

AN ESSAY
ON
NEWTON'S
PRINCIPIA

W. W. R. BALL

MACMILLAN & CO.

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1708

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~~GABINET MATEMATYCZNY
Towarzystwa Naukowego Warszawskiego~~



AN ESSAY
ON NEWTON'S "PRINCIPIA"

BY
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CHAPTER I.

INTRODUCTORY.

NEWTON'S *Principia* is the classic of English mathematical writings. Three editions were brought out during Newton's lifetime—the first in 1687; the second, edited by Cotes, in 1713; the third, edited by Pemberton, in 1726. I had at one time hoped to publish a critical edition of the work, with a prefatory account of its origin and history, notes showing the form in which it was printed originally and the changes introduced in 1713 and 1726, accompanied where desirable by an analytical commentary, together with various other propositions on the subject which are extant among Newton's papers though they were not incorporated in any of the editions he issued. I am unlikely in the immediate future to find time to carry out this plan, but I think it possible that there may be a sufficient number of mathematicians interested in the subject to justify the publication of this essay on the history of Newton's work, though even when thus limited the following notes are on some points less full than I should have wished to make them had leisure permitted.

The history of the *Principia* was discussed by the late Sir David Brewster in his *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, Edinburgh, second edition, 1860, to which the citations hereafter given refer, but, naturally, he treated it there in connection with the whole of Newton's career; it had been also told previously by the

late Prof. S. P. Rigaud in his *Historical Essay on the first publication of Sir Isaac Newton's Principia*, Oxford, 1838, an admirable exposition of the facts then known. Both these works, however, are now out of print*, and I think that a time has come when it may be useful to collect from these and other sources the references to the leading events in the preparation and publication of the *Principia*. Except for a few letters hitherto unpublished, which are given below in chapter viii, there is not much that is new in the following pages, but the documents quoted have not before been collected in a single memoir, and I hope that the whole will provide a convenient summary of what is known on the subject.

This sketch of the origin and history of the *Principia* falls naturally into six divisions. These deal (*a*) with Newton's investigations in 1666; (*b*) with his investigations in 1679; (*c*) with his investigations in 1684, of which the chief results are embodied in the *De Motu* of 1685; (*d*) with the compilation and publication of the *Principia*, 1685-1687; (*e*) with the contents of the *Principia*; and (*f*) with the subsequent history of the work and the preparation of the later editions, but this head I do not propose to discuss in any great detail.

The bulk of the manuscripts and correspondence of Newton on scientific subjects which are now extant are in four collections: namely, (i) that in the possession of the Royal Society, (ii) the Portsmouth Collection, (iii) that in the Library of Trinity College, Cambridge, and (iv) that in the Macclesfield Collection.

(i) The papers in the possession of the Royal Society include or are copied from the originals of Newton's communications to that Society. These were examined by Rigaud, and many of the more important of them, together with other contemporary documents referring to them, are printed in the appendix to his

* A new edition, by Mr. Lynn, of Brewster's popular life of Newton, originally issued in 1831, was published in London in 1875; but most of the correspondence given in the appendices to the 1860 edition is omitted.

Essay. I believe that these transcripts are accurate, and I therefore give references to them as being, to most readers, more accessible than the originals.

(ii) The papers which were in Newton's possession at his death came into the possession of Mr. Conduitt, who in 1717 had married Catherine Barton, Newton's housekeeper and favourite niece. The only child of Mr. and Mrs. Conduitt married Lord Lymington, and through her the papers passed into the possession of the Portsmouth family. The portion of the collection which is concerned with science was presented to the University of Cambridge by the present Earl of Portsmouth, and a descriptive catalogue of the whole collection was published by the University in 1888.

The Portsmouth Collection contains holograph manuscript copies of the *Principia*, lists of proposed additions and corrections to the third as well as to the second edition, various notes on the subject, and numerous sheets of calculations and rough work. Besides these there are a good many papers on the lunar theory which have not been printed or described except in the Portsmouth Catalogue.

An acquaintance with the contents of this collection is necessary for any complete account of the history of the *Principia* or a critical edition of it. Unfortunately it was never examined by Rigaud, and he knew only generally of its existence. It was put at the disposal of Horsley when he was preparing the edition of Newton's works which was issued in 1779-1785, but he did not make much use of it. It was also placed in the hands of Brewster for his above-mentioned life of Newton, and in that work allusion is made to several of the mathematical papers contained in the collection, and a portion of the correspondence is printed.

(iii) The collection in the Library at Trinity College, Cambridge, has been obtained from various sources.

The larger part consists of the letters from Newton to Cotes relative to the preparation of the second edition of the *Principia*. After Cotes's death these letters came into

the possession of his cousin, Robert Smith, who bequeathed them to Edward Howkins, who in his turn bequeathed them, in 1779, to the College, where they have since remained. By order of the College they, together with drafts of the corresponding letters from Cotes to Newton and various other Newtonian papers, were printed in 1851, under the care of Dr. Edleston.

This collection also contains the four letters of 1692 and 1693 from Newton to Bentley, which were given by Bentley's nephew and executor to Richard Cumberland, who published them in 1756, and subsequently presented them to the College. From the same source is derived Newton's note on a course of mathematical reading which would give that preliminary knowledge of the subject necessary for the comprehension of the *Principia*.

The letter of Nov. 28, 1679, from Newton to Hooke, and the memoranda of Hooke on the controversy concerning the origin of the law of inverse squares, were recently purchased by the College, and are printed below for the first time.

(iv) The Macclesfield Collection at Sherborn Castle contains the greater part of the library formed by William Jones, which, besides his own correspondence, included the libraries and papers of Collins, Oughtred, and others. This came into the possession of Lord Macclesfield on Jones's death in 1749. The letters in this collection were published by Rigaud under the title, *Correspondence of Scientific Men of the seventeenth century in the collection of the Earl of Macclesfield*, Oxford, 1841, and a much-needed table of contents and index were issued in 1862. This book contains more than fifty letters from Newton himself, besides others with reference to his works. It would seem that the mathematical manuscripts in the collection (other than the letters) have not been catalogued or even carefully examined*. Rigaud, who saw them, says they "contain a "number of Newton's own MSS.," and it is likely that, but

* De Morgan in the *Athenaeum*, Oct. 18, 1862, p. 491, col. 1.

for Rigaud's premature death, he would have read them and published the results. Until the whole of this collection has been examined by some competent mathematician we cannot be sure that we have all the available data before us, but it would seem probable that the bulk of the documents in it are not directly connected with the history of the *Principia*.

Besides the four collections mentioned above there are letters from Newton in many public libraries, notably in the British Museum and in Corpus Christi College, Oxford. To a few of these letters I allude below, but most of them are on matters unconnected with the *Principia*.

CHAPTER II.

INVESTIGATIONS IN 1666.

It was in the year 1666 that Newton began to investigate the question of gravitation. He had taken the degree of B.A. in 1665, and, having thus completed the earlier part of his University course, he had leisure to pursue in his own way such studies as he pleased. Owing to the prevalence of the plague at Cambridge, he lived for a considerable part of the years 1665 and 1666 at his home in Lincolnshire, and it was there that the investigations described in this chapter were made.

The question of gravitation was one of the problems of the time, hence it was natural that he should consider it. It is, however, probable that at this period his enquiries on the subject were but of a slight character, and would have passed almost unnoticed had they not proved the earliest steps to his later discoveries.

His conclusions were not published, nor, as far as we know, are they contained in any manuscript now extant; but the following authorities enable us to form a general idea of their character and extent.

(i) First, there are allusions to the subject in letters from Newton, dated June 20, 1686, and July 14, 1686, which are printed below (pp. 156-162, 165).

(ii) Secondly, I may refer to the Portsmouth draft memorandum*, which he wrote some years later—perhaps about

* Portsmouth Collection, section i. division xi. number 41.

1714. The original is cancelled, and hence it is not indisputable evidence; but it is believed that, except for certain dates in the paragraph next below, it is substantially correct. This draft is as follows :

I found the Method [of fluxions] by degrees in the years 1665 and 1666. In the beginning of the year 1665 I found the method of approximating Series and the Rule for reducing any dignity of any Binomial into such a series. The same year in May I found the method of tangents of Gregory and Slusius, and in November had the direct method of fluxions, and the next year in January had the Theory of colours, and in May following I had entrance into y^e inverse method of fluxions. And the same year I began to think of gravity extending to y^e orb of the Moon, and having found out how to estimate the force with w^{ch} [a] globe revolving within a sphere presses the surface of the sphere, from Kepler's Rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the centers of their Orbs I deduced that the forces w^{ch} keep the Planets in their Orbs must [be] reciprocally as the squares of their distances from the centers about w^{ch} they revolve: and thereby compared the force requisite to keep the Moon in her Orb with the force of gravity at the surface of the earth, and found them answer pretty nearly. All this was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded Mathematicks and Philosophy more than at any time since. What Mr. Hugen's has published since about centrifugal forces I suppose he had before me. At length in the winter between the years 1676 and 1677 [probably this should be 1679 and 1680] I found the Proposition that by a centrifugal force reciprocally as the square of the distance a Planet must revolve in an Ellipsis about the center of the force placed in the lower umbilicus of the Ellipsis and with a radius drawn to that center describe areas proportional to the times. And in the winter between the years 1683 and 1684 [this should be the winter between 1684 and 1685] this Proposition with the Demonstration was entered in the Register book of the R. Society.

And on the next page, Newton continues :

By this Method [of fluxions] I invented the Demonstration of Kepler's Proposition in the year 1679, and almost all the rest of the Difficult Propositions of the Book of Principles in the years 1684, 1685, and part of the year 1686.

(iii) Thirdly, the subject is mentioned by Whiston in his autobiography, and he says that he obtained his information from Newton's conversations soon after 1694. The paragraphs referring to it are as follows* :

I proceed now in my own history. After I had taken Holy Orders, I returned to the College, and went on with my own Studies there, particularly the Mathematicks, and the *Cartesian* Philosophy; which was alone in Vogue with us at that Time. But it was not long before I, with immense Pains, but no Assistance, set myself with the utmost Zeal to the Study of Sir *Isaac Newton's* wonderful Discoveries in his *Philosophiæ Naturalis Principia Mathematica*, one or two of which Lectures I had heard him read in the publick Schools, though I understood them not at all at that Time. . . . We at *Cambridge*, poor Wretches, were ignominiously studying the fictitious Hypotheses of the *Cartesian*, which Sir *Isaac Newton* had also himself done formerly, as I have heard him say. What the Occasion of Sir *Isaac Newton's* leaving the *Cartesian* Philosophy, and of discovering his amazing Theory of Gravity was, I have heard him long ago, soon after my first Acquaintance with him, which was 1694, thus relate, and of which Dr. *Pemberton* gives the like Account, and somewhat more fully, in the Preface to his Explication of his Philosophy: It was this. An Inclination came into Sir *Isaac's* Mind to try, whether the same Power did not keep the Moon in her Orbit, notwithstanding her projectile Velocity, which he knew always tended to go along a strait Line the Tangent of that Orbit, which makes Stones and all heavy Bodies with us fall downward, and which we call *Gravity*? Taking this Postulatum, which had been thought of before, that such Power might decrease, in a duplicate Proportion of the Distances from the Earth's Center. Upon Sir *Isaac's* first Trial, when he took a Degree of a great Circle on the Earth's Surface, whence a Degree at the Distance of the Moon was to be determined also, to be 60 measured Miles only, according to the gross Measures then in Use. He was, in some Degree, disappointed, and the Power that restrained the Moon in her Orbit, measured by the versed Sines of that Orbit, appeared not to be quite the same that was to be expected, had it been the Power of Gravity alone, by which the Moon was there influenc'd. Upon this Disappointment, which made Sir *Isaac* suspect that this Power was partly that of Gravity, and partly that of *Cartesius's* Vortices, he threw aside the Paper of his

* *Memoirs of the Life of Mr. William Whiston by himself*, London, 1749, vol. i. pp. 35-38.

Calculation, and went to other Studies. However, some time afterward, when Monsieur *Picart* had much more exactly measured the Earth, and found that a Degree of a great Circle was $69\frac{1}{2}$ such Miles, Sir *Isaac*, in turning over some of his former Papers, light[ed] upon this old imperfect Calculation; and, correcting his former Error, discover'd that this Power, at the true correct Distance of the Moon from the Earth, not only tended to the Earth's Center, as did the common Power of Gravity with us, but was exactly of the right Quantity; and that if a Stone was carried up to the Moon, or to 60 Semid[i]ameters of the Earth, and let fall downward by its Gravity, and the Moon's own menstrual Motion was stopt, and she was let fall by that Power which before retained her in her Orbit, they would exactly fall towards the same Point, and with the same Velocity; which was therefore no other Power than that of Gravity. And since that Power appear'd to extend as far as the Moon, at the Distance of 240000 Miles, it was but natural, or rather necessary, to suppose it might reach twice, thrice, four Times, &c. the same Distance, with the same Diminution, according to the Squares of such Distances perpetually. Which noble Discovery proved the happy Occasion of the Invention of the wonderful *Newtonian* Philosophy.

(iv) Fourthly, a sketch of the history of the subject is given in the preface by Pemberton to his *View of Sir Isaac Newton's Philosophy*, London, 1728, and the information seems to have been derived direct from Newton's conversations in 1725 or 1726. The portion of the preface which bears on this point is as follows:

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 center 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, 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 center 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 center of the earth. This gave occasion to his resuming his former thoughts concerning the moon; and *Picart* 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 center 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 ellipse, the center 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 one year and an half.

It will be noticed that the above authorities deal but lightly with the investigations of 1666. We may, however, infer from them (*a*) that in 1666 Newton believed it probable that the force which retained the moon in its orbit about the earth was the same as terrestrial gravity; (*b*) that he tried to verify this on the hypothesis that the lunar orbit was a circle having the earth as centre; (*c*) and that he concluded from his calculations that gravity extended to the moon, and in a general way varied inversely as the square of the distance. On the other hand it should be added that his papers and correspondence of 1679 and 1685 show that at this time he did not know how to determine the attraction of a solid body (like the sun or earth) on an external particle; he did not think that the earth's attraction was directed exactly to its centre; and lastly, it is probable that previous to 1685 he considered that the attraction of a planet on its moon (or of the sun on a planet) could be regarded as directed to a fixed point, and varying as the inverse square of the distance from it, only when the two bodies were at a distance so great that their dimensions could be neglected compared with the distance between them.

(*a*) As to the origin of the idea that the force which retained the moon in her orbit about the earth was the same as terrestrial gravity, there is nothing to add to the account given by Pemberton. If the power of gravity were felt even on the summits of the highest mountains, it was reasonable to suppose that it extended farther, and if so, "why not as high "as the moon," in which case "her motion must be influenced "by it; perhaps she is retained in her orbit thereby."

The well-known anecdote that this idea was suggested by the fall of an apple rests on good authority. It is explicitly

stated to be the fact by Conduitt*, who, as the friend and assistant of Newton at the Mint, and as the husband of Newton's favourite niece, had exceptional means of information. It was repeated later by Mrs. Conduitt in a conversation with Voltaire†. It is also mentioned by R. Greene‡ on the authority of Martin Folkes, the Vice-President of the Royal Society during the last few years of Newton's tenure as President, than whom no one would be more likely to have heard the story from Newton. It will be noticed further that it is quite consistent with the interesting sketch of the chain of reasoning followed by Newton which is given by Pemberton, and which he expressly says originated when Newton was sitting in the garden. Finally the story is confirmed by local tradition, and the reputed tree was tended so carefully that it was kept alive until 1820§.

(b) The reason that Newton assumed the orbit of the moon relative to the earth to be a circle seems to have been that in that case the centripetal force on the moon to the centre of the circle could be determined exactly, and therefore could be compared with gravity. He must have been aware that the lunar orbit was very approximately an ellipse; but it is so nearly circular, and his attempted verification was so rough that he might well think that, for his purpose, it was sufficient to take it as circular.

Assuming the orbit to be circular, he was able to show that the centripetal force to the centre varied inversely as the square of the distance. This would appear to have been effected in

* See the paper, drawn up by Conduitt to assist de Fontenelle in preparing for the French Academy his *éloge* on Newton, printed in E. Turnor's *History of Grantham*, London, 1806, p. 160. See also Conduitt's memorandum, quoted by Brewster, vol. ii. p. 340.

† Voltaire's *Elémens de la Philosophie de Newton*, 1738, Beuchot's edition of Voltaire's works, vol. xxxviii. p. 196.

‡ *The Principles of the Philosophy of the Expansive and Contractive Forces*, Cambridge, 1727, p. 972.

§ Brewster, vol. i. p. 24; vol. ii. p. 340, note; see also Biot, *Journal des Savans*, 1832, p. 265.

two steps, as follows. Let v be the velocity of the moon treated as a particle, r the radius of its circular orbit, T its periodic time, and f the acceleration to the centre of the circle. In the first place he proved the result $f = v^2/r$, and next deduced that f varied inversely as r^2 .

It is not unlikely that the first part of his proof was that given below. At the close of his letter to Halley of July 14, 1686, and mentioning the form in which he proposed to print the scholium to prop. 4 of book i. of the *Principia*, he says: "in turning over some old papers I met with another demonstration of that proposition which I have added." This demonstration is as follows:

In circulo quovis describi intelligatur Polygonum laterum quotcumq; Et si corpus in Polygona lateribus data cum velocitate movendo, ad ejus angulos singulos a circulo reflectatur; vis qua singulis reflexionibus impingit in circulum erit ut ejus velocitas, adeoq; summa virium in dato tempore erit ut velocitas illa et numerus reflexionum conjunctim, hoc est (si Polygonum detur specie) ut longitudo dato illo tempore descripta et longitudo eadem applicata ad Radium circuli; id est ut quadratum longitudinis illius applicatum ad Radium; adeoq; si Polygonum lateribus infinite diminutis coincidat cum circulo, ut quadratum arcus dato tempore descripti applicatum ad radium. Haec est vis [centrifuga] qua corpus urget circulum, et huic aequalis est vis contraria qua circulus continuo repellit corpus centrum versus.

The deduction of the law of force is now easy*. We have $v = 2\pi r/T$; but, by Kepler's third law, T^2 varies as r^3 ; hence

$$v^2 \propto r^2/T^2 \propto r^2/r^3 \propto 1/r.$$

Therefore

$$f = v^2/r \propto 1/r^2.$$

It remained to apply this proposition to show that the centripetal force on the moon was the same as gravity. Presumably Newton's method of verification was the same as that which he published subsequently † when he used more accurate

* *Principia*, book i. prop. 4, cor. 6; or *De Motu*, theor. 2, cor. 5, see below, p. 37.

† *Principia*, book iii. prop. 4. Newton discussed the question in more detail in the second and third editions in a corollary to book iii. prop. 37.

data. This was as follows. He calculated the space x through which the moon (considered as a particle) was drawn towards the earth in one second. He knew the space x' through which a particle near the surface of the earth was drawn by gravity in one second: it is about 16.1 feet. If his calculation showed that the ratio of x to x' was inversely as the ratio of the square of the distance of the moon from the earth's centre to the square of the distance of the particle from the earth's centre, it would follow that the force which kept the moon in its (circular) orbit was the same as gravity.

Newton made the numerical calculations, "though," as he says, "not accurately enough." Most likely they were made in the same way as that given in the proposition above alluded to. This was as follows. The mean distance of the moon may be taken as $60a$ where a is the radius of the earth; the periodic time of the moon is 39,343 minutes. Hence, in one minute the moon describes an arc $2\pi \times 60a/39343$, and, therefore, is drawn towards the earth through a distance equal to the versine of this arc. If a be taken as 4,000 miles, this distance is about 16 English feet, and it would follow that in one second the moon would be drawn through a distance x equal to $16/(60)^2$ feet; thus the moon and a particle near the earth would be drawn towards the earth in one second through spaces very nearly inversely proportional to the squares of their distances from the earth's centre; and the approximation is so close that we might reasonably infer that the force which keeps the moon in its orbit is the same as gravity.

This verification requires that the value of a , the earth's radius, shall be known approximately. Now, Pemberton and Whiston agree that Newton assumed that a degree of latitude on the earth contained 60 miles (whereas, in fact, it contains about $69\frac{1}{2}$ miles). This is equivalent to saying that Newton supposed that the radius of the earth was $10800/\pi$ miles, *i.e.* rather more than 3,400 miles (whereas, in fact, it contains about 4,000 miles). Hence his value of a was about one-eighth too small, and thus his calculations seemed to show that

in one second the moon fell through only seven-eighths of the distance through which gravity alone would have pulled it: the calculation with his data gives $x = 13.9$. . feet.

This discrepancy does not appear to have shaken Newton's faith in the view that gravity, diminishing inversely as the square of the distance, acted on the moon, but led him to infer that some other force acted as well; and Whiston adds that Newton believed the other cause to be "Cartesius's vortices," while Pemberton states that Newton's "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."

Newton's assumption of so inaccurate a value of the radius of the earth is somewhat strange. Rigaud, however, pointed out* that this value had been given by Edward Wright, of Caius College, Cambridge, in 1610, in his *Certaine Errors in Navigation*, and conjectured that it was not unlikely that this book was a standard one in the University, and thus that the result might have been familiar to Newton; this opinion is somewhat strengthened by the fact that I have found among the library left by Robert Smith (B.A., 1711) a copy of the edition of 1657 of Wright's work, which seems to have been purchased by Smith as a second-hand book, and, from its appearance, may be not unreasonably suspected to have been a student's University text-book. To this edition a note is prefixed calling attention to some new matter inserted therein on a "demonstration for the finding of the quantitie of the Earth's Semidiameter," and the result of the calculation [p. 95] is that the radius is 18,312,621 feet, that is, about $3,468\frac{1}{3}$ miles, which gives for the length of a degree of latitude almost exactly 60 miles.

It seems not unlikely that this was the source of Newton's error. At the same time it should be observed that fairly accurate estimates of the earth's radius were then current. In

* Rigaud, pp. 3-12.

an appendix* to this very edition of 1657 of Wright's work the length of a degree of latitude is taken as $17\frac{1}{2}$ leagues or 350,000 feet, that is, about $66\frac{1}{3}$ miles. But Norwood's estimate†, first published in 1636, of 367,196 feet for the length of a degree was in common use in 1666, and is substantially correct—it would have made x almost exactly equal to 16 feet. Even Willebrod Snell's estimate‡, made in 1617, of 28,473 Rhinland perches for the length of a degree, is equivalent to taking a value which gives x equal to 15.5 feet; and this estimate was probably known to Newton, as it is specifically mentioned by Varenius in his *Geographia*§, Amsterdam, 1650, of which Newton himself brought out an edition in 1672, though, as this edition contains several numerical misstatements, it may be that Newton did not attach much importance to it or revise it carefully.

No doubt Newton would take "the common estimate," as Pemberton says, for his calculations were made in Lincolnshire, where he was "absent from books," but the above references show that the then common estimate was substantially correct. It is also curious that, if Newton attached any importance to the inquiry, he should not have renewed his calculations as soon as he returned to Cambridge, where he could have checked the accuracy of the data he had used. Possibly the explanation is to be found in the fact that his investigations at this period were mainly concerned with optics, fluxions, and algebra, and that he regarded gravitation as a subject outside his own special line of study.

On the other hand, the late Prof. Adams told me that he believed that Pemberton and Whiston were mistaken as to the insufficiency of the verification. Newton knew that the orbit was not actually circular, and that his numerical data were only approximate; hence he could have expected only a rough

* *The Division of the whole Art of Navigation*, p. 6.

† *The Seaman's Practice*, London, 1636, chap. ii.

‡ *Eratosthenes Batavus*, Leyden, 1617, p. 197.

§ B. Varenius, *Geographia*, book i. chap. iv. (p. 39 in edition of 1650).

verification of the hypothesis, and as he asserted (see above, p. 7) that he found his results agree or "answer pretty nearly," Prof. Adams considered that these calculations were sufficient to convince Newton that it was gravity alone that retained the moon in its orbit, and further he strongly suspected that Newton already believed that gravity was due to the fact that every particle of matter attracts every other particle, and that this attraction varied as the product of the masses and inversely as the square of the distance between them. Any opinion that Prof. Adams expressed on the subject must carry great weight, and the matter is one which may be fairly left to the judgment of the reader. Fortunately, the question whether Newton in 1666 came to the conclusion that gravity is only the chief cause (as Pemberton and Whiston imply), or whether he then came to the conclusion that it was the sole cause by which the moon is retained in its orbit, is comparatively unimportant, because there is no doubt as to what his conclusions ultimately were, and the question of the date when he convinced himself that gravity was sufficient by itself, and that the Cartesian vortices did not exist, is mainly a matter of antiquarian interest.

It may be added that an exact verification is not easily made. It happens that, if the moon's distance be taken as 60×4000 miles, the consequent calculation leads to a result fairly consistent with the hypothesis that the motion is due only to gravity, but when the most accurate available data are used for determining the distances of the moon in two consecutive minutes it will be found that the result agrees only approximately with the result deduced from the hypothesis, for actually the moon's orbit is subject to numerous disturbing forces which materially affect the result.

CHAPTER III.

INVESTIGATIONS IN 1679.

WHATEVER were Newton's conclusions in 1666, there appears to be no doubt that he did not pursue the subject again until the year 1679, though in or about 1677 he discussed it with Wren and Donne*, and explained his views especially as to the law of the inverse square of the distance according to which he supposed gravity to act.

In 1679 his attention was recalled to the question by some correspondence with Hooke. All but two of the letters that passed between them at this time are printed below (chapter viii. A. 1-7), as also the subsequent comments of Newton (chapter viii. B. 2, 4, 6, 7) and of Hooke (chapter viii. A. 8).

This correspondence—especially the letter from Newton of Nov. 28, 1679—will be read with interest, but its chief value lies in the fact that it turned Newton's attention again to the problem of planetary motion. It will be convenient, however, to summarise very briefly the correspondence itself. On Nov. 24, 1679, Hooke, at the request of the Royal Society, wrote a friendly letter to Newton expressing a hope that he would continue to make communications to the Society, and informing him of the results of various recent scientific investigations, among the rest that Picard and others were making geodetical observations in France. In Newton's reply, dated Nov. 28, 1679, he said that he had ceased to interest

* Newton's letter of May 27, 1686, printed below, p. 155.

himself in philosophy, except as a diversion from other studies, but from a feeling of courtesy he discussed some of the opinions quoted by Hooke, and mentioned that it had occurred to him that a demonstration of the earth's rotation on its axis might be obtained by seeing whether a stone, when falling freely, deviated in an easterly direction from the perpendicular, and added that the free path was part of a spiral which passed through the earth's centre. The letter was read to the Society on Dec. 4, 1679, and they asked Hooke to make the experiment suggested by Newton. On Dec. 9, 1679, Hooke wrote to Newton and stated (i) that the stone would fall to the south-east (and not to the east) of the perpendicular, and (ii) that the path of a falling body would be "an excentrical elliptoid" and not a spiral*. Newton's reply is missing, but in it he seems to have admitted the justice of these criticisms, and, in writing about the latter point, to have worked out the path of a particle moving under gravity on the concave surface of an inverted cone, for which he assumed (correctly) that gravity was sensibly constant. In Hooke's answer, dated Jan. 6, 1680†, he stated that he supposed that the attraction always varied inversely as the square of the distance; he also alluded to the possibility of explaining the planetary motions; and he added further that the experiment suggested by Newton in his first letter had been successful. On Jan. 17, Hooke wrote again saying that the experiment had succeeded; and he concluded by reverting to the question of motion under a central attractive force, assuring Newton that if he had time to consider the matter, a word or two on his thoughts would be grateful both to Hooke and to the Royal Society where the problem had been discussed.

* Some doubt has been expressed as to whether by an eccentric elliptoid Hooke meant merely an oval curve or meant an exact ellipse. His letter of Jan. 17, 1680 (see below, p. 149), seems to make the first of these views the more probable.

† That is, Jan. 6, 1679, or as it is sometimes written 1679-80. I usually write such dates as if the year was taken for all purposes to begin on Jan. 1.

This correspondence, says Pemberton*, put Newton "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 center of the earth. This gave occasion to his resuming his former thoughts concerning the moon; and Picart 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 the power decreases, as you recede from the center of the earth in the manner" he had formerly conjectured.

The question then arose whether a particle projected under a force varying inversely as the square of the distance from a fixed point and directed to it would describe an ellipse, as by Kepler's first law the planets were known to do. Using the hypothesis of the law of the inverse square of the distance†, Newton "found the line described by a falling body to be an ellipsis, the center 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."

The statements made by Newton agree with this account. Thus Newton, in his letter to Halley of July 14, 1686, says that Hooke's "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." So again in the Portsmouth draft (see above, p. 7) Newton,

* Pemberton's account, see above, p. 10.

† *Ibid*, see above, p. 10.

referring to his investigations of this period, says that he "found the Proposition that by a centrifugal force reciprocally "as the square of the distance a Planet must revolve in an "Ellipsis about the center of the force placed in the lower "umbilicus of the Ellipsis and with a radius drawn to that "center describe areas proportional to the times." Finally, Conduitt's account of Newton's conversation with Halley in August, 1684, is consistent with this statement (see below, p. 26).

I feel some doubt whether Pemberton in mentioning the "near a dozen propositions" composed at this time was not thinking of the tract *De Motu*, which was written in 1684, and is described later. Newton himself in the memorandum printed below (see p. 58), in describing his work of this time, mentions only the two propositions afterwards printed in the *Principia* as book i. props. 1 and 11; but perhaps I ought to remind the reader that the converse of the latter proposition is proved in what is printed as the first corollary to prop. 13, though it is really a corollary to props. 11, 12, and 13, and as, from the foregoing authorities, there can be no doubt that this result was included in the discoveries of 1679, it is probable that it was proved in this way and not as in prop. 17.

We may say, therefore, that Newton at this time demonstrated (i) the conservation of areas under a centripetal force; (ii) that, if an ellipse were described about a focus under a centripetal force in the focus, the law was that of the inverse square of the distance; (iii) and conversely that the orbit of a particle projected under a central force varying as the inverse square of the distance was a conic—or perhaps he would have said an ellipse—having the centre of force in a focus.

It is certain that Newton took up the subject again in consequence of Hooke's letters—see especially the letter of Jan. 17, 1680—but it is equally certain that the whole investigation was due to Newton. At a subsequent period, however, Hooke claimed that no inconsiderable share of the credit was due to him because of the assertions or hints contained in his papers as to what the result of these investigations would be.

The material parts of those papers are printed below, and the reader can judge for himself as to the justice of the claim. Newton at once repudiated it, and in a letter* to Halley of July 27, 1686, expresses, though somewhat ungraciously, his opinion as follows: "Though 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 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, the suggestion made by Hooke in his letter of Jan. 6, 1680, was really a guess; but though he had the sagacity to divine the right conclusion he could not—or at any rate did not—verify it, and there can be no comparison between an unverified conjecture and a rigorous demonstration such as Newton gave. This question was not raised by Hooke till 1686, and I will defer any further mention of it to chapter v. (see below, pp. 68–73).

No doubt Pemberton is correct in saying that at this time Newton repeated his calculations of 1666, and satisfied himself that the centripetal force which retained the moon in its orbit was the same as terrestrial gravity. Whiston seems to imply (see above, p. 9) that this revision of Newton's former work was due to the accident of his coming across it in turning over some old papers, but though he may have found his former calculations in this way, I think it is clear that Hooke's letters were the real cause of the repetition of the calculations of 1666. Pemberton and Whiston assert that in this revision Picard's estimate of the earth's radius was used instead of the inaccurate one previously employed. Picard had given this value in 1671 in his *Mésure de la Terre*, a work which was communicated to the Royal Society on Jan. 11, 1672, and seems to have been tolerably

* See below, p. 167.

well known, at any rate after 1675; but it does not appear that Newton's attention was called to it specially, and as his time was fully occupied with other researches it may have escaped his attention. Hooke, however, in his letter of Nov. 24, 1679, alluded to Picard's geodetical measurements, and probably this led Newton to use Picard's estimate in the revision of his calculation.

There does not seem to be any contemporary evidence opposed to the above account, but Robison*, writing in 1804, says that Newton having become a member of the Royal Society—he was elected in 1672 and admitted in 1675—there “learned “the accurate measurement of the Earth by Picard, differing “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 calculations. As they drew to 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 phenomena with the utmost precision.”

No authorities for this story are cited, and the source from which it was obtained is not known. No credence was given to it by Rigaud, De Morgan, or Brewster. I think the correspondence of 1679 and 1686—published subsequently to Robison's work—may be taken as proving conclusively that Robison had been misinformed. This story was repeated by Biot†, who further conjectured that the event might have taken place in 1682, but it should be remembered that Biot was not aware of the existence of the letters of 1679–80 and 1686; in the English translation of Biot's biography of Newton this conjecture is converted into a positive assertion of the fact‡, and it has been thence copied into various popular works.

* *System of Mechanical Philosophy*, by J. Robison, Edinburgh, 1804, p. 288.

† *Biographie universelle*, 1822, vol. xxxi. p. 154.

‡ *Life of Newton*, published by the Society for Promoting Useful Knowledge, p. 17.

Jones informed the writers of the General Dictionary that Newton "ascertained the law of the motion in an ellipse, "when the force was inversely as the squares of the distance, "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." It is just possible that Jones was thinking of Newton's conversation in 1677 with Donne and Wren, but certainly the allusion to the year 1683 is wrong, and we may safely assert that the time mentioned for the discovery of the law of the motion in an ellipse under gravitation is also inaccurate.

CHAPTER IV.

INVESTIGATIONS IN 1684.

NEWTON never showed an undue haste in publishing his discoveries, though he was willing enough to communicate them to his friends if directly asked. Hence the results of his investigations in 1679, like those in 1666, were not for some years sent to any Society, and the scientific world remained unaware of their extent and value.

In January, 1684, Halley*, "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"; but was unable to deduce from that hypothesis the motion of the heavenly bodies.

In the same month he went to London, where he met Wren and Hooke. He found that Wren had come to the same opinion as himself, but equally was unable to find the consequences thereof. Hooke, however, boasted "that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it." But nothing could be extracted from Hooke, and, though at last he promised to show his demonstration to Wren, he revealed nothing; and there can be no doubt that Wren and Halley considered that his boast was not justified by the facts.

On this, in August, 1684, Halley went to Cambridge, and mentioned the problem to Newton, and "then learned the

* Halley's letter of June 29, 1686, printed below, p. 162.

“good news that” Newton “had brought this demonstration “to perfection,” of which demonstration he promised to send a copy to Halley. Newton could not, however, lay his hands on the original paper, and so, “not finding it, did it again, and “reduced it into the propositions” * which he sent to Halley by Paget in November, 1684.

The account given by Conduitt is substantially the same †. After mentioning the inability of Halley, Wren, and Hooke to find the orbit described under a central force which varied inversely as the square of the distance, he says that Halley set out for Cambridge in May (which obviously is a slip for August), 1684, to consult Newton. “Without mentioning “either his own speculations, or those of Hooke and Wren, he “at once indicated the object of his visit by asking Newton “what would be the curve described by the planets on the “supposition that gravity diminished at the square of the “distance. Newton immediately answered, *an Ellipse*. Struck “with joy and amazement, Halley asked him how he knew it? “Why, replied he, I have calculated it; and being asked for “the calculation, he could not find it, but promised to send it “to him. After Halley left Cambridge, Newton endeavoured “to reproduce the calculation, but did not succeed in obtaining “the same result. Upon examining carefully his diagram and “calculation, he found that in describing an ellipse coarsely “with his own hand, he had drawn the two axes of the curve “instead of two conjugate diameters somewhat inclined to one “another. When this mistake was corrected he obtained the “result which he had announced to Halley.”

The propositions were not sent to Halley until November, 1684, and presumably were on the conservation of areas under a central force, the law of force in a focus under which an ellipse can be described, and the converse on the orbit described by a particle under a central force which varies inversely as

* Newton's letter of July 14, 1686, printed below, p. 165.

† Brewster, vol. i. p. 259, whence I take the quotation.

the square of the distance (*Principia*, book i. props. 1, 11, and cor. 1 of prop. 13). It is possible that they also included the law of force to the centre under which a circle is described, and the law of force to any point under which any curve is described (book i. props. 4, 6).

Fortunately, Newton, after Halley's visit in August, did not confine himself to writing out his old demonstrations, but continued his investigations, though doubtless he had no idea of the extent to which they could be pushed, or how far they were likely to lead him. By the Michaelmas Term, 1684, he had put them together in manuscript in a connected form, and this manuscript he read as his professorial lectures in that term.

Newton had been appointed Lucasian professor on Oct. 29, 1669, and under the statutes relating to the chair it was his duty to lecture for one hour at least once a week during term, and to be accessible to students during two hours on two days in every week in term, and on one day in every week in vacation if he were in residence; but it seems to have been universally understood that the duty of lecturing was confined to one term in the year, and his amanuensis states that the lectures usually lasted only half an hour. It is most likely that the lectures were read or dictated as rapidly as they could be taken down, and that any explanations were given in interviews in his rooms in the week following the lecture. All junior sophs and all bachelors of arts under the standing of master were required to attend, but the rule was not enforced.

