Compound, which cannot fall back again into the ſame Form.

Note, *The Paper hitherto deſcrib'd, ſeems to have been a continued Diſcourſe; but what follows are ſhort Minutes of Thoughts relating to the ſame Subject.*

Nitre, in Diſtillation, leaving its Earthy Part behind, turns moſt of it into an Acid Spirit; becauſe the Acid of the Nitre attracts the Phlegm, and therefore they aſcend together, and conſtitute a Spirit. But Nitre, kindled with a Coal, turns chiefly into a Salt of Tartar; becauſe the Fire applied this Way, drives the Acid and Earthy Parts towards, and makes them impinge on, and more ſtrongly unite one with another.

The Reaſon why Water hath no great diſſolving Force, is, becauſe there is but a ſmall Quantity of Acid in it: For whatever doth ſtrongly attract, and is ſtrongly attracted, may be call'd an Acid: And ſuch things as are diſſolv'd in Water, we ſee, become ſo, *eaſily*, without any Efferveſcence: But where the Attraction is ſtrong, and the Particles of the Menſtruum are every where attracted by thoſe of the Metal, or rather, where the Particles of the Metal are every way attracted by thoſe of the Menſtruum; then the Particles of the Menſtruum environ thoſe of the Metal, tear them to pieces, and diſſolve it.

So when theſe Acid Particles are applied to the Tongue, or to any excoriated Part of the Body, leaving the ſubtile Earth in which they were before, they ruſh into the liquid of the Senſory, tear and disjoint its Parts, and cauſe a painful Senſation.

Mercury is attracted, and therefore corroded by Acids; and as it opens Obſtructions by its great Weight; ſo it breaks and obtunds the Power of Acids (in the Body) by its attractive Force.

All Bodies have Particles which do mutually attract one another: The Summs of the leaſt of which may be called Particles of the *firſt Compoſition*, and the Collections or Aggregates ariſing from the, Primary Summs; or the Summs of theſe Summs may be call'd Particles of the *ſecond Compoſition*, *&c.*

Mercury and *Aqua Regis* can pervade thoſe Pores of Gold or Tin, which lie between the Particles of *its laſt Compoſition*; but they can't get any further into it; for if any Menſtruum could do that, or if the Particles of the firſt, or perhaps of the ſecond Compoſition of Gold could be ſeparated; that Metal might be made to become a Fluid, or at leaſt more ſoft. And if Gold could be brought once to ferment and putrefie, it might be turn'd into any other Body whatſoever.

And ſo of Tin, or any other Bodies; as common Nouriſhment is turn'd into the Bodies of Animals and Vegetables.

N. B. *The ſmall Difference which there is between this Tranſlation and the* Latin *above, was its being taken from another Copy a little different from this* Latin *Paper. And having been ſupervis'd and approved of by the Illuſtrious Author, I have not alter'd it ſince.*

VII. *Scala graduum Caloris.*

Calorum Descriptiones & signa.

		Calor aeris hyberni ubi aqua incipit gelu rigescere. Innotescit hic calor accurate locando Thermometrum in nive compressa quo tempore gelu solvitur.
0,1,2,		Calores aeris hyberni.
2,3,4.		Calores aeris verni & autumnalis.
4,5,6.		Calores aeris æstivi.
6		Calor aeris meridiani circa mensem Julium.
12	1	Calor maximus quem Thermometer ad contactum

		tactum corporis humani concipit. Idem circiter eſt calor avis ova incubantis.
14 $\frac{3}{11}$	1 $\frac{1}{4}$	Calor balnei prope maximus quem quis manu immerſa & conſtanter agitata diutius perferre poteſt. Idem fere eſt calor ſanguinis recens effuſi.
17	1 $\frac{1}{2}$	Calor balnei maximus quem quis manu immerſa & immobili manente diutius perferre poteſt.
20 $\frac{2}{11}$	1 $\frac{3}{4}$	Calor balnei quo cera innatans & liquefacta deferendo regiſcit & diaphaneitatem amittit.
24	2	Calor balnei quo cera innatans incaleſcendo, liqueſcit & in continuo fluxu ſine ebullitione conſervatur.
28 $\frac{6}{11}$	2 $\frac{1}{4}$	Calor mediocris inter calores quo cera liqueſcit & aqua ebullit.
34	2 $\frac{1}{2}$	Calor quo aqua vehementer ebullit & miſtura duarum partium plumbi trium partium ſtanni & quinque partium biſmuti defervendo rigeſcit. Incipit aqua ebullire calore partium 33 & calorem partium pluſquam 34 $\frac{1}{2}$ ebulliendo vix concipit. Ferrum verò deferveſcens calore partium 35 vel 36, ubi aqua calida & 37 ubi frigida in ipſum guttatim incidit, deſinit ebullitionem excitare.
40 $\frac{4}{11}$	2 $\frac{3}{4}$	Calor minimus quo miſtura unius partis Plumbi quatuor partium Stanni & quinque partium Biſmuti incaleſcendo liqueſcit, & in continuo fluxu conſervatur.
48	3	Calor minimus quo miſtura æqualium partium ſtanni & biſmuti liqueſcit. Hæc miſtura calore partium 47 defervendo coagulatur.
57	3 $\frac{1}{4}$	Calor quo miſtura duarum partium ſtanni & unius partis biſmuti funditur, ut & miſtura trium partium ſtanni & duarum plumbi ſed miſtura quinq; partium ſtanni & duarum

		partium biſmuti hoc calore deſervendo rigeſcit. Et idem facit miſtura æqualium partium plumbi & biſmuti.
68	$3\frac{1}{2}$	Calor minimus quo miſtura unius partis biſmuti & octo partium ſtanni funditur. Stannum per ſe funditur calore partium 72 & Deſervendo rigeſcit calore partium 70.
81	$3\frac{3}{4}$	Calor quo biſmutum funditur ut & miſtura quatuor partium plumbi & unius partis ſtanni. Sed miſtura quinque partium plumbi & unius partis ſtanni ubi fuſa eſt & deſervet in hoc calore rigeſcit.
96	4	Calor minimus quo plumbum funditur. Plumbum incaleſcendo funditur calore partium 96 vel 97 & deſervendo rigeſcit calore partium 95.
114	$4\frac{1}{4}$	Calor quo corpora ignita deſervendo penitus deſinunt in tenebris nocturnis lucere, & viciſſim incaleſcendo incipiunt in iiſdem tenebris lucere ſed luce tenuiſſima quæ ſentiri vix poſſit. Hoc calore liqueſcit miſtura æqualium partium Stanni & Reguli martis, & miſtura ſeptem partium biſmuti & quatuor partium ejuſdem Reguli deſervendo rigeſcit.
136	$4\frac{1}{2}$	Calor quo corpora ignita in tenebris nocturnis candent, in crepuſculo vero neutiquam. Hoc calore tum miſtura duarum partium reguli martis & unius partis Biſmuti tum etiam miſtura quinq; partium reguli martis & unius partis Stanni deſervendo rigeſcit. Regulus per ſe rigeſcit calore partium 146.
161	$4\frac{3}{4}$	Calor quo corpora ignita in crepuſculo proxime ante ortum ſolis vel poſt occaſum ejus manifeſto candent in clara vero diei luce neutiquam, aut non niſi perobſcure.

Calor

(827)

192	5	Calor prunarum in igne parvo culinari ex carbonibus fossilibus bituminosis constructo & absq; usu follium ardente. Idem est calor ferri in tali igne quantum potest candentis. Ignis parvi culinaris qui ex lignis constat calor paulo major est nempe partium 200 vel 210. Et ignis magni major adhuc est calor, præsertim si follibus cieatur.

In hujus Tabulæ columna prima habentur gradus caloris in proportione arithmetica computum inchoando a calore quo aqua incipit gelu rigescere tanquam ab infimo caloris gradu seu commune termino caloris & frigoris, & ponendo calorem externum corporis humani esse partium duodecim. In secunda columna habentur gradus caloris in ratione geometrica sic ut secundus gradus sit duplo major primo, tertius item secundo & quartus tertio, & primus sit calor externus corporis humani sensibus æquatus. Patet autem per hancTabulam quod calor aquæ bullientis sit fere triplo major quam calor corporis humani, & quod calor stanni liquescentis sit sextuplo major & calor plumbi liquescentis octuplo major & calor Reguli liquescentis duodecuplo major & calor ordinarius ignis culinaris sexdecim vel septemdecim vicibus major quam calor idem corporis humani.

Constructa fuit hæc Tabula ope Thermometri & ferri candentis. Per Thermometrum inveni mensuram calorum omnium usq; ad calorem quo stannum funditur & per ferrum calefactum inveni mensuram reliquorum. Nam calor quem ferrum calefactum corporibus frigidis sibi contiguis dato tempore communicat, hoc est calor quem ferrum dato tempore amittit est ut calor totus ferri. Ideoq; si tempora refrigerii sumantur æqualia calores erunt in ratione geometrica, & propterea per tabulam logarithmorum facile inveniri possunt.

Primum igitur per Thermometrum ex oleo lini constructum inveni quod si oleum ubi Thermometer in nive liquescente locabatur occupabat spatium partium 10000,

idem

idem oleum calore primi gradus ſeu corporis humani rarefactum occupabat ſpatium 10256 & calore aquæ jamjam ebullire incipientis ſpatium 10705 & calore aquæ vehementer ebullientis ſpatium 10725 & calore ſtanni liquefacti de fervientis ubi incipit rigeſcere & conſiſtentiam amalgamentis induere ſpatium 11516 & ubi omnino rigeſcit ſpatium 11496. Igitur oleum rarefactum fuit ac dilatatum in ratione 40 ad 39 per calorem corporis humani, in ratione 15 ad 14 per calorem aquæ bullientis, in ratione 15 ad 13 per calorem ſtanni defervientis ubi incipit coagulari & rigeſcere & in ratione 23 ad 20 per calorem quo ſtannum deferviens omnio rigeſcit. Rarefactio aeris æquali calore fuit decuplo major quam rarefactio olei, & rarefactio olei quaſi quindecim vicibus major quam rarefactio ſpiritus vini. Et ex his inventis ponendo calores olei ipſius rarefactioni proportionales & pro calore corporis humani ſcribendo partes 12 prodijt calor aquæ ubi incipit ebullire partium 33 & ubi vehementius ebullit partium 34; & calor ſtanni ubi vel liqueſcit vel deferviendo incipit rigeſcere & conſiſtentiam amalgamatis induere prodijt partium 72, & ubi defervendo rigeſcit & induratur partium 70.

His cognitis ut reliqua inveſtigarem calefeci ferrum ſatis craſſum donec ſatis canderet & ex igne cum forcipe etiam candente exemptum locavi ſtatim in loco frigido ubi ventus conſtanter ſpirabat & huic imponendo particulas diverſorum metallorum & aliorum corporum liquabilium notavi tempora refrigerij donec particulæ omnes amiſſa fluiditate rigeſcerent & calor ferri æquaretur calori corporis humani. Deinde ponendo quod exceſſus calorum ferri & particularum rigeſcentium ſupra calorem atmoſphæræ Thermometro inventum eſſent in progreſſione geometrica ubi tempora ſunt in progreſſione Arithmetica, calores omnes innotuere. Locavi autem ferrum, non in aere tranquillo ſed in vento uniformiter ſpirante ut aer a ferro calefactus ſemper abriperetur a vento & aer frigidus in locum ejus uniformi cum motu ſuccederet. Sic enim aeris partes æquales æqualibus temporibus calefactæ ſunt & calorem conceperunt calori ferri proportionalem. Ca-

(829)

Calores autem sic inventi eandem habuerunt rationem inter se cum caloribus per Thermometrum inventis & propterea rarefactiones olei ipsius caloribus proportionales esse recte assumpsimus.

A Scale of the Degrees of Heat. N° 270, p. 824. *Translated from the Latin.*

0 .. 0 .. The heat of the air in winter, when the water begins to freeze; and it is discovered exactly by placing the thermometer in compressed snow, when it begins to thaw.

0,1,2 .. 0 .. The heat of the air in winter.

2,3,4 .. 0 .. The same in spring and autumn.

4,5,6 .. 0 .. The same in summer.

6 .. 0 .. Heat of the air at noon about the month of July.

12 .. 1 .. Greatest heat the thermometer received on the contact of a man's body, as also that of a bird hatching her eggs.

$14\frac{3}{11}$ .. $1\frac{1}{4}$ .. Almost the greatest heat of a bath, which a man can bear by moving his hand in it for some time; also that of blood newly drawn.

17 .. $1\frac{1}{2}$ .. Greatest degree of heat of a bath, which a man can bear for some time without stirring his hand in it.

$20\frac{2}{11}$ .. $1\frac{3}{4}$ .. Heat of a bath, by which melted wax swimming on it by cooling hardens and loses its transparency.

24 .. 2 .. Heat of a bath, by which wax swimming on it is melted by growing hot, and kept in continual fusion without ebullition.

$28\frac{6}{11}$ .. $2\frac{1}{4}$ .. Mean heat between that by which wax melts and water boils.

34 .. $2\frac{1}{2}$ .. Heat by which water has a strong ebullition, and a mixture of two parts of lead, three of tin, and five of bismuth, by cooling hardens; water begins to boil with a degree of heat of 33 parts, and by boiling scarcely acquires any greater degree than that of $34\frac{1}{2}$; but iron growing cold with the heat of 35 or 36 parts, when hot water, and 37, when cold water is dropped on it, ceases to cause any ebullition.

$40\frac{4}{11}$ .. $2\frac{3}{4}$ .. Least degree of heat by which a mixture of one part of lead, four parts of tin, and five parts of bismuth, by growing hot is melted and kept in continual fusion.

48 .. 3 .. Least degree of heat, by which a mixture of equal parts of tin and bismuth is melted; this mixture with the heat of 47 parts, by cooling coagulates.

57 .. $3\frac{1}{4}$ .. Degree of heat, by which a mixture of two parts of tin and one part of bismuth is melted, as also a mixture of three parts of tin and two of lead; but a mixture of five parts of tin and two of bismuth, with this degree of heat, by cooling hardens, and in like manner a mixture of equal parts of lead and bismuth.

68 .. $3\frac{1}{2}$ .. Least degree of heat, that melts a mixture of one part of bismuth and eight parts of tin; tin by itself is put into fusion with the heat of 72 parts, and by cooling hardens with the heat of 70 parts.

81 .. $3\frac{3}{4}$ .. Degree of heat that melts bismuth, as also a mixture of four parts of lead and one part of tin; but a mixture of five parts of lead and one part of tin, when melted, and cooling again, it hardens with this heat.

96 .. 4 .. Least degree of heat that melts lead; lead, by growing hot, is melted with the heat of 96 or 97 parts, and cooling it hardens with 95 parts.

114 .. $4\frac{1}{4}$ .. Degree of heat, by which ignited bodies in cooling quite cease to shine by night, and again, by growing hot begin to shine in the dark, but with a very faint light, which is scarcely perceptible; in such a degree of heat there melts a mixture of equal parts of tin and regulus martis, and a mixture of seven parts of bismuth and four parts of the said regulus by cooling hardens.

6

136 .. $4\frac{1}{4}$.. Degree of heat with which ignited bodies glow by night, but not at all in the twilight, and with this degree of heat both a mixture of two parts of regulus martis and one part of bismuth, as also a mixture of five parts of the said regulus and one part of tin, by cooling hardens; the regulus by itself hardens with the heat of 146 parts.

161 .. $4\frac{3}{4}$.. Degree of heat, by which ignited bodies manifestly glow in the twilight immediately preceding the rising of the sun, or after his setting, but not at all in a clear day, or but very faintly.

192 .. 5 .. Degree of heat of live coals in a small kitchen fire, made up of bituminous pit-coals, and that burn without using bellows; as also, the heat of iron made as hot as it can be in such a fire; the degree of heat of a small kitchen fire made of faggots is somewhat greater, viz. 200 or 210 parts, and that of a large fire is still greater, especially if blown with bellows.

In the first column of this table are the degrees of heat in arithmetical proportion, beginning with that which water has when it begins to freeze, being as it were the lowest degree of heat, or the common boundary between heat and cold; and supposing that the external heat of the human body is 12 parts. In the second column are set down the degrees of heat in geometrical proportion, so that the second degree is double the first, the third double the second, and the fourth double the third; and making the first degree the external heat of the human body in its natural state. It appears by this table, that the heat of boiling water is almost 3 times that of the human body, of melted tin 6 times, of melted lead 8 times, of melted regulus 12 times, and the heat of an ordinary kitchen fire is 16 or 17 times greater than that of the human body.

This table was constructed by means of the thermometer and red-hot iron. By the thermometer were found all the degrees of heat, down to that which melted tin; and by the hot iron were discovered all the other degrees; for the heat which hot iron, in a determinate time, communicates to cold bodies near it, that is, the heat which the iron loses in a certain time, is as the whole heat of the iron; and therefore, if equal times of cooling be taken, the degrees of heat will be in geometrical proportion, and therefore easily found by the tables of logarithms. First it was found by the thermometer with linseed oil, that if, when it was placed in melted snow, the oil possessed the space of 10000 parts; then the same oil rarefied with the heat of the first degree, or that of a human body, possessed the space of 10256 parts, with the heat of water just beginning to boil, the space of 10705; with that of water strongly boiling, the space of 10725 parts; with that of melted tin, beginning to cool, and to be of the consistence of an amalgama, the space of 11516; and when it is quite hardened the space of 11496; therefore the rarefied oil was to the same expanded by the heat of the human body, as 40 is to 39; by that of boiling water, as 15 to 14; by that of tin beginning to cool, coagulate, and harden, as 15 to 13; and by the heat of cooling tin when quite hardened, as 23 is to 20; the rarefaction of air by an equal heat was 10 times greater than that of oil, and the rarefaction of oil was 15 times greater than that of spirits of wine. From these data, putting the degrees of the heat of the oil proportional to its rarefaction, and taking 12 parts for the heat of the human body, we then have the degree of the heat of water when it begins to boil, viz. 33 parts, and when it boils more vehemently 34; of tin when melted, or when it begins in cooling to harden, and have the consistence of an amalgama, 72 parts, and in cooling is quite hard, 70 parts.

Having discovered these things; in order to investigate the rest, there was heated a pretty thick piece of iron red-hot, which was taken out of the fire with a pair of pincers, which were also red-hot, and laid in a cold place, where the wind blew continually upon it, and putting on it particles of several metals, and other fusible bodies, the time of its cooling was marked, till all the particles were hardened, and the heat of the iron was equal to the heat of the human body; then supposing that the excess of the degrees of the heat of the iron, and the particles above the heat of the atmosphere, found by the thermometer, were in geometrical progression, when the times are in an arithmetical progression, the several degrees of heat were discovered; the iron was laid not in a calm air, but in a wind that blew uniformly upon it, that the air heated by the iron might be always carried off by the wind, and the cold air succeed it alternately; for thus equal parts of air were heated in equal times, and received a degree of heat proportional to the heat of the iron; the several degrees of heat thus found had the same ratio among themselves with those found by the thermometer: and therefore the rarefactions of the oil were properly assumed proportional to its degrees of heat.*

* A method if not more accurate, at least more expeditious than the above, of measuring high degrees of heat, was invented some years ago by the late Mr. Wedgewood; founded on the property which argillaceous earth possesses, of contracting its dimensions when placed in the fire. See Phil. Trans. Vols. 72, 74, and 76.

IV.

Newton's Four Letters to Bentley, and the Boyle Lectures Related to Them

Bentley and Newton

PERRY MILLER

Richard Bentley was born in 1662 in a family of substantial Yorkshire yeomen.[1] He achieved fame (and left an impress on British scholarship that still is felt) as a classical scholar of prodigious erudition, and also a certain infamy as the tempestuous Master of Trinity College, Cambridge, which he ruled from 1700 until his death in 1742 with so tyrannical a hand that he excited repeated insurrections of the Fellows. He was a massive philologist, who found the supreme felicity of life in the emendation of a corrupted text or in the exposure of a forgery. He made a sensation among the learned in 1699 by demonstrating that a body of letters long attributed to a Sicilian tyrant of the 6th century B.C., named Phalaris, was a fabrication made some five or ten centuries later. These epistles had been publicly admired by gentlemen such as Sir

[1] See Bishop James Henry Monk, *The Life of Richard Bentley, with an account of his writings* (London, 1830); R. C. Jebb, *Bentley* (New York and London, 1901 [1882]: "English Men of Letters," edited by John Morley); Rev. Alexander Dyce, ed., *The Works of Richard Bentley* (3 vols., London, 1836–1838). Material concerning Bentley is also to be found in the standard literature on Newton; see, especially, Edleston's volume of Newton-Cotes correspondence and Brewster's two-volume biography of Newton.

William Temple who believed that the writers of antiquity were far superior to all moderns, including Shakespeare and Milton. By showing the letters to be spurious, Bentley impeached both the acumen and the taste of these "ancients." The greatest classicist of his time thus appeared a barbarous and ruthless modernist, and so was furiously attacked in a squib called *The Battle of the Books,* written by an erstwhile secretary of Sir William Temple, one Jonathan Swift.

In 1691, Bentley, having taken his degree at St. John's College, was chaplain to Bishop Stillingfleet of Worcester, a leader of liberal theologizing, who early said of Bentley that "had he but the gift of humility, he would be the most extraordinary man in Europe." On December 30 died Robert Boyle, a great physicist and chemist, a gentleman, and one who devoutly believed the new science to be a bulwark against the "atheism" so widely affected during the Restoration by the wits of the taverns and coffeehouses. He left funds sufficient to yield £ 50 a year for endowing an annual lectureship of eight discourses on the evidences of Christianity. There were four trustees, one of whom was John Evelyn; another was Bishop Tenison of Lincoln, who had encountered and appraised the chaplain of his colleague in Worcester. The trustees took what seemed a long chance, and nominated Bentley. He threw himself into the challenge with the same energy he expended upon Greek manuscripts or in opposing dons.

The principal source of the atheism Bentley had to counteract was Thomas Hobbes, who had been under fire from the pious and the orthodox for forty years. Platonists like Ralph Cudworth had belabored him with preëxistent ideas, Richard Cumberland with inherent moral law, and ecclesiastical authoritarians, most notably John Dryden, with general abuse. But, so far, it seemed to the guardians of Christianity that the tide of atheism had not been checked; clearly a new method was required. Bentley was exactly the man for the occasion, because he was one of the first to grasp the importance of a book published in 1690 by John Locke, *An Essay Concerning Human Understanding;* Bentley saw at once that thereafter nobody in the age would give credence to the notion of an innate idea. If his *a Confutation of Atheism* was really going to confute, it would need *proofs.* Bentley was the sort of bulldog who, ordered to find proofs, would bring back dozens of them between his jaws.

A mind that operated in this fashion would already have been thinking that if theological propositions were now to rest their defense exclusively on demonstrations satisfactory to reason, the defender would have to know something about a book that Isaac Newton of Trinity College had published in 1687, the *Principia.* So far, it appeared, few if any were able to understand it, and many said it was nonsense, but Bentley had to see for himself. However, he was a linguist and a literary scholar, and needed help; in the summer of 1691, before the lectureship was instituted, he had asked a Scottish mathematician, John Craige, to tell him what books he would need to master in order to qualify himself for following the *Principia.* Craige sent back, as an essential minimum, a bibliography so tremendous that even a Bentley was aghast; but characteristically he began to look about for short cuts, and, taking his courage in his hands, addressed Newton directly. From Trinity College came a much shorter list, encouraging directions, and apparently full sympathy. "At the first perusal of my Book," said Newton, "it's enough if you understand the propositions with some of the demonstrations which are easier than the rest." He thought Bentley should read the first sixty pages, then skip to the third book and get the design of that; then he might at leisure go back to such propositions as he had a desire to know. With the task thus cut down to manageable proportions, Bentley rapidly comprehended (so he thought) the whole design. When the call came, he was ready. He devoted the first six of his Boyle Lectures to proving the existence of God from such data as the faculties of the human soul and the structure of the body, but he triumphantly expounded in the last two (reprinted below) the new, difficult, and mathematically irrefutable physics. His success was immense, and in the opinion of many (including Bentley himself), *A Confutation of Atheism* so routed the atheists that they did not dare any longer show their faces openly, and so took refuge in the pretense of "deism."

The two sermons are important in the history of Western thought not only because they were the first popular attempt to lay open the "sublime discoveries" of Newton, but because they set the precedent for the entire Enlightenment. So far, neither the infidel nor the believer had been able to cope with the new wisdom; Bentley seized the initiative, and gave believers the assurance (or perhaps one should say the illusion) that the Newtonian physics, by conclusively showing that the order of the universe could not have been produced mechanically, was now the chief support of faith. Whether employed by Christians or deists, Bentley's technique for deducing religious propositions out of the equations of the *Principia* became an indispensable ingredient in the whole complex of 18th-century optimism.

But, for our purposes, the sermons are still more important because, whatever their merits as expositions of the system, they called forth from the great man himself four letters which are major declarations in modern history of the method and of the mentality of the scientist. While the manuscript was being printed, Bentley found himself worried for fear he had not sufficiently disposed of the theory of Lucretius (from whom Hobbes derived) that the cosmic system began with chance bumpings together of descending atoms, each endowed with an innate power of gravity. He wrote to Newton for further clarification, so that he could make last-minute changes in his proof. It took Newton four letters, from December 10 to February 25, to set Bentley straight (in fact, we may wonder whether Bentley fully got the point!), and Bentley appreciated their importance. He carefully preserved them, so that his executor could publish them in 1756. Dr. Johnson, observing that the questions had caused Newton to think out further consequences of his principles than he had yet anticipated, said of them about the finest thing that can be said, that they show "how even the mind of Newton gains ground gradually upon darkness."

The sermons show that Bentley had indeed perceived the general thesis, though the letters suggest that in the printed form Bentley made it more precise than he had done in the pulpit. This is the argument that, had gravity been the only force active at the moment of creation, the planets of our system would have fallen quickly into the sun. Hence must be assumed a specific intervention of force (only a divine force would do) which arrested the descents at the appropriate places and sent the planets spinning on their transverse orbits. Likewise, when one considers the spacing of these orbits, no principle of science will determine the relations of the distances except that "The Author of the system thought it convenient." Bentley seemed to Newton on the right track insofar as he argued that the operations of gravity over empty spaces could mean only that an "agent" was constantly guiding the stars and planets according to certain laws. Assuredly, this agent must have a volition, and must be "very well skilled in mechanics and geometry." Bentley was eager to call the agent God; Newton had no objection.

But evidently, either in the first draft of the sermons or in a letter, Bentley said something which implied that gravity was in some sense an inherent property of matter, implicit in the very substance, a sort of "occult quality," or a kind of eternal magnetism. The vehemence with which Newton rejects any such opinion is striking. Between the letters numbered II and III in this printing, Bentley wrote back a worried answer: he was so fully aware that in Newton's system universal gravitation could never be solved "mechanically" that he was surprised to have Newton warn him against the heresy. "If I used that word, it was only for brevity's sake." Well, brevity to a philologist might be one thing, but another to Newton. He wanted language exact, and certainly in the printed version Bentley took care that not even for brevity's sake should there be any suggestion that gravity is synonymous with material existence. Thus corrected, Bentley was able to conclude that mutual gravitation can operate at a distance only because it is simultaneously regulated by the "agent" and not by the system itself; here then was what he and the age most wanted, "a new and invincible argument for the being of God." From this point the sailing was clear, and Bentley goes ahead like a ship in full rig, to the joyous conclusion that everything concerning this system and particularly this globe, including the inclination of its axis and the irregular distribution of land and ocean, has been appointed for the best by a divine intelligence.

The letters show that Newton wanted to be helpful, and he was eager that Bentley should not misrepresent him; yet they are not prolix, they do not volunteer anything beyond replies to particular questions, and the careful reader does not get the impression of an outgoing enthusiasm Newton was human enough to be eager for fame and almost pathologically jealous for his reputation; but he was shrewd enough to be able to utilize Bentley without being taken in by him. For years after the Boyle Lectures, Bentley made a public parade of his friendship with Newton, and took upon himself the office of urging a second edition of the *Principia.* When Newton at last consented in 1708 to allow Roger Cotes, Fellow of Trinity College (in whom Newton did have confidence), to prepare the text, Bentley officiously acted as middleman—and pocketed the profits! John Conduitt records that he was disgusted, and asked Newton point-blank why he let Bentley "print his *Principia*" when Bentley obviously did not understand it. "Why," replied the lordly Newton, "he was covetous, and I let him do it to get the money."

In the light of this revelation we may wonder what, back in February of 1693, Bentley made, if anything, of the extraordinary clause in the third letter, where Newton says that whether the agent who is the cause of gravity "be material or immaterial, I have left to the consideration of my readers." This hardly seems the tone of one who has joined a crusade against materialistic atheism! But still more startling is the sentence that comes in the next paragraph, where Newton shows Bentley that mathematically speaking he is entirely at sea in handling the concept of the infinite, and briefly informs him that, even though the mathematical language may seem to common sense an impropriety of speech, still "those things which men understand by improper and contradictious phrases may be sometimes really in nature without any contradiction at all." There is no suggestion in Bentley's two sermons that he had even a dim sense of what Newton tried in this passage to convey to him. For Bentley, the Newtonian system was clear, rational, simple; it could be translated at once and throughout into declarative affirmations of natural theology. That was its beauty and its utility. That there was any incongruity between the process of the human mind and those of the universe would henceforth be unthinkable. Newton had, Bentley was assured, linked them indissolubly.

This conviction, as I have said, became the major premise of the Age of Reason. Bentley's tactics were taken over by Voltaire in 1738 when he conquered the mind of the Continent by popularizing Newton. Actually, the assumption remained undisturbed—or indeed strengthened—by that revolution in sensibility which we call the Romantic movement. Even after the character of reason had been radically transformed from Bentley's solid prose to the inward intuitions of the poet, the assumption that there is a perfect "correspondence" between the structures of the psyche and those of physics endured. Emerson summarized the Romantic optimism by declaring that the laws of nature answer to those of mind as image in the mirror. Only recently, and mainly in our own distracted time, has science freed itself from the literary incubus that Bentley fastened upon it. But we should find this worth meditating upon, that Newton explicitly warned him that what men are apt to consider self-contradictory may, nevertheless, be the rule in nature.

There is a mystery in these letters—the enigma that is Newton himself. Nobody in 1692, nor for a century thereafter, noted that, when Bentley confidently brought God into action as the diverter of falling bodies into "this transverse and violent motion," the Creator became, in a sense, only a half-creator of the system. God's action was made once and once for all; it was that "first impulse impressed upon them, not only for five or six thousand years, but many millions of millions." But the gravitating motion, the descent toward the sun, is continuous; despite his effort to make clear his agreement with Newton, Bentley still calls it "a constant energy infused into matter by the Author of all things." Did Newton, in his secret heart, have the wit, which no contemporary possessed, to see that such toying with the notion that gravity was a constant energy infused into matter raised the question of whether the infusion really had been made by the author of all things? Might this not be only a gratuitious addition, made by a mind precommitted to the thesis, by one incapable properly of dealing with the meaning of the infinite? Whether Newton had read Pascal we do not know, but assuredly Bentley never had!

If the letters mean anything, then, they mean that Newton was not quite a Newtonian. He was holding something in reserve, not giving himself entirely to his own discoveries, stupendous as he realized them to be. As for ultimate causes, he knew how to say that he did not know. Our curiosity is aroused, but never shall be satisfied, by the evasive ending of the first letter: Isaac Newton had still another argument to prove the existence of God, potentially very strong, but because the principles on which it was grounded were not yet widely enough received, "I think it more advisable to let it sleep." What were those principles? Perhaps he meant simply the realm of optics into which he was now venturing, already musing upon ideas he was to let see the light only in the form of a question at the end of Query 31 in the 1717 edition of the *Opticks*, when even then he was "not yet satisfied about it for want of experiments." But we cannot help asking if in his subtle consciousness there was a sense of still more complex principles which would need to wait still longer before becoming "better received." And were these withheld principles possibly just those dark and inexplicable discrepancies between the mind of the creature and the methods of the creation which he could dare to contemplate, but of which the Bentleys of this world never attain even a rudimentary awareness?

FOUR LETTERS FROM SIR ISAAC NEWTON TO DOCTOR BENTLEY.

CONTAINING

SOME ARGUMENTS IN PROOF of a DEITY.

LONDON:
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LETTERS, &c.

LETTER I.

To the Reverend Dr. RICHARD BENTLEY, *at the Biſhop of* Worceſter'*s Houſe in* Park-ſtreet, Weſtminſter.

SIR,

WHEN I wrote my Treatiſe about our Syſtem, I had an Eye upon ſuch Principles as might work with conſidering Men, for the Belief of a Deity, and nothing can rejoice me more than to find it uſeful for that Purpoſe. But if I

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have done the Public any Service this way, it is due to nothing but Induſtry and patient Thought.

As to your firſt Query, it ſeems to me that if the Matter of our Sun and Planets, and all the Matter of the Univerſe, were evenly ſcattered throughout all the Heavens, and every Particle had an innate Gravity towards all the reſt, and the whole Space, throughout which this Matter was ſcattered, was but finite; the Matter on the outſide of this Space would by its Gravity tend towards all the Matter on the inſide, and by conſequence fall down into the middle of the whole Space, and there compoſe one great ſpherical Maſs. But if the Matter was evenly diſpoſed throughout an infinite Space, it could never convene into one Maſs, but ſome of it would convene into one Maſs and ſome into another, ſo as to make an infinite Number of great Maſſes, ſcattered at great Diſtances from one to another through-

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throughout all that infinite Space. And thus might the Sun and fixt Stars be formed, ſuppoſing the Matter were of a lucid Nature. But how the Matter ſhould divide itſelf into two ſorts, and that Part of it, which is fit to compoſe a ſhining Body, ſhould fall down into one Maſs and make a Sun, and the reſt, which is fit to compoſe an opaque Body, ſhould coaleſce, not into one great Body, like the ſhining Matter, but into many little ones; or if the Sun at firſt were an opaque Body like the Planets, or the Planets lucid Bodies like the Sun, how he alone ſhould be changed into a ſhining Body, whilſt all they continue opaque, or all they be changed into opaque ones, whilſt he remains unchanged, I do not think explicable by meer natural Cauſes, but am forced to aſcribe it to the Counſel and Contrivance of a voluntary Agent.

The ſame Power, whether natural or ſupernatural, which placed the Sun in

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the Center of the ſix primary Planets, placed *Saturn* in the Center of the Orbs of his five ſecondary Planets, and *Jupiter* in the Center of his four ſecondary Planets, and the Earth in the Center of the Moon's Orb; and therefore had this Cauſe been a blind one, without Contrivance or Deſign, the Sun would have been a Body of the ſame kind with *Saturn*, *Jupiter*, and the Earth, that is, without Light and Heat. Why there is one Body in our Syſtem qualified to give Light and Heat to all the reſt, I know no Reaſon, but becauſe the Author of the Syſtem thought it convenient; and why there is but one Body of this kind I know no Reaſon, but becauſe one was ſufficient to warm and enlighten all the reſt. For the *Carteſian* Hypotheſis of Suns loſing their Light, and then turning into Comets, and Comets into Planets, can have no Place in my Syſtem, and is plainly erroneous; becauſe it is certain that as often as they appear to us, they deſcend into the Syſtem

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of our Planets, lower than the Orb of *Jupiter*, and ſometimes lower than the Orbs of *Venus* and *Mercury*, and yet never ſtay here, but always return from the Sun with the ſame Degrees of Motion by which they approached him.

To your ſecond Query, I anſwer, that the Motions which the Planets now have could not ſpring from any natural Cauſe alone, but were impreſſed by an intelligent Agent. For ſince Comets deſcend into the Region of our Planets, and here move all manner of ways, going ſometimes the ſame way with the Planets, ſometimes the contrary way, and ſometimes in croſs ways, in Planes inclined to the Plane of the Eclip*t*ick, and at all kinds of Angles, 'tis plain that there is no natural Cauſe which could determine all the Planets, both primary and ſecondary, to move the ſame way and in the ſame Plane, without any conſiderable Variation: This muſt have been the Ef-

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ſect of Counſel. Nor is there any natural Cauſe which could give the Planets thoſe juſt Degrees of Velocity, in Proportion to their Diſtances from the Sun, and other central Bodies, which were requiſite to make them move in ſuch concentrick Orbs about thoſe Bodies. Had the Planets been as ſwift as Comets, in Proportion to their Diſtances from the Sun (as they would have been, had their Motion been cauſed by their Gravity, whereby the Matter, at the firſt Formation of the Planets, might fall from the remoteſt Regions towards the Sun) they would not move in concentrick Orbs, but in ſuch eccentrick ones as the Comets move in. Were all the Planets as ſwift as *Mercury*, or as ſlow as *Saturn* or his Satellites; or were their ſeveral Velocities otherwiſe much greater or leſs than they are, as they might have been had they aroſe from any other Cauſe than their Gravities; or had the Diſtances from the Centers about which they move, been greater or

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lefs than they are with the fame Velocities; or had the Quantity of Matter in the Sun, or in *Saturn*, *Jupiter*, and the Earth, and by confequence their gravitating Power been greater or lefs than it is; the primary Planets could not have revolved about the Sun, nor the fecondary ones about *Saturn*, *Jupiter*, and the Earth, in concentrick Circles as they do, but would have moved in Hyperbolas, or Parabolas, or in Ellipfes very eccentrick. To make this Syftem therefore, with all its Motions, required a Caufe which underftood, and compared together, the Quantities of Matter in the feveral Bodies of the Sun and Planets, and the gravitating Powers refulting from thence; the feveral Diftances of the primary Planets from the Sun, and of the fecondary ones from *Saturn*, *Jupiter*, and the Earth; and the Velocities with which thefe Planets could revolve about thofe Quantities of Matter in the central Bodies; and to compare and adjuft all thefe

Things

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Things together, in ſo great a Variety of Bodies, argues that Cauſe to be not blind and fortuitous, but very well ſkilled in Mechanicks and Geometry.

To your third Query, I anſwer, that it may be repreſented that the Sun may, by heating thoſe Planets moſt which are neareſt to him, cauſe them to be better concocted, and more condenſed by that Concoction. But when I conſider that our Earth is much more heated in its Bowels below the upper Cruſt by ſubterraneous Fermentations of mineral Bodies than by the Sun, I ſee not why the interior Parts of *Jupiter* and *Saturn* might not be as much heated, concocted, and coagulated by thoſe Fermentations as our Earth is; and therefore this various Denſity ſhould have ſome other Cauſe than the various Diſtances of the Planets from the Sun. And I am confirmed in this Opinion by conſidering, that the Planets of *Jupiter* and *Saturn*, as they are rarer than

than the reſt, ſo they are vaſtly greater, and contain a far greater Quantity of Matter, and have many Satellites about them; which Qualifications ſurely aroſe not from their being placed at ſo great a Diſtance from the Sun, but were rather the Cauſe why the Creator placed them at great Diſtance. For by their gravitating Powers they diſturb one another's Motions very ſenſibly, as I find by ſome late Obſervations of Mr. *Flamſteed*, and had they been placed much nearer to the Sun and to one another, they would by the ſame Powers have cauſed a conſiderable Diſturbance in the whole Syſtem.

To your fourth Query, I anſwer, that in the Hypotheſis of Vortices, the Inclination of the Axis of the Earth might, in my Opinion, be aſcribed to the Situation of the Earth's Vortex before it was abſorbed by the neighbouring Vortices, and the Earth turned from a Sun to a

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Comet; but this Inclination ought to decreafe conftantly in Compliance with the Motion of the Earth's Vortex, whofe Axis is much lefs inclined to the Eclip-tick, as appears by the Motion of the Moon carried about therein. If the Sun by his Rays could carry about the Planets, yet I do not fee how he could thereby effect their diurnal Motions.

Laftly, I fee nothing extraordinary in the Inclination of the Earth's Axis for proving a Deity, unlefs you will urge it as a Contrivance for Winter and Summer, and for making the Earth habitable towards the Poles; and that the diurnal Rotations of the Sun and Planets, as they could hardly arife from any Caufe purely mechanical, fo by being determined all the fame way with the annual and menftrual Motions, they feem to make up that Harmony in the Syftem, which, as I explaind above, was the Effect of Choice rather than Chance.

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There is yet another Argument for a Deity, which I take to be a very ſtrong one, but till the Principles on which it is grounded are better received, I think it more adviſable to let it ſleep.

I am,

Your moſt humble Servant,

to command,

Cambridge,
Decemb. 10, 1692.

IS. NEWTON.

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LETTER II.

For Mr. BENTLEY, *at the Palace at* Worceſter.

SIR,

I Agree with you, that if Matter evenly diffuſed through a finite Space, not ſpherical, ſhould fall into a ſolid Maſs, this Maſs would affect the Figure of the whole Space, provided it were not ſoft, like the old Chaos, but ſo hard and ſolid from the Beginning, that the Weight of its protuberant Parts could not make it yield to their Preſſure. Yet by Earthquakes looſening the Parts of this Solid, the Protuberances might ſometimes ſink a little by their Weight, and thereby the Maſs might, by Degrees, approach a ſpherical Figure.

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The Reaſon why Matter evenly ſcattered through a finite Space would convene in the midſt, you conceive the ſame with me; but that there ſhould be a central Particle, ſo accurately placed in the middle, as to be always equally attracted on all Sides, and thereby continue without Motion, ſeems to me a Suppoſition fully as hard as to make the ſharpeſt Needle ſtand upright on its Point upon a Looking-Glaſs. For if the very mathematical Center of the central Particle be not accurately in the very mathematical Center of the attractive Power of the whole Maſs, the Particle will not be attracted equally on all Sides. And much harder it is to ſuppoſe all the Particles in an infinite Space ſhould be ſo accurately poiſed one among another, as to ſtand ſtill in a perfect Equilibrium. For I reckon this as hard as to make not one Needle only, but an infinite number of them (ſo many as there are Particles in an infinite Space) ſtand accurately poiſed upon their

Points.

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Points. Yet I grant it poſſible, at leaſt by a divine Power; and if they were once to be placed, I agree with you that they would continue in that Poſture without Motion for ever, unleſs put into new Motion by the ſame Power. When therefore I ſaid, that Matter evenly ſpread through all Space, would convene by its Gravity into one or more great Maſſes, I underſtand it of Matter not reſting in an accurate Poiſe.

But you argue, in the next Paragraph of your Letter, that every Particle of Matter in an infinite Space, has an infinite Quantity of Matter on all Sides, and by conſequence an infinite Attraction every way, and therefore muſt reſt in Equilibrio, becauſe all Infinites are equal. Yet you ſuſpect a Paralogiſm in this Argument; and I conceive the Paralogiſm lies in the Poſition, that all Infinites are equal. The generality of Mankind conſider Infinites no other ways than indefinitely; and

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and in this Senſe, they ſay all Infinites are equal; tho' they would ſpeak more truly if they ſhould ſay, they are neither equal nor unequal, nor have any certain Difference or Proportion one to another. In this Senſe therefore, no Concluſions can be drawn from them, about the Equality, Proportions, or Differences of Things, and they that attempt to do it uſually fall into Paralogiſms. So when Men argue againſt the infinite Diviſibility of Magnitude, by ſaying, that if an Inch may be divided into an infinite Number of Parts, the Sum of thoſe Parts will be an Inch; and if a Foot may be divided into an infinite Number of Parts, the Sum of thoſe Parts muſt be a Foot, and therefore ſince all Infinites are equal, thoſe Sums muſt be equal, that is, an Inch equal to a Foot.

The Falſeneſs of the Concluſion ſhews an Error in the Premiſes, and the Error lies in the Poſition, that all Infinites are equal.

equal. There is therefore another Way of confidering Infinites ufed by Mathematicians, and that is, under certain definite Reftrictions and Limitations, whereby Infinites are determined to have certain Differences or Proportions to one another. Thus Dr. *Wallis* confiders them in his *Arithmetica Infinitorum*, where by the various Proportions of infinite Sums, he gathers the various Proportions of infinite Magnitudes: Which way of arguing is generally allowed by Mathematicians, and yet would not be good were all Infinites equal. According to the fame way of confidering Infinites, a Mathematician would tell you, that tho' there be an infinite Number of infinite little Parts in an Inch, yet there is twelve times that Number of fuch Parts in a Foot, that is, the infinite Number of thofe Parts in a Foot is not equal to, but twelve Times bigger than the infinite Number of them in an Inch. And fo a Mathematician will tell you, that if a

Body ſtood in Equilibrio between any two equal and contrary attracting infinite Forces; and if to either of theſe Forces you add any new finite attracting Force, that new Force, how little ſoever, will deſtroy their Equilibrium, and put the Body into the ſame Motion into which it would put it were thoſe two contrary equal Forces but finite, or even none at all; ſo that in this Caſe the two equal Infinites by the Addition of a Finite to either of them, become unequal in our ways of Reckoning; and after theſe ways we muſt reckon, if from the Conſiderations of Infinites we would always draw true Concluſions.

To the laſt Part of your Letter, I anſwer, Firſt, that if the Earth (without the Moon) were placed any where with its Center in the *Orbis Magnus*, and ſtood ſtill there without any Gravitation or Projection, and there at once were infuſed into it, both a gravitating Energy towards the

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the Sun, and a tranſverſe Impulſe of a juſt Quantity moving it directly in a Tangent to the *Orbis Magnus*; the Compounds of this Attraction and Projection would, according to my Notion, cauſe a circular Revolution of the Earth about the Sun. But the tranſverſe Impulſe muſt be a juſt Quantity; for if it be too big or too little, it will cauſe the Earth to move in ſome other Line. Secondly, I do not know any Power in Nature which would cauſe this tranſverſe Motion without the divine Arm. *Blondel* tells us ſomewhere in his Book of Bombs, that *Plato* affirms, that the Motion of the Planets is ſuch, as if they had all of them been created by God in ſome Region very remote from our Syſtem, and let fall from thence towards the Sun, and ſo ſoon as they arrived at their ſeveral Orbs, their Motion of falling turned aſide into a tranſverſe one. And this is true, ſuppoſing the gravitating Power of the Sun was double

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at that Moment of Time in which they all arrive at their ſeveral Orbs; but then the divine Power is here required in a double reſpect, namely, to turn the deſcending Motions of the falling Planets into a ſide Motion, and at the ſame time to double the attractive Power of the Sun. So then Gravity may put the Planets into Motion, but without the divine Power it could never put them into ſuch a circulating Motion as they have about the Sun; and therefore, for this, as well as other Reaſons, I am compelled to aſcribe the Frame of this Syſtem to an intelligent Agent.

