HISTORICAL ESSAY,

&c. &c. &c.

Alexander Ziwet

HISTORICAL ESSAY

ON THE

FIRST PUBLICATION

OF

SIR ISAAC NEWTON'S

PRINCIPIA.

BY

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PREFACE.

Sir David Brewster is well known to be engaged on an enlarged edition of his Life of Newton, for which, in addition to other valuable documents, he has had access to the papers belonging to the Earl of Portsmouth. It may be right, therefore, to mention, that no steps were taken towards printing this Essay, before it had been ascertained that he would not consider the publication of it as an interference with his more important object. Indeed, minute details on a particular point would hardly be compatible with a general history, although it is hoped that they may supply some materials for the greater work.

From the MSS. of David Gregory, and other writers, several facts have been either recovered or illustrated; but the two great sources of original information have been the collections of the Earl of Macclesfield, and the papers belonging to the Royal Society. The first contain a number of Newton's own MSS., and having belonged to Mr. W. Jones are of indisputable authenticity; the second have indeed been often examined, but not so completely as to preclude some new facts being recovered from them by a repeated search. The extracts in the following tract sometimes vary from what has been printed by Birch in his History of the Royal Society: but the difference will be found not to affect the sense; and has been occasioned by the use of the originals, which he did not always copy verbatim.

For these last papers some references are used, which may not be obviously intelligible to every one, and any future examination may be facilitated by giving a short explanation of the different terms used to describe the arrangement of them.

The *Journals* contain an abridgment of the business transacted by the Society at large in their weekly meeting.

The *Minutes* are the records of what has passed in the Council.

The *Registers* contain copies of the papers, which have been communicated.

The *Letter Books* preserve copies of letters, and, in some instances, of shorter papers, which have been drawn up in that form.

There are likewise original papers and letters, pasted into *Guard Books*, and arranged according to the initials of the writers' names.

The examination of collections, which lie so wide apart from each other, could only be undertaken by one, who was permanently resident in England: and, supported by this accidental advantage, I have sometimes ventured to differ from so able a writer as M. Biot. Truth, however, has been our common object, and it has always the best chance of being elicited by the fair statement of honest opinions.

It may be thought that the Essay is prolonged beyond its immediate purpose by what is said, at the end, on the publication of the second and third editions of the Principia; but this, having been made as concise as possible, seemed to obviate what otherwise might appear to be an abrupt termination of the work; and it may not be without some use to those, who shall have the means and the inclination to continue this History to the time, when Newton set his last hand to the greatest work of his most powerful mind.

CORRECTIONS.

P. 19. lin. 14. "such oversights" add "(as, in this instance, of 1683 for 1684)"
P. 35. note f. for "III." read "IV."
P. 48. lin. 12. "which he adopted," add "in this correspondence with Hooke"
P. 49. lin. 8. "order of effect varied" read "this law of the effect ceased"
P. 78. head line. "Printing" read "Printing."

HISTORICAL ESSAY

ON

THE FIRST PUBLICATION

OF THE

PRINCIPIA.

DR. PEMBERTON tells us[a] that the first thoughts, which gave rise to Newton's Principia, occurred to him when he had retired from Cambridge into Lincolnshire, in 1666, on account of the plague. Voltaire[b] had his information from Mrs. Catharine Barton, Newton's favourite niece, who married Conduitt, a member of the Royal Society, and one of his intimate friends: from having spent a great portion of her life in his society, she was good authority for such an anecdote, and she related that some fruit, falling from a tree,

[a] Preface to his View of Sir Isaac Newton's Philosophy. See Append. XII. p. 49. From Dr. Hodges's Λοιμολογία, we learn that the plague began first in Westminster, at the end of 1664; that in London it was most violent in the hotter months of 1665, and had so far abated in the following winter that the inhabitants returned to their homes in December. The Royal Society did not indeed resume their regular meetings till March, 1666; but the disease had then subsided, and some writers have therefore referred Newton's speculation to 1665. Pemberton, however, is certainly right in assigning his retirement into Lincolnshire to the following year. In the sixth volume of the Philosophical Transactions there is a paper, in which he says, (p. 3075,) that in the beginning of the year 1666 he procured a glass prism "to try "therewith the phænomena of "colours;" and, after further particulars, he adds, (p. 3080,) "Amidst these thoughts I was "forced from Cambridge by the "intervening plague."

[b] Philosophie de Newton. 3me partie, chap. iii.

was the accidental cause of this direction to Newton's speculations. Conduitt himself drew up an account of facts, which he transmitted to Fontenelle, to form the foundation of the Eloge of Newton, which is inserted in the Histoire de l'Académie des Sciences, for 1727. This paper, in its original state, has been published by Mr. Turnor in his History of Grantham; and it mentions the same circumstance. It was not noticed by the French writer, to whom it was at first communicated, but tradition marked the site of the tree [c] from which the apple fell, till being decayed it was taken down about 1820, when the wood of it was carefully preserved. The anecdote indeed is neither devoid of interest, nor improbable in itself. He was sitting alone in a garden at the time, and one of the peculiar faculties of his powerful mind was to be able to follow out a chain of reasoning, in which the connecting links would escape the notice of a common observer. The tendency of bodies to fall towards the earth does not fail at the top of the highest mountains, and, as he was carried in thought to more remote regions, there appeared to be no assignable limit to its influence. He had found from the motions of the planets, that they were acted upon by a force towards the sun, which was reciprocally as the squares of their distances from it; and, as an experimentum crucis, he proceeded to examine whether the moon's motion would, in a circular orbit, agree with a similar variation; but the result of his calculations did not

[c] At Woolsthorpe, where his mother lived. (Hist. of Grantham, p. 160). The farm is the property of Mr. Turnor, who has contributed his best endeavours to preserve the records of Newton's family. His brother, the Rev. C. Turnor, of Wendover, has been likewise actuated by the same zeal for collecting what may be connected with the history of so great a man.

agree with such an hypothesis; and disappointed in his hopes, he turned his attention to other pursuits.—Whiston's [d] account also confirms these last particulars.

It is said that his failure was occasioned by his having no books with him, to which he could refer. His object was to determine whether the moon, being deflected from the rectilinear direction of its motion, was drawn down towards the earth by a quantity, which (according to the law which he assumed) would agree with the space, through which heavy bodies in the same time have been found to fall at the earth's surface. The moon's distance being taken in semidiameters of our globe, the absolute space, through which it moved in its orbit during a given time, was to be deduced from the magnitude of the earth. Now Newton took the common measure of 60 miles for a degree of latitude, which had been used by the old geographers and seamen. This was considerably too small, and consequently the distance, by which the moon would under such circumstances have been drawn from the tangent of its orbit, came out less than the truth. Possibly the quantity, which formed the foundation of his calculation, had been the more impressed on his mind from its agreement with the result of the observations, which Edw. Wright had published in 1610. Wright was a Cambridge man, (a fellow of Caius college,) which may have made his work more familiar to the members of the university. Conclusions, indeed, much nearer to the right quantities, had been arrived at by Snellius [e] and Norwood [f]; but Voltaire distinctly says that Newton, at the time, was not acquainted with either of them, and that Nor-

[d] Memoirs of his Life and Writings, vol. i. p. 33.

[e] Eratosthenes Batavus, 1617.

[f] Seaman's Practice, 1636.

wood's measure had been universally and completely forgotten. It is not improbable that Newton, whose mind was occupied by his own deep and important thoughts, may, when (as he then was) only four and twenty years of age, have had but partial intervals for becoming familiar with the writings of others: and, although it is not easy to conceive him ignorant of the fact, still it would be less easy to account, in any other way, for his not having recourse to such a method of correcting his reasoning, as was not only obvious but absolutely necessary. It may be added, however, that Voltaire seems to have been certainly mistaken, when he said of Norwood's work, that the civil wars "l'avaient enseveli dans l'oubli." The Seaman's Practice, which was first published in 1636, must have continued for a long time to be a book very extensively in use: it was often reprinted; the seventh edition was published in 1667, and the eighth came out in the following year[g]. Neither is there the slightest foundation for the supposition of Norwood himself having been forgotten; indeed there is indisputable evidence to the contrary, exactly at the very time of which we are now speaking. In the Philosophical Transactions for 1667, there is a paper of his, the original of which is preserved at the Royal Society. It was a portion of a letter to Oldenburg, dated, "Sommer Islands, June 18, 1667," and was communicated in consequence of an application made to him by the secretary of the society, which will be found

[g] Wilson, in his Dissertation prefixed to Robertson's Navigation, also mentions, (p. xix.,) that the work was noticed with commendation by many contemporary writers, and continued to be reprinted till 1732: and it may be added that Sir Jonas Moore, in his Mathematical Compendium, (p. 94,) quotes Norwood's Measure as a standard, with which his readers might be supposed to be acquainted.

in the Appendix to the present volume[h]. There is likewise in the letter-book[i] of the Royal Society a copy of one from Richard Stafford[k], (probably to Oldenburg,) dated "Bermudas, 16 July, 1668," in which he says that he was solicited to write by his "honoured "friend Mr. Richard Norwood," who was prevented from doing so himself by "weighty business lying "upon him." It concludes by saying that, "as for "the eclipses of the moon, you would have observed "here, and be informed about, I can say little of them, "but I suppose my worthy friend, Mr. Norwood, will "give an account thereof to your content. If any "thing should cause him to fail, it will be his age "and weakness." Norwood's writing is that of an old man; and it is probable that our not having more of his communications in the Transactions was occasioned by his advanced period of life. At all events it is clear that he was not forgotten by his scientific friends in England. Among the manuscripts belonging to the Ashmolean Museum, there are some notices of him in Aubrey's Biographical Memoranda, who says, "By a "letter from Nich. earl of Thanet to me concerning "his purchase in Bermudas, not dated, but writ about "1674 or 5, thus: 'As to old Mr. Norwood, to whom "the Royal Society would send some queries, [he] is "lately dead, as his son[l] informs me, who lately went "captain in the ship wherein I sent my gardener and "vines to the Bermudas: he was aged above 90.'"

[h] No. XVI. p. 60.

[i] Vol. ii. p. 241.

[k] Sheriff of the island. (Birch's History of the Royal Society, vol. ii. p. 314. For the contents of this letter, see Phil. Trans. vol. iii. p. 792; but the mention of Norwood is there omitted.

[l] Oldenburg has noted Norwood's letter as having been brought to him by the son; and in the Phil. Trans. for 1668, (vol. iii. p. 824,) there are Particulars respecting Jamaica by "Mr. Norwood, the younger."

However this may have been, some years seem to have elapsed before Newton returned to the subject; but great as the importance of it might be, the interval was not lost in the temporary relaxation from the study of it. Each branch of his pursuits was the means of advancement for the rest. Without the new systems, which he created for himself in mathematics, the discoveries, which were developed by his physical researches, could never have been established; and the delay enabled him, in the present instance, to return with additional strength to the inquiry. Of this return, however, the common accounts seem to require some correction.

Dr. Robison [m] tells us that Newton, having become a member of the Royal Society, "there learned the "accurate measurement of the earth by Picard, differ-"ing very much from the estimation by which he had "made his calculations in 1666; and he thought his "conjecture now more likely to be just. He went "home, took out his old papers, and resumed his cal-"culations. As they drew near a close, he was so "much agitated, that he was obliged to desire a friend "to finish them. His former conjecture was now "found to agree with the phænomena with the utmost "precision." There does not appear to be any contemporary authority for these particulars; but tradition often preserves what is very valuable. Such evidence, however, ought to be received with considerable caution. Every one must decide, from his own view of Newton's character, how far he thinks it consistent with the latter part of the statement; but some reasonable doubt may be entertained of the strict accuracy of the facts previously related in it. There is ground for believing that Newton, at first,

[m] Mechanical Philosophy, p. 288 (1804); and vol. ii. p. 94 (1822).

was seldom present at the meetings of the Royal Society. He was elected Jan. 11, 1672, but did not attend for admission till Feb. 18, 1675 [n]; and in March, 1673, he wrote to Oldenburg, to desire that he might be "put out from being any longer a fellow of the "Royal Society. For though I honour that body, yet "since I see I shall neither profit them, nor (by reason "of this distance) can partake of the advantage of their "assemblies, I desire to withdraw [o]." It is not improbable that, on this occasion, he was more particularly actuated by painful feelings from the controversy, in which he was engaged about his Optics; but the fact of his general absence, at the period, from the meetings is equally clear: Oldenburg, in his answer, distinctly admits it, and endeavours to meet the alleged difficulty by sending him down the Philosophical Transactions, and promising to manage his correspondence for him in London. Pemberton [p], likewise, describes the use of Picard's measures as a consequence of Newton's returning to the inquiry, and not as an occasion, which led him to it. M. Biot [q], however, in his Life of Newton, adopts the more modern account, and says, "à ce que l'on peut conjec-"turer, vers le mois de Juin 1682, se trouvant à Lon-"dres a une séance de la Société Royale on vint à parler "de la nouvelle mesure d'un degré terrestre, réçem-"ment executé en France par Picard; et l'on donna "beaucoup d'éloges aux soins qu'il avoit employés "pour la rendre exacte;" and he then adds Dr. Robison's account of the result. It must, in the first place, be remarked, that M. Biot proposes this date only as

[n] Birch's Hist. of Royal Society, vol. iii. p. 1 and 181.

[o] From an original letter in the possession of the Earl of Macclesfield.

[p] Appendix, No. XII. p. 51.

[q] Biographie Universelle, vol. xxxi. p. 154.

a conjecture; and, admitting the information to have been collected at the Royal Society, there was some ground for it, since Picard's measurement was certainly discussed there on the 7th of June, 1682 [r]. This, however, is not a solitary instance of the subject having been attended to in that place: consequently it is not conclusive. The Biographie Universelle, also, in which the account was printed, extends to 52 volumes, and is not accessible to every one. The English translation, therefore, of the Life of Newton, as published by the Society for promoting Useful Knowledge, has been generally referred to, in which [s], the conjecture of the original is converted into a positive assertion, and by mistake has consequently been admitted as a fact, which the author had ascertained. This has been confirmed by an oversight in the Biographia Britannica, where [t] it is expressly said, that Picard's measure was executed in 1679. He published, in the following year, his Voyage d'Uranibourg, to which are added some astronomical observations for determining the latitude of several places in France, but his Mesure de la Terre came out many years before. It was magnificently executed at the Louvre press in 1671; and even at present, when the whole impression must have been long ago distributed, the copies of it are very scarce [u]. It is not in the library of the Royal Society; from whence we may infer that it was not presented to them; but this would be no proof of their not being soon acquainted with its contents; and that they were so cannot admit of a doubt.

[r] Birch's Hist. of the Royal Society, vol. iv. p. 150.

[s] P. 18.

[t] Vol. v. p. 3224. Pemberton also, when referring to what occurred in 1679, speaks by mistake of Picard's measure as then "lately" made.

[u] There is one at the British Museum, in the library collected by king George the Third.

At the meeting, held on Jan. 11, 1672 [x], Oldenburg read a letter, written to him by Mr. Vernon from Paris, describing the method followed by Picard in measuring his degree, and specifically stating the precise length that he found for it. This created so much attention at the time, that it was read again on the 1st of February, with two other letters, which Mr. Vernon had written on the same subject on the 27th and 30th of January [y]. In the Philosophical Transactions for 1675 [z], there is also a full detail of all the quantities resulting from the different parts of the French observations and calculations; and the subject was again recurred to in the Transactions for the following year [a]. The dates of these publications seem to give the time when Picard's work became generally known to scientific men in England. There is among Smith's papers, in the Bodleian library, a letter written from the "Observatory, (Jan. 22, 1677–8,)" by Flamsteed to Bernard, in which he says, "I have seen Mr. Picard's "book, and am of opinion, that in a modern letter it "would make but a small volume. When I came "hither first" [which was in 1675] "we had fre-

[x] Birch, vol. iii. p. 3.

[y] Birch, vol. iii. p. 8. It is most probable that Newton was not present at the meeting of the Society on the 11th of January; for a letter was then read from him, to which the secretary was ordered to write an answer, (Birch, p. 1–3,) the original of it is in Lord Macclesfield's collection, and is dated Cambridge, Jan. 6: it is also clear that he was in college on the 29th of January, from the date of a letter mentioned in the Phil. Trans. vol. vii. p. 4006.

[z] Vol. x. p. 261, No. 112. The reason given for the insertion of this paper is the great scarcity of the original: "very "few copies of it being come "abroad (for what reason it is "hard to discover)."

[a] Vol. xi. p. 591. No. 124. This however is a mere notice, almost the whole of the paper being occupied by an account of a work on Natural History, to which Picard's Dissertation is annexed. This last only extends to 30 pages. In this place it is again said that it was "very dif"ficult to get any copies of these "books."

" quent discourse about measuring a degree from this " place, but having got an account of his, we find it " needless, he has been so careful, and his agrees so " well both with the Arabian and Mr. Norwood's. " The Arabian, reduced to our measures, gives $69\frac{1}{2}$ " miles to a degree, Mr. Norwood's some little less, " the French $69\frac{1}{10}$; so that in this we may acquiesce. " It were to be wished Mr. Picard's book were either " turned into Latin or English; but I fear 'tis hard " to get one of them. I never saw more than one, " and that by great chance." Newton appears to have been regularly supplied with the Phil. Trans. by Oldenburg; and it does not appear to be possible, that a matter of so much real importance could have escaped his attention for a long series of years, and should at last have been the subject of unexpected information to him. It is very possible, however, that he may have heard much of Picard's Measure at the Royal Society at the time of his admission in Feb. 1675. There is no notice of it having been discussed at that meeting: the Journals mention that a letter was then read from Hugens, who was at Paris; but that is still in existence, and contains nothing on the subject; the work, however, must have been fresh in the mind of the secretary, who printed the account of the volume, which contained it, in the number of the Philosophical Transactions for the following month; but there is good reason for concluding, that Newton did not make use of it till long after.

The plain truth seems to be, as Pemberton has related, that in the same manner as in the fourth proposition of the third book of the Principia, he took the moon's distance as equal to sixty of the earth's semidiameters. This estimate, which was sufficiently near for his purpose, gave the deflection from the tangent in one minute to be equal to the space through which a body would freely fall on the earth's surface in one second. So fortunate a coincidence shortened the calculation, and therefore satisfied him, even after he went more accurately into the particulars, in his investigations de Systemate Mundi. He there shews [b] the causes of minute errors in the general method which he pursued; but he knew that the data were not, at his time, sufficiently settled for him to do more than he endeavoured to establish from them. A body falls at the earth's surface by the force of gravity through 16.1 feet in a second of time: but assuming the length of a degree to be only 60 miles, the moon would be drawn down by the same force only through 13.9 feet in a minute. Norwood's degree would have given the exact quantity required; but after Newton became acquainted with the Seaman's Practice, he might have wanted confidence in the conclusions which it contains; for although the amplitude of the arc is determined in it as well as the circumstances of the times would allow, the geodetical method, by which its length was found, was very imperfect. From the result of his labours it is evident that Norwood exerted great care and judgment in making allowances for the difficulties which it involved; and his industry must have been indefatigable. Aubrey mentions, in the manuscript account, that at his own charge, he also measured with a chain from Berwick to Christ Church, in order to the finding the quantity of a degree. But still Newton's opinion on this subject may be collected from what he says in the second edition of the Prin-

[b] Principia, p. 406, 407. The conditions of the problem are more minutely detailed in Woodhouse's Physical Astronomy, pref. p. xxv: and the conclusions from modern measures may be seen in Delambre's Astronomie, vol. iii. p. 515.

cipia, (book III. prop. 19,) where Norwood's name is introduced, but second to that of Picard. We do not, likewise, know when the English determination became known to him; but we are certain that he was well aware of Snellius's measures, quite as soon as he was of Picard's—probably much sooner; since the specific mention of them is made in Bernh. Varenius's Geography[c], of which he edited a new edition at Cambridge in 1672. The method used for this measurement was that of regular triangulation; and although Cassini[d] and Musschenbroek[e] have pointed out errors that were committed in it, still these corrections were long subsequent to the period of Newton's inquiry; and the 28500 Rhinland perches of the Eratosthenes Batavus[f] would have been the length of a degree, which would have made the moon's deflection, in the minute, as much as 15.5 feet, which might have well prepared Newton, for the 16.0 resulting from Picard's, if he had been anxious and at leisure to renew his calculations at an earlier period, than either is assigned to his doing so, or there is reason to believe that he returned to them.

Pemberton tells us that some years after his first attempt had failed, "a letter, which he received from "Dr. Hooke, put him on inquiring what was the real "figure, in which a body, let fall from any high place, "descends; taking the motion of the earth round its "axis into consideration[g]." Now this occurred in 1679; for, on the 11th of December in that year, Hooke read to the Royal Society[h] his answer to a letter of Newton, "wherein he explained what the

c Cap. iv. p. 24, 26. ed. 1672.

d Mém. de l'Ac. R. des Science, 1701, p. 175.

e Dissertatio de Magnitudine Terræ, 1729.

f P. 212.

g App. XII. p. 51.

h Birch, vol. iii. p. 516.

"line described by a falling body must be supposed "to be, moved circularly by the diurnal motion of the "earth, and perpendicularly by the power of gravity: "and he shewed, that it would not be a spiral line, as "Mr. Newton seemed to suppose, but an excentrical "ellipsoid, 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." There will be occasion to return again to this correspondence, and to enter more fully into the nature and subject of it. This passage is now introduced only to mark the date, to which Dr. Pemberton's account must be referred. It is also corroborated by Newton himself, who told Halley, at a subsequent period, that Hooke's letters on this occasion were the cause of his "finding the "method of determining figures, which when I had "tried in the ellipsis, I threw the calculations by, "being upon other studies; and so it rested for about "five years, 'till upon your request" [in 1684] "I "sought for the papers[i]". Now it is clear that the figures here alluded to were the paths of bodies, acted upon by a central force; and as the doctrine was applied to cases, in which the attraction varied inversely as the squares of the distances, it is quite natural to conclude that the same occasion induced him, (as Dr. Pemberton distinctly states[k],) to resume his former thoughts concerning the moon. From this authority, likewise, which was derived from conversation with Newton himself, we learn that it was on this same occasion that he used Picard's measures to correct his calculations. Mr. Jones, therefore, was evidently mistaken when he reported to the writers of the

[i] App. VII. p. 40. [k] App. XII. p. 51.

General Dictionary[l] that Newton ascertained the law of the motion in an ellipse, when the force was inversely as the squares of the distances, during the winter between 1676 and 1677, and that having resumed the consideration of it in 1683, he then added some other propositions concerning the motion of the heavenly bodies.

Halley's request, which has been just mentioned, is of considerable importance to our history. Having in January, 1684, "from the sesquialter proportion of "Kepler, concluded that the centripetal force decreased "in the proportion of the squares of the distances re- "ciprocally[m]," he was unable to trace out the foundation of this property, or to understand the manner of its operation on the heavenly bodies. He soon after[n] met Sir Christopher Wren and Dr. Hooke; and, falling into conversation with them on this subject, he found that the former had been long under the same circumstances with himself. Wren honestly avowed it; but from Hooke, who boasted that he was then in possession of the whole system, nothing satisfactory could be obtained in explanation of the proof, by which he could establish it. In August, however, of the same year, Halley visited Newton at Cambridge, "and then learned the good news of his having brought "this demonstration to perfection[o]." Newton not being able to meet with the paper, on which he had written his theorems, worked them out again, and communicated the propositions to him through Mr. Paget[p], who, on Newton's recommendation to Flamsteed[q], had been made Mathematical Master at Christ's Hospital

[l] Vol. viii. p. 781.

[m] App. VI. p. 36.

[n] He says he "came on Wed- "nesday to town." Halley at the time was living at Islington, and possibly came in on that day, as the meeting of the Royal Society was on the Thursday.

[o] App. VI. p. 37.

[p] Ibid.

[q] Baily's Account of Flamsteed, p. 125.

in 1682. This fulfilment of a promise, which Newton had made him at Cambridge, took place in November[r], and induced Halley to pay another visit to that place to confer with him on the subject, wishing to gain more complete information about it, and to induce the great author to pursue the inquiry. These last particulars are derived from a letter written by Halley to Newton in 1686, in which he goes on to speak of an entry in the Registers of the Royal Society, as if it had been made from what had been sent through Mr. Paget. This, however, does not appear to be strictly the case. We find from the Journal Book, that on the 10th of December, 1684, "Mr. "Halley gave an account, that he had lately seen "Mr. Newton at Cambridge, and that he had shewed "him a curious treatise, 'de Motu,' which upon his "desire, he said, was promised to be sent to the So-"ciety to be entered upon their Register." Paget was upon this requested to join with Halley in keeping Newton in mind of his promise; which must of course refer to something beyond what had been communicated in November. No mention is made of any part, even of this, being at the time exhibited to the Society. Neither has any reason been discovered to believe that it was so[s]. Most probably the communication was made in the following February. The exact day is uncertain, but it appears to have been about the middle of the month; for in a letter written on the 23rd by

[r] App. VI. p. 37.

[s] Newton, in his letter of the 14th of July, 1686, (App. VII. p. 41,) says, "in turning over "some old papers I met with "another demonstration of that "proposition," [the 4th of the first book of the Principia,] "which I have added to the "scholium." This is a proof for circular motion; and the manner, in which it is said to be found, shews that it is no part of what is above alluded to; but it is very curious from being a specimen of the method of reasoning, by which he satisfied himself in the first steps towards the great truth for which he was searching.

Newton to Aston, (one of the secretaries,) he thanks him for entering "his notions about motion[t]" in the Register; and immediately adds, "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." This letter seems to be a reply to one, in which Aston had informed him of the record made of his propositions, and appears to indicate that the paper had not been sent long before. "Dec. 10, 1684," is written at the beginning of the copy of it in the Register; but that is most probably a mere reference (as commonly is made in these books) to the place in the Journals where particulars respecting their several contents may be found.

Mr. Jones reported that Newton made some additions to his demonstrations in 1683; and, with the exception of M. Biot, who notices the error, almost every other writer on the subject has spoken of "near "a dozen propositions relating to the motion of the "primary planets about the sun, which he communi-"cated to the Royal Society in the latter end of the "year 1683[u]." That this should have been so generally repeated is not extraordinary, for it is derived from what certainly appears to be the very best authority. In some remarks which constitute N°. LXXI[x] of the Commercium Epistolicum, the following passage occurs: "Anno . . . 1684, in . . . Actis Lipsicis pro "mense Octobri, Calculi differentialis elementa pri-"mum edidit D. Leibnitius, literis G. G. L. designatus. "Anno autem 1683 ad finem vergente, D. Newtonus

[t] App. III. p. 24.

[u] Biographia Britannica, vol. v. p. 3224. Pemberton likewise (App. XII. p. 51) mentions the same number, but he refers to an earlier period of time, at which he describes them to have been drawn up. M. Biot's objection is stated in p. 156 (1). This note is omitted in the English edition.

[x] P. 205, in the edition of 1722.

"propositiones principales, earum quæ in Philosophiæ "Principiis Mathematicis habentur, Londinum misit: "eædemque cum Societate Regia mox communicatæ "sunt, annoque 1686 liber ille ad societatem missus "est ut imprimeretur, proximoque anno mense Martio "lucem vidit: et exemplar ejus D. Nicolao Fatio da-"tum est ut ad Leibnitium mitteretur." Now the Commercium Epistolicum was first printed in 1712, and was drawn up by a committee [y], the members of which were zealous and intimate friends of Newton. It may be thought, therefore, very presumptuous to question their accuracy; but there are difficulties attached to this part of their statement, which are well worth considering. The mass of papers preserved by the Royal Society makes it impossible to look through all of them for a particular fact; but the minutes of the Council, the Journals of the general meetings, and the letter-books of the period in question, have been carefully examined without any traces being discovered of such a circumstance. Nothing has been found of an earlier date than the paper, which was indisputably sent a full twelvemonth later. Again, if this account is admitted, it is hardly possible that Halley could have been unacquainted with the circumstance, for he was elected into the council on the 30th of November, 1683, and attended several of the subsequent meetings. Wren, also, had been in the former councils, and was reelected at the same time; yet neither of them was able to understand the law of the inverse squares, when they discussed it in January, 1684 [z]. Hooke, again, could not have been ignorant

[y] Dr. Arburthnot, Mr. Hill, Dr. Halley, Mr. Jones, Mr. Machin, and Mr. Burnet (Thomson's Hist. of the Royal Society, p. 292): others appear afterwards to have been added to them.

[z] App. VI. p 36.

of it; for he took a share in the business of every meeting from Oct. 24, 1683, when the Society reassembled after the summer vacation, till the end of February, 1684. Yet in the spring of 1684 he affected to be in possession of the truth from his own inquiries, and to be resolved to make a secret of it. If we can suppose that the fact had, in some unaccountable manner, escaped Halley, he might indeed have been agreeably surprised[a] in the following August, by discovering that Newton was in possession of the demonstration, which he had in vain attempted to discover; but Newton would have referred him to what was already in London, instead of troubling himself to draw out again the propositions, which were not to be found from several years having elapsed, since the time when he had invented them: and these were to be communicated to the Royal Society, which, on this supposition, would have been already in possession of them. Lastly, we must imagine that the business had been wholly forgotten in the lapse of a twelvemonth by the Royal Society itself, and that even its officers had lost all recollection of it; for the minute of Dec. 10, 1684, gives as a reason for Newton's being pressed to send the fuller account of his discoveries, that by doing so he might secure his invention to himself, until such time as he could be at leisure to publish it. Now all this would have been superfluous, if he had already done what the Commercium Epistolicum attributes to him.

On the other hand, it is very difficult to imagine how such an error, as we here suppose, could have originated. The same date of 1683 occurs in both editions of the book (in 1712 and 1722), and it cannot be a press error, for the argument with reference to Leib-

[a] App. VI. p. 37.

nitz would fall to the ground, if 1684 were substituted for it. Many years had elapsed, and we can easily conceive a mistake to have arisen if the writers trusted to their memory, (which is the solution which M. Biot has suggested,) but they would hardly have argued such a point without reference to some written document. Newton's letter to Aston is distinctly dated Feb. 23, 1684–5, and we cannot well imagine that the error could have been occasioned by the Journal's mentioning it in a minute, which is dated Feb. 25, 1684. There is indeed no notice of the designation of the year being unchanged, as the time was previous to the 25th of March; but although such oversights are now of very frequent occurrence, they were not so likely to be made before the alteration in the style. A loose paper, preserved in Lord Macclesfield's collection, seems to offer the most probable solution of the difficulty. It is in Newton's handwriting, and is entitled, "Animadversiones in "schediasma Leibnitii, De resistentia medii et motu "projectorum gravium in medio resistente, in Actis "Lipsiensibus anno 1689 mense Feb. impressum." It is not complete, but rather the rough draught for the beginning of some regular remarks. Newton is well known to have been in the habit of repeatedly transcribing his papers, and copying them with alterations; and accordingly we find on the same sheet of paper both the entries, which have been printed in the Appendix[b]. This was certainly in the possession of Jones, and contains several of the expressions used in the Commercium Epistolicum, with the same date, in the first instance, annexed to them. The writers, however, seem not to have observed more than the first entry; the second being on the opposite side

[b] N°. XIX. p. 67.

of the sheet; it could otherwise have hardly failed to strike them, that the year had there also at first been written, 1683, since the last figure, having evidently been altered to a 4, is much blacker than the rest. They would then have either had at once the correct period, or would have been led to ascertain which of the two they were to adopt. If the doubt had been once suggested, there could have been no difficulty in resolving it.

The hasty manner, in which this paragraph was drawn up is likewise proved by what is added at the conclusion of it, for it goes on to say, that in March 1687 the Principia was published, and that a copy of the book was put into the hands of Fatio de Duillier. Now in Professor Uylenbroek's collection [c] there is a letter from Fatio himself, dated, London, June 14–24, 1687, in which he speaks to Hugens of the Principia—" d'un livre de M. Newton, qui " s'imprime présentement et qui se débitera dans trois " semaines d'ici."

It is evident that the Principia is referred to in this place, because in the controversy with Leibnitz, an argument was thus derived from the analytical methods, which Newton used for investigating those propositions, which he afterwards reduced to another form [d]. These were the first great results, which came before the world, of the mighty effects of the new analysis, and they were prefaced by the Lemmata

[c] Hugenii aliorumque Exercitationes Mathematicæ et Philosophicæ, Fasc. ii. p. 99.

[d] Comm. Epist. (1722) p. 39. Leibnitz also says of Newton (Op. vol. iii. p. 363); Certe cum elementa calculi mei edidi anno 1684, ne constabat quidem mihi aliud de inventis ejus in hoc genere, quam quod ipse olim significaverat in literis, posse se tangentes invenire non sublatis irrationalibus, quod Hugenius quoque se posse mihi significavit postea, etsi cæterorum istius calculi adhuc expers: sed majora multa consecutum Newtonum, viso demum libro Principiorum ejus, satis intellexi.

of Prime and Ultimate ratios, which are the real basis of the infinitesimal calculus. But the committee does not appear to have been sufficiently impressed with the importance of the algebraical notation, by which the processes were to be generalized in their application. They would not, otherwise, have taken the line of reasoning, which involved them in the mistake now alluded to, nor would they have left occasion for John Bernoulli's erroneous statement, in his well known letter of June 1713, where he supposes that Fluxions were derived from Leibnitz's Differential method. He was mistaken, likewise, in his assertion that the "lettres pointées n'ont parus que dans le "3 volume des Œuvres de M. Wallis[e];" for they occur in the 2nd volume of that collection which was published six years sooner. This was noticed by the editors of the Commercium Epistolicum[f], but their statement, though true, is open to the objection of some obscurity, if not of apparent contradiction. They say that the earlier publication took place "annis uti-"que duobus antequam fama calculi differentialis ad "aures Wallisii pervenerunt." This assertion rests on a letter to Leibnitz, written in 1696, in which Wallis says, "Neque Calculi Differentialis vel nomen audi-"visse me memini, nisi postquam utrumque volumen "absolverint operæ, eratque præfationis (præfigendæ) "postremum folium sub prelo, ejusque typos jam po-"suerant typothetæ. Quippe tum me monuit amicus "quidam (horum rerum gnarus) qui peregre fuerat, "tum talem methodum in Belgio prædicari, tum illam "cum Newtoni methodo Fluxionum quasi coincidere.