Newton's lectures for eighteen out of the first nineteen years of his tenure of the chair are extant*; except in the session

* They are in four manuscript volumes preserved in the University Library, containing respectively the lectures from 1669 to 1672, those from 1673 to 1683, those for 1684 and 1685, and those for 1687. The lectures from 1669 to 1672 are not holograph, but the marginal notes and corrections are in Newton's handwriting; they were deposited in the Library in 1674. The rest are probably holograph. The volumes

1669–70, they were always given in the Michaelmas Term. He seems never to have repeated his lectures, and, roughly speaking, the lectures of one October continue from the point at which those in the preceding December terminated. The lectures from 1669 to 1672 were on optics, those from 1673 to 1683 on arithmetic and algebra, those in 1684 and 1685 comprised some of the propositions afterwards printed in the *Principia*; those in 1686 are missing; and those in 1687 were a popular exposition of the third book of the *Principia*.

The lectures given in the Michaelmas Term, 1684, were nine in number, and are entitled *De Motu Corporum*. The manuscript of them may be taken as being a rough draft of the beginning of the first book of the *Principia*, to which the numbers of the propositions given below refer. The first lecture is on the definitions as given in the *Principia*; the second on the laws of motion; the third on the laws of motion and the first four lemmas; the fourth on the remaining seven lemmas in section i; the fifth on props. 1–9, lemma 12, and prop. 10; the sixth on props. 11, 12, lemmas 13, 14, and props. 13–15; the seventh on props. 16–19; the eighth on lemma 16, and

containing the lectures for 1684, 1685, and 1687, have George I.'s book-plate (Bishop Moore's library) in them, and this seems to indicate that they were not deposited in the Library till 1715; but probably the plate has been pasted in by error, for Cotes, writing to Jones on Sept. 30, 1711, says: "We have nothing of St Isaac's that I know of "in Manuscript at Cambridge besides the first draught of his *Principia* "as he read it in his Lectures, his Algebra Lectures which are printed, "and his Optick Lectures the substance of which is for y^e most part "contained in his printed Book but with further Improvements;" and there appear to be no other manuscripts save the volumes for 1684–5 and 1687 to which Cotes's remark about the first draft of the *Principia* can refer. Only five of the lectures for 1687 are written out in Newton's manuscript; these are divided into 28 sections. There is also another copy of them in the Library of Trinity College, written by Cotes in 1700 when he was an undergraduate, and contained in a note-book wherein he has also copied the (then unpublished) lectures of 1673–1683. See the *Cotes Correspondence*, pp. lv, xci–xcviii, 209.

props. 21, 30 ; and the ninth on props. 32-35*. Newton remarked that demonstrations of props. 18 and 19 were so easy that it was unnecessary to give them. To prop. 30 he added a scholium containing an approximate solution of the problem of which he subsequently gave a solution in prop. 31. Prop. 35 is left unfinished, and it is not improbable that his course of lectures did not actually extend beyond prop. 32 or 33. The differences between the language and demonstrations of these propositions as given in the *Principia* and in these lectures are not sufficiently important to make it worth while to reproduce them here, though they might find a place in a critical edition of the *Principia*.

The first draft of the manuscript (which probably represents the lectures actually delivered) has been altered by Newton in several details ; perhaps this is more noticeable from and after prop. 16, cor. 6, and of the following part of the manuscript (which contains the substance of prop. 16, cors. 6-9, props. 17, 18, 19, 21, and lemma 16) Newton wrote out (in the same book) a clean copy, embodying the alterations, to which he added prop. 20 and lemmas 15, 17, 18, 19, 20, 21 of the *Principia*. The enunciations in this clean copy are numbered as in the *Principia*, and the text of the demonstrations follows closely that of the first edition ; it is not divided into lectures. The first lecture of the Michaelmas Term, 1685, was on prop. 22, and it immediately follows the conclusion of the clean copy. Most likely this clean copy does not represent lectures actually delivered, but is identical with the manuscript of the first book of the *Principia* of which we know the draft was finished by the summer of 1685, and

* By a clerical error props. 16 and 17 of the *Principia* are in the manuscript both numbered 16, and hence props. 18, 19, 21, 30, 32, 33, 34, 35 of the *Principia* are in the manuscript numbered respectively 17, 18, 19, 20, 21, 22, 23, 24 ; also as lemma 15 of the *Principia* was not inserted in the manuscript, lemma 16 of the *Principia* is numbered 15 in the manuscript ; but hereafter I refer to these propositions by the numbers affixed to them in the *Principia*.

therefore before Newton began his lectures in October of that year.

On the receipt in November* of Newton's communication, Halley "took another journey to Cambridge on purpose to confer with Newton" about it, "since which time," says he, "it "has been entered upon the Register Books of the Society." It is, I think, probable that it was at this visit that Halley pressed Newton to send some account of his discoveries to the Royal Society† in order that the priority of his discoveries might not be impugned—a precaution which Hooke's conduct would seem to show was not unnecessary—and further urged him to continue his investigations which, as we shall see, led to the work now known as the *Principia*, but which then and for some months after was described as *Propositiones De Motu*, and of the earlier part of which the tract now known by that name‡ may be regarded as a summary.

On his return, at the meeting of the Royal Society§ on Dec. 10, 1684, "Mr. Halley gave an account, that he had "lately seen Mr. Newton at Cambridge, who had shewed him "a curious treatise, *De Motu*; which, upon Mr. Halley's "desire, was, he said, promised to be sent to the Society to be "entered upon their register. Mr. Halley was desired to put "Mr. Newton in mind of his promise for the securing his "invention to himself till such time as he could be at leisure "to publish it"; and Paget was requested to join Halley in reminding Newton of his promise. I take it that the "curious treatise" which Halley asserts that he saw on this visit to Cambridge in November or December, 1684, and to

* Halley's letter of June 29, 1686, printed below, p. 163.

† Preface to the first edition of the *Principia*, see below, p. 57.

‡ This double use of the title *De Motu* is illustrated by Newton's letter of Feb, 23, 1685 (see below, p. 32). Note also that the three books of the *Principia* were published with the headings *De Motu Corporum, Liber Primus*; *Ibid, Liber Secundus*; and *De Mundi Systemate, Liber Tertius*.

§ Birch, *History of the Royal Society*, London, 1757, vol. iv. p. 347.

which he referred at the meeting of the Royal Society on Dec. 10 of that year, was the manuscript of the lectures Newton was then giving: at any rate, these lectures were then written, and bear the title that he quoted. I also suppose that the tract *De Motu* was written in December, 1684, or January, 1685, in consequence of Halley's solicitations that some note of Newton's discoveries should be put on record, and that the preparation of the *Principia* was commenced at about the same time.

Some writers have, however, given a slightly different interpretation* to the evidence above referred to. Some have supposed that Newton's communication in November, 1684, was confined to the results of book i. props 1, 11, and cor. 1 of 13, and that the curious treatise seen at Cambridge by Halley in November or December, 1684, was the tract entitled *Propositiones De Motu* which, in accordance with the minute of Dec. 10, Newton sent to the Society in February, 1685. Others have supposed that Newton sent this tract to Halley in November, 1684, and that the curious treatise seen by Halley a few weeks later was that part of the *Principia* which was then written. In support of this view it may be noticed that Halley, writing in 1686 of his visit to Newton in the autumn of 1684, says that since that visit, Newton's communication was entered on the register books of the Royal Society. Now the only memoir registered in the books of the Society is the tract entitled *Propositiones De Motu*, and hence it may be argued that the tract was the manuscript sent to Halley in November, 1684, which was produced at the meeting of the Society on Dec. 10, and this view is strengthened by the fact that some one has written at the beginning of it "December "10, 1684." On the other hand we find that on Feb. 23, 1685, Newton wrote† as follows to Aston, the then Secretary of the

* On these views, see Edleston in the *Cotes Correspondence*, p. lv; and Brewster, vol. i. pp. 260-262. I have followed Rigaud on this point.

† Rigaud, Appendix, p. 24.

Royal Society: "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." This letter was communicated to the
 Royal Society at their meeting* on Feb. 25, 1680; and on the
 whole it seems to make it probable that the tract was not sent
 until February, 1685, and that the headline reference to
 Dec. 10, 1684, relates, as is usual in the books of the
 Society, merely to the date on which the matter was first
 brought forward. Moreover, we have no reason to think that
 Newton had commenced or even intended to write a treatise,
 such as the *Principia*, until after Halley's visit towards the end
 of 1684.

The only other letter of Halley on the subject with which
 I am acquainted leaves the question undecided, for he merely
 says—writing to Wallis on Dec. 11, 1686—"Mr. Is. Newton
 "about two years since gave me the enclosed propositions."
 The point is not material to our history, and is not worth
 discussing further; all the available evidence is before the
 reader, and he can form his own conclusions. It is, at any
 rate, certain that Newton's lectures in the Michaelmas Term
 of 1684 covered the same ground as the earlier propositions in
 the *Principia*, and that he sent the tract, known as the *De
 Motu*, to London before the end of February, 1685.

Here I may mention that there is a curious blunder in the
Commercium Epistolicum where† this tract is said to have been
 sent to the Royal Society in the latter end of 1683, and the
 same date is mentioned in the Portsmouth draft (see above,
 p. 7). It is certain that this is a mistake. The way in which

* Birch, *History of the Royal Society*, vol. iv. p. 370.

† *Commercium Epistolicum*, No. lxxi; edition of 1712, p. 97; edition
 of 1722, p. 206; edition of 1856, p. 157.

the error arose has been satisfactorily explained by Rigaud and Edleston*. Another blunder, which may be noticed in passing, is that made by Newton in a memorandum quoted below (see below, p. 58), where the discovery of some of the results given in his lectures of 1684 is assigned to June and July, 1684; they cannot have been made before August, but must have been made before the end of November.

The tract *Propositiones De Motu* is memorable as marking the point at which Newton had arrived about the end of 1684; it contains the chief propositions given in his lectures of that year, as also two or three theorems on motion in a resisting medium.

The tract contains eleven propositions, preceded by three definitions, a statement of four hypotheses or assumptions, and two lemmas. The proofs are geometrical. The propositions are as follows—the corresponding proposition in the *Principia* being indicated by a reference in square brackets: (i) the equable description of areas by the radius vector of a body moving under a central force [book i. prop. 1]; (ii) comparison of the forces under which bodies describe circles uniformly, with five corollaries [book i. prop. 4]; (iii) the law of centripetal force to a given point under which any curve can be described [book i. prop. 6]; (iv) application to a circle described under a force directed to a point in the circumference [book i. prop. 7 of the first edition]; (v) application to an ellipse described about the centre [book i. prop. 10]; (vi) application to an ellipse described about the focus, with a scholium on the application to the planets [book i. prop. 11]; (vii) if ellipses be described under a central force which varies inversely as the square of the distance, then the squares of the periodic times are proportional to the cubes of the major axes; to

* Rigaud, pp. 16-20; Edleston, *Cotes Correspondence*, p. 207; see also Brewster, vol. i. pp. 258, 259. Jones was one of the committee appointed to draw up the *Commercium Epistolicum*, and his statement on this point, quoted above, at the end of the last chapter, is not independent evidence.

which a note is added on the application to planetary motion [book i. prop. 15]; (viii) determination of the orbit of a body projected with a given velocity under the action of a central force which varies inversely as the square of the distance [book i. prop. 17]; followed by a note on cometary paths which is obscure and partly incorrect; (ix) determination of the space through which a body will fall in a given time towards a fixed point under the action of a force which varies inversely as the square of the distance from that point and which is directed to that point, *i.e.* a body falling under gravity [book i. prop. 32]; (x) determination of the motion of a projectile in a resisting medium—the resistance varying as the velocity—under no force [book ii. prop. 2]; (xi) determination of the motion of a projectile in a resisting medium—the resistance varying as the velocity—under a constant centripetal force [book ii. props. 3 and 4].

This tract has been printed only once, and copies are now so scarce that I think it may be interesting to reproduce it. Five manuscripts of it are extant; of these one is in the Royal Society's Register, vol. vi. pp. 218–234; one is in the Macclesfield Collection; and three are in the Portsmouth Collection, section i. division viii. number 7. The example in the possession of the Royal Society is presumably taken from the original memoir of Newton, and therefore may be regarded as the standard text; it was printed by Rigaud. Unfortunately, says Rigaud, "the person who transcribed the paper . . . could not have understood what he was employed to copy. "The divisions of the sentences are not well attended to, " words are sometimes wrongly spelt, and the diagrams, as well " as the references to them in the text, are in several instances " very faulty."

I have never seen the Macclesfield copy, but I gather that it is merely a transcript, made by or for William Jones, of that owned by the Royal Society. The three Portsmouth manuscripts are more interesting. The chief points in which they differ from the copy in the possession of the Royal

Society are mentioned below. Two of them are incomplete drafts in Newton's handwriting, and made, I conjecture, prior to the memoir being sent to London. The third, probably also holograph, is carefully written out, and is somewhat fuller than that in the archives of the Royal Society; it may have been written subsequently to the copy sent to London, but I think it is of a date not later than the spring of 1685; at any rate, it contains a remark (see below, p. 56) which implies that though Newton knew that, at a considerable distance from the earth's centre, gravity might be taken for purposes of calculation to vary inversely as the square of the distance from that point, yet he was then uncertain whether the same law held for points near the earth's surface. As, however, the date and history of the Royal Society's copy are accurately known, I have printed the tract in the form there written except in three respects: in the first place I have corrected the diagrams, and all purely clerical blunders, by Newton's own copy; in the second place I have inserted punctuation, and in some cases have altered the initial letters of words to capitals or small letters as the case may be, so as to make the practice uniform (in these respects Rigaud has not followed the manuscript exactly); and in the third place I have written the symbol \times instead of $+$, and instead of *et*, when $+$ and *et* are used (as in these manuscripts they not infrequently are) to indicate multiplication. I have also replaced any symbol like *ABC* by $AB \times BC$ when it is used to denote the product of *AB* and *BC*. I need hardly add that in the writings of mathematicians of the seventeenth century expressions like x^a , x^c , x^{aq} , x^{qc} stand for what we usually write as x^2 , x^3 , x^4 , x^5 .

The tract is as follows:

ISAACI NEWTONI PROPOSITIONES DE MOTU.

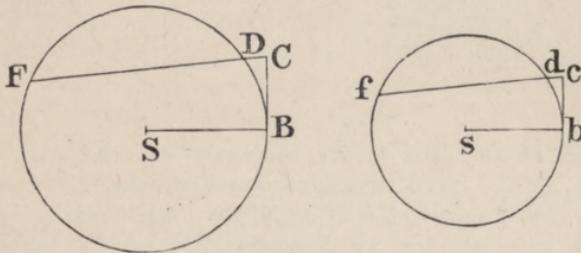
Definitiones.

1. VIM centripetam appello, qua corpus impellitur vel attrahitur versus aliquod punctum, quod ut centrum spectatur.
2. Et vim corporis, seu corpori insitam, qua id conatur perseverare in motu suo secundum lineam rectam.
3. Et resistentiam, quae est medii regulariter impediens.

igitur temporibus aequales areae describuntur. Sunt jam haec 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 circularum uniformiter gyrantibus, vires centripetas esse ut arcuum simul descriptorum quadrata applicata ad radios circularum.

Corpora B, b , in circumferentiis circularum BD, Bd gyrantia, simul describant arcus BD, bd . Sola vi insita describerent tangentes BC, bc ,



his arcibus aequales; vires centripetae sunt quae perpetuo retrahunt corpora de tangentibus ad circumferentias, atque adeo hae sunt ad invicem ut spatia ipsis superata CD, cd ; id est productis CD, cd ad F et f ut $\frac{BC^{quad}}{CF}$ ad $\frac{bc^{quad}}{cf}$, sive ut $\frac{BD^{quad}}{\frac{1}{2}CF}$ ad $\frac{bd^{quad}}{\frac{1}{2}cf}$. Loquor de spatiis BD, bd minutissimus, inque infinitum diminuendis, sic ut pro $\frac{1}{2}CF, \frac{1}{2}cf$, scribere liceat circularum radios SB, sb , quo facto constabit propositio.

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

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

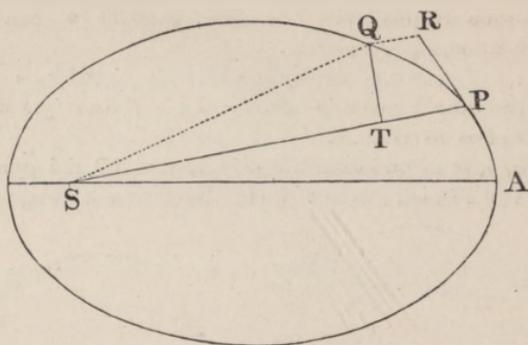
Cor. 3. Unde si quadrata temporum periodicorum sunt ut radii circularum, vires centripetae sunt aequales; et vice versa.

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

Cor. 5. Si quadrata temporum periodicorum sunt ut cubi radiorum, vires centripetae 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 distantiae SP parallela, ac demittatur QT perpendicularis ad distantiam SP ; dico quod vis centripeta sit reciproce ut solidum $\frac{SP^{quad} \times QT^{quad}}{QR}$, si modo solidi illius ea semper sumatur quantitas, ubi coeunt puncta P et Q .

Namque in figura indefinite parva $QRPT$, lineola nascens QR dato tempore est ut vis centripeta, et data vi ut quadratum temporis,

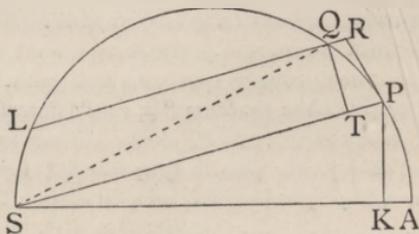


atque adeo neutro dato ut vis centripeta et quadratum temporis conjunctim, id est, ut vis centripeta semel, et area SPQ tempori proportionalis, seu duplum ejus $SP \times QT$, bis. Applicetur hujus proportionalitatis pars utraque ad lineolam QR , et fiet unitas ut vis centripeta, et $\frac{SP^q \times QT^q}{QR}$ conjunctim, hoc est vis centripeta reciproce ut $\frac{SP^q \times QT^q}{QR}$. Q. E. D.

Cor. Hinc si detur figura quaevis, et in ea punctum ad quod vis centripeta dirigitur, invenire potest lex vis centripetae, quae corpus in figurae illius perimetro gyrare faciet. Nimirum computandum est solidum $\frac{SP^q \times QT^q}{QR}$ huic vi reciproce proportionale. Ejus rei dabimus exempla in problematis sequentibus.

Prob. I. Gyrat corpus in circumferentia circuli; requiritur lex vis centripetae tendentis ad punctum aliquod in circumferentia.

Esto circuli circumferentia $SPQA$, centrum vis centripetae S , corpus in circumferentia latum P , locus proximus in quem movebitur Q . Ad



SA diametrum et SP demitte perpendiculara PK , QT ; et per Q ipsi SP parallelam age LR , occurrentem circulo in L , et tangenti PR in R .

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 $\frac{QT^a \times PC^a}{QR} = \frac{2BC^a \times CA^a}{PC}$, et ergo vis centripeta reciproce ut $\frac{2BC^a \times CA^a}{PC}$. Id est, ob datum $2BC^a \times CA^a$, ut $\frac{1}{PC}$. Hoc est directe ut distantia PC . Q. E. I.

Prob. III. Corpus gyrat in ellipsi; requiritur lex vis centripetae tendentis ad umbilicum.

Esto ellipseos superioris umbilicens S , agatur SP secans ellipseos diametrum DK in E , et lineam QU in x , et compleatur parallelogrammum $QxPR$. Patet EP aequalem esse semiaxi majori AC , eo quod, acta ab altero ellipseos umbilico H linea HI , ipsi FC parallela, ob aequales CS , CH aequentur ES , EI , adeo ut EP semisumma sit ipsarum PS , PI , id est (ob parallelas HI , PR et angulos aequales IPR , HPZ) ipsarum PS , PH quae conjunctim totum axem $2AC$ adaequant; ad SP demittatur perpendicularis Qt , et ellipseos latere recto principali (seu $\frac{2BC^a}{AU}$) dicto L , erit

$L \times QR$ ad $L \times PU$ ut QR ad PU ,
 id est, ut PE (seu AC) ad PC .
 Et $L \times PU$ ad $GU \times UP$ ut L ad GU
 Et $GU \times UP$ ad QU^a ut CP^a ad CD^a
 Et QU^a ad Qx^a fiat ut m ad n
 Et Qx^a ad Qt^a ut EP^a ad PF^a ,
 id est, ut CA^a ad PF^a ,
 sive, ut CD^a ad CB^a .
 Et, conjunctis his omnibus rationibus,

$$\frac{L \times QR}{Qt^a} = \frac{AC}{PC} \times \frac{L}{GU} \times \frac{CP^a}{CD^a} \times \frac{m}{n} \times \frac{CD^a}{CB^a}$$
 id est, ut $\frac{AC \times L (\text{seu } 2BC^a)}{PC \times GU} \times \frac{CP^a}{CB^a} \times \frac{m}{n}$
 sive, ut $\frac{2PC}{GU} \times \frac{m}{n}$;
 sed, punctis Q et P coeuntibus, rationes $\frac{2PG}{GU}$ et $\frac{m}{n}$ fiunt aequalitatis,
 ergo $L \times QR$ et Qt^a aequantur. Ducatur pars utraque in $\frac{SP^a}{QR}$, et fiet

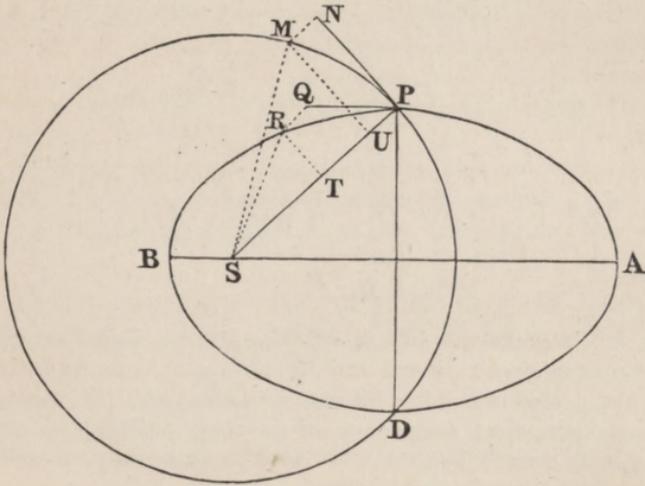
$$L \times SP^a = \frac{SP^a \times Qt^a}{QR}$$

Ergo vis centripeta reciproce est ut $L \times SP^a$, id est, reciproce in ratione duplicata distantiae. Q. E. I.

Scholium. Gyraut ergo planetae majores in ellipsis habentibus umbilicum in centro solis; et radiis ad solem ductis, describunt areas temporibus proportionales, omnino ut supposuit Keplerus. Et harum ellipseon latera recta sunt $\frac{Qt^a}{QR}$; punctis P et Q spatio quam minimo et quasi infinite parvo distantibus.

Theor. IV. Posito quod vis centripeta sit reciproce proportionalis quadrato distantiae a centro, quadrata temporum periodicorum in ellipsis sunt ut cubi transversorum axium.

Sunt ellipseos axis transversus AB , axis alter PD latus rectum L , umbilicus alteruter S . Centro S intervallo SP describatur circulus

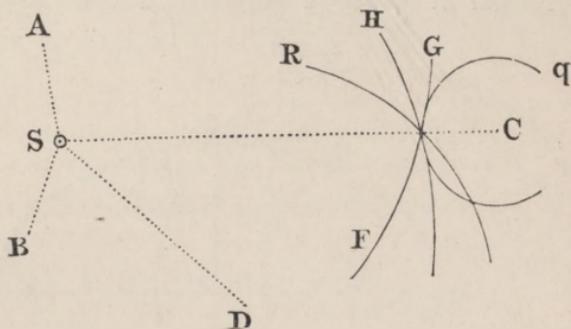


PMD. Et eodem tempore describant duo corpora gyrantia arcum ellipticum PR et circulearem PM , vi centripeta ad umbilicum S tendente. Ellipsin et circulum tangant PQ et PN in puncto P . Ipsi PS parallelae agantur QR , MN tangentibus occurrentes in Q et N . Sint autem figurae PQR , PMN indefinite parvae, sic ut (per Schol. Prob. III.) fiat $L \times QR = RT^a$, et $2SP \times MN = MU^a$; ob communem a centro S distantiam SP , et inde aequales vires centripetas, sunt MN et QR aequales. Ergo RT^a ad MU^a est ut L ad $2SP$, et RT ad MU ut medium proportionale inter $L \times 2SP$ (seu PD) ad $2SP$. Hoc est area SPR ad aream SPM ut area tota ellipseos ad aream totam circuli. Sed partes arearum singulis momentis sunt ut areae SPR et SPM , atque adeo ut areae totae, et proinde per numerum momentorum multiplicatae, simul evadent totis aequales.

Revoluciones igitur eodem tempore in ellipsis perficiuntur ac in

circulis, quorum diametri sunt axibus transversis ellipseon aequales. Sed (per Cor. 5. Theor. II.) quadrata temporum periodicorum in circulis sunt ut cubi diametrorum, ergo et in ellipseibus. Q. E. D.

Hinc in systemate caelesti, ex temporibus periodicis planetarum, innotescunt proportionales transversorum axium orbitalium. Axem unum licebit assumere, inde dabuntur caeteri. Datis autem axibus, determinabuntur orbitae in hunc modum. Sit S locus solis seu umbilicus unus ellipseos, A, B, C, D , loca planetae 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 quocumque 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 plurimae. 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 digressionem factis, habentur orbitalium tangentibus; ad ejusmodi tangentem KL demittatur a sole perpendiculum SL . Centroque L et intervallo dimidii axis ellipseos describatur circulus

(per Schol. Prob. III.) ratio lateris recti ellipseos ad diametrum circuli. Datur igitur latus rectum ellipseos, sit istud L ; datur praeterea 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 perpendicularo SK et erecto semiaxe minore BC , est

$$\begin{aligned} SP^2 - 2KP \times PH + PH^2 &= SH^2 \\ &= 4CH^2 \\ &= 4BH^2 - 4BC^2 \\ &= (SP + PH)^{\text{quad}} - L \times (SP + PH) \\ &= SP^2 + 2SP \times PH + PH^2 - L \times (SP + PH) \end{aligned}$$

Addantur utrobique $2KP \times PH + L \times (SP + PH) - SP^2 - PH^2$, et fiet

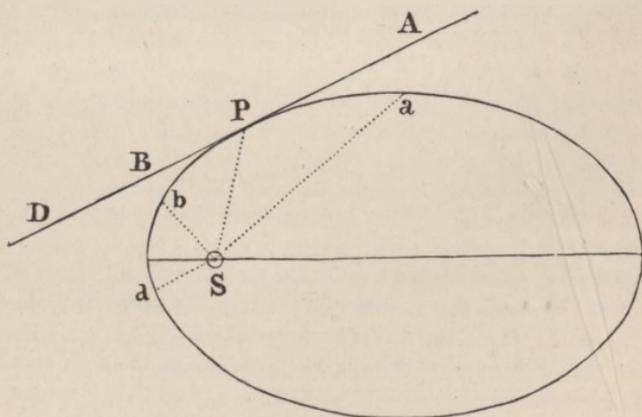
$$L \times (SP + PH) = 2SP \times PH + 2KP \times PH;$$

seu $SP + PH$ ad PH ut $2SP + 2KP$ ad L .

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

Haec 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 aequale $2SP + 2KP$, figura erit parabola umbilicum habens in puncto S , et diametros omnes parallelas lineae 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 aequalem differentiae linearum PS et PH .

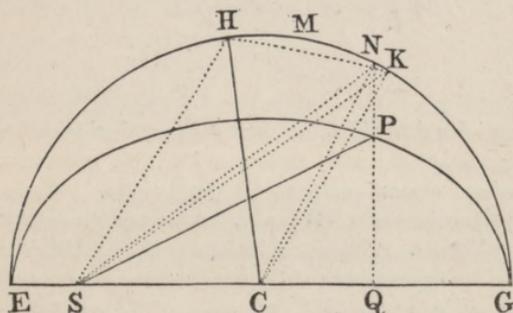
Scholium. Jam vero, beneficio hujus problematis soluti, planetarum orbitas definire concessum est, et inde revolutionum tempora;



et ex orbitarum magnitudine, excentricitate, aphelliis, inclinationibus ad planum eclipticae et nodis inter se collatis, cognocere

an idem cometa ad nos saepius redeat. Nimirum ex quatuor observationibus locorum cometae, juxta hypothesin quod cometa moveatur uniformiter in linea recta, determinanda est ejus via rectilinea. Sit ea $APBD$, sintque A, P, B, D loca cometae 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 $Pbda$. Haec ellipsis determinanda est ut in superiori problemate. In ea sunt a, P, b, d loca cometae temporibus observationum. Cognoscantur horum locorum e terra longitudes et latitudes. Quanto majores vel minores sunt hae longitudes et latitudes observatae, tanto majores vel minores observatis sumantur longitudes et latitudes novae. Ex his novis inveniatu denuo via rectilinea cometae, et inde via rectilinea cometae et inde via elliptica ut prius. 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 haec 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

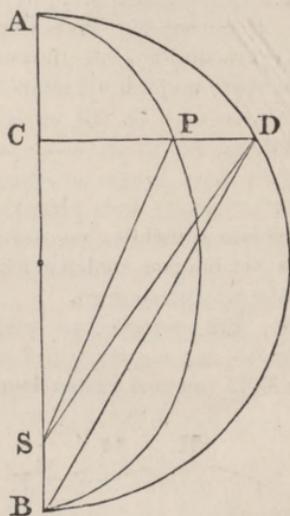


parallela CK circulo occurrens in K ; jungatur HK , et circuli segmento HKM (per tabulam segmentorum vel secus) aequale fiat triangulum SKN ; ad EG demittatur perpendicularum NQ , et in eo cape PQ ad NQ , ut ellipseos axis minor ad majorem, et erit punctum P in ellipsi, atque acta recta PS abscindetur area ellipseos EPS tempori proportionalis. Namque area $HSNM$ triangulo SNK aucta, et huic aequali segmento HKM diminutae, fit triangulo HSK id est triangulo HSC aequale. Haec aequalia adde areae ESH , fiet areae aequales $EHNS$ et EHC ;

cum igitur sector EHC tempori proportionalis sit, et area EPS areae $EHNS$, erit etiam area EPS tempori proportionalis.

Prob. V. Posito quod vis centripeta sit reciproce proportionalis quadrato distantiae a centro, spatia definire quae corpus recta cadendo datis temporibus describit.

Si corpus non cadit perpendiculariter, describit id ellipsin puta APB , cujus umbilicus inferior, puta S , congruet cum centro terrae. Id ex jam demonstratis constat. Super ellipseos axi majore AB

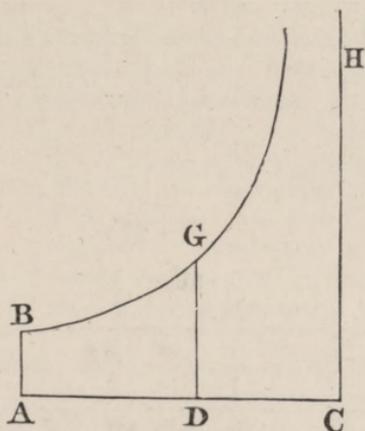


describatur semicirculus ADB , et per corpus decidens transeat recta DPC perpendicularis ad axem, actisque DS , PS , erit area ASD areae ASP atque adeo etiam tempori proportionalis. Manente axi AB minuatur perpetuo latitudo ellipseos, et semper manebit area ASD tempori proportionalis. Minuatur latitudo 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.

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

Prob. VI. Corporis, sola vi insita per medium simile resistens delati, motum definire.

Asymptotis rectangulis ADC , CH describatur hyperbola secans perpendicularia AB , DG , exponatur tum corporis celeritas, tum resistentia medii ipso motus initio per lineam AC , elapso tempore aliquo per



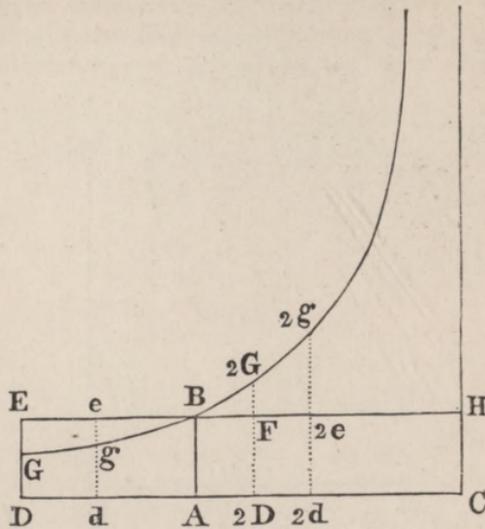
lineam DC , et tempus exponi potest per aream $ABGD$ atque spatium eo tempore descriptum per lineam AD . Nam celeritati proportionalis est resistentia medii, et resistentiae proportionale est decrementum celeritatis; hoc est si tempus dividatur in partes aequales, celeritates ipsarum initiis sunt differentiis suis proportionales. Decrescit ergo celeritas in proportione geometrica dum tempus crescit in arithmetica. Sed tale est decrementum lineae DC et incrementum areae $ABGD$, ut notum est. Ergo tempus per aream et celeritas per lineam illam recte exponitur. Q. E. F.

Porro celeritati atque adeo decremento celeritatis proportionale est incrementum spatii descripti, sed et decremento lineae DC proportionale est incrementum lineae AD . Ergo incrementum spatii per incrementum lineae AD , atque adeo spatium ipsum per lineam illam recte exponitur. Q. E. F.

Prob. VII. Posita uniformi vi centripeta, motum corporis in medio simili recte ascendentis ac descendentis definire.

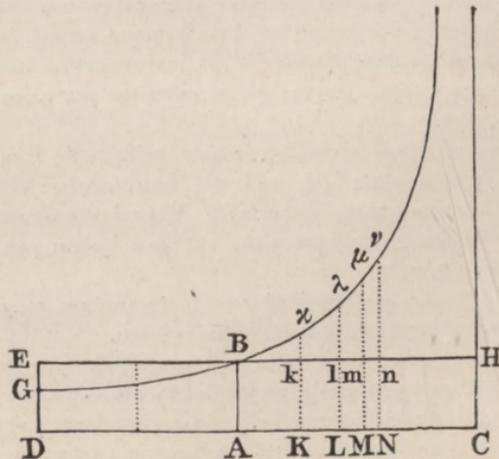
Corpore ascendente, exponatur vis centripeta per datum quodvis rectangulum BC et resistentia medii initio ascensus per rectangulum BD sumptum ad contrarias partes. Asymptotis rectangulis AC , CH , per punctum B describatur hyperbola, secans perpendicularia DE , de in G , g , et corpus ascendendo tempore $DGgd$ describet spatium $EGge$, tempore $DGBA$ spatium ascensus totius EGB , tempore $AB2G2D$

spatium descensus $BF2G$ atque tempore $2D2G2g2d$ spatium descensus $2GF2e2g$, et celeritas corporis resistentiae 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. quae sint ut incrementa celeritatum aequalibus totidem



temporibus facta, et erunt Ak , Al , Am , An , &c. ut celeritates totae, et adeo ut resistentiae medii in fine singulorum temporum aequalium.

Fiat AC ad AK , vel $ABHC$ ad $AbkK$, ut vis centripeta ad resistantiam in fine temporis primi, et erunt $ABHC$, $KkHC$, $LlHC$, $NnHC$, &c. ut vires absolutae, quibus corpus urgetur, atque adeo ut incrementa celeritatum, id est ut rectangula Ak , Kl , Lm , Mn , &c. et proinde in progressionem geometricam. Quare si rectae Kk , Ll , Mm , Nn , productae occurrant hyperbolae in κ , λ , μ , ν , &c. erunt areae $ABK\kappa$, $K\kappa\lambda L$, $L\lambda\mu M$, $M\mu\nu N$ aequales, adeoque tum temporibus aequalibus tum viribus centripetis semper aequalibus analogae. Subducantur rectangula Ak , Kl , Lm , Mn , &c. viribus absolutis analogae, et relinquuntur areae $Bk\kappa$, $k\kappa\lambda l$, $l\lambda\mu m$, $m\mu\nu n$, &c. resistantiis medii in fine singulorum temporum, hoc est celeritatibus atque adeo descriptis spatiis analogae. Sumantur analogarum summae; et erunt areae $Bk\kappa$, $Bl\lambda$, $Bm\mu$, $Bn\nu$, &c. spatiis totis descriptis analogae, nec non areae $AB\kappa K$, $AB\lambda L$, $AB\mu M$, $AB\nu N$, &c. temporibus. Corpus igitur inter descendendum, tempore quovis $AB\lambda L$, describit spatium $Bl\lambda$, et tempore $L\lambda\nu n$ spatium $\lambda\nu\nu$. Q. E. D. Et similis est demonstratio motus expositi in ascensu. Q. E. D.