You ſometimes ſpeak of Gravity as eſſential and inherent to Matter. Pray do not aſcribe that Notion to me; for the Cauſe of Gravity is what I do not pretend to know, and therefore would take more Time to conſider of it.

I fear what I have ſaid of Infinites, will ſeem obſcure to you; but it is enough if you

you underſtand, that Infinites when conſidered abſolutely without any Reſtriction or Limitation, are neither equal nor unequal, nor have any certain Proportion one to another, and therefore the Principle that all Infinites are equal, is a precarious one.

Sir, I am,

Your moſt humble Servant,

Trinity College,
Jan. 17, 1692-3.

IS. NEWTON.

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LETTER III.

For Mr. BENTLEY, *at the Palace at* Worcefter.

SIR,

BEcaufe you defire Speed, I will anfwer your Letter with what Brevity I can. In the fix Pofitions you lay down in the Beginning of your Letter, I agree with you. Your affuming the *Orbis Magnus* 7000 Diameters of the Earth wide, implies the Sun's horizontal Parallax to be half a Minute. *Flamfteed* and *Caffini* have of late obferved it to be about 10″, and thus the *OrbisMagnus* muft be 21,000, or in a rounderNumber 20,000 Diameters of the Earth wide. Either Computation I think

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think will do well, and I think it not worth while to alter your Numbers.

In the next Part of your Letter you lay down four other Pofitions, founded upon the fix firft. The firft of thefe four feems very evident, fuppofing you take Attraction fo generally as by it to underftand any Force by which diftant Bodies endeavour to come together without mechanical Impulfe. The fecond feems not fo clear; for it may be faid, that there might be other Syftems of Worlds before the prefent ones, and others before thofe, and fo on to all paft Eternity, and by confequence, that Gravity may be co-eternal to Matter, and have the fame Effect from all Eternity as at prefent, unlefs you have fomewhere proved that old Syftems cannot gradually pafs into new ones; or that this Syftem had not its Original from the exhaling Matter of former decaying Syftems, but from a Chaos of Matter evenly difperfed

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difperfed throughout all Space; for fomething of this Kind, I think, you fay was the Subject of your fixth Sermon; and the Growth of new Syftems out of old ones, without the Mediation of a divine Power, feems to me apparently abfurd.

The laft Claufe of the fecond Pofition I like very well. It is inconceivable, that inanimate brute Matter fhould, without the Mediation of fomething elfe, which is not material, operate upon, and affect other Matter without mutual Contact, as it muft be, if Gravitation in the Senfe of *Epicurus*, be effential and inherent in it. And this is one Reafon why I defired you would not afcribe innate Gravity to me. That Gravity fhould be innate, inherent and effential to Matter, fo that one Body may act upon another at a Diftance thro' a *Vacuum*, without the Mediation of any thing elfe, by and through which their Action and Force may be conveyed from

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one to another, is to me ſo great an Abſurdity, that I believe no Man who has in philoſophical Matters a competent Faculty of thinking, can ever fall into it. Gravity muſt be cauſed by an Agent acting conſtantly according to certain Laws; but whether this Agent be material or immaterial, I have left to the Conſideration of my Readers.

Your fourth Aſſertion, that the World could not be formed by innate Gravity alone, you confirm by three Arguments. But in your firſt Argument you ſeem to make a *Petitio Principii*; for whereas many ancient Philoſophers and others, as well Theiſts as Atheiſts, have all allowed, that there may be Worlds and Parcels of Matter innumerable or infinite, you deny this, by repreſenting it as abſurd as that there ſhould be poſitively an infinite arithmetical Sum or Number, which is a Contradiction *in Terminis*; but you do not

prove

prove it as abſurd. Neither do you prove, that what Men mean by an infinite Sum or Number, is a Contradiction in Nature; for a Contradiction *in Terminis* implies no more than an Impropriety of Speech. Thoſe things which Men underſtand by improper and contradictious Phraſes, may be ſometimes really in Nature without any Contradiction at all: a Silver Inkhorn, a Paper Lanthorn, an Iron Whetſtone, are abſurd Phraſes, yet the Things ſignified thereby, are really in Nature. If any Man ſhould ſay, that a Number and a Sum, to ſpeak properly, is that which may be numbered and ſummed, but Things infinite are numberleſs, or, as we uſually ſpeak, innumerable and ſumleſs, or inſummable, and therefore ought not to be called a Number or Sum, he will ſpeak properly enough, and your Argument againſt him will, I fear, loſe its Force. And yet if any Man ſhall take the Words, Number and Sum, in a larger Senſe, ſo

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as to underſtand thereby Things, which in the proper way of ſpeaking are numberleſs and ſumleſs (as you ſeem to do when you allow an infinite Number of Points in a Line) I could readily allow him the Uſe of the contradictious Phraſes of innumerable Number, or ſumleſs Sum, without inferring from thence any Abſurdity in the Thing he means by thoſe Phraſes. However, if by this, or any other Argument, you have proved the Finiteneſs of the Univerſe, it follows, that all Matter would fall down from the Outſides, and convene in the Middle. Yet the Matter in falling might concrete into many round Maſſes, like the Bodies of the Planets, and theſe by attracting one another, might acquire an Obliquity of Deſcent, by means of which they might fall, not upon the great central Body, but upon the Side of it, and fetch a Compaſs about, and then aſcend again by the ſame Steps and Degrees of Motion and Velocity with

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with which they defcended before, much after the Manner that the Comets revolve about the Sun; but a circular Motion in concentrick Orbs about the Sun, they could never acquire by Gravity alone.

And tho' all the Matter were divided at firft into feveral Syftems, and every Syf-tem by a divine Power conftituted like ours; yet would the Outfide Syftems de-fcend towards the Middlemoft; fo that this Frame of Things could not always fubfift without a divine Power to conferve it, which is the fecond Argument; and to your third I fully affent.

As for the Paffage of *Plato*, there is no common Place from whence all the Pla-nets being let fall, and defcending with uniform and equal Gravities (as *Galileo* fuppofes) would at their Arrival to their feveral Orbs acquire their feveral Veloci-ties, with which they now revolve in them.

them. If we ſuppoſe the Gravity of all the Planets towards the Sun to be of ſuch a Quantity as it really is, and that the Motions of the Planets are turned upwards, every Planet will aſcend to twice its Height from the Sun. *Saturn* will aſcend till he be twice as high from the Sun as he is at preſent, and no higher; *Jupiter* will aſcend as high again as at preſent, that is, a little above the Orb of *Saturn*; *Mercury* will aſcend to twice his preſent Height, that is, to the Orb of *Venus*; and ſo of the reſt; and then by falling down again from the Places to which they aſcended, they will arrive again at their ſeveral Orbs with the ſame Velocities they had at firſt, and with which they now revolve.

But if ſo ſoon as their Motions by which they revolve are turned upwards, the gravitating Power of the Sun, by which their Aſcent is perpetually retarded, be

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be diminiſhed by one half, they will now aſcend perpetually, and all of them at all equal Diſtances from the Sun will be equally ſwift. *Mercury* when he arrives at the Orb of *Venus*, will be as ſwift as *Venus*; and he and *Venus*, when they arrive at the Orb of the *Earth*, will be as ſwift as the *Earth*; and ſo of the reſt. If they begin all of them to aſcend at once, and aſcend in the ſame Line, they will conſtantly in aſcending become nearer and nearer together, and their Motions will conſtantly approach to an Equality, and become at length ſlower than any Motion aſſignable. Suppoſe therefore, that they aſcended till they were almoſt contiguous, and their Motions inconſiderably little, and that all their Motions were at the ſame Moment of Time turned back again; or, which comes almoſt to the ſame Thing, that they were only deprived of their Motions, and let fall at that Time, they would all at once arrive at their ſeveral Orbs, each

with

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with the Velocity it had at firſt; and if their Motions were then turned Side-ways, and at the ſame Time the gravi-tating Power of the Sun doubled, that it might be ſtrong enough to retain them in their Orbs, they would revolve in them as before their Aſcent. But if the gravitate-ing Power of the Sun was not doubled, they would go away from their Orbs into the higheſt Heavens in parabolical Lines. Theſe Things follow from my *Princ. Math. Lib.* i. *Prop.* 33, 34, 36, 37.

I thank you very kindly for your de-ſigned Preſent, and reſt

Your moſt

humble Servant

to command,

Cambridge,
Feb. 25, 1692-3.

IS. NEWTON.

LETTER IV.

To Mr. BENTLEY, *at the Palace at* Worcefter.

SIR,

THE Hypothefis of deriving the Frame of the World by mechanical Principles from Matter evenly fpread through the Heavens, being inconfiftent with my Syftem, I had confidered it very little before your Letters put me upon it, and therefore trouble you with a Line or two more about it, if this comes not too late for your Ufe.

In my former I reprefented that the diurnal Rotations of the Planets could not be derived from Gravity, but required a divine Arm to imprefs them. And tho'

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Gravity might give the Planets a Motion of Defcent towards the Sun, either directly or with fome little Obliquity, yet the tranfverfe Motions by which they revolve in their feveral Orbs, required the divine Arm to imprefs them according to the Tangents of their Orbs. I would now add, that the Hypothefis of Matter's being at firft evenly fpread through the Heavens, is, in my Opinion, inconfiftent with the Hypothefis of innate Gravity, without a fupernatural Power to reconcile them, and therefore it infers a Deity. For if there be innate Gravity, it is impoffible now for the Matter of the Earth and all the Planets and Stars to fly up from them, and become evenly fpread throughout all the Heavens, without a fupernatural Power; and certainly that which can never be hereafter without a fupernatural Power, could never be heretofore without the fame Power.

You

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You queried, whether Matter evenly ſpread throughout a finite Space, of ſome other Figure than ſpherical, would not in falling down towards a central Body, cauſe that Body to be of the ſame Figure with the whole Space, and I anſwered, yes. But in my Anſwer it is to be ſuppoſed that the Matter deſcends directly downwards to that Body, and that that Body has no diurnal Rotation.

This, Sir, is all I would add to my former Letters.

I am,

Your moſt humble

Servant,

Cambridge,
Feb. 11, 1693.

IS. NEWTON.

F I N I S.

A

Confutation of Atheism

FROM THE

Origin and Frame of the WORLD.

PART II.

A

SERMON

Preached at

St. *Martin's* in the Fields,

NOVEMBER the 7th. 1692.

Being the *Seventh* of the Lecture Founded by the Honourable *ROBERT BOYLE*, Esquire.

By *RICHARD BENTLEY*, M. A.
Chaplain to the Right Reverend Father in God, *EDWARD*, Lord Bishop of *Worcester*.

LONDON,
Printed for *H. Mortlock* at the *Phœnix* in St. *Paul's* Church-yard. 1693.

Imprimatur.

Ra. Barker, R^{mo} in Chriſto Patri ac D^{no} D^{no} *Johanni* Archiep. *Cantuar.* à Sacris Domeſt.

LAMBHITH,
Novemb. 10.
1692.

Acts XIV. 15, &c.

That ye ſhould turn from theſe vanities unto the living God, who made Heaven and Earth and the Sea, and all things that are therein: Who in times paſt ſuffer'd all Nations to walk in their own ways. Nevertheleſs, he left not himſelf without witneſs, in that he did Good, and gave us Rain from Heaven, and fruitfull Seaſons, filling our hearts with Food and Gladneſs.

WHen we firſt enter'd upon this Topic, the demonſtration of God's Exiſtence from the Origin and Frame of the World, we offer'd to prove four Propoſitions.

1. That this preſent Syſtem of Heaven and Earth cannot poſſibly have ſubſiſted from all Eternity.

2. That Matter conſider'd generally, and abſtractly from any particular Form and Concretion, cannot poſſibly have been eternal: Or, if Matter could be ſo; yet Motion cannot have coexiſted with it eternally, as an inherent property and eſſential attribute of Matter. Theſe two we have already eſtabliſhed

in the preceding Diſcourſe; we ſhall now ſhew in the third place,

3. That, though we ſhould allow the Atheiſts, that Matter and Motion may have been from everlaſting; yet if (as they now ſuppoſe) there were once no Sun nor Starrs nor Earth nor Planets; but the Particles, that now conſtitute them, were diffuſed in the mundane Space in manner of a Chaos without any concretion and coalition; thoſe diſperſed Particles could never of themſelves by any kind of Natural motion, whether call'd Fortuitous or Mechanical, have conven'd into this preſent or any other like Frame of Heaven and Earth.

I. And firſt as to that ordinary Cant of illiterate and puny Atheiſts, the *fortuitous or caſual concourſe of Atoms*, that compendious and eaſy Diſpatch of the moſt important and difficult affair, the Formation of a World; (beſides that in our next undertaking it will be refuted all along) I ſhall now briefly diſpatch it, from what hath been formerly ſaid concerning the true notions of Fortune and Chance. Whereby it is evident, that in the Atheiſtical Hypotheſis of the World's production, Fortuitous and Mechanical muſt be the ſelf-ſame thing. Becauſe *Fortune* is no real entity nor phyſical eſſence, but a mere relative ſignification, denoting

Serm. V. p. 6, 7.

noting only this; That ſuch a thing ſaid to fall out by Fortune, was really effected by material and neceſſary Cauſes; but the Perſon, with regard to whom it is called Fortuitous, was ignorant of thoſe Cauſes or their tendencies, and did not deſign nor foreſee ſuch an effect. This is the only allowable and genuine notion of the word Fortune. But thus to affirm, that the World was made *fortuitouſly*, is as much as to ſay, That before the World was made, there was ſome Intelligent Agent or Spectator; who deſigning to do ſomething elſe, or expecting that ſomething elſe would be done with the Materials of the World, there were ſome occult and unknown motions and tendencies in Matter, which mechanically formed the World beſide his deſign or expectation. Now the Atheiſts, we may preſume, will be loth to aſſert a fortuitous Formation in this proper ſenſe and meaning; whereby they will make Underſtanding to be older than Heaven and Earth. Or if they ſhould ſo aſſert it; yet, unleſs they will affirm that the Intelligent Agent did diſpoſe and direct the inanimate Matter, (which is what we would bring them to) they muſt ſtill leave their Atoms to their mechanical Affections; not able to make one ſtep toward the production

duction of a World beyond the neceſſary Laws of Motion. It is plain then, that *Fortune*, as to the matter before us, is but a ſynonymous word with Nature and Neceſſity. It remains that we examin the adequate meaning of *Chance*; which properly ſignifies, That all events called Caſual, among inanimate Bodies, are mechanically and naturally produced according to the determinate figures and textures and motions of thoſe Bodies; with this negation only, That thoſe inanimate Bodies are not conſcious of their own operations, nor contrive and caſt about how to bring ſuch events to paſs. So that thus to ſay, that the World was made *caſually* by the concourſe of Atoms, is no more than to affirm, that the Atoms compoſed the World mechanically and fatally; only they were not ſenſible of it, nor ſtudied and conſider'd about ſo noble an undertaking. For if Atoms formed the World according to the eſſential properties of Bulk, Figure and Motion, they formed it *mechanically*; and if they formed it mechanically without perception and deſign, they formed it *caſually*. So that this negation of Conſciouſneſs being all that the notion of Chance can add to that of Mechaniſm; We, that do not diſpute this matter with the Atheiſts,

Serm. V. p. 12, 13.

iſts, nor believe that Atoms ever acted by Counſel and Thought, may have leave to conſider the ſeveral names of *Fortune* and *Chance* and *Nature* and *Mechaniſm*, as one and the ſame Hypotheſis. Wherefore once for all to overthrow all poſſible Explications which Atheiſts have or may aſſign for the formation of the World, we will undertake to evince this following Propoſition :

II. That the Atoms or Particles which now conſtitute Heaven and Earth, being once ſeparate and diffuſed in the Mundane Space, like the ſuppoſed *Chaos*, could never *without a God by their Mechanical affections* have convened into this preſent Frame of Things or any other like it.

Which that we may perform with the greater clearneſs and conviction; it will be neceſſary, in a diſcourſe about the Formation of the World, to give you a brief account of ſome of the moſt principal and ſyſtematical *Phænomena*, that occurr in the World now that it is formed.

(1.) The moſt conſiderable *Phænomenon* belonging to Terreſtrial Bodies is the general action of *Gravitation*, whereby All known Bodies in the vicinity of the Earth do tend and preſs toward its Center; not only ſuch as are ſenſibly

ſenſibly and evidently Heavy, but even thoſe that are comparatively the Lighteſt, and even in their proper place, and natural Elements, (as they uſually ſpeak) as Air gravitates even in Air and Water in Water. This hath been demonſtrated and experimentally proved beyond contradiction, by ſeveral ingenious Perſons of the preſent Age, but by none ſo perſpicuouſly and copiouſly and accurately, as by the Honourable Founder of this Lecture in his incomparable Treatiſes of the *Air* and *Hydroſtaticks*.

Mr. *Boyle's* Phyſico-Exp. of Air. Hydroſtat. Paradoxes.

(2.) Now this is the conſtant Property of *Gravitation*; That the weight of all Bodies around the Earth is ever proportional to the Quantity of their Matter: As for inſtance, a Pound weight (examin'd Hydroſtatically) of all kinds of Bodies, though of the moſt different forms and textures, doth always contain an equal quantity of ſolid Maſs or corporeal Subſtance. This is the ancient Doctrine of the *Epicurean* Phyſiology, then and ſince very probably indeed, but yet precariouſly aſſerted: But it is lately demonſtrated and put beyond controverſy by that very excellent and divine Theoriſt Mr. *Iſaac Newton*, to whoſe moſt admirable ſagacity and induſtry we ſhall frequently be obliged in this and the following Diſcourſe.

Lucret. lib. 1.

Newton Philoſ. Natur. Princ. Math. lib. 3. prop. 6.

I

I will not entertain this Auditory with an account of the Demonſtration; but referring the Curious to the Book it ſelf for full ſatisfaction, I ſhall now proceed and build upon it as a Truth ſolidly eſtabliſhed, *That all Bodies weigh according to their Matter*; provided only that the compared Bodies be at equal diſtances from the Center toward which they weigh. Becauſe the further they are removed from the Center, the lighter they are: decreaſing gradually and uniformly in weight, in a duplicate proportion to the Increaſe of the Diſtance.

(3.) Now ſince Gravity is found proportional to the Quantity of Matter, there is a manifeſt Neceſſity of admitting a *Vacuum*, another principal Doctrine of the *Atomical* Philoſophy. Becauſe if there were every-where an abſolute plenitude and denſity without any empty pores and interſtices between the Particles of Bodies, then all Bodies of equal dimenſions would contain an equal Quantity of Matter; and conſequently, as we have ſhewed before, would be equally ponderous: ſo that Gold, Copper, Stone, Wood, *&c.* would have all the ſame ſpecifick weight; which Experience aſſures us they have not: neither would any of them deſcend in the Air, as we all ſee they do; becauſe, if all Space was Full, even the Air would be as denſe and ſpecifically as

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heavy as they. If it be ſaid, that, though the difference of ſpecifick Gravity may proceed from variety of Texture, the lighter Bodies being of a more looſe and porous compoſition, and the heavier more denſe and compact; yet an æthereal ſubtile Matter, which is in a perpetual motion, may penetrate and pervade the minuteſt and inmoſt Cavities of the cloſeſt Bodies, and adapting it ſelf to the figure of every Pore, may adequately fill them; and ſo prevent all Vacuity, without increaſing the weight: To this we anſwer; That that ſubtile Matter it ſelf muſt be of the ſame Subſtance and Nature with all other Matter, and therefore It alſo muſt weigh proportionally to its Bulk; and as much of it as at any time is comprehended within the Pores of a particular Body muſt gravitate jointly with that Body: ſo that if the Preſence of this æthereal Matter made an abſolute Fullneſs, all Bodies of equal dimenſions would be equally heavy: which being refuted by experience, it neceſſarily follows, that there is a Vacuity; and that (notwithſtanding ſome little objections full of cavil and ſophiſtry) mere and ſimple Extenſion or Space hath a quite different nature and notion from real Body and impenetrable Subſtance.

(4.) This

(4.) This therefore being eſtabliſhed; in the next place it's of great conſequence to our preſent enquiry, if we can make a computation, How great is the whole Summ of the Void ſpaces in our ſyſtem, and what proportion it bears to the corporeal ſubſtance. By many and accurate Trials it manifeſtly appears, that Refined Gold, the moſt ponderous of known Bodies, (though even that muſt be allowed to be porous too, being diſſoluble in *Mercury* and *Aqua Regis* and other Chymical Liquors; and being naturally a thing impoſſible, that the Figures and Sizes of its conſtituent Particles ſhould be ſo juſtly adapted, as to touch one another in every Point,) I ſay, Gold is in ſpecifick weight to common Water as 19 to 1; and Water to common Air as 850 to 1: ſo that Gold is to Air as 16150 to 1. Whence it clearly appears, ſeeing Matter and Gravity are always commenſurate, that (though we ſhould allow the texture of Gold to be intirely cloſe without any vacuity) the ordinary Air in which we live and reſpire is of ſo thin a compoſition, that 16149 parts of its dimenſions are mere emptineſs and Nothing; and the remaining One only material and real ſubſtance. But if Gold it ſelf be admitted, as it muſt be, for a porous Concrete, the proportion of Void to Body in the texture of common Air will be ſo much the greater.

Mr. *Boyle* of Air and Poroſity of Bodies.

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And thus it is in the loweſt and denſeſt region of the Air near the ſurface of the Earth, where the whole Maſs of Air is in a ſtate of violent compreſſion, the inferior being preſs'd and conſtipated by the weight of all the incumbent. But, ſince the Air is now certainly known to conſiſt of elaſtick or ſpringy Particles, that have a continual tendency and endeavour to expand and diſplay themſelves; and the dimenſions, to which they expand themſelves, to be reciprocally as the Compreſſion; it follows, that the higher you aſcend in it, where it is leſs and leſs compreſs'd by the ſuperior Air, the more and more it is rarefied. So that at the hight of a few miles from the ſurface of the Earth, it is computed to have ſome million parts of empty ſpace in its texture for one of ſolid Matter. And at the hight of one Terreſtrial Semid. (not above 4000 miles) the Æther is of that wonderfull tenuity, that by an exact calculation, if a ſmall Sphere of common Air of one Inch Diameter (already 16149 parts Nothing) ſhould be further expanded to the thinneſs of that Æther, it would more than take up the Vaſt Orb of *Saturn*, which is many million million times bigger than the whole Globe of the Earth. And yet the higher you aſcend above that region, the Rarefaction ſtill gradually increaſes without ſtop or limit: ſo that,

Mr. *Boyle* ibid.

Newton Philoſ. Nat. Principia. Math. p. 503.

that, in a word, the whole Concave of the Firmament, except the Sun and Planets and their Atmoſpheres, may be conſider'd as a mere Void. Let us allow then, that all the Matter of the Syſtem of our Sun may be 50000 times as much as the whole Maſs of the Earth; and we appeal to Aſtronomy, if we are not liberal enough and even prodigal in this conceſſion. And let us ſuppoſe further, that the whole Globe of the Earth is intirely ſolid and compact without any void interſtices; notwithſtanding what hath been ſhewed before, as to the texture of Gold it ſelf. Now though we have made ſuch ample allowances; we ſhall find, notwithſtanding, that the void Space of our Syſtem is immenſly bigger than all its corporeal Maſs. For, to proceed upon our ſuppoſition, that all the Matter within the Firmament is 50000 times bigger than the ſolid Globe of the Earth; if we aſſume the Diameter of the *Orbis Magnus* (wherein the Earth moves about the Sun) to be only 7000 times as big as the Diameter of the Earth (though the lateſt and moſt accurate Obſervations make it thrice 7000) and the Diameter of the Firmament to be only 100000 times as long as the Diameter of the *Orbis Magnus* (though it cannot poſſibly be leſs than that, but may be vaſtly and unſpeakably bigger) we muſt pronounce, after ſuch large conceſſions on on that ſide and ſuch great abatements on ours, That the Summ of Empty Spaces within the Concave of the Firmament is 6860 million million million times bigger than All the Matter contain'd in it.

Now from hence we are enabled to form a right conception and imagination of the ſuppoſed Chaos; and then we may proceed to determin the controverſy with more certainty and ſatisfaction; whether a World like the Preſent could poſſibly without a Divine Influence be formed in it or no?

(1.) And *firſt*, becauſe every Fixt Star is ſuppoſed by Aſtronomers to be of the ſame Nature with our Sun; and each may very poſſibly have Planets about them, though by reaſon of their vaſt diſtance they be inviſible to Us: we will aſſume this reaſonable ſuppoſition, That the ſame proportion of Void Space to Matter, which is found in our Sun's Region within the Sphere of the Fixt Starrs, may competently well hold in the whole Mundane Space. I am aware, that in this computation we muſt not aſſign the whole Capacity of that Sphere for the Region of our Sun; but allow half of its Diameter for the *Radii* of the ſeveral Regions of the next Fixt Starrs. So that diminiſhing our former number, as this laſt conſideration requires; we may ſafely affirm from cer-

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tain and demonſtrated Principles, That the empty Space of our Solar Region (comprehending half of the Diameter of the Firmament) is 8575 hundred thouſand million million times more ample than all the corporeal ſubſtance in it. And we may fairly ſuppoſe, that the ſame proportion may hold through the whole Extent of the Univerſe.

(2.) And *ſecondly* as to the ſtate or condition of Matter before the World was a-making, which is compendiouſſy expreſt by the word *Chaos*; they muſt ſuppoſe, that either All the Matter of our Syſtem was *evenly* or well-nigh evenly diffuſed through the Region of the Sun, this would repreſent a particular Chaos: or All Matter univerſally ſo ſpread through the whole Mundane Space; which would truly exhibit a General Chaos; no part of the Univerſe being rarer or denſer than another. Which is agreeable to the ancient Deſcription of it, That * *the Heavens and Earth had* μίαν ἰδέαν, μίαν μορφὴν, *one form*, one texture and conſtitution: which could not be, unleſs all the Mundane Matter were *uniformly* and evenly diffuſed. 'Tis indifferent to our Diſpute, whether they ſuppoſe it to have continued a long time or very little in the ſtate of Diffuſion. For if there

* Diod. Sicul. lib. I. Κατὰ τὴν ἐξ ἀρχῆς τῶν ὅλων σύστασιν μίαν ἔχειν ἰδέαν οὐρανόν τε καὶ γῆν, μεμιγμένης αὐτῶν τῆς φύσεως. Apoll. Rhodius lib. I. Ἤειδεν δ' ὡς γαῖα καὶ οὐρανὸς ἠδὲ θάλασσα, τὸ πρὶν ἐπ' ἀλλήλοισι μιῇ συναρηρότα μορφῇ.

there was but one ſingle Moment in all paſt Eternity, when Matter was ſo diffuſed: we ſhall plainly and fully prove, that it could never have convened afterwards into the preſent Frame and Order of Things.

(3.) It is evident from what we have newly proved, that in the Suppoſition of ſuch a Chaos or ſuch an even diffuſion either of the whole Mundane Matter or that of our Syſtem (for it matters not which they aſſume) every ſingle Particle would have a Sphere of Void Space around it 8575 hundred thouſand million million times bigger than the dimenſions of that Particle. Nay further, though the proportion already appear ſo immenſe; yet every ſingle Particle would really be ſurrounded with a Void ſphere Eight times as capacious as that newly mention'd; its Diameter being compounded of the Diameter of the Proper ſphere, and the Semi-diameters of the contiguous Spheres of the neighbouring Particles. From whence it appears, that every Particle (ſuppoſing them globular or not very oblong) would be above Nine Million times their own length from any other Particle. And moreover in the whole Surface of this Void ſphere there can only Twelve Particles be *evenly* placed (as the Hypotheſis requires) that is, at equal Diſtances from the Central one and each other. So that

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if the Matter of our Syſtem or of the Univerſe was equally diſperſed, like the ſuppoſed Chaos; the reſult and iſſue would be, not only that every Atom would be many Million times its own length diſtant from any other: but if any One ſhould be moved Mechanically (without direction or attraction) to the limit of that diſtance; 'tis above a hundred million millions Odds to an unit, that it would not ſtrike upon any other Atom, but glide through an empty interval without any contact.

(4.) 'Tis true, that while I calculate theſe Meaſures, I ſuppoſe all the Particles of Matter to be at abſolute reſt among themſelves, and ſituated in an exact and mathematical evenneſs; neither of which is likely to be allowed by our Adverſaries, who not admitting the former, but aſſerting the eternity of Motion, will conſequently deny the latter alſo: becauſe in the very moment that Motion is admitted in the Chaos, ſuch an exact evenneſs cannot poſſibly be preſerved. But this I do, not to draw any argument againſt them from the Univerſal Reſt or accurately equal diffuſion of Matter; but only that I may better demonſtrate the great Rarity and Tenuity of their imaginary Chaos, and reduce it to computation. Which computation will hold with exactneſs enough, though we allow the Parti-

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cles of the Chaos to be variouſly moved, and to differ ſomething in ſize and figure and ſituation. For if ſome Particles ſhould approach nearer each other than in the former Proportion; with reſpect to ſome other Particles they would be as much remoter. So that notwithſtanding a ſmall diverſity of their Poſitions and Diſtances, the whole Aggregate of Matter, as long as it retain'd the name and nature of Chaos, would retain well-nigh an uniform tenuity of Texture, and may be conſider'd as an homogeneous Fluid. As ſeveral Portions of the ſame ſort of Water are reckon'd to be of the ſame ſpecifick gravity; though it be naturally impoſſible that every Particle and Pore of it, conſider'd Geometrically, ſhould have equal ſizes and dimenſions.

We have now repreſented the true ſcheme and condition of the Chaos; how all the Particles would be diſunited; and what vaſt intervals of empty Space would lye between each. To form a Syſtem therefore, 'tis neceſſary that theſe ſquander'd Atoms ſhould convene and unite into great and compact Maſſes, like the Bodies of the Earth and Planets. Without ſuch a coalition the diffuſed Chaos muſt have continued and reign'd to all eternity. But how could Particles ſo widely diſperſed combine into that cloſeneſs of Texture? Our Adverſa-

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ries can have only theſe two ways of accounting for it. *Either* by the Common Motion of Matter, proceeding from external Impulſe and Conflict (without attraction) by which every Body moves uniformly in a direct line according to the determination of the impelling force. For, they may ſay, the Atoms of the Chaos being variouſly moved according to this catholic Law, muſt needs knock and interfere; by which means ſome that have convenient figures for mutual coherence might chance to ſtick together, and others might join to thoſe, and ſo by degrees ſuch huge Maſſes might be formed, as afterwards became Suns and Planets: or there might ariſe ſome vertiginous Motions or Whirlpools in the Matter of the Chaos; whereby the Atoms might be thruſt and crowded to the middle of thoſe Whirlpools, and there conſtipate one another into great ſolid Globes, ſuch as now appear in the World. Or *ſecondly* by mutual Gravitation or Attraction. For they may aſſert, that Matter hath inherently and eſſentially ſuch an intrinſeck energy, whereby it inceſſantly tends to unite it ſelf to all other Matter: ſo that ſeveral Particles placed in a Void ſpace at any diſtance whatſoever would without any external impulſe ſpontaneouſly convene and unite together. And thus the Atoms of the Chaos, though never ſo widely diffuſed, might by this innate property of Attraction ſoon aſſemble themſelves into great ſphærical Maſſes, and conſtitute Syſtems like the preſent Heaven and Earth. This is all that can be propoſed by Atheiſts, as an efficient cauſe of a World. For as to the *Epicurean* Theory, of Atoms deſcending down an infinite ſpace by an inherent principle of Gravitation, which tends not toward other Matter, but toward a *Vacuum* or Nothing; and verging from the Perpendicular * *no body knows why nor when nor where*; 'tis ſuch miſerable abſurd ſtuff, ſo repugnant to it ſelf, and ſo contrary to the known Phænomena of Nature (yet it contented ſupine unthinking Atheiſts for a thouſand years together) that we will not now honour it with a ſpecial refutation. But what it hath common with the other Explications, we will fully confute together with Them in theſe three Propoſitions.

* Lucret. *Nec regione loci certa, nec tempore certo.*

(1.) That by Common Motion (without attraction) the diſſever'd Particles of the Chaos could never make the World; could never convene into ſuch great compact Maſſes, as the Planets now are; nor either acquire or continue ſuch Motions, as the Planets now have.

(2.) That ſuch a mutual Gravitation or ſpontaneous Attraction can neither be inherent and eſſential to Matter; nor ever ſupervene to it, unleſs

unleſs impreſs'd and infuſed into it by a Divine Power.

(3.) That though we ſhould allow ſuch Attraction to be natural and eſſential to all Matter; yet the Atoms of a Chaos could never ſo convene by it, as to form the preſent Syſtem: or if they could form it, it could neither acquire ſuch Motions, nor continue permanent in this ſtate, without the Power and Providence of a Divine Being.

I. And firſt, that by Common Motion the Matter of Chaos could never convene into ſuch Maſſes, as the Planets now are. Any man, that conſiders the ſpacious void Intervals of the Chaos, how immenſe they are in proportion to the bulk of the Atoms, will hardly induce himſelf to believe, that Particles ſo widely diſſeminated could ever throng and crowd one another into a cloſe and compact texture. He will rather conclude, that thoſe few that ſhould happen to claſh, might rebound after the colliſion; or if they cohered, yet by the next conflict with other Atoms might be ſeparated again, and ſo on in an eternal viciſſitude of Faſt and Looſe, without ever conſociating into the huge condenſe Bodies of Planets; ſome of whoſe Particles upon this ſuppoſition muſt have travell'd many millions of Leagues through the gloomy regions of Chaos, os, to place themſelves where they now are. But then how rarely would there be any claſhing at all? how very rarely in compariſon to the number of Atoms? The whole multitude of them, generally ſpeaking, might freely move and rove for ever with very little occurring or interfering. Let us conceive two of the neareſt Particles according to our former Calculation; or rather let us try the ſame proportions in another Example, that will come eaſier to the Imagination. Let us ſuppoſe two Ships, fitted with durable Timber and Rigging, but without Pilot or Mariners, to be placed in the vaſt *Atlantick* or the *Pacifique* Ocean, as far aſunder as may be. How many thouſand years might expire, before thoſe ſolitary Veſſels ſhould happen to ſtrike one againſt the other? But let us imagin the Space yet more ample, even the whole face of the Earth to be covered with Sea, and the two Ships to be placed in the oppoſite Poles: might not they now move long enough without any danger of claſhing? And yet I find, that the two neareſt Atoms in our evenly diffuſed Chaos have ten thouſand times leſs proportion to the two Void circular Planes around them, than our two Ships would have to the whole Surface of the Deluge. Let us aſſume then another Deluge ten thouſand times larger than *Noah's*. Is it not now utterly incredible,

credible, that our two Veſſels, placed there Antipodes to each other, ſhould ever happen to concur? And yet let me add, that the Ships would move in one and the ſame Surface; and conſequently muſt needs encounter, when they either advance towards one another in direct lines, or meet in the interſection of croſs ones; but the Atoms may not only fly ſide-ways, but over likewiſe and under each other: which makes it many million times more improbable, that they ſhould interfere than the Ships, even in the laſt and unlikelieſt inſtance. But they may ſay, Though the Odds indeed be unſpeakable that the Atoms do not convene in any ſet number of Trials, yet in an infinite Succeſſion of them may not ſuch a Combination poſſibly happen? But let them conſider, that the improbability of Caſual Hits is never diminiſhed by repetition of Trials; they are as unlikely to fall out at the Thouſandth as at the Firſt. So that in a matter of mere Chance, when there is ſo many Millions odds againſt any aſſignable Experiment; 'tis in vain to expect it ſhould ever ſucceed, even in endleſs Duration. Serm. V. p. 32.

But though we ſhould concede it to be ſimply poſſible, that the Matter of Chaos might convene into great Maſſes, like Planets: yet it's abſolutely impoſſible, that thoſe Maſſes ſhould *acquire* ſuch revolutions about the Sun. Let us us ſuppoſe any one of thoſe Maſſes to be the Preſent Earth. Now the annual Revolution of the Earth muſt proceed (in this Hypotheſis) *either* from the Summ and Reſult of the ſeveral motions of all the Particles that formed the Earth, *or* from a new Impulſe from ſome external Matter, after it was formed. The *former* is apparently abſurd, becauſe the Particles that form'd the round Earth muſt needs convene from all points and quarters toward the middle, and would generally tend toward its Center; which would make the whole Compound to reſt in a Poiſe: or at leaſt that overplus of Motion, which the Particles of one Hemiſphere could have above the other, would be very ſmall and inconſiderable; too feeble and languid to propell ſo vaſt and ponderous a Body with that prodigious velocity. And *ſecondly*, 'tis impoſſible, that any external Matter ſhould impell that compound Maſs, after it was formed. 'Tis manifeſt, that nothing elſe could impell it, unleſs the Æthereal Matter be ſuppoſed to be carried about the Sun like a *Vortex* or Whirlpool, as a Vehicle to convey It and the reſt of the Planets. But this is refuted from what we have ſhewn above, that thoſe Spaces of the Æther may be reckon'd a mere Void, the whole Quantity of their Matter ſcarce amounting to the weight of a Grain. 'Tis refuted alſo from

from Matter of Fact in the Motion of Comets; which, as often as they are vifible to Us, are in the Region of our Planets; and there are obferved to move, fome in quite contrary courfes to Theirs, and fome in crofs and oblique ones, in Planes inclined to the Plane of the Ecliptick in all kinds of Angles: which firmly evinces, that the Regions of the Æther are empty and free, and neither refift nor affift the Revolutions of Planets. But moreover there could not poffibly arife in the Chaos any *Vortices* or Whirlpools at all; either to form the Globes of the Planets, or to revolve them when formed. 'Tis acknowledged by all, that inanimate unactive Matter moves always in a ftreight Line, nor ever reflects in an Angle, nor bends in a Circle (which is a continual reflexion) unlefs *either* by fome external Impulfe, that may divert it from the direct motion, *or* by an intrinfec Principle of Gravity or Attraction, that may make it defcribe a curve line about the attracting Body. But this latter Caufe is not now fuppofed: and the former could never beget Whirlpools in a Chaos of fo great a Laxity and Thinnefs. For 'tis matter of certain experience and univerfally allowed, that all Bodies moved circularly have a perpetual endeavour to recede from the Center, and every moment would fly out in right Lines, if they were

Newton ibidem p. 480.

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not violently reſtrain'd and kept in by contiguous Matter. But there is no ſuch reſtraint in a Chaos, no want of empty room there; no poſſibility of effecting one ſingle Revolution in way of a *Vortex*, which neceſſarily requires either an abſolute Fulneſs of Matter, or a pretty cloſe Conſtipation and mutual Contact of its Particles.

And for the ſame reaſon 'tis evident, that the Planets could not *continue* their Revolutions about the Sun; though they could poſſibly acquire them. For to drive and carry the Planets in ſuch Orbs as they now deſcribe, that Æthereal Matter muſt be compact and denſe, as denſe as the very Planets themſelves: otherwiſe they would certainly fly out in Spiral Lines to the very circumference of the *Vortex*. But we have often inculcated, that the wide Tracts of the Æther may be reputed as a mere extended Void. So that there is nothing (in this Hypotheſis) that can retain and bind the Planets in their Orbs for one ſingle moment; but they would immediately deſert them and the neighbourhood of the Sun, and vaniſh away in Tangents to their ſeveral Circles into the Abyſs of Mundane Space.

II. Secondly we affirm, that mutual Gravitation or ſpontaneous Attraction cannot poſſibly be innate and eſſential to Matter. By Attraction

traction we do not here underſtand what is improperly, though vulgarly, called ſo, in the operations of drawing, ſucking, pumping, *&c.* which is really Pulſion and Truſion; and belongs to that Common Motion, which we have already ſhewn to be inſufficient for the formation of a World. But we now mean (as we have explain'd it before) ſuch a power and quality, whereby all parcels of Matter would mutually attract or mutually tend and preſs to all others; ſo that (for inſtance) two diſtant Atoms *in vacuo* would ſpontaneouſly convene together without the impulſe of external Bodies. Now we ſay, if our Atheiſts ſuppoſe this power to be inherent and eſſential to Matter; they overthrow their own Hypotheſis: there could never be a Chaos at all upon theſe terms, but the preſent form of our Syſtem muſt have continued from all Eternity; againſt their own Suppoſition, and what we have proved in our Laſt. *Vide* Serm. VI. & Serm. VIII.
For if they affirm, that there might be a Chaos notwithſtanding innate Gravity; then let them aſſign any Period though never ſo remote, when the diffuſed Matter might convene. They muſt confeſs, that before that aſſigned Period Matter had exiſted eternally, inſeparably endued with this principle of Attraction; and yet had never attracted nor convened before, during that infinite duration: which is

ſo monſtrous an abſurdity, as even They will bluſh to be charged with. But ſome perhaps may imagin, that a former Syſtem might be diſſolved and reduced to a Chaos, from which the preſent Syſtem might have its Original, as that Former had from another, and ſo on: new Syſtems having grown out of old ones in infinite Viciſſitudes from all paſt eternity. But we ſay, that in the Suppoſition of innate Gravity no Syſtem at all could be diſſolved. For how is it poſſible, that the Matter of ſolid Maſſes like Earth and Planets and Starrs ſhould fly up from their Centers againſt its inherent principle of mutual Attraction, and diffuſe it ſelf in a Chaos? This is abſurder than the other: That only ſuppoſed innate Gravity not to be exerted; This makes it to be defeated, and to act contrary to its own Nature. So that upon all accounts this eſſential power of Gravitation or Attraction is irreconcilable with the Atheiſt's own Doctrine of a Chaos. And ſecondly 'tis repugnant to Common Senſe and Reaſon. 'Tis utterly unconceivable, that inanimate brute Matter (without the mediation of ſome Immaterial Being) ſhould operate upon and affect other Matter without mutual Contact; that diſtant Bodies ſhould act upon each other through a *Vacuum* without the intervention of ſomething elſe by and through which

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the action may be conveyed from one to the other. We will not obſcure and perplex with multitude of words, what is ſo clear and evident by its own light, and muſt needs be allowed by all, that have any competent uſe of Thinking, and are initiated into, I do not ſay the Myſteries, but the plaineſt Principles of Philoſophy. Now mutual Gravitation or Attraction (in our preſent acception of the Words) is the ſame thing with This; 'tis an operation or vertue or influence of diſtant Bodies upon each other through an empty Interval, without any *Effluvia* or Exhalations or other corporeal Medium to convey and tranſmit it. This Power therefore cannot be innate and eſſential to Matter. And if it be not eſſential; it is conſequently moſt manifeſt (ſeeing it doth not depend upon Motion or Reſt or Figure or Poſition of Parts, which are all the ways that Matter can diverſify it ſelf) that it could never *ſupervene* to it, unleſs impreſs'd and infuſed into it by an immaterial and divine Power.

We have proved, that a Power of mutual Gravitation, without contact or impulſe, can in no-wiſe be attributed to mere Matter: or if it could; we ſhall preſently ſhew, that it would be wholly unable to form the World out of *Chaos*. But by the way; what if it be made appear, that there is really ſuch a Power of Gravity

Gravity perpetually acting in the conſtitution of the preſent Syſtem? This would be a new and invincible Argument for the Being of God: being a direct and poſitive proof, that an immaterial living Mind doth inform and actuate the dead Matter, and ſupport the Frame of the World. I will lay before you ſome certain *Phænomena* of Nature; and leave it to your conſideration from what Principle they can proceed. 'Tis demonſtrated, That the Sun, Moon and all the Planets do reciprocally gravitate one toward another: that the Gravitating power of each of Theſe is exactly proportional to their Matter, and ariſes from the ſeveral Gravitations or Attractions of every individual Particle that compoſe the whole Maſs: that all Matter near the Surface of the Earth, for example, doth not only gravitate downwards, but upwards alſo and ſide-ways and toward all imaginable Points; though the Tendency downwards be prædominant and alone diſcernible, becauſe of the Greatneſs and Nearneſs of the attracting Body, the Earth: that every Particle of the whole Syſtem doth attract and is attracted by all the reſt, All operating upon All: that this *Univerſal Attraction or Gravitation* is an inceſſant, regular and uniform Action by certain and eſtabliſhed Laws according to Quantity of Matter and Longitude of Diſtance:

Diſtance: that it cannot be deſtroyed nor impair'd nor augmented by any thing, neither by Motion nor Reſt, nor Situation nor Poſture, nor alteration of Form, nor diverſity of Medium: that it is not a Magnetical Power, nor the effect of a Vortical Motion; thoſe common attempts toward the Explication of Gravity: Theſe things, I ſay, are fully demonſtrated, as matters of Fact, by that very ingenious Author, whom we cited before. Now how is it poſſible that theſe things ſhould be effected by any Material and Mechanical Agent? We have evinced, that mere Matter cannot operate upon Matter without mutual Contact. It remains then, that theſe Phænomena are produced *either* by the intervention of Air or Æther or other ſuch medium, that communicates the Impulſe from one Body to another; *or* by Effluvia and Spirits that are emitted from the one, and pervene to the other. We can conceive no other way of performing them Mechanically. But what impulſe or agitation can be propagated through the Æther from one Particle entombed and wedged in the very Center of the Earth to another in the Center of *Saturn*? Yet even thoſe two Particles do reciprocally affect each other with the ſame force and vigour, as they would do at the ſame diſtance in any other Situation imaginable. And becauſe the

Newton Philoſophiæ Naturalis Princ. Math. lib. III.

the Impulse from this Particle is not directed to That only; but to all the rest in the Universe, to all quatters and regions, at once invariably and incessantly: to do this mechanically; the same physical Point of Matter must move all manner of ways equally and constantly in the same instant and moment; which is flatly impossible. But if this Particle cannot propagate Motion; much less can it send out Effluvia to all points without intermission or variation; such multitudes of Effluvia as to lay hold on every Atom in the Universe without missing of one. Nay every single Particle of the very Effluvia (seeing they also attract and gravitate) must in this Supposition emit other secondary Effluvia all the World over; and those others still emit more, and so *in infinitum.* Now if these things be repugnant to human reason; we have great reason to affirm, That Universal Gravitation, a thing certainly existent in Nature, is above all Mechanism and material Causes, and proceeds from a higher principle, a Divine energy and impression.

