Sir Isaac Newton's
MATHEMATICAL PRINCIPLES
OF NATURAL PHILOSOPHY AND HIS SYSTEM OF THE WORLD

Translated into English by Andrew Motte in 1729. The translations revised, and supplied with an historical and explanatory appendix, by

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Volume Two: THE SYSTEM OF THE WORLD

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Book Three

SYSTEM OF THE WORLD

(IN MATHEMATICAL TREATMENT)

In the preceding books I have laid down the principles of philosophy; principles not philosophical but mathematical: such, namely, as we may build our reasonings upon in philosophical inquiries. These principles are the laws and conditions of certain motions, and powers or forces, which chiefly have respect to philosophy; but, lest they should have appeared of themselves dry and barren, I have illustrated them here and there with some philosophical scholiums, giving an account of such things as are of more general nature, and which philosophy seems chiefly to be founded on; such as the density and the resistance of bodies, spaces void of all bodies, and the motion of light and sounds. It remains that, from the same principles, I now demonstrate the frame of the System of the World. Upon this subject I had, indeed, composed the third Book in a popular method, that it might be read by many; but afterwards, considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore, to prevent the disputes which might be raised upon such accounts, I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which should be read by those only who had first made themselves masters of the principles established in the preceding Books: not that I would advise anyone to the previous study of every Proposition of those Books; for they abound with such as might cost too much time, even to readers of good mathematical learning. It is enough if one carefully reads the Definitions, the Laws of Motion, and the first three sections of the first Book. He may then pass on to this Book, and consult such of the remaining Propositions of the first two Books, as the references in this, and his occasions, shall require.

RULES OF REASONING IN PHILOSOPHY

RULE I

We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

RULE II

Therefore to the same natural effects we must, as far as possible, assign the same causes.

As to respiration in a man and in a beast; the descent of stones in *Europe* and in *America;* the light of our culinary fire and of the sun; the reflection of light in the earth, and in the planets.

RULE III

The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

For since the qualities of bodies are only known to us by experiments, we are to hold for universal all such as universally agree with experiments; and such as are not liable to diminution can never be quite taken away. We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which is wont to be simple, and always consonant to itself. We no other way know the extension of bodies than by our senses, nor do these reach it in all bodies; but because we perceive extension in all that are sensible, therefore we ascribe it universally to all others also. That abundance of bodies are hard, we learn by experience; and because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles not only of the bodies we feel but of all others. That all bodies are impenetrable, we gather not from reason, but from sensation. The bodies which we handle we find impenetrable, and thence conclude impenetrability to be an universal property of all bodies whatsoever. That all bodies are movable, and endowed with certain powers (which we call the inertia) of persevering in their motion, or in their rest, we only infer from the like properties observed in the bodies which we have seen. The extension, hardness, impenetrability, mobility, and inertia of the whole, result from the extension, hardness, impenetrability, mobility, and inertia of the parts; and hence we conclude the least particles of all bodies to be also all extended, and hard and impenetrable, and movable, and endowed with their proper inertia. And this is the foundation of all philosophy. Moreover, that the divided but contiguous particles of bodies may be separated from one another, is matter of observation; and, in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished, and not yet divided, may, by the powers of Nature, be actually divided and separated from one another, we cannot certainly determine. Yet, had we the proof of but one experiment that any undivided particle, in breaking a hard and solid body, suffered a division, we might by virtue of this rule conclude that the undivided as well as the divided particles may be divided and actually separated to infinity.

Lastly, if it universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth, and that in proportion to the quantity of matter which they severally contain; that the moon likewise, according to the quantity of its matter, gravitates towards the earth; that, on the other hand, our sea gravitates towards the moon; and all the planets one towards another; and the comets in like manner towards the sun; we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a principle of mutual gravitation.

For the argument from the appearances concludes with more force for the universal gravitation of all bodies than for their impenetrability; of which, among those in the celestial regions, we have no experiments, nor any manner of observation. Not that I affirm gravity to be essential to bodies: by their *vis insita* I mean nothing but their inertia. This is immutable. Their gravity is diminished as they recede from the earth.

RULE IV

In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

[NOTE: In the following parts of Book III, scattered words and phrases in italics (except in Latin expressions and in names of places, months, persons, and writings) are, in Motte's translation, interpolations of words and phrases not in the Latin text of the *Principia;* and a few are departures from a literal translation of the Latin.]

PHENOMENA

PHENOMENON I

That the circumjovial planets, by radii drawn to Jupiter's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are as the $^3/_2$th power of their distances from its centre.

This we know from astronomical observations. For the orbits of these planets differ but insensibly from circles concentric to Jupiter; and their motions in those circles are found to be uniform. And all astronomers agree that their periodic times are as the $^3/_2$th power of the semidiameters of their orbits; and so it manifestly appears from the following table.

The periodic times of the satellites of Jupiter.

$1^d. 18^h. 27^m. 34^s.$, $3^d. 13^h. 13^m. 42^s.$, $7^d. 3^h. 42^m. 36^s.$, $16^d. 16^h. 32^m. 9^s.$

The distances of the satellites from Jupiter's centre.

	1	2	3	4	
From the observations of:					
Borelli	$5^2/_3$	$8^2/_3$	14	$24^2/_3$	
Townly *by the micrometer*	5.52	8.78	13.47	24.72	*Semi-diameter of Jupiter*
Cassini *by the telescope*	5	8	13	23	
Cassini *by the eclipse of the satellites*	$5^2/_3$	9	$14^{23}/_{60}$	$25^3/_{10}$	
From the periodic times	5.667	9.017	14.384	25.299	

Mr. *Pound* hath determined, by the help of excellent micrometers, the diameters of Jupiter and the elongation of its satellites after the following manner. The greatest heliocentric elongation of the fourth satellite from

Jupiter's centre was taken with a micrometer in a 15-foot telescope, and at the mean distance of Jupiter from the earth was found about 8′ 16″. The elongation of the third satellite was taken with a micrometer in a telescope of 123 feet, and at the same distance of Jupiter from the earth was found 4′ 42″. The greatest elongations of the other satellites, at the same distance of Jupiter from the earth, are found from the periodic times to be 2′ 56″ 47‴, and 1′ 51″ 6‴.

The diameter of Jupiter taken with the micrometer in a 123-foot telescope[1] several times, and reduced to Jupiter's mean distance from the earth, proved always less than 40″, never less than 38″, generally 39″. This diameter in shorter telescopes is 40″, or 41″; for Jupiter's light is a little dilated by the unequal refrangibility of the rays, and this dilatation bears a less ratio to the diameter of Jupiter in the longer and more perfect telescopes than in those which are shorter and less perfect. The times in which two satellites, the first and the third, passed over Jupiter's body, were observed, from the beginning of the ingress to the beginning of the egress, and from the complete ingress to the complete egress, with the long telescope. And from the transit of the first satellite, the diameter of Jupiter at its mean distance from the earth came forth 37⅛″, and from the transit of the third 37⅜″. There was observed also the time in which the shadow of the first satellite passed over Jupiter's body, and thence the diameter of Jupiter at its mean distance from the earth came out about 37″. Let us suppose its diameter to be 37¼″, very nearly, and then the greatest elongations of the first, second, third, and fourth satellite will be respectively equal to 5.965, 9.494, 15.141, and 26.63 semidiameters of Jupiter.

PHENOMENON II

That the circumsaturnal planets, by radii drawn to Saturn's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are as the ³⁄₂th power of their distances from its centre.

For, as *Cassini* from his own observations hath determined, their distances from Saturn's centre and their periodic times are as follows:

[1 Appendix, Note 39.]

The periodic times of the satellites of Saturn.

$1^{d}. 21^{h}. 18^{m}. 27^{s}.$, $2^{d}. 17^{h}. 41^{m}. 22^{s}.$, $4^{d}. 12^{h}. 25^{m}. 12^{s}.$, $15^{d}. 22^{h}. 41^{m}. 14^{s}.$, $79^{d}. 7^{h}. 48^{m}. 00^{s}.$

The distances of the satellites from Saturn's centre, in semidiameters of its ring.

From observations	$1\frac{19}{20}$	$2\frac{1}{2}$	$3\frac{1}{2}$	8	24
From the periodic times . .	1.93	2.47	3.45	8	23.35

The greatest elongation of the fourth satellite from Saturn's centre is commonly determined from the observations to be eight of those semidiameters, very nearly. But the greatest elongation of this satellite from Saturn's centre, when taken with an excellent micrometer in Mr. *Huygens'* telescope of 123 feet, appeared to be eight semidiameters and $\frac{7}{10}$ of a semidiameter. And from this observation and the periodic times the distances of the satellites from Saturn's centre in semidiameters of the ring are 2.1, 2.69, 3.75, 8.7, and 25.35. The diameter of Saturn observed in the same telescope was found to be to the diameter of the ring as 3 to 7; and the diameter of the ring, *May* 28-29, 1719, was found to be 43″; and hence the diameter of the ring when Saturn is at its mean distance from the earth is 42″, and the diameter of Saturn 18″. These things appear so in very long and excellent telescopes, because in such telescopes the apparent magnitudes of the heavenly bodies bear a greater proportion to the dilatation of light in the extremities of those bodies than in shorter telescopes. If, then, we reject all the spurious light, the diameter of Saturn will not amount to more than 16″.

PHENOMENON III

That the five primary planets, Mercury, Venus, Mars, Jupiter, and Saturn, with their several orbits, encompass the sun.

That Mercury and Venus revolve about the sun, is evident from their moon-like appearances. When they shine out with a full face, they are, in respect of us, beyond or above the sun; when they appear half full, they are about the same height on one side or other of the sun; when horned, they are below or between us and the sun; and they are sometimes, *when* *directly under,* seen like spots traversing the sun's disk. That Mars surrounds the sun, is as plain from its full face when near its conjunction with the sun, and from the gibbous figure which it shows in its quadratures. And the same thing is demonstrable of Jupiter and Saturn, from their appearing full in all situations; for the shadows of their satellites that appear sometimes upon their disks make it plain that the light they shine with is not their own, but borrowed from the sun.

PHENOMENON IV

That the fixed stars being at rest, the periodic times of the five primary planets, and (whether of the sun about the earth, or) of the earth about the sun, are as the ³⁄₂th power of their mean distances from the sun.

This proportion, first observed by *Kepler,* is now received by all astronomers; for the periodic times are the same, and the dimensions of the orbits are the same, whether the sun revolves about the earth, or the earth about the sun. And as to the measures of the periodic times, all astronomers are agreed about them. But for the dimensions of the orbits, *Kepler* and *Boulliau,* above all others, have determined them from observations with the greatest accuracy; and the mean distances corresponding to the periodic times differ but insensibly from those which they have assigned, and for the most part fall in between them; as we may see from the following table.

The periodic times with respect to the fixed stars, of the planets and earth revolving about the sun, in days and decimal parts of a day.

♄	♃	♂	♁	♀	☿
10759.275	4332.514	686.9785	365.2565	224.6176	87.9692

The mean distances of the planets and of the earth from the sun.

	♄	♃	♂
According to *Kepler*	951000	519650	152350
" " *Boulliau*	954198	522520	152350
" " the periodic times	954006	520096	152369
	♁	♀	☿
According to *Kepler*	100000	72400	38806
" " *Boulliau*	100000	72398	38585
" " the periodic times	100000	72333	38710

As to Mercury and Venus, there can be no doubt about their distances from the sun; for they are determined by the elongations of those planets from the sun; and for the distances of the superior planets, all dispute is cut off by the eclipses of the satellites of Jupiter. For by those eclipses the position of the shadow which Jupiter projects is determined; from this we have the heliocentric longitude of Jupiter. And from its heliocentric and geocentric longitudes compared together, we determine its distance.

PHENOMENON V

Then the primary planets, by radii drawn to the earth, describe areas in no wise proportional to the times; but the areas which they describe by radii drawn to the sun are proportional to the times of description.

For to the earth they appear sometimes direct, sometimes stationary, nay, and sometimes retrograde. But from the sun they are always seen direct, and to proceed with a motion nearly uniform, that is to say, a little swifter in the perihelion and a little slower in the aphelion distances, so as to maintain an equality in the description of the areas. This is a noted proposition among astronomers, and particularly demonstrable in Jupiter, from the eclipses of his satellites; by the help of these eclipses, as we have said, the heliocentric longitudes of that planet, and its distances from the sun, are determined.

PHENOMENON VI

That the moon, by a radius drawn to the earth's centre, describes an area proportional to the time of description.

This we gather from the apparent motion of the moon, compared with its apparent diameter. It is true that the motion of the moon is a little disturbed by the action of the sun: but in laying down these Phenomena, I neglect those small and inconsiderable errors.

PROPOSITIONS

PROPOSITION I. THEOREM I

That the forces by which the circumjovial planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to Jupiter's centre; and are inversely as the squares of the distances of the places of those planets from that centre.

The former part of this Proposition appears from Phen. I, and Prop. II or III, Book I; the latter from Phen. I, and Cor. VI, Prop. IV, of the same Book.

The same thing we are to understand of the planets which encompass Saturn, by Phen. II.

PROPOSITION II. THEOREM II

That the forces by which the primary planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to the sun; and are inversely as the squares of the distances of the places of those planets from the sun's centre.

The former part of the Proposition is manifest from Phen. V, and Prop. II, Book I; the latter from Phen. IV, and Cor. VI, Prop. IV, of the same Book. But this part of the Proposition is, with great accuracy, demonstrable from the quiescence of the aphelion points; for a very small aberration from the proportion according to the inverse square of the distances would (by Cor. I, Prop. XLV, Book I) produce a motion of the apsides sensible enough in every single revolution, and in many of them enormously great.

PROPOSITION III. THEOREM III

That the force by which the moon is retained in its orbit tends to the earth; and is inversely as the square of the distance of its place from the earth's centre.

The former part of the Proposition is evident from Phen. VI, and Prop. II or III, Book I; the latter from the very slow motion of the moon's apogee; which in every single revolution amounting but to $3^\circ\ 3'$ forwards, may be neglected. For (by Cor. I, Prop. XLV, Book I) it appears, that, if the distance of the moon from the earth's centre is to the semidiameter of the earth as D to 1, the force, from which such a motion will result, is inversely as $D^{2\frac{4}{243}}$, i.e., inversely as the power of D, whose exponent is $2\frac{4}{243}$; that is to say, in the proportion of the distance somewhat greater than the inverse square, but which comes 59¾ times nearer to the proportion according to the square than to the cube. But since this increase is due to the action of the sun (as we shall afterwards show), it is here to be neglected. The action of the sun, attracting the moon from the earth, is nearly as the moon's distance from the earth; and therefore (by what we have shown in Cor. II, Prop. XLV, Book I) is to the centripetal force of the moon as 2 to 357.45, or nearly so; that is, as 1 to $178\frac{29}{40}$. And if we neglect so inconsiderable a force of the sun, the remaining force, by which the moon is retained in its orb, will be inversely as D^2. This will yet more fully appear from comparing this force with the force of gravity, as is done in the next Proposition.

COR. If we augment the mean centripetal force by which the moon is retained in its orb, first in the proportion of $177\frac{29}{40}$ to $178\frac{29}{40}$, and then in the proportion of the square of the semidiameter of the earth to the mean distance of the centres of the moon and earth, we shall have the centripetal force of the moon at the surface of the earth; supposing this force, in descending to the earth's surface, continually to increase inversely as the square of the height.

PROPOSITION IV.[1] THEOREM IV

That the moon gravitates towards the earth, and by the force of gravity is continually drawn off from a rectilinear motion, and retained in its orbit.

The mean distance of the moon from the earth in the syzygies in semidiameters of the earth, is, according to *Ptolemy* and most astronomers, 59; according to *Vendelin* and *Huygens,* 60; to *Copernicus,* 60⅓; to *Street,* 60⅖; and to *Tycho,* 56½. But *Tycho,* and all that follow his tables of refraction, making the refractions of the sun and moon (altogether against the nature of light) to exceed the refractions of the fixed stars, and that by four or five minutes *near the horizon,* did thereby increase the moon's *horizontal* parallax by a like number of minutes, that is, by a twelfth or fifteenth part of the whole parallax. Correct this error, and the distance will become

[1 Appendix, Note 40.]

about 60½ semidiameters of the earth, near to what others have assigned. Let us assume the mean distance of 60 diameters in the syzygies; and suppose one revolution of the moon, in respect of the fixed stars, to be completed in $27^d. 7^h. 43^m.$, as astronomers have determined; and the circumference of the earth to amount to 123249600 *Paris* feet, as the *French* have found by mensuration. And now if we imagine the moon, deprived of all motion, to be let go, so as to descend towards the earth with the impulse of all that force by which (by Cor., Prop. III) it is retained in its orb, it will in the space of one minute of time, describe in its fall 15¹⁄₁₂ *Paris* feet. This we gather by a calculus, founded either upon Prop. XXXVI, Book I, or (which comes to the same thing) upon Cor. IX, Prop. IV, of the same Book. For the versed sine of that arc, which the moon, in the space of one minute of time, would by its mean motion describe at the distance of 60 semidiameters of the earth, is nearly 15¹⁄₁₂ *Paris* feet, or more accurately 15 feet, 1 inch, and 1 line 4⁄9. Wherefore, since that force, in approaching to the earth, increases in the proportion of the inverse square of the distance, and, upon that account, at the surface of the earth, is 60 · 60 times greater than at the moon, a body in our regions, falling with that force, ought in the space of one minute of time, to describe 60 · 60 · 15¹⁄₁₂ *Paris* feet; and, in the space of one second of time, to describe 15¹⁄₁₂ of those feet; or more accurately 15 feet, 1 inch, and 1 line 4⁄9. And with this very force we actually find that bodies here upon earth do really descend; for a pendulum oscillating seconds in the latitude of *Paris* will be 3 *Paris* feet, and 8 lines ½ in length, as Mr. *Huygens* has observed. And the space which a heavy body describes by falling in one second of time is to half the length of this pendulum as the square of the ratio of the circumference of a circle to its diameter (as Mr. *Huygens* has also shown), and is therefore 15 *Paris* feet, 1 inch, 1 line 7⁄9. And therefore the force by which the moon is retained in its orbit becomes, at the very surface of the earth, equal to the force of gravity which we observe in heavy bodies there. And therefore (by Rule 1 and 2) the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity, and in the space of one second of time would describe 30⅙ *Paris* feet; altogether against experience.

This calculus is founded on the hypothesis of the earth's standing still; for if both earth and moon move about the sun, and at the same time about their common centre of gravity, the distance of the centres of the moon and earth from one another will be 60½ semidiameters of the earth; as may be found by a computation from Prop. LX, Book I.

SCHOLIUM

The demonstration of this Proposition may be more diffusely explained after the following manner. Suppose several moons to revolve about the earth, as in the system of Jupiter or Saturn; the periodic times of these moons (by the argument of induction) would observe the same law which *Kepler* found to obtain among the planets; and therefore their centripetal forces would be inversely as the squares of the distances from the centre of the earth, by Prop. I, of this Book. Now if the lowest of these were very small, and were so near the earth as almost to touch the tops of the highest mountains, the centripetal force thereof, retaining it in its orbit, would be nearly equal to the weights of any terrestrial bodies that should be found upon the tops of those mountains, as may be known by the foregoing computation. Therefore if the same little moon should be deserted by its centrifugal force that carries it through its orbit, and be disabled from going onward therein, it would descend to the earth; and that with the same velocity, with which heavy bodies actually fall upon the tops of those very mountains, because of the equality of the forces that oblige them both to descend. And if the force by which that lowest moon would descend were different from gravity, and if that moon were to gravitate towards the earth, as we find terrestrial bodies do upon the tops of mountains, it would then descend with twice the velocity, as being impelled by both these forces conspiring together. Therefore since both these forces, that is, the gravity of heavy bodies, and the centripetal forces of the moons, are directed to the centre of the earth, and are similar and equal between themselves, they will (by Rule I and 2) have one and the same cause. And therefore the force which retains the moon in its orbit is that very force which we commonly call gravity; because otherwise this little moon at the top of a mountain must either be without gravity, or fall twice as swiftly as heavy bodies are wont to do.

PROPOSITION V. THEOREM V

That the circumjovial planets gravitate towards Jupiter; the circumsaturnal towards Saturn; the circumsolar towards the sun; and by the forces of their gravity are drawn off from rectilinear motions, and retained in curvilinear orbits.

For the revolutions of the circumjovial planets about Jupiter, of the circumsaturnal about Saturn, and of Mercury and Venus, and the other circumsolar planets, about the sun, are appearances of the same sort with the revolution of the moon about the earth; and therefore, by Rule 2, must be owing to the same sort of causes; especially since it has been demonstrated, that the forces upon which those revolutions depend tend to the centres of Jupiter, of Saturn, and of the sun; and that those forces, in receding from Jupiter, from Saturn, and from the sun, decrease in the same proportion, and according to the same law, as the force of gravity does in receding from the earth.

COR. I. There is, therefore, a power of gravity tending to all the planets; for, doubtless, Venus, Mercury, and the rest, are bodies of the same sort with Jupiter and Saturn. And since all attraction (by Law III) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets.

COR. II. The force of gravity which tends to any one planet is inversely as the square of the distance of places from that planet's centre.

COR. III. All the planets do gravitate towards one another, by Cor. I and II. And hence it is that Jupiter and Saturn, when near their conjunction, by their mutual attractions sensibly disturb each other's motions. So the sun disturbs the motions of the moon; and both sun and moon disturb our sea, as we shall hereafter explain.

SCHOLIUM

The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of that centripetal force which retains the moon in its orbit will extend itself to all the planets, by Rule 1, 2, and 4.

PROPOSITION VI. THEOREM VI

That all bodies gravitate towards every planet; and that the weights of bodies towards any one planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.

It has been, now for a long time, observed by others, that all sorts of heavy bodies (allowance being made for the inequality of retardation which they suffer from a small power of resistance in the air) descend to the earth *from equal heights* in equal times; and that equality of times we may distinguish to a great accuracy, by the help of pendulums. I tried experiments with gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two wooden boxes, round and equal: I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes, hanging by equal threads of 11 feet, made a couple of pendulums perfectly equal in weight and figure, and equally receiving the resistance of the air. And, placing the one by the other, I observed them to play together forwards and backwards, for a long time, with equal vibrations. And therefore the quantity of matter in the gold (by Cor. I and VI, Prop. XXIV, Book II) was to the quantity of matter in the wood as the action of the motive force (or *vis motrix*) upon all the gold to the action of the same upon all the wood; that is, as the weight of the one to the weight of the other: and the like happened in the other bodies. By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of matter less than the thousandth part of the whole, had any such been. But, without all doubt, the nature of gravity towards the planets is the same as towards the earth. For, should we imagine our terrestrial bodies taken to the orbit of the moon, and there, together with the moon, deprived of all motion, to be let go, so as to fall together towards the earth, it is certain, from what we have demonstrated before, that, in equal times, they would describe equal spaces with the moon, and of consequence are to the moon, in quantity of matter, as their weights to its weight. Moreover, since the satellites of Jupiter perform their revolutions in times which observe the $\frac{3}{2}$th power of the proportion of their distances from Jupiter's centre, their accelerative gravities towards Jupiter will be inversely as the squares of their distances from Jupiter's centre; that is, equal, at equal distances. And, therefore, these satellites, if supposed to fall *towards Jupiter* from equal heights, would describe equal spaces in equal times, in like manner as heavy bodies do on our earth. And, by the same argument, if the circumsolar planets were supposed to be let fall at equal distances from the sun, they would, in their descent towards the sun, describe equal spaces in equal times. But forces which equally accelerate unequal bodies must be as those bodies: that is to say, the weights of the planets *towards the sun* must be as their quantities of matter. Further, that the weights of Jupiter and of his satellites towards the sun are proportional to the several quantities of their matter, appears from the exceedingly regular motions of the satellites (by Cor. III, Prop. LXV, Book I). For if some of those bodies were more strongly attracted to the sun in proportion to their quantity of matter than others, the motions of the satellites would be disturbed by that inequality of attraction (by Cor. II, Prop. LXV, Book I). If, at equal distances from the sun, any satellite, in proportion to the quantity of its matter, did gravitate towards the sun with a force greater than Jupiter in proportion to his, according to any given proportion, suppose of d to e; then the distance between the centres of the sun and of the satellite's orbit would be always greater than the distance between the centres of the sun and of Jupiter, nearly as the square root of that proportion: as by some computations I have found. And if the satellite did gravitate towards the sun with a force, less in the proportion of e to d, the distance of the centre of the satellite's orbit from the sun would be less than the distance of the centre of Jupiter from the sun as the square root of the same proportion. Therefore if, at equal distances from the sun, the accelerative gravity of any satellite towards the sun were greater or less than the accelerative gravity of Jupiter towards the sun but by one $\frac{1}{1000}$ part of the whole gravity, the distance of the centre of the satellite's orbit from the sun would be greater or less than the distance of Jupiter from the sun by one $\frac{1}{2000}$ part of the whole distance; that is, by a fifth part of the distance of the utmost satellite from the centre of Jupiter; an eccentricity of the orbit which would be very sensible. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter, and of all its satellites towards the sun, are equal among themselves. And by the same argument, the weights of Saturn and of his satellites towards the sun, at equal distances from the sun, are as their several quantities of matter; and the weights of the moon and of the earth towards the sun are either none, or accurately proportional to the masses of matter which they contain. But some weight they have, by Cor. I and III, Prop. V.

But further; the weights of all the parts of every planet towards any other planet are one to another as the matter in the several parts; for if some parts did gravitate more, others less, than for the quantity of their matter, then the whole planet, according to the sort of parts with which it most abounds, would gravitate more or less than in proportion to the quantity of matter in the whole. Nor is it of any moment whether these parts are external or internal; for if, for example, we should imagine the terrestrial bodies with us to be raised to the orbit of the moon, to be there compared with its body; if the weights of such bodies were to the weights of the external parts of the moon as the quantities of matter in the one and in the other respectively, but to the weights of the internal parts in a greater or less proportion, then likewise the weights of those bodies would be to the weight of the whole moon in a greater or less proportion; against what we have shown above.

COR. I. Hence the weights of bodies do not depend upon their forms and textures; for if the weights could be altered with the forms, they would be greater or less, according to the variety of forms, in equal matter; altogether against experience.

COR. II. Universally, all bodies about the earth gravitate towards the earth; and the weights of all, at equal distances from the earth's centre, are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments; and therefore (by Rule 3) to be affirmed of all bodies whatsoever. If the ether, or any other body, were either altogether void of gravity, or were to gravitate less in proportion to its quantity of matter, then, because (according to *Aristotle, Descartes,* and others) there is no difference between that and other bodies but in *mere* form of matter, by a successive change from form to form, it might be changed at last into a body of the same condition with those which gravitate most in proportion to their quantity of matter; and, on the other hand, the heaviest bodies, acquiring the first form of that body, might by degrees quite lose their gravity. And therefore the weights would depend upon the forms of bodies, and with those forms, might be changed: contrary to what was proved in the preceding Corollary.

Cor. iii. All spaces are not equally full; for if all spaces were equally full, then the specific gravity of the fluid which fills the region of the air, on account of the extreme density of the matter, would fall nothing short of the specific gravity of quicksilver, or gold, or any other the most dense body; and, therefore, neither gold, nor any other body, could descend in air; for bodies do not descend in fluids, unless they are specifically heavier than the fluids. And if the quantity of matter in a given space can, by any rarefaction, be diminished, what should hinder a diminution to infinity?

Cor. iv. If all the solid particles of all bodies are of the same density, and cannot be rarefied without pores, then a void, space, or vacuum must be granted. By bodies of the same density, I mean those whose inertias are in the proportion of their bulks.

Cor. v. The power of gravity is of a different nature from the power of magnetism; for the magnetic attraction is not as the matter attracted. Some bodies are attracted more by the magnet; others less; most bodies not at all. The power of magnetism in one and the same body may be increased and diminished; and is sometimes far stronger, for the quantity of matter, than the power of gravity; and in receding from the magnet decreases not as the square but almost as the cube of the distance, as nearly as I could judge from some rude observations.

PROPOSITION VII. THEOREM VII

That there is a power of gravity pertaining to all bodies, proportional to the several quantities of matter which they contain.

That all the planets gravitate one towards another, we have proved before; as well as that the force of gravity towards every one of them, considered apart, is inversely as the square of the distance of places from the centre of the planet. And thence (by Prop. lxix, Book i, and its Corollaries) it follows, that the gravity tending towards all the planets is proportional to the matter which they contain.

Moreover, since all the parts of any planet A gravitate towards any other planet B; and the gravity of every part is to the gravity of the whole as the matter of the part to the matter of the whole; and (by Law iii) to every action corresponds an equal reaction; therefore the planet B will, on the other hand, gravitate towards all the parts of the planet A; and its gravity towards any one part will be to the gravity towards the whole as the matter of the part to the matter of the whole. Q.E.D.

COR. I. Therefore the force of gravity towards any whole planet arises from, and is compounded of, the forces of gravity towards all its parts. Magnetic and electric attractions afford us examples of this; for all attraction towards the whole arises from the attractions towards the several parts. The thing may be easily understood in gravity, if we consider a greater planet, as formed of a number of lesser planets, meeting together in one globe; for *hence it would appear* that the force of the whole must arise from the forces of the component parts. If it is objected, that, according to this law, all bodies with us must gravitate one towards another, whereas no such gravitation anywhere appears, I answer, that since the gravitation towards these bodies is to the gravitation towards the whole earth as these bodies are to the whole earth, the gravitation towards them must be far less than to fall under the observation of our senses.

COR. II. The force of gravity towards the several equal particles of any body is inversely as the square of the distance of places from the particles; as appears from Cor. III, Prop. LXXIV, Book I.

PROPOSITION VIII. THEOREM VIII

In two spheres gravitating each towards the other, if the matter in places on all sides round about and equidistant from the centres is similar, the weight of either sphere towards the other will be inversely as the square of the distance between their centres.

After I had found that the force of gravity towards a whole planet did arise from and was compounded of the forces of gravity towards all its parts, and towards every one part was in the inverse proportion of the squares of the distances from the part, I was yet in doubt whether that proportion inversely as the square of the distance did accurately hold, or but nearly so, in the total force compounded of so many partial ones; for it might be that the proportion which accurately enough took place in greater distances should be wide of the truth near the surface of the planet, where the distances of the particles are unequal, and their situation dissimilar. But by the help of Prop. LXXV and LXXVI, Book I, and their Corollaries, I was at last satisfied of the truth of the Proposition, as it now lies before us.

COR. I. Hence we may find and compare together the weights of bodies towards different planets; for the weights of bodies revolving in circles about planets are (by Cor. II, Prop. IV, Book I) directly as the diameters of the circles and inversely as the squares of their periodic times; and their weights at the surfaces of the planets, or at any other distances from their centres, are (by this Proposition) greater or less inversely as the square of the distances. Thus from the periodic times of Venus, revolving about the sun, in $224^d. 16\frac{3}{4}^h.$; of the utmost circumjovial satellite revolving about Jupiter, in $16^d. 16\frac{8}{15}^h.$; of the *Huygenian* satellite about Saturn, in $15^d. 22\frac{2}{3}^h.$; and of the moon about the earth, in $27^d. 7^h. 43^m.$; compared with the mean distance of Venus from the sun, and with the greatest heliocentric elongations of the outmost circumjovial satellite from Jupiter's centre, 8′ 16″; of the *Huygenian* satellite from the centre of Saturn, 3′ 4″; and of the moon from the earth, 10′ 33″; by computation I found that the weight of equal bodies, at equal distances from the centres of the sun, of Jupiter, of Saturn, and of the earth, towards the sun, Jupiter, Saturn, and the earth, were one to another, as 1, $\frac{1}{1067}$, $\frac{1}{3021}$, and $\frac{1}{169282}$ respectively. Then because as the distances are increased or diminished, the weights are diminished or increased in a squared ratio, the weights of equal bodies towards the sun, Jupiter, Saturn, and the earth, at the distances 10000, 997, 791, and 109 from their centres, that is, at their very surfaces, will be as 10000, 943, 529, and 435 respectively. How much the weights of bodies are at the surface of the moon, will be shown hereafter.

COR. II. Hence likewise we discover the quantity of matter in the several planets; for their quantities of matter are as the forces of gravity at equal distances from their centres; that is, in the sun, Jupiter, Saturn, and the earth, as 1, $\frac{1}{1067}$, $\frac{1}{3021}$, and $\frac{1}{169282}$ respectively. If the parallax of the sun be taken greater or less than 10″ 30‴, the quantity of matter in the earth must be augmented or diminished as the cube of that proportion.

COR. III. Hence also we find the densities of the planets; for (by Prop. LXXII, Book I) the weights of equal and similar bodies towards similar spheres are, at the surfaces of those spheres, as the diameters of the spheres;

and therefore the densities of dissimilar spheres are as those weights applied to the diameters of the spheres. But the true diameters of the sun, Jupiter, Saturn, and the earth, were one to another as 10000, 997, 791, and 109; and the weights towards the same as 10000, 943, 529, and 435 respectively; and therefore their densities are as 100, 94½, 67, and 400. The density of the earth, which comes out by this computation, does not depend upon the parallax of the sun, but is determined by the parallax of the moon, and therefore is here truly defined. The sun, therefore, is a little denser than Jupiter, and Jupiter than Saturn, and the earth four times denser than the sun; for the sun, by its great heat, is kept in a sort of rarefied state. The moon is denser than the earth, as shall appear afterwards.

COR. IV. The smaller the planets are, they are, other things being equal, of so much the greater density; for so the powers of gravity on their several surfaces come nearer to equality. They are likewise, other things being equal, of the greater density, as they are nearer to the sun. So Jupiter is more dense than Saturn, and the earth than Jupiter; for the planets were to be placed at different distances from the sun, that, according to their degrees of density, they might enjoy a greater or less proportion of the sun's heat. Our water, if it were removed as far as the orbit of Saturn, would be turned into ice, and in that of Mercury would quickly fly away in vapor; for the light of the sun, to which its heat is proportional, is seven times denser in the orb of Mercury than with us: and by the thermometer I have found that a sevenfold heat of our summer sun will make water boil. Nor are we to doubt that the matter of Mercury is adapted to its heat, and is therefore more dense than the matter of our earth; since, in a denser matter, the operations of Nature require a stronger heat.

PROPOSITION IX. THEOREM IX

That the force of gravity, considered downwards from the surface of the planets, decreases nearly in the proportion of the distances from the centre of the planets.

If the matter of the planet were of an uniform density, this Proposition would be accurately true (by Prop. LXXIII, Book I). The error, therefore, can be no greater than what may arise from the inequality of the density.

PROPOSITION X. THEOREM X

That the motions of the planets in the heavens may subsist an exceedingly long time.

In the Scholium of Prop. XL, Book II, I have shown that a globe of water frozen into ice, and moving freely in our air, in the time that it would describe the length of its semidiameter, would lose by the resistance of the air $\frac{1}{4586}$ part of its motion; and the same proportion holds nearly in all globes, however great, and moved with whatever velocity. But that our globe of earth is of greater density than it would be if the whole consisted of water only, I thus make out. If the whole consisted of water only, whatever was of less density than water, because of its less specific gravity, would emerge and float above. And upon this account, if a globe of terrestrial matter, covered on all sides with water, was less dense than water, it would emerge somewhere; and, the subsiding water falling back, would be gathered to the opposite side. And such is the condition of our earth, which in a great measure is covered with seas. The earth, if it was not for its greater density, would emerge from the seas, and, according to its degree of levity, would be raised more or less above their surface, the water of the seas flowing backwards to the opposite side. By the same argument, the spots of the sun, which float upon the lucid matter thereof, are lighter than that matter; and, however the planets have been formed while they were yet in fluid masses, all the heavier matter subsided to the centre. Since, therefore, the common matter of our earth on the surface thereof is about twice as heavy as water, and a little lower, in mines, is found about three, or four, or even five times heavier, it is probable that the quantity of the whole matter of the earth may be five or six times greater than if it consisted all of water; especially since I have before shown that the earth is about four times more dense than Jupiter. If, therefore, Jupiter is a little more dense than water, in the space of thirty days, in which that planet describes the length of 459 of its semidiameters, it would, in a medium of the same density with our air, lose almost a tenth part of its motion. But since the resistance of mediums decreases in proportion to their weight or density, so that water, which is $13\frac{3}{5}$ times lighter than quicksilver, resists less in that proportion; and air, which is 860 times lighter than water, resists less in the same proportion;

therefore in the heavens, where the weight of the medium in which the planets move is immensely diminished, the resistance will almost vanish.

It is shown in the Scholium of Prop. XXII, Book II, that at the height of 200 miles above the earth the air is more rare than it is at the surface of the earth in the ratio of 30 to 0.0000000000003998, or as 75000000000000 to 1, nearly. And hence the planet Jupiter, revolving in a medium of the same density with that superior air, would not lose by the resistance of the medium the 1000000th part of its motion in 1000000 years. In the spaces near the earth the resistance is produced only by the air, exhalations, and vapors. When these are carefully exhausted by the air pump from under the receiver, heavy bodies fall within the receiver with perfect freedom, and without the least sensible resistance: gold itself, and the lightest down, let fall together, will descend with equal velocity; and though they fall through a space of four, six, and eight feet, they will come to the bottom at the same time; as appears from experiments. And therefore, the celestial regions being perfectly void of air and exhalations, the planets and comets meeting no sensible resistance in those spaces will continue their motions through them for an immense tract of time.

HYPOTHESIS I

That the centre of the system of the world is immovable.

This is acknowledged by all, while some contend that the earth, others that the sun, is fixed in that centre. Let us see what may from hence follow.

PROPOSITION XI. THEOREM XI

That the common centre of gravity of the earth, the sun, and all the planets, is immovable.

For (by Cor. IV of the Laws) that centre either is at rest, or moves uniformly forwards in a right line; but if that centre moved, the centre of the world would move also, against the Hypothesis.

PROPOSITION XII. THEOREM XII

That the sun is agitated by a continual motion, but never recedes far from the common centre of gravity of all the planets.

For since (by Cor. II, Prop. VIII) the quantity of matter in the sun is to the quantity of matter in Jupiter as 1067 to 1; and the distance of Jupiter from the sun is to the semidiameter of the sun in a proportion but a small matter greater, the common centre of gravity of Jupiter and the sun will fall upon a point a little without the surface of the sun. By the same argument, since the quantity of matter in the sun is to the quantity of matter in Saturn as 3021 to 1, and the distance of Saturn from the sun is to the semidiameter of the sun in a proportion but a small matter less, the common centre of gravity of Saturn and the sun will fall upon a point a little within the surface of the sun. And, pursuing the principles of this computation, we should find that though the earth and all the planets were placed on one side of the sun, the distance of the common centre of gravity of all from the centre of the sun would scarcely amount to one diameter of the sun. In other cases, the distances of those centres are always less; and therefore, since that centre of gravity is continually at rest, the sun, according to the various positions of the planets, must continually be moved every way, but will never recede far from that centre.

Cor. Hence the common centre of gravity of the earth, the sun, and all the planets, is to be esteemed the centre of the world; for since the earth, the sun, and all the planets gravitate one towards another, and are, therefore, according to their powers of gravity, in continual agitation, as the Laws of Motion require, it is plain that their movable centres cannot be taken for the immovable centre of the world. If that body were to be placed in the centre, towards which other bodies gravitate most (according to common opinion), that privilege ought to be allowed to the sun; but since the sun itself is moved, a fixed point is to be chosen from which the centre of the sun recedes least, and from which it would recede yet less if the body of the sun were denser and greater, and therefore less apt to be moved.

PROPOSITION XIII. THEOREM XIII

The planets move in ellipses which have their common focus in the centre of the sun; and, by radii drawn to that centre, they describe areas proportional to the times of description.

We have discoursed above on these motions from the Phenomena. Now that we know the principles on which they depend, from those principles we deduce the motions of the heavens *a priori*. Because the weights of the planets towards the sun are inversely as the squares of their distances from the sun's centre, if the sun were at rest, and the other planets did not act one upon another, their orbits would be ellipses, having the sun in their common focus; and they would describe areas proportional to the times *of description,* by Prop. I and XI, and Cor. I, Prop. XIII, Book I. But the actions of the planets one upon another are so very small, that they may be neglected; and by Prop. LXVI, Book I, they disturb the motions of the planets around the sun in motion, less than if those motions were performed about the sun at rest.

It is true, that the action of Jupiter upon Saturn is not to be neglected; for the force of gravity towards Jupiter is to the force of gravity towards the sun (at equal distances, Cor. II, Prop. VIII) as 1 to 1067; and therefore in the conjunction of Jupiter and Saturn, because the distance of Saturn from Jupiter is to the distance of Saturn from the sun almost as 4 to 9, the gravity of Saturn towards Jupiter will be to the gravity of Saturn towards the sun as 81 to $16 \cdot 1067$; or, as 1 to about 211. And hence arises a perturbation of the orbit of Saturn in every conjunction of this planet with Jupiter, so sensible, that astronomers are puzzled with it. As the planet is differently situated in these conjunctions, its eccentricity is sometimes augmented, sometimes diminished; its aphelion is sometimes carried forwards, sometimes backwards, and its mean motion is by turns accelerated and retarded; yet the whole error in its motion about the sun, though arising from so great a force, may be almost avoided (except in the mean motion) by placing the lower focus of its orbit in the common centre of gravity of Jupiter and the sun (according to Prop. LXVII, Book I), and therefore that error, when it is greatest, scarcely exceeds two minutes; and the greatest error in the mean motion scarcely exceeds two minutes yearly. But in the conjunction of Jupiter and Saturn, the accelerative forces of gravity of the sun towards Saturn, of Jupiter towards Saturn, and of Jupiter towards the sun, are almost as 16, 81, and $\frac{16 \cdot 81 \cdot 3021}{25}$, or 156609; and therefore the difference of the forces of gravity of the sun towards Saturn, and of Jupiter towards Saturn, is to the force of gravity of Jupiter towards the sun as 65 to 156609, or as 1 to 2409. But the greatest power of Saturn to disturb the motion of Jupiter is proportional to this difference; and therefore the perturbation of the orbit of Jupiter is much less than that of Saturn's. The perturbations of the other orbits are yet far less, except that the orbit of the earth is sensibly disturbed by the moon. The common centre of gravity of the earth and moon moves in an ellipse about the sun in the focus thereof, and, by a radius drawn to the sun, describes areas proportional to the times of description. But the earth in the meantime by a menstrual motion is revolved about this common centre.

PROPOSITION XIV. THEOREM XIV

The aphelions and nodes of the orbits of the planets are fixed.

The aphelions are immovable by Prop. XI, Book I; and so are the planes of the orbits, by Prop. I of the same Book. And if the planes are fixed, the nodes must be so too. It is true, that some inequalities may arise from the mutual actions of the planets and comets in their revolutions; but these will be so small, that they may be here passed by.

COR. I. The fixed stars are immovable, seeing they keep the same position to the aphelions and nodes of the planets.

COR. II. And since these stars are liable to no sensible parallax from the annual motion of the earth, they can have no force, because of their immense distance, to produce any sensible effect in our system. Not to mention that the fixed stars, everywhere promiscuously dispersed in the heavens, by their contrary attractions destroy their mutual actions, by Prop. LXX, Book I.

SCHOLIUM

Since the planets near the sun (viz., Mercury, Venus, the earth, and Mars) are so small that they can act with but little force upon one another, therefore their aphelions and nodes must be fixed, except so far as they are disturbed by the actions of Jupiter and Saturn, and other higher bodies. And hence we may find, by the theory of gravity, that their aphelions move forwards a little, in respect of the fixed stars, and that as the $\frac{3}{2}$th power of their several distances from the sun. So that if the aphelion of Mars, in the space of a hundred years, is carried forwards 33′ 20″, in respect of the fixed stars, the aphelions of the earth, of Venus, and of Mercury, will in a hundred years be carried forwards 17′ 40″, 10′ 53″, and 4′ 16″, respectively. But these motions are so inconsiderable, that we have neglected them in this Proposition.

PROPOSITION XV. PROBLEM I

To find the principal diameters of the orbits of the planets.

They are to be taken as the $\frac{2}{3}$th power of the periodic times, by Prop. xv, Book I, and then to be severally augmented in the proportion of the sum of the masses of matter in the sun and each planet to the first of two mean proportionals between that sum and the quantity of matter in the sun, by Prop. LX, Book I.

PROPOSITION XVI. PROBLEM II

To find the eccentricities and aphelions of the planets.

This Problem is resolved by Prop. XVIII, Book I.