Scholium. Beneficio duorum novissimorum problematum innotescit motus projectilium in aere nostro, ex hypothesi quod aer iste similis 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 resistantia 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 rectae DC punctum quodvis R erecto perpendiculo RT , quod occurrat hyperbolae in T , et rectae 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 areae $DFsE$, $DfSBG$ aequantur, et postea semper appropinquans asymptoto PCL . Estque celeritas ejus in puncto quovis r ut tangens rL .

Si proportio resistantiae aeris ad vim gravitatis nondum innotescit,

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 determinet 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.

I mentioned above that there are, among the Portsmouth papers, three manuscript drafts of the tract, all of which I believe to be in Newton's handwriting. That which I take to be the earliest is headed *De motu corporum in gyrum*. The draft of the definitions and hypotheses is rough and is cancelled. There are no lemmas, although marginal references to them have been inserted in the text. The demonstrations of the propositions are almost identical with those printed above; but there is a scholium at the end of theorem 2 in the form given below on p. 54. The manuscript ends abruptly in the middle of the scholium to problem 7.

The next manuscript has no heading, and is, I should suppose, just anterior in date to the copy sent to the Royal Society. It commences with the definitions and hypotheses as in that copy. These are followed by a table of contents; after which come the two lemmas, which are, however, here numbered 3 and 4. Then comes the text, similar to that above printed, but the manuscript after the middle of the scholium to problem 5 is missing. There are no marginal references.

The third copy is more interesting because it is somewhat fuller and is complete.

The heading, which perhaps is of a later date than the manuscript, and possibly not in Newton's handwriting, is *De motu sphaericorum Corporum in fluidis*.

The manuscript commences with four definitions as printed below—of which the first three are substantially the same as those given in the Royal Society's copy.

Def. 1. Vim centripetam appello qua corpus attrahitur vel impellitur versus punctum aliquod quod ut centrum spectatur.

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

Def. 3. Et resistentiam quae est medii regulariter impediētis.

Def. 4. Exponentes quantitatum sunt aliae quaevis quantitates proportionales expositis.

Next come five laws, the term *hypothesis* here and in the subsequent references in the manuscript being altered to *lex*. These are as follows :

Lex 1. Sola vi insita corpus uniformiter in linea recta semper pergere si nil impediat.

Lex 2. Mutationem status movendi vel quiescendi proportionalem esse vi impressae et fieri secundum lineam rectam qua vis illa imprimitur.

Lex 3. Corporum dato spatio inclusorum eosdem esse motus inter se sive spatium illud quiescat sive moveat id perpetuo et uniformiter in directum absque motu circulari.

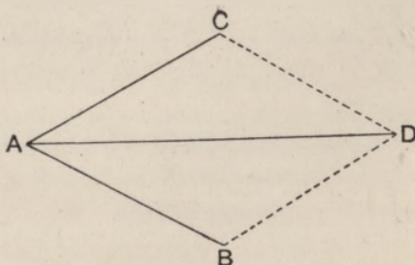
Lex 4. Mutuis corporum actionibus commune centrum gravitatis non mutare statum suum motus vel quietis. Constat ex Lege 3.

Lex 5. Resistentiam medii esse ut medii illius densitas et corporis moti sphaerica superficies et velocitas conjunctim.

These are succeeded by four lemmas as printed below—of which the third and fourth are the same as the first and second in the Royal Society's copy.

Lemma 1. Corpus viribus conjunctis diagonalem parallelogrammi eodem tempore describere quo latera separatim.

Si corpus dato tempore vi sola m ferretur ab A ad B , et vi sola n ab A ad C , compleatur parallelogrammum $ABDC$, et vi utraque

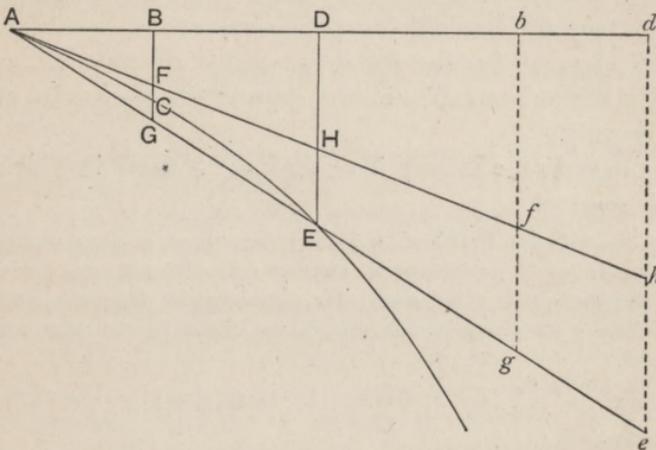


feretur id eodem tempore ab A ad D . Nam quoniam vis m agit secundum lineam AC ipsi BD parallelam, haec vis, per Legem 2, nihil

mutabit celeritatem accedendi ad lineam illam BD vi altera impressam. Accedet igitur corpus eodem tempore ad lineam BD sive vis AC imprimatur sive non, atque adeo in fine illius temporis reperitur alicubi in linea illa BD . Eodem argumento in fine temporis ejusdem reperietur alicubi in linea CD , et proinde in utriusque lineae concursu D reperiri necesse est.

Lemma 2. Spatium quod corpus urgente quacunque vi centripeta ipso motus initio describit esse in duplicata ratione temporis.

Exponentur tempora per lineas AB , AD . Datis Ab , Ad proportionales, et urgente vi centripeta aequabili exponentur spatia descripta



per areas rectilineas ABF , ADH perpendicularis BF , DH , et recta quavis AFH terminatas, ut exposuit Galilaeus. Urgente autem vi centripeta inaequabili exponentur spatia descripta per areas ABC , ADE , curva quavis ACE quam recta AFH tangit in A , comprehensas. Age rectam AE , parallelis BF , bf , dh occurrentem in G , g , e , et ipsis bf , dh occurrat AFH producta in f et h . Quoniam area ABC major est area ABF [et] minor area ABG , et area curvilinea $ADEC$ [est] major area ADH [et] minor area $ADEG$, erit area ABC ad area $ADEC$ major quam area ABF ad aream $ADEG$, [et] minor quam area ABG ad aream ADH , hoc est major quam area Abf ad aream Ade , [et] minor quam area Abg ad aream Adh . Diminuantur jam lineae AB , AD in ratione sua data usque dum puncta A , B , D coeunt, et linea Ae conveniet cum tangente Ah , adeoque ultime rationes Abf ad Ade et Abg ad Adh evadent eadem cum ratione Abf ad Adh . Sed haec ratio est dupla rationis Ab ad Ad , seu AB ad AD . Ergo ratio ABC ad $ADEC$ ultimis illis intermedia jam fit

dupla rationis AB ad AD , id est ratio ultima evanescentium spatiorum seu prima nascentium dupla est rationis temporum.

Lemma 3. Quantitates differentiis suis proportionales sunt continue proportionales.

Ponatur A ad $A-B$ ut B ad $B-C$, et C ad $C-D$, &c. et dividendo fiet A ad B ut B ad C , et C ad D , &c.

Lemma 4. Parallelogramma omnia circa datam ellipsin descripta esse inter se aequalia.

Constat ex Conicis.

Next come eleven propositions. The first nine of them are headed *De motu corporum in mediis non resistantibus*. The last two are headed *De motu corporum in mediis resistantibus*. Except for the following additions, the text is substantially the same as that in the copy in the possession of the Royal Society.

At the end of corollary 5 to theorem 2 the following paragraph is inserted :

Scholium. Casus Corollarii quinti obtinet in corporibus coelestibus. Quadrata temporum periodicorum sunt ut cubi distantiarum a communi centre circum quod volvuntur. Id obtinere in Planetis majoribus circa Solem gyranibus inque minoribus circa Jovem jam statuunt Astronomi.

At the end of the scholium to theorem 4 is the following paragraph :

Caeterum totum coeli Planetarii Spatium vel quiescit (ut vulgo creditur) vel uniformiter movetur in directum et perinde Planetarum commune centrum gravitatis (per Legem 4) vel quiescit vel una movetur. Utrouque in casu motus Planetarum inter se (per Legem 3) eodem modo se habent, et eorum commune centrum gravitatis respectu spatii totius quiescit, atque adeo pro centro immobili Systematis totius Planetarii haberi debet. Inde vero systema Copernicæum probatur a priori. Nam si in quovis Planetarum situ computetur commune centrum gravitatis hoc vel incidet in corpus Solis vel ei semper proximum erit. Eo Solis a centro gravitatis errore fit ut vis centripeta non semper tendat ad centrum illud immobile, et inde ut planetae nec moveantur in Ellipsis exacte neque bis revolvant in eadem orbita. Tot sunt orbitae Planetæ cujusque quot revolutiones, ut fit in motu Lunæ et pendet orbita unaquæque ab omnium Planetarum motibus conjunctis, ut taceam eorum omnium actiones in se invicem. Tot autem motuum causas simul considerare et legibus exactis calculum

commodum admittentibus motus ipsos definire superat ni fallor vim omnium humani ingenii. Omitte minutias illas et orbita simplex et inter omnes errores mediocris erit Ellipsis de qua jam egi. Si quis hanc Ellipsin ex tribus observationibus per computum trigonometricum (ut solet) determinare tentaverit, hic minus caute rem aggressus fuerit. Participabunt observationes illae de minutis motuum irregularium hic negligendis adeoque Ellipsin de justa sua magnitudine et positione (quae inter omnes errores mediocris esse debet) aliquantulum deflectere facient, atque tot dabunt Ellipses ab invicem discrepantes quot adhibentur observationes trinae. Coniungendae sunt igitur et una operatione inter se conferendae observationes quam plurimae, quae se mutuo contemperent et Ellipsin positione et magnitudine mediocrem exhibeant.

At the end of problem 5 and before problem 6 is the following scholium :

Scholium. Hactenus motum corporum in mediis non resistentibus exposui ; id adeo ut motus corporum coelestium in aethere determinarem. Aetheris enim puri resistentia quantum sentio vel nulla est vel perquam exigua. Valide resistit argentum vivum, longe minus aqua, aer vero longe adhuc minus. Pro densitate sua quae ponderi fere proportionalis est atque adeo (poene dixerim) pro quantitate materiae suae crassae resistunt haec media. Minuatur igitur aeris materia crassa et in eadem circiter proportione minuetur medii resistentia usque dum ad aetheris tenuitatem perventum sit. Celeri cursu equitantes vehementer aeris resistentiam sentiunt, at navigantes exclusis e mari interiore ventis nihil omnino ex aethere praeter fluente patiuntur. Si aer libere interflueret particulas corporum et sic ageret, non modo in externam totius superficiem, sed etiam in superficies singularum partium, longe major foret ejus resistentia. Interfluit aether liberrime nec tamen resistit sensibiliter. Cometas infra orbitam Saturni descendere jam sentiunt Astronomi saniores quotquot distantias eorum ex orbis magni parallaxi praeterpropter colligere norunt ; hi igitur celeritate immensa in omnes coeli nostri partes indifferenter feruntur, nec tamen vel crinem seu vaporem capiti circumdatum resistentia aetheris impeditum et abreptum amittunt. Planetae vero jam per annos milenos in motu suo perseverarunt, tantum abest ut impedimentum sentiant.

Demonstratis igitur legibus reguntur motus in coelis. Sed in aere nostro, si resistentia ejus non consideratur, innotescunt motus projectilium per Prob. 4 et motus gravium perpendiculariter cadentium per Prob. 5 posito nimirum quod gravitas sit reciproce proportionalis quadrato distantiae a centro terrae. Nam virium centripetarum

species una est gravitas; et computanti mihi prodiit vis centripeta qua luna nostra detinetur in motu suo menstruo circa terram ad vim gravitatis [his] in superficie terrae reciproce ut quadrata distantiarum a centro terrae quamproxime. Ex horologii oscillatorii motu tardiore in cacumine montis praealti quam in valle liquet etiam gravitatem ex aucta nostra a terrae centro distantia diminui, sed qua proportione nondum observatum est.

Caeterum projectilium motus in aere nostro referendi sunt ad immensum et revera immobile coelorum spatium, non ad spatium mobile quod una cum terra et aere nostro convolvitur et a rusticis ut immobile spectatur. Invenienda est Ellipsis quam projectile describit in spatio illo vere immobili et inde motus ejus in spatio mobili determinandus. Hoc pacto colligitur grave, quod de aedificii sublimis vertice demittitur, inter cadendum deflectere aliquantulum a perpendiculari, ut et quanta sit illa deflexio et quam in partem. Et vicissim ex deflexione experimentis comprobata colligitur motus terrae. Cum ipse olim hanc deflexionem Clarissimo Hookio significarem, is experimento ter facto rem ita se habere confirmavit, deflectente semper gravi a perpendiculari versus orientem et austrum ut in latitudine nostra boreali oportuit.

CHAPTER V.

PREPARATION OF THE PRINCIPIA, 1685-1687.

It seems probable that Halley, on his visit to Cambridge about the end of November, 1684, not only induced Newton to publish the results he had then established, but urged him to proceed with his investigations and put them together in a connected form, though most likely it was only later that it was decided to print them as a book and not as memoirs in the *Transactions* of the Royal Society. In the preface to the *Principia* Newton says that it was to Halley's solicitations that its publication was due, "quippe cum demonstratum a me "figuram Orbium coelestium impetraverat, rogare non destitit "ut eadem cum *Societate Regali* communicarem, Quae deinde "hortatibus et benignis suis auspiciis effectit ut de eadem in "lucem emittenda cogitare inciperem." And Pemberton says that it was Halley's discourse that engaged "Newton to "resume again the consideration of this subject; and gave "occasion to his writing the treatise."

As soon as the university term was over Newton threw himself into this work, but he seems to have anticipated that it would take only a short time to put his conclusions together—probably intending to write little more than an amplification of his tract *De Motu*—and evidently he had no idea of the magnitude of the task on which he was entering. Thus already in February, 1685*, he speaks of his investigations

* Newton's letter to Aston of Feb. 23, 1685, see above, p. 32.

occupying more time than he "expected, and a great deal of it "to no purpose." This is also suggested by the preface to the first edition of the *Principia*, where he says that after he had begun to consider certain questions he deferred publication till he had investigated them further.

As a matter of fact, however, the work was finished in less than two years. "I wrote it," says he*, "in seventeen or "eighteen months, beginning in the end of December, 1684, "and sending it to the Royal Society in May, 1686, excepting "that about ten or twelve of the propositions were composed "before; viz. the 1st and 11th in December, 1679, the 6th, "7th, 8th, 9th, 10th, 12th, 13th, and 17th, Lib. i. and the "1st, 2nd, 3rd, and 4th, Lib. ii. in [June and July] 1684." There can be no doubt that the statement about his work in June and July, 1684, is incorrect, since we cannot suppose that these propositions were established till after Halley's visit in August. I think we may with more probability assign the composition of these propositions to August and September, 1684. Moreover, the results of props. 18, 19, 21, 30, 32, 33, 34, 35 were given in his lectures in the Michaelmas Term, 1684, which ended on Dec. 10; and, as it is unlikely that he would have discovered the truth of these without also proving props. 36, 37, 38, 39, we must suppose that by the beginning of December, 1684, he was acquainted with most of the results given in sections i., ii., iii., vi., and vii. of book i. I should also suppose that it is not unlikely that he was already familiar with a good deal of the pure geometry which for convenience he incorporated in the *Principia*. Subject to this we shall see that the first two books were really written in about six months, and the period of eighteen months which the whole composition is said to have occupied includes the time in which copies of them for the press were prepared, and much of the material for the third book collected.

* Newton's memorandum quoted by Brewster, vol. i. p. 426; see also below, p. 60.

That the composition occupied a year and a half is repeated by Newton* in the following note: "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 shortness of the "time, in which I wrote it, makes me not ashamed of having "committed some faults."

Of the breaks which altogether are said to have occupied about two months, one occurred in March, 1685, and probably the only other one in June or July of the same year†. It should be remarked, however, that although during the year 1685 Newton worked steadily at the composition of the *Principia*, he did not devote the whole of his attention to it, and his note-books show that his chemical researches were carried on simultaneously, and occupied in all a considerable time during this year.

As might be expected, he chose the subject-matter of the *Principia* for his lectures in the Michaelmas Term, 1685. These lectures are ten in number, and are written in the same manuscript as those for 1684. The first lecture is on what is printed in the *Principia* as props. 22-25 (inclusive), and lemma 22; the second is on props. 26-28, and lemmas 23-26; the third is

* In the Macclesfield Collection, quoted by Rigaud, p. 92. The fact was well known, and is mentioned by Pemberton, see above, p. 11.

† The time of the first break is indicated by Newton in his letter to Aston (see above, p. 32). A record of the residence of fellows of the College was kept, as they were entitled during residence to 3s. 4d. a week "pro pane et potu," namely, 10 loaves of bread at 1d. each, 10 quarts of small beer at 1d. a quart, and 10 quarts of strong beer at 2d. a quart. The only period during the years 1685 and 1686 in which Newton is entered in this record as having been absent from College is from March 27 to April 11, 1685, and from June 11 to June 20, 1685, but as one day's residence may have covered some days on each side of it, this may indicate an absence in all of some six or seven weeks. The College Buttery books would be a more conclusive test as to his continuous residence, but the volume for the period immediately previous to Oct. 9, 1686, is missing. See the *Cotes Correspondence*, pp. lxxxiv-lxxxvi.

on props. 29, 30, and lemmas 27, 28; the fourth is on prop. 31; the fifth is on the seventh section, namely, props. 32-39; the sixth is on the eighth section, props. 40-42; the seventh is on props. 43, 44; the eighth is on prop. 45; the ninth is on props. 46-51; and the tenth lecture is on props. 52-54.

His lectures in 1686 are lost, but it would seem most likely that they were on the results given in the fifty-fifth and following propositions of book i. Those in 1687 are a popular exposition of the results of the third book.

The considerable mechanical labour involved in writing out the manuscript of the *Principia* for the press was lightened by the employment of an amanuensis, named Humphrey Newton but apparently no relation of Sir Isaac's. Humphrey Newton commenced his work "in the last year of King Charles II.," and as the King died on Feb. 2, 1685, we may reasonably suppose that Sir Isaac first employed his amanuensis when he began the composition of the *Principia**. Humphrey Newton left Cambridge in 1689, thus serving Sir Isaac, says he, "for about five years. In such time he wrote his *Principia Mathematica*, which stupendous work, by his order, I copied out before it went to the press." The same fact is mentioned by Newton in the memorandum from which an extract is quoted above on p. 58, where, after noting a mistake in book ii. prop. 10 of the first edition, he says: "there may have been some other mistakes occasioned by the shortness of the time in which the book was written, and by its being copied by an amanuensis, who understood not what he copied, besides the press faults."

It would serve no useful purpose to go here into minute details relating to the preparation of the *Principia*. Early drafts of the commencement and other parts of it are in the Portsmouth Collection. In a few cases where these notes are fuller or differ materially from the corresponding propositions

* Brewster, vol. ii. p. 50. Brewster's remark that H. Newton was engaged from 1683 to 1689 must be a slip.

as printed in the first edition, they might be added to a critical edition of the work, but their production here would only overload this essay.

It may be interesting, however, to note the dates at which the various parts of the work were completed. Newton's lectures in the Michaelmas Term of 1684, and his tract *De Motu*, contain the substance of sections ii and iii of book i; it was not until 1685*—probably early in the spring—that he determined the attraction of a spherical body on any external point. “No sooner,” to quote from Dr. Glaisher's address on the bicentenary of the publication of the *Principia*, “had Newton proved this superb theorem—and we know from his own words that he had no expectation of so beautiful a result till it emerged from his mathematical investigation—than all the mechanism of the universe at once lay spread before him. When he discovered the theorems that form the first three sections of book i., when he gave them in his lectures of 1684, he was unaware that the sun and earth exerted their attractions as if they were but points. How different must these propositions have seemed to Newton's eyes when he realised that these results, which he had believed to be only approximately true when applied to the solar system, were really exact! Hitherto they had been true only in so far as he could regard the sun as a point compared to the distance of the planets or the earth as a point compared to the distance of the moon—a distance amounting to only about sixty times the earth's radius—but now they were mathematically true, excepting only for the slight deviation from a perfectly spherical form of the sun, earth, and planets. We can imagine the effect of this sudden transition from approximation to exactitude in stimulating Newton's mind to still greater efforts. It was now in his power to apply mathematical analysis with absolute precision to the actual problems of astronomy.”

* Newton's letter of June 20, 1686, printed below, p. 157.

It is most likely that the manuscript of the first book was finished by about Easter, 1685, though certain corrections and additions were made* in the following winter.

At the meeting of the Royal Society on April 21, 1686, Halley read a discourse concerning gravity wherein† it is stated that his "worthy countryman Mr. Isaac Newton has an "incomparable *Treatise of Motion* almost ready for the press," and that the law of inverse squares "is the principle on which "Mr. Newton has made out all the phenomena of the celestial "motions so easily and naturally that its truth is past "dispute."

At the meeting‡ of the Society on April 28, 1686, "Dr. "Vincent presented to the Society a manuscript treatise intitled "*Philosophiæ Naturalis principia mathematica*, and dedicated "to the Society by Mr. Isaac Newton, wherein he gives a "mathematical demonstration of the Copernican hypothesis as "proposed by Kepler, and makes out all the phaenomena of the "celestial motions by the only supposition of a gravitation "towards the center of the sun decreasing as the squares of the "distances therefrom reciprocally. It was ordered that a letter "of thanks be written to Mr. Newton; and that the printing "of his book be referred to the consideration of the council; "and that in the mean time the book be put into the hands of "Mr. Halley, to make a report thereof to the council."

The preface was added later, and in the first edition bears no date, but in the second edition the date May 8, 1686, was inserted.

At the meeting§ of the Royal Society on May 19, 1686, it was ordered "that Mr. Newton's *Philosophiæ naturalis* "*principia mathematica* be printed forthwith in quarto in a fair "letter; and that a letter be written to him to signify the "Society's resolution, and to desire his opinion as to the print,

* Newton's letter of June 20, 1686, printed below, p. 153.

† *Philosophical Transactions*, No. 179, 1686, pp. 6-8.

‡ Birch, *History of Royal Society*, London, 1757, vol. iv. pp. 479-480.

§ *Ibid.*, p. 484.

“volume, cuts, &c.” To this was appended the following note: “Mr. Halley wrote accordingly to Mr. Newton the 22d of May the following letter,” and then follows the letter printed below on pp. 154-155.

At the meeting* of the Council of the Society on June 2, 1686, “it was ordered, that Mr. Newton’s book be printed, and “that Mr. Halley undertake the business of looking after it, “and printing it at his own charge; which he engaged to do.”

The extant correspondence of Newton and Halley in connection with the printing of the work is printed below in chapter viii.

The manuscript of the first book went to press† before June 7, 1686; and in the records of the meeting‡ of the Royal Society on June 30, 1686, there is a minute, “Ordered . . . “that the president be desired to license Mr. Newton’s book “intituled *Philosophiæ naturalis Principia mathematica*, and “dedicated to the Society.” The consequent imprimatur of Pepys, the then president, is dated July 5.

This first book is given up to the consideration of the motion of particles or bodies in free space either in known orbits, or under the action of known forces, or under their mutual attraction. In it Newton generalised the law of attraction into a statement that every particle of matter in the universe attracts every other particle with a force which varies directly as the product of their masses and inversely as the square of the distance between them; and he thence deduced the law of attraction for spherical shells of constant density.

The rough manuscript of the second book was finished in the summer of 1685, but was not written out as a clean copy§ till after June 20, 1686; whether it was formally communicated to the Royal Society is doubtful, and I am not aware of any minute on the subject; at any rate the Society seem to

* Birch, *History of Royal Society*, London, 1757, vol. iv. p. 486.

† Halley’s letter of June 7, 1686, printed below, p. 156.

‡ Birch, vol. iv. p. 491.

§ Newton’s letter of June 20, 1686, printed below, p. 158.

have had no knowledge of its contents, for at a meeting* on Jan. 26, 1687, when a communication from Dr. Wallis was read concerning motion in a resisting medium, "it was ordered "that Mr. Newton be consulted whether he designs to treat of "the opposition of the medium to bodies moving in it in the "treatise *de Motu Corporum* then in the press." Newton replied on Feb. 18, 1687, that he had added considerably to his paper of 1684, and that the new results were inserted in his second book, which was still in his possession, though it had been finished† for the press in the autumn of 1686. In consequence of the slow progress in setting up the type of the first book, the printers were not ready for the manuscript of the second book; and finally, it was not until March, 1687‡, that a commencement of printing it was made—a different printer to the one to whom the first book had been entrusted being then employed§, so as to expedite the publication of the whole work.

This second book treats of motion in a resisting medium, and of hydrostatics and hydrodynamics, with special applications to waves, tides, and acoustics. Newton concluded it by showing that the Cartesian theory of vortices is inconsistent with known facts and with the laws of motion.

In his original scheme Newton intended|| to confine the third book of the *Principia* to a bare sketch of the applications to the solar system of the results proved in the first book. The work entitled *De Systemate Mundi*, issued posthumously in 1731 (but of which a translation had been published in 1728),

* Birch, vol. iv. p. 521. The manuscript of the first book was probably in the continuous possession of the Society (except for the sheets temporarily sent to the printers) from the date when it was formally presented to them, for it was referred to in connection with a point that arose from a communication by Wallis at the meeting on March 9, 1687 (*Ibid.*, p. 528).

† Newton's letter of Feb. 18, 1687, printed below, p. 169.

‡ Newton's letter of March 1, 1687, printed below, p. 171.

§ Halley's letter of Feb. 24, 1687, printed below, p. 170.

|| Preface to book iii, see below, p. 106.

answers this description, and it may be that it is a copy of this sketch. This opinion is strengthened by the fact that Newton's professorial lectures given at the commencement of the Michaelmas Term, 1687—which have the same heading as the first and second books of the *Principia*, namely, *De Motu Corporum*, of which they may have been intended to be the continuation—agree with the opening pages of the *De Systemate Mundi* (see above, p. 28). If this be the correct view, these lectures are to be regarded as the germ of the third book, and for that reason are referred to here. It may be, however, that they were written merely as lectures and embodying some of the results given in the third book; if so, they were probably written in 1687, after the completion of the third book.

The preparation of the third book required a knowledge of numerous astronomical data with most of which Newton must have been previously unacquainted. It was fortunate that he was able to consult so skilful an astronomer as Flamsteed, from whom he obtained much of the necessary information*.

* Of the letters that passed between Newton and Flamsteed during the years 1684-1686, eleven are extant, namely, Flamsteed to Newton, Dec. 27, 1684; Jan. 5, 1685; and Jan. 27, 1685; Newton to Flamsteed, Sept. 19, 1685; Flamsteed to Newton, Sept. 26, 1685, and Oct. 10, 1685; Newton to Flamsteed, Oct. 14, 1685; Dec. 30, 1685; Jan. 13, 1686; and Sept. 3, 1686; Flamsteed to Newton, Sept. 9, 1686: of these the six from Flamsteed are in the *Portsmouth Collection*, section vi. division iv; the five from Newton, and Flamsteed's draft of the letter of Sept. 26, 1685 (which differs considerably from the letter sent) are printed in the *General Dictionary*, London, 1738, vol. vii. pp. 793-797. Of previous communications concerning the comet of 1680, extracts from (or copies of) five letters are extant, namely, extract of letter from Flamsteed, Feb. 12, 1681; Newton to Flamsteed through Crompton, Feb. 28, 1681; Flamsteed to Crompton (for Newton), March 7, 1681; Newton to Flamsteed (and Newton's first draft of April 12, and his memoranda when writing it), April 16, 1681; draft of Newton to Flamsteed, Dec. 29, 1681; see *Portsmouth Collection*, section vi. division iv; *General Dictionary*, vol. vii. pp. 788-793; Browster, vol. i. pp. 262-264; vol. ii. pp. 59-62. The originals of the letters received by Flamsteed are in the library of Corpus Christi College, Oxford.

Moreover, Halley—to whom the inception of the work was due, and who, as I shall show directly, bore the cost of producing it—assisted* him in collecting the requisite information. The rough manuscript of this book (except for the parts dealing with cometary motion) was finished† by June, 1686, and Rigaud‡ seems to have thought that the principal propositions on cometary motion were also finished by that time, though they were not written out in the manuscript.

I have already alluded to the dilatory manner in which the printers set up the first book, and have stated that after an unnecessary delay of six months the second book was given to another firm. The manuscript of the third book was ready for press in March, 1687, and it was at first proposed to hand it to a third printer§; but, the printers of the first book having promised to work off the manuscript rapidly, it was finally sent to them|| on April 5, 1687; and at the meeting¶ of the Royal Society on April 6, 1687, it was formally presented to the Society. “After the minutes of the last meeting were read, “the third book of Mr. Newton’s treatise *De Systemate Mundi* “was produced and presented to the Society. It contained the “whole system of celestial motions, as well of the secondary “as primary planets, with the theory of comets; which he “illustrates by the example of the great comet of 1680^o/_I, “proving that, which appeared in the morning in the month of “Nov. preceding, to have been the same comet, that was “observed in Dec. and Jan. in the evening.”

In this third book the theorems proved in the first book are applied to the chief phenomena of the solar system, the masses and distances of the planets and (whenever sufficient data existed) of their satellites are determined. In particular the

* Probably so, see Rigaud, p. 71, and preface to *Principia*.

† Newton’s letter of June 20, 1686, printed below, p. 158.

‡ Rigaud, p. 78.

§ Halley’s letter of March 7, 1687, printed below, p. 171.

|| Halley’s letter of April 5, 1687, printed below, p. 173.

¶ Birch, vol. iv. pp. 529-530.

motion of the moon, the various inequalities therein, and the theory of the tides are worked out in detail. Newton also investigated the theory of comets, showed that they belong to the solar system, explained how from three observations the orbit could be determined, and illustrated his results by considering certain special comets.

The *Principia* containing all three books was issued* from the press in July, 1687; and with it was bound a singularly graceful set of verses by Halley addressed to Newton.

From the minutes of the Royal Society of May 19, 1686 (see above, p. 62), it will be seen that the Society, immediately after the presentation of the first book in April, 1686, directed that it should be printed and published at their expense, but the minute of the Council of June 2, 1686, shows that in fact the whole cost was borne by Halley. The truth seems to be that the Society had not sufficient funds in hand to enable the Council to carry out the order of May 19, and they were relieved from the embarrassment by the generosity of Halley†. The burden that he thus undertook was heavier than appears on the face of the documents. He had been brought up as a son of a rich man, and had no profession; he had married in 1682, and by this time had a wife and young children dependent on him; but on the death of his father in the first half of 1684 he found himself unexpectedly reduced to comparative poverty, and on Jan. 27, 1686, he was glad to accept the position of clerk and assistant to the secretaries of the Royal Society at a stipend‡ of £50 a year—a post he continued to occupy till 1698. Moreover, the liability for which he thus made himself responsible was considerable; for the edition was a small one—perhaps comprising not more than 250 copies—he gave away 24 copies, and the work was sold in sheets at the wholesale price of 5 shillings cash, or retail 9 shillings a

* Halley's letter of July 5, 1687, printed below, p. 173.

† Rigaud, pp. 33-37.

‡ Birch, *History of the Royal Society*, London, 1757, vol. iv. p. 450.

copy bound in calf, so that in any case he must have been considerably out of pocket by it.

Halley did not confine his generosity to paying the bill for printing the work ; he took the liveliest interest in its progress, he criticised the parts which he failed to understand, he collected materials for the third book, and he bore the chief burden of revising the sheets for the press. Finally, in 1687, he wrote* a review which was intended to call general attention to the work, and which contains an exposition of the chief discoveries in it and the methods by which they are proved. As far as I know, the only other contemporary review of the work was one which appeared in the *Acta Eruditorum* for June, 1688 [pp. 303-315]; but it is, and purports to be, little more than a synopsis of the contents.

When Halley wrote the letter of May 22, 1686, acquainting Newton with the decision of the Royal Society to publish the *Principia*, he unfortunately found it necessary also to describe Hooke's conduct at the meeting at which the manuscript was presented to the Society. It would seem † that Dr. Vincent, in presenting the work to the Society, had passed a high encomium on it both as to the novelty and dignity of the subject; on which one member remarked that Newton had carried the thing so far that there was no more to be added; and Sir John Hoskyns, the particular friend of Hooke, who was in the chair, added that it was so much the more to be prized that it was invented and perfected at the same time. This gave offence to Hooke, who asserted that he had previously informed Sir John of his own investigations. After the meeting broke up some of the members adjourned to a coffee-house, and there Hooke endeavoured to gain belief that

* *Philosophical Transactions*, vol. xvi. No. 186, p. 291 *et seq.* This number should have been issued at the end of March, 1687, but it is prefaced by an advertisement which implies that the publication was some months later; see Rigaud, pp. 83-85.

† Halley's letter of June 29, 1686, printed below, p. 163.

he had some such thing by him, and that he had given Newton the first hint of the invention, but the members do not seem to have given Hooke much credit or sympathy; and it may be stated that neither in the opinion of his contemporaries nor in that of posterity was Hooke's claim justified except in the very limited sense indicated below. Newton at once—on May 27, and again on June 20—warmly repudiated what he regarded as an attack on his honesty.

The greater part of the correspondence of 1679 on which Hooke based his claim is printed below (see chapter viii.), as also are Newton's letters of 1686 to Halley on the subject; to these is added Hooke's statement of his case, so that the reader can judge for himself of the justice of the assertion.

These documents prove (what was never denied) that Hooke, like other physicists of the time, had pondered on how the celestial motions were produced; that he suggested gravitation as influencing these motions, and perhaps conjectured that it was the sole cause of them; that he believed that gravitation was proportional to the inverse square of the distance, and possibly that it would be found that an ellipse could be described under such a force directed to one of its foci. This was highly creditable, and compares favourably with the contemporary investigations of Huygens, Wren, and Halley; but it is a long step from a surmise, however ingenious, to a rigorous demonstration. Such examples, says Clairaut, quoted by Rigaud*, "servent à faire voir quelle distance il y a entre une vérité entrevue et une vérité démontrée."

It would seem that Hooke also believed that it was an inherent property of celestial bodies to attract other bodies by a force directed to their centres, by which, perhaps, he meant their centres of gravity. In the case of spheres this is true, but apparently Hooke deemed it to be true whatever was the shape of the body, and that the law was that of the inverse square of the distance inside as well as outside the bodies. This

* Du Chastelet, *Principes*, Paris, 1759, vol. ii. p. 6; Rigaud, p. 66.

also was a mere conjecture, though on the strength of it he asserted (correctly) that the spiral path mentioned by Newton in his letter of Nov. 28, 1678, was wrong.

In Hooke's statement of his case he lays stress (i) on a certain hypothesis he had published in 1674, and (ii) on the correspondence of 1679.

The introduction of the hypothesis of 1674 seems to have been an afterthought, and he did not bring it forward till much later. It is not therefore alluded to either by Newton or by Halley, but it may be well to describe it here briefly. In a letter from Hooke of June 7, 1679, which I have found among his papers, he explains this hypothesis thus: "The reason of "the inequality of the celestiall motions I conceive (as I have "mentioned in some of those later pamphlets I have published) "to be from [the] double cause of their motion: the first of which "being the direct I suppose [it has from the first mover] to be "equall and always the same, but the second, being a gravita- "tion to the sun or centrall body which always bends that "direct motion into a circular or ovall one, I suppose unequall "and always lesse powerfull at a greater than lesser distance "from the centrall or attracting body—those moving powers I "suppose noe other than what are common to all bodys. And "the circumstances of magnitude, distance, medium, &c., being "considered, the same rules of motion that make out the curved "motions of heavy bodys on the earth will make out all the "celestiall motion, and give a physicall ground mathematically "to calculate tables for them which I could easily doe had I "time to spend in that employ, but I hope there will be found "others that will save me the labour." The reader must estimate for himself the value of the view thus put forward; but in any case the fact that Hooke either made no attempt to apply his hypothesis, or failed to deduce any results, renders it little more than a clever guess by an able man, and there can be no doubt that Newton's work was uninfluenced by it.

The chief claim made by Hooke was to the effect that from him Newton had, in 1679, obtained the idea of a central force

whose magnitude varied inversely as the square of the distance. Newton at once replied* giving a summary of what took place in 1679; he admitted that Hooke then mentioned the law, and stated that according to it "the motions of the planets might "be explained and their orbs defined." But this† was a mere surmise of Hooke which confessedly he had not verified (see *ex. gr.* his letter of Jan. 17, 1680, printed below, p. 149); further, to show that he, Newton, was not indebted to Hooke for the idea of the law of inverse squares, Newton specifies three cases in which, prior to 1679, he had used the law or alluded to it, namely, (i) his investigations in 1666, (ii) his letter of June 23, 1673, to Oldenburg, and (iii) his paper on light presented to the Royal Society on Dec. 7, 1675, wherein he had not only applied the law in explaining certain optical phenomena, but had suggested a similar law as explaining gravity between the earth, sun, and planets, with the dependence of the celestial motions thereon. He also pointed out that in 1673 Huygens had explicitly stated the law of inverse squares in the case of circular motion, and implied that thenceforward any mathematician who considered the subject of elliptic planetary motion would naturally take the law of inverse squares as the most probable hypothesis—the only difficulty being to verify it, which Hooke seems to have admitted he had not done. Moreover, the idea of universal gravity varying according to the law of the inverse square of the distance was not itself a new one, for it had been mentioned by Kepler and Bullialdus‡, though the former rejected it in favour of a force varying directly as the distance. To this we may add (i) that Halley and Wren regarded Hooke's statements to them in 1684 as not justified by his knowledge; (ii) that Hooke was aware of Newton's investigations in 1684 (which involve this point), but he made no claim till 1686;

* Newton's letter of May 27, 1686, printed below, p. 155.