III. Thirdly we affirm; That, though we should allow, that reciprocal Attraction is essential to Matter; yet the Atoms of a Chaos could never so convene by it, as to form the present System; or if they could form it, yet it could neither acquire these Revolutions, nor subsist in in the preſent condition, without the Conſervation and Providence of a Divine Being.

(1.) For firſt, if the Matter of the Univerſe, and conſequently the Space through which it's diffuſed, be ſuppoſed to be *Finite* (and I think it might be demonſtrated to be ſo; but that we have already exceeded the juſt meaſures of a Sermon) then, ſince every ſingle Particle hath an innate Gravitation toward all others, proportionated by Matter and Diſtance: it evidently appears, that the outward Atoms of the Chaos would neceſſarily tend inwards and deſcend from all quarters toward the Middle of the whole Space (for in reſpect to every Atom there would lie through the Middle the greateſt quantity of Matter and the moſt vigorous Attraction) and would there form and conſtitute one huge ſphærical Maſs; which would be the only Body in the Univerſe. It is plain therefore, that upon this Suppoſition the Matter of the Chaos could never compoſe ſuch divided and different Maſſes, as the Starrs and Planets of the preſent World.

But allowing our Adverſaries, that The Planets might be compoſed: yet however they could not poſſibly acquire ſuch Revolutions in Circular Orbs, or (which is all one to our preſent purpoſe) in Ellipſes very little Eccentric. For let them aſſign any place where the Planets were formed. Was it nearer to the Sun, than the preſent diſtances are? But that is notoriouſly abſurd: for then they muſt have aſcended from the place of their Formation, againſt the eſſential property of mutual Attraction. Or were each formed in the ſame Orbs, in which they now move? But then they muſt have moved from the Point of Reſt, in an horizontal Line without any inclination or deſcent. Now there is no natural Cauſe, neither Innate Gravity nor Impulſe of external Matter, that could beget ſuch a Motion. For Gravity alone muſt have carried them downwards to the Vicinity of the Sun. And that the ambient Æther is too liquid and empty, to impell them horizontally with that prodigious celerity, we have ſufficiently proved before. Or were they made in ſome higher regions of the Heavens; and from thence deſcended by their eſſential Gravity, till they all arrived at their reſpective Orbs; each with its preſent degree of Velocity, acquired by the fall? But then why did they not continue their deſcent, till they were contiguous to the Sun; whither both Mutual Attraction and Impetus carried them? What natural Agent could turn them aſide, could impell them ſo ſtrongly with a tranſverſe Sideblow againſt that tremendous Weight and Rapidity, when whole Worlds are a falling? But though we ſhould ſuppoſe, that by ſome croſs attraction or other they might acquire an obliquity of deſcent, ſo as to miſs the body of the Sun, and to fall on one ſide of it: then indeed the force of their Fall would carry them quite beyond

beyond it; and ſo they might fetch a compaſs about it, and then return and aſcend by the ſame ſteps and degrees of Motion and Velocity, with which they deſcended before. Such an eccentric Motion as this, much after the manner that Comets revolve about the Sun, they might poſſibly acquire by their innate principle of Gravity: but circular Revolutions in concentric Orbs about the Sun or other central Body could in no-wiſe be attain'd without the power of the Divine Arm. For the Caſe of the Planetary Motions is this. Let us conceive all the Planets to be formed or conſtituted with their Centers in their ſeveral Orbs; and at once to be impreſs'd on them this Gravitating Energy toward all other Matter, and a tranſverſe Impulſe of a juſt quantity in each, projecting them directly in Tangents to thoſe Orbs. The Compound Motion, which ariſes from this Gravitation and Projection together, deſcribes the preſent Revolutions of the Primary Planets about the Sun, and of the Secondary about Thoſe: the Gravity prohibiting, that they cannot recede from the Centers of their Motions; and the tranſverſe Impulſe with-holding, that they cannot approach to them. Now although Gravity could be innate (which we have proved that it cannot be) yet certainly this projected, this tranſverſe and violent Motion can only be aſcribed to the Right hand of the *moſt high God, Creator of Heaven and Earth.*

But finally, though we grant, that theſe Circular Revolutions could be naturally attained; or, if they will, that this very individual World in its preſent poſture and motion was actually formed out of Chaos by Mechanical Cauſes: yet it requires a Divine Power and Providence to have conſerved it ſo long in the preſent ſtate and condition. We have ſhewed, that there is a Tranſverſe Impulſe impreſs'd upon the Planets, which retains them in their ſeveral Orbs, that they be not drawn down by their gravitating Powers toward the Sun or other central Bodies. Gravity we underſtand to be a conſtant Energy or Faculty (which God hath infuſed into Matter) perpetually acting by certain Meaſures and (naturally) inviolable Laws; I ſay, a *Faculty* and Power: for we cannot conceive that the *Act* of Gravitation of this preſent Moment can propagate it ſelf or produce that of the next. But 'tis otherwiſe as to the Tranſverſe Motion; which (by reaſon of the Inactivity of Matter and its inability to change its preſent State either of Moving or Reſting) would from one ſingle Impulſe continue for ever equal and uniform, unleſs changed by the reſiſtence of occurring Bodies or by a Gravitating Power; ſo that the Planets, ſince they move Horizontally (whereby Gravity doth not affect their ſwiftneſs) and through the liquid and unreſiſting Spaces of the Heavens (where either no Bodies at all or inconſiderable ones do occur) may

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preferve the fameVelocity which the firft Impulfe impreft upon them, not only for five or fix thoufand years, but many Millions of Millions. It appears then, that if there was but One Vaft Sun in the Univerfe, and all the reft were Planets, revolving around him in Concentric Orbs, at convenient Diftances: fuch a Syftem as that would very long endure; could it but naturally have a Principle of Mutual Attraction, and be once actually put into Circular Motions. But the Frame of the prefent World hath a quite different ftructure: here's an innumerable multitude of Fixt Starrs or Suns; all of which are demonftrated (and fuppofed alfo by our Adverfaries) to have Mutual Attraction: or if they have not; even Not to have it is an equal Proof of a Divine Being, that hath fo arbitrarily indued Matter with a Power of Gravity not effential to it, and hath confined its action to the Matter of its own Solar Syftem: I fay, all the Fixt Starrs have a principle of mutual Gravitation; and yet they are neither revolved about a common Center, nor have any Tranfverfe Impulfe nor any thing elfe to reftrain them from approaching toward each other, as their Gravitating Powers incite them. Now what Natural Caufe can overcome Nature it felf? What is it that holds and keeps them in fixed Stations and Intervals againft an inceffant and inherent Tendency to defert them? Nothing could hinder, but that the Outward Starrs with their

their Syſtems of Planets muſt neceſſarily have deſcended toward the middlemoſt Syſtem of the Univerſe, whither all would be the moſt ſtrongly attracted from all parts of a Finite Space. It is evident therefore that the preſent Frame of Sun and Fixt Starrs could not poſſibly ſubſiſt without the Providence of that almighty Deity, *who ſpake* Pſal. 148. *the word and they were made, who commanded and they were created; who hath made them Faſt for ever and ever, and hath given them a Law, which ſhall not be broken.*

(2.) And ſecondly in the Suppoſition of an *infinite* Chaos, 'tis hard indeed to determin, what would follow in this imaginary Caſe from an innate Principle of Gravity. But to haſten to a concluſion, we will grant for the preſent, that the diffuſed Matter might convene into an infinite Number of great Maſſes at great diſtances from one another, like the Starrs and Planets of this viſible part of the World. But then it is impoſſible, that the Planets ſhould naturally attain theſe circular Revolutions, either by intrinſec Gravitation or the impulſe of ambient Bodies. It is plain, here is no difference as to this; whether the World be Infinite or Finite: ſo that the ſame Arguments that we have uſed before, may be equally urged in this Suppoſition. And though we ſhould concede, that theſe Revolutions might be acquired, and that all were ſettled and conſtituted in the preſent State and Poſture of Things; yet,

yet, we ſay, the continuance of this Frame and Order for ſo long a duration as the known ages of the World muſt neceſſarily infer the Exiſtence of God. For though the Univerſe was Infinite, the Fixt Starrs could not be fixed, but would naturally convene together, and confound Syſtem with Syſtem: for, all mutually attracting, every one would move whither it was moſt powerfully drawn. This, they may ſay, is indubitable in the caſe of a Finite World, where ſome Syſtems muſt needs be Outmoſt, and therefore be drawn toward the Middle: but when Infinite Syſtems ſucceed one another through an Infinite Space, and none is either inward or outward; may not all the Syſtems be ſituated in an accurate Poiſe; and, becauſe equally attracted on all ſides, remain fixed and unmoved? But to this we reply; That unleſs the very mathematical Center of Gravity of every Syſtem be placed and fixed in the very mathematical Center of the Attractive Power of all the reſt; they cannot be evenly attracted on all ſides, but muſt preponderate ſome way or other. Now he that conſiders, what a mathematical Center is, and that Quantity is infinitly diviſible; will never be perſuaded, that ſuch an Univerſal Equilibrium ariſing from the coincidence of Infinite Centers can naturally be acquired or maintain'd. If they ſay; that upon the Suppoſition of Infinite Matter, every Syſtem would be infinitly, and therefore equally attracted on all ſides; and conſequently

sequently would rest in an exact Equilibrium, be the Center of its Gravity in what Position soever: This will overthrow their very Hypothesis; at this rate in an *infinite Chaos* nothing at all could be formed; no Particles could convene by mutual Attraction; for every one there must have Infinite Matter around it, and therefore must rest for ever being evenly balanced between Infinite Attractions. Even the Planets upon this principle must gravitate no more toward the Sun, than any other way: so that they would not revolve in curve Lines, but fly away in direct Tangents, till they struck against other Planets or Starrs in some remote regions of the Infinite Space. An equal Attraction on all sides of all Matter is just equal to no Attraction at all: and by this means all the Motion in the Universe must proceed from external Impulse alone; which we have proved before to be an incompetent Cause for the Formation of a World.

And now, O thou almighty and eternal Creator, *having consider'd the Heavens the work of thy*
Psal. 8. *fingers, the Moon and the Starrs which thou hast ordained*, with all the company of Heaven we laud and magnify thy glorious Name, evermore praising thee and saying; Holy, Holy, Holy, Lord God of Hosts, Heaven and Earth are full of thy Glory: Glory be to thee, O Lord most High.

F I N I S.

A

Confutation of Atheism

FROM THE

Origin and Frame of the WORLD.

The Third and Last PART.

A

SERMON

Preached at

S^t Mary-le-Bow,

DECEMBER the 5th. 1692.

Being the *Eighth* of the Lecture Founded by the Honourable *ROBERT BOYLE*, Esquire.

By *RICHARD BENTLEY*, M. A.
Chaplain to the Right Reverend Father in God,
EDWARD, Lord Bishop of *Worcester*.

LONDON,
Printed for *H. Mortlock* at the *Phœnix* in
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Ra. Barker, R^mo^ in Chriſto Patri ac D^no^ D^no^ *Johanni* Archiep. *Cantuar.* à Sacris Domeſt.

LAMBHITH,
Maij 30. 1693.

Acts XIV. 15, &c.

That ye ſhould turn from theſe vanities unto the living God, who made Heaven and Earth and the Sea, and all things that are therein: Who in times paſt ſuffer'd all Nations to walk in their own ways. Nevertheleſs, he left not himſelf without witneſs, in that he did Good, and gave us Rain from Heaven, and fruitfull Seaſons, filling our hearts with Food and Gladneſs.

HAving abundantly proved in our Laſt Exerciſe, That the Frame of the preſent World could neither be made nor preſerved without the *Power* of God; we ſhall now conſider the ſtructure and motions of our own Syſtem, if any characters of Divine *Wiſdom* and *Goodneſs* may be diſcoverable by us. And even at the firſt and general View it very evidently appears to us (which is our FOURTH and Laſt Propoſition,) That the Order and Beauty of the Syſtematical Parts of the World, the Diſcernible Ends and Final Cauſes of them, the τὸ βέλτιον or Meliority above what was neceſſary to be, do evince by a reflex Argument, that it could not be produced by Mechaniſm or Chance, but by an

Intelligent and Benign Agent, *that by his excellent Wiſdom made the Heavens.*

But before we engage in this Diſquiſition, we muſt offer one neceſſary Caution; that we need not nor do not confine and determin the purpoſes of God in creating all Mundane Bodies, merely to Human Ends and Uſes. Not that we believe it laborious and painfull to Omnipotence to create a World out of Nothing; or more laborious to create a great World, than a ſmall one: ſo as we might think it diſagreeable to the Majeſty and Tranquillity of the Divine Nature to take ſo much pains for our ſakes. Nor do we count it any abſurdity, that ſuch a vaſt and immenſe Univerſe ſhould be made for the ſole uſe of ſuch mean and unworthy Creatures as the Children of Men. For if we conſider the Dignity of an Intelligent Being, and put that in the ſcales againſt brute inanimate Matter; we may affirm, without over valuing Humane Nature, that the Soul of one vertuous and religious Man is of greater worth and excellency than the Sun and his Planets and all the Starrs in the World. If therefore it could appear, that all the Mundane Bodies are ſome way conducible to the ſervice of Man; if all were as beneficial to us, as the Polar Starrs were formerly for Navigation: as the Moon is for the flowing and ebbing of Tides, by which an ineſtimable

mable advantage accrues to the World; for her officious Courtesy on dark Winter nights, especially to the more Northern Nations, who in a continual Night it may be of a whole month are so pretty well accommodated by the Light of the Moon reflected from frozen Snow, that they do not much envy their *Antipodes* a month's presence of the Sun: if all the Heavenly Bodies were thus serviceable to us, we should not be backward to assign their usefulness to Mankind, as the sole end of their Creation. But we dare not undertake to shew, what advantage is brought to Us by those innumerable Starrs in the Galaxy and other parts of the Firmament, not discernible by naked eyes, and yet each many thousand times bigger than the whole body of the Earth: If you say, they beget in us a great Idea and Veneration of the mighty Author and Governer of such stupendious Bodies, and excite and elevate our minds to his adoration and praise; you say very truly and well. But would it not raise in us a higher apprehension of the infinite Majesty and boundless Beneficence of God, to suppose that those remote and vast Bodies were formed, not merely upon Our account to be peept at through an Optick Glass, but for different ends and nobler purposes? And yet who will deny, but that there are great multitudes of lucid Starrs even beyond the reach of the best

beſt Teleſcopes; and that every viſible Starr may have opake Planets revolve about them, which we cannot diſcover? Now if they were not created for Our ſakes; it is certain and evident, that they were not made for their own. For Matter hath no life nor perception, is not conſcious of its own exiſtence, nor capable of happineſs, nor gives the Sacrifice of Praiſe and Worſhip to the Author of its Being. It remains therefore, that all Bodies were formed for the ſake of Intelligent Minds: and as the Earth was principally deſigned for the Being and Service and Contemplation of Men; why may not all other Planets be created for the like Uſes, each for their own Inhabitants which have Life and Underſtanding? If any man will indulge himſelf in this Speculation, he need not quarrel with revealed Religion upon ſuch an account. The Holy Scriptures do not forbid him to ſuppoſe as great a Multitude of Syſtems and as much inhabited, as he pleaſes. 'Tis true; there is no mention in *Moſes*'s Narrative of the Creation, of any People in other Planets. But it plainly appears, that the Sacred Hiſtorian doth only treat of the Origins of Terreſtrial Animals: he hath given us no account of God's creating the Angels; and yet the ſame Author in the enſuing parts of the Pentateuch makes not unfrequent mention of the *Angels of God*. Neither need we be be ſollicitous about the condition of thoſe Planetary People, nor raiſe frivolous Diſputes, how far they may participate in the miſeries of *Adam's* Fall, or in the benefits of *Chriſt's* Incarnation. As if, becauſe they are ſuppoſed to be *Rational* they muſt needs be concluded to be *Men?* For what is Man? not a *Reaſonable Animal* merely, for that is not an adequate and diſtinguiſhing Definition; but a Rational Mind of ſuch particular Faculties, united to an Organical Body of ſuch a certain Structure and Form, in ſuch peculiar Laws of Connexion between the Operations and Affections of the Mind and the Motions of the Body? Now God Almighty by the inexhauſted fecundity of his creative Power may have made innumerable Orders and Claſſes of Rational Minds; ſome higher in natural perfections, others inferior to Human Souls. But a Mind of ſuperior or meaner capacities than Human would conſtitute a different Species, though united to a Human Body in the ſame Laws of Connexion: and a Mind of Human Capacities would make another Species, if united to a different Body in different Laws of Connexion: For this Sympathetical Union of a Rational Soul with Matter, ſo as to produce a Vital communication between them, is an arbitrary inſtitution of the Divine Wiſdom: there is no reaſon nor foundation in the ſeparate natures of of either ſubſtance, why any Motion in the Body ſhould produce any Senſation at all in the Soul; or why This motion ſhould produce That particular Senſation, rather than any other. God therefore may have join'd Immaterial Souls, even of the ſame Claſs and Capacities in their ſeparate State, to other kinds of Bodies and in other Laws of Union; and from thoſe different Laws of Union there will ariſe quite different affections and natures and ſpecies of the compound Beings. So that we ought not upon any account to conclude, that if there be Rational Inhabitants in the *Moon* or *Mars* or any unknown Planets of other Syſtems, they muſt therefore have Human Nature, or be involved in the Circumſtances of Our World. And thus much was neceſſary to be here inculcated (which will obviate and preclude the moſt conſiderable objections of our Adverſaries) that we do not determin the Final Cauſes and Uſefulneſs of the Syſtematical parts of the World, merely as they have reſpect to the Exigencies or Conveniencies of Human Life.

Let us now turn our thoughts and imaginations to the Frame of our Syſtem, if there we may trace any viſible footſteps of Divine Wiſdom and Beneficence. But we are all liable to many miſtakes by the prejudices of Childhood and Youth, which few of us ever correct by a ſerious ſcrutiny

tiny in our riper years, and a Contemplation of the *Phænomena* of Nature in their Cauſes and Beginnings. What we have always ſeen to be done in one conſtant and uniform manner; we are apt to imagin there was but that one way of doing it, and it could not be otherwiſe. This is a great error and impediment in a diſquiſition of this nature: to remedy which, we ought to conſider every thing as not yet in Being; and then diligently examin, if it muſt needs have been at all, or what other ways it might have been as poſſibly as the preſent; and if we find a greater Good and Utility in the preſent conſtitution, than would have accrued either from the total Privation of it, or from other frames and ſtructures that might as poſſibly have been as It: we may then reaſonably conclude, that the preſent conſtitution proceeded neither from the neceſſity of material Cauſes nor the blind ſhuffles of an imaginary Chance, but from an Intelligent and Good Being, that formed it that particular way out of choice and deſign. And eſpecially if this Uſefulneſs be conſpicuous not in one or a few only, but in a long train and ſeries of Things, this will give us a firm and infallible aſſurance, that we have not paſs'd a wrong Judgment.

I. Let us proceed therefore by this excellent Rule in the contemplation of Our Syſtem. 'Tis evident that all the Planets receive Heat and Light from the body of the Sun. Our own Earth in particular would be barren and deſolate, a dead dark lump of Clay, without the benign influence of the Solar Rayes; which without queſtion is true of all the other Planets. It is *good* therefore, that there ſhould be a Sun to warm and cheriſh the Seeds of Plants, and excite them to Vegetation; to impart an uninterrupted Light to all parts of his Syſtem for the Subſiſtence of Animals. But how came the Sun to be Luminous? not from the neceſſity of natural Cauſes, or the conſtitution of the Heavens. All the Planets might have moved about him in the ſame Orbs and the ſame degrees of Velocity as now; and yet the Sun might have been an opake and cold Body like Them. For as the ſix Primary Planets revolve about Him, ſo the Secondary ones are moved about Them, the Moon about the Earth, the Satellites about *Jupiter*, and others about *Saturn*; the one as regularly as the other, in the ſame Seſquialteral proportion of their Periodical motions to their Orbs. So that, though we ſuppoſe the preſent Exiſtence and Conſervation of the Syſtem, yet the Sun might have been a Body without Light or Heat of the ſame kind with the

Earth

Earth and *Jupiter* and *Saturn*. But then what horrid darkneſs and deſolation muſt have reign'd in the World? It had been unfit for the Divine purpoſes in creating vegetable and ſenſitive and rational Creatures. It was therefore the contrivance and choice of a *Wiſe and Good* Being; that the Central Sun ſhould be a Lucid Body, to communicate warmth and light and life to the Planets around him.

II. We have ſhewed in our Laſt, that the concentric Revolutions of the Planets about the Sun proceed from a compound Motion; a Gravitation toward the Sun, which is a conſtant Energy infuſed into Matter by the Author of all things, and a projected tranſverſe Impulſe in Tangents to their ſeveral Orbs, that was impreſs'd at firſt by the Divine Arm, and will carry them around till the end of the World. But now admitting that Gravity may be eſſential to Matter; and that a tranſverſe Impulſe might be acquired too by Natural Cauſes, yet to make all the Planets move about the Sun in circular Orbs; there muſt be given to each a determinate Impulſe, theſe preſent particular degrees of Velocity which they now have, in proportion to their Diſtances from the Sun and to the quantity of the Solar Matter. For had the Velocities of the ſeveral Planets been greater or leſs than they are now, at the ſame di-

Newton Phil. Natur. Princip. Math.

ſtances from the Sun; or had their Diſtances from the Sun, or the quantity of the Sun's Matter and conſequently his Attractive Power been greater or leſs than they are now, with the ſame Velocities: they would not have revolved in concentric Circles as they do, but have moved in Hyperbola's or Parabola's or in Ellipſes very Eccentric. The ſame may be ſaid of the Velocities of the Secondary Planets with reſpect to their Diſtances from the Centers of Their Orbs, and to the Quantities of the Matter of thoſe Central Bodies. Now that all theſe Diſtances and Motions and Quantities of Matter ſhould be ſo accurately and harmoniouſly adjuſted in this great Variety of our Syſtem, is above the fortuitous Hits of blind material Cauſes, and muſt certainly flow from that eternal Fountain of Wiſdom, the Creator of Heaven and Earth, who *always acts Geometrically*, by juſt and adequate numbers and weights and meaſures. And let us examin it further by our Critical Rule: Are the preſent Revolutions in circular Orbs more beneficial, than the other would be? If the Planets had moved in thoſe Lines above named; ſometimes they would have approached to the Sun as near as the Orb of *Mercury*, and ſometimes have exorbitated beyond the diſtance of *Saturn*: and ſome have quite left the Sun without ever returning. Now the very conſtitution

ὁ θεὸς ἀεὶ γεωμετρεῖ. Plat.

ſtitution of a Planet would be corrupted and deſtroyed by ſuch a change of the Interval between it and the Sun: no living thing could have endured ſuch unſpeakable exceſſes of Heat and Cold: all the Animals of our Earth muſt inevitably have periſhed, or rather never have been. So that as ſure as it is *good, very good,* that Human Nature Gen. 1. ſhould exiſt; ſo certain it is that the circular Revolutions of the Earth (and Planets) rather than thoſe other Motions which might as poſſibly have been, do declare not only the Power of God, but his *Wiſdom and Goodneſs.*

III. It is manifeſt by our laſt Diſcourſe, that the Æthereal Spaces are perfectly fluid; they neither aſſiſt nor retard, neither guide nor divert the Revolutions of the Planets; which rowl through thoſe Regions as free and unreſiſted, as if they moved in a *vacuum*. So that any of them might as poſſibly have moved in oppoſite Courſes to the preſent, and in Planes croſſing the Plane of the Eccliptic in any kind of Angles. Now if the Syſtem had been fortuitouſly formed by the convening Matter of a Chaos; how is it conceivable, that all the Planets both Primary and Secondary, ſhould revolve the ſame Way from the Weſt to the Eaſt, and that in the ſame Plane too without any conſiderable variation? No natural and neceſſary Cauſe could ſo determin their motions; and

and 'tis millions of millions odds to an unit in ſuch a Caſt of a Chance. Such an apt and regular Harmony, ſuch an admirable Order and Beauty muſt deſervedly be aſcribed to Divine Art and Conduct. Eſpecially if we conſider, that the ſmalleſt Planets are ſituated neareſt the Sun and each other; whereas *Jupiter* and *Saturn*, that are vaſtly greater than the reſt and have many Satellites about them, are wiſely removed to the extreme Regions of the Syſtem, and placed at an immenſe Diſtance one from the other. For even now at this wide interval they are obſerved in their Conjunctions to diſturb one anothers motions a little by their gravitating Powers: but if ſuch vaſt Maſſes of Matter had been ſituated much nearer to the Sun or to each other (as they might as eaſily have been, for any mechanical or fortuitous Agent) they muſt neceſſarily have cauſed a conſiderable diſturbance and diſorder in the whole Syſtem.

IV. But let us conſider the particular Situation of our Earth and its diſtance from the Sun. It is now placed ſo conveniently, that Plants thrive and flouriſh in it, and Animals live: this is matter of fact, and beyond all diſpute. But how came it to paſs at the beginning, that the Earth moved in its preſent Orb? We have ſhewed before, that if Gravity and a Projected Motion be

fitly

fitly proportion'd, any Planet would freely revolve at any aſſignable diſtance within the Space of the whole Syſtem. Was it mere Chance then, or Divine Counſel and Choice, that conſtituted the Earth in its preſent Situation? To know this; we will enquire, if this particular Diſtance from the Sun be better for our Earth and its Creatures, than a greater or leſs would have been. We may be mathematically certain, That the Heat of the Sun is according to the denſity of the Sun-beams, and is reciprocally proportional to the ſquare of the diſtance from the Body of the Sun. Now by this Calculation, ſuppoſe the Earth ſhould be removed and placed nearer to the Sun, and revolve for inſtance in the Orbit of *Mercury*; there the whole Ocean would even boil with extremity of Heat, and be all exhaled into Vapors; all Plants and Animals would be ſcorched and conſumed in that fiery Furnace. But ſuppoſe the Earth ſhould be carried to the great Diſtance of *Saturn*; there the whole Globe would be one *Frigid Zone*, the deepeſt Seas under the very Equator would be frozen to the bottom; there would be no Life, no Germination; nor any thing that comes now under our knowledge or ſenſes. It was much better therefore, that the Earth ſhould move where it does, than in a much greater or leſs Interval from the body of the Sun. And if you place it at any

Newton ibidem, p. 415.

any other Diſtance, either leſs or more than *Saturn* or *Mercury*; you will ſtill alter it for the worſe proportionally to the Change. It was ſituated therefore where it is, by the Wiſdom of ſome voluntary Agent; and not by the blind motions of Fortune or Fate. If any one ſhall think with himſelf, How then can any thing live in *Mercury* and *Saturn* in ſuch intenſe degrees of Heat and Cold? Let him only conſider, that the Matter of each Planet may have a different denſity and texture and form, which will diſpoſe and qualifie it to be acted on by greater or leſs degrees of Heat according to their ſeveral Situations; and that the Laws of Vegetation and Life and Suſtenance and Propagation are the arbitrary pleaſure of God, and may vary in all Planets according to the Divine Appointment and the Exigencies of Things, in manners incomprehenſible to our Imaginations. 'Tis enough for our purpoſe, to diſcern the tokens of Wiſdom in the placing of our Earth; if its preſent conſtitution would be ſpoil'd and deſtroy'd, if we could not wear Fleſh and Blood, if we could not have Human Nature at thoſe different Diſtances.

V. We have all learnt from the Doctrine of the Sphere, that the Earth revolves with a double motion. For while it is carried around the Sun in the *Orbis Magnus* once a year, it perpetually wheels

about

about its own Axis once in a day and a night: ſo that in 24 hours ſpace it hath turn'd all the parts of the Equinoctial to the rayes of the Sun. Now the Uſes of this vertiginous motion are very conſpicuous; for this is it, that gives Day and Night ſucceſſively over the face of the whole Earth, and makes it habitable all around: without this Diurnal Rotation one Hemiſphere would lye dead and torpid in perpetual Darkneſs and Froſt, and the beſt part of the Other would be burnt up and depopulated by ſo permanent a Heat. It is better therefore, that the Earth ſhould move about its own Center, and make theſe uſefull Viciſſitudes of Night and Day, than expoſe always the ſame ſide to the action of the Sun. But how came it to be ſo moved? not from any neceſſity of the Laws of Motion or the Syſtem of the Heavens. It might annually have compaſſed the Sun, and yet never have once turned upon its own Axis. This is matter of Fact and Experiment in the motion of the Moon; which is carried about the Earth in the very ſame manner as the Earth about the Sun, and yet always ſhews the ſame face to Us, not once wheeling upon her own Center. She indeed, notwithſtanding this, turns all her globe to the Sun by moving in her menſtrual Orb, and enjoys Night and Day alternately, one day of Hers being equal to about 14 Days and Nights of Ours. But ſhould the Earth be deprived of its Diurnal Motion; one half of it could never ſee the Day, but muſt eternally be condemned to Solitude and Darkneſs. That the Earth therefore revolves about its own Center, is another eminent token of the Divine Wiſdom and Goodneſs.

VI. But let us compare the mutual proportion of theſe Diurnal and Annual Revolutions; for they are diſtinct from one another, and have a different degree of Velocity. The Earth rowls once about its Axis in a natural day: in which time all the parts of the Equator move ſomething more than 3 of the Earths Diameters; which makes about 1100 in the ſpace of a year. But within the ſame annual time the Center of the Earth is carried above 50 times as far once round the *Orbis Magnus*, whoſe wideneſs we now aſſume to be 20000 Terreſtrial Diameters. So that the annual motion is more than 50 times ſwifter than the Diurnal Rotation, though we meaſure the latter from the Equator, where the Celerity is the greateſt. But it muſt needs be acknowledged, ſince the Earth revolves not upon a material and rugged but a geometrical Plane, that their proportions may be varied in innumerable degrees; any of which might have happen'd as probably as the preſent. What was it then that preſcribed this

Tacquet de Circulorum volutionibus.

this particular Celerity to each Motion, this proportion and temperament between them both? Let us examin it by our former Rule: if there be any *Meliority* in the preſent conſtitution; if any conſiderable Change would be for the worſe. We will ſuppoſe then, that the *Annual* Motion is accelerated doubly; ſo that a periodical Revolution would be performed in 6 Months. Such a Change would be pernicious; not only becauſe the Earth could not move in a Circular Orb, which we have conſider'd before; but becauſe the Seaſons being then twice as ſhort as they are now, the cold Winter would overtake us, before our Corn and Fruits could poſſibly be ripe. But ſhall this Motion be as much retarded, and the Seaſons lengthen'd in the ſame proportion? This too would be as fatal as the other: for in moſt Countries the Earth would be ſo parched and effete by the drought of the Summer, that it would afford ſtill but one Harveſt, as it doth at the preſent: which then would not be a ſufficient ſtore for the conſumption of a double Year. But let us ſuppoſe, that the *Diurnal* Rotation is either conſiderably ſwifter or ſlower. And firſt let it be retarded; ſo as to make (for example) but 12 Circuits in a year: then every day and night would be as long as Fifteen are now, not ſo fitly proportion'd neither to the common affairs of Life, nor to the exigencies of Sleep and Suſtenance in a conſtitution of Fleſh and Blood. But let it then be accelerated; and wheel a thouſand times about its Center, while the Center deſcribes one circle about the Sun: then an Equinoctial day would conſiſt but of four Hours, which would be an inconvenient Change to the inhabitants of the Earth; ſuch haſty Nights as thoſe would give very unwelcome interruptions to our Labours and Journeys and other Tranſactions of the World. It is *better* therefore, that the Diurnal and Annual Motions ſhould be ſo proportion'd as they are. Let it therefore be aſcribed to the tranſcendent Wiſdom and Benignity of that God, *who hath made all things very good, and loveth all things that he hath made.*

VII. But let us conſider not the Quantity and Proportion only but the Mode alſo of this Diurnal Motion. You muſt conceive an imaginary Plane, which paſſing through the Centers of the Sun and the Earth extends it ſelf on all ſides as far as the Firmament: this Plane is called the Ecliptic; and in this the Center of the Earth is perpetually carried without any deviation. But then the Axis of the Earth, about which its Diurnal Rotation is made, is not erect to this Plane of the Ecliptick, but inclines toward it from the Perpendiculum in an Angle of 23 degrees and a half. Now why is the Axis of the Earth in this particular

cular poſture, rather than any other? did it happen by Chance, or proceed from Deſign? To determin this queſtion, let us ſee, as we have done before, if This be more beneficial to us, than any other Conſtitution. We all know from the very Elements of Aſtronomy, that this inclined Poſition of the Axis, which keeps always the ſame Direction and a conſtant Paralleliſm to it ſelf, is the ſole cauſe of theſe gratefull and needfull Viciſſitudes of the four Seaſons of the Year, and the Variation in length of Days. If we take away the *Inclination*; it would abſolutely undo theſe Northern Nations; the Sun would never come nearer us, than he doth now on the tenth of *March* or the twelfth of *September*. But would we rather part with the *Paralleliſm?* Let us ſuppoſe then that the Axis of the Earth keeps always the ſame inclination toward the body of the Sun: this indeed would cauſe a variety of Days and Nights and Seaſons on the Earth; but then every particular Country would have always the ſame diverſity of Day and Night and the ſame conſtitution of Seaſon without any alternation: ſome would always have long Nights and ſhort Days, others again perpetually long Days and ſhort Nights: one Climate would be ſcorched and ſwelter'd with everlaſting Dog-days; while an eternal *December* blaſted another. This ſurely is not quite ſo good as the preſent Order

of

of Seaſons. But ſhall the Axis rather obſerve no conſtant inclination to any thing, but vary and waver at uncertain times and places? This would be a happy Conſtitution indeed. There could be no health, no life nor ſubſiſtence in ſuch an irregular Syſtem; by thoſe ſurprizing Nods of the Pole we might be toſſed backward or forward from *January* to *June*, nay poſſibly from the *January* of *Greenland* to the *June* of *Abeſſinia*. It is *better* therefore upon all accounts that the Axis ſhould be continued in its preſent poſture and direction: ſo that This alſo is a ſignal Character of Divine Wiſdom and Goodneſs.

But becauſe ſeveral have imagin'd, that this skue poſture of the Axis is a moſt unfortunate and pernicious thing; that if the Poles had been erect to the Plane of the Ecliptic, all mankind would have enjoyed a very Paradiſe upon Earth; a perpetual Spring, an eternal Calm and Serenity, and the Longævity of *Methuſelah* without pains or diſeaſes; we are obliged to conſider it a little further. And firſt as to the *Univerſal and Perpetual Spring*, 'tis a mere Poetical Fancy, and (bating the equality of Days and Nights, a thing of ſmall value) as to the other properties is naturally impoſſible, being repugnant to the very form of the Globe. For to thoſe People that dwell under or near the Æquator, this Spring would be a moſt peſtilent

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and inſupportable Summer; and as for thoſe Countries that are nearer the Poles, in which number are our own and the moſt conſiderable Nations of the World, a Perpetual Spring will not do their buſineſs; they muſt have longer Days, a nearer approach of the Sun, and a leſs Obliquity of his Rayes; they muſt have a Summer and a Harveſt-time too to ripen their Grain and Fruits and Vines, or elſe they muſt bid an eternal adieu to the very beſt of their ſuſtenance. For it is plain, that the Center of the Earth muſt move all along in the *Orbis Magnus*; whether we ſuppoſe a Perpetual Æquinox, or an oblique Poſition of the Axis. So that the whole Globe would continue in the ſame Diſtance from the Sun, and receive the ſame quantity of Heat from him in a Year or any aſſignable time, in either Hypotheſis. Though the Axis then had been perpendicular; yet take the whole Year about, and we ſhould have had the ſame meaſure of Heat, that we have now. So that here lies the queſtion; Whether is more beneficial, that we ſhould have the ſame Yearly quantity of Heat diſtributed equally every day, or ſo diſpoſed as it is, a greater ſhare of it in Summer and in Winter a leſs? It muſt needs be allowed, that we have no Heat to ſpare in Summer; 'tis very well if it be ſufficient for the maturation of Fruits. Now this being granted; 'tis as certain and manifeſt, that an an even diſtribution of the ſame Yearly Heat would never have brought thoſe Fruits to maturity, as this is a known and familiar experiment, That ſuch a quantity of Fewel all kindled at once will cauſe Water to boil, which being lighted gradually and ſucceſſively will never be able to do it. It is clear therefore, that in the conſtitution of a Perpetual Æquinox the beſt part of the Globe would be deſolate and uſeleſs: and as to that little that could be inhabited, there is no reaſon to expect, that it would conſtantly enjoy that admired *Calm and Serenity*. If the aſſertion were true; yet ſome perhaps may think, that ſuch a Felicity, as would make Navigation impoſſible, is not much to be envied. But it's altogether precarious, and has no neceſſary foundation neither upon Reaſon nor Experience. For the Winds and Rains and other affections of the Atmoſphere do not ſolely depend (as that aſſertion ſuppoſeth) upon the courſe of the Sun; but partly and perhaps moſt frequently upon Steams and Exhalations from ſubterraneous Heat, upon the Poſitions of the Moon, the Situations of Seas or Mountains or Lakes or Woods, and many other unknown or uncertain Cauſes. So that, though the Courſe of the Sun ſhould be invariable, and never ſwerve from the Equator; yet the temperament of the Air would be mutable neverthelеſs, according to the abſence or preſence

ſence or various mixture of the other Cauſes. The ancient Philoſophers for many ages together unanimouſly taught, that the Torrid Zone was not habitable. The reaſons that they went upon were very ſpecious and probable; till the experience of theſe latter ages evinced them to be erroneous. They argued from cœleſtial Cauſes only, the conſtant Vicinity of the Sun and the directneſs of his Rayes; never ſuſpecting, that the Body of the Earth had ſo great an efficiency in the changes of the Air; and that then could be the coldeſt and rainieſt ſeaſon, the Winter of the Year, when the Sun was the neareſt of all, and ſteer'd directly over mens heads. Which is warning ſufficient to have deterred any man from expecting ſuch eternal Serenity and Halcyon-days from ſo incompetent and partial a Cauſe, as the conſtant Courſe of the Sun in the Æquinoctial Circle. What general condition and temperament of Air would follow upon that Suppoſition, we cannot poſſibly define; for 'tis not cauſed by certain and regular Motions, nor ſubject to Mathematical Calculations. But if we may make a conjecture from the preſent Conſtitution; we ſhall hardly wiſh for a Perpetual Æquinox to ſave the charges of Weather-glaſſes: for 'tis very well known, that the Months of *March* and *September*, the two Æquinoxes of Our year, are the moſt windy and tempeſtuons, the moſt unſettled and unequable of Seaſons in moſt Countries of the World. Now if this notion of an uniform Calm and Serenity be falſe or precarious; then even the laſt ſuppoſed advantage, the *conſtant Health and Longævity* of Men muſt be given up alſo, as a groundleſs conceit: for this (according to the Aſſertors themſelves) doth ſolely, as an effect of Nature, depend upon the other. Nay further, though we ſhould allow them their Perpetual Calm and Æquability of Heat; they will never be able to prove, that therefore Men would be ſo vivacious as they would have us believe. Nay perhaps the contrary may be inferr'd, if we may argue from the preſent experience: For the Inhabitants of the Torrid Zone, who ſuffer the leaſt and ſhorteſt receſſes of the Sun, and are within one ſtep and degree of a Perpetual Æquinox, are not only ſhorter lived (generally ſpeaking) than other Nations nearer the Poles; but inferior to them in Strength and Stature and Courage, and in all the capacities of the Mind. It appears therefore, that the gradual Viciſſitudes of Heat and Cold are ſo far from ſhortning the thread of man's Life, or impairing his intellectual Faculties; that very probably they both prolong the one in ſome meaſure and exalt and advance the other. So that ſtill we do profeſs to adore the Divine Wiſdom and Goodneſs for this variety

riety of Seaſons, for *Seed-time and harveſt, and cold and heat, and ſummer and winter.* Gen. 8.

VIII. Come we now to conſider the Atmoſphere, and the exterior Frame and Face of the Globe; if we may find any tracks and footſteps of Wiſdom in the Conſtitution of Them. I need not now inform you, that the Air is a thin fluid Body, endued with Elaſticity or Springineſs, and capable of Condenſation and Rarefaction. Neither Can you be ignorant, that if the Air ſhould be much more expanded or condenſed than it naturally is, no Animals could live and breath: it is probable alſo, that the Vapors could not be duly raiſed and ſupported in it; which at once would deprive the Earth of all its ornament and glory, of all its living Inhabitants and Vegetables too. But 'tis certainly known and demonſtrated, that the Condenſation and Expanſion of any portion of the Air, is always proportional to the weight and preſſure incumbent upon it: ſo that if the Atmoſphere had been either much greater or leſs than it is, as it might eaſily have been, it would have had in its loweſt region on the Surface of the Earth a much greater denſity or tenuity of texture; and conſequently have been unſerviceable for Vegetation and Life. It muſt needs therefore be an Intelligent Being that could ſo juſtly adapt it to thoſe excellent purpoſes. 'Tis

See Mr. *Boyle* of the Air.

concluded by Aſtronomers, that the Atmoſphere of the Moon hath no Clouds nor Rains, but a perpetual and uniform ſerenity: becauſe nothing diſcoverable in the Lunar Surface is ever covered and abſconded by the interpoſition of any clouds or miſts, but ſuch as riſe from our own Globe. Now if the Atmoſphere of Our Earth had been of ſuch a Conſtitution; there could nothing, that now grows or breaths in it have been formed or preſerved; Human Nature muſt have been quite obliterated out of the Works of the Creation. If our Air had not been a ſpringy elaſtical Body, no Animal could have exerciſed the very function of Reſpiration: and yet the ends and uſes of Reſpiration are not ſerved by that Springineſs, but by ſome other unknown and ſingular Quality. For the Air, that in exhauſted Receivers of Air-pumps is exhaled from Minerals and Fleſh and Fruits and Liquors, is as true and genuine as to Elaſticity and Denſity or Rarefaction, as that we reſpire in: and yet this factitious Air is ſo far from being fit to be breathed in, that it kills Animals in a moment, even ſooner than the very abſence of all Air, than a Vacuum it ſelf. All which do inferr the moſt admirable Providence of the Author of Nature; who foreknew the neceſſity of Rains and Dews to the preſent ſtructure of Plants, and the uſes of Reſpiration

Mr. *Boyle's* Second Continuation of Phyſico-mechanical Exp. about the Air.

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to Animals; and therefore created thoſe correſpondent properties in the Atmoſphere of the Earth.

IX. In the next place let us conſider the ample proviſion of Waters, thoſe inexhauſted Treaſures of the Ocean: and though ſome have grudged the great ſhare that it takes of the Surface of the Earth, yet we ſhall propoſe this too, as a conſpicuous mark and character of the Wiſdom of God. For that we may not now ſay, that the vaſt *Atlantick* Ocean is really greater Riches and of more worth to the World, than if it was changed into a fifth Continent; and that the Dry Land is as yet much too big for its Inhabitants; and that before they ſhall want Room by increaſing and multiplying, there may be *new Heavens and a new Earth*: We dare venture to affirm, that theſe copious Stores of Waters are no more than neceſſary for the preſent conſtitution of our Globe. For is not the whole Subſtance of all Vegetables mere modified Water? and conſequently of all Animals too; all which either feed upon Vegetables or prey upon one another? Is not an immenſe quantity of it continually exhaled by the Sun, to fill the Atmoſphere with Vapors and Clouds, and feed the Plants of the Earth with the balm of Dews and the fatneſs of Showrs? It ſeems incredible at firſt hearing, that all the Blood

Lucret. Et mare, quod late terrarum diſtinet oras.

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in our Bodies ſhould circulate in a trice, in a very few minutes: but I believe it would be more ſurprizing, if we knew the ſhort and ſwift periods of the great Circulation of Water, that vital Blood of the Earth which compoſeth and nouriſheth all things. If we could but compute that prodigious Maſs of it, that is daily thrown into the channel of the Sea from all the Rivers of the World: we ſhould then know and admire how much is perpetually evaporated and caſt again upon the Continents to ſupply thoſe innumerable Streams. And indeed hence we may diſcover not only the *Uſe* and *Neceſſity* but the *Cauſe* too of the vaſtneſs of the Ocean. I never yet heard of any Nation, that complained they had too broad or too deep or too many Rivers, or wiſhed they were either ſmaller or fewer: they underſtand better than ſo, how to value and eſteem thoſe ineſtimable gifts of Nature. Now ſuppoſing that the multitude and largeneſs of Rivers ought to continue as great as now; we can eaſily prove, that the extent of the Ocean could be no leſs than it is. For it's evident and neceſſary, if we follow the moſt fair and probable Hypotheſis, that the Origin of Fountains is from Vapors and Rain, that the Receptacle of Waters, into which the mouths of all thoſe Rivers muſt empty themſelves, ought to have ſo ſpacious a Surface, that as much Water

may

may be continually brushed off by the Winds and exhaled by the Sun, as (besides what falls again in Showers upon its own Surface) is brought into it by all the Rivers. Now the Surface of the Ocean is just so wide and no wider: for if more was evaporated than returns into it again, the Sea would become less; if less was evaporated, it would grow bigger. So that, because since the memory of all ages it hath continu'd at a stand without considerable variation, and if it hath gain'd ground upon one Country, hath lost as much in another; it must consequently be exactly proportioned to the present constitution of Rivers. How rash therefore and vain are those busy Projectors in Speculation, that imagin they could recover to the World many new and noble Countries, in the most happy and temperate Climates, without any damage to the old ones, could this same Mass of the Ocean be lodged and circumscribed in a much deeper Channel and within narrower Shores! For by how much they would diminish the present extent of the Sea, so much they would impair the Fertility and Fountains and Rivers of the Earth: because the quantity of Vapors, that must be exhaled to supply all these, would be lessened proportionally to the bounds of the Ocean; for the Vapors are not to be measured from the bulk of the Water but from the space of the Surface. So that

that this alſo doth inferr the ſuperlative Wiſdom and Goodneſs of God, that he hath treaſured up the Waters in ſo deep and ſpacious a Storehouſe, *the place that he hath founded and appointed for them.*

Pſal. 104.