PROPOSITION XVII. THEOREM XV

That the diurnal motions of the planets are uniform, and that the libration of the moon arises from its diurnal motion.

The Proposition is proved from the first Law of Motion, and Cor. XXII, Prop. LXVI, Book I. Jupiter, with respect to the fixed stars, revolves in $9^h. 56^m.$; Mars in $24^h. 39^m.$; Venus in about $23^h.$; the earth in $23^h. 56^m.$; the sun in $25\frac{1}{2}^d.$, and the moon in $27^d. 7^h. 43^m.$ These things appear by the Phenomena. The spots in the sun's body return to the same situation on the sun's disk, with respect to the earth, in $27\frac{1}{2}$ days; and therefore with respect to the fixed stars the sun revolves in about $25\frac{1}{2}$ days. But because the lunar day, arising from its uniform revolution about its axis, is menstrual, *that is, equal to the time of its periodic revolution in its orbit,* hence the same face of the moon will be always nearly turned to the upper focus of its orbit; but, as the situation of that focus requires, will deviate a little to one side and to the other from the earth in the lower focus; and this is the libration in longitude; for the libration in latitude arises from the moon's latitude, and the inclination of its axis to the plane of the ecliptic. This theory of the libration of the moon, Mr. *N. Mercator,* in his *Astronomy,* published at the beginning of the year 1676, explained more fully out of the letters I sent him. The utmost satellite of Saturn seems to revolve about its axis with a motion like this of the moon, respecting Saturn continually with the same face; for in its revolution round Saturn, as often as it comes to the eastern part of its orbit, it is scarcely visible, and generally quite disappears; this is probably occasioned by some spots in that part of its body, which is then turned towards the earth, as M. *Cassini* has observed. So also the utmost satellite of Jupiter seems to revolve about its axis with a like motion, because in that part of its body which is turned from Jupiter it has a spot, which always appears as if it were in Jupiter's own body, whenever the satellite passes between Jupiter and our eye.

PROPOSITION XVIII. THEOREM XVI

That the axes of the planets are less than the diameters drawn perpendicular to the axes.

The equal gravitation of the parts on all sides would give a spherical figure to the planets, if it was not for their diurnal revolution in a circle. By that circular motion it comes to pass that the parts receding from the axis endeavor to ascend about the equator; and therefore if the matter is in a fluid state, by its ascent towards the equator it will enlarge the diameters there, and by its descent towards the poles it will shorten the axis. So the diameter of Jupiter (by the concurring observations of astronomers) is found shorter between pole and pole than from east to west. And, by the same argument, if our earth was not higher about the equator than at the poles, the seas would subside about the poles, and, rising towards the equator, would lay all things there under water.

PROPOSITION XIX. PROBLEM III

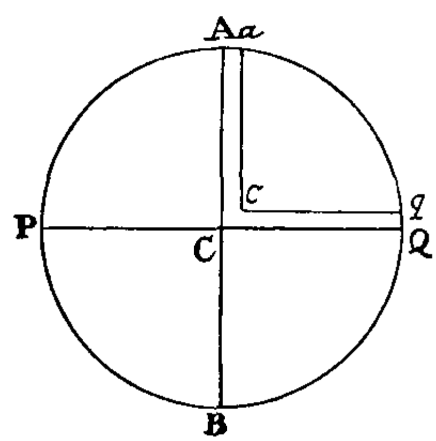
To find the proportion of the axis of a planet to the diameters perpendicular thereto.

Our countryman, Mr. *Norwood,* measuring a distance of 905751 feet of *London* measure between *London* and *York,* in 1635, and observing the difference of latitudes to be 2° 28′, determined the measure of one degree to be 367196 feet of *London* measure, that is, 57300 *Paris* toises. M. *Picard,* measuring an arc of one degree, and 22′ 55″ of the meridian between *Amiens* and *Malvoisine,* found an arc of one degree to be 57060 *Paris* toises. M. *Cassini,* the father, measured the distance upon the meridian from the town of *Collioure* in *Roussillon* to the Observatory of *Paris*; and his son added the distance from the Observatory to the Citadel of *Dunkirk.* The whole distance was 486156½ toises and the difference of the latitudes of *Collioure* and *Dunkirk* was 8 degrees, and 31′ 11⅚″. Hence an arc of one degree appears to be 57061 *Paris* toises. And from these measures we conclude that the circumference of the earth is 123249600, and its semidiameter 19615800 *Paris* feet, upon the supposition that the earth is of a spherical figure.

In the latitude of *Paris* a heavy body falling in a second of time describes 15 *Paris* feet, 1 inch, 1 7/9 lines, as above, that is, 2173 7/9 lines. The weight of the body is diminished by the weight of the ambient air. Let us suppose the weight lost thereby to be $\frac{1}{11000}$ part of the whole weight; then that heavy body falling in a vacuum will describe a height of 2174 lines in one second of time.

A body in every sidereal day of 23^h. 56^m. 4^s. uniformly revolving in a circle at the distance of 19615800 feet from the centre, in one second of time describes an arc of 1433.46 feet; the versed sine of which is 0.0523656 1 feet, or 7.54064 lines. And therefore the force with which bodies descend in the latitude of *Paris* is to the centrifugal force of bodies in the equator arising from the diurnal motion of the earth as 2174 to 7.54064.

The centrifugal force of bodies in the equator is to the centrifugal force with which bodies recede directly from the earth in the latitude of *Paris,* 48° 50′ 10″, as the square of the ratio of the radius to the cosine of the latitude, that is, as 7.54064 to 3.267. Add this force to the force with which bodies descend by their weight in the latitude of *Paris,* and a body, in the latitude of *Paris,* falling by its whole undiminished force of gravity, in the time of one second, will describe 2177.267 lines, or 15 *Paris* feet, 1 inch, and 5.267 lines. And the total force of gravity in that latitude will be to the centrifugal force of bodies in the equator of the earth as 2177.267 to 7.54064, or as 289 to 1.



Therefore if APBQ represent the figure of the earth, now no longer spherical, but generated by the rotation of an ellipse about its lesser axis PQ; and ACQ*qca* a canal full of water, reaching from the pole Q*q* to the centre C*c,* and thence rising to the equator A*a*; the weight of the water in the leg of the canal AC*ca* will be to the weight of water in the other leg QC*cq* as 289 to 288, because the centrifugal force arising from the circular motion sustains and takes off one of the 289 parts of the weight (in the one leg), and the weight of 288 in the other sustains the rest. But by computation (from Cor. II, Prop XCI, Book I) I find, that, if the matter of the earth was all uniform, and without any motion, and its axis PQ were to the diameter AB as 100 to 101, the force of gravity in the place Q towards the earth would be to the force of gravity in the same place Q towards a sphere described about the centre C with the radius PC, or QC, as 126 to 125. And, by the same argument, the force of gravity in the place A towards the spheroid generated by the rotation of the ellipse APBQ about the axis AB is to the force of gravity in the same place A, towards the sphere described about the centre C with the radius AC, as 125 to 126. But the force of gravity in the place A towards the earth is a mean proportional between the forces of gravity towards the spheroid and this sphere; because the sphere, by having its diameter PQ diminished in the proportion of 101 to 100, is transformed into the figure of the earth; and this figure, by having a third diameter perpendicular to the two diameters AB and PQ diminished in the same proportion, is converted into the said spheroid; and the force of gravity in A, in either case, is diminished nearly in the same proportion. Therefore the force of gravity in A towards the sphere described about the centre C with the radius AC, is to the force of gravity in A towards the earth as 126 is to 125½. And the force of gravity in the place Q towards the sphere described about the centre C with the radius QC, is to the force of gravity in the place A towards the sphere described about the centre C with the radius AC, in the proportion of the diameters (by Prop. LXXII, Book I), that is, as 100 to 101. If, therefore, we compound those three proportions 126 to 125, 126 to 125½, and 100 to 101, into one, the force of gravity in the place Q towards the earth will be to the force of gravity in the place A towards the earth as 126 · 126 · 100 to 125 · 125½ · 101; or as 501 to 500.

Now since (by Cor. III, Prop. XCI, Book I) the force of gravity in either leg of the canal AC*ca*, or QC*cq*, is as the distance of the places from the centre of the earth, if those legs are conceived to be divided by transverse, parallel, and equidistant surfaces, into parts proportional to the wholes, the weights of any number of parts in the one leg AC*ca* will be to the weights of the same number of parts in the other leg as their magnitudes and the accelerative forces of their gravity conjointly, that is, as 101 to 100, and 500 to 501, or as 505 to 501. And therefore if the centrifugal force of every part in the leg *ACca,* arising from the diurnal motion, was to the weight of the same part as 4 to 505, so that from the weight of every part, conceived to be divided into 505 parts, the centrifugal force might take off four of those parts, the weights would remain equal in each leg, and therefore the fluid would rest in an equilibrium. But the centrifugal force of every part is to the weight of the same part as 1 to 289; that is, the centrifugal force, which should be $\frac{4}{505}$ parts of the weight, is only $\frac{1}{289}$ part thereof. And, therefore, I say, by the rule of proportion, that if the centrifugal force $\frac{4}{505}$ make the height of the water in the leg *ACca* to exceed the height of the water in the leg *QCcq* by $\frac{1}{100}$ part of its whole height, the centrifugal force $\frac{1}{289}$ will make the excess of the height in the leg *ACca* only $\frac{1}{289}$ part of the height of the water in the other leg *QCcq*; and therefore the diameter of the earth at the equator[1] is to its diameter from pole to pole as 230 to 229. And since the mean semidiameter of the earth, according to *Picard's* mensuration, is 19615800 *Paris* feet, or 3923.16 miles (reckoning 5000 feet to a mile), the earth will be higher at the equator than at the poles by 85472 feet, or 17⅒ miles. And its height at the equator will be about 19658600 feet, and at the poles 19573000 feet.

If, the density and periodic time of the diurnal revolution remaining the same, the planet was greater or less than the earth, the proportion of the centrifugal force to that of gravity, and therefore also of the diameter between the poles to the diameter at the equator, would likewise remain the same. But if the diurnal motion was accelerated or retarded in any proportion, the centrifugal force would be augmented or diminished nearly in the same proportion squared; and therefore the difference of the diameters will be increased or diminished in the same squared ratio, very nearly. And if the density of the planet was augmented or diminished in any proportion, the force of gravity tending towards it would also be augmented or diminished in the same proportion: and the difference of the diameters on the contrary would be diminished in proportion as the force of gravity is augmented, and augmented in proportion as the force of gravity is diminished. Therefore, since the earth, in respect of the fixed stars, revolves in $23^h. 56^m.$, but Jupiter in $9^h. 56^m.$, and the squares of their periodic times are as 29 to 5, and their densities as 400 to 94½, the difference of the diameters of Jupiter

[1 Appendix, Note 41.]

will be to its lesser diameter as $\frac{29}{5} \cdot \frac{400}{94\frac{1}{2}} \cdot \frac{1}{229}$ to 1, or as 1 to $9\frac{1}{3}$, nearly. Therefore the diameter of Jupiter from east to west is to its diameter from pole to pole nearly as $10\frac{1}{3}$ to $9\frac{1}{3}$. Therefore, since its greatest diameter is 37″, its lesser diameter lying between the poles will be 33″ 25‴. Add thereto about 3″ for the irregular refraction of light, and the apparent diameters of this planet will become 40″ and 36″ 25‴; which are to each other as $11\frac{1}{6}$ to $10\frac{1}{6}$, very nearly. These things are so upon the supposition that the body of Jupiter is uniformly dense. But now if its body be denser towards the plane of the equator than towards the poles, its diameters may be to each other as 12 to 11, or 13 to 12, or perhaps as 14 to 13.

And *Cassini* observed, in the year 1691, that the diameter of Jupiter reaching from east to west is greater by about a fifteenth part than the other diameter. Mr. *Pound* with his 123-foot telescope, and an excellent micrometer, measured the diameters of Jupiter in the year 1719, and found them as follows:

The times			Greatest diameter	Lesser diameter	The diameters to each other
	days	hours	Parts	Parts	
January	28	6	13.40	12.28	As 12 to 11
March	6	7	13.12	12.20	$13\frac{3}{4}$ to $12\frac{3}{4}$
March	9	7	13.12	12.08	$12\frac{2}{3}$ to $11\frac{2}{3}$
April	9	9	12.32	11.48	$14\frac{1}{2}$ to $13\frac{1}{2}$

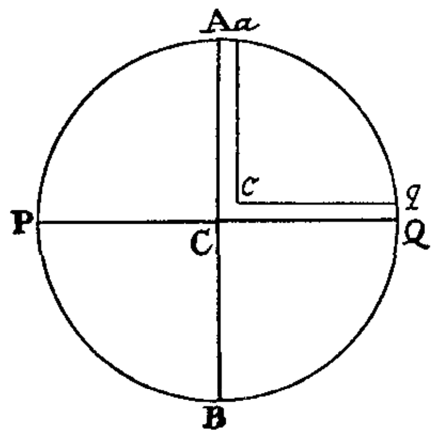
So that the theory agrees with the phenomena; for the planets are more heated by the sun's rays towards their equators, and therefore are a little more condensed by that heat than towards their poles.

Moreover, that there is a diminution of gravity occasioned by the diurnal rotation of the earth, and therefore the earth rises higher there than it does at the poles (supposing that its matter is uniformly dense), will appear by the experiments of pendulums related under the following Proposition.

PROPOSITION XX. PROBLEM IV

To find and compare together the weights of bodies in the different regions of our earth.

Because the weights of the unequal legs of the canal of water ACQ*qca* are equal; and the weights of the parts proportional to the whole legs, and alike situated in them, are one to another as the weights of the wholes, and therefore equal between themselves; the weights of equal parts, and alike situated in the legs, will be inversely as the legs, that is, inversely as 230 to 229. And the case is the same in all homogeneous equal bodies alike situated in the legs of the canal. Their weights are inversely as the legs, that is, inversely as the distances of the bodies from the centre of the earth. Therefore if the bodies are situated in the uppermost parts of the canals, or on the surface of the earth, their weights will be one to another inversely as their distances from the centre. And, by the same argument, the weights in all other places round the whole surface of the earth are inversely as the distances of the places from the centre; and, therefore, on the hypothesis of the earth's being a spheroid, are given in proportion.



From this arises the theorem, that the increase of weight in passing from the equator to the poles is nearly as the versed sine of double the latitude; or, which comes to the same thing, as the square of the sine of the latitude; and the arcs of the degrees of latitude in the meridian increase nearly in the same proportion. And, therefore, since the latitude of *Paris* is 48° 50′, that of places under the equator 00° 00′, and that of places under the poles 90°; and the versed sines of double those arcs are 1133400000 and 20000, the radius being 10000; and the force of gravity at the pole is to the force of gravity at the equator as 230 to 229; and the excess of the force of gravity at the pole to the force of gravity at the equator is as 1 to 229; the excess of the force of gravity in the latitude of *Paris* will be to the force of gravity at the equator as $1\cdot\frac{11334}{20000}$ to 229, or as 5667 to 2290000. And therefore the whole forces of gravity in those places will be one to the other as 2295667 to 2290000. Therefore, since the lengths of pendulums vibrating in equal times are as the forces of gravity, and in the latitude of *Paris* the length of a pendulum vibrating seconds is 3 *Paris* feet and $8\frac{1}{2}$ lines, or rather, because of the weight of the air, $8\frac{5}{9}$ lines, the length of a pendulum vibrating in the same time under the equator will be shorter by 1.087 lines. And by a like calculus the following table is made (see p. 430).

By this table, therefore, it appears that the inequality of degrees is so small that the figure of the earth, in geographical matters, may be considered as spherical; especially if the earth be a little denser towards the plane of the equator than towards the poles.

Now several astronomers, sent into remote countries to make astronomical observations, have found that pendulum clocks do accordingly move slower near the equator than in our climates. And, first of all, in the year 1672, M. *Richer* took notice of it in the island of *Cayenne;* for when, in the month of *August,* he was observing the transits of the fixed stars over the meridian, he found his clock to go slower than it ought in respect of the mean motion of the sun at the rate of $2^m. 28^s.$ a day. Therefore, fitting up a simple pendulum to vibrate in seconds, which were measured by an excellent clock, he observed the length of that simple pendulum; and this he

Latitude of the place	Length of the pendulum		Measure of one degree in the meridian	Latitude of the place	Length of the pendulum		Measure of one degree in the meridian
degrees	feet	lines	toises[1]	degrees	feet	lines	toises[1]
0	3 .	7.468	56637	6	3 .	8.461	57022
5	3 .	7.482	56642	7	3 .	8.494	57035
10	3 .	7.526	56659	8	3 .	8.528	57048
15	3 .	7.596	56687	9	3 .	8.561	57061
20	3 .	7.692	56724	50	3 .	8.594	57074
25	3 .	7.812	56769	55	3 .	8.756	57137
30	3 .	7.948	56823	60	3 .	8.907	57196
35	3 .	8.099	56882	65	3 .	9.044	57250
40	3 .	8.261	56945	70	3 .	9.162	57295
1	3 .	8.294	56958	75	3 .	9.258	57332
2	3 .	8.327	56971	80	3 .	9.329	57360
3	3 .	8.361	56984	85	3 .	9.372	57377
4	3 .	8.394	56997	90	3 .	9.387	57382
45	3 .	8.428	57010				

did over and over every week for ten months together. And upon his return to *France,* comparing the length of that pendulum with the length of the pendulum at *Paris* (which was 3 *Paris* feet and 8⅗ lines), he found it shorter by 1¼ lines.

Afterwards, our friend Dr. *Halley,* about the year 1677, arriving at the island of St. *Helena,* found his pendulum clock to go slower there than at

[[1] Appendix, Note 42.]

London without marking the difference. But he shortened the rod of his clock by more than 1/8 of an inch, or 1½ lines; and, to effect this, because the length of the screw at the lower end of the rod was not sufficient, he interposed a wooden ring between the nut and the ball.

Then, in the year 1682, M. *Varin* and M. *des Hayes* found the length of a simple pendulum vibrating in seconds at the *Royal Observatory* of *Paris* to be 3 feet and 8 5/9 lines. And by the same method in the island of *Goree,* they found the length of an isochronal pendulum to be 3 feet and 6 5/9 lines, differing from the former by two lines. And in the same year, going to the islands of *Guadaloupe* and *Martinico,* they found that the length of an isochronal pendulum in those islands was 3 feet and 6½ lines.

After this, M. *Couplet,* the son, in the month of *July,* 1697, at the *Royal Observatory* of *Paris,* so fitted his pendulum clock to the mean motion of the sun, that for a considerable time together the clock agreed with the motion of the sun. In *November* following, upon his arrival at *Lisbon,* he found his clock to go slower than before at the rate of $2^m. 13^s.$ in 24 hours. And the following *March,* coming to *Paraiba,* he found his clock to go slower than at *Paris,* and at the rate $4^m. 12^s.$ in 24 hours; and he affirms that the pendulum vibrating in seconds was shorter at *Lisbon* by 2½ lines, and at *Paraiba* by 3⅔ lines, than at *Paris.* He would have done better to have reckoned those differences 1⅓ and 2 5/9: for these differences correspond to the differences of the times $2^m. 13^s.$ and $4^m. 12^s.$ But this gentleman's observations are so gross, that we cannot confide in them.

In the following years, 1699 and 1700, M. *des Hayes,* making another voyage to *America,* determined that in the islands of *Cayenne* and *Granada* the length of the pendulum vibrating in seconds was a small matter less than 3 feet and 6½ lines; that in the island of St. *Christopher* it was 3 feet and 6¾ lines; and in the island of St. *Domingo* 3 feet and 7 lines.

And, in the year 1704, *Feuillé,* at *Puerto Bello* in *America,* found that the length of the pendulum vibrating in seconds was 3 *Paris* feet and only 5 7/12 lines, that is, almost 3 lines shorter than at *Paris;* but the observation was faulty. For afterwards, going to the island of *Martinico,* he found the length of the isochronal pendulum there 3 *Paris* feet and 5 10/12 lines.

Now the latitude of *Paraiba* is 6° 38′ south; that of *Puerto Bello,* 9° 33′ north; and the latitudes of the islands *Cayenne, Goree, Guadaloupe, Marti-*

nico, Granada, St. *Christopher,* and St. *Domingo,* are respectively 4° 55′, 14° 40″, 15° 00′, 14° 44′, 12° 06′, 17° 19′, and 19° 48′, north. And the excesses of the length of the pendulum at *Paris* above the lengths of the isochronal pendulums observed in those latitudes are a little greater than by the table of the lengths of the pendulum before computed. And therefore the earth is a little higher under the equator than by the preceding calculus, and a little denser at the centre than in mines near the surface, unless, perhaps, the heats of the torrid zone have a little extended the length of the pendulums.

For M. *Picard* has observed that a rod of iron, which in frosty weather in the winter season was one foot long, when heated by fire, was lengthened into one foot and ¼ line. Afterwards M. *de la Hire* found that a rod of iron, which in the like winter season was 6 feet long, when exposed to the heat of the summer sun, was extended into 6 feet and ⅔ line. In the former case the heat was greater than in the latter; but in the latter it was greater than the heat of the external parts of a human body; for metals exposed to the summer sun acquire a very considerable degree of heat. But the rod of a pendulum clock is never exposed to the heat of the summer sun, nor ever acquires a heat equal to that of the external parts of a human body; and, therefore, though the 3-foot rod of a pendulum clock will indeed be a little longer in the summer than in the winter season, yet the difference will scarcely amount to ¼ line. Therefore the total difference of the lengths of isochronal pendulums in different climates cannot be ascribed to the difference of heat; nor indeed to the mistakes of the *French* astronomers. For although there is not a perfect agreement between their observations, yet the errors are so small that they may be neglected; and in this they all agree, that isochronal pendulums are shorter under the equator than at the *Royal Observatory* of *Paris,* by a difference not less than 1¼ lines, nor greater than 2⅔ lines. By the observations of M. *Richer,* in the island of *Cayenne,* the difference was 1¼ lines. That difference being corrected by those of M. *des Hayes,* becomes 1½ lines or 1¾ lines. By the less accurate observations of others, the same was made about 2 lines. And this disagreement might arise partly from the errors of the observations, partly from the dissimilitude of the internal parts of the earth, and the height of mountains; partly from the different temperatures of the air.

I take an iron rod 3 feet long to be shorter by a sixth part of one line in winter time with us here in *England* than in the summer. Because of the great heats under the equator, subtract this quantity from the difference of $1\frac{1}{4}$ lines observed by M. *Richer,* and there will remain $1\frac{1}{12}$ lines, which agrees very well with $1\ \frac{87}{1000}$ lines, obtained earlier by the theory. M. *Richer* repeated his observations, made in the island of *Cayenne,* every week for ten months together, and compared the lengths of the pendulum which he had there noted in the iron rods with the lengths thereof which he observed in *France.* This diligence and care seems to have been wanting to the other observers. If this gentleman's observations are to be depended on, the earth is higher under the equator than at the poles, and that by an excess of about 17 miles; as appeared above by the theory.

PROPOSITION XXI. THEOREM XVII

That the equinoctial points go backwards, and that the axis of the earth, by a nutation in every annual revolution, twice vibrates towards the ecliptic, and as often returns to its former position.

The Proposition appears from Cor. xx, Prop. LXVI, Book I; but that motion of nutation must be very small, and, indeed, scarcely perceptible.

PROPOSITION XXII. THEOREM XVIII

That all the motions of the moon, and all the inequalities of those motions, follow from the principles which we have laid down.[1]

That the greater planets, while they are carried about the sun, may in the meantime carry other lesser planets, revolving about them, and that those lesser planets must move in ellipses which have their foci in the centres of the greater, appears from Prop. LXV, Book I. But then their motions will be in several ways disturbed by the action of the sun, and they will suffer such inequalities as are observed in our moon. Thus our moon (by Cor. II, III, IV, and V, Prop. LXVI, Book I) moves faster, and, by a radius drawn to the earth, describes an area greater for the time, and has its orbit less curved, and therefore approaches nearer to the earth in the syzygies than in the quadratures, excepting so far as these effects are hindered by the motion of eccentricity; for (by Cor. IX, Prop. LXVI, Book I) the eccentricity is greatest when the apogee of the moon is in the syzygies, and least when the same is in the quadratures; and upon this account the perigean moon is swifter, and nearer

[[1] Appendix, Note 43.]

to us, but the apogean moon slower and farther from us, in the syzygies than in the quadratures. Moreover, the apogee goes forwards, and the nodes backwards; and this is done not with a regular but an unequal motion. For (by Cor. VII and VIII, Prop. LXVI, Book I) the apogee goes more swiftly forwards in its syzygies, more slowly backwards in its quadratures; and, by the excess of its progress above its regress, advances yearly forwards. But the nodes, on the contrary (by Cor. XI, Prop. LXVI, Book I), are quiescent in their syzygies, and go fastest back in their quadratures. Further, the greatest latitude of the moon (by Cor. X, Prop. LXVI, Book I) is greater in the quadratures of the moon than in its syzygies. And (by Cor. VI, Prop. LXVI, Book I) the mean motion of the moon is slower in the perihelion of the earth than in its aphelion. And these are the principal inequalities (of the moon) taken notice of by astronomers.

But there are yet other inequalities not observed by former astronomers, by which the motions of the moon are so disturbed that to this day we have not been able to bring them under any certain rule. For the velocities or hourly motions of the apogee and nodes of the moon, and their equations, as well as the difference between the greatest eccentricity in the syzygies and the least eccentricity in the quadratures, and that inequality which we call the variation, are (by Cor. XIV, Prop. LXVI, Book I) in the course of the year augmented and diminished as the cube of the sun's apparent diameter. And besides (by Cor. I and II, Lem. X, and Cor. XVI, Prop. LXVI, Book I) the variation is augmented and diminished nearly as the square of the time between the quadratures. But, in astronomical calculations, this inequality is commonly thrown into and combined with the equation of the moon's centre.

PROPOSITION XXIII. PROBLEM V

To derive the unequal motions of the satellites of Jupiter and Saturn from the motions of our moon.

From the motions of our moon we deduce the corresponding motions of the moons or satellites of Jupiter in this manner, by Cor. XVI, Prop. LXVI, Book I. The mean motion of the nodes of the outmost satellite of Jupiter is to the mean motion of the nodes of our moon in a proportion compounded of the squared ratio of the periodic times of the earth about the sun to the periodic times of Jupiter about the sun, and the simple ratio of the periodic time of the satellite about Jupiter to the periodic time of our moon about the earth; and therefore, those nodes, in the space of an hundred years, are carried 8°24′ backwards or forwards. The mean motions of the nodes of the inner satellites are to the mean motion of the nodes of the outmost as their periodic times are to the periodic time of the former, by the same Corollary, and are thence given. And the forward motion of the apse of every satellite is to the backward motion of its nodes as the motion of the apogee of our moon to the motion of its nodes (by the same Corollary), and is thence given. But the motions of the apsides thus found must be diminished in the proportion of 5 to 9, or of about 1 to 2, on account of a cause which I cannot here stop to explain. The greatest equations of the nodes, and of the apse of every satellite, are to the greatest equations of the nodes, and apogee of our moon respectively, as the motions of the nodes and apsides of the satellites, in the time of one revolution of the former equations, to the motions of the nodes and apogee of our moon, in the time of one revolution of the latter equations. The variation of a satellite seen from Jupiter is to the variation of our moon in the same proportion as the whole motions of their nodes respectively during the times in which the satellite and our moon (after parting from) are revolved (again) to the sun, by the same Corollary; and therefore in the outmost satellite[1] the variation does not exceed $5^{s}\ 12^{th}$.

PROPOSITION XXIV. THEOREM XIX

That the flux and reflux of the sea arise from the actions of the sun and moon.

By Cor. XIX and XX, Prop. LXVI, Book I, it appears that the waters of the sea ought twice to rise and twice to fall every day, as well lunar as solar; and that the greatest height of the waters in the open and deep seas ought to follow the approach of the luminaries to the meridian of the place by a less interval than six hours; as happens in all that eastern tract of the *Atlantic* and *Ethiopic* seas between *France* and the *Cape of Good Hope;* and on the coasts of *Chile* and *Peru* in the *South Sea*; in all which shores the flood falls out about the second, third, or fourth hour, unless where the motion propagated from the deep ocean is by the shallowness of the channels, through which it passes to some particular places, retarded to the fifth, sixth, or seventh hour, and even later. The hours I reckon from the ap-

[[1] Appendix, Note 44.]

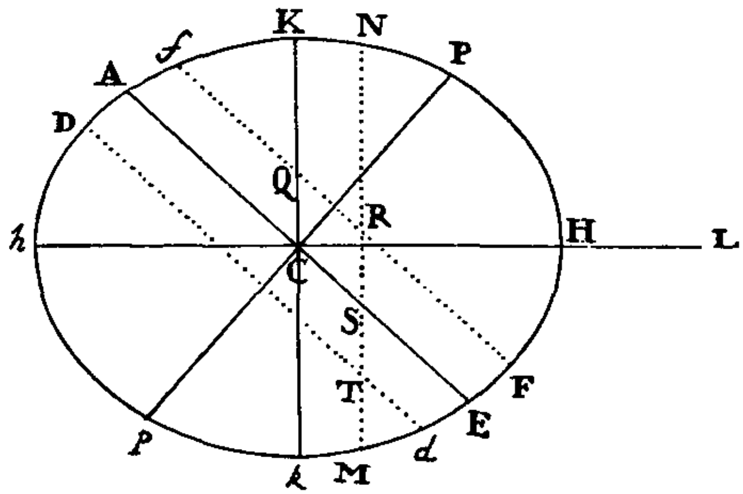
proach of each luminary to the meridian of the place, as well under as above the horizon; and by the hours of the lunar day I understand the 24th parts of that time which the moon, by its apparent diurnal motion, employs to come about again to the meridian of the place which it left the day before. The force of the sun or moon in raising the sea is greatest in the approach of the luminary to the meridian of the place; but the force impressed upon the sea at that time continues a little while after the impression, and is afterwards increased by a new though less force still acting upon it. This makes the sea rise higher and higher, till, this new force becoming too weak to raise it any more, the sea rises to its greatest height. And this will come to pass, perhaps, in one or two hours, but more frequently near the shores in about three hours, or even more, where the sea is shallow.

The two luminaries excite two motions, which will not appear distinctly, but between them will arise one mixed motion compounded out of both. In the conjunction or opposition of the luminaries their forces will be conjoined, and bring on the greatest flood and ebb. In the quadratures the sun will raise the waters which the moon depresses, and depress the waters which the moon raises, and from the difference of their forces the smallest of all tides will follow. And because (as experience tells us) the force of the moon is greater than that of the sun, the greatest height of the waters will happen about the third lunar hour. Out of the syzygies and quadratures, the greatest tide, which by the single force of the moon ought to fall out at the third lunar hour, and by the single force of the sun at the third solar hour, by the compounded forces of both must fall out in an intermediate time that approaches nearer to the third hour of the moon than to that of the sun. And, therefore, while the moon is passing from the syzygies to the quadratures, during which time the third hour of the sun precedes the third hour of the moon, the greatest height of the waters will also precede the third hour of the moon, and that, by the greatest interval, a little after the octants of the moon; and, by like intervals, the greatest tide will follow the third lunar hour, while the moon is passing from the quadratures to the syzygies. Thus it happens in the open sea; for in the mouths of rivers the greater tides come later to their height.

But the effects of the luminaries depend upon their distances from the earth; for when they are less distant, their effects are greater, and when more distant, their effects are less, and that as the cube of their apparent diameter. Therefore it is that the sun, in the winter time, being then in its perigee, has a greater effect, and makes the tides in the syzygies somewhat greater, and those in the quadratures somewhat less than in the summer season; and every month the moon, while in the perigee, raises greater tides than at the distance of fifteen days before or after, when it is in its apogee. From this it comes to pass that two highest tides do not follow one the other in two immediately succeeding syzygies.

The effect of either luminary doth likewise depend upon its declination or distance from the equator; for if the luminary was placed at the pole, it would constantly attract all the parts of the waters without any intensification or remission of its action, and could cause no reciprocation of motion. And, therefore, as the luminaries decline from the equator towards either pole they will, by degrees, lose their force, and on this account will excite lesser tides in the solstitial than in the equinoctial syzygies. But in the solstitial quadratures they will raise greater tides than in the quadratures about the equinoxes; because the force of the moon, then situated in the equator, most exceeds the force of the sun. Therefore the greatest tides occur in those syzygies, and the least in those quadratures, which happen about the time of both equinoxes; and the greatest tide in the syzygies is always succeeded by the least tide in the quadratures, as we find by experience. But, because the sun is less distant from the earth in winter than in summer, it comes to pass that the greatest and least tides more frequently appear before than after the vernal equinox, and more frequently after than before the autumnal.

Moreover, the effects of the luminaries depend upon the latitudes of places. Let A*p*EP represent the earth covered with deep waters; C its centre; P, *p* its poles; AE the equator; F any place without the equator; F*f* the parallel of the place; D*d* the correspondent parallel on the other side of the equator; L the place of the moon three hours before; H the place of the earth directly under it; *h* the opposite place; K, *k* the places at 90 degrees distance; CH, C*h*, the greatest heights of the sea from the centre of the earth; and CK, C*k* its least heights: and if with the axes H*h*, K*k*, an ellipse is described, and by the revolution of that ellipse about its longer axis H*h* a spheroid HPK*hpk* is formed, this spheroid will nearly represent the figure of the sea; and CF, C*f*, CD, C*d*, will represent the heights of the sea in the places F*f*, D*d*. But further; in the said revolution of the ellipse any point N describes the circle NM cutting the parallels F*f*, D*d*, in any places RT, and the equator AE in S; CN will represent the height of the sea in all those places, R, S, T, situated in this circle. Therefore, in the diurnal revolution of any place F, the greatest flood will be in F, at the third hour after the appulse of the moon to the meridian above the horizon; and afterwards the greatest ebb in Q, at the third hour after the setting of the moon; and then the greatest flood in *f*, at the third hour after the appulse of the moon to the meridian under the horizon; and, lastly, the greatest ebb in Q, at the third hour after the rising of the moon; and the latter flood in *f* will be less than the preceding flood in F. For the whole sea is divided into two hemispherical floods, one in the hemisphere KH*k* on the north side, the other in the opposite hemisphere K*hk*, which we may therefore call the northern and the southern floods. These floods, being always opposite the one to the other, come by turns to the meridians of all places, after an interval of twelve lunar hours. And as the northern countries partake more of the northern flood, and the southern countries more of the southern flood, thence arise tides, alternately greater and less in all places without the equator, in which the luminaries rise and set. But the greatest tide will happen when the moon declines towards the vertex of the place, about the third hour after the appulse of the moon to the meridian above the horizon; and when the moon changes its declination *to the other side of the equator*, that which was the greater tide will be changed into a lesser. And the greatest difference of the floods will fall out about the times of the solstices; especially if the ascending node of the moon is about the first of Aries. So it is found by experience that the morning tides in winter exceed those of the evening, and the evening tides



in summer exceed those of the morning; at *Plymouth* by the height of one foot, but at *Bristol* by the height of fifteen inches, according to the observations of *Colepress* and *Sturmy*.

But the motions which we have been describing suffer some alteration from that force of reciprocation,[1] which the waters, being once moved, retain a little while *by their inertia*. Whence it comes to pass that the tides may continue for some time, though the actions of the luminaries should cease. This power of retaining the impressed motion lessens the difference of the alternate tides, and makes those tides which immediately succeed after the syzygies greater, and those which follow next after the quadratures less. And hence it is that the alternate tides at *Plymouth* and *Bristol* do not differ much more from each other than by the height of a foot or fifteen inches, and that the greatest tides at those ports are not the first but the third after the syzygies. And, besides, all the motions are retarded in their passage through shallow channels, so that the greatest tides of all, in some straits and mouths of rivers, are the fourth or even the fifth after the syzygies.

Further, it may happen that the tide may be propagated from the ocean through different channels towards the same port, and may pass quicker through some channels than through others; in which case the same tide, divided into two or more succeeding one another, may compound new motions of different kinds. Let us suppose two equal tides flowing towards the same port from different places, one preceding the other by six hours; and suppose the first tide to happen at the third hour of the approach of the moon to the meridian of the port. If the moon at the time of the approach to the meridian was in the equator, every six hours alternately there would arise equal floods, which, meeting with as many equal ebbs, would so balance each other that for that day the water would stagnate and be quiet. If the moon then declined from the equator, the tides in the ocean would be alternately greater and less, as was said; and from thence two greater and two less tides would be alternately propagated towards that port. But the two greater floods would make the greatest height of the waters to fall out in the middle time between both; and the greater and less floods would make the waters to rise to a mean height in the middle time between them, and in the middle time between the two less floods the waters would rise to their least height. Thus in the space of twenty-four hours the waters

[[1] Appendix, Note 45.]

would come, not twice, as commonly, but once only to their greatest, and once only to their least height; and their greatest height, if the moon declined towards the elevated pole, would happen at the sixth or thirtieth hour after the approach of the moon to the meridian; and when the moon changed its declination, this flood would be changed into an ebb. An example of this Dr. *Halley* has given us, from the observations of seamen in the port of *Batshaw,* in the kingdom of *Tunquin,* in the latitude of 20° 50′ north. In that port, on the day which follows after the passage of the moon over the equator, the waters stagnate: when the moon declines to the north, they begin to flow and ebb, not twice, as in other ports, but once only every day; and the flood happens at the setting, and the greatest ebb at the rising of the moon. This tide increases with the declination of the moon till the seventh or eighth day; then for the seven or eight days following it decreases at the same rate as it had increased, and ceases when the moon changes its declination, crossing over the equator to the south. After which the flood is immediately changed into an ebb; and thenceforth the ebb happens at the setting and the flood at the rising of the moon; till the moon, again passing the equator, changes its declination. There are two inlets to this port and the neighboring channels, one from the seas of *China,* between the continent and the island of *Leuconia*; the other from the *Indian* sea, between the continent and the island of *Borneo*. But whether there be really two tides propagated through the said channels, one from the *Indian* sea in the space of twelve hours, and one from the sea of *China* in the space of six hours, which therefore happening at the third and ninth lunar hours, by being compounded together, produce those motions; or whether there be any other circumstances in the state of those seas, I leave to be determined by observations on the neighboring shores.

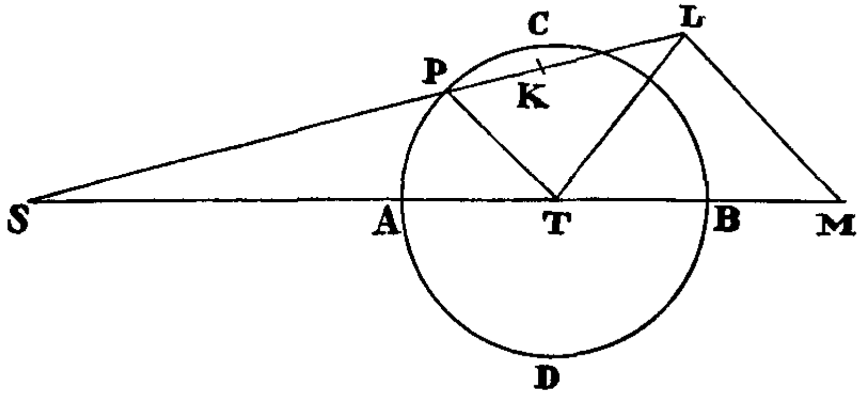
Thus I have explained the causes of the motions of the moon and of the sea. Now it is fit to subjoin something concerning the amount of those motions.

PROPOSITION XXV. PROBLEM VI

To find the forces with which the sun disturbs the motions of the moon.

Let S represent the sun, T the earth, P the moon, CADB the moon's orbit. In SP take SK equal to ST; and let SL be to SK as the square of SK to

SP: draw LM parallel to PT; and if ST or SK is supposed to represent the accelerated force of gravity of the earth towards the sun, SL will represent the accelerative force of gravity of the moon towards the sun. But that force is compounded of the parts SM and LM, of which the force LM, and that part of SM which is represented by TM, disturb the motion of the moon, as we have shown in Prop. LXVI, Book I, and its Corollaries. Foras-

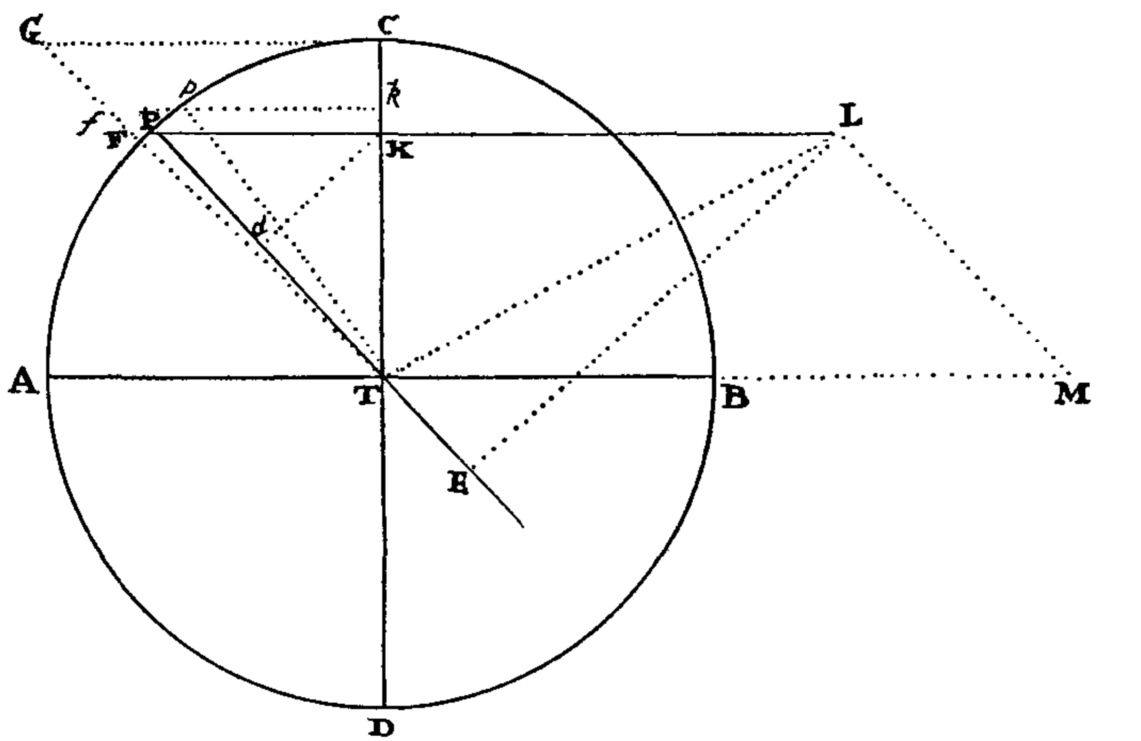


much as the earth and moon are revolved about their common centre of gravity, the motion of the earth about that centre will be also disturbed by the like forces; but we may consider the sums both of the forces and of the motions as in the moon, and represent the sum of the forces by the lines TM and ML, which are analogous to them both. The force ML (in its mean amount) is to the centripetal force by which the moon may be retained in its orbit revolving about the earth at rest, at the distance PT, as the square of the ratio of the periodic time of the moon about the earth to the periodic time of the earth about the sun (by Cor. XVII, Prop. LXVI, Book I); that is, as the square of $27^{d}. 7^{h}. 43^{m}.$ to $365^{d}. 6^{h}. 9^{m}.$; or as 1000 to 178725; or as 1 to $178\frac{29}{40}$. But in Prop. IV of this Book we found, that, if both earth and moon were revolved about their common centre of gravity, the mean distance of the one from the other would be nearly $60\frac{1}{2}$ mean semidiameters of the earth; and the force by which the moon may be kept revolving in its orbit about the earth at rest at the distance PT of $60\frac{1}{2}$ semidiameters of the earth, is to the force by which it may be revolved in the same time, at the distance of 60 semidiameters, as $60\frac{1}{2}$ is to 60: and this force is to the force of gravity with us very nearly as 1 is to $60 \cdot 60$. Therefore the mean force ML is to the force of gravity on the surface of our earth as $1 \cdot 60\frac{1}{2}$ to $60 \cdot 60 \cdot 60 \cdot 178\frac{29}{40}$, or as 1 to 638092.6; hence, by the proportion of the lines TM, ML, the force TM is also given; and these are the forces with which the sun disturbs the motions of the moon. Q.E.I.

PROPOSITION XXVI. PROBLEM VII

To find the hourly increment of the area which the moon, by a radius drawn to the earth, describes in a circular orbit.