† Newton's letter of June 20, 1686, printed below, p. 159.

‡ J. Bullialdus, *Astronomia Philolaica*, Paris, 1645, p. 23.

and (iii) that A. Wood, the author of *Athenae Oxonienses*, whose natural bias would not have been against Hooke, and to whom Hooke had applied through Aubrey for a favourable mention, declined to insert it*. I may also remark that, until the actual publication of the *Principia*, Hooke so little suspected or realised the idea of universal gravitation in the sense in which Newton used it, that in a lecture † given on May 25, 1687, he said: "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 tho' 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 assignable) why there may be something Specifick in each of them."

Hooke has been described as "the universal claimant." He attacked nearly every scientific question then discussed, and, had he concentrated his efforts, his great ability would doubtless have led him to many valuable discoveries; but usually his conclusions were hasty and incomplete, while his assertions as to what he had proved were sometimes reckless, and are unconfirmed by his extant papers. He seems however to have deemed that his guesses would be carefully examined by his contemporaries, and naturally would be the starting point of any further investigations by them. In two previous cases Hooke had come into collision with Newton, and in both had suffered somewhat in reputation in consequence. When Newton had given an account of his construction of a reflecting telescope Hooke seems to have implied ‡ that he had already invented some such instrument, but when challenged on the subject he failed to substantiate his claim. Similarly, when

* See Rigaud, p. 66, and appendix, pp. 52-57. Hooke altered Aubrey's letter of Sept. 15, 1689, in a way which made it more favourable to himself, but the original draft was found by Rigaud.

† *Hooke's Posthumous Works*, edited by R. Waller, London, 1705, p. 546.

‡ Brewster, vol. i. p. 78.

Newton had propounded his views on light Hooke made* some depreciatory remarks on their originality, and, though Newton maintained his ground, he was so annoyed at the manner in which Hooke had spoken that he refused to allow his optical researches to be published in book-form until after Hooke's death in 1703. It is probable that Newton's refusal in 1679 to open a "philosophical correspondence" with Hooke was largely based on his knowledge of Hooke's character; the correspondence, such as it was, had been forced on Newton against his will, and he was not unnaturally angry at Hooke's assertion of plagiarism, and at the fact that his conduct should have been misrepresented behind his back.

The first two books of the *Principia* had been already finished, but Newton at once declared his intention to suppress the third book rather than suffer these unjust accusations, and have disputes thus forced on him. Halley seems to have behaved in the matter with great tact. He assured Newton that all the Society regarded him as the inventor, and were greatly satisfied by the honour done them by the dedication of the work, and begged him not to let his resentment deprive them of the third book. To this Newton agreed, and the matter dropped.

* Brewster, vol. i. pp. 136-141.

CHAPTER VI.

ANALYSIS OF THE PRINCIPIA.

IN the last chapter I mentioned in the briefest possible terms the object and contents of the three books of the *Principia*. In this chapter I propose to indicate the subjects of the successive propositions. The work itself is so easily accessible that I shall not describe the proofs nor discuss the arrangement of the work, but shall confine myself to a mere list of the results established. Moreover, I wish explicitly to call the attention of the reader to the fact that in general the enunciations of the propositions and the prefatory and other notes are not translated literally.

A complete edition of the *Principia* should, I think, show the changes made in the second edition, and the further changes introduced in the third edition. I possess in manuscript a list of the additions and variations made in the second edition; the changes are very numerous, in fact I find that of the 494 (*i.e.* 510-16) pages in the first edition 397 are more or less modified in the second edition. The most important alterations are the new preface by Cotes; the propositions on the resistance of fluids, book ii. section vii. props. 34-40; the lunar theory in book iii.; the proposition on the precession of the equinoxes, book iii. prop. 39; and the propositions on the theory of comets, book iii. props. 41, 42. I have not formed a list of the changes introduced into the third edition, but I believe that the bulk of them are given in the list by

Adams which is printed by Brewster* ; the most important being the scholium on fluxions, book ii. lemma 2, and the addition of a new scholium on the motion of the moon's nodes, book iii. prop. 33, to which I may add an account of some additional experiments on the resistance of the air to bodies falling through it, and the use of some fresh astronomical observations in book iii.

Any of the three editions will serve almost equally well for drawing up a synopsis of the contents of the work where (as in this chapter) the proofs are not described, but in most cases if the proofs differ materially I have stated the fact.

PREFACE.

The first edition is preceded by a preface by Newton, to which, in the second edition, the date May 8, 1686, was added. Here Newton describes briefly the object of the work and his ideas of philosophy ; he acknowledges his obligations to Halley and the Royal Society, and mentions a few facts connected with the preparation of the book for the press. To the second edition he prefixed a short note dated March 28, 1713, while Cotes added a long preface, dated May 12, 1713, in which he indicates the changes introduced. In the third edition Newton inserted a short prefatory note dated Jan. 12, 1725-6.

The preface to the first edition commences with a statement of the object of the work and a comparison of the methods used in mechanics and geometry.

Newton then explains that the book is entitled the mathematical principles of philosophy, because, says he, all the difficulty of philosophy seems to consist in this—to find from the phenomena of motion the laws of the forces in nature, and then from these laws to deduce other phenomena ; this is the object of the general propositions in the first and second books, while in the third book the laws are applied to explain the phenomena of the solar system. I wish, he continues, that we could derive the rest of the phenomena in nature by the same kind of reasoning from mechanical principles ; for I am

* Brewster, vol. ii. pp. 304-309, 414-419.

led by many reasons to suspect that all these phenomena may depend upon certain forces by which the particles of bodies (by some causes as yet unknown) are either mutually impelled towards each other and cohere in regular figures, or are repelled and recede from each other; which forces being unknown, philosophers have hitherto interrogated nature in vain. But I hope that the principles here laid down will afford some light either to this or some truer method of philosophy. In preparing this work, he proceeds, that most acute and learned scholar, Mr. Edmund Halley, has zealously assisted me by correcting the proof sheets and taking an interest in the general arrangement; nor is this all, for it was to his solicitations that its publication is due. For when he had obtained from me my proof of the figure of the celestial orbits, he continually urged me to communicate it to the Royal Society, who then by their requests and kind encouragement induced me to think of publishing it. But, after I had commenced to treat of the inequalities of the moon's motion, and had begun to consider other questions relating to the laws and measurement of gravity and other forces, the curves that would be described by bodies attracted according to given laws, the motion of several bodies moving among themselves, the motion of bodies in resisting mediums, the resistances, densities, and motions of the mediums themselves, the orbits of comets, and similar problems, I thought the publication should be deferred until I had investigated these questions, and could put the whole together in one book. What relates to the lunar theory (being imperfect) I have collected in one place in the corollaries of prop. 66, in order to avoid being obliged to demonstrate the several effects there enumerated by a method more prolix than the subject deserved, and thus interrupt the sequence of the propositions. Some things, discovered after the rest, I chose to insert in places less suitable, rather than change the numbers of the propositions and the references. I heartily beg, he concludes, that what I have here done may be read leniently; and that the defects in so difficult a subject be not so much blamed as kindly corrected and investigated afresh by my readers.

DEFINITIONS.

The *Principia* commences with eight definitions, each being followed by an explanation or illustration of its meaning. These definitions are to the following effect :

Def. 1. The *mass* of a body is measured by the product of its density and volume. Newton adds that practically the mass of a body is estimated by its weight, to which the mass is proportional.

Def. 2. The *momentum* of a body is measured by the product of its mass and velocity.

Def. 3. The *inertia* of a body is its tendency to continue in its state of rest or of uniform motion in a straight line.

Def. 4. A force *impressed* on a body is an action which tends to change its state of rest or of uniform motion in a straight line.

Def. 5. A force is said to be *centripetal* if it tend to move the body towards a point. In the second and third editions this is illustrated by a discussion of the action of gravity.

Def. 6. An *absolute force* or the absolute quantity of a centripetal force is proportional to and is measured by the efficacy of the cause that propagates it from the centre [and thus *ex. gr.* by its magnitude at a unit distance].

Def. 7. An *accelerative force* or the accelerative quantity of a centripetal force is proportional to and is measured by the velocity generated in a given time.

Def. 8. A *moving force* or the moving quantity of a centripetal force is proportional to and is measured by the momentum generated in a given time.

Note. To the above definitions Newton appends a memorandum that his object is to discuss the mathematical effects of forces, and that his language is not to be taken as implying any physical theory as to the cause or origin of force.

Scholium. On the conceptions of time, space, place, and motion; which ideas are classified into absolute and relative, true and apparent, mathematical and common.

AXIOMS OR LAWS OF MOTION.

The definitions are followed by three laws of motion, which are as follows :

Law 1. Every body continues in its state of rest or of uniform motion in a straight line unless compelled to change that state by forces impressed on it. [This seems to be a consequence of the second law, and if so it is not clear why it was enunciated as a separate law.]

Law 2. The change of momentum [per unit of time] is always proportional to the moving force impressed, and takes place in the direction in which the force is impressed.

Law 3. To every action of one body on another there is always opposed an equal and opposite reaction of the second body on the first.

To these laws six corollaries are added, namely, (i) the parallelogram of forces; (ii) on the composition and resolution of forces; (iii)

the total momentum of a system of bodies in any direction is unaffected by their mutual actions ; (iv) the state of motion or rest of the centre of gravity of a system of bodies is unaffected by their mutual actions ; (v) the relative motions of bodies in a given space are the same whether that space is at rest or in uniform motion in a straight line ; and (vi) the relative motions of bodies are unaffected by imposing equal and parallel accelerations on each of them.

Scholium. On the history of the laws of motion ; the relations between the space described, the time occupied, and the velocity acquired in uniformly accelerated motion ; the evidence for the truth of the laws of motion ; and the meaning to be assigned to them. This scholium contains also an account of various experiments made by Newton, and an indication of the principle of conservation of energy as far as machines are concerned.

BOOK I. ON THE MOTION OF BODIES IN UNRESISTING MEDIUMS.

Newton divides the *Principia* into three books : the first on the motion of bodies in unresisting mediums, the second on the motion of bodies in resisting mediums, and the third on the application of the results of the first two books to the explanation of the solar system.

The first book is divided into fourteen sections as follows :

SECTION I.—*On the method of prime and ultimate ratios.*

Lemma 1. On the definition of a limit.

Lemmas 2, 3, 4. On the quadrature of curves.

Lemma 5. On similar figures.

Lemmas 6, 7, 8. On the ultimate equality of the evanescent arc, chord, and tangent of a continuous curve.

Lemma 9. On the ratio of the areas of certain evanescent triangles.

Lemma 10. The spaces which a body describes [from rest] under any finite force, whether constant or continually increasing or continually decreasing, are in the very beginning of the motion in the duplicate ratio of the times of description.

Lemma 11. On the measure of curvature.

Scholium. In this is included a comparison of the method of limits, the method of exhaustions, and the method of indivisibles.

SECTION II.—*On the determination of centripetal forces.*

Prop. 1. If a body describe an orbit under forces to a fixed point, the areas which it describes by radii drawn to the fixed centre of force

are in one fixed plane, and are proportional to the times of describing them. Hence (cor. 1 of second and third editions) the velocity at any point is inversely proportional to the perpendicular from the centre of force on the tangent at the point.

[The extension (attributed to Machin) to the problem of these bodies is as follows: If a particle P move under the attractions of two central forces directed to points S and T respectively, and in a direction not lying in the plane PST , then the volume swept out by the triangle PST is proportional to the time of description. Also the velocity of P at any point of its orbit is inversely proportional to the rectangle $TZ \times SM$, where TZ is the perpendicular drawn from T on the tangent to the orbit at P , and SM is the perpendicular drawn from S on the plane TPZ . Also the radius vector TP will, in equal moments of time, sweep out unequal areas whose difference is proportional to $f \times TZ \times PY/SP$, where f is the force to S , and SY the perpendicular from S on the tangent to the orbit at P (see the *Philosophical Transactions*, 1769, pp. 74-78; and Horsley, *Newtoni Opera*, vol. iv. pp. 419-425).]

Props. 2, 3. If the areas thus described about a point be proportional to the times of description, the body is acted on by a centripetal force to that point and the forces (if any) on that point.

Prop. 4. On the centripetal force on a body which describes a circle uniformly. The proof in the first edition was rewritten in the later editions. To this proposition was added a scholium on the investigations of Wren, Hooke, Halley, and Huygens (see below, p. 165), and a copy of Newton's earliest proof of this proposition (see above, p. 13).

Prop. 5. A body describes an orbit under a centripetal force, and its velocity at three points of the orbit is known: to find the centre of force.

Prop. 6. On the law of force to a given point under which a body describes a given orbit. The proof given in the first edition was rewritten in the later editions.

Prop. 7. Application of prop. 6 to the case of a body describing a circular orbit about a point in its plane. In the first edition the proposition was confined to the particular case when the point is on the circumference.

Prop. 8. Application of prop. 6 to the case of a body describing a semicircle under a force perpendicular to the bounding diameter.

Prop. 9. Application of prop. 6 to the case of a body describing an equiangular spiral about the pole.

Lemma 12. The areas of parallelograms, formed by tangents to

an ellipse (or hyperbola) and parallel to any pair of conjugate diameters are equal; see lemma 1 of the tract *De Motu*, quoted above on p. 36.

Prop. 10. On the law of force under which a body describes an ellipse about the centre. A second and alternative proof was added in the second and third editions.

Scholium. On motion (similar to that described in prop. 10) in a parabola or hyperbola.

SECTION III.—*On the motion of bodies in eccentric conics.*

Props. 11, 12. On the law of centripetal force under which a body describes an ellipse about a focus, and a hyperbola about a focus. The alternative proofs were added in the second edition.

Lemma 13. In a parabola the latus rectum of a vertex is four times the focal distance of that vertex.

Lemma 14. In a parabola the length of the perpendicular from the focus on a tangent is a mean proportional between the distances of the focus from the point of contact and from the principal vertex.

Prop. 13. On the law of centripetal force under which a body describes a parabola about the focus.

To this proposition and to props. 11, 12 were added two corollaries in which it is proved that, if a body be projected in any manner under a centripetal force which varies as the inverse square of the distance, the orbit must be a conic having that point as focus.

Prop. 14. If any number of bodies revolve about a common centre, and the centripetal force vary inversely as the square of the distance, then the latera recta of such orbits are in the duplicate ratio of the areas described by the focal radii in the same time.

Prop. 15. The periodic times in such elliptic orbits are in the sesquiquiplicate ratio of their major axes.

Prop. 16. The velocities of the bodies at any points in such orbits are in the ratio compounded of the inverse ratio of the lengths of the perpendiculars drawn from the focus on the tangents at those points and the subduplicate ratio of the latera recta of the orbits.

Prop. 17. To determine the orbit described by a body projected from any given point in any given direction with any given velocity under a central force which varies inversely as the square of the distance.

To prop. 17 were added four corollaries—two of which describe the method of treating disturbing forces—and in the second edition a scholium was appended on motion in a conic under a centripetal force to any point.

SECTION IV.—*Determination of conics when a focus and certain other conditions are given.*

[This and the next section are a digression on pure geometry, and have but little connection with the rest of the *Principia*.]

Lemma 15. If from H , one of the two foci of an ellipse (or hyperbola), any line HV be drawn equal to the principal (or major) axis of the conic, and if V be joined to the other focus S , then the orthogonal bisector of SV will touch the conic; and vice versa if the orthogonal bisector of SV touch the conic, then HV will be equal to the principal axis.

Prop. 18. To describe an ellipse and a hyperbola with a given focus; and a given principal axis; and to pass through two given points, or to touch two given straight lines, or to pass through one given point and touch one given straight line.

Prop. 19. To describe a parabola with a given focus; and to pass through two given points, or to touch two given straight lines, or to pass through one given point and touch one given straight line.

Prop. 20. To describe a conic with a given focus; and of given eccentricity; and to pass through two given points, or to touch two given straight lines, or to pass through one given point and touch one given straight line, or to touch a given straight line at a given point. [The constructions here and in many of the following propositions refer only to central conics; the parabola being treated as a limiting case, unless it is explicitly considered.]

Lemma 16. Having given three points A, B, C , to find a point Z such that $ZA - ZB$ and $ZA - ZC$ shall be equal to any given quantities.

Prop. 21. To describe a conic with a given focus; and to satisfy three conditions, either of passing through given points or touching given straight lines.

Scholium. On conjugate hyperbolas; and on another construction for the problem in prop. 21 applicable to the case when three points are given.

SECTION V.—*Determination of conics from given conditions, neither focus being given.*

Lemma 17. If a trapezium $ABCD$ be inscribed in a given conic, and P be any point on the conic, then the rectangle under the perpendiculars from P on AB, CD will be to the rectangle under the perpendiculars from P on AD, BC in a given ratio. Newton enunciates the proposition so as to include also the case when the lines drawn from P meet the lines AB, CD, AD, BC at any constant angles.

Lemma 18. Conversely, if these rectangles be in a given ratio, P will lie somewhere on a conic described about $ABCD$.

Scholium. In lemmas 17 and 18 the conics may be degenerate: also the results are true even though some of the points in the figure are at an infinite distance.

Lemma 19. Under the same conditions, to find a point P such that these rectangles shall be to one another in a given ratio; and (cor. 2) to find the locus of P .

Lemma 20. If A, B, C, D, P be points on a conic, and if from P lines PS, PQ drawn respectively parallel to AB, AC cut AC, AB in S and Q , and if BD cut PS in T , and if CD cut PQ in R , then will the ratio $PR : PT$ be determined. And, vice versâ, if $PR : PT$ be in a given ratio, then the locus of D will be a conic passing through A, B, C, P .

Hence (cor. 3) two conics cannot cut in more than four points.

Lemma 21. If any point M on a given straight line be joined to two fixed points B and C , and if through B a line BD be drawn making a given angle with MB , and through C a line CD be drawn making a given angle with MC , then the locus of the point of intersection of BD and CD is a conic passing through B and C . And, vice versâ, if D be a point on a conic passing through A, B, C , and if the angle DBM be made equal to the angle ABC , and DCM equal to ACB , then the locus of M will be a straight line.

Prop. 22. To describe a conic to pass through five given points. Newton gives two solutions, one geometrical and one mechanical, and shows how to find the centre and latus rectum of the conic.

Scholium. On a simplification of the geometrical construction given in prop. 22.

Prop. 23. To describe a conic to pass through four given points and touch a given straight line. Newton gives two solutions.

Prop. 24. To describe a conic to pass through three given points and touch two given straight lines.

Lemma 22. On the transformation of figures by conical projection.

Prop. 25. To describe a conic to pass through two given points and touch three given straight lines.

Prop. 26. To describe a conic to pass through one given point and touch four given straight lines.

Lemma 23. If on two given lines AX and BY points C and D be taken so that the ratio $AC : BD$ is equal to a given ratio, and if CD be divided in K in a given ratio, then the locus of K is a straight line.

Lemma 24. If two parallel tangents AF and BG touch a given conic in A and B , and be cut by another tangent in F and G , then the semi-diameter parallel to AF and BG is the mean proportional between AF and BG .

Lemma 25. If a conic be inscribed in a parallelogram $IKLM$

and touch LM in A , and if a tangent to the conic cut IK in Q , KL in H , LM in F , and MI in E ; then will $ME : MI = BK : KQ$, and $KH : KL = AM : MF$.

Prop. 27. To describe a conic to touch five given straight lines.

Scholium. On the determination of the centre, asymptotes, axes, and foci of conics; and on lemmas (other than those given above) by which conics might be described to satisfy given conditions.

Lemma 26. To place a given triangle so that its three vertices may lie on three given non-parallel lines.

Prop. 28. To place a given conic so that given parts may be cut off by three given non-parallel lines.

Lemma 27. To place a trapezium similar to a given trapezium so that its four vertices may lie on four given non-parallel and non-concurrent lines.

Prop. 29. To place a conic similar to a given conic so that parts given in order, kind, and proportion may be cut off by four given non-parallel and non-concurrent lines.

Scholium. On an alternative construction for the problem in prop. 29.

SECTION VI.—On the determination of the motion in given conics.

Prop. 30. A body moves in a given parabola under a centripetal force to the focus; to find its position at any assigned time.

Lemma 28. There is no oval such that the area cut off from it by arbitrary lines can be expressed by a relation involving only finite terms and dimensions.

This is not correct, for it has been pointed out that the exact quadrature of ovals of the form $y^{2n} = (2n)x^{2m(2n-1)}(a^{2n} - x^{2n})$, where m and n are positive integers, is possible.

Prop. 31. A body moves in a given ellipse under a centripetal force to a focus; to find its position at any assigned time. (*Kepler's Problem.*)

Scholium. On approximate solutions of the problem considered in prop. 31, and of the analogous problem for motion in a hyperbola.

SECTION VII.—On motion in a straight line under a centripetal force to a point in the line.

Prop. 32. To find the space described in a given time by a body falling under such a force varying inversely as the square of the distance.

Props. 33, 34. To find the velocity at any assigned place of a body falling under such a force varying inversely as the square of the distance.

Props. 32, 33, 34 are solved by considering the motion of a particle in a related conic.

Prop. 35. On the areas swept out by the vector in the related conic.

Prop. 36. To find the time of descent to the centre of force by a body falling under such a force varying inversely as the square of the distance.

Prop. 37. To find the time to the centre of force by a body projected from a given point with a given velocity under such a force varying inversely as the square of the distance. This is equivalent to a geometrical determination of the value of the integral of $\int \frac{x}{(a-x)^{\frac{3}{2}}}$.

Prop. 38. To find the relations between the time, velocity, and space described by a body falling from a given point under such a force varying directly as the distance.

Prop. 39. To find the velocity at any given point and the time required to reach that point by a body moving in a straight line under any centripetal force to a point in the line: the quadrature of certain subsidiary curvilinear figures used in the construction being assumed.

SECTION VIII.—*On the determination of the orbits in which bodies move when acted on by given centripetal forces.*

Prop. 40. Comparison of the velocity of a body in such an orbit with the velocity of a body having a corresponding rectilinear motion.

Prop. 41. On the determination of the orbit in which a body moves when acted on by a given centripetal force, also of the position of the body at any assigned time: the quadrature of curvilinear figures being assumed to be possible.

Prop. 42. A body is projected under a given centripetal force in a given direction with a given velocity; to determine the motion.

Note. Newton adds that in the above propositions although the law of centripetal force is arbitrary, yet it is assumed that at equal distances from the centre it is everywhere the same.

[The application of the Newtonian methods given in the above Section is involved and inconvenient as compared with the analytical methods now current.]

SECTION IX.—*On the motion of bodies in orbits which revolve round the centre of force, and on the motion of the apsides.*

Prop. 43. To find the force under which a body will move equi-arcally in an orbit revolving about the centre of force in the same manner as another body in the same orbit at rest.

Prop. 44. The difference of the forces under which two bodies move equally, one in a quiescent orbit, the other in the same orbit revolving, varies inversely as the triplicate ratio of their common distances.

Prop. 45. To find the motion of the apses in orbits which are nearly circular. The result is illustrated by examples when the law of force is (i) μ , (ii) μr^{n-3} , (iii) $\mu r^{m-3} + \nu r^{n-3}$; and by two corollaries on the determination of the law of centripetal force from the motion of the apses, and on how the motion of the apses is affected by the addition to the centripetal force of an extraneous force.

The second corollary is illustrated by showing that if this extraneous addition be $1/357.45$ th part of the force under which the body would revolve in the ellipse, then in each revolution the apse line would progrede $1^{\circ} 31' 14''$, a number which in the second edition was corrected to $1^{\circ} 31' 28''$. It seems clear from the Portsmouth papers that this was given merely as an illustration of the method, but in the third edition the words "Apsis lunae est duplo velocior circiter" were added. It may be that this remark was inserted in order to show that the corollary was not applicable to the case of the moon—in fact only one part of the sun's disturbing force is here treated—but a reader might also think that the remark was intended to point out a discrepancy between the theory and observations. As Newton had explained the similar difficulty in the case of the node, some writers suspected (*ex. gr.* Godfray, in his *Lunar Theory*, second edition, 1859, art. 68) that the scholium in the first edition to book iii. prop. 35 meant that he had found the explanation: but nowhere in the *Principia* does Newton explicitly give this explanation, though in book iii. prop. 25 he estimates that the total disturbing force of the sun on the moon bears to the earth's centripetal force the ratio 1 to $178\frac{2}{3}$, which would make the annual progression of the apse line about what it actually is. The remark at the end of book i. prop. 45 was, however, read by many as indicative of a variance between observation and the Newtonian theory, and the explanation of a difference which had become an obstacle to the universal acceptance of the Newtonian system was first given by Clairaut. The Portsmouth papers contain Newton's original work, and show that he had found, by carrying the approximation to a sufficiently high order, that the mean annual motion of the apse line was $38^{\circ} 51' 51''$, which is within 2° of the true value (see below, p. 109; also the Portsmouth Collection, *Catalogue*, pp. xi-xiii, xxvi-xxx, and section I. division ix. numbers 7, 12).

SECTION X.—*On the motion of bodies on smooth planes which do not contain the centre of force, and on the motion of pendulums.*

Prop. 46. To determine the motion of a body moving on a given plane, under a given centripetal force, when projected in any direction

on the plane and with any velocity: the quadrature of curvilinear figures being assumed to be possible.

Prop. 47. All bodies moving in any plane, under a centripetal force which varies directly as the distance, describe ellipses in equal periodic times; and rectilinear motion may be treated as a particular case of elliptic motion.

Scholium. On motion on curved surfaces.

Prop. 48. On the rectification of the epicycloid.

Prop. 49. On the rectification of the hypocycloid.

Prop. 50. To make a pendulum whose bob shall oscillate in a given hypocycloid. This is effected by cycloidal checks.

Prop. 51. If the bob of a pendulum oscillate in a hypocycloid, under a centripetal force to the centre of the fixed circle and which varies directly as the distance from that centre, all oscillations are isochronous.

Prop. 52. To find the velocity of the bob of such a hypocycloidal pendulum at any assigned place, and the time occupied in describing any given arc. The second part of this proposition was rewritten in the second edition.

Cor. 1. The above propositions may be used to compare the times of all oscillating, falling, and revolving bodies.

Cor. 2. The above propositions are directly applicable to the motion of pendulums in mines. Also the results previously enunciated by Wren and Huygens concerning motion in a cycloid can be deduced as particular cases of these propositions.

Prop. 53. To find the law of force under which a body oscillating in a given curve may oscillate isochronously: the quadrature of curvilinear figures being assumed to be possible. With applications to the common (circular) pendulum and clocks.

Prop. 54. To find the time in which a body describes any arc of a given curve under a given force to a centre in the plane of the curve: the quadrature of curvilinear figures being assumed to be possible.

Prop. 55. If a body T move on a surface of revolution whose axis passes through the centre of force, and if P be the projection of T on a plane perpendicular to the axis and cutting it in O , then the area described by OP will be proportional to the time.

Prop. 56. A body is projected with a given velocity in a given direction along a given surface of revolution under a given centripetal force to a given centre on the axis of the surface; to find the orbit: the quadrature of curvilinear figures being assumed to be possible.

[In several of the above propositions Newton has given general solutions on the assumption that certain quadratures can be effected, *i.e.*

that certain functions can be integrated. It would seem from his draft in the Portsmouth papers* that he intended to insert at the end of this section a classification of algebraical curves whose quadrature could be effected—"tandem ut compleatur solutis superiorum problematum "adjicienda est quadratura figuram toties assumpta." Doubtless this is the rule to which he alluded in his letter to Collins of Nov. 8, 1676 (*Macclesfield Correspondence*, number cclxxii. vol. ii. pp. 403-405), and which (as far as I know) has not been hitherto published.

Curves defined by a binomial equation $x = Lv^a$ are obviously capable of quadrature. In the draft he next discusses curves defined by a trinomial equation of the form

$$dv^a + ev\beta x^e + f\alpha x^f = 0,$$

where x and v are current co-ordinates, d, e, f are given constants, and $a, \beta, \epsilon, \zeta$ are any numerical indices; and says that in three cases the area is a multiple of the corresponding area of another curve which he constructs. These theorems appear to be of less practical use than Newton supposed. He failed in his attempt to extend the method so as to find what curves defined by the quadrinomial equation

$$dv^a + ev\beta x^e + fv\gamma x^f + gx^n = 0$$

are capable of exact quadrature.]

SECTION XI.—*On the motion of bodies under their mutual attractions.*

Newton commences this section by remarking that the previous propositions treat of the motions of bodies attracted to fixed centres, but that probably there is no such thing in nature as a fixed centre, for attractions are towards bodies, and action and reaction are equal. Moreover, in the following propositions when describing centripetal forces as attractions he uses the terms in their familiar mathematical sense, and is not to be supposed to be expressing a theory—for perhaps such forces may be more truly called impulses, "fortasse, si physice loquamur, verius dicantur impulsus."

Prop. 57. Two attracting bodies describe similar figures about their centre of gravity and about each other.

Prop. 58. If two attracting bodies revolve about their centre of gravity, then under the same forces a similar and equal orbit might be described about one of the bodies if it were fixed. Hence (cors. 1, 2, 3) the results of book i. props. 1, 10, 11, 12, 13 are applicable to such motions.

Prop. 59. If two bodies, of masses S and P , revolve round their centre of gravity C in the periodic time T , and if P would describe a

* Section I. division v. number 5.

similar and equal orbit about S , supposed fixed, in the periodic time t , then $T : t = \sqrt{S} : \sqrt{S+P}$.

Prop. 60. If S and P attract each other with forces inversely proportional to the square of their distance, then the ratio of the major axis of the ellipse described by P about S to the major axis of the ellipse which would be described in the same periodic time by P about S when fixed, is equal to the ratio $S + P : \{S(S+P)^2\}^{\frac{1}{2}}$.

Prop. 61. Two bodies acted on by their mutual attractions will move as if attracted according to the same law of force by a certain body placed at their centre of gravity.

Prop. 62. To determine the motions of two bodies which attract each other with forces inversely proportional to the square of the distance between them, and which are let fall from given places.

Prop. 63. To determine the motions of two bodies which attract each other with forces inversely proportional to the square of the distance between them, and which are projected from given places in given directions with given velocities.

Prop. 64. To find the relative motions of a system of bodies which mutually attract each other with forces which vary directly as the distance.

Prop. 65. Bodies whose attractive forces vary inversely as the square of the distance may move relatively to one another approximately in ellipses, and the radii drawn to the foci may describe areas approximately proportional to the times of description. To this proposition were added three corollaries on perturbed orbits and disturbing forces.

Prop. 66. If three bodies, T, S, P , attract each other with forces which vary inversely as the square of the distances; and if round the greatest of them, T , the two others, P and S , revolve, and of the latter the body P describes the interior orbit; then the areas described by P round T will be more nearly proportional to the times of description, and the orbit of P about T will approximate more nearly to an ellipse with T as focus than would be the case if T were not attracted by S and P but remained at rest, or if T were attracted (or moved) very much more or very much less.

This is proved for two cases according as to whether the orbits are in the same or different planes.

To this proposition were appended twenty-two corollaries in which it is applied to explain the chief effects of the disturbing action of a body like the sun on the motion of a body like the moon, and in particular to the motion in longitude, the motion in latitude, the annual equation, the motion of the apse line, the motion of the nodes, the

vection, the change of inclination of the lunar orbit, and the procession of the equinoxes; the proposition is also applied to the theory of the tides, and to the determination of the interior constitution of the earth as deduced from the motion of its nodes.

The general problem of the motion of three bodies under their mutual attraction still remains unsolved, and that Newton should have been able, with the limited analysis at his command, to work it out so far in the case of the moon, is worthy of special notice.

Prop. 67. Under the same hypotheses as in prop. 66, and if O be the centre of gravity of P and T , then the areas described by S round O will be more nearly proportional to the times of description than the areas described by S round T , and the orbit of P will approximate more nearly to an ellipse with O as focus than to an ellipse with T as focus.

Prop. 68. Under the same hypotheses, the areas described by S round O will be more nearly proportional to the times of description, and the orbit of S about O will approximate more nearly to an ellipse with O as focus than would be the case if T were not attracted by S and P but remained at rest, or if T were attracted (or moved) very much more or very much less.

Prop. 69. The absolute force of any one of a system of attracting bodies is, under the usual hypotheses, proportional to the mass of the body.

Scholium. The above propositions naturally lead to the consideration as to how the attraction of a body depends on its form; this is to be determined by summing the attractions of all its component particles, and to effect this it is not necessary to propound a theory as to how attraction is produced, whether “*ab actione corporum vel se mutuo petentium, vel per Spiritus emissos se invicem agitantium; sive is ab actione Ætheris aut Æris mediivæ cujuscunque seu corporei seu incorporei oriatur corpora innatantia in se invicem utcunque impellentis. . . . In Mathesi investigandæ sunt virium quantitates et rationes illæ, quæ ex conditionibus quibuscunque positis consequentur: deinde ubi in Physicam descenditur, conferendæ sunt hæ rationes cum Phænomenis.*”

SECTION XII.—On the attractions of spherical bodies.

Prop. 70. If to every point of a spherical surface there tend a force varying inversely as the square of the distance, and if all these forces be of equal absolute magnitude, then the resultant on a particle inside the surface is nothing, that is, a particle placed anywhere inside a homogeneous spherical shell is in equilibrium.

Prop. 71. Under the same hypotheses a particle outside the shell is attracted to the centre of the shell with a force inversely proportional to the square of the distance from that centre.

Prop. 72. If to every point of a solid sphere centre S , radius r , and of given density, there tend a force varying inversely as the square of the distance, and if all these forces be of equal absolute magnitude, and if the distance of a particle P from S be a given multiple of r , then the attraction of the sphere on P is proportional to r .

Prop. 73. If to every point of a solid sphere centre S there tend a force varying inversely as the square of the distance, and if all these forces be of equal absolute magnitude, then the attraction of the sphere, on a particle P inside it, is proportional to SP .

Scholium. On the physical meaning to be attached to the points of which lines, surfaces, and solids are said to be composed.

Prop. 74. Under the same hypotheses the attraction of the sphere, on a particle P outside it, is inversely proportional to SP^2 .

Prop. 75. Under the same hypotheses the attraction of the sphere, centre S , on another similar sphere, centre P , is inversely proportional to SP^2 .

Prop. 76. If to every point of a sphere, centre S , there tend a force varying inversely as the square of the distance, and if the absolute magnitude of the force at any point A and the density at A be functions only of SA , then the attraction of the sphere on another such sphere, centre P , is inversely proportional to SP^2 .

To this proposition were appended nine corollaries on the motion of such spheres about one another in conics.

Prop. 77. If to all the points of [homogeneous] spheres there tend forces directly proportional to the distances, then two spheres, centres S and P , attract each other with a force proportional to SP .

Prop. 78. The result of prop. 77 is true if the spheres be dissimilar and heterogeneous, provided the density at any point of a sphere is a function only of its distance from the centre of that sphere.

Scholium. The above propositions cover the two most important cases of attractions; other cases can be deduced from the general method explained in the following propositions. [It is arguable that the Newtonian method here indicated is not less powerful than direct analytical methods; see Brougham and Routh on the *Principia*, pp. 140-145.]

Lemma 29. If two points S and P be taken; and if about S there be described any circle [whose radius is less than SP], and about P there be also described two circles very close to one another, cutting the circle about S in E and e (situated on the same side of SP), and cut-

ting the line PS in F and f ; and there be let fall to PS the perpendiculars ED , ed ; then, in the limit when e coincides with E , and f with F , the ultimate ratio of Dd to Ff is equal to the ratio of PE to PS .

Prop. 79. Determination of the attraction of the segment of an infinitely thin homogeneous spherical shell on a particle at its centre under any law of force.

Prop. 80. Determination of the attraction of a solid homogeneous sphere on an external particle under any law of force. The result is given as a multiple of a certain area.

Prop. 81. Under the same hypotheses, to find this area.

Newton then applies the results of props. 80, 81 to the cases where the force is μr^n and (i) $n = -1$, (ii) $n = -3$, (iii) $n = -4$.

Prop. 82. Determination of the attraction of a solid homogeneous sphere on an internal particle. This is found as a multiple of the attraction on an external inverse particle.

Prop. 83. To find the force with which a segment of a sphere attracts a particle placed at the centre of the sphere; the force varying inversely as the n^{th} power of the distance.

Prop. 84. To find the force with which a segment of a sphere attracts a particle placed at a point on the axis of the segment; the force varying inversely as the n^{th} power of the distance.