[e] Des Maizeaux Recueil de divers pièces par MM. Leibniz, Clarke, Newton, &c. vol. ii. p. 38. or Leibnitii et Jo. Bernouilli Comm. Phil. et Mathemat. vol. ii. p. 309. and vol. i. p. 191.

[f] P. 248.

"Quod fecit ut (transmotis typis jam positis) id moni-"tum interseruerim[g]." Now the Vice-chancellor's imprimatur for the first volume is dated Oxon, Mar. 26, 1695, and that for the second, Aug. 23, 1693, which explains the "annis utique duobus;" but in the second volume, (p. 396,) near the end of the account of Fluxions, it is said, "Huic methodo affinis est metho-"dus differentialis Leibnitii." This variation from the other statement probably did not occur to the writers, and it certainly at the time escaped Wallis himself. There is in the Savilian library a copy of his works, in which he has inserted many additions and corrections. In this, against the passage just quoted from his letter, he has written, "ad Algebræ "meæ cap. 95. p. 396. lin. 15. fit mentio methodi diffe-"rentialis Leibnitii; sed ea sunt Newtoni verba; et "quorum ego non memineram neque sciebam quæ sit "ea methodus." Another manuscript memorandum, which he has written in the second volume, will make this still more clear. It is noticed in the Commercium Epistolicum, that Newton in 1692, at Wallis's request, sent him the first proposition of the book of Quadratures, with his examples of first, second, and third fluxions—"id quod cuivis videre est in Wallisii Ope-"rum Tomo secundo pag. 391, 392, 393, et 396[h]." From this it must have been clearly understood that Newton furnished the materials for what appears in the place referred to; but the truth goes still further; for Wallis has written against the 18th line of p. 390, "Quæ hic sequuntur sunt ipsius Newtoni verba, ab "ipso scripta, atque ad me missa, eo animo ut hic in-"serantur, sed quasi meo nomine, usque ad pag. 396. "lin. 19." Hence we see the motives of Newton's reserve. He does not appear to have hesitated in

g Wallisii Opera vol. iii. p. 654.

h P. 41.

complying with Wallis's request: he freely furnished him with the account of his method and of the notation which he adopted in the use of it: he did this with the express view of its being given to the public; but, although a formidable rival had already challenged the invention, he could not induce himself to communicate his own, unless under conditions which might exempt him from the danger of any personal controversy.

Newton told Oldenburg in 1676, with reference to what he had done in the higher Mathematics, "eo "tempore pestis ingruens, [quæ contigit annis 1665, "1666,] coegit me hinc fugere et alia cogitare[i];" and in his remarks on Leibnitz, addressed to the Abbé Conti in May 1716[k], he says, that he had many papers written in 1664, 1665, and 1666, among which was one dated 13 Nov. 1665, which contained the direct method of Fluxions. This is the same as is given by Horsley from Raphson[l], and in Lord Macclesfield's collection is an imperfect paper in Newton's handwriting containing a still fuller statement of it, with some additional dates, among which there is the following[m]. "In another leaf of the same waste-book the "same method is set down in other words, and fluxions "applied to their fluents are represented by pricked "letters. And this paper is dated May 20, 1665." It is clear therefore that Newton had invented his system of Fluxions, and devised his notation for it, some twenty years before he drew up his Principia; nor is

[i] Wallisii Opera vol. iii. p. 635. The date inserted between the brackets is printed in Italics, and possibly is an explanation supplied by the author.

[k] Des Maizeaux vol. ii. p. 89.

[l] Newtoni Opera vol. iv. p. 611. Newton saw the first sheets of Raphson's History of Fluxions, and was much dissatisfied with them. See Lord Teignmouth's Life of Sir Wm. Jones, p. 8.

[m] The whole is printed in the Appendix, II. p. 20.

this contradicted by the argument derived from its not being introduced in those parts of that work, where, thirty years afterwards, Bernoulli thought that the use of them would have been advantageous.

Newton's well known predilection for the geometry of the ancients has occasioned it to be thought, that, in his treatment of the subject, allowance was to be made for the author's taste, without looking too closely to the judgment with which he indulged it. It may, however, be fairly contended that this is more than can in justice be conceded. If he had printed his conclusions, as worked out by the infinitesimal calculus, he would have greatly increased the difficulty which those had to encounter, who endeavoured to study the book. Science had made those advances, which in some measure prepared the minds of men for his discoveries, but still his gigantic strides, both in mathematics and physics, left others far behind him. If the Principia had rested upon analytical demonstrations, Newton must not only have previously published an elementary treatise of Fluxions, but must have allowed time for this new branch of science to become familiar to his readers; otherwise the conclusions to be established in this manner could not to any purpose have been laid before them.

For the developement of the fundamental principles of motion in particular curves, it was more perspicuous to place diagrams before the eye, than to argue from the equations that belonged to them. But when he had reduced a proposition to the quadrature, for example, of a curve, he had completed what he considered as strictly belonging to the general Principles of Natural Philosophy; he had brought his conclusion into a tangible form, and had attained a restingplace for the minds of his readers. To those few, who were then able to proceed to the numerical determinations, there could be no difficulty in translating the geometrical results into algebraical formulæ. Many, however, at first were able to comprehend the great mechanical truths of the work, without attempting to follow out the particular applications of them, and to this class of readers the geometrical expressions were more satisfactory than a set of artificial symbols.

The Principia was not a protracted compilation from memoranda, which might have been written down under the impression of different trains of thought: it had the incalculable advantage of being composed by one continued effort, during which the mutual bearing of all the several parts was vividly present to the author's mind. His letter to Aston shews he was not in the first instance prepared with any ample details[n]. Pemberton[o] says that the work was composed from scarce any materials than the few propositions, which had been mentioned to have been drawn up at the time of the controversy with Hooke in 1679; and in a certain sense this may be admitted; but there was the subsequent Specimen which was sent to the Royal Society, for the insertion of which, in the Register, he returns his thanks to Aston. This was certainly much more than had been previously demonstrated: indeed he says that it had taken up a longer time than he expected. He adds, that part of it had been spent "to "no purpose;" but this labour had, likewise, enabled him to establish more[p] than he then brought forward,

[n] App. III. p. 24.

[o] App. XII. p. 51.

[p] There is incidentally a strong proof of this. At the end of the first Problem (App. p. 5.) he mentions that the central force for a body, moving in the logarithmic spiral, will be reciprocally in the triplicate ratio of the polar ordinates, a proposition of which he gives in this place no proof, but which he afterwards brought forward in a complete form as the ninth proposi-

and probably to see, not only the general truths which he had in view, but the means by which he was to arrive at them. The tract may have lost some of its first value from its contents being immediately expanded into the greater work; and the neglect which it thence incurred, produced in subsequent generations a general inattention to it. The impression may have been taken of its consisting merely of short enunciations of general properties, and little curiosity seems of late years to have been felt about it; but this is by no means right; it affords no trifling addition to our stock of historical knowledge, and has therefore been inserted in the Appendix[q]. When we view this paper as the foundation, on which the explanation was to be raised of the system of the whole universe, it cannot but possess the greatest value and interest. If it were possible to divest our minds of the recollection of what was afterwards accomplished, we then, and only then, should be able to form some idea of the admiration, with which this first communication must have been received.

The paper begins by certain definitions, hypotheses, and lemmata, and then lays down three theorems: the 1st proves, exactly in the same manner as is done in the first proposition of the Principia, the equable de-

tion of the first book of his Principia.

[q] No. I. p. 1. The person who transcribed the paper into the Register of the Royal Society, unfortunately could not have understood what he was employed to copy. The divisions of the sentences are not well attended to, words are sometimes wrongly spelled, and the diagrams, as well as the references to them in the text, are in several instances very faulty. Care has been taken to correct as many of these oversights as possible, but no alteration has been introduced where the least doubt could exist of the necessary correction. Jones had a transcript of these propositions, which is now in the possession of the Earl of Macclesfield, but the original autograph has not been discovered.

scription of areas by the radius vector of a body acted upon by a central force. As the whole was intended for a short exposition of the system, which was to be traced out, it does not give the converse of the propositions; and only has the "vice versa" in the third corollary to the second theorem. This second theorem gives the fundamental property of the motions of bodies, in the circumferences of circles, when acted upon by forces directed to their centres. It is demonstrated without any diagram in the fourth proposition of the second and third editions, and the figure is drawn for it in the first, as if the two circles had a common centre. Two different centres are given in Newton's earlier paper, which answer better to the general demonstration; but the form adopted in the Principia seems to have had its origin in the use, which had been made of this proposition for the planetary orbits, or for the comparison of the deflection of the moon from its tangent with the fall of bodies on the earth's surface. The third theorem investigates the general formula to express the ratio of a centripetal force in any curve. This is the same as that, which is determined in the sixth proposition of the Principia; but it is not extended to the expression, which is derived from the perpendicular drawn from the centre to the tangent. This was an addition made in the second edition, but it is remarkable in the first, that SY occurs in the diagram for the motions in the ellipse, (Prop. 10, 11,) although no use whatever is made of it in the demonstrations.

These general theorems are then applied to as many problems. The 1st determines the central force in a circle to any point on the circumference, which is the same with the 7th proposition of the Principia in the first edition. The problem in the 2nd and 3rd editions was made general for any point, of which the position was given. The other two problems are for motion in an ellipse when the force is at the centre or at the focus. These are both demonstrated from the same diagram; but that was the case in the Principia itself, till the obvious improvement was made in the third edition, of annexing a distinct figure to each. The SY likewise was then omitted.

A fourth theorem is afterwards introduced, answering to the 15th proposition of the Principia, to prove that, when the central force varies inversely as the squares of the distances, the square of the periodic time in an ellipsis is proportionate to the cube of the transverse axis. Having established this great truth, he shortly shews how his system is applicable to the planets; and then adds a fourth problem to determine, as in the 17th proposition of the Principia, the path in which a body will move, when it is projected with a given velocity and direction from a given place, and is acted upon by a force, which varies inversely as the squares of the distances from a given centre. There is a little error in the enunciation of this problem, since it assumes the path to be an ellipse, but this is merely a casual oversight; for it is afterwards shewn, that the nature of the conic section will depend upon the particular conditions of each case.

These four theorems and four problems evidently embrace the principal conclusions which are established in the second and third sections of the first book of the Principia; and, with the exception of the fourth theorem, (in which the Q and R are transposed), the letters P, Q, S, &c. are applied throughout to the same points, to which they have since been uniformly and specifically attached. He then goes on to say, that motions, in this way defined for planetary orbits, may enable us to ascertain whether the same comet may have repeatedly returned to us. His meaning in this place is rather obscure. He certainly at the time[r] had not resolved the difficult question of the paths of comets. In the[s] Arithmetica Universalis he had proceeded on their supposed uniform rectilinear motion, and in the present case he still holds expressly to that earlier theory. How, under such conditions, (if strictly adhered to,) they could return is not easy to understand; but waving this question, his reasoning seems to shew, that, if they did, they might be recognised by a similarity in their motions. To determine this he proposes to reduce the places of the comet to analogous points in an imaginary ellipse, of which the focus is occupied by the sun, and these places having been calculated by the means of the auxiliary curve, were to be verified by their application to the rectilinear path. It seems wonderful, when we consider his extraordinary acuteness, that such an hypothesis did not immediately lead him to the truth; but as he so repeatedly and so distinctly describes the supposed motion of the comet to be in a straight line, it is impossible not to conclude, that even his most powerful mind required the assistance of time to emancipate itself from preconceived opinions.

The remainder of the paper is employed upon projectiles. The fifth Problem is to determine the space through which a body will fall towards the centre of attraction when the force varies like that of gravity. It is the same with the 32d Proposition of the Principia, and is illustrated, not with the complex dia-

[r] App. V. p. 29.

[s] Prob. LVI. As in that problem he derives his calculation from four observations, and does not, as in the Principia, (lib. 3. prop. 41,) deduce his trajectory from three observed places.

gram, annexed to it in the first and second editions, but with that of the more simple description, which occurs in the third. This proposition would enable us to calculate the fall of bodies to the earth, if no account is taken of the air's resistance; and he then goes on, in Problems 6 and 7, to determine the effect of resisting media on the rise or fall of bodies, in the same manner as in the Cor. to Prop. 2, and in Prop. 3 of the second book of the Principia. The 4th Proposition of that same book treats of the motion of bodies projected obliquely through a medium of a given resistance: and the paper concludes with a scholium which is analogous to it.

These four theorems and seven problems would seem to answer in number to "near a dozen propositions" of which Dr. Pemberton speaks. He says indeed that they were "relating to the motion of the "primary planets about the sun;" but he might express himself in this manner from general recollection, so as to describe the whole from what strictly belonged only to the most important part. There is, however, a stronger reason against supposing them to be the same; because he distinctly speaks of what was done in 1679, and these were clearly drawn up at a later period [t].

Newton concluded the letter, which he wrote to Aston in Feb. 1685, with reference to his book, by saying, "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 [u]." He can, therefore, be

[t] App. XII. p. 51. He, likewise, expressly says that they had been completed several years before Halley's visit to Cambridge. And it is evident from the letter to Aston, that what was sent to the Royal Society in the beginning of 1685 was much more elaborate than what had been written in 1679.

[u] App. III. p. 24.

hardly considered to have engaged seriously in the work until April of that year; and on the 21st of that same month, in 1686, Halley read a paper to the Royal Society, on the affections and properties of gravity, in which, after speaking of the truths established by Galileo, Torricelli, Hugens, and others, he mentions those "now lately discovered by our worthy country-"man, Mr. Isaac Newton, who has an incomparable "Treatise of Motion almost ready for the press[u]." This was not anticipating too much; for on the following Thursday, (April 28,) "Dr. Vincent presented the So-"ciety with a manuscript treatise, entitled, Philosophiæ "Naturalis Principia Mathematica, and dedicated to "the Royal Society, by Mr. Isaac Newton." This was only the first book; but such was the confident admiration with which it was received, that an order was immediately given for a letter of thanks to be written to the author; and a resolution was at the same time made, that the printing the book should be referred to the consideration of the Council; the work in the mean time being left in Halley's hands, that he might report to them upon it[x].

Of the tenor of that report there could be no doubt: no advance, however, was made in the publication. At

[u] Phil. Trans. vol. XVI. p. 6.

[x] Birch's Hist. of Royal Society, vol. IV. p. 480. In Mr. Turnor's Hist. of Grantham, mention is made of a lady, to whom Newton in early life was attached, and who married a gentleman of the name of Vincent. Whether this is the same person is uncertain; but in the Cambridge graduate book we find, "Vincent Nath. Clar. Aul. "A. M. 1660. S. T. P. per lite-"ras regias 1679;" he became F. R. S. in 1683, and it appears, from the minutes of the Council, that he was readmitted in 1694, after having withdrawn himself about 1688. March 25, 1691, it was "ordered that a "letter be drawn up by E. Hal-"ley, and presented to the pre-"sident, to be signed by him, "and sent to Dr. N. Vincent, "in answer to a proposition "made by him to the Society in "relation to his will." The result is not known.

another meeting of the Society on the 19th of May, there was some regret exhibited for this delay, since it was ordered, that the work should "be printed forthwith in a quarto, of a fair letter, and that a letter should be written to him, [Newton,] to signify the Society's resolution, and to desire his opinion as to the print, volume, cuts, and so forth." The Biographia Britannica[y] mentions these circumstances, but takes no notice of any further proceedings on the subject, which has left a common persuasion of the Principia having been printed at the expense of the Royal Society. M. Biot[z] evidently understood this to be the case, and Sir David Brewster[a] has taken the same impression; but this appears to be a mistake. The Council met on the 2nd of June, when they were very far from adopting the wishes of the general meeting. They ordered indeed, that "Mr. Newton's book be printed," but they added, "that E. Halley shall undertake the business of looking after it, and printing it at his own charge, which he engaged to do."

There is some mystery in this part of the business. Halley, in the letter, which, according to the directions of the Society, he wrote on the 22nd of May, 1686, apologizes to Newton for the delay which had occurred in bringing the business before the Council. There is indeed no minute of any one having been assembled between the 21st of April and the 2nd of June; and this interval elapsed, as he alleges, "by reason of the President's attendance on the King, and the absence of the Vice-presidents, whom the good weather has drawn out of town[b]." Now Pepys,

[y] Vol. v. p. 3225.

[z] Biographie Univ. vol. xxxi. p. 157; and note (2), p. 160. The first notice is inserted in the English Translation, p. 19; the second is omitted.

[a] Life of Newton, p. 157.

[b] App. IV. p. 25.

who was president at the time, might have been occupied by the business of the office, which he held under government; but on the 5th of May, Dr. Gale, V. P. was in the chair at the general meeting of the Society, and on the 19th Sir Joseph Williamson, another V. P., was in the same seat. It appears, therefore, that the real cause, which prevented the meeting of the Council, was not exactly what was professed; and it is probably to be found in their being unwilling to come to any resolution about a publication, which they were not prepared to undertake. Halley, however, stood forward, and relieved them from their difficulty. By the constitution of the Royal Society, the vote of the 19th of May was of no force in itself, and could not be carried into execution, unless it was ratified by the Council, with whom rested the entire management of the pecuniary concerns[c]. Now it is clear that they did not ratify it to the full extent on the 2nd of June; and, from a careful and repeated examination made for this purpose, of the minutes from 1686 to 1699, (inclusive,) I can venture to say, that there is no allusion in them to any sum of money, that was ordered to be applied to this object, or to any thing, which can lead to a supposition of funds being appropriated to it. Nay, more—Assistance was sometimes given to the secretaries, by the purchase of copies of the Transactions that they printed; and the publication of works, if not wholly undertaken, was promoted by partial subscriptions; but there is no notice of any pecuniary aid, even of this kind, having been extended to the Principia.

The backwardness of the Council on this occasion

[c] No sum exceeding 5*l.* could by the statutes be paid, without an order from the Council.

is not to be attributed to the work being as yet incomplete, for they approved of its going immediately to the press. No unwillingness can be supposed to have arisen from a wish to avoid making the body at large responsible for what, in this manner, might be sent out to the world. The charter, by giving the power of appointing their own printer, and engraver, must have intended the employment of them, and the Society from its first establishment repeatedly patronised and adopted the works of different authors. In 1686 "Fr. Willughbeii Arm. de historia piscium "libri quatuor," were published "jussu et sumptibus "societatis regiæ Londinensis," and this seems to have deprived them of the funds, which would otherwise have been available for the present purpose. A great number of the plates in Willughby's book were contributed by individuals [d], but still the Society itself expended several hundred pounds upon it, and was in great difficulty to dispose of the copies—even in 1740, no less than 125 complete sets were still remaining in the warehouse. Their finances were thus for the time completely crippled; they were obliged to sell property, which they had in the India stocks, and were in arrears to their officers, so as even to pay them in copies of this very book, which were to be converted by themselves, as they could, into money. It seems indeed to have been long before they recovered themselves; for in 1697 measures were taken to publish Malpighi's Posthumous Works, but they came out after all "Impensis A. and J. Churchill [e]."

[d] Pepys was at the expense of no less than 79 or 80 of them.

[e] The dealings of the Society with these publishers will account for Sir Isaac Newton having recourse to them, for the edition of Flamsteed's Historia Cœlestis, which was sent to the press in 1705. He might naturally have been unwilling to do

In this state of things it seems impossible for them to have undertaken any further expenses in 1686, and the Principia appeared in the following year "jussu" (but not sumptibus) "Societatis Regiæ." The minute of the 2nd of June is entered in Halley's own handwriting, and there can remain no doubt of its meaning. The expression used in it must be understood in the same sense, as that in which he announced to Newton, on the 22nd of May, that the Society had determined to print his work "at their own charge[f]."

Under these circumstances it is hardly possible to form a sufficient estimate of the immense obligation which the world owes in this respect to Halley, without whose great zeal, able management, unwearied perseverance, scientific attainments, and disinterested generosity, the Principia might never have been published. Every one of these qualities was required for the success of the undertaking. Newton printed nothing of any importance for himself, and, if not urged to it, would probably never have drawn up this great work; or after it had been drawn up he would, if left to himself, have suppressed a considerable portion of it. It likewise was not a treatise, which, if it had failed of immediately finding an editor, might after a time have come out under other auspices: all but the first book was finished for the press while the printing was going on, and even during that time the earlier part received corrections and alterations. The booksellers of the day were very unwilling to incur the risk of publishing mathematical works[g], and the pecu-

business in a different way, from that to which he was accustomed; although Flamsteed was disappointed at it, and indignant at the employment of these "undertakers." See Mr. Baily's Account of Flamsteed p. 78, &c.

[f] App. III. p. 25.

[g] Lord Macclesfield has a letter, in which Collins tells Newton "our Latin booksellers here "[in London] are averse to the

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niary risk in the present case must have been considerable; but Halley nobly met it, though he had to provide for the disbursement, precisely at that period of his life, when he could least afford it. He had no professional source of income, and in early life depended on his father, who was then a man in affluent circumstances. In 1682 he[h] married, and soon had a family rising round him; but his father died not long afterwards, having wasted his fortune almost to nothing; and Halley was interrupted in his scientific pursuits by the necessity of looking after the wrecks of his patrimony[i]. The outlay on the Principia was therefore a most serious charge upon him; for although we know that the whole was eventually disposed of, still a very small number of purchasers could at first be expected, and we have no account of the copies which he gave away to his friends. There is no notice of the publication in the records of the Stationers' Company[k], and the extent of the edition is not known, but it probably was small. In 1692, when the reputation of the work was established, and Hu-

"printing of mathematical books, "there being scarce any of them "that have foreign correspondence for vent, and so when "such a copy is offered, instead "of rewarding the author, they "rather expect a dowry with "the treatise.". . . . "The Royal "Society gave five pounds with "the copy [of Horrox's Opera "Posthuma] to encourage a "bookseller, whereas scarce any "were willing to undertake it."

h Biographia Britannica, vol. iv. p. 2500.

i Ibid. p. 2503. and particularly note [R]. In a letter written by Flamsteed to Bernard in 1678, (and which is printed by Mr. Baily), he speaks of Halley's "friends being wealthy;" and Aubrey also especially says, he was the "son of a wealthy "citizen of London." The father died in 1684, since letters of administration for his estate were granted on the 30th of June in that year to Sir John Buckworth and Richard Young, "in usum "et beneficium Joannæ Halley "relictæ dicti defuncti, et Ed-"mundi Halley filii dicti de-"functi."

k The privileges of the Royal Society possibly made it unnecessary to enter the publication in their books.

gens[l] was anxious for a second edition, he was of opinion that "200 exemplaires suffiroient." Neither did the price make up the deficiency; for Sir Wm. Browne[m] speaks of its not having been more than ten or twelve shillings.

But Halley had a considerable difficulty to surmount, even before he entered on the task which he had undertaken. In his official letter he felt bound to apprize Newton of the conduct of Hooke[n], who, when the manuscript was presented by Dr. Vincent in April 1686, claimed to have first discovered the law of the inverse squares, and to have communicated it, with a statement of other parts of the discoveries, to Newton.

To understand this claim, as well as Newton's answer to it, we must go back to the correspondence which took place between them in 1679. Unfortunately we have not the letters themselves, and some conjectures will be necessary to make out the bearing of his remarks, from the short notices which Hooke communicated to the Royal Society[o]. Something, however, of a connected narrative may be traced, and those parts being noted which do not depend upon direct evidence, may be hereafter corrected, if the original documents should ever be discovered.

Hooke published, in 1674, his "Attempt to prove the "motion of the Earth," which he had previously read to the Royal Society at Gresham College, and he closes it with an account of his system of the world. This, he says, depends upon three suppositions. "1st, "That all celestial bodies whatsoever, have an attrac-

l Hugenii aliorumque exercitationes math. et phil. a Uylenbroek; fasc. ii. p. 125.

m Nichols's Literary Anecdotes vol. iii. p. 322. Search has been made, but without success, for the original tract from which he quotes.

n App. IV. p. 25.

o Birch, Hist. of Roy. Soc. vol. iii. p. 512, 516. iv. 1.

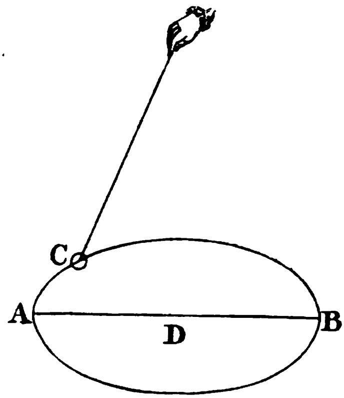
" tion or gravitating power towards their own centres, " whereby they attract not only their own parts, and " keep them from flying from them, as we may observe " the earth to do, but that they do also attract all the " other celestial bodies that are within the sphere of " their activity....2nd, That all bodies whatsoever that " are put into a direct simple motion, will so continue " to move forward in a straight line till they are, by " some other effectual powers, deflected and bent into " a motion describing a circle, ellipsis, or some other " more compound curve. 3rd, That these attractive " powers are so much the more powerful in operating, " by how much the nearer the body wrought upon is " to their own centres. Now what these several de- " grees are I have not yet experimentally verified."

In his book de Cometa, published in 1678, he speaks not only distinctly of a " kind of gravitation by which " the planets are attracted and have a tendency to- " wards the sun as terrestrial bodies have towards " the centre of the earth[p]," but considers comets as probably acted upon by the same force and moving by the effect of it in curvilinear paths[q].

At a much earlier period indeed he had adopted the idea of orbits, returning into themselves, and produced by the combination of a central with a projectile force. This he laid before the Royal Society in the year 1666, on the 23rd of May[r]; and in support of his theory he then exhibited the experiment of a pendulous body, to which a tangential force was so applied as to make it swing round in an oval. It is remarkable that this very illustration, which had occurred to Horrox, (who died in 1640) is described in his Opera Posthuma[s].

[p] P. 31. [q] P. 44. [r] Birch's History of the Royal Society, vol. ii. p. 91. [s] P. 312.

He says that a pendulum left to the effect of its own gravity will oscillate in a plane, "si vero moveatur "manus, quæ filum sustinet in circuitu, describet "grave pendulum figuram "ovalem, ut ACB. Ita tamen (quod oppido notandum est) ut rectæ AB "apsides (ut dicam) A, B, "continue mutentur, versus "eas partes movendo ad "quas movetur pendulum "grave, sed multum tardius. Ut si, verbi gratia, in una revolutione, maxima "a centro D distantia sit in A, erit in proxima revolutione in C. Eritque hic motus eo celerior, prout "figura magis a circulo ad ovalem deflectit; modo non "nimis oblongetur, ita ut curvatura fuerit valde exigua, tum enim ægrius omnino mutantur apsides."



"Est autem natura unica, omniaque inter se consensum habent et harmoniam. Cum itaque planetarum motus, tum quoad figuram orbium, tum quoad "motum aphelii, imitentur hunc penduli motum, quidni "et similis fuerit utriusque causa?"

Horrox's papers were known to the members of the Royal Society as early as Feb. 1664[t], but they afterwards were in Dr. Wallis's hands at Oxford, and Hooke's omission of any allusion to the motion of the apsides[u] seems to shew that he was not acquainted, at least, with this part of their contents. He likewise improved the practical form of the experiment. He attached a large ball of lignum vitæ by a long wire to

t Birch's Hist. of the R. S. vol. i. p. 386, 412.

u See this effect fully discussed by Sir J. Herschell, Astronomy art. 570.

the roof of the room, which was much better than holding the line by the hand, and he added a smaller ball, which he made to swing round the first by means of a short string, that was fastened to the wire; he then remarked "that neither the bigger ball which re-"presented the earth, nor the less which represented "the moon, were moved in so perfect a circle or ellip-"sis as otherwise they would have moved in, if either "of them had been suspended and moved singly; but "that a certain point, which seemed to be the centre "of gravity of these two bodies, however posited, (con-"sidered as one,) seemed to be regularly moved in such "a circle or ellipsis, the two balls having other pecu-"liar motions in small epicycles about the same point[x]." This last observation reminds us of the fundamental property of motion in the eleventh section of the 1st book of the Principia; but that was much more than Hooke could have demonstrated; and indeed his experiment, though it answered the immediate purpose of illustrating his hypothesis, admitted no further reasoning to be derived from it to the motion of the planets. He was aware that it did not represent the true action of gravitation, because the tendency towards the middle (there was no fixed point to which the body was constantly drawn) increased with the horizontal distance, which he knew to be wrong, although he gave no indication, at the time, of his knowing what the law really was, by which the variation of gravity was regulated.

These suggestions were highly creditable to Hooke's ingenuity; but they contain no notice of the specific law, by which gravitation is governed, and this would hardly have been omitted, if he had been really master of it. Bailly, in his History of Modern Astronomy[y],

[x] Birch, vol. ii. p. 92. [y] Vol. ii. p. 475.

says that Hooke, feeling himself in want of geometrical assistance, applied to Newton for the solution; but it is clear that his conduct did not originate in any motive so candid and ingenuous. In 1813 a collection of letters was printed from originals in the Bodleian, with some biographical notices, taken from the manuscripts of J. Aubrey in the Ashmolean Museum. Among these last was a letter from him to Wood on the claims of Hooke to the discoveries, which were said to pass for Newton's. Aubrey's own authority on such a point is not great, and the letter therefore, as it is printed, is not only obscure, but deprived of its real value. The editor indeed has given a long passage, which he states to be taken from what is in Hooke's handwriting; but he has inserted it in the wrong place, and in such a manner as to make it appear to be merely a loose paper, which Aubrey had sent to his correspondent. By a reference, however, to the original, it was found that the letter must have been written under Hooke's immediate direction; it is not only corrected and altered by him in many places, but by far the greater part consists of additions, which he has made with his own hand[z]. The substance really is his, and lets us into much of his views and feelings. It is clear from other letters in the Bodleian, of which extracts may be seen in the Appendix[a], that Hooke had been incessantly urging Aubrey to procure some notice of him by Wood. What may have been the cause of his disappointment in this respect is uncertain; even this last letter had no influence on the historian, and the contents of it might now have been lost, but for the rough copy which was left by the writers of it. It assists us, however, in tracing the order of some of the particulars, to which Hooke alludes for the establishment of his claim on

[z] See App. XIII. p. 52. [a] App. XIV. p. 56.

having furnished Newton with the foundation of his system.

At a meeting of the Royal Society on the 4th of Dec. 1679, "Mr. Hooke produced and read a letter "of Mr. Newton to himself, dated 28th November "1679, containing his sentiments of Mons. Malle-"mont's new hypothesis of the heavens; and also sug-"gesting 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 height, "which, he alleged must fall to the eastward of the "perpendicular, if the earth moved[b]." Mr. Biot[c], with reference to this passage, considers that Newton's letter was occasioned by the Royal Society having applied to him for an opinion on the book, which is here mentioned. No express authority has been found for this supposition, but it is by no means improbable, for on the 1st Jan. 1680[d] Hooke read another letter of Mr. Balle giving his thoughts on the same hypothesis. Besides, the work does not appear to have been one[e], which was likely in itself to occupy Newton's attention, much less to lead him of his own accord to offer an opinion of it. It must have been the Nouveau Systême du monde inventé par M. Mallemont de Messanges, published in 1679, of which a copy had been immediately sent to the Royal Society. Hooke also wrote to Mallemont in Dec. 1679, acknowledging the

[b] Birch, vol. iii. p. 512.

[c] Biog. Univ. vol. xxxi. p. 149. Eng. Trans. p. 14.

[d] Birch, vol. iv. p. 1.

[e] In the Phil. Trans. vol. xvi. p. 245, there is a review of another work of this same author, in which he is said to be "one "of those unhappy geometri-"cians, who, without having ac-"quired a thorough understand-"ing of the principles, have yet "thought themselves able to "master the abstrusest difficul-"ties." The review, though printed anonymously, was certainly written by Halley; for the original, in his handwriting, is still preserved by the Royal Society.

receipt of his work, and his letter, of which a copy is preserved, bears indications of containing the mild but cautious language, which Newton would have used on such an occasion[f]. The author imagines that the sun, with Mercury as a satellite, moves round a common centre with the rest of the planets. The sun being nearest to this central point, is with the rest carried on by a vortex, so as to complete its revolution in little more than six months. Lalande[g] might therefore well think that it was some praise, when he speaks of the same writer's dissertation on comets, and says, "il n'est pas aussi absurde que de coutume."

Hooke had controverted Newton's views of Optics as soon as they were published, and he seems to have taken this opportunity of trying his strength on another subject; for in reference to this correspondence he says, in the letter which was to pass for Aubrey's, that he wrote to Newton, "to make a demonstration of "this theory," [in the Attempt to prove the Motion of the Earth,] "not[h] telling him at first the proportion "of the gravity to the distance, nor what was the "curved line that was thereby made." Mr. Newton, he also adds, in his answer, "did express that he had "not thought of it; and in his first attempt about it, "he calculated the curve by supposing the attraction "to be the same at all distances." Newton acknowledged that he "had laid Philosophy aside[i]," which in some measure corroborates the former part of this statement, but he adds that his answer was chiefly about projectiles, in which the variation of the force of gravity not being an element of his calculation, the neglect of it did not shew his ignorance of the fact.

[f] After giving the hypothesis credit for ingenuity, it says, "mais la difficulté est de scavoir "si elle s'accordera avec les "phænomenes."

[g] Bibliog. Astron. p. 302.

[h] App. XIII. p. 52.

[i] App. V. p. 27.

The interruption thus given to his other studies was painful to Newton; but while he refused to enter further into a correspondence, his kind feelings induced him to send an "experiment of projectiles, (rather "shortly hinted than carefully described,) in compli-"ment to sweeten" his answer. This most probably is what is mentioned in the latter part of the minute of the 4th of December, 1679.