Nequaquam nobis divinitus eſſe creatam
Naturam rerum, tanta ſtat prædita culpa.
Principio, quantum cæli tegit impetus ingens,
Inde avidam partem montes Sylvæq; ferarum
Poſſedere, tenent rupes, vaſtæq; paludes,
Et mare, quod late terrarum diſtinet oras.
Lucret *lib.* 5.

X. But ſome men are out of Love with the features and meen of our Earth; they do not like this rugged and irregular Surface, theſe Precipices and Valleys and the gaping Channel of the Ocean. This with them is Deformity, and rather carries the face of a Ruin or a rude and indigeſted Lump of Atoms that caſually convened ſo, than a Work of Divine Artifice. They would have the vaſt Body of a Planet to be as elegant and round as a factitious Globe repreſents it; to be every where ſmooth and equable, and as plain as the *Elyſian Fields.* Let us examin, what weighty reaſons they have to diſparage the preſent conſtitution of Nature in ſo injurious a manner. Why, if we ſuppoſe the Ocean to be dry, and that we look down upon the empty Channel from ſome higher Region of the Air, how horrid and ghaſtly and unnatural would it look? Now admitting this Suppoſition; Let us ſuppoſe too that the Soil of this dry Channel is covered with Graſs and Trees in manner of the Continent, and then ſee what would follow. If a man could be carried aſleep

aſleep and placed in the very middle of this dry Ocean; it muſt be allowed, that he could not diſtinguiſh it from the inhabited Earth; for if the bottom ſhould be unequal with Shelves and Rocks and Precipices and Gulfs; theſe being now apparel'd with a veſture of Plants, would only reſemble the Mountains and Valleys that he was accuſtomed to before; but very probably he would wake in a large and ſmooth Plain: for though the bottom of the Sea were gradually inclin'd and ſloping from the Shore to the middle: yet the additional Acclivity, above what a Level would ſeem to have, would be imperceptible in ſo ſhort a proſpect as he could take of it. So that to make this Man ſenſible what a deep Cavity he was placed in; he muſt be carried ſo high in the Air, till he could ſee at one view the whole Breadth of the Channel, and ſo compare the depreſſion of the Middle with the elevation of the Banks. But then a very ſmall skill in Mathematicks is enough to inſtruct us, that before he could arrive to that diſtance from the Earth, all the inequality of Surface would be loſt to his View: the wide Ocean would appear to him like an even and uniform Plane (uniform as to its Level, though not as to Light and Shade) though every Rock of the Sea was as high as the *Pico* of *Teneriff*. But though we ſhould grant, that the dry Gulf of the

Ocean would appear vaſtly hollow and horrible from the top of a high Cloud: yet what a way of reaſoning is this from the freaks of Imagination, and impoſſible Suppoſitions? Is the Sea ever likely to be evaporated by the Sun, or to be emptied with Buckets? Why then muſt we fancy this impoſſible dryneſs; and then upon that fictitious account calumniate Nature, as deformed and ruinous and unworthy of a Divine Author? Is there then any phyſical deformity in the Fabric of a Human Body; becauſe our Imagination can ſtrip it of its Muſcles and Skin, and ſhew us the ſcragged and knotty Backbone, the gaping and ghaſtly Jaws, and all the Sceleton underneath? We have ſhewed before, that the Sea could not be much narrower than it is, without a great loſs to the World: and muſt we now have an Ocean of mere Flats and Shallows, to the utter ruin of Navigation; for fear our heads ſhould turn giddy at the imagination of gaping Abyſſes and unfathomable Gulfs? But however the Sea-ſhores at leaſt ſhould have been even and uniform, not crooked and broken as they are into innumerable Angles and Creeks and In-lets and Bays, without Beauty or Order, which carry the Marks more of Chance and Confuſion, than of the production of a wiſe Creator. This would be a fine bargain indeed; to part with all our commodious Ports and

and Harbours, which the greater the In-let is, are ſo much the better, for the imaginary pleaſure of an open and ſtreight Shore without any retreat or ſhelter from the Winds; which would make the Sea of no uſe at all as to Navigation and Commerce. But what apology can we make for the horrid deformity of Rocks and Crags, of naked and broken Cliffs, of long Ridges, of barren Mountains; in the convenienteſt Latitudes for Habitation and Fertility, could thoſe rude heaps of Rubbiſh and Ruins be removed out of the way? We have one general and ſufficient anſwer for all ſeeming defects or diſorders in the conſtitution of Land or Sea; that we do not contend to have the Earth paſs for a Paradiſe, or to make a very Heaven of our Globe, we reckon it only as the Land of our *peregrination*, and aſpire after *a better*, Heb. 11. *and a cœleſtial Country*. 'Tis enough, if it be ſo framed and conſtituted, that by a carefull Contemplation of it we have great reaſon to acknowledge and adore the Divine Wiſdom and Benignity of its Author. But to wave this general Reply; let the Objectors conſider, that theſe ſuppoſed irregularities muſt have neceſſarily come to paſs from the eſtabliſh'd Laws of Mechaniſm and the ordinary courſe of Nature. For ſuppoſing the Exiſtence of Sea and Mountains; if the Banks of that Sea muſt never be jagged and torn by the

impetuous aſſaults or the ſilent underminings of Waves; if violent Rains and Tempeſts muſt not waſh down the Earth and Gravel from the tops of ſome of thoſe Mountains, and expoſe their naked Ribbs to the face of the Sun; if the Seeds of ſubterraneous Minerals muſt not ferment, and ſometimes cauſe Earthquakes and furious eruptions of *Volcano's*, and tumble down broken Rocks, and lay them in confuſion: then either all things muſt have been over-ruled miraculouſly by the immediate interpoſition of God without any mechanical Affections or ſettled Laws of Nature, or elſe the body of the Earth muſt have been as fixed as Gold or as hard as Adamant and wholly unfit
Gen. 1. for Our habitation. So that if it *was good* in the ſight of God, that the preſent Plants and Animals, and Human Souls united to Fleſh and Blood ſhould be upon this Earth under a ſettled conſtitution of Nature: theſe ſuppoſed Inconveniences, as they were foreſeen and permitted by the Author of that Nature, as neceſſary conſequences of ſuch a conſtitution; ſo they cannot inferr the leaſt imperfection in his Wiſdom and Goodneſs. And to murmure at them is as unreaſonable, as to complain that he hath made us Men and not Angels, that he hath placed us upon this Planet, and not upon ſome other in this or another Syſtem which may be thought better than Ours. Let them

alſo

alſo conſider, that this objected Deformity is in our Imaginations only, and not really in the Things themſelves. There is no Univerſal Reaſon (I mean ſuch as is not confined to Human Fancy, but will reach through the whole Intellectual Univerſe) that a Figure by us called Regular, which hath equal Sides and Angles, is abſolutely more beautifull than any irregular one. All Pulchritude is relative; and all Bodies are truly and phyſically beautifull under all poſſible Shapes and Proportions; that are good in their Kind, that are fit for their proper uſes and ends of their Natures. We ought not then to believe, that the Banks of the Ocean are really deformed, becauſe they have not the form of a regular Bulwark; nor that the Mountains are miſhapen, becauſe they are not exact Pyramids or Cones; nor that the Starrs are unskilfully placed, becauſe they are not all ſituated at uniform diſtances. Theſe are not Natural Irregularities, but with reſpect to our Fancies only; nor are they incommodious to the true Uſes of Life and the Deſigns of Man's Being on the Earth. Let them conſider, that theſe Ranges of barren Mountains by condenſing the Vapors and producing Rains and Fountains and Rivers, give the very Plains and Valleys themſelves that Fertility they boaſt of. Let them conſider, that thoſe Hills and

and Mountains ſupply Us and the Stock of Nature with a great variety of excellent Plants. If there were no inequalities in the Surface of the Earth, nor in the Seaſons of the Year; we ſhould loſe a conſiderable ſhare of the Vegetable Kingdom: for all Plants will not grow in an uniform Level and the ſame temper of Soil, nor with the ſame degree of Heat. Let them conſider, that to thoſe Hills and Mountains we are obliged for all our Metals, and with them for all the conveniencies and comforts of Life. To deprive us of Metals is to make us mere Savages; to change our Corn or Rice for the old *Arcadian* Diet, our Houſes and Cities for Dens and Caves, and our Cloathing for Skins of Beaſts: 'tis to bereave us of all Arts and Sciences, of Hiſtory and Letters, nay of Revealed Religion too that ineſtimable favour of Heaven, by making the whole Goſpel a mere Tradition and old Cabala without certainty, without authority. Who would part with theſe Solid and Subſtantial Bleſſings for the little fantaſtical pleaſantneſs of a ſmooth uniform Convexity and Rotundity of a Globe? And yet the misfortune of it is, that the pleaſant View of this imaginary Globe, as well as the deformed Spectacle of the true one, is founded upon impoſſible Suppoſitions. For this equal Convexity could never be ſeen and enjoyed by any man

man living. The Inhabitants of ſuch an Earth could have only the ſhort proſpect of a little Circular Plane about three Miles around them; tho' neither Woods nor Hedges nor artificial Banks ſhould intercept it: which little too would appear to have an Acclivity on all ſides from the Spectators; ſo that every man would have the Satisfaction of fancying himſelf the loweſt, and that he always dwelt and moved in a Bottom. Nay, conſidering that in ſuch a conſtitution of the Earth they could have no means nor inſtruments of Mathematical Knowledg; there is great reaſon to believe, that the period of the final Diſſolution might overtake them, ere they would have known or had any Suſpicion that they walked upon a round Ball. Muſt we therefore, to make this Convexity of the Earth diſcernible to the Eye, ſuppoſe a man to be lifted up a great hight in the Air, that he may have a very ſpacious Horizon under one View? But then again, becauſe of the diſtance, the convexity and gibbouſneſs would vaniſh away; he would only ſee below him a great circular Flat, as level to his thinking as the face of the Moon. Are there then ſuch raviſhing Charms in a dull unvaried Flat, to make a ſufficient compenſation *for the chief things of the ancient Mountains, and for the precious things of the laſting Hills?* Deut. 33. 15. Nay we appeal to the ſentence of Mankind;

kind; if *a land of Hills and Valleys* with an infinite Variety of Scenes and Proſpects, beſides the Profit that accrues from it, have not more of Beauty too and Pleaſantneſs than a wide uniform Plain; which if ever it may be ſaid to be very delightfull, is then only, when 'tis viewed from the top of a Hill. What were the *Tempe* of *Theſſaly*, ſo celebrated in ancient ſtory for their unparallelled pleaſantneſs, but a Vale divided with a River & terminated with Hills? Are not all the deſcriptions of Poets embelliſh'd with ſuch Ideas, when they would repreſent any places of ſuperlative Delight, and blisfull Seats of the Muſes or the Nymphs, any ſacred habitations of Gods or Goddeſſes? They will never admit that a wide Flat can be pleaſant, no not in the very *Elyſian Fields* *; but thoſe too muſt be diverſified with depreſſed Valleys and ſwelling Aſcents. They cannot imagin even † Paradiſe to be a place of Pleaſure nor Heaven it ſelf to be || Heaven without them. Let this therefore be another Argument of the Divine Wiſdom & Goodneſs, that the Surface of the Earth is not uniformly Convex (as many think it would naturally have been, if mechanically formed by a Chaos) but diſtinguiſhed

vide Ælian. var. Hiſt. lib. III.

* Virg. Æn. 6. *At pater Anchiſes penitus convalle virenti.* & ibid. *Hoc ſuperate jugum.* & ib. *Et tumulum cœpit.*

† Flours worthy of Paradiſe, which not nice Art
In Beds and curious Knots, but Nature boon
Powr'd forth profuſe on Hill and Dale and Plain.
Paradiſe Loſt, lib. 4.

|| For Earth hath this variety from Heaven
Of Pleaſure ſituate in Hill and Dale.
Ibid. lib. 6.

ſtinguiſhed with Mountains and Valleys, and furrowed from Pole to Pole with the Deep Channel of the Sea; and that becauſe of the τὸ βέλτιον, it is better that it ſhould be ſo.

Give me leave to make one ſhort Inference from what has been ſaid, which ſhall finiſh this preſent Diſcourſe, and with it our Task for the Year. We have clearly diſcovered many Final Cauſes and Characters of Wiſdom and Contrivance in the Frame of the inanimate World; as well as in the Organical Fabrick of the Bodies of Animals. Now from hence ariſeth a new and invincible Argument, that the preſent Frame of the World hath not exiſted from all Eternity. For ſuch an uſefulneſs of things or a fitneſs of means to Ends, as neither proceeds from the neceſſity of their Beings, nor can happen to them by Chance, doth neceſſarily inferr that there was an Intelligent Being, which was the Author and Contriver of that Uſefulneſs. We have formerly demonſtrated, that the Body of a Man, Serm. 5 which conſiſts of an incomprehenſible variety of Parts all admirably fitted for their peculiar Functions and the Conſervation of the Whole, could no more be formed fortuitouſly; than the *Æneis* of *Virgil* or any other long Poem with good Senſe and juſt Meaſures could be compoſed by

the Caſual Combinations of Letters. Now to purſue this Compariſon; as it is utterly impoſſible to be believed, that ſuch a Poem may have been eternal, tranſcribed from Copy to Copy without any firſt Author and Original: ſo it is equally incredible and impoſſible, that the Fabrick of Human Bodies, which hath ſuch excellent and Divine Artifice, and if I may ſo ſay, ſuch good Senſe and true Syntax and harmonious Meaſures in its Conſtitution, ſhould be propagated and tranſcribed from Father to Son without a firſt Parent and Creator of it. An eternal uſefulneſs of Things, an eternal Good Senſe, cannot poſſibly be conceived without an eternal Wiſdom and Underſtanding. But that can be no other than that eternal and omnipotent God; *that by Wiſdom hath*
Prov. 3. *founded the Earth, and by Underſtanding hath eſtabliſhed the Heavens*: To whom be all Honour and Glory and Praiſe and Adoration from henceforth and for evermore. *AMEN.*

FINIS.

V.

Halley and the *Principia*

Halley and the *Principia*

Robert E. Schofield

The association of Edmond Halley and Isaac Newton was long and happy, both for them and for us. From 1684 until Newton's death, Halley seems to have participated in some way in every one of the important developments of Newton's career. In addition to the major role that Halley played in the publication of the *Principia,* we find his name associated with Newton's in connection with the Mint, the *Opticks,* the administration of the Royal Society, and even Newton's work on Biblical chronology. From Brewster, we gather that Halley was involved in the effort to obtain for Newton a position at the Mint.[1] Shortly after Newton began his work as Warden, Halley also began a period of service at the Mint and, from 1696, for two years was Deputy Comptroller of the Mint at Chester, one of five branch mints set up to facilitate the recoinage that took place during the reign of William III. Halley left that position in 1698, when the branch mints were broken up, to sail as Master of H.M.S. *Paramour Pink* on a scientific expedition to study the variation of magnetic declination in various parts of the

[1] Sir David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton* (Edinburgh, 1855), vol. 2, pp. 190–192.

world. When Newton presented a copy of the *Opticks* to the Royal Society in 1704, it was Halley who "was desired to peruse it and give an abstract of it" to the Society.[2] Newton was president of the Royal Society from 1703 to his death in 1727. For eight years of that presidency (1713–1721), Halley was one of the Secretaries of the Royal Society and was editor and publisher of the *Philosophical Transactions* from 1714 to 1719 (he had earlier been editor-publisher of the *Phil. Trans.* from 1685 to 1692). He appears to have been almost as jealous of Newton's reputation as Newton himself, and in 1727, shortly after Newton's death, when an article appeared questioning Newton's chronology, Halley even undertook a partial defense of that, explaining and, in some measure, attempting to justify the method by which Newton had arrived at the dates in question.[3]

The frequently told story of our debt to Halley for promoting the publication of the *Principia* cannot better be epitomized than in the statements Newton made in his Preface signed May 8, 1686:

> In the publication of this work the most acute and universally learned Mr. *Edmund Halley* not only assisted me in correcting the errors of the press and preparing the geometrical figures, but it was through his solicitations that it came to be published; for when he had obtained of me my demonstrations of the figure of the celestial orbits, he continually pressed me to communicate the same to the *Royal Society,* who afterwards, by their kind encouragement and entreaties, engaged me to think of publishing them.[4]

From August 1684, when Halley visited Newton at Cambridge and encouraged the work that resulted in the *Principia,* through the period before the presentation of the first book to the Royal Society, the discovery that the council of the Royal Society was

[2] Quoted from the Journal book of the Royal Society, 16th February 1703/4, by Isaac Weld, *A History of the Royal Society* (London, 1848), vol. 1, p. 375.

[3] *Phil. Trans. 34,* p. 205 (1727).

[4] Sir Isaac Newton, *Mathematical Principles of Natural Philosophy . . . ,* Cajori edition of the English translation of Andrew Motte (Berkeley, California, 1947), p. xviii. See also Brewster, *Memoirs,* vol. 1, pp. 296–299, 304–307; and the contemporary and authoritative information supplied by the letter of June 29, 1686 from Halley to Newton, excerpt quoted by Brewster, vol. 1, pp. 446–447; and printed in entirety by W. W. Rouse Ball, *An Essay on Newton's Principia* (London, 1893), pp. 162–163; and by Stephen P. Rigaud, *Historical Essay on the First Publication of Sir Isaac Newton's Principia* (Oxford, 1838), pp. 35–39.

financially unable to pay for its publication, and Halley's decision to undertake the "business of looking after it, and printing it at his own charge,"[5] Halley was almost as important in the publication as was Newton himself. Moreover, not only did Halley pay for the publication, correct the proofs, check the calculations, and work with the printer; it was even necessary for him to persuade Newton to submit a major portion of the work for publication.[6] There is considerable justification for the belief, frequently expressed, that but for Halley the *Principia* would never have been published.

Under the circumstances, it is not unreasonable that Halley should have made the first public announcement of the publication of the *Principia*. This announcement took the form of a book review in the *Philosophical Transactions*. According to Ball, this was the only real book review of the *Principia* to appear at the time, since the other contemporary reviews, in the *Acta Eruditorum* and *Bibliothèque Universelle* are and purport to be little more than synopses of the contents.[7] That the publisher and, in a sense, editor of the work should be the one to write a review of it may indeed seem odd. It is

[5] T. Birch, *The History of the Royal Society of London* (London, 1756), vol. 4, p. 486. The finances of the Royal Society appear to have been in serious danger owing to their publication of Willughby's *De historia piscium*. While there seems general agreement that Halley was not ultimately a loser because of his undertaking, in spite of the initial risk involved, there is some disagreement as to Halley's ability, at the time, to afford such a risk. Both Ball, *Essay*, pp. 67–68, and Rigaud, *Historical Essay*, p. 36, seem to feel (in Rigaud's words) that Halley undertook to meet the expense of publishing the *Principia* "precisely at that period of his life when he could least afford it." Sir Henry Lyons, *The Royal Society* (Cambridge, 1944), p. 103, states that Halley was "in fairly comfortable circumstances when he undertook to finance . . . the 'Principia'." Though it is not easy at this point to resolve this difference, some support is given to the opinion of Rigaud and Ball by the fact that a large portion of Halley's income up to 1684 had been an allowance from his father. The death intestate of his father in 1684 instituted a long litigation between Halley and his stepmother over Halley's patrimony, which was not settled until 1693.

[6] Newton had taken offense at some claims to priority made by Hooke and suggested, in a letter to Halley, that the third book, "De Systemate Mundi," be suppressed. This letter, quoted by Rigaud, p. 63, by Ball, pp. 158–159, and by Brewster, vol. 1, pp. 439–445, contains that familiar passage: "Philosophy is such an impertinently litigious lady, that a man had as good be engaged in lawsuits, as have to do with her. I found it so formerly, and now I am no sooner come near her again, but she gives me warning."

[7] Ball, *Essay*, p. 68; *cf. Acta Eruditorum* 305–315 (June 1688), *Bibliothèque Universelle 8*, 436–450 (Mar. 1688). Concerning these reviews, see I. B. Cohen: *Introduction to Newton's 'Principia'* (Cambridge, 1971), pp. 145–56.

true, however, that next to Newton few other persons were more capable of reviewing a book of that scope—and certainly Halley is not the last reviewer to have an interest, personal or financial, in the success of a book he reviews.[8]

It is not surprising that the publication of the *Principia* should today be regarded as one of the most important events in the history of science. For over two hundred fifty years the work has been tested and, in that time, its real stature has scarcely been reduced. What is perhaps surprising, and is certainly to their credit, is that large numbers of Newton's contemporaries, scientific and not, recognized its importance. While the *Principia* was being written, the Royal Society was kept informed of its progress and frequently expressed its interest. Only the serious depletion of its treasury prevented the Society from financing the publication. Because of the printing laws of the period, a book could not be published without a license and the first edition of the *Principia* bears the imprimatur of Samuel Pepys, as President of the Royal Society.[9] From at least as early as a Star Chamber decree of 1637, the English government had made a formal attempt to control book publishing by a licensing procedure. The Commonwealth adopted its own technics of censorship, but after the Restoration the decree of 1637 was renewed, in substance, by parliament in 1662 (13 and 14 Car. II, c. 33) and again in 1685 (1 Jac. II, c. 8, §15). In spite of the zeal of some of its enforcers, this attempt at control was never wholly effective; books were published and new printers established themselves without regard for the law. But Newton was not the person nor the *Principia* the type of book to publish outside the law. By provision of the act of 1685, the *Principia* could be licensed by the Archbishop of Canterbury, the Bishop of London, the Chancellors or Vice-chancellors of Oxford or Cambridge, or the representatives of any of these. Finally, though not listed in act of 1662 or of 1685, the President, Council, and Fellows of the

[8] Sherman B. Barnes, "The Editing of Early Learned Journals," *Osiris 1,* 160 (1936), note 27, states: "There were instances of authors sending reviews of their own books to editors. Leibniz publicized himself through the journals of the time."

[9] Samuel Pepys, though best known to us as the author of a charming but indiscreet diary, was a highly efficient administrator, secretary to the admiralty, and a dedicated president to the Royal Society for two years, December 1, 1684 to November 30, 1686.

Royal Society could license it by the authority granted them in their charters.[10]

Probably none of the persons legally competent to sign an imprimatur was capable of reading and understanding the full significance of the *Principia,* but, under the circumstances, there is no doubt that the proper authority for licensing it would be the Council and President of the Royal Society. The *Philosophical Transactions* was regularly issued under the imprimatur of the President of the Society and Halley, as publisher of the *Phil. Trans.,* was acquainted with the Royal Society printers, with editing and printing procedures, and with the licensing problems.

There is no indication that any other licensing authority was considered, but we may reasonably ask what would have happened had the Society failed to approve the publication of the *Principia.* Scientific books were licensed and published through trade channels throughout this period, the licensing being done on application of the publisher and usually because he stood responsible for the contents of the book. But English booksellers were notoriously reluctant to publish scientific books of a mathematical nature,[11] and one may reasonably doubt that a trade bookseller would have solicited a publishing license for the *Principia.* Neither Oxford nor Cambridge was interested in books of this sort, and, in any event, the problem of getting an imprimatur outside the Society might well seem to present complications beyond even Halley's enthusiasm. In this sense, one can suggest that, without the approval of a society of men most of whom were probably unable to read and understand it, and the signature of a man who, able as he was in many respects, certainly did not understand it, the publication of the *Principia* might not have been possible.

[10] For the provisions of the licensing acts, see, for example, *The Term Catalogues, 1668–1709 A.D.,* ed. Edward Arber (London, 1903), vol. 1, pp. x ff; *A Transcript of the Registers of the Company of Stationers of London; 1554–1640,* ed. Edward Arber (London, 1875), vol. 1, pp. xvi ff; or any standard work on English printing history, as Henry R. Plomer, *Short History of English Printing* (New York, 1927). For the provisions of the charters of the Royal Society, see, for example, Lyons, *Royal Society,* pp. 329–338.

[11] See A. N. L. Munby, "The Distribution of the First Edition of Newton's *Principia,*" *Notes and Records of the Royal Society of London 10,* 29–31, (1952) for a discussion of this problem.

Indeed, one of the most striking things about the *Principia* is the interest of nonscientists in a book that they could not read. Not that every physical scientist could understand it either; then, as today, there were probably many more scientists who claimed to have read it than there were who actually had, but the book was written in a style that the scientists, at least, were equipped to understand. The *Principia* is an austere book, written in Latin and using the geometrical methods of Apollonius which Newton made obsolete with his invention of fluxions. It was, however, probably less austere to its day than even the English translation is today, for Latin was still the language of science in 1687 and the mathematical tools of geometry had been known to generations of scientists who had yet to learn the fluxions. Because of the substitution of calculus for geometrical methods of analysis, scientists today are almost in the position of the learned nonscientists of the late 17th century and we can sympathize with men who, like Dr. Richard Bentley, were told that they must read upwards of forty books, mostly on geometry, for the "shortest and most proper method for such an end" as to understand the *Principia.*[12] Bentley, it is true, wrote to Newton and got a shorter list of books, instructions that for a "first perusal" it was "enough if you understand the Propositions with some of the Demonstrations which are easier than the rest,"[13] and, as we have seen in the previous section, some letters of specific explanation. This was a course that most nonscientists were not prepared to follow. John Locke wrote to Huygens to find out the soundness of the mathematical demonstrations and, "being told that he might depend upon their certainty; he took them for granted, and carefully examined the Reasonings and Corollaries drawn from them, became Master of all the Physics and was fully convinc'd of all the great Discoveries contained in that Book."[14] For most people, however, knowledge of what the *Principia* contained was acquired through popularizations and simplified extracts from it. Newton, himself, had originally intended to write

[12] Letter of John Craige to Bentley printed in Brewster, *Memoirs,* vol. 1, p. 340, and appendix, pp. 460 ff.

[13] *Ibid,* p. 464.

[14] Desaguliers, *Course of Experimental Philosophy,* 3rd ed. (London, 1763), vol. 1, p. viii.

the third book, "De Systemate Mundi," in a popular style "that it might be read by the many," but had changed his mind.[15] This left a gap which was rapidly filled by numerous authors such as Voltaire, Desaguliers, Pemberton, and others, who wrote books specifically intended, as Pemberton says, "to convey to such, as are not used to mathematical reasoning, some idea of the philosophy of a person, who has acquired an universal reputation, and rendered our nation famous for these speculations in the learned world."[16]

One of the most interesting of these popularizations was that prepared by Halley for James II. The publication of the *Principia* was considered so important that a special meeting was appointed for the purpose of presenting a copy of it to the King.[17] Halley accompanied the presentation with a paper that contained an outline of the book and gave a special explanation of the doctrine of tides. This paper was printed separately and then later reprinted in the *Philosophical Transactions* with the beginning and end omitted. These omissions, not included in the section reproduced below, read as follows:

To King James II. 1687
May it please Your most Excellent Majesty.

I could not have presumed to approach Your Majesties Royall presence with a Book of this Nature, had I not been assured, that when the weighty affaires of Your Government permit it; Your Majesty has frequently shown Yourself enclined to favour Mechanicall and Philosophicall discoveries: And I may be bold to say, that if ever Book was worthy the favourable acceptance of a Prince, this, wherein so many and so great discoveries concerning the constitution of the Visible World are made out, and put past dispute, must needs be gratefull to Your Majestie; Being especially the labours of a worthy subject of your own, and a member of the Royall Society founded by Your late Royall

[15] Cajori edition of the *Principia,* p. 397. After Newton's death, there was printed *The System of the World demonstrated in an easy and popular manner by the illustrious Sir Isaac Newton* which is included in the Cajori edition and which is described by Rigaud, *Historical Essay,* p. 78, as a translation from the original Latin of the first draft of what formed the third book.

[16] H. Pemberton, *A View of Sir Isaac Newton's Philosophy* (London, 1728), preface.

[17] Newton personally presented a copy of the second edition to Queen Anne in 1713.

Brother for the advancement of Naturall knowledge, and which now flourishes under your Majesties most Gracious Protection.

But being sencible of the little leisure which care of the Publick leaves to Princes, I believed it necessary to present with the Book a short Extract of the matters contained, together with a Specimen thereof, in the genuine Solution of the Cause of the Tides in the Ocean. A thing frequently attempted But till now without success. Whereby Your Majestie may Judge of the rest of the Performances of the Author.

The body of the letter is reproduced in facsimile below.

If by reason of the difficulty of the matter there be anything herein not sufficiently Explained, or if there be any materiall thing observable in the Tides that I have omitted wherein Your Majestie shall desire to be satisfied, I doubt not but if Your Majesty shall please to suffer me to be admitted to the honour of Your Presence, I may be able to give such an account thereof as may be to Your Majesties full content:

I am Great Sir, Your Majesties most Dutifull & obedient Subject

EDMOND HALLEY[18]

Despite the courtly language, there is reason to doubt that James did, or could, take an interest in the *Principia*. Far from having frequently shown himself "enclined to favour Mechanicall and Philosophicall discoveries," James gave little evidence of being interested in science. Although he had become a Fellow of the Royal Society (as Duke of York) on the same occasion that his brother Charles II had signed the Charter book as Patron, there is no indication that he ever attended another meeting of the Society or emulated Charles or his uncle, Prince Rupert, in performing private experiments. One may reasonably suppose that it was the expected lack of understanding, rather than any tribute to interest, that prompted this choice of extract. The only technical interest of James was the Navy, which most likely explains the choice of tides as the subject of Halley's discourse.

[18] Quoted in E. F. MacPike, *Correspondence and Papers of Edmond Halley* (Oxford, 1932), p. 85. I have spelled out the abbreviations. In the Pepysian library, Magdalene College, Cambridge, one may find the diarist's own copy of the first edition of the *Principia;* bound with it is an example of the first printing of Halley's letter, described by A. N. L. Munby, *op. cit.,* p. 33 (with a facsimile of one page of this letter).

II. *Philoſophiæ Naturalis Principia Mathematica, Autore* Iſ. Newton Trin. Coll. Cantab. *Soc. Matheſeos Profeſſore* Lucaſiano, *&* *Societatis Regalis Sodali.* 4to. Londini. *Proſtat apud plures Bibliopolas.*

THis incomparable Author having at length been prevailed upon to appear in publick, has in this Treatiſe given a moſt notable inſtance of the extent of the powers of the Mind; and has at once ſhewn what are the Principles of Natural Philoſophy, and ſo far derived from them their conſequences, that he ſeems to have exhauſted his Argument, and left little to be done by thoſe that ſhall ſucceed him. His great skill in the old and new Geometry, helped by his own improvements of the latter, (I mean his method of *infinite Series*) has enabled him to maſter thoſe Problems, which for their difficulty would have ſtill lain unreſolved, had one leſs qualified than himſelf attempted them.

This Treatiſe is divided into three Books, whereof the two firſt are entituled *de Motu Corporum*, the third *de Syſtemate Mundi.*

The firſt begins with definitions of the Terms made uſe of, and diſtinguiſhes *Time*, *Space*, *Place* and *Motion* into abſolute and relative, real and apparent, Mathematical and vulgar: ſhewing the neceſſity of ſuch diſtinction. To theſe definitions are ſubjoyned, the Laws of Motion, with ſeveral Corollaries therefrom; as concerning the compoſition and reſolution of any direct force out of, or into any oblique forces, (whereby the powers of all ſorts of Mechanical Engines are demonſtrated:) the Laws of the reflection of Bodies in Motion after their Collision: and the like.

These necessary *Præcognita* being delivered, our Author proceeds to consider the Curves generated by the composition of a direct impressed motion with a gravitation or tendency towards a Center: and having demonstrated that in all cases the Areas at the Center, described by a revolving Body, are proportional to the Times; he shews how from the Curve described, to find the Law or Rule of the decrease or increase of the Tendency or Centripetal forces (as he calls it) in differing distances from the Center. Of this there are several examples: as if the Curve described be a Circle passing through the Center of tendency; then the force or tendency towards that Center is in all points as the fift power or squared-cube of the distance therefrom reciprocally. If in the proportional Spiral, reciprocally as the cube of the distance. If in an Ellipse about the Center thereof directly as the distance. If in any of the *Conick* Sections about the *Focus* thereof; then he demonstrates that the *Vis Centripeta*, or tendency towards that *Focus*, is in all places reciprocally as the square of the distance therefrom; and that according to the Velocity of the impressed Motion, the Curve described is an *Hyperbola*; if the Body moved be swift to a certain degree than a *Parabola*; if slower an *Ellipse* or Circle in one case. From this sort of tendency or gravitation it follows likewise that the squares of the Times of the periodical Revolutions are as the Cubes of the *Radii* or *transverse Axes* of the *Ellipses*. All which being found to agree with the *Phenomena* of the Celestial Motions, as discovered by the great Sagacity and Diligence of *Kepler*, our Author extends himself upon the consequences of this sort of *Vis centripeta*; shewing how to find the *Conick* Section which a Bodie shall describe when cast with any velocity in a given Line, supposing the quantity of the said force known: and laying down several neat constructions to determine

termine the Orbs,either from the *Focus* given and two points or Tangents; or without it by five points or Tangents or any number of Points and Tangents making together five. Then he ſhews how from the Time given to find the Point in a given Orb anſwering thereto; which he performs accurately in the *Parabola*,and by conciſe approximations comes as near as he pleaſes in the *Ellipſe* and *Hyperbola*: all which are Problems of the higheſt concern in Aſtronomy. Next he lays down the Rules of the perpendicular deſcent of Bodies towards the Center, particularly in the caſe where the tendency thereto is reciprocally as the ſquare of the diſtance; and generally in all other caſes, ſuppoſing a general quadrature of Curve lines: upon which ſuppoſion likewiſe he delivers a general method of diſcovering the Orbs deſcribed by a Body moving in ſuch a tendency towards a Center, increaſing or decreaſing in any given relation to the diſtance from the Center; and then with great ſubtilty he determines in all caſes the Motion of the *Apſides* (or of the Points of greateſt diſtance from the Center in all theſe *Curves*, in ſuch Orbs as are nearly Circular. Shewing the *Apſides* fixt, if the tendency be reciprocally as the ſquare of the diſtance; direct in Motion in any *Ratio* between the Square and the Cube and retrograde; if under the Square: which Motion he determines exactly from the Rule of the increaſe or decreaſe of the *Vis Centripeta.*

Next the Motion of bodies in given Surfaces is conſidered,as likewiſe the Oſcillatory Motion of Pendules, where is ſhewn how to make a *Pendulum* Vibrate always in equal times, tho' the center or point of tendency be never ſo near; to which, the Demonſtration of Mr. *Hugens de Cycloide* is but a *Corollary*. And in another Propoſition is ſhewn the Velocity in each Point, and the time ſpent in each part of the Arch deſcribed by the Vibrating Body. After this the Effects of two or more Bodies, towards each of which there is a tendency, is conſidered; and 'tis made out that two Bodies, ſo drawing or attracting each other, deſcribe

Oo 2 about

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about the common center of Gravity, Curve Lines, like to thoſe they ſeem to deſcribe about one another. And of three Bodies, attracting each other, reciprocally as the Square of the diſtance between their Centers, the various Conſequences are conſidered and laid down, in ſeveral *Corollarys* of great uſe in explicating the *Phenomena* of the *Moons* Motions, the Flux and Reflux of the Sea, the Preceſſion of the *Equinoctial* Points; and the like.

This done our Author with his uſual Acuteneſs proceeds to examine into the Cauſes of this Tendency or centripetal Force, which from undoubted Arguments is ſhown to be in all the great Bodies of the Univerſe. Here he finds that if a Sphere be compoſed of an infinity of Atoms, each of which have a *Conatus accedendi ad invicem*, which decreaſes in duplicate Proportion of the Diſtance between them; then the whole *Congeries* ſhall have the like tendency towards its Center, decreaſing, in Spaces without it, in duplicate Proportion of the Diſtances from the Center; and decreaſing, within its Surface, as the diſtance from the Center directly; ſo as to be greateſt on the Surface, and nothing at the Center: and tho' this might ſuffice, yet to compleat the Argument, there is laid down a Method to determine the forces of Globes compoſed of Particles whoſe Tendencies to each other do decreaſe in any other *Ratio* of the Diſtances: Which Speculation is carryed on likewiſe to other Bodies not Spherical, whether finite or indeterminate. Laſtly is propoſed a Method of explaining the Refractions and Reflections of tranſparent Bodies from the ſame Principles; and ſeveral Problems ſolved of the greateſt Concern in the Art of *Dioptricks*.

Hitherto our Author has conſidered the Effects of compound Motions *in Mediis non reſiſtentibus*, or wherein a Body once in Motion would move equably in a direct Line, if not diverted by a ſupervening Attraction or tendency toward ſome other Body. Here is demonſtrated what would

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would be the conſequence of a reſiſtence from a *Medium*, either in the ſimple or duplicate *Ratio* of the Velocity, or elſe between both: and to compleat this Argument is laid down a general Method of determining the denſity of the *Medium* in all places, which, with a uniform Gravity tending perpendicularly to the plain of the *Horizon*, ſhall make a *Project* move in any curve Line aſſigned; which is the 10*th*. *Prop*. *Lib*. II. Then the circular Motion of Bodies in reſiſting *Media* is determined, and 'tis ſhown under what Laws of decreaſe of Denſity, the Circle will become a proportional Spiral. Next the denſity and compreſſion of Fluids is conſidered, and the Doctrine of *Hydroſtaticks* demonſtrated; and here 'tis propoſed to the Contemplation of Natural Philoſophers, whether the ſurprizing *Phenomena* of the Elaſticity of the Air and ſome other Fluids may not ariſe from their being compoſed of Particles which flie each other; which being rather a Phyſical than Mathematical Inquiry, our Author forbears to Diſcuſs.

Next the Oppoſition of the *Medium* and its Effects on the Vibrations of the *Pendulum* is conſidered, which is followed by an Inquiry into the Rules of the Oppoſition to Bodies, as their Bulk, Shape, or Denſity may be varyed: Here with great exactneſs is an Account given of ſeveral Experiments tried with *Pendula*, in order to verify the aforegoing Speculation, and to determine the quantity of the Airs Oppoſition to Bodies moving in it.

From hence is proceeded to the undulation of Fluids, the Laws whereof are here laid down, and by them the Motion and Propagation of Light and Sound are explained. The laſt *Section* of this Book is concerning the Circular Motion of Fluids, wherein the Nature of their *Vortical* Motions is conſidered, and from thence the *Carteſian* Doctrine of the *Vortices* of the Celeſtial Matter carrying with them the Planets about the *Sun*, is proved to be alltogether impoſſible.

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The III. and last Book is entituled *de Systemate Mundi*, wherein the Demonstrations of the two former Books are applyed to the Explication of the principal *Phenomena* of Nature: Here the verity of the *Hypothesis* of *Kepler* is demonstrated; and a full Resolution given to all the difficulties that occur in the *Astronomical* Science; they being nothing else but the necessary consequences of the *Sun*, *Earth*, *Moon*, and *Planets*, having all of them a gravitation or tendency towards their Centers proportionate to the Quantity of Matter in each of them, and whose Force abates in duplicate proportion of the Distance reciprocally. Here likewise are indisputably solved the Appearances of the Tides, or Flux and Reflux of the Sea; and the Spheroidical Figure of the *Earth* and *Jupiter* determined, (from which the precession of the Equinoxes, or rotation of the Earths Axis is made out,) together with the retrocession of the *Moons* Nodes, the Quantity and inequalities of whose Motion are here exactly stated *a priore*: Lastly the Theory of the Motion of Comets is attempted with such success, that in an Example of the great Comet which appeared in 168$\frac{0}{1}$, the Motion thereof is computed as exactly as we can pretend to give the places of the primary Planets; and a general Method is here laid down to state and determine the *Trajectoriæ* of Comets, by an easy Geometrical Construction; upon supposition that those Curves are *Parabolick*, or so near it that the *Parabola* may serve without sensible Error; tho' it be more probable, saith our Author, that these Orbs are *Elliptical*, and that after long periods Comets may return again. But such *Ellipses* are by Reason of the immense distance of the *Foci*, and smallness of the *Latus Rectum*, in the Parts near the Sun where Comets appear, not easily distinguished from the Curve of the *Parabola*: as is proved by the Example produced.

The whole Book is interspersed with *Lemma's* of General use in *Geometry*, and several new Methods applyed, which

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which are well worth the confidering; and it may be juftly faid, that fo many and fo Valuable *Philofophical Truths*, as are herein difcovered and put paft Difpute, were never yet owing to the Capacity and Induftry of any one Man.

ADVERTISEMENT;

Whereas the Publication of thefe Tranfactions *has for fome Months laft paft been interrupted; The Reader is defired to take notice that the care of the Edition of this Book of Mr.* Newton *having lain wholly upon the Publifher (wherein he conceives he hath been more ferviceable to the Commonwealth of Learning) and for fome other preffing reafons, they could not be got ready in due time; but now they will again be continued as formerly, and come out regularly, either of three fheets, or five with a Cutt; according as Materials fhall occur.*

LONDON,

Printed by *J. Streater*, and are to be fold by *Samuel Smith* at the *Princes Arms* in St. *Paul's* Church-yard.

II. *The true Theory of the Tides, extracted from that admired Treatise of Mr.* Isaac Newton, *Intituled,* Philosophiæ Naturalis Principia Mathematica; *being a Discourse presented with that Book to the late King* James, *by Mr.* Edmund Halley.

IT may, perhaps, seem strange, that this Paper, being no other than a partile Account of a Book long since published, and whereof a fuller Extract was given in Numb. 187. *of these Transactions, should again appear here; but the Desires of several honourable Persons, which could not be withstood, have obliged us to insert it here, for the sake of such, who being less knowing in Mathematical Matters; and therefore, not daring to adventure on the Author himself, are notwithstanding, very curious to be informed of the Causes of Things; particularly of so general and extraordinary* Phænomena, *as are those of the Tides. Now this Paper having been drawn up for the late King* James's *Use, (in whose Reign the Book was published) and having given good Satisfaction to those that got Copies of it; it is hoped the Savans of the higher Form will indulge us this liberty we take to gratifie their Inferiours in point of Science; and not be offended, that we here insist more largely upon Mr.* Newton's Theory of the Tides, *which, how plain and easie soever we find, is very little understood by the common Reader.*

The sole Principle upon which this Author proceeds to explain most of the great and surprising Appearances of Nature, is no other than that of *Gravity*, whereby in the Earth all Bodies have a tendency towards its Centre;

as is moſt evident: and from undoubted Arguments its proved, that there is ſuch a Gravitation towards the Centre of the Sun, Moon, and all the Planets.

From this Principle, as a neceſſary Conſequence, follows the Sphærical Figure of the Earth and Sea, and of all the other Cæleſtial Bodies: and tho' the tenacity and firmneſs of the Solid Parts, ſupport the Inequalities of the Land above the Level; yet the Fluids, preſſing equally and eaſily yielding to each other, ſoon reſtore the *Æquilibrium*, if diſturbed, and maintain the exact Figure of the Globe.

Now this force of Deſcent of Bodies towards the Center, is not in all places alike, but is ſtill leſs and leſs, as the diſtance of the Center encreaſes: and in this Book it is demonſtrated, that this Force decreaſes as the Square of the diſtance increaſes; that is, the weight of Bodies and the force of their Fall is leſs, in parts more removed from the Center, in the proportion of the Squares of the Diſtance. So as for Example, a Ton weight on the Surface of the Earth, if it were raiſed to the height of 4000 Miles, which I ſuppoſe the ſemidiamiter of the Earth, would weigh but $\frac{1}{4}$ of a Ton, or 5 Hundred weight: if to 12000 Miles, or 3 ſemidiameters from the Surface, that is 4 from the Center, it would weigh but $\frac{1}{16}$ part of the Weight on the Surface, or a Hundred and Quarter: So that it would be as eaſie for the Strength of a Man at that height to carry a Ton weight, as here on the Surface a 100 $\frac{1}{4}$. And in the ſame Proportion does the Velocities of the fall of Bodies decreaſe: For whereas on the Surface of the Earth all things fall 16 Foot in a ſecond, at one ſemidiameter above this Fall is but 4 Foot; and at 3 ſemidiameters, or 4 from the Centre, it is but $\frac{1}{16}$ of the Fall at the Surface, or but one Foot in a ſecond: And at greater Diſtances both Weight and Fall become very ſmall,

ſmall, but yet at all given Diſtances is ſtill ſome thing, tho' the Effect become inſenſible. At the diſtance of the Moon (which I will ſuppoſe 60 Semidiameters of the Earth) 3600 Pounds weigh but one Pound, and the fall of Bodies is but $\frac{16}{3600}$ of a Foot in a ſecond, or 16 Foot in a minute; that is, a Body ſo far off deſcends in a Minute no more than the ſame at the Surface of the Earth would do in a Second of Time.