We have above shown that the area which the moon describes by a radius drawn to the earth is proportional to the time of description, excepting so far as the moon's motion is disturbed by the action of the sun; and here we propose to investigate the inequality of the moment, or hourly increment *of that area or motion so disturbed.* To render the calculus more easy, we shall suppose the orbit of the moon to be circular, and neglect all inequalities but that only which is now under consideration; and, because of the immense distance of the sun, we shall further suppose that the lines SP and ST are parallel. By this means, the force LM will be always reduced to its mean amount TP, as well as the force TM to its mean amount 3PK.



These forces (by Cor. II of the Laws of Motion) compose the force TL; and this force, by letting fall the perpendicular LE upon the radius TP, is resolved into the forces TE, EL; of which the force TE, acting constantly in the direction of the radius TP, neither accelerates nor retards the description of the area TPC made by that radius TP; but EL, *acting on the radius* TP in a perpendicular direction, accelerates or retards *the description of* *the area* in proportion as it accelerates or retards the moon. That acceleration of the moon, in its passage from the quadrature C to the conjunction A, is in every moment of time as the *generating* accelerative force EL, that is, as $\frac{3PK \cdot TK}{TP}$. Let the time be represented by the mean motion of the moon, or (which comes to the same thing) by the angle CTP, or even by the arc CP. At right angles upon CT erect CG equal to CT; and, supposing the quadrantal arc AC to be divided into an infinite number of equal parts P*p*, &c., these *parts* may represent the like *infinite* number of the equal parts of time. Let fall *pk* perpendicular on CT, and draw TG meeting with KP, *kp* produced in F and *f*; then will FK be equal to TK, and K*k* be to PK as P*p* is to T*p*, that is, in a given proportion; and therefore FK · K*k*, or the area FK*kf*, will be as $\frac{3PK \cdot TK}{TP}$, that is, as EL; and, compounding, the whole area GCKF will vary as the sum of all the forces EL impressed upon the moon in the whole time CP; and therefore also as the velocity generated by that sum, that is, as the acceleration of the description of the area CTP, or as the increment of the moment *thereof*. The force by which the moon may in its periodic time CADB of $27^{d}.\ 7^{h}.\ 43^{m}.$ be retained revolving about the earth at rest at the distance TP, would cause a body falling in the time CT to describe the length ½CT, and at the same time to acquire a velocity equal to that with which the moon is moved in its orbit. This appears from Cor. IX, Prop. IV, Book I. But since K*d*, drawn perpendicular on TP, is *but* a third part of EL, and *equal* to the half of TP, or ML, in the octants, the force EL in the octants, where it is greatest, will exceed the force ML in the ratio of 3 to 2; and therefore will be to that force by which the moon in its periodic time may be retained revolving about the earth at rest as 100 is to ⅔ · 17872½, or 11915; and in the time CT will generate a velocity equal to $\frac{100}{11915}$ parts of the velocity of the moon; but in the time CPA will generate a greater velocity in the proportion of CA to CT or TP. Let the greatest force EL in the octants be represented by the area FK · K*k*, or by the rectangle ½TP · P*p*, which is equal thereto; and the velocity which that greatest force can generate in any time CP will be to the velocity which any other lesser force EL can generate in the same time as the rectangle ½TP · CP to the area KCGF; but the velocities generated in the whole time CPA will be one to the other as the rectangle ½TP · CA is to the triangle TCG, or as the quadrantal arc CA is to the radius TP; and therefore the latter velocity generated in the whole time will be $\frac{100}{11915}$ parts of the velocity of the moon. To this velocity of the moon, which is proportional to the mean moment of the area (supposing this mean moment to be represented by the number 11915), we add and subtract the half of the other velocity; the sum 11915 + 50, or 11965, will represent the greatest moment of the area in the syzygy A; and the difference 11915 − 50, or 11865, the least moment thereof in the quadratures. Therefore the areas which in equal times are described in the syzygies and quadratures are one to the other as 11965 to 11865. And if to the least moment 11865 we add a moment which shall be to 100, the difference of the two former moments, as the trapezium FKCG is to the triangle TCG, or, which comes to the same thing, as the square of the sine PK is to the square of the radius TP (that is, as P*d* to TP), the sum will represent the moment of the area when the moon is in any intermediate place P.

But these things take place only in the hypothesis that the sun and the earth are at rest, and that the synodical revolution of the moon is finished in $27^d. 7^h. 43^m$. But since the moon's synodical period is really $29^d. 12^h. 44^m$., the increments of the moments must be enlarged in the same proportion as the time is, that is, in the proportion of 1080853 to 1000000. Upon which account, the whole increment, which was $\frac{100}{11915}$ parts of the mean moment, will now become $\frac{100}{11023}$ parts thereof; and therefore the moment of the area in the quadrature of the moon will be to the moment thereof in the syzygy as 11023 − 50 to 11023 + 50; or as 10973 to 11073; and to the moment thereof, when the moon is in any intermediate place P, as 10973 to 10973 + P*d*; that is, supposing TP = 100.

The area, therefore, which the moon, by a radius drawn to the earth, describes in the several little equal parts of time, is nearly as the sum of the number 219.46, and the versed sine of the double distance of the moon from the nearest quadrature, considered in a circle which hath unity for its radius. Thus it is when the variation in the octants is in its mean quantity. But if the variation there is greater or less, that versed sine must be augmented or diminished in the same proportion.

PROPOSITION XXVII. PROBLEM VIII

From the hourly motion of the moon to find its distance from the earth.

The area which the moon, by a radius drawn to the earth, describes in every moment of time, is as the hourly motion of the moon and the square of the distance of the moon from the earth conjointly. And therefore the distance of the moon from the earth varies directly as the square root of the area and inversely as the square root of the hourly motion, taken jointly. Q.E.I.

COR. I. Hence the apparent diameter of the moon is given; for it is inversely as the distance of the moon from the earth. Let astronomers try how accurately this rule agrees with the phenomena.

COR. II. Hence also the orbit of the moon may be more exactly defined from the phenomena than hitherto could be done.

PROPOSITION XXVIII. PROBLEM IX

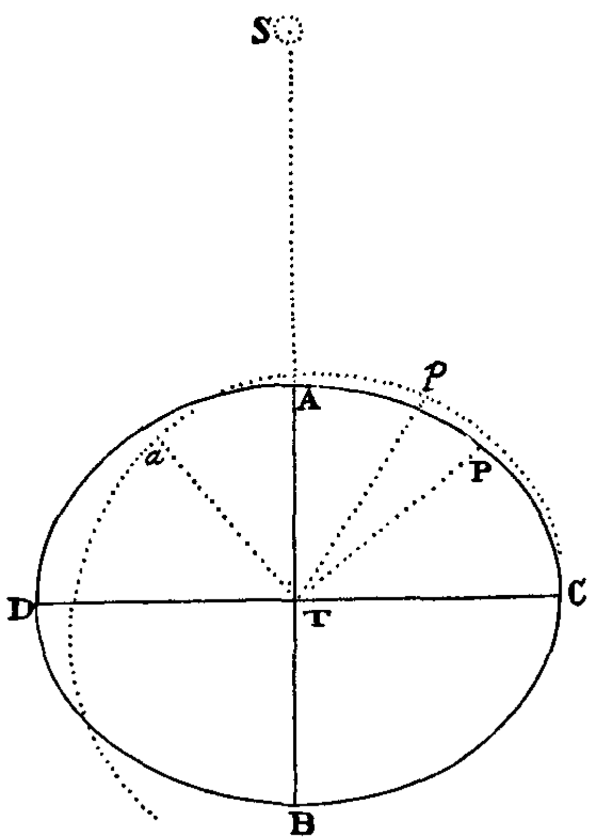
To find the diameters of the orbit, in which, without eccentricity, the moon would move.

The curvature of the orbit which a body describes, if attracted in lines perpendicular to the orbit, is directly as the force of attraction, and inversely as the square of the velocity. I estimate the curvatures of lines compared one with another according to the evanescent ratio of the sines or tangents of their angles of contact to equal radii,[1] supposing those radii to be infinitely diminished. But the attraction of the moon towards the earth in the syzygies is the excess of its gravity towards the earth above the force of the sun 2PK (see fig., Prop. xxv), by which force the accelerative gravity of the moon towards the sun exceeds the accelerative gravity of the earth towards the sun or is exceeded by it. But in the quadratures that attraction is the sum of the gravity of the moon towards the earth, and the sun's force KT, by which the moon is attracted towards the earth. And these attractions, putting N for $\frac{AT+CT}{2}$, are nearly as $\frac{178725}{AT^2}-\frac{2000}{CT\cdot N}$ and $\frac{178725}{CT^2}+\frac{1000}{AT\cdot N}$, or as $178725N\cdot CT^2-2000AT^2\cdot CT$, and $178725N\cdot AT^2+1000CT^2\cdot AT$. For if the accelerative gravity of the moon towards the earth

[1 Appendix, Note 46.]

be represented by the number 178725, the mean force ML, which in the quadratures is PT or TK, and draws the moon towards the earth, will be 1000, and the mean force TM in the syzygies will be 3000; from which, if we subtract the mean force ML, there will remain 2000, the force by which the moon in the syzygies is drawn from the earth, and which we above called 2PK. But the velocity of the moon in the syzygies A and B is to its velocity in the quadratures C and D as CT is to AT, and as the moment of the area, which the moon by a radius drawn to the earth describes in the syzygies, is to the moment of that area *described* in the quadratures conjointly; that is, as 11073CT is to 10973AT. Take the square of this ratio inversely, and the former ratio directly, and the curvature of the moon's orbit in the syzygies will be to the curvature thereof in the quadratures as $120406729 \cdot 178725AT^2 \cdot CT^2 \cdot N - 120406729 \cdot 2000AT^4 \cdot CT$ is to $122611329 \cdot 178725AT^2 \cdot CT^2 \cdot N + 122611329 \cdot 1000CT^4 \cdot AT$, that is, as $2151969AT \cdot CT \cdot N - 24081AT^3$ is to $2191371AT \cdot CT \cdot N + 12261CT^3$.

Because the figure of the moon's orbit is unknown, let us, in its stead, assume the ellipse DBCA, in the centre of which we suppose the earth to be situated, and the greater axis DC to lie between the quadratures as the lesser AB between the syzygies. But since the plane of this ellipse is revolved about the earth by an angular motion, and the orbit, whose curvature we now examine, should be described in a plane void of such motion, we are to consider the figure which the moon, while it is revolved in that ellipse, describes in this plane, that is to say, the figure C*pa,* the several points *p* of which are found by assuming any point P in the ellipse, which may represent the place of the moon, and drawing T*p* equal to TP in such manner that the angle PT*p* may be equal to the apparent motion of the sun from the time of the last quadrature



in C; or (which comes to the same thing) that the angle C*Tp* may be to the angle CTP as the time of the synodic revolution of the moon to the time of the periodic revolution thereof, or as $29^d. 12^h. 44^m.$ to $27^d. 7^h. 43^m.$ If, therefore, in this proportion we take the angle CT*a* to the right angle CTA, and make T*a* of equal length with TA, we shall have *a* the lower and C the upper apse of this orbit C*pa*. But, by computation, I find that the difference between the curvature of this orbit C*pa* at the vertex *a*, and the curvature of a circle described about the centre T with the interval TA, is to the difference between the curvature of the ellipse at the vertex A, and the curvature of the same circle, as the square of the ratio of the angle CTP to the angle CT*p*; and that the curvature of the ellipse in A is to the curvature of that circle as the square of the ratio of TA is to TC; and the curvature of that circle is to the curvature of a circle described about the centre T with the radius TC as TC is to TA; but that the curvature of this *last arch* is to the curvature of the ellipse in C as the square of the ratio of TA is to TC; and that the difference between the curvature of the ellipse in the vertex C, and the curvature of this last circle, is to the difference between the curvature of the figure T*pa*, at the vertex C, and the curvature of this same *last* circle, as the square of the ratio of the angle CT*p* to the angle CTP. All these relations are easily derived from the sines of the angles of contact, and of the differences of those angles. But, by comparing those ratios, we find the curvature of the figure C*pa* at *a* to be to its curvature at C as $AT^3 - \frac{16824}{100000} CT^2 \cdot AT$ is to $CT^3 + \frac{16824}{100000} AT^2 \cdot CT$; where the number $\frac{16824}{100000}$ represents the difference of the squares of the angles CTP and CT*p*, divided by the square of the lesser angle CTP; or (which is all one) the difference of the squares of the times $27^d. 7^h. 43^m.$ and $29^d. 12^h. 44^m.$ divided by the square of the time $27^d. 7^h. 43^m.$

Since, therefore, *a* represents the syzygy of the moon, and C its quadrature, the ratio now found must be the same as the ratio of the curvature of the moon's orb in the syzygies to the curvature thereof in the quadratures, which we found above. Therefore, in order to find the ratio of CT to AT, let us multiply the extremes and the means of the resulting proportion, and the terms which come out, divided by $AT \cdot CT$, yield the following equation: $2062.79CT^4 - 2151969N \cdot CT^3 + 368676N \cdot AT \cdot CT^2 + 36342AT^2 \cdot CT^2 - 362047N \cdot AT^2 \cdot CT + 2191371N \cdot AT^3 + 4051.4AT^4 = 0$. Now if for the half sum N of the terms AT and CT we put 1, and x for their half difference, then CT will be $= 1 + x$, and AT $= 1 - x$. And substituting those values in the equation, after resolving thereof, we shall find $x = 0.00719$; and from thence the semidiameter CT $= 1.00719$, and the semidiameter AT $= 0.99281$, which numbers are nearly as $70\frac{1}{24}$, and $69\frac{1}{24}$. Therefore the moon's distance from the earth in the syzygies is to its distance in the quadratures (setting aside the consideration of eccentricity) as $69\frac{1}{24}$ to $70\frac{1}{24}$; or, in round numbers, as 69 to 70.

PROPOSITION XXIX. PROBLEM X

To find the variation of the moon.

This inequality is due partly to the elliptic figure of the moon's orbit, partly to the inequality of the moments of the area which the moon by a radius drawn to the earth describes. If the moon P revolved in the ellipse DBCA about the earth quiescent in the centre of the ellipse, and by the radius TP, drawn to the earth, described the area CTP, proportional to the time *of description;* and the greatest semidiameter CT of the ellipse was to the least TA as 70 to 69; the tangent of the angle CTP would be to the tangent of the angle of the mean motion, computed from the quadrature C, as the semidiameter TA of the ellipse to its semidiameter TC, or as 69 to 70. But the description of the area CTP as the moon advances from the quadrature to the syzygy, ought to be in such manner accelerated, that the moment of the area in the moon's syzygy may be to the moment thereof in its quadrature as 11073 to 10973; and that the excess of the moment in any intermediate place P above the moment in the quadrature may be as the square of the sine of the angle CTP; which we may effect with accuracy enough, if we diminish the tangent of the angle CTP in the ratio obtained from the square root of the ratio of the number 10973 to the number 11073, that is, in the ratio of the number 68.6877 to the number 69. On this account the tangent of the angle CTP will now be to the tangent of the mean motion as 68.6877 is to 70; and the angle CTP in the octants, where the mean motion is 45°, will be found 44° 27′ 28″, which subtracted from 45°, the angle of the mean motion, leaves the greatest variation 32′ 32″. Thus it would be, if the moon, in passing from the quadrature to the syzygy, described an angle CTA of 90° only. But because of the motion of the earth, by which the sun is apparently transferred forwards, the moon, before it overtakes the sun, describes an angle CT*a*, greater than a right angle, in the ratio of the time of the synodic revolution of the moon to the time of its periodic revolution, that is, in the ratio of $29^d. 12^h. 44^m.$ to $27^d. 7^h. 43^m.$ Whence it comes to pass that all the angles about the centre T are dilated in the same ratio; and the greatest variation, which otherwise would be *but* 32′ 32″, now augmented in the said proportion, becomes 35′ 10″.

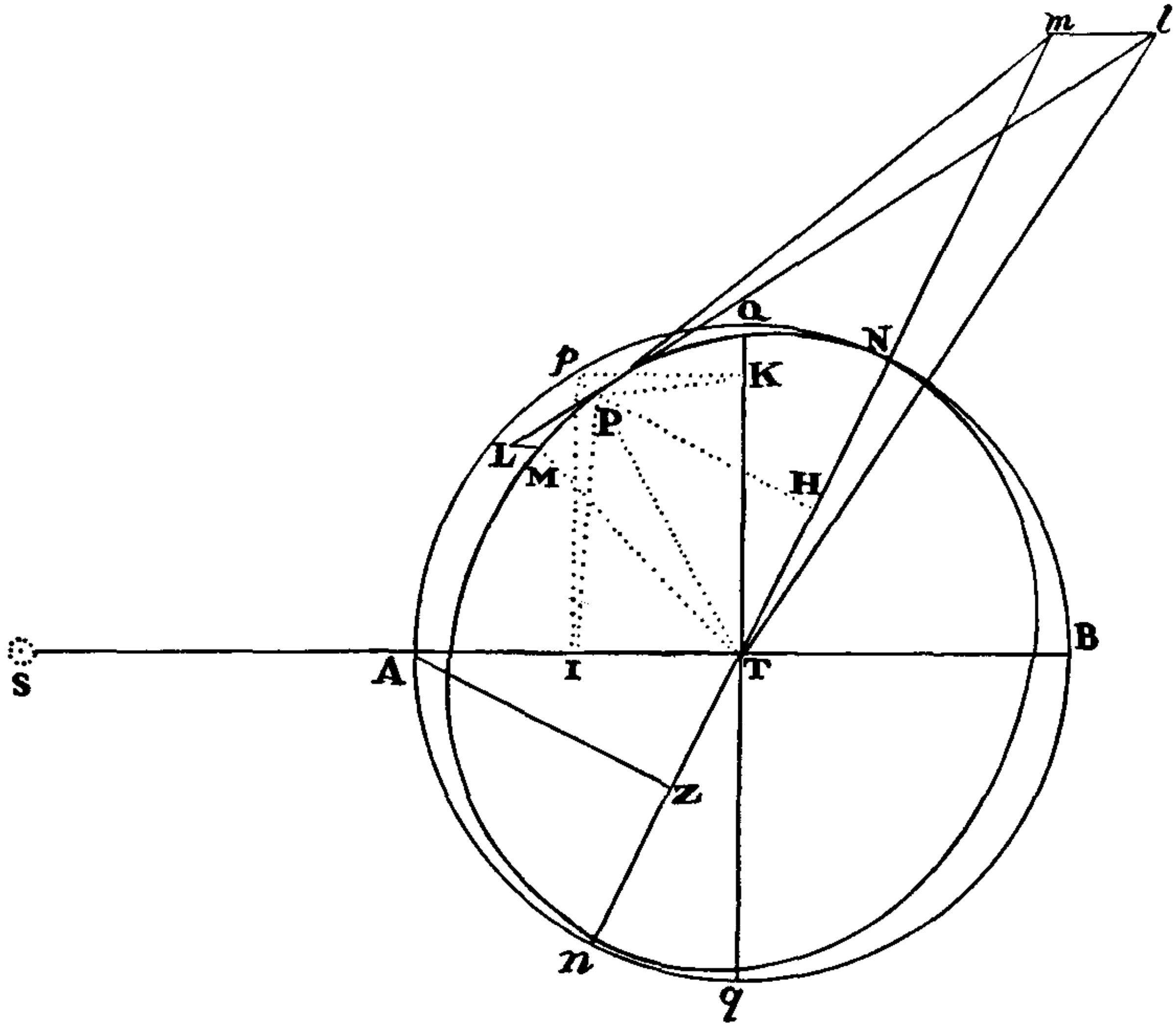
And this is its magnitude in the mean distance of the sun from the earth, neglecting the differences which may arise from the curvature of the great orbit, and the stronger action of the sun upon the moon when horned and new, than when gibbous and full. In other distances of the sun from the earth, the greatest variation is in a ratio compounded, directly of the square of the ratio of the time of the synodic revolution of the moon (the time of the year being given), and inversely as the cube of the ratio of the distance of the sun from the earth. And, therefore, in the apogee of the sun, the greatest variation is 33′ 14″, and in its perigee 37′ 11″, if the eccentricity of the sun is to the transverse semidiameter of the great orbit as $16^{15}/_{16}$ to 1000.

Hitherto we have investigated the variation in an orbit not eccentric, in which, to wit, the moon in its octants is always in its mean distance from the earth. If the moon, on account of its eccentricity, is more or less removed from the earth than if placed in this orbit, the variation may be something greater, or something less, than according to this rule. But I leave the excess or defect to the determination of astronomers from the phenomena.

PROPOSITION XXX. PROBLEM XI

To find the hourly motion of the nodes of the moon in a circular orbit.

Let S represent the sun, T the earth, P the moon, NP*n* the orbit of the moon, N*pn* the orthographic projection of the orbit upon the plane of the ecliptic; N, *n* the nodes, *n*TN*m* the line of the nodes produced indefinitely; PI, PK perpendiculars upon the lines ST, Q*q*; P*p* a perpendicular upon the plane of the ecliptic; A, B the moon's syzygies in the plane of the ecliptic; AZ a perpendicular let fall upon N*n*, the line of the nodes; Q, *q* the quadratures of the moon in the plane of the ecliptic, and *p*K a perpendicular on the line Q*q* lying between the quadratures. The force of the sun to disturb the motion of the moon (by Prop. xxv) is twofold, one proportional to the



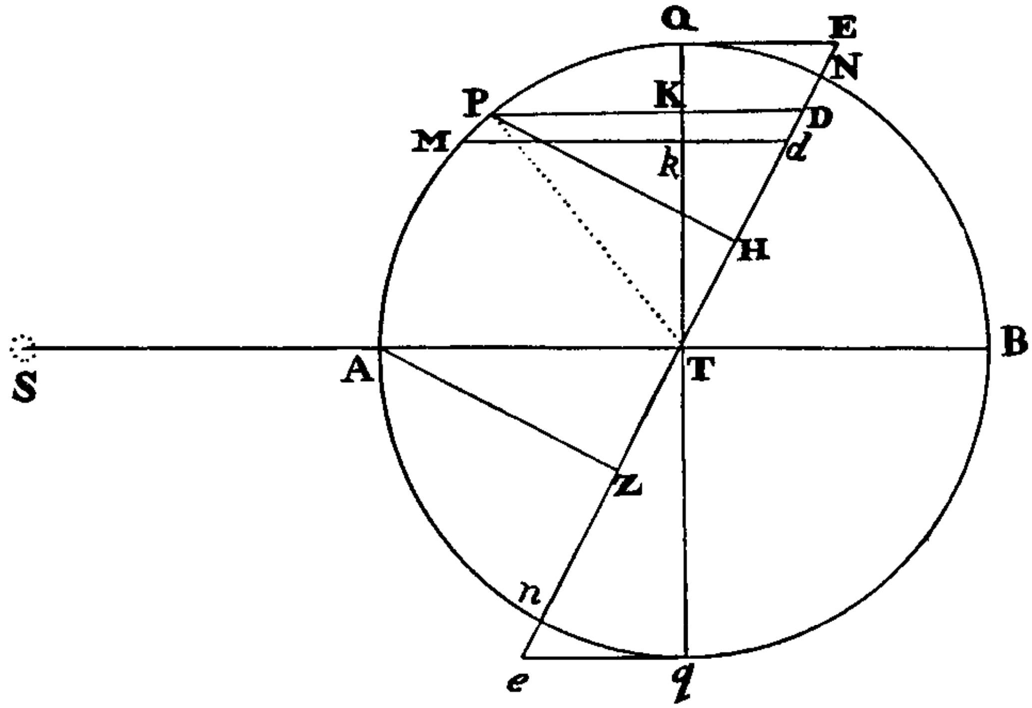
line LM, the other to the line MT, in the scheme of that Proposition; and the moon by the former force is drawn towards the earth, by the latter towards the sun, in a direction parallel to the right line ST joining the earth and the sun. The former force LM acts in the direction of the plane of the moon's orbit, and therefore makes no change upon the situation thereof, and is upon that account to be neglected; the latter force MT, by which the plane of the moon's orbit is disturbed, is the same with the force 3PK or 3IT. And this force (by Prop. xxv) is to the force by which the moon may, in its periodic times, be uniformly revolved in a circle about the earth at rest, as 3IT to the radius of the circle multiplied by the number 178.725, or as IT to the radius thereof multiplied by 59.575. But in this calculus, and all that follows, I consider all the lines drawn from the moon to the sun as parallel to the line which joins the earth and the sun; because what inclination there is almost as much diminishes all effects in some cases as it augments them in others; and we are now inquiring after the mean motions of the nodes, neglecting such niceties as are of no moment and would only serve to render the calculus more complicated.

Now suppose PM to represent an arc which the moon describes in the least moment of time, and ML a little line, the half of which the moon, by the impulse of the said force 3IT, would describe in the same time; and joining PL, MP, let them be produced to *m* and *l*, where they cut the plane of the ecliptic, and upon T*m* let fall the perpendicular PH. Now, since the right line ML is parallel to the plane of the ecliptic, and therefore can never meet with the right line *ml* which lies in that plane, and yet both those right lines lie in one common plane LMP*ml*, they will be parallel, and upon that account the triangles LMP, *lm*P will be similar. And seeing MP*m* lies in the plane of the orbit, in which the moon did move while in the place P, the point *m* will fall upon the line N*n*, which passes through the nodes N, *n*, of that orbit. And because the force by which the half of the little line LM is generated, if the whole had been together, and at once impressed in the point P, would have generated that whole line, and caused the moon to move in the arc whose chord is LP; that is to say, would have transferred the moon from the plane MP*m*T into the plane LP*l*T; therefore the angular motion of the nodes generated by that force will be equal to the angle *m*T*l*. But *ml* is to *m*P as ML to MP; and since MP, because of the time given, is also given, *ml* will be as the rectangle ML · *m*P, that is, as the rectangle IT · *m*P. And if T*ml* is a right angle, the angle *m*T*l* will be as $\frac{ml}{\mathrm{T}m}$, and therefore as $\frac{\mathrm{IT}\cdot \mathrm{P}m}{\mathrm{T}m}$, that is (because T*m* and *m*P, TP and PH are proportional), as $\frac{\mathrm{IT}\cdot\mathrm{PH}}{\mathrm{TP}}$; and, therefore, because TP is given, as IT · PH. But if the angle T*ml* or STN is oblique, the angle *m*T*l* will be yet less, in proportion of the sine of the angle STN to the radius, or AZ to AT. And therefore the velocity of the nodes is as IT · PH · AZ, or as the product of the sines of the three angles TPI, PTN, and STN.

If these are right angles, as happens when the nodes are in the quadratures, and the moon in the syzygy, the little line *ml* will be removed to an infinite distance, and the angle *mTl* will become equal to the angle *mPl*. But in this case the angle *mPl* is to the angle PTM, which the moon in the same time by its apparent motion describes about the earth, as 1 to 59.575. For the angle *mPl* is equal to the angle LPM, that is, to the angle of the moon's deflection from a rectilinear path; which angle, if the gravity of the moon should have then ceased, the said force of the sun 3IT would by itself have generated in that given time; and the angle PTM is equal to the angle of the moon's deflection from a rectilinear path; which angle, if the force of the sun 3IT should have then ceased, the force alone by which the moon is retained in its orbit would have generated in the same time. And these forces (as we have above shown) are the one to the other as 1 to 59.575. Since, therefore, the mean hourly motion of the moon (in respect of the fixed stars) is $32^{m}\ 56^{s}\ 27^{th}\ 12\frac{1}{2}^{iv}$ the hourly motion of the node in this case will be $33^{s}\ 10^{th}\ 33^{iv}\ 12^{v}$. But in other cases the hourly motion will be to $33^{s}\ 10^{th}\ 33^{iv}\ 12^{v}$ as the product of the sines of the three angles TPI, PTN, and STN (or of the distances of the moon from the quadrature, of the moon from the node, and of the node from the sun) to the cube of the radius. And as often as the sine of any angle is changed from positive to negative, and from negative to positive, so often must the regressive be changed into a progressive, and the progressive into a regressive motion. Whence it comes to pass that the nodes are progressive as often as the moon happens to be placed between either quadrature, and the node nearest to that quadrature. In other cases they are regressive, and by the excess of the regress above the progress, they are monthly transferred backwards.

COR. 1. Hence if from P and M, the extreme points of a least arc PM, on the line Q*q* joining the quadratures we let fall the perpendiculars PK, M*k*, and produce the same till they cut the line of the nodes N*n* in D and *d*, the hourly motion of the nodes will be as the area MPD*d*, and the square of the line AZ, conjointly. For let PK, PH, and AZ be the three said sines, viz., PK the sine of the distance of the moon from the quadrature, PH the sine of the distance of the moon from the node, and AZ the sine of the distance of the node from the sun; and the velocity of the node will be as the product PK · PH · AZ. But PT is to PK as PM to K*k*; and, therefore, because PT and PM are given, K*k* will be as PK. Likewise AT is to PD as AZ is to PH, and therefore PH is as the rectangle PD · AZ; and, by compounding those proportions, PK · PH is as the solid content K*k* · PD · AZ, and PK · PH · AZ as K*k* · PD · AZ^2; that is, as the area PD*d*M and AZ^2 conjointly. Q.E.D.

COR. II. In any given position of the nodes their mean hourly motion is half their hourly motion in the moon's syzygies; and therefore is to 16^s 35^{th} 16^{iv} 36^v as the square of the sine of the distance of the nodes from



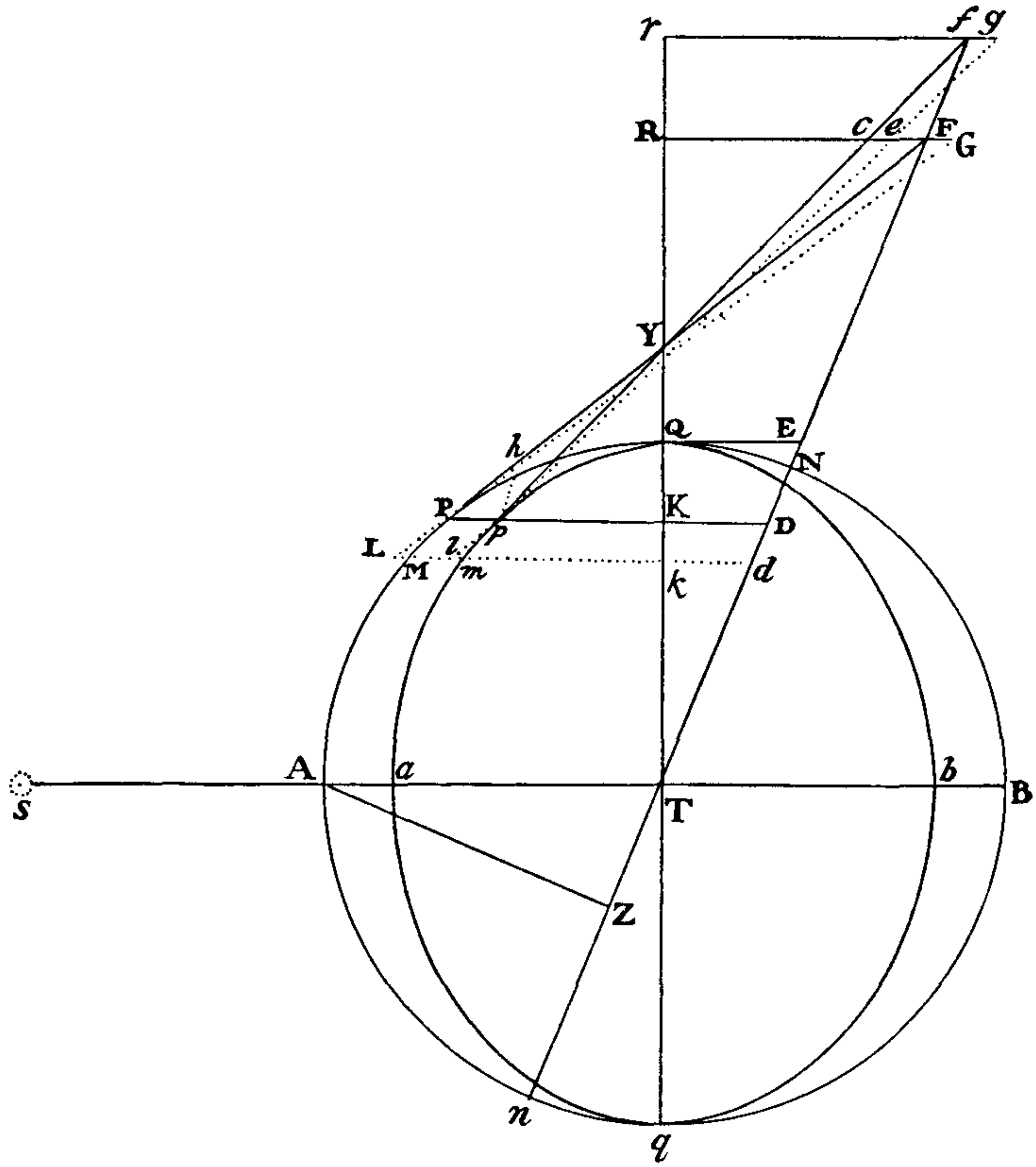
the syzygies is to the square of the radius, or as AZ^2 to AT^2. For if the moon, by an uniform motion, describes the semicircle QA*q,* the sum of all the areas PD*d*M, during the time of the moon's passage from Q to M, will make up the area QM*d*E, terminating at the tangent QE of the circle; and by the time that the moon has arrived at the point *n,* that sum will make up the whole area EQA*n* described by the line PD: but when the moon proceeds from *n* to *q,* the line PD will fall without the circle, and describe the area *nqe,* terminating at the tangent *qe* of the circle, which area, because the nodes were before regressive, but are now progressive, must be subtracted from the former area, and, being itself equal to the area QEN, will leave the semicircle NQA*n*. While, therefore, the moon describes a semicircle, the sum of all the areas PD*d*M will be the area of that semicircle; and while the moon describes a complete circle, the sum of those areas will be the area of the whole circle. But the area PD*d*M, when the moon is in the syzygies, is the rectangle of the arc PM into the radius PT; and the sum of all the areas, *every one* equal to this area, in the time that the moon describes a complete circle, is the rectangle of the whole circumference into the radius of the circle; and this rectangle, being double the area of the circle, will be double the former sum. If, therefore, the nodes went on with that velocity uniformly continued which they acquire in the moon's syzygies, they would describe a space double that which they describe in fact; and, therefore, the mean motion, by which, if uniformly continued, they would describe the same space with that which they do in fact describe by an unequal motion, is *but* one-half of that motion which they are possessed of in the moon's syzygies. Wherefore, since their greatest hourly motion, if the nodes are in the quadratures, is $33^{s}\ 10^{th}\ 33^{iv}\ 12^{v}$, their mean hourly motion in this case will be $16^{s}\ 35^{th}\ 16^{iv}\ 36^{v}$. And seeing the hourly motion of the nodes is everywhere as AZ^2 and the area PD*d*M conjointly, and, therefore, in the moon's syzygies, the hourly motion of the nodes is as AZ^2 and the area PD*d*M conjointly, that is (because the area PD*d*M described in the syzygies is given), as AZ^2, therefore the mean motion also will be as AZ^2; and, therefore, when the nodes are without the quadratures, this motion will be to $16^{s}\ 35^{th}\ 16^{iv}\ 36^{v}$ as AZ^2 to AT^2. Q.E.D.

PROPOSITION XXXI. PROBLEM XII

To find the hourly motion of the nodes of the moon in an elliptic orbit.

Let Q*pmaq* represent an ellipse described with the greater axis Q*q* and the less axis *ab*; QA*q*B a circle circumscribed; T the earth in the common centre of both; S the sun; *p* the moon moving in this ellipse; and *pm* an arc which it describes in the least moment of time; N and *n* the nodes joined by the line N*n*; *p*K and *mk* perpendiculars upon the axis Q*q*, produced both ways till they meet the circle in P and M, and the line of the nodes in D and *d*. And if the moon, by a radius drawn to the earth, describes an area proportional to the time *of description,* the hourly motion of the node in the ellipse will be as the area *p*D*dm* and AZ^2 conjointly.

For let PF touch the circle in P, and produced meet TN in F; and *pf* touch the ellipse in *p,* and produced meet the same TN in *f,* and both tangents concur in the axis TQ at Y; and let ML represent the space which the moon, by the impulse of the above-mentioned force 3IT or 3PK, would describe with a transverse motion, in the meantime while revolving in the circle it describes the arc PM; and *ml* denote the space which the moon revolving in the ellipse would describe in the same time by the impulse of the same force 3IT or 3PK; and let LP and *lp* be produced till they meet the plane of the ecliptic in G and *g*, and FG and *fg* be joined, of which FG



produced may cut *pf, pg,* and TQ, in *c, e,* and R respectively; and *fg* produced may cut TQ in *r*. Because the force 3IT or 3PK in the circle is to the force 3IT or 3*p*K in the ellipse as PK to *p*K, or as AT to *a*T, the space ML generated by the former force will be to the space *ml* generated by the latter as PK to *p*K; that is, because of the similar figures PYK*p* and FYR*c*, as FR to *c*R. But (because of the similar triangles PLM, PGF) ML is to FG as

PL is to PG, that is (on account of the parallels L*k*, PK, GR), as *pl* is to *pe*, that is (because of the similar triangles *plm*, *cpe*), as *lm* is to *ce*; and inversely as LM is to *lm*, or as FR is to *c*R, so is FG to *ce*. And therefore if *fg* was to *ce* as *fy* to *c*Y, that is, as *fr* to *c*R (that is, as *fr* to FR and FR to *c*R conjointly, that is, as *f*T to FT, and FG to *ce* conjointly), because the ratio of FG to *ce*, expunged on both sides, leaves the ratios *fg* to FG and *f*T to FT, *fg* would be to FG as *f*T to FT; and, therefore, the angles which FG and *fg* would subtend at the earth T would be equal to each other. But these angles (by what we have shown in the preceding Proposition) are the motions of the nodes, while the moon describes in the circle the arc PM, in the ellipse the arc *pm*; and therefore the motions of the nodes in the circle and in the ellipse would be equal to each other. Thus, I say, it would be, if *fg* was to *ce* as *f*Y to *c*Y, that is, if *fg* was equal to $\frac{ce \cdot fY}{cY}$. But because of the similar triangles *fgp*, *cep*, *fg* is to *ce* as *fp* to *cp*; and therefore *fg* is equal to $\frac{ce \cdot fp}{cp}$; and therefore the angle which *fg* subtends in fact is to the former angle which FG subtends, that is to say, the motion of the nodes in the ellipse is to the motion of the same in the circle as this *fg* or $\frac{ce \cdot fp}{cp}$ to the former *fg* or $\frac{ce \cdot fY}{cY}$, that is, as $fp \cdot cY$ to $fY \cdot cp$, or as *fp* to *f*Y, and *c*Y to *cp*; that is, if *ph* parallel to TN meet FP in *h*, as F*h* to FY and FY to FP; that is, as F*h* to FP or D*p* to DP, and therefore as the area D*pmd* to the area DPM*d*. And, therefore, seeing (by Cor. I, Prop. XXX) the latter area and AZ^2 conjointly are proportional to the hourly motion of the nodes in the circle, the former area and AZ^2 conjointly will be proportional to the hourly motion of the nodes in the ellipse. Q.E.D.

Cor. Since, therefore, in any given position of the nodes, the sum of all the areas *p*D*dm*, in the time while the moon is carried from the quadrature to any place *m*, is the area *mp*QE*d* terminated at the tangent of the ellipse QE; and the sum of all those areas, in one entire revolution, is the area of the whole ellipse; the mean motion of the nodes in the ellipse will be to the mean motion of the nodes in the circle as the ellipse to the circle; that is, as T*a* to TA, or 69 to 70. And, therefore, since (by Cor. II, Prop. XXX) the mean hourly motion of the nodes in the circle is to $16^s\ 35^{th}\ 16^{iv}\ 36^{v}$ as AZ^2 to AT^2, if we take the angle $16^s\ 21^{th}\ 3^{iv}\ 30^{v}$ to the angle $16^s\ 35^{th}\ 16^{iv}\ 36^{v}$ as 69 to 70, the mean hourly motion of the nodes in the ellipses will be to $16^s\ 21^{th}\ 3^{iv}\ 30^{v}$ as AZ^2 to AT^2; that is, as the square of the sine of the distance of the node from the sun to the square of the radius.

But the moon, by a radius drawn to the earth, describes the area in the syzygies with a greater velocity than it does that in the quadratures, and upon that account the time is contracted in the syzygies, and prolonged in the quadratures; and together with the time the motion of the nodes is likewise augmented or diminished. But the moment of the area in the quadratures of the moon was to the moment thereof in the syzygies as 10973 to 11073; and therefore the mean moment in the octants is to the excess in the syzygies, and to the defect in the quadratures, as 11023, the half-sum of those numbers, is to their half-difference 50. Wherefore, since the time of the moon in the several little equal parts of its orbit is inversely as its velocity, the mean time in the octants will be to the excess of the time in the quadratures, and to the defect *of the time* in the syzygies arising from this cause, nearly as 11023 to 50. But, reckoning from the quadratures to the syzygies, I find that the excess of the moments of the area, in the several places above the least moment in the quadratures, is nearly as the square of the sine of the moon's distance from the quadratures; and therefore the difference between the moment in any place, and the mean moment in the octants, is as the difference between the square of the sine of the moon's distance from the quadratures, and the square of the sine of 45 degrees, or half the square of the radius; and the increment of the time in the several places between the octants and quadratures, and the decrement thereof between the octants and syzygies, is in the same proportion. But the motion of the nodes, while the moon describes the several little equal parts of its orbit, is accelerated or retarded as the square of the time; for that motion, while the moon describes PM, is (other things being equal) as ML, and ML varies as the square of the time. Wherefore, the motion of the nodes in the syzygies, in the time while the moon describes given little parts of its orbit, is diminished as the square of the ratio of the number 11073 to the number 11023; and the decrement is to the remaining motion as 100 to 10973; but to the whole motion is as 100 to 11073, nearly. But the decrement in the places between the octants and syzygies, and the increment in the places between the octants and quadratures, is to this decrement nearly as the whole motion in these places to the whole motion in the syzygies, and the difference between the square of the sine of the moon's distance from the quadrature, and the half-square of the radius, is to the half-square of the radius conjointly. Wherefore, if the nodes are in the quadratures, and we take two places, one on one side, one on the other, equally distant from the octant and other two distant by the same interval, one from the syzygy, the other from the quadrature, and from the decrements of the motions in the two places between the syzygy and octant we subtract the increments of the motions in the two other places between the octant and the quadrature, the remaining decrement will be equal to the decrement in the syzygy, as will easily appear by computation; and therefore the mean decrement, which ought to be subtracted from the mean motion of the nodes, is the fourth part of the decrement in the syzygy. The whole hourly motion of the nodes in the syzygies (when the moon by a radius drawn to the earth was supposed to describe an area proportional to the time) was $32^{s}\ 42^{th}\ 7^{iv}$. And we have shown that the decrement of the motion of the nodes, in the time while the moon, now moving with greater velocity, describes the same space, was to this motion as 100 to 11073; and therefore this decrement is $17^{th}\ 43^{iv}\ 11^{v}$. The fourth part of which $4^{th}\ 25^{iv}\ 48^{v}$ subtracted from the mean hourly motion above found, $16^{s}\ 21^{th}\ 3^{iv}\ 30^{v}$, leaves $16^{s}\ 16^{th}\ 37^{iv}\ 42^{v}$, their correct mean hourly motion.