It was an old objection, that if the earth was in a state of rotation, its surface would pass on from under any thing which was projected upwards, and which would thus be left behind, in its fall, to the westward. The fallacy of this argument had been pointed out, but Newton saw how this same experiment, with a little variation, might be actually made to prove the contrary of that, for which it had been originally designed. As the velocity of rotation is greater at greater distances from the axis, he argued that a body let fall from a great height ought to pass to the eastward of the place perpendicularly under it. This suggestion arrested the attention of the Royal Society, and Hooke, who had been directed to try the experiment, reported, on the 22nd Jan. 1680[j], that it had succeeded. On the 11th of Dec. 1679[k], he had explained that the body in its fall would be carried towards the south as well as towards the east, and this he described as having occurred in actual trial; but at the same time it is to be acknowledged that the coincidence must have been the consequence of accidental circumstances, since the fall being only 27 feet, was too small to produce a deviation, which he could have appreciated. Thomas Simpson examined the question in his Mathematical Dissertations[l], and D'Alembert thought the subject of

[j] Birch, Hist. of Roy. Soc. vol. iv. p. 5.

[k] Ibid. vol. iii. p. 11.

[l] (1743) p. 160.

sufficient interest to compose a memoir on it, which was read in 1771 before the Academy of Sciences at Paris, when the Margrave and Margravine of Baden Dourlac attended their session on the 6th of September[m]. At more recent periods the experiment has been tried with success by Gulielmini, Henzenberg, and Flaugergues, from stations of very great height; but still there are so many difficulties attending it, that Delambre[n] considers it as by no means conclusive. It is found indeed by mathematicians that the deviation to the south is at most exceedingly small for any case, in which it can be actually tried. Newton, however, did not deny the possibility of it, and honestly admitted that another part of his statement was even founded in mistake. If the earth were at rest, an heavy body would fall in a straight line towards its centre, but if the motion communicated by the rotation acts as a projectile force, the combination of it with gravity would, he at first hastily stated, make the body descend in a spiral. M. Biot says that he came to this conclusion, "sans doute parcequ'il supposoit la chute "opérée dans un milieu résistant, comme l'air[o]," which at first sight seems very probable, especially when we consider what is laid down in the fourth section of the second book of the Principia; but Newton says nothing to confirm this conjecture: on the contrary, he rests his defence upon being engaged on other pursuits, so that I "thought no further of philosophical matters "than his letters put me upon it, and therefore may "be allowed not to have had my thoughts of that "kind about me so well at that time." Indeed the proposition was probably not communicated as the re-

[m] Hist. de l'Académie 1771 p. 10.

[n] Astronomie, vol. ii. p. 192.

[o] Biographie Un. vol. xxxi. p. 149. Eng. Trans. p. 14.

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sult of his own speculations, but as it casually occurred to his recollection in the act of writing. He had not then demonstrated the law of central forces, and the early impression of their effect seems very generally to have been, that revolving bodies might ultimately fall down to the point, to which they were attracted. This naturally led to the idea of a spiral path. Leonardo da Vinci[p] had in consequence assigned it to them, and it had been maintained nearer to Newton's own time, and under circumstances which were more likely to be present to his thoughts. There had been a controversy, in which the arguments derived by Riccioli, from falling bodies, against the earth's motion, were answered by Stephanus de Angelis. An account of what passed between them was drawn up by James Gregory, and published in the second volume of the Philosophical Transactions[q]. Mention is made, at the end, of the experiment tried by Descartes of discharging cannon balls perpendicularly in the air, and of their falling back to the surface more frequently to the west than to the east. This experiment, possibly too difficult of accurate execution to be at all relied upon, was brought forward in confirmation of the earth's motion, and it may have suggested that which Newton recommended for the same purpose. As S. de Angelis had argued against the false position of his antagonist, his views may have been received with the more favour. Now he assumed the spiral motion, although it is not noticed by Gregory, for in the Preface to his tract De infinitis spiralibus inversis, (Pat. 1667) he says, "Jam anni "quadrans evolavit, ex quo discursus quidam nostri "Italice conscripti prodiere; in quibus rationes ali- "quot physico-mathematicas contra Coperniceum sys-

[p] Venturi Essai sur les ouvrages physico-mathématiques de Leonard de Vinci, p. 7.

[q] P. 693.

"tema a viro eximio Ricciolio excogitatas, ad tru-
"tinam revocavimus. Affirmabamus tunc bina ge-
"nera infinitarum spiralium a nobis fuisse contem-
"plata; spatiaque ab ipsis clausa geometrice mensu-
"rata; quarum una illa foret, quæ a gravi naturaliter
"cadente in plano æquatoris describeretur, si Tellus,
"ex falsa hypothesi, motu diurno duntaxat moveretur,
"et grave deorsum latum taliter suum concitaret mo-
"tum, ut spatia, ab ipso peracta, forent ad invicem
"ut ipsa quadrata temporum." See also p. 17; and Cartesii Epist. Part. prim. p. 162. 173.

The different manner, in which Newton and Hooke contemplated the great law of the inverse ratio of the squares of the distances, is not only of importance to the right understanding of this part of the history, but is highly characteristic of the conduct of these two great men in their scientific pursuits. There can be no doubt about the application of it to the planetary motions of the solar system. Halley saw [r] that it followed from "the sesquialter proportion," and Newton [s] says, that "he gathered it from Kepler's theorem." His dates, as we shall see, will carry us back for this to the time, when he made his first speculations on the effects of terrestrial gravity. The great question (as we have already mentioned) which then presented itself, was the probable identity of this force with that which regulated the motions of the solar system. Newton,

[r] App. VI. p. 36.

[s] App. VII. p. 40. Among Dr. David Gregory's papers there is a memorandum that "Mr. "Newton pursued the motion "of the planets first on suppo- "sition of an equal gravity, "but afterwards wrote to Mr. "Hooke, asking what he sus- "pected the law of gravity to "be." That Newton, before 1666, may at first have entertained such an idea, as is here described, of gravitation is very possible; but it is most evident from Hooke's own statement, that his letter in 1679 originated with himself, and was no reply to any application which Newton had made to him.

with his usual caution, appealed to facts; and when he found a discordance between them and his hypothesis, he so far abandoned it, as to conclude, not indeed that there was no terrestrial gravitation, similar in its nature and variation to that, by which the planets are attracted to the sun, but "that some other cause "must at least join with the action of the power of "gravity on the moon [t]." It is true that this inaccurate view was the consequence of his computing from erroneous data, but he had drawn his conclusion strictly from them, and he abided by what he believed to be truth. The line of argument, which he adopted, supplies a confirmation of his not having yet made use of Picard's measures for the correction of his first calculations. If he had himself "experimentally verified" his hypothesis, he would have hardly failed to mention it, when he repeatedly pointed out Hooke's deficiency in this respect; and he distinctly says [u], that "before a certain demonstration" which he found in 1685, he had suspected that the law of the inverse squares "did not reach accurately enough "down so low" as "the superficies of the earth;" "and "therefore in the doctrine of projectiles never used it "nor considered the motion of the heavens." However this may be, he clearly followed his own precise rule of philosophizing, "propositiones ex phænomenis "per inductionem collectæ, non obstantibus contrariis "hypothesibus, pro veris aut accurate aut quam proxime haberi debent, donec alia occurrerint phænomena, "per quæ aut accuratiores reddantur aut exceptionibus "obnoxiæ."

the corres-
lence with
oke

[t] App. XII. p. 50. Whiston (Memoirs, vol. i. p. 33.) says, that he imagined this other cause was produced by "Cartesius's "vortices."

[u] App. V. p. 27.

Hooke's process was much less strict; and although it enabled him to take one great step towards the truth, did not put him in possession of it, either completely or with certainty. He saw the analogy between the moon's motions and that of the primary planets, and accordingly extended the law of the inverse squares to terrestrial gravitation, without knowing that the order of effect varied at the surface of the earth [x]. He had left the verification of his theory in 1674 to his readers [y], which made Newton very justly complain [z] that he should claim the honour of discovery for himself, and throw upon others the drudgery of ascertaining whether after all it was applicable to the truth. It is probable that he had only adopted the precise law of variation, when he wrote to Newton in 1679. Hooke satisfied himself "that the "proportion was duplicate quam proxime at great dis-"tances from the centre, and only guessed it to be so "accurately [a]," but without Newton's demonstrations, to which he was a stranger, he had advanced no further than to an imperfect though beautiful conjecture. It may likewise be added, that he had not that clear and steady view of his own first principles which, if he had attempted it, would have enabled him to arrive at more certain conclusions. In a lecture which he delivered as late as the 25th of May, 1687, he says, "I "know that if the gravitating power in the sun and "moon be exactly the same with that of the earth, "the query I propounded can have no ground; but "though they may in most particulars be consonant, "as I shall prove in my theory of gravity, yet there "may be a cause (and there seems to be some assign-

[x] App. XIII. p. 54. [y] Ib. p. 55. [z] App. V. p. 31. [a] Ib. p. 29.

" able) why there may be something specific in each " of them [b]."

Arguing from the analogies which he had adopted, Hooke rejected Newton's suggestion of a spiral motion for the projectile, and said, that its path would be an elliptical curve, but he did not venture the assertion of its being an ellipse: he only calls it an eccentrical ellipsoid; and adds, that supposing the motion to take place in a resisting medium, the path would become an eccentric ellipti-spiral.

It is not worth while to consider how far it was generous in Hooke to blazon an error, into which by partial statements he had drawn his correspondent; but though he confined himself to this ungracious topic in the communication, which he made to the Royal Society respecting his answer to Newton, we find from Aubrey's letter that, in the answer itself, he added to it his whole hypothesis, with the law of the inverse squares of the distances. This second letter Newton could hardly persuade himself to answer [c]; but still Hooke assailed him with a third, and took care to inform the Royal Society of his having done so. From their minutes [d] we learn that it contained some further account of his theory of circular motion and attraction, to which he added remarks on the effects, that climate and elevation would have upon the swing of the pendulum, giving as an instance the variation of Halley's clock at different heights in St. Helena. The only way of checking such importunity was by abstaining from any reply; but Newton knew what was due to the courtesies of society, and when he had occasion to write in the following December, to introduce

[b] Posthumous Works, p. 546.

[c] App. V. p. 27.

[d] Birch, vol. iv. p. 1.

Dr. Gasparani to the notice of the Royal Society, he did not neglect to express thanks to Hooke[e] for the trouble he had taken in trying his experiment on the falling bodies.

It must occasionally happen that persons will be in possession of scientific truth, which others, (though much their superiors,) if unexpectedly called upon, do not immediately discover; but Hooke, to his own feelings, had come off triumphant in this last short correspondence; and from his jealous temper, he may be conceived to have been greatly mortified, when the individual, whose views he thought less expanded than his own, came afterwards forward with profound proofs of what he had never himself been able to demonstrate. He claimed, therefore, to have been the real discoverer of the whole foundation, on which Newton had raised his system, as not only having been the first to make out these truths, but having instructed Newton in the principles of them.

Newton warmly repelled the claim for his acknowledgment of that scientific obligation, which he was conscious of not having incurred[f]. And to prove that he was not indebted to Hooke for the law of the inverse ratio of the squares he appeals to three instances, in which he had shewn his knowledge of it before Hooke had given any intimation of his having adopted such an opinion.

1. "That in one of my papers writ (I cannot say in " what year, but I am sure some time before I had " any correspondence with Mr. Oldenburg, and that's) " above fifteen years ago, the proportion of the forces " of the planets from the sun, reciprocally duplicate of " their distances from him, is expressed, and the propor- " tion of our gravity to the moon's conatus recedendi

[e] Birch, vol. iv. p. 61. [f] App. V. p. 33.

"a centro terræ is calculated, though not accurately "enough[g]." Now this answer was written in June 1686, and Birch mentions[h] from the minutes of the Royal Society, that Oldenburg had received a letter from Newton early in Jan. 1672, which reaches nearly to his date for the beginning of their correspondence; but Newton afterwards extended the time, which had elapsed since the paper had been written, to eighteen or nineteen years[i], which will carry it back to 1668 or 1667. This is a remarkable time, especially as it is stated in the same letter, in which he tells Halley, "for the duplicate proportion I can affirm, that I ga-"thered it from Kepler's theorem about twenty years "ago," that is, about 1666. Now all these facts put together and added to the acknowledgment of the calculation not being sufficiently correct, leave little doubt of the paper, alluded to, being the result of the early speculations at Woolsthorpe. And it may be observed that the intervals are not so precisely stated, as to preclude the admittance of their still wanting one year of the exact period; now, we know that Newton, for a long interval, did not return to the inquiry after his first failure; it is therefore most probable that the deduction from Kepler, which is said to have preceded the calculation by a twelvemonth, took place in 1665.

2nd. When Hugens published his Horologium Oscillatorium in 1673, a copy was sent to Newton, who wrote a letter of thanks to him, through Oldenburg for the present[j]. At the end of the work were some valuable theorems of the properties of centrifugal forces, which Newton particularly noticed. He added,

[g] App. V. p. 28.

[h] Hist. Royal Society, vol. iii. p. 1.

[i] App. VII. p. 40.

[j] App. V. p. 28, 32; VIII. p. 41.

that such "speculations may prove of good use in "Natural Philosophy and Astronomy, as well as Me-"chanics;" and instanced it by the explanation, which might be derived from them, both of the moon's always turning the same face towards the earth, and of the limit which they might afford for the relative quantity of the sun's parallax, when compared with that of the moon. He mentions these instances of the usefulness of the doctrine in his letter for Hugens, which appears from the original to have been written in July, 1673; and was therefore clearly anterior to the publication even of Hooke's pamphlet. Newton suggests that he might have seen this letter among Oldenburg's papers, but it appears from what Hooke inserted in Aubrey's letter to Wood, that he suspected Newton to be unacquainted (in 1679) with the law in question; and without such a possible aggravation, Newton had enough to complain of in having been "corrected magisterially, and taught a theory, which" others "knew, and he had a truer notion of than" the person, who undertook to instruct him[k].

It may indeed be objected to this last argument that Hugens treated of centrifugal, and the present discussion referred to centripetal forces. But Hugens drew up his theorems of the force, which was produced "ex motu circulari;" and Pemberton expressly tells us[l], that Newton made his first calculations on the supposition of the heavenly bodies moving in perfect circles, the orbits of the primary planets being "concentrical to the sun." Now, in such a case, the centrifugal and centripetal forces must be equal to each other in every part of the curve; and although they acted in opposite directions, whatever determined the

[k] App. V. p. 32, 33. Oldenburg died in 1677. [l] Ibid. XII. p. 50.

quantity of the one determined also that of the other. Neither could Hooke have been justified in making this objection; for he says himself of his own theory, that "he that understands the nature of the circular "pendulum, and circular motion, will easily under-"stand the whole ground of this principle, and will "know where to find direction in nature for the true "stating thereof[m]." He must, therefore, have admitted the use of those properties, which Hugens especially devised for the pendulum, when "motu co-"nico latum."

3rd. Newton refers to an hypothesis on light which he had communicated to the Royal Society, and which had been entered on their Register[n]. This paper was read in December, 1675[o]. It considers the affections of light as dependent on the vibrations in an ethereal medium of very great elasticity. This ether, being absorbed by the earth, might become a portion of its constituent substance. Streams would, by these means, be always setting to the centre, and produce a tendency in bodies towards it; while exhalations which slowly rose from the earth might gradually be resolved into their first principle, and, becoming ethereal, supply the place of that which was consumed. It occurred to him that this hypothesis might be extended to the solar system, and that the circulation of particles might afford food for the sun and planets in the same manner as he suggests, at the end of the Principia, that the solar system may derive its aliment from comets.

The application of his hypothesis to explain the effects of universal gravity was only incidental, and was in-

[m] App. XIII. p. 55. See also what Newton says App. V. p. 32.

[n] App. V. p. 33; and XX. p. 68.

[o] Birch, vol. iii. p. 247, 260.

serted in the manuscript as an interlineation [p]. This did not admit of his entering into the law, by which the action of the ether is regulated; but he explains it to Halley [q], and shews that he considered it could be "no other from the superficies of the planet than reciprocally duplicate of the distance from the centre:" which is all that is necessary for his argument.

Newton then says that Hooke borrowed from Borelli's hypothesis, (who "did something,") and that he may have learned the proportion of gravity from Bullialdus [r]. But it must be acknowledged what Borelli did was not much: with Kepler, he admits the elliptical motion of the planets, and with him he adopts the origin of that motion, from the rotation of the central body about its axis. With him, likewise, he takes the decrease of this force to be regulated by the inverse ratio of the simple distances of the revolving bodies from their centre: on this there can be no doubt, for the demonstration of the property is derived from the action of a given power on the longer or shorter arm of a lever [s]. Bullialdus indeed suggested the right proportion [t]; but his, like all the other speculations previous to Newton, are neither complete nor demonstrative. He derived this conclusion only by analogy from the diminution, which occurs in the intensity of light; for since that proceeds as the squares of the distances are increased from the luminous point, it was supposed that the same effect must take place in the diffusion of the force of gravity over

[p] App. V. p. 28. 34.

[q] App. VI. p. 34. VIII. p. 43.

[r] Ibid. V. p. 30, 32.

[s] Theoricæ Mediceorum planetarum, (1666), p. 63–65.

[t] Astronomia Philolaica, (1645), p. 23. The argument from the areas of concentric spheres is that used by Halley in the passage, (Phil. Trans. vol. xvi. p. 8.) to which Newton refers in his letter, (App. V. p. 32.).

the surfaces of successive concentric spheres. Dr. Robison [u] objects to the generalization of this reasoning, as if it would, of necessity, belong to all central forces; but we cannot wonder at many early writers adopting it. Leibnitz himself, in speaking of the hypothesis, by which Hugens endeavoured to account for the effect of gravitation, says, "J'avois jugé, qu'en cas "qu'on ne puisse point expliquer, par là, la diminu- "tion en raison inverse des quarres des distances, "verifiée ce semble par les astres, il faudroit avoir "recours à une cause semblable à la lumiere, qui ob- "serve cette raison réciproque [x]." The deductions from Kepler's laws, which were made by Newton and Halley, were more worthy of attention; but we do not know, that the inverse squares were established in them for more than circular motion. The general impression was that the virtue, whatever it might be, resided in the centre of the attracting bodies; and this led to the mistake, which Newton points out in Hooke's theory, of imagining the ratio of variation to be the same above and below the earth's surface [y]. He acknowledges the difficulty which occurs in not admitting it to be so [z]: his suspicions that the duplicate ratio did not reach accurately even to the superficies of the earth, were not removed, till, in 1685, he demonstrated the truth of that proposition.

Hooke, in his paper on gravity, which has been

[u] Mechanical Philosophy, p. 688. (1804).

[x] Opera, vol. iii. p. 662.

[y] App. V. p. 27, 29, 31, 32.

[z] App. V. p. 29, 27. There is some ambiguity in the passage referred to, but it could hardly be said that, previous to Newton's demonstration, no "judi- "cious philosopher" could have believed that for the heavenly bodies the law of the inverse square was accurate. It seems more reasonable, therefore, to confine the alleged difficulty to the decrease of the force of gravity for the earth of which he had been more particularly speaking.

mentioned as read to the Royal Society on the 21st of March, 1666, says, "If all the parts of the terrestrial "globe be magnetical, then a body, at a considerable "depth below the surface of the earth, should lose "somewhat of its gravitation or endeavour down-"wards, by the attraction of the earth placed above "it. This opinion some experiments, made by some "worthy persons of this honourable society, seem to "countenance[a];" but he points out the difficulty of conducting them in a conclusive manner, and refers to the trial by clocks at certain depths and heights, which, as Mr. Whewell[b] remarks, had been previously suggested by Bacon. Hooke indeed says, "Gilbert be-"gan to imagine it" [gravity] "a magnetical attrac-"tive power, inherent in the parts of the terrestrial "globe: the noble Verulam also, in part, embraced "this opinion;" but the latter philosopher seems, in the place referred to, not to have followed out the necessary consequence of it; for his object was to try "si "inveniatur virtus ponderum minui sublimi, aggravari "in subterraneis[c]." He questions this fact, however, very distinctly in his Natural History, where he speaks of "experiment solitary touching the decrease of the "natural motion of gravity in great distance from the "earth, or within some depth of the earth[d]." What

[a] Birch's Hist. of the Royal Society, vol. ii. p. 70.

[b] History of the Inductive Sciences, vol. ii. p. 173.

[c] Novum Organum, lib. ii. c. 36.

[d] Cent. I. art. 33. He says, "It is affirmed constantly by "many, as an usual experiment, "that a lump of ore, in the bot-"tom of a mine, will be tum-"bled and stirred by two men's "strength; which, if you bring "it to the top of the earth, will "ask six men's strength at the "least to stir it. It is a noble "instance, and is fit to be tried "to the full." This, however, would prove too much: the loss of two-thirds would require a greater depth than ever was or ever will be reached; and the exertion of man's strength is too indefinite for establishing any

Hooke's own opinion may have been at the time, he does not tell us: probably he had not decided, nor indeed is it evident how he could, from his idea of gravity, which he supposed to be produced by "an internal motion in the earth every way uniform, so as to "cause an equal attraction to the centre[e]." With others he may therefore have speculated, like Bacon, on general possibilities, but that gives him no real priority to one, who discovered the true nature of the force. Newton saw that all masses of matter, whatever might be their form, still possessed the power of attraction, which did not vary in kind, but only in intensity, according to the respective quantities which were contained in them. There was consequently no reason to believe, but that every body, however minute, was possessed of this virtue, though it might be in so small a degree as to escape physical observation. He therefore considered it to reside in the primordial particles of matter; and, supposing it to follow in each the law of the inverse squares, he shewed that a spherical mass, made up of such constituent parts, would act at different distances from its centre in the manner, which he has shewn in the 73rd and 74th propositions of the first book of the Principia—directly as the distances for the particles within the surface, and inversely as the squares of the distances for those beyond it. Indeed, his views and his deductions were in this respect so completely his own, and so remote from common acceptation, that

precise proportion. About ten years ago, a clock was carried to the bottom of one of the deepest mines in Cornwall; and although the experiment was conducted by some of the ablest men in England, it was found impossible, from local circumstances, to obtain satisfactory results from it.

[e] Posthumous Works, p. 181.

[f] See Newton's argument. App. V. p. 29.

after they had been published, and, as it appears to us, had been incontrovertibly established, Hugens himself denied that he could possibly admit them [g].

The fertile source of error and difficulty seems to have been in the useless anxiety to devise some mechanical cause for the effects of gravitation. Earlier writers imagined that the rays of gyration which emanated from the sun in its rotation, not only carried the other bodies of the system round with them, but in this action, likewise, exerted a retentive power: it was, as Bullialdus expresses it, "virtus illa, qua sol "prehendit seu harpagat planetas, corporalis quæ ipsi "pro manibus est." Hugens formed his hypothesis on another foundation. He supposed that the earth was surrounded by a material fluid, the parts of which were confined, at their upper surface, from passing beyond a certain limited distance from the centre. In addition to this, he supposed that these parts had become possessed of a constant circular motion, by which they exerted so great a centrifugal force as to press down every thing else towards the surface of the earth [h]. To illustrate this, he took a cylindrical vessel of water, into which he threw some powdered sealing-wax [i], which being a little heavier, dispersed itself at the bottom of the fluid. He closed the whole down by a plate, which was in contact with the water, and then fixed the vessel on the whirling table, so that it might be made rapidly to revolve about its axis. The centrifugal force of course drove the particles of sealing-wax to the extremity; and after the water had likewise acquired

[g] Cause de la Pesanteur, App. XVIII. p. 66. In another place (Uylenbroek Fasc. II. p. 102.) he likewise says of Newton, "Je "veux bien qu'il ne soit pas "Cartesien, pourvu qu'il ne "nous fasse pas des suppositions, comme celle de l'attraction."

[h] App. XVIII. p. 65.

[i] Cause de la Pesanteur, p. 132.

the rotation, he suddenly stopped the motion of the table; when the sealing-wax, losing its circular motion sooner than the water, receded to a heap at the centre, "qui me representa l'effet de la pesanteur." It is impossible to speak of such a man as Hugens but with respect; it may however be said, that his theory carries with it quite as much difficulty as the facts, for which it was intended to be an explanation. He saw that the circular motion of his fluid could not be the consequence of the earth's rotation about its axis, for that would produce a centrifugal force in planes, which were not vertical to their respective horizons, but parallel to the equator; and the exterior surface, which by its reflections was to produce this effect, (if it is possible to conceive that they would produce it,) is the mere creature of fancy. Hugens would not have admitted the existence of a crystalline orb; and if the extreme portions of the fluid were prevented from flying off by other surrounding systems, they could not have assumed a circular or even a regular form, to produce the supposed gyrations.

Leibnitz employed himself upon the same subject. He was inclined also to think favourably of Hugens's theory in general; but he saw the insuperable objections to it: he thought indeed that gravitation might be accounted for "avec plausibilité" by a series of emanations from the central body; "une explosion continuelle "dans les corps qui attirent les astres[k]." But the most detailed account of such a theory is in a memoir of John Bernoulli, entitled, Nouvelle Physique Celeste. Two of his principal objections to the doctrine of gravitation are derived, first, from its giving no explanation of the planetary motions being all in one

[k] Opera, vol. iii. p. 662.

direction, while those of comets are often retrograde; and, secondly, from its being incompatible with the motion of the apsides[l]. He therefore suggests modifications of the vortices of Descartes, which might receive the sun's rays, and return them in constant "central torrents[m]." These, he thought, would afford the most natural causes of all that can be observed in the economy of the solar system. The second objection is stated with reference to the defects, which had then been found from calculation, but it is now known to be without any real foundation: and the first is only the consequence of two effects being brought together, which are perfectly distinct; the impressed force, which occasions progressive motion being, as far as we know, perfectly independent of gravitation. Bernoulli's theory was honoured with the prize of the Royal Academy of Paris in 1734; and was one of the last great efforts to support the Cartesian philosophy in opposition to the views of Newton. An object, which even his powerful talents were unable to accomplish.

Newton, indeed, himself speculated on the cause of gravity. His first hypothesis was that to which he refers in his letters to Halley[n]. The fundamental principles, on which it rests, have been inserted in the Appendix[o], but he speaks of the whole theory as one of his "guesses," which he "did not rely on," and even admits that a qnestion may be made whether "it "be true." Indeed he alludes to it not with any view to its accuracy, but only as a proof of his having entertained the doctrine of the inverse squares at an early period.

A letter (probably to Oldenburg), which accompa-

[l] Opera Jo. Bernoulli, tom. iii. p. 267, 327.

[m] Ibid. p. 271.

[n] App. V. 28, 33; VIII. p. 43.

[o] No. XX. p. 68.

nied the hypothesis, is printed by Birch; and Newton says in it that he did not concern himself "whether it "shall be thought probable or improbable, so it do but "render the papers I send you, and others sent for- "merly, more intelligible." In the same manner he expresses little confidence in the views which he submitted on the same subject to Boyle, in 1679[o]. In the beginning of that discourse he says, "My notions about things "of this kind are so undigested, that I am not well "satisfied myself 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." And at the end he adds, in the same spirit, "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." Under these circumstances it would be injustice to him to enter upon the particulars of his hypothesis: his maturer judgment is exhibited in the last query at the end of the Optics. "To derive two or three general principles of motion "from phænomena, and afterwards to tell us how the "properties and actions of all corporeal things follow "from those manifest principles, would be a very "great step in Philosophy, though the causes of those "principles were not yet discovered." Indeed the ether, of which the existence has been so repeatedly and so generally assumed, was only calculated for that stage of the inquiry, in which the consideration was confined to the action of gravity in the large collective masses of the world. Leibnitz saw this, and says in his letter[p] to M. de Beyrie, "Si chaque partie "d'un corps attire chaque partie de l'autre corps, il

[o] App. XVII. p. 62. [p] Opera vol. iii. p. 661.

" faudra dire que c'est une qualité inhérente à la ma-" tière, en vertu de la première loi du Createur, ce que " M. Newton ne croit pas être hors d'apparence."

To return to Hooke. Newton, conscious of the originality as well as importance of his own discoveries, was naturally hurt at this interruption for the third[q] time of the peace, in which he was desirous to pursue his studies; and as the contest had in each instance been raised on questions of physics, he determined upon foregoing the pursuit, and not to expose himself to any more controversies. The world in consequence had nearly lost the completion of one of the greatest works which was ever produced for the advancement of human knowledge. Upon receiving the report of Hooke's claims, he wrote to Halley and said, " I designed the whole to consist of three books; ... the " third I now design to suppress. Philosophy is such " an impertinently litigious lady, that a man had as " good be engaged in law-suits, as have to do with " her. I found it so formerly, and now I am no " sooner come near her again, than she gives me warn-" ing. The two first books without the third will not " so well bear the name of Philosophiæ Naturalis Prin-" cipia Mathematica, and therefore I altered it to this,

[q] App. V. p. 33. What occurred in 1679 and 1686 was confined to Hooke; but when Newton communicated his views of Optics to the Royal Society, he had to defend them also against others: to this he feelingly alludes in a letter dated May 25, 1672. " I take much " satisfaction in being a member " of that honourable body, the " Royal Society, and could be " glad of doing any thing which " might deserve it: which makes " me a little troubled to find " myself cut short of that free-" dom of communication which " I hoped to enjoy, but cannot " any longer without giving of-" fence to some persons whom " I have ever respected. But " 'tis no matter, since it was not " for my own sake or advan-" tage that I should have used " that freedom."

"De motu corporum libri duo[r]; but, upon second "thoughts, I retain the former title. 'Twill help the "sale of the book, which I ought not to diminish now "'tis yours[s]." This extract is taken from Newton's letter of the 20th of June, and the last sentence shews that he was then acquainted with the arrangements made by the council on the 2nd of that month, by which the expense, and consequently the property of the edition, was transferred to Halley.

The first two books of the Principia might have formed a valuable work in themselves, but the application of them to physical astronomy, which is contained in the third, would have made the loss of it a serious impediment to the advance of science. Halley was very sensible of this, and lost no time in writing a soothing reply on the 29th. He assured Newton that his friends in London were fully convinced of what was due to him, and of no confidence being put in Hooke's assertion that he had previously possessed the whole system[t], although he chose to keep it to himself. In-

[r] There seems to have been an inclination for this title. In his letter to Aston he speaks of his "notions about motion," (App. III. p. 24.) and in the original manuscript of the Principia, which is preserved by the Royal Society, the title has at first been written "De Motu," which has been erased in order for Philosophiæ Naturalis Principia to be substituted in its place. Halley also spoke (see p. 15.) of the first propositions as a treatise "De Motu," and mentions (p. 31.) the whole work under the same designation.

[s] App. V. p. 29, 30.

[t] App. VI. p. 38. Halley says, (App. p. 36.) "according to your "desire in your former, I waited "on Sir Christopher Wren." Now we do not meet with any such request in Newton's letter of the 20th of June, the beginning of which seems likewise to imply that it is an explanation of one which had preceded it. It is probable also that he would not have let nearly a month elapse without answering Halley's letter of the 22nd of May. If, however, there was a letter, which is now lost, it may have contained nothing but what is found more at large in N°. V. of the Appendix.

deed he had now moderated his tone, and alleged, that what he had supplied to Newton was "but one small "part of an excellent system of nature, which he had "conceived, but had not yet completely made out, so "that he thought not fit to publish one part without "the other[q]." This, however, was not more credited than the broader assertion; for, as Halley says, the discovery should have been vindicated as his own, (if it had existed,) as soon as Newton's had been sent to the Royal Society[r]. It may be added that this argument is more particularly applicable, since Hooke was aware of all that was passing, and was himself present when Halley made his report on the 10th of December 1684.

Halley strongly entreated that no curtailment should be made, from what was intended to be introduced into the work, and expressed the disappointment which it would occasion to others as well as to himself. Some officious friend had[s] interfered, and after Newton's letter was written on the 20th, had represented Hooke's conduct as more offensive than Halley had described it to be. This occasioned Newton's writing an angry postscript, for which he afterwards expressed his regret. A nervous habit made him quick in feeling any disturbance or injury; but he was always anxious to make amends, when he considered that he had given way to unjust or immoderate irritation. He seems to have been readily appeased in the present instance; he had struck[t] "uti posthac docebitur" out of the first page, "as referring to the third book," but it was re-

q App. VI. p. 37.

r App. VI. p. 38.

s App. V. p. 30.

t Ibid. The original manuscript was already in Halley's hands, and of course has no mark of this erasure, which must have been made in the proof sheet, that had been received from him.

stored, as we find it in its proper place, and it is thus clear that, from the very first, he at once acquiesced in the request which was made to him. Hooke, however, did not so readily accommodate himself to the thoughts of peace. The letter, which was to pass for Aubrey's writing, is dated in September 1689, more than three years after the period, when the first book of the Principia was presented to the Royal Society, and Hooke has renewed in it his claims, with the hopes that Antony Wood might admit what he said into the Athenæ Oxonienses, and so perpetuate the charge against Newton. This is not deduced from what is in Aubrey's hand, but from the parts which Hooke himself inserted in the letter, where he distinctly says, that his hypothesis was "the whole celestial theory concerning which Mr. Newton hath made a demonstration." But although he had acknowledged in 1686[u] that Newton was really entitled to the discovery of the curves, in which bodies were made to move by the action of central forces, he could not conquer his envious feelings: not being sensible that the comparison, which he pressed on the attention of the scientific world, would not contribute to his credit. M. Clairaut remarks on this very subject, that such examples "servent à faire voir quelle distance il y a entre une "verité entrevue, et une verité demontrée, et combien "les plus grandes lumières de l'esprit servent peu dans "les sciences, quand elles cessent d'être guidées par la "géométrie[x]." As the whole of the Principia had been then published, Hooke found another cause of complaint in addition to what he made at first. For the letter goes on[y] to say, "likewise Mr. Newton has

[u] App. IV. p. 26.

[x] Principes — par Mad. de Chastellet vol. ii. — Exposition Abrégée du Systême du Monde, p. 6.

[y] App. XIII. p. 53.

" in the same book printed some other theories and " experiments of Mr. Hooke, as that about the oval " figure of the earth and sea, without acknowledging " from whom he had them." Could Hooke have been contented with what was justly his due, there could have been no difficulty: he certainly saw that the earth must be oblate at the poles; mention is made of this theory in many parts of his writings, and particularly in his Discourse on Earthquakes, which Mr. Waller, the editor of his Posthumous works, considers to have been written before 1667[z]. He considered the solid part of the earth to be an exterior shell[a], and reasoned to the figure which it assumed, from the different degrees of centrifugal force of the parts at different distances from the axis of rotation. Now this was a fact which Newton did not want to be informed of, for in comparing the moon's orbit in 1666 with the effect of gravity, he calculated (as he said) " the force of ascent " at the equator arising from the earth's motion[b]:" and he resolved the problem more completely than it had ever been contemplated by Hooke, who assumes as the foundation of his argument, that " the gravitating " power of the earth be every where equal, as I know " no reason to suppose the contrary:" and says the waters of the sea may, at the poles, have a plane, if not a concave surface[c].