As was ſaid before, the ſame force decreaſing after the ſame manner is evidently found in the Sun, Moon, and all the Planets; but more eſpecially in the Sun, whoſe Force is prodigious; becoming ſenſible even in the immenſe diſtance of *Saturn:* This gives room to ſuſpect, that the force of Gravity is in the Celeſtial Globes proportional to the quantity of Matter in each of them: And the Sun being at leaſt ten Thouſand times as big as the Earth, its Gravitation or attracting Force, is found to be at leaſt ten Thouſand times as much as that of the Earth, acting on Bodies at the ſame diſtances.

This Law of the decreaſe of Gravity being demonſtratively proved, and put paſt contradiction; the Author with great Sagacity, inquires into the neceſſary Conſequences of this Suppoſition; whereby he finds the genuine Cauſe of the ſeveral Appearances in the Theory of the Moon and Planets, and diſcovers the hitherto unknown Laws of the Motion of Comets, and of the Ebbing and Flowing of the Sea. Each of which are Subjects that have hitherto taken up much larger Volumes; but Truth being uniform, and always the ſame, it is admirable to obſerve how eaſily we are enabled to make out very abſtruſe *and difficult Matters,* when once true and genuine Principles are obtained: And on the other hand it may be wondred, that, notwithſtanding the great facility of truth, and the perplexity and nonconſequences that always attend erroneous Suppoſitions, theſe

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great Diſcoveries ſhould have eſcaped the acute Diſquiſitions of the beſt Philoſophical Heads of all paſt Ages, and be reſerved to theſe our Times. But that wonder will ſoon ceaſe, if it be conſidered how great Improvements Geometry has received in our Memory, and particularly from the profound Diſcoveries of our incomparable Author.

The Theory of the Motion of the primary *Planets* is here ſhewn to be nothing elſe, but the contemplation of the Curve Lines which Bodies caſt with a given Velocity, in a given Direction, and at the ſame time drawn towards the Sun by its gravitating Power, would deſcribe. Or, which is all one, that the Orbs of the Planets are ſuch Curve Lines as a Shot from a Gun deſcribes in the Air, being caſt according to the direction of the Piece, but bent into a crooked Line by the ſupervening Tendency towards the Earths Centre: And the Planets being ſuppoſed to be projected with a given Force, and attracted towards the Sun, after the aforeſaid manner, are here proved to deſcribe ſuch Figures, as anſwer punctually to all that the Induſtry of this and the laſt Age has obſerved in the Planetary Motions. So that it appears, that there is no need of ſolid Orbs and Intelligences, as the Ancients imagined, nor yet of *Vortices* or Whirlpools of the Celeſtial Matter, as *Des Cartes* ſuppoſes; but the whole Affair is ſimply and mechanically performed, upon the ſole Suppoſition of a Gravitation towards the Sun; which cannot be denied.

The Motion of *Comets* is here ſhewn to be compounded of the ſame Elements, and not to differ from Planets, but in their greater ſwiftneſs, whereby overpowering the Gravity that ſhould hold them to the Sun, as it doth the Planets, they flie off again, and diſtance themſelves from the Sun and Earth, ſo that they ſoon are out of our ſight. And the imperfect Accounts and Obſer-

Obſervations Antiquity has left us, are not ſufficient to determine whether the ſame Comet ever return again. But this Author has ſhewn how Geometrically to determine the Orb of a Comet from Obſervations, and to find his diſtance from the Earth and Sun, which was never before done.

The third thing here done is the Theory of the Moon, all the Inequalities of whoſe Motion are proved to ariſe from the ſame Principles, only here the effect of two Centers operating on, or attracting a projected Body comes to be conſidered; for the Moon, tho' principally attracted by the Earth, and moving round it, does, together with the Earth, move round the Sun once a Year, and is according as ſhe is, nearer or farther from the Sun, drawn by him more or leſs than the Center of the Earth, about which ſhe moves; whence ariſe ſeveral Irregularities in her Motion, of all which, the Author in this Book, with no leſs Subtility than Induſtry, has given a full Account. And tho' by reaſon of the great Complication of the Problem, he has not yet been able to make it purely Geometrical, 'tis to be hoped, that in ſome farther Eſſay he may ſurmount the difficulty: and having perfected the Theory of the Moon, the long deſired diſcovery of the Longitude (which at Sea is only practicable this way) may at length be brought to light, to the great Honour of your Majeſty and Advantage of your Subjects.

All the ſurprizing Phenomena of the Flux and Reflux of the Sea, are in like manner ſhewn to proceed from the ſame Principle; which I deſign more largely to inſiſt on, ſince the Matter of Fact is in this caſe much better known to your Majeſty than in the foregoing.

If the Earth were alone, that is to ſay, not affected by the Actions of the Sun and Moon, it is not to be doubted, but the Ocean, being equally preſſed by the force

force of Gravity towards the Center, would continue in a perfect ſtagnation, always at the ſame height, without ever Ebbing or Flowing ; but it being here demonſtrated, that the Sun and Moon have a like Principle of Gravitation towards their Centers, and that the Earth is within the Activity of their Attractions, it will plainly follow, that the Equality of the preſſure of Gravity towards the Center will thereby be diſturbed ; and tho' the ſmallneſs of theſe Forces, in reſpect of the Gravitation towards the Earths Center, renders them altogether imperceptible by any Experiments we can deviſe, yet the Ocean being fluid and yielding to the leaſt force, by its riſing ſhews where it is leſs preſt, and where it is more preſt by its ſinking.

Now if we ſuppoſe the force of the Moons attraction to decreaſe as the Square of the Diſtance from its Center increaſes (as in the Earth and other Celeſtial Bodies) we ſhall find,that where the Moon is perpendicularly either above or below the Horizon, either in Zenith or Nadir, there the force of Gravity is moſt of all diminiſhed, and conſequently that there the Ocean muſt neceſſarily ſwell by the coming in of the Water from thoſe parts where the Preſſure is greateſt, *viz.* in thoſe places where the Moon is near the Horizon : but that this may be the better underſtood, I thought it needful to add the following Figure, where *M* is the Moon, *E* the Earth, *C* its Centre, and *Z* the place where the Moon is in the Zenith, *N* where in the Nadir.

Now

Now by the Hypothesis it is evident, that the Water in *Z*, being nearer, is more drawn by the Moon, than the Center of the Earth *C*, and that again more tha the Water in *N*, wherefore the Water in *Z* has a tendency towards the Moon, contrary to that of Gravity, being equal to the Excess of the Gravitation in *Z*, above that in *C*: And in the other case, the Water in *N*, tending less towards the Moon than the Center *C*, will be less pressed, by as much as is the difference of the Gravitations towards the Moon in *C* and *N*. This rightly understood, it follows plainly, that the Sea, which otherwise would be Spherical, upon the Pressure of the Moon, must form it self into a Spheroidal or Oval Figure, whose longest Diameter is where the Moon is Vertical, and shortest where she is in the Horizon; and that the Moon shifting her Position as she turns round the Earth once a day, this Oval of Water shifts with her, occasioning thereby the two Floods and Ebbs observable in each 25 Hours.

And this may suffice as to the general Cause of the Tides; it remains now to shew how naturally this Motion accounts for all the Particulars that has been observed about them; so that there can be no room left to doubt, but that this is the true cause thereof.

The Spring Tides upon the new and full Moons, and Neap Tides on the Quarters, are occasioned by the attractive Force of the Sun in the New and Full, conspiring with the Attraction of the Moon, and producing a Tide by their united Forces: Whereas in the Quarters, the Sun raises the Water where the Moon depresses it, and the contrary; so as the Tides are made only by the difference of their Attractions. That the force of the Sun is no greater in this case, proceeds from the very small Proportion the Semidiameter of the Earth bears to the vast distance of the Sun.

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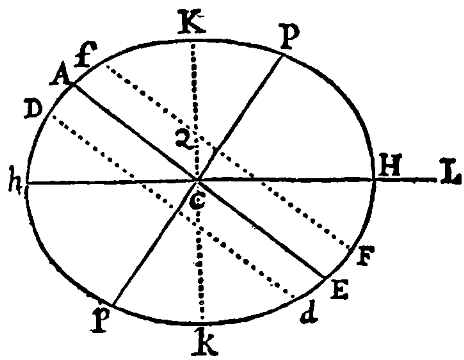
It is alſo obſerved, that *cæteris paribus*, the Æquinoctial Spring Tides in *March* and *September*, or near them, are the Higheſt, and the Neap Tides the Loweſt; which proceeds from the greater Agitation of the Waters, when the fluid *Sphæroid* reſolves about a great Circle of the Earth, than when it turns about in a leſſer Circle; it being plain, that if the Moon were conſtituted in the Pole and there ſtood, that the Sphæroid would have a fixt Poſition, and that it would be always high Water under the Poles, and low Water every where under the Æquinoctial:. and therefore the nearer the Moon approaches the Poles, the leſs is the agitation of the Ocean, which is of all the greateſt, when the Moon is in the Æquinoctial, or fartheſt diſtant from the Poles. Whence the Sun and Moon, being either conjoyned or oppoſite in the Æquinoctial, produce the greateſt Spring Tides; and the ſubſequent Neap Tides, being produced by the Tropical Moon in the Quarters, are always the leaſt Tides; whereas in *June* and *December*, the Spring Tides are made by the Tropical Sun and Moon, and therefore leſs vigorous; and the Neap Tides by the Æquinoctial Moon, which therefore are the ſtronger: Hence it happens, that the difference between the Spring and Neap Tides in theſe Months, is much leſs conſiderable than in *March* and *September*. And the reaſon why the very higheſt Spring Tides are found to be rather before the Vernal and after the Antumnal Equinox, *viz.* in *February* and *October*, than preciſely upon them, is, becauſe the Sun is nearer the Earth in the Winter Months, and ſo comes to have a greater Effect in producing the Tides.

Hitherto we have conſidered ſuch Affections of the Tides as are Univerſal, without relation to particular Caſes; what follows from the differing Latitudes of places, will be eaſily underſtood by the following Figure.

Let

Let *A p E P* be the Earth covered over with very deep Waters, *C* its Center, *P*, *p*, its Poles, *A E* the Æquinoctial, *F f* the parallel of Latitude of a place, *D d* another Parallel at equal diſtance on the other ſide of the Æquinoctial, *H h* the two Points where the Moon is vertical, and let *K k* be the great Circle, wherein the Moon appears Horizontal. It is evident, that a Spheroid deſcribed upon *Hh*, and *Kk* ſhall nearly repre-



ſent the Figure of the Sea, and *C f*, *C D*, *C F*, *C d* ſhall be the hights of the Sea in the places *f*, *D*, *F*, *d*, in all which it is High-water: and ſeeing that in twelve Hours time, by the diurnal Rotation of the Earth, the point *F* is transferred to *f*, and *d* to *D*: the hight of the Sea *C F* will be that of the High-water when the Moon is preſent, and *C f* that of the other High water, when the Moon is under the Earth: which in the caſe of this Figure is leſs than the former *C F*. And in the oppoſite Parallel *D d* the contrary happens. The Riſing of the Water being always alternately greater and leſs in each place, when it is produced by the Moon declining ſenſibly from the Æquinoctial; that being the greateſt of the two High-waters in each diurnal Revolution of

Yyy the

the Moon, wherein ſhe approaches neareſt either to the Zenith or Nadir of the place: whence it is that the Moon in the Northern Signs, in this part of the World, makes the greateſt Tides when above the Earth, and in Southern Signs, when under the Earth; the Effect being always the greateſt where the Moon is fartheſt from the Horizon, either above or below it. And this alternate increaſe and decreaſe of the Tides has been obſerved to hold true on the Coaſt of *England*, at *Briſtol* by Capt. *Sturmy*, and at *Plymouth* by Mr. *Colepreſſe*.

But the Motions hitherto mentioned are ſomewhat altered by the Libration of the Water, whereby, tho' the Action of the *Luminaries* ſhould ceaſe, the Flux and Reflux of the Sea would for ſome time continue: This Conſervation of the impreſſed Motion diminiſhes the differences that otherwiſe would be between two conſequent Tides, and is the reaſon why the higheſt Spring Tides are not preciſely on the new and full Moons, nor the Neaps on the Quarters; but generally they are the third Tides after them, and ſometimes later.

All theſe things would regularly come to paſs, if the whole Earth were covered with Sea very deep; but by reaſon of the ſhoalneſs of ſome places, and the narrowneſs of the Streights, by which the Tides are in many caſes propagated, there ariſes a great diverſity in the Effect, and not to be accounted for, without an exact Knowledge of all the Circumſtances of the Places, as of the Poſition of the Land, and the Breadth and Depth of the Channels by which the Tide flows; for a very ſlow and imperceptible Motion of the whole Body of the Water, where it is (for example) 2 Miles deep, will ſuffice to raiſe its Surface 10 or 12 Feet in a Tides time; whereas, if the ſame quantity of Water were to be conveyed upon a Channel of 40 Fathoms deep, it would

would require a very great Stream to effect it, in fo large Inlets as are the Channel of *England* and the *German* Ocean; whence the Tide is found to fet ftrongeft in thofe places where the Sea grows narroweft; the fame quantity of Water being to pafs through a fmaller Paffage: This is moft evident in the *Streights*, between *Portland* and *Cape de Hague* in *Normandy*, where the Tide runs like a Sluce; and would be yet more between *Dover* and *Calis*, if the Tide coming about the Ifland from the North did not check it. And this force being once impreffed upon the Water, continues to carry it about the level of the ordinary height in the Ocean, particularly where the Water meets a direct Obftacle, as it is at St. *Malo's*; and where it enters into a long Channel, which running far into the Land grows very ftreight at its Extremity; as it is in the *Severn-Sea* at *Chepftow* and *Briftol*.

This fhoalnefs of the Sea and the intercurrent Continents are the reafon, that in the open Ocean the time of High-water is not at the Moons appulfe to the Meridian, but always fome Hours after it; as it is obferved upon all the Weft-Coaft of *Europe* and *Africa*, from *Ireland* to the *Cape of Good-Hope*: In all which a S. W. Moon makes High-water, and the fame is reported to be on the Weft fide of *America*. But it would be endlefs to account all the particular Solutions, which are eafie Corollaries of this *Hypothefis*; as why the *Lakes*, fuch as the *Cafpian Sea*, and *Mediterranian Seas*, fuch as the *Black Sea*, the *Streights* and *Baltick*, have no fenfible Tides: For *Lakes* having no Communication with the Ocean, can neither increafe nor diminifh their Water, whereby to rife and fall; and Seas that communicate by fuch narrow Inlets, and are of fo immenfe an Extent, cannot in a few Hours time receive or empty Water enough to raife or fink their Surface any thing fenfibly.

 Laftly,

Laſtly, to demonſtrate the excellency of this Doctrine, the Example of the Tides in the Port of *Tunking* in *China*, which are ſo extraordinary, and differing from all others we have yet heard of, may ſuffice. In this Port there is but one Flood and Ebb in 24 Hours; and twice in each Month, *viz.* when the Moon is near the Æquinoctial there is no Tide at all, but the Water is ſtagnant; but with the Moons declination there begins a Tide, which is greateſt when ſhe is in the Tropical Signs: only with this difference, that when the Moon is to the Northward of the Æquinoctial, it Flows when ſhe is above the Earth, and Ebbs when ſhe is under, ſo as to make High-water at Moons-ſetting, and Low-water at Moons-riſing: But on the contrary, the Moon being to the Southward, makes High-water at riſing and Low-water at ſetting; it Ebbing all the time ſhe is above the Horizon. As may be ſeen more at large in the *Philoſophical Tranſaction*, Num. 162.

The Cauſe of this odd Appearance is propoſed by Mr. *Newton*, to be from the concurrence of two Tides; the one propagated in ſix Hours out of the great *South-Sea* along the Coaſt of *China*; the other out of the *Indian-Sea*, from between the Iſlands in twelve Hours, along the Coaſt of *Malacca* and *Cambodia*. The one of theſe Tides, being produced in North-Latitude, is, as has been ſaid, greater, when the Moon being to the North of the Equator is above the Earth, and leſs when ſhe is under the Earth. The other of them, which is propagated from the *Indian-Sea*, being raiſed in South Latitude, is greater when the Moon declining to the South is above the Earth, and leſs when ſhe is under the Earth: So that of theſe Tides alternately greater and leſſer, there comes always ſucceſſively two of the greater and two of the leſſer together every day; and the High-water falls always between the times of the arrival

val of the two greater Floods ; and the Low-water be tween the arrival of the two leſſer Floods. And the Moon coming to the Æquinoctial, and the alternate Floods becoming equal, the Tide ceaſes and the Water ſtagnates : but when ſhe has paſſed to the other ſide of the Equator, thoſe Floods which in the former Order were the leaſt, now becoming the greateſt, that that before was the time of High-water now becomes the Low-water, and the Converſe. So that the whole appearance of theſe ſtrange Tides, is without any forcing naturally deduced from theſe Principles, and is a great Argument of the certainty of the whole *Theory*.

VI.

The First Biography of Newton

Fontenelle and Newton

Charles Coulston Gillispie

There is a certain piquancy in the chance that Sir Isaac Newton's first biographer should have been a Frenchman and a Cartesian. So it happened, however, in consequence of Newton's position as *associé étranger* of the *Académie Royale des Sciences*. On the death of a member, the custom of that body is to commemorate his life and accomplishments in an essay composed by the permanent secretary. When Newton died in 1727, this post was occupied, as it had been for thirty years, by Bernard le Bovier de Fontenelle, who was then at the height of the career that made him the intermediary between the science of the 17th century and the ideology of the Enlightenment. Immediately translated, his *éloge* became the first biography to appear in England. This is the document that is here reproduced, and, in order to place it in its historical context, it may be well to enter into a little introductory detail on the ambiguity of Newton's early relations with his French colleagues, to indicate the place of the *éloge* in Newtonian biography, and to point out certain casts given to the exposition by Fontenelle's inability to accept, or perhaps to appreciate, Newton's conception of what it is that science explains.

One sometimes reads that Newton was the first foreigner elected to the *Académie des Sciences* upon its reorganization in 1699, and the inference is that by this gesture the French were magnanimously recognizing the magnitude of his challenge to Descartes. Neither the fact nor the implication is correct. When the Academy began operating under its new charter, it already included three foreign members, Leibniz, Tschirnhaus, and Guglielmini, and it filled the five additional vacancies authorized in the following order: Hartsoeker, the brothers Bernoulli, Roemer, and Newton.[1] Nor, although *Philosophiae Naturalis Principia Mathematica* had been in print since 1687, was the Academy yet aware that this book posed a fundamental challenge to the science known to its members, or that they were bringing into their company the founder of classical physical science, in the consciousness of which the world was to live ever after. In choosing Newton eighth on the list, the Academy thought itself to be electing simply a mathematician of extraordinary geometrical skill, and the author of important experiments (and a very questionable theory) bearing on the nature of light.

Professor Cohen has pointed out that it was through the *Opticks,* not the *Principia,* that Newton exerted his influence on the imagination of his 18th-century admirers.[2] The optical work attracted attention from the outset, even before the *Opticks* itself was published in 1704. In the volumes that record the proceedings of the Academy from 1666, the year of its foundation, to 1699, that of Newton's election, the only reference to him is a letter of 1672 from Huygens on the advantages of his reflecting telescope.[3] In 1688 the *Journal des Savants* noticed the *Principia* in three paragraphs, describing the work as "une Mecanique la plus parfaite qu'on puisse imaginer," but pointing out (somewhat misleadingly) that Newton himself says of his proofs "qu'il n'a pas consideré leurs principes en Physicien, mais en simple Géometre," and urging him to "nous donner une Physique aussi exacte qu'est la Me-

[1] See, *ante,* p. 8; also, *Les membres et les correspondants de l'Académie royale des sciences (1666–1793)* (Paris, 1931); and on the reorganization, Alfred Maury, *L'Ancienne Académie des Sciences* (Paris, 1864), pp. 40–45.

[2] I. B. Cohen, preface to Newton's *Opticks* (New York, 1952).

[3] *Mémoires de l'Académie Royale des Sciences depuis 1666 jusqu'à 1699,* vol. 10 (1730), pp. 505–507. It should perhaps be explained that the volumes covering this period were published through the efforts of Fontenelle long after the events.

canique" by substituting "de vrais mouvemens en la place de ceux qu'il a supposez."[4] Thereafter, Newton was not again discussed in the *Journal* until 1703, when a passing reference appears in an article drawn from Jean Bernoulli's *Recherche de Catoptrique et Dioptrique* of 1701. Newton is introduced casually and only in order to be dismissed, but the turn of argument is interesting, for, although it refers to an optical passage of the *Principia,* it too is all unwittingly prophetic of the larger issues in the offing. To certain considerations on refrangibility which follow from the views of one Herigone (and indeed of Descartes), the author objects that in a homogeneous medium the relative obliquity of rays is meaningless except with reference to a second medium, which cannot be supposed to affect the path of the ray before ever it arrives,

à moins qu'avec le subtil M. Newton, (*Princ. Math. Phil. Nat. pag.* 231.) on ne veüille mettre dans le second milieu quelque vertu attractive qui agisse sur les rayons lors qu'ils sont encore dans le premier milieu, & qui les attire plus fortement les uns que les autres. C'est en effet par là que M. Newton explique la nature de la reflexion, & de la refraction: mais son explication est plus ingenieuse qu'elle n'est vraye; car il ne nous apprend point ce que c'est que cette vertu attractive, ni d'où elle vient: il la suppose seulement. J'avoüe que si on la lui accorde, l'explication qu'il donne est forte élegante, & peut contenter un Mathematicien.[5]

In fact, Newton had addressed himself mathematically to reality, and not just abstractly to mathematics. And that he had undertaken a radical approach to the great question of how the world is made was borne in on his French colleagues less by perusal of the *Principia,* that intractable book, than by attending to the discussions raised by philosophers who did perceive how deep the

[4] *Journal des Savants 16,* 237–238 (1688). The survey of this important journal for French reaction to Newton is greatly facilitated by the availability of Jacqueline de La Harpe, "Le Journal des Savants et l'Angleterre, 1702–1789," *University of California Publications in Modern Philology,* XX (1937–1941), pp. 289–520, especially pp. 319–323, 358–363.

A second account in French of the first edition of the *Principia* appeared in the *Bibliothèque Universelle 8,* 436–50 (1688); apparently written by John Locke, this was an outline of the *Principia,* and not really a critical review. See I. B. Cohen, *Introduction to Newton's 'Principia'* (Cambridge, 1971), pp. 145–148.

[5] *Journal des Savants 31,* 1002 (1703). I can find no discussion of the principle of gravity before the review of the second edition of the *Principia* (*Journal des Savants,* June 1715, Pt. I, 667–674). Even here the principle of attraction is simply set off against the theory of vortices in a literal and superficial fashion.

issues went: Malebranche, the final edition of whose *Recherche de la vérité* appeared in 1712; Leibniz, who attacked the theory of gravity in 1710 and the publication of whose ensuing correspondence with Clarke was the most important single event in bringing home the problem; Roger Cotes, whose preface to the second edition of the *Principia* (along with Newton's new General Scholium) in 1713 joined issue with continental philosophy.[6] As everyone knows, the acceptance of Newton's principles in France had to await his death—and Voltaire.[7] But even the full awareness of Newton was delayed until the period, after 1710 or thereabouts, when he had long since withdrawn from the arena of science, if not of controversy.

Nor were his relations ever close with France. There were a few letters from Fontenelle thanking Newton for copies of certain books, and a few complaints by Newton of the credit allowed to Leibniz on the invention of the calculus in the *éloges* of Leibniz and L'Hôpital.[8] There was some discussion in the Academy, mathemati-

[6] Although Malebranche (who admired the *Opticks*) does not allude directly to the *Principia,* the discussion of gravity and the adaptation of Villemot's theory of spherical vortices were directed against Newton; see *Recherche de la verité,* ed. Francisque Bouillier, 2 vols. (Paris, 1880), Ph. Villemot, *Nouveau système, ou nouvelle explication du mouvement des planètes* (Lyon, 1707), and, for a discussion of these works, P. Mouy, *Le Développement de la physique cartésienne* (Paris, 1934), pp. 271, 310–314. In the opinion of the latest student of Fontenelle's science, this last edition of Malebranche was the point of departure of Fontenelle's comprehensive and thought-out opposition to Newton; see F. Grégoire, *Fontenelle, une "philosophie" désabusée* (Nancy, 1947), p. 130. A translation of the text of the Leibniz-Clarke correspondence has just been republished in a critical edition, H. G. Alexander, ed., *The Leibniz-Clarke Correspondence* (Manchester, 1956), with a most useful analytical introduction. Though far from satisfactory, the most accessible edition of the *Principia,* containing the Cotes preface and the General Scholium, is that by Florian Cajori, *Sir Isaac Newton's Mathematical Principles of Natural Philosophy* (Berkeley, 1934), based on Andrew Motte's translation of the 3rd edition (1729). A facsimile reproduction of the first edition has recently been published by William Dawson (London, 1955). Mention, too, must be made of the work which prints (unfairly) selected documents in the *Streit* with Leibniz over the invention of the calculus, from which controversy the larger argument emerged, *Commercium epistolicum D. Johannis Collins et aliorum de analysi promota* (London, 1712) and of the compilation which was very influential in bringing the whole issue before French readers, P. des Maizeaux, *Recueil de diverses pièces sur la philosophie . . . par Messieurs Leibniz, Clarke, Newton, et autres auteurs celèbres (Amsterdam, 1720).*

[7] Pierre Brunet, *L'Introduction des théories de Newton en France* (Paris, 1931).

[8] Fontenelle was pleased and touched by the kindly reference to his own work

cal rather than physical, of "forces centrales."[9] In 1715 certain optical experiments were demonstrated by Desaguliers in London in the presence of the Chevalier de Bouville and other members of the Academy. Many of them were verified in Paris by Père Sebastien in the presence of the Cardinal de Polignac, Varignon, and Fontenelle.[10] And it appears that the image of Newton which Fontenelle develops in the *éloge*, compounded of admiration for his talent and rejection of his principles, was not fully formed until the fifteen years or so before Newton's death. The *éloge* stands, then, at the middle stage in that passage from incomprehension through rejection to idealization that was the route by which Newton penetrated and ultimately transformed scientific understanding in France.

A persistent current of interest in Fontenelle himself runs through the scholarly literature—persistent, but a little thin, for the one point on which all his interpreters agree is that he cuts at best a minor figure, if a witty one. Thus, Laborde-Milaà makes him the *philosophe* who transformed Cartesianism into positivism,[11] while Louis Maigron presents a comprehensive picture of the transformation of Sainte-Beuve's "bel esprit . . . au gout détestable"[12] into the *accoucheur* of ideas who brought science to bed of the Enlightenment.[13] Carré discovers in him a sort of preincarnation of Voltaire, without the fire and passion that informed the life and work of Voltaire.[14] Cosentini, in turn, offers us Fontenelle as a lesser master of the art of philosophic dialogue,[15] and Shackleton gives us a

that the translator of the *Opticks*, one Coste, included in the preface and which Fontenelle took as coming from Newton himself; see G. Bonno, "Deux lettres inédites de Fontenelle à Newton," *Modern Language Notes 44,* 188–190 (1939); David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton,* 2 vols. (Edinburgh, 1855), II, 290–295, 494–500.

[9] The suggestion of Robert Shackleton that Fontenelle's treatment of this question may be taken as the beginning of his systematic anti-Newtonianism is to be treated with reserve. See Shackleton's introduction to his edition of Fontenelle, *Entretiens sur la pluralité des mondes* (Oxford, 1955), pp. 20–28.

[10] *Journal des Savants 67,* 546 (1720), in a review of the French translation of the *Opticks* (Amsterdam, 1720).

[11] A. Laborde-Milaà, *Fontenelle* (Paris, 1905).

[12] C. A. Sainte-Beuve, *Causeries du Lundi* (Paris, n. d.), III, 314–335.

[13] Louis Maigron, *Fontenelle, l'homme, l'oeuvre, l'influence* (Paris, 1906).

[14] J. R. Carré, *La Philosophie de Fontenelle, ou la sourire de la raison* (Paris, 1932).

[15] John W. Cosentini, *Fontenelle's Art of Dialogue* (New York, 1952).

Fontenelle *savant* and dignifies the *Entretiens,* which made his reputation, with all the apparatus of an elaborate variorum edition.[16] Grégoire, finally, finds that the career of the permanent secretary of the Academy was a mask to philosophic disenchantment and his commitment to science a role played but not believed by a secret nihilist.[17] This is not the place to choose between these Fontenelles, or to add another to the list. But it may perhaps be permissible to suggest that the sardonic manner, the tendency to denigrate his own commitments, which give rise to such varying interpretations may have been in part the expression of uneasy consciousness that he remained an amateur, a science reporter and not a *savant.* The *Éloge* of Malebranche includes a remark disconcerting to the intellectual historian. "On peut savoir," writes Fontenelle, "l'histoire des pensées des hommes sans penser." [18] And whatever else he was, Fontenelle was a historian of ideas.

He was also a humanist of science, and his best efforts were devoted to the men of science, to his colleagues. His *éloges* remain his finest work.[19] Here his distinctive qualities appear to best advantage: the personal dispassionateness, the respect for knowledge, the real if not always discriminating comprehension of scientific accomplishments, the faith he expressed (whether or not he felt it) in the civilizing mission of science, the talent for lucid exposition if not for profound discussion (he was never profound). He disliked the term *éloge* since he conceived these essays not as eulogies, but as historical sketches supplementing the account of the work of the year which he prepared for each volume of the *Histoire et mémoires*

[16] See above, note 9. Shackleton also prints the *Digression sur les anciens et les modernes* in this useful volume.

[17] Grégoire, *Fontenelle;* see especially his summaries, pp. 270–271 and 465–466. This work contains the best discussion of Fontenelle's science, and of his attitude to Newtonianism; see especially pp. 119–184.

[18] Fontenelle, *Oeuvres,* 8 vols. (Paris, 1790–1792), VI, 416.

[19] First published in the current volumes of the *Histoire de l'Académie,* they were collected in volumes VI and VII of the edition of the *Oeuvres* cited in note 18, and selections have been several times reprinted. An English translation of the early *éloges* was published in 1717, under the title *The Lives of the French, Italian and German Philosophers,* which contains, too, a selection from "some of the most curious Relations of Philosophical Matters," in offering which the translator (John Chamberlayne) has "affected to join the *Utile* with the *Dulce,* according to the Poet's Advice."

de l'Académie.[20] The vein is ceremonial and impartial, elegant and concise, objective and decently respectful—combinations achieved more readily by the French mind and language than the English.

For a long time the *éloge* of Newton served as the cornerstone of Newtonian biography, and not only in the sense that it was the first. Though somewhat obscured by translation, the trail of Fontenelle's phrases can be followed through successive accounts of Newton's life well into the nineteenth century. Even the structure of Fontenelle's essay proved remarkably durable. Here appear the essential features of posterity's image of Newton. Here Newton and Descartes are set over against each other as the prototypes of the inductive and deductive philosophers, though of the many commentators who elaborated this comparison, none hit upon so happy a thought as Fontenelle in balancing their merits. Here, too, occur, among other things, the story of the youthful Newton's inattention to business and absorption in his studies; the description of how mathematics came to him at a glance (only the nineteenth century turned him also into a mechanical prodigy); the portrait of the insatiable investigator, whose "accurate and importunate" manner of research is an object lesson to all who would interrogate nature; the account of his entry into public affairs to defend the university from James II and of his later practical life at the Mint and the Royal Society; the attributing of his reluctance to publish his discoveries to his loathing for controversy; the tale of his solution of Bernoulli's problem at the end of a tiring day; and the delineation of his outstanding personal characteristics—manners, modesty, kindliness, generosity, and appearance (unfortunately Fontenelle was misinformed about Newton's appearance, and there were in fact unhappy episodes in his life in which the qualities appropriate to the role of selfless and retiring searcher into nature were honored in the breach).

One crucial episode, featured in all later biographies, does not appear in the *éloge.* In 1727, Fontenelle did not know of that most famous meditation in the history of science, the train of thought about the force retaining the moon in her orbit, which had come to Newton as he sat at home in the garden in the plague year of

[20] Francisque Bouillier, in the introduction to his edition of the *Eloges* (Paris, 1883), pp. xxiii ff.

1666 and had led him to his theory of gravitation. The first account of this event was published in the preface to Henry Pemberton's *View of Sir Isaac Newton's Philosophy* (1728). In the article on Newton printed in 1738 in the *General Dictionary,* Pemberton's information was incorporated into that found in the *éloge,* in the first appearance of what became the standard narrative of Newton's personal history.[21] The same article gave a rather more detailed and documented treatment of Newton's thought than had Fontenelle, and for this purpose the author published a selection from the scientific papers and correspondence. In 1760, the *Biographia Britannica* printed an even larger selection.[22] Neither of these articles, however, altered the picture derived from Fontenelle and Pemberton. Nor did the other accounts that appeared here and there throughout the eighteenth century.[23]

The first work really to supersede Fontenelle was Sir David Brew-

[21] The *General Dictionary,* 10 vols. (1734–1741), was the English translation and adaptation of Bayle's *Dictionary,* based on the latest Paris edition, and "interspersed with several thousand lives never before published. The whole containing the history of the most illustrious persons of all ages and nations, particularly those of Great Britain and Ireland . . ." Newton's life was one of the additions.

It will be noticed that the famous story of the apple does not appear in the *éloge.* Neither is it mentioned in Pemberton. Fontenelle knew of it, from the biographical information sent him by John Conduitt (see below, n. 30), who, however, says only that "He first thought of his system of gravity . . . by observing an apple fall from a tree," and does not describe the train of thought to which it led. Not knowing this, Fontenelle would not have seen the point, and it was left to Voltaire to work this anecdote into the biographical corpus. For the authenticity of the story, see Jean Pelseneer, "La Pomme de Newton," *Ciel et Terre* (1937), 1–4, and G. R. de Beer and Douglas McKie, "Newton's Apple" and "Newton's Apple —an Addendum," *Notes and Records of the Royal Society 9,* 46–54, 333–335 (1951–52). The authors omit only to point out that it was not until the article by Benjamin Martin (below, n. 23) that what Voltaire referred to as "fruits" were generally identified as apples. The story of a single apple was apparently not canonical until the 19th century.

[22] To the material from Fontenelle, Pemberton, and the scientific correspondence itself, this article added a few anecdotes drawn from Whiston's reminiscences, available only since 1749: *Memoirs of the Life and Writings of Mr. William Whiston,* 2 vols. (London, 1749). Here appears for the first time the explanation that in 1666 Newton supposed the discrepancy between the theoretical and the observed positions of the moon, which caused him to set aside his work on gravity, to be the consequence of the disturbing influence of the Cartesian vortex.

[23] For example, *Universal Magazine 3,* 289–300 (1748); Benjamin Martin, *Biographia Philosophica* (London, 1764), pp. 361–376; Paolo Frisi, *Elogio del Cavaliere I. Newton* (Milan, 1778).

ster's *Life of Sir Isaac Newton.*[24] Suggestions had been advanced by Biot that Newton had suffered a period of mental derangement, that, though he recovered his sanity, he never regained his scientific powers, and that his religious writings were the products of intellecual decay. [25] These theories shocked Brewster into producing the first full-length biography. It was a work of national and scientific piety, which displayed Newton, in Brewster's own phrase, as the "high priest," not to say the Sir Galahad, of science. The tendency, well developed even before Brewster, to make Newton's life an edifying object lesson, reached its nadir in an anonymous work published in 1860, *The Triumphs of Perseverance and Enterprise,* "written with the view to inspire the youthful reader with a glow of emulation, and to induce him to toil and advance in the peaceful achievements of science and benevolence, remembering the adage, 'Whatever man has done, man may do.'" This sort of thing produced a reaction, of course,[26] and now the wheel has come full circle, turning through disputes about Newton's character and theories about his mental processes, until J. W. N. Sullivan advances as the key to his life the proposition that, the greatest of scientists, he thought science unimportant, [27] and Lord Keynes, with Bloomsbury perversity, describes him as the last of the magicians.[28] It has, indeed, been Newton's fate that other people have always projected their philosophies or theologies of science upon him in explanation of his achievements. It is refreshing, therefore, to turn back to the plainer

[24] London, 1831. In 1855 appeared a second and enlarged edition, which printed a few selections from Newton's correspondence; see note 8, above.

[25] Biot wrote the article "Newton" (1821) in the Michaud *Biographie universelle,* vol. 30, pp. 367–404; see especially pp. 390–391, 402. A selection of Newton's theological writings has recently been published, Herbert MacLachlan, ed., *Sir Isaac Newton's Theological Manuscripts* (Liverpool, 1950). In 1829 Henry (later Lord) Brougham published what was essentially a translation of the Biot article, *Life of Sir Isaac Newton* (London, 1829), which appeared in the Library of Useful Knowledge series and appears to have been the immediate occasion of Brewster's work. For Biot's criticism of the Brewster *Memoirs* (see preceding note), see *Journal des Savants,* Oct. and Nov. 1855, 589–606, 662–677.

[26] See particularly Augustus de Morgan, *Essays on the Life and Work of Newton* (Chicago, 1914). The first of these essays appeared in *The Cabinet Portrait Gallery of British Worthies* (London, 1846).

[27] *Isaac Newton* (London, 1938).

[28] "Newton, the Man," *Newton Tercentenary Celebrations, 15–19 July 1946,* published by The Royal Society (Cambridge, 1947), 27–34.

account of Fontenelle, embellished only with literary grace, where, if the mystery of Newton's genius is not dispelled, neither is it deepened.[29]

Fontenelle drew his discussion of Newton's scientific accomplishments from his own knowledge; for the biographical facts, however, he relied entirely on notes sent him by John Conduitt, who had married Newton's niece.[30] Unfortunately, Conduitt was most dissatisfied with the use Fontenelle made of the information. "I fear," wrote Conduitt of Fontenelle after the *éloge* was published, "he had neither abilities nor inclination to do justice to that great man, who has eclipsed the glory of their hero, Descartes."[31] There were no real grounds for these complaints, but Fontenelle had, in fact, omitted several points included by Conduitt, among them a number of derogatory remarks about Descartes's hypotheses and the statement that Newton originally undertook the study of mathematics to discover whether there was anything in judicial astrology. He ignored Conduitt's request that he recall the passages in his *éloges* of l'Hôpital and Leibniz which allowed Leibniz a portion of the credit for developing the calculus. He passed lightly and tactfully over Newton's ventures into history, chronology, and divinity, about which Conduitt had given him a considerable amount of information. And though he compares England favorably to France in regard to the respect which society accorded to men of science, it is also clear from his reserve that he regarded the contemporary apotheosis of Newton as excessive, and not only in contrast to the neglect encountered by Descartes in his last years. To a temperament like Fontenelle's, apotheosis was a repellent process, no matter whom it involved.

Fontenelle always remained faithful to the cosmology he had

[29] For a complete guide to the biographical literature, see G. J. Gray, *A Bibliography . . . of Sir Isaac Newton* (2nd ed., Cambridge, 1907), together with *A Descriptive Catalogue of the Grace K. Babson Collection of the Works of Sir Isaac Newton* (New York, 1950) and its *Supplement* (Babson Institute, 1955), corrected in a few particulars in the *compte-rendu* by G. F. Shirras, *Archives internationales d'histoire des sciences 29,* 949–953 (1950). The most comprehensive biography is that by Louis T. More, *Isaac Newton* (New York and London, 1934).

[30] These notes were published in Edmund Turnor, *Collections for the History of the Town and Soke of Grantham* (London, 1806).

[31] *Dictionary of National Biography,* article "John Conduitt."

learned and expounded as a young man. His last book, *Théorie des tourbillons cartésiens* (1752), was also one of the last general defenses of the system to see print. But though his life embraced almost the full span of Cartesian science, he must not be regarded as the moribund champion of some fossilized doctrine. Cartesianism was a living body of thought about nature. On the basis of the principles laid down by Descartes, there developed a real physics—indeed *too* real because too literal.[32] Nor was Descartes exempt from the spirit of criticism he enjoined. Already by 1700, there were several schools. Malebranche had created a Catholic Cartesianism, to which was opposed the skeptical Cartesianism represented by Fontenelle. Yet it was from Malebranche that Fontenelle took the doctrine of spherical vortices to oppose to the Newtonian theory of gravity.

The web of resistance to Newton was, in fact, complex. Some strands ran parallel and others counter to each other. In the *éloge* there is apparent the influence, not just of Descartes and Malebranche, but of Huygens and of Leibniz. For Huygens, the true physicist, the decisive objection was concrete. What was inadmissible in Newtonianism was primarily the idea of a universal attraction subsisting between all the particles of the world as an inherent property of matter "parce qu'une telle hypothèse nous éloignerait fort des principes mathématiques ou mécaniques." [33] But for Leibniz and the general run of Cartesians the problem arose from differing conceptions of what science does.[34] In the Cartesian view, for all its hostility to scholasticism, science moves through nature from definition to explanation; in that of Leibniz it moves rather from

[32] See the excellent book by Paul Mouy, cited in note 6.

[33] Nor did Huygens think that Newton could have seriously meant that gravity is an essential property of matter. See "Théorie de la pesanteur," *Oeuvres complètes de Christiaan Huygens* (The Hague, 1944), XXI, 474; quoted too by Mouy, pp. 260–261, in his discussion of Huygens and Newton.

[34] This whole question has been treated by many writers, of course, and from many points of view. For the Cartesian side, see (in addition to Mouy and Grégoire, note 6), Francisque Bouillier, *Histoire de la philosophie cartésienne,* 2 vols. (Paris and Lyon, 1854). For the differences between Newton and Leibniz, see Alexander, *The Leibniz-Clarke Correspondence,* with the editor's introduction; Ernst Cassirer, *Leibniz' System in seinem wissenchaftlichen Grundlagen* (Marburg, 1902), esp. pp. 245–282; Josef Durdik, *Leibniz und Newton* (Halle, 1869); F. S. C. Northrop, "Leibniz's Theory of Space," *Journal of the History of Ideas 7,* 422–446 (1946); and an interesting unpublished doctoral dissertation in the library of Princeton Uni-

principles to values; and in that of Newtonians from descriptions to abstract generalizations. Strictly speaking, therefore, Newtonian science could never get outside itself, and might in a sense be said to be a tautology, or at least to accomplish nothing of interest or value.

More immediately, the theory of gravity was unacceptable to Cartesians because of their commitment to a mechanistic universe. For Leibniz, though a strict mechanist in practice, the planetary theory was excluded rather by his commitment to a finalism unreconcilable with Newton's way of taking the phenomena as given, as the data of thought. Disagreements were profound on the fundamental question of space. Descartes having unified his science by identifying space and matter, it remained for them to be properly distinguished: unsuccessfully by Leibniz, who turned space from a substance into a relation (that of simultaneous events), after which he sought to unite his system metaphysically by the principle of preëstablished harmony; successfully by Newton's bolder stroke of emptying space to turn it into the physical expression of an abstract geometry (and an attribute of God), after which he did unite his system around the principle of gravity.[35] Fontenelle, for his part, rejected the very different providentialisms of Malebranche, of Leibniz—and of Newton. But for Leibniz (seeing more deeply, showing more insight into the rationalizing powers of the calculus), what was abhorrent in Newtonianism was not its providentialism, but the exact contrary, its tendency to lead in the direction already marked by Hobbes, a self-sufficient materialism destructive of natural religion. One important matter found Newtonians and Cartesians standing together. Both rejected finalism in scientific explanation, and, in the controversy over *vis viva,* both held as against Leibniz that the

versity, Nicholas Rescher, "Leibniz' Cosmology: A Reinterpretation of the Philosophy of Leibniz in the Light of his Physical Theories" (1951). For Fontenelle's respect for Leibniz, see his *Eloge de Leibniz, Oeuvres,* VI, 450–505. The best guides to Newton's principles are Alexandre Koyré, "The Significance of the Newtonian Synthesis," *Archives internationales d'histoire des sciences 29,* 291–311 (1950), and F. Rosenberger, *I. Newton und seine physikalischen Prinzipien* (Leipzig, 1895).

[35] I owe this way of seeing it to Koyré, "The Significance of the Newtonian Synthesis." On space as a divine attribute, see Alexander, *Leibniz-Clarke Correspondence,* pp. xiv, 47 (Clarke's Fourth Reply to Leibniz).

quantity conserved in a dynamical situation is momentum and not kinetic energy.[36]

On the whole, the confrontation of Newton with Leibniz was philosophically more interesting and deeper; that of Newton with Descartes was historically more influential and more obvious—in some respects, indeed, this latter was the kind of opposition which in mathematics is expressed by a change of sign from plus to minus. But on specific points about the actual working of the real world, the influences of Leibniz and Descartes came together in the way reflected in the *caveats* and passing emphases in Fontenelle's *éloge* which imply his own disbelief that Newton's was a satisfactory picture of what happens.

For example, Newton's adherents did not, like Fontenelle, describe the *Principia* as resting equally on two leading theories, one concerned with the force of attraction exerted by bodies, the other with the resistance offered by fluid mediums to motion. Expositions by Newtonians generally emphasized the former, the positive, constructive side of Newton's work, rather than the latter which, though it is indeed the subject of much of Book II, served rather the negative purpose of disproving the existence of Cartesian vortices.[37] Further on, at the close of his discussion, Fontenelle complained in passing that Newton never makes clear what causes gravity itself, wherein it consists, or how action at a distance is mechanically possible. Now this was the crucial objection which united all the opponents of Newton. To call gravity a force of attraction was no clarification. Attraction had not even the elementary merit of working properly. From time to time Newton's cosmos got out of order, and Providence had to step in to repair it. Nor was the idea comprehensible, and in place of attraction Fontenelle suggested the term "impulse" as more appropriate. In Cartesian mechanics, force was transmitted in good, concrete, di-

[36] There is a good account of this well-known issue in Martial Gueroult, *Dynamique et métaphysique leibniziennes* (Paris, 1934).