Scholium. The attractions of non-spherical bodies next require attention.

SECTION XIII.—On the attractions of non-spherical bodies.

Prop. 85. If the attraction of a body on a contiguous body be much greater than on the same body when it is separated from the attracting body by a small interval; then the attractive forces of the particles of the attracting body decrease in a higher ratio than the inverse square of the distance.

Prop. 86. If the attractive forces of the particles of a body vary as the inverse cube of the distance (or in a higher ratio), then the attraction of the body on a contiguous body at the point of contact is much greater than it would be if the attracting and attracted bodies be separated from each other, though by ever so small an interval.

Prop. 87. If two similar bodies of the same material attract separately two particles whose masses are proportional to those bodies, and which are similarly situated to them, then the attractions of the particles on the bodies will be proportional to the attractions of the particles towards particles of the bodies whose masses are proportional to the bodies and which are similarly situated in them.

Prop. 88. If the particles of any body, whose centre of gravity is G ,

attract with forces directly proportional to the distance, then the attraction of the body on any particle Z will tend to G , and will be the same as that of a sphere of equal and similar matter whose centre is G .

Prop. 89. The result of prop. 88 is true also for a system of bodies.

Prop. 90. To find the attraction of a uniform circular lamina on a particle placed on its axis, under any law of attraction. This is applied in cor. 1 to the case when the law is μ/r^2 , in cor. 2 to the case when the law is μ/r^n , and in cor. 3 to the case of an infinite plate when the law is μ/r^n and $n > 1$.

Prop. 91. To find the attraction of a solid of revolution on a particle placed on its axis, under any law of attraction. This is applied in cor. 1 to a cylinder when the law of force is μ/r^2 , in cor. 2 to a spheroid on an external axial particle, and in cor. 3 to a spheroid on an internal axial particle.

Prop. 92. To find by experiment the law of attraction of the particles of a given body.

Prop. 93. If an infinite homogeneous solid terminated on one side by a plane be composed of particles which attract with a force which varies inversely as the n^{th} power of the distance, where n is greater than 2, then the attraction of the solid on a particle is inversely proportional to y^{n-3} where y is the distance of the particle from the plane.

Scholium. On the determination of the orbit described by a body attracted perpendicularly towards a given plane according to a given law; and conversely on the determination of the law of force under which a body will describe a given orbit.

Newton adds that if the equation of the [plane] orbit (referred to a line on the plane as axis of x and a line inclined to Ox at a fixed angle as axis of y) be given in such a form that the ordinate can be expanded in a convergent series, then the general method for finding the law of force parallel to the ordinates under which a body will describe the curve may be replaced by a simple rule which will be sufficiently illustrated by the case of the curve $cy = x^{m/n}$. The ordinate at a point whose abscissa is $x + o$, where o is very small, is determined by the equation

$$cy = x^{\frac{m}{n}} + \frac{m}{n} x^{\frac{m}{n}-1} o + \frac{1}{2} \frac{m}{n} \left(\frac{m}{n} - 1 \right) x^{\frac{m}{n}-2} o^2 + \dots,$$

and the force required will be proportional to the coefficient of the term on the right-hand side involving o^2 . For example, in the parabola $cy = x^2$, we have $m = 2$, $n = 1$, and the force is constant; in the hyperbola $xy = d^2$ we have $m = -1$, $n = 1$, and the force is proportional to x^{-3} that is to y^3 [in fact, generally, if the curve be $y = f(x)$, the force

varies as y'']. But, says he, leaving propositions of this kind, I shall proceed to some others concerning a kind of motion which I have not yet discussed.

SECTION XIV.—*On the motion of minute corpuscles when acted on by centripetal forces tending to the several parts of any large body.*

Prop. 94. If two similar mediums be separated by a space contained between parallel planes, and a body in its passage through that space be attracted towards either of these bounding planes with a force depending only on the distance from the plane, and be not acted on by any other force; then the sine of the angle (ϕ) of incidence upon the first plane will be to the sine of the angle (ϕ') of emergence from the second plane in a given ratio.

Prop. 95. Under the same hypotheses the velocity of the corpuscle before incidence is to the velocity after emergence as $\sin\phi'$ to $\sin\phi$.

Prop. 96. Under the same hypotheses, and assuming that the velocity before incidence is greater than afterwards, then if the angle of incidence be increased continually, the corpuscle will be at last reflected, and the angle of reflexion will be equal to the angle of incidence.

Scholium. On the application of the above propositions to the theory of light, on the finite velocity of light, and on diffraction phenomena. Since there is an analogy between the propagation of the rays of light and the motion of bodies, Newton adds two propositions which are applicable to optics, and in establishing which it is unnecessary to consider the nature of the rays of light or whether the corpuscular theory is true.

Prop. 97. A system of rays diverge from a given point; to find an (aplanatic) surface which will refract them to a given point.

Prop. 98. A system of rays diverge from a given point, and are refracted at a surface of revolution about an axis through the point; to find an (aplanatic) surface which will refract them to a given point on the axis, *i.e.* to construct an aplanatic lens.

Scholium. The above methods are applicable when the rays are refracted at more surfaces. In constructing optical instruments it is preferable to use only lenses whose surfaces are spherical, not only because they can be made more readily and accurately, but also because rays incident obliquely would be refracted to a point more accurately than by spheroidal lenses. It is chromatism, however, that is the real obstacle to perfecting practical optics, and unless the errors thence arising can be corrected, "labor omnis in caeteris corrigendis imperite collocabitur."

BOOK II. ON THE MOTION OF BODIES IN RESISTING MEDIUMS.

The second book is devoted to the discussion of the motion of bodies in resisting mediums. It is divided into nine sections as follows :

SECTION I.—*On the motion of bodies in a medium whose resistance varies directly as the velocity.*

Prop. 1. If a body be resisted in the ratio of its velocity the momentum lost by the resistance is proportional to the space traversed.

Lemma 1. Quantities proportional to their differences are continually proportional. That is, if $a : a - b = b : b - c = c : c - d$, etc., then $a : b = b : c = c : d$, etc. : see lemma 2 of the tract *De Motu* quoted above (p. 36).

Prop. 2. If a body move under the action of no external force in a homogeneous medium whose resistance varies as the velocity, and the time of motion be divided into a number of equal intervals, then the velocities at the beginnings of those intervals are in a geometrical progression, and the spaces described in each of those intervals are as those velocities.

Prop. 3. A body moves in a straight line under gravity (supposed uniform) in a homogeneous medium whose resistance varies as the velocity ; to find the motion.

Prop. 4. A body is projected under gravity (supposed uniform and constant in direction) in a homogeneous medium whose resistance varies as the velocity ; to find the motion.

The first and second of the seven corollaries attached to this proposition were added in the second edition.

Scholium. The above law of resistance is to be regarded as a mathematical hypothesis rather than a physical one. In mediums void of tenacity the resistance varies as the square of the velocity, and to the consideration of motion under that law the next section is devoted.

SECTION II.—*On the motion of bodies in a medium whose resistance varies as the square of the velocity.*

Prop. 5. If a body move under the action of no external force in a homogeneous medium whose resistance varies as the square of the velocity, and if the time of motion [reckoned from a certain era] be divided into a number of intervals in a geometrical progression whose ratio is greater than unity, then the velocities at the beginnings of those

intervals are as the reciprocals of the corresponding terms of that geometrical progression, and the spaces described in each of those intervals are equal.

Prop. 6. Homogeneous and equal spheres moving under no external force in a medium whose resistance varies as the square of the velocity will, in times which are reciprocally as their velocities at the beginnings of those times, describe equal spaces and lose parts of their velocities proportional to the wholes.

Prop. 7. Under the same hypotheses any homogeneous spheres will, in times which are directly as their momenta and inversely as the squares of their velocities at the beginnings of those times, describe spaces proportional to those times and the velocities at the beginnings of those times conjointly, and lose parts of their momenta proportional to the wholes.

Lemma 2. On the rule for forming the fluxions (or moments) of products, quotients, and powers of simple algebraical quantities.

Scholium. To this lemma a scholium was added.

In the first edition this scholium was to the following effect: In some letters which passed about ten years ago between that most skillful geometrician, G. G. Leibnitz, and myself, I informed him that I possessed a method of finding maxima and minima, of drawing tangents, and of performing similar operations, which was applicable to both rational and irrational quantities, and I concealed this method in transposed letters involving this sentence [Data aequatione quocunque fluentes quantitates involvente, fluxiones invenire, et vice versa]: that illustrious man replied that he also had lighted on a method of the same kind, and he communicated his method which hardly differed from my own except in the language and notation (and in the idea of the generation of quantities). The fundamental principle of both is contained in this lemma.

Leibnitz appealed to this as evidence of his invention of the calculus independently of Newton, but Newton asserted that it was not written in that sense, and was merely a statement of an historical fact.

In the second edition the scholium remained unaltered, save that in the last line but one the words in brackets () were inserted.

In the third edition this scholium was replaced by another to the following effect: In a certain letter of mine to Mr. J. Collins, dated December 10, 1672, having described a method of tangents which I suspected to be the same as that of Slusius, at that time not yet published, I added these words: "*Hoc est unum particulare vel corollarium potius methodi generalis, quae extendit se citra molestum ullum calculum, non modo ad ducendum tangentes ad quavis curvas sive geometricas sive*

“mechanicas vel quomodocunque rectas lineas aliasve curvas respicientes, “verum etiam ad resolvendum alia abstrusiora problematum genera de “curvitatibus, arcibus, longitudinibus, centris gravitatis curvarum, &c. “neque (quemadmodum Huddenii methodus de maximis et minimis) ad “solas restringitur aequationes illas quae quantitibus surdis sunt im- “munes. Hanc methodum intertexui alteri isti qua aequationum exegesis “instituo reducendo eas ad series infinitas.” Thus far that letter. And these last words relate to a tract which I had written on these matters in the year 1671. The fundamental principle of that general method is contained in the preceding lemma.

Prop. 8. If a body move in a straight line under the action of gravity (supposed uniform) in a homogeneous medium, and the space described be divided into equal parts, then the resultants of gravity and the resistances at the beginnings of those spaces are in a geometrical progression.

Prop. 9. Under the hypotheses of prop. 8, to find the time of ascent to the highest point and the time of descent to any point.

Prop. 10. To find the density of a medium which shall make a body move in a given curve, it being supposed that gravity is uniform and constant in direction, and that the resistance of the medium varies jointly as its density and the square of the velocity: also to find the velocity of the body at any point.

Several necessary corrections were introduced in the demonstration given in the second edition (see *passim* the Cotes Correspondence, letters lxxviii, lxxxvii.) The problem is solved by the use of fluxions (moments); is illustrated by applying the results to the cases of (i) a semicircle, (ii) a parabola, (iii) a hyperbola, (iv) the curve $xy^m = a^{m+1}$; and the results were extended by a scholium to the case where the resistance varies as the n^{th} power of the velocity with numerous illustrations; but Newton failed to find a curve which made the density constant.

SECTION III.—*On the motion of bodies in a medium whose resistance consists of two terms, one varying as the velocity and the other as the square of the velocity.*

Prop. 11. If a body move in such a medium under no external forces, and a series of times be taken in arithmetical progression, then the sums of a constant and quantities inversely proportional to the velocities at the beginnings of these times are in geometrical progression.

Prop. 12. Under the same hypotheses, if a series of spaces described be taken in arithmetical progression, then the sums of a

constant and quantities proportional to the velocities at the beginnings of those spaces are in geometrical progression.

Prop. 13. A body moves in a straight line under gravity (supposed uniform) in such a medium ; if the velocity be represented graphically by a line in a certain way, then the time of motion can be represented graphically by an area ; and conversely.

A short scholium on this was added in the third edition.

Prop. 14. Under the same hypotheses, and if the resultants of the resistance and gravity be taken in geometrical progression, then the space described in a certain time can be represented graphically by the difference between two areas.

Scholium. On the resistance of fluids as caused partly by the tenacity, partly by the attrition, and partly by the density of fluids ; and on the extension of the foregoing propositions. This was added in the third edition.

SECTION IV.—*On the spiral motion of bodies in a resisting medium.*

Lemma 3. On the radius of curvature at any point of an equiangular spiral.

Prop. 15. A body moves in a resisting medium under a central force which varies inversely as the square of the distance ; if the density of the medium (to which, other things being equal, the resistance is proportional) vary inversely as the distance from the centre of force, the body may revolve in an equiangular spiral. The argument was somewhat altered in the second edition.

To this were appended nine corollaries on motion in such an orbit, and when such motion is possible.

Prop. 16. The result of prop. 15 is true also when the central force varies inversely as the n^{th} power of the distance.

Scholium. On motion in mediums such as those discussed in props. 15, 16.

Prop. 17. To find the central force and the resistance of a medium in order that a body may move in a given spiral, the law of velocity being given.

Prop. 18. To find the density of a medium in order that a body may move in a given spiral, the law of force being given.

SECTION V.—*On the density and pressure of fluids, and on Hydrostatics.*

Definition. A fluid is a body whose parts yield to any force acting on it, and in yielding are easily moved among themselves.

Prop. 19. The pressure at every point of a homogeneous fluid at rest, contained in a vessel at rest, is the same and is equal in all directions — the consideration of condensation, gravity, and centripetal forces being neglected—and the parts of the fluid remain at rest unmoved by such pressure.

Prop. 20. To find the pressure on the surface of a sphere covered by a mass of fluid which gravitates to the centre of the sphere, the strata of equal density being concentric spheres; with corollaries on the theory of floating bodies.

Prop. 21. An elastic fluid in which the density is proportional to the pressure is attracted by a central force which varies inversely as the distance; if a series of distances from the centre be taken in geometrical progression, then the densities at those distances will be also in geometrical progression.

Prop. 22. An elastic fluid in which the density is proportional to the pressure is attracted by a central force which varies inversely as the square of the distance; if a series of distances from the centre be taken in harmonical proportion, then the densities at those distances will be in geometrical progression.

Scholium. On the theorems analogous to props. 21 and 22 under other laws of centripetal force or other laws connecting the density and pressure. Newton, however, says that to discuss all these cases would be tedious, and it would be but of little use, since (he adds in the third edition) experiments show that the density of our atmosphere is either accurately, or at least extremely nearly, proportional to the pressure.

Prop. 23. A fluid is composed of particles which are mutually repulsive; if the density be proportional to the pressure, the force of repulsion must vary inversely as the distance; and vice versa.

Scholium. If the force of repulsion vary inversely as the n^{th} power of the distance, the pressure will vary as the $\frac{1}{3}(n+2)^{\text{th}}$ power of the density; and vice versa. But in the above propositions it must be assumed that the repulsion does not extend indefinitely. Also it must be remembered that whether elastic fluids do consist of mutually repulsive particles is a physical question on which no opinion is expressed.

SECTION VI.—On the motion of pendulums in resisting mediums.

Prop. 24. The masses of pendulums, such that the distances between their centres of oscillation and of suspension are constant, vary jointly as the weights and the squares of the times of oscillation in vacuo. And (cor. 5) universally the mass of a pendulum varies directly

as the weight and the square of the time of oscillation and inversely as the length.

Prop. 25. The bob of a cycloidal pendulum, moving under gravity in a cycloid whose axis is vertical, and in a medium whose resistance is constant, will perform its oscillations in the same time as it would in a non-resisting medium of the same density, and proportional parts of the arcs are described simultaneously.

Prop. 26. Cycloidal pendulums, moving under gravity in cycloids whose axes are vertical, and in a medium whose resistance is proportional to the velocity, are isochronous.

Prop. 27. The difference between the time of oscillation of a pendulum moving in a medium whose resistance is proportional to the square of the velocity, and the time of oscillation of a similar pendulum moving in a non-resisting medium of the same density is approximately proportional to the arc described.

Prop. 28. If a cycloidal pendulum of length L move in a medium whose resistance is constant, then the ratio of the resistance to gravity is equal to the ratio of the excess of the whole arc described in the descent of the pendulum above the whole arc described in the subsequent ascent to $2L$.

Prop. 29. A cycloidal pendulum oscillates under gravity in a cycloid whose axis is vertical, and in a medium whose resistance varies as the square of the velocity; to find the resistance at each point; and therefore (cor. 3), the velocity at each point.

Prop. 30. On constrained motion in a cycloid under any law of resistance; from which the result of prop. 28 is deduced, also approximate solutions of prop. 29, and of the corresponding proposition when the resistance varies as the velocity. The proof was somewhat simplified in the second edition.

Prop. 31. A pendulum oscillates in a resisting medium. If the resistance in each of the proportional parts of the arc described be altered in any ratio, then the difference between the arc of descent and the arc of subsequent ascent is altered in the same ratio.

General Scholium. On Newton's pendulum experiments. In the first edition, this was printed at the end of section vii.

SECTION VII.—*On the motion of fluids, and the resistance to projectiles.*

The problems treated in this section are far from easy, and in general their treatment here is incomplete, but there is much that is interesting in studying the way in which Newton attacked questions which seemed to be beyond the analysis at his command.

Prop. 32. If two similar systems of bodies consist of an equal number of particles, and if the corresponding particles (each in one system to each in the other) be similar, proportional, and similarly situated among themselves, and their densities have to each other the same given ratio; and if they begin to move among themselves in proportional times, and with similar motions (that is, those in one system among one another, and those in the other among one another); and if the particles that are in the same system do not touch one another, except at the instants of reflexion; nor attract, nor repel each other, except with accelerations that are inversely as the diameters of the corresponding particles, and directly as the squares of the velocities; then the particles of these systems will continue to move among themselves with like motions and in proportional times.

Prop. 33. Under the same hypotheses the resistance offered by finite parts of the systems varies at any point as the square of the velocity of the particles there, as the squares of their diameters, and as the density of the part of the system there.

To which were added corollaries in which the proposition is applied to give the law of resistance to motion in the air and other fluids.

[Propositions 34-40 inclusive, and the corollaries, lemmas, and scholiums thereto attached, were rewritten in the second edition. I have here followed the order of the second edition. In the first edition prop. 34 is on an extension of the results of props. 32, 33 to cases where the particles of the systems are contiguous but frictionless; prop. 35 was what is here printed as 34; prop. 36 was on the resistance experienced by a sphere moving in a rare and elastic fluid; prop. 37 (of which the argument is erroneous) was on the motion of water passing through a hole in a vessel; prop. 38 was on the resistance experienced by the front of a sphere moving in a fluid; lemma 4 and prop. 39 were on the effect of acceleration impressed on a vessel containing fluid and floating bodies in relative equilibrium; prop. 40 was on the resistance experienced by a sphere moving in a fluid of given density; with a general scholium the substance of which was subsequently transferred to the end of section vi, see above, p. 99.]

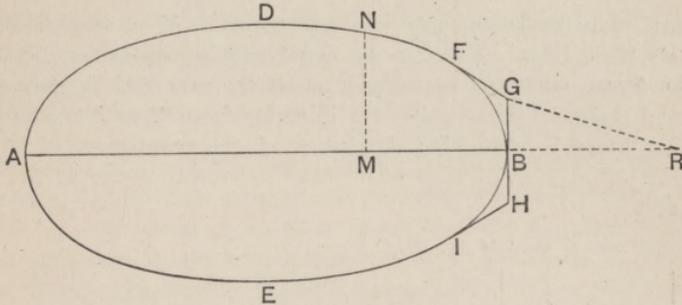
Prop. 34. A rare medium consists of small quiescent particles of equal magnitude and freely disposed at equal distances. If a globe and a cylinder of equal diameters move in such a medium with equal velocities in the direction of the axis of the cylinder, then the resistance to the motion of the globe is half that to the motion of the cylinder.

Scholium. On similar propositions concerning the motion of other figures.

Newton commences by determining the conical frustrum of given

base and altitude which will meet with least resistance when moving in the direction of its axis.

Next consider a solid generated by the revolution of the oval *ADFB* about *AB* and moving in the direction *AB*. Then he shows that, if at *B* we draw the tangent *HBG*, and if we take points *H* and *G* on



it so that the tangents *GF* and *HI* make the angles *FGB* and *IHB* each equal to 135° , then the solid generated by the revolution of the figure *ADFGHBHIE* about *AB* will encounter less resistance than the original solid.

Also, if *N* be any point on the generating curve, and if *NM* be drawn perpendicular to *AB*, and if a line through *G* parallel to the tangent at *N* cut *AB* produced in *R*, then the solid described by a curve such that

$$4 MN \cdot BR \cdot GB^2 = GR^4$$

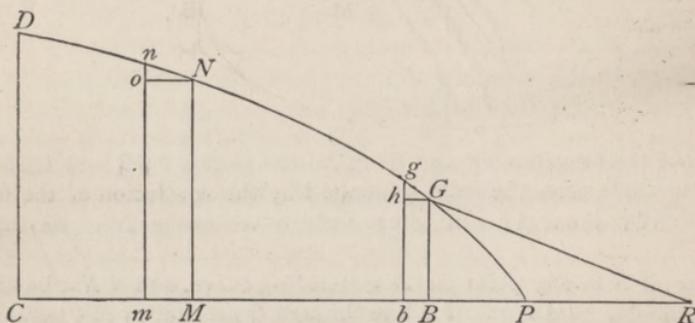
will encounter less resistance than any other solid of revolution of the same length and breadth.

[Newton's determination of the solid of least resistance is deducible from the differential equation of the generating curve, but in the *Principia* he gave no proof. The problem may be solved by the calculus of variations, but it long remained a puzzle to know how Newton had arrived at the result. A letter contained in the Portsmouth Collection* has set the matter at rest, and as all my readers may not have seen it, I reproduce here the part dealing with this scholium. It is also interesting as showing how freely Newton made use of fluxions in establishing results given in the *Principia*. The letter seems to have been written to David Gregory shortly after his visit to Cambridge in May, 1694 (see below, p. 122), and after alluding to that visit and some other matters Newton proceeds :

The figure which feels the least resistance in the Schol. of Prop. xxxv. Lib. ii. is demonstrable by these steps.

* Catalogue, pp. xxi-xxiii.

1. If upon BM be erected infinitely narrow parallelograms $BGhb$ and $MNom$ and their distance Mb and altitudes MN , BG be given, and the semi sum of their bases $\frac{Mm+Bb}{2}$ be also given and called s and their semi difference $\frac{Mm-Bb}{2}$ be called x : and if the lines BG , bh , MN , mo , butt upon the curve $nNgG$ in the points n , N , g , and G , and the infinitely little lines on and hg be equal to one another and called c , and the figure $mnNgGB$ be turned about its axis BM to generate a solid, and this solid move uniformly in water from M to B according to the direction of its axis BM : the sum of the resistances of the two



surfaces generated by the infinitely little lines Gg , Nn shall be least when gG^{qq} is to nN^{qq} as $BG \times Bb$ to $MN \times Mm$.

For the resistances of the surfaces generated by the revolution of Gg and Nn are as $\frac{BG}{Gg^{quad}}$ and $\frac{MN}{Nn^{quad}}$, that is, if Gg^{quad} and Nn^{quad} be called p and q , as $\frac{BG}{p}$ and $\frac{MN}{q}$ and their sum $\frac{BG}{p} + \frac{MN}{q}$ is least when the fluxion thereof $-\frac{BG \times \dot{p}}{pp} - \frac{MN \times \dot{q}}{qq}$ is nothing, or $-\frac{BG \times \dot{p}}{pp} = +\frac{MN \times \dot{q}}{qq}$.

Now $p = Gg^{quad} = Bb^{quad} + gh^{quad} = ss - 2sx + xx + cc$ and therefore $\dot{p} = -2s\dot{x} + 2x\dot{x}$, and by the same argument $\dot{q} = 2s\dot{x} + 2x\dot{x}$ and therefore $\frac{BG \times 2s\dot{x} - 2x\dot{x}}{pp} = \frac{MN \times 2s\dot{x} + 2x\dot{x}}{qq}$, or $\frac{BG \times s - x}{pp} = \frac{MN \times s + x}{qq}$ and thence pp is to qq as $BG \times s - x$ to $MN \times s + x$, that is, gG^{qq} to nN^{qq} as $BG \times Bb$ to $MN \times Mm$.

2. If the curve line $DnNgG$ be such that the surface of the solid generated by its revolution feels the least resistance of any solid with the same top and bottom BG and CD , then the resistance of the two narrow annular surfaces generated by the revolution of the [infinitely

little lines nN] and Gg is less than if the intermediate solid $bgNM$ be removed [along CB without altering Mb , until bg comes [to BG], supposing as before that on is equal to hg ,] and by consequence it is the least that can be, and therefore gG^{99} is to nN^{99} as $BG \times Bb$ [is to $MN \times Mm$].

[Also if] gh be equal to hG so that the angle [gGh is 45^{deg}] then will $4Bb^{99}$ be [to nN^{99} as $BG \times Bb$ is to] $MN \times Mm$, and by consequence $4BG^{99}$ is to GR^{99} as BG^9 is to $MN \times BR$ or $4BG^9 \times BR$ is to GR^{cub} [as GR to MN].

Whence the proposition to be demonstrated easily follows.]

Prop. 35. To find the resistance of a sphere moving uniformly forward in a medium such as that described in prop. 34. If the sphere and the particles of the medium be perfectly elastic, the resistance is to the force by which the motion could be destroyed in the time in which the sphere describes two-thirds of its diameter as the density of the medium to the density of the sphere. Also (cor. 1) if the sphere and the particles be perfectly inelastic, the resistance is diminished one-half.

Scholium. On motion in continuous mediums such as water.

Prop. 36. On the motion of water running out of a cylindrical vessel through a circular hole in the bottom.

Lemma 4. The resistance to the motion through a fluid of a cylinder in the direction of its axis is independent of its length.

Prop. 37. If a cylinder move uniformly in the direction of its length through a compressed infinite inelastic fluid, the ratio of the resistance (due to its transverse section) to the force by which motion may be destroyed in the time it takes to move four times its length, is approximately equal to the ratio of the density of the fluid to the density of the cylinder.

Scholium. In the above proposition the stream lines of the fluid are assumed to be parallel to the axis of the cylinder. This is not accurately true.

Lemmas 5, 6, 7, and Scholium. All smooth convex solids of revolution (such as cylinders, spheres, spheroids) with their axes along the axis of a canal containing inelastic frictionless fluid (such as water) will equally hinder and be equally acted on by the fluid flowing through the canal. Consideration of the circumstances under which this is true.

Prop. 38. If a sphere move uniformly through a compressed infinite inelastic fluid, the ratio of its resistance to the force by which its motion may be destroyed in the time it takes to move eight-thirds of its diameter is approximately equal to the ratio of the density of the fluid to the density of the sphere.

Prop. 39. On the resistance to the uniform motion of a sphere along the axis of a canal containing compressed inelastic fluid.

Scholium. On a correction which in strictness ought to be applied to the result of props. 38 and 39, but which practically may be neglected.

Prop. 40. On the manner of making experiments on the resistance the motion of a sphere in an inelastic perfect fluid.

Scholium. On the results of Newton's experiments on the resistance to the motion of spheres in fluids.

SECTION VIII.—*On undulatory motion propagated through a fluid.*

Prop. 41. A pressure is propagated through a fluid in straight lines only if the particles of the fluid lie in straight lines.

Prop. 42. Motion propagated from one side of a screen through a hole in it diverges from the direct path into the unmoved fluid on the other side of the screen.

Prop. 43. A body vibrating in an elastic medium propagates waves on every side, but a body vibrating in an inelastic medium excites a circular motion.

Prop. 44. On the time of oscillation of water in two vertical pipes connected at their base by a similar horizontal pipe.

Prop. 45. The velocities of waves [in water] is in the subduplicate ratio of their breadths. This is rather an instance of Newton's power of intuition than a demonstration.

Prop. 46. On the determination of the velocities of waves [in water].

Prop. 47. The particles of a fluid through which waves are being propagated are accelerated according to the law of the oscillating pendulum. This was prop. 48 of the first edition. Lagrange showed that Newton's result is only an involved statement of a truism.

Prop. 48. The velocities of waves propagated in an elastic fluid (whose pressure is proportional to the density) vary as $\sqrt{e/d}$, where e is the elastic force and d the density. This was prop. 47 of the first edition.

Prop. 49. On the determination of the velocities of waves in an elastic fluid whose elastic force and density are known.

Prop. 50. To find the breadth of a wave.

Scholium. Application of the above propositions to the velocities of light and sound. These propositions give for the velocity of sound in air under normal conditions 979 feet a second, whereas Newton says that experiments show it to be about 1,142 feet a second; and he

suggests that the difficulty might be overcome if the particles of air were small rigid spheres whose distance apart was about nine times the diameter of any one of them. This view is untenable for various reasons, and the true explanation of the difficulty was given first by Laplace in 1816 (see the *Mécanique Céleste*, vol. v. book xii. chap. 3, section 1). A large part of this scholium was rewritten in the second edition.

SECTION IX.—*On Vortices, or the circular motion of fluids.*

Hypothesis. The resistance of a viscous fluid at any point is, *cæteris paribus*, proportional to the velocity with which consecutive particles separate from each other.

Prop. 51. If a vortex be created by the revolution of an infinitely long cylinder round its axis in such a uniform infinite fluid, the periodic times of particles of the fluid thus set in motion are as their distances from the axis of the cylinder.

Prop. 52. If a vortex be created by the revolution of a sphere about a diameter in such a uniform infinite fluid, the periodic times of particles of the fluid thus set in motion are as the squares of their distances from the centre of the sphere.

Scholium. If the above hypothesis be true, the Cartesian theory of vortices is wrong; and if (as is probably the case) the resistance be less than that there assumed, the divergence of the consequences of that theory from the truth is increased.

Prop. 53. Bodies revolving in a re-entrant orbit in a vortex must be of the same density and move with the same velocity and in the same direction as the parts of the vortex there.

Scholium. The Cartesian theory of vortices is untenable.

BOOK III. ON THE SYSTEM OF THE WORLD.

The third book is devoted to an explanation of the phenomena of the solar system by means of the results established in the first two books. It is preceded by a preface, the rules of reasoning in philosophy, and a list of data obtained by astronomical observations, etc.

PREFACE.

In the preceding books, says Newton, I have laid down principles of mathematical philosophy from which we may argue in philosophical

inquiries : these principles are the laws and conditions of motion and force. . . . It remains to apply these principles to the explanation of the constitution of the system of the world. In treating this subject I at first wrote the third book in a popular manner in order that it might be read widely ; but afterwards, reflecting that those who had not sufficiently considered these principles could not easily appreciate the strength of the conclusions nor lay aside the prejudices to which they had been habituated for many years, and wishing to avoid the disputes which might be thus caused, I reduced the substance of this book into propositions, in the mathematical way, which should be understood only by those who had first made themselves masters of the principles previously established. But I do not recommend every reader to master all the propositions already established, for even good mathematicians will find that a lengthy task ; it will be enough if a reader carefully studies the definitions, the laws of motion, and the first three sections of the first book, he may then pass on to this book on the system of the world, and may subsequently consult such of the remaining propositions in the first two books which are here referred to as he shall desire.

RULES OF REASONING IN PHILOSOPHY.

Rule 1. We are not to assume more causes than are sufficient and necessary for the explanation of the observed facts.

Rule 2. Hence as far as possible similar effects must be assigned to the same causes : *ex. gr.* the fall of stones in Europe and America.

Rule 3. Properties common to all bodies within reach of our experiments are to be assumed as pertaining to all bodies : *ex. gr.* extension.

Rule 4. Propositions in experimental philosophy obtained by wide induction are to be regarded as accurate, or at least very nearly true, until phenomena or experiments show that they may be corrected or are liable to exceptions.

The above rules are taken from the third edition. The hypotheses enumerated in the first edition are less clear and less full.

PHENOMENA.

Newton states the astronomical observations and other evidence which indicate that, except for small and inconsiderable errors, Kepler's three laws are true of the five primary planets and of the moons of the earth, Jupiter, and Saturn.

PROPOSITIONS.

Props. 1-6. The astronomical evidence by which it is shown that the planets and their moons are retained in their respective orbits by gravity.

Prop. 7. Gravity is universal.

Prop. 8. On the attraction between two gravitating spheres composed of concentric shells of uniform density; with numerical applications (revised in the second edition) to the masses and densities of the planets.

Prop. 9. On the force of gravity in the interior of a planet.

Prop. 10. The solar system may exist for a very long time.

Hypothesis 1. The centre of the solar system is fixed.

Prop. 11. The centre of gravity of the solar system is fixed.

Prop. 12. The sun is ever in motion, but never recedes far from the centre of gravity of the solar system.

Prop. 13. Deduction of the approximate truth of Kepler's first two laws concerning the planets; and considerations (revised in the second edition) of the perturbation of Saturn's elliptic orbit by Jupiter, and of the earth's elliptic orbit by the moon.

Prop. 14. The aphelions and nodes of the orbits of the planets are (apart from the mutual actions of the latter) fixed.

Scholium. On the action of Jupiter and Saturn on the aphelions of the other planets. This was added in the second edition.

Props. 15, 16. Method of finding the major axis, eccentricity, and aphelion of any planetary orbit.

Prop. 17. The diurnal motion of a planet is uniform, and the libration of the moon arises from its diurnal motion. This argument was amplified in the third edition.

Prop. 18. The polar axis of a planet is less than an equatorial diameter.

Prop. 19. Discussion of the figure of the earth (or any planet, as Jupiter), and determination of the ellipticity. This argument was amplified in the second edition, and again altered in the third edition; but it continued to involve the assumptions that the spheroid is a form of equilibrium of a liquid planet, and that the ellipticity is proportional to the ratio of the centrifugal force to gravity. Demonstrations of these assumptions were given by Mac'aurin.

Prop. 20. On the weight of a body in any latitude. This argument was amplified in the second edition.

Prop. 21. The equinoctial points regrede, and the earth's axis nutates towards and from the ecliptic twice every year.

Prop. 22. On the motion of the moon and the inequalities thereof.

Prop. 23. The inequalities in the motions of the moons of Jupiter and Saturn can be derived by proportion from those of our moon.

Prop. 24. On the solar and lunar tides.

Prop. 25. Determination of the disturbing force of the sun on the moon.

Prop. 26. Determination of the inequality (the horary increment) of the area described by the moon in a circular orbit about the earth caused by the sun's disturbing force.

Prop. 27. From the horary motion of the moon to find its distance from the earth.

Prop. 28. To find the principal diameters of the non-eccentric orbit in which the moon would move: Newton's conclusion is that approximately the distance in syzygy is to that in quadrature as 69 to 70.

Prop. 29. On the variation of the moon. This argument was amplified in the second edition.

Prop. 30. On the horary motion of the nodes of the moon, the orbit being taken as a circle.

Prop. 31. On the horary motion of the nodes of the moon, the orbit being taken as an ellipse.

Prop. 32. On the mean motion of the nodes of the moon.

Prop. 33. On the true motion of the nodes of the moon.

Scholium. In the third edition Newton inserted two propositions, due to Machin, to show that the mean rate of motion of the sun from the moon's node is a mean proportional between the rates of motion with which the sun separates from the node when in syzygy and when in quadrature respectively. Pemberton had independently arrived at the same results. The methods differ from those of Newton, but the conclusions are consistent.

Prop. 34. On the horary change in the inclination to the ecliptic of the moon's orbit.

Prop. 35. Determination of the inclination to the ecliptic of the moon's orbit at any given time.

Scholium. A short scholium in the first edition was in the second edition replaced by a long scholium dealing with the application of the theory of gravity to other inequalities of the moon's motion; namely, the annual equation, the equations of the mean motions of the apogee and of the nodes, the "semi-annual equation," the "second semi-annual equation," the "semi-annual equation of the apogee," the "second equation of the moon's centre," and on the lunar motion. The scholium in the first edition indicates that Newton had calculated the motion of the moon's apogee (see above, p. 85).

[The Portsmouth papers contain some of Newton's calculations of

the inequalities above described. They appear to have been written in 1686. The most interesting are those on the motion of the moon's apogee, to which he alluded in the first edition, though the reference was struck out in the second edition.

The account of these calculations in the next paragraph is taken from the statement drawn up by the late Prof. Adams, and printed in the preface to the Catalogue of the Portsmouth Collection.

Two lemmas, carefully written out, are first established which give the motion of the apogee in an elliptic orbit of very small eccentricity due to given small disturbing forces acting, (1) in the direction of the radius vector, and (2) in the direction perpendicular to it. Next follows the application of the lemmas to the particular case of the Moon, in which the supposition that the disturbances are represented by changes in the elements of a purely elliptic orbit of small eccentricity would lead to practical inconvenience, and consequently Newton is led to modify that supposition. In the *Principia* he shows that if the moon's orbit be supposed to have no independent eccentricity, its form will be approximately an oval with the earth in the centre, the smaller axis being in the line of syzygies and the larger in that of quadratures, the ratio of these axes being nearly that of 69 to 70. Now when the proper eccentricity of the orbit is taken into account, supposing that eccentricity to be small, Newton assumes that the form of the orbit in which the moon really moves will be related to the form of the oval orbit before mentioned, nearly as an elliptic orbit of small eccentricity with the earth in its focus is related to a circular orbit about the earth in the centre. He then attempts to deduce the horary motion of the moon's apogee for any given position of the apogee with respect to the sun, and his conclusion is that, if C denote the cosine of double the angle of elongation of the sun from the moon's apogee, then the mean hourly motion of the moon's apogee when in that position is to the mean hourly motion of the moon as

$$1 + \frac{1}{2}C : 238\frac{9}{15}.$$

The investigation on this point is not entirely satisfactory, and from the alterations made in the MS. Newton evidently felt doubts about the correctness of the coefficient $\frac{1}{2}$ which occurs in this formula. From this, however, he deduces quite correctly that the mean annual motion of the apogee resulting would amount to $38^{\circ} 51' 51''$, whereas the annual motion given in the *Astronomical Tables* is $40^{\circ} 41' 30''$. The result stated in the scholium to the first edition appears to have been found by a more complete and probably a much more complicated investigation than that contained in the extant MSS.