It appears, therefore, that Newton's first impressions of Hooke's conduct were by no means beyond the truth; and that he put no unfair construction on it, when he complained[d], that if such claims were allowed, " mathematicians, that find out, settle, and do all the " business, must content themselves with being nothing

[z] Posthumous Works, p. 349. see also p. 343, 350, 357.

[a] Cometa, p. 10.

[b] App. VII. p. 40.

[c] Posth. Works, p. 349, 351.

[d] App. V. p. 31.

"but dry calculators and drudges; and another, that "does nothing but pretend and grasp at all things, "must carry away all the invention."

Halley, however, was anxious to make peace, and by softening, as much as he could, the account of what had passed, he quieted Newton's feelings, who then agreed to comply with what had at first been proposed, when it was said that "Mr. Hooke[d] seems to expect "that you should make some mention of him." The fourth proposition of the first book of the Principia treats of bodies revolving in circles; and the sixth corollary to it shews that if the squares of the times are as the cubes of the distances, the force towards the centres will be inversely as the squares of the radii. This was the proper place for the expected notice, because it was the particular case which, as an undemonstrated fact, had been observed by the others to obtain in the solar system; Newton therefore here added a scholium, in which he says, that this property "obtinet in cor-"poribus cœlestibus (ut seorsum collegerunt etiam nos-"trates Wrennus, Hookius et Hallæus)." It may be observed that he arranges the names in the order of the time[e] in which he was persuaded the individuals had come to a knowledge of the truth, and it is extraordinary that Dr. Thomson[f] should have so far mistaken the passage as to represent that "Newton in his "Principia informs us ... that the doctrine of gravita-

[d] App. IV. p. 26.

[e] App. V. p. 27. It is remarkable, nevertheless, how the more inaccurate statement continued to be repeated, especially by persons who were not able to judge of the real merit of the case. Hearne, in his memorandum books, speaks of stories, which had been told him (in 1726 and 1730), of Newton having collected the materials for his Principia from Wren and Hooke. The account which Halley gives (App. VI. p. 36.) of his conversation, will shew that Wren set up no such claims, and did not allow that Hooke had any right to them.

[f] History of the Royal Society, p. 340.

"tion had occurred to Hooke and Halley about the "same time that it did to himself." Praise is bestowed on the candour of this declaration; but, if it had been such as is here stated, it would not have been entitled to any such character—it would have been a pusillanimous allowance of what Newton was convinced to be unfounded. Newton's genuine candour, however, induced him fairly to tell Halley for what he was really indebted to Hooke's correspondence. "This," he says[g], "is true, that his letters occasioned my finding "the method of determining figures," which he adds that he tried in the ellipse. After the concealment which Hooke professed, in 1684, to maintain to Wren and Halley, it cannot possibly be understood that any hint of a demonstration had come from him, but only that Newton's attention had been turned to the subject by what was passing between them. Indeed he confirms this view of the question in a subsequent letter to Halley[h], which he concludes by saying, "Though his "correcting my spiral occasioned my finding the theo-"rem, by which I afterwards examined the ellipsis, "yet am I not beholden to him for any light into the "business, but only for the diversion he gave me from "my other studies to think of these things, and for "his dogmaticalness in writing, as if he had found the "motion in the ellipsis, which inclined me to try it, "after I saw by what method it was to be done." In fact, as he says[i], "Kepler knew the orb to be not cir-"cular but oval, and guessed it to be elliptical;" in which he was right; and yet this did not take from the claims of him who demonstrated the general truth, whereas Hooke had not even an accurate idea of what he considered to be his own discovery. He only states

[g] App. VII. p. 40. [h] App. VIII. p. 44. [i] App. V. p. 29.

in general that the projectile[i] would move in an ellipsoid, but he evidently contemplated no alteration in the curve from the motion being continued, (which was a supposed condition of the problem,) below the surface of the earth: the ellipse nevertheless would be changed under these circumstances from one, in which the force was at the focus, to another, in which that power was at the centre. It may be observed likewise that in the communication which he made to the Royal Society in Dec. 1679, he speaks of the earth's impressing a circular motion on the projectile, which is by no means precise. Secondly, Newton acknowledges that he was indebted to Hooke for the argument in favour of the earth's rotation from the alteration of the rate, that Halley had observed in the clock, which he took with him to St. Helena. The principle of the variation, he says, had long been familiar to him; but in his anxiety to do full justice, he allows that he had not been previously acquainted with this particular fact for its illustration. At the same time it must be remarked, that Hooke had built more upon this observation, than Halley himself considered it could legitimately prove. He says, "'Tis true, at St. Helena, in the latitude of 16 "degrees south, I found that the pendulum of my clock, "which vibrated seconds, needed to be made shorter "than it had been in England, by a very sensible space "... before it would keep time.... Yet I dare not "affirm that it proceeded from any other cause, than "the great height of my place of observation above "the surface of the sea[j]." Thirdly, Newton says that the deflection of falling bodies in northern latitudes towards the south as well as to the east, in consequence of the earth's rotation, was new to him when

[i] Birch, vol. iii. p. 516.

[j] Phil. Trans. vol. xvi. p. 7.

Hooke communicated it. This last suggestion, which, as M. Biot remarks[k], was of no great difficulty, closed the sum of all, for which Newton was indebted to his correspondence with Dr. Hooke.

Halley, in the latter part of his letter of explanation[l], after entreating that the third book should not be suppressed, wisely assumed that this question should make no delay in the commencement of his undertaking. He had had a specimen set up, which was sent to Newton for his inspection, and as it had given satisfaction, he tells him, "Now you approve of the character "and paper, I will push on the edition vigorously." Lalande[m] tells us of the Principia, "que M. Halley "avoit une grande part aux découvertes, qu'il ren-"ferme." This probably is too much, but it seems to be said particularly with reference to the moon's motions, and he certainly communicated all the facts and observations which he was able to contribute. He also introduced (if they met with Newton's approval) and freely suggested any alterations in the plan, or in the work itself, which he thought could improve it. The first intention had been to give a different name to the several parts, from that which they now bear; for Newton[n] tells him, "the articles are, with the largest, "to be called by that name; if you please, you may "change the word to Sections, though it be not mate-"rial:" and we know that the change was made. The diagrams likewise were to have been engraved on

[k] Biogr. Univ. xxxi. p. 149. The passage is omitted in the English Translation.

[l] App. VI. p. 38; V. p. 29.

[m] Tables Astronomiques de Halley (1759), vol. ii. Preface, p. iv.

[n] App. V. p. 30. The designation of "articles" is still to be seen in the original manuscript for the first book; the several parts of the second are called sections, as they stand in the printed copies.

copper, but he told Newton[o], "I have sometimes had "thoughts of having the cuts neatly cut in wood, so "as to stand in the page with the demonstrations. It "will be more convenient, and not much more charge. "If it please you to have it so, I will try how well it "can be done; otherwise I will have them in some- "what a larger[p] size than you have sent up." Newton's answer to this letter is printed in the General Dictionary[q], without the notice of this proposal, although it shews Halley's unwillingness to spare any expense, which he thought might be advantageous to the publication; but the acceptance of such an offer should not have been suppressed by the biographer. "I have considered," Newton says, "your proposal "about wooden cuts, and believe it will be much con- "venienter for the reader, and may be sufficiently "handsome, but I leave it to your determination. If "you go this way, then I desire you would divide the "first figure into these two," (the parallelogram for the composition of forces, and the diagram for the fundamental property of the lever,) "I crowded them into "one, to save the trouble of altering the numbers in "the schemes you have."

It has been mentioned that Newton probably sent his first specimen to the Royal Society in Feb. 1685: and, with not more than one exception, he had completed his first draught of the whole before the end of the year; for after the manuscript of the first book had

[o] App. VI. p. 38.

[p] They were drawn on separate papers, for there are none in the manuscript. Halley notices likewise in another place, that they were too small, for he asks "particularly whether you "think it not better, that the "schemes should be enlarged, "which is the opinion of some "here?" (App. IV. p. 25.)

[q] Vol. vii. p. 800. App. VII. p. 39.

been delivered, he tells Halley, in June 1686, that "the[r] second book was finished last summer, being "short, and only wants transcribing and drawing the "cuts fairly. Some new propositions I have since "thought on, which I can as well let alone. The "third wants the theory of comets. In autumn last "I spent two months in calculations to no purpose for "want of a good method, which made me afterwards "return to the first book, and enlarge it with divers "propositions, some relating to comets, others to other "things, found out last winter."

Thus far the account has been in great measure collected from letters already in print, but there are three more written by Newton during the course of the publication, which have hitherto remained unnoticed at the Royal Society, although they supply several very curious particulars respecting the progress of the work.

The first was written on the 18th of October 1686, from which it[s] appears that the sheet, with the signature M, was then completed; for Newton refers to a passage in page 87, and gives an explanation, for which Halley seems to have applied, to illustrate the transmutation of figures as laid down in the 22nd Lemma. It appears likewise that sheet P had been worked off, since he thanks Halley for having pointed out some oversights in the beginning of the general scholium at the end of the sixth section, and sends him the corrections[t] which were requisite. These are inserted in their proper place: and it may be seen, in the printed copies, that the leaf has been cancelled for p. 109, 10.

r App. V. p. 29.

s App. IX. p. 45.

t In the manuscript all this part, as it was first written, is crossed out, and the text, as it now stands, is added on an additional sheet.

The second[u] letter is of the 18th of February 1687, and alludes to circumstances which may require explanation.

From a letter of Halley to Molyneux, of which an extract is printed by Birch[v], we learn that on the 9th of December 1685 both the secretaries of the Royal Society unexpectedly resigned, in consequence of which it was determined that their places should be supplied by two members, who should be considered as merely honorary officers, while all the correspondence, and the "whole burthen of their business," should be assigned to a clerk or amanuensis, who was to be accountable to them. On the 27th of the following month Halley was chosen, in preference to Sloane, Papin, and Mr. Salisbury, (who were his competitors,) for this office, which brought with it not only much immediate business, but some that was retrospective[x]. On the third of March also the council determined "that Mr. Halley draw up the Transactions, and that, "when they are so drawn up, they shall be perused "and approved by one of the secretaries[y]." With all

[u] App. X. p. 47.

[v] History of the Royal Society, vol. iv. p. 450. note.

[x] Ibid. p. 454, 462.

[y] Birch's Hist. of the Royal Society, vol. iv. p. 462. Halley continued the Transactions to the end of 1687, after which time the publication was not resumed till the beginning of 1691. How necessary he then was to the undertaking, is clear from the minutes of the Council. On the 21st of January 1691, "a "discourse arising about publishing the Philosophical Transactions again, it was referred to "the next Council to determine "thereon, E. Halley not being "now present." On the 28th of January, the undertaking having been resolved on, "Dr. Tyson, "Dr. Slare, Dr. Sloane, Mr. "Walter, and Mr. Hooke, were "desired to be assistant to E. "Halley in compiling and drawing up the Transactions;" but this plan was not effective. Three numbers came out in the first nine months of 1691, and one more on the 19th of October 1692, and then the business again stopped; but at a meeting of the Council on the 7th of December, measures were taken for renewed efforts, and "Halley

this accumulation of business any common man might have been overwhelmed, nor would it have been at all improbable that Halley himself might have shrunk from it, if differences in the Society should have added discomfort to his labours. But, although he persevered in the duties of his situation, Newton had heard a report of his having resigned it, and not having any letter from him, desired his friend Mr. Montague (afterwards Earl of Halifax) to inquire of Mr. Paget "how things were[z]." Part of the information, which he received in answer, referred to a paper of Wallis on motions in resisting media, which is printed in the sixteenth volume of the Phil. Trans. An order had been made to inquire of Newton whether he designed "to "treat of the opposition of a medium to bodies moving "in it, in his treatise de Motu Corporum, then in the "press[a];" and he answered that he had done so in the paper communicated through Mr. Paget in 1684[b]. He said also that he had added considerably to what he had then sent to Halley, and had inserted the whole in his second book, which had been revised and made ready for the press in the autumn of 1686.

Halley could not have received Newton's consent to[c] substitute the wooden cuts in the place of copper plates till after the middle of July 1686. The regular print-

"offered, if it shall be under- "taken to print a book of Phi- "losophical matters, such as the "Transactions used to consist "of, that he would undertake "to furnish, de proprio, five "sheets in twenty." This is the well known instance of confidence in his own resources, and of his readiness to draw upon them in furtherance of science; but the occasion of the offer has been mistaken. Dr. Thomson, in his History of the Royal Society, (p. 7,) says that it took place upon his becoming clerk in 1686.

[z] App. X. p. 47.

[a] Birch, vol. iv. p. 521.

[b] They were also inserted, and probably more fully, in his paper of 1685. See App. I. p. 15–19.

[c] App. VII. p. 39.

ing of the first book could therefore have hardly been begun before the month of August. There is indeed at the Royal Society the copy of a letter dated July 19, 1686, in which Halley gives an account to Reiselius of scientific works going on in England, and says, "Jamque sub prelo est liber vere egregius, cui titulus "Philosophiæ Naturalis Principia Mathematica, autore "Isaaco Newton, Matheseos professore Cantab., geo-"metrarum, quotquot unquam extiterunt, forsan sum-"mo." The dates, however, of Newton's letters shew that this could only refer to the proof sheet, which had been previously set up as a specimen of the manner, in which the work should be executed. At all events no time was lost, but it has been seen that we are not certain of more than 104 pages having been printed off in the middle of October. The first book extends to 235 pages, and Newton had calculated that it might be finished in November or December[c]; but the occupation at the Royal Society had probably interrupted the progress of the work, for he speaks in Feb. 1687 of only having received eleven[d] sheets. Halley, however, was now putting the press again forward; for which Newton thanks him, and requests that as soon as seven more sheets shall be ready they may be sent to him. This request is accompanied with acknowledgments and thanks for the great exertions which were bestowed on the publication, amidst the burden of so much other business.

The third of the hitherto unpublished letters is short, and written on the 1st of March 1687. Newton had told Halley in the former letter, that if he wished to see the second book it should be sent to him[e]; "though otherwise I should choose to let it lie

[c] App. X. p. 47. [d] App. X. p. 48. [e] Ibid.

"by me 'till you are ready for it." That time was most probably come; for the present letter mentions, in the first sentence[f], that the manuscrip thad been dispatched by the coach, and would be received in London on the following Thursday or Friday. This letter was read to the Royal Society on the 2nd of March, and the papers were probably received in due course; although there is no notice of the time of their arrival, or of any presentation of them, in the Journals of the Society.

Newton also, on this same occasion, expressed a[g] wish to see a short account of the heads of Wallis's investigations, but it is evident that his own had been completed long before he could have received it. Wallis on the other hand, before his paper was read on the 26th of January, had been acquainted with what Newton had discovered on the subject; for Halley wrote[h] to him on the 11th of December 1686, sending two of Newton's problems, which were probably the 6th and 7th, with the concluding scholium of his early paper[i]: and the originals (not copies) were sent to Oxford, as appears from Wallis's letter of the 14th of Dec. which Birch has annexed to the minute from the Journals.

On the 6th of April 1687, "the third book of Mr. "Newton's treatise, entitled De Systemate Mundi, was "produced and presented to the Society. It contains "the whole system of the celestial motions, as well of "the secondary as primary planets, with the theory of "comets, which he illustrates by the example of the "comet of 1680–1, proving that, which appeared in "the morning the November before, to have been the "same comet, that was observed in December and "January in the evening[j]."

[f] App. XI. p. 48.
[g] App. XI. p. 49.
[h] Birch, History of the Royal Society, vol. iv. p. 514.
[i] App. I. p. 15.
[j] Journal of R. S.

One of Halley's arguments, to induce Newton not to suppress the third book, was "the[k] application of your "mathematical doctrine to the theory of comets and "several curious experiments, which, as I guess by "what you write ought to compose it, will undoubt- "edly render it acceptable to those, who will call them- "selves Philosophers without Mathematics, which are "much the greater number." Newton himself also talks in the Principia[l] of having drawn it up "methodo "populari." Such was probably his primary intention; and after his death there was printed "The System of "the World demonstrated in an easy and popular "manner by the illustrious Sir I. Newton" (8vo. London, 1728). This was a translation from the original Latin, which was not published till some months after. It is evidently the first draught of what formed his third book. It is not broken into propositions, the doctrine of comets is but briefly treated in it, and it has not the details of the moon's motions, which afterwards added so much to the scientific improvement of the work. Whether, when it was thus completed, it could be considered as retaining its popular character, may be doubted; but the value of the change was an ample compensation for the diminished number of readers, who, in consequence, would be likely to study it.

The period, at which Newton had finished the whole, was probably the latter end of 1686; for Pemberton says, that "this treatise .. was composed by him ... in "the space of a year and an half[m];" and although he had not drawn out the theory of comets in June 1686, yet he seems even then to have prepared the principal propositions, which were necessary for its developement. To have performed such a stupendous work in so short a time, is not only a proof of his wonderful

[k] App. VI. p. 38. [l] P. 401. [m] App. XII. p. 51.

powers, but of his having in no way relaxed in his exertion of them. Indeed when once engaged, he seems to have gone on, with regularity and good-will, until he had completed the object of his labours. During the interval between October 1686 and Feb. 1687, when there appears to have been an interruption of the printing, he did not become negligent of the business; nor did he indulge any reluctance in forwarding what was ready, although he would have wished for more time to digest and improve the different parts of it. He says to Halley, in his letter of the 1st of March 1687, "I am obliged to you for pushing on the edition, because "of people's expectation, though otherwise I could be "as well satisfied to let it rest a year or two longer[n]."

In Dr. Gleig's Supplement[o] to the Encyclopædia Britannica, it is said that David Gregory "was en-"trusted with a manuscript copy of the Principia, for "the purpose of making observations on it. Of these "Newton availed himself in his second edition, they "having come too late for the first publication, which "was exceedingly hurried by Dr. Halley, from fears "that Newton's backwardness would not let it appear "at all." It is clear, from what has just been stated, that the work was completed before Halley increased his expedition in printing, and that the whole was delivered into his hands soon after he renewed his efforts to send it out speedily to the world. There is no reason therefore for the motive, which is assigned by the writer to his efforts, and it does not seem improbable that there may be some mistake in the more essential parts of the story. In 1829 Dr. James Craufurd Gregory communicated to the Royal Society of Edinburgh a notice of some autograph manuscripts of Newton, which had belonged to David Gregory, and had been preserved

[n] App. XI. p. 48. [o] 1801. Vol. i. p. 721.

by his family. If such a treasure, as an original manuscript of the Principia, had been among them, it would, no doubt, have been kept with the greatest care, and would have stood foremost in any account of the papers. But he only mentions[p] that he "found (along "with several other autograph fragments on mathe- "matical subjects) one manuscript consisting of twelve "folio pages in the handwriting of Newton, and con- "taining, in the form of additions and scholia to some "propositions in the third book of the Principia, an "account of the opinions of the ancient philosophers "on gravitation and motion, and on natural theology, "with various quotations from his works." Gregory, who had a warm and kind friend in Newton, certainly employed himself most carefully in studying and making observations on the Principia; but Flamsteed[q] speaks of his having done so after the printed work had been presented to him. Having, by the kindness of D. F. Gregory, Esq. of Trinity College, Cambridge, been indulged with the opportunity of examining the manuscripts in question, I shall have occasion to say more hereafter of the real nature of this reference to the Principia; when it will be seen that they contain nothing to bear out the assertion in the Encyclopædia Britannica. Newton was, indeed, indefatigable in writing out his works, but if he had wished for Gregory's advice and remarks, he would surely have put the copy into his hands, while there was yet time for him to obtain what he desired; nor would he have suffered himself to be deprived of it by the speed of the editor, of which, in fact, we have seen that he ap-

p Transactions of the Royal Society of Edinburgh, vol. xii. p. 67.

q Mr. Baily's Account of Flamsteed, p. 164.

proved. Besides there is reason for believing that his personal intimacy with Gregory was not of so early a date. There is in the Bodleian Library a copy of the certificate[r] which he gave to promote Gregory's election to the Savilian Professorship of Astronomy, and it is copied by the hand of the person in whose favour it was drawn up. Now the ground on which the good opinion was formed, was "from having "known him by his printed mathematical perform- "ances, and by discoursing with travellers from Scot- "land, and of late by conversing with him." This is dated July 28, 1691, and certainly, does not imply that a confidential intercourse, which could induce Newton to submit the Principia to his criticism, had existed for several years.

By an order of the Council of the Royal Society, made on the 20th of June 1686, the President was desired "to license Mr. Newton's book," and the imprimatur signed by Pepys is dated on the 5th of the next month. The whole appears to have been completed and published about the same time in the following year. In the library of the Ashmolean Museum the minutes are preserved of the Philosophical Society, which met in Oxford during a part of the seventeenth century, and they contain the following memorandum. "1687, May 20th, Mr. President" [Dr. Wallis] "was pleased to communicate a letter from "Mr. Halley, which gives an account of Mr. Newton's "book De Systemate Mundi, now in the press, giving

[r] See Letters by eminent persons (1813), vol. i. p. 177.—The editor seems to think the paper was in Newton's handwriting, but in that he is mistaken. It was this certificate which probably determined the election in favour of Gregory. His competitor was Halley, against whom prejudices were entertained, the nature and particulars of which have been delivered down by Whiston in a form, which seems to be very inaccurate.

"an account of the reason of the celestial motions, "&c.;" and the Biographia Britannica[s] seems to be correct in stating that the book came out about Midsummer. This is corroborated by what has been already quoted from the letter of Fatio de Duilliers[t]. Professor Uylenbroek gives the answer[u] to it of the 11th of July, in which Hugens evidently speaks as if he looked forward to the immediate possession of the work. His last words are, "ayons le livre de Newton." Although this makes the publication a little later than some other accounts, it is for that very reason the more worthy of credit. When the press of business, which fell on Halley, is taken into consideration, it is only wonderful that, with the most indefatigable exertions, he could have brought out the book as soon as he did. The volume contains 64 sheets, and, as it is probable that not more, at the utmost, than 14 were finished before the middle of February, the remaining 50 must have been worked off in four months, which will average about three for every week. For this last part alone there were above an hundred different diagrams, to be cut in wood, (besides an engraving on copper,) all of which were new, and some very complicated, so that there was not only the delay to be encountered from the workmen, but from the care which was required in instructing and correcting them. There may be some excuse, therefore, if the errata, which were detected, did extend to a whole page, and it must be recollected that, although the newness of the work rendered it particularly desirable that it should be correctly printed, that same circumstance increased the difficulty of the execution. Halley seems to have done all he could to make himself master of the several

[s] Vol. v. p. 3226.
[t] P. 20.
[u] Uylenbroek, fasciculus ii. p. 102.

parts. Newton likewise did not hurry him—he says, "Pray take your own time"—"if you meet with any "thing which you think need correcting or further ex-"plaining, be pleased to signify it to me[x]." In the same letter, in which he points out some errata that Mr. Paget had discovered, he evidently considers that any negligence which may have occurred, was not attributable to Halley; for he says, "I wish the printer "be careful to mend all you note[y]."

During the time the book was in the press, considerable interest was felt about it; and its authority was appealed to even before it was published. On the 9th of March 1687, a letter from Dr. Wallis was read to the Royal Society, in which he made "some "reflections on Mr. Hooke's hypothesis of the muta-"bility of the poles of the earth;" and "on this occa-"sion there was read a paragraph of Mr. Newton's "mathematical philosophy, concerning the direction "and position of the axis of a globe turning about it-"self, 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[z]." Against this paragraph in the original Journal Book there is added in the margin, "Newton Philos. Prop. 66. cor. ult." Wallis's letter was addressed to Halley, no one was so likely as himself to have been able to bring forward this illustration, and the note is in his handwriting. In the sixteenth volume of the Philosophical Transactions[a], there is also a general account of the contents of the Principia, on which Dr. Hutton has remarked[b], "This

[x] App. IX. p. 47.

[y] App. IX. p. 46.

[z] Birch's History of the Royal Society, vol. iv. p. 528.

[a] P. 291.

[b] Abridgment of the Phil. Trans. vol. iii. p. 358.

"... has much the appearance of having been drawn "up by the masterly talents of Dr. Halley." Newton indeed was not likely to have written it, and possibly, with the exception of Halley, there was no one else who could have done it; it came out also in the 186th number, of which he was the responsible editor. Indeed there can be no doubt about its author, since the paper, in his own handwriting, still exists in the Archives of the Royal Society. It has been reprinted in the Appendix[c], not only on account of its intrinsic merit, but of its being particularly adapted to our present purpose. With Halley's thorough knowledge of the subject, he was better able than any one in the present day, to fix upon what was new at the time and most prominent in the Principia. The account is drawn up as a review, which is headed by the title of the work; to which is added, "Prostat apud plu-"res bibliopolas," which is precisely the expression used in the titlepage of the first edition of the Principia. The opening sentence also speaks of the "author hav-"ing at length been prevailed upon to appear in pub-"lic;" all which may probably have occasioned the mistake made by the editors of the Commercium Epistolicum[d]. For as the number was that for January, February, and March, 1687, they might have taken the impression of the book having been published in the last of these months. We have seen, however, that the manuscript was not all in Halley's hands until April, and there is a very probable solution of this apparent opposition. Halley annexed an Advertisement[e] to the end of this number, saying, that in consequence of his having been occupied by the care of the Principia, the publication of the Transactions had, for some

[c] No. XXI. p. 70. [d] See p. 20. [e] App. XXI. p. 77.

months last past, been interrupted. Now the 185th number was for Nov. and Dec. 1686, the interruption therefore was not in the series, but in the periods at which they had come out; and the delay of "some "months" may be well understood to have extended beyond the time when the work was actually published.

Halley prefixed to the Principia a set of Latin hexameters "in viri præstantissimi D. Isaaci Newtoni "opus hocce mathematico-physicum sæculi gentisque "nostræ decus egregium." There is, at the Royal Society, a copy of an unpublished letter to Valvasor, at the end of which Halley says, "Quod autem ad . . . "carmina spectat, velim tuo utaris arbitrio; corrige, "omitte quod tibi videbitur. Si quid dictum sit, quod "nomini et honori tuo minus conveniat, quæso, can-"dide interpreteris." This seems to indicate that he had addressed some verses to Valvasor, but none have been discovered. This is to be regretted; for it may be seen from what he printed with the Principia that his powers were not confined to those branches of study, in which he was most eminently distinguished. These lines have been constantly quoted for their appropriate praise of his author; and there can be little doubt of Voltaire having had them in his mind when he wrote the poem addressed to the Marquise du Chastellet, which was printed in the first editions of his Philosophie de Newton. Delambre[f] says of them, "le "dernier est,

"Nec fas est propius mortali attingere divos,

[f] Histoire de l'Astronomie au dixhuitième siècle, p. 2. He did not think it necessary to print the second and third lines of Voltaire's verses, but they are inserted here to complete the sentence.

" eloge que personne n'a taxé d'exaggération, et sur " lequel Voltaire a peutêtre enchéri quand il a dit,

" Confidens du Très-Haut, substances immortelles,
" Qui brulez de ses feux, qui couvrez de vos ailes
" Le trone, ou votre Maitre est assis parmi vous,
" Parlez : du grand Newton n'etiez-vous point jaloux ?"

In the Biographia Britannica [g] it is suggested that Halley may have taken the plan of his verses from some, which were addressed by king James the First to Tycho Brahe; but there seems to be little similarity in the two compositions, excepting in the common object of eulogizing discoveries in Astronomy; and the royal author, even if he had in this case been the prototype, could not claim any superiority either in perspicuity or poetry. The text varies in the three editions of the Principia. There is in the Bodleian [h] library a letter of Keill to Charlett, which contains the following observations: " Oxford, July 18, 1713. You " know there is a new edition of Sir Is. Newton's " Principles. Published before them there was a copy " of verses of Dr. Halley's, which in the new edition " Dr. Bentley has made bold to emend, and alter in " several places, without asking his leave. I am of " opinion the emendations are not near so good as the " original: some of them are intolerable. I will here " write you one. Dr. Halley had in the first edition,

. . . . . . " Quas dum primordia rerum
" Pangeret, omniparens leges violare Creator
" Noluit, æternique operis fundamina fixit."

[g] Vol. iv. p. 2506, note [Y]. Reference is there made to a translation of Halley's verses into English, which is printed in B. Martin's Misc. Correspondence, (vol. i. p. 4); but it is not well done. Thorpe acted with good taste when he gave the original Latin with his translation of the first book of the Principia in 1777.

[h] Ballard's Collection, vol. xxiv. No. 16.

"Dr. Bentley has turned it thus in the new edition:

. . . . "et quas dum primordia rerum
"Conderet, omnipotens sibi leges ipse Creator
"Dixerit; atque operum quæ fundamenta locarit."

Halley's labour and expense in publishing the Principia were freely undertaken, and do not appear to have been considered by him as forming a claim to any right beyond that, which he had of course to the copies which he printed; but the verses were his own exclusive property, and in the third edition most of the original readings were restored [i]. Keill, however, though an able man, had not the varied talents of his brother professor; and, in his eagerness to notice what he thought was wrong, has unfortunately instanced the passage, in which alone Halley retained, in 1726, any thing considerable from the alterations, which had been made without his concurrence. Bentley's conduct, however, was not fair. There could be no competition of scholarship, nor any doubt of his being able to write better Latin than Halley, but every man has a right to determine how his own thoughts shall be expressed: and another person, in his improvement of grammatical precision or elegance of language, can never be secure of preserving the exact sense, which the author meant to convey.

In 1687, and probably as soon as the Principia was ready for publication, a copy of it was presented to king James the Second [k], accompanied by a paper, in

[i] See App. XV. p. 57.

[k] The Royal Society had then been established little more than twenty years; and Newton describes it, in his Dedication of the Principia, "a serenissimo "rege Carolo II. ad philosophiam promovendam fundatæ, "et auspiciis potentissimi mo-"narchæ Jacobi II. florenti." In the second edition the expression is varied to "auspiciis "Augustissimæ Reginæ Annæ," and Newton presented it to her

which Halley gave a general account of the book, and more particularly explained the doctrine of the tides, as deduced by Newton from the effects of gravitation: this subject being chosen as one, which was most likely to interest the king, who had been lord high admiral, and had commanded the British fleet in the war with the United Provinces. It would seem, that the presentation was made at some special meeting which had been appointed for that purpose. Halley in the conclusion of his dissertation makes an offer of further explanation, if there should be occasion for it, and "if "your Majesty shall please to suffer me to be admitted "to your presence." This, at first sight, seems to imply that he was not at the time in the situation to which he alludes, but it may only be a courtly phrase, which is not to be construed with so much rigour. The paper was printed separately[l], but it was afterwards introduced into the Philosophical Transactions for 1697[m]. The beginning and ending were omitted[n], and an apology was thought necessary, for this reprint, by Sloane who was the editor of the volume: he derived it from the tract having been well received, and being calculated for usefulness from its popular view of the subject.

In the Acta Lipsiensia for 1688, there is a long article on the Principia; but from the state of science, and the few persons at the time who could understand the work, it is not probable that the first impression, if it had been large, could have been soon disposed of.

on Monday, the 27th of July, 1713. (See Baily's Account of Flamsteed, pp. 98 and 229). In the third edition the words are, "serenissimi regis Georgii."

[l] There is a copy in the British Museum: it has no titlepage.

[m] Vol. xix. p. 445.

[n] The original tract is very scarce; and the omissions have been therefore reprinted in the Appendix, No. XXII. p. 77.

Within a very few years, however, a new edition was looked for. In December, 1691, Fatio de Duilliers wrote to Hugens [o], "Il est assez inutile de prier Mr. Newton de faire une nouvelle édition de son livre. Je l'ai importuné plusieurs fois sur ce sujet, sans l'avoir jamais pu fléchir. Mais il n'est pas impossible que j'entreprenne cette édition; à quoi je me sens d'autant plus porté, que je ne crois pas qu'il y ait personne, qui entende à fonds une si grande partie de ce livre que moi, graces aux peines que j'ai prises, et au temps que j'ay employé pour en surmonter l'obscurité. D'ailleurs je pourrois facilement aller faire un tour à Cambridge, et recevoir de Mr. Newton même l'explication de ce que je n'ai point entendu. Mais la longueur de cet ouvrage m'épouvante, puisque par les différentes choses que j'y voudrois ajouter, il feroit un folio assez raisonnable. Ce folio néanmoins se liroit et s'entendroit en beaucoup moins de temps que l'on ne peut lire ou entendre le quarto de Mr. Newton." Hugens has written in the margin, "n'y ajoutez pas tant," "plutôt en 4to.;" but it may be doubted whether Fatio's materials were so copious as he seems to esteem them. His own copies of the first and third editions of the Principia, with his manuscript notes in the margin, are in the Bodleian library. Nothing more is known of their history than the purchase of them for a small sum in 1755 from a bookseller in Oxford. In the copy of the third edition (of 1726) is written, "Exemplar hocce Nicolao Facio Duillierio dono dedit clarissimus autor anno 1713;" from which it may be fairly concluded that he was also in possession of the second edition, and had by mistake written the memo-

[o] Hugenii aliorumque exerc. Math. fasc. ii. p. 124.

randum in the wrong volume: nothing, however, is known of what may have become of it. The first edition was presented to him by Halley the same year in which it was published; and in the beginning of the book is written, "Multa ex iis, quæ in hoc exem-"plari adnotaveram, in secundam et tertiam editionem "Newtonus inseruit, multa vero neglexit. Cujus etiam "manu nonnulla ad paginam tertiam, et alibi forsan, "conscripta sunt. Volebam quoque corrigenda ex ipso "Newtoni codice ad hujusce exemplaris finem addere, "Martii 13, 1689–90: sed aliis occupatus negotiis, et "brevi Hollandiam profecturus, istud intermisi fa-"cere [p]."