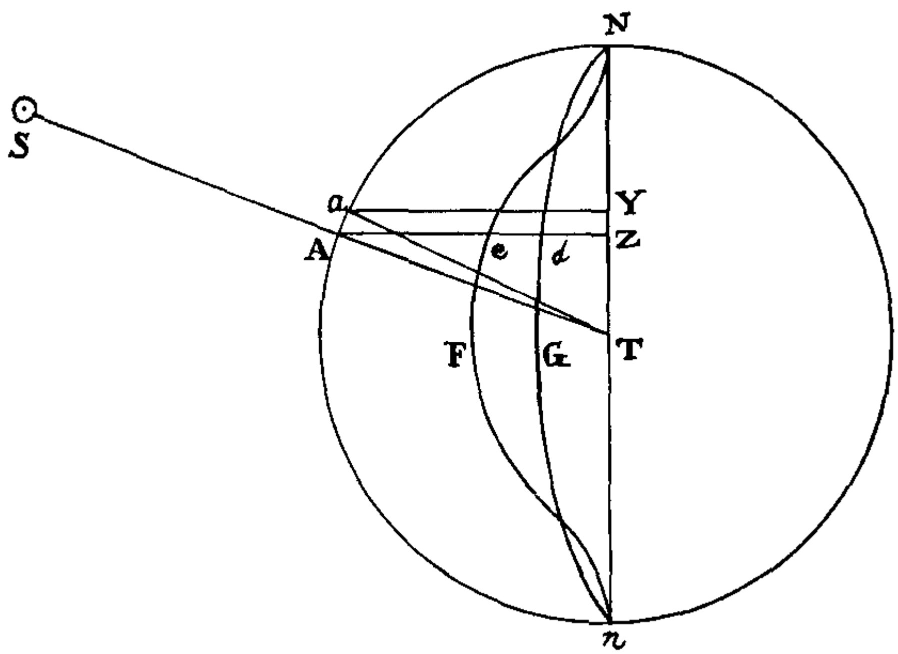
If the nodes are without the quadratures, and two places are considered, one on one side, one on the other, equally distant from the syzygies, the sum of the motions of the nodes, when the moon is in those places, will be to the sum of their motions, when the moon is in the same places and the nodes in the quadratures, as AZ^2 to AT^2. And the decrements of the motions arising from the causes but now explained will be mutually as the motions themselves, and therefore the remaining motions will be mutually between themselves as AZ^2 to AT^2; and the mean motions will be as the remaining motions. And, therefore, in any given position of the nodes, their correct mean hourly motion is to $16^{s}\ 16^{th}\ 37^{iv}\ 42^{v}$ as AZ^2 to AT^2; that is, as the square of the sine of the distance of the nodes from the syzygies to the square of the radius.

PROPOSITION XXXII. PROBLEM XIII

To find the mean motion of the nodes of the moon.

The yearly mean motion is the sum of all the mean hourly motions throughout the course of the year. Suppose that the node is in N, and that, after every hour is elapsed, it is drawn back again to its former place; so that, notwithstanding its proper motion, it may constantly remain in the same situation with respect to the fixed stars; while in the meantime the sun S, by the motion of the earth, is seen to leave the node, and to proceed till it completes its apparent annual course by an uniform motion. Let A*a* represent a given least arc, which the right line TS always drawn to the sun, by its intersection with the circle NA*n*, describes in the least given moment of time; and the mean hourly motion (from what we have above shown) will be as AZ^2, that is (because AZ and ZY are proportional), as the rectangle of AZ into ZY, that is, as the area AZY*a*; and the sum of all the mean hourly motions from the beginning will be as the sum of all the areas *a*YZA, that is, as the area NAZ. But the greatest AZY*a* is equal to the rectangle of the arc A*a* into the radius of the circle; and therefore the sum of all these rectangles in the whole circle will be to the like sum of all the greatest rectangles as the area of the whole circle to the rectangle of the whole circumference into the radius, that is, as 1 to 2. But the hourly motion corresponding to that greatest rectangle was $16^s\ 16^{th}\ 37^{iv}\ 42^v$ and this motion in the complete course of the sidereal year, $365^d.\ 6^h.\ 9^s.$, amounts to $39°\ 38'\ 7''\ 50'''$; and therefore the half thereof, $19°\ 49'\ 3''\ 55'''$, is the mean motion of the nodes corresponding to the whole circle. And the motion of the nodes, in the time while the sun is carried from N to A, is to $19°\ 49'\ 3''\ 55'''$ as the area NAZ to the whole circle.

Thus it would be if the node was after every hour drawn back again to its former place, that so, after a complete revolution, the sun at the year's end would be found again in the same node which it had left when the year began. But, because of the motion of the node in the meantime, the sun must needs meet the node sooner; and now it remains that we compute the abbreviation of the time. Since, then, the sun, in the course of the year, travels 360 degrees, and the node in the same time by its greatest motion would be carried $39°\ 38'\ 7''\ 50'''$, or 39.6355 degrees; and the mean motion of the node in any place N is to its mean motion in its quadratures as AZ^2 to AT^2; the motion of the sun will be to the motion of the node in N as 360 AT^2 to $39.6355AZ^2$; that is, as $9.0827646AT^2$ to AZ^2. Therefore, if we

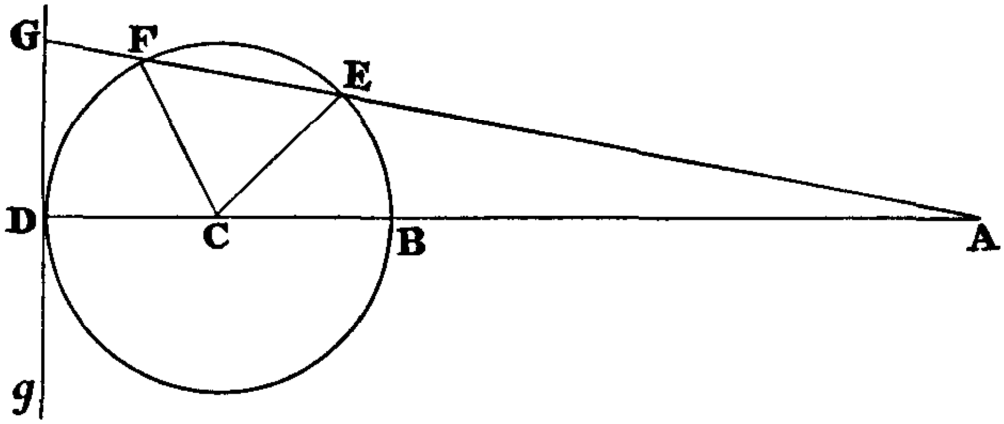


suppose the circumference N*An* of the whole circle to be divided into little equal parts, such as A*a*, the time in which the sun would describe the little arc A*a*, if the circle was quiescent, will be to the time in which it would describe the same arc, supposing the circle together with the nodes to be revolved about the centre T, inversely as $9.0827646AT^2$ to $9.0827646AT^2 + AZ^2$; for the time is inversely as the velocity with which the little arc is described, and this velocity is the sum of the velocities of both sun and node. If, therefore, the sector NTA represent the time in which the sun by itself, without the motion of the node, would describe the arc NA, and the indefinitely small part AT*a* of the sector represent the little moment of the time in which it would describe the least arc A*a*; and (letting fall *a*Y perpendicular upon N*n*) if in AZ we take *d*Z of such length that the rectangle of *d*Z into ZY may be to the least part AT*a* of the sector as AZ^2 to $9.0827646AT^2 + AZ^2$; that is to say, that *d*Z may be to ½AZ as AT^2 to $9.0827646AT^2 + AZ^2$; the rectangle of *d*Z into ZY will represent the decrement of the time arising from the motion of the node, while the arc A*a* is described; and if the curve N*d*G*n* is the locus where the point *d* is always found, the curvilinear area N*d*Z will be as the whole decrement *of time* while the whole arc NA is described; and, therefore, the excess of the sector NAT above the area N*d*Z will be as the whole time. But because the motion of the node in a less time is less in proportion to the time, the area A*a*YZ must also be diminished in the same proportion; which may be done by taking in AZ the line *e*Z of such length, that it may be to the length of AZ as AZ^2 to $9.0827646AT^2 + AZ^2$; for so the rectangle of *e*Z into ZY will be to the area AZY*a* as the decrement of the time in which the arc A*a* is described to the whole time in which it would have been described, if the node had been quiescent; and, therefore, that rectangle will be as the decrement of the motion of the node. And if the curve N*e*F*n* is the locus of the point *e*, the whole area N*e*Z, which is the sum of all the decrements *of that motion*, will be as the whole decrement *thereof* during the time in which the arc AN is described; and the remaining area NA*e* will be as the remaining motion, which is the true motion of the node, during the time in which the whole arc NA is described by the joint motions of both sun and node. Now the area of the semicircle is to the area of the figure N*e*F*n* found by the method of infinite series nearly as 793 to 60. But the motion corresponding *or proportional* to the whole circle was 19° 49′ 3″ 55‴; and therefore the motion corresponding to double the figure N*e*F*n* is 1° 29′ 58″ 2‴, which taken from the former motion leaves 18° 19′ 5″ 53‴, the whole motion of the node with respect to the fixed stars in the interval between two of its conjunctions with the sun; and this motion subtracted from the annual motion of the sun, 360°, leaves 341° 40′ 54″ 7‴, the motion of the sun in the interval between the same conjunctions. But as this motion is to the annual motion 360°, so is the motion of the node but just now found 18° 19′ 5″ 53‴ to its annual motion, which will therefore be 19° 18′ 1″ 23‴; and this is the mean motion of the nodes in the sidereal year. By astronomical tables, it is 19° 21′ 21″ 50‴. The difference is less than $\frac{1}{300}$ part of the whole motion, and seems to arise from the eccentricity of the moon's orbit, and its inclination to the plane of the ecliptic. By the eccentricity of this orbit the motion of the nodes is too much accelerated; and, on the other hand, by the inclination of the orbit, the motion of the nodes is somewhat retarded, and reduced to its just velocity.

PROPOSITION XXXIII. PROBLEM XIV

To find the true motion of the nodes of the moon.

In the time which is as the area NTA – N*d*Z (in the preceding fig.) that motion is as the area NA*e*, and hence is given; but because the calculus is too difficult, it will be better to use the following construction of the Problem. About the centre C, with any radius CD, describe the circle BEFD;



produce DC to A so as AB may be to AC as the mean motion to half the mean true motion when the nodes are in their quadratures (that is, as 19° 18′ 1″ 23‴ to 19° 49′ 3″ 55‴; and therefore BC is to AC as the difference of those motions 0° 31′ 2″ 32‴ to the latter motion 19° 49′ 3″ 55‴, that is, as 1 to 38³⁄₁₀). Then through the point D draw the indefinite line G*g*, touching the circle in D; and if we take the angle BCE, or BCF, equal to the double distance of the sun from the place of the node, as found by the mean motion, and drawing AE or AF cutting the perpendicular DG in G, we take another angle which shall be to the whole motion of the node in the interval between its syzygies (that is, to 9° 11′ 3″) as the tangent DG to the whole circumference of the circle BED, and add this *last* angle (for which the angle DAG may be used) to the mean motion of the nodes, while they are passing from the quadratures to the syzygies, and subtract it from their mean motion while they are passing from the syzygies to the quadratures, we shall have their true motion; for the true motion so found will nearly agree with the true motion which comes out from assuming the times as the area NTA – N*d*Z, and the motion of the node as the area NA*e*; as anyone who chooses to examine and make the computations will find: and this is the semimenstrual equation of the motion of the nodes. But there is also a menstrual equation, but which is by no means necessary for finding of the moon's latitude; for since the variation of the inclination of the moon's orbit to the plane of the ecliptic is liable to a twofold inequality, the one semimenstrual, the other menstrual, the menstrual inequality *of this variation,* and the menstrual equation of the nodes, so moderate and correct each other, that in computing the latitude of the moon both may be neglected.

COR. From this and the preceding Proposition it appears that the nodes are quiescent in their syzygies, but regressive in their quadratures, by an hourly motion of $16^{s}\ 19^{th}\ 26^{iv}$; and that the equation of the motion of the nodes in the octants is $1^\circ\ 30'$; all of which exactly agree with the phenomena of the heavens.

SCHOLIUM

Mr. *Machin,* Professor *Gresham,* and Dr. *Henry Pemberton,* separately found out the motion of the nodes by a different method. Mention has been made of this method in another place. Their papers, which I have seen, contained two Propositions, and exactly agreed with each other in both of them. Mr. *Machin's* paper, coming first to my hands, I shall here insert.

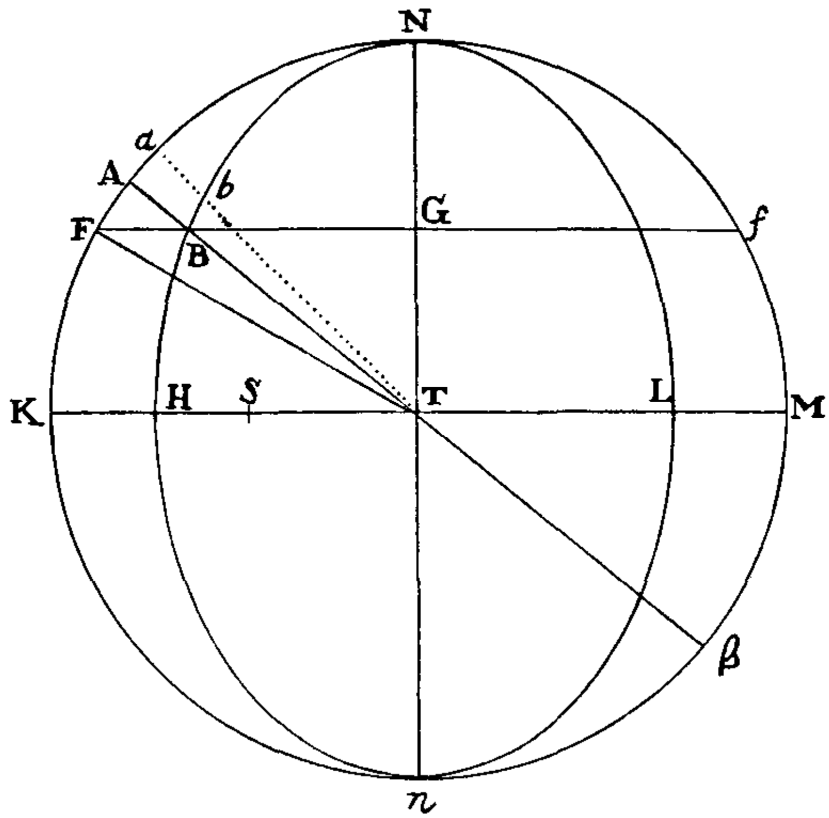
THE MOTION OF THE MOON'S NODES

"PROPOSITION I

"The mean motion of the sun from the node is defined by a geometric mean proportional between the mean motion of the sun and that mean motion with which the sun recedes with the greatest swiftness from the node in the quadratures.

"Let T be the earth's place, N*n* the line of the moon's nodes at any given time, KTM a perpendicular thereto, TA a right line revolving about the centre with the same angular velocity with which the sun and the node recede from each other, in such sort that the angle between the quiescent right line N*n* and the revolving line TA may be always equal to the distance of the places of the sun and node. Now if any right line TK be divided into parts TS and SK, and those parts be taken as the mean hourly motion of the sun to the mean hourly motion of the node in the quadratures, and there be taken the right line TH, a mean proportional between the part TS and the whole TK, this right line will be proportional to the sun's mean motion from the node.

"For let there be described the circle NK*n*M from the centre T and with the radius TK, and about the same centre, with the semiaxes TH and TN, let there be described an ellipse NH*n*L; and in the time in which the sun recedes from the node through the arc N*a*, if there be drawn the right line T*ba,* the area of the sector NT*a* will be the exponent of the sum of the motions of the sun and node in the same time. Let, therefore, the extremely small arc *a*A be that which the right line T*ba,* revolving according to the aforesaid law, will uniformly describe in a given interval of time, and the extremely small sector TA*a* will be as the sum of the velocities with which the sun and node are carried two different ways in that time. Now the sun's velocity is almost uniform, its inequality being so small as scarcely to produce the least inequality in the mean motion of the nodes. The other part of this sum, namely, the mean quantity of the velocity of the node, is increased in the recess from the syzygies in a squared ratio of the sine of its distance from the sun (by Cor., Prop. XXXI of this Book), and, being greatest in its quadratures with the sun in K, is in the same ratio to the sun's velocity as SK to TS, that is, as (the difference of the squares of TK and TH, or) the rectangle KHM to TH^2. But the ellipse NBH divides the sector AT*a*, the exponent of the sum of these two velocities, into two parts AB*ba* and

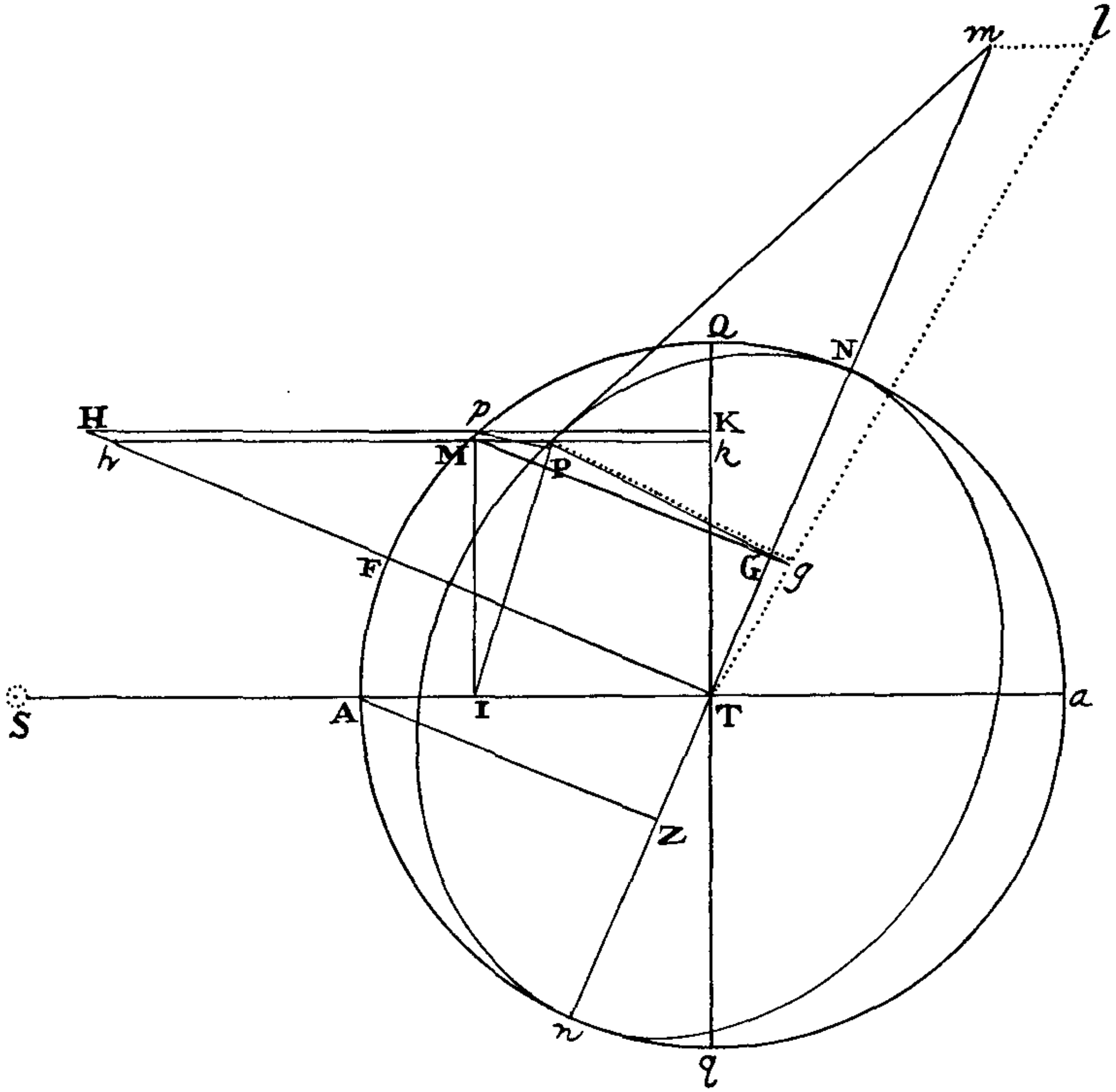


BT*b*, proportional to the velocities. For, produce BT to the circle in β, and from the point B let fall upon the greater axis the perpendicular BG, which being produced both ways may meet the circle in the points F and *f*; and because the space AB*ba* is to the sector TB*b* as the rectangle ABβ is to BT^2 (that rectangle being equal to the difference of the squares of TA and TB, because the right line Aβ is equally cut in T, and unequally in B), therefore when the space AB*ba* is the greatest of all in K, this ratio will be the same as the ratio of the rectangle KHM to HT^2. But the greatest mean velocity of the node was shown above to be in that very ratio to the velocity of the sun; and therefore in the quadratures the sector AT*a* is divided into parts

PROPOSITION XXXIV. PROBLEM XV

To find the hourly variation of the inclination of the moon's orbit to the plane of the ecliptic.

Let A and *a* represent the syzygies; Q and *q* the quadratures; N and *n* the nodes; P the place of the moon in its orbit; *p* the orthographic projection of that place upon the plane of the ecliptic; and *m*T*l* the momentary motion of the nodes as above. If upon T*m* we let fall the perpendicular PG,



and joining *p*G we produce it till it meet T*l* in *g*, and join also P*g*, the angle PG*p* will be the inclination of the moon's orbit to the plane of the ecliptic when the moon is in P; and the angle P*gp* will be the inclination of the same after a small moment of time is elapsed; and therefore the angle GP*g* will be the momentary variation of the inclination. But this angle GP*g* is to the angle GT*g* as TG to PG and P*p* to PG conjointly. And, therefore, if for the momnt of time we assume an hour, since the angle GT*g* (by Prop. xxx) is to the angle $33'' \ 10''' \ 33^{iv}$ as $IT \cdot PG \cdot AZ$ to AT^3, the angle GP*g* (or the hourly variation of the inclination) will be to the angle $33'' \ 10''' \ 33^{iv}$ as

$IT \cdot AZ \cdot TG \cdot \frac{Pp}{PG}$ to AT^3. Q.E.I.

And thus it would be if the moon were uniformly revolved in a circular orbit. But if the orbit is elliptical, the mean motion of the nodes will be diminished in proportion of the less axis to the greater, as we have shown above; and the variation of the inclination will be also diminished in the same proportion.

Cor. I. Upon N*n* erect the perpendicular TF, and let *p*M be the hourly motion of the moon in the plane of the ecliptic; upon QT let fall the perpendiculars *p*K, M*k*, and produce them till they meet TF in H and *h;* then IT will be to AT as K*k* to M*p*; and TG to H*p* as TZ to AT; and, therefore, $IT \cdot TG$ will be equal to $\frac{Kk \cdot Hp \cdot TZ}{Mp}$, that is, equal to the area H*p*M*h* multiplied into the ratio $\frac{TZ}{Mp}$: and therefore the hourly variation of the inclination will be to $33'' \ 10''' \ 33^{iv}$ as the area H*p*M*h* multiplied into $AZ \cdot \frac{TZ}{Mp} \cdot \frac{Pp}{PG}$ is to AT^3.

Cor. II. And, therefore, if the earth and nodes were after every hour drawn back from their new and instantly restored to their old places, so that their situation might continue given for a whole periodic month together, the whole variation of the inclination during that month would be to $33'' \ 10''' \ 33^{iv}$ as the aggregate of all the areas H*p*M*h,* generated in the time of one revolution of the point *p* (with due regard in summing to their proper signs + −), multiplied into $AZ \cdot TZ \cdot \frac{Pp}{PG}$ to $Mp \cdot AT^3$; that is, as the whole circle QA*qa* multiplied into $AZ \cdot TZ \cdot \frac{Pp}{PG}$ to $Mp \cdot AT^3$, that is, as the circumference QA*qa* multiplied into $AZ \cdot TZ \cdot \frac{Pp}{PG}$ to $2Mp \cdot AT^2$.

COR. III. And, therefore, in a given position of the nodes, the mean hourly variation, from which, if uniformly continued through the whole month, that menstrual variation might be generated, is to $33'' \, 10''' \, 33^{iv}$ as $AZ \cdot TZ \frac{Pp}{PG}$ is to $2AT^2$, or as $Pp \cdot \frac{AZ \cdot TZ}{\frac{1}{2}AT}$ is to $PG \cdot 4AT$; that is (because Pp is to PG as the sine of the aforesaid inclination to the radius, and $\frac{AZ \cdot TZ}{\frac{1}{2}AT}$ to $4AT$ as the sine of double the angle ATn to four times the radius), as the sine of the same inclination multiplied into the sine of double the distance of the nodes from the sun to four times the square of the radius.

COR. IV. Seeing the hourly variation of the inclination, when the nodes are in the quadratures, is (by this Prop.) to the angle $33'' \, 10''' \, 33^{iv}$ as $IT \cdot AZ \cdot TG \cdot \frac{Pp}{PG}$ is to AT^3, that is, as $\frac{IT \cdot TG}{\frac{1}{2}AT} \cdot \frac{Pp}{PG}$ to $2AT$, that is, as the sine of double the distance of the moon from the quadratures multiplied into $\frac{Pp}{PG}$ is to twice the radius, the sum of all the hourly variations during the time that the moon, in this situation of the nodes, passes from the quadrature to the syzygy (that is, in the space of $177\frac{1}{6}$ hours) will be to the sum of as many angles $33'' \, 10''' \, 33^{iv}$ or $5878''$, as the sum of all the sines of double the distance of the moon from the quadratures multiplied into $\frac{Pp}{PG}$ is to the sum of as many diameters; that is, as the diameter multiplied into $\frac{Pp}{PG}$ is to the circumference; that is, if the inclination be $5° \, 1'$, as $7 \cdot \frac{874}{10000}$ is to 22, or as 278 to 10000. And, therefore, the whole variation, composed out of the sum of all the hourly variations in the aforesaid time, is $163''$, or $2' \, 43''$.

PROPOSITION XXXV. PROBLEM XVI

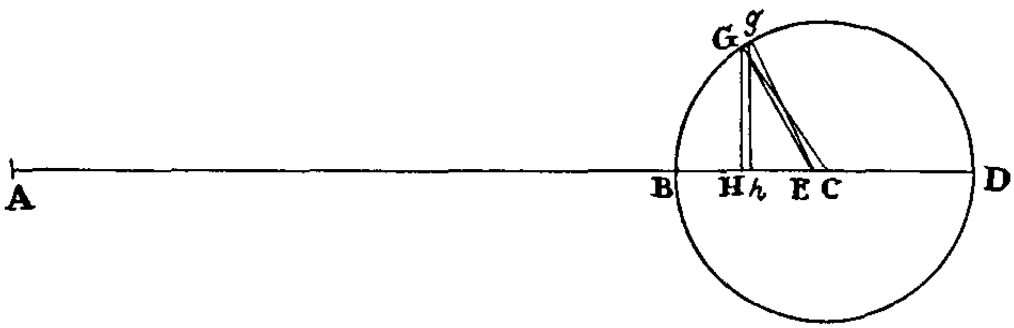
To a given time to find the inclination of the moon's orbit to the plane of the ecliptic.

Let AD be the sine of the greatest inclination, and AB the sine of the least. Bisect BD in C; and round the centre C, with the radius BC, describe the circle BGD. In AC take CE in the same proportion to EB as EB to twice BA. And if to the time given we set off the angle AEG equal to double the distance of the nodes from the quadratures, and upon AD let fall the perpendicular GH, AH will be the sine of the inclination required.

For GE^2 is equal to

$$GH^2 + HE^2 = BHD + HE^2 = HBD + HE^2 - BH^2 = HBD + BE^2 - 2BH \cdot BE = BE^2 + 2EC \cdot BH = 2EC \cdot AB + 2EC \cdot BH = 2EC \cdot AH;$$

wherefore, since 2EC is given, GE^2 will be as AH. Now let AE*g* represent double the distance of the nodes from the quadratures, in a given moment



of time after, and the arc G*g*, on account of the given angle GE*g*, will be as the distance GE. But H*h* is to G*g* as GH to GC, and, therefore, H*h* is as the rectangle $GH \cdot Gg$, or $GH \cdot GE$, that is, as $\frac{GH}{GE} \cdot GE^2$, or $\frac{GH}{GE} \cdot AH$; that is, as AH and the sine of the angle AEG conjointly. If, therefore, in any one case, AH be the sine of inclination, it will increase by the same increments as the sine of inclination doth (by Cor. III of the preceding Prop.), and therefore will always continue equal to that sine. But when the point G falls upon either point B or D, AH is equal to this sine, and therefore remains always equal thereto. Q.E.D.

In this demonstration I have supposed that the angle BEG, representing double the distance of the nodes from the quadratures, increaseth uniformly; for I cannot descend to every minute circumstance of inequality. Now suppose that BEG is a right angle, and that G*g* is in this case the hourly increment of double the distance of the nodes from the sun; then (by Cor. III of the last Prop.) the hourly variation of the inclination in the same case will be to $33'' \; 10''' \; 33^{iv}$ as the rectangle of AH, the sine of the inclination, into the sine of the right angle BEG, double the distance of the nodes from the sun, is to four times the square of the radius; that is, as AH, the sine of the mean inclination, is to four times the radius; that is, seeing the mean inclination is about $5° \; 8\frac{1}{2}'$, as its sine 896 is to 40000, the quadruple of the radius, or as 224 to 10000. But the whole variation corresponding to BD, the difference of the sines, is to this hourly variation as the diameter BD is to the arc *Gg,* that is, conjointly as the diameter BD to the semicircumference BGD, and as the time of 2079⅞₀ hours, in which the node proceeds from the quadratures to the syzygies, is to one hour, that is, as 7 to 11, and $2079\frac{7}{10}$ to 1. Therefore, compounding all these proportions, we shall have the whole variation BD to $33'' \ 10''' \ 33^{iv}$ as $224 \cdot 7 \cdot 2079\frac{7}{10}$ is to 110000, that is, as 29645 to 1000; and from thence that variation BD will come out $16' \ 23\frac{1}{2}''$.

And this is the greatest variation of the inclination, abstracting from the situation of the moon in its orbit; for, if the nodes are in the syzygies, the inclination suffers no change from the various positions of the moon. But if the nodes are in the quadratures, the inclination is less when the moon is in the syzygies than when it is in the quadratures by a difference of $2' \ 43''$, as we showed (Cor. IV of the preceding Prop.); and the whole mean variation BD, diminished by $1' \ 21\frac{1}{2}''$, the half of this excess, becomes $15' \ 2''$, when the moon is in the quadratures; and, increased by the same, becomes $17' \ 45''$ when the moon is in the syzygies. If, therefore, the moon be in the syzygies, the whole variation in the passage of the nodes from the quadratures to the syzygies will be $17' \ 45''$; and, therefore, if the inclination be $5° \ 17' \ 20''$, when the nodes are in the syzygies, it will be $4° \ 59' \ 35''$ when the nodes are in the quadratures and the moon in the syzygies. The truth of all this is confirmed by observations.

Now if the inclination of the orbit should be required when the moon is in the syzygies, and the nodes anywhere between them and the quadratures, let AB be to AD as the sine of $4° \ 59' \ 35''$ is to the sine of $5° \ 17' \ 20''$, and take the angle AEG equal to double the distance of the nodes from the quadratures; and AH will be the sine of the inclination desired. To this inclination of the orbit the inclination of the same is equal, when the moon is 90° distant from the nodes. In other situations of the moon, this menstrual inequality, to which the variation of the inclination is subject in the calculus of the moon's latitude, is balanced, and in a manner taken off, by the menstrual inequality of the motion of the nodes (as we said before), and therefore may be neglected in the computation of the said latitude.

SCHOLIUM

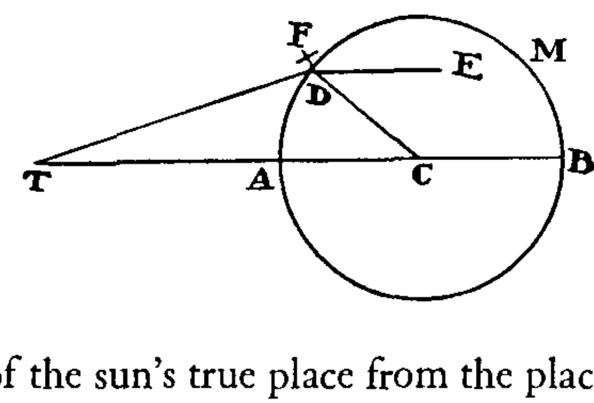
By these computations of the lunar motions I was desirous of showing that by the theory of gravity the motions of the moon could be calculated from their physical causes. By the same theory I moreover found that the annual equation of the mean motion of the moon arises from the varying dilatation which the orbit of the moon suffers from the action of the sun according to Cor. VI, Prop. LXVI, Book I. The force of this action is greater in the perigean sun, and dilates the moon's orbit; in the apogean sun it is less, and permits the orbit to be again contracted. The moon moves slower in the dilated and faster in the contracted orbit; and the annual equation, by which this inequality is regulated, vanishes in the apogee and perigee of the sun. In the mean distance of the sun from the earth it rises to about 11′ 50″; in other distances of the sun it is proportional to the equation of the sun's centre, and is added to the mean motion of the moon, while the earth is passing from its aphelion to its perhelion, and subtracted while the earth is in the opposite semicircle. Taking for the radius of the great orbit 1000, and 16⅞ for the earth's eccentricity, this equation, when of the greatest magnitude, by the theory of gravity comes out 11′ 49″. But the eccentricity of the earth seems to be somewhat greater, and with the eccentricity this equation will be augmented in the same proportion. Suppose the eccentricity $16^{11}/_{12}$, and the greatest equation will be 11′ 51″.

Further; I found that the apogee and nodes of the moon move faster in the perihelion of the earth, where the force of the sun's action is greater, than in the aphelion thereof, and that inversely as the cube of the ratio of the earth's distance from the sun; and hence arise annual equations of those motions proportional to the equation of the sun's centre. Now the motion of the sun varies inversely as the square of the earth's distance from the sun; and the greatest equation of the centre which this inequality generates is 1° 56′ 20″, corresponding to the above-mentioned eccentricity of the sun, $16^{11}/_{12}$. But if the motion of the sun had been inversely as the cube of the distance, this inequality would have generated the greatest equation 2° 54′ 30″; and therefore the greatest equations which the inequalities of the motions of the moon's apogee and nodes do generate are to 2° 54′ 30″ as the mean diurnal motion of the moon's apogee and the mean diurnal motion of its nodes are to the mean diurnal motion of the sun. Hence the greatest equation of the mean motion of the apogee comes out 19′ 43″, and the greatest equation of the mean motion of the nodes 9′ 24″. The former equation is added, and the latter subtracted, while the earth is passing from its perihelion to its aphelion, and contrariwise when the earth is in the opposite semicircle.

By the theory of gravity I likewise found that the action of the sun upon the moon is somewhat greater when the transverse diameter of the moon's orbit passes through the sun than when the same is perpendicular upon the line which joins the earth and the sun; and therefore the moon's orbit is somewhat larger in the former than in the latter case. And hence arises another equation of the moon's mean motion, depending upon the situation of the moon's apogee in respect of the sun, which is greatest when the moon's apogee is in the octants of the sun, and vanishes when the apogee arrives at the quadratures or syzygies; and it is added to the mean motion while the moon's apogee is passing from the quadrature of the sun to the syzygy, and subtracted while the apogee is passing from the syzygy to the quadrature. This equation, which I shall call the semiannual, when greatest in the octants of the apogee, arises to about 3′ 45″, so far as I could determine from the phenomena: and this is its quantity in the mean distance of the sun from the earth. But it is increased and diminished inversely as the cube of the sun's distance, and therefore is nearly 3′ 34″ when that distance is greatest, and 3′ 56″ when least. But when the moon's apogee is without the octants, it becomes less, and is to its greatest amount as the sine of double the distance of the moon's apogee from the nearest syzygy or quadrature is to the radius.

By the same theory of gravity, the action of the sun upon the moon is somewhat greater when the line of the moon's nodes passes through the sun than when it is at right angles with the line which joins the sun and the earth; and hence arises another equation of the moon's mean motion, which I shall call the second semiannual; and this is greatest when the nodes are in the octants of the sun, and vanishes when they are in the syzygies or quadratures; and in other positions of the nodes is proportional to the sine of double the distance of either node from the nearest syzygy or quadrature. And it is added to the mean motion of the moon, if the sun is behind the node which is nearest to him, and is subtracted, if forward; and in the octants, where it is of the greatest magnitude, it arises to 47″ in the mean distance of the sun from the earth, as I find from the theory of gravity. In other distances of the sun, this equation, greatest in the octants of the nodes, is inversely as the cube of the sun's distance from the earth; and therefore in the sun's perigee it comes to about 49″, and in its apogee to about 45″.

By the same theory of gravity, the moon's apogee goes forwards at the greatest rate when it is either in conjunction with or in opposition to the sun, but in its quadratures with the sun it goes backwards; and the eccentricity comes, in the former case, to its greatest quantity; in the latter, to its least, by Cor. VII, VIII, and IX, Prop. LXVI, Book I. And those inequalities, by the Corollaries we have named, are very great, and generate the principle which I call the semiannual equation of the apogee; and this semiannual equation in its greatest quantity comes to about 12° 18′, as nearly as I could determine from the phenomena. Our countryman, *Horrox,* was the first who advanced the theory of the moon's moving in an ellipse about the earth placed in its lower focus. Dr. *Halley* improved the notion, by putting the centre of the ellipse in an epicycle whose centre is uniformly revolved about the earth; and from the motion in this epicycle the mentioned inequalities in the progress and regress of the apogee, and in the quantity of eccentricity, do arise. Suppose the mean distance of the moon from the earth to be divided into 100000 parts, and let T represent the earth, and TC the moon's mean eccentricity of 5505 such parts. Produce TC to B, so as CB may be the sine of the greatest semiannual equation 12° 18′ to the radius TC; and the circle BDA described about the centre C, with the radius CB, will be the epicycle spoken of, in which the centre of the moon's orbit is placed, and revolved according to the order of the letters BDA. Set off the angle BCD equal to twice the annual argument, or twice the distance of the sun's true place from the place of the moon's apogee once corrected, and CTD will be the semiannual equation of the moon's apogee, and TD the eccentricity of its orbit, tending to the place of the apogee now twice corrected. But, having the moon's



mean motion, the place of its apogee, and its eccentricity, as well as the longer axis of its orbit 200000, from these data the true place of the moon in its orbit, together with its distance from the earth, may be determined by the methods commonly known.

In the perihelion of the earth, where the force of the sun is greatest, the centre of the moon's orbit moves faster about the centre C than in the aphelion, and that inversely as the cube of the sun's distance from the earth. But, because the equation of the sun's centre is included in the annual argument, the centre of the moon's orbit moves faster in its epicycle BDA, inversely as the square of the sun's distance from the earth. Therefore, that it may move yet faster, inversely as the distance, suppose that from D, the centre of the orbit, a right line DE is drawn, tending towards the moon's apogee once corrected, that is, parallel to TC; and set off the angle EDF equal to the excess of the aforesaid annual argument above the distance of the moon's apogee from the sun's perigee forwards; or, which comes to the same thing, take the angle CDF equal to the complement of the sun's true anomaly to 360°; and let DF be to DC as twice the eccentricity of the great orbit to the sun's mean distance from the earth, and the sun's mean diurnal motion from the moon's apogee to the sun's mean diurnal motion from its own apogee conjointly, that is, as 33⅞ to 1000, and 52′ 27″ 16‴ to 59′ 8″ 10‴ conjointly, or as 3 to 100; and imagine the centre of the moon's orbit placed in the point F to be revolved in an epicycle whose centre is D, and radius DF, while the point D moves in the circumference of the circle DABD: for by this means the centre of the moon's orbit comes to describe a certain curved line about the centre C, with a velocity which will be almost inversely as the cube of the sun's distance from the earth, as it ought to be.

The calculus of this motion is difficult, but may be rendered easier by the following approximation. Assuming, as above, the moon's mean distance from the earth of 100000 parts, and the eccentricity TC of 5505 such parts, the line CB or CD will be found 1172¾, and DF 35⅕ of those parts; and this line DF at the distance TC subtends the angle at the earth, which the removal of the centre of the orbit from the place D to the place F generates in the motion of this centre; and double this line DF in a parallel position, at the distance of the upper focus of the moon's orbit from the earth, subtends at the earth the same angle as DF did before, which that removal generates in the motion of this upper focus; but at the distance of the moon from the earth this double line 2DF at the upper focus, in a parallel position to the first line DF, subtends an angle at the moon, which the said removal generates in the motion of the moon, which angle may be therefore called the second equation of the moon's centre; and this equation, in the mean distance of the moon from the earth, is nearly as the sine of the angle which that line DF contains with the line drawn from the point F to the moon, and when greatest amounts to 2′ 25″. But the angle which the line DF contains with the line drawn from the point F to the moon is found either by subtracting the angle EDF from the mean anomaly of the moon, or by adding the distance of the moon from the sun to the distance of the moon's apogee from the apogee of the sun; and as the radius is to the sine of the angle thus found, so is 2′ 25″ to the second equation of the centre: to be added, if the fore-mentioned sum be less than a semi-circle; to be subtracted, if greater. And from the moon's place in its orbit thus corrected, its longitude may be found in the syzygies of the luminaries.

The atmosphere of the earth to the height of 35 or 40 miles refracts the sun's light. This refraction scatters and spreads the light over the earth's shadow; and the dissipated light near the limits of the shadow dilates the shadow. On this account, to the diameter of the shadow, as it comes out by the parallax, I add 1 or 1⅓ minutes in lunar eclipses.

But the theory of the moon ought to be examined and proved from the phenomena, first in the syzygies, then in the quadratures, and last of all in the octants; and whoever pleases to undertake the work will find it not amiss to assume the following mean motions of the sun and moon at the *Royal Observatory* of *Greenwich,* to the last day of *December* at noon, in the year 1700, o.s.: the mean motion of the sun ♑ 20° 43′ 40″, and of its apogee ♋ 7° 44′ 30″; the mean motion of the moon ♒ 15° 21′ 00″; of its apogee, ♓ 8° 20′ 00″; and of its ascending node ♌ 27° 24′ 20″; and the difference of meridians between the Observatory at *Greenwich* and the *Royal Observatory* at *Paris,* 0^{h}. 9^{m}. 20^{s}.: but the mean motion of the moon and of its apogee are not yet obtained with sufficient accuracy.

PROPOSITION XXXVI. PROBLEM XVII

To find the force of the sun to move the sea.

The sun's force ML or PT to disturb the motions of the moon, was (by Prop. xxv), in the moon's quadratures, to the force of gravity with us, as 1 to 638092.6; and the force TM – LM or 2PK in the moon's syzygies is double that quantity. But, descending to the surface of the earth, these forces are diminished in proportion of the distances from the centre of the earth, that is, in the proportion of 60½ to 1; and therefore the former force on the earth's surface is to the force of gravity as 1 to 38604600; and by this force the sea is depressed in such places as are 90 degrees distant from the sun. But by the other force, which is twice as great, the sea is raised not only in the places directly under the sun, but in those also which are directly opposed to it; and the sum of these forces is to the force of gravity as 1 to 12868200. And because the same force excites the same motion, whether it depresses the waters in those places which are 90 degrees distant from the sun, or raises them in the places which are directly under and directly opposed to the sun, the aforesaid sum will be the total force of the sun to disturb the sea, and will have the same effect as if the whole was employed in raising the sea in the places directly under and directly opposed to the sun, and did not act at all in the places which are 90 degrees removed from the sun.

And this is the force of the sun to disturb the sea in any given place, where the sun is at the same time both vertical, and in its mean distance from the earth. In other positions of the sun, its force to raise the sea is directly as the versed sine of double its altitude above the horizon of the place, and inversely as the cube of the distance from the earth.

Cor. Since the centrifugal force of the parts of the earth, arising from the earth's diurnal motion, which is to the force of gravity as 1 is to 289, raises the waters under the equator to a height exceeding that under the poles by 85472 *Paris* feet, as above, in Prop. xix, the force of the sun, which we have now shown to be to the force of gravity as 1 is to 12868200, and therefore is to that centrifugal force as 289 to 12868200, or as 1 to 44527, will be able to raise the waters in the places directly under and directly opposed to the sun to a height exceeding that in the places which are 90 degrees removed from the sun only by one *Paris* foot and $113\frac{1}{30}$ inches; for this measure is to the measure of 85472 feet as 1 to 44527.

PROPOSITION XXXVII. PROBLEM XVIII

To find the force of the moon to move the sea.

The force of the moon to move the sea is to be deduced from its ratio to the force of the sun, and this ratio is to be determined from the ratio of the motions of the sea, which are the effects of those forces. Before the mouth of the river *Avon,* three miles below *Bristol,* the height of the ascent of the water in the vernal and autumnal syzygies of the luminaries (by the observations of *Samuel Sturmy*) amounts to about 45 feet, but in the quadratures to 25 only. The former of those heights arises from the sum of the aforesaid forces, the latter from their difference. If, therefore, S and L are supposed to represent respectively the forces of the sun and moon while they are in the equator, as well as in their mean distances from the earth, we shall have $L+S$ to $L-S$ as 45 to 25, or as 9 to 5.