[37] See, for example, Henry Pemberton, *A View of Sir Isaac Newton's Philosophy* (London, 1728); Colin Maclaurin, *Account of Sir Isaac Newton's Philosophical Discoveries* (London, 1748); Voltaire, *Elémens de la philosophie de Newton* (Amsterdam, 1738). For a useful guide, see W. W. Rouse Ball, *An Essay on Newton's Principia* (London, 1893).

rect ways: by impact, by pressure, by the frictional drag of swirling vortices of cosmic stuff, none the less real for being subtle, which carried the planets around in their courses. (Fontenelle, it will be noticed, included no discussion of the laws of motion or of Newtonian mechanics.) To describe the fall of an apple or the motion of the moon and the tides not as a definite push by something against something else, but as the pull of an intangible force, itself inexplicable, was to offer not an explanation, but, like the scholastics, a mystification, a word in place of a fact. And as gravity seemed simply an occult force or a perpetual miracle, so the Newtonian idea of an empty space across which it flings its influence seemed a reversion to the mysterious void that had only recently been filled up by the Cartesian plenum. So nebulous was the whole conception that, in the 1730's, Fontenelle saw the theory of gravity as a passing fancy, of some value perhaps for having posed certain criticisms which inspired improvements in the system of vortices at the hands of Privat de Moliéres.

Fontenelle did admire the matchless mathematical virtuosity displayed in the *Principia*. The trouble was not in the mathematics, but that, taken as an explanation of the universe, the system failed —or rather that it was no explanation at all since no cause could be assigned for its central principle, the principle of attraction, and since it substituted for a concrete, working, mechanical picture a set of mathematical and geometrical abstractions. At issue, in fact, was the question, as old as Aristotle, whether mathematics and nature really fit. Newton (writes Fontenelle) "s'est mis dans le Vuide, à des forces mouvantes connuës & Méchaniques il a substitué une force inconnuë & Métaphisique, une Attraction, dont on ne peut prêvoir les effets, mais que l'on suppose telle que certains faits établis la demandent, & qui par conséquent satisfait toujours précisément à tout. M. l'Abbé de Moliéres lui reproche même assés finement cette extrême précision, les principes Phisiques n'en ont pas tant, lorsqu'on vient à les appliquer aux Phénomenes." [38] The trouble with Newton's mathematical approach was that the fit with phenomena is impossibly tight. It squeezes out reality, where things rub against each other physically in a looser, a more com-

[38] *Histoire de l'Academie Royale des Sciences* (1733), p. 94.

prehensible meshing with ordinary experience, and where there is always something left over from an explanation in case it is needed. Much earlier, Fontenelle had remarked approvingly of Malebranche's numberless "petits tourbillons" that their being applicable to the explanation of so many phenomena—light, heat, sound, electricity, weight, whatnot—created a strong presumption in their favor. "Voilà un grand fonds de force pour tous les besoins de la physique"—even those not yet foreseen.[39] For Fontenelle, a theory that comes out precisely *even,* so to say, simply *circles* (whether through reality or not) right back to its starting place.

It may at first seem odd that the Cartesians, whose very definition of matter was mathematical, should have accused the Newtonians of excessive abstraction. But the penalty attached to over-mathematicizing nature in the fashion of Descartes was precisely that the process simultaneously coarsened and adulterated mathematics by confusing its province with that of mechanics and its procedures with common sense. In retrospect, of course, it is apparent that the two arguments never really met, that the two sides were talking about different things. The Newtonians—at least when answering Cartesian critics—claimed only that Newton discovered a relation; the Cartesians accused him of not having found a cause. To the Cartesian complaint that, the cause of attraction being unexplained, the force of gravity was a figment of Newton's mind, an Aristotelian tendency, the Newtonians retorted that, explicable or not, the relation subsisted in phenomena, and that it was Descartes who had imagined, not a force to be sure, but a substance and a motion to explain mechanically what no one could yet understand, the ultimate cause that lies behind the laws of nature, laws which themselves are to be taken only as descriptive generalizations of appearances and not as causes, which derive (it may be) from God whose ways it would be as impious as impossible to prescribe.[40]

Since Newton actually did not provide what Fontenelle required, a system which accounted at once for the behavior and the cause

[39] Fontenelle, "Eloge de Malebranche," *Oeuvres,* VI, 422.

[40] See the General Scholium to the *Principia,* 2nd ed. This, together with other relevant passages from Newton's writings, is printed as an appendix in Alexander, *Leibniz-Clarke Correspondence,* pp. 143–183.

of phenomena, which saw nature steadily and saw it whole, it is not surprising that, like most Cartesians, he remained unconvinced. There is, perhaps, a certain irony in the circumstance that Newton's theory, which countless intellectual historians have described as responsible for the 18th-century picture of a soulless, deterministic world machine, should have been rejected at the time on the ground that it was overly abstract and insufficiently mechanical and that it called the hand of Providence into the workings of the world. And on reflection, it may, after all, seem appropriate that Newton's first biographer was a Cartesian and a Frenchman. For, if the English deified Newton, the French rationalized him. Belief in the self-sufficiency of natural order, expressed by the materialist *philosophes* who followed Fontenelle, must be attributed not to the legacy of Newton, but to that of Descartes[41]—tempered less by Newton than by Hobbes. The perfectly synchronized world machine that is supposed to have sprung out of Newton's brain to place itself at the service of the Enlightenment was actually a fairly uncertain mechanism until, well after the Enlightenment was past its zenith, it was tidied up mathematically by Laplace—inspired (it might be argued) by the Cartesian spirit which insists on order and unity. And, on the other hand, the adoption of Newtonianism and the challenge presented by its irregularities were among the chief influences that carried the rational genius of France to the leadership of the world of science in the late 18th century.

The number of editions of the *éloge* published in London confirms the admiration of the English public for Newton at the time of his death. In addition to the Tonson edition reproduced here, there were at least three others, two of which were different translations. Even in Paris, Newton's death appears to have aroused considerable interest. The *éloge* of Newton was one of the few that Fontenelle published separately, and besides that edition (1728), there was also a single-sheet folio *abrégé* of the same year.

[41] See the stimulating book by Aram Vartanian, *Diderot and Descartes* (Princeton, 1953), where this point is argued with much force—perhaps with too much force, seeing that Mr. Vartanian takes no account of the influence of the associationist psychology on conceptions of scientific explanation from Locke and Condillac through the taxonomists and chemists of the latter part of the century to the *idéologues*.

The style of the original is graceful, urbane, and good-humored, with here and there a hint of reserve. These qualities are largely lost in the translation, which is frequently clumsy and nowhere better than adequate. In the 18th century as now, "Etranger" meant foreigner rather than stranger (see the last paragraph), and there must have been a better expression for "Grandeur de la surface" than "Magnitude of the Superficies." But the most curious feature of the *éloge* appears also in the original, and that is that Fontenelle should have concluded in good Victorian style by holding up Leibniz and Newton to admiration as exemplars of thrift!

THE

ELOGIUM

OF

Sir *ISAAC NEWTON*:

BY

Monſieur *FONTENELLE*,

Perpetual Secretary of the Royal Academy of Sciences at *Paris*.

LONDON:

Printed for J. TONSON in the *Strand*, and J. OSBORN and T. LONGMAN in *Pater-noſter Row*.

MDCCXXVIII.

THE

ELOGIUM

OF

Sir *ISAAC NEWTON*,

BY

Monſieur *FONTENELLE*.

SIR Iſaac Newton, who was born at Woolſtrope in the county of Lincoln, on Chriſtmas day in the year 1642, deſcended from the elder branch of the family of Sir John Newton Baronet. The Manor of Woolſtrope had been in his Family near 200 years. The Newtons came thither from Weſtby in the ſame County, but originally from Newton in Lancaſhire. Sir Iſaac's Mother, whoſe maiden name was Hannah Aſcough, was likewiſe of an ancient family; ſhe married again after his Father's death.

When her Son was twelve years old ſhe put him to the Free-ſchool at Grantham; from whence ſhe removed him ſome years after, that he might be accuſtomed betimes to look into his affairs, and to manage them himſelf. But ſhe found him ſo careleſs of ſuch Buſineſs, and ſo taken up with his books, that ſhe ſent him again to Grantham, that he might be at liberty to follow his inclinations; which he farther indulged by going to Trinity college in Cambridge, where he was admitted in 1660, being then eighteen years of age.

In learning Mathematicks he did not ſtudy Euclid, who ſeemed to him too plain and too ſimple, and not worthy of taking up his time; he underſtood him almoſt before he read him, and a caſt of his eye upon the contents of the Theorems was ſufficient to make him maſter of them. He advanced at once to the Geometry of Des Cartes, Kepler's Opticks, &c. ſo that we may apply to him what Lucan ſaid of the Nile, whoſe head was not known by the Ancients,

Arcanum Natura caput non prodidit ulli,
Nec licuit populis parvum te, Nile, videre. Lucan. l. x.

Nature conceals thy infant Stream with care,
Nor lets thee, but in Majeſty appear.

It is certain that Sir Iſaac had made his great Diſcoveries in Geometry, and laid the foundation of his two famous pieces the *Principia* and the *Opticks* by the time that he was twenty four years of age. If thoſe Beings that are ſuperior to Man have likewiſe a progreſſion in Knowledge, they fly whilſt we creep, and leap over thoſe *mediums* by which we proceed

ceed ſlowly and with difficulty from one Truth to another that has a relation to it.

Nicholas Mercator, who was born in Holſtein, but ſpent moſt of his time in England, publiſhed in 1668 his *Logarithmotechnia*, in which he gave the Quadrature of the Hyperbola by an infinite Series. This was the firſt appearance, in the learned world, of a Series of this ſort, drawn from the particular nature of the Curve, and that in a manner very new and abſtracted. The famous Dr. Barrow, then at Cambridge, where Mr. Newton, who was about 26 years of age, reſided, recollected that he had met with the ſame thing in the writings of that young gentleman, and there not confined to the Hyperbola only, but extended by general forms to all ſorts of Curves, even ſuch as are mechanical, to their quadratures, their rectifications and their centers of Gravity, to the ſolids formed by their rotations, and to the ſuperficies of thoſe ſolids; ſo that ſuppoſing their determinations to be poſſible, the Series ſtopt at a certain point, or at leaſt their ſums were given by ſtated rules: But if the abſolute determinations were impoſſible, they could yet be infinitely approximated which is the happieſt and moſt refined method of ſupplying the defects of Human knowledge that Man's imagination could poſſibly invent. To be maſter of ſo fruitful and general a Theory was a mine of gold to a Geometrician, but it was a greater glory to have been the diſcoverer of ſo ſurprizing and ingenious a Syſtem. So that Sir Iſaac finding by Mercator's book that he was in the way to it, and that others might follow in his track, ſhould naturally have been forward to open his treaſures, and

[6]

ſecure the property, which conſiſted in making the diſcovery. But he contented himſelf with his treaſure which he had found, without regarding the glory. He himſelf ſays in a letter of the *Commercium epiſtolicum*, *that he thought Mercator had entirely diſcovered the ſecret, or that others would diſcover it before he was of an age to write himſelf.* He without any concern ſuffered that to be taken from him, from which he might propoſe to himſelf abundance of glory, and flatter himſelf with the moſt pleaſing expectations. He waited with patience till he was of a fit age to write, or to make himſelf known to the world, though he was already capable of the greateſt things.

His manuſcript upon Infinite ſeries was communicated to none but Mr. Collins, and the Lord Brounker, both learned in that way. And even this had not been done, but for Dr. Barrow, who would not ſuffer him to indulge his modeſty ſo much as he deſired.

This Manuſcript was taken out of the Author's ſtudy in the year 1669, entitled, *The method which I formerly found out*, &c. and ſuppoſing this *formerly* to mean no more than three years, he muſt then have diſcovered this admirable Theory of his ſeries when he was not twenty four years of age; but what is ſtill more, this manuſcript contains both the diſcovery and method of Fluxions, or thoſe *infinitely ſmall quantities*, which have occaſioned ſo great a conteſt between M. Leibnits and him, or rather between Germany and England; of which I have given an account in 1716, in * the Elogium upon M. Leibnits; and tho' it was in the Elogium

* p. 109, &c.

Elogium of M. Leibnits, the impartiality of an Hiſtorian was ſo exactly kept that there now remains nothing new to be ſaid of Sir Iſaac Newton. It was there particularly obſerved *that Sir Iſaac was undoubtedly the Inventor, that his glory was ſecure, and that the only queſtion was, whether M. Leibnits did take this notion from him.* All England is convinced that he did take it from him, tho' the Royal Society have not declared ſo in their Determination, but only hinted it at moſt. However Sir Iſaac Newton was certainly the firſt Diſcoverer, and that too by many years. M. Leibnits on the other ſide was the firſt that publiſhed the Method, and if he did take it from Sir Iſaac, he at leaſt reſembled Prometheus in the fable, who ſtole fire from the Gods to impart it to Mankind.

In 1687 Sir Iſaac at length reſolved to unveil himſelf and ſhew what he was, and accordingly the *Philoſophiæ Naturalis principia Mathematica* appeared in the world. This book, in which the moſt profound Geometry ſerves for a baſis to a new ſyſtem of Philoſophy, had not at firſt all the reputation which it deſerved, and which it was afterwards to acquire. As it is written with great learning, conceived in few words, and the conſequences often ariſe ſo ſuddenly from their principles, that the Reader is obliged himſelf to ſupply the connection, it required time for the Publick to become maſters of it. Conſiderable Geometricians could not underſtand it without great application; and thoſe of a lower claſs undertook it not, 'till they were excited by the applauſe of the moſt skillful, but at length when the book was ſufficiently underſtood, all theſe applauſes which it ſo ſlowly acquired broke out on all ſides, and united in a general admiration. Every body was ſtruck with that Original ſpirit that ſhines throughout the whole work, that maſterly genius which in the whole compaſs of the happieſt age was ſhared only amongſt three or four men picked out from all the moſt learned Nations.

There are two Theories which chiefly prevail in the *Principia*, That of the Central power, and that of the Reſiſtance which mediums make to Motion, both almoſt entirely new, and treated of according to the ſublime Geometry of the Author. We can never touch upon either of theſe ſubjects without having Sir Iſaac before us, without repeating what he has ſaid, or following his track, and if we endeavour'd to diſguiſe it, what skill could prevent Sir Iſaac Newton's appearing in it?

The relation between the revolutions of the Heavenly bodies and their diſtances from the common center of thoſe revolutions, found out by Kepler, prevails throughout the whole Celeſtial ſyſtem. If we ſuppoſe, as it is neceſſary, that a certain force hinders theſe great bodies from purſuing, above an inſtant, their natural motion in a ſtreight line from Weſt to Eaſt, and continually draws them towards a center; it follows, by Kepler's rule, that this force, which will be central or rather centripetal, will act differently upon the ſame body according to its different diſtances from that center, and this in the reciprocal proportion of the ſquares of thoſe diſtances; that is, for

inſtance,

inſtance, if a body be at twice the diſtance from the center of its revolution, the action of the central force upon it will be four times weaker. It appears that Sir Iſaac ſet out from hence when he entered upon his phyſicks of the world in general: We may likewiſe ſuppoſe or imagine that he firſt conſidered the Moon, becauſe the Earth is the center of her motion.

If the Moon ſhould loſe all her impulſe or inclination to move from Weſt to Eaſt in a ſtraight line, and if nothing but the central power remained which forces her towards the center of the Earth, ſhe would then only obey that power, only follow its directions, and move in a ſtrait line towards that center. The velocity of her motion being known, Sir Iſaac demonſtrates from that motion that in the firſt minute of her deſcent ſhe would fall 15 Paris feet: her diſtance from the Earth is 60 ſemi-diameters of the Earth, therefore when the Moon comes to the ſurface of the Earth, the action of the force which brought her thither will be encreaſed as the ſquare of 60, that is, it would be 3600 times ſtronger; ſo that the Moon in her laſt minute would fall 3600 times 15 feet.

Now if we ſuppoſe that the force which would have acted upon the Moon is the ſame which we call Gravity in terreſtrial bodies, it will follow from the ſyſtem of Galileo that the Moon, which at the ſurface of the Earth would have fallen 3600 times 15 feet in a minute, ſhould likewiſe have fallen 15 feet in the firſt 60th part, or in the firſt ſecond of that minute. Now it is known by all experiments, and they only can be made at ſmall diſtances from the ſurface of the Earth, that heavy bodies fall 15 feet in the firſt ſecond of their fall: Therefore as to the velocity of their fall they are exactly in the ſame condition, as if having made the ſame revolution round the Earth that the Moon doth and at the ſame diſtance, they ſhould happen to fall by the mere force of their Gravity; and if they are in the ſame condition as the Moon, the Moon is in the ſame condition as they, and is only moved each inſtant towards the Earth by the ſame Gravity. So exact an agreement of effects, or rather this perfect identity can proceed from nothing elſe but the cauſes being the ſame.

It is true that in the ſyſtem of Galileo, which is here followed, the Gravity is equal, and the central force of the Moon is not ſo, even in the demonſtration that has juſt been given; but Gravity may well not diſcover its inequality, or rather, it only appears equal in all our experiments, becauſe the greateſt height from which we can obſerve bodies falling is nothing in compariſon of 1500 leagues, the diſtance which they all are from the center of the Earth. It is demonſtrated that a Canon bullet ſhot horizontally deſcribes, in the Hypotheſis of equal Gravity, a parabolic line, terminated at a certain point, where it meets with the Earth, but if it was ſhot from an height conſiderable enough to make the inequality of the action of its Gravity perceptible, inſtead of a Parabola it would deſcribe an Ellipſis, of which the center of the Earth

Earth would be one of the Foci, that is, it would perform exactly what the Moon doth.

If the Moon hath Gravity like terreſtrial bodies, if ſhe is moved towards the Earth by the ſame power, by which they are moved; if, according to Sir Iſaac's expreſſion, ſhe gravitates towards the Earth, the ſame cauſe acts upon all the reſt of that wonderful concourſe of heavenly bodies; for all nature is one and the ſame, there is every where the ſame diſpoſition, every where Ellipſes will be deſcribed by bodies, whoſe motions are directed to a body placed in one of their Foci. The Satellites of Jupiter will gravitate towards Jupiter, as the Moon gravitates towards the Earth; the Satellites of Saturn towards Saturn, and all the Planets together towards the Sun.

It is not known in what Gravity conſiſts. Sir Iſaac Newton himſelf was ignorant of it. If Gravity acts only by impulſe, we may conceive that a block of marble falling, may be puſhed towards the Earth, without the Earth being in any manner puſhed towards it; and in a word all the centers to which the motions cauſed by Gravity have relation, may be immoveable. But if it acts by Attraction the Earth cannot draw the block of marble, unleſs the block of marble likewiſe draw the Earth, why then ſhould that attractive power be in ſome bodies rather than others? Sir Iſaac always ſuppoſes the action of Gravity in all bodies to be reciprocal and in proportion only to their bulk; and by that ſeems to determine Gravity to be really an attraction. He all along makes uſe of this word to expreſs the active power of bodies, a power indeed unknown, and which he does not take upon him to explain; but if it can likewiſe act by Impulſe, why ſhould not that clearer term have the preference? for it muſt be agreed that it is by no means poſſible to make uſe of them both indifferently, ſince they are ſo oppoſite. The continual uſe of the word Attraction ſupported by great authority, and perhaps too by the inclination which Sir Iſaac is thought to have had for the thing itſelf, at leaſt makes the Reader familiar with a notion exploded by the Carteſians, and whoſe condemnation had been ratified by all the reſt of the Philoſophers; and we muſt now be upon our guard, leſt we imagine that there is any reality in it, and ſo expoſe our ſelves to the danger of believing that we comprehend it.

However all bodies according to Sir Iſaac gravitate towards each other, or attract each other in proportion to their Bulk: and when they revolve about a common center, by which conſequently they are attracted, and which they attract, their attractive powers are in the reciprocal proportion of their diſtances from that center, and if all of them together with their common center revolve round another center common to them, and to others, this will again produce new proportions, which will become ſtrangely complex. Thus each of the five Satellites of Saturn gravitate towards the other four, and the other four gravitate towards it; all the five gravitate towards Saturn, and Saturn towards them; all together gravitate towards the Sun, and the Sun again towards them. What an excellent Geometrician

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muſt he have been to ſeparate ſuch a Chaos of relations! the very undertaking ſeems raſhneſs; and we cannot without aſtoniſhment conceive that from ſo abſtracted a Theory, compoſed of ſo many ſeparate Theories, all very difficult to handle, ſuch neceſſary concluſions ſhould ariſe, and all conformable to the approved axioms of Aſtronomy.

Sometimes theſe concluſions even foretel events, which the Aſtronomers themſelves had not remarked. It is aſſerted, and more eſpecially in England, that when Jupiter and Saturn are neareſt, which is at 165 millions of leagues diſtance, their motions have no longer the ſame regularity as in the reſt of their courſe; and the Syſtem of Sir Iſaac at once accounts for it, which cannot be done by any other Syſtem. Jupiter and Saturn attract each other with greater force, becauſe they are nearer; and by this means the regularity of the reſt of their courſe is very ſenſibly diſordered; nay, they go farther ſtill, and determine the quantity and the bounds of this irregularity.

The motion of the Moon is the leaſt regular of any of the Planets, the moſt exact tables are ſometimes wrong, and ſhe makes certain excurſions which could not before be accounted for. Dr. Halley, whoſe profound skill in mathematicks has not hindered his being a good Poet, ſays in the Latin verſes prefixt to the *Principia*,

Diſcimus hinc tandem qua cauſa argentea phœbe
Paſſibus haud æquis graditur; cur ſubdita nulli
Hactenus Aſtronomo numerorum frena recuſet.

That

That the Moon till then never ſubmitted to the bridle of calculations, nor was ever broke by any Aſtronomer; but that at laſt ſhe is ſubdued in this new Syſtem. All the irregularities of her courſe are there ſhewn to proceed from a neceſſity by which they are foretold. It is difficult to imagine that a Syſtem in which they take this form ſhould be no more than a lucky conjecture; eſpecially if we conſider this but as a ſmall part of a Theory, which with the ſame ſucceſs comprehends an infinite number of other ſolutions. The ebbing and flowing of the Tyde ſo naturally ſhews it ſelf to proceed from the operation of the Moon upon the Sea, combined with that of the Sun, that the admiration which this phenomenon uſed to raiſe in us ſeems to be leſſened by it.

The ſecond of theſe two great Theories, upon which the *Principia* chiefly runs, is that of the Reſiſtance of *mediums* to motion, which muſt enter into the conſideration of all the chief phenomena of Nature, ſuch as the motions of the celeſtial bodies, of Light and Sound. Sir Iſaac, according to his uſual method, lays his foundations in the moſt ſolid proofs of Geometry, he conſiders all the cauſes from which reſiſtance can poſſibly ariſe; the denſity of the medium, the ſwift motion of the body moved, the magnitude of its ſuperficies, and from thence he at laſt draws concluſions which deſtroy all the Vortices of Des Cartes, and overturn that immenſe celeſtial edifice, which we might have thought immoveable. If the Planets move round the Sun in a certain medium whatever it be, in an ætherial matter which fills up the whole, and which notwithſtanding

withſtanding its being extreamly ſubtil, will yet cauſe reſiſtance as is demonſtrated, whence comes it then that the motions of the Planets are not perpetually, nay inſtantly leſſened? but beſides this, how can Comets traverſe thoſe Vortices freely every way, ſometimes with a tendency abſolutely oppoſite to theirs, without receiving any ſenſible alteration in their motions, tho' of never ſo long a continuance? whence comes it that theſe immenſe torrents whirling round with almoſt incredible velocity, do not inſtantly deſtroy the particular motion of any body, which is but an atom in compariſon of them, and why do they not force it to follow their courſe? The Celeſtial Bodies do then move in a vaſt vacuum, unleſs their exhalations and the rays of Light which together form a thouſand different mixtures, ſhould mingle a ſmall quantity of matter with the almoſt infinite immaterial ſpaces. Thus Attraction and Vacuum baniſhed from Phyſicks by Des Cartes, and in all appearance for ever, are now brought back again by Sir Iſacc Newton, armed with a power entirely new, of which they were thought incapable, and only perhaps a little diſguiſed.

Theſe two great men, whoſe Syſtems are ſo oppoſite, reſembled each other in ſeveral reſpects, they were both Genius's of the firſt rank, both born with ſuperior underſtandings, and fitted for the founding of Empires in Knowledge. Being excellent Geometricians, they both ſaw the neceſſity of introducing Geometry into Phyſicks; For both founded their Phyſicks upon diſcoveries in Geometry, which may almoſt be ſaid of none but themſelves. But one of them taking a bold flight, thought at once to reach the Fountain of All things, and by clear and fundamental ideas to make himſelf maſter of the firſt principles; that he might have nothing more left to do, but to deſcend to the phenomena of Natures as to neceſſary conſequences; the other more cautious, or rather more modeſt, began by taking hold of the known phenomena to climb to unknown principles; reſolved to admit them only in ſuch manner as they could be produced by a chain of conſequences. The former ſets out from what he clearly underſtands, to find out the cauſes of what he ſees; the latter ſets out from what he ſees, in order to find out the cauſe, whether it be clear or obſcure. The ſelf-evident principles of the one do not always lead him to the cauſes of the phenomena as they are; and the phenomena do not always lead the other to principles ſufficiently evident. The boundaries which ſtop'd two ſuch men in their purſuits through different roads, were not the boundaries of Their Underſtanding, but of Human underſtanding it ſelf.

While Sir Iſaac was compoſing his great work, the *Principia*, he had alſo another in hand, as much an original and as new; which, tho' by the title it did not ſeem ſo genetal, is yet as extenſive by the manner in which he has treated that particular ſubject. *This work* was his *Opticks, or treatiſe of Light and Colours*, which firſt appeared in the year 1704, after he had been making the neceſſary experiments for thirty years together.

It

It is no ſmall art to make experiments exactly. Every matter of fact which offers it ſelf to our conſideration is complicated with ſo many others, which either compound or modify it, that without abundance of skill they cannot be ſeparated; nay without an extraordinary ſagacity, the different elements that enter into the compoſition can hardly be gueſſed at. The fact therefore to be conſidered muſt be reſolved into the different ones of which it is compoſed; and they themſelves are perhaps compoſed of others; ſo that if we have not choſen the right road, we may ſometimes be engaged in endleſs Labyrinths. The Principles and Elements of things ſeem to have been conceal'd from us by Nature, with as much care as the Cauſes, and when we attain to the diſcovery of them, it is a ſight entirely new and unexpected.

What Sir Iſaac Newton aims at quite through his *Opticks*, is the Anatomy of Light; this expreſſion is not too bold ſince it is no more than the thing it ſelf. By his experiments, the ſmalleſt ray of Light that is convey'd into a dark room, and which cannot be ſo ſmall, but that it is yet compounded of an infinite number of other rays, is divided and diſſected in ſuch manner, that the Elementary rays of which it is compoſed, are ſeparated from each other, and diſcover themſelves every one tinged with its particular colour, which after this ſeparation can no more be altered. The firſt total ray before the diſſection, is white, and this whiteneſs aroſe from all the particular colours of the Primitive rays. The ſeparating theſe rays is ſo difficult, that when Mariotte undertook it upon the firſt news of Sir Iſaac's experiments, he miſcarried in the attempt, even he who had ſuch a genius for experiments, and had been ſo ſucceſsful on many other ſubjects.

No primitive coloured rays could be ſeparated, unleſs they were ſuch by their nature, that in paſſing through the ſame medium, or through the ſame glaſs priſm, they are refracted at different angles, and by that means ſeparate when they are received at proper diſtances. This different Refrangibility of rays, red, yellow, green, blue, purple, and all other colours infinite in number, a property which was never before ſuſpected, and to which we could hardly be led by conjecture, is the fundamental diſcovery of Sir Iſaac Newton's treatiſe. The different Refrangibility leads us to the different Reflexibility. But there is ſomething more; for the rays which fall at the ſame angle upon a ſurface are refracted and reflected alternately, with a kind of play only diſtinguiſhable to a quick eye, and well aſſiſted by the judgment of the Obſerver. The only point, the firſt idea of which does not entirely belong to Sir Iſaac Newton, is, that the rays which paſs near the extremities of a body without touching it, do ſomewhat turn from a ſtrait line, which is called Inflection. But the whole together forms a body of Opticks ſo perfectly new, that we may henceforward look upon that ſcience as almoſt wholly owing to this Author.

That he might not confine himſelf to theſe bare ſpeculations, which are ſometimes unjuſtly ſtyled idle, he gave us the deſign of a Teleſcope by reflection,

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which was not thoroughly put in execution 'till a long time after. It has here been experienc'd that one of theſe Teleſcopes but 2 foot and a half long, had as good an * effect as a tolerable common Teleſcope of 8 or 9 feet, which is a very extraordinary advantage, and the whole improvement of it will probabily be better known hereafter.

One advantage of this book, equal perhaps to that of the many new diſcoveries with which it abounds, is that it furniſhes us with an excellent model of proceeding in Experimental Philoſophy. When we are for prying into Nature, we ought to examine her like Sir Iſaac, that is, in as accurate and importunate a manner. Things that almoſt hide themſelves from our enquiries, as being of too abſtracted a nature, he knows how to reduce to calculation, tho' ſuch calculations might elude the skill of the beſt Geometricians, without that Dexterity which was peculiar to himſelf; and the uſe which he makes of his Geometry, is as artful as the Geometry it ſelf is ſublime.

He did not finiſh his *Opticks*, becauſe ſeveral neceſſary experiments had been interrupted, and he could not begin them again. The parts of this building, which he left unfiniſhed, could by no means be carried on but by as able hands as thoſe of the firſt Architect: However he hath put ſuch who are inclined to carry on this work in a proper method, and even chalks out to them a way to proceed from Opticks, to a compleat body of Phyſicks, under the form of *Doubts*, or *Queries*

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* N. B. *By accurate tryals made here, a reflecting Teleſcope of 2 foot and a half, hath been found no ways inferior to one of the common ſort, of between 40 and 50 foot long.*

propoſing a great many deſigns which will help future Philoſophers, or which at leaſt will make a curious hiſtory of the Conjectures of a great Philoſopher.

Attraction is the governing principle in this ſhort plan of Phyſicks; that property which is called the *Hardneſs* of bodies, is the mutual attraction of their parts, which cloſes them together, and if they are of ſuch a figure as that whole ſurfaces are capable of being every where joined, without leaving any void ſpaces, the bodies are then perfectly hard. Of this kind there are only certain ſmall bodies, which are primitive and unalterable, and which are the elements of all other bodies. *Fermentations*, or chimical *Efferveſcences*, whoſe motion is ſo violent, that they may ſometimes be compared to ſtorms, are the effects of this powerful attraction, which acts upon ſmall bodies only at ſmall diſtances.

He conceives in general, that attraction is the active principle of every thing in Nature, and the cauſe of all motion. If a certain degree of motion that is once given to any thing by the hand of God, did afterwards only diſtribute it ſelf according to the laws of Percuſſion, it appears that it would continually decreaſe in its motion by contrary Percuſſions, without ever being able to recover itſelf, and the Univerſe would very ſoon fall into ſuch a ſtate of reſt, as would prove the deſtruction of the whole. The power of attraction, which always ſubſiſts and is not weakned by being exerted, is a perpetual ſpring of action and life. It may likewiſe happen that the effects of this power may at length combine in ſuch a manner, as that

that the Syſtem of the Univerſe may be diſordered, and require, according to Sir Iſaac's expreſſion, *a hand to repair it.*

He declares very freely that he lays down this attraction, only as a cauſe which he knows not, and whoſe effects he only conſiders, compares and calculates; and in order to avoid the reproach of reviving the *Occult qualities* of the Schoolmen, he ſays, that he eſtabliſhes none but ſuch Qualities as are *manifeſt* and very viſible by their phenomena, but that the cauſes of theſe Qualities are indeed *occult*, and that he leaves it to other Philoſophers to ſearch into them; but are they not properly cauſes which the Schoolmen call *occult qualities*; ſince their effects are plainly ſeen? beſides, could Sir Iſaac think that others would find out theſe *Occult cauſes* which he could not diſcover? with what hopes of ſucceſs can any other man ſearch after them?

At the end of his Opticks he put two treatiſes of pure Geometry, one concerning the *Quadrature of Curves* and the other of the *Enumeration of Lines*, which he ſtyles of the *third order*; he hath ſince left them out, becauſe the ſubject was too different from that of the Opticks, and they were printed ſeparately in 1711, with an *Analyſis by Infinite equations* and the *Differential method.* It would be only repetition to ſay, that throughout all his works there appears a refined ſort of Geometry that is peculiar to himſelf.

Being ſo taken up with theſe ſpeculations, he ſhould naturally ſeem to have had no inclination to Buſineſs, and to have been incapable of it; but yet when the priviledges of the Univerſity of Cambridge, where he had been Mathematical Profeſſor from 1669, by Dr. Barrow's reſignation to him, were attackt by King *James* II, in 1687. (in which year he publiſhed the *Principia*) he was very zealous in aſſerting them, and the Univerſity named him one of the Delegates to the *High Commiſſion* court. He was likewiſe one of their Repreſentatives in the Convention-Parliament of 1688, and ſate in it 'till it was diſſolved.

In 1696 the Earl of Hallifax, who was Chancellor of the Exchequer and a great patron to learned men; (for the Engliſh Nobility do not think it a point of honour to ſlight them, but are frequently ſuch themſelves) obtained from King William the office of Warden of the Mint for Sir Iſaac Newton; and in this employment he was very ſerviceable in the great re-coynage at that time. Three years after he was made Maſter and Worker, a place of conſiderable profit which he enjoyed 'till his death.

It may be thought that this place in the Mint was ſuitable to him only becauſe he was an excellent Geometrician and had great skill in Phyſicks; and indeed this buſineſs often requires very difficult Calculations, and a great number of Chimical experiments, of his skilfulneſs in which there are many proofs in his *Table of the Eſſays of foreign Coins* printed at the End of Dr. Arbuthnot's book. But his genius extended likewiſe to matters merely political, and in which there was no mixture of ſpeculative Sciences; for upon the calling of the Parliament in 1701, he was again choſen Repreſentative for the Univerſity

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of Cambridge. After all, it is perhaps an error to look upon the Sciences and Buſineſs as incompatible, eſpecially to Men of a certain turn. Political Affairs, when well underſtood, are naturally reduced to refined Calculations, and have ſo near an affinity, that thoſe who are uſed to ſublime ſpeculations comprehend them with greater facility and more certainty, as ſoon as they are acquainted with the facts and furniſhed with proper materials.

It was Sir Iſaac Newton's peculiar happineſs, to enjoy the reward of his merit in his life-time, quite contrary to Des Cartes, who did not receive any honours 'till after his death. The Engliſh do not reſpect great Genius's the leſs for being born amongſt them; and ſo far are they from endeavouring to depreciate them by malicious criticiſms, ſo far from approving the envy which attacks them, that they all conſpire to raiſe them; and that great degree of Liberty which occaſions their differences in the moſt important points, does not hinder them from uniting in this. They are all very ſenſible how much the glory of the *Underſtanding* ſhould be valued in a State, and whoever can procure it to their Country becomes extremely dear to them. All the learned Men of a Nation, which produces ſo many, placed Sir Iſaac at their head by a kind of unanimous applauſe, they acknowledged him for their Chief and their Maſter: not ſo much as one oppoſer durſt appear, nay they would not even have admitted of a moderate admirer. His Philoſophy hath been adopted throughout England, it prevails in the Royal Society, and in all the excellent

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performances which have come from thence; as if it had been already made ſacred by the reſpect of a long ſeries of ages. In ſhort He was reverenced to ſo great a degree that death could not procure him new honours, and he himſelf ſaw his own *Apotheoſis*. Tacitus who has reproach'd the Romans with their extreme indifference for the great men of their Nation, would certainly have given the Engliſh the quite contrary Character. In vain would the Romans have excuſed themſelves by pretending that great merit was no more than what was common amongſt them. Tacitus would have told them that it never was ſo, or that we ſhould even endeavour to make it ſo by the honour we annex to it.

In 1703, Sir Iſaac Newton was choſen Preſident of the Royal Society, and continued ſo without any interruption 'till the time of his death, for the ſpace of 23 years; a ſingular example, and one from which they could fear no ill conſequences hereafter. Queen Anne Knighted him in 1705, a title of honour which at leaſt ſerves to ſhew that his name had reached the Throne, to which the moſt celebrated names do not always arrive.

He was more known than ever in the court of the late King. The Princeſs of Wales, who is now Queen of Great Britain, has ſo excellent an underſtanding and ſo much knowledge that ſhe was capable of asking queſtions of ſo great a Man, and could receive ſatisfactory anſwers from none but himſelf. She has often declared publickly that ſhe thought it an happineſs to live in his time and to be acquainted with him. In how many other Ages, in how many other Nations might

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he have been placed without meeting with ſuch another Princeſs!

He had compoſed a treatiſe of Ancient Chronology, which he had no thoughts of publiſhing, but that Princeſs, to whom he imparted ſome of the chief points, thought them ſo new, and ſo full of art, that ſhe deſired a ſummary of the whole Work, which ſhe never would part with, and would be alone in poſſeſſion of. She ſtill keeps it amongſt her choiceſt treaſures. However there eſcaped a copy of it. A curioſity excited by ſuch a particular piece of Sir Iſaac Newton could hardly be hindered from employing the utmoſt addreſs to come at ſo great a treaſure, and in truth they muſt have been very ſevere who would have condemned ſuch a curioſity. This Copy was brought into France, by the perſon who was ſo happy as to procure it, and the value which he had for it hindered his being very careful of it; ſo that it was ſeen, tranſlated, and at length printed.

The main deſign of this Syſtem of Chronology of Sir Iſaac, as appears by the extract we have of it, is to find out by following with abundance of Sagacity ſome of the tracks, however faint they are, of the moſt ancient Greek Aſtronomy, what was the poſition of the colure of the Equinoxes with reſpect to the fix'd ſtars, in the time of Chiron the Centaur. As it is now known that theſe Stars have a motion in longitude of one degree in 72 years, if it is once known that in Chiron's time the Colure paſſed through certain fixt Stars, it may be known by taking their diſtance from thoſe, through which it now paſſes, how much time hath elapſed from

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Chiron until our days. Chiron was one of thoſe who went along with the Argonauts in their famous expedition; this would therefore fix the Epocha of that expedition, and conſequently afterwards that of the Trojan War; two great Events upon which all ancient Chronology depends. Sir Iſaac places them 500 years nearer the Chriſtian Æra than they are uſually placed by other Chronologers.

This Syſtem has been attackt by two learned Frenchmen; who are blamed in England for not having ſtaid for the whole work, and with having been ſo haſty in their Criticiſm. But is not this their earneſtneſs an honour to Sir Iſaac? They ſeized as ſoon as poſſible the glory of having ſuch an adverſary; and they are like to find others in his ſtead: For the famous Dr. Halley, chief Aſtronomer to the King of Great Britain, has already written in the defence of the Aſtronomical part of the Syſtem; and his friendſhip for the great man deceaſed, as well as his great skill in this Science make him a formidable adverſary. But after all the conteſt is not determined; the publick, ſuch I mean as are capable of judging of it, and who are but few in number, have not yet done it, and tho' it ſhould happen that the ſtrongeſt arguments were on one ſide, and only Sir Iſaac's name on the other, perhaps the World would remain ſome time in ſuſpence, and perhaps too with reaſon.

As ſoon as the Academy of Sciences, by their Regulation in *1699*, could chuſe foreigners into the number of their aſſociates, they failed not to make Sir Iſaac Newton one of them. He all along held correſpondence with them, by ſending them whatever he publiſhed.

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This was ſome of his former works which he either cauſed to be reprinted, or which he now firſt publiſhed. But after he was employed in the Mint where he had now been for ſome time, he no more engaged himſelf in any conſiderable new undertaking either in Mathematicks or Philoſophy. For tho' his ſolution of the famous problem of the *Trajectoriæ* propoſed to the Engliſh by way of challenge by M. Leibnits during his conteſt with them, and which was much ſought after both for the perplexity and difficulty of it, may be reckon'd a conſiderable attempt, it was hardly more than diverſion to Sir Iſaac Newton. He received this problem at four of the clock in the afternoon, at his return from the Mint very much fatigued, and never went to bed 'till he had maſtered it.

After having been ſo ſerviceable to all the learned part of Europe in ſpeculative Sciences, he devoted himſelf entirely to the ſervice of his country in affairs that were more viſibly and directly advantageous to it, a ſenſible pleaſure to every good ſubject; but all his leiſure time he devoted to the curioſity of his Mind; he thought no kind of knowledge beneath his conſideration, and he knew how to improve himſelf by every thing. After his death there were found amongſt his papers ſeveral writings, upon Antiquity, Hiſtory, and even Divinity it ſelf, which is ſo widely different from thoſe Sciences, for which he is ſo much diſtinguiſhed. He never ſuffered a moment to paſs unemployed, and he never ſpent his time after a trifling manner, or with ſlight attention to what he was about.

He all along enjoyed a ſettled and equal ſtate of health untill he was fourſcore years old; a very eſſential circumſtance of the extraordinary happineſs which he enjoyed. He then began to be afflicted with an Incontinence of Urine, and yet the five years following which preceded his death, he enjoyed long intervals of health, or was tolerably well by means of the regularity of his diet, or by taking that care of himſelf which he had hitherto had no occaſion for. He was then forced to rely upon Mr. Conduit, who had married his Neice, to manage his buſineſs at the Mint; which he had not done but that he was very confident that he repoſed a truſt that was of ſo important and delicate a nature, in good hands; and his opinion has been confirmed ſince his death by the choice of the King, who has given that Employment to Mr. Conduit. Sir Iſaac Newton did not undergo much pain till the laſt twenty days of his life, when it was thought that he certainly had the Stone in his bladder, and that he could not recover. In theſe fits of pain, which were ſo violent that drops of ſweat fell from his face, he never cried out, nor expreſſed the leaſt impatience; and as ſoon as he had a moment's eaſe, he ſmiled, and ſpoke with his uſual cheerfulneſs. Till that time he had always read and writ ſeveral hours every day. He read the News Papers on Saturday morning the eighteenth of March, and talked a great while with the famous phyſician Dr. Mead, and perfectly enjoyed all his ſenſes and his underſtanding, but at night he entirely loſt all manner of ſenſe, and never recovered it again; as if

the

the Faculties of his Soul were ſubject only to be totally extinguiſhed,and not to be leſſened by degrees. He died on the Monday following the twentieth of March, being in his Eighth-fifth year.

His corps was laid in ſtate in the Jeruſalem Chamber, from whence perſons of the greateſt quality and ſometimes crowned heads are carried to their grave. He was buried in Weſtminſter Abby, his pall being held up by the Lord Chancellor, the Dukes of Montroſe and Roxburgh, and by the Earls of Pembroke, Suſſex and Macclesfield. By theſe ſix peers of England you may eaſily judge how many perſons of diſtinction attended his funeral. The Biſhop of Rocheſter (as dean of Weſtminſter) performed the ſervice, attended by all the Clergy belonging to the Abby, and the body was interred juſt at the entrance into the choire. We muſt look back to the Ancient Greeks if we would find out examples of ſo extraordinary a veneration for learning. His family imitate the Grecians as near as poſſible by a monument which they intend to erect for him, and which will coſt a conſiderable ſum of money; and the Dean and Chapter of Weſtminſter have allowed it to be put up in a place in the Abby, which hath often been refuſed to Nobility of the firſt rank. Both his Country and Family were as remarkable in expreſſing their grateful reſpect towards him; as if by voluntary choice he had made them his.

He was of a middle ſtature, ſomewhat inclined to be fat in the latter part of his life; he had a very lively and piercing eye; his countenance was pleaſing and venerable

nerable at the ſame time, eſpecially when he pulled off his peruke and ſhewed his white head of hair that was very thick. He never made uſe of ſpectacles, and loſt but one tooth in all his life. His name is a ſufficient excuſe for our giving an account of theſe minute circumſtances.

He was born with a very meek diſpoſition, and an inclination for quietneſs. He could rather have choſen to have remained in obſcurity, than to have the calm of his life diſturbed by thoſe ſtorms of Literature, which Wit and Learning brings upon thoſe who ſet too great a value upon themſelves. We find by one of his letters in the *Commercium Epiſtolicum*, that his treatiſe of Opticks being ready for the preſs, certain unſeaſonable objections which happened to ariſe made him lay aſide this deſign at that time. *I upbraided my ſelf*, ſays he, *with my imprudence, in loſing ſuch a reality as Quiet in order to run after a ſhadow.* But this ſhadow did not eſcape him in the concluſion; it did not coſt him his quiet which he ſo much valued, and it proved as much a reality to him as that quiet it ſelf.

A meek diſpoſition naturally promiſes modeſty, and it is affirmed that his was always preſerved without any alteration, tho' the whole world conſpired againſt it. He never talked of himſelf, or with contempt of others, and never gave any reaſon even to the moſt malicious obſervers to ſuſpect him of the leaſt notion of Vanity. In truth he had little need of the trouble and pains of commending himſelf; but how many others are there who would not have omitted that part, which men ſo

willingly

willingly take upon themſelves, and do not care to truſt with others? How many great men who are univerſally eſteemed, have ſpoiled the concert of their praiſe, by mixing their own voices in it!

He had a natural plainneſs and affability, and always put himſelf upon a level with every body. Genius's of the firſt rank never deſpiſe thoſe who are beneath them, whilſt others contemn even what is above them. He did not think himſelf diſpenſed with, either by his merit, or reputation, from any of the ordinary duties of life; he had no ſingularity either natural or affected, and when it was requiſite he knew how to be no more than one of the common rank.

Tho' he was of the Church of England, he was not for perſecuting the Non-conformiſts in order to bring them over to it. He judged of men by their manners, and the true Non-conformiſts with him were the vicious and the wicked. Not that he relied only on natural religion, for he was perſuaded of Revelation; and amongſt the various kind of books which he had always in his hands, he read none ſo conſtantly as the Bible.