It is probable that codex is not here to be understood in the technical sense of a manuscript. The original sheets, from which the book was printed, were carefully preserved and bound together, and are at present in the library of the Royal Society. The manuscript is on folio paper, and is written on only one side of each leaf. Halley's care is seen in many corrections, (where his hand may be easily recognised,) and in verification of calculations, which he has made on the opposite blank pages. Variations of more importance likewise occur; thus, for example, cor. 2 and 3 to prop. 91, do not appear in the MS.: and alterations were made (of course with Newton's approbation), even after the press was set; for the printer's marks appear for the limits of the several pages, and these have been found to differ in some instances from the printed text.

[p] This wish appears afterwards to have been accomplished; for among Dr. D. Gregory's papers is one entitled, "Notata Mathe-"matica, Martio, 1702–3," and it is said in the beginning of it, "Mr. Fatio gave Mr. Newton's "corrections of his own book to "M. Hugens, and he to Leib-"nitz, who has published them "in a Dutch book, of no kin to "this purpose."

What Fatio here refers to, may have been the copy of the first edition, in which Newton had written his own remarks. The corrections of the third page now spoken of, were all introduced into the second edition, and have therefore little other interest excepting from their having been inserted by the author's hand.

Fatio's own additions are, 1st, a head-line, which he has written on every right hand page; a plan, which if it had been adopted, would have contributed much to the improvement of the work: but the usefulness of these running-titles was not at the time sufficiently felt.

2nd. A large number of verbal corrections and press errors, which probably assisted in afterwards pointing out the places, where corrections of this description were required. It is remarkable that he notes the word "reciproce" as repeatedly omitted: this very mistake was detected by Mr. Paget in p. 14, l. 10; but Newton, when he mentioned the circumstance to Halley, acknowledges that it was "a fault "in the copy [q]." The cause of this omission is probably to be found in Newton having adopted a general form, which was not directly but inversely as the centripetal force; since each case, which Fatio has marked, is to be found in the original MS., and so is the redundance of the same word in p. 226, which is noted among the errata at the end of the volume. Upon an examination of each particular instance, it has indeed been found, that the press errors are few, when compared with the oversights, which occurred in writing out the text. A mind like Newton's, occupied by intense thought, may well be excused for slight verbal inaccuracies of this kind; and the intimate knowledge, which an author has of his own ideas, recalls them so

[q] App. IX. p. 46.

readily to his mind, that it is often most difficult for him to keep his attention alive to the words that he is using. A consciousness of this may have increased Newton's natural aversion from committing himself to the press, and have made him, for that purpose, more ready in availing himself of assistance from friendly editors. It must, however, be stated, that his letters are in general very well and clearly written, with few erasures. But in his book he was conscious of there being need of corrections, and in one of Lord Macclesfield's papers he says, "The book of the Principles was "writ in about seventeen or eighteen months, whereof "about two were taken up with journeys, and the MS. "was sent to the R. S. in spring 1686; and the short- "ness of the time, in which I wrote it, makes me not "ashamed of having committed some faults."

3rd. Many annotations and memoranda of more extent. The most considerable of them are as follow:

The introduction of T and L into the third proposition; this abbreviation for Terra and Luna, though not a very recondite was an useful thought: Newton esteemed it so, and adopted these initials in his letter to Flamsteed[r] of Nov. 1, 1694, as well as in the subsequent editions of the Principia.

In p. 112, against the 8th line, he has written, "Hic "latet error, quemadmodum Cl. Newtonus post ma- "turam examinationem mihi confessus est. Vel quan- "titas $\frac{1}{2}$D prorsus delenda est, vel ejus loco $\frac{1}{2}$E re- "ponendum, quod quidem erit accuratius. Sic paulo "inferius loco $\frac{1}{2}$F scribendum est $\frac{1}{2}$G, et loco $\frac{1}{2}$H "scribendum $\frac{1}{2}$I, nisi ambas quantitates $\frac{1}{2}$F et $\frac{1}{2}$G "delere visum fuerit, quod jam tuto facias, &c."

Some remarks on the 43rd and 44th Propositions: they are of a certain extent, but were not adopted

[r] Mr. Baily's Account of Flamsteed, p. 138.

by Newton. These are very important propositions. The controversy is well known, which was occasioned by Newton's application of the doctrine of gravitation to the progression of the apsides. Fatio's comment does not touch upon the point, (mentioned in the second corollary to the 45th proposition), of the difference between the calculated and true motion for the moon.

A curious notice annexed to prop. 37 of the second book, against which is written, "Newtonum ab erro-"ribus in hac propositione contentis nullatenus libe-"rare potui, quam per experimentum; constructo sci-"licet vase ad hunc usum destinato." Newton, in the second edition of the Principia, certainly introduced considerable alterations into this part of the work.

Fatio seems really to have studied the work with minute attention; and, besides his various additions and corrections proposed for the text, there are repeated instances of his remarks on the diagrams. To that for prop. 25, book 3, he has made additions, which however were not adopted by Newton. For prop. 39, book 1, the curve LM was originally made concave to the axis AE; in Fatio's copy this has been corrected with a pencil, in a manner so exactly similar to what was ultimately introduced by Newton himself, that it might be supposed to be copied from it. Fatio indeed has many short notes, in the margin of his first edition, on what he found in the third, (of which he does not express uniform approbation,) but such remarks occur principally in the last book. There is likewise, in the present case, another pencil alteration, which he would not have made if he had seen the figure accurately drawn. F and G are the summits of two ordinates, that express the relative quantities of centripetal force, and he has continued the curve, to which they belong, so as to make it pass through A, which is manifestly wrong: for the central force would thus be represented, as if it vanished at the point from which the body was to fall. His correction, however, was probably subsequent to the publication of the second edition of the Principia, for no alteration is there made in the diagram: and this is the more remarkable, since the figures in this edition were not printed, as in most instances they might have been, from Halley's blocks, (if still in existence,) but were cut anew for the especial purpose[s].

Among Dr. Gregory's papers is one, dated 1691, in which he says, "Mr. Fatio designs a new edition of "Mr. Newton's book in folio, wherein among a great "many notes and elucidations in the preface, he will "explain Gravity, acting, as Mr. Newton shews it "doth, from the rectilinear motions of particles, the "aggregate of all which is but a given quantity of "matter dispersed in a given space. He says that "he hath satisfied Mr. Newton, Mr. Hugens, and Mr. "Halley in it." But he must have been mistaken in thinking so; for, immediately under this note, Gregory has written, evidently at some subsequent period, that Newton and Halley both disapproved of "Mr. Fatio's "manner of explaining gravity." Indeed it could not have been otherwise; for Hugens says[t] that it was like Varignon's, who imagined that the greater or less weight of bodies depended on the relative height of the superincumbent columns of the atmosphere. This may have been one reason for Newton's unwillingness to comply with his proposal for republishing the Principia. It is not improbable, likewise, that Fatio, with

[s] New blocks were again cut for the third edition, even where there was no variation to be made in them.

[t] Hugenii aliorumque Ex. Math. fasc. i. p. 227.

considerable talents and acquirements, may have betrayed the high value which he set upon them. It is too much to be feared, that, if he had engaged in this work, the editor would have thought more of himself than of his author. In his copy of the third edition of the Principia, he has written the following lines at the bottom of p. 430.

Optima fundamenta jacis, Newtone; sed exis
Infelix operis summa. Systemata mundi
Corrumpis; nec digna Deo, nec consona fingis.

And he has then added,

Tu nebulas remove: facies tua clara nitebit.

Newtoni Responsum.

Si bona fundamenta; tamen structura bene illis
Non quadret, sed obliqua labet minitata ruinam:
Ergo cadat: templumque novum cœleste resurgat.
Sed maneat jactum solide fundamen in ævum.
Insculptoque basi NEWTONI nomine; in ipso
Culmine scribatur, FACIUS multum addidit ædi:
Ædi, quæ immensi typus est templi Omnipotentis.

Hugens, indeed, who was evidently desirous to see the republication, and was probably not aware of all the particulars of the case, thought [u] Newton fortunate in the offer of such assistance: and Leibnitz, in writing to him in Feb. 1692 [v], speaks with approbation of Fatio's intention. But all the notes, which he has written in his copy of the first edition of the Principia, would not, if printed in 4to, fill more than a few sheets, and there can be no reason to regret that the task was reserved for Cotes. In the following December Hugens says that the work was delayed by the number of alterations which were found to be necessary in it. This information he gave to the Marq. de l'Hospital, who was equally desirous with himself for

u "M. Newton seroit heureux." fasc. ii. p. 124.

v Hugenii Exer. Math. fasc. i. p. 121.

the publication [x]; and in June 1694, he wrote again to him on the subject, and said that all expectations of the new edition had been suspended by the author's illness. This refers to the attack of which Hugens had given Leibnitz the account in the same month [y], and which has been the subject of so much discussion. Leaving the decision of this question to the able hands, which have been engaged in it, I will only remark, that most probably the duration, as well as the nature of the disease were much exaggerated in Colm's report [z], and that it did not prevent Newton's contemporaries from still looking to himself for the improvement of the Principia. In the Journals of the Royal Society we find, 1694, Oct. 31, "a letter from Mr. Leibnitz to "Mr. Bridges was produced and read, wherein he re-"commends to the Society to use their endeavours to "induce Mr. Newton to publish his further thoughts "and improvements on the subject of his late book, "Principia Philosophiæ Mathematica, and his other "Physical and Mathematical discoveries, lest by his "death they should happen to be lost."

Although this application was not attended with any immediate success, the want of a new edition continued to be felt, and the hopes were cherished of its being undertaken by the author himself. Leibnitz wrote to Jo. Bernoulli in Jan. 1697, and tells him, "Dominus "Groningius nihil de te, vel de manuscriptis Huge-"nianis: unde ego quoque dissimulavi talia mihi ex "te esse nota, quæ ipse attingere noluerat. Præsertim "cum sese novam Newtoniani operis editionem moliri "scripserit: quam tamen dissuasi, quod de ea cogitare "Newtonum ipsum intellexissem. Et suspicor Huge-"niana ibi adjicere voluisse [a]." From Bernoulli's an-

[x] Ibid. fasc. i. p. 243, 247.
[y] Ibid. p. 319. 190.
[z] Ibid. fasc. ii. p. 171.
[a] Leibnitii et Bernoulli Comm Phil. et Math. t. i. p. 241.

swer [b], it appears that the person here spoken of was wholly unequal to the task, that he wished to undertake, and he probably was guided by the good advice which was given him.

Newton, though he could not for many years be induced to undertake what was so anxiously desired, appears, from the first, to have been most ready in assisting those, who were desirous of making themselves acquainted with the result of his investigations. It was most probably with the view of giving to Locke the outline of his fundamental doctrines, that, in March 1689, he drew up the paper, which lord King has published [c]. In 1692 Bentley was the first to preach at Boyle's Lecture: he took for his subject the Confutation of Atheism; and his seventh as well as his eighth sermon is principally derived from the discoveries of Newton. This use of his great work, for the furtherance of religion, must have been most gratifying to the author; and among Bentley's papers a manuscript was found in Newton's hand, which contained directions for the books to be read in preparation for the study of the Principia [d], and which was most probably drawn up for this object. Whiston [e] speaks of the dis-

[b] Ibid. p. 245.

[c] Life of Locke, 8vo. vol. i. p. 388.

[d] Bishop Monk's Life of Bentley, 4to. p. 31.

[e] Memoirs, vol. i. p. 32. Newton lecturing on his discoveries in Physics and Optics, only conveys to us the idea of the instructor of mankind opening the paths of knowledge to those, who were most immediately connected with him; but there is something melancholy in the thought of such a man's time being taken up in teaching the elements of Mathematics. The beginning of the Arithmetica Universalis is of this kind; but so much of the work is taken up with higher speculations, that we are reconciled to what seems only introductory to them. There is however in the archbishop's library at Lambeth a manuscript (No. 592) entitled, "Trigonometriæ fundamenta ab Isaaco Newton, "Matheseos Professore Lucasiano in Academia Cantab., data "1683." It is not in his handwriting, but contains the notes of common propositions, which had been written out by some pupil.—And Flamsteed (p. 166)

coveries in the Principia as delivered by Newton in the public schools at Cambridge: and Cotes mentions to Jones the first draught in manuscript of the Principia, "as he read it in his lectures [e]." If any difficulties occurred, Fatio felt (see p. 89) that he could get them cleared up by the original authority; and it is evident, from the number of his manuscripts which passed into the hands of Collins and Jones, that he could not have been reserved in the communication of them.

David Gregory appears, likewise, to have been under considerable personal obligation of this kind; and he certainly was entitled to it by the time and pains, which he employed upon the study of the Principia. He did not read it cursorily or partially, but went regularly through the whole, noting what occurred to him, and examining every step in detail. His papers on this subject extend to a very closely written folio of 213 pages: the sheets were originally used separately, and Gregory himself collected them together into a volume, because his handwriting may occasionally be traced on the guards, to which they are pasted. These must be the notes referred to in the Supplement to the Encyclopædia Britannica, which have been already mentioned at p. 79: for the article says, that "there is a complete copy of these observations pre-"served in the library of the University of Edinburgh "presented to it by Dr. James Gregory, the present "professor of the Practice of Medicine:" and there is no doubt that the transcript in question was taken from the volume now under consideration. Dr. James Craufurd Gregory, in speaking of it, describes "a "running date" to the several parts, "from 1687 to

could taunt him with reading "mathematics for a salary at "Cambridge;" as if any thing but a deep sense of public duty could have induced him to undergo such unworthy drudgery.

[e] Gen. Dict. iv. p. 444.

"1697[f]," and this contributes to the right understanding of the character belonging to the manuscript. In the first place, it gives us the times of Gregory's applying himself to the study. The earliest period stated is against the 3rd cor. to the 17th prop. of the first book, and it is Sept. 22, 1687: the days which are noted then go on with intervals, but still regularly to April 3, 1688, which stands against the 1st cor. of the 44th proposition. An interruption then took place for above four years, and he did not return to the work till after he became Savilian Professor of Astronomy; for we next meet against the 48th and 49th propositions, "Oxoniæ, 23 Dec. 1692;" and the dates then proceed with certain interruptions to the end; the last being, "Oxonii, 29 Jan." (1694). All this makes it perfectly clear that there is not the least ground for the story of these notes having been written as suggestions for Newton, while he was preparing the Principia for the press: if further evidence could possibly be wanted, it would be found in the manuscript, at the very beginning, containing distinct references to the pages of the printed book; and these references are evidently contemporaneous with the other writing[g]. Dr. J. C. Gregory's last date goes a little beyond those which have just been mentioned; it belongs, however, not to the regular work, but to some occasional addition, made after the rest had been completed. In the same manner

[f] Trans. of R. S. of Edinb. vol. xii. p. 71.

[g] It makes no further for the present argument, than an additional proof of the looseness, with which the account is drawn up in the Enc. Brit.; but it may be right to add, that the writer of it is wholly erroneous, when he talks of "paragraphs in the "handwriting of Hugens, rela-"tive to his theory of light." References occur to Hugens with respect to this subject, but there is not a sentence—probably not a word—of his insertion, from the beginning to the end of the manuscript. This, of course, would not be asserted without a careful search having been made for this especial purpose.

there are two full sides inserted between p. 90 and 91, with the following memorandum: "Quoniam varii errores in propositiones 37 et 38[h] irrepsere, illos omnes restitutos hic apponam, prout in autoris exemplari inveni, ineunte Maio 1694, dum Cantabrigiæ hærerem, consulendi divini autoris gratia." And there is one notice of a still later date on the first Lemma of the third book: "Auctor oblitus est penitus ratiocinationis primæ, cui hanc propositionem inædificavit, posteaque aliam excogitabat, quamque etiam amisit, ut mihi dixit 29 Octobris 1704;" to which is added in ink of very different tint, "recuperavit ratiocinationem primam, eamque mihi ostendit, ☉ 21 Junii 1706, olim scriptam."

In lord Macclesfield's collection there is a letter from N. Saunderson to Jones, from which it appears that in the beginning of 1714 it was proposed to publish these remarks. The plan failed, and most probably the author himself would not have wished them to go out to the world, without a complete and careful revision. When Horsley, however, was preparing his edition of Newton's works, he had the use of them; and in the second volume, which contains the Principia, there are several references[i] "notis MSS. Davidis Gregorii." The first of them is accompanied by an extract, which is evidently taken verbatim from the present volume.

The Encyclopædia Britannica goes on to say, that the work contains "many sublime mathematical discussions." This, however, is hardly applicable to the nature of these notes, and was probably intended for some

[h] Of the second book; the place in which Fatio says that he convinced Newton of his mistakes. But neither he nor Gregory seems to have entered into the difficulty, which Bernoulli pointed out in the 10th proposition of the same book.

[i] P. 51, 124, 134, 225.

other papers, which are transcribed at the end of them. These are the "twelve folio pages" which Dr. J. C. Gregory mentions that he found, among his family papers, "in the handwriting of Newton, con-"taining, in the form of additions and scholia to some "propositions in the third book of the Principia, an "account of the opinions of the ancient philosophers "on gravitation and motion, and on natural theology, "with various quotations from their works." The part, which is connected with theology, has been printed by Dr. Gregory [k], and the others contain a very large collection of extracts from ancient writers, which must have required much trouble to collect and transcribe.

Among Dr. Gregory's papers is one containing memoranda about his Astronomy [l]; it is dated, 21 May 1701; and the sixth is, "to consult Mr. Newton about "a preface, and upon the whole:" it is not improbable therefore that Newton, having abandoned the intention of printing these scholia, (to which no allusion is made in the second edition of the Principia,) gave them to him upon this occasion, with liberty to use them as he thought proper. Indeed they may be traced almost verbatim in the Preface to Gregory's Astronomiæ Physicæ and Geometricæ Elementa from the second to the last page. The parts may be known by references being attached to them at the foot of the pages, which are an abridgment of what Newton had fully copied out. Harassed as he was by opposition to his philosophy, his object was evidently to shew that many portions of it had occurred to the philosophers of antiquity. Amidst a mass of fanciful obscurity, curious physical truths often break out in their writings, and when these were elicited from minds

[k] Trans. R. S. Edin. vol. xii. p. 67, 69, 70. [l] App. XXIII. p. 79.

untutored to legitimate philosophising, they became good proofs of there being nothing unnatural in the opinions which they illustrated. So far his scholia afforded an answer to those, who opposed his doctrine as unsupported by the common sense of mankind; and he therefore annexed them to particular [m] propositions of his third book; but the Preface seems to go beyond this, and to convert these partial views into a general system, that had been well established; which is far beyond what could ever be admitted.

In one of Thomas Hearne's memorandum books [n] there is the following notice. "1705, Dec. 10. Sir Isaac "Newton has complained that Dr. Gregory, who bor-"rowed most of the best materials in his book of "Astronomy from Sir Isaac, has made little or no "mention of him, but just in the preface; so that Sir "Isaac, fearing lest that, in the process of time, Dr. "Gregory's book might happen to be printed without "this preface, and consequently he be thought the au-"thor of what Sir Isaac himself had before him dis-"covered, resolved to make another edition of his book "called Principia Math." It is true that Gregory's

m The subjects are,

Prop. IV. Lunam esse corpus, grave, terrestre, et vi gravitatis in Terram nostram casuram esse, nisi vi motus circularis cohiberetur.

Prop. V. Gravitatem in Solem et Lunam æque ac in Terram fieri.

Prop. VI. Corpora omnia, quæ circa Terram sunt,...gravia i Terram, et eorum gravitatem proportionalem esse quantitati materiæ ex qua constant.

Prop. VII. Gravitatem non fieri per vim puncti alicujus, in quod gravia undique tendunt, sed per vim materiæ totius, in globo Terræ, corpora omnia ad se trahentis.

Prop. VIII. Qua proportione gravitas, recedendo a planetis, decrescit, veteres non satis explicuerunt. Adumbrasse tamen videntur per harmoniam sphærarum cælestium.

Prop. IX. Hactenus proprietates gravitatis explicui. Causam ejus minime expendo. Dicam tamen quid veteres hac de re senserint.

n Vol. vi.

work was greatly indebted to the Principia: it was the first in which Newton's discoveries connected with Astronomy were given in detail to the public; but Hearne was a man of strong prejudices, which were not favourable to Gregory; for which some allowance must therefore be made in receiving his report. Flamsteed had complained[o] in 1700 of Newton, as if he had been (in his opinion) too communicative to Gregory, who was also entrusted in 1702 to publish[p] Nova et accurata motuum lunarium Theoria ab Is. Newtono Anglice conscripta, et Latine reddita a D. Gregorio. From his paper of memoranda, already alluded[q] to, it may be seen how much he looked to his patron for advice, and it is not likely that, exactly at the same time, he should be either unwilling to acknowledge the obligation, or to arrogate to himself what did not justly belong to him. Newton indeed was anxious about his own fame, but he could not have had any just ground either for jealousy of Gregory, or for suspecting that the Elements of Astronomy could have a more durable existence, much less that they could surpass the Principia in credit. Hearne's evidence, however, is good to a certain point; for it proves that expectations were entertained in 1705 of Newton's having resolved upon printing a new edition of his work.

The eyes indeed of the world had been long upon him. In 1700 Leibnitz, under circumstances which make his words the more remarkable, took the opportunity of saying in the Leipsic Acts[r], "Etsi post tanta "jam beneficia in publicum collata, iniquum sit aliquid "a Domino Newtono exigere, quod novum quærendi "laborem postulet, non possum tamen mihi temperare,

[o] Baily's Flamsteed, p. 174.

[p] Lalande Bibliographie Astron. p. 345.

[q] App. XXIII. p. 79.

[r] P. 203.

"quin, hac oblata occasione, maximi ingenii mathematicum publice rogam, ut, memor humanorum casuum et communis utilitatis, diutius ne premat præclaras reliquas et jam paratas meditationes suas, quibuscum scientias mathematicas, tum præsertim naturæ arcana porro illustrare potest. Quod si nulla movet tantarum gloria rerum, (quanquam vix quicquam ei, quam nactus est, addi potest,) illud saltem cogitet, generosum animum nihil magis ad se pertinere putare, quam ut optime de humano genere mereatur." Many years, however, elapsed before this wish was gratified. The task could not be accomplished to Newton's satisfaction, till he had enucleated the moon's motions according to the laws of gravitation: and Flamsteed's Correspondence proves how assiduously he worked at it. He appears to have engaged in the task about the year 1692, but in 1696 he became Warden, and in 1699 Master of the Mint. The claims on his time, occasioned by the duties of these offices, were a great interruption to his scientific pursuits, and, although Flamsteed did not understand it, there was much upright feeling in his unwillingness to be thought negligent of "the King's business[s]."

In the mean time it had become difficult to procure the book. Sir William Browne says, that when a young man in college, he "gave no less than two guineas for one," which was then thought a very cheap purchase[t]; the father of Dr. James Moor, of Glasgow, from inability to lay out sufficient money, was induced to transcribe the whole with his own hand[v]; and Cotes says in his Preface, the copies of the first edition had become "rarissima admodum et immani pretio coe-

[s] P. 166.

[t] Quoted by Nichols in his Literary Anecdotes, vol. iii. p. 322.

[v] Encycl. Brit. (1817) vol. xv. p. 443.

"menda" They are not now of such extreme scarcity and high price, but that only proves more strongly how much they must then have been prized for their intrinsic value.

There is a tradition that Gregory was to have had the superintendence of the second edition, if his death had not prevented it; but there does not appear to be any written evidence to corroborate the account: it was in June 1709 (precisely the year in which he died) that the papers were put into Cotes's hands for this purpose, and we are informed that Bentley had long solicited and urged Newton to the undertaking before he could succeed in prevailing on him to engage in it[u]. From a letter of Cotes to Jones in Sept. 1711, it appears that the progress was then but slow. "I "am desirous," he says, "to have the edition of the "Principia finished; but I never think the time lost, "when we stay for Sir Isaac's further corrections and "improvements of so very valuable a book, especially "when this seems to be the last time he will concern "himself with it. I am sensible his other business "allows him little time for these things, and therefore "I cannot hasten him so much as I might otherwise "do. I am very well satisfied to wait till he has lei-"sure." As, however, the work advanced, Newton became more deeply interested in its completion, and N. Saunderson afterwards wrote to Jones and said, "Sir Is. Newton is much more intent upon his Prin-"cipia than formerly, and writes almost every post "about it, so that we are in great hopes to have it out "in a very little time." This correspondence, amounting to nearly three hundred letters[w], is preserved in Trinity College, Cambridge, and must afford the most valuable and interesting information of the advances

[u] Monk's Life of Bentley, 4to. p. 179.

[w] Ibid. p. 180.

which Newton made from his first brilliant discoveries to the more complete and accurate developement of them.

The impression of this second edition at Cambridge, in 1713, was probably not large. It does not now occur in the market much more frequently than the first, and there is a letter from Reyneau[x] to Jones, in which he mentions that in November 1714, it was difficult to meet with it at Paris. In speaking of Newton he says, "J'avois vu, dans la premiere édition du "savant et profond ouvrage des Principes de la Philo-"sophie Naturelle, les belles applications qu'il y a fait "de ses méthodes à découvrir tout ce qu'il y a de plus "caché dans la nature. La seconde édition en est en-"core si rare ici, que je n'ai pu la voir que des in-"stants par le moyen de ceux, qui ont enlevé ce qui "en étoit venu d'abord." This deficiency of the supply on the continent, may be considered as the cause of the reprint at Amsterdam; and the Journal Littéraire de la Haye (for July and Aug. 1713[y]) says, "une "compagnie de libraires imprime ici Philosophiæ Na-"turalis Principia Mathematica, sur la second édi-"tion qui vient de paroître en Angleterre. Deux "presses roulent continuellement pour avancer cet "ouvrage." It did not, however, come out till 1714, and in 1723 the booksellers at Amsterdam again reprinted the work, with the addition of Newton's Analysis per quantitatum series, which had been published by Jones in 1711.

In the library at Christ Church there is a paper in Horsley's handwriting, on which he seems to have made memoranda of books, which he might wish to examine

[x] The originals of this letter, and of those from Cotes and Saunderson, are in the collection of the Earl of Macclesfield.

[y] P. 483.

for his edition of Newton's works. Among them he sets down copies of the first and second editions of the Principia, with the author's manuscript notes. He does not mention where these books are preserved, but the notice proves that Newton, after all that had been done in 1713, still kept his mind on the improvements which he could make in the work. Thus he collected materials for the third edition, which came out in 1726, under the care of Dr. Henry Pemberton. Newton was eighty-four years of age, and unequal to the exertions which he had made for Cotes, but he seems to have been, even then, very actively alive to the great object which he had in view. Pemberton tells us[z] that he had frequent personal intercourse with him, and that "a great number of letters passed between us "on this account." Such a correspondence, if it could be recovered, would be very valuable, and it can hardly be believed that it has not been carefully preserved. Pemberton died in 1771, and left his printed books to his friend Dr. Wilson, but his papers were most probably included in the residue of his property, which was bequeathed to a gentleman of the name of Miles, who had married his niece. He is described as a timber merchant at Rotherhithe; he appears to have been alive in 1788, and certainly had sons; but whether they are now alive, or where, in that case, they may reside, has not been discovered[a]. When the second edition of Hutton's Dictionary was published in 1815, a life of Pemberton was inserted in it, with particulars of his papers which might lead to the supposition of the writer having drawn them up from an immediate knowledge of the originals. But this is not the case, the whole being taken from the biographical Preface,

[z] Preface to View of Sir I. Newton's Philosophy.

[a] See Philosophical Magazine for May 1836, vol. viii. p. 441.

which was prefixed by Dr. Wilson to Pemberton's Course of Chemistry (1771), and we have no notices, which bring the account nearer to our own time. The hope, however, must not be abandoned of these records being yet traced out; and it is hardly possible, without them, to complete the history of Newton's last efforts for the improvement of his Principia.

APPENDIX.

CONTENTS.

* The asterisk is prefixed to those articles, which are now printed for the first time.

ERRATA IN APPENDIX.

P. 1.	lin. 9.	ad	*lege*	ut
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APPENDIX.

No. I.

ISAACI NEWTONI

PROPOSITIONES DE MOTU[a].

Definitiones.

1. VIM centripetam appello, qua corpus impellitur vel attrahitur versus aliquod punctum, quod ad Centrum spectatur.

2. Et vim corporis, seu corpori insitam, qua id conatur perseverare in motu suo secundum lineam rectam.

3. Et resistentiam, quæ est medii regulariter impedientis.

Hypotheses.

1. Resistentiam in proximis novem propositionibus nullam esse, in sequentibus esse ut medii densitas et celeritas conjunctim.

2. Corpus omne sola vi insita uniformiter secundum lineam rectam in infinitum progredi, nisi aliquid extrinsecus impediat.

3. Corpus in dato tempore, viribus conjunctis, eo ferri quo viribus divisis in temporibus æqualibus successive.

4. Spatium quod corpus, urgente quacunque vi centripeta, ipso motus initio describit, esse in duplicata ratione temporis.

[a] From the Register of the Royal Society, vol. vi. p. 218.

Febr. 168$\frac{4}{5}$, cf. p. 24, post.

cf. p. 25-6, ante.

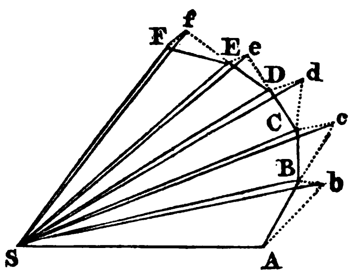
LEMMATA.

1. Parallelogramma omnia, circa datam ellipsin descripta, esse æqualia inter se. Patet ex Conicis.

2. Quantitates differentiis suis proportionales sunt continue proportionales. Ponatur A ad A – B ut B ad B – C, et C ad C – D, &c. dividendo fiet A ad B, ut B ad C, [ut C] ad D, &c.

THEOR. I.

Gyrantia omnia Radiis ad centrum ductis areas temporibus proportionales describere. i.e. a central force ma particle descr. eq. areas i ti

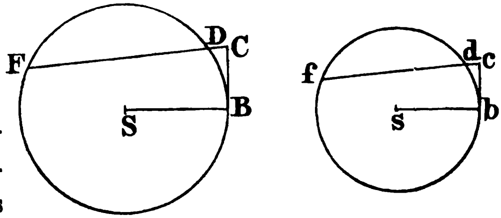


Dividatur tempus in partes æquales, et prima temporis parte describat corpus vi insita rectam AB; idem secunda temporis parte, si nihil impediet, recta pergeret ad c, describens rectam Bc æqualem ipsi AB, adeo ut radiis AS, BS, cS ad centrum actis, confectæ forent areæ æquales ASB, BSc. Verum ubi corpus venit ad B, agat vis centripeta impulsu unico at magno, faciatque corpus a recta Bc deflectere et pergere in recta BC. Ipsi BS parallela agatur cC occurrens BC in C, et, completa secunda temporis parte, corpus reperietur in C. Junge SC et triangulum SBC ob parallelas SB, Cc æquale erit triangulo SBc, atque adeo etiam triangulo SAB. Simili argumento, si vis centripeta successive agat in C, D, E, &c. faciens corpus singulis temporis momentis singulas describere rectas CD, DE, EF, &c., triangulum SCD triangulo SBC, et SDE ipsi SCD, et SEF ipsi SDE æquale erit. Æqualibus igitur temporibus æquales areæ describuntur. Sunto jam hæc triangula numero infinita, et infinite parva, sic ut singulis temporis momentis singula respondeant triangula, cogente vi centripeta sine remissione, et constabit Propositio.

THEOR. II.

Corporibus in circumferentiis circulorum uniformiter gyrantibus, vires centripetas esse ut arcuum simul descriptorum quadrata applicata ad radios circulorum.

Corpora B, b, in circumferentiis circulorum BD, Bd gyrantia, simul describant arcus



BD, bd. Sola vi insita describerent tangentes BC, bc, his arcubus æquales; vires centripetæ sunt quæ perpetuo retrahunt corpora de tangentibus ad circumferentias, atque adeo hæ sunt ad invicem ut spatia ipsis superata CD, cd; id est productis CD, cd ad F et f ut $\frac{BC^2}{CF}$ ad $\frac{bc^2}{cf}$, sive ut $\frac{BD^2}{\frac{1}{2}CF}$ ad $\frac{bd^2}{\frac{1}{2}cf}$; loquor de spatiis BD, bd minutissimis, inque infinitum diminuendis, sic ut pro $\frac{1}{2}$ CF, $\frac{1}{2}$ cf, scribere liceat circulorum radios SB, sb, quo facto constabit Propositio.

Cor. 1. Vires centripetæ sunt ut celeritatum quadrata applicata ad radios.

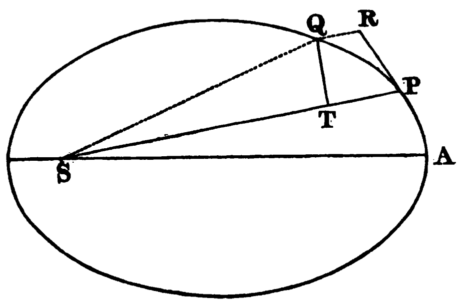
2. Et reciproce ut quadrata temporum periodicorum applicata ad radios.

3. Unde si quadrata temporum periodicorum sunt ut radii circulorum, vires centripetæ sunt æquales; et vice versa.

4. Si quadrata temporum periodicorum sunt ut quadrata radiorum, vires centripetæ sunt reciproce ut radii.

5. Si quadrata temporum periodicorum sunt ut cubi radiorum, vires centripetæ sunt reciproce ut quadrata radiorum.

THEOR. III.



Si corpus circa centrum S gyrando describat lineam quamvis curvam APQ; et si tangat recta PR curvam illam in puncto quovis P, et ad tangentem ab alio quovis puncto Q agatur QR distantiæ SP parallela, ac demittatur QT perpendicularis ad distantiam SP; dico quod vis centripeta sit reciproce ut solidum $\frac{SP^2 \times QT^2}{QR}$, si modo solidi illius ea semper sumatur quantitas, ubi coeunt puncta P et Q. Namque in figura indefinite parva QRPT, lineola QR dato tempore est ut vis centripeta, et data vi ut quadratum temporis, atque adeo neutro dato ut vis centripeta et quadratum temporis conjunctim, id est, ut vis centripeta semel, et area SPQ tempori proportionalis, seu duplum ejus $SP \times QT$, bis. Applicetur hujus proportionalitatis pars utraque ad lineolam QR, et fiet unitas ut vis centripeta et $\frac{SP^2 \times QT^2}{QR}$ conjunctim, hoc est vis centripeta reciproce ut $\frac{SP^2 \times QT^2}{QR}$. Q. E. D.