At *Plymouth* (by the observations of *Samuel Colepress*) the tide in its mean height rises to about 16 feet, and in the spring and autumn the height thereof in the syzygies may exceed that in the quadratures by more than 7 or 8 feet. Suppose the greatest difference of those heights to be 9 feet, and $L+S$ will be to $L-S$ as $20\frac{1}{2}$ to $11\frac{1}{2}$, or as 41 to 23; a proportion that agrees well enough with the former. But because of the great tide at *Bristol,* we are rather to depend upon the observations of *Sturmy*; and, therefore, till we procure something that is more certain, we shall use the proportion of 9 to 5.

But because of the reciprocal motions of the waters, the greatest tides do not happen at the times of the syzygies of the luminaries, but, as we have said before, are the third in order after the syzygies; or (reckoning from the syzygies) follow next after the third approach of the moon to the meridian of the place after the syzygies; or rather (as *Sturmy* observes) are the third after the day of the new or full moon, or rather nearly after the twelfth hour from the new or full moon, and therefore fall nearly upon the forty-third hour after the new or full moon. But in this port they come to pass about the seventh hour after the approach of the moon to the meridian of the place; and therefore follow next after the approach of the moon to the meridian, when the moon is distant from the sun, or from opposition with the sun by about 18 or 19 degrees forwards. So the summer and winter seasons come not to their height in the solstices themselves, but when the sun is advanced beyond the solstices by about a tenth part of its whole course, that is, by about 36 or 37 degrees. In like manner, the greatest tide is raised after the approach of the moon to the meridian of the place, when the moon has passed by the sun, *or the opposition thereof*, by about a tenth part of the whole motion from *one greatest* tide to *the next following greatest* tide. Suppose that distance about 18½ degrees; and the sun's force in this distance of the moon from the syzygies and quadratures will be of less moment to augment and diminish that part of the motion of the sea which proceeds from the motion of the moon than in the syzygies and quadratures themselves in the proportion of the radius to the cosine of double this distance, or of an angle of 37 degrees; that is, in the ratio of 10000000 to 7986355; and, therefore, in the preceding analogy, in place of S we must put 0.7986355S.

But further, the force of the moon in the quadratures must be diminished, on account of its declination from the equator; for the moon in those quadratures, or rather in 18½ degrees past the quadratures, declines from the equator by about 23° 13′; and the force of either luminary to move the sea is diminished as it declines from the equator nearly as the square of the cosine of the declination; and therefore the force of the moon in those quadratures is only 0.8570327L; hence we have L + 0.7986355S to 0.8570327L − 0.7986355S as 9 to 5.

Further yet, the diameters of the orbit in which the moon should move, setting aside the consideration of eccentricity, are one to the other as 69 to 70; and therefore the moon's distance from the earth in the syzygies is to its distance in the quadratures, other things being equal, as 69 to 70; and its distances, when 18½ degrees advanced beyond the syzygies, where the greatest tide was excited, and when 18½ degrees passed by the quadratures, where the least tide was produced, are to its mean distance as 69.098747 and 69.897345 to 69½. But the force of the moon to move the sea varies inversely as the cube of its distance; and therefore its forces, in the greatest and least of those distances, are to its force in its mean distance as 0.9830427 and

1.017522 is to 1. From this we have 1.017522L·0.7986355S to 0.9830427·0.8570327L − 0.7986355S as 9 to 5; and S to L as 1 to 4.4815. Therefore, since the force of the sun is to the force of gravity as 1 to 12868200, the moon's force will be to the force of gravity as 1 to 2871400.

COR. 1. Since the waters attracted by the sun's force rise to the height of 1 foot and 11 1/30 inches, the moon's force will raise the same to the height of 8 feet and 7 5/22 inches; and the joint forces of both will raise the same to the height of 10 1/2 feet; and when the moon is in its perigee to the height of 12 1/2 feet, and more, especially when the wind sets the same way as the tide. And a force of that amount is abundantly sufficient to produce all the motions of the sea, and agrees well with the ratio of those motions; for in such seas as lie free and open from east to west, as in the *Pacific* sea, and in those tracts of the *Atlantic* and *Ethiopic* seas which lie without the tropics, the waters commonly rise to 6, 9, 12, or 15 feet; but in the *Pacific* sea, which is of a greater depth, as well as of a larger extent, the tides are said to be greater than in the *Atlantic* and *Ethiopic* seas; for, to have a full tide raised, an extent of sea from east to west is required of no less than 90 degrees. In the *Ethiopic* sea, the waters rise to a less height within the tropics than in the temperate zones: because of the narrowness of the sea between *Africa* and the southern parts of *America.* In the middle of the open sea the waters cannot rise without falling together, and at the same time, upon both the eastern and western shores, when, notwithstanding, in our narrow seas, they ought to fall on those shores by alternate turns; upon this account there is commonly but a small flood and ebb in such islands as lie far distant from the continent. On the contrary, in some ports, where to fill and empty the bays alternately the waters are with great violence forced in and out through shallow channels, the flood and ebb must be greater than ordinary; as at *Plymouth* and *Chepstow Bridge* in *England,* at the mountains of St. *Michael,* and the town of *Avranches,* in *Normandy,* and at *Cambaia* and *Pegu* in the *East Indies.* In these places the sea is hurried in and out with such violence as sometimes to lay the shores under water, sometimes to leave them dry for many miles. Nor is this force of the influx and efflux to be stopped till it has raised and depressed the waters to 30, 40, or 50 feet and above. And a like account is to be given of long and shallow channels or straits, such as the *Magellanic* straits, and those channels which environ

England. The tide in such ports and straits, by the violence of the influx and efflux, is augmented greatly. But on such shores as lie towards the deep and open sea with a steep descent, where the waters may freely rise and fall without that precipitation of influx and efflux, the ratio of the tides agrees with the forces of the sun and moon.

COR. II. Since the moon's force to move the sea is to the force of gravity as 1 to 2871400, it is evident that this force is inappreciable in statical or hydrostatical experiments, or even in those of pendulums. It is in the tides only that this force shows itself by any sensible effect.

COR. III. Because the force of the moon for moving the sea is to the like force of the sun as 4.4815 to 1, and those forces (by Cor. XIV, Prop. LXVI, Book I) are as the densities of the bodies of the sun and moon and the cubes of their apparent diameters conjointly, the density of the moon will be to the density of the sun directly as 4.4815 to 1, and inversely as the cube of the moon's diameter to the cube of the sun's diameter; that is (seeing the mean apparent diameters of the moon and sun are 31′ 16½″, and 32′ 12″), as 4891 to 1000. But the density of the sun was to the density of the earth as 1000 to 4000; and therefore the density of the moon is to the density of the earth as 4891 is to 4000, or as 11 to 9. Therefore the body of the moon is more dense and more earthly than the earth itself.

COR. IV. And since the true diameter of the moon (from the observations of astronomers) is to the true diameter of the earth as 100 to 365, the mass of matter in the moon will be to the mass of matter in the earth as 1 to 39.788.

COR. V. And the accelerative gravity on the surface of the moon will be about three times less than the accelerative gravity on the surface of the earth.

COR. VI. And the distance of the moon's centre from the centre of the earth will be to the distance of the moon's centre from the common centre of gravity of the earth and moon as 40.788 to 39.788.

COR. VII. And the mean distance of the centre of the moon from the centre of the earth will be (in the moon's octants) nearly 60⅖ of the greatest semidiameters of the earth; for the greatest semidiameter of the earth was 19658600 *Paris* feet, and the mean distance of the centres of the earth and moon, consisting of 60⅖ such semidiameters, is equal to 1187379440 feet. And this distance (by the preceding Cor.) is to the distance of the moon's centre from the common centre of gravity of the earth and moon as 40.788 to 39.788; which latter distance, therefore, is 1158268534 feet. And since the moon, in respect of the fixed stars, performs its revolution in $27^{d}.\ 7^{h}.\ 43\frac{4}{9}^{m}.$, the versed sine of that angle which the moon in a minute of time describes is 12752341 to the radius 1000,000000,000000; and as the radius is to this versed sine, so are 1158268534 feet to 14.7706353 feet. The moon, therefore, falling towards the earth by that force which retains it in its orbit, would in one minute of time describe 14.7706353 feet; and, if we augment this force in the proportion of $178\frac{29}{40}$ to $177\frac{29}{40}$, we shall have the total force of gravity at the orbit of the moon, by Cor., Prop. III; and the moon falling by this force, in one minute of time would describe 14.8538067 feet. And at the 60th part of the distance of the moon from the earth's centre, that is, at the distance of 197896573 feet from the centre of the earth, a body falling by its weight, would, in one second of time, likewise describe 14.8538067 feet. And, therefore, at the distance of 19615800, which compose one mean semidiameter of the earth, a heavy body would describe in falling 15.11175, or 15 feet, 1 inch, and $4\frac{1}{11}$ lines, in the same time. This will be the descent of bodies in the latitude of 45 degrees. And by the foregoing table, to be found under Prop. XX, the descent in the latitude of *Paris* will be a little greater by an excess of about ⅔ parts of a line. Therefore, by this computation, heavy bodies in the latitude of *Paris* falling in a vacuum will describe 15 *Paris* feet, 1 inch, $4\frac{25}{33}$ lines, very nearly, in one second of time. And if the gravity be diminished by taking away a quantity equal to the centrifugal force arising in that latitude from the earth's diurnal motion, heavy bodies falling there will describe in one second of time 15 feet, 1 inch, and 1½ lines. And with this velocity heavy bodies do really fall in the latitude of *Paris,* as we have shown above in Prop. IV and XIX.

COR. VIII. The mean distance of the centres of the earth and moon in the syzygies of the moon is equal to 60 of the greatest semidiameters of the earth, subtracting only about one 30th part of a semidiameter; and in the moon's quadratures the mean distance of the same centres is 60⅚ such semidiameters of the earth; for these two distances are to the mean distance of the moon in the octants as 69 and 70 to 69½, by Prop. XXVIII.

COR. IX. The mean distance of the centres of the earth and moon in the syzygies of the moon is 60 mean semidiameters of the earth, and a 10th part of one semidiameter; and in the moon's quadratures the mean distance of the same centres is 61 mean semidiameters of the earth, subtracting one 30th part of one semidiameter.

COR. X. In the moon's syzygies its mean horizontal parallax in the latitudes of 0, 30, 38, 45, 52, 60, 90 degrees is 57′ 20″, 57′ 16″, 57′ 14″, 57′ 12″, 57′ 10″, 57′ 8″, 57′ 4″, respectively.

In these computations I do not consider the magnetic attraction of the earth, whose quantity is very small and unknown: if this quantity should ever be found out, and the measures of degrees upon the meridian, the lengths of isochronous pendulums in different parallels, the laws of the motions of the sea, and the moon's parallax, with the apparent diameters of the sun and moon, should be more exactly determined from phenomena: we should then be enabled to bring this calculation to a greater accuracy.

PROPOSITION XXXVIII. PROBLEM XIX

To find the figure of the moon's body.

If the moon's body were fluid like our sea, the force of the earth to raise that fluid in the nearest and remotest parts would be to the force of the moon by which our sea is raised in the places under and opposite to the moon as the accelerative gravity of the moon towards the earth is to the accelerative gravity of the earth towards the moon, and the diameter of the moon is to the diameter of the earth conjointly; that is, as 39.788 to 1, and 100 to 365 conjointly, or as 1081 to 100. Therefore, since our sea, by the force of the moon, is raised to 8⅗ feet, the lunar fluid would be raised by the force of the earth to 93 feet; and upon this account the figure of the moon would be a spheroid, whose greatest diameter produced would pass through the centre of the earth, and exceed the diameters perpendicular thereto by 186 feet. Such a figure, therefore, the moon possesses, and must have had from the beginning. Q.E.I.

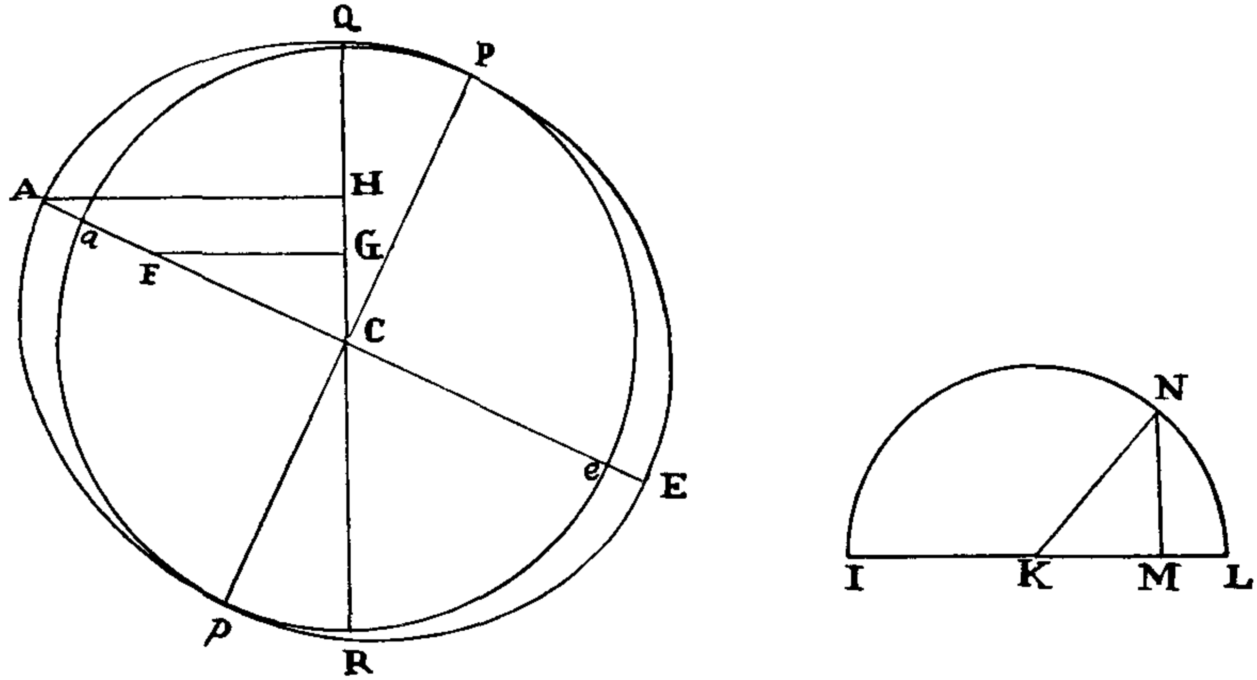
COR. Hence it is that the same face of the moon always is turned toward the earth; nor can the body of the moon possibly rest in any other position, but would return always by a libratory motion to this situation; but those librations, however, must be exceedingly slow, because of the weakness of the forces which excite them; so that the face of the moon, which should be always directed to the earth, may, for the reason assigned in Prop. XVII, be turned towards the other focus of the moon's orbit, without being immediately drawn back, and turned again towards the earth.

LEMMA I

If APEp *represent the earth uniformly dense, marked with the centre* C, *the poles* P, p, *and the equator* AE; *and if about the centre* C, *with the radius* CP, *we suppose the sphere* Pape *to be described, and* QR *to denote the plane on which a right line, drawn from the centre of the sun to the centre of the earth, stands at right angles; and further suppose that the several particles of the whole exterior earth* PapAPepE, *without the height of the said sphere, endeavor to recede towards this side and that side from the plane QR, every particle by a force proportional to its distance from that plane: I say, in the first place, that the whole force and efficacy of all the particles that are situated in* AE, *the circle of the equator, and disposed uniformly without the globe, encompassing the same after the manner of a ring, to wheel the earth about its centre, is to the whole force and efficacy of as many particles in that point* A *of the equator which is at the greatest distance from the plane* QR, *to wheel the earth about its centre with a like circular motion as is* 1 *to* 2. *And that circular motion will be performed about an axis lying in the common section of the equator and the plane* QR.

For let there be described from the centre K, with the diameter IL, the semicircle INL. Suppose the semicircumference INL to be divided into innumerable equal parts, and from the several parts N to the diameter IL let fall the sines NM. Then the sums of the squares of all the sines NM will be equal to the sums of the squares of the sines KM, and both sums together will be equal to the sums of the squares of as many semidiameters KN; and therefore the sum of the squares of all the sines NM will be but half so great as the sum of the squares of as many semidiameters KN.

Suppose now the circumference of the circle AE to be divided into the like number of little equal parts, and from every such part F a perpendicular FG to be let fall upon the plane QR, as well as the perpendicular AH from the point A. Then the force by which the particle F recedes from the plane QR will (by supposition) be as that perpendicular FG; and this force multiplied by the distance CG will represent the power of the particle F to



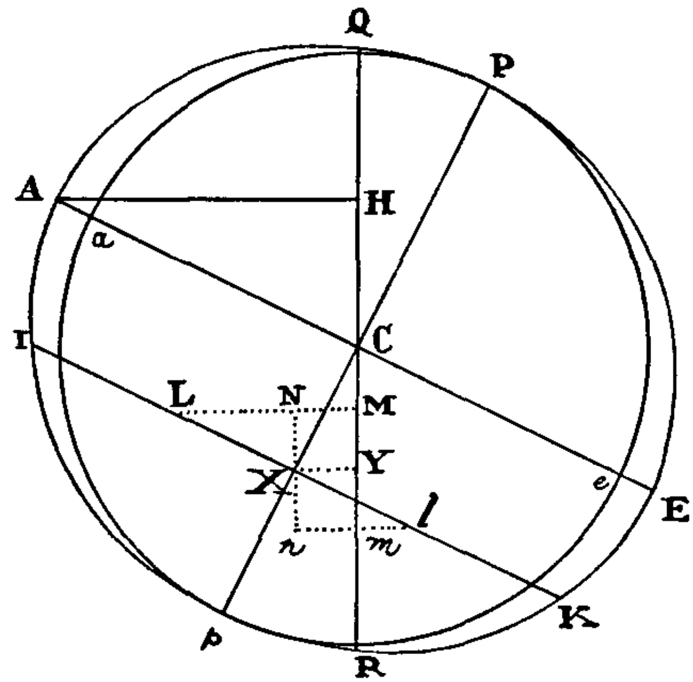
turn the earth round its centre. And, therefore, the power of a particle in the place F will be to the power of a particle in the place A as $FG \cdot GC$ is to $AH \cdot HC$; that is, as FC^2 to AC^2: and therefore the whole power of all the particles F, in their proper places F, will be to the power of the like number of particles in the place A as the sum of all the FC^2 is to the sum of all the AC^2, that is (by what we have demonstrated before), as 1 to 2. Q.E.D.

And because the action of those particles is exerted in the direction of lines perpendicularly receding from the plane QR, and that equally from each side of this plane, they will wheel about the circumference of the circle of the equator, together with the adherent body of the earth, round an axis which lies as well in the plane QR as in that of the equator.

LEMMA II

The same things still supposed, I say, in the second place, that the total force or power of all the particles situated everywhere about the sphere to turn the earth about the said axis is to the whole force of the like number of particles, uniformly disposed round the whole circumference of the equator AE *in the fashion of a ring, to turn the whole earth about with the like circular motion as is 2 to 5.*

For let IK be any lesser circle parallel to the equator AE, and let L*l* be any two equal particles in this circle, situated without the sphere P*ape*; and if upon the plane QR, which is at right angles with a radius drawn to the sun, we let fall the perpendiculars LM, *lm,* the total forces by which these particles recede from the plane QR will be proportional to the perpendiculars LM, *lm*. Let the right line L*l* be drawn parallel to the plane P*ape,* and bisect the same in X; and through the point X draw N*n* parallel to the plane QR, and meeting the perpendiculars LM, *lm,* in N and *n;* and upon the plane QR let fall the perpendicular XY. And the contrary forces of the particles L and *l* to wheel about the earth contrariwise are as LM · MC,



and *lm* · *m*C; that is, as LN · MC + NM · MC, and *ln* · *m*C − *nm* · *m*C; or LN · MC + NM · MC, and LN · *m*C − NM · *m*C, and LN · M*m* − NM · (MC + *m*C), the difference of the two, is the force of both taken together to turn the earth round. The positive part of this difference LN · M*m*, or 2LN · NX, is to 2AH · HC, the force of two particles of the same size situated in A, as LX^2 to AC^2; and the negative part NM · (MC + *m*C), or 2XY · CY, is to 2AH · HC, the force of the same two particles situated in A, as CX^2 to AC^2. And therefore the difference of the parts, that is, the force of the two particles L and *l,* taken together, to wheel the earth about, is to the force of two particles, equal to the former and situated in the place A, to turn in like manner the earth round, as $LX^2 - CX^2$ is to AC^2. But if the circumference IK of the circle IK is supposed to be divided into an infinite number of little equal parts L, all the LX^2 will be to the like number of IX^2 as 1 to 2 (by Lem. 1); and to the same number of AC^2 as IX^2 is to $2AC^2$; and the same number of CX^2 to as many AC^2 as $2CX^2$ is to $2AC^2$. Therefore the united forces of all the particles in the circumference of the circle IK are to the joint forces of as many particles in the place A as $IX^2 - 2CX^2$ is to

$2AC^2$; and therefore (by Lem. 1) to the united forces of as many particles in the circumference of the circle AE as $IX^2 - 2CX^2$ is to AC^2.

Now if P*p*, the diameter of the sphere, is conceived to be divided into an infinite number of equal parts, upon which a like number of circles IK are supposed to stand, the matter in the circumference of every circle IK will be as IX^2; and therefore the force of that matter to turn the earth about will be as IX^2 into $IX^2 - 2CX^2$; and the force of the same matter, if it was situated in the circumference of the circle AE, would be as IX^2 into AC^2. And therefore the force of all the particles of the whole matter situated without the sphere in the circumferences of all the circles is to the force of the like number of particles situated in the circumference of the greatest circle AE as all the IX^2 into $IX^2 - 2CX^2$ is to as many IX^2 into AC^2; that is, as all the $AC^2 - CX^2$ into $AC^2 - 3CX^2$ to as many $AC^2 - CX^2$ into AC^2; that is, as all the $AC^4 - 4AC^2 \cdot CX^2 + 3CX^4$ to as many $AC^4 - AC^2 \cdot CX^2$; that is, as the whole fluent quantity, whose fluxion is $AC^4 - 4AC^2 \cdot CX^2 + 3CX^4$, is to the whole fluent quantity, whose fluxion is $AC^4 - AC^2 \cdot CX^2$; and, therefore, by the method of fluxions, as $AC^4 \cdot CX - \frac{4}{3}AC^2 \cdot CX^3 + \frac{3}{5}CX^5$ is to $AC^4 \cdot CX - \frac{1}{3}AC^2 \cdot CX^3$; that is, if for CX we write the whole C*p*, or AC, as $\frac{4}{15}AC^5$ is to $\frac{2}{3}AC^5$; that is, as 2 is to 5. Q.E.D.

LEMMA III

The same things still supposed, I say, in the third place, that the motion of the whole earth about the axis above named arising from the motions of all the particles, will be to the motion of the aforesaid ring about the same axis in a ratio compounded of the ratio of the matter in the earth to the matter in the ring; and the ratio of three squares of the quadrantal arc of any circle to two squares of its diameter, that is, in the ratio of the matter to the matter, and of the number 925275 *to the number* 1000000.

For the motion of a cylinder revolved about its quiescent axis is to the motion of the inscribed sphere revolved together with it as any four equal squares are to three circles inscribed in three of those squares,[1] and the motion of this cylinder is to the motion of an exceedingly thin ring surrounding both sphere and cylinder in their common contact as double the matter in the cylinder is to triple the matter in the ring; and this motion of the ring, uniformly continued about the axis of the cylinder, is to the uniform

[[1] Appendix, Note 47.]

motion of the same about its own diameter performed in the same periodic time as is the circumference of a circle to double its diameter.

HYPOTHESIS II

If the other parts of the earth were taken away, and the remaining ring was carried alone about the sun in the orbit of the earth by the annual motion, while by the diurnal motion it was in the meantime revolved about its own axis inclined to the plane of the ecliptic by an angle of 23½ degrees, the motion of the equinoctial points would be the same, whether the ring were fluid, or whether it consisted of a hard and rigid matter.

PROPOSITION XXXIX. PROBLEM XX

To find the precession of the equinoxes.

The middle hourly motion of the moon's nodes in a circular orbit, when the nodes are in the quadratures, was $16'' \, 35''' \, 16^{iv} \, 36^{v}$; the half of which, $8'' \, 17''' \, 38^{iv} \, 18^{v}$ (for the reasons above explained), is the mean hourly motion of the nodes in such an orbit, which motion in a whole sidereal year becomes $20° \, 11' \, 46''$. Since, therefore, the nodes of the moon in such an orbit would be yearly transferred $20° \, 11' \, 46''$ backwards, and, if there were more moons, the motion of the nodes of every one (by Cor. XVI, Prop. LXVI, Book I) would be as its periodic time, if upon the surface of the earth a moon was revolved in the time of a sidereal day, the annual motion of the nodes of this moon would be to $20° \, 11' \, 46''$ as $23^{h}. \, 56^{m}.$, the sidereal day, is to $27^{d}. \, 7^{h}. \, 43^{m}.$, the periodic time of our moon, that is, as 1436 is to 39343. And the same thing would happen to the nodes of a ring of moons encompassing the earth, whether these moons did not mutually touch each the other, or whether they were molten, and formed into a continued ring, or whether that ring should become rigid and inflexible.

Let us, then, suppose that this ring is in quantity of matter equal to the whole exterior earth P*ap*AP*ep*E, which lies without the sphere P*ape* (see fig., Lem. II); and because this sphere is to that exterior earth as aC^2 is to $AC^2 - aC^2$, that is (seeing PC or *a*C the least semidiameter of the earth is to AC the greatest semidiameter of the same as 229 is to 230), as 52441 is to 459; if this ring encompassed the earth round the equator, and both together were revolved about the diameter of the ring, the motion of the ring (by Lem. III) would be to the motion of the inner sphere as 459 to 52441 and 1000000 to 925275 conjointly, that is, as 4590 to 485223; and therefore the motion of the ring would be to the sum of the motions of both ring and sphere as 4590 is to 489813. Therefore, if the ring adheres to the sphere, and communicates its motion to the sphere, by which its nodes or equinoctial points recede, the motion remaining in the ring will be to its former motion as 4590 is to 489813; on account of which the motion of the equinoctial points will be diminished in the same ratio. Therefore, the annual motion of the equinoctial points of the body, composed of both ring and sphere, will be to the motion 20° 11′ 46″ as 1436 to 39343 and 4590 to 489813 conjointly, that is, as 100 to 292369. But the forces by which the nodes of a number of moons (as we explained above), and therefore by which the equinoctial points of the ring recede (that is, the forces 3IT, in fig., Prop. XXX), are in the several particles as the distances of those particles from the plane QR; and by these forces the particles recede from that plane: and therefore (by Lem. II) if the matter of the ring was spread all over the surface of the sphere, after the fashion of the figure P*ap*AP*ep*E, in order to make up that exterior part of the earth, the total force or power of all the particles to wheel about the earth round any diameter of the equator, and therefore to move the equinoctial points, would become less than before in the proportion of 2 to 5. Therefore the annual regress of the equinoxes now would be to 20° 11′ 46″ as 10 is to 73092; that is, would be 9″ 56‴ 50^{iv}.

But because the plane of the equator is inclined to that of the ecliptic, this motion is to be diminished in the ratio of the sine 91706 (which is the cosine of 23½ degrees) to the radius 100000; and the remaining motion will now be 9″ 7‴ 20^{iv}, which is the annual precession of the equinoxes arising from the force of the sun.

But the force of the moon to move the sea was to the force of the sun nearly as 4.4815 to 1; and the force of the moon to move the equinoxes is to that of the sun in the same proportion. Whence the annual precession of the equinoxes proceeding from the force of the moon comes out 40″ 52‴ 52^{iv}, and the total annual precession arising from the united forces of both will be 50″ 00‴ 12^{iv}, the amount of which motion agrees with the phenomena; for the precession of the equinoxes, by astronomical observations, is about 50″ yearly.

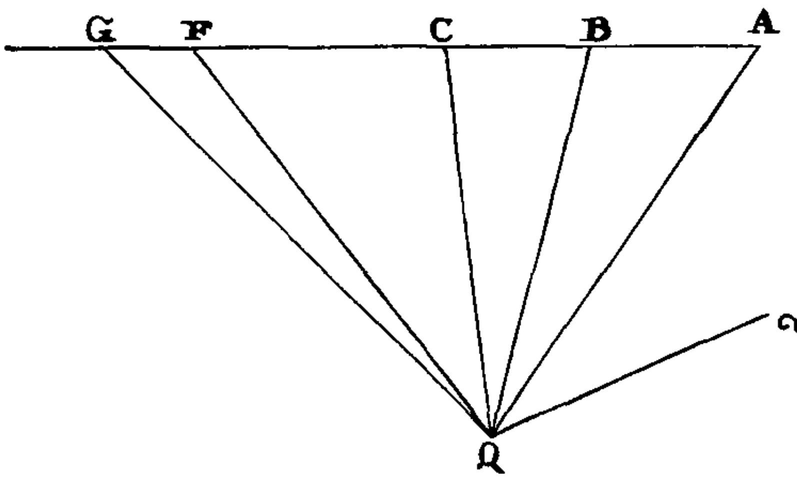
If the height of the earth at the equator exceeds its height at the poles by more than $17^{1}/_{6}$ miles, the matter thereof will be more rare near the surface than at the centre; and the precession of the equinoxes will be augmented by the excess of height, and diminished by the greater rarity.

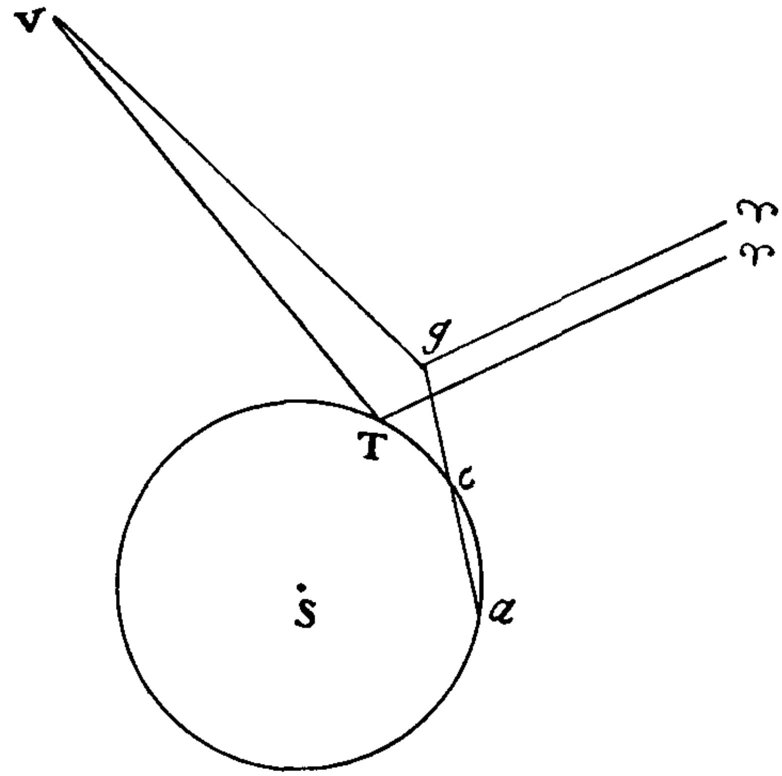
And now we have described the system of the sun, the earth, moon, and planets, it remains that we add something about the comets.

LEMMA IV

The comets are more remote than the moon, and are in the regions of the planets.

As the comets were placed by astronomers beyond the moon, because they were found to have no diurnal parallax, so their annual parallax is a convincing proof of their descending into the regions of the planets; for all the comets which move in a direct course according to the order of the signs, about the end of their appearance become more than ordinarily slow or retrograde, if the earth is between them and the sun; and more than ordinarily swift, if the earth is approaching to a heliocentric opposition with them; on the other hand, those which move against the order of the signs, towards the end of their appearance appear swifter than they ought to be, if the earth is between them and the sun; and slower, and perhaps retrograde, if the earth is in the other side of its orbit. And these appearances proceed chiefly from the diverse situations which the earth acquires in the course of its motion, after the same manner as it happens to the planets, which appear sometimes retrograde, sometimes more slowly, and sometimes more swiftly, progressive, according as the motion of the earth falls in with that of the planet, or is directed in the contrary way. If the earth move the same way with the comet, but, by an angular motion about the sun, so much swifter that right lines drawn from the earth to the comet converge towards the parts beyond the comet, the comet seen from the earth, because of its slower motion, will appear retrograde; and even if the earth is slower than the comet, the motion of the earth being subtracted, the motion of the comet will at least appear retarded; but if the earth tends the contrary way to that of the comet, the motion of the comet will from thence appear accelerated; and from this apparent acceleration, or retardation, or regressive motion, the distance of the comet may be inferred in this manner. Let ♈QA, ♈QB, ♈QC be three observed longitudes of the comet about the time of its first appearing, and ♈QF its last observed longitude before its disappearing. Draw the right line ABC, whose parts AB, BC, intercepted between the right lines QA and QB, QB and QC, may be one to the other as the two times between the three first observations. Produce AC to G, so that AG may be to AB as the time between the first and last observations is to the time between the first and second; and join QG. Now if the comet did move uniformly in a right line, and the earth either stood still, or was likewise carried forwards in a right line by an uniform motion, the angle ♈QG would be the longitude of the comet at the time of the last observation. Therefore, the angle FQG, which is the difference of the longitude, proceeds from the inequality of the motions of the comet and the earth; and if the earth and comet move contrary ways, this angle is added to the angle ♈QG, and accelerates the apparent motion of the comet; but if the comet moves the same way with the earth, it is subtracted, and either retards the motion of the comet, or perhaps renders it retrograde, as we have just now explained. This angle, therefore, proceeding chiefly from the motion of the earth, is justly to be esteemed the parallax of the comet, there being neglected thereby some little increment or decrement that may arise from the unequal motion of the comet in its orbit. From this parallax we thus deduce the distance of the comet. Let S represent the sun, *ac*T the great orbit, *a* the earth's place in the first observation, *c* the place of the earth in the third observation, T the place of the earth in the last observation, and T♈ a right line drawn to the beginning of Aries. Set off the angle ♈TV equal to the angle ♈QF, that is, equal to the longitude of the comet at the time when the earth is in T; join *ac*, and produce it to *g*, so that *ag* may be to *ac* as AG is to AC; and *g* will be the place at which the earth





would have arrived in the time of the last observation, if it had continued to move uniformly in the right line *ac*. Therefore, if we draw *g*♈ parallel to T♈, and make the angle ♈*g*V equal to the angle ♈QG, this angle ♈*g*V will be equal to the longitude of the comet seen from the place *g*, and the angle TV*g* will be the parallax which arises from the earth's being transferred from the place *g* into the place T; and therefore V will be the place of the comet in the plane of the ecliptic. And this place V is commonly lower than the orbit of Jupiter.

The same thing may be deduced from the incurvation of the way of the comets; for these bodies move almost in great circles, while their velocity is great; but about the end of their course, when that part of their apparent motion which arises from the parallax bears a greater proportion to their whole apparent motion, they commonly deviate from those circles, and when the earth goes to one side, they deviate to the other; and this deflection, because of its corresponding with the motion of the earth, must arise chiefly from the parallax; and the quantity thereof is so considerable, as, by my computation, to place the disappearing comets a good deal lower than Jupiter. Hence it follows that when they approach nearer to us in their perigees and perihelions they often descend below the orbits of Mars and the inferior planets.

The near approach of the comets is further confirmed from the light of their heads; for the light of a celestial body, illuminated by the sun, and receding to remote parts, diminishes as the fourth power of the distance; namely, as the square, on account of the increase of the distance from the sun, and as another square, on account of the decrease of the apparent diameter. Therefore, if both the quantity of light and the apparent diameter of a comet are given, its distance will be given also, by taking the distance of the comet to the distance of a planet directly as their diameters and inversely as the square root of their lights. Thus, in the comet of the year 1682, Mr. *Flamsteed* observed with a telescope of 16 feet, and measured with a micrometer, the least diameter of its head, 2′ 00″; but the nucleus or star in the middle of the head scarcely amounted to the tenth part of this measure, and therefore its diameter was only 11″ or 12″; but in the light and splendor of its head it surpassed that of the comet in the year 1680, and might be compared with the stars of the first or second magnitude. Let us suppose that Saturn with its ring was about four times more lucid; and because the light of the ring was almost equal to the light of the globe within, and the apparent diameter of the globe is about 21″, and therefore the united light of both globe and ring would be equal to the light of a globe whose diameter is 30″, it follows that the distance of the comet was to the distance of Saturn inversely as 1 to $\sqrt{4}$, and directly as 12″ to 30″; that is, as 24 to 30, or 4 to 5. Again; the comet in the month of *April*, 1665, as *Hewelcke* informs us, excelled almost all the fixed stars in splendor, and even Saturn itself, as being of a much more vivid color; for this comet was more lucid than that other which had appeared about the end of the preceding year, and had been compared to the stars of the first magnitude. The diameter of its head was about 6′; but the nucleus, compared with the planets by means of a telescope, was plainly less than Jupiter; and sometimes judged less, sometimes judged equal, to the globe of Saturn within the ring. Since, then, the diameters of the heads of the comets seldom exceed 8′ or 12′, and the diameter of the nucleus or central star is but about a tenth or perhaps fifteenth part of the diameter of the head, it appears that these stars are generally of about the same apparent magnitude with the planets. But since their light may be often compared with the light of Saturn, yea, and sometimes exceeds it, it is evident that all comets in their perihelions must either be placed below or not far above Saturn; and they are much mistaken who remove them almost as far as the fixed stars; for if it were so, the comets could receive no more light from our sun than our planets do from the fixed stars.

So far we have gone, without considering the obscuration which comets suffer from that plenty of thick smoke which encompasses their heads, and through which the heads always show dull, as through a cloud. But the more a body is obscured by this smoke, the nearer must it be allowed to come to the sun, that it may vie with the planets in the quantity of light which it reflects. Hence it is probable that the comets descend far below the orbit of Saturn, as we proved before from their parallax. But, above all, the thing is evinced from their tails, which must be due either to the sun's light reflected by a smoke arising from them, and dispersing itself through the ether, or to the light of their own heads. In the former case, we must shorten the distance of the comets, lest we be obliged to allow that the smoke arising from their heads is propagated through such a vast extent of space, and with such a velocity and expansion as will seem altogether incredible; in the latter case, the whole light of both head and tail is to be ascribed to the central nucleus. But, then, if we suppose all this light to be united and condensed within the disk of the nucleus, certainly the nucleus will by far exceed Jupiter itself in splendor, especially when it emits a very large and lucid tail. If, therefore, under a less apparent diameter, it reflects more light, it must be much more illuminated by the sun, and therefore much nearer to it; and the same argument will bring down the heads of comets sometimes within the orbit of Venus, viz., when, being hid under the sun's rays, they emit such huge and splendid tails, like beams of fire, as sometimes they do; for if all that light was supposed to be gathered together into one star, it would sometimes exceed not one Venus only, but a great many such united into one.

Lastly, the same thing is inferred from the light of the heads, which increases in the recess of the comets from the earth towards the sun, and decreases in their return from the sun towards the earth. Thus, the comet of the year 1665 (by the observations of *Hewelcke*), from the time that it was first seen, was always losing of its apparent motion, and therefore had already passed its perigee; but yet the splendor of its head was daily increasing, till, being hid under the sun's rays, the comet ceased to appear. The comet of the year 1683 (by the observations of the same *Hewelcke*), about the end of *July,* when it first appeared, moved at a very slow rate, advancing only about 40 or 45 minutes in its orbit in a day's time; but from that time its diurnal motion was continually upon the increase, till *September* 4, when it arose to about 5 degrees; and therefore, in all this interval of time, the comet was approaching to the earth. This is likewise proved from the diameter of its head, measured with a micrometer; for, on *August* 6, *Hewelcke* found it only 6′ 5″, including the coma, which, on *September* 2, he observed to be 9′ 7″; and therefore its head appeared far less about the beginning than towards the end of the motion, though about the beginning, because nearer to the sun, it appeared far more lucid than towards the end, as the same *Hewelcke* declares. Therefore in all this interval of time, on account of its recess from the sun, it decreased in splendor, notwithstanding its approach towards the earth. The comet of the year 1618, about the middle of *December,* and that of the year 1680, about the end of the same month, did both move with their greatest velocity, and were therefore then in their perigees, but the greatest splendor of their heads was seen two weeks before, when they had just got clear of the sun's rays, and the greatest splendor of their tails a little earlier, when yet nearer to the sun. The head of the former comet (according to the observations of *Cysat*), on *December* 1, appeared greater than the stars of the first magnitude; and, on *December* 16 (then in the perigee), it was diminished but little in magnitude, but much diminished in the splendor and brightness of its light. On *January* 7, *Kepler,* being uncertain about the head, left off observing. On *December* 12, the head of the latter comet was seen and observed by Mr. *Flamsteed,* when but 9 degrees distant from the sun, which is scarcely to be done in a star of the third magnitude. On *December* 15 and 17, it appeared as a star of the third magnitude, its luster being diminished by the brightness of the clouds near the setting sun. On *December* 26, when it moved with the greatest velocity, being almost in its perigee, it was less than the mouth of Pegasus, a star of the third magnitude. On *January* 3, it appeared as a star of the fourth. On *January* 9, as one of the fifth. On *January* 13, it was hid by the splendor of the moon, then in her increase. On *January* 25, it was scarcely equal to the stars of the seventh magnitude. If we compare equal intervals of time, taken on one side of the perigee and then on the other, we shall find that the head of the comet, which at both intervals of time was far, but yet equally removed from the earth, and should therefore have shone with equal splendor, appeared brightest on the side of the perigee towards the sun, and disappeared on the other. Therefore, from the great difference of light in the one situation and in the other, we conclude the great vicinity of the sun and comet in the former, for the light of comets tends to be regular, and to appear greatest when the heads move fastest, and are therefore in their perigees, except so far as it is increased by their nearness to the sun.

COR. I. Therefore the comets shine by the sun's light, which they reflect.

COR. II. From what has been said, we may likewise understand why comets are so frequently seen in that region in which the sun is, and so seldom in the other. If they were visible in the regions far above Saturn, they would appear more frequently in the parts opposite to the sun; for such as were in those parts would be nearer to the earth, whereas the presence of the sun must obscure and hide those that appear in the region in which he is. Yet, looking over the history of comets, I find that four or five times more have been seen in the hemisphere towards the sun than in the opposite hemisphere; besides, without doubt, not a few, which have been hid by the light of the sun: for comets descending into our parts neither emit tails, nor are so well illuminated by the sun as to reveal themselves to our naked eyes, until they have come nearer to us than Jupiter. But the far greater part of that spherical space, which is described about the sun with so small a radius, lies on that side of the earth which faces the sun; and the comets in that greater part are commonly more strongly illuminated, for they are for the most part nearer to the sun.

COR. III. Hence also it is evident that the celestial spaces are void of resistance; for though the comets are carried in oblique paths, and sometimes contrary to the course of the planets, yet they move every way with the greatest freedom, and preserve their motions for an exceeding long time, even where contrary to the course of the planets. I am out in my judgment, if they are not a sort of planets revolving in orbits returning into themselves with a continual motion; for the opinion of some writers, that they are no other than meteors, an opinion based on the continual changes that happen to their heads, seems to have no foundation; for the heads of comets are encompassed with huge atmospheres, and the lowermost parts of these atmospheres must be the densest; and therefore it is in the clouds only, not in the bodies of the comets themselves, that these changes are seen. Thus the earth, if it were viewed from the planets, would, without all doubt, shine by the light of its clouds, and the solid body would scarcely appear through the surrounding clouds. Thus also the belts of Jupiter are formed in the clouds of that planet, for they change their position to each other, and the solid body of Jupiter is hardly to be seen through them; and much more must the bodies of comets be hid under their atmospheres, which are both deeper and thicker.

PROPOSITION XL. THEOREM XX

That the comets move in some of the conic sections, having their foci in the centre of the sun, and by radii drawn to the sun describe areas proportional to the times.

This Proposition appears from Cor. I, Prop. XIII, Book I, compared with Prop. VIII, XII, and XIII, Book III.