We know that Newton was by no means satisfied with this part of

the *Principia*, and intended to recast large parts of it. A list of propositions, probably drawn up about 1694, for insertion in this part of the second edition of the *Principia*, is given in the next chapter.]

Prop. 36. On the solar tide.

Prop. 37. Consequent deductions of the magnitude of the lunar tide, from which the mass and density of the moon are concluded. The argument was amplified in the second edition, and three corollaries were added, two more being added in the third edition.

Prop. 38. On the (spheroidal) figure of the moon; and the explanation of why, save for librations, the same face of the moon is always turned to the earth.

Lemma 1. Assuming the earth to be a uniform rotating spheroid; and supposing it to be divided into a solid sphere described on the polar axis, and a protuberant circumscribed shell, and that QR is the line where the equator cuts a plane through the earth's centre and perpendicular to the line drawn thence to the sun; then the equatorial ring of protuberant matter will tend to turn the earth about QR with a force equal to one-half that of an equal mass placed at that point on the equator which is most distant from QR . This lemma was rewritten in the second edition.

Lemma 2. And the protuberant shell will exert two-fifths of the force of an equatorial ring of the same mass. This was added in the second edition.

Lemma 3. And the motion of the whole earth about QR will be about $925275/10^6$ of the motion of an equatorial ring of the same mass.

Hypothesis 2. If this ring moved round the sun with the same annual and diurnal rotations as it would have if part of the earth, then the motion of the equinoctial points would be the same whether the ring were fluid or rigid. In the first edition this was printed as a lemma, but no demonstration was given. Laplace was the first writer to prove it.

Prop. 39. To find the precession of the equinoxes. The argument was altered in the second edition.

Lemma 4. Comets are higher than the moon and traverse the solar system; and (cor. 1) shine by the sun's reflected light.

Prop. 40. Comets move in conics having the sun in a focus, and they describe areas about the sun proportional to the times of description.

Lemma 5. To describe a parabola of the form $y = a + bx + cx^2 + \dots$ which shall pass through any number of given points.

Lagrange has pointed out that lemmas 4 and 5 of this book contain implicitly the result known as Lambert's Theorem, though it was first enunciated by Euler in 1744.

Lemma 6. Certain positions of a comet being given, to find its place at any intermediate time.

Lemma 7. Two straight lines AB and AC being given, and P being a given point; to draw a straight line BPC through P and terminated by AB and AC , so that PB may be to PC in a given ratio.

Lemma 8. Let ABC be a parabola whose focus is S . Bisect the chord AC in I , and let μI be the diameter and μ the vertex of the segment $ABCI$. In $I\mu$ take a point O so that $\mu O = \frac{1}{2}I\mu$. Join OS and produce it to ξ so that $S\xi = 2OS$. Let ξB cut AC in E . Then, if a comet move along the parabolic arc $A\mu BC$, the segment AE is nearly proportional to the time of description of the arc AB ; and (cor. added in second edition), when B is at μ , it is accurately so.

Scholium. If in $\mu\xi$ we take n so that $\xi n : \mu B = 27MI : 16M\mu$ (n being on $\mu\xi$ produced or on $\mu\xi$, according as B or μ is more distant from the vertex of the curve), and if Bn cut AC in F , then the segment AF is still more nearly proportional to the time of description of the arc AB . This was added in the second edition.

Lemma 9. In the figure of lemma 8 let OS cut AC in M , then will $I\mu = \mu M = AI^2/4S\mu$.

Lemma 10. In the figure of lemma 8, produce $S\mu$ to N and P so that $\mu N = \frac{1}{2}\mu I$ and $SP = SN^2/S\mu$. Then, if a particle moved with the velocity due to the distance SP for the time T which the comet takes to describe the arc $A\mu C$, it would describe a length equal to the chord AC .

Lemma 11. If a particle were let fall from rest at N towards the sun at S , under a uniform force equal to the force at N , it would in the time $\frac{1}{2}T$ describe a space equal to μI .

Prop. 41. From three observations of a comet moving in a parabolic orbit, to determine the orbit. The question is reduced to the case treated in book i. prop. 19. The method is illustrated by an elaborate discussion of the comet of 1680 (Halley's comet), with a disquisition on the tails of comets. The argument was amplified in the second edition.

Newton says of this question "problema hocce longe difficillimum multimode aggressus," and it would seem that he had another solution which he discarded for the one here given. The solution in the *Principia*, however, may become indeterminate, as Boscovich pointed out in 1749, and, as a matter of fact, it is inapplicable in practice. Probably Newton's other solution, to which he refers, is that given in the two problems which are printed at the end of his essay *De Systemate Mundi*, and I consider it to be simpler than the solution printed in the *Principia*.

Prop. 42. On the correction of a comet's trajectory as found by prop. 41. This is accompanied by a discussion of astronomical observations on various comets. The argument was amplified in the second edition.

General Scholium. On the phenomena of the solar system, the explanations thereof, and the eternal Deity by and through whom the universe exists. This scholium was added in the second edition.

CHAPTER VII.

INVESTIGATIONS FROM 1687 TO 1726.

IN the preceding chapters I have traced in some detail the history of the development of the views on gravitation published in the *Principia*. As far as this essay is concerned I shall deal only briefly with the subsequent history of Newton's work, and on some points shall indicate the authorities rather than quote or discuss them at length.

The publication of the *Principia* in 1687 marks a distinct advance in theoretical astronomy. The presentation, in 1684 or 1685, to the Royal Society of the memoir *De Motu*, had served to call the attention of the scientific world to the importance of the discoveries there outlined, and the most eminent mathematicians of the time eagerly procured and read the *Principia* as soon as it came out.

The fame to which Newton thus attained materially affected his subsequent career. Until this time he had led the life of a student, and almost of a recluse. His reputation now caused his company to be courted, and his presence was sought in London and elsewhere by people of rank and of learning.

In passing I may here say that his enhanced celebrity led to the gratification of a long-standing desire to make the personal acquaintance of Huygens who, in the summer of 1689, came to England in order to meet Newton; and at the meeting of the Royal Society on June 12 Huygens spoke at length on gravitation, and, to return the compliment,

Newton spoke on double refraction and polarization of light. The position which Newton thus came to occupy induced him to take a more prominent part in public affairs than formerly. He had been one of the delegates sent by the University to defend their privileges against the encroachments in February, 1687, of James II.; and in 1689 Newton was elected as one of their representatives in Parliament. All this led him to hope for some place which would secure an assured income, and we may say that after 1689 he was looking for office, and that the offer in 1696 of the post of Warden of the Mint was the direct outcome of his own efforts and of those of his friends.

Putting aside his public life as outside the range of this essay, I may call attention in succession (i) to his correspondence on the subject-matter of the *Principia*, (ii) to his revision of the work and his extension of the results there given, (iii) to the preparation of a second edition, and (iv) to the preparation of a third edition.

The correspondence, entailed on Newton in the first few years after the publication of the *Principia* by requests for information, appears to have been considerable. In fact, the work was not cast in a form which was likely to facilitate its appreciation by readers of average ability and industry, for the demonstrations are concise to a fault, and in the first edition there were a considerable number of misprints; while the novelty of the subject, and the immense range covered by the book, tended to make its comprehension a matter of severe study. Newton, however, seems to have been willing to take a good deal of trouble to explain the difficulties of those who wrote to him. Thus—to take one instance—Gilbert Clerke, apparently a complete stranger to Newton, wrote*, on Sept. 26, 1687, that he could not well understand even the first three sections, although Newton says that for their comprehension a man need not be mathematicè doctus; but, being in the evening of his declining age, he hopes that Newton will permit him

* Portsmouth Collection, section vi. division xiv. number 10.

to point out some difficulties; and thereupon he criticises the demonstrations of book i. props. 15 and 17. In a graceful reply, Newton says: "I do not wonder that in reading a hard book you meet with some scruples, and hope that the removal of those you propound may help you to understand it more easily." He then answers the questions very fully, and says that he trusts that his correspondent will write again if there be anything else at which he sticks much. Clerke interpreted this permission in a very liberal manner, for in subsequent letters he criticises not only the matter of the book, but Newton's language and even his handwriting, though Clerke half justifies himself by saying that he and Barrow were mainly responsible for introducing such studies into the University some forty years previously. Newton's later replies are not extant, but he seems to have written out full demonstrations of the points on which he was asked for information.

The conclusions of the *Principia* excited almost as much interest among philosophers and literary men as among mathematicians. For example, the Earl of Halifax, an eminent man of letters of the time, was most anxious to understand the general argument of the *Principia*. He appealed to Newton to know if there were no way of mastering the subject except by the aid of mathematics, and, when he was told that it was impossible, he set himself to learn mathematics from Machin, to whom he presented fifty guineas as an encouragement; but the task was too hard, and finally Halifax, whose tastes were literary rather than scientific, abandoned it in despair.

The correspondence of Newton with two scholars—Locke and Bentley—whose private friendship he enjoyed, possesses a more permanent interest.

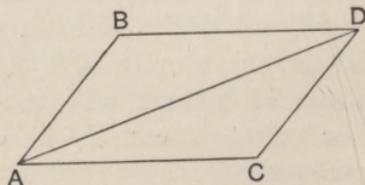
Locke's knowledge of mathematics was too limited to enable him to understand the arguments by which the propositions in the *Principia* were proved. He accordingly inquired of Huygens if he might rely on the accuracy of the demonstrations, and when assured that he might do so, he confined himself to examining the general scheme of the work.

Locke made Newton's acquaintance at the close of 1688 or beginning of 1689, apparently at the weekly receptions given by Lord Pembroke, and seems to have asked if the truth of the two fundamental propositions, namely, props. 1 and 11 in book i, could not be demonstrated in some more simple way. To this request Newton, in March, 1689, sent in manuscript an alternative proof of prop. 11, which he thought Locke might find more easy to follow than the one given in the *Principia*. This proof was found among Locke's papers, and was published in 1829 or 1830*. There is another copy of it in the Portsmouth Collection†, in Newton's handwriting, written on two double sheets of paper, making eight pages, of which the first six are closely written. As it may be new to some of my readers I give extracts from it. For the punctuation and the use of small or capital initial letters I am responsible; in most cases I have also written abbreviations at length. In the copy published by Lord King, prop. 2, as given below, is missing.

Hypoth. 1. Bodies move uniformly in straight lines unless so far as they are retarded by the resistance of the medium, or disturbed by some other force.

Hyp. 2. The alteration of motion is ever proportional to the force by which it is altered.

Hyp. 3. Motions imprest in two different lines, if those two lines be taken in proportion to the motions and completed into a parallelo-



gram, compose a motion whereby the diagonal of the parallelogram shall be described in the same time in which the sides thereof would

* Lord King's *Life of Locke*, second edition, London, 1830, vol. i. pp. 389-400; third edition, 1858, pp. 210-216.

† Section 1. division viii. number 1.

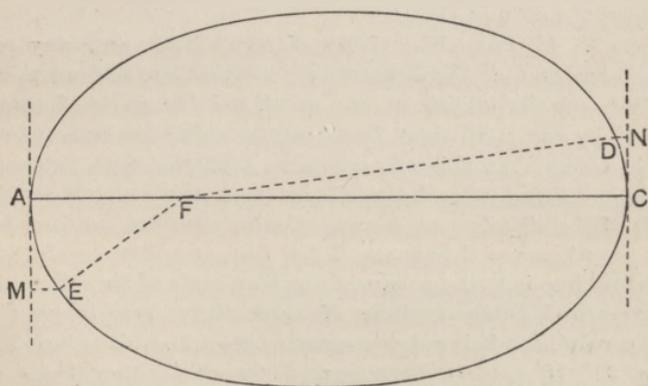
have been described by those compounding motions apart. The motions AB and AC compound the motion AD .

Prop. 1. If a body move in vacuo and be continually attracted towards an immoveable center, it shall constantly move in one and the same plane, and in that plane [with a right line drawn continually from its own center to the immoveable center of attraction] describe equal areas in equal times.

[The proof in the manuscript is similar to that given in the *Propositiones De Motu*, prop. 1, and to that given in the *Principia*, book i. prop. 1.]

Prop. 2. If a body be attracted towards either focus of an ellipsis, and the quantity of attraction be such as suffices to make the body revolve in the circumference of the ellipsis: the attraction at the two ends of the ellipsis shall be reciprocally as the squares of the [distances of the] body in those ends from that focus.

Let $AECD$ be the ellipsis; A, C its two ends or vertices; F that focus towards which the body is attracted; and AFE, CFD areas which the body, with a ray drawn from that focus to its center, describes at both ends in equal times. And those areas by the foregoing



proposition must be equal because proportionall to the times, that is, the rectangles $\frac{1}{2}AF \times AE$ and $\frac{1}{2}FC \times DC$ must be equal, supposing the arches AE and CD to be so very short that they may be taken for right lines. And therefore AE is to CD as FC to FA .

Suppose now that AM and CN are tangents to the ellipsis at its two ends A and C , and that EM and DN are perpendiculars let fall from the points E and D upon those tangents: and because the ellipsis is alike crooked at both ends those perpendiculars EM and DN will be to one another as the squares of the arches AE and CD , and there-

fore EM is to DN as FC^a to FA^a . Now in the times that the body by means of the attraction moves in the arches AE and CD from A to E and from C to D it would, without attraction, move in the tangents from A to M and from C to N . 'Tis by the force of the attractions that the bodies are drawn out of the tangents from M to E and from N to D ; and therefore the attractions are as the distances ME and ND , that is, the attraction at the end of the ellipsis A is to the attraction at the other end of the ellipsis C as ME to ND , and by consequence as FC^a to FA^a .

W. w. to be dem.

Lemma 1. If a right line touch an ellipsis in any point thereof, and parallel to that tangent be drawn another right line from the center of the ellipsis which shall intersect a third right line drawn from the touch-point through either focus of the ellipsis; the segment of the last-named right line lying between the point of intersection and the point of contact shall be equal to half the long axis of the ellipsis.

Lemma 2. Every line drawn through either focus of any ellipsis and terminated at both ends by the ellipsis is to that diameter of the ellipsis which is parallel to this line as the same diameter is to the long axis of the ellipsis.

Lemma 3. If from either focus of any ellipsis unto any point in the perimeter of the ellipsis be drawn a right line, and another right line doth touch the ellipsis in that point, and the angle of contact be subtended by any third right line drawn parallel to the first line: the rectangle which that subtense contains with the same subtense produced to the other side of the ellipsis is to the rectangle which the long axis of the ellipsis contains with the first line produced to the other side of the ellipsis as the square of the distance between the subtense and the first line is to the square of the short axis of the ellipsis.

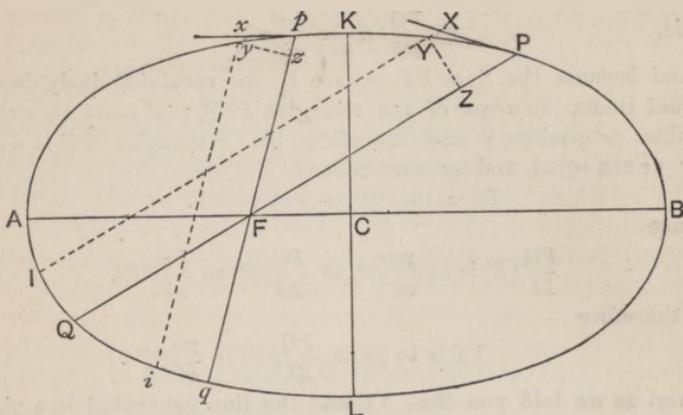
[Geometrical demonstrations of these three lemmas are given in the manuscript, but it is not necessary to reproduce them here.]

Prop. 3. If a body be attracted towards either focus of any ellipsis, and by that attraction be made to revolve in the perimeter of the ellipsis: the attraction shall be reciprocally as the square of the distance of the body from that focus of the ellipsis.

Let P be the place of the body in the ellipsis at any moment of time, and PX the tangent in which the body would move uniformly were it not attracted, and X the place in that tangent at which it would arrive in any given part of time, and Y the place in the perimeter of the ellipsis at which the body doth arrive in the same time by means of the attraction.

Let us suppose the time to be divided into equal parts, and that

those parts are very little ones so that they may be considered as physical moments, and that the attraction acts not continually but by



intervalls once in the beginning of every physical moment; and let the first action be upon the body in P , the next upon it in Y , and so on perpetually; so that the body may move from P to Y in the chord of the arch PY , and from Y to its next place in the ellipsis in the chord of the next arch, and so on for ever.

And because the attraction in P is made towards F , and diverts the body from the tangent PX into the chord PY , so that in the end of the first physical moment it be not found in the place X where it would have been without the attraction but in Y , being by the force of the attraction in P translated from X to Y ; the line XY generated by the force of the attraction in P must be proportional to that force and parallel to its direction, that is, parallel to PF .

Produce XY and PF till they cut the ellipsis in I and Q . Join FY , and upon FP let fall the perpendicular YZ , and let AB be the long axis and KL the short axis of the ellipsis. And, by the third lemma

$$YX \times XI \text{ will be to } AB \times PQ \text{ as } YZ^2 \text{ to } KL^2;$$

and by consequence,

$$YX \text{ will be equal to } \frac{AB \times PQ \times YZ^2}{XI \times KL^2}.$$

And in like manner, if py be the chord of another arch py which the revolving body describes in a physical moment of time, and px be the tangent of the ellipsis at p , and xy the subtense of the angle of contact drawn parallel to pF , and if pF and xy produced cut the ellipsis in q and i , and from y upon pF be let fall the perpendicular

$$yz; \text{ the subtense } yx \text{ shall be equal to } \frac{AB \times pq \times yz^2}{xi \times KL^2}.$$

And therefore

$$YX \text{ shall be to } yx \text{ as } \frac{AB \times PQ \times YZ^3}{XI \times KL^3} \text{ to } \frac{AB \times pq \times yz^3}{xi \times KL^3},$$

that is, as $\frac{PQ}{XI} YZ^3$ to $\frac{pq}{xi} yz^3$.

And because the lines PY , py are by the revolving body described in equal times, the areas of the triangles PYF , pyF must be equal] by the first proposition; and therefore the rectangles $PF \times YZ$ and $pF \times yz$ are equal, and by consequence

$$YZ \text{ is to } yz \text{ as } pF \text{ to } PF.$$

Whence

$$\frac{PQ}{XI} YZ^3 \text{ is to } \frac{pq}{xi} yz^3 \text{ as } \frac{PQ}{XI} pF^3 \text{ to } \frac{pq}{xi} PF^3.$$

And therefore

$$YX \text{ is to } yx \text{ as } \frac{PQ}{XI} pF^3 \text{ to } \frac{pq}{xi} PF^3.$$

And as we told you that XY was the line generated in a physical moment of time by the force of the attraction in P , so for the same reason is xy the line generated in the same quantity of time by the force of the attraction in p . And therefore the attraction in P is to the attraction in p as the line XY to the line xy ,

that is, as $\frac{PQ}{XI} pF^3$ to $\frac{pq}{xi} PF^3$.

Suppose now that the equal times in which the revolving body describes the lines PY and py become infinitely little, so that the attraction may become continual, and the body by this attraction revolve in the perimeter of the ellipsis; and the lines PQ , XI , as also pq , xi , becoming coincident and by consequence equal, the quantities $\frac{PQ}{XI} pF^3$ and $\frac{pq}{xi} PF^3$ will become pF^3 and PF^3 . And therefore the attraction in P will be to the attraction in p as pF^3 to PF^3 , that is, reciprocally as the squares of the distances of the revolving bodies from the focus of the ellipsis [towards which the attraction is directed].

W. w. to be Dem.

Bentley was another famous scholar who was desirous of understanding how Newton had been able to establish such far-reaching conclusions. In 1691 he asked William Wotton to obtain from Craige a list of works which would give that preliminary knowledge of the subject which was necessary to enable one to understand the *Principia*. The books named

by Craige* are unreasonably numerous and unnecessarily hard, and Bentley, astonished at the receipt of such a list, applied direct to Newton, who in July, 1691, sent the following directions† :

Next after Euclid's Elements the Elements of y^e Conic sections are to be understood. And for this end you may read either the first part of y^e *Elementa Curvarum* of *John De Witt*, or *De la Hire's* late treatise of y^e conick sections, or *Dr Barrow's* Epitome of Apollonius.

For Algebra read first *Barth[ol]in's* introduction, and then peruse such Problems as you will find scattered up and down in y^e Commentaries on *Cartes's* Geometry and other Alegraical [*sic*] writings of *Franciss Schooten*. I do not mean y^t you should read over all those Commentaries, but only y^e solutions of such Problems as you will here and there meet with. You may meet with *De Witt's* *Elementa curvarum* and *Bartholin's* Introduction bound up together wth *Carte's* Geometry and *Schooten's* Commentaries.

For Astronomy read first y^e short account of y^e Copernican System in the end of *Gassendus's* Astronomy and then so much of *Mercator's* Astronomy as concerns y^e same system and the new discoveries made in the heavens by Telescopes in the Appendix.

These are sufficient for understanding my book : but if you can procure *Hugenius's* *Horologium oscillatorium*, the perusal of that will make you much more ready.

At y^e first perusal of my Book it's enough if you understand y^e Propositions wth some of y^e Demonstrations w^{ch} are easier than the rest. For when you understand y^e easier they will afterwards give you light into y^e harder. When you have read y^e first 60 pages, pass on to y^e 3^d Book and when you see the design of that you may turn back to such Propositions as you shall have a desire to know, or peruse the whole in order if you think fit.

In 1692 Bentley was appointed lecturer on Boyle's newly-created foundation, and he devoted his lectures to a refutation of atheism, concluding them with the argument that the

* Craige's letter dated June 24, 1691, is given in Bentley's correspondence, see vol. ii. pp. 736-740, and vol. i. p. xxxli.; also by Brewster, see vol. i. pp. 297, 422.

† From the original in the Library of Trinity College, Cambridge ; it has been previously printed by Dr. Edleston and by Brewster.

physical constitution of the universe, as described by Newton, necessitates the existence of a Divine Being. Before publishing them he consulted Newton on some points; and in reply the latter wrote four letters*—dated respectively, Dec. 10, 1692, Jan. 17, 1693, Feb. 11, 1693, and Feb. 25, 1693—which are of special interest, as indicating his views on the origin of the solar system, the nature of gravity, and the philosophy of the universe.

If comparative strangers wrote freely to Newton on the subject of the *Principia*, it may be supposed that his own friends also communicated with him on it: probably such explanations and assistance were generally given in personal interviews. For instance, David Gregory devoted himself to a study of the book as soon as it came out, noting the points that occurred to him as he read it. His memoranda† run to 213 closely written folio pages. His first note is on book i. prop. 17, cor. 3, and is dated Sept. 22, 1687; in 1691 he was appointed—largely on Newton's recommendation—to the Savilian chair of astronomy at Oxford, and he does not seem to have had leisure to continue his detailed study of the *Principia* till December, 1692; he finished his memoranda in January, 1694. In May of that year he paid a visit to Cambridge, as he says, “quoniam varii errores in propositiones 37 et 38 (Lib. 2), irrepsere illos omnes restitutos hic apponam, prout in auctoris exemplari inveni, ineunte Maio 1694, dum Cantabrigiæ haererem consulendi divini auctoris gratia.”

This visit was followed by a correspondence, most of which is now lost; but the draft of one letter (which probably forms part of it) is in the Portsmouth Collection, and contains Newton's investigations of the form of the solid of least

* The originals are in the Library of Trinity College, Cambridge; they were printed by R. Cumberland, London, 1756; by S. Horsley in his edition of Newton's works, London, vol. iv. 1782, pp. 429-442; and by J. Nichols in his *Literary History of the Eighteenth Century*, London, 1822, vol. iv. pp. 50-60.

† Rigaud, p. 98 *et seq.*

resistance of which the result alone was given in the *Principia**.

I turn now to the history of Newton's own investigations on gravitation subsequent to the publication of the *Principia*. The part of that work with which Newton was least satisfied was the explanation of the lunar irregularities, and it is most likely that this subject continued to occupy his attention even immediately after the publication of the first edition in 1687; indeed, on July 5, 1687, we find Halley pressing him to go on with the subject of the lunar theory. It was a matter of extreme annoyance to Newton that he was unable to elaborate the theory completely. Many years afterwards he told Machin that his head had never ached but when he was studying the lunar theory, and Conduitt asserts that Halley said that when Newton was pressed to complete his theory of the moon† he replied that "it made his head ache "and kept him awake so often that he would think of it "no more."

That immediately after the publication of the *Principia* Newton continued to work at the subject of the lunar theory seems well established, for in July, 1691, Newton had arranged‡ a visit to Greenwich in order to obtain some observations which would check or verify his results. The fact appears to have been generally known. Thus in a public journal in January, 1692, De la Croze writes§: "Mr. Newton "is preparing a *new System of Philosophy* which will be much "larger and plainer than his *Principia*"; and in the same journal two months later there is a note that "nothing considerable is doing new at Cambridge, but Mr. Newton's new "System of Philosophy."

We may take it, then, that in and after 1687 Newton was working at the theory of gravitation, and it is tolerably certain

* See above, pp. 101-103.

† Brewster, vol. ii. p. 108.

‡ *Ibid.*, p. 109.

§ I take these quotations from the *Cotes Correspondence*, p. xii.

that he looked forward to ultimately publishing his investigations, but it is by no means clear that he had any immediate intention of revising the work already issued. Indeed, in a letter to Flamsteed, dated Aug. 10, 1691, introducing David Gregory to his notice, Newton* says: "I would willingly have your observations of Jupiter and Saturn for the 4 or 5 next years at least before I think further of their theory; but I had rather have them for the next 12 or 15 years. If you and I live not long enough, Mr. Gregory and Mr. Halley are young men. When you observe the eclipses of Jupiter's satellites, I should be glad to know if in long telescopes the light of the satellite, immediately before it disappears, incline either to red or blue, or become more ruddy or more pale than before."

The first edition of the *Principia* was out of print by 1691, and as time went on it became increasingly difficult to procure copies†; they were, says Cotes in his preface, "rarissima admodum et immani pretio coemenda." Sir William Browne, about 1708, secured one for two guineas, which was thought very cheap, but to poor students such a price was prohibitive, and we read of one who was reduced to transcribing the whole work in order to obtain a copy.

In December, 1691, Fatio de Duilliers wrote‡ to Huygens that it was "assez inutile de prier M. Newton de faire une nouvelle édition, . . . mais il n'est pas impossible que j'entreprenne cette édition"; and he added that, with a view to it, he had collected a considerable mass of materials, and could consult Newton on various points on which further explanation

* Baily's *Life of Flamsteed*, p. 129; Flamsteed sent an answer through David Gregory (Aug. 27, 1691) that he had not noticed any change of colour when Jupiter's satellites were occulted, but it would seem from David Gregory's letter of Nov. 7, 1691, that Cassini considered that a change of colour did occur.

† See Rigaud, pp. 104-105.

‡ *Hugenii Aliorumque Exercitationes Mathematicae*, The Hague, 1833, vol. ii. p. 124; see Rigaud, p. 89.

was required; Gregory also in a memorandum of 1691 alludes to the new edition designed by Fatio, but Fatio, writing on Feb. 5, 1692, says that the question was still undecided.

That Newton seriously entertained the idea of allowing Fatio to undertake the work is rendered the more probable from the fact that he urged Fatio to come and live at Cambridge, and on March 14, 1693, Newton* wrote "the chamber
"next me is disposed of; but that which I was contriving
"was, that since your want of health would not give you leave
"to undertake your design for a subsistence at London, to
"make you such an allowance as might make your subsistence
"here easy to you. And, if your affairs in Switzerland be not
"so pressing but that without damage to them you may stay
"still some time in England (as your last letter gives me
"hopes), you will much oblige me by returning hither."

According to his own statement Fatio was eminently well qualified for the task, and Huygens and Leibnitz thought that Newton was fortunate to have had the offer of such assistance; but Fatio's extant notes on the *Principia* hardly bear out this view—they show care and industry, but no marked ability; and they are far from justifying the lines which he wrote in his copy of the third edition:

"Insculptoque basi Newtoni nomine; in ipso

"Culmine scribatur, Facius multum addidit aedi:

"Ædi, quae immensi typus est templi Omnipotentis."

Certainly we have no reason to regret that the task of revising the *Principia* was ultimately entrusted to hands other than Fatio's.

It is, however, probable that any idea entertained by Newton of accepting Fatio's proposal was abandoned when the latter expounded his "explanation" of gravity, according to which the weight of a body was caused by the pressure of the atmosphere. Fatio asserted that he had satisfied Newton,

* Letter from Newton to Fatio quoted by J. Nichols, *Literary History of the Eighteenth Century*, London, 1822, vol. iv. p. 58.

Huygens, and Halley that this view was correct, but David Gregory added a note that as far as Newton and Halley were concerned they disapproved of it, and there is no reason to think that Huygens held a different opinion.

It would appear that in 1694 Newton began to revise the work himself with a view to ultimately bringing out a second edition; and that in the new edition he proposed to add largely to the third book, and in particular to include the results of his lunar theory as far as they were worked out. Many of the calculations and memoranda in the Portsmouth Collection* must, I think, be referred to this period, but I should far exceed the limits of this essay were I to discuss them here.

The following list of propositions on the lunar theory, which presumably were to be included in this edition, is taken from the preface to the Catalogue of the Portsmouth Collection, and will be read with interest as indicating the line of research pursued by Newton.

In Theoria Lunae tractentur hae Propositiones.

8 PROP. XXV. PROB. V. PAGE 434, PRINCIP.

Orbem Lunae ad aequilibrium reducere.

5 PROP. XXVI.

Aream orbis totius Lunaris in plano immobili descriptam mensi synodico proportionalem esse.

6 PROP. XXVII.

Invenire distantiam mediam Lunae a Terra.

7 PROP. XXVIII.

Invenire motum medium Lunae,

1 PROP. XXIX.

In mediocri distantia Terrae a Sole invenire vires solis tam ad perturbandos motus Lunae quam ad mare movendum.

2 PROP.

Invenire vires Lunae ad mare movendum.

* *Ex. gr.*, section I. divisions viii. and ix.

3 PROP. XXX.

Invenire incrementum horarium areae quam Luna in orbe non excentrico revolvens radio ad terram ducto in plano immobili describit.

4 PROP. XXXI.

Ex motu horario Lunae invenire distantiam ejus a terra.

10 PROP.

Invenire formam orbis Lunaris non excentrici.

11 PROP.

Invenire variationem Lunae in orbe non excentrico.

9 PROP.

Invenire aequationem parallacticam.

12 PROP.

Invenire formam orbis Lunaris excentrici.

13 PROP.

Invenire incrementum horarium areae quam Luna in orbe excentrico revolvens radio ad terram ducto in plano immobili describit.

14 PROP.

Invenire variationem Lunae in orbe excentrico.

PROP.

Invenire aequationem parallacticam in orbe excentrico.

PROP.

Invenire parallaxim solis.

PROP.

Invenire motum horarium Apogaei Lunaris in Quadraturis consistentis.

PROP.

Invenire motum horarium Apogaei Lunaris in conjunctione et oppositione consistentis.

PROP.

Ex motu medio Apogaei invenire ejus motum verum.

De Sole.

PROP.

Invenire locum solis.

Ex Solis motu medio et prostaphaeresi dabitur locus centri gravitatis Terrae et Lunae deinde ex hoc loco et parallaxi menstrua (quae in quadraturis Lunae est 20'' vel 30'' circiter) dabitur locus terrae cum loco opposito solis.

PROP.

Invenire motum Apheliorum.

PROP.

Invenire motum nodorum.

Nodus orbium Jovis et Saturni movetur in plano immobili quod transit per nodum illum & secat angulum orbium in ratione corporum in distantias ductorum inverse, id est in ratione equalitatis circiter, existente angulo quem hoc planum continet cum angulo orbis Jovis minore quam angulo altero quem continet cum orbe Saturni. Serventur forte inclinationes orbium omnium ad hoc planum, & quaerantur motus intersectionum quas orbes cum ipso faciunt et habebuntur motus planorum orbium respectu fixarum.

PROP.

Invenire perturbationes Orbis Saturni ab ejus gravitate in Jovem oriundas.

PROP.

Invenire perturbationes Orbis Jovis ab ejus gravitate in Saturnum oriundas.

PROP.

In systemate Planetarum invenire planum immobile.

A centro solis per orbes Planetarum ducatur linea recta sic ut si Planetæ singuli in minimas suas ab hac linea distantias ducantur, summa contentorum ad unam lineæ partem aequetur summa contentorum ad alteram; et hæc linea jacebit in plano immobili quam proxime.

Vel sic accuratius :

Per solem et orbes Planetarum et commune centrum gravitatis eorum omnium ducatur linea recta sic ut si sol et semisses Planetarum in minimis orbium ab hac linea distantias ad utramque solis partem siti angeantur vel minuantur in ratione distantiarum verarum a centro solis ad distantias mediocres ab eodem centro, deinde ducantur in distantias suas ab hac linea : summa productorum ab una rectæ parte et ab una etiam parte communis centri gravitatis, conjuncta cum summa productorum ex altera utriusque parte aequetur summa productorum reliquorum : jacebit hæc recta in plano immobili, et hujusmodi rectæ duæ planum illud determinabunt.

It seems that some knowledge of the fact that Newton had extended the applications of the theory of gravitation spread abroad. Thus, at the meeting of the Royal Society on Oct. 31, 1694, "a letter from Mr. Leibnitz to Mr. Bridges

“ was produced and read, wherein he recommends 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 Mathematicæ*, and his other “ Physical and Mathematical discoveries, lest by his death they “ should happen to be lost.” But to this and similar applications Newton turned a deaf ear, and his discoveries remained locked up in his note-books, while he proceeded with his researches and the further revision of the first edition.

In connection with this revision he required various astronomical data, for which he applied to Flamsteed, the astronomer-royal. Newton visited Flamsteed on Sept. 1, 1694, and a considerable number of letters passed between them. Thus, on Oct. 7, 1694, Newton sent a memorandum of what further observations were needed, with which he says he hopes he can “ set right the moon’s theory this winter.” On Oct. 24 he acknowledges the receipt of some observations, and on Nov. 1, in making some remarks, and pointing out certain errors in some of the observations, he adds: “ I desire only such observations as tend to perfecting the theory of the planets in “ order to a second edition of my book.” The correspondence that ensued has been discussed carefully by Brewster*, whose conclusions seem to me to be just and judicious. According to Brewster, Flamsteed gave Newton his best assistance, though perhaps with some unnecessary delay, very often in an irritating form, and accompanied by reflections on Halley—of whom Flamsteed had an unreasonable dislike—which were particularly annoying to Newton. Newton himself was far from well at this time, suffering from insomnia and general nervous irritability; he had spent months of labour in assisting†

* Brewster, vol. ii. pp. 115-132.

† Newton sent his first table of refractions on Nov. 18, 1694; the Portsmouth papers show that it was formed by finding an approximate solution of the differential equation of the path of a ray of light through the atmosphere. Newton explained the theory and sent other tables in subsequent letters (see especially the table sent on March 15, 1695).

Flamsteed for the purpose of making some return for these observations, but Newton did not show much tact in pressing his claims, which were moral rather than legal. It should be also remembered that Bentley's ill-advised remark, made in 1695, to the effect that Newton could not get lunar observations by which to check his theory, which was repeated to Flamsteed, was naturally resented by the latter. The correspondence ceased temporarily in September, 1695, as Flamsteed was travelling and in poor health; but it was subsequently resumed, and finally was brought to a not unfriendly conclusion. Of course I need not say that these letters have nothing to do with the quarrel concerning the *Historia Coelestis*, which did not commence till at least ten years later.

It is most likely that Halley also continued to give Newton some assistance in collecting the necessary astronomical data; we find, for instance, half-a-dozen letters in 1695 from Halley on comets, and further we read of visits of Halley to Newton at Cambridge.

In 1696 Newton was appointed Warden of the Mint in London, and three years later was promoted to the Mastership. His duties at the Mint necessitated his removal to the metropolis, and put a stop to his further investigations.

It was hoped that as he became accustomed to the duties of his new office he would find leisure to continue his scientific researches, and it seems to have been generally understood that the work was only delayed. Thus, in a letter dated Nov. 4, 1697, preserved in the records of the Royal Society, W. Molyneux writes* to Sloane, "I hear Mr. Newton's *Phil. Nat. Prin. Math.* is out of press, and that he designs a 2nd Edition. "Pray advise him to make it a little more plain to Readers not "so well versed in Abstruse Mathematicks, a few Marginal "Notes and references and Quotations would doe the business."