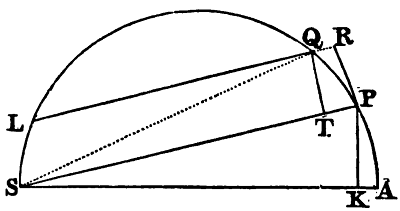
Cor. Hinc si detur figura quævis, et in ea punctum ad quod vis centripeta dirigitur, invenire potest lex vis centripetæ, quæ corpus in figuræ illius Perimetro gyrare faciet. Nimirum computandum est solidum

$\frac{SP^2 \times QT^2}{QR}$ huic vi reciproce proportionale. Ejus rei dabimus exempla in Problematis sequentibus.

PROB. I.

Gyrat corpus in circumferentia circuli; requiritur lex vis centripetæ tendentis ad punctum aliquod in circumferentia.

Esto circuli circumferentia SQPA, centrum vis centripetæ S, corpus in circumferentia latum P, locus proximus in quem movebitur Q. Ad SA diametrum et SP demitte perpendicula PK, QT; et per Q ipsi SP parallelam age LR, occurrentem circulo in L, et tangenti PR in R. Erit RP^2 (hoc est $QR \times RL$) ad QT^2 ut SA^2 ad SP^2. Ergo

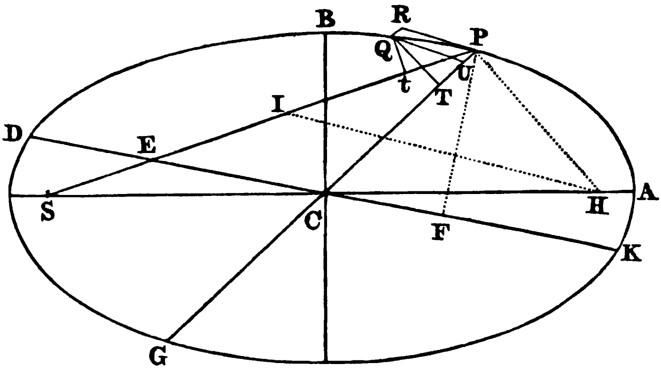


$\frac{QR \times RL \times SP^2}{SA^2} = QT^2$. Ducantur hæc æqualia in $\frac{SP^2}{QR}$, et punctis P et Q coeuntibus scribatur SP pro RL, sic fiet $\frac{SP^5}{SA^2} = \frac{QT^2 \times SP^2}{QR}$. Ergo vis centripeta reciproce est ut $\frac{SP^5}{SA^2}$, id [est,] ob datam SA^2, ut quadrato-cubus distantiæ SP. Q. E. I.

Cæterum in hoc casu et similibus concipiendum est, quod postquam corpus pervenerit ad centrum S, id non amplius redibit in orbem, sed abibit in tangente. In spirali, quæ secat radios omnes in dato angulo, vis centripeta tendens ad spiralis principium est in ratione triplicata distantiæ reciproce; sed in principio illo recta nulla, positione determinata, spiralem tangit.

PROB. II.

Corpus gyrat in ellipsi veterum; requiritur lex vis centripetæ tendentis ad centrum ellipseos.



Sunto CA, CB semiaxes ellipseos; GP, DK diametri conjugatæ; PF, QT perpendicula ad diametros; QU ordinatim applicata ad diametrum GP; et QUPR parallelogrammum. His constructis, erit $PU \times UG$ ad QU^2 ut PC^2 ad CD^2, et $\frac{QU^2}{QT^2} = \frac{PC^2}{PF^2}$, et conjunctis rationibus $\frac{PU \times UG}{QT^2} = \frac{PC^2}{CD^2}$ et $\frac{PC^2}{PF^2}$: id est UG ad $\frac{QT^2}{PU}$ ut PC^2 ad $\frac{CD^2 \times PF^2}{PC^2}$. Scribe QR pro PU et $BC \times CA$ pro $CD \times PF$, necnon (punctis Q et P coeuntibus) 2PC pro UG, et ductis extremis et mediis in se mutuo fiet $\frac{QT^2 \times PC^2}{QR} = \frac{2BC^2 \times CA^2}{PC}$, et ergo vis centripeta reciproce ut $\frac{2BC^2 \times CA^2}{PC}$. Id est, ob datum $2BC^2 \times CA^2$, ut $\frac{1}{PC}$. Hoc est directe ut distantia PC. Q. E. I.

PROB. III.

Corpus gyrat in ellipsi; requiritur lex vis centripetæ tendentis ad umbilicum.

Esto ellipseos superioris umbilicus S, agatur SP secans ellipseos diametrum DK in E. Patet EP æqualem esse semiaxi majori AC, eo quod, acta ab altero ellipseos umbilico H linea HI, ipsi FC parallela, ob æquales CS, CH æquentur ES, EI, adeo ut EP semisumma sit ipsarum PS, PI, sc. ipsarum PS, PH quæ conjunctim totum axem 2 AC adæquant; ad SP demittatur perpendicularis Qt, et ellipseos latere recto principali (seu $\frac{2\,BC^2}{AC}$) dicto L, erit $L \times QR$ ad $L \times PU$, sicut QR ad PU; id est ut PE seu AC, ad PC. Et $L \times PU$ ad $GU \times PU$, ut L ad GU. Et GU et UP ad QU^2, sicut CP^2 ad CD^2. Et QU^2 ad QX^2 [a] fiat ut M ad N. Et QX^2 est ad Qt^2, sicut EP^2 ad PF^2; id est ut CA^2 ad PF^2 sive CD^2 ad CB^2. Et conjunctis his omnibus rationibus erit $\frac{L \times QR}{Qt^2}$ æqualis $\frac{AC}{PC} \times \frac{L}{GU} \times \frac{CP^2}{CD^2} \times \frac{M}{N} \times \frac{CD^2}{CB^2}$, [b] id est ut $\frac{AC \times L \text{ seu } 2\,BC^2}{PC \text{ et } GU} \times \frac{CP^2}{CB^2} \times \frac{M}{N}$ sive ut $\frac{2PC}{GU} \times \frac{M}{N}$; sed punctis Q et P coeuntibus rationes $\frac{2\,PC}{GU}$ et $\frac{M}{N}$ fiunt æqualitatis; ergo et ex his composita ratio $\frac{L \times QR}{Qt^2}$. Ducatur pars utraque in $\frac{SP^2}{QR}$ et fiet $\frac{SP^2 \times Qt^2}{QR}$ $[= L \times SP^2]$. Ergo vis centripeta reciproce est ut $L \times SP^2$, id est in ratione duplicata distantiæ. Q.E.I.

Schol.

Gyrant ergo planetæ majores in ellipsibus habentibus umbilicum in centro solis; et radiis ad solem ductis, describunt areas temporibus proportionales, omnino ut supposuit Keplerus. Et harum ellipseon

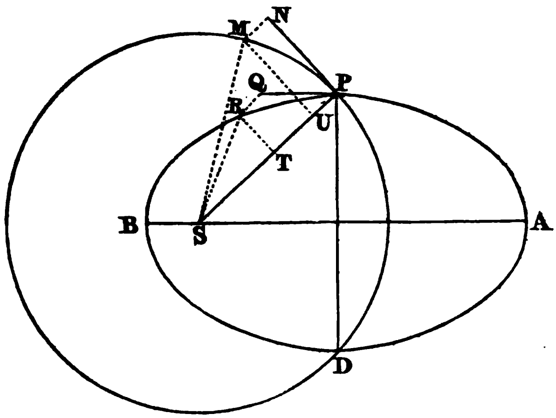
a X is the point in which QU cuts SP.

b × is here written in several instances +, being used in the same sense as "et" a few lines higher—"GU et UP."

latera recta sunt $\frac{Qt^2}{QR}$; punctis P et Q spatio quam minimo et quasi infinite parvo distantibus.

THEOR. IV.

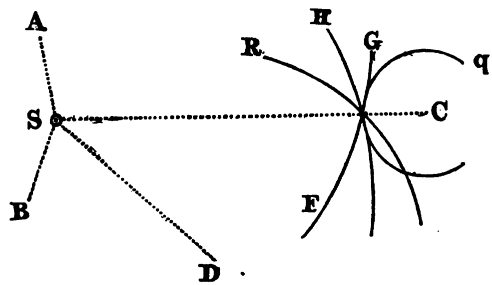
Posito quod vis centripeta sit reciproce proportionalis quadrato distantiæ a centro, quadrata temporum periodicorum in ellipsibus sunt ut cubi transversorum axium.



Sunto ellipseos axis transversus AB, axis alter PD latus rectum L, umbilicus alteruter S. Centro S intervallo SP describatur circulus PMD. Et eodem tempore describant duo corpora gyrantia arcum ellipticum PR et circularem PM, vi centripeta ad umbilicum S tendente. Ellipsin et circulum tangant PQ et PN in puncto P. Ipsi PS parallelæ agantur QR, MN tangentibus occurrentes in Q et N. Sint autem figuræ PQR, PMN indefinite parvæ, sic ut (per Schol. Prob. III.) fiat $L \times QR = RT^2$ et $2SP \times MN = MU^2$; ob communem a centro S distantiam SP, et inde æquales vires centripetas, sunt MN et QR æquales. Ergo RT^2, ad MU^2 est ut L ad 2 SP; et RT ad MU ut medium proportionale inter L et 2 SP [ad 2 SP]: hoc est area SPR ad aream SPM, ut area tota ellipseos ad aream totam circuli. Sed partes arearum singulis momentis sunt ut areæ SPR et SPM, atque adeo ut areæ totæ, et proinde per numerum momentorum multiplicatæ, simul evadent totis æquales.

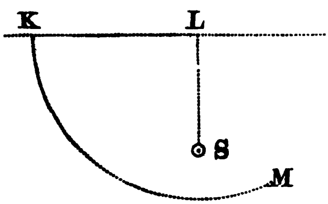
Revolutiones igitur eodem tempore in ellipsibus perficiuntur ac in circulis, quorum diametri sunt axibus transversis ellipseon æquales; [sed] (per Cor. 5. Theor. II.) quadrata temporum periodicorum in circulis sunt ut cubi diametrorum, ergo et in ellipsibus. Q. E. D.

Hinc in systemate cælesti, ex temporibus periodicis planetarum, innotescunt proportiones transversorum axium orbitarum. Axem unum licebit assumere, inde dabuntur cæteri. Datis autem axibus, determinabuntur orbitæ in hunc modum. Sit S locus solis seu umbilicus unus ellipseos, A, B, C, D, loca planetæ observatione inventa, et Q axis transversus ellipseos. Centro A radio Q−AS describatur circulus FG et erit ellipseos umbilicus alter in hujus circumferentia. Centris B, C, D, &c. intervallis Q−BS, Q−CS, Q−DS, &c. describantur itidem alii quotcunque circuli, et erit umbilicus ille alter



in omnium circumferentiis, atque adeo in omnium intersectione communi. Si intersectiones omnes non coincidunt, sumendum est punctum medium pro umbilico. Praxeos hujus commoditas est quod ad unam conclusionem eliciendam adhiberi possunt et inter se expedite comparari observationes quam plurimæ. Planetæ autem loca singula A, B, C, D, &c. ex binis observationibus, cognito telluris orbe magno, invenire docuit Hallæus; si orbis ille magnus nondum satis exacte determinatus habetur, ex eo prope cognito determinabitur planetæ alicujus, puta Martis, propius, deinde ex orbita planetæ per eandem methodum determinabitur orbita telluris adhuc propius. Tum ex orbita telluris determinabitur orbita planetæ multo exactius quam prius. Et sic per vices, donec circulorum intersectiones in uno loco orbitæ utriusque exacte satis conveniant.

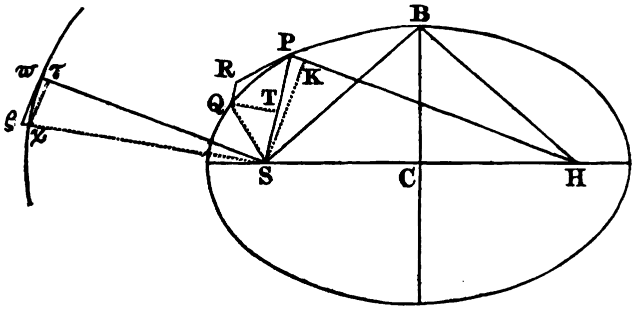
Hac methodo determinare licet orbitas Telluris, Martis, Jovis, et Saturni: orbitas autem Veneris et Mercurii sic. Observationibus in maxima planetarum a sole digressione factis, habentur orbitarum tangentes; ad ejusmodi tangentem KL demittatur a sole perpendiculum SL. Centroque L et intervallo dimidii axis ellipseos describatur circulus KM, erit centrum ellipseos in hujus circumferentia: adeoque descriptis hujusmodi plurimis circulis reperietur in omnium intersectione. Tum, cognitis orbitarum dimensionibus, longitudines horum planetarum exactius ex transitu per discum solis determinabuntur.



PROB. IV.

Posito quod vis centripeta sit reciproce proportionalis quadrato distantiæ a centro, et cognita vis illius quantitate, requiritur ellipsis quam corpus describet de loco dato, cum data celeritate secundum rectam emissum.

Vis centripeta tendens ad punctum S ea sit quæ corpus π in circulo $\pi\chi$, centro S intervallo quovis descripto, gyrare faciat de loco π. Secundum PR emittatur corpus P, et mox inde cogente vi centripeta deflectat in

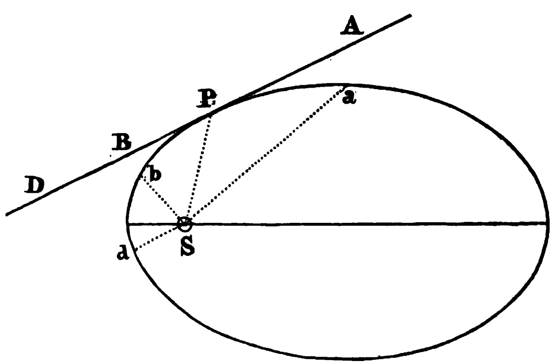


ellipsin PQ. Hanc, igitur, recta PR tangat in P: tangat itidem recta $\pi\rho$ circulum in π, sitque PR ad $\pi\rho$, ut prima celeritas corporis emissi P ad uniformem celeritatem corporis π. Ipsis SP et $S\pi$ parallelæ agantur RQ et $\rho\chi$, hæc circulo in χ, illa ellipsi in Q occurrens, et a Q et χ ad SP et $S\pi$ demittantur perpendicula QT et $\chi\tau$. Est QR ad $\chi\rho$ ut vis centripeta in P ad vim centripetam in π, id est ut $S\pi^2$ ad SP^2, adeoque datur illa ratio. Datur etiam ratio QT ad RP, et ratio RP ad $\rho\pi$ seu $\chi\tau$ et inde composita ratio QT ad $\chi\tau$. De hac ratione duplicata auferatur ratio data QR ad $\chi\rho$ et manebit data ratio $\frac{QT^2}{QR}$ ad $\frac{\chi\tau^2}{\chi\rho}$, id est (per Schol. Prob. III.) ratio lateris recti ellipseos ad diametrum circuli. Datur igitur latus rectum ellipseos, sit istud L; datur præterea ellipseos umbilicus S. Anguli RPS complementum ad duos rectos fiat angulus RPH, et dabitur positione linea PH, in qua umbilicus alter H locatur. Demisso ad PH perpendiculo SK et erecto semiaxe minore BC, est $SP^2 \pm 2KP \times PH + PH^2 = SH^2 = 4BH^2 - 4BC^4 = (SP + PH)^2 - L \times \overline{SP + PH} = SP^2 + 2SP \times PH + PH^2 - L \times \overline{SP + PH}$. Addantur utrobique $\mp 2KP \times PH + L \times \overline{SP + PH} - SP^2 - PH^2$, et fiet $L \times \overline{SP + PH} = 2SP \times PH \mp 2KP \times PH$, seu $SP + PH$ ad PH ut $2SP \mp 2KP$

ad L. Unde datur umbilicus alter H. Datis autem umbilicis una cum axe transverso SP + PH datur ellipsis. Q. E. I.

Hæc ita se habent ubi figura ellipsis est; fieri enim potest ut corpus moveat in parabola vel hyperbola. Nimirum, si tanta est corporis celeritas ut sit latus rectum L æquale $2SP \mp 2KP$, figura erit parabola umbilicum habens in puncto S, et diametros omnes parallelas lineæ PH. Sin corpus majori adhuc celeritate emittatur, movebitur id in hyperbola habente umbilicum unum in puncto S, alterum in puncto H sumpto ad contrarias partes puncti P, et axem transversum æqualem differentiæ linearum PS et PH.

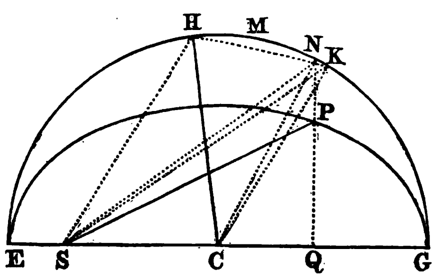
Jam vero, beneficio hujus problematis soluti, planetarum orbitas definire concessum est, et inde revolutionum tempora; et ex orbitarum magnitudine, excentricitate, apheliis, inclinationibus ad planum eclipticæ et nodis inter se collatis, cognocere an idem cometa ad nos sæpius redeat. Nimirum ex quatuor observationibus locorum planetæ [a], juxta hypothesin quod cometa moveatur uniformiter in linea recta, determinanda est ejus via rectilinea. Sit ea APBD, sintque A, P, B, D



[a] Against this word "Qu." is written in Lord Macclesfield's copy.

loca cometæ in via illa temporibus observationum, et S locus solis. Ea celeritate qua cometa uniformiter percurreret rectam AD, finge ipsum emitti de locorum suorum aliquo P, et vi centripeta mox correptum deflectere a recto tramite et abire in ellipsi Pbda. Hæc ellipsis determinanda est ut in superiori problemate. In ea sunto a, P, b, d loca cometæ temporibus observationum. Cognoscantur horum locorum e terra longitudines et latitudines. Quanto majores vel minores sunt hæ longitudines et latitudines observatæ, tanto majores vel minores observatis sumantur longitudines et latitudines novæ. Ex his novis inveniatur denuo via rectilinea cometæ, et inde via rectilinea ut prius [b]. Et loca quatuor nova in via elliptica, prioribus erroribus aucta vel diminuta, jam congruent cum observationibus exacte satis. Aut si forte errores etiamnum sensibiles manserint, potest opus totum repeti. Et ne computa astronomos moleste habeant, suffecerit hæc omnia per praxin geometricam determinari.

Sed areas aSP, PSb, bSd temporibus proportionales assignare difficile est. Super ellipseos axe majore EG describatur semicirculus EHG: sumatur angulus ECH tempori proportionalis; agatur SH eique parallela CK circulo occurrens in K; jungatur HK, et circuli segmento HKM (per tabulam segmentorum vel secus) æquale fiat triangulum SKN; ad EG demittatur perpendiculum NQ, et in eo cape PQ ad NQ, ut ellipseos axis minor ad majorem, et erit punctum P in ellipsi, at-



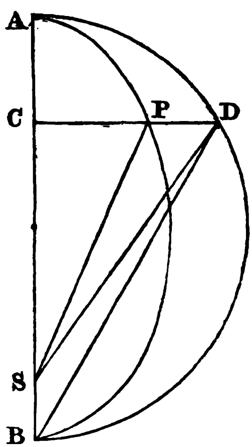
[c] Against this sentence "Qu." is written in Lord Macclesfield's copy.

que acta recta PS abscindetur area ellipseos EPS tempori proportionalis. Namque area HSNM triangulo SNK aucta, et huic æquali segmento HKM diminutæ, fit triangulo HSK id est triangulo HSC æquale. Hæc æqualia adde areæ ESH, fient areæ æquales EHNS et EHC; cum igitur sector EHC tempori proportionalis sit, et area EPS areæ EHNS, erit etiam area EPS tempori proportionalis.

PROB. V.

Posito quod vis centripeta sit reciproce proportionalis quadrato distantiæ a centro, spatia definire quæ corpus recta cadendo datis temporibus describit.

Si corpus non cadit perpendiculariter, describit id ellipsin puta APB, cujus umbilicus inferior, puta S, congruet cum centro terræ. Id ex jam demonstratis constat. Super ellipseos axi majore AB describatur semicirculus ADB, et per corpus decidens transeat recta DPC perpendicularis ad axem, actisque DS, PS, erit area ASD areæ ASP atque adeo etiam tempori proportionalis. Manente axi AB minuatur perpetuo latitudo ellipseos, et semper manebit area ASD tempori proportionalis. Minuatur latitudo illa in infinitum, et orbita APB jam coincidente cum axe AB, et umbilico S cum axis termino B, descendet corpus in recta AC, et area ABD evadet tempori proportionalis. Definietur itaque spatium AC quod corpus de loco A perpendiculariter cadendo tempore dato describit, si modo tempori proportionalis capiatur area ABD et a puncto D ad rectam AB demittatur perpendicularis DC. Q. E. F.



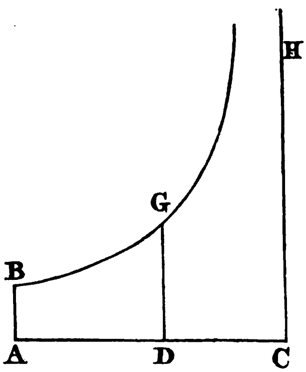
Schol.

Priore problemate definiuntur motus projectilium in aere nostro, [atque[d]] motus gravium perpendiculariter cadentium, ex hypothesi quod gravitas reciproce proportionalis sit quadrato distantiæ a centro terræ, quodque medium aeris nihil resistat. Nam gravitas est species una vis centripetæ.

Prob. VI.

Corporis, sola vi insita per medium similare resistens delati, motum definire.

Asymptotis rectangulis ADC, CH describatur hyperbola secans perpendicula AB, DG, exponatur tum corporis celeritas, tum resistentia medii ipso motus initio per lineam AC, elapso tempore aliquo per lineam DC, et tempus exponi potest per aream ABGD atque spatium eo tempore descriptum per lineam AD. Nam celeritati proportionalis est resistentia medii, et resistentiæ proportionale est decrementum celeritatis; hoc est si tempus dividatur in partes æquales, celeritates ipsarum initiis sunt differentiis suis proportionales. Decrescit ergo celeritas in proportione geometrica dum tempus crescit in arithmetica. Sed tale est decrementum lineæ DC et incrementum areæ ABGD, ut notum est. Ergo tempus per aream et celeritas per lineam illam recte exponitur. Q. E. D.



Porro celeritati atque adeo decremento celeritatis proportionale est incrementum spatii descripti, sed et

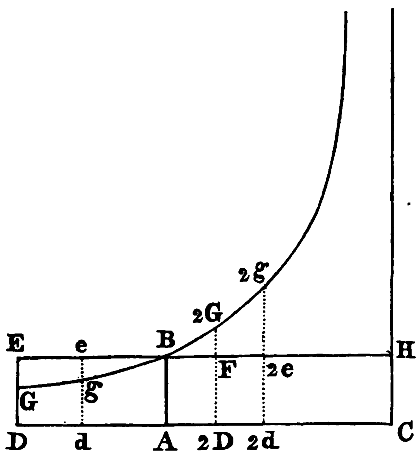
[d] hanc in MS.

decremento lineæ DC proportionale est incrementum lineæ AD. Ergo incrementum spatii per incrementum lineæ AD, atque adeo spatium ipsum per lineam illam recte exponitur. Q. E. D.

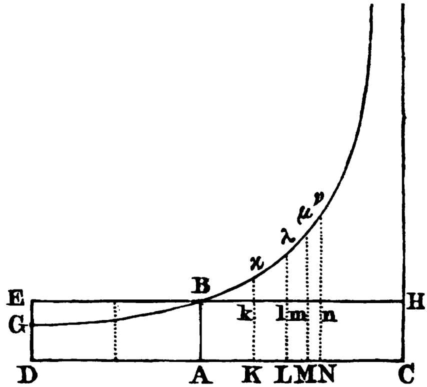
PROB. VII.

Posita uniformi vi centripeta, motum corporis in medio similari recte ascendentis ac descendentis definire.

Corpore ascendente, exponatur vis centripeta per datum quodvis rectangulum BC et resistentia medii initio ascensus per rectangulum BD sumptum ad contrarias partes. Asymptotis rectangulis AC, CH, per punctum B describatur hyperbola, secans perpendicula DE, de in G, g, et corpus ascendendo tempore DGgd describet spatium EGge, tempore DGBA spatium ascensus totius EGB, tempore AB2G2D spatium descensus BF2G atque tempore 2D2G2g2d spatium descensus 2GF2e2g, et celeritas corporis resistentiæ medii proportionalis erit in horum temporum periodis ABED, ABed, nulla, ABF2D, AB2e2d; atque maxima celeritas, quam corpus descendendo potest acquirere erit BC.



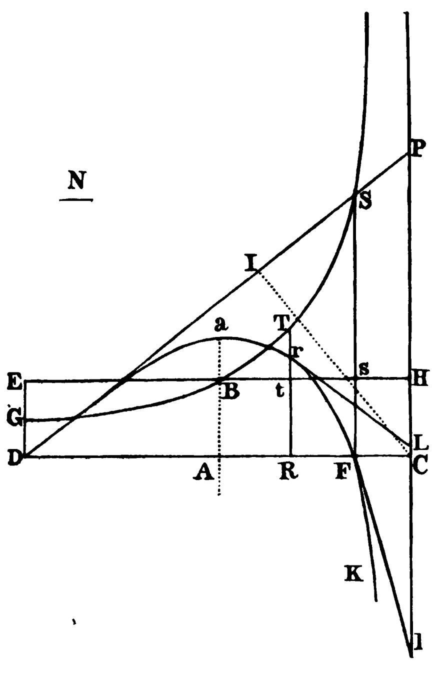
Resolvatur enim rectangulum AH in rectangula innumera Ak, Kl, Lm, Mn, &c. quæ sint ut incrementa celeritatum æqualibus totidem temporibus facta, et erunt Ak, Al, Am, An, &c. ut celeritates totæ, et adeo ut resistentiæ medii in fine singulorum temporum æqualium. Fiat AC ad AK, vel ABHC ad



ABkK, ut vis centripeta ad resistentiam in fine temporis primi, et erunt ABHC, KkHC, LlHC, NnHC, &c. ut vires absolutæ, quibus corpus urgetur, atque adeo ut incrementa celeritatum, id est ut rectangula Ak, Kl, Lm, Mn, &c. et proinde in progressione geometrica. Quare si rectæ Kk, Ll, Mm, Nn, productæ occurrant hyperbolæ in κ, λ, μ, ν, &c. erunt areæ ABKκ, K$\kappa\lambda$L, L$\lambda\mu$M, M$\mu\nu$N æquales, adeoque tum temporibus æqualibus tum viribus centripetis semper æqualibus analogæ. Subducantur rectangula Ak, Kl, Lm, Mn, &c. viribus absolutis analoga, et relinquentur areæ Bkκ, k$\kappa\lambda$l, l$\lambda\mu$m, m$\mu\nu$n, &c. resistentiis medii in fine singulorum temporum, hoc est celeritatibus atque adeo descriptis spatiis analogæ. Sumantur analogarum summæ; et erunt areæ Bkκ, Blλ, Bmμ, Bnν, &c. spatiis totis descriptis analogæ, nec non areæ ABκK, ABλL, ABμM, ABνN, &c. temporibus. Corpus igitur inter descendendum, tempore quovis ABλL, describit spatium Blλ, et tempore L$\lambda\nu$n spatium λlnν. Q. E. D. Et similis est demonstratio motus expositi in ascensu. Q. E. D.

Schol. Beneficio duorum novissimorum problematum innotescent motus projectilium in aere nostro, ex hypothesi quod aer iste similaris sit, quodque gravitas uniformiter et secundum lineas parallelas agat. Nam si omnis motus obliquus corporis projecti distinguatur in duos, unum ascensus vel descensus alterum projectus horizontalis, motus posterior determinabitur per problema sextum, prior per septimum, ut in hoc diagrammate.

Ex loco quovis D ejaculetur corpus secundum lineam aliquam rectam DP, et per longitudinem DP exponatur ejusdem celeritas sub initio motus. A puncto P ad lineam horizontalem DC demittatur perpendiculum PC, ut et ad DP perpendiculum CI, ad quod sit DA ut resistentia medii ipso motus initio ad vim gravitatis. Erigatur perpendiculum AB cujusvis longitudinis, et completis parallelogrammis DABE, CABH, per punctum B asymptotis DC, CP, describatur hyperbola secans DE in G: capiatur linea N ad EG ut est DC ad CP, et ad rectæ DC punctum quodvis R erecto perpendiculo RT, quod occurrat hyperbolæ in T, et rectæ EH in t, in eo capiatur $Rr = \frac{DR \times DE - DRTBG}{N}$;



et projectile tempore DRTBG perveniet ad punctum r, describens curvam lineam DarFK, quam punctum r semper tangit: perveniens autem ad maximam altitudinem (a) in perpendiculo AB, deinde incidens lineam horizontalem DC ad F, ubi areæ DFsE, DFSBG æquantur, et postea semper appropinquans asymptoto PCl. Estque celeritas ejus in puncto quovis r ut tangens rL.

Si proportio resistentiæ aeris ad vim gravitatis nondum innotescit, cognoscantur ex observatione aliqua angulorum, ADP, AFl, in quibus curva DarFK secat lineam horizontalem DC. Super DF constituatur rectangulum DFsE altitudinis cujusvis et describatur hyperbola rectangula ea lege, ut ejus una asymptotos sit DF et areæ DFsE, DFSBG æquantur, et ut sS sit ad EG sicut tangens anguli AFr ad tangentem anguli ADP; ab hujus hyperbolæ centro C ad rectam DP demitte perpendiculum CI, ut et a puncto B, ubi ea secat rectam EH ad rectam DC perpendicularem BA, et habebitur proportio quæsita DA ad CI, quæ est resistentiæ medii ipso motus initio ad gravitatem projectilis. Quæ omnia ex prædemonstratis facile eruuntur. Sicut et alii modi inveniendi resistentiam aeris, quos lubens prætereo. Postquam autem inventa est hæc resistentia in uno casu, capienda est ea, in aliis quibusvis, ut corporis celeritas et superficies sphærica conjunctim, (nam projectile sphæricum esse passim suppono.) Vis autem gravitatis innotescit ex pondere. Sic habebitur semper proportio resistentiæ ad gravitatem, seu lineæ DA ad lineam CI. Hac proportione et angulo ADP determinatur specie figura DarFK; et capiendo longitudinem DP proportionalem celeritati projectilis in loco D, determinatur eadem magnitudine, sic ut altitudo Aa, inter ascensum et casum projectilis, semper sit proportionalis: atque adeo ex longitudine DF, in agro semel mensurata, semper deter [e] tum longitudinem illam DF tum alias omnes dimensiones figuræ DaFK quam projectile describit in agro. Sed in colligendis hisce dimensionibus usurpandi sunt logarithmi pro area hyperbolica DRTBG.

Eadem ratione determinantur etiam motus corporum, gravitate vel levitate et vi quacunque semel et simul impressa, moventium in aqua.

[e] Something here has been omitted by the transcriber.

No. II.[f]

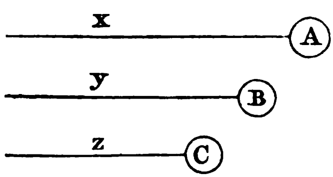
EARLY NOTICE OF FLUXIONS.

Nov. 13, 1665.

An equation being given expressing the relation of two or more lines x, y, z, &c. described in the same time by two or more moving bodies A, B, C, &c. to find the relation of their velocities p, q, r, &c.

RESOLUTION.

Set all the terms on one side of the equation that they become equal to nothing. And first multiply each term by so many times $\frac{p}{x}$ as x hath dimensions in that term.



Secondly, multiply each term by so many times $\frac{q}{y}$ as y hath dimensions in it. Thirdly, multiply each term by so many times $\frac{r}{z}$ as z hath dimensions in it, &c. The sum of all these products shall be equal to nothing. Which equation gives the relation of p, q, r, &c.

Or more generally thus. Order the equation according to the dimensions of x, and, putting a and b for any two numbers, whether rational or not, multiply the terms of it by any part of this progression, viz. $\frac{ap-3bp}{x}$, $\frac{ap-2bp}{x}$, $\frac{ap-bp}{x}$, $\frac{ap}{x}$, $\frac{ap+bp}{x}$, $\frac{ap+2bp}{x}$, $\frac{ap+3bp}{x}$, &c. Also order the equation according to y, and multiply the terms of it by this

[f] From an original paper, in Newton's handwriting, belonging to the Earl of Macclesfield.

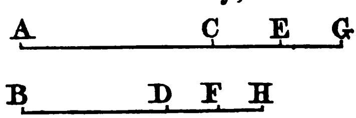
progression $\frac{aq-2bq}{y}$, $\frac{aq-bq}{y}$, $\frac{aq}{y}$, $\frac{aq+bq}{y}$, $\frac{aq+2bq}{y}$, $\frac{aq+3bq}{y}$, &c. Also order it according to the dimensions of z, and multiply its terms by this progression $\frac{ar-3br}{z}$, $\frac{ar-2br}{z}$, $\frac{ar-br}{z}$, $\frac{ar}{z}$, $\frac{ar+br}{z}$, $\frac{ar+2br}{z}$, $\frac{ar+3br}{z}$, &c. The sum of all these products shall be equal to nothing. Which equation gives the relation of p, q, r, &c.