Cor. I. Hence if comets revolve in orbits returning into themselves, the orbits will be ellipses; and their periodic times will be to the periodic times of the planets as the $\frac{3}{2}$th power of their principal axes. And therefore the comets, which for the most part of their course are more remote than the planets, and upon that account describe orbits with greater axes, will require a longer time to finish their revolutions. Thus if the axis of a comet's orbit was four times greater than the axis of the orbit of Saturn, the time of the revolution of the comet would be to the time of the revolution of Saturn, that is, to 30 years, as $4\sqrt{4}$ (or 8) is to 1, and would therefore be 240 years.

Cor. II. But their orbits will be so near to parabolas, that parabolas may be used for them without sensible error.

Cor. III. And, therefore, by Cor. VII, Prop. XVI, Book I, the velocity of every comet will always be to the velocity of any planet, supposed to be revolved at the same distance in a circle about the sun, nearly as the square root of double the distance of the planet from the centre of the sun to the distance of the comet from the sun's centre. Let us suppose the radius of the great orbit, or the greatest semidiameter of the ellipse which the earth describes, to consist of 100000000 parts; and then the earth by its mean diurnal motion will describe 1720212 of those parts, and 71675½ by its hourly motion. And therefore the comet, at the same mean distance of the earth from the sun, with a velocity which is to the velocity of the earth as $\sqrt{2}$ to 1, would by its diurnal motion describe 2432747 parts, and 101364½ parts by its hourly motion. But at greater or less distances both the diurnal and hourly motion will be to this diurnal and hourly motion inversely as the square root of the distances, and is therefore given.[1]

[[1] Appendix, Note 48.]

Cor. iv. Therefore if the latus rectum of the parabola is four times the radius of the great orbit, and the square of that radius is supposed to consist of 100000000 parts, the area which the comet will daily describe by a radius drawn to the sun will be 1216373½ parts, and the hourly area will be 50682¼ parts. But, if the latus rectum is greater or less in any ratio, the diurnal and hourly area will be less or greater inversely as the square root of that ratio.

LEMMA V

To find a curved line of the parabolic kind which shall pass through any given number of points.[1]

Let those points be A, B, C, D, E, F, &c., and from the same to any right line HN, given in position, let fall as many perpendiculars AH, BI, CK, DL, EM, FN, &c.

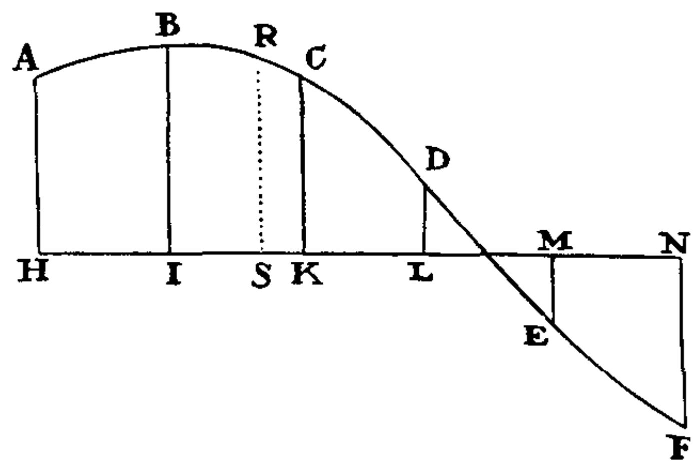
$b \quad 2b \quad 3b \quad 4b \quad 5b$

$c \quad 2c \quad 3c \quad 4c$

$d \quad 2d \quad 3d$

$e \quad 2e$

f



Case i. If HI, IK, KL, &c., the intervals of the points H, I, K, L, M, N, &c., are equal, take $b, 2b, 3b, 4b, 5b$, &c., the first differences of the perpendiculars AH, BI, CK, &c.; their second differences, $c, 2c, 3c, 4c$, &c.; their third, $d, 2d, 3d$, &c., that is to say, so as AH − BI may be $= b$, BI − CK $= 2b$, CK − DL $= 3b$, DL + EM $= 4b$, − EM + FN $= 5b$, &c.; then $b - 2b = c$, &c., and so on to the last difference, which is here f. Then, erecting any perpendicular RS, which may be considered as an ordinate of the curve required, in order to find the length of this ordinate, suppose the intervals HI, IK, KL, LM, &c., to be units, and let AH $= a$, − HS $= p$, $\frac{1}{2}p$ into − IS $= q$, $\frac{1}{3}q$ into + SK $= r$, $\frac{1}{4}r$ into + SL $= s$, $\frac{1}{5}s$ into + SM $= t$; proceeding in this manner, to ME, the last perpendicular but one, and prefixing negative signs before the terms HS, IS, &c., which lie from S towards A; and positive signs before the terms SK, SL, &c., which lie on the other side of the point S; and, observing well the signs, RS will be $= a + bp + cq + dr + es + ft$, + &c.

[[1] Appendix, Note 49.]

CASE 2. But if HI, IK, &c., the intervals of the points H, I, K, L, &c., are unequal, take b, $2b$, $3b$, $4b$, $5b$, &c., the first differences of the perpendiculars AH, BI, CK, &c., divided by the intervals between those perpendiculars; c, $2c$, $3c$, $4c$, &c., their second differences, divided by the intervals between every two; d, $2d$, $3d$, &c., their third differences, divided by the intervals between every three; e, $2e$, &c., their fourth differences, divided by the intervals between every four; and so forth; that is, in such manner, that b may be $= \frac{AH-BI}{HI}$, $2b = \frac{BI-CK}{IK}$, $3b = \frac{CK-DL}{KL}$, &c., then $c = \frac{b-2b}{HK}$, $2c = \frac{2b-3b}{IL}$, $3c = \frac{3b-4b}{KM}$, &c., then $d = \frac{c-2c}{HL}$, $2d = \frac{2c-3c}{IM}$, &c. And those differences being found, let AH be $=a$, $-HS=p$, p into $-IS=q$, q into $+SK=r$, r into $+SL=s$, s into $+SM=t$; proceeding in this manner to ME, the last perpendicular but one; and the ordinate RS will be $=a+bp+cq+dr+es+ft+$ &c.

COR. Hence the areas of all curves may be nearly found; for if some number of points of the curve to be squared are found, and a parabola be supposed to be drawn through those points, the area of this parabola will be nearly the same with the area of the curvilinear figure proposed to be squared: but the parabola can be always squared geometrically by methods generally known.

LEMMA VI

Certain observed places of a comet being given, to find the place of the same at any intermediate given time.

Let HI, IK, KL, LM (in the preceding fig.) represent the times between the observations; HA, IB, KC, LD, ME, five observed longitudes of the comet; and HS the given time between the first observation and the longitude required. Then if a regular curve ABCDE is supposed to be drawn through the points A, B, C, D, E, and the ordinate RS is found out by the preceding Lemma, RS will be the longitude required.

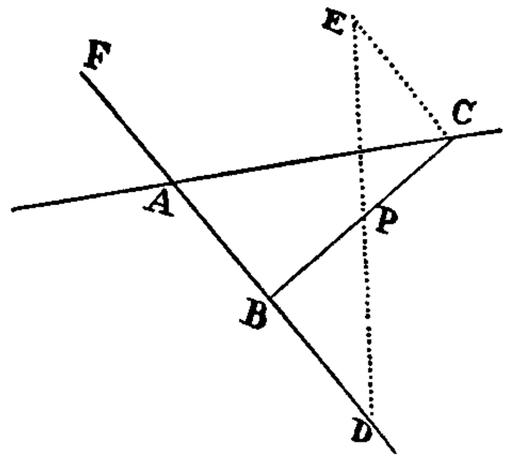
By the same method, from five observed latitudes, we may find the latitude at a given time.

If the differences of the observed longitudes are small, let us say 4 or 5 degrees, then three or four observations will be sufficient to find a new longitude and latitude; but if the differences are greater, as of 10 or 20 degrees, five observations ought to be used.

LEMMA VII

Through a given point P *to draw a right line* BC, *whose parts* PB, PC, *cut off by two right lines* AB, AC, *given in position, may be one to the other in a given ratio.*

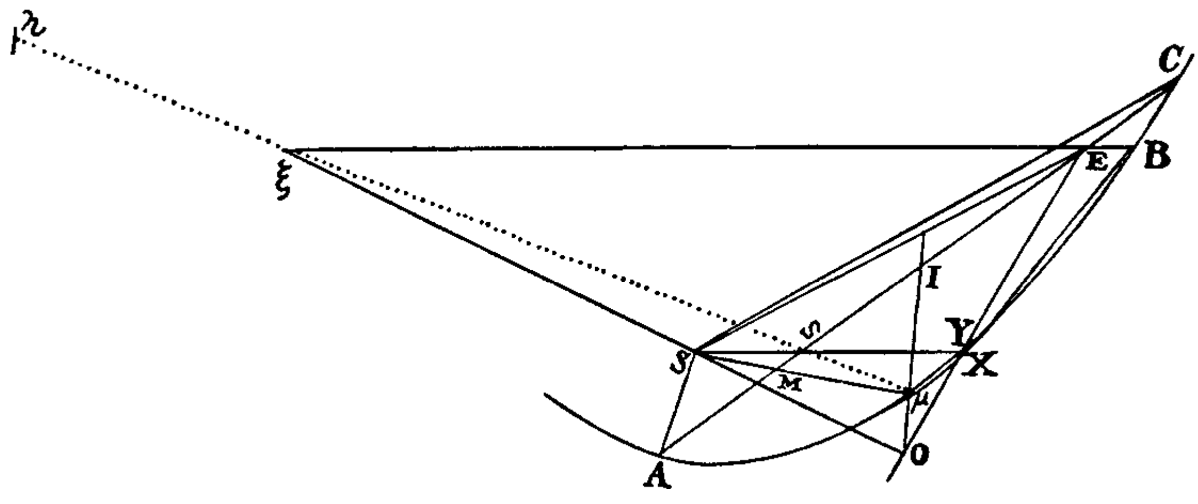
From the given point P suppose any right line PD to be drawn to either of the right lines given, as AB; and produce the same towards AC, the other given right line, as far as E, so as PE may be to PD in the given ratio. Let EC be parallel to AD. Draw CPB, and PC will be to PB as PE to PD. Q.E.F.



LEMMA VIII

Let ABC *be a parabola, having its focus in* S. *By the chord* AC *bisected in* I *cut off the segment* ABCI, *whose diameter is* Iμ *and vertex* μ. *In* Iμ *produced take* μO *equal to one-half of* Iμ. *Join* OS, *and produce it to* ξ, *so that* Sξ *may be equal to* 2SO. *Now, supposing a comet to revolve in the arc* CBA, *draw* ξB, *cutting* AC *in* E: *I say, the point* E *will cut off from the chord* AC *the segment* AE, *nearly proportional to the time.*

For if we join EO, cutting the parabolic arc ABC in Y, and draw μX touching the same arc in the vertex μ, and meeting EO in X, the curvilinear



area AEXμA will be to the curvilinear area ACYμA as AE to AC; and, therefore, since the triangle ASE is to the triangle ASC in the same ratio, the whole area ASEXμA will be to the whole area ASCYμA as AE is to AC. But, because ξO is to SO as 3 to 1, and EO to XO in the same ratio, SX will be parallel to EB; and, therefore, joining BX, the triangle SEB will be equal to the triangle XEB. Therefore, if to the area ASEXμA we add the triangle EXB, and from the sum subtract the triangle SEB, there will remain the area ASBXμA, equal to the area ASEXμA, and therefore in the ratio to the area ASCYμA as AE to AC. But the area ASBYμA is nearly equal to the area ASBXμA; and this area ASBYμA is to the area ASCYμA as the time of description of the arc AB is to the time of description of the whole arc AC; and, therefore, AE is to AC nearly in the proportion of the times. Q.E.D.

COR. When the point B falls upon the vertex μ of the parabola, AE is to AC accurately in the proportion of the times.

SCHOLIUM

If we join μξ cutting AC in δ, and in it take ξ*n* in proportion to μB as 27MI to 16 Mμ, and draw B*n*, this B*n* will cut the chord AC, in the ratio of the times, more accurately than before; but the point *n* is to be taken beyond or on this side the point ξ, according as the point B is more or less distant from the principal vertex of the parabola than the point μ.

LEMMA IX

The right lines Iμ *and* μM, *and the length* $\frac{AI^2}{4S\mu}$, *are equal among themselves.*

For 4Sμ is the latus rectum of the parabola belonging to the vertex μ.

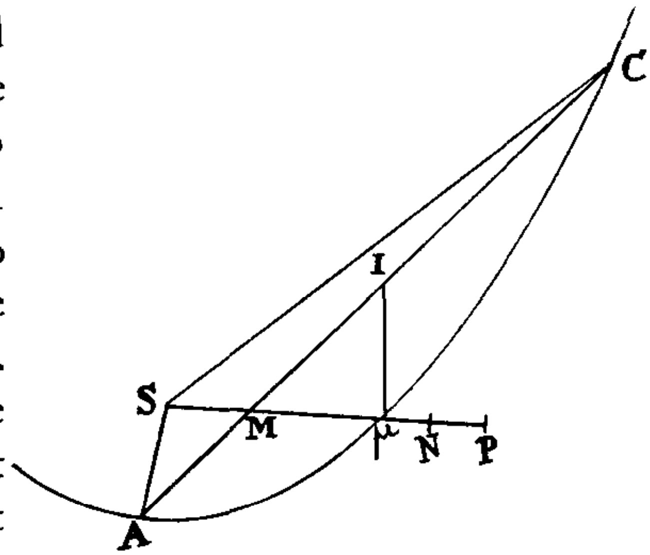
LEMMA X[1]

Produce Sμ *to* N *and* P, *so that* μN *may be one-third of* μI, *and* SP *may be to* SN *as* SN *to* Sμ; *and in the time that a comet would describe the arc* AμC, *if it was supposed to move always forwards with the velocity which it has in a height equal to* SP, *it would describe a length equal to the chord* AC.

For if the comet with the velocity which it hath in μ was in the said time supposed to move uniformly forwards in the right line which touches the

[1 Appendix, Note 50.]

parabola in μ, the area which it would describe by a radius drawn to the point S would be equal to the parabolic area ASCμA; and therefore the space contained under the length described in the tangent and the length Sμ would be to the space contained under the lengths AC and SM as the area ASCμA is to the triangle ASC, that is, as SN to SM. Therefore AC is to the length described in the tangent as Sμ to SN. But since the velocity of the comet in the height SP (by Cor. VI, Prop. XVI, Book I) is to the velocity of the same in the height Sμ, inversely as the square root of SP to Sμ, that is, in the ratio of Sμ to SN, it follows that the length described with this velocity will be to the length in the same time described in the tangent, as Sμ to SN. Therefore since AC, and the length described with this new velocity, are in the same proportion to the length described in the tangent, they must be equal between themselves. Q.E.D.

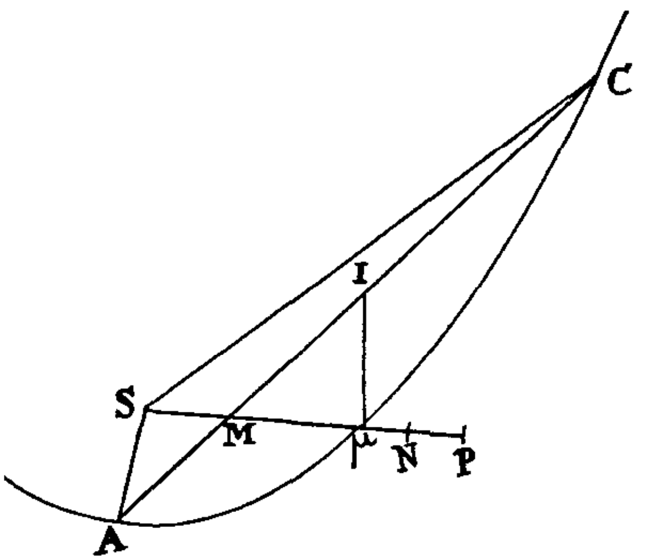


COR. Therefore a comet, with that velocity which it hath in the height Sμ + ⅔Iμ, would in the same time nearly describe the chord AC.

LEMMA XI

If a comet void of all motion was let fall from the height SN, *or* Sμ + ⅓Iμ, *towards the sun, and was still impelled to the sun by the same force uniformly continued by which it was impelled at first, the same, in one-half of that time in which it might describe the arc* AC *in its own orbit, would in descending describe a space equal to the length* Iμ.

For in the same time that the comet would require to describe the parabolic arc AC, it would (by the last Lemma), with that velocity which it hath in the height SP, describe the chord AC; and, therefore (by Cor. VII, Prop. XVI, Book I), if it was in the same time supposed to revolve by the force of its own gravity in a circle whose semidiameter was SP, it would describe an arc of that circle, the length of which would be to the chord of the parabolic arc AC in the ratio of 1 to $\sqrt{2}$. Therefore if with that weight, which in the height SP it hath towards the sun, it should fall from that height towards the sun, it would (by Cor. IX, Prop. XVI, Book I) in half the said time describe a space equal to the square of half the said chord, divided by four times the height SP, that is, it would describe the space $\frac{AI^2}{4SP}$. But since the weight of the comet towards the sun in the height SN is to the weight of the same towards the sun in the height SP as SP to Sμ, the comet, by the weight which it hath in the height SN, in falling from that height towards the sun, would in the same time describe the space $\frac{AI^2}{4S\mu}$; that is, a space equal to the length Iμ or μM. Q.E.D.



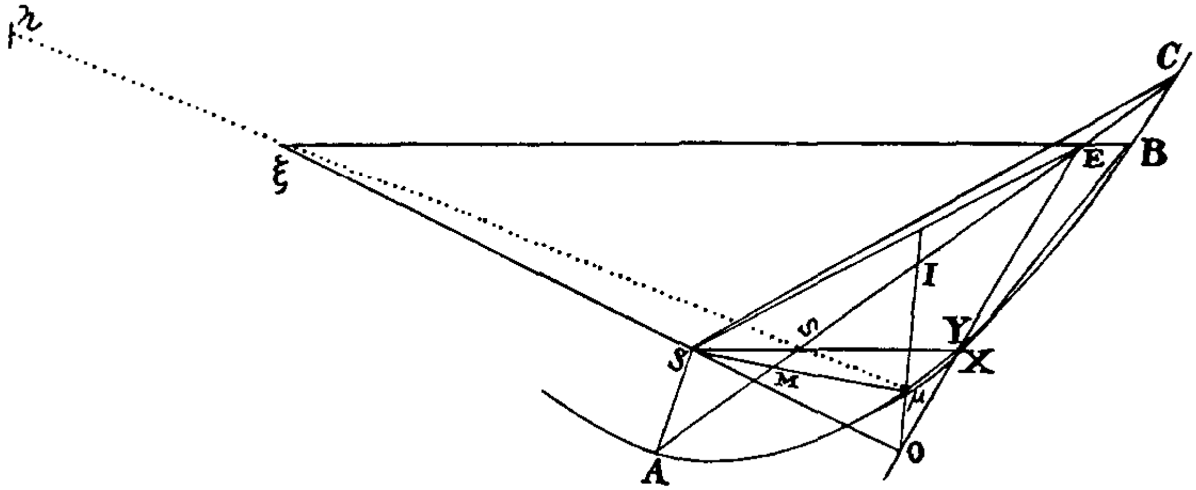
PROPOSITION XLI. PROBLEM XXI

From three given observations to determine the orbit of a comet moving in a parabola.

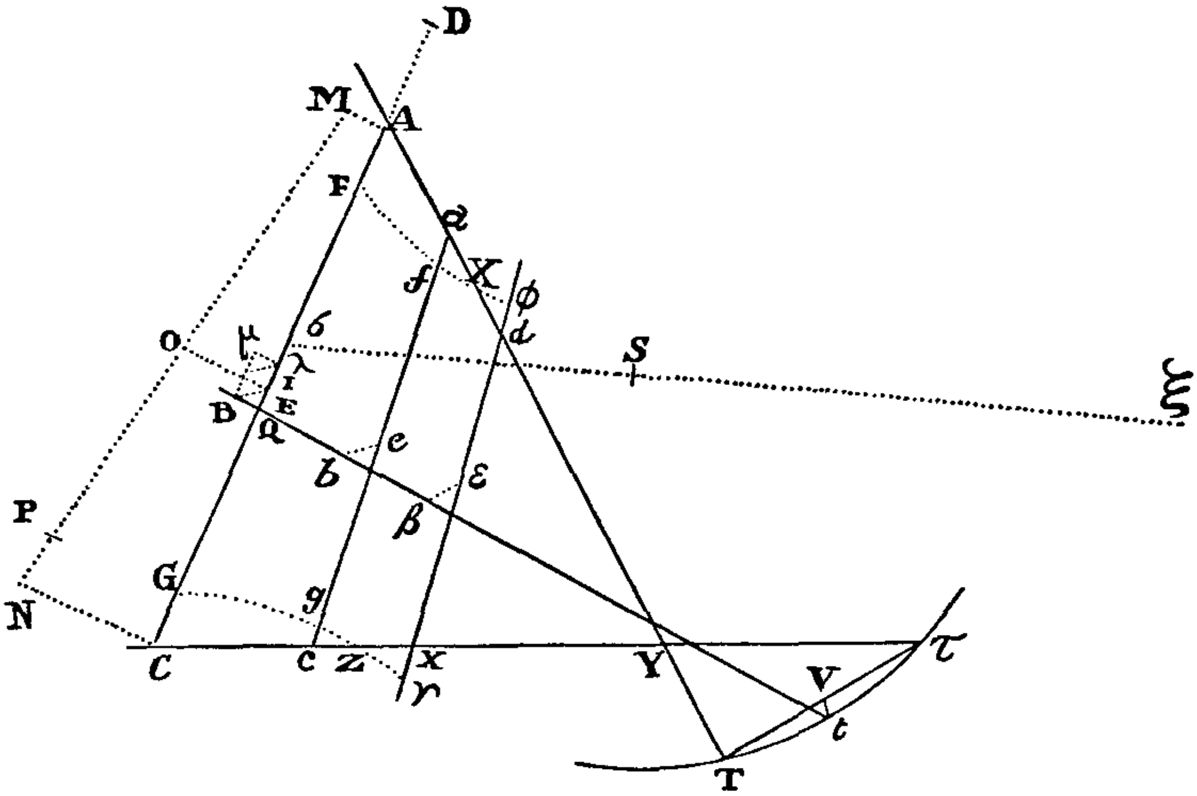
This being a Problem of very great difficulty, I tried many methods of resolving it; and several of those Problems, the composition whereof I have given in the first Book, tended to this purpose. But afterwards I contrived the following solution, which is somewhat more simple.

Select three observations distant one from another by intervals of time nearly equal; but let that interval of time in which the comet moves more slowly be somewhat greater than the other; namely, so that the difference of the times may be to the sum of the times as the sum of the times is to about 600 days; or that the point E may fall nearly upon M and may err therefrom rather towards I than towards A. If such direct observations are not at hand, a new place of the comet must be found, by Lemma VI.

Let S represent the sun; T, *t*, τ three places of the earth in the earth's orbit; TA, *t*B, τC three observed longitudes of the comet; V the time be-



tween the first observation and the second; W the time between the second and the third; X the length which in the whole time V + W the comet might describe with that velocity which it has in the mean distance of the



earth from the sun, which length is to be found by Cor. III, Prop. XL, Book III; and *t*V a perpendicular upon the chord Tτ. In the mean observed longitude *t*B take at pleasure the point B, for the place of the comet in the plane of the ecliptic; and from thence, towards the sun S, draw the line BE, which may be to the perpendicular *t*V as the product of SB and St^2 is to the cube of the hypothenuse of the right-angled triangle whose sides are SB and the tangent of the latitude of the comet in the second observation to the radius *t*B. And through the point E (by Lem. VII) draw the right line AEC, whose parts AE and EC, terminating in the right lines TA and τC, may be one to the other as the times V and W: then A and C will be nearly the places of the comet in the plane of the ecliptic in the first and third observations, if B was its place rightly assumed in the second.

Upon AC, bisected in I, erect the perpendicular I*i*. Through B imagine the line B*i* drawn parallel to AC. Imagine the line S*i* drawn, cutting AC in λ, and complete the parallelogram *i*Iλμ. Take Iσ equal to 3Iλ; and through the sun S imagine the line σξ drawn equal to 3Sσ + 3*i*λ. Then, canceling the letters A, E, C, I, from the point B towards the point ξ, imagine the new line BE drawn, which may be to the former BE as the square of the ratio of the distance BS to the quantity Sμ + ⅓*i*λ. And through the point E draw again the right line AEC by the same rule as before; that is, so that its parts AE and EC may be one to the other as the times V and W between the observations. Thus A and C will be the places of the comet more accurately.

Upon AC, bisected in I, erect the perpendiculars AM, CN, IO, of which AM and CN may be the tangents of the latitudes in the first and third observations, to the radii TA and τC. Join MN, cutting IO in O. Draw the rectangular parallelogram *i*Iλμ, as before. In IA produced take ID equal to Sμ + ⅔*i*λ. Then in MN, towards N, take MP, which may be to the above-found length X as the square root of the ratio of the mean distance of the earth from the sun (or of the semidiameter of the earth's orbit) to the distance OD. If the point P fall upon the point N; A, B, and C will be three places of the comet, through which its orbit is to be described in the plane of the ecliptic. But if the point P falls not upon the point N, in the right line AC take CG equal to NP, so that the points G and P may lie on the same side of the line NC.

By the same method as the points E, A, C, G were found from the assumed point B, from other points *b* and *β* assumed at pleasure, find out the new points *e, a, c, g;* and ε, α, κ, γ. Then through G, *g,* and γ draw the circumference of a circle G*g*γ, cutting the right line τC in Z: and Z will be one place of the comet in the plane of the ecliptic. And in AC, *ac,* *a*κ, taking AF, *af,* αφ, equal respectively to CG, *cg,* κγ; through the points F, *f,* and φ, draw the circumference of a circle $Ff\phi$, cutting the right line AT in X; and the point X will be another place of the comet in the plane of the ecliptic. And at the points X and Z, erecting the tangents of the latitudes of the comet to the radii TX and τZ, two places of the comet in its own orbit will be determined. Lastly, if (by Prop. XIX, Book I) to the focus S a parabola is described passing through those two places, this parabola will be the orbit of the comet. Q.E.I.

The demonstration of this construction follows from the preceding Lemmas, because the right line AC is cut in E in the proportion of the times, by Lemma VII, as it ought to be by Lemma VIII; and BE, by Lemma XI, is a portion of the right line BS or Bξ in the plane of the ecliptic, intercepted between the arc ABC and the chord AEC; and MP (by Cor., Lem. X) is the length of the chord of that arc, which the comet should describe in its proper orbit between the first and third observations, and therefore is equal to MN, providing B is a true place of the comet in the plane of the ecliptic.

But it will be convenient to assume the points B, *b*, β, not at random, but nearly true. If the angle AQ*t*, at which the projection of the orbit in the plane of the ecliptic cuts the right line *t*B, is roughly known, at that angle with B*t* draw the line AC, which may be to $\frac{4}{3}$Tτ as the square root of the ratio of SQ to S*t*; and, drawing the right line SEB so as its part EB may be equal to the length V*t*, the point B will be determined, which we are to use for the first time. Then, canceling the right line AC and drawing anew AC according to the preceding construction, and, moreover, finding the length MP, in *t*B take the point *b*, by this rule, that, if TA and τC intersect each other in Y, the distance Y*b* may be to the distance YB in a ratio compounded of the ratio of MP to MN, and the square root of the ratio of SB to S*b*. And by the same method you may find the third point β, if you please to repeat the operation the third time; but if this method is followed, two operations generally will be sufficient; for if the distance B*b* happens to be very small, after the points F, *f*, and G, *g*, are found, draw the right lines F*f* and G*g*, and they will cut TA and τC in the points required, X and Z.

EXAMPLE

Let the comet of the year 1680 be proposed. The following table shows the motion thereof, as observed by *Flamsteed*, and calculated afterwards by him from his observations, and corrected by Dr. *Halley* from the same observations.

	Time		Sun's longitude	Comet's	
	Apparent	True		Longitude	Latitude north
	h m	h m s	° ′ ″	° ′ ″	° ′ ″
1680, *Dec.* 12	4.46	4.46. 0	♑ 1.51.23	♑ 6.32.30	8.28. 0
21	6.32½	6.36.59	11.06.44	♒ 5.08.12	21.42.13
24	6.12	6.17.52	14.09.26	18.49.23	25.23. 5
26	5.14	5.20.44	16.09.22	28.24.13	27.00.52
29	7.55	8.03.02	19.19.43	♓13.10.41	28.09.58
30	8.02	8.10.26	20.21.09	17.38.20	28.11.53
1681, *Jan.* 5	5.51	6.01.38	26.22.18	♈ 8.48.53	26.15. 7
9	6.49	7.00.53	♒ 0.29.02	18.44.04	24.11.56
10	5.54	6.06.10	1.27.43	20.40.50	23.43.52
13	6.56	7.08.55	4.33.20	25.59.48	22.17.28
25	7.44	7.58.42	16.45.36	♉ 9.35. 0	17.56.30
30	8.07	8.21.53	21.49.58	13.19.51	16.42.18
Feb. 2	6.20	6.34.51	24.46.59	15.13.53	16.04. 1
5	6.50	7.04.41	27.49.51	16.59.06	15.27. 3

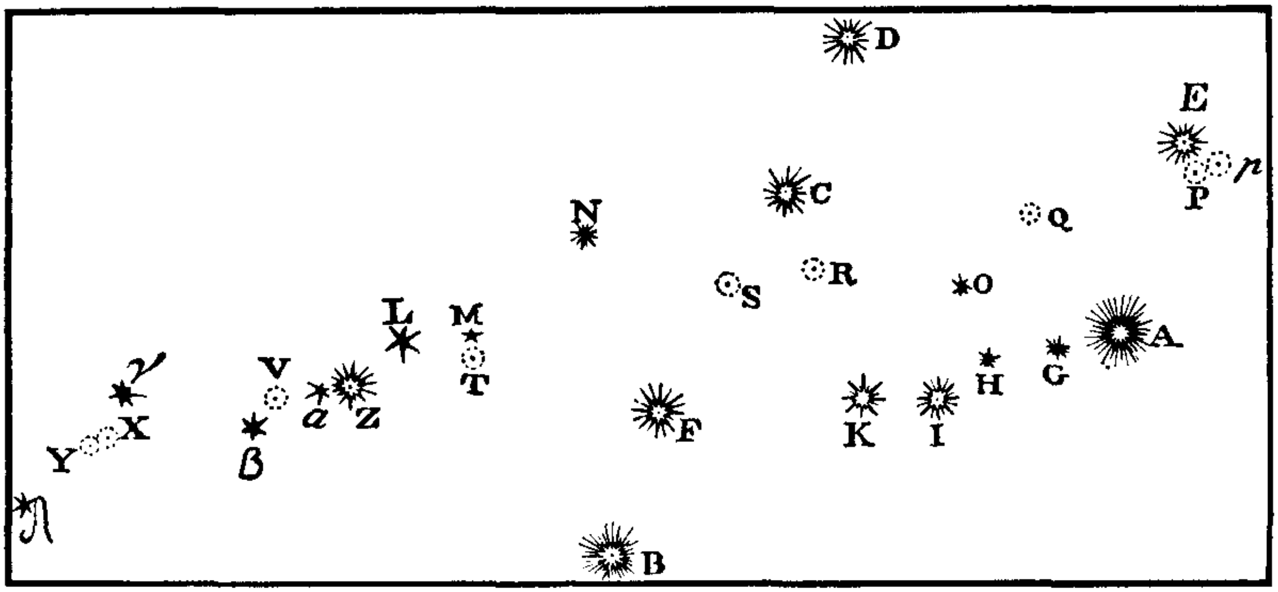
To these you may add some observations of mine.[1]

	Apparent time	Comet's	
		Longitude	Latitude north
	h m	° ′ ″	° ′ ″
1681, *Feb.* 25	8.30	♉ 26.18.35	12.46.46
27	8.15	27.04.30	12.36.12
Mar. 1	11. 0	27.52.42	12.23.40
2	8. 0	28.12.48	12.19.38
5	11.30	29.18. 0	12.03.16
7	9.30	♊ 0. 4. 0	11.57. 0
9	8.30	0.43. 4	11.45.52

These observations were made by a telescope of 7 feet, with a micrometer and threads placed in the focus of the telescope; by these instruments we

[[1] Appendix, Note 51.]

determined the positions both of the fixed stars among themselves, and of the comet in respect of the fixed stars. Let A represent the star of the fourth magnitude in the left heel of Perseus (*Bayer's o*), B the following star of the third magnitude in the left foot (*Bayer's ζ*), C a star of the sixth magni-



tude (*Bayer's n*) in the heel of the same foot, and D, E, F, G, H, I, K, L, M, N, O, Z, α, β, γ, δ other smaller stars in the same foot; and let *p*, P, Q, R, S, T, V, X represent the places of the comet in the observations above set down; and, reckoning the distance AB of $80\frac{7}{12}$ parts, AC was $52\frac{1}{4}$ of those parts; BC, $58\frac{5}{6}$; AD, $57\frac{5}{12}$; BD, $82\frac{6}{11}$; CD, $23\frac{2}{3}$; AE, $29\frac{4}{7}$; CE, $57\frac{1}{2}$; DE, $49\frac{11}{12}$; AI, $27\frac{7}{12}$; BI, $52\frac{1}{6}$; CI, $36\frac{7}{12}$; DI, $53\frac{5}{11}$; AK, $38\frac{2}{3}$; BK, 43; CK,

The fixed stars	Their longitudes	Latitude north	The fixed stars	Their longitudes	Latitude north
	° ′ ″	° ′ ″		° ′ ″	° ′ ″
A	♉ 26.41.50	12. 8.36	L	♉ 29.33.34	12. 7.48
B	28.40.23	11.17.54	M	29.18.54	12. 7.20
C	27.58.30	12.40.25	N	28.48.29	12.31. 9
E	26.27.17	12.52. 7	Z	29.44.48	11.57.13
F	28.28.37	11.52.22	α	29.52. 3	11.55.48
G	26.56. 8	12. 4.58	β	♊ 0. 8.23	11.48.56
H	27.11.45	12. 2. 1	γ	0.40.10	11.55.18
I	27.25. 2	11.53.11	δ	1. 3.20	11.30.42
K	27.42. 7	11.53.26			

31⅝; FK, 29; FB, 23; FC, 36¼; AH, 18⁶⁄₇; DH, 50⅞; BN, 46⁵⁄₁₂; CN, 31⅓; BL, 45⁵⁄₁₂; NL, 31⁵⁄₇. HO was to HI as 7 to 6, and, produced, did pass between the stars D and E, so as the distance of the star D from this right line was ⅙CD. LM was to LN as 2 to 9, and, produced, did pass through the star H. Thus were the positions of the fixed stars determined in respect to one another.

Mr. *Pound* has since observed a second time the positions of these fixed stars amongst themselves, and obtained their longitudes and latitudes according to the preceding table (p. 509).

The positions of the comet to these fixed stars were observed to be as follows:

Friday, *February* 25, O.S., at 8½^{h}. P.M., the distance of the comet in p from the star E was less than $\frac{3}{13}$AE, and greater than ⅕AE, and therefore nearly equal to $\frac{3}{14}$AE; and the angle ApE was a little obtuse, but almost right. For from A, letting fall a perpendicular on pE, the distance of the comet from that perpendicular was ⅕pE.

The same night, at 9½^{h}., the distance of the comet in P from the star E was greater than $\frac{1}{4\frac{1}{2}}$ AE, and less than $\frac{1}{5\frac{1}{4}}$ AE, and therefore nearly equal to $\frac{1}{4\frac{7}{8}}$ of AE, or $\frac{8}{39}$AE. But the distance of the comet from the perpendicular let fall from the star A upon the right line PE was ⅘PE.

Sunday, *February* 27, 8¼^{h}. P.M., the distance of the comet in Q from the star O was equal to the distance of the stars O and H; and the right line QO produced passed between the stars K and B. I could not, by reason of intervening clouds, determine the position of the star to greater accuracy.

Tuesday, *March* 1, 11^{h}. P.M., the comet in R lay exactly in a line between the stars K and C, so as the part CR of the right line CRK was a little greater than ⅓CK, and a little less than ⅓CK + ⅛CR, and therefore = ⅓CK + $\frac{1}{16}$CR, or $\frac{16}{45}$CK.

Wednesday, *March* 2, 8^{h}. P.M., the distance of the comet in S from the star C was nearly $\frac{4}{9}$FC; the distance of the star F from the right line CS produced was $\frac{1}{24}$FC; and the distance of the star B from the same right line was five times greater than the distance of the star F; and the right line NS produced passed between the stars H and I five or six times nearer to the star H than to the star I.

Saturday, *March* 5, $11\frac{1}{2}^h$. P.M., when the comet was in T, the right line MT was equal to $\frac{1}{2}$ML, and the right line LT produced passed between B and F four or five times nearer to F than to B, cutting off from BF a fifth or sixth part thereof towards F; and MT produced passed on the outside of the space BF towards the star B four times nearer to the star B than to the star F. M was a very small star, scarcely to be seen by the telescope; but the star L was greater, and of about the eighth magnitude.

Monday, *March* 7, $9\frac{1}{2}^h$. P.M., the comet being in V, the right line Va produced did pass between B and F, cutting off, from BF towards F, $\frac{1}{10}$ of BF, and was to the right line Vβ as 5 to 4. And the distance of the comet from the right line $a\beta$ was $\frac{1}{2}$Vβ.

Wednesday, *March* 9, $8\frac{1}{2}^h$. P.M., the comet being in X, the right line γX was equal to $\frac{1}{4}\gamma\delta$; and the perpendicular let fall from the star δ upon the right line γX was $\frac{2}{5}$ of $\gamma\delta$.

The same night, at 12^h., the comet being in Y, the right line γY was equal to $\frac{1}{3}$ of $\gamma\delta$, or a little less, as perhaps $\frac{5}{16}$ of $\gamma\delta$; and a perpendicular let fall from the star δ on the right line γY was equal to about $\frac{1}{6}$ or $\frac{1}{7}$ $\gamma\delta$. But the comet, being then extremely near the horizon, was scarcely discernible, and therefore its place could not be determined with the same certainty as in the foregoing observations.

From these observations, by constructions of figures and calculations, I deduced the longitudes and latitudes of the comet; and Mr. *Pound,* by correcting the places of the fixed stars, has determined more correctly the places of the comet, which correct places are set down above. Though my micrometer was none of the best, yet the errors in longitude and latitude (as derived from my observations) scarcely exceed one minute. The comet (according to my observations), about the end of its motion, began to decline sensibly towards the north, from the parallel which it described about the end of *February.*

Now, in order to determine the orbit of the comet from the observations above described, I selected those three which *Flamsteed* made (*Dec.* 21, *Jan.* 5, and *Jan.* 25); from which I found St of 9842.1 parts, and Vt of 455, supposing the semidiameter of the earth's orbit contains 10000. Then, for the first observation, assuming tB of 5657 of those parts, I found SB 9747, BE for the first time 412, Sμ 9503, $i\lambda$ 413, BE for the second time 421, OD 10186, X 8528.4, PM 8450, MN 8475, NP 25; from this, by the second operation, I obtained the distance *tb* 5640; and by this operation I at last deduced the distances TX 4775 and τZ 11322. From these values, determining the orbit, I found its descending node in ♋, and ascending node in ♑ 1° 53′; the inclination of its plane to the plane of the ecliptic 61° 20⅓′, the vertex thereof (or the perihelion of the comet) distant from the node 8° 38′, and in ♐ 27° 43′, with latitude 7° 34′ south; its latus rectum 236.8; and the diurnal area described by a radius drawn to the sun 93585, supposing the square of the semidiameter of the earth's orbit 100000000; that the comet in this orbit moved directly according to the order of the signs, and on *Dec.* 8d. 00h. 04m. P.M. was in the vertex or perihelion of its orbit. All this I determined by scale and compass, and the chords of angles, taken from the table of natural sines, in a pretty large figure, in which, to wit, the radius of the earth's orbit (consisting of 10000 parts) was equal to 16⅓ inches of an *English* foot.

Lastly, in order to discover whether the comet did truly move in the orbit so determined, I investigated its places in this orbit partly by arithmetical operations, and partly by scale and compass, to the times of some of the observations, as may be seen in the following table:

	The Comet's						
	Distance from sun	Longitude computed	Latitude computed	Longitude observed	Latitude observed	Difference longitude	Difference latitude
Dec. 12	2792	♑ 6°32′	8°18½	♑ 6°31½	8°26	+1	− 7½
29	8403	♓13.13⅔	28.00	♓13.11¾	28.10 1/12	+2	−10 1/12
Feb. 5	16669	♉17.00	15.29⅔	♉16.59⅞	15.27⅖	+0	+ 2¼
Mar. 5	21737	29.19¾	12. 4	29.20 6/7	12. 3½	−1	+ ½

But afterwards Dr. *Halley* did determine the orbit to a greater accuracy by an arithmetical calculus than could be done by graphic operations; and, retaining the place of the nodes in ♋ and ♑ 1° 53′, and the inclination of the plane of the orbit to the ecliptic 61° 20⅓′, as well as the time of the comet's being in perihelion, *Dec.* 8d. 00h. 04m. he found the distance of the perihelion from the ascending node measured in the comet's orbit 9° 20′, and the latus rectum of the parabola 2430 parts, supposing the mean distance of the sun from the earth to be 100000 parts; and from these data by an accurate arithmetical calculus, he computed the places of the comet to the times of the observations as given in the table on this page.

This comet also appeared in the *November* before, and at *Coburg,* in *Saxony,* was observed by Mr. *Gottfried Kirch,* on the 4th of that month, on the 6th and 11th, o.s.; from its positions to the nearest fixed stars observed with sufficient accuracy, sometimes with a two-foot, and sometimes with a ten-foot telescope; from the difference of longitudes of *Coburg* and *London,* 11°; and from the places of the fixed stars observed by Mr. *Pound,* Dr. *Halley* has determined the places of the comet as follows:

Nov. 3, $17^h. 2^m.$, apparent time at *London,* the comet was in ♌ 29° 51′, with 1° 17′ 45″ latitude north.

Nov. 5, $15^h. 58^m.$, the comet was in ♍ 3° 23′, with 1° 6′ latitude north.

Nov. 10, $16^h. 31^m.$, the comet was equally distant from two stars in ♌, which are designated σ and τ in *Bayer;* but it had not quite touched the right line that joins them, but was very little distant from it. In *Flamsteed's*

True time	The Comet's			Errors in	
	Distance from the sun	Longitude computed	Latitude computed	Longitude	Latitude
d h m		° ′ ″	° ′ ″	′ ″	′ ″
Dec. 12. 4.46.	28028	♑ 6.29.25	8.26. 0 bor.	−3. 5	−2. 0
21. 6.37.	61076	♒ 5. 6.30	21.43.20	−1.42	+1. 7
24. 6.18.	70008	18.48.20	25.22.40	−1. 3	−0.25
26. 5.20.	75576	28.22.45	27. 1.36	−1.28	+0.44
29. 8. 3.	84021	♓ 13.12.40	28.10.10	+1.59	+0.12
30. 8.10.	86661	17.40. 5	28.11.20	+1.45	−0.33
Jan. 5. 6. 1.½	101440	♈ 8.49.49	26.15.15	+0.56	+0. 8
9. 7. 0.	110959	18.44.36	24.12.54	+0.32	+0.58
10. 6. 6.	113162	20.41. 0	23.44.10	+0.10	+0.18
13. 7. 9.	120000	26. 0.21	22.17.30	+0.33	+0. 2
25. 7.59.	145370	♉ 9.33.40	17.57.55	−1.20	+1.25
30. 8.22.	155303	13.17.41	16.42. 7	−2.10	−0.11
Feb. 2. 6.35.	160951	15.11.11	16. 4.15	−2.42	+0.14
5. 7. 4.½	166686	16.58.55	15.29.13	−0.41	+2. 0
25. 8.41	202570	26.15.46	12.48. 0	−2.49	+1.10
Mar. 5.11.39.	216205	29.18.35	12. 5.40	+0.35	+2.14

catalogue this star ο was then in ♍ 14° 15′, with 1° 41′ lat. north nearly, and τ in ♍ 17° 3½′ with 0° 34′ lat. south; and the middle point between those stars was ♍ 15° 39¼′, with 0° 33½′ lat. north. Let the distance of the comet from that right line be about 10′ or 12′; and the difference of the longitude of the comet and that middle point will be 7′; and the difference of the latitude nearly 7½′; and thence it follows that the comet was in ♍ 15° 32′, with about 26′ lat. north.