The assumption that a new edition was in progress was justified, and by 1698, if not before, Newton was again

* *Cotes Correspondence*, p. xiii.

working, as far as his occupations permitted, at it. Thus, "on Sunday the 4th December, 1698, in the time of evening "service," he went to Greenwich to obtain twelve computed places of the moon, those formerly sent him being erroneous; and the intercourse between Flamsteed and Newton, in connection with the observations required, continued for some time subsequently.

Thenceforward there are constant allusions* to the expected new edition. Thus, on Feb. 13, 1700, Leibnitz, writing to Burnet, said, "J'ai appris aussi (je ne sçai où) qu'il donnera "encore quelque chose sur le mouvement de la Lune; et on "m'a dit aussi qu'il y aura une nouvelle édition de ses principes "de la nature." And in the *Acta Eruditorum*† for that year, in replying to the charge of plagiarism brought against him by Duillier, Leibnitz said:

Etsi post tanta jam beneficia in publicum collata, iniquum sit aliquid a Domino Newtono exigere, quod novum quaerendi laborem postulet, non possum tamen mihi temperare, quin, hac oblata occasione, maximi ingenii mathematicum publice rogam, ut, memor humanorum casuum et communis utilitatis, diutius ne premat praeclaras reliquas et jam paratas meditationes suas, quibuscum scientias mathematicas, tum praesertim naturae 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.

On July 4, 1700, Sloane‡ wrote to Leibnitz, "The Royal "Society have laboured to get his [*i.e.* Newton's] Theory of the "Moon, Book of Colours, etc. printed, but his excessive "modesty has hitherto hindered him, but the Society will do "what further they can with him." So Greves, writing to Lord Ashton on Nov. 30, 1702, and describing an interview he had had with Newton on Nov. 26, says: "He owns there are "a great many faults in his book, and has crossed it, and

* *Cotes Correspondence*, p. xiv.

† *Acta Eruditorum*, May, 1700, p. 203.

‡ I take these quotations from the *Cotes Correspondence*, pp. xiv-xv.

“interleaved it, and writ in the margin of it, in a great many places. It is talked he designs to reprint it, though he would not own it. I asked him about his proof of a vacuum, and said that if there is such a matter as escapes through the pores of all sensible bodies, this could not be weighed. . . . I find he designs to alter that part, for he has writ in the margin, *Materia sensibilis*; perceiving his reasons do not conclude in all matter whatsoever.” And lastly, Flamsteed, writing to Pound on Nov. 15, 1704, says that Newton’s *Optics* “makes no noise in town as the *Principia* did, which I hear he is preparing again for the press with necessary corrections.”

As time went on it became increasingly clear that Newton would not have the leisure to complete the work himself. There is a tradition* that Newton, as soon as he was satisfied of this, intended to place his corrections and notes in the hands of David Gregory, with a view to the publication of a second edition. No direct evidence for this exists, but it is in itself quite probable, and Gregory’s name is mentioned more than once in connection with the proposed edition. Thus, on May 29, 1694, Huygens†, writing to Leibnitz, says: “la nouvelle édition que doit procurer D. Gregorius”; and on July 15, 1699, J. Monroe says that Malebranche “mightily commends Mr. Newton, adding at the same time that there were many things in his book that passed the bounds of his penetration, and that he would be very glad to see Dr. Gregory’s critick upon it.” On the other hand, in a manuscript‡ by Gregory there is a note dated May 21, 1701, on a variety of points upon which he wished to consult Newton, and the tenth memorandum is “to see if he has any design of reprinting his *Principia Mathematica*,” which certainly implies that it had not been then put into Gregory’s hands.

* Rigaud, p. 105.

† *Cotes Correspondence*, p. xiii.

‡ Rigaud, Appendix, p. 80.

It may be that it was at this time that David Gregory obtained from Newton those results in the lunar theory which Gregory published in the summer of 1702 in his *Astronomiæ Physicæ et Geometricæ Elementa*, p. 336 *et seq.*, under the title *Lunæ Theoria Newtoniana*. An English translation of the *Lunæ Theoria* was issued at London a few weeks later, in August, 1702; it was reprinted by John Harris in his *Lexicon Technicum*, 1704, and in the *Miscellanea Curiosa*, London, 1705 (edition 1708, vol. i. p. 268). Gregory's visit to Cambridge in 1701, for which he prepared his memoranda of May 21, seems to have been largely due to his desire to consult Newton on various points in the preparation of this book.

David Gregory died in October, 1708, but the idea of asking him to edit the second edition (if ever entertained) had been by that time abandoned, for we have a letter* from Bentley, dated June 10 of that year, from which it appears that Newton had then agreed to allow Bentley to act as editor, and had given him a corrected copy of the work from which to work. Bentley had even begun to print, and had bought the necessary paper; but for some reason, of which we now know nothing, abandoned the work—possibly because he found the task involved a greater knowledge of mathematics than he possessed.

Bentley, having given up the work himself, approached Cotes, and obtained his consent to act as editor, to which arrangement Newton agreed at an interview † with Bentley on May 21, 1709. Newton asked that Cotes should call on him in London, when he said he would at once hand over one part of the book corrected for press. Cotes called, but Newton seems to have been reluctant to part with his corrected copy, to which he wanted to make some further additions; however, in the middle of July he told Cotes that he would send it in another fortnight. As it did not arrive, Cotes wrote on Aug.

* Brewster, vol. ii. pp. 188–190.

† *Cotes Correspondence*, Letter i.

18 to remind Newton of his promise. Finally, probably towards the end of September, a revised copy of the first 320 pages was sent to Cotes by Whiston.

By April 15, 1710, about half the work (pp. 1-224 of the second edition) was printed, but all save one letter of the correspondence of this time concerning it is lost. The bulk of the subsequent correspondence between Newton, Cotes, and Bentley relating to the work is extant, and is reprinted* in the volume to which I have so often given references. The whole story is told there with a fulness of detail which renders it unnecessary for me to do more than refer the reader to it.

It will be sufficient perhaps to say that Newton, who at first had been somewhat indifferent, gradually became keenly interested in the work, and wrote "almost every post about it." The printing of the latter half of the book was rather slow, and it was not finally issued till July, 1713. The whole of the wood-blocks were cut afresh for it. It would seem that the price † of a copy in quires (unbound) was 15s. ; while that of a copy bound ‡ varied from 18s. (the sum paid by Flamsteed) to 21s. (the sum mentioned by Ch. Morgan); probably the number § of copies struck off was 750, of which 200 were sent to France and Holland, "though at great abatement." The whole profits of the edition were taken by Bentley, who had a keen eye for business in the matter; apparently Cotes received no remuneration for his work except 12 copies of the book; while even the cost of some corrections (in a revise of a little more than a sheet, which necessitated its being re-set) was charged to Newton||. In fact, Bentley, though doubtless anxious that the work should be published, treated the

* See below, chapter viii., C, pp. 174-175.

† Letter from Bentley to Newton, July 1, 1713, quoted by Brewster, vol. ii. p. 194.

‡ *Cotes Correspondence*, p. 159, note.

§ *Ibid.*, Letter lxxi.

|| *Ibid.*, Letter lxxviii.

publication as a commercial speculation* of his own; and he even went so far as (unknown to Newton and Halley) to alter the verses which the latter had prefixed to the first edition—an action quite indefensible. The most important of the changes introduced into the second edition have been already mentioned (see above, p. 74).

The history of the preparation of the third edition may be treated very briefly. Some (perhaps all) of Newton's manuscript corrections of the second edition, and additions to it, are in the Portsmouth Collection; and if a critical edition of the text of the *Principia* should be in the future issued, they will no doubt be there described.

In 1724 these materials were put into the hands of Henry Pemberton, with a view to the issue of a third edition. Newton himself was then over eighty years of age, and he left the revision largely to Pemberton. At the same time he seems to have answered all the questions directly addressed to him. Pemberton † speaks both of personal interviews and of letters passing between them. Of the former no record exists; of the latter, twenty-three letters from Pemberton to Newton—of which those that are dated are between February, 1724, and February, 1726—and seven sheets of queries by Pemberton, all relating to the preparation of the third edition of the *Principia*, are in the Portsmouth Collection‡. Unfortunately Newton's replies are lost§, but the letters are interesting even as they stand, and if Newton's complete scientific correspondence should be ever published, I hope these letters may be added to it. New blocks were again cut for all the diagrams.

* Conduitt says he asked Newton "how he came to let Bentley print his *Principia*, which he did not understand—'Why,' said he, 'he was covetous, and I let him do it to get money.'"—Conduitt's MS., quoted by Brewster, vol. i. p. 274.

† Preface to Pemberton's *View of Newton's Philosophy*, London, 1728.

‡ Section vi. division xii.

§ See below, p. 175.

In February or March, 1726, the third edition was issued, with a preface by Newton, dated Jan. 12, in which he mentions some of the additions; the most important of these have been already enumerated (see above, p. 75). The verses by Halley were restored almost to their original form, and the scholium on fluxions was rewritten. Perhaps I should add that Newton sent Pemberton two hundred guineas as an acknowledgment of his trouble in revising the work, and allowed him to take the profits of the edition.

The following editions of the complete *Principia* have been issued: (i) The original edition, London, 1686. (ii) The second edition, edited by R. Cotes, London, 1713. (iii) Reprint of the second edition, Amsterdam, 1714. (iv) Another reprint of the second edition, with the addition of some tracts, Amsterdam, 1723. (v) The third edition, edited by H. Pemberton, London, 1726. (vi) Reprint of the third edition, with a commentary by T. Le Seur and F. Jacquier, and a list of errata, 3 volumes, Geneva, 1739-40-42. (vii) The same reprinted, 3 volumes, Colonia Allobrogum (*i.e.* Geneva), 1760. (viii) The third edition, with a commentary and notes by J. Tessanek, Prague, 2 volumes, 1780-85. (ix) The third edition reprinted in S. Horsley's edition of Newton's collected works, vol. ii., 1779, and vol. iii., 1782. (x) The third edition, with errors corrected by J. M. F. Wright, Glasgow, 4 volumes, 1822. (xi) Reprint of the third edition by Sir Wm. Thomson and H. Blackburn, Glasgow, 1871.

Besides the above, the following editions of an English translation by A. Motte have been issued: (i) Motte's original edition, London, 2 volumes, 1729. (ii) Second edition of the above, together with a translation of the *System of the World* (which had been published in 1728) and addenda, edited by W. Davis, London, 3 volumes, 1803. (iii) Third edition of both the above, said to be revised, and with a life of Newton by N. W. Chittenden, New York, 1846 (or perhaps 1848). (iv) Reissue of this third edition, unaltered save for a new title-

page, New York, 1850. The above translations are too literal to be altogether satisfactory.

A French translation of the third edition of the *Principia*, with a commentary attributed to the Marquise Du Chastellet but believed to have been inspired by Clairaut, was published in 2 volumes, Paris, 1759.

A German translation of the third edition, edited by J. P. Wolfers, and with an appendix on similar and more recent investigations, was published at Berlin in 1872.

An immense number of commentaries have appeared on the *Principia* or on parts of it, but it would be difficult, and would serve no useful purpose, to compile a list of these.

CHAPTER VIII.

APPENDICES.

THE following documents and memoranda are printed, as appendices to the foregoing essay.

A. *Correspondence between Hooke and Newton, 1679-1680, and Memoranda relating thereto.*

1. Hooke to Newton, Nov. 24, 1679.
2. Newton to Hooke, Nov. 28, 1679.
3. [Hooke to Newton, Dec. 9, 1679.]
4. [Newton to Hooke, Dec. —, 1679.]
5. Hooke to Newton, Jan. 6, 1680.
6. Hooke to Newton, Jan. 17, 1680.
7. Newton to Hooke, Dec. 3, 1680.
8. Hooke's comments on this correspondence.

B. *Correspondence between Halley and Newton, 1686-1687.*

1. Halley to Newton, May 22, 1686.
2. Newton to Halley, May 27, 1686.
3. Halley to Newton, June 7, 1686.
4. Newton to Halley, June 20, 1686.
5. Halley to Newton, June 29, 1686.
6. Newton to Halley, July 14, 1686.
7. Newton to Halley, July 27, 1686.
8. [Newton to Halley, Aug. 20, 1686.]
9. Halley to Newton, Oct. 14, 1686.
10. Newton to Halley, Oct. 18, 1686.

11. Newton to Halley, Feb. 18, 1687.
12. Halley to Newton, Feb. 24, 1687.
13. Newton to Halley, March 1, 1687.
14. Halley to Newton, March 7, 1687.
15. Halley to Newton, March 14, 1687.
16. Halley to Newton, April 5, 1687.
17. Halley to Newton, July 5, 1687.

C. *Memoranda on the correspondence concerning the production of the second edition of the Principia.*

D. *Memoranda on the correspondence concerning the production of the third edition of the Principia.*

A. CORRESPONDENCE BETWEEN HOOKE AND NEWTON, 1679-1680, AND MEMORANDA RELATING THERETO.

Of the letters here given, those numbered 1, 5 are copied from the rough drafts in Hooke's handwriting, and those numbered 2, 6 are from the originals; these (together with Hooke's comments, which are numbered 8) are in the Library of Trinity College, Cambridge. The quotation from the letter numbered 7 is taken from the *Cotes Correspondence*, Appendix, number xviii. The letters numbered 3 and 4 are known to have been written, but they have never been published, and it is possible that no copies of them are now extant. In printing these documents I have divided them into paragraphs, systematised the use of capitals, added punctuation, and in a few cases have written contractions at length.

A. 1. *Hooke to Newton, Nov. 24, 1679.*

SIR,

Finding by our registers that you were pledged to correspond with Mr. Oldenburg, and having also had the happiness of receiving some letters from you my self make me presume to trouble you with this present scribble—Dr. Grew's more urgent occasions having made him decline the holding correspondence. And the Society hath devolved it on me. I hope therefore that you will please to continue your former

favours to the Society by communicating what shall occur to you that is philosophical, and for returne I shall be sure to acquaint you with what we shall receive considerable from other parts or find out new here. And you may be assured that whatever shall be soe communicated shall be noe otherwise further imparted or disposed of than you yourself shall praescribe. I am not ignorant that both heretofore, and not long since also, there have been some who have indeavoured to misrepresent me to you, and possibly they or others have not been wanting to doe the like to me, but difference in opinion if such there be (especially in philosophical matters where interest hath little concerne) me thinks should not be the occasion of enmity—'tis not with me I am sure. For my part I shall take it as a great favour if you shall please to communicate by letter your objections against any hypothesis or opinion of mine; and particularly if you will let me know your thoughts of that of compounding the celestiall motions of the planetts of a direct motion by the tangent and an attractive motion towards the centrall body, or what objections you have against my hypothesis of the lawes or causes of springynesse.

I have lately received from Paris a new hypothesis invented by Mo^r Mallement de Messanges, D^r of the Sorbon, who desires much to have what can be objected against it. He supposes then a center of this our vortex about which all the primary planets move in perfect circles, each of them in his own aequall spaces in aequall times. The next to it he places the Sun; and about the Sun, Mercury as a satellit; the next Venus; the next the earth, about which the Moon as a satellit; then Mars; then Jupiter and his satellits; and Saturn with his. He supposes the Sun to make its revolution in about half the time the earth makes its, and the plaine of it to be inclined to the plaine of the ecliptick as much as the trepidation requires. He is not precise in defining any thing, as reserving a liberty to himself to help him out where objections might stick.

I am informed likewise from Paris that they are there about another work, viz.^t of setling the longitude and latitude of the most considerable places: the former of those by the eclipses of the satellites of Jupiter. M^r Picart and De la Hire travell, and Mo^r Cassini and Romer observe at Paris. They have already found that Brest in Britaigne is 18 leagues nearer Paris than all the mappes make it. I have written to a correspondent in Deavonshire to see if we can doe somewhat of that kind here, and I should be glad if by perpendicular observations, we could determine the difference of latitude between London and Cambridge. If you know of any one that will observe at Cambridge, I will procure it to be done here very exactly.

M^r Collins shewed me a book he received from Paris of De la Hire containing first a new method of the conick sections and secondly a treatise De locis solidis. I have not perused the book but M^r Collins commends it. M^r Flamstead by some late perpendicular observations hath confirmed the paralax of the orb of the earth.

But I fear I have too much trespassed, and therefore to put an end to your further trouble I shall subscribe my self, Sir,

Your very humble Servant,

R. H.

Gresham Colledge, Nov. 24, 1679.

A. 2. *Newton to Hooke, Nov. 28, 1679.*

SIR,

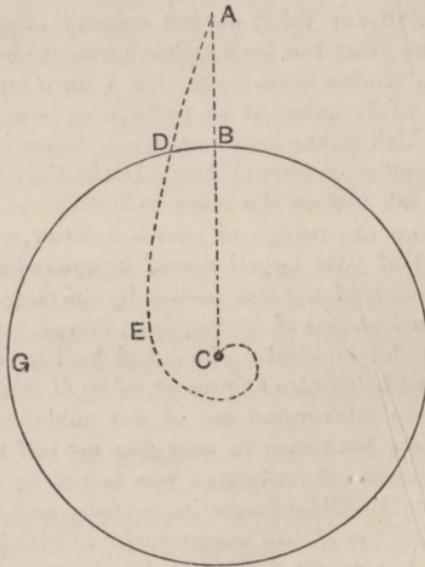
I cannot but acknowledge my self every way by the kindness of your letter tempted to concur with your desires in a philosophical correspondence. And heartily sorry I am that I am at present unfurnished with matter answerable to your expectations—for I have been this last half year in Lincolnshire cumbered with concerns amongst my relations till yesterday when I returned hither; so that I have had no time to entertain philosophical meditations, or so much as to study or mind any thing els but country affairs. And before that, I had for some years last been endeavouring to bend myself from philosophy to other studies in so much that I have long grutched the time spent in that study unless it be perhaps at idle hours sometimes for a diversion; which makes me almost wholly unacquainted with what philosophers at London or abroad have of late been employed about. And perhaps you will incline the more to believe me when I tell you that I did not, before the receipt of your last letter, so much as heare (that I remember) of your hypothesis of compounding the celestial motions of the planets, of a direct motion by the tangent to the curve, and of the laws and causes of springyness, though these no doubt are well known to the philosophical world. And having thus shook hands with philosophy and being also at present taken of with other business, I hope it will not be interpreted out of any unkindness to you or the R. Society that I am backward in engaging my self in these matters, though formerly I must acknowledge I was moved by other reasons to decline, as much as M^r Oldenburg's importunity and ways to engage me in disputes would permit, all correspondence with him about them. However I cannot but return my hearty thanks for your thinking me worthy of so noble a commerce and in order thereto francly imparting to me several things in your letter.

As to the hypothesis of Mons^r Mallement, though it should not be true yet if it would answer to phaenomena it would be very valuable by reason of its simplicity. But how the orbits of all the primary planets but Mercury can be reduced to so many concentric circles through each of which the planet moves equal spaces in equal times (for that's the hypothesis if I mistake not your description) I do not yet understand. The readiest way to convince the world of this truth would be I conceive to set forth first in some two of the planets, suppose Mars and the earth, a specimen thereof stated and determined in numbers.

I know no body in the University addicted to making astronomical observations: and my own short sightedness and tenderness of health makes me something unfit. Yet it's likely I may sometime this winter when I have more leisure than at present attempt what you propound for determining the difference of latitude between Cambridge and London.

I am glad to hear that so considerable a discovery as you made of the earth's annual parallax is seconded by M^r Flamstead's observations.

In requital of this advertisement I shall communicate to you a fancy of my own about discovering the earth's diurnal motion. In order thereto I will consider the earth's diurnal motion alone, without



the annual, that having little influence on the experiment I shall here propound. Suppose then BDG represents the globe of the earth

carried round once a day about its center C from west to east according to the order of the letters BDG ; and let A be a heavy body suspended in the air, and moving round with the earth so as perpetually to hang over the same point thereof B . Then imagine this body A [the MS. has B , which is obviously a slip] let fall, and its gravity will give it a new motion towards the center of the earth without diminishing the old one from west to east. Whence the motion of this body from west to east, by reason that before it fell it was more distant from the center of the earth than the parts of the earth at which it arrives in its fall, will be greater than the motion from west to east of the parts of the earth at which the body arrives in its fall; and therefore it will not descend the perpendicular AC , but outrunning the parts of the earth will shoot forward to the east side of the perpendicular describing in its fall a spiral line $ADEC$, quite contrary to the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular. The advance of the body from the perpendicular eastward will in a descent of but 20 or 30 yards be very small, and yet I am apt to think it may be enough to determine the matter of fact. Suppose then in a very calm day a pistol bullet were let down by a silk line from the top of a high building or well, the line going through a small hole made in a plate of brass or tinn fastened to the top of the building or well, and that the bullet when let down almost to the bottom were settled in water so as to cease from swinging, and then let down further on an edge of steel lying north and south to try if the bullet in settling thereon will almost stand in aequilibrio but yet with some small propensity (the smaller the better) decline to the west side of the steel as often as it is so let down thereon. The steel being so placed underneath, suppose the bullet be then drawn up to the top and let fall by cutting clipping or burning the line of silk, and if it fall constantly on the east side of the steel it will argue the diurnall motion of the earth. But what the event will be I know not, having never attempted to try it. If any body would think this worth their trial, the best way in my opinion would be to try it in a high church or wide steeple, the windows being first well stopped; for in a narrow well the bullet possibly may be apt to receive a ply from the straitened air neare the sides of the well, if in its fall it come nearer to one side than to another. It would be convenient also that the water into which the bullet falls be a yard or two deep or more, partly that the bullet may fall more gently on the steel, partly that the motion which it has from west to east at its entering into the water may by means of the longer time of descent through the water, carry it on further eastward and so make the experiment more manifest.

If I were not so unhappy as to be unacquainted with your hypothesis abovementioned (as I am with almost all things which have of late been done or attempted in philosophy) I should so far comply with your desire as to send you what objections I could think of against them, if I could think of any. And on the other hand I could with pleasure heare and answer any objections made against any notions of mine in a transient discourse for a divertisement. But yet my affection to philosophy being worn out, so that I am almost as little concerned about it as one tradesman uses to be about another man's trade or a country man about learning, I must acknowledge my self avers from spending that time in writing about it which I think I can spend otherwise more to my own content and the good of others: and I hope neither you nor any body els will blame me for this aversness. To let you see that it is not out of any shyness, reservedness, or distrust that I have of late and still do decline phi[losophi]call commerce but only out of my applying my self to other things, I have communicated to you the notion above set down (such as it is) concerning the descent of heavy bodies for proving the motion of the earth; and shall be as ready to communicate in oral discourse anything I know, if it shall ever be my happiness to have familiar convers frequently with you. And possibly if any thing usefull to mankind occurs to me I may sometimes impart it to you by letter. So wishing you all happiness and success in your endeavours, I rest,

Sir,

Your humble Servant

to command

IS. NEWTON.

Mr. Cock has cast two pieces of metal for me in order to a further attempt about the reflecting tube which I was the last year inclined to by the instigation of some of our ffellows. If I do any thing you may expect to hear from me. But I doubt the tool on which they were to be ground, being in the keeping of one lately deceased who was to have wrought the metals, is lost.

Cambridge.

Novemb. 28, 1679.

Endorsed. For his ever Hon^d ffriend M^r Robert Hook at his Lodgings in Gresham College in London.

The above is copied from Newton's original holograph letter. Across the beginning of it Hooke has written "Hooke's

“hypothesis here hinted at,” and before the last paragraph he has inserted the words “he here pretends he knew not H’s “hypothesis.”

The letter was read to the Royal Society at their meeting on Dec. 4, 1679, as appears by the following minute :

Mr. Hooke produced and read a letter of Mr. Newton to himself, dated 28th November, 1679, containing his sentiments of Mone. Mallemont’s new hypothesis of the heavens ; and also suggesting an experiment, whereby to try, whether the earth moves with a diurnal motion or not, viz. by the falling of a body from a considerable hight, which, he alledged, must fall to the eastward of the perpendicular, if the earth moved.

This proposal of Mr. Newton was highly approved of by the Society ; and it was desired, that it might be tried as soon as could be with convenience.

Sir Christopher Wren supposed, that there might be something of this kind tried by shooting a bullet upwards at a certain angle from the perpendicular round every way, thereby to see whether the bullets so shot would all fall in a perfect circle round the place, where the barrell was placed. This barrell he desired might be fixed in a frame upon a plain foot, and that foot placed upon a true plain every way, and the mouth of the gun be almost in the same point over the plain which way soever shot.

Mr. Flamstead hereupon alledged, that it was an observation of the gunners, that to make a ball fall into the mouth of the piece, it must be shot at eighty-seven degrees ; and that he knew the reason thereof ; and that it agreed with his theory : and that a ball shot perpendicularly would never fall perpendicularly : and he mentioned the recoiling of a perpendicular jet of waters. But this was conceived to arise from some mistake of the gunners, in not well taking notice of all circumstances ; since a body shot perpendicularly would also descend perpendicularly ; and a body shot at eighty-seven degrees would fall considerably distant from the place where it was shot. (Birch, *History of the Royal Society of London*, London, 1757, vol. iii. pp. 512-513.)

A. 3. *Hooke to Newton, Dec. 9, 1679.*

Hooke replied on Dec. 9, 1679, to Newton’s letter of Nov. 28, 1679. This reply is lost, though a quotation from it is printed below (see p. 152). It was, however, read to the

Royal Society at their meeting on Dec. 11, 1679, for in their minutes it is stated that

Upon the mentioning of Mr. Newton's letter, and the experiment proposed in it, Mr. Hooke read his answer to him upon that subject, wherein he explained what the 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 excentric elliptoid [*sic*], supposing no resistance in the medium : but supposing a resistance, it would be an excentric ellipti-spiral, which, after many revolutions, would rest at last in the centre : that the fall of the heavy body would not be directly east, as Mr. Newton supposed ; but to the south-east, and more to the south than the east. It was desired, that what was tryable in this experiment might be done with the first opportunity. (Birch, vol. iii. p. 516.)

. A. 4. *Newton to Hooke, Dec. —, 1679.*

We know from Hooke's letter of Jan. 6, 1680 (A. 5), and from Newton's letter of June 20, 1686 (B. 4), as well as from the minutes of the Royal Society, that Newton replied to Hooke's letter of Dec. 9, for at the meeting of the Society on Dec. 18,

Mr. Hooke read his answer to Mr. Newton's former letter ; as also another letter, which he had received from Mr. Newton, containing his farther thoughts and examinations of what had been propounded by Mr. Hooke.

Mr. Hooke gave also an account, that he had made three trials of the experiment propounded by Mr. Newton, and had found the ball in every one of the said experiments fall to the south-east of the perpendicular point, found by the same ball hanging perpendicular. But the distance of it from the perpendicular point being not always the same, and the experiment having been made without doors, in the open air, nothing of certainty could be concluded from it. But he alledged, that he designed to make a trial of it within doors, where there would be less motion of the air ; and he hoped to be able to do it before the next meeting of the Society. (Birch, vol. iii. p. 519.)

Unfortunately this reply from Newton is lost, and I know nothing of its contents, except so far as they may be inferred

from the letter next given (A. 5), and from the letter of May 27, 1686 (B. 2).

A. 5. *Hooke to Newton, Jan. 6, 1680.*

SIR,

Your calculation of the curve described by a body attracted by an aequal power at all distances from the center, such as that of a ball rolling in an inverted concave cone, is right, and the two auges will not unite by about a third of a revolution; but my supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocal, and consequently that the velocity will be in a subduplicate [proportion] to the attraction, and consequently as Kepler supposes reciprocal to the distance: and that with such an attraction the auges will unite in the same part of the circle, and that the nearest point of the access to the center will be opposite to the furthest distant, which I conceive doth very intelligibly and truly make out all the appearances of the heavens. And therefore (though in truth I agree with you that the explicating the curve in which a body descending to the center of the earth would circumgyrate were a speculation of noe use yet) the finding out the propriety of a curve made by two such principles will be of great concerne to mankind because the invention of the longitude by the heavens is a necessary consequence of it, for the composition of two such motions I conceive will make out that of the moon. What I mentioned in my last concerning the descent within the body of the earth was but upon the supposall of such an attraction, not that I really believe there is such an attraction to the very center of the earth, but on the contrary I rather conceive that the more the body approaches the center the lesse will it be urged by the attraction, possibly somewhat like the gravitation on a pendulum or a body moved in a concave sphere where the power continually decreases the nearer the body inclines to a horizontal motion which it hath when perpendicular under the point of suspension or in the lowest point, and there the auges are almost opposite, and the nearest approach to the center is at about a quarter of a revolution. But in the celestiall motions the sun, earth, or centrall body are the cause of the attraction, and though they cannot be supposed mathematicall points yet they may be conceived as physicall, and the attraction at a considerable distance may be computed according to the former proportion as from the very center. This curve truly calculated will shew the error of those many lame shifts made use of by astronomers to approach the true motions of the planets with their tables. But of this more hereafter.

In the mean time I must acquaint you that I have (with as much care as I could) made 3 tryalls of the experiment of the falling body, in every of which the ball fell towards the south-east of the perpendicular, and that very considerably, the least being above a quarter of an inch, but because they were not all the same I know not which was true. What the reason of the variation was I know not, whether the unequall spherick figure of the iron ball, or the motion of the air, for they were made without doors, or the insensible vibration of the ball suspended by the thread before it was cut. But it being a very noble experiment I shall not leave it before I have made a prooffe free from objections, of which I will send you an account. If it doth succeed there will follow severall other consequences not less considerable—as, first, that all bodys will of consequence grow lighter the nearer they approach the aequinoctiall, the circular motion being swifter, and for the same reason the further a body is from the center the less will be its gravitation, not only upon the account of the decrease of the attractive power which I have a long time supposed, but upon the increase of the endeavour of recess. And this gives us another way to try whether the earth has a diurnall motion though much short of what you proposed. But that I may tell you somewhat of observation, Mr Halley, when he returned from S^t Helena, told me that his pendulum at the top of the hill went slower than at the bottom which he was much surpris'd at, and could not imagine a reason. But I presently told him that he had solv'd me a query I had long desired to be answered but wanted opportunity, and that was to know whether the gravity did actually decrease at a greater height from the center. To examine this decrease of attraction I have formerly made many experiments on Paule's steeple and Westminster Abby, but none that were fully satisfactory. This will spoyle the universall standard by the pendulum and the equality of pendulum clocks carry'd from one climate to another. And many other consequences will follow which would be too long to trouble you with at present, of which I long since gave the Society an account in writing upon the supposall of the decrease of gravity and the increase of the circular motion.

Noe more but that I am,

Sir, Your most humble servant,

R. H.

Grm. Coll.

Jan. 6, 1679 [*i.e.* 1680 in our current calendar].

At the meeting of the Royal Society on Jan. 8, 1680,

Hooke read the above letter, as appears from the following minute :

Mr. Hooke read another letter of his to Mr. Newton concerning some farther account of his theory of circular motion and attraction; as also several observations and deductions from that theory; as 1. That pendulum clocks must vary their velocity in several climates. 2. That this variation must also happen at different heights in the same climate: which last remark he confirmed by an observation of Mr. Halley at St. Helena; and 3. as a consequence of these, that a pendulum was unfit for an universal standard of measure. (Birch, vol. iv. p. 1.)

At the same meeting Hooke

Was desired to make his trials as soon as possible of Mr. Newton's experiment concerning the earth's diurnal motion. (Birch, vol. iv. p. 2.)

A. 6. *Hooke to Newton, Jan 17, 1680.*

SIR,

I gave you an account by my last of the 6th instant that by the tryalls I had made without doors your experiment succeeded very well. I can now assure you that by two tryalls since made in two severall places within doors it succeeded also. Soe that I am now persuaded the experiment is very certaine, and that it will prove a demonstration of the diurnall motion of the earth as you have very happily intimated.

It now remaines to know the proprietyes of a curve line (not circular nor concentricall) made by a centrall attractive power which makes the velocityes of descent from the tangent line or equal straight motion at all distances in a duplicate proportion to the distances reciprocally taken. I doubt not that by your excellent method you will easily find out what that curve must be, and its proprietyes, and suggest a physicall reason of this proportion. If you have had any time to consider of this matter, a word or two of your thoughts of it will be very gratefull to the Society (where it has been debated) And more particularly to,

Sir,

Your very humble Servant

R. HOOKE.

Gresham Coll.

Jan : 17, 1679 [*i.e.* 1680 in our current calendar].

For my much Honrd freind Mr Isaac Newton Lucasian Professor at his Chamber in Trinity Colledge in Cambridge.

On these experiments we read that at the meeting of the Royal Society on Jan. 22, 1680,

Mr. Hooke shewed the ball, that had been let fall from the height of 27 feet, and fell into a box full of tobacco pipe-clay, sticking in the clay, upon the surface of which were made lines crossing each other: which shewed the true perpendicular point indicated by the ball, when it hung suspended by a thread from the top, and how much the ball had varied from that perpendicular in its descent towards the South and East: and he explained the manner, how the same was performed in all particulars. It was desired, that this experiment might be made before a number of the Society, who might be witnesses of it before the next meeting. The time appointed was the Monday following at three in the afternoon. (Birch, vol. iv. p. 5.)

The result of these experiments agrees with theory, but the effect is so small, and the difficulty of making the experiment so considerable, that the coincidence may have been partly due to luck.

A. 7. *Newton to Hooke, Dec. 3, 1680.*

On Dec. 3, 1680, at the end of a letter to Hooke on another subject, Newton says :

For the trials you made of an experiment suggested by me about falling bodies, I am indebted to you thanks, which I thought to have returned by word of mouth, but not having yet the opportunity must be content to do it by letter. (The *Cotes Correspondence*, p. 264.)

A. 8. *Hooke's Comments on the above Correspondence.*

Hooke claimed that the above letters from him, together with some earlier memoirs, suggested to Newton the idea of centripetal forces varying as the inverse square of the distance. Newton indignantly denied this, and his opinion on the subject is given in his letters of May 27 and of June 20, 1686, printed below as B. 2 and B. 4. The following is taken from a manuscript which (I believe) is in Hooke's handwriting, wherein he states his view and the evidence for it. The manuscript is

not dated, and probably it was written later than 1686. Newton was not knighted till 1705, but the use of the prefix "Sir," though unusual, is not unprecedented.

A True state of the Case and Controversy between Sr Isaak Newton and Dr. Robert Hooke as to the Priority of that Noble Hypothesis of Motion of the Planets about the sun as their Centre.

I. In the year 1666 May the 23^d, there was read a paper of Mr H's explicating the inflection of a direct motion into a curve by a supervening attractive principle. The discourse contained therein is an introduction to an experiment to shew that circular motion is compounded of an endeavour by a direct motion by the tangent and of another endeavour tending to the centre. To which purpose there was fastened to the rooffe a pendule with a Ligni Vitae ball on its end; and it was found that if the impetus of the endeavour by the tangent at the first setting out was stronger than the endeavour to the centre there was generated such an ellipticall motion whose longest diameter was parallel to the direct endeavour of the body at the first impulse, if both were equal there was made a perfect circular motion. There was also made another experiment by fastening another pendulous body by a short string on the lower part of the wire by which the greater weight was suspended that it might freely make a circular or elliptical motion round the bigger whilst the bigger moved circularly or elliptically about the first center. The intention wherof was to explicate the manner of the moon's motion about the earth, it appearing evidently thereby that neither the bigger ball representing the earth nor the less which represented the Moon were moved in so perfect a circle or ellipsis as otherwise they would have been if either had been suspended or moved singly, but that a certain point which seemed to be the center of gravity of the two bodys (howsoever posited and considered as one) seemed to be regularly moved in such a circle or ellipsis, the two balls having other pecul[i]ar motions in small epicycles about the said point.

II. In the year 1674, he publisht his *Attempt to prove the Motion of the Earth*, where, at page 27, he says thus: "I shall only hint for the present that I have in some of my foregoing observations discovered some new motions even in the earth it selfe, which perhaps were not dreamt of before, which I shall hereafter more at large describe when farther tryalls have more fully confirmed and completed these beginnings. 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 common rules of mechanical motions. This depends upon three suppositions. First, that all celestiall bodys whatever have an attraction or a gravitating power towards their own centers 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 coelestiall bodys that are within the sphere of their activity; and consequently that not only the sun and moon have an influence upon the body and motion of the earth, and the earth upon them, but that Mercury, Mars, Saturn, and Jupiter by their attractive powers have a considerable influence upon its motion, as in the same manner the corresponding attractive power of the earth hath a considerable influence upon every one of their motions also. The second supposition is this, that all bodys whatsoever that are put into a direct and simple motion will so continue to move forward in a strait line, till they are by some other effectuall powers deflected and bent into a motion describing a circle, ellipsis, or some other more compounded curve line. The third supposition is that these attractive powers are so much the more powerfull in operating by how much the nearer the body wrought upon is to their own centers. Now what these severall degrees are I have not yet experimentally verified: but it is a notion which, if fully prosecuted as it ought to be, will mightily assist the astronomer to reduce all the celestial motions to a certain rule which I doubt will never be done true without it. He that understands the nature 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, etc. This I dare promise the undertaker, that he will find all the great motions in the world to be influenced by this principle, and that the true understanding thereof will be the true perfection of astronomy."