The plenty which he enjoyed, both by his paternal eſtate, and by his Employments, being ſtill increaſed by the wiſe ſimplicity of his manner of living, gave him opportunities of doing good, which were not neglected. He did not think that giving by his laſt Will, was indeed giving; ſo that he left no Will; and he ſtript himſelf whenever he performed any act of generoſity, either to his Relations or to thoſe whom he thought in want.

And

And the good actions which he did in both capacities were neither few nor inconſiderable. When decency required him upon certain occaſions to be expenſive and make a ſhew, he was magnificent with unconcern, and after a very graceful manner. At other times all this pomp, which ſeems conſiderable to none but people of a low genius, was laid aſide, and the expence reſerved for more important occaſions. It would really have been a prodigy, for a mind uſed to reflection and as it were fed with reaſoning, to be at the ſame time fond of this vain magnificence.

He never married, and perhaps he never had leaſure to think of it; being immerſed in profound and continual ſtudies during the prime of his age, and afterwards engaged in an Employment of great importance, and his intenſe application never ſuffered him to be ſenſible of any void ſpace in his life, or of his having occaſion for domeſtick ſociety.

He left behind him about 32000 pounds Sterling. M. Leibnitz, his rival, likewiſe died in good Circumſtances, tho' not ſo rich: But he left a conſiderable ſum of money which he had hoarded up. * Theſe two extraordinary examples, and both of Strangers, ſeemed to deſerve our remembrance.

* V. L'Hiſt. 1716. p. 128.

FINIS.

Appendix

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Note on the Printing of
Bentley's Sermons

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Appendix

Comments on Birch's *History of the Royal Society* and an index to its references to Newton

ROBERT E. SCHOFIELD

Thomas Birch, D.D. (1705–1766)[1] was one of the many historian-antiquarian Fellows of the Royal Society who were such a large part of its membership in the first part of the 18th century. Largely self-educated,[2] he appears to have earned his living through his Whig connections and by a prolific pen. He wrote histories and biographies in great numbers, was a frequent pamphleteer, and assisted in the editing of many other works, including the *General Dictionary, Historical and Critical* (1734–1741) and the *Gentleman's Magazine.*

His writing style was a pondorous one and excited unfavorable comment from such judges as Horace Walpole and Samuel Johnson. His biographer in the *D. N. B.* mentions the "wearisome minuteness of detail" and the "dulness of style" apparent in his works. These characteristics, however, while they may be flaws in a literary sense, are prized by many a historian who finds the works of Dr. Birch to be indispensable sources.

The historian of science has three major reasons for taking an interest in Birch. For thirteen years (1752–1765) he discharged the duties of

[1] Most of the material following is taken from *The Dictionary of National Biography* (1920–21 printing; Oxford University Press, London), vol. II, pp. 530 ff.

[2] The D.D. is honorary; he was created D.D. of the Marischal College, Aberdeen and of Lambeth in 1753.

THE

HISTORY

OF THE

ROYAL SOCIETY of LONDON

FOR IMPROVING OF

NATURAL KNOWLEDGE,

FROM ITS FIRST RISE.

IN WHICH

The moſt conſiderable of thoſe Papers communicated to the SOCIETY, which have hitherto not been publiſhed, are inſerted in their proper order,

AS A SUPPLEMENT TO

THE PHILOSOPHICAL TRANSACTIONS.

By THOMAS BIRCH, D.D.

SECRETARY to the ROYAL SOCIETY.

VOL. I.

Talem intelligo PHILOSOPHIAM NATURALEM, *quæ non abeat in fumos ſpeculationum ſubtilium aut ſublimium, ſed quæ efficaciter operetur ad ſublevanda vitæ humanæ incommoda.* BACON de Augm. Scient. L. ii. c. 2.

LONDON:
Printed for A. MILLAR in the Strand.
MDCCLVI.

secretary of the Royal Society, and his work in that position was graced by his passion for detail. In 1744, he edited the *Works of the Honourable Robert Boyle,* an edition which is still useful to scholars who can obtain it. Finally, in 1756–57, he published a history of the Royal Society.

Birch's *History of the Royal Society of London*[3] cannot properly be called a history at all. In this respect it resembles its predecessor, Sprat's *History of the Royal Society.* But, like Sprat, Birch has provided us with the material out of which histories can be written. Bishop Sprat wrote his "history" before the Society really had much history to detail, but it is in Sprat that we find the philosophy which lay behind the organization of the Society and learn of the type of opposition it faced. Birch's "history" contains next to nothing by Birch, and no analyses of any type, but, for the historian of science who does not have access to the papers of the Royal Society, it provides a transcription of the minutes of the Society and the council from its founding through December 1687, and reprints numerous papers which were read before the Society but never printed in its *Transactions.*

This period covers the most productive years of Newton's scientific career. The index below follows Newton in his relation with the Society from the date of his election in 1671/2 down to the publication of the *Principia* in 1687. All references, in parentheses, to publication of letters in the *Philosophical Transactions* are given by Birch.

INDEX

VOLUMES I AND II

Nothing by Newton; the only item about him is a proposal of Newton for membership (last page, vol. II) made by the Bishop of Salisbury (Seth Ward).

VOLUME III

Page 1. January 11, 1671/2. Newton elected Fellow of the Royal Society. Discussion of Newton's "improvement of telescopes by contracting them"; the telescope sent by Newton to the Society had been seen by the King and others. A "description and scheme of it" sent by the secretary to Huygens; Newton wrote a letter to Oldenburg (January 6, 1671/2) "altering and enlarging the description of his instrument."

Pages 2–3. Text of the aforementioned letter of Newton (see also *Phil. Trans.,* No. 81, p. 4004).

[3] See reproduction of title page to vol. I. Volume II was printed in the same year; vols. III and IV were printed the following year, 1757.

Page 3. The secretary ordered to write Newton acquainting him with his election into the Society, thanking him for the communication of his telescopes.

Page 4. January 18, 1671/2. Newton's "new telescope was examined and applauded."

January 25, 1671/2. A "reflecting telescope of four feet long, of Mr. Newton's invention" produced; the "metalline concave was not duly polished." Ordered that the instrument "be perfected against the next meeting."

Robert Boyle having made a type of "opaque glass . . . to serve for reflecting concaves," ordered that Boyle be asked whether larger pieces could be made "for the use of Mr. Newton's telescopes." A letter from Newton to Oldenburg (Cambridge, January 18, 1671/2) read, in which Newton discussed a way to prepare "metalline matter for reflecting concaves," and hinted at "a considerable philosophical discovery" which he would send to the Society.

Page 5. Text of the aforementioned letter. Newton "thanked for his respect to the Society," and asked "to impart to them the intimated discovery, as soon as he conveniently could."

Page 8. February 1, 1671/2. "The four foot telescope of Mr. Newton's invention was produced again, being improved since the last meeting." Recommended that Hooke "see it perfected as far as it was capable of being."

Page 9. February 8, 1671/2. Newton's letter on light and colors (Cambridge, February 6, 1671/2) read. (Printed in *Phil. Trans.*, No. 80, p. 3075.) Newton to be thanked by the Society (reference to Oldenburg's letter to Newton) and asked for consent "to have it forthwith published," to protect him "against the pretensions of others." Ordered that Newton's communication "be entered into the register-book; and that the bishop of Salisbury, Mr. Boyle, and Mr. Hooke be desired to peruse and consider it, and bring in a report of it to the Society."

Page 10. February 15, 1671/2. Reading of Hooke's "considerations upon Mr. Newton's discourse on light and colours." Hooke thanked for "ingenious reflections." Ordered that Hooke's paper be registered, and a copy of it sent to Newton. Hooke's paper not to be printed together with Newton's, "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."

Pages 10–15. Text of Hooke's criticism of Newton.

Page 15. February 15, 1671/2. "Mons. Schroter presented for the repository a glass, which by a metallic body he had tinged red." Hooke "put in mind of the six foot tube of Mr. Newton's invention, and of bringing in a specimen of the effect of his own proposition."

Page 15. February 22, 1671/2. Reading of Newton's letter to Oldenburg (Cambridge, February 20, 1671/2) "promising an answer to Mr. Hooke's observations upon his new theory of light and colour." Text of Newton's letter, which also refers to Huygens' "several handsome and ingenious remarks."

Page 19. March 14, 1671/2. "Mr. Cock was ordered to make, for the use of the Society, a telescope of Mr. Newton's invention."

Page 21. March 21, 1671/2. A letter of Hevelius, concerning a comet, which he had observed in Andromeda, read; ordered that "notice should be given of this phænomenon" to persons in both universities for observation, "and particularly to Dr. Wallis and Mr. Newton."

A letter of Newton to Oldenburg (Cambridge, March 19, 1671/2) read; letter said to contain "several particulars relating to his new telescope." (Printed in *Phil. Trans.,* No. 81, p. 4009.)

Page 30. March 28, 1672. A letter from Newton to Oldenburg (Cambridge, March 26, 1672) read, containing "some more particulars relating to his new telescope, especially the proportions of the apertures." (Printed in *Phil. Trans.,* No. 82, p. 4032.)

Page 41. April 4, 1672. A letter from Newton to Oldenburg (Cambridge, March 30, 1672) communicated, answering difficulties raised by Auzout and queries raised by Denys; a proposal by Newton to use "instead of the little oval metal in that telescope, a crystal figured like a triangular prism." (Extract printed in *Phil. Trans.,* No. 82, p. 4034.) Hooke ordered to make "such a crystalline prism" and to "try the same."

Page 43. April 18, 1672. Hooke "ready to make an experiment by a prism" showing that it is possible "to destroy all colours by one prism, which had appeared before through another." There being no sun, the experiment was deferred.

Among letters read, that of Pardies (April 9, 1672) contained "some objections against Mr. Newton's theory of light and colours." (Printed in *Phil. Trans.,* No. 84, p. 4087.) Also a letter from Newton (Cambridge, April 13, 1672), answering "the objections of the said jesuit." (Printed, *Phil. Trans.,* No. 84, p. 4091.) Also another letter of Newton with same date, "answering some experiments proposed by Sir Robert Moray for the clearing of his theory of light and colours." (Printed in *Phil. Trans.,* No. 83, p. 4059.)

Page 47. April 24, 1672. Hooke made the experiments with prisms.

Page 49. May 8, 1672. A letter of Newton to Oldenburg read (Cambridge, May 4, 1672) with Newton's "judgment of Mons. Cassegraine's telescope, like that of Mr. James Gregory . . . with a hole in the midst of the optic metal to transmit the light to an eye-glass placed behind it." (Printed in *Phil. Trans.,* No. 83, p. 4057.)

Page 50. May 15, 1672. Hooke performed "experiments relating to Mr. Newton's theory of light and colours, which he was desired to bring in writing to be registered."

Page 50. May 22, 1672. Hooke made "more experiments with two prisms, confirming what Mr. Newton had said in his discourse on light and colours." Hooke suggested that "these experiments were not cogent to prove, that light consists of different substances or divers powders, as it were."

Page 52. June 12, 1672. Newton's "answer to Mr. Hooke's considerations upon his discourse on light and colours" produced; answer read in part, and ordered "to be copied for the perusal of Dr. Wren and Mr. Hooke." (Printed in *Phil. Trans.,* No. 88, p. 5084.)

Pages 52–54. June 19, 1672. Hooke's "account of some experiments on refractions and colours" read and registered. The text, as printed, deals with an experiment "which seems at first much to confirm Mr. Newton's theory of colours and light; but yet I think it not an *experimentuum crucis,* as I may possibly shew hereafter." Hooke requested "to make more experiments of the same nature, for a farther examination of Mr. Newton's doctrine of light and colours."

Page 56. July 3, 1672. A letter of Huygens (Paris, July 1, 1672) read, dealing with several topics, including "Mr. Newton's reflecting telescope, and applauding his new doctrine of light."

Page 57. July 10, 1672. Society to "make a recess for some time," but the members "desired" to "meet on Fridays" to "discourse of philosophical matters, and prosecute experiments . . . such, as might determine the queries lately sent by Mr. Newton . . . which involve his theory of light," and such "as might improve Mr. Newton's reflecting telescope."

Page 58. October 30, 1672. Examination "of what had been done concerning the queries of Mr. Newton, to be determined by experiments," referred to next meeting. As to "trials . . . made for the improvement of the reflecting telescope of Mr. Newton," Hooke said he "had wanted a mould of a sufficient bigness for a speculum, designed by him, of fifteen inches diameter."

Pages 79–82. March 26, 1673. Letter from Gregory to Collins (March 7, 1672/3), about telescopes, read. The text of the letter. Ordered that the letter "be communicated to Mr. Newton."

Page 83. April 9, 1673. A letter read from Huygens to Oldenburg (Paris, January 14, 1672/3) containing "some considerations upon Mr.

Newton's theory of light," and Newton's answer (Cambridge, April 3, 1673). (Part of Huygens' letter printed in *Phil. Trans.,* No. 96, p. 6086; Newton's letter printed in *Phil. Trans.,* No. 97, p. 6108.)

Page 122. February 5, 1673/4. Hooke produced "a new kind of reflecting telescope of his own contrivance, differing from that of Mr. Newton."

Page 178. January 28, 1674/5. Oldenburg said "that Mr. Newton had intimated his being now in such circumstances, that he desired to be excused from the weekly payments," and the Council excused him.

Page 181. February 18, 1674/5. "Mr. Isaac Newton, James Hoare, junior, Esq; were admitted."

Pages 193–194. March 11, 1674/5. Hooke's thoughts on the nature of light: "That light is a vibrating or tremulous motion in the medium, (which is thence called pellucid) produced from a like motion in the luminous body, after the same manner as sound was then generally explained by a tremulous motion of the medium conveying sound, produced therein by a tremulous motion of the sounding body: and that, as there are produced in sounds several harmonies by proportionate vibrations, so there are produced in light several curious and pleasant colours, by the proportionate and harmonious motions of vibrations intermingled; and as those of the one are sensated by the ear, so those of the other are by the eye." Hooke desired to have "ready for the next meeting, the apparatus necessary for the making Mr. Newton's experiments formerly alledged by him, for evincing the truth of his new theory of light and colours," especially in reference to a letter from Francis Linus (February 25, 1674/5) containing "assertions directly opposite to those of Mr. Newton." (Printed in *Phil. Trans.,* No. 121, p. 499.)

Page 194. March 18, 1674/5. Hooke's discourse "concerning the nature and properties of light."

Pages 216–217. April 15, 1675. A letter from Leibniz (Paris, March 30, 1675) read, containing "remarks on several algebraical subjects relating to Mr. James Gregory, Mr. Newton, and Mr. Collins, together with the different sentiments of the Parisian astronomers concerning common and telescopical sights."

Page 232. November 18, 1675. Oldenburg communicated Newton's letter (Cambridge, November 13, 1673) written in reply to a letter of Linus to Oldenburg (February 25, 1674/5), concerning "an experiment relating to Mr. Newton's new theory of light and colours"; Newton "directs his antagonist again very punctually, in what manner to try the experiment, to satisfy himself about his veracity in relating the same." (Printed in *Phil. Trans.,* No. 121, pp. 499, 500.)

Newton offering to send to the Society "a discourse of his about colours," Oldenburg "ordered to thank him for that offer, and to desire him to send the said discourse as soon as he pleased."

Pages 247–260. December 9, 1675. Newton's manuscript, "touching his theory of light and colours, containing partly an hypothesis to explain the properties of light discoursed of by him in his former papers," produced. "Of the hypothesis only the first part was read, giving an account of refraction, reflection, transparency, and opacity." Newton's letter printed, followed by "an hypothesis explaining the properties of light, discoursed of in my several papers." Newton's paper having contained reference to an electrostatic experiment, some of the members "desired, that it might be tried." This experiment "Newton proposed to be varied with a larger glass placed farther from the table." Ordered "that this experiment should be tried at the next meeting; and Mr. Hooke promised to prepare it for that meeting."

Newton to be asked by letter "whether he would consent, that a copy might be taken of his papers, for the better consideration of their contents."

Pages 260–269. December 16, 1675. "Mr. Newton's experiment of glass rubbed to cause various motions in bits of paper underneath" tried unsuccessfully, following the reading of Newton's letter to Oldenburg (December 14, 1675). Text of Newton's letter. Ordered that Oldenburg write to Newton to "acquaint him with the want of success of his experiment, and desire him to send his own apparatus, with which he had made it." Then "the sequel of his hypothesis, the first part of which was read at the preceding meetings, was read to the end." Text of the remainder of the hypothesis. After reading "this discourse," Hooke said "that the main of it was contained in his *Micrographia,* which Mr. Newton had only carried farther in some particulars."

Page 270. December 30, 1675. Newton's letter to Oldenburg (December 21, 1675), "in answer to what had been written to him by Mr. Oldenburg concerning the want of success of his experiment made with a glass rubbed," read. Text of the letter. Ordered "that Mr. Newton's directions in this letter should be observed in the experiment to be made at the next meeting of the Society."

Page 271. December 30, 1675. "Mr. Oldenburg read a letter to himself from Mr. John Gascoigne" (December 15, 1675) announcing the death of Linus and stating "the resolution of Mr. Linus's disciples, to try Mr. Newton's experiment concerning light and colours more clearly and carefully . . . according to the directions given them by Mr. Newton's last letter: intimating withal, that if the said experiment be made before the Royal Society, and be attested by them to succeed, as Mr. Newton affirmed, they would rest satisfied."

Page 271. January 13, 1675/6. Newton's "experiment of glass rubbed, to cause various motions in bits of paper underneath," succeeded. Newton thanked for "the trouble of imparting . . . such full instructions for making the experiment."

Pages 272–278. January 20, 1675/6. The Society heard "the beginning of Mr. Newton's discourse, containing such observations, as conduce to further discoveries for completing his theory of light and colours, especially as to the constitution of natural bodies, on which their colours or transparency depend." Text of this part of the discourse.

Pages 278–279. Newton's observations "pleased the Society," and Oldenburg ordered "to desire Mr. Newton to permit them to be published."

A portion was then read of Newton's letter to Oldenburg (December 21, 1675), "stating the difference between his hypothesis and that of Mr. Hooke." Text of the relevant passage. The reading of "the rest of Mr. Newton's discourse" referred to the next meeting.

Page 280. January 27, 1675/6. Newton's letter (January 25, 1675/6) read, acknowledging "the favour of the Society in their kind acceptance of his late papers." A request that "the printing of his observations about colours might be suspended for a time, because he had some thoughts of writing such another set of observations for determining the manner of the production of colours by the prism: which observations, he said, ought to precede those now in the Society's possession, and would be most proper to be joined with them." Ordered that the reading of Mr. Newton's "observations about colours" be continued at the next meeting.

Pages 280–295. February 3, 1675/6. The reading of Newton's "observations on colours" continued. Text of this portion.

Newton's theory discussed, and a debate as to "whether the rays of light, which, though alike incident in the same medium, yet exhibit different colours, may not reasonably be said to owe that exhibition of different colours to the several degrees of the velocity of pulses, rather than, as Mr. Newton thought, to the several connate degrees of refrangibility in the rays themselves." Hooke's opinion that "the former of these ways was sufficient to give a good account of the diversity of colours."

Pages 296–305. February 10, 1675/6. The "last part of Mr. Newton's *observations,* wherein he considered in nine propositions, how the phænomena of thin transparent plates stand related to those of all other natural bodies;" read. Among other things, Newton showed "how the bigness of the component parts of natural bodies may be conjectured by their colours." Text of the last part of Newton's discourse.

Page 309. March 2, 1675/6. The "sun and season being likely to serve for the making of Mr. Newton's experiment called in question by Mr. Linus," it is proposed that "an apparatus might be prepared for that purpose." Hooke's statement that he had the apparatus ready "to make the experiment, when the Society should call for it."

Pages 313–314. April 27, 1676. Newton's experiment "tried before the Society, according to Mr. Newton's directions, and succeeded, as he all along had asserted it would do." The experiment described.

Page 318. June 8, 1676. A letter (May 27, 1676) from Lucas, the successor of Linus, "containing partly an account of the success of Mr. Newton's experiment there; partly some new objections against Mr. Newton's theory of light and colours." The letter ordered to be copied and a copy sent to Newton for his answer. (Letter printed in *Phil. Trans.*, No. 128, p. 692.)

Page 319. June 15, 1676. A letter of Newton's (June 13, 1676) read. "Partly" an answer to Lucas's letter (*Phil. Trans.*, No. 128, p. 698) and containing a "promise of a particular one; partly some communications of an algebraical nature for Mons. Leibnitz, who by an express letter to Mr. Oldenburg had desired them." [This letter later became an important document in the controversy between Newton and Leibniz about the discovery of the calculus.]

Page 369. January 2, 1677/8. A "common letter to be sent to all the correspondents was read, and altered; and somewhat of return for encouragement of the correspondence was ordered to be added." Of thirteen correspondents named, Newton is last in the list.

Page 512. December 4, 1679. A letter of Newton to Hooke (November 28, 1679) "produced and read," with Newton's "sentiments of Mons. Mallemont's new hypothesis of the heavens; and also suggesting an experiment, whereby to try, whether the earth moves with a diurnal motion or not, viz. by the falling of a body from a considerable hight, which, he alledged, must fall to the eastward of the perpendicular, if the earth moved." Newton's proposal "highly approved of by the Society." The experiment to be "tried as soon as could be with convenience."

Page 516. December 11, 1679. Hooke's answer to Newton's letter read, Hooke showing that the path described by a falling body "would not be a spiral line, as Mr. Newton seemed to suppose, but an excentrical elliptoid, supposing no resistance in the medium: but supposing a resistance, it would be an excentric ellipti-spiral, which, after many revolutions, would rest at last in the centre: that the fall of the heavy body would not be directly east, as Mr. Newton supposed; but to the southeast, and more to the south than the east. It was desired, that what was tryable in this experiment might be done with the first opportunity."

Page 519. December 18, 1679. Hooke's answer to Newton's "former letter" read; "as also another letter, which he had received from Mr. Newton, containing his farther thoughts and examinations of what had been propounded by Mr. Hooke." Hooke's account of "three trials of the experiment propounded by Mr. Newton," in each case of which the ball was found to "fall to the south-east of the perpendicular point, found by the same ball hanging perpendicular." Since the experiment had been made out of doors, "nothing of certainty could be concluded from it." A new trial to be made "within doors, where there would be less motion of the air."

VOLUME IV

Page 1. January 8, 1679/80. Hooke read "another letter of his to Mr. Newton concerning some farther account of his theory of circular motion and attraction; as also several observations and deductions from that theory," such as (1) "pendulum clocks must vary their velocity in several climates," (2) "this variation must also happen at different hights in the same climate," confirmed by an observation of Halley at St. Helena, (3) thus "a pendulum was unfit for an universal standard of measure."

Page 2. Hooke "desired to make his trials as soon as possible of Mr. Newton's experiment concerning the earth's diurnal motion."

Page 4. January 21, 1679/80. "Dr. Croune proposing from Mr. Collins, that the latter was ready to print two volumes of algebra, written by Dr. Wallis, Mr. Baker, Mr. Newton, &c. provided the society would engage to take off 60 copies," it was ordered that the proposal be made "in writing."

Page 30. March 25, 1680. An "account of the experiments made on the Tuesday before . . . was brought in by Mr. Hooke, and read." There had been "made a regulus of equal parts of antimony and iron." Part was "melted with equal parts of tin," which when polished "gave a strong reflection . . . We conceive it may be very useful for making speculative glasses for Mr. Newton's experiment."

Page 38. May 13, 1680. Hooke mentioned "a way of hardening an amalgama of mercury and iron by a vegetable powder, which would make it almost as hard as hardened steel. This, he conceived, would be an excellent material for making specular planes for telescopes in Mr. Newton's way."

Page 60. December 8, 1680. Ordered by the Council that "the secretary send Mr. Newton an answer to his letter, that the Society give their consent for the Italian to dedicate his book, &c." [The Italian in question was Gasparini.]

Page 61. December 16, 1680. A letter from Newton to Hooke read (Cambridge, December 3, 1680), in which an account was given "that Dominico Gasparini, doctor of physic of Lucca in Italy, had lately written a treatise of the method of administering the *Cortex Peruvianus* in fevers . . . and that upon the fame of the Royal Society spread every where abroad, he was ambitious to submit his discourse to so great and authentic a judgment as that of the Society," and hoped "the Society would give him leave to dedicate his book to them." Gasparini had requested another doctor "of his acquaintance in Italy to write to his correspondent an Italian in London" to this effect. "The said Italian being gone from London to Cambridge before the arrival of the letters, on the receit of them applied himself to Mr. Newton, who promised him, that he would desire Mr. Hooke to acquaint the Society with Dr. Gasparini's request . . . Mr. Hooke was desired to answer Mr. Newton's letter, which he did in one dated 18 Decemb. 1680, in which he took notice, that the Society was pleased with the subject of Dr. Gasparini's book." As to "Dr. Gasparini's dedication of his book to the Society, he needed no leave, things of that nature being usually done without asking a consent."

In the above-mentioned letter, Newton included "thanks to Mr. Hooke for the trials, which the latter had made of an experiment suggested" by Newton "about falling bodies."

Page 62. December 16, 1680. Trial of an experiment "for examining the electricity of glass after Mr. Newton's method, by rubbing one side of a glass to make the other attract: But it was found, that though at first it succeeded two or three times, yet afterwards, for what reason could not be discovered, it did not succeed."

Page 65. January 19, 1680/1. Reference to "undertaking of Mr. John Adams to survey all England, by measuring, taking angles, and also the latitudes of places; and in order to this running three several meridians clear through England . . . Mr. Newton of Cambridge had promised to assist him."

Page 234. November 30, 1683. Following an obituary of Mr. John Collins ("born at Wood-Eaton near Oxford, on Saturday March 5, 1624/5" and died "in London, on Saturday November 10, 1683"), it is stated that "about five and twenty years after his death, all his papers and most of his books came into the hands of Mr. William Jones, F. R. S. amongst which were found manuscripts upon mathematical subjects of Mr. Briggs, Mr. Oughtred . . . Dr. Barrow, and Mr. Isaac Newton, with a multitude of letters received from, and copies of letters sent to, many learned persons, particularly Dr. Pell, Dr. Wallis, Dr. Barrow, Mr. Newton, Mr. James Gregory, Mr. Flamstead, Mr. Thomas Baker . . . Mons. Slusius, Mons. Leibnitz . . .

"From these papers it appeared, that Mr. Collins was so sollicitous in his search after useful truths, so indefatigably industrious in prosecuting these inquiries, and of so communicative a disposition, that he held a constant correspondence for many years with all the eminent mathematicians of his time . . . It was from his papers chiefly, that the great Newton's claim to the invention of fluxions was established."

Page 347. December 10, 1684. Halley's report "that he had lately seen Mr. Newton at Cambridge, who had shewed him a curious treatise, *De Motu;* which, upon Mr. Halley's desire, was, he said, promised to be sent to the Society to be entered upon their register."

Halley was "desired to put Mr. Newton in mind of his promise for the securing his invention to himself till such time as he could be at leisure to publish it." "Mr. Paget was desired to join with Mr. Halley."

Page 370. February 25, 1684/5. A letter of Newton "to Mr. Aston, dated at Cambridge, Feb. 23, 1684/5, mentioning, that the design of a philosophical meeting there had been pushed forward by Mr. Paget, when he was last there; with whom himself had concurred, and engaged Dr. More to be of the Society; and that others were spoken to, partly by him, and partly by Mr. Charles Montagu." According to Newton, that "which chiefly dashed the business, was the want of persons willing to try experiments, he, whom we chiefly relied on, refusing to concern himself in that kind. And more what to add farther about this business, I know not, but only this, that I should be very ready to concur with any persons for promoting such a design, so far as I can do it without engaging the loss of my own time in those things.

"I thank you for entering in your register my notions about motion. I designed them for you before now; but the examining several things has taken a greater part of my time than I expected, and a great deal of it to no purpose: and now I am to go into Lincolnshire for a month or six weeks. Afterwards I intend to finish it as soon as I can conveniently."

Pages 479–480. April 28, 1686. "Dr. Vincent presented to the Society a manuscript treatise intitled, *Philosophiæ Naturalis principia mathematica,* and dedicated to the Society by Mr. Isaac Newton, wherein he gives a mathematical demonstration of the Copernican hypothesis as proposed by Kepler, and makes out all the phænomena of the celestial motions by the only supposition of a gravitation towards the center of the sun decreasing as the squares of the distances therefrom reciprocally.

"It was ordered, that a letter of thanks be written to Mr. Newton; and that the printing of his book be referred to the consideration of the council."

Page 484. May 19, 1686. Ordered that "Mr. Newton's *Philosophiæ naturalis principia mathematica* be printed forthwith in quarto in a fair letter; and that a letter be written to him to signify the Society's resolution, and to desire his opinion as to the print, volume, cuts, &c." [In a footnote, there is the text of Halley's letter to Newton, dated May 22, 1686, informing Newton that his "incomparable treatise" had been presented to the Royal Society who "were so very sensible of the great honour you have done them by your dedication, that they immediately ordered you their most hearty thanks, and that the council should be summoned to consider about the printing thereof." The Society "judging, that so excellent a work ought not to have its publication any longer delayed, resolved to print it at their own charge in a large quarto of a fair letter; and that this their resolution should be signified to you and your opinion thereon be desired, that so it might be gone about with all speed. I am intrusted to look after the printing of it, and will take care, that it shall be performed as well as possible. Only I would first have your directions in what you shall think necessary for the embelishing thereof, and particularly whether you think it not better, that the schemes should be inlarged, which is the opinion of some here: but what you signify as your desire shall be punctually observed." Remaining portion of the letter takes up Hooke's "pretensions upon the invention of the rule of decrease of gravity being reciprocally as the squares of the distances from the center. He says you had the notion from him, though he owns the demonstration of the curves generated thereby to be wholly your own. How much of this is so, you know best; as likewise what you have to do in this matter. Only Mr. Hooke seems to expect you should make some mention of him in the preface, which it is possible you may see reason to prefix."]

Page 486. June 2, 1686. Ordered "that Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it, and printing it at his own charge; which he engaged to do."

Page 491. June 30, 1686. Ordered, that "the president be desired to license Mr. Newton's book . . . dedicated to the Society."

Page 514. December 22, 1686. A letter of Wallis to Halley (Oxford, December 14, 1686) read. Wallis's letter deals with "the minutes of the Philosophical Society at Oxford." He had received "the two problems of Mr. Newton." Wallis found that Newton "hath considered the measure of the air's resistance to bodies moved in it; which is the thing I suggested in one of my late letters, and thereby saves me the labour of doing the same thing over again. For I should have proceeded upon the same principle; that the resistance (cæteris paribus) is proportional to the celerity (because in such proportion is the quantity of air to be removed in equal times) nor do I know from what more likely principle to take my measures therein. His computation from this principle I have not yet had leisure to examine; but do presume, a person of his accuracy hath not failed in his computation or reductions from it."

Page 521. January 26, 1686/7. A letter from Wallis read, "concerning the resistance of the medium to bodies projected through it, as likewise to the fall of bodies." Ordered, "that Mr. Newton be consulted, whether he designed to treat of the opposition of the medium to bodies moving in it in his treatise *De Motu Corporum* then in the press."

Page 527. March 2, 1686/7. A letter of Newton's read, "mentioning his having sent up the second book of his mathematical philosophy."

Page 528. March 9, 1686/7. A letter of Wallis to Halley (Oxford, March 4, 1686/7) read, discussing the air's resistance to the motion of projectiles and Hooke's "hypothesis of the mutability of the poles of the earth." This was the occasion for reading "a paragraph of Mr. Newton's mathematical philosophy ["Propos. 66 Cor. ult."] concerning the direction and position of the axis of a globe turning about itself, and shewing, that by the addition of some new matter on one side of a globe so turning, it shall make the axis of the globe change its position, and revolve about the point of the surface, where the new matter is added. It was thought, that the same translation of the axis might be occasioned in the globe of the earth by the blowing up of mountains by subterraneous fire."

Page 529. April 6, 1687. The "third book of Mr. Newton's treatise *De Systemate Mundi* was produced and presented to the Society. It contained the whole system of celestial motions, as well of the secondary as primary planets, with the theory of comets; which he illustrates by the example of the great comet of 1680/1, proving that, which appeared in the morning in the month of Nov. preceding, to have been the same comet, that was observed in Dec. and Jan. in the evening."

Bibliographical Notes

ROBERT E. SCHOFIELD

The Newton material appearing in this volume is reproduced in facsimile from the texts as they originally appeared, preserving the pagination, spelling, and general format. In a few instances, at the beginning or end of an article, material at the top or bottom of a page has been blanked out, being the work of another person and unrelated to the text reproduced.

There are three general bibliographies of Newton material: a short one by H. Zeitlinger in the Newton bicentenary volume, *Isaac Newton* (London: G. Bell and Sons, Ltd., 1927), edited for the Mathematical Association by W. J. Greenstreet; George J. Gray's *A Bibliography of the Works of Sir Isaac Newton* (first edition, Cambridge: MacMillan and Bowes, 1888; second edition, Cambridge: Bowes and Bowes, 1907); and *A Descriptive Catalogue of the Grace K. Babson Collection of the Works of Sir Isaac Newton* (New York: Herbert Reichner, 1950). None of these is complete and in all of them the listing of Newton's papers in the *Philosophical Transactions* is more or less inadequate. We believe that the table of contents of this volume contains a complete list of all of Newton's papers in the *Philosophical Transactions* and the related letters, except the writings on mathematics and those on biblical chronology.[1]

The optical papers from the *Philosophical Transactions* are reproduced from the copies owned by the Burndy Library. All other papers from the *Philosophical Transactions*—namely, II, 17: "An Instrument for observing the Moon's Distance from the fixed Stars at Sea"; III, 4: "Scala Graduum Caloris"; and V, 2 and 3: Halley's review of the *Principia* and the "True theory of the tides," are reproduced from the numbers of the *Philosophical Transactions* in the Harvard College Library. The citations to the *Philosophical Transactions* (in the table of contents) are by number rather than volume, as this seemed the only reasonably satisfactory way of identifying the original sources without confusion. Although present custom dictates reference by volume, the erratic publication of the early issues of the *Transactions* is inimical to the consistent assigning of volume numbers, while the issue numbers offer a consistent continuous pattern.

The English translations from the Latin originals are reproduced from the *Philosophical Transactions, Abridged* (London, 1809).

Two of the documents (II, 9: Hooke's critique of Newton's theory of light and colors, and II, 16: Newton's second paper on color and light) were read at meetings of the Royal Society, but never printed in the *Philosophical Transactions*. The Hooke critique is discussed, in a somewhat misleading way, in document II, 9: "Mr. Isaac Newtons Answer to some Considerations upon his Doctrine of Light and Colors." The "second paper on color and light" was originally withheld from publication at Newton's request. Much of the information contained therein appeared publicly for the first time in Newton's *Opticks* (1704), but we may assume that some of it was in the air from the time of its presentation at the meetings of the Royal Society in 1675–76. The reproduction of these papers, taken from the Burndy Library copy of the first edition[2] of Birch's *History of the Royal Society of London* (London, 1756–57 [see facsimile of title page on page 478, below]), provides a more complete opportunity to follow the course of Newton's thinking, leading to the *Opticks*, than is generally available.

[1] *A Supplement to the Catalogue of the Grace K. Babson Collection of the Works of Sir Isaac Newton* was published by Babson Institute in 1955. Gray's *Bibliography* has been reprinted in facsimile by Dawson of Pall Mall (London).

[2] Birch's *History* has been reprinted in facsimile in 1968 by Editions "Culture et Civilisation" (Brussels), and by Johnson Reprint Corporation (New York and London, 1968) with an introduction, analyses, and supplementary entries by A. Rupert Hall.

Newton's letter to Boyle (III, 2) first appeared in the introduction to Birch, *The Works of the Honourable Robert Boyle* (London: A. Millar, 1744). Our reproduction is taken from the Harvard College Library copy [see facsimile of the title page on page 249 below] (not in Babson, Gray, or Greenstreet).

"De Natura Acidorum" first appeared in both a Latin and an English version in the introduction to the second volume, first edition, of John Harris's *Lexicon Technicum.* The first volume of this edition appeared in 1704, the second volume not until 1710. The *Lexicon Technicum* was a general "dictionary" of arts and sciences, justly famous in its day, and has been called the prototype of the numerous "dictionaries" of the sciences that were published in the 18th century. It went through at least five editions (the fifth printed for T. Walthoe, etc., in 1736, with a supplement by a Society of Gentlemen, London, 1744); all editions subsequent to the first edition of the second volume in 1710 contain "De Natura Acidorum." Our reproduction [the title page of volume II is reproduced on page 255 below] is from the copy of the 1710 (volume II) first edition, in the Harvard College Library (not in Gray or Babson; mentioned briefly by Greenstreet).

The *Four Letters from Sir Isaac Newton to Doctor Bentley* . . . first appeared in a pamphlet printed in 1756. Our reproduction is from the Harvard College Library copy [title page, page 279 below] (Babson 226, Gray 345, not in Greenstreet).

The sermons of Richard Bentley are reproduced from a collection, in one volume, of the eight sermons preached by Bentley in 1692 as the Boyle Lectures. According to Rev. Alexander Dyce, editor of *The Works of Richard Bentley, D. D.,* (London: Francis MacPherson, 1838, vol. 3, pp. v and vi) each sermon was originally published independently, the first six in 1692, the seventh and eighth in 1693, each with its own title page, imprimatur (that of the seventh and eighth is signed Ra. Barker), and separate pagination. In 1693, a general title page was prefixed to them reading: *The Folly and Unreasonableness of Atheism Demonstrated from the Advantage and Pleasure of a Religious Life, the Faculties of Human Souls, the Structure of Animate Bodies, & the Origin and Frame of the World* . . . London, printed by J. H. for H. Mortlock . . . 1693 (Babson 40; not in Gray or Greenstreet).

The Elogium of Sir Isaac Newton (London: J. Tonson, 1728) is reproduced from the copy in the Yale Medical School Library, loaned by Dr. John F. Fulton. Mr. A. N. L. Munby, Fellow and Librarian of King's College, Cambridge, describes the Tonson *Elogium* as probably the "official" translation. In addition to the 1728 Tonson printing of the

Elogium (Babson 270, Gray 388), there were published several other English translations (for example, Babson 271 and Gray 389, 390). One of the most interesting of these is *An account of the life and writings of S^r^. Isaac Newton. Trans. from the Eloge of M. Fontenelle. . . .* The Second Edition. 8°. London, T. Warner, 1728 (not in Babson; perhaps this is Gray 390); the Harvard College Library contains a copy of the T. Warner "second edition," dated 1727. There is some question whether the date 1727 is a typographical error or whether possibly this is a printing made during the months of January to March, a period during which dates could be given as 1727, 1727/28, or 1728, depending upon feelings toward the old or the new style of dating since the official acceptance of "new style" dating did not occur in England until 1751/52. There is also a question about the designation "Second Edition." The Harvard 1727 Warner second edition is the earliest translation that we have found, but we have encountered no reference to a Warner first edition. Unfortunately the Harvard 1727 copy is imperfect, lacking a first leaf which is presumably the half title. Mr. Munby has kindly sent us a copy of the following advertisement which appears on the verso of the half title of a Warner, 1728, Second Edition, in the Trinity College, Cambridge, Library:

> The first Edition of this Translation was printed in Quarto, in order to be bound up with Sir Isaac Newton's Chronology: But for the Benefit of those who cannot afford to purchase a Book of so high a Price, it was thought necessary to publish this edition . . .

The Harvard College Library copy of John Conduitt's edition of *Newton's Chronology of Ancient Kingdoms Amended* . . . printed in quarto for J. Tonson, J. Osborn and T. Longman, London, 1728 does not include the translation of Fontenelle's *éloge,* nor do the descriptions of Babson 214 or Gray 309 indicate its presence. However, a copy in the possession of the London booksellers, Wm. Dawson & Sons Ltd, in 1954 was bound in with a quarto 1728 Paris edition of the *éloge* in French and a quarto 1728 London *Elogium* which appear to have been the same as the Tonson et al. 1728 quarto which we reproduce here. It is possible, then, that our reproduction is from the first edition mentioned by Warner and also published separately for the same reasons that Warner issued his edition.

Note on the Printing of Bentley's Sermons

William B. Todd

Each of Bentley's last two discourses against atheism is here reproduced from a previously undifferentiated first edition represented at the Yale University Library (Mpd50.B69.1692). Both of these copies, together with the six earlier sermons, were separately issued, much thumbed by the original owner, bound in a single volume, and eventually rebound in modern library buckram.

At Harvard, the copies of these two sermons (*EC65.B4465.B693f) represent a later edition, not hitherto recognized. These were issued under a general title as part of a collected set and shortly thereafter included with other tracts in an early 18th-century binding. As the collected set was first advertised in the Term Catalogue for Easter, 1693 (Arber II.449), it doubtless comprised, upon issue, the original edition of the final sermon, bearing an imprimatur dated 30 May 1693. The Harvard set, however, though still exhibiting a general title dated 1693, appears to be of a later issue, since it incorporates, among the eight sermons, three reprints dated 1694. One of these is properly called a "Second" and the two others a "Third Edition."

Of these reprints the Second Edition, part 1 of the last three sermons (No. VI), is especially significant, for it indicates that the two other parts are of a date somewhat later than that assigned by the printer. Like the corresponding piece in the Yale series, this sermon, in title, imprimatur, and scriptural text heading page 1, is composed of type retained for the most part in the other two tracts. The settings both old and new provide the various points listed in the accompanying table, all of which demonstrate not only successive presswork in each series but, for the latter, a printing of the two "1693" sermons some time in 1694.

			First Edition		Second Edition	
	Line	*Reading*	*Part*	*Variant*	*Part*	*Variant*
Title	7	SERMON	1	S intact	1	S correctly imposed
			2–3	S broken	2–3	S reversed
	11	Being	1–3	B broken	1–3	B intact
Edition			1–3	—	1	Second Edition
					2–3	—
Imprint		*LONDON*	1–3	swash italic	1–3	straight italic
			1	for *Henry Mortlock* ... 1692.	1	by *J. H.* for *Henry Mortlock* ... 1694.
			2–3	for *H. Mortlock* ... 1693.	2	by *J. H.* for *Henry Mortlock* ... 1693.
					3	for *H Mortlock* ... 1693.
Imprimatur	3	D^{no} D^{no}	1–3	1st D broken	1–3	D intact
	3	Archiep.	1–3	A intact	1	A intact
					2–3	A broken
	5	[place]	1	*LAMBETH*	1–3	*LAMBETH*
			2–3	*LAMBHITH*		
Text		[italic heading]	1–3	9 lines, first ends *unto the*	1–3	7 lines, first ends *the living*
					1	1st setting, double *s* ligatured, lines 5, 7.
					2–3	2d setting, double *s* separate.

Supplement

This Supplement intends to call attention to certain new information or points of view that have emerged since the first edition, and to selected scholarly publications that bear on topics of the several introductions to the sections of this work. There are two general guides to the literature concerning Newton. The first is Clelia Pighetti's "Cinquant'anni di studi newtoniani (1908–1959)," *Rivista critica di storia della filosofia 2–3,* 181–203, 295–318 (1960), which lists all editions of Newton's works and studies on Newton in alphabetical sequences by author, year-by-year. The second is the classified bibliography in I. B. Cohen, "Newton, Isaac," in the *Dictionary of Scientific Biography,* ed. by Charles Coulston Gillispie (New York: Charles Scribner's Sons, 1974), vol. 10, pp. 93–103, including a presentation of the "Soviet Literature on Newton" by A. P. Youschkevitch; an expanded and revised version of this biography and bibliography is scheduled for publication by Charles Scribner's Sons in 1977. These should be supplemented by Magda Whitrow, ed., *Isis Cumulative Bibliography* (London: Mansell, 1971), vol. 2, pp. 221–232.

There are a number of review-articles concerning Newton, of which some major ones are: I. B. Cohen, "Newton in the Light of Recent Scholarship," *Isis 51,* 489–514 (1960); D. T. Whiteside, "The Expanding World of Newtonian Research," *History of Science 1,* 16–29 (1962); J. E. McGuire, "Newton and the Demonic Furies: Some Current Problems and Approaches in the History of Science," *History of Science 11,* 21–48 (1973); and Richard S. Westfall, "The Changing World of the Newtonian Industry," *Journal of the History of Ideas 37,* 175–184 (1976).

II Newton's Optical Papers

Newton's correspondence about light and colors and his new reflecting telescope has been reproduced from the originals or from drafts or fair copies in vol. 1 of *The Correspondence of Isaac Newton,* ed. H. W. Turnbull (Cambridge: at the University Press, 1959). Some differences between these versions and the ones printed in the present volume are discussed above, in the editor's General Introduction, and in the Notes on the Texts, below.

Since 1958, when the first edition of the present book was published, a large number of studies have appeared dealing with various aspects of Newton's work in optics. On the ways in which Newton may have made his discoveries (rather than the mode of discovery presented by Newton in the first letter, 6 February 1671/72), especially the possible role of the "disproportion" of the length of the spectrum to its breadth (mentioned on our page 48 above), see J. A. Lohne's discussion of the problems of accepting Newton's proposed historical narrative, in his "Isaac Newton: The Rise of a Scientist 1661–1671," *Notes and Records of the Royal Society of London 20,* 125–139 (1965), and "Experimentum crucis," *idem 23,* 169–199 (1968). See also A. I. Sabra, *Theories of Light from Descartes to Newton* (London: Oldbourne, 1967), chaps. 9–12, esp., p. 246. On this subject, see also R. S. Westfall, "The Development of Newton's Theory of Color," *Isis 53,* 339–358 (1962). In these publications, there is developed *inter alia* the role of Newton's concept of "globules" of light, those of any given color having a different speed from those of any other color, which is discussed by T. S. Kuhn on p. 43 of the present book. Lohne, in "Experimentum crucis," has shown the many difficulties with respect to Newton's actual experiments; see, further, J. A. Lohne and Bernhard Sticker, *Newtons Theorie der*

Prismenfarben, mit Übersetzung und Erläuterung der Abhandlung von 1672 (Munich: Werner Fritsch ["Neue Münchner Beiträge zur Geschichte der Medizin und Naturwissenschaften"], 1969).