The first observation from the position of the comet with respect to certain small fixed stars had all the exactness that could be desired; the second also was accurate enough. In the third observation, which was the least accurate, there might be an error of 6′ or 7′, but hardly greater. The longitude of the comet, as found in the first and most accurate observation, being

True time		Longitude observed	Latitude N observed	Longitude computed	Latitude computed	Errors in longitude	Errors in latitude
	d h m	° ′ ″	° ′ ″	° ′ ″	° ′ ″	′ ″	′ ″
Nov.	3.16.47	♌ 29.51. 0	1.17.45	♌ 29.51.22	1.17.32 N	+0.22	−0.13
	5.15.37	♍ 3.23. 0	1. 6. 0	♍ 3.24.32	1. 6. 9	+1.32	+0. 9
	10.16.18	15.32. 0	0.27. 0	15.33. 2	0.25. 7	+1. 2	−1.53
	16.17.00			♎ 8.16.45	0.53. 7 S		
	18.21.34			18.52.15	1.26.54		
	20.17. 0			28.10.36	1.53.35		
	23.17. 5			♏ 13.22.42	2.29. 0		
Dec.	12. 4.46	♑ 6.32.30	8.28. 0	♑ 6.31.20	8.29. 6 N	−1.10	+1. 6
	21. 6.37	♒ 5. 8.12	21.42.13	♒ 5. 6.14	21.44.42	−1.58	+2.29
	24. 6.18	18.49.23	25.23. 5	18.47.30	25.23.35	−1.53	+0.30
	26. 5.21	28.24.13	27. 0.52	28.21.42	27. 2. 1	−2.31	+1. 9
	29. 8. 3	♓ 13.10.41	28. 9.58	♓ 13.11.14	28.10.38	+0.33	+0.40
	30. 8.10	17.38. 0	28.11.53	17.38.27	28.11.37	+0. 7	−0.16
Jan.	5. 6. 1½	♈ 8.48.53	26.15. 7	♈ 8.48.51	26.14.57	−0. 2	−0.10
	9. 7. 1	18.44. 4	24.11.56	18.43.51	24.12.17	−0.13	+0.21
	10. 6. 6	20.40.50	23.43.32	20.40.23	23.43.25	−0.27	−0. 7
	13. 7. 9	25.59.48	22.17.28	26. 0. 8	22.16.32	+0.20	−0.56
	25. 7.59	♉ 9.35. 0	17.56.30	♉ 9.34.11	17.56. 6	−0.49	−0.24
	30. 8.22	13.19.51	16.42.18	13.18.28	16.40. 5	−1.23	−2.13
Feb.	2. 6.35	15.13.53	16. 4. 1	15.11.59	16. 2.17	−1.54	−1.54
	5. 7. 4½	16.59. 6	15.27. 3	16.59.17	15.27. 0	+0.11	−0. 3
	25. 8.41	26.18.35	12.46.46	26.16.59	12.45.22	−1.36	−1.24
Mar.	1.11.10	27.52.42	12.23.40	27.51.47	12.22.28	−0.55	−1.12
	5.11.39	29.18. 0	12. 3.16	29.20.11	12. 2.50	+2.11	−0.26
	9. 8.38	♊ 0.43. 4	11.45.52	♊ 0.42.43	11.45.35	−0.21	−0.17

computed in the aforesaid parabolic orbit, comes out ♌ 29° 30′ 22″, its latitude north 1° 25′ 7″, and its distance from the sun 115546.

Moreover, Dr. *Halley,* observing that a remarkable comet had appeared four times at equal intervals of 575 years (that is, in the month of *September* after *Julius Caesar* was killed; *An. Chr.* 531, in the consulate of *Lampadius* and *Orestes; An. Chr.* 1106, in the month of *February;* and at the end of the year 1680; and that with a long and remarkable tail, except when it was seen after *Caesar's* death, at which time, by reason of the inconvenient situation of the earth, the tail was not so conspicuous), set himself to find out an elliptic orbit whose greater axis should be 1382957 parts, the mean distance of the earth from the sun containing 10000 such; in this orbit a comet might revolve in 575 years; and, placing the ascending node in ♋ 2° 2′, the inclination of the plane of the orbit to the plane of the ecliptic in an angle of 61° 6′ 48″, the perihelion of the comet in this plane in ♐ 22° 44′ 25″, the equal time of the perihelion *Dec.* 7^{d}. 23^{h}. 9^{m}., the distance of the perihelion from the ascending node in the plane of the ecliptic 9° 17′ 35″, and its conjugate axis 18481.2, he computed the motions of the comet in this elliptic orbit. The places of the comet, as deduced from the observations, and as arising from computation made in this orbit, may be seen in the preceding table (p. 514).

The observations of this comet from the beginning to the end agree as perfectly with the motion of the comet in the orbit just now described as the motions of the planets do with the theories from whence they are calculated, and by this agreement plainly evince that it was one and the same comet that appeared all that time, and also that the orbit of that comet is here rightly defined.

In the foregoing table we have omitted the observations of *Nov.* 16, 18, 20, and 23, as not sufficiently accurate, for at those times several persons had observed the comet. *Nov.* 17, O.S., *Ponthio* and his companions, at 6^{h}. in the morning at *Rome* (that is, $5^{h}.10^{m}$. at *London*), by threads directed to the fixed stars, observed the comet in ♎ 8° 30′, with latitude 0° 40′ south. Their observations may be seen in a treatise which *Ponthio* published concerning this comet. *Cellio,* who was present, and communicated his observations in a letter to *Cassini,* saw the comet at the same hour in ♎ 8° 30′, with latitude 0° 30′ south. It was likewise seen by *Gallet* at the same hour at

Avignon (that is, at 5^{h}. 42^{m}. morning at *London*) in ♎ 8° without latitude. But by the theory the comet was at that time in ♎ 8° 16′ 45″, and its latitude was 0° 53′ 7″ south.

Nov. 18, at 6^{h}. 30^{m}. in the morning at *Rome* (that is, at 5^{h}. 40^{m}. at *London*), *Ponthio* observed the comet in ♎ 13° 30′, with latitude 1° 20′ south; and *Cellio* in ♎ 13° 30′, with latitude 1° 00′ south. But at 5^{h}. 30^{m}. in the morning at *Avignon, Gallet* saw it in ♎ 13° 00′, with latitude 1° 00′ south. In the University of *La Fleche,* in *France,* at 5^{h}. in the morning (that is, at 5^{h}. 9^{m}. at *London*), it was seen by *Ango,* in the middle between two small stars, one of which is the middle of the three which lie in a right line in the southern hand of Virgo, *Bayer's* ψ; and the other is the outmost of the wing, *Bayer's* θ. Hence the comet was then in ♎ 12° 46′ with latitude 50′ south. And I was informed by Dr. *Halley,* that on the same day at *Boston* in *New England,* in the latitude of 42½′, at 5^{h}. in the morning (that is, at 9^{h}. 44^{m}. in the morning at *London*), the comet was seen near ♎ 14°, with latitude 1° 30′ south.

Nov. 19, at $4\frac{1}{2}^{h}$. at *Cambridge,* the comet (by the observation of a young man) was distant from Spica ♍ about 2° towards the northwest. Now the Spike was at that time in ♎ 19° 23′ 47″, with latitude 2° 1′ 59″ south. The same day, at 5^{h}. in the morning, at *Boston* in *New England,* the comet was distant from Spica ♍ 1°, with the difference of 40′ in latitude. The same day, in the island of *Jamaica,* it was about 1° distant from Spica ♍. The same day, Mr. *Arthur Storer,* at the river *Patuxent,* near *Hunting Creek,* in *Maryland,* in the confines of *Virginia,* in lat. 38½°, at 5 in the morning (that is, at 10^{h}. at *London*), saw the comet above Spica ♍, and very nearly joined. with it, the distance between them being about ¾ of one degree. And from these observations compared, I conclude, that at 9^{h}. 44^{m}. at *London* the comet was in ♎ 18° 50′, with about 1° 25′ latitude south. Now by the theory the comet was at that time in ♎ 18° 52′ 15″, with 1° 26′ 54″ lat. south.

Nov. 20, *Montenari,* Professor of Astronomy at *Padua,* at 6^{h}. in the morning at *Venice* (that is, 5^{h}. 10^{m}. at *London*), saw the comet in ♎ 23°, with latitude 1° 30′ south. The same day, at *Boston,* it was distant from Spica ♍ by about 4° of longitude east, and therefore was in ♎ 23° 24′, nearly.

Nov. 21, *Ponthio* and his companions, at $7\frac{1}{4}^{h}$. in the morning, observed the comet in ♎ 27° 50′, with latitude 1° 16′ south; *Cellio,* in ♎ 28°; *Ango* at 5^h. in the morning, in ♎ 27° 45′; *Montenari* in ♎ 27° 51′. The same day, in the island of *Jamaica,* it was seen near the beginning of ♏, and of about the same latitude with Spica ♍, that is, 2° 2′. The same day, at 5^h. morning, at *Ballasore,* in the *East Indies* (that is, at 11^h. 20^m. of the night preceding at *London*), the distance of the comet from Spica ♍ was taken 7° 35′ to the east. It was in a right line between the Spike and the Balance, and therefore was then in ♎ 26° 58′, with about 1° 11′ lat. south; and after 5^h. 40^m. (that is, at 5^h. in the morning at *London*), it was in ♎ 28° 12′ with 1° 16′ lat. south. Now by the the theory the comet was then in ♎ 28° 10′ 36″, with 1° 53′ 35″ lat. south.

Nov. 22, the comet was seen by *Montenari* in ♏ 2° 33′; but at *Boston* in *New England* it was found in about ♏ 3°, and with almost the same latitude as before, that is, 1° 30′. The same day, at 5^h. morning at *Ballasore,* the comet was observed in ♏ 1° 50′; and therefore at 5^h. morning at *London,* the comet was in ♏ 3° 5′, nearly. The same day at $6\frac{1}{2}^h$. in the morning at *London,* Dr. *Hooke* observed it in about ♏ 3° 30′, and that in the right line which passeth through Spica ♍ and Cor Leonis; not, indeed, exactly, but deviating a little from that line towards the north. *Montenari* likewise observed, that this day, and some days after, a right line drawn from the comet through Spica passed by the south side of Cor Leonis at a very small distance therefrom. The right line through Cor Leonis and Spica ♍ did cut the ecliptic in ♍ 3° 46′ at an angle of 2° 51′; and, if the comet had been in this line and in ♏ 3°, its latitude would have been 2° 26′; but since *Hooke* and *Montenari* agree that the comet was at some small distance from this line towards the north, its latitude must have been somewhat less. On the 20th, by the observation of *Montenari,* its latitude was almost the same with that of Spica ♍, that is, about 1° 30′. But by the agreement of *Hooke, Montenari,* and *Ango,* the latitude was continually increasing, and therefore must now, on the 22d, be sensibly greater than 1° 30′; and, taking a mean between the extreme limits but now stated, 2° 26′ and 1° 30′, the latitude will be about 1° 58′. *Hooke* and *Montenari* agree that the tail of the comet was directed towards Spica ♍, declining a little from that star towards the south according to *Hooke,* but towards the north according to *Montenari;* and, therefore, that declination was scarcely sensible; and the tail, lying nearly parallel to the equator, deviated a little from the opposition of the sun towards the north.

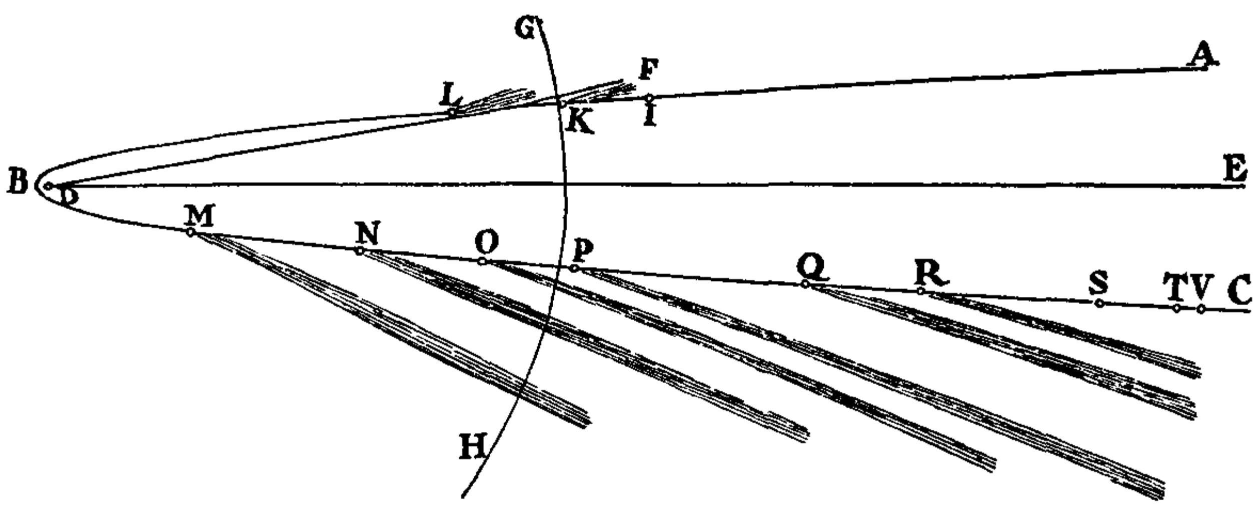
Nov. 23, o.s., at 5^h. morning, at *Nuremberg* (that is, at $4\frac{1}{2}^h$. at *London*), Mr. *Zimmerman* saw the comet in ♏ 8° 8′, with 2° 31′ south lat., its place being obtained by taking its distances from fixed stars.

Nov. 24, before sunrise, the comet was seen by *Montenari* in ♏ 12° 52′ on the north side of the right line through Cor Leonis and Spica ♍, and therefore its latitude was somewhat less than 2° 38′; and since the latitude, as we said, by the concurring observations of *Montenari, Ango,* and *Hooke,* was continually increasing, therefore, it was now, on the 24th, somewhat greater than 1° 58′; and, taking the mean quantity, may be reckoned 2° 18′, without any considerable error. *Ponthio* and *Gallet* will have it that the latitude was now decreasing; and *Cellio,* and the observer in *New England,* that it continued the same, viz., of about 1°, or 1½°. The observations of *Ponthio* and *Cellio* are rougher, especially those which were made by taking the azimuths and altitudes; as are also the observations of *Gallet.* Those are better which were made by taking the position of the comet to the fixed stars by *Montenari, Hooke, Ango,* and the observer in *New England,* and sometimes by *Ponthio* and *Cellio.* The same day, at 5^h. morning, at *Ballasore,* the comet was observed in ♏ 11° 45′; and, therefore, at 5^h. morning at *London,* was in ♏ 13°, nearly. And, by the theory, the comet was at that time in ♏ 13° 22′ 42″.

Nov. 25, before sunrise, *Montenari* observed the comet in ♏ 17¾°, nearly; and *Cellio* observed at the same time that the comet was in a right line between the bright star in the right thigh of Virgo and the southern scale of Libra; and this right line cuts the comet's way in ♏ 18° 36′. And, by the theory, the comet was in ♏ 18⅓°, nearly.

From all this it is plain that these observations agree with the theory, so far as they agree with one another; and by this agreement it is made clear that it was one and the same comet that appeared all the time from *Nov.* 4 to *Mar.* 9. The path of this comet did twice cut the plane of the ecliptic, and therefore was not a right line. It did cut the ecliptic not in opposite parts of the heavens, but in the end of Virgo and beginning of Capricorn, including an arc of about 98°; and therefore the way of the comet did very much deviate from the path of a great circle; for in the month of *Nov.* it declined at least 3° from the ecliptic towards the south; and in the month of *Dec.* following it declined 29° from the ecliptic towards the north; the two parts of the orbit in which the comet descended towards the sun, and ascended again from the sun, declining one from the other by an apparent angle of above 30°, as observed by *Montenari.* This comet traveled over nine signs, namely, from the last deg. of ♌ to the beginning of ♊, beside the sign of ♌, through which it passed before it began to be seen; and there is no other theory by which a comet can go over so great a part of the heavens with a regular motion. The motion of this comet was very unequable; for about the 20th of *Nov.* it described about 5° a day. Then its motion being retarded between *Nov.* 26 and *Dec.* 12, to wit, in the space of 15½ days, it described only 40°. But the motion thereof being afterwards accelerated, it described near 5° a day, till its motion began to be again retarded. And the theory which justly corresponds with a motion so unequable, and through so great a part of the heavens, which observes the same laws with the theory of the planets, and which accurately agrees with accurate astronomical observations, cannot be otherwise than true.

And, thinking it would not be improper, I have given in the annexed figure, plotted in the plane of the curve, a true representation of the orbit which this comet described, and of the tail which it emitted in several places. In this drawing ABC represents the orbit of the comet, D the sun,



DE the axis of the orbit, DF the line of the nodes, GH the intersection of the sphere of the earth's orbit with the plane of the comet's orbit, I the place of the comet *Nov.* 4, *Ann.* 1680; K the place of the same *Nov.* 11; L the place of the same *Nov.* 19; M its place *Dec.* 12; N its place *Dec.* 21; O its place *Dec.* 29; P its place *Jan.* 5 following; Q its place *Jan.* 25; R its place *Feb.* 5; S its place *Feb.* 25; T its place *March* 5; and V its place *March* 9. In determining the length of the tail, I made the following observations.

Nov. 4 and 6, the tail did not appear; *Nov.* 11, the tail just began to show itself, but did not appear above ½ deg. long through a 10-foot telescope; *Nov.* 17, the tail was seen by *Ponthio* more than 15° long; *Nov.* 18, in *New England,* the tail appeared 30° long, and directly opposite to the sun, extending itself to the planet Mars, which was then in ♍, 9° 54′; *Nov.* 19, in *Maryland,* the tail was found 15° or 20° long; *Dec.* 10 (by the observation of Mr. *Flamsteed*), the tail passed through the middle of the distance intercepted between the tail of the serpent of Ophiuchus and the star δ in the south wing of Aquila, and did terminate near the stars A, ω, *b* in *Bayer's* tables. Therefore the end of the tail was in ♑ 19½°, with latitude about 34¼° north; *Dec.* 11, it ascended to the head of Sagitta (*Bayer's* α, β), terminating in ♑ 26° 43′, with latitude 38° 34′ north; *Dec.* 12, it passed through the middle of Sagitta, nor did it reach much farther; terminating in ♒ 4°, with latitude 42½° north, nearly. But these things are to be understood of the length of the brighter part of the tail; for, with a more faint light, observed, too, perhaps, in a serener sky, at *Rome, Dec.* 12, $5^h. 40^m.$, by the observation of *Ponthio,* the tail arose to 10° above the rump of the Swan, and the side thereof towards the west and towards the north was 45′ distant from this star. But about that time the tail was 3° broad towards the upper end; and therefore the middle thereof was 2° 15′ distant from that star towards the south, and the upper end was ♓ in 22°, with latitude 61° north; and thence the tail was about 70° long; *Dec.* 21, it extended almost to Cassiopeia's Chair, equally distant from β and from Schedir, so as its distance from either of the two was equal to the distance of the one from the other, and therefore did terminate in ♈ 24°, with latitude 47½°; *Dec.* 29, it reached to a contact with Scheat on its left, and exactly filled up the space between the two stars in the northern foot of Andromeda, being 54° in length; and therefore terminated in ♉ 19°, with 35° of latitude; *Jan.* 5, it touched the star π in the breast of Andromeda on its right side, and the star μ of the girdle on its left; and, according to our observations, was 40° long; but it was curved, and the convex side thereof lay to the south; and near the head of the comet it made an angle of 4° with the circle which passed through the sun and the comet's head; but towards the other end it was inclined to that circle in an angle of about 10° or 11°; and the chord of the tail contained with that circle an angle of 8°. *Jan.* 13, the tail terminated between Alamech and Algol, with a light that was sensible enough; but with a faint light it ended over against the star κ in Perseus' side. The distance of the end of the tail from the circle passing through the sun and the comet was 3° 50′; and the inclination of the chord of the tail to that circle was 8½°. *Jan.* 25 and 26, it shone with a faint light to the length of 6° or 7°; and, for a night or two after, when there was a very clear sky, it extended to the length of 12°, or somewhat more, with a light that was very faint and very hardly to be seen; but the axis thereof was exactly directed to the bright star in the eastern shoulder of Auriga, and therefore deviated from the opposition of the sun towards the north by an angle of 10°. Lastly, *Feb.* 10, with a telescope I observed the tail 2° long; for that fainter light which I spoke of did not appear through the glasses. But *Ponthio* writes, that, on *Feb.* 7, he saw the tail 12° long. *Feb.* 25, the comet was without a tail, and so continued till it disappeared.

Now if one reflects upon the orbit described, and duly considers the other appearances of this comet, he will be easily satisfied that the bodies of comets are solid, compact, fixed, and durable, like the bodies of the planets; for if they were nothing else but the vapors or exhalations of the earth, of the sun, and other planets, this comet, in its passage by the neighborhood of the sun, would have been immediately dissipated; for the heat of the sun is as the density of its rays, that is, inversely as the square of the distance of the places from the sun. Therefore, since on *Dec.* 8, when the comet was in its perihelion, the distance thereof from the centre of the sun was to the distance of the earth from the same as about 6 to 1000, the sun's heat on the comet was at that time to the heat of the summer-sun with us as 1000000 to 36, or as 28000 to 1. But the heat of boiling water is about three times greater than the heat which dry earth acquires from the summer-sun, as I have tried; and the heat of red-hot iron (if my conjecture is right) is about three or four times greater than the heat of boiling water. And therefore the heat which dry earth on the comet, while in its perihelion, might have received from the rays of the sun, was about 2000 times greater than the heat of red-hot iron. But by so fierce a heat, vapors and exhalations, and every volatile matter, must have been immediately consumed and dissipated.

This comet, therefore, must have received an immense heat from the sun, and retained that heat for an exceeding long time; for a globe of iron of an inch in diameter, exposed red-hot to the open air, will scarcely lose all its heat in an hour's time; but a greater globe would retain its heat longer in the ratio of its diameter, because the surface (in proportion to which it is cooled by the contact of the ambient air) is in that ratio less in respect of the quantity of the included hot matter; and therefore a globe of red-hot iron equal to our earth, that is, about 40000000 feet in diameter, would scarcely cool in an equal number of days, or in above 50000 years. But I suspect that the duration of heat may, on account of some latent causes, increase in a yet less ratio than that of the diameter; and I should be glad that the true ratio was investigated by experiments.

It is further to be observed, that the comet in the month of *December,* just after it had been heated by the sun, did emit a much longer tail, and much more splendid, than in the month of *November* before, when it had not yet arrived at its perihelion; and, universally, the greatest and most fulgent tails always arise from comets immediately after their passing by the neighborhood of the sun. Therefore the heat received by the comet conduces to the greatness of the tail: from this, I think I may infer, that the tail is nothing else but a very fine vapor, which the head or nucleus of the comet emits by its heat.

But we have had three several opinions about the tails of comets: for some will have it that they are nothing else but the beams of the sun's light transmitted through the comets' heads, which they suppose to be transparent; others, that they proceed from the refraction which light suffers in passing from the comet's head to the earth; and, lastly, others, that they are a sort of cloud or vapor constantly rising from the comets' heads, and tending towards the parts opposite to the sun. The first is the opinion of such as are yet unacquainted with optics; for the beams of the sun are seen in a darkened room only in consequence of the light that is reflected from them by the little particles of dust and smoke which are always flying about in the air; and, for that reason, in air impregnated with thick smoke, those beams appear with great brightness, and impress the eye more strongly; in a yet finer air they appear more faint, and are less easily discerned; but in the heavens, where there is no matter to reflect the light, they can never be seen at all. Light is not seen as it is in the beam, but as it is thence reflected to our eyes; for vision can be produced in no other way than by rays falling upon the eyes; and, therefore, there must be some reflecting matter in those parts where the tails of the comets are seen: for otherwise, since all the celestial spaces are equally illuminated by the sun's light, no part of the heavens could appear with more splendor than another. The second opinion is liable to many difficulties. The tails of comets are never seen variegated with those colors which commonly are inseparable from refraction; and the distinct transmission of the light of the fixed stars and planets to us is a demonstration that the ether or celestial medium is not endowed with any refractive power: for, as to what is alleged, that the fixed stars have been sometimes seen by the *Egyptians* environed with a coma, because that has but rarely happened, it is rather to be ascribed to a casual refraction of clouds; and so the radiation and scintillation of the fixed stars to the refractions both of the eyes and air; for, upon laying a telescope to the eye, those radiations and scintillations immediately disappear. By the tremulous agitation of the air and ascending vapors, it happens that the rays of light are alternately turned aside from the narrow space of the pupil of the eye; but no such thing can have place in the much wider aperture of the object glass of a telescope; and hence it is that a scintillation is occasioned in the former case, which ceases in the latter; and this cessation in the latter case is a demonstration of the regular transmission of light through the heavens, without any perceptible refraction. But, to obviate an objection that may be made from the appearing of no tail in such comets as shine but with a faint light, as if the secondary rays were then too weak to affect the eyes, and for that reason it is that the tails of the fixed stars do not appear, we are to consider, that by the means of telescopes the light of the fixed stars may be augmented above an hundredfold, and yet no tails are seen; that the light of the planets is yet more copious without any tail; but that comets are seen sometimes with huge tails, when the light of their heads is but faint and dull. For so it happened in the comet of the year 1680, when in the month of *December* it was scarcely equal in light to the stars of the second magnitude, and yet emitted a notable tail, extending to the length of 40°, 50°, 60°, or 70°, and upwards; and afterwards, on the 27th and 28th of *January,* when the head appeared but as a star of the 7th magnitude, yet the tail (as we said above), with a light that was clearly perceptible, though faint, was stretched out to 6° or 7° in length, and with a languishing light that was more difficult to see, even to 12°, and upwards. But on the 9th and 10th of *February,* when to the naked eye the head appeared no more, through a telescope I viewed the tail of 2° in length. But further: if the tail was due to the crumbling of the celestial matter, and did deviate from the opposition of the sun, according to the figure of the heavens, that deviation in the same places of the heavens should be always directed towards the same parts. But the comet of the year 1680, *December* 28^d. $8\frac{1}{2}^h$. P.M. at *London,* was seen in ♓ 8° 41′, with latitude north 28° 6′; while the sun was in ♑ 18° 26′. And the comet of the year 1577, *December* 29^d., was in ♓ 8° 41′, with latitude north 28° 40′, and the sun, as before, in about ♑ 18° 26′. In both cases the situation of the earth was the same, and the comet appeared in the same place of the heavens; yet in the former case the tail of the comet (as well by my observations as by the observations of others) deviated from the opposition of the sun towards the north by an angle of 4½ degrees; whereas in the latter there was (according to the observations of *Tycho*) a deviation of 21 degrees towards the south. The crumbling, therefore, of the heavens being thus disproved, it remains that the phenomena of the tails of comets must be derived from some reflecting matter.

And that the tails of comets do arise from their heads, and tend towards the parts opposite to the sun, is further confirmed from the laws which the tails observe: As that, lying in the planes of the comets' orbits which pass through the sun, they constantly deviate from the opposition of the sun towards the parts which the comets' heads in their progress along these orbits have left. That to a spectator, placed in those planes, they appear in the parts directly opposite to the sun; but, as the spectator recedes from those planes, their deviation begins to appear, and daily becomes greater. That the deviation, other things being equal, appears less when the tail is more oblique to the orbit of the comet, as well as when the head of the comet approaches nearer to the sun, especially if the angle of deviation is estimated near the head of the comet. That the tails which have no deviation appear straight, but the tails which deviate are likewise bended into a certain curvature. That this curvature is greater when the deviation is greater; and is more sensible when the tail, other things being equal, is longer; for in the shorter tails the curvature is hardly to be perceived. That the angle of deviation is less near the comet's head, but greater towards the other end of the tail; and that because the convex side of the tail regards the parts from which the deviation is made, and which lie in a right line drawn out infinitely from the sun through the comet's head. And that the tails that are long and broad, and shine with a stronger light, appear more resplendent and more exactly defined on the convex than on the concave side. Upon these accounts it is plain that the phenomena of the tails of comets depend upon the motions of their heads, and by no means upon the places of the heavens in which their heads are seen; and that, therefore, the tails of comets do not proceed from the refraction of the heavens, but from their own heads, which furnish the matter that forms the tail. For, as in our air, the smoke of a heated body ascends either perpendicularly if the body is at rest, or obliquely if the body is moved obliquely, so in the heavens, where all bodies gravitate towards the sun, smoke and vapor must (as we have already said) ascend from the sun, and either rise perpendicularly if the smoking body is at rest, or obliquely if the body, in all the progress of its motion, is always leaving those places from which the upper or higher parts of the vapor had risen before; and that obliquity will be least where the vapor ascends with most velocity, namely, near the smoking body, when that is near the sun. But, because the obliquity varies, the column of vapor will be incurvated; and because the vapor in the preceding side is something more recent, *that is, has ascended something more late from the body,* it will therefore be somewhat more dense on that side, and must on that account reflect more light, as well as be better defined. I add nothing concerning the sudden uncertain agitation of the tails of comets, and their irregular figures, which authors sometimes describe, because they may arise from the mutations of our air, and the motions of our clouds, in part obscuring those tails; or, perhaps, from parts of the Milky Way which might have been confounded with and mistaken for parts of the tails of the comets as they passed by.

But that the atmospheres of comets may furnish a supply of vapor great enough to fill so immense spaces, we may easily understand from the rarity of our own air; for the air near the surface of our earth possesses a space 850 times greater than water of the same weight; and therefore a cylinder of air 850 feet high is of equal weight with a cylinder of water of the same breadth, and but one foot high. But a cylinder of air reaching to the top of the atmosphere is of equal weight with a cylinder of water about 33 feet high: and, therefore, if from the whole cylinder of air the lower part of 850 feet high is taken away, the remaining upper part will be of equal weight with a cylinder of water 32 feet high: and from thence (and by the hypothesis, confirmed by many experiments, that the compression of air is as the weight of the incumbent atmosphere, and that the force of gravity is inversely as the square of the distance from the centre of the earth) proceeding by calculation, by Cor., Prop. XXII, Book II, I found, that, at the height of one semidiameter of the earth, reckoned from the earth's surface, the air is more rare than with us in a far greater ratio than that of the whole space within the orbit of Saturn to a spherical space one inch in diameter; and therefore, if a sphere of our air of but one inch in thickness was equally rarefied with the air at the height of one semidiameter of the earth from the earth's surface, it would fill all the regions of the planets to the orb of Saturn, and far beyond it. Therefore, since the air at greater distances is immensely rarefied, and the coma or atmosphere of comets is ordinarily about ten times higher, reckoning from their centres, than the surface of the nucleus, and the tails rise yet higher, they must therefore be exceedingly rare; and though, on account of the much thicker atmospheres of comets, and the great gravitation of their bodies towards the sun, as well as of the particles of their air and vapors towards each other, it may happen that the air in the celestial spaces and in the tails of comets is not so vastly rarefied, yet from this computation it is plain that a very small quantity of air and vapor is abundantly sufficient to produce all the appearances of the tails of comets; for that they are, indeed, of a very notable rarity appears from the shining of the stars through them. The atmosphere of the earth, illuminated by the sun's light, though but of a few miles in thickness, quite obscures and extinguishes the light not only of all the stars, but even of the moon itself; whereas the smallest stars are seen to shine through the immense thickness of the tails of comets, likewise illuminated by the sun, without the least diminution of their splendor. Nor is the brightness of the tails of most comets ordinarily greater than that of our air, an inch or two in thickness, reflecting in a darkened room the light of the sunbeams let in by a hole of the window shutter.

And we may pretty nearly determine the time spent during the ascent of the vapor from the comet's head to the extremity of the tail, by drawing a right line from the extremity of the tail to the sun, and marking the place where that right line intersects the comet's orbit; for the vapor that is now in the extremity of the tail, if it has ascended in a right line from the sun, must have begun to rise from the head at the time when the head was in the point of intersection. It is true, the vapor does not rise in a right line from the sun, but, retaining the motion which it had from the comet before its ascent, and compounding that motion with its motion of ascent, arises obliquely; and, therefore, the solution of the Problem will be more exact, if we draw the line which intersects the orbit parallel to the length of the tail; or rather (because of the curvilinear motion of the comet) diverging a little from the line or length of the tail. And by means of this principle I found that the vapor which, *January* 25, was in the extremity of the tail, had begun to rise from the head before *December* 11, and therefore had spent in its whole ascent 45 days; but that the whole tail which appeared on *December* 10 had finished its ascent in the space of the two days then elapsed from the time of the comet's being in its perihelion. The vapor, therefore, about the beginning and in the neighborhood of the sun rose with the greatest velocity, and afterwards continued to ascend with a motion constantly retarded by its own gravity; and the higher it ascended, the more it added to the length of the tail; and while the tail continued to be seen, it was made up of almost all that vapor which had risen since the time of the comet's being in its perihelion; nor did that part of the vapor which had risen first, and which formed the extremity of the tail, cease to appear, till its too great distance, as well from the sun, from which it received its light, as from our eyes, rendered it invisible. Whence also it is that the tails of other comets which are short do not rise from their heads with a swift and continued motion, and soon after disappear, but are permanent and lasting columns of vapors and exhalations, which, ascending from the heads with a slow motion of many days, and partaking of the motion of the heads which they had from the beginning, continue to go along together with them through the heavens. From this again we have another argument proving the celestial spaces to be free, and without resistance, since in them not only the solid bodies of the planets and comets, but also the extremely rare vapors of comets' tails, maintain their rapid motions with great freedom, and for an exceeding long time.

Kepler ascribes the ascent of the tails of the comets to the atmospheres of their heads; and their direction towards the parts opposite to the sun to the action of the rays of light carrying along with them the matter of the comets' tails; and without any great incongruity we may suppose, that, in so free spaces, so fine a matter as that of the ether may yield to the action of the rays of the sun's light, though those rays are not able sensibly to move the gross substances in our parts, which are clogged with so palpable a resistance. Another author thinks that there may be a sort of particles of matter endowed with a principle of levity, as well as others are with a power of gravity; that the matter of the tails of comets may be of the former sort, and that its ascent from the sun may be owing to its levity; but, considering that the gravity of terrestrial bodies is as the matter of the bodies, and therefore can be neither more nor less in the same quantity of matter, I am inclined to believe that this ascent may rather proceed from the rarefaction of the matter of the comets' tails. The ascent of smoke in a chimney is due to the impulse of the air with which it is entangled. The air rarefied by heat ascends, because its specific gravity is diminished, and in its ascent carries along with it the smoke which floats in it; and why may not the tail of a comet rise from the sun after the same manner? For the sun's rays do not act upon the mediums which they pervade otherwise than by reflection and refraction; and those reflecting particles heated by this action, heat the matter of the ether which is involved with them. That matter is rarefied by the heat which it acquires, and because, by this rarefaction, the specific gravity with which it tended towards the sun before is diminished, it will ascend therefrom, and carry along with it the reflecting particles of which the tail of the comet is composed. But the ascent of the vapors is further promoted by their circumgyration about the sun; in consequence thereof they endeavor to recede from the sun, while the sun's atmosphere and the other matter of the heavens are either altogether quiescent, or are only moved with a slower circumgyration derived from the rotation of the sun. And these are the causes of the ascent of the tails of the comets in the neighborhood of the sun, where their orbits are bent into a greater curvature, and the comets themselves are plunged into the denser and therefore heavier parts of the sun's atmosphere: upon which account they do then emit tails of an huge length; for the tails which then arise, retaining their own proper motion, and in the meantime gravitating towards the sun, must be revolved in ellipses about the sun in like manner as the heads are, and by that motion must always accompany the heads, and freely adhere to them. For the gravitation of the vapors towards the sun can no more force the tails to abandon the heads, and descend to the sun, than the gravitation of the heads can oblige them to fall from the tails. They must by their common gravity either fall together towards the sun, or be retarded together in their common ascent therefrom; and, therefore (whether from the causes already described, or from any others), the tails and heads of comets may easily acquire and freely retain any position one to the other, without disturbance or impediment from that common gravitation.

The tails, therefore, that rise in the perihelian positions of the comets will go along with their heads into far remote parts, and together with the heads will either return again from thence to us, after a long course of years, or rather will be there rarefied, and by degrees quite vanish away; for afterwards, in the descent of the heads towards the sun, new short tails will be emitted from the heads with a slow motion; and those tails by degrees will be augmented immensely, especially in such comets as in their perihelian distances descend as low as the sun's atmosphere; for all vapor in those free spaces is in a perpetual state of rarefaction and dilatation; and from hence it is that the tails of all comets are broader at their upper extremity than near their heads. And it is not unlikely but that the vapor, thus continually rarefied and dilated, may be at last dissipated and scattered through the whole heavens, and by little and little be attracted towards the planets by its gravity, and mixed with their atmosphere; for as the seas are absolutely necessary to the constitution of our earth, that from them, the sun, by its heat, may exhale a sufficient quantity of vapors, which, being gathered together into clouds, may drop down in rain, for watering of the earth, and for the production and nourishment of vegetables; or, being condensed with cold on the tops of mountains (as some philosophers with reason judge), may run down in springs and rivers; so for the conservation of the seas, and fluids of the planets, comets seem to be required, that, from their exhalations and vapors condensed, the wastes of the planetary fluids spent upon vegetation and putrefaction, and converted into dry earth, may be continually supplied and made up; for all vegetables entirely derive their growths from fluids, and afterwards, in great measure, are turned into dry earth by putrefaction; and a sort of slime is always found to settle at the bottom of putrefied fluids; and hence it is that the bulk of the solid earth is continually increased; and the fluids, if they are not supplied from without, must be in a continual decrease, and quite fail at last. I suspect, moreover, that it is chiefly from the comets that spirit comes, which is indeed the smallest but the most subtle and useful part of our air, and so much required to sustain the life of all things with us.

The atmospheres of comets, in their descent towards the sun, by running out into the tails, are spent and diminished, and become narrower, at least on that side which regards the sun; and in receding from the sun, when they less run out into the tails, they are again enlarged, if *Hewelcke* has justly marked their appearances. But they are seen least of all just after they have been most heated by the sun, and on that account then emit the longest and most resplendent tails; and, perhaps, at the same time, the nuclei are environed with a denser and blacker smoke in the lowermost parts of their atmosphere; for smoke that is raised by a great and intense heat is commonly the denser and blacker. Thus the head of that comet which we have been describing, at equal distances both from the sun and from the earth, appeared darker after it had passed by its perihelion than it did before; for in the month of *December* it was commonly compared with the stars of the third magnitude, but in *November* with those of the first or second; and such as saw both appearances have described the first as of another and greater comet than the second. For, *November* 19, this comet appeared to a young man at *Cambridge,* though with a pale and dull light, yet equal to Spica Virginis; and at that time it shone with greater brightness than it did afterwards. And *Montenari, November* 20, o.s., observed it larger than the stars of the first magnitude, its tail being then 2 degrees long. And Mr. *Storer* (by letters which have come into my hands) writes, that in the month of *December,* when the tail appeared of the greatest bulk and splendor, the head was but small, and far less than that which was seen in the month of

November before sun rising; and, conjecturing at the cause of the appearance, he judged it to proceed from the existence of a greater quantity of matter in the head at first, which was afterwards gradually spent.

And, for the same reason, I find, that the heads of other comets, which did put forth tails of the greatest bulk and splendor, have appeared but obscure and small. For in *Brazil, March* 5, 1668, N.S., 7^h. P.M., *Valentin Estancel* saw a comet near the horizon, and towards the southwest, with a head so small as scarcely to be discerned, but with a tail above measure splendid, so that the reflection thereof from the sea was easily seen by those who stood on the shore; it looked like a fiery beam extended 23 degrees in length from the west to south, almost parallel to the horizon. But this excessive splendor continued only three days, decreasing apace afterwards; and while the splendor was decreasing, the bulk of the tail increased: also in *Portugal* it is said to have taken up one-quarter of the heavens, that is, 45 degrees, extending from west to east with a very notable splendor, though the whole tail was not seen in those parts, because the head was always hid under the horizon: and from the increase of the bulk and decrease of the splendor of the tail, it appears that the head was then in its recess from the sun, and had been very near to it in its perihelion, as the comet of 1680 was. And we read, in the *Saxon* Chronicle, of a like comet appearing in the year 1106, *the star whereof was small and obscure* (as that of 1680), *but the splendor of its tail was very bright, and like a huge fiery beam stretched out in a direction between the east and north,* as *Hewelcke* has it also from *Simeon,* the monk of *Durham.* This comet appeared in the beginning of *February,* about the evening, and towards the southwest part of heaven; from this, and from the position of the tail, we infer that the head was near the sun. *Matthew Paris* says, *It was distant from the sun by about a cubit, from three o'clock* (rather six) *till nine, putting forth a long tail.* Such also was that resplendent comet described by *Aristotle,* Book 1, *Meteor.* 6. *The head whereof could not be seen, because it had set before the sun, or at least was hid under the sun's rays; but next day it was seen as well as might be; for, having left the sun but a very little way, it set immediately after it. And the scattered light of the head, obscured by the too great splendor* (of the tail) *did not yet appear. But afterwards* (as *Aristotle* says) *when the splendor* (of the tail) *had diminished,* (the head of) *the comet recovered its* *native brightness; and the splendor* (of its tail) *reached now to a third part of the heavens* (that is, to 60°). *This appearance was in the winter season* (an. 4, Olymp. 101), *and, rising to* Orion's *girdle, it there vanished away.* It is true that the comet of 1618, which came out directly from under the sun's rays with a very large tail, seemed to equal, if not to exceed, the stars of the first magnitude; but then, abundance of other comets have appeared yet greater than this, that put forth shorter tails; some of which are said to have appeared as big as Jupiter, others as big as Venus, or even as the moon.

We have said, that comets are a sort of planets revolved in very eccentric orbits about the sun; and as, in the planets which are without tails, those are commonly less which are revolved in lesser orbits, and nearer to the sun, so in comets it is probable that those which in their perihelion approach nearer to the sun are generally of less magnitude, that they may not agitate the sun too much by their attractions. But as to the transverse diameters of their orbits, and the periodic times of their revolutions, I leave them to be determined by comparing comets together which after long intervals of time return again in the same orbit. In the meantime, the following Proposition may give some light in that inquiry.

PROPOSITION XLII. PROBLEM XXII

To correct a comet's orbit found as above.

Operation 1. Assume that position of the plane of the orbit which was determined according to the preceding Proposition; and select three places of the comet, deduced from very accurate observations, and at great distances one from the other. Then suppose A to represent the time between the first observation and the second, and B the time between the second and the third; but it will be convenient that in one of those times the comet be in its perigee, or at least not far from it. From those apparent places find, by trigonometric operations, the three true places of the comet in that assumed plane of the orbit; then through the places found, and about the centre of the sun as the focus, describe a conic section by arithmetical operations, according to Prop. XXI, Book I. Let the areas of this figure which are terminated by radii drawn from the sun to the places found be D and E; namely, D the area between the first observation and the second, and E the area between the second and third; and let T represent the whole time in which the whole area D + E should be described with the velocity of the comet found by Prop. XVI, Book I.

OPER. 2. Retaining the inclination of the plane of the orbit to the plane of the ecliptic, let the longitude of the nodes of the plane of the orbit be increased by the addition of 20′ or 30′, which call P. Then from the aforesaid three observed places of the comet let the three true places be found (as before) in this new plane; as also the orbit passing through those places, and the two areas of the same described between the two observations, which call *d* and *e*; and let *t* be the whole time in which the whole area *d* + *e* should be described.

OPER. 3. Retaining the longitude of the nodes in the first operation, let the inclination of the plane of the orbit to the plane of the ecliptic be increased by adding thereto 20′ or 30′, which call Q. Then from the aforesaid three observed apparent places of the comet let the three true places be found in this new plane, as well as the orbit passing through them, and the two areas of the same described between the observation, which call δ and ε; and let τ be the whole time in which the whole area $\delta + \varepsilon$ should be described.