[III.] Nov. the 24th, 1679, Dr. Hooke invited Newton to a friendly philosophicall correspondence. In which letter there are these words:— for my own part I shall take it as a great favour if you shall please to communicate the objections against any hypothesis or opi[ni]on of mine, particularly if you will let me know your thoughts of that of compounding the celestiall motions of the planets of a direct motion by the tangent and an attractive motion towards the centrall body, etc.

In answer to this Newton pretends he knew not Hooke's hypothesis, as by his answer to the former dated Nov. 28, 1679; and in the same letter says his affection to philosophicall studys was quite worn out.

Dec. 9, 1679, in a letter to Newton, Hook has these words upon account of an experiment about the falling of [a] ball from a considerable height:—I could add many other considerations consonant to my

theory of circular motions compounded by a direct motion and an attractive one to the center, etc.

Jan. 6, 167 $\frac{9}{10}$, Hook has these words:—In the celestial motions the sun, earth, or central body are the cause of the attraction, and though they cannot be supposed mathematical points yet they may be supposed as physical, and the attraction at a considerable distance may be computed according to the former proportion as from the center (that is, as is before in the same letter, thus) my supposition is that the attraction always is in a duplicate proportion to the distance from the center reciprocalall, and consequently that the velocity will be in a sub-duplicate proportion to the attraction, etc. Note, in the same letter H. mentions the gravitation to be less under the aequinoctial, and consequently the prolated spheroid figure of the earth.

In a letter from Hook, Jan. 17, 1679, are these words:—It now remains to know the propriety of a curve not circular nor concentricall made by a centrall attractive power which makes the velocity of descent from the tangent line or equall strait motion at all distances in a duplicate proportion to the distances reciprocalall taken, etc.

B. CORRESPONDENCE BETWEEN HALLEY AND NEWTON, 1686–1687.

Of the seventeen (or more) letters with reference to the *Principia* which passed between Halley and Newton in 1686–7, sixteen are extant. The eight letters here numbered 1, 4, 5, 6, 7, 10, 11, 13 were printed by Rigaud in his essay. The seven letters numbered 3, 9, 12, 14, 15, 16, 17 were printed in Brewster's *Life of Newton*. The letter numbered 2 is now printed for the first time; it is taken from a copy in Hooke's handwriting, which is preserved in the Library of Trinity College, Cambridge. The nine letters numbered 1, 3, 5, 9, 12, 14, 15, 16, 17 are (save for a few corrections of punctuation) printed here in their original form as shown in the Portsmouth copies which have been collated with the originals. The remaining six letters numbered 4, 6, 7, 10, 11, 13 are reprinted from the works of Rigaud and Brewster; the originals

of those numbered 4, 10, 11, 13 are preserved in the archives of the Royal Society.

B. 1. *Halley to Newton, May 22, 1686.*

May 22, 1686.

SIR,

Your incomparable treatise, intituled *Philosophiae 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 councell should be summon'd to consider about the printing thereof; but by reason of the presidents attendance upon the King, and the absence of our vice-presidents, whom the good weather had drawn out of town, there has not since been any authentick councell 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 delayd, resolved to print it at their own charge in a large quarto of a fair tre [*i.e.* 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 signifie as your desire shall be punctually observed.

There is one thing more that I ought to informe you of, *viz.* that Mr. Hook has some pretensions upon the invention of *y^e* 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. Hook seems to expect you should make some mention of him in a preface, which it is possible you may see reason to prefix. I must beg your pardon, that it is I that send you this account; but I thought it my duty to let you know it, that so you may act accordingly; being in myself fully satisfied, that nothing but the greatest candour imaginable is to be expected from a person, who of all men has the least need to borrow reputation.

When I shall have received your directions, the printing shall be pushed on with all expedition, which therefore I entreat you to send me

as soon as may be. You may please to direct to me, to be left with Mr. Hunt at Gresham College, and your tre will come to the hands of,

S^r,

Your most affectionate humble Serv^t.

EDM. HALLEY.

To his honoured friend M^r Isaac Newton,
Professor of Mathematicks in y^e University of Cambridg.

B. 2. *Newton to Halley, May 27, 1686.*

I thank you for what you write concerning M^r Hooke, for I desire that a good understanding may be kept between us. In the papers in your hands there is not one proposition to which he can pretend, and soe I had noe proper occasion of mentioning him there. In those behind where I state the systeme of the world I mention him and others. But now we are upon this businesse, I desire it may be understood. The summe of what past between M^r Hooke and me (to the best of my remembrance) was this. He solliciting me for some philosophical communications or other I sent him this notion, that a falling body ought by reason of the earth's diurnall motion to advance eastward and not fall to the west as the vulgar opinion is. And in the scheme wherein I explained this I carelessly described the descent of the falling body in a spirall to the center of the earth: which is true in a resisting medium, such as our air is. M^r Hooke replyed it would not descend to the center but at a certaine limit returne upwards againe. I then took the simplest case for computation, which was that of gravity uniform in a medium not resisting—imagining he had learned the limit from some computation, and for that end had considered the simplest case first. And in this case I granted what he contended for, and stated the limit as nearly as I could. He replyed that gravity was not uniform but increased in descent to the center in a reciprocall duplicate proportion of the distance from it, and thus the limit would be otherwise than I had stated it, namely, at the end of every intire revolution, and added that according to this duplicate proportion the motions of the planets might be explained and their orbs defined. This is the summe of what I remember. If there was any thing more materiall or any thing otherwise I desire M^r Hooke would help my memory. Further that I remember about 9 years since Sir Christopher Wren, upon a visit D^r Done and I gave him at his lodgings, discoursed of this problem of determining the planetary motions upon philosophical principles. This was about a year or two before I received M^r Hooke's

letters. You are acquainted with Sir Christopher. Pray know where and whence he first learnt the decrease of the force in a duplicate ratio of the distance from the center.

Sir, I am your most affectionate and humble servant, I. N.
May 27, 1686.

B. 3. *Halley to Newton, June 7, 1686.*

London, June 7, 1686.

SR,

I here send you a proof of the first sheet of your Book, which we think to print on this paper, and in this Character; if you have any objection, it shall be altered: and if you approve it, we will proceed; and care shall be taken that it shall not be published before the end of Michaelmass term, since you desire it. I hope you will please to bestow the second part, or what remains of this, upon us as soon as you shall have finished it, for the application of this Mathematical part to the system of the world, is what will render it acceptable to all Naturalists, as well as Mathematicians; and much advance the sale of y^e book. Pray, please to revise this proof, and send it me up with your answer. I have already corrected it, but cannot say I have spied all the faults. When it has past your eye, I doubt not but it will be clear from errata. The printer begs your excuse of the Diphthongs, which are of a character a little bigger, but he has some a casting of the just size. This sheet being a proof is not so clear as it ought to be; but the letter is new, and I have seen a book of a very fair character, which was the last thing printed from this set of letter; so that I hope the Edition may in that particular be to your satisfaction. I am, Sr,

Your most affectionate humble servt,

E. HALLEY.

Please to send by the coach, directed to me, to be left with Mr. Hunt, at Gresham College.

To his honoured Friend,

MR. ISAAC NEWTON,

at his Chamber in TRINITY COLL.

CAMBRIDGE.

B. 4. *Newton to Halley, June 20, 1686.*

SIR,

In order to let you know the case between Mr. Hooke and me, I gave you an account of what passed 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 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 farther 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 book. 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 terrae 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 deter-

mining a problem about the moon's phase, and putting a limit to the sun's parallax, which shows 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 though 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; which is all at present from

your affectionate friend,
and humble servant,

IS. NEWTON.

Cambridge, June 20, 1686.

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 priut, 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 me of 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 show 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 phaenomena 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 ?

B. 5. *Halley to Newton, June 29, 1686.*

SR,

I am heartily sorry, that in this matter, wherein all mankind ought to acknowledg 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 shee, but your rivalls envying your happiness that endeavour to disturb your quiet enjoyment, which when you consider, I hope you will see cause to alter your former resolution of supressing your third book, there being nothing which you can have compiled therein, which the learned world will not be concerned to have concealed. Those gentlemen of the Society, to whom I have communicated it, are very much troubled at it, and that this unlucky business should have hapned to give you trouble, having a just sentiment of the author thereof. According to your desire in your former, I waited upon Sr Christopher Wren, to inquire of him, if he had the first notion of the recipocall 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 imprest motion; but that at length he gave over, not finding the means of doing it. Since which time Mr. Hook had frequently told him that he had done it, and attempted to make it out to him, but that he never satisfied him that his demonstrations were cogent. And this I know to be true, that in January 8 $\frac{1}{2}$, I, having from the consideration of the sesquialter proportion of Kepler, concluded that the centripetall force decreased in the proportion of the squares of the distances recipocally, came one Wednesday to town, where I met with Sr Christ. Wrenn and Mr. Hook, and falling in discourse about it, Mr. Hook affirmed, that upon that principle all the laws of the celestiaall motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts; and Sr Christopher, to encourage the inquiry, s^d that he would give Mr. Hook 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^s. Mr. Hook then s^d that he had it, but he would conceale it for some time, that others triing and failing might know how to value it, when he should make it publick; however I remember Sr Christ. was little satisfied that he could do it, and tho Mr. Hook then promised to shew it him, I do not yet find that in that particular he has been as good as his word. The August following when I did myself the honour to viset you, I then learnt 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 jour[ne]y down to Cambridg, on purpose to confer with you about it, since which time it has been enterd upon the Register Books of the Society. As all this past Mr. Hook 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 conceald it, the reason he told Sr Christopher and I, now ceasing. But now he sais that this is but one small part of an excellent system of nature, which he has conceived, but has not yet compleatly 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. Hook's claiming this discovery, I fear it has been represented in worse colours than it ought; for he neither made publick application to the Society for justice, nor pretended you had all from him. The truth is this. Sr John Hoskins, his particular frie[n]d being in the chair, when Dr. Vincent presented your book, the Dr 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. Hook offence, that Sr John did not, at that time, make mention of what he had, as he s^d, 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 coffe-house, Mr. Hook 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

the 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, 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.

B. 7. *Newton to Halley, July 27, 1686.*

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 Monmouth, 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 some time 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, No. 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 everywhere 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,

Is. NEWTON.

July 27, 1686.

B. 8. *Newton to Halley, Aug. 20, 1686.*

This letter is lost, but Dr. Edleston believes that it related to the attraction of a spheroid on a particle on its axis. (*The Cotes Correspondence*, pp. xxx, lvii.)

B. 9. *Halley to Newton, Oct. 14, 1686.*

London, October 14, 1686.

SR,

By reason you are desirous that your book should not be publick before Hillary Term, the impression has not been expedited as it might have been; but I hope that it is the more correct for proceeding so slow. I have sent you by the coach which goes from hence to-morrow morning all the sheets that are done, desiring you would please to mark all the errata you shall find, that

so if there be any material one, the reader may be advertised thereof, but this at your leasure. At present I more immediately want to be informed concerning your geometrical effecton of the problem XXIII, as much as relates to the 63 figure, for upon triall (there being no demonstration annexd) there seems to be some mistake committed: wherfore I entreat you would please to send me, revised by your self, those few lines that relate thereto, and if it be not too much trouble, be prevailed upon to subjoyn something of the Demonstration. In your transmutation of figures according to the 22th lemma, which you use in the 2 following problems, to me it seems that the manner of transmuting a trapezium into a parallelogram needs some further explication; I have printed it as you sent it, but I pray you please a little farther to describe by an example the manner of doing it, for I am not perfectly master of it, a short hint will suffice. Pray defer the answer hereto as little as may stand with your convenience, for we are now within a sheet of the 23d problem, and shall want your amendments, if there be occasion for them. If there be any service I can do you here in town pray command Sr,

Your most affectionate humble servant,

To his honoured Friend,

EDM. HALLEY.

MR. ISAAC NEWTON,

at TRINITY COLLEDG in CAMBRIDG,

These.

B. 10. *Newton to Halley, Oct. 18, 1686.*

SIR,

In the scholium you write of, the words "vel hyperbolae" in the 3d line are to be struck out, and in the 5th and 6th lines the words "quae sit ad G K" should be "quae sit ad $\frac{1}{2}$ G K." I send you inclosed the beginning of this scholium with the 63d 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 rectae quaevis convergentes transmutantur in parallelas, adhibendo pro radio ordinato primo A O lineam quamvis rectam, quae per concursum convergentium transit: id adeo quia concursus ille hoc pacto abit in infinitum, lineae autem parallelae sunt quae ad punctum infinite distans tendunt." In the figure, pag. 86, conceive the curve H G I to be produced both ways till it meet and intersect itself any where in the radius ordinatus primus A O: and when the point G moving up and down in the curve H I arrives at that intersection point, I say the point g moving in like manner up and down in the curve h i will

become infinitely distant. For the point g falling upon the line $o A$, the point D will fall upon the point A , and the line $o D$ upon the line $o A$; and so becoming parallel to $a B$ their intersection point d will become infinitely distant, and consequently the line $d g$ will become infinitely distant, and so will its point g . Q. E. D. So then if any two lines of the primary figure $H G I D$ intersect in the radius ordinatus primus $A O$, their intersection in the new figure $h g i d$ 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 $o A$, 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 3d is a fault in the copy.

P. 6, l. 27, velocitate; p. 8, l. 19, tur Sunt.; p. 14, l. 30, reciproce ut $D O$; 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.

B. 11. *Newton to Halley, Feb. 18, 1687.*

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

desire 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 $4\frac{1}{2}$ 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.

IS. NEWTON.

B. 12. *Halley to Newton, Feb. 24, 1687.*

London, Feb. 24.

HONOURD SR,

I return you most hearty thanks for the copy you sent me of the sheet which was lost by the printers negligence; I will now do nothing else till the whole be finished, which I hope may be soon after Easter; and to redeem the time I have lost, I will employ another press to go on with the second part, which I am glad to understand you have perfected, and if you please to send it up to me, as soon as I have it, I will sett the printer to work on it, and will not be wanting to do my part to let it appear to y^e world to your satisfaction. I am sorry the Societie should be represented to you so unsteady as to fall so frequently into variance, but there is no such thing; and I am bold to say that I serve them to their satisfaction, though 6 out of 38 last general election day, did their endeavour to have put me by. Dr. Wallis his papers I will send you, the result is much the same with yours, and he had the hint from an account I gave him of what you had demonstrated, I will send it you with some more sheets the next week; it is, as yours, founded upon the Hypothesis of the opposition being proportionate to the celerity which you say you find reason to dispute. Your demonstration of the parallax of the sunn from the inequalitys of the moons motions, is what the Societie has commanded me to request of you, it being the best means of determining the dimensions of the planetary

Systeme, which all other ways are deficient in; and they entreat you not to desist when you are come so near the Solution of so noble a probleme. This done, there remains nothing more to be enquired in this matter, and you will do your self the honour of perfecting scientifically what all past ages have but blindly groped after. I have your two propositions you sent me some time since, and shall insert them in their proper place.

I am, Sr, to the utmost of my power,

Your most affectionate humble servt,

EDM. HALLEY.

To his Honoured Friend,
Mr. ISAAC NEWTON,
in TRINITY COLLEDG, CAMBRIDG,
These.

B. 13. *Newton to Halley, March 1, 1687.*

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 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,

IS. NEWTON.

March 1, 86-7.

B. 14. *Halley to Newton, March 7, 1687.*

London, March 7, 1686-7.

HONOURED SR,

I received yours, and according to it your Second Book, which this week I will putt to the press, having agreed with one that promises

me to get it done in 7 weeks, it making much about 20 sheets. The First Book will be about 30, which will be finished much about the same time. This week you shall have the 18th sheet according to your direction. You mention in this second, your third Book *De Systemate Mundi*, which from such firm principles, as in the preceding you have laid down, cannot chuse but give universall satisfaction, if this be likewise ready, and not too long to be got printed by the same time, and you think fit to send it; I will endeavour by a third hand, to get it all done together, being resolved to engage upon no other business till such time as all is done; desiring herby to clear my self from all imputations of negligence in a business wherein I am much rejoyced to be any wais concerned in handing to the world that that all future ages will admire; and as being,

Sr, your most obedient servant,

EDM. HALLEY.

To Mr. ISAAC NEWTON,
at TRINITY COLLEDG CAMBRIDG.

B. 15. *Halley to Newton, March 14, 1687.*

Mart 14, 1687.

Sr,

I have now sent you the 18th sheet of your book, but could not be as good as my word, by reason of the extraordinary trouble of the last sheet, which was the reason that it could not be finished time enough to send it you the last week. I have not been wanting to endeavour the clearing it of errata, but am sensible that notwithstanding all my care some have crept in, but I hope none of consequence. Pray please to examine it yourself, and note what mistakes are committed, that so they may be noted at the end; and if they be very materiall, the sheet shall be done over again, as I was forced to do the sheet D, and half the sheet P must be done, for the figure is turnd upside down by y^e negligence of the printer, in pag 112. I hope in a fortnight more to send you as many more sheets, and very suddenly to have the first part finished: being,

Sr, your most humble servt,

EDM. HALLEY.

To Mr. ISAAC NEWTON,
in TRINITY COLL.,
CAMBRIDG.

Postp^d.

These present
With a small parcell.

B. 16. *Halley to Newton, April 5, 1687.*

London, April 5, 1687.

HONOURED SR,

I received not the last part of your divine Treatise till yesterday, though it came to town that day sennight; having had occasion to be out of town the last week. The first part will be finished within this three weeks, and considering the shortness of the third over the second, the same press that did the first will get it done so soon as the second can be finished by another press; but I find some difficulty to match the letter justly. Your method of determining the Orb of a Comet deserves to be practised upon more of them, as far as may ascertain whether any of those that have passed in former times may have returned again: for their nodes and perihelia being fixed, will prove it sufficiently, and by their periods the transverse diameters will be given, which possibly may render the problem more easy. If you can remove the fault in the Comets Latitudes, twill better do, but as it is, the Numbers you have laid down do make out the verity of your Hypothesis past dispute. I do not find that you have touched that notable appearance of comets tayles, and their opposition to the sunn; which seems rather to argue an efflux from the sunn than a gravitation towards him. I doubt not but this may follow from your principles with the like ease as all the other phenomena; but a proposition or two concerning these will add much to the beauty and perfection of your Theory of Comets. I find I shall not get the whole completed before Trinity term, when I hope to have it published; when the world will not be more instructed by the demonstrative doctrine thereof, than it will pride it self to have a subject capable of penetrating so far into the abstrusest secrets of nature, and exalting humane Reason to so sublime a pitch by this utmost effort of the mind. But least my affection should make me transgress, I remain,

Your most obedient servant,

To MR. ISAAC NEWTON,

EDM. HALLEY.

to be left with Mr. Parish Rector of
Coulsterworth, in Lincolnshire.

These.

B. 17. *Halley to Newton, July 5, 1687.*

London, July 5, 1687.

HONOURED SR,

I have at length brought your Book to an end, and hope it will please you. The last errata came just in time to be inserted. I

will present from you the books you desire to the R. Society, Mr. Boyle, Mr. Pagit, Mr. Flamsteed, and if there be any else in town that you design to gratifie that way; and I have sent you to bestow on your friends in the University 20 Copies, which I entreat you to accept. In the same parcell you will receive 40 more, wch, having no acquaintance in Cambridg, I must entreat you to put into the hands of one or more of your ablest Booksellers to dispose of them: I intend the price of them bound in calves leather and letterd, to be 9 shillings here. Those I send you I value in quires at 6 shillings, to take my money as they are sold, or at 5^{sh.} a price certain for ready, or else at some short time; for I am satisfied that there is no dealing in books without interesting the Booksellers, and I am contented to let them go halves with me, rather than have your excellent work smothered by their combinations. I hope you will not repent you of the pains you have taken in so laudable a piece, so much to your own and the nations credit, but rather, after you shall have a little diverted your self with other studies, that you will resume those contemplations, wherein you have had so good success, and attempt the perfection of the Lunar Theory, which will be of prodigious use in Navigation, as well as of profound and subtile speculation. Sr I shall be glad to hear that you have received the Books, and to know what farther presents you would make in town which shall be accordingly done. You will receive a box from me on Thursday next by the Waggon, that parts from hence to-morrow. I am Your most obliged humble servt,

EDM. HALLEY.

To Mr. Isaac Newton,
In Trinity Colledg. Cambridg. These.

C. MEMORANDA ON THE CORRESPONDENCE NOW EXTANT
(91 LETTERS), CONCERNING THE PRODUCTION OF THE SECOND
EDITION OF THE PRINCIPIA.

This correspondence commences with a letter from Bentley to Newton, dated June 10, 1708 (see Brewster, vol. ii. pp. 188-190). It is continued in letters 1 to 85 (inclusive) of the *Cotes Correspondence*, edited by Dr. Edleston, London, 1850 (see above, p. 4); to which should be added 6 letters from Cotes to Newton, relating mainly to the velocity of effluent water, of the dates Sept. 21, 1710, Oct. 5, 1710, Oct. 26, 1710, March 31, 1711, June 4, 1711, and June 9,

1711, which are in the Portsmouth Collection : portions of the first and last of the above letters are given in Dr. Edleston's work. The letters from Newton were printed by Dr. Edleston from the originals ; but those from Bentley and Cotes were printed from drafts, and in most cases there are minute verbal differences between these and the originals which are preserved in the Portsmouth Collection. The paragraph printed by Dr. Edleston on p. 119 should be inserted as the last paragraph of the letter of April 26, 1712. There are also in the Portsmouth Collection two letters from the printer (Crownfield) to Newton relating to the progress of the work.

D. MEMORANDA ON THE CORRESPONDENCE CONCERNING THE PRODUCTION OF THE THIRD EDITION OF THE PRINCIPIA.

On this I have little to add to what I have stated in chapter vii. (see above, p. 135). There are in the Portsmouth Collection 23 letters from Pemberton to Newton, and seven sheets of queries—all on questions connected with the preparation of the third edition of the *Principia*. Pemberton speaks of letters passing between Newton and himself on this subject. If the communications from Newton consisted merely of notes written on the margins of the proof sheets, probably they are irrevocably lost ; but Rigaud seems to have thought that they were letters, and had been preserved. Pemberton died in 1771 ; he left his printed books to Dr. Wilson, but most likely his papers went with the residue of his property to a Mr. Henry Miles, a timber merchant of Rotherhithe. Mr. Miles had sons, but all efforts to trace them or Mr. Miles's papers have failed*.

* See Rigaud, p. 107 ; and *Philosophical Magazine*, May, 1836, series 3, vol. viii. pp. 441-442.

Works suitable for School Prizes.

MATHEMATICAL

RECREATIONS AND PROBLEMS.

By W. W. ROUSE BALL.

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THIS work is divided into two parts, the first on mathematical recreations and puzzles, the second on some problems of historical interest; but in both parts questions which involve advanced mathematics are excluded.

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The second part commences with a sketch of the history, first, of three classical problems in geometry—namely, the duplication of the cube, the trisection of an angle, and the quadrature of the circle—and second, of astrology. The last three chapters are devoted to an account of the hypotheses as to the nature of space and mass, and the means of measuring time.

Mr Ball has already attained a position in the front rank of writers on subjects connected with the history of mathematics, and this brochure will add another to his successes in this field. In it he has collected a

mass of information bearing upon matters of more general interest, written in a style which is eminently readable, and at the same time exact. He has done his work so thoroughly that he has left few ears for other gleaners. The nature of the work is completely indicated to the mathematical student by its title. Does he want to revive his acquaintance with the *Problèmes Plaisans et Dèlectables* of Bachet or the *Récréations Mathématiques et Physiques* of Ozanam? Let him take Mr Ball for his companion, and he will have the cream of these works put before him with a wealth of illustration quite delightful. Or, coming to more recent times, he will have full and accurate discussion of 'the fifteen puzzle,' 'Chinese rings,' 'the fifteen schoolgirls problem' *et id genus omne*. Sufficient space is devoted to accounts of magic squares and unicursal problems (such as mazes, the knight's path, and geometrical trees). These, and many other problems of equal interest, come under the head of 'Recreations.' The problems and speculations include an account of the Three Classical Problems; there is also a brief sketch of Astrology; and interesting outlines of the present state of our knowledge of hyperspace and of the constitution of matter. This enumeration baldly indicates the matter handled, but it sufficiently states what the reader may expect to find. Moreover for the use of readers who may wish to pursue the several heads further, Mr Ball gives detailed references to the sources from whence he has derived his information. These *Mathematical Recreations* we can commend as suited for mathematicians and equally for others who wish to while away an occasional hour.—*The Academy*.

The idea of writing some such account as that before us must have been present to Mr Ball's mind when he was collecting the material which he has so skilfully worked up into his *History of Mathematics*. We think this because the extent of ground covered by these *Recreations* is commensurate with that of the *History*, and many bits of ore which would not suit the earlier work find a fitting niche in this. Howsoever the case may be, we are sure that non-mathematical, as well as mathematical, readers will derive amusement, and, we venture to think, profit withal, from a perusal of it. The author has gone very exhaustively over the ground, and has left us little opportunity of adding to or correcting what he has thus reproduced from his note-books. The work before us is divided into two parts: mathematical recreations and mathematical problems and speculations. All these matters are treated lucidly, and with sufficient detail for the ordinary reader, and for others there is ample store of references....Our analysis shows how great an extent of ground is covered by the *Mathematical Recreations*, and when we add that the account is fully pervaded by the attractive charm Mr Ball knows

so well how to infuse into what many persons would look upon as a dry subject, we have said all we can to commend it to our readers.—*Nature*.

A fit sequel to its author's valuable and interesting works on the history of mathematics. There is a fascination about this volume which results from a happy combination of puzzle and paradox. There is both milk for babes and strong meat for grown men...A great deal of the information is hardly accessible in any English books; and Mr Ball would deserve the gratitude of mathematicians for having merely collected the facts. But he has presented them with such lucidity and vivacity of style that there is not a dull page in the book; and he has added minute and full bibliographical references which greatly enhance the value of his work.—*The Cambridge Review*.

Mathematicians with a turn for the paradoxes and puzzles connected with number, space, and time, in which their science abounds, will delight in *Mathematical Recreations and Problems of Past and Present Times*.—*The Times*.

Mathematicians have their recreations; and Mr Ball sets forth the humours of mathematics in a book of deepest interest to the clerical reader, and of no little attractiveness to the layman. The notes attest an enormous amount of research.—*The National Observer*

Mr Ball has produced a book of extreme and all but unique interest to general readers who dabble in science as well as to professed mathematicians.—*The Scottish Leader*.

Mr Ball, to whom we are already indebted for two excellent Histories of Mathematics, has just produced a book which will be thoroughly appreciated by those who enjoy the setting of the wits to work...He has collected a vast amount of information about mathematical quips, tricks, cranks, and puzzles—old and new; and it will be strange if even the most learned do not find something fresh in the assortment.—*The Observatory*.

Mr Rouse Ball has the true gift of story-telling, and he writes so pleasantly that though we enjoy the fulness of his knowledge we are tempted to forget the considerable amount of labour involved in the preparation of his book. He gives us the history and the mathematics of many problems...and where the limits of his work prevent him from dealing fully with the points raised, like a true worker he gives us ample references to original memoirs...The book is warmly to be recommended, and should find a place on the shelves of every one interested in mathematics and on those of every public library.—*The Manchester Guardian*.

Mr Ball's explanations are all clear and suggestive, and the book is one which is calculated to arouse in many a dormant taste for mathematics.—*The Scotsman*.

A work which will interest all who delight in mathematics and mental exercises generally. The student will often take it up, as it contains many problems which puzzle even clever people.—*The English Mechanic and World of Science*.

This is a book which the general reader should find as interesting as the mathematician. At all events, an intelligent enjoyment of its contents presupposes no more knowledge of mathematics than is nowadays possessed by almost everybody.—*The Athenæum*.

Once more the author of a *Short History of Mathematics* and a *History of the Study of Mathematics at Cambridge* gives evidence of the width of his reading and of his skill in compilation. From the elementary arithmetical puzzles which were known in the sixteenth and seventeenth centuries to those modern ones, the mathematical discussion of which has taxed the energies of the ablest investigator, very few questions have been left unrepresented. The sources of the author's information are indicated with great fulness...The book is a welcome addition to English mathematical literature.—*The Oxford Magazine*.

A book which deserves to be widely known by those who are fond of solving puzzles...and will be found to contain an admirable classified collection of ingenious questions capable of mathematical analysis. As the author is himself a skilful mathematician, and is careful to add an analysis of most of the propositions, it may easily be believed that there is food for study as well as amusement in his pages...Is in every way worthy of praise.—*The School Guardian*.

The work is a very judicious and suggestive compilation, not meant mainly for mathematicians, yet made doubly valuable to them by copious references. The style in the main is so compact and clear that what is central in a long argument or process is admirably presented in a few words. One great merit of this, or any other really good book on such a subject, is its suggestiveness; and in running through its pages, one is pretty sure to think of additional problems on the same general lines.—*Bulletin of the New York Mathematical Society*.

To the mathematician especially this will be a most welcome book.—*The Glasgow Herald*.

The book is very interesting, and judiciously combines instruction and recreation.—*The Educational Times*.

A SHORT ACCOUNT OF THE
HISTORY OF MATHEMATICS.

By W. W. ROUSE BALL.

[*Second Edition.* Pp. xxiv + 520. Price 10s. net.]

MACMILLAN AND CO., LONDON AND NEW YORK.

THIS book gives an account of the lives and discoveries of those mathematicians to whom the development of the subject is mainly due. The use of technicalities has been avoided and the work is intelligible to any one acquainted with the elements of mathematics.

The author commences with an account of the origin and progress of Greek mathematics, from which the Alexandrian, the Indian, and the Arab schools may be said to have arisen. Next the mathematics of mediæval Europe and the renaissance are described. The latter part of the book is devoted to the history of modern mathematics (beginning with the invention of analytical geometry and the infinitesimal calculus) the account of which is brought down to the present time.

This excellent summary of the history of mathematics supplies a want which has long been felt in this country. The extremely difficult question, how far such a work should be technical, has been solved with great tact.... The work contains many valuable hints, and is thoroughly readable. The biographies, which include those of most of the men who played important parts in the development of culture, are full and general enough to interest the ordinary reader as well as the specialist. Its value to the latter is much increased by the numerous references to authorities, a good table of contents, and a full and accurate index.—*The Saturday Review*.

Mr Ball's book should meet with a hearty welcome, for though we possess other histories of special branches of mathematics, this is the first serious attempt that has been made in the English language to give a systematic account of the origin and development of the science as a whole. It is written too in an attractive style. Technicalities are not too numerous or obtrusive, and the work is interspersed with biographical sketches and anecdotes likely to interest the general reader. Thus the tyro and the advanced mathematician alike may read it with pleasure and profit.—*The Athenæum*.

A wealth of authorities, often far from accordant with each other, renders a work such as this extremely formidable; and students of mathematics have reason to be grateful for the vast amount of information which has been condensed into this short account....In a survey of so wide extent it is of course impossible to give anything but a bare sketch of the various lines of research, and this circumstance tends to render a narrative scrappy. It says much for Mr Ball's descriptive skill that his history reads more like a continuous story than a series of merely consecutive summaries.—*The Academy*.

We can heartily recommend to our mathematical readers, and to others also, Mr Ball's *History of Mathematics*. The history of what might be supposed a dry subject is told in the pleasantest and most readable style, and at the same time there is evidence of the most careful research.—*The Observatory*.

All the salient points of mathematical history are given, and many of the results of recent antiquarian research; but it must not be imagined that the book is at all dry. On the contrary the biographical sketches frequently contain amusing anecdotes, and many of the theorems mentioned are very clearly explained so as to bring them within the grasp of those who are only acquainted with elementary mathematics.—*Nature*.

Le style de M. Ball est clair et élégant, de nombreux aperçus rendent facile de suivre le fil de son exposition et de fréquentes citations permettent à celui qui le désire d'approfondir les recherches que l'auteur n'a pu qu'effleurer....Cet ouvrage pourra devenir très utile comme manuel d'histoire des mathématiques pour les étudiants, et il ne sera pas déplacé dans les bibliothèques des savants.—*Bibliotheca Mathematica*.

The author modestly describes his work as a compilation, but it is thoroughly well digested, a due proportion is observed between the various parts, and when occasion demands he does not hesitate to give an independent judgment on a disputed point. His verdicts in such instances appear to us to be generally sound and reasonable....To many readers who have not the courage or the opportunity to tackle the ponderous volumes of Montucla or the (mostly) ponderous treatises of German writers on special periods, it may be somewhat of a surprise to find what a wealth of human interest attaches to the history of so "dry" a subject as mathematics. We are brought into contact with many remarkable men, some of whom have played a great part in other fields, as the names of Gerbert, Wren, Leibnitz, Descartes, Pascal, D'Alembert, Carnot, among others may testify, and with at least one thorough black-guard (Cardan); and Mr Ball's pages abound with quaint and amusing touches characteristic of the authors under consideration, or of the times in which they lived.—*Manchester Guardian*.

There can be no doubt that the author has done his work in a very excellent way....There is no one interested in almost any part of mathematical science who will not welcome such an exposition as the present, at once popularly written and exact, embracing the entire subject....Mr Ball's work is destined to become a standard one on the subject.—*The Glasgow Herald*.

A HISTORY OF THE STUDY OF MATHEMATICS AT CAMBRIDGE.

BY W. W. ROUSE BALL.

[*Pp.* xvi + 264. *Price* 6s.]

THE UNIVERSITY PRESS, CAMBRIDGE.

THIS work contains an account of the development of the study of mathematics in the university of Cambridge from the twelfth century to the middle of the nineteenth century, and a description of the means by which proficiency in that study was tested at various times.

The first part of the book is devoted to an enumeration of the more eminent Cambridge mathematicians, arranged chronologically: the subject-matter of their more important works is stated, and the methods of exposition which they used are indicated. Any reader who may wish to omit details will find a description of the characteristic features of each period in the introductory paragraphs of the chapter concerning it.

The second part of the book treats of the manner in which mathematics was taught, and of the exercises and examinations required of students in successive generations. To explain the relation of mathematics to other departments of study a brief outline of the general history of the university and of the organization of education therein is added.

The present volume is very pleasant reading, and though much of it necessarily appeals only to mathematicians, there are parts—*e.g.* the chapters on Newton, on the growth of the tripos, and on the history of the university—which are full of interest for a general reader.... The book is well written, the style is crisp and clear, and there is a humorous appreciation of some of the curious old regulations which have been superseded by time and change of custom. Though it seems light, it must represent an extensive study and investigation on the part of the author, the essential results of which are skilfully given. We can most thoroughly commend Mr Ball's volume to all readers who are interested in mathematics or in the growth and the position of the Cambridge school of mathematicians.—*The Manchester Guardian.*

Voici un livre dont la lecture inspire tout d'abord le regret que des travaux analogues n'aient pas été faits pour toutes les Écoles célèbres, et avec autant de soin et de clarté... Toutes les parties du livre nous ont vivement intéressé.—*Bulletin des sciences mathématiques.*

A book of pleasant and useful reading for both historians and mathematicians. Mr Ball's previous researches into this kind of history have already established his reputation, and the book is worthy of the reputation of its author. It is more than a detailed account of the rise and progress of mathematics, for it involves a very exact history of the University of Cambridge from its foundation.—*The Educational Times.*

Mr Ball is far from confining his narrative to the particular science of which he is himself an acknowledged master, and his account of the study of mathematics becomes a series of biographical portraits of eminent professors and a record not only of the intellectual life of the *élite* but of the manners, habits and discussions of the great body of Cambridge men from the sixteenth century to our own.—*The Daily News.*

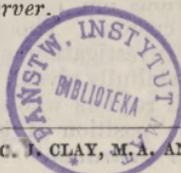
Mr Ball has not only given us a detailed account of the rise and progress of the science with which the name of Cambridge is generally associated but has also written a brief but reliable and interesting history of the university itself from its foundation down to recent times... The book is pleasant reading alike for the mathematician and the student of history.—*St James's Gazette.*

A very handy and valuable book containing, as it does, a vast deal of interesting information which could not without inconceivable trouble be found elsewhere... It is very far from forming merely a mathematical biographical dictionary, the growth of mathematical science being skillfully traced in connection with the successive names. There are probably very few people who will be able thoroughly to appreciate the author's laborious researches in all sorts of memoirs and transactions of learned societies in order to unearth the material which he has so agreeably condensed... Along with this there is much new matter which, while of great interest to mathematicians, and more especially to men brought up at Cambridge, will be found to throw a good deal of new and important light on the history of education in general.—*The Glasgow Herald.*

Exceedingly interesting to all who care for mathematics.—*The Literary World.*

The book is very enjoyable, and gives a capital and accurate digest of many excellent authorities which are not within the reach of the ordinary reader.—*The Scots Observer.*

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