Example 1. If the propounded equation be $x^3 - 2xxy + 4x^2 + 7xyy - y^3 - 103 = 0$, by the precedent rule the first operation will produce $3xxp - 4xyp + 8xp + 7yyp$; the second produceth $-2xxq + 14xyq - 3yyq$; which two added together make $3xxp - 4xyp + 8xp + 7yyp - 2xxq + 14xyq - 3yyq = 0$.

Example 2. If the equation be $x^3 - 2a^2y + zzx - yyx + zyy - z^3 = 0$, the first operation will produce $3pxx + pzz - pyy$; the second produceth $-2aaq - 2yxq + 2zyq$; the third $2zxr + yyr - 3zzr$. The sum of which is $3pxx + pzz - pyy - 2aaq - 2yxq + 2zyq + 2zxr + yyr - 3zzr = 0$.

Demonstration.

If two bodies A and B move uniformly, the one from A to C, E, and G, the other from B to D, F and H, in the same lines, then



are the lines AC and BD, CE and DF, EG and FH, as their velocities p and q: and though they move not uniformly, yet are the infinitely little lines, which each moment they describe, as their velocities are, which they have while they describe them. As, if the body A, with the velocity p, describe the infinitely little line o in one moment, in the same moment the body

B, with the velocity q, will describe the line $\frac{oq}{p}$. For $p : q :: o : \frac{oq}{p}$. So that if the described lines be x and y in one moment, they will be $x+o$, and $y+\frac{oq}{p}$ in the next.

Now, if the equation expressing the relation of the lines x and y be $rx+xx-yy=0$, I may substitute $x+o$ and $y+\frac{oq}{p}$ into the place of x and y, because they, as well as x and y, do signify the lines described by the bodies A and B. By doing so there results $rx+ro+xx+2\,xo+oo-yy-\frac{2\,qoy}{p}-\frac{qqoo}{pp}=0$. But $rx+xx-yy=0$ by supposition: there remains therefore $ro+2xo+oo-\frac{2qoy}{p}-\frac{qqoo}{pp}=0$, and dividing it by o, 'tis $r+2\,x+o-\frac{2\,qy}{p}-\frac{oqq}{pp}=0$. Also those terms in which o is, are infinitely less than those in which it is not. Therefore, blotting them out, there rests $r+2\,x-\frac{2qy}{p}=0$: or $pr+2px=2qy$.

Hence may be observed: first, that those terms ever vanish in which o is not, because they are the propounded equation; secondly, the remaining equation being divided by o, those terms also vanish in which o still remains, because they are infinitely little; thirdly, that the still remaining terms will ever have that form, which by the first preceding rule they should have.

The rule may be demonstrated after the same manner, if there be three or more unknown quantities y, z, &c.

Hitherto we have copied the words of this discourse.

In the same discourse there follows the application of this method to the drawing of tangents, by finding the determination of the motion of any point which describes the curve: and also to the finding the radius of curvity of any curve at any point, by making the perpendicular to the curve move upon it at right angles, and finding that point of the perpendicular which is in least motion. For that point will be the centre of the curvity of the curve at that point, upon which the perpendicular stands.

In another leaf of the same waste-book the same method is set down in other words, and fluxions applied to their fluents are represented by pricked letters. And this paper is dated May 20, 1665.

In another leaf of the same waste-book the method of fluxions is described without pricked letters, and dated May 16, 1666.

In a small tract, dated in October 1666, the same method is again set down without pricked letters, and how to proceed in equations involving facts or surds, and several rules are given for returning back from the fluents to the fluxions; and in doing this the areas of curves are represented by prefixing rectangles to the ordinates. As if x be the absciss, and a, b, represent given quantities, and $\frac{axx - x^3}{ab + xx}$ be the ordinate, the area or fluent of the ordinate is represented by $\square \frac{axx - x^3}{ab + xx}$. After this, the method is applied in this Tract to the solving of problems concerning tangents, curvatures of curves, the greatest or least curvatures, the squaring of curvilinear figures, comparing their areas with the areas of simpler curves, finding the lengths of curves, finding such curves whose lengths may be defined by equations or by the lengths of other curves, and about the centres of gravity of figures. And, for finding the curvature of curves without calculation, the following rule is set down. Let X signify the given equation, * * * * *

No. III.

NEWTON TO ASTON.[g]

The design of a philosophical meeting here Mr. Paget, when last with us, pushed forward, and I concurred with him, and engaged Dr. More to be of it, and others were spoke to partly by me, partly by Mr. Charles Montague; but 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 further on 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, &c.

Cambridge, Feb. 23, 1684-5.

This letter is referred to by Birch in his History of the Royal Society, vol. iv. p. 370.

[g] From the copy in the Letter Book of the Royal Society (vol. x. p. 28). The original has not been met with.

N°. IV.

HALLEY TO NEWTON.[h]

Sir, May 22, 1686.

Your incomparable treatise, entitled Philosophiæ Naturalis Principia Mathematica, was by Dr. Vincent presented to the Royal Society on the 28th past; and they were so very sensible of the great honour you do them by your dedication, that they immediately ordered you their most hearty thanks, and that a council should be summoned to consider about the printing thereof; but by reason of the president's attendance upon the King, and the absence of our vice-presidents, whom the good weather has drawn out of town, there has not since been any authentic council to resolve what to do in the matter: so that on Wednesday last the society, in their meeting, 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 therein be desired, that so it might be gone about with all speed. I am intrusted to look after the printing 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 embellishing thereof, and particularly whether you think it not better, that the schemes should be enlarged, which is the opinion of some here: but what you signify as your desire shall be punctually observed.

There is one thing more that I ought to inform you of, viz. that Mr. Hooke has some pretensions upon

[h] From the original draught in Halley's handwriting, preserved in the guard book (H. 3.) of the Royal Society.

the invention of the rule of the decrease of gravity being reciprocally as the squares of the distances from the centre. 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 'tis possible you may see reason to prefix. I must beg your pardon, that 'tis I that send you this ungrateful account; but I thought it my duty to let you know it, that so you might act accordingly, being in myself fully satisfied, that nothing but the greatest candour imaginable is to be expected from a person, who has of all men the least need to borrow reputation.

This letter was printed from the copy in the Letter Book of the Royal Society (Supplement, vol. iv. p. 340), by Birch, in his History of the Royal Society (vol. iv. p. 484). It is also printed in the Biographia Britannica (vol. v. p. 3225).

N°. V.

NEWTON TO HALLEY.[i]

Sir,

In order to let you know the case between Mr. Hooke and me, I gave you an account of what past between us in our letters, so far as I could remember; for 'tis long since they were writ, and I do not know that I have seen them since. I am almost confident

[i] From the original in the guard-book of the Royal Society (N. 1.)

by circumstances, that Sir Chr. Wren knew the duplicate proportion when I gave him a visit; and then Mr. Hooke, (by his book Cometa written afterwards,) will prove the last of us three that knew it. I intended in this letter to let you understand the case fully; but it being a frivolous business, I shall content myself to give you the heads of it in short, viz. that I never extended the duplicate proportion lower than to the superficies of the earth, and before a certain demonstration I found the last year, have suspected it did not reach accurately enough down so low; and therefore in the doctrine of projectiles never used it nor considered the motions of the heavens; and consequently Mr. Hooke could not from my letters, which were about projectiles and the regions descending hence to the centre, conclude me ignorant of the theory of the heavens. That what he told me of the duplicate proportion was erroneous, namely, that it reached down from hence to the centre of the earth. That it is not candid to require me now to confess myself, in print, then ignorant of the duplicate proportion in the heavens; for no other reason, but because he had told it me in the case of projectiles, and so upon mistaken grounds accused me of that ignorance. That in my answer to his first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles, (rather shortly hinted than carefully described,) in compliment to sweeten my answer, expected to hear no further from him; could scarce persuade myself to answer his second letter; did not answer his third, was upon other things; thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time. That by the same reason he concludes me then ignorant of the rest of the duplicate proportion, he may as well conclude me ignorant of the rest of that theory I had read before in his books. That in one of my papers writ (I cannot say in what year, but I am sure some time before I had any correspondence with Mr. Oldenburg, and that's) above fifteen years ago, the proportion of the forces of the planets from the sun, reciprocally duplicate of their distances from him, is expressed, and the proportion of our gravity to the moon's conatus recedendi a centro terræ is calculated, though not accurately enough. That when Hugenius put out his Horol. Oscil., a copy being presented to me, in my letter of thanks to him, I gave those rules in the end thereof a particular commendation for their usefulness in Philosophy, and added out of my aforesaid paper an instance of their usefulness, in comparing the forces of the moon from the earth, and earth from the sun; in determining a problem about the moon's phase, and putting a limit to the sun's parallax, which shews that I had then my eye upon comparing the forces of the planets arising from their circular motion, and understood it; so that a while after, when Mr. Hooke propounded the problem solemnly, in the end of his Attempt to prove the Motion of the Earth, if I had not known the duplicate proportion before, I could not but have found it now. Between ten and eleven years ago there was an hypothesis of mine registered in your books, wherein I hinted a cause of gravity towards the earth, sun, and planets, with the dependence of the celestial motions thereon; in which the proportion of the decrease of gravity from the superficies of the planet (though for brevity's sake not there expressed) can be no other than reciprocally duplicate of the distance from the centre. And I hope I shall not be urged to declare, in print, that I understood not the obvious mathematical conditions of my own hypothesis. But grant I received it afterwards from Mr. Hooke, yet have I as great a right to it as to the ellipsis. For as Kepler knew the orb to be not circular but oval, and guessed it to be elliptical, so Mr. Hooke, without knowing what I have found out since his letters to me, can know no more, but that the proportion was duplicate quam proximè at great distances from the centre, and only guessed it to be so accurately, and guessed amiss in extending that proportion down to the very centre, whereas Kepler guessed right at the ellipsis. And so Mr. Hooke found less of the proportion than Kepler of the ellipsis. There is so strong an objection against the accurateness of this proportion, that without my demonstrations, to which Mr. Hooke is yet a stranger, it cannot be believed by a judicious philosopher to be any where accurate. And so, in stating this business, I do pretend to have done as much for the proportion as for the ellipsis, and to have as much right to the one from Mr. Hooke and all men, as to the other from Kepler; and therefore on this account also he must at least moderate his pretences.

The proof you sent me I like very well. I designed the whole to consist of three books; the second was finished last summer being short, and only wants transcribing, and drawing the cuts fairly. Some new propositions I have since thought on, which I can as well let alone. The third wants the theory of comets. In autumn last I spent two months in calculations to no purpose for want of a good method, which made me afterwards return to the first book, and enlarge it with divers propositions, some relating to comets, others to other things, found out last winter. The third I now design to suppress. 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. The two first books, without the third, will not so well bear the title of Philosophiæ Naturalis Principia Mathematica; and therefore I had altered it to this, De Motu Corporum libri duo. But, upon second thoughts, I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours. The articles are, with the largest, to be called by that name; if you please you may change the word to *sections*, though it be not material. In the first page, I have struck out the words "uti posthac docebitur," as referring to the third book; which is all at present, from

your affectionate friend,

and humble servant,

Cambridge, June 20, 1686.

IS. NEWTON.

Since my writing this letter, I am told by one, who had it from another lately present at one of your meetings, how that Mr. Hooke should there make a great stir, pretending that I had all from him, and desiring they would see that he had justice done him. This carriage towards me is very strange and undeserved; so that I cannot forbear, in stating the point of justice, to tell you further, that he has published Borell's hypothesis in his own name; and the asserting of this to himself, and completing it as his own, seems to me the ground of all the stir he makes. Borell did something in it, and wrote modestly. He has done nothing, and yet written in such a way, as if he knew and had sufficiently hinted all but what remained to be determined by the drudgery of calculations and observations, excusing himself from that labour by reason of his other business, whereas he should rather have excused himself by reason of his inability. For 'tis plain, by his words, he knew not how to go about it. Now is not this very fine? Mathematicians, that find out, settle, and do all the business, must content themselves with being nothing but dry calculators and drudges; and another, that does nothing but pretend and grasp at all things, must carry away all the invention, as well of those that were to follow him, as of those that went before. Much after the same manner were his letters writ to me, telling me that gravity, in descent from hence to the centre of the earth, was reciprocally in a duplicate ratio of the altitude, that the figure described by projectiles in this region would be an ellipsis, and that all the motions of the heavens were thus to be accounted for; and this he did in such a way, as if he had found out all, and knew it most certainly. And, upon this information, I must now acknowledge, in print, I had all from him, and so did nothing myself but drudge in calculating, demonstrating, and writing, upon the inventions of this great man. And yet, after all, the first of those three things he told of me is false, and very unphilosophical; the second is as false; and the third was more than he knew, or could affirm me ignorant of by any thing, that past between us in our letters. Nor do I understand by what right he claims it as his own; for as Borell wrote, long before him, that by a tendency of the planets towards the sun, like that of gravity or magnetism, the planets would move in ellipses, so

Bullialdus wrote that all force, respecting the sun as its centre, and depending on matter, must be reciprocally in a duplicate ratio of the distance from the centre, and used that very argument for it, by which you, sir, in the last Transactions, have proved this ratio in gravity. Now if Mr. Hooke, from this general proposition in Bullialdus, might learn the proportion in gravity, why must this proportion here go for his invention? My letter to Hugenius, which I mentioned above, was directed to Mr. Oldenburg, who used to keep the originals. His papers came into Mr. Hooke's possession. Mr. Hooke, knowing my hand, might have the curiosity to look into that letter, and thence take the notion of comparing the forces of the planets arising from their circular motion; and so what he wrote to me afterwards, about the rate of gravity, might be nothing but the fruit of my own garden. And it's more than I can affirm, that the duplicate proportion was not expressed in that letter. However, he knew it not, (as I gather from his books,) till five years after any mathematician could have told it him. For when Hugenius had told how to find the force in all cases of circular motion, he had told 'em how to do it in this as well as in all others. And so the honour of doing it in this is due to Hugenius. For another, five years after, to claim it as his own invention is as if some mechanic, who had learned the art of surveying from a master, should afterwards claim the surveying of this or that piece of ground for his own invention, and keep a heavy quarter to be in print for't. But what if this surveyor be a bungler, and give an erroneous survey? Mr. Hooke has erred in the invention he pretends to, and his error is the cause of all the stir he makes. For his extending the duplicate proportion down to the centre, (which I do not,) made him correct me, and tell me the rest of his theory as a new thing to me, and now stand upon it, that I had all from that his letter, notwithstanding that he had told it to all the world before, and I had seen it in his printed books, all but the proportion. And why should I record a man for an invention, who founds his claim upon an error therein, and on that score gives me trouble? He imagines he obliged me by telling me his theory, but I thought myself disobliged by being, upon his own mistake, corrected magisterially, and taught a theory, which every body knew, and I had a truer notion of than himself. Should a man who thinks himself knowing, and loves to shew it in correcting and instructing others, come to you, when you are busy, and notwithstanding your excuse press discourses upon you, and through his own mistakes correct you, and multiply discourses; and then make this use of it, to boast that he taught you all he spake, and oblige you to acknowledge it, and cry out injury and injustice, if you do not; I believe you would think him a man of strange unsociable temper. Mr. Hooke's letters in several respects abounded too much with that humour, which Hevelius and others complain of; and therefore he may do well in time to consider, whether, after this new provocation, I be much more bound, (in doing him that justice he claims,) to make an honourable mention of him in print, especially since this is the third time that he has given me trouble in this kind. For your further satisfaction in this business, I beg the favour you would consult your books for a paper of mine entitled, An Hypothesis explaining properties of Light. It was dated Dec. 7, 1675, and registered in your book about January or February following. Not far from the beginning there is a paragraph ending with these words: "And as the earth,

"so perhaps may the sun imbibe this spirit copiously
"to conserve his shining, and keep the planets from
"receding further from him; and they that will may
"also suppose that this spirit affords or carries thither
"the solary fuel and material principle of light.
"And that the vast ethereal spaces between us and
"the stars are for a sufficient repository for this food
"of the sun and planets. But this of the constitution
"of ethereal natures by the by."

In these and the foregoing words you have the common cause of gravity towards the earth, sun, and all the planets, and that by this cause the planets are kept in their orbs about the sun. And this is all the philosophy Mr. Hooke pretends I had from his letters some years after, the duplicate proportion only excepted. The preceding words contain the cause of the phænomena of gravity, as we find it on the surface of the earth, without any regard to the various distances from the centre. For at first I designed to write of nothing more. Afterwards, as my manuscript shews, I interlined the words above cited relating to the heavens; and in so short and transitory an interlined hint of things, the expression of the proportion may well be excused. But if you consider the nature of the hypothesis, you'll find that gravity decreases upwards, and can be no other from the superficies of the planet than reciprocally duplicate of the distance from the centre, but downwards that proportion does not hold. This was but an hypothesis, and so to be looked upon only as one of my guesses, which I did not rely on; but it sufficiently explains to you, why in considering the descent of a body down to the centre, I used not the duplicate proportion. In the small ascent and descent of projectiles above the earth, the variation of gravity is so inconsiderable, that Mathematicians neglect it. Hence the vulgar hypothesis with them is uniform gravity. And why might not I, as a Mathematician, use it frequently, without thinking on the philosophy of the heavens, or believing it to be philosophically true?

This letter, with the postscript belonging to it, was printed in the General Dictionary by Bernard, Birch, and Lockman (vol. vii. p. 797). The writers in the Biographia Britannica likewise adopted it; but have separated the different parts, and printed one portion twice. They have given the beginning (p. 26 to p. 29) in the life of Hooke (vol. iv. p. 2659); the next part (p. 29 to p. 30) in the life of Newton (vol. v. p. 3225), and the end (p. 30) is repeated in the life of Halley (vol. iv. p. 2504). The postscript was added to the part annexed to the life of Hooke (vol. iv. p. 2660).

N°. VI.

HALLEY TO NEWTON.[1]

Sir, London, 29 June, 1686.

I am heartily sorry, that in this matter, wherein all mankind ought to acknowledge their obligations to you, you should meet with any thing that should give you disquiet; or that any disgust should make you think of desisting in your pretensions to a Lady, whose favours you have so much reason to boast of. 'Tis not she, but your rivals, envying your happiness, that endeavour to disturb your quiet enjoyment; which when you consider, I hope you will see cause to alter your resolution of suppressing your third book, there being nothing which you can have compiled therein, which the learned world will not be concerned to have con-

[1] From the guard-book of the Royal Society (H. I.)

cealed. Those gentlemen of the Society, to whom I have communicated it, are very much troubled at it, and that this unlucky business should have happened to give trouble, having a just sentiment of the author thereof. According to your desire in your former, I waited upon Sir Christopher Wren, to inquire of him, if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was, that he himself very many years since had had his thoughts upon the making out the planets' motions by a composition of a descent towards the sun, and an impressed motion; but that at length he gave it over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him, that he had done it, and attempted to make it out to him; but that he never was satisfied that his demonstrations were cogent. And this I know to be true, that in January $168\frac{3}{4}$, I having, from the considerations of the sesquialter proportion of Kepler, concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came on Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and falling in discourse about it, Mr. Hooke affirmed, that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my own attempts; and Sir Christopher, to encourage the inquiry, said, that he would give Mr. Hooke, or me, two months' time, to bring him a convincing demonstration thereof; and besides the honour, he of us, that did it, should have from him a present of a book of 40 shillings. Mr. Hooke then said, that he had it, but he would conceal it for some time, that others trying and failing might know how to value it, when he should make it public. However I remember, that

Sir Christopher was little satisfied that he could do it; and though Mr. Hooke then promised to shew it him, I do not find, that in that particular he has been so good as his word. The August following, when I did myself the honour to visit you, I then learned the good news, that you had brought this demonstration to perfection: and you were pleased to promise me a copy thereof, which the November following I received with a great deal of satisfaction from Mr. Paget; and thereupon took another journey to Cambridge, on purpose to confer with you about it, since which time it has been entered upon the Register Books of the Society. As all this passed, Mr. Hooke was acquainted with it, and according to the philosophically ambitious temper he is of, he would, had he been master of a like demonstration, no longer have concealed it, the reason, he told Sir Christopher and me, now ceasing. But now he says, this is but one small part of an excellent system of nature, which he has conceived, but has not yet completely made out, so that he thinks not fit to publish one part without the other. But I have plainly told him, that unless he produce another differing demonstration, and let the world judge of it, neither I nor any one else can believe it. As to the manner of Mr. Hooke's claiming the discovery, I fear it has been represented in worse colours than it ought; for he neither made public application to the Society for justice, nor pretended you had all from him. The truth is this: Sir John Hoskyns, his particular friend, being in the chair, when Dr. Vincent presented your book, the Doctor gave it its just encomium both as to the novelty and dignity of the subject. It was replied by another gentleman, that you had carried the thing so far, that there was no more to be added. To which the Vice-president replied, that it was so much the more to be prized, for that it was both invented and perfected at the same time. This gave Mr. Hooke offence, that Sir John did not, at that time, make mention of what he had, as he said, discovered to him; upon which they two, who till then were the most inseparable cronies, have since scarce seen one another, and are utterly fallen out. After the breaking up of that meeting, being adjourned to the coffee-house, Mr. Hooke did there endeavour to gain belief, that he had some such thing by him, and that he gave you the first hint of this invention. But I found, that they were all of opinion, that nothing thereof appearing in print, nor on the books of the Society, you ought to be considered as the inventor. And if in truth he knew it before you, he ought not to blame any but himself, for having taken no more care to secure a discovery, which he puts so much value on. What application he has made in private, I know not; but I am sure that the Society have a very great satisfaction, in the honour you do them, by the dedication of so worthy a treatise. Sir, I must now again beg you, not to let your resentments run so high, as to deprive us of your third book, wherein the application of your mathematical doctrine to the theory of comets and several curious experiments, which, as I guess by what you write, ought to compose it, will undoubtedly render it acceptable to those, who will call themselves Philosophers without Mathematics, which are much the greater number. Now you approve of the character and paper, I will push on the edition vigorously. I have sometimes had thoughts of having the cuts neatly done in wood, so as to stand in the page with the demonstrations. It will be more convenient, and not much more charge. If it please you to have it so, I will try how well it can be done; otherwise I will have them in somewhat a larger size than those you have sent up. I am, Sir,

Your most affectionate humble servant,

E. HALLEY.

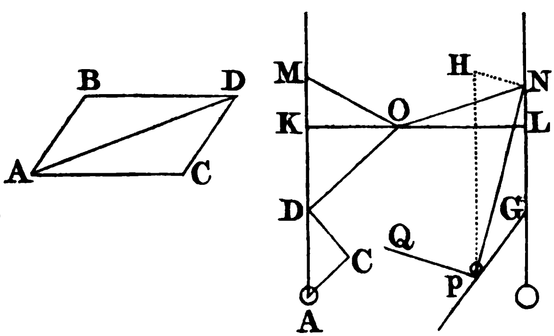
This letter was printed in the Gen. Dict. (vol. vii. p. 799). In the Biographia Britannica the parts are separated, and some, as was done for No. V. are repeated. The beginning (p. 35 to p. 36) is annexed to the life of Newton (vol. v. p. 3226), and the first part of it appears also (to p. 37) in the life of Halley (vol. iv. p. 2504). The middle (p. 37 to p. 38) will be found in the life of Hooke (vol. iv. p. 2661); the end (p. 38 to p. 39) is printed in the life of Newton (vol. v. p. 3226), and the latter part also in the life of Halley (vol. iv. p. 2504). The words (p. 37) "as all this passed, Mr. Hooke "was acquainted with it, and"—are wholly omitted.

No. VII.

NEWTON TO HALLEY.[m]

Sir, July 14, 1686.

I have considered your proposal about wooden cuts, and believe it will be much convenienter for the reader, and may be sufficiently handsome, but I leave it to your determination. If you go this way, then I desire you would divide the first figure into these two: I crowded them into one to save the trouble of altering the numbers in the



[m] From the guard-book of the Royal Society (N.1.)

schemes you have. I am very sensible of the great kindness of the gentlemen of your Society to me, far beyond what I could ever expect or deserve, and know how to distinguish between their favour and another's humour. Now I understand he was in some respects misrepresented to me, I wish I had spared the postscript to my last. This is true, that his letters occasioned my finding the method of determining figures, which when I had tried in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for about five years, till upon your request I sought for that paper; and not finding it, did it again, and reduced it into the propositions shewed you by Mr. Paget: but for the duplicate proportion I can affirm that I gathered it from Kepler's theorem about twenty years ago. And so Sir Christopher Wren's examining the ellipsis over against the focus shews, that he knew it many years ago, before he left off his enquiry after the figure by an impressed motion and a descent compounded together. There was another thing in Mr. Hooke's letters, which he will think I had from him. He told me, that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity's being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for in that very paper, which I told you was writ some time above fifteen years ago, and to the best of my memory was writ eighteen or nineteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet to do this business right, is a thing of far greater difficulty than I was aware of. A third thing there was in his letters, which was new to me, and I shall acknowledge it, if I make use of it. 'Twas the deflexion of falling bodies to the south-east in our latitude. And now having sincerely told you the case between Mr. Hooke and me, I hope I shall be free for the future from the prejudice of his letters. I have considered how best to compose the present dispute, and I think it may be done by the inclosed scholium to the fourth proposition. In turning over some old papers I met with another demonstration of that proposition, which I have added at the end of this scholium. Which is all at present from

your affectionate friend,
and humble servant,
Is. Newton.

This letter was printed in the Gen. Dict. (vol. vii. p. 800); but the first fourteen lines and the diagrams belonging to them are omitted. It was reprinted in the Biographia Britannica (life of Hooke, vol. iv. p. 2661), where the last sentence ("In turning over," &c. p. 41) is omitted, as well as the beginning, which was left out in the General Dictionary.

No. VIII.

NEWTON TO HALLEY.[n]

Sir,

Yesterday I unexpectedly struck upon a copy of the letter, I told you of, to Hugenius. 'Tis in the hand of one Mr. John Wickins, who was then my chamber-fellow, and is now parson of Stoke Edith near Mon-

[n] From the guard-book of the Royal Society, (N. 1.)

mouth, [Hereford,] and so is authentic. It begins thus, being directed to Mr. Oldenburg.

"Sir,

"I receiv'd your letters, with M. Hugens's kind "present, which I have viewed with great satisfaction, "finding it full of very subtile and useful speculations "very worthy of the author. I am glad, that we are "to expect another discourse of the Vis Centrifuga, "which speculation may prove of good use in Natural "Philosophy and Astronomy, as well as Mechanics. "Thus, for instance, if the reason, why the same side "of the moon is ever towards the earth, be the greater "conatus of the other side to recede from it, it will "follow, (upon supposition of the earth's motion about "the sun,) that the greatest distance of the sun from "the earth is to the greatest distance of the moon "from the earth, not greater than 10000 to 56; and "therefore the parallax of the sun not less than "$\frac{56}{10000}$ of the parallax of the moon; because were "the sun's distance less in proportion to that of the "moon, she would have a greater conatus from the "sun than from the earth. I thought also sometime "that the moon's libration might depend upon her "conatus from the sun and earth compared together, "till I apprehended a better cause."

Thus far this letter concerning the Vis Centrifuga. The rest of it, for the most part concerning colours, is printed in the Phil. Trans. of July 21, 1673, N°. 96. Now from these words it's evident, that I was at that time versed in the theory of the force arising from circular motion, and had an eye upon the forces of the planets, knowing how to compare them by the proportions of their periodical revolutions and distances from the centre they move about: an instance of which you have here in the comparison of the forces of the moon arising from her menstrual motion about the earth, and annual about the sun. So then in this theory I am plainly before Mr. Hooke. For he about a year after, in his Attempt to prove the Motion of the Earth, declared expressly, that the degrees, by which gravity decreased, he had not then experimentally verified; that is, he knew not how to gather it from phenomena; and therefore he there recommends it to the prosecution of others.

Now, though I do not find the duplicate proportion expressed in this letter, (as I hoped it might,) yet if you compare this passage of it here transcribed, with that hypothesis of mine, registered by Mr. Oldenburg in your book, you will see, that I then understood it. For I there suppose, that the descending spirit acts upon bodies here on the superficies of the earth with force proportional to the superficies of their parts; which cannot be, unless the diminution of its velocity in acting upon the first parts of any body it meets with, be recompensed by the increase of its density arising from that retardation. Whether this be true is not material. It suffices, that 'twas the hypothesis. Now if this spirit descend from above with uniform velocity, its density, and consequently its force, will be reciprocally proportional to the square of its distance from the centre. But if it descend with accelerated motion, its density will every where diminish as much as its velocity increases; and so its force (according to the hypothesis) will be the same as before, that is, still reciprocally as the square of its distance from the centre.

In short, as these things compared together shew, that I was before Mr. Hooke in what he pretends to have been my master, so I learned nothing by his letters but this, that bodies fall not only to the east, but also in our latitude to the south. In the rest his correcting and informing me was to be complain'd of. And tho' his correcting my spiral occasioned my finding the theorem, by which I afterwards examined the ellipsis; yet am I not beholden to him for any light into the business, but only for the diversion he gave me from my other studies to think on these things, and for his dogmaticalness in writing, as if he had found the motion in the ellipsis, which inclined me to try it, after I saw by what method it was to be done. Sir, I am,

your affectionate friend,

and humble servant,

July 27, 1686. IS. NEWTON.

This letter was printed in the Gen. Dict. (vol. vii. p. 800), and reprinted in the Biographia Britannica (life of Hooke, vol. iv. p. 2661). The original letter to Oldenburg, from which an extract is given here by Newton, is in the guard-book (N. 1), and the date of it is there preserved, "June 23. "73." It likewise contains a passage, in addition to what Newton has quoted, and which is omitted in the copy that is printed in the Phil. Transactions (vol. viii. p. 6087). It does not indeed bear upon the present subject, but still the completion of the letter may be some apology for inserting it in this place. It is as follows:

In the demonstration of the 8th Proposition[n] de descensu gravium, there seems to be an illegitimate supposition, namely, that the flexures at B and C do not hinder the motion of the descending body. For in reality they will hinder it, so that a body which descends from A shall not acquire so great velocity, when arrived at D, as one which descends from E. If this supposition be made because a body descending

[n] [Hugenii Horol. Oscil.]

by a curve line meets with no such opposition, and this proposition is laid down in order to the contemplation of motion in curve lines, then it should have been shewn that though rectilinear flexures do hinder, yet the infinitely little flexures which are in curves, though infinite in number, do not at all hinder the motion.

The rectifying curve lines by that way which Mr. Hugens calls evolution, I have been sometimes considering also, and here met with a way of resolving it, which seems more ready and free from the trouble of calculation than that of M. Hugens. If he please, I will send it him. The problem also is capable of being improved by being propounded thus more generally.

"Curvas invenire quotascunque, quarum longitudines cum propositæ alicujus curvæ longitudine, vel cum area ejus ad datam lineam applicata, comparari possunt."

N°. IX.

NEWTON TO HALLEY.[o]

Sir,

In the scholium you write of, the words "vel hyperbolæ" in the 3rd line are to be struck out, and in the 5th and 6th lines the words "quæ sit ad GK" should be "quæ sit ad $\frac{1}{2}$ GK." I send you inclosed the beginning of this scholium with the 63rd figure as I would have them printed. I thank you heartily for giving me notice that it was amiss. The ground of the transmutation of a trapezium into a parallelogram I lay down, pag. 87, in these words: "Nam rectæ quævis

[o] From the original in the guard-book of the Royal Society, (N. 1.)

"convergentes transmutantur in parallelas, adhibendo "pro radio ordinato primo AO lineam quamvis rectam, "quæ per concursum convergentium transit: id adeo "quia concursus ille hoc pacto abit in infinitum, lineæ "autem parallelæ sunt quæ ad punctum infinite distans "tendunt." In the figure, pag. 86, conceive the curve HGI to be produced both ways till it meet and intersect itself any where in the radius ordinatus primus AO: and when the point G moving up and down in the curve HI arrives at that intersection point, I say the point g moving in like manner up and down in the curve hi will become infinitely distant. For the point G falling upon the line OA, the point D will fall upon the point A, and the line OD upon the line OA; and so becoming parallel to aB their intersection point d will become infinitely distant, and consequently the line dg will become infinitely distant, and so will its point g. Q. E. D. So then if any two lines of the primary figure HGID intersect in the radius ordinatus primus AO, their intersection in the new figure hgid shall become infinitely distant; and therefore, if the two intersecting lines be right ones, they shall become parallel. For right lines, which lead to a point infinitely distant, do not intersect one another and diverge, but are parallel. Therefore, if in the primary figure there be any trapezium, whose opposite sides converge to points in the radius ordinatus primus OA, those sides in the new figure shall become parallel, and so the trapezium be converted into a parallelogram.

The printed sheets I intend to look over. Mr. Paget, in his stay here, has noted these errata, of which the 3rd is a fault in the copy.

P. 6, l. 27, velocitate; p. 8, l. 19, tur. Sunt; p. 14, l. 30, reciproce ut DO; p. 18, l. 1, recta. I wish the printer be careful to mend all you note. Sir, I am very sensible of the great trouble you are at in this business, and the great care you take about it. Pray take your own time. And if you meet with any thing else, which you think need either correcting or further explaining, be pleased to signify it to

your humble and obliged servant,

Trin. Coll. Is. NEWTON.
Octob. 18, 1686.

My thanks for your note of De la Hire.

N°. X.

NEWTON TO HALLEY.[p]

Sir,

I have sent you the sheet you want. The second book I made ready for you in autumn, having wrote to you in summer that it should come out with the first, and be ready against the time you might need it, and guessing by the rate of the press in summer you might need it about November or December. But not hearing from you, and being told (though not truly) that, upon some differences in the Royal Society, you had left your secretary's place, I desired my intimate friend Mr. C. Montague to enquire of Mr. Paget how things were, and send me word. He writes, that Dr. Wallis has sent up some things about projectiles pretty like those of mine in the papers Mr. Paget first shewed you, and that 'twas ordered I should be consulted whether I intend to print mine. I have inserted them into the beginning of the second book with divers others of that kind: which therefore, if you de-

[p] From the original in the guard-book of the Royal Society. (N. 1.)

sire to see, you may command the book when you please, though otherwise I should choose to let it lie by me till you are ready for it. I think I have the solution of your problem about the sun's parallax, but through other occasions shall scarce have time to think further on these things: and besides, I want something of observation, for if my notion be right, the sun draws the moon in the quadratures, so that there needs an equation of about 4 or 4½ minutes to be subducted from her motion in the first quarter and added in the last. I hope you received a letter with two corollaries I sent you in autumn. I have eleven sheets already, that is, to M. When you have seven more printed off I desire you would send them. I thank you for putting forward the press again, being very sensible of the great trouble I give you amidst so much business of your own, and the Royal Society's. In this, as well as in divers other things, you will much oblige

your affectionate friend,

and humble servant,

Trin. Coll., Cambridge,
Feb. 18, 1686. [1686-7]. Is. NEWTON.