Concerning Newton's views on the possibility or impossibility of making lens combinations with corrections for chromatic aberration, see vol. 3 of D. T. Whiteside's edition of *Newton's Mathematical Papers* (Cambridge: at the University Press, 1969), pp. 442–443, 512–513 (n. 61), 553 (n. 13), and 554–555 (nn. 5–6), and also Zev Bechler's detailed study, "'A Less Agreeable Matter': The Disagreeable Case of Newton and Achromatic Refraction," *British Journal for the History of Science 8*, 101–126 (1975).

Whiteside has also written an introduction to *The Unpublished First Version of Isaac Newton's Cambridge Lectures on Optics 1670–1671* (Cambridge: The University Library, 1973), a facsimile of Newton's autograph MS of his *Lectiones Opticæ;* a critical edition and translation of these lectures (based on the two versions) has been announced by Alan Shapiro. For an account of this early version, see further, R. S. Westfall, "Newton's Reply to Hooke and the Theory of Colors," *Isis 54*, 82–96 (esp. 83–84, 95–96) (1963). For a discussion of the texts deposited by Newton in the University Library as his professorial lectures, see I. B. Cohen, *Introduction to Newton's 'Principia'* (Cambridge, Mass.: Harvard University Press; Cambridge: at the University Press, 1971), Suppl. III.

Concerning an abortive attempt in Newton's lifetime to produce an edition of his letters on light and color, see I. B. Cohen, "Versions of Isaac Newton's First Published Paper, With Remarks on . . . an Edition of his Early Papers on Light and Color," *Archives internationales d'histoire des sciences 11*, 357–375 (1958); D. J. De Solla Price, "Newton in a Church Tower: The Discovery of an Unknown Book by Isaac Newton," *Yale University Library Gazette 34*, 124–126 (1960); A. Rupert Hall, "Newton's First Book," *Archives internationales d'histoire des sciences 13*, 39–61 (1960). Newton's notes on his own letter to Oldenburg, containing his new theory of light and colors, are printed below in the section of Notes on the Texts of Newton's *Papers & Letters,* in the text following footnote 13.

On Newton's work on color, see George Biernson, "Why did Newton see Indigo in the Spectrum?," *American Journal of Physics 40*, 526–533 (1972), and Torger Holtzmark, "Newton's *Experimentum* *crucis* Reconsidered," *idem, 28,* 1229–1235 (1970). Newton, early in his scientific career, began to write a *Fundamentum Opticæ,* the text of which has been shown to be reconstructible from Newton's MSS; see vol. 3 of Whiteside's edition of *Newton's Mathematical Papers,* p. 552.

Some other relevant articles are: Zev Bechler: "Newton's 1672 Optical Controversies: A Study in the Grammar of Scientific Dissent," in Y. Elkana, ed., *The Interaction Between Science and Philosophy* (Atlantic Highlands, N.J.: Humanities Press, 1974) pp. 115–142, and his "Newton's Search for a Mechanistic Model of Colour Dispersion: A Suggested Interpretation," *Archive for History of Exact Sciences 11*, 1–37 (1973); I. B. Cohen, "I prismi del Newtone e i prismi dell' Algarotti," *Atti della Fondazione "Giorgio Ronchi"* (Florence) *12*, 1–11 (1957); J. A. Lohne, "Newton's 'Proof' of the Sine Law," *Archive for History of Exact Sciences 1*, 389–405 (1961); R. S. Westfall, "Newton and his Critics on the Nature of Colors," *Archives internationales d'histoire des sciences 15*, 47–58 (1962), and his "Isaac Newton's Coloured Circles Twixt Two Contiguous Glasses," *Archive for History of Exact Sciences 2*, 181–196 (1965).

III Newton's Chemical Papers

Since the pioneering study made by A. R. and M. B. Hall of "Newton's Chemical Experiments" (in *Archives internationales d'histoire des sciences 11*, 113–152 (1958), others have been examining and publishing extracts from Newton's chemical notebooks and from the large corpus of his alchemical notes. On the topic of alchemy see P. M. Rattansi, "Newton's Alchemical Studies," pp. 167–182 of Allen G. Debus, ed., *Science, Medicine, and Society in the Renaissance,* vol. 1 (New York: Science History Publications, 1972); R. S. Westfall, "The Role of Alchemy in Newton's Career," pp. 189–232 of M. L. Righini Bonelli and William R. Shea, eds., *Reason, Experiment and Mysticism in the Scientific Revolution* (New York: Science History Publications, 1975), together with commentaries by Paolo Casini (pp. 233–238) and especially by Marie Boas Hall (pp. 239–246); and Betty Jo Teeter Dobbs, *The Foundations of Newton's Alchemy* (Cambridge: at the University Press, 1975).

See also A. R. and M. B. Hall, "Newton's Mechanical Principles," *Journal of the History of Ideas 20*, 167–178 (1959); "Newton's

Theory of Matter," *Isis 51*, 131–144 (1960); "Newton and the Theory of Matter," pp. 54–68 of Robert Palter, ed., *The Annus Mirabilis of Sir Isaac Newton 1666–1966* (Cambridge, London: The M.I.T. Press, 1970). Also A. R. Hall and M. B. Hall, eds., *Unpublished Scientific Papers of Isaac Newton* (Cambridge: at the University Press, 1962), pt. III, "Theory of Matter."

Some further related studies are Robert Kargon, *Atomism in England from Hariot to Newton* (Oxford: at the Clarendon Press, 1966); Alexandre Koyré, "Les Queries de l'Optique," *Archives internationales d'histoire des sciences 13,* 15–29 (1960); J. E. McGuire, "Body and Void and Newton's *De Mundi Systemate:* Some New Sources," *Archive for History of Exact Sciences 3,* 206–248 (1966), and his "Transmutation and Immutability," *Ambix 14*, 69–95 (1967).

IV Bentley and Newton and the Boyle Lectures

Newton's letters to Bentley concerning the Boyle Lectures have been published in vol. 3 of *The Correspondence of Isaac Newton* (Cambridge, at the University Press, 1961), pp. 233–241, 244–245, 253–256 (letters 398, 399, 404, 406), along with one letter from Bentley to Newton (letter 405, pp. 246–253).

On Newton's possible influence in the choice of Bentley as inaugural Boyle Lecturer, see Henry Guerlac and M. C. Jacob, "Bentley, Newton, and Providence (The Boyle Lectures Once More)," *Journal of the History of Ideas 30*, 307–318 (1969), where it is suggested (p. 318) that Newton "encouraged—if he did not suggest—the theme of Bentley's last sermons." See, also Margaret C. Jacob, "Early Newtonianism," *History of Science 12*, 142–146 (1974), and her exploration of the intellectual and political aspects of the early Boyle Lectures in relation to the acceptance of the Newtonian philosophy, in her book, *The Newtonians and the English Revolution* (Ithaca: Cornell University Press, 1976).

A new biography of Bentley has been written by R. J. White, *Dr. Bentley. A Study in Academic Scarlet* (London: Eyre & Spottiswoode, 1965). White repeats the claim of an earlier biographer that "there seems to be little doubt that he [Bentley] attended Newton's lectures as Lucasian Professor of Mathematics while at Cambridge" (p. 47) and even refers to Bentley as "Newton's pupil" (p. 71). There is no evidence to support such claims, all the more doubtful since

Newton was lecturing on arithmetic and algebra while Bentley was a student, and not on general physics or natural philosophy, or even cosmology. See, on this topic, "Richard Bentley and the 'Principia'" in ch. VIII, §6 of I. B. Cohen, *Introduction to Newton's 'Principia'* (Cambridge, Mass.: Harvard University Press; Cambridge: at the University Press, 1971).

V Halley and the *Principia*

Halley's correspondence with Newton in relation to the composition and publication of the *Principia* has been published in vol. 2 of *The Correspondence of Isaac Newton,* ed. by H. W. Turnbull (Cambridge: at the University Press, 1960). For Halley's relations with Newton during those years see I. B. Cohen, *Introduction to Newton's 'Principia'* (Cambridge, Mass.: Harvard University Press; Cambridge: at the University Press, 1971), esp. §§1, 2, and 7 of ch. III, §§1 and 8 of ch. IV, §§1, 2, and 3 of ch. V, and §3 of ch. VI; in §8 of ch. IV (pp. 122–124) and in Suppl. VII (pp. 336–344), attention is called to an extensive critique—presumably by Halley—of an early MS version of portions of the *Principia.*

VI Fontenelle and Newton

On Fontenelle, see the whole issue of *Revue d'histoire des sciences* devoted to him (vol. 10, no. 4, Oct.-Dec. 1957), containing articles by Suzanne Delorme, Douglas McKie, Geneviève Martin, Arthur Birembaut, and François Grégoire, including a valuable "Contribution à la bibliographie de Fontenelle" by S. Delorme (pp. 300–309). Articles by S. Delorme, A. Adam, A. Couder, J. Rostand, and A. Robinet appear in "Fontenelle, sa vie et son oeuvre, 1657–1757 (Journées Fontenelle)," *Revue de synthèse 82*, 1–91 (1961); also J. Vendryès, G. Canguilhem, A. Dupont-Sommer, R. Pintard, A. Adam, and A. Maurois in *Annales de l'Université de Paris* 27^{e} année, 378–415 (1957); and the *Catalogue de l'Exposition Fontenelle à la Bibliothèque Nationale* (Paris: Bibliothèque Nationale, 1957). See, further, Leonard M. Marsak, "Cartesianism in Fontenelle and French Science 1686–1752," *Isis 50*, 51–60 (1959), "Bernard de Fontenelle: In Defense of Science," *Journal of the History of Ideas 20*, 111–122 (1959), "Bernard de Fontenelle: The Idea of Science in the French Enlightenment," *Transactions of the American Philosophical*

Society 49, pt. 7, 1–64 (plus bibliography) (1959). Marsak has also edited a volume of selections from Fontenelle in English, under the title, *The Achievement of Bernard le Bovier de Fontenelle* (New York and London: Johnson Reprint Corporation, 1970); Fontenelle's *éloge* of Newton is not, however, included in this volume of selections. An excellent edition of *Textes choisis* (*1683–1702*) (Paris: Editions Sociales, 1967) has been edited by Maurice Roelens. A summary of Fontenelle's life in science, with a general bibliography, is given by Suzanne Delorme in the *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons, 1972), vol. 5, pp. 57–63.

To the list of biographies of Newton (pp. 435–436, nn. 24, 29), there should now be added E. N. da C. Andrade, *Sir Isaac Newton, his Life and Work* (Garden City, N.Y.: Doubleday & Company, Anchor Books, 1954), Frank E. Manuel, *A Portrait of Isaac Newton* (Cambridge, Mass.: Harvard University Press, 1968), and I. B. Cohen: "Newton, Isaac," *Dictionary of Scientific Biography,* ed. by Charles Coulston Gillispie (New York: Charles Scribner's Sons, 1974), vol. 10, pp. 42–103, scheduled to be published by Charles Scribner's Sons in 1977 in an expanded and revised version. R. S. Westfall is in the process of writing a full-length biography of Newton.

On Newton's election as *associé étranger* of the Paris *Académie des Sciences,* see I. B. Cohen: "Isaac Newton, Hans Sloane and the Académie Royale des Sciences," pp. 61–116 of *L'aventure de la science: Mélanges Alexandre Koyré,* vol. 1 (Paris: Hermann, 1964).

Appendix

The four volumes of Birch's *History of the Royal Society* have been reprinted in facsimile in 1968 by the Editions "Culture et Civilisation" of Brussels, and by Johnson Reprint Corporation of New York and London (ed. by A. R. Hall).

Notes on the Texts of Newton's *Papers & Letters*

Many of the differences between the versions of Newton's communications in the 17th century in the *Philosophical Transactions* and the manuscript documents appear to represent the editorial judgments of Henry Oldenburg, then Secretary of the Royal Society.[1] Wholly apart from excising the beginnings and ends of letters, it may be observed that he made other omissions which seem arbitrary. Thus between the first two paragraphs on our page 145 above, the following three short paragraphs were omitted:

Meane time since M. Hugens seems to allow that white is a composition of two colours at least if not of more; give me leave to rejoyn these Quæres.

1. Whether the whiteness of the suns light be compounded of the like colours?

[1] The differences between the versions printed in this edition and those in the *Correspondence* (edited from MS texts) are listed below.

On Oldenburg, we are fortunate to have the edition by A. Rupert Hall and Marie Boas Hall, *The Correspondence of Henry Oldenburg,* vol. 1 (Madison: University of Wisconsin Press, 1965) and later volumes: beginning with vol. 10 (1975), for the years 1673–1674, the publisher of this series is Mansell of London.

2. Whether the colours that emerg by refracting the light be those component colours separated by the different refrangibility of the rays in which they inhere?[2]

A philosophically or methodologically interesting omission of real significance occurs in Newton's letter to Oldenburg of 6 February 1671/2, in the second paragraph on our page 53 above. Following the first sentence (ending with a colon), which concludes "wherein the *Origin of Colours* is unfolded," and before the next sentence, beginning "Concerning which I shall lay down the *Doctrine* first, and then . . . give you an instance or two of the *Experiments* as a specimen of the rest," Newton's manuscript letter (as published in the *Correspondence*) contains the following:[3]

> . . . wherein the *Origin of Colours* is infolded. A naturalist would scearce expect to see the science of those become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick) but evinced by the mediation of experiments concluding directly & without any suspicion of doubt. To continue the historicall narration of these experiments would make a discourse too tedious & confused, & therefore I shall rather lay down the *Doctrine* first, and then, for its examination, give you an instance or two of the *Experiments,* as a specimen of the rest.[4]

In another philosophical discussion, Newton said that the science of colors was mathematical and as certain as any other part of optics (for example, geometric optics), but he later felt the need to clarify his statement. The clarification was omitted from the publication in the *Philosophical Transactions* but has been printed in Horsley's edition of Newton's *Opera* (vol. 4, p. 342) and in the more recent *Correspondence* (vol. 1, p. 187):

> In the last place I should take notice of a casuall expression which intimates a greater certainty in these things then I ever promised, viz. The

[2] Newton to Oldenburg, 3 April 1673, *The Correspondence of Isaac Newton,* vol. 1, ed. by H. W. Turnbull (Cambridge: at the University Press, 1959) p. 266.

[3] The omitted section, like the one printed below, eliminated Newton's considerations concerning mathematics and optics; see Z. Bechler, "Newton's 1672 Optical Controversies" (1974, cited in the Supplement), pp. 117ff.

[4] *Correspondence,* vol. 1, pp. 96–97.

certainty of *Mathematicall Demonstrations.* I said indeed that the *Science of Colours was Mathematicall & as certain as any other part of Optiques;* but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by which I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall:* Whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science. And if those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in which severall colours are originally inherent; I suppose the *Science of Colours* will be granted *Mathematicall* & as certain as any part of *Optiques.* And that this may be done I have good reason to believe, because ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.

Despite these omissions, Newton's letters—in the versions published in the *Philosophical Transactions* and reprinted below—abound in philosophical and methodological discussions. One of the most renowned of these occurs in Newton's letter of 11 June 1672 to Oldenburg, in reply to the second communication from Pardies. Here Newton states his opinion concerning the proper place of hypotheses in scientific investigations. In translation (as on our page 106 above) this reads:

For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments.

For some unaccountable reason, Florian Cajori stated unequivocally that, "This part of the letter was not printed in the *Philosophical Transactions.*"[5]

In Newton's letter to Oldenburg of 13 November 1675 (our page 154 above), two final paragraphs were crossed out, along with the

[5] See footnote 6 on page 676 of his revision of Andrew Motte's translation of the *Principia* (Berkeley and Los Angeles: University of California Press, 1934, sixth printing [first paper-bound edition] 1966).

signature and salutation, before Oldenburg sent the letter to the printer to be published in the *Philosophical Transactions.* The second of these paragraphs reads:

> I had some thoughts of writing a further discours about colours 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 . . . I have one discourse by me of that subject written when I sent my first letters to you about colours & of which I then gave you notice. This you may command when you think it will be convenient if the custome of reading weekly discourses still continue.[6]

This "one discourse by me" refers to Newton's "Hypothesis explaining the Properties of Light discoursed of in my several Papers," sent to Oldenburg on 7 December 1675, with an accompanying letter, both of which were read at the Royal Society, the "discourse" requiring several meetings for the full reading (these are printed on our pages 177 ff.). Newton apparently did write "a further discours about colours," for which see our pages 202 ff. above, which may have been what Newton had in mind.[7] As is observed in the long note in vol. 1 of *The Correspondence of Isaac Newton* (pages 390–392), the three parts of this discourse were published again without very many changes in Parts 1, 2, and 3 of Book 2 of the *Opticks;* and they were also printed in Birch's *History of the Royal Society,* from which our text is reproduced.

Certain of the changes made on publishing this document in the *Opticks* are given in the same editorial note. In the manuscript sent by Newton to the Royal Society, and still preserved (as is Newton's personal manuscript copy), there are deleted phrases and paragraphs. One of these refers to Newton's primary discovery that "whiteness is a dissimilar mixture of all colours, and that light is a mixture of rays endowed with all those colours." This conclusion had been the outstanding feature of his first communication to the Royal Society (see our pages 47–59 above); most of the letters in Part 2 of our volume are devoted to discussions arising from the publication of this discovery of Newton's. And so it is especially interesting to read a passage that was omitted from the published

[6] *Correspondence,* vol. 1, p. 358.

[7] A critical edition of Newton's *Opticks,* together with related MSS, is greatly needed.

version of Newton's observations: in the fourth paragraph of our p. 224 above, in the third line, between the sentence ending ". . . those colours," and the one beginning "For, considering . . .":

This I believe hath seemed the most Paradoxicall of all my assertions, & met with the most universall & obstinate Prejudice. But to me it appeares as infallibly true & certaine, as it can seem extravagant to others. For hitherto I never tryed any way to mix all colours by which I could not in some degree or other produce whitenesse, & yet I have made as many tryalls as I could excogitate ways of mixing colours; of which I may take occasion to discourse hereafter.[8]

Again, at the end of our first paragraph on our page 225, the final sentence is somewhat longer. In our version, this sentence merely reads:

And, in this respect it is, that the science of colours becomes a speculation more proper for mathematicians than naturalists.

In the MS, this sentence reads in full as follows:

And in this respect it is, that the science of colours becomes a speculation more proper for mathematicians then naturalists and deserves rather to be esteemed Mathematicall then Physicall, as I told you in my former letter & may hereafter explain more fully.

As is observed in the edition of Newton's *Correspondence,* vol.1, p. 389, this important statement was somewhat revised in the *Opticks* to read as follows:

And in this respect the Science of Colours becomes a Speculation as truly mathematical as any other part of Opticks. I mean, so far as they depend on the Nature of Light, and are not produced or alter'd by the Power of Imagination, or by striking or pressing the eye.

Finally, the last paragraph of this composition of Newton's, to be found on our page 235 above, has, in the MS, some additional sentences which read as follows:

If you now ask how rays are reflected without impinging on the parts of a body, & how those which impinge on its parts may be stopped & stifled, it requires an Hypothesis to explain it by, the description of which is besides

[8] This occurs in the fourth paragraph of our page 224 above, in the third line: following the sentence that ends ". . . those colours." See *Correspondence,* vol. 1, p. 385. This was apparently deleted by Newton before the MS was sent on to the Royal Society.

my designe. And so the manner how severall rays are unequally refrangible & reflexible & originally indued with severall colours remaines to be explained Hypothetically: But I shall content my self with haveing shewn that *de facto* the rays of light are indued with those properties.

But in Newton's own copy, which he retained for his own records, he "modified the wording."[9] In the final sentence, the phrase "severall rays . . . originally indued with several colours" reads "severall rays . . . originally indued with powers of striking the sense with several colours." The final sentence "But I shall content myself . . ." reads quite different in Newton's own altered MS version:

> And the inventing of such an Hypothesis is no part of my designe. I undertook only to discover the properties of light so far as I could derive them from experiments; & therefore content my self with having shewn those properties.

Some minor differences between the texts published in the current edition of Newton's *Correspondence* (and based on MS copies or originals) and those in *Philosophical Transactions* (and reproduced in our volume) may come from the fact that a MS represents an early draft, which Newton kept for record purposes, when he sent a second and final version to Oldenburg. This could account for the variation in phrasing in the first paragraph of Newton's letter of 26 March 1672 (printed on our page 68 above). But other differences are plainly editorial alterations by Oldenburg, who also left out certain paragraphs—whether with or without Newton's consent we do not know. In the correspondence as published in the *Philosophical Transactions,* most of the references to Hooke are changed, so that they become "the Considerer" and "the Objector" and "the Animadversor" (pages 116 ff. above). In Newton's reply to Pardies of 13 April 1672 (pages 83–85 above), Newton's concluding reference to Hooke (bottom of page 85) was not only changed to "N. N." but his courteous and complimentary "Celeberrimi nostri Hookij . . ." ("Our most famous [or well known] colleague [or countryman] Hooke . . .") was reduced to a mere "Domini N. N." or "Mr. N. N."[10] In the same manner, Huygens became disguised as "Monsieur *N.*"

[9] *Correspondence,* vol. 1, p. 389.
[10] See note 1 above.

One very amusing alteration by Oldenburg occurs in Newton's Latin letter of 13 April 1672, replying to some comments by Pardies. Newton would appear to have said (in the *Philosophical Transactions*), "Sed hallucinatus est R. P."—which was tactfully rendered into English (on our page 90) as, "But the Rev. Father is under a mistake." It would have been more accurate to have this read that "the Rev. Father's mind is wandering," or that he is "dreaming" or "talking idly" or "prating." But, in fact, the letter that Newton actually sent to Oldenburg contains these words, "Sed extra oleas evagatus est R. P."[11]—which may be translated as, "But the Rev. Father has been straying beyond the olives." In this case, there is no doubt as to who made the editorial change, since Newton's first phrase is found in the letter that he sent to Oldenburg, of which the original is still in the Library of the Royal Society of London. The very phrase itself is not without interest, since it proves to be a reference to the grove of olives at the end or at the edge of a race-course; Newton's expression comes from the *Frogs* of Aristophanes.[12] From the catalogue of Newton's Library, we may learn that Newton owned a volume in 8^{vo} of the comedies of Aristophanes, in an edition in Greek and in Latin prepared by Joseph Scaliger (1624).[13]

In the version of Newton's letter on light and colors found in the sheets of an abortive edition (see the articles by I. B. Cohen, D. J. de Solla Price, and A. R. Hall mentioned in the Supplement (II)), three footnotes have been added by Newton to one of the concluding paragraphs:

> These things being so, it can no longer be disputed, that there are colours in the dark, nor that they are the qualities of the

[11] *Correspondence,* vol. 1, p. 140.

[12] The context is, "Take care lest your anger carry you beyond the olives."

[13] See Richard de Villamil, *Newton, the Man* (London: Gordon D. Knox, [1931], p. 65, where it appears that Newton's library contained "Aristophanis Comædiæ Gr. Lat. Scaliger, 8vo. 1624." De Villamil's book is available in a reprint made by Johnson Reprint Corporation, New York and London, 1972, with a new introduction by I. B. Cohen.

John Harrison, of the university Library (Cambridge) is currently preparing a catalogue of Newton's library, in which he proposes to give present locations of as many books as possible, and indications of any markings or annotations.

objects we see, no nor perhaps, whether Light be a [c]Body. For since Colours are the [d] *qualities* of Light, having its Rays for their intire and immediate subject, how can we think those Rays *qualities* also, unless one quality may be the subject of, and sustain another; which in effect is, to call it *Substance.* We should not know Bodies for substances, were it not for their sensible qualities, and the Principal of those being now found due to something else, we have a good reason to believe that to be a Substance also. Besides, who ever thought any quality to be a *heterogeneous* aggregate, such as Light is discovered to be? But, [e]to determine more absolutely, what Light is, after what manner refracted, and by what modes or actions it produceth in our minds the Phantasms of Colours, is not so easie. And I shall not mingle conjectures with certainties.

On the score of "whether Light be a [c]Body," the first note reads:

[c]Through an improper distinction which some make of mechanical Hypotheses, into those where Light is put a body, and those where it is put the action of a body, understanding the first of bodies trajected through a medium, the last of motion or pression propagated through it, this place may be by some unwarily understood of the former: Whereas light is equally a body or the action of a body in both cases. If you call its' rays the bodies trajected in the former case, then in the latter case they are the bodies which propagate motion from one to another in right lines till the last strike the sense. The only difference is, that in one case a ray is but one body, in the other many. So in the latter case, if you call the rays motion propagated through bodies, in the former it will be motion continued in the same bodies. The bodies in both cases must cause vision by their motion. Now in this place my design being to touch upon the notion of the Peripateticks I took not body in opposition to motion as in the said distinction, but in opposition to a Peripatetick quality, stating the question between the Peripatetick and Mechanick Philosophy by inquiring whether light be a quality or body. Which that it was my meaning may appear by my joyning this question with others hitherto disputed between the two Philosophies; and using in respect of one side the Peripatetick terms *Quality, Subject, Substance,*

> *Sensible qualities;* in respect of the other the Mechanick ones *Body, Modes, Actions;* and leaving undetermined the kinds of those actions (suppose whether they be pressions, strokes, or other dashings), by which light may produce in our minds the phantasms of colours.

As to colors being "the [d] *qualities* of Light," the second note reads:

> [d]Understand therefore these expressions to be used here in respect of the Peripatetick Philosophy. For I do not my self esteem colours the qualities of light, or of any thing else without us, but modes of sensation excited in our minds by light. Yet because they are generally attributed to things without us, to comply in some measure with this notion, I have in other places of these letters, attributed them to the rays rather then to bodies, calling the rays from their effects on the senses, red, yellow, &c. whereas they might be more properly called rubriform, flaviform, &c.

As to the final problem, "[e]to determine more absolutely, what Light is," the third note reads:

> [e]To determine after what manner light is a body, or whether it be a body more then by . . .

Unfortunately, the sheets are trimmed so that the final line of this note is illegible. It may be observed that Newton wrote "then" rather than "than" in his manuscripts.

A comparison between the texts of Newton's letters and papers printed in this volume (from 17th- and 18th-century printed works) and the versions given in the Royal Society's edition of *The Correspondence of Isaac Newton,* vols. 1–3, edited by Herbert Westren Turnbull (Cambridge: at the University Press, 1959, 1960, 1961), is given below, for Newton's own papers and letters.[14] No attention is paid here to differences in paragraphing, orthography, abbreviation, capitalization, or italicization; only substantive differences have

[14] Prepared with the aid of Joan Livingston Richards.

been noted. The letters in the *Philosophical Transactions* were edited for publication by Henry Oldenburg. Additional information concerning the early letters (and Henry Oldenburg) may be obtained from A. Rupert Hall and Marie Boas Hall, eds., *The Correspondence of Henry Oldenburg* (Madison: University of Wisconsin Press, 1965–); vol. 10 (1975), for the years 1673–1674, is published by Mansell in London.

Pages 47–59, Newton's letter to Oldenburg, 6 February 1671/72.

Page 49, paragraph 3, line 3, the *Correspondence* has "by my own & others Experience" for our "by my own Experience." Page 53, line 9, following the word "unfolded" (for which the *Correspondence* has "infolded"), the *Correspondence* has the following passage, omitted from our version:

A naturalist would scearce expect to see the science of those [that is, colours] become mathematicall & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick,) but evinced by the mediation of experiments concluding directly & without any suspicion of doubt. To continue the historicall narration of these experiments would make a discourse too tedious & confused, & therefore I shall rather lay down the *Doctrine* first. . . . (*Correspondence,* vol. 1, pp. 92–107).

Pages 60–64, Oldenburg's description of Newton's telescope. This document is not printed in the *Correspondence.*

Pages 65–66, Huygens's reaction to Newton's telescope in a letter to Oldenburg, 13 February 1672 [N.S.].

Huygens's letter (dated 3 February 1671/2 [O.S.] in the *Correspondence*) is in French; this version may well have been made by Oldenburg. In any event, Oldenburg omitted an opening portion, in which Huygens referred to the "merveilleux telescope de Mr Newton: dont j'ay beaucoup meilleure opinion maintenant que lors que par le raport imparfait qu'on m'en avoit fait je m'imaginois qu'il s'estoit proposé d'accourcir les lunettes ordinaires par la reflexion de ses miroirs." Among Newton's papers, there is a different English version of Huygens's communication, very literal and stilted, printed in *Correspondence,* vol. 1, pp. 91–92. In printing this letter, Oldenburg identified Huygens by name ("Monsieur *Christian*

Hugens de Zulichem": our p. 65 above), whereas he later referred to him in the printed *Philosophical Transactions* as "Monsieur *N.*" (*Correspondence,* vol. 1, pp. 89–92).

Pages 66–67, Newton to Oldenburg, 19 March 1672.

Page 66, last paragraph, the version in the *Correspondence* begins, "I told you that here is another instrument. . . ." Page 67, end of first paragraph, the version in the *Correspondence* has two additional sentences, of which the first has been expanded by Oldenburg into his note ("N.B.") added to Newton's letter, and the second refers to Newton's reply to Hooke's observations, which may be found on our pages 116–135. (*Correspondence,* vol. 1, pp. 121–123).

Pages 68–70, Newton's letter to Oldenburg, 26 March 1672.

An opening paragraph about Newton's observation of a comet has been omitted from our version. Page 68, paragraph 1, lines 4–7, in place of our

> that I am not very well assured of the goodness of the other, which I borrowed to make the Comparison; and therefore desire, that the other Experiment should be rather confided in, or reading at the distance of between a 100 and 120 foot. . .

the *Correspondence* has

> that I find that other (which I borrowed to make the comparison) to be none of the best in the kind, & therefore I would not have you rely on the observations made with it but rather estimate the performances of the metalline Telescope by the distance of between a 100 & 120 foot. . . .

Page 70, at the end of the letter, the *Correspondence* has an additional short paragraph, containing a courteous conclusion. (*Correspondence,* vol. 1, pp. 123–126).

Pages 70–71, Newton's reply to Auzout, in a letter to Oldenburg, 30 March 1672.

The three paragraphs together form the first paragraph of the letter printed in the *Correspondence.* The remainder of the letter, omitted from our version, deals with the design of a prism to reflect the rays from the curved mirror to the eyepiece and with Newton's reply to J.-B. Denis. (*Correspondence,* vol. 1, pp. 126–130).

Pages 72–75, Notes on the Cassegrain telescope, and Newton's comments thereon, in a letter to Oldenburg, 4 May 1672.

Page 75, at the end of the letter, the *Correspondence* has a short paragraph in which Newton says that he is "not at all concerned whether Objections be printed with or without the Objectors names." Thus those of "the Jesuite Pardies may be conveniently so printed if he desire it," but, said Newton, "I see not what it can signify to Mr Hook since the Contents will evidently discover them to be his. And besides, it is publiquely known that he hath writ objections & my Answer is expected." (*Correspondence,* vol. 1, pp. 153–155).

Pages 75–78, Some experiments proposed by Sir Robert Moray, and Newton's comments on them, in a letter to Oldenburg, 13 April 1672.

Pages 75–76, the numbered experiments are not identified in our version as having come from Moray. Two opening paragraphs (omitted in our version) discuss Pardies, Hooke, and Huygens; they are followed by a brief reference to Moray.

In the *Correspondence,* Newton then goes on to discuss a change he wishes to have introduced in an earlier communication (see reference to Newton's letter of 26 March 1672, and our p. 68 above); Newton asks that the change be made "least the freind of whome it was borrowed should thinke I depreciate it." Page 77, paragraph 2, line 2, the *Correspondence* has "Mr Hook" for our "another person." (*Correspondence,* vol. 1, pp. 136–140).

Pages 79–82, Pardies to Oldenburg, 9 April 1672. In the *Correspondence* this letter is dated 30 March 1672 (9 April 1672 [N.S.]). (*Correspondence,* vol. 1, pp. 130–134).

Pages 83–85, Newton to Oldenburg, in response to Pardies, 13 Apr. 1672.

Page 83, paragraph 1, lines 11–12, the *Correspondence* has "Sed extra oleas evagatus est R. P." for our "Sed hallucinatus est R. P." Page 85, end of the letter, the *Correspondence* has "Celeberrimi nostri Hookij" for our "*Domini* N. N." (*Correspondence,* vol 1, pp. 140–144).

Pages 93–96, "Quere's" proposed by Newton in a letter to Oldenburg, 8 July 1672.

In the *Correspondence,* this letter is dated 6 July 1672; an opening paragraph, responding to an "inquiry" by Oldenburg, has been omitted from our version. Page 94, end of paragraph 7, our version omits an additional paragraph referring to the numbering of "12 Particulars" in Newton's "Answer to Mr Hook." (*Correspondence,* vol. 1, pp. 208–211).

Pages 97–98. Pardies to Oldenburg, 21 May 1672. In the *Correspond-*

ence, this letter is dated 11 May 1672 [O.S.]. (*Correspondence,* vol. 1, pp. 156–159).

Pages 99–103, Newton to Oldenburg, in reply to Pardies, 11 June 1672. The *Correspondence* dates this letter 10 June, noting that Oldenburg changed the date to 11 June. (*Correspondence,* vol. 1, pp. 163–171).

Page 103, Pardies's reply, 9 July 1672.

The *Correspondence* prints this paragraph under the date 30 June 1672 [O.S.]; the whole letter, written in French, is evidently available in the Library of the Royal Society; the extract on our page 103 was translated into Latin (presumably by Oldenburg), and hence it is misleading to describe the French extract given in the *Correspondence* by saying that "the present extract was printed in *Phil. Trans.* 7 (1672)." (*Correspondence,* vol. 1, pp. 205–206).

Pages 110–115, Hooke to Oldenburg, 15 February 1671/2.

Throughout this letter, our version has "solving" for "salving" (a change apparently made by Birch), as in the phrase "salving the phænomena", page 111, line 5, the *Correspondence* has "light" for our *"white"* (an error also occurring in the Register Book); page 113, paragraph 3, lines 3–4, the *Correspondence* has

intire and uniforme again; and soe the compound motions are made to coalesse into one simple, where they meet, but keep their disturbd and compounded [motions], when they begin againe to Diverge and Separate . . .

for our "intire and uniform, again to diverge and separate." (*Correspondence,* vol. 1, pp. 110–116).

The *Correspondence* omits the adjective "excellent" in reference to Newton's "excellent discourse" on our page 110, line 3 from bottom.

Pages 116–135, Newton's reply to Hooke, in a letter to Oldenburg, 11 June 1672.

The numbering of the paragraphs differs in our version and the one in the *Correspondence,* for which see the preceding comment (for page 94); page 135, this paragraph is followed by two others omitted from our version and reading as follows:

12. *That the Science of Colours is most properly a Mathematicall Science.*

In the last place I should take notice of a casuall expression which intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematicall Demonstrations.* I said indeed that the *Science of Colours was Mathematicall & as certain as any other part of Optiques;* but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by which I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall:* Whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science. And if those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in which severall colours are originally inherent; I suppose the *Science of Colours* will be granted *Mathematicall* & as certain as any part of *Optiques.* And that this may be done I have good reason to beleive, because ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.

Thus much I have thought fit to returne to Mr Hooks Considerations: which that it may bring satisfaction in this part of Optiques to the Honourable Members of the R. Society hath been the Rule of my Intensions.

Throughout this letter, the expressions "the *Considerer*" and "the *Animadversor*" are used in our version for the "Mr Hook" of the original (as printed in the *Correspondence*), save for paragraph 4, p. 125, where "the Ingenious Mr. *Hook*" appears as the author of "the *Micrography.*" The numbered queries at the top of our p. 124 are neither numbered nor separated from the text in the *Correspondence.* Although note (1) on p. 188 of the *Correspondence* says that "the final paragraph of section 8" was omitted from the version published "in *Phil. Trans.* 7 (1672), 5084–103, no. 88, as Oldenburg has noted in the margin of the letter at line 2," this is not exact.[15] (*Correspondence,* vol. 1, pp. 171–193).

[15] According to the text printed in *Correspondence,* vol. 1, pp. 171–193, what was omitted was the end of the opening sentence (as given on our page 128, par. 3): "which I doe the more willingly because Mr Hook hath made such excellent use of that Instrument, & I shall be glad if it will contribute any thing to the promotion of those his ingenious endeavours, or add to his Inventions of that kind." Also, corresponding to our page 116, line 4, the *Correspondence* has: "But I must confesse at the first receipt of those Considerations I was a little troubled to find a person so much concerned for an *Hypothesis,* from whome in particular I most expected an unconcerned & indifferent examination of what I propounded," where our text has: "And though I find the *Considerer* somewhat more concern'd for an *Hypothesis,* than I expected". On our page 116, par. 2, line 6, the following sentence, found in

Page 136, extract of a letter from Huygens to Oldenburg, 14 January 1672/3 [*N.S.*]

The portion printed in our version, in an English translation, was sent to Newton in a letter of Oldenburg's of 18 January 1672/3. According to a note in the *Correspondence,* the word "holds" in line 3 of paragraph 2, page 137, is a translation of "tient" which was a mistranscription by Oldenburg of Huygens's "trouve"; and in the next line the word "distinct" is lacking ("distinct picture") in the translation and was omitted by Oldenburg in his transcription. Huygens's name is suppressed in the *Philosophical Transactions* version, where he becomes (our page 136) "*an ingenious person from* Paris." (*Correspondence,* vol. 1, pp. 255–257).

Pages 137–142, extract of Newton's reply to Huygens, in a letter to Oldenburg, 23 June 1673.

Our version omits an introductory section on dynamics and mathematics and concluding section on mathematics, plus a statement by Newton that he intends "to be no further sollicitous about matters of Philosophy"; some were not part of Newton's communication to Huygens. Oldenburg also omitted a sentence, following the conclusion on page 142 above, reading: "Pray with these Notes return my thanks to M. Hugens for his book." In the letters, the *Philosophical Transactions* version has *"Monsieur N."* in place of Newton's "M. Huygens." (*Correspondence,* vol. 1, pp. 290–297).

Pages 143–146, Newton's answer to a letter from Huygens, addressed to Oldenburg, 3 April 1673.

On our page 145, three paragraphs have been omitted, which are in the *Correspondence:*

Meane time since M. Huygens seems to allow that white is a composition

the *Correspondence,* is omitted: "But he knows well that it is not for one man to prescribe Rules to the studies of another, especially not without understanding the grounds on which he proceeds."

On page 133, par. 2, last line, our text has "dilate" where the *Correspondence* has "dilute." Such word differences have not in every case been noted. Some other examples occur on our page 66, line 7 (bott.), where the *Correspondence* has "insensible" for our "considerable"; our page 76, par. 4, line 6, and par. 5, line 9, where the *Correspondence* has "refraction" for our "reflexion"; and on our page 93, par. 1, line 3 (bott.) where the *Correspondence* has "The decision of which I could wish to be stated, & the events" for our "which I could wish were determined by the Event of proper Experiments."

of two colours at least if not of more; give me leave to rejoyn these Quæres.

1. Whether the whiteness of the suns light be compounded of the like colours?

2. Whether the colours that emerg by refracting that light be those component colours separated by the different refrangibility of the rays in which they inhere?

Page 143, first paragraph, line 10, "I confess" lacking in MS; second paragraph, line 5, the *Correspondence* has "fals" for "groundless"; page 145, paragraph 3, line 4, the *Correspondence* has "mended" for "waved"; page 146, line 13, the *Correspondence* has "easily" for "certainly"; again, in this letter, Huygens becomes "N." (*Correspondence,* vol. 1, pp. 264–267).

Page 147, extract of a letter from Huygens to Oldenburg, 31 May 1673 (10 June 1673 [N.S.]).

The printed portion of this letter, published by Oldenburg in an English translation from the French, appears in the *Correspondence* as it was copied out (in French) for Newton in a letter from Oldenburg to Newton of 7 June 1673; the first part of the letter is printed separately in the *Correspondence.* In the *Philosophical Transactions* version, Huygens is referred to as "the same Parisian Philosopher, that was lately said to have written the Letter already extant in No. 96. p. 6086." (*Correspondence,* vol. 1, pp. 285–287).

Pages 148–150, Linus (or Line, i.e. *Hall) to Oldenburg, 6 October 1674 [N.S.], and Oldenburg's reply, 17 December 1674.* Linus's letter, and Oldenburg's reply, are printed in the *Correspondence,* vol. 1, pp. 317–319, 332.

Pages 151–152, Linus to Oldenburg, 25 February 1675 [N.S.].

In the *Correspondence,* a final portion appears that is not present in our version:

. . . I cease to detaine you any longer heerein; and only now desire this favour of you, that you wilbee pleased to lett mee know what objections are made against My questioning that soe universally receaved Axiome, *Quæ sub maiore angulo etc* which I mentioned in my former letter to you. For seeing that learned Optike and Member of the R. S. Mr. Isaac Barrow in his Lect. Opt: p. 22, not to doubt of the truth therof makes mee conceave, that both hee and others in that learned Assembly have more grounds for themselves then meere authority. Which if I may know, you shall therin oblige

Honoured: Sr:
Your humble Servant
Francis Line.

(*Correspondence,* vol. 1, pp. 334–336).

Pages 153–154, Newton's comments on Linus's reply, addressed to Oldenburg, 13 November 1675.

Page 154, paragraph 3, lines 4–5, the *Correspondence* has "Mr Hill (a member of the R. Society), whom you are well acquainted with" for our "*A. H.* (a member of the *R. Society*)"; paragraph 3, line 6 and line 12, "Mr Hook" for our *"R. H."*

The *Correspondence* prints two final paragraphs omitted from our version, of which the second one reads:

> I had some thoughts of writing a further discours about colours 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 of that subject written when I sent my first letters to you about colours & of which I then gave you notice. This you may command when you think it will be convenient if the custome of reading weekly discourses still continue. In the meane while I am Sir. . . .

Newton has reference here to the lengthy communication read at the weekly meetings of the Royal Society beginning on 9 December 1675; for the text, see our pages 177 ff. above. (*Correspondence,* vol. 1, pp. 356–359).

Pages 155–156, extract of a letter from Newton to Oldenburg, 10 January 1675/6, in reply to a letter from Gascoines, a student of Linus.

The rest of this letter refers to Newton's electrical experiments, and his considerable annoyance at Hooke's insinuation that Newton had taken the major part of his "Hypothesis" (see our pages 177 ff. above) from Hooke's *Micrographia.* In a P. S., Newton requests that in printing his "former letter to Mr Linus," Oldenburg should "leave out what I mention of Mr Hill & Mr Hook, or at least put letters for their names: for I beleive they had rather not be mentiond." (*Correspondence,* vol. 1, pp. 407–412).

Pages 157–162, Newton to Oldenburg in reply to Linus, 29 Feb. 1675/6.

(*Correspondence,* vol. 1, pp. 421–426; where it is printed from Samuel Horsley's 18th-century edition of Newton's *Opera*).

Pages 163–169, Lucas to Oldenburg, 27 May 1676 [N.S.].

In the *Correspondence,* this letter is dated 17 May 1676 [O.S.]. In the *Correspondence,* the second paragraph on our page 167 is numbered "[7^0]" by the editors; and there is no paragraph division, as between our lines 9/10 from bottom. (*Correspondence,* vol. 2, pp. 8–14).

Pages 169–176, Newton to Oldenburg, 18 Aug. 1676, in reply to Linus and Lucas.

(*Correspondence,* vol. 2, pp. 76–81, where the letter is substantially the same.)

Pages 178–235, Newton's "Hypothesis explaining the Properties of Light," etc.

The *Correspondence* prints only selected portions of the "Discourse of Observations," with the comment that the latter "is accessible in the *Opticks* and in Horsley," whereas the "Hypothesis . . ." is not, "although both are printed in Birch." (*Correspondence,* vol. 1, pp. 362–392, with extensive bibliographical and explanatory notes).

Pages 280–312. Newton's letters to Bentley.

The following are textually significant differences:

	Our version	*Correspondence,* vol. 3	
pp. 281, *lines 6-7 bott.*	evenly disposed	eavenly diffused	(p. 234)
line 5 (bott.)	could	would	(p. 234)
line 4 (bott.)	some of it would	some of it	(p. 234)
283, *line 1*	Center of	center of the Orbs of	(p. 234)
line 4	Planets	ones	(p. 234)
285, *line 5*	Bodies, which were	bodies about which they move & to the quantity of matter conteined in those bodies, which were	(p. 235)
lines 3–5 (bott.)	are, as . . . their Gravities; or	are (as . . . their gravity) or	(p. 235)
286, *line 3 (bott.)*	revolve about those	revolve at those distances about those	(p. 235)
296, *line 2 (bott.)*	and there	& then	(p. 239)
300, *last line*	Computation I	assumption	(p. 253)

301, *line 1*	think will do	will do	(p. 253)
line 2	worth while	worth your while	(p. 253)
line 4 (*bott.*)	gradually pass	gradually wast[e] & pass	(p. 253)
302, *par.2, line 1*	the second position	your second position	(p. 253)
last line	and Force	or force	(p. 254)
303, *line 3*	a competent Faculty	any competent faculty	(p. 254)
line 8	terial, I have left	terial, is a question I have left	(p. 254)
304, *line 4*	implies no	argues nothing	(p. 254)
305, *line 3*	you seem to do	you do when you seem to	(p. 254)
line 3 (*bott.*)	upon the Side	on one side	(p. 254)
line 2 (*bott.*)	about,	about it	(p. 254)
306, *line 2*	that the Comets	that Comets	(p. 255)
par.2, *line 4*	Outside	outward	(p. 255)
par.2, *line 8*	the second	your second	(p. 255)
310, *last line*	divine Arm	divine power	(p. 244)

Index

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(In this index, the numbers refer to the page-numbers of this work, not of the original seventeenth- and eighteenth-century articles and books reproduced. The index includes the introductory essays written for this work and Fontenelle's *Éloge,* pp. 444–473.)