N°. XI.

NEWTON TO HALLEY.[q]

Sir,

You'll receive the 2nd book on Thursday night or Friday by the coach. I have directed it to be left with Mr. Hunt at Gresham Coll. Pray let me beg the favour of a line or two to know of the receipt. I am obliged to you for pushing on the edition, because of

[q] From the original in the guard-book of the Royal Society, (N. 1.)

people's expectation, tho' otherwise I could be as well satisfied to let it rest a year or two longer. 'Tis a double favour, that you are pleased to double your pains about it. Dr. Wallis's papers may be long, and I would not give you the trouble of transcribing them all. The heads may suffice. The resistance, in swift motions, is in a duplicate proportion to the celerity. The deduction of the sun's parallax from the moon's variation, I cannot promise now to consider. When astronomers have examined whether there be such an inequality of her motion in the quadratures, as I mentioned in my last, and determined the quantity thereof, I may take some occasion perhaps to tell them the reason. No more at present from

your most affectionate humble servant,

Cambridge,
March 1, 86–7.

Is. Newton.

N°. XII.

ORIGIN OF THE PRINCIPIA.[r]

The first thoughts, which gave rise to his Principia, he had, when he retired from Cambridge in 1666, on account of the plague. As he sat alone in a garden, he fell into a speculation on the power of gravity: that as this power is not found sensibly diminished at the remotest distance from the centre of the earth, to which we can rise, neither at the tops of the loftiest buildings, nor even on the summits of the highest mountains, it appeared to him reasonable to conclude,

[r] From Dr. Pemberton's Preface to his View of Sir Isaac Newton's Philosophy.

that this power must extend much farther than was usually thought; why not as high as the moon? said he to himself; and if so, her motion must be influenced by it; perhaps she is retained in her orbit thereby. However, though the power of gravity is not sensibly weakened in the little change of distance, at which we can place ourselves from the centre of the earth, yet it is very possible that, so high as the moon, this power may differ much in strength from what it is here. To make an estimate what might be the degree of this diminution, he considered with himself that, if the moon be retained in her orbit by the force of gravity, no doubt the primary planets are carried round the sun by the like power. And, by comparing the periods of the several planets with their distances from the sun, he found that if any power like gravity held them in their courses, its strength must decrease in the duplicate proportion of the increase of distance. This he concluded by supposing them to move in perfect circles concentrical to the sun, from which the orbits of the greatest part of them do not much differ. Supposing therefore the power of gravity, when extended to the moon, to decrease in the same manner, he computed whether that force would be sufficient to keep the moon in her orbit. In this computation, being absent from books, he took the common estimate, in use among geographers and our seamen before Norwood had measured the earth, that 60 English miles were contained in one degree of latitude on the surface of the earth. But as this is a very faulty supposition, each degree containing about $69\frac{1}{2}$ of our miles, his computation did not answer expectation; whence he concluded, that some other cause must at least join with the action of the power of gravity on the moon. On this account he laid aside, for that time, any farther thoughts upon this matter. But some years after, a letter, which he received from Dr. Hooke, put him on inquiring what was the real figure, in which a body let fall from any high place descends, taking the motion of the earth round its axis into consideration. Such a body, having the same motion, which by the revolution of the earth the place has whence it falls, is to be considered as projected forward and at the same time drawn down to the centre of the earth. This gave occasion to his resuming his former thoughts concerning the moon, and Picard in France having lately measured the earth, by using his measures the moon appeared to be kept in her orbit purely by the power of gravity; and consequently, that this power decreases, as you recede from the centre of the earth, in the manner our author had formerly conjectured. Upon this principle he found the line described by a falling body to be an ellipsis, the centre of the earth being one focus. And the primary planets moving in such orbits round the sun, he had the satisfaction to see, that this inquiry, which he had undertaken merely out of curiosity, could be applied to the greatest purposes. Hereupon he composed near a dozen propositions, relating to the motion of the primary planets about the sun. Several years after this, some discourse he had with Dr. Halley, who at Cambridge made him a visit, engaged Sir Isaac Newton to resume again the consideration of this subject; and gave occasion to his writing the treatise, which he published under the title of Mathematical Principles of Natural Philosophy. This treatise, full of such a variety of profound inventions, was composed by him, from scarce any other materials than the few propositions before mentioned, in the space of a year and a half.

N°. XIII.

AUBREY AND HOOKE'S LETTER TO A. WOOD.[s]

Sept. 15, 1689.

Mr. Wood!

Mr. Rob. Hooke, R. S. S., did in anno 1670 write a discourse called, An Attempt to prove the Motion of the Earth, which he then read to the Royal Society; but printed it in the beginning of the year 1674 . . . .[t] to Sir John Cutler, to whom it is dedicated, wherein he has delivered the theory of explaining the celestial motions mechanically; his words are these, pag. 27, 28, viz.[u].

About 9 or 10 years ago Mr. Hooke writ to Mr. Isaac Newton of Trin. Coll. Cambridge, to make a demonstration of [it] *this Theory*, *not* telling him *at first* the proportion of the gravity to the distance, [and] *nor what was* the curved line that was thereby made.

Mr. Newton [did express], in his answer to the letter, *did express* that he had not thought[x] of it; and in his first attempt about it, he calculated the curve by supposing the attraction to be the same at all distances: upon which Mr. Hooke told him in his

[s] In printing the following letter whatever was written by Aubrey is in the Roman, whatever was inserted by Hooke is in the Italic types. Aubrey's words are inclosed in brackets when they have been erased in order for others to be substituted for them.

[t] The words here are not legible, but probably they make mention of Hooke "as lecturer to Sir John Cutler," or something to that purport.

[u] Here a space is left in which Aubrey evidently intended to insert the passage.

[x] "known" is written over "thought;" but the first word is not erased.

next letter the whole of his Hypothesis, scil. that the gravitation was reciprocal to the square of the distance, *which would make the motion in an ellipsis, in one of whose foci the sun being placed, the aphelion and perihelion of the planet would be opposite to each other in the same line, which is the whole celestial theory, concerning which Mr. Newton hath made a demonstration,* not at all owning he received the first intimation of it from Mr. Hooke. Likewise Mr. Newton has in the same book printed some other theories and experiments of Mr. Hooke's, *as that* about the oval figure of the earth and sea: without acknowledging from whom he had [it] *them,* though *he had not sent it up with the other parts of his book, till near a month after this theory was read to the Society by R. H.,* (Mr. Hooke,) *when it served to help to answer Dr. Wallis his arguments produced in the R. S. against it.*

In the Attempt to prove the Motion of the Earth, &c. printed 1674, *but read to the Royal Society* 1671, *pag.* 27, *lin.* 31.

" *I shall only for the present hint, that I have, in*
" *some of my foregoing observations, discovered some*
" *new motions even in the earth itself, which perhaps*
" *were not dreamt of before, which I shall hereafter*
" *more at large describe, when further trials have*
" *more fully confirmed and completed these begin-*
" *nings. At which time also I shall explain a system*
" *of the world, differing in many particulars from*
" *any yet known, answering in all things to the com-*
" *mon rules of mechanical motions. This depends*
" *upon three suppositions. First, that all celestial*
" *bodies whatsoever have an attraction or gravitating*
" *power towards their own centres, whereby they at-*
" *tract not only their own parts and keep them from*

"flying from them, as we may observe the earth to "do, but that they do also attract all the other ce- "lestial bodies, that are within the sphere of their "activity, and consequently that not only the sun and "the moon have an influence upon the body and mo- "tions of the earth, and the earth upon them, but that "Mercury also, Venus, Mars, Saturn, and Jupiter, "by their attractive powers have a considerable in- "fluence upon its motion, as in the same manner the "corresponding attractive power of the earth hath a "considerable influence upon every one of their mo- "tions also. The second supposition is this: that "all bodies whatsoever, that are put into direct and "simple motion, will so continue to move forwards "in a straight line, till they are by some other "effectual powers deflected and bent into a motion "describing a circle, ellipsis, or some other more com- "pounded curve line. The third supposition is, that "these attractive powers are so much the more power- "ful in operating, by how much nearer the body "wrought upon is to their own centres. Now what "these several degrees are I have not yet experi- "mentally verified." But these degrees and propor- tions of the power of attraction in the celestial bodies and motions were communicated to Mr. New- ton by R. Hooke in the year 1678 *by letters, as will plainly appear both by the copies of the said letters, and the letters of Mr. Newton in answer to them, which are both in the custody of the said R. H., both which also were read before the Royal Society at their public meeting, as appears by the Journal Book of the said Society*[y]. *"But it is a notion which, if*

[y] In the Journal Book of the Royal Society there is no mention of any such correspondence in 1678, or in 1679 till December. It was a mistake therefore in Hooke to refer to the former of these two years.

"*fully prosecuted as it ought to be, will mightily*
"*assist the astronomer to reduce all the celestial mo-*
"*tions to a certain rule, which I doubt will never be*
"*done true without it. He that understands the na-*
"*ture of the circular pendulum and circular motion,*
"*will easily understand the whole ground of this*
"*principle, and will know where to find direction in*
"*nature for the true stating thereof. This I only*
"*hint at present, to such as have ability and oppor-*
"*tunity of prosecuting this inquiry, and are not*
"*wanting of industry for observing and calculating,*
"*wishing heartily such may be found, having myself*
"*many other things in hand, which I will first com-*
"*plete, and therefore cannot so well attend it. But*
"*this I durst promise the undertaker. That he will*
"*find all the great motions of the world to be in-*
"*fluenced by this principle, and that the true under-*
"*standing thereof will be the true perfection of*
"*astronomy.*"

Mr. Wood!

This is the greatest discovery in nature, that ever was since the world's creation: it never was so much as hinted by any man before. I know you will do him right. I hope you may read his hand: I wish he had writ plainer, and afforded a little more paper.

Tuus,

J. AUBREY.

Before I leave this town I will get of him a catalogue of what he hath wrote, and as much of his inventions as I can; but they are many hundreds; he believes not fewer than a thousand. 'Tis such a hard matter to get people to do themselves right.

N°. XIV.

EXTRACTS FROM LETTERS[z] OF AUBREY TO A. WOOD.

London, Sept. 15, 1674. Mr. Hooke told me, (who has looked over your book[a]), that you have left out several eminent men. You have not either mentioned him, which I desired. England has hardly produced a greater wit, viz. for mechanics.

Gresham Coll., March 2, 1691–2. Mr. Wood! I acquainted you, some weeks since, that Mr. Hooke (now Dr. Hooke) desired you to do him the favour to send him a transcript of what you are to print concerning him. I have not yet heard from you about it: and Dr. Hooke doth again this day earnestly desire you would be pleased to write as aforesaid, as soon as you can possibly; for it doth (he says) exceedingly concern him. He will repay you for the transcription, which I shall deliver to you when I come to you.

London, March 3, 1691–2. Mr. Wood! I sent you a letter some weeks since, that Dr. Hooke remembers him very kindly to you, and does earnestly request you to do him the favour to send him a transcript of what you intend to write of him, with all possible speed; and he will repay you for the transcribing. To this purpose I yesterday left a letter with Mr. Bennet; but to-day speaking with Mr. Bennet, he tells me that he sent a letter from you to me, by the penny-post, on Saturday last: my landlady affirms

[z] From the originals in the Bodleian Library.

[a] Historia et Antiquitates Univers. Oxon.; published in 1674.

she received it not. Now your book[b] drawing on to an end, I, not knowing what the consequence of that letter may be, thought it a sure way to trouble [you] with this letter by the post.

April 13, 1692, Gresham College. Dr. Hooke does again desire, that, if you do make any mention concerning him, you would favour him with a copy of it, before it goes to the press, and he will gratify you in any thing that is equivalent. He remembers him kindly to you, and will be ready to serve you, in any thing that may lie in his way.

No. XV.

HALLEY'S VERSES PREFIXED TO THE PRINCIPIA.[c]

In
viri præstantissimi
D. Isaaci Newtoni
opus hocce
mathematico-physicum
sæculi gentisque nostræ decus egregium.

En tibi norma Poli, et divæ libramina Molis,
[en] [et]
Computus atque Jovis; quas, dum primordia rerum

[b] Athenæ Oxonienses, of which the first edition was completed and published in 1692.

[c] The verses are here printed as they were originally prefixed by Halley to the first edition of the Principia in 1687; what is in smaller letters was substituted by Bentley in 1713 for the words immediately under them; those parts which are between brackets are the alterations from the original text, which were adopted in 1726.

Conderet, omnipotens sibi ipse
Pangeret, omniparens Leges violare Creator
Dixerit, [atque operum quæ fundamenta locarit.]
Noluit, æternique operis fundamina fixit.
Intima panduntur victi penetralia cœli,
circumrotet,
Nec latet extremos quæ Vis circumrotat Orbes.
Sol solio residens ad se jubet omnia prono
Tendere descensu, nec recto tramite currus
Sidereos patitur vastum per inane moveri;
Sed rapit immotis, se centro, singula Gyris.
Hinc qua
Jam patet horrificis quæ sit via flexa Cometis;
Jam non miramur barbati Phænomena Astri [d].
Discimus hinc tandem qua causa argentea Phœbe
eat, et
Passibus haud æquis graditur; cur subdita nulli
Hactenus Astronomo numerorum fræna recuset:
remeent progrediantur
Cur remeant Nodi, curque Anges progrediuntur.
Discimus et quantis refluum vaga Cynthia Pontum
impellat; [fessis dum]
Viribus impellit, dum fractis fluctibus Ulvam
Deserit, ac Nautis suspectas nudat arenas;
Alternisve ruens spumantia pulsat.
Alternis vicibus suprema ad litora pulsans.
Quæ toties animos veterum torsere Sophorum,
hodie
Quæque Scholas frustra rauco certamine vexant
Obvia conspicimus nubem pellente Mathesi.
Jam dubios nulla caligine prægravat error [e]
Quæ superas
Queis Superum penetrare domos atque ardua Cœli
Newtoni auspiciis, jam dat contingere Templa.
Scandere sublimis Genii concessit acumen.

[d] This line was entirely omitted in 1713, and restored in 1726.

[e] This line also was omitted in 1713, and restored in 1726.

Surgite Mortales, terrenas mittite curas
cognoscite
Atque hinc cœligenæ vires dignoscite Mentis
A pecudum vita longe lateque remotæ.
primus
Qui scriptis jussit Tabulis compescere Cædes
Furta et Adulteria, et perjuræ crimina Fraudis;
Quive vagis populis circumdare mœnibus Urbes
Autor erat; Cererisve beavit munere gentes;
Vel qui curarum lenimen pressit ab Uva;
Vel qui Niliaca monstravit arundine pictos
Consociare sonos, oculisque exponere Voces;
Humanam sortem minus extulit; utpote pauca
In commune ferens miseræ solatia
[*tantum solamina*]
Respiciens miseræ solummodo commoda vitæ.
Jam vero Superis convivæ admittimur, alti
diæ
Jura poli tractare licet, jamque abdita cœcæ
Naturæ, et
Claustra patent Terræ, rerumque[f] immobilis ordo,
præteritis latuere incognita sæclis.
Et quæ præteriti latuerunt sæcula mundi.
justis
Talia monstrantem mecum celebrate Camœnis,
[*o cœlicolum gaudentes*]
Vos qui cœlesti gaudetis nectare vesci,
Newtonum clausi reserantem scrinia Veri
carum
Newtonum Musis charum, cui pectore puro
Phœbus adest, totoque incessit Numine mentem:
Nec fas est propius Mortali attingere Divos.

EDM. HALLEY.

[f] que—omitted in 1713, restored in 1726. The parts in Italics are alterations, made in the third, though not in the second edition.

N°. XVI.

OLDENBURG TO NORWOOD.[g]

Sir, Oct. 24, 1666.

I will believe you have heard that his Majesty hath, some few years since, founded a corporation of a number of ingenious and knowing persons, by the name of the Royal Society of London for improving natural knowledge, whose design is by observations and experiments, faithfully and frequently made, to advance the contemplations of Nature unto use and practice, and thereby to render them more serviceable for the necessities and accommodations of the life of man. Such a foundation being laid by our gracious King, the persons, thus incorporated, judge it very conducive to their purpose to bespeak and engage all sorts of intelligent and public-minded men, in all parts of the world, to contribute what they can to so noble and useful an undertaking. And this, sir, was the substance of what I acquainted you with in March 1664, inviting you at the same time, in the said Society's name, to send in what observables you might meet with in the Bermudas, as also to make what observations you could of the conjunction of the planet Mercury with the sun, which was then, according to our best calculations, to happen October 25th of the same year. But having, from your silence, great cause to doubt of the receipt of that letter, I embrace this opportunity of giving you notice of what was written formerly to you, and of assuring you of the good opinion we have, not only of your abilities and willingness in making such observations and experiments, as conveniently as

[g] From the Letter Book of the Royal Society, vol. i. p. 352.

you may yourself, in those parts where you are, concerning natural and artificial things, but also of your interest in the other English plantations, for exciting them to join in the same with you. Which being so, I am particularly to recommend to you and your ingenious friends in America, that you would impart to the Royal Society whatever, in the Bermudas and the other colonies, occurs considerable for the enriching of the History of Nature, (a faithful composure whereof is one of the main things they have in their eye,) and more especially of the history of the tides, the particulars whereof, if well observed about such islands, as lie in the open ocean, (the Bermudas, St. Helena,) would probably give much light for the finding out of a good theory to solve those puzzling phænomena, that occur about that subject. Besides, we being informed of a new whale-fishing, undertaken about the Bermudas [h], we should be very glad to receive from you the truth, method, and success of that enterprise, with a description of the kind and qualities of those whales, and whether any of that substance, called sperma ceti, be found in them, and if so, in what part and quantity, and how 'tis ordered: to which if you would please to add what observations you make of eclipses, of the motions of the satellites of Jupiter, and such like, you would thereby exceedingly oblige the public, and gratify the Royal Society, and particularly,

Sir,

your very humble servant,

H. Oldenburg, Soc. R. Secr.

P.S. I send hereby inclosed two of those tracts, called Philosophical Transactions, which are here printed once a month, wherein you will find, in the directions

[h] See Phil. Trans. vol. i. p. 11, 132.

for seamen[i], and in the inquiries and considerations of Sir Robert Moray[k] and Dr. Wallis[l], what the particulars are we desire chiefly to be informed about, in the matter of the tides. Sir, what you shall think fit to send of this nature for the Royal Society, if you please only to address it to me, as one of their secretaries, living in the middle of the Pall Mall of St. James's Fields in Westminster, will safely come to their hands, if the ship that brings it, comes safe to port.

Sir, we are informed that you have by you many accurate maps, as well of other parts of the world as of America, and of the particular plantations of the English, Dutch, &c. Our request is that they may be carefully preserved; and, if you would please to lodge them in the repository, or with the books and writings of the Royal Society, they would receive them as a singular testimony of your respect and affection to them; as also, if you should consent to the publishing thereof, [they would] cause them to be printed as yours, as indeed they are, with a character due to your person and merits.

N°. XVII.

NEWTON[m] ON THE CAUSE OF GRAVITATION.

1st. I suppose that there is diffused through all places an ethereal substance, capable of contraction

[i] N°. 8, p. 140; N°. 9, p. 147.

[k] N°. 17, p. 298. There were other papers of Sir R. Moray on the tides (N°. 4, p. 53; and N°. 18, p. 311).

[l] N°. 17, p. 297. Wallis had likewise published on the same subject in N°. 16, p. 263, 281; but 8 and 17 were most probably the two numbers which were sent, with his letter, by Oldenburg.

[m] From his letter to Boyle. Horsley's Newton, vol. iv. p. 385.

and dilatation, strongly elastic, and, in a word, much like air in all respects, but far more subtile.

2. I suppose this ether 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. * * *

3. I suppose the rarer ether within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another: the external ether 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. * * *

4. When two bodies, moving towards one another, come near together, I suppose the ether 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 ether cannot move and play up and down so freely in the straight passage between the bodies, as it could before they came so near together. * * *

5. Now from the fourth supposition, it follows, that when two bodies, approaching one another, come so near together as to make the ether between them begin to rarify, 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 increase as they come nearer together, because thereby they cause the interjacent ether to rarify more and more: but at length, when they come so near together, that the excess of pressure of the external ether, which surrounds the bodies, above that of the rarified ether, 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. * * *

I shall set down one conjecture more, which came into my mind now as I was writing this letter: it is about the cause of gravity. For this end, I will suppose ether 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 ether 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 ether in proportion to the finer, than in the regions of the air: and that yet the grosser ether in the air affects the upper regions of the earth, and the finer ether 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 ether is insensibly finer and finer. Imagine now any body suspended in the air, or lying on the earth; and the ether being, by the hypothesis, grosser in the pores which are in the upper parts of the body, than in those which are in the lower parts; and that grosser ether, being less apt to be lodged in those pores, than the finer ether below; it will endeavour to get out, and give way to the finer ether below, which cannot be, without the body's descending to make room above for it to go out into. * * *

The original of this letter is in the collection of the Earl of Macclesfield: and the above extracts have been corrected by it.

N°. XVIII.

HUGENS ON THE CAUSE OF GRAVITATION[o].

P. 135. Pour expliquer donc la pesanteur de la manière que je la conçois, je supposerai que dans l'espace sphérique, qui comprend la terre et les corps qui sont autour d'elle jusqu'à une grande étendue, il y a une matiere fluide qui consiste en des parties très petites, et qui est diversement agitée en tous sens, avec beaucoup de rapidité. Laquelle matiere, ne pouvant sortir de cet espace, qui est entouré d'autres corps, je dis que son mouvement doit devenir en partie circulaire autour du centre; non pas tellement pourtant qu'elle vienne à tourner toute d'un même sens, mais en sorte que la plupart de ses mouvemens différens se fassent dans des surfaces sphériques, à l'entour du centre du dit espace, qui pour cela devient aussi le centre de la terre.

La raison de ce mouvement circulaire est que la matière, contenue dans quelque espace, se meut plus aisément de cette manière que, par des mouvemens droits contraires les uns aux autres, lesquels même en se refléchissant, (parceque la matière ne peut pas sortir de l'espace qui l'enferme,) sont reduits à se changer en circulaires.

P. 137. Si parmi la matière fluide, qui tourne dans l'espace que nous avons supposé, il se rencontre des parties beaucoup plus grosses que celles qui la composent, ou des corps faits d'un amas de petites parties accrochées ensemble, et que ces corps ne suivent pas le mouvement rapide de la dite matière, ils seront nécessairement poussés vers le centre du mouvement,

[o] From Hugens' Discours de la Cause de la Pesanteur.

et y formeront le globe terrestre s'il y en a assez pour cela, supposé que la terre ne fut pas encore. . . . . . C'est donc en cela que consiste vraisemblablement la pesanteur des corps : laquelle on peut dire, que c'est l'effort que fait la matière fluide, qui tourne circulairement autour du centre de la terre en tous sens, à s'éloigner de ce centre, et à pousser en sa place les corps qui ne suivent pas ce mouvement.

P. 159. J'ai supposé . . que la pesanteur est la même au dedans de la Terre qu'à sa surface, ce qui me paroit fort vraisemblable . . .

Monsieur Newton . . trouve . . . que la figure de la terre diffère bien plus de la sphérique, ... aussi bien je ne suis pas d'accord d'un principe qu'il suppose dans ce calcul et ailleurs, qui est, que toutes les petites parties, qu'on peut imaginer dans deux ou plusieurs différens corps s'attirent ou tendent à s'approcher mutuellement. Ce que je ne sçaurois admettre, parceque je crois voir clairement, que la cause d'une telle attraction n'est point explicable par aucun principe de mécanique, ni des règles du mouvement. Comme je ne suis pas persuadé non plus de la nécessité de l'attraction mutuelle des corps entiers ; ayant fait voir que, quand il n'y auroit point de terre, les corps ne laisseroient pas, par ce qu'on appelle leur pesanteur, de tendre vers un centre.

N°. XIX.

FIRST COMMUNICATION OF THE PRINCIPIA TO THE ROYAL SOCIETY[p].

1.

Newtonus principales de motu planetarum proposi-

[p] From an original paper of Newton, belonging to the Earl of Macclesfield.

tiones anno 1683 Londinum misit cum philosophis communicandas. Anno 1686 Principia Mathematica ad Regiam Societatem misit ut in lucem emitteretur; et liber anno proximo prodiit, et epitome ejus anno sequente 1688 in Actis Lipsiensibus impressa est, et anno tandem 1689 Leibnitius selecta Newtoni inventa de lineis opticis, de resistentia mediorum, deque motibus planetarum novis verbis et novis calculis differentialibus ornavit, et pro suis in lucem statim emitti curavit, affirmans quod librum Newtoni nondum viderat. Qua licentia concessa, author omnis inventis suis facile privari possit. Sic et inventa Moutoni sibi asserere conatus est, quia hæc invenerat priusquam Moutoni librum viderat. Sed Newtoni librum prius consulere debuisset quam scripta sua de iisdem ederet, idque ne et Newtono injurius esset auferendo inventa ejus, et lectori molestus repetendo quæ Newtonus antea dixerat. Dum tantopere festinaret scripta sua edere non veritati consuluit, non scientiis, non lectoribus, sed sibi soli. Inventa Newtoni sibi arrogare festinabat. Et ex erroribus ejus manifestum est, insuper, quod methodum fluxionum in fluxionibus secundis nondum probe intellexerat. Calculum illum in fluxionibus primis jam ab anno 1677 exercuerat. Inventis Newtoni jam anno 1688 et 1681 excitatus est, ut methodum eandem in fluxionibus secundis exerceret, sed primo conamine multipliciter erravit, et erroribus patefecit se nec propositiones Newtonianas invenisse, nec methodum qua Newtonus easdem invenerat.

2.

Anno 1684 Newtonus propositiones principales earum, quæ in Philosophiæ Principiis Mathematicis habentur, cum Societate Regia communicare cœpit, annoque 1686 liber ille MS. ad Societatem Regiam missus est . . . . . . Proximo anno prodiit liber Newtoni, et anno sequente epitome ejus in Actis Lipsiensibus impressa est. Qua lecta, D. Leibnitius epistolam de lineis opticis, schediasma de resistentia medii et motu projectorum gravium in medio resistente, et tentamen de motuum cœlestium causis composuit et in Actis Lipsiensibus anni 1689 imprimi curavit, prætendens, quod ipse quoque præcipuas Newtoni his de rebus propositiones invenisset, et quod librum Newtoni nondum vidisset. Qua licentia concessa, autores quilibet inventis suis facile privari possunt. Quamprimum liber Newtoni lucem vidit, exemplar ejus Domino Fatio datum est ut ad Leibnitium mitteretur. Viderat Leibnitius epitomen ejus in Actis Leipsicis. Per commercium epistolicum, quod cum viris doctis habebat, cognoscere posset propositiones cujuscunque generis in libro illo descriptas. In scriptis illis nihil invenit novi. Newtoniana tantum descripsit suo more ac describendo nonnunquam. . . .

N°. XX.

EXTRACT FROM NEWTON'S HYPOTHESIS OF LIGHT[n].

That there is an ethereal medium, much of the same constitution with air, but far rarer, subtiler, and more strongly elastic. . . . Perhaps the whole frame of nature may be nothing but various contextures of some certain ethereal spirits or vapours, condensed as it were by precipitation, much after the manner that vapours are condensed into water, or exhalations into

[n] From Birch's Hist. of Royal Soc. vol. iii. p. 249–251.

grosser substances, though not so easily condensible. . . . .

The elastic effluvia seem to instruct us that there is something, of an ethereal nature, condensed in bodies. I have sometimes laid upon a table a round piece of glass, about two inches broad, set in a brass ring, so that the glass might be about one eighth or one sixth of an inch from the table, and the air between them being inclosed on all sides by the ring; . . . 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. . . . Now, whence all these irregular motions should spring, I cannot imagine, unless from some kind of subtile 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 . . . so may the gravitating attraction of the earth be caused by the continual condensation of some other such like ethereal spirit, not of the main body of phlegmatic ether, but of something very thinly and subtilely diffused through it, perhaps of an unctuous or gummy, tenacious, and springy nature, and bearing much of the same relation to ether, which the vital aereal spirit, requisite for the conservation of flame and vital motions, does to air. For, if such an ethereal 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; the vast body of the earth, which may be every where to the very centre in perpetual working, may continually condense so much of this spirit, as to cause it from above to descend with great celerity for a supply; in which descent it may bear down with it the bodies it pervades, with force proportional to the superficies of all their parts it acts upon; nature making a circulation by the slow ascent of as much matter out of the bowels of the earth in an aereal form, which for a time constitutes the atmosphere; but being continually buoyed up by a new air, exhalations and vapours rising underneath, at length, (some part of the vapours which return in rain excepted,) vanishes again into the ethereal spaces, and there perhaps in time relents, and is attenuated into its first principle: for nature is a perpetual worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile; some things to ascend, and make the upper terrestrial juices, rivers, and atmosphere; and by consequence others to descend for a requital to the former.

The passage then immediately follows, which is quoted by Newton in App. VI. p. 33, 4.

N°. XXI.

HALLEY'S REVIEW OF THE PRINCIPIA[o].

Philosophiæ Naturalis Principia Mathematica, autore Is. Newton Trin. Coll. Cantab. Soc. Mathe-

[o] From the Phil. Trans. vol. xvi. p. 291.

seos professore Lucasiano, et Societatis Regalis Sodali. 4to. Londini. Prostat apud plures bibliopolas.

This incomparable author, having at length been prevailed upon to appear in public, has in this treatise given a most notable instance of the extent of the powers of his 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 entitled 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 distinctions. To these definitions are subjoined 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 centre; and having demonstrated that, in all cases, the areas at the centre, described by a revolving body, are proportional to the times, he shews how from curves described to find the law or rule of the decrease or increase of the tendency or centripetal force (as he calls it) in differing distances from the centre. Of this there are several examples; as, if the curve described be a circle passing through the centre of tendency, then the force or tendency towards the centre is, in all points, as the fifth 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 centre thereof directly as the distance. If in any of the conic 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, then 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 phænomena 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 conic section, which a body 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 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 centre, 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 supposition, likewise, he delivers a general method of discovering the orbs described by a body, moving in such a tendency towards a centre, increasing or decreasing in any given relation to the distance from the centre; and then with great subtilty he determines in all cases the motion of the apsides, (or of the points of greatest distance from the centre,) in all these curves, in such orbs as are nearly circular; shewing the apsides fixed, 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 the 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, though the centre or point of tendency be never so near; to which the demonstration of M. 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 about the common centre 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 the centres, the various consequences are considered and laid down, in several corollaries of great use in explicating the phænomena of the moon's 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 shewn 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 the centre, decreasing, in spaces without it, in duplicate proportion of the distances from the centre, and decreasing, within its surface, as the distance from the centre directly, so as to be greatest on the surface, and nothing at the centre. And though this might suffice, yet to complete 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 carried 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 [are] solved, of the greatest concern in the art of Dioptrics.

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 be the consequence of a resistance from a medium, either in the simple or duplicate ratio of velocity, or else between both; and to complete 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 plane of the horizon, shall make a project move in any curve line assigned; which is the 10 Prop. Lib. 2. Then the circular motion of bodies in resisting media is determined, and 'tis shewn 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 hydrostatics demonstrated; and here 'tis proposed to the contemplation of natural philosophers, whether the surprising phænomena of the elasticity of the air and some other fluids may not arise from their being composed of particles, which fly 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 are considered, which are followed by an inquiry into the rules of the opposition to bodies, as their bulk, shape, or density may be varied. 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 air's 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 altogether impossible.

The third and last book is entitled, De Systemate Mundi, wherein the demonstrations of the two former books are applied to the explication of the principal phænomena 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 centres, 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 Earth's axis, is made out,) together with the retrocession of the Moon's nodes, the quantity and inequalities of whose motion are here exactly stated a priori. Lastly, the theory of the motion of comets is attempted with such success, that in an example of the great comet which appeared in 1680–1, 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 parabolic, or so near it that the parabola may serve without sensible error; though 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 small-ness 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 lemmas of general use in geometry, and several new methods applied, which are well worth the considering; and it may be justly said, that so many and so valuable philosophical truths, as are herein discovered and put past dispute, were never yet owing to the capacity and industry of any one man.

Advertisement.

Whereas the publication of these Transactions has for some months last past been interrupted, the reader is desired to take notice that the care of the edition of this book of Mr. Newton having lain wholly upon the publisher, (wherein he conceives he hath been more serviceable to the commonwealth of learning,) and for some other pressing reasons, 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 sheets or five, with a cut, according as materials shall occur.

N°. XXII.

PREFACE AND CONCLUSION OF HALLEY'S PAPER ON THE TIDES.

May it please
THE
KING'S
MOST EXCELLENT
MAJESTY.

I would not have presumed to approach your Majesty's royal presence with a book of this nature, had I not been assured that, when the weighty affairs of your government permit it, your Majesty has frequently shewn yourself inclined to favour mechanical and philosophical 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 grateful to your Majesty, especially being the labours of a worthy subject of your own, and a member of that Royal Society founded by your late royal brother, for the advancement of natural knowledge, and which now flourishes under your Majesty's most gracious protection.

But being sensible of the little leisure, which care of the public 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 Majesty may judge of the rest of the performance of the author.

(Then follows the statement as printed in the Philosophical Transactions, vol. xix. p. 445, and at the end there is the following addition.)

If by reason of the difficulty of the matter, there be any thing herein not sufficiently explained, or if there be any material thing, observable in the tides, that I have omitted, wherein your Majesty 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 royal presence, I shall be able to give you such an account thereof as may be to your Majesty's full content.