Then taking C to 1 as A to B; and G to 1 as D to E; and *g* to 1 as *d* to *e*; and γ to 1 as δ to ε; let S be the true time between the first observation and the third; and, observing well the signs + and −, let such numbers *m* and *n* be found out as will make $2G - 2C = mG - mg + nG - n\gamma$; and $2T - 2S = mT - mt + nT - n\tau$. And if, in the first operation, I represents the inclination of the plane of the orbit to the plane of the ecliptic, and K the longitude of either node, then $I + nQ$ will be the true inclination of the plane of the orbit to the plane of the ecliptic, and $K + mP$ the true longitude of the node. And, lastly, if in the first, second, and third operations, the quantities R, *r*, and ρ, represent the parameters of the orbit, and the quantities $\frac{1}{L}, \frac{1}{l}, \frac{1}{\lambda}$, the transverse diameters of the same, then $R + mr - mR + n\rho - nR$ will be the true parameter, and $\frac{1}{L + ml - mL + n\lambda - nL}$ will be the true transverse diameter of the orbit which the comet describes; and from the transverse diameter given the periodic time of the comet is also given. Q.E.I. But the periodic times of the revolutions of comets, and the transverse diameters of their orbits, cannot be accurately enough determined but by comparing comets together which appear at different times. If, after equal intervals of time, several comets are found to have described the same orbit, we may thence conclude that they are all but one and the same comet revolved in the same orbit; and then from the times of their revolutions the transverse diameters of their orbits will be given, and from those diameters the elliptic orbits themselves will be determined.

To this purpose the orbits of many comets ought to be computed, supposing those orbits to be parabolic; for such orbits will always nearly agree with the phenomena, as appears not only from the parabolic orbit of the comet of the year 1680, which I compared above with the observations, but likewise from that of the notable comet which appeared in the year 1664 and 1665, and was observed by *Hewelcke,* who, from his own observations, calculated the longitudes and latitudes thereof, though with little accuracy. But from the same observations Dr. *Halley* did again compute its places; and from those new places determined its orbit, finding its ascending node in ♊ 21° 13′ 55″; the inclination of the orbit to the plane of the ecliptic 21° 18′ 40″; the distance of its perihelion from the node, estimated in the comet's orbit, 49° 27′ 30″, its perihelion in ♌ 8° 40′ 30″, with heliocentric latitude south 16° 01′ 45″; the comet to have been in its perihelion *November* 24^{d}. 11^{h}. 52^{m}. P.M. equal time at *London,* or 13^{h}. 8^{m}. at *Dantzick,* O.S.; and that the latus rectum of the parabola was 410286 of such parts as the sun's mean distance from the earth is supposed to contain 100000. And how nearly the places of the comet computed in this orbit agree with the observations, will appear from the table calculated by Dr. *Halley* (p. 535).

In *February,* the beginning of the year 1665, the first star of Aries, which I shall hereafter call γ, was in ♈ 28° 30′ 15″, with 7° 8′ 58″ north lat.; the second star of Aries was in ♈ 29° 17′ 18″, with 8° 28′ 16″ north lat.; another star of the seventh magnitude, which I call A, was in ♈ 28° 24′ 45″, with 8° 28′ 33″ north lat. The comet *Feb.* 7^{d}. 7^{h}. 30^{m}. at *Paris* (that is, *Feb.* 7^{d}. 8^{h}. 37^{m}. at *Dantzick*), O.S., made a triangle with those stars γ and A, which was right-angled in γ; and the distance of the comet from the star γ was equal to the distance of the stars γ and A, that is, 1° 19′ 46″ of a great circle; and therefore in the parallel of the latitude of the star γ it was 1° 20′ 26″. Therefore if from the longitude of the star γ there be subtracted the longitude 1° 20′

Apparent time at Dantzick	The observed distances of the comet from		The observed places	The places computed in the orbit
December d h m		° ′ ″	° ′ ″	° ′ ″
3.18.29½	The Lion's heart The Virgin's spike	46.24.20 22.52.10	Long. ♎ 7.01.00 Lat. S. 21.39. 0	♎ 7. 1.29 21.38.50
4.18. 1½	The Lion's heart The Virgin's spike	46. 2.45 23.52.40	Long. ♎ 6.15. 0 Lat. S. 22.24. 0	♎ 6.16. 5 22.24. 0
7.17.48	The Lion's heart The Virgin's spike	44.48. 0 27.56.40	Long. ♎ 3. 6. 0 Lat. S. 25.22. 0	♎ 3. 7.33 25.21.40
17.14.43	The Lion's heart Orion's right shoulder	53.15.15 45.43.30	Long. ♌ 2.56. 0 Lat. S. 49.25. 0	♌ 2.56. 0 49.25. 0
19. 9.25	Procyon Bright star of Whale's jaw	35.13.50 52.56. 0	Long. ♊28.40.30 Lat. S. 45.48. 0	♊ 28.43. 0 45.46. 0
20. 9.53½	Procyon Bright star of Whale's jaw	40.49. 0 40.04. 0	Long. ♊13.03. 0 Lat. S. 39.54. 0	♊ 13. 5. 0 39.53. 0
21. 9. 9½	Orion's right shoulder Bright star of Whale's jaw	26.21.25 29.28. 0	Long. ♊ 2.16. 0 Lat. S. 33.41. 0	♊ 2.18.30 33.39.40
22. 9. 0	Orion's right shoulder Bright star of Whale's jaw	29.47. 0 20.29.30	Long. ♉24.24. 0 Lat. S. 27.45. 0	♉ 24.27. 0 27.46. 0
26. 7.58	The bright star of Aries Aldebaran	23.20. 0 26.44. 0	Long. ♉ 9. 0.0 Lat. S. 12.36.0	♉ 9. 2.28 12.34.13
27. 6.45	The bright star of Aries Aldebaran	20.45. 0 28.10. 0	Long. ♉ 7. 5.40 Lat. S. 10.23. 0	♉ 7. 8.45 10.23.13
28. 7.39	The bright star of Aries Palilicium	18.29. 0 29.37. 0	Long. ♉ 5.24.45 Lat. S. 8.22.50	♉ 5.27.52 8.23.37
31. 6.45	Andromeda's girdle Palilicium	30.48.10 32.53.30	Long. ♉ 2. 7.40 Lat. S. 4.13. 0	♉ 2. 8.20 4.16.25
Jan. 1665 7. 7.37½	Andromeda's girdle Palilicium	25.11. 0 37.12.25	Long. ♈28.24.47 Lat. N. 0.54. 0	♈ 28.24. 0 0.53. 0
13. 7. 0	Andromeda's head Palilicium	28. 7.10 38.55.20	Long. ♈27. 6.54 Lat. N. 3. 6.50	♈ 27. 6.39 3. 7.40
24. 7.29	Andromeda's girdle Palilicium	20.32.15 40. 5. 0	Long. ♈26.29.15 Lat. N. 5.25.50	♈ 26.28.50 5.26. 0
February 7. 8.37			Long. ♈27. 4.46 Lat. N. 7. 3.29	♈ 27.24.55 7. 3.15
22. 8.46			Long. ♈28.29.46 Lat. N. 8.12.36	♈ 28.29.58 8.10.25
March 1. 8.16			Long. ♈29.18.15 Lat. N. 8.36.26	♈ 29.18.20 8.36.12
7. 8.37			Long. ♉ 0. 2.48 Lat. N. 8.56.30	♉ 0. 2.42 8.56.56

26″, there will remain the longitude of the comet ♈ 27° 9′ 49″. M. *Auzout,* from this observation of his, placed the comet in ♈ 27° 0′, nearly; and, by the drawing in which Dr. *Hooke* delineated its motion, it was then in ♈ 26° 59′ 24″. I place it in ♈ 27° 4′ 46″, taking the middle between the two extremes.

From the same observations, M. *Auzout* made the latitude of the comet at that time 7° and 4′ or 5′ to the north; but he had done better to have made it 7° 3′ 29″, the difference of the latitudes of the comet and the star γ being equal to the difference of the longitude of the stars γ and A.

February 22^d^. 7^h^. 30^m^. at *London,* that is, *February* 22^d^. 8^h^. 46^m^. at *Dantzick,* the distance of the comet from the star A, according to Dr. *Hooke's* observation, as was delineated by himself in a scheme, and also by the observations of M. *Auzout,* delineated in like manner by M. *Petit,* was a fifth part of the distance between the star A and the first star of Aries, or 15′ 57″; and the distance of the comet from a right line joining the star A and the first of Aries was a fourth part of the same fifth part, that is, 4′; and therefore the comet was in ♈ 28° 29′ 46″, with 8° 12′ 36″ north lat.

March 1, 7^h^. 0^m^. at *London,* that is, *March* 1, 8^h^. 16^m^. at *Dantzick,* the comet was observed near the second star in Aries, the distance between them being to the distance between the first and second stars in Aries, that is, to 1° 33′, as 4 to 45 according to Dr. *Hooke,* or as 2 to 23 according to M. *Gottignies.* And, therefore, the distance of the comet from the second star in Aries was 8′ 16″ according to Dr. *Hooke,* or 8′ 5″ according to M. *Gottignies;* or, taking a mean between both, 8′ 10″. But, according to M. *Gottignies,* the comet had gone beyond the second star of Aries about a fourth or a fifth part of the space that it commonly went over in a day, to wit, about 1′ 35″ (in which he agrees very well with M. *Auzout*); or, according to Dr. *Hooke,* not quite so much, as perhaps only 1′. Therefore if to the longitude of the first star in Aries we add 1′, and 8′ 10″ to its latitude, we shall have the longitude of the comet ♈ 29° 18′, with 8° 36′ 26″ north lat.

March 7, 7^h^. 30^m^. at *Paris,* that is, *March* 7, 7^h^. 37^m^. at *Dantzick,* from the observations of M. *Auzout,* the distance of the comet from the second star in Aries was equal to the distance of that star from the star A, that is, 52′ 29″; and the difference of the longitude of the comet and the second star in Aries was 45′ or 46′, or, taking a mean quantity, 45′ 30″; and therefore the comet was in ♉ 0° 2′ 48″. From the drawing constructed by M. *Petit,* based on the observations of M. *Auzout, Hewelcke* determined the latitude of the comet 8° 54′. But the engraver did not rightly trace the curvature of the comet's way towards the end of the motion; and *Hevelius,* in the drawing of M. *Auzout's* observations which he constructed himself, corrected this irregular curvature, and so made the latitude of the comet 8° 55′ 30″. And, by further correcting this irregularity, the latitude may become 8° 56′, or 8° 57′.

This comet was also seen *March* 9, and at that time its place must have been in ♉ 0° 18′, with 9° 3½′ north lat., nearly.

This comet appeared for three months, in which space of time it traveled over almost six signs, and in one of the days described almost 20 degrees. Its course did very much deviate from a great circle, bending towards the north, and its motion towards the end from retrograde became direct; and, notwithstanding that its course was so uncommon, yet by the table it appears that the theory, from beginning to end, agrees with the observations no less accurately than the theories of the planets usually do with the observations of them; but we are to subtract about 2′ when the comet was swiftest, which we may effect by taking off 12″ from the angle between the ascending node and the perihelion, or by making that angle 49° 27′ 18″. The annual parallax of both these comets (this and the preceding) was very conspicuous, and by its quantity demonstrates the annual motion of the earth in the earth's orbit.

This theory is likewise confirmed by the motion of that comet, which in the year 1683 appeared retrograde, in an orbit whose plane contained almost a right angle with the plane of the ecliptic, and whose ascending node (by the computation of Dr. *Halley*) was in ♍ 23° 23′; the inclination of its orbit to the ecliptic 83° 11′; its perihelion in ♊ 25° 29′ 30″; its perihelian distance from the sun 56020 of such parts as the radius of the earth's orbit contains 100000; and the time of its perihelion was *July* $2^d. 3^h. 50^m$. And the places thereof, computed by Dr. *Halley* in this orbit, are compared with the places observed by Mr. *Flamsteed,* in the following table (p. 538).

This theory is yet further confirmed by the motion of that retrograde comet which appeared in the year 1682. The ascending node of this (by Dr. *Halley's* computation) was in ♉ 21° 16′ 30″; the inclination of its orbit

1683 Equatorial time	Sun's place	Comet's longitude computed	Latitude north computed	Comet's longitude observed	Latitude north observed	Difference longitude	Difference latitude
d h m	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	′ ″	′ ″
July 13.12.55	♌ 1.02.30	♋13.05.42	29.28.13	♋13. 6.42	29.28.20	+1.00	+0.07
15.11.15	2.53.12	11.37.48	29.34. 0	11.39.43	29.34.50	+1.55	+0.50
17.10.20	4.45.45	10. 7. 6	29.33.30	10. 8.40	29.34. 0	+1.34	+0.30
23.13.40	10.38.21	5.10.27	28.51.42	5.11.30	28.50.28	+1.03	−1.14
25.14. 5	12.35.28	3.27.53	24.24.47	3.27. 0	28.23.40	−0.53	−1. 7
31. 9.42	18.09.22	♊27.55. 3	26.22.52	♊27.54.24	26.22.25	−0.39	−0.27
31.14.55	18.21.53	27.41. 7	26.16.57	27.41. 8	26.14.50	+0. 1	−2. 7
Aug. 2.14.56	20.17.16	25.29.32	25.16.19	25.28.46	25.17.28	−0.46	+1. 9
4.10.49	22.02.50	23.18.20	24.10.49	23.16.55	24.12.19	−1.25	+1.30
6.10. 9	23.56.45	20.42.23	22.47. 5	20.40.32	22.49. 5	−1.51	+2. 0
9.10.26	26.50.52	16. 7.57	20. 6.37	16. 5.55	20. 6.10	−2. 2	−0.27
15.14. 1	♍ 2.47.13	3.30.48	11.37.33	3.26.18	11.32. 1	−4.30	−5.32
16.15.10	3.48. 2	0.43. 7	9.34.16	0.41.55	9.34.13	−1.12	−0. 3
18.15.44	5.45.33	♉24.52.53	5.11.15	♉24.49. 5	5. 9.11	−3.48	−2. 4
			South		South		
22.14.44	9.35.49	11. 7.14	5.16.58	11.07.12	5.16.58	−0. 2	−0. 3
23.15.52	10.36.48	7. 2.18	8.17. 9	7. 1.17	8.16.41	−1. 1	−0.28
26.16. 2	13.31.10	♈24.45.31	16.38. 0	♈24.44.00	16.38.20	−1.31	+0.20

to the plane of the ecliptic 17° 56′ 00″; its perihelion in ♒ 2° 52′ 50″; its perihelian distance from the sun 58328 parts, of which the radius of the earth's orbit contains 100000; the equal time of the comet's being in its perihelion *September* 4ᵈ. 7ʰ. 39ᵐ. And its places determined from Mr. *Flamsteed's* observations, are compared with its places computed from our theory in the following table:

1682 App. time	Sun's place	Comet's longitude computed	Latitude north computed	Comet's longitude observed	Latitude north observed	Difference longitude	Difference latitude
d h m	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	′ ″	′ ″
Aug. 19.16.38	♍ 7. 0. 7	♌18.14.28	25.50. 7	♌18.14.40	25.49.55	−0.12	+0.12
20.15.38	7.55.52	24.46.23	26.14.42	24.46.22	26.12.52	+0. 1	+1.50
21. 8.21	8.36.14	29.37.15	26.20. 3	29.38.02	26.17.37	−0.47	+2.26
22. 8. 8	9.33.55	♍ 6.29.53	26. 8.42	♍ 6.30. 3	26. 7.12	−0.10	+1.30
29.08.20	16.22.40	♎12.37.54	18.37.47	♎12.37.49	18.34. 5	+0. 5	+3.42
30. 7.45	17.19.41	15.36. 1	17.26.43	15.35.18	17.27.17	+0.43	−0.34
Sept. 1. 7.33	19.16. 9	20.30.53	15.13. 0	20.27. 4	15. 9.49	+3.49	+3.11
4. 7.22	22.11.28	25.42. 0	12.23.48	25.40.58	12.22. 0	+1. 2	+1.48
5. 7.32	23.10.29	27. 0.46	11.33.08	26.59.24	11.33.51	+1.22	−0.43
8. 7.16	26. 5.58	29.58.44	9.26.46	29.58.45	9.26.43	−0. 1	+0. 3
9. 7.26	27. 5. 9	♏ 0.44.10	8.49.10	♏ 0.44. 4	8.48.25	+0. 6	+0.45

This theory is also confirmed by the retrograde motion of the comet that appeared in the year 1723. The ascending node of this comet (according to the computation of Mr. *Bradley*, Savilian Professor of Astronomy at *Oxford*) was in ♈ 14° 16′, the inclination of the orbit to the plane of the ecliptic 49° 59′. Its perihelion was in ♉ 12° 15′ 20″, its perihelian distance from the sun 998651 parts, of which the radius of the earth's orbit contains 1000000, and the equal time of its perihelion *September* $16^{d}.\ 16^{h}.\ 10^{m}$. The places of this comet computed in this orbit by Mr. *Bradley*, and compared with the places observed by himself, his uncle Mr. *Pound*, and Dr. *Halley*, may be seen in the following table.

1723 Equatorial time	Comet's longitude observed	Latitude north observed	Comet's longitude computed	Latitude north computed	Difference longitude	Difference latitude
d h m	° ′ ″	° ′ ″	° ′ ″	° ′ ″	″	″
Oct. 9.8. 5	♒7.22.15	5. 2. 0	♒7.21.26	5. 2.47	+49	−47
10.6.21	6.41.12	7.44.13	6.41.42	7.43.18	−50	+55
12.7.22	5.39.58	11.55. 0	5.40.19	11.54.55	−21	+ 5
14.8.57	4.59.49	14.43.50	5. 0.37	14.44. 1	−48	−11
15.6.35	4.47.41	15.40.51	4.47.45	15.40.55	− 4	− 4
21.6.22	4. 2.32	19.41.49	4. 2.21	19.42. 3	+11	−14
22.6.24	3.59. 2	20. 8.12	3.59.10	20. 8.17	− 8	− 5
24.8. 2	3.55.29	20.55.18	3.55.11	20.55. 9	+18	+ 9
29.8.56	3.56.17	22.20.27	3.56.42	22.20.10	−25	+17
30.6.20	3.58. 9	22.32.28	3.58.17	22.32.12	− 8	+16
Nov. 5.5.53	4.16.30	23.38.33	4.16.23	23.38. 7	+ 7	+26
8.7. 6	4.29.36	24. 4.30	4.29.54	24. 4.40	−18	−10
14.6.20	5. 2.16	24.48.46	5. 2.51	24.48.16	−35	+30
20.7.45	5.42.20	25.24.45	5.43.13	25.25.17	−53	−32
Dec. 7.6.45	8. 4.13	26.54.18	8. 3.55	26.53.42	+18	+36

From these examples it is abundantly evident that the motions of comets are no less accurately represented by our theory than the motions of the planets commonly are by the theories of them; and, therefore, by means of this theory, we may enumerate the orbits of comets, and so discover the periodic time of a comet's revolution in any orbit; hence, at last, we shall have the transverse diameters of their elliptic orbits and their aphelian distances.

That retrograde comet which appeared in the year 1607 described an orbit whose ascending node (according to Dr. *Halley's* computation) was in ♉ 20° 21′; and the inclination of the plane of the orbit to the plane of the ecliptic 17° 2′; whose perihelion was in ♒ 2° 16′; and its perihelian distance from the sun 58680 of such parts as the radius of the earth's orbit contains 100000; and the comet was in its perihelion *October* 16^{d}. 3^{h}. 50^{m}.; which orbit agrees very nearly with the orbit of the comet which was seen in 1682. If these were not two different comets, but one and the same, that comet will finish one revolution in the space of 75 years; and the greater axis of its orbit will be to the greater axis of the earth's orbit as $\sqrt[3]{75^2}$ to 1, or as 1778 to 100, nearly. And the aphelian distance of this comet from the sun will be to the mean distance of the earth from the sun as about 35 to 1; from these data it will be no hard matter to determine the elliptic orbit of this comet. But these things are to be supposed on condition, that, after the space of 75 years, the same comet shall return again in the same orbit. The other comets seem to ascend to greater heights, and to require a longer time to perform their revolutions.

But, because of the great number of comets, of the great distance of their aphelions from the sun, and of the slowness of their motions in the aphelions, they will, by their mutual gravitations, disturb each other; so that their eccentricities and the times of their revolutions will be sometimes a little increased, and sometimes diminished. Therefore, we are not to expect that the same comet will return exactly in the same orbit, and in the same periodic times: it will be sufficient if we find the changes no greater than may arise from the causes just spoken of.

And hence a reason may be assigned why comets are not comprehended within the limits of a zodiac, as the planets are; but, being confined to no bounds, are with various motions dispersed all over the heavens; namely, to this purpose, that in their aphelions, where their motions are exceedingly slow, receding to greater distances one from another, they may suffer less disturbance from their mutual gravitations: and hence it is that the comets which descend the lowest, and therefore move the slowest in their aphelions, ought also to ascend the highest.

The comet which appeared in the year 1680 was in its perihelion less distant from the sun than by a sixth part of the sun's diameter; and because of its extreme velocity in that proximity to the sun, and some density of the sun's atmosphere, it must have suffered some resistance and retardation; and therefore, being attracted somewhat nearer to the sun in every revolution, will at last fall down upon the body of the sun. Nay, in its aphelion, where it moves the slowest, it may sometimes happen to be yet further retarded by the attractions of other comets, and in consequence of this retardation descend to the sun. So fixed stars, that have been gradually wasted by the light and vapors emitted from them for a long time, may be recruited by comets that fall upon them; and from this fresh supply of new fuel those old stars, acquiring new splendor, may pass for new stars. Of this kind are such fixed stars as appear on a sudden, and shine with a wonderful brightness at first, and afterwards vanish by little and little. Such was that star which appeared in Cassiopeia's Chair; which *Cornelis Gemma* did not see upon the 8th of *November,* 1572, though he was observing that part of the heavens upon that very night, and the sky was perfectly serene; but the next night (*November* 9) he saw it shining much brighter than any of the fixed stars, and scarcely inferior to Venus in splendor. *Tycho Brahe* saw it upon the 11th of the same month, when it shone with the greatest lustre; and from that time he observed it to decay by little and little; and in 16 months' time it entirely disappeared. In the month of *November,* when it first appeared, its light was equal to that of Venus. In the month of *December,* its light was a little diminished, and was now become equal to that of Jupiter. In *January,* 1573, it was less than Jupiter, and greater than Sirius, and about the end of *February* and the beginning of *March* became equal to that star. In the months of *April* and *May* it was equal to a star of the second magnitude; in *June, July,* and *August,* to a star of the third magnitude; in *September, October,* and *November,* to those of the fourth magnitude; in *December* and *January,* 1574, to those of the fifth; in *February* to those of the sixth magnitude; and in *March* it entirely vanished. Its color at the beginning was clear, bright, and inclining to white; afterwards it turned a little yellow; and in *March,* 1573, it became ruddy, like Mars or Aldebaran; in *May* it turned to a kind of dusky whiteness, like that we observe in Saturn; and that color it retained ever after, but growing always more and more obscure. Such also was the star in the right foot of Serpentarius, which

Kepler's scholars first observed *September* 30, o.s., 1604, with a light exceeding that of Jupiter, though the night before it was not to be seen; and from that time it decreased by little and little, and in 15 or 16 months entirely disappeared. Such a new star appearing with an unusual splendor is said to have moved *Hipparchus* to observe, and make a catalogue of, the fixed stars. As to those fixed stars that appear and disappear by turns, and increase slowly and by degrees, and scarcely ever exceed the stars of the third magnitude, they seem to be of another kind, which revolve about their axes, and, having a light and a dark side, show those two different sides by turns. The vapors which arise from the sun, the fixed stars, and the tails of the comets, may meet at last with, and fall into, the atmospheres of the planets by their gravity, and there be condensed and turned into water and humid spirits; and from thence, by a slow heat, pass gradually into the form of salts, and sulphurs, and tinctures, and mud, and clay, and sand, and stones, and coral, and other terrestrial substances.

GENERAL SCHOLIUM

The hypothesis of vortices is pressed with many difficulties. That every planet by a radius drawn to the sun may describe areas proportional to the times of description, the periodic times of the several parts of the vortices should observe the square of their distances from the sun; but that the periodic times of the planets may obtain the $\frac{3}{2}$th power of their distances from the sun, the perodic times of the parts of the vortex ought to be as the $\frac{3}{2}$th power of their distances. That the smaller vortices may maintain their lesser revolutions about Saturn, Jupiter, and other planets, and swim quietly and undisturbed in the greater vortex of the sun, the periodic times of the parts of the sun's vortex should be equal; but the rotation of the sun and planets about their axes, which ought to correspond with the motions of their vortices, recede far from all these proportions. The motions of the comets are exceedingly regular, are governed by the same laws with the motions of the planets, and can by no means be accounted for by the hypothesis of vortices; for comets are carried with very eccentric motions through all parts of the heavens indifferently, with a freedom that is incompatible with the notion of a vortex.

Bodies projected in our air suffer no resistance but from the air. Withdraw the air, as is done in Mr. *Boyle's* vacuum, and the resistance ceases; for in this void a bit of fine down and a piece of solid gold descend with equal velocity. And the same argument must apply to the celestial spaces above the earth's atmosphere; in these spaces, where there is no air to resist their motions, all bodies will move with the greatest freedom; and the planets and comets will constantly pursue their revolutions in orbits given in kind and position, according to the laws above explained; but though these bodies may, indeed, continue in their orbits by the mere laws of gravity, yet they could by no means have at first derived the regular position of the orbits themselves from those laws.

The six primary planets are revolved about the sun in circles concentric with the sun, and with motions directed towards the same parts, and almost in the same plane. Ten moons are revolved about the earth, Jupiter, and Saturn, in circles concentric with them, with the same direction of motion, and nearly in the planes of the orbits of those planets; but it is not to be conceived that mere mechanical causes could give birth to so many regular motions, since the comets range over all parts of the heavens in very eccentric orbits; for by that kind of motion they pass easily through the orbs of the planets, and with great rapidity; and in their aphelions, where they move the slowest, and are detained the longest, they recede to the greatest distances from each other, and hence suffer the least disturbance from their mutual attractions. This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being. And if the fixed stars are the centres of other like systems, these, being formed by the like wise counsel, must be all subject to the dominion of One; especially since the light of the fixed stars is of the same nature with the light of the sun, and from every system light passes into all the other systems: and lest the systems of the fixed stars should, by their gravity, fall on each other, he hath placed those systems at immense distances from one another.

This Being[1] governs all things, not as the soul of the world, but as Lord over all; and on account of his dominion he is wont to be called *Lord God* παντοκράτωρ, or *Universal Ruler;* for *God* is a relative word, and has a respect to servants; and *Deity* is the dominion of God not over his own body, as those imagine who fancy God to be the soul of the world, but over servants. The Supreme God is a Being eternal, infinite, absolutely perfect; but a being, however perfect, without dominion, cannot be said to be Lord God; for we say, my God, your God, the God of *Israel,* the God of Gods, and Lord of Lords; but we do not say, my Eternal, your Eternal, the Eternal of *Israel,* the Eternal of Gods; we do not say, my Infinite, or my Perfect: these are titles which have no respect to servants. The word God* usually signifies *Lord;* but every lord is not a God. It is the dominion of a spiritual being which constitutes a God: a true, supreme, or imaginary dominion

[[1] Appendix, Note 52.]

* Dr. *Pocock* derives the Latin word *Deus* from the *Arabic du* (in the oblique case *di*), which signifies *Lord*. And in this sense princes are called *gods*, *Psal.* lxxxii. ver. 6; and *John* x. ver. 35. And *Moses* is called a *god* to his brother *Aaron*, and a *god* to *Pharaoh* (*Exod.* iv. ver. 16; and vii. ver. 1). And in the same sense the souls of dead princes were formerly, by the Heathens, called *gods*, but falsely, because of their want of dominion.

makes a true, supreme, or imaginary God. And from his true dominion it follows that the true God is a living, intelligent, and powerful Being; and, from his other perfections, that he is supreme, or most perfect. He is eternal and infinite, omnipotent and omniscient; that is, his duration reaches from eternity to eternity; his presence from infinity to infinity; he governs all things, and knows all things that are or can be done. He is not eternity and infinity, but eternal and infinite; he is not duration or space, but he endures and is present. He endures forever, and is everywhere present; and, by existing always and everywhere, he constitutes duration and space. Since every particle of space is *always,* and every indivisible moment of duration is *everywhere,* certainly the Maker and Lord of all things cannot be *never* and *nowhere.* Every soul that has perception is, though in different times and in different organs of sense and motion, still the same indivisible person. There are given successive parts in duration, coexistent parts in space, but neither the one nor the other in the person of a man, or his thinking principle; and much less can they be found in the thinking substance of God. Every man, so far as he is a thing that has perception, is one and the same man during his whole life, in all and each of his organs of sense. God is the same God, always and everywhere. He is omnipresent not *virtually* only, but also *substantially;* for virtue cannot subsist without substance. In him* are all things contained and moved; yet neither affects the other: God suffers nothing from the motion of bodies; bodies find no resistance from the omnipresence of God. It is allowed by all that the Supreme God exists necessarily; and by the same necessity he exists *always* and *everywhere.* Whence also he is all similar, all eye, all ear, all brain, all arm, all power to perceive, to understand, and to act; but in a manner not at all human, in a manner not at all corporeal, in a manner utterly unknown to us. As a blind man has no idea of colors, so have we no idea of the manner by which the all-wise God perceives and understands all things. He is utterly void of all body and bodily figure, and can therefore neither be seen, nor heard, nor touched; nor ought he to be worshiped under the representation of any

* This was the opinion of the Ancients. So *Pythagoras,* in *Cicer. de Nat. Deor.* lib. i. *Thales, Anaxagoras, Virgil,* Georg. lib. iv. ver. 220; and Aeneid, lib. vi. ver. 721. *Philo Allegor,* at the beginning of lib. i. *Aratus,* in his Phaenom. at the beginning. So also the sacred writers: as St. *Paul, Acts* xvii. ver. 27, 28. St. *John's* Gosp. chap. xiv. ver. 2. *Moses,* in *Deut.* iv. ver. 39; and x. ver. 14. *David, Psal.* cxxxix, ver. 7, 8, 9. *Solomon,* 1 *Kings* viii ver. 27. *Job,* xxii. ver. 12, 13, 14. *Jeremiah,* xxiii. ver. 23, 24. The Idolaters supposed the sun, moon, and stars, the souls of men, and other parts of the world, to be parts of the Supreme God, and therefore to be worshiped; but erroneously.

corporeal thing. We have ideas of his attributes, but what the real substance of anything is we know not. In bodies, we see only their figures and colors, we hear only the sounds, we touch only their outward surfaces, we smell only the smells, and taste the savors; but their inward substances are not to be known either by our senses, or by any reflex act of our minds: much less, then, have we any idea of the substance of God. We know him only by his most wise and excellent contrivances of things, and final causes;[1] we admire him for his perfections; but we reverence and adore him on account of his dominion: for we adore him as his servants; and a god without dominion, providence, and final causes, is nothing else but Fate and Nature. Blind metaphysical necessity, which is certainly the same always and everywhere, could produce no variety of things. All that diversity of natural things which we find suited to different times and places could arise from nothing but the ideas and will of a Being necessarily existing. But, by way of allegory, God is said to see, to speak, to laugh, to love, to hate, to desire, to give, to receive, to rejoice, to be angry, to fight, to frame, to work, to build; for all our notions of God are taken from the ways of mankind by a certain similitude, which, though not perfect, has some likeness, however. And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy.

Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always as the inverse square of the distances.[2] Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately as the inverse square of the distances as far as the orbit of Saturn, as evidently appears from the quiescence of the aphelion of the planets; nay, and even to the remotest aphelion of the comets, if those aphelions are also quiescent.

[1 Appendix, Note 53.] [2 Appendix, Note 54.]

But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses;[1] for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.[2]

And now we might add something concerning a certain most subtle spirit which pervades and lies hid in all gross bodies; by the force and action of which spirit the particles of bodies attract one another at near distances, and cohere, if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighboring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of this spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which this electric and elastic spirit operates.

[1 Appendix, Note 55.] [2 Appendix, Note 56.]

[END OF THE MATHEMATICAL PRINCIPLES.]

THE SYSTEM OF THE WORLD[1]

[1.] *The matter of the heavens is fluid.*

It was the ancient opinion of not a few, in the earliest ages of philosophy, that the fixed stars stood immovable in the highest parts of the world; that under the fixed stars the planets were carried about the sun; that the earth, as one of the planets, described an annual course about the sun, while by a diurnal motion it was in the meantime revolved about its own axis; and that the sun, as the common fire which served to warm the whole, was fixed in the centre of the universe.

This was the philosophy taught of old by *Philolaus, Aristarchus* of *Samos, Plato* in his riper years, and the whole sect of the *Pythagoreans;* and this was the judgment of *Anaximander,* more ancient still; and of that wise king of the *Romans, Numa Pompilius,* who, as a symbol of the figure of the world with the sun in the centre, erected a round temple in honor of *Vesta,* and ordained perpetual fire to be kept in the middle of it.

The *Egyptians* were early observers of the heavens; and from them, probably, this philosophy was spread abroad among other nations; for from them it was, and the nations about them, that the *Greeks,* a people more addicted to the study of philology than of Nature, derived their first, as well as soundest, notions of philosophy; and in the Vestal ceremonies we may yet trace the ancient spirit of the *Egyptians;* for it was their way to deliver their mysteries, that is, their philosophy of things above the common way of thinking, under the veil of religious rites and hieroglyphic symbols.

It is not to be denied that *Anaxagoras, Democritus,* and others, did now and then start up, who would have it that the earth possessed the centre of the world, and that the stars were revolved towards the west about the earth quiescent in the centre, some at a swifter, others at a slower rate.

However, it was agreed on both sides that the motions of the celestial bodies were performed in spaces altogether free and void of resistance. The whim of solid orbs was of a later date, introduced by *Eudoxus, Calippus,*

[[1] Appendix, Note 57.]

and *Aristotle;* when the ancient philosophy began to decline, and to give place to the new prevailing fictions of the *Greeks.*

But, above all things, the phenomena of comets can by no means tolerate the idea of solid orbits. The *Chaldeans,* the most learned astronomers of their time, looked upon the comets (which of ancient times before had been numbered among the celestial bodies) as a particular sort of planets, which, describing eccentric orbits, presented themselves to view only by turns, once in a revolution, when they descended into the lower parts of their orbits.

And as it was the unavoidable consequence of the hypothesis of solid orbits, while it prevailed, that the comets should be thrust into spaces below the moon; so, when later observations of astronomers restored the comets to their ancient places in the higher heavens, these celestial spaces were necessarily cleared of the incumbrance of solid orbits.

[2.] *The principle of circular motion in free spaces.*

After this time, we do not know in what manner the ancients explained the question, how the planets came to be retained within certain bounds in these free spaces, and to be drawn off from the rectilinear courses, which, left to themselves, they should have pursued, into regular revolutions in curvilinear orbits. Probably it was to give some sort of satisfaction to this difficulty that solid orbs had been introduced.

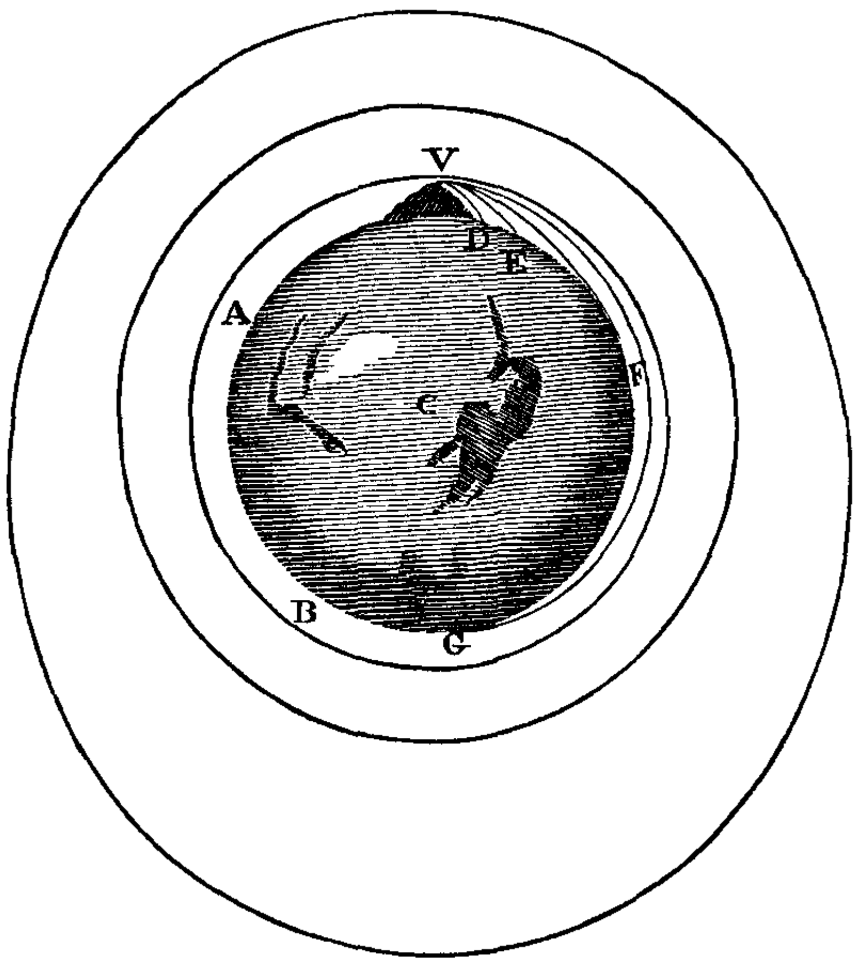
The later philosophers pretend to account for it either by the action of certain vortices, as *Kepler* and *Descartes;* or by some other principle of impulse or attraction, as *Borelli, Hooke,* and others of our nation; for, from the laws of motion, it is most certain that these effects must proceed from the action of some force or other.

But our purpose is only to trace out the quantity and properties of this force from the phenomena (p. 192), and to apply what we discover in some simple cases as principles, by which, in a mathematical way, we may estimate the effects thereof in more involved cases; for it would be endless and impossible to bring every particular to direct and immediate observation.

We said, *in a mathematical way,* to avoid all questions about the nature or quality of this force, which we would not be understood to determine by any hypothesis; and therefore call it by the general name of a centripetal force, as it is a force which is directed towards some centre; and as it regards more particularly a body in that centre, we call it circumsolar, circumterrestrial, circumjovial; and so in respect of other central bodies.

[3.] *The action of centripetal forces.*

That by means of centripetal forces the planets may be retained in certain orbits, we may easily understand, if we consider the motions of projectiles (pp. 2–4); for a stone that is projected is by the pressure of its own weight forced out of the rectilinear path, which by the initial projection alone it should have pursued, and made to describe a curved line in the air; and



through that crooked way is at last brought down to the ground; and the greater the velocity is with which it is projected, the farther it goes before it falls to the earth. We may therefore suppose the velocity to be so increased, that it would describe an arc of 1, 2, 5, 10, 100, 1000 miles before it arrived at the earth, till at last, exceeding the limits of the earth, it should pass into space without touching it.

Let AFB represent the surface of the earth, C its centre, VD, VE, VF the curved lines which a body would describe, if projected in an horizontal direction from the top of an high mountain successively with more and more velocity (p. 418); and, because the celestial motions are scarcely retarded by the little or no resistance of the spaces in which they are performed, to keep up the parity of cases, let us suppose either that there is no air about the earth, or at least that it is endowed with little or no power of resisting; and for the same reason that the body projected with a less velocity describes the lesser arc VD, and with a greater velocity the greater arc VE, and, augmenting the velocity, it goes farther and farther to F and G, if the velocity was still more and more augmented, it would reach at last quite beyond the circumference of the earth, and return to the mountain from which it was projected.

And since the areas which by this motion it describes by a radius drawn to the centre of the earth are (by Prop. 1, Book 1, *Princip. Math.*) proportional to the times in which they are described, its velocity, when it returns to the mountain, will be no less than it was at first; and, retaining the same velocity, it will describe the same curve over and over, by the same law.

But if we now imagine bodies to be projected in the directions of lines parallel to the horizon from greater heights, as of 5, 10, 100, 1000, or more miles, or rather as many semidiameters of the earth, those bodies, according to their different velocity, and the different force of gravity in different heights, will describe arcs either concentric with the earth, or variously eccentric, and go on revolving through the heavens in those orbits just as the planets do in their orbits.

[4.] *The certainty of the proof.*

As when a stone is projected obliquely, that is, any way but in the perpendicular direction, the continual deflection thereof towards the earth from the right line in which it was projected is a proof of its gravitation to the earth, no less certain than its direct descent when suffered to fall freely from rest; so the deviation of bodies moving in free spaces from rectilinear paths, and continual deflection therefrom towards any place, is a sure indication of the existence of some force which from all quarters impels those bodies towards that place.

And as, from the supposed existence of gravity, it necessarily follows that all bodies about the earth must press downwards, and therefore must either descend directly to the earth, if they are let fall from rest, or at least continually deviate from right lines towards the earth, if they are projected obliquely; so, from the supposed existence of a force directed to any centre, it will follow, by the like necessity, that all bodies upon which this force acts must either descend directly to that centre, or at least deviate continually towards it from right lines, if otherwise they should have moved obliquely in these right lines.

And how from the motions given we may infer the forces, or from the forces given we may determine the motions, is shown in the first two Books of our *Principles of Philosophy*.

If the earth is supposed to stand still, and the fixed stars to be revolved in free spaces in the space of 24 hours, it is certain the forces by which the fixed stars are held in their orbits are not directed to the earth, but to the centres of those orbits, that is, of the several parallel circles, which the fixed stars, declining to one side and the other from the equator, describe daily; also that by radii drawn to the centres of the orbits the fixed stars describe areas exactly proportional to the times of description. Then, because the periodic times are equal (by Cor. III, Prop. IV, Book I), it follows that the centripetal forces are as the radii of the several orbits, and that they will continually revolve in the same orbits. And the like consequences may be drawn from the supposed diurnal motion of the planets.

That forces should be directed to no body on which they physically depend, but to innumerable imaginary points in the axis of the earth, is an hypothesis too incongruous. It is more incongruous still that those forces should increase exactly in proportion of the distances from this axis; for this is an indication of an increase to immensity, or rather to infinity; whereas the forces of natural things commonly decrease in receding from the fountain from which they flow. But, what is yet more absurd, neither are the areas described by the same star proportional to the times, nor are its revolutions performed in the same orbit; for as the star recedes from the neighboring pole, both areas and orbits increase; and from the increase of the area it is demonstrated that the forces are not directed to the axis of the earth. And this difficulty (Cor. I, Prop. II, Book I) arises from the twofold motion that is observed in the fixed stars, one diurnal round the axis of the earth, the other exceedingly slow round the axis of the ecliptic. And the explication thereof requires a composition of forces so involved and so variable, that it is hardly to be reconciled with any physical theory.

[5.] *Centripetal forces are directed to the individual centres of the planets.*

That there are centripetal forces actually directed to the bodies of the sun, of the earth, and other planets, I thus infer.

The moon revolves about our earth, and by radii drawn to its centre (p. 406) describes areas nearly proportional to the times in which they are described, as is evident from its velocity compared with its apparent diameter; for its motion is slower when its diameter is less (and therefore its distance greater), and its motion is swifter when its diameter is greater.

The revolutions of the satellites of Jupiter about that planet are more regular (p. 401); for they describe circles concentric with Jupiter by uniform motions, as exactly as our senses can perceive.

And so the satellites of Saturn are revolved about this planet with motions nearly (p. 402) circular and uniform, scarcely disturbed by any eccentricity hitherto observed.

That Venus and Mercury are revolved about the sun, is demonstrable from their moon-like appearances (p. 403); when they shine with a full face, they are in those parts of their orbits which in respect of the earth lie beyond the sun; when they appear half full, they are in those parts which lie over against the sun; when horned, in those parts which lie between the earth and the sun; and sometimes they pass over the sun's disk, when directly interposed between the earth and the sun.

And Venus, with a motion almost uniform, describes an orbit nearly circular and concentric with the sun.

But Mercury, with a more eccentric motion, makes remarkable approaches to the sun, and goes off again by turns; but it is always swifter as it is near to the sun, and therefore by a radius drawn to the sun still describes areas proportional to the times.

Lastly, that the earth describes about the sun, or the sun about the earth, by a radius from the one to the other, areas exactly proportional to the times, is demonstrable from the apparent diameter of the sun compared with its apparent motion.

These are astronomical experiments; from which it follows, by Prop. I, II, III, in the first Book of our *Principles,* and their Corollaries (pp. 40–45), that there are centripetal forces actually directed (either accurately or without considerable error) to the centres of the earth, of Jupiter, of Saturn, and of the sun. In Mercury, Venus, Mars, and the lesser planets, where experiments are wanting, the arguments from analogy must be allowed in their place.

[6.] *Centripetal forces decrease inversely as the square of the distances from the centres of the planets.*

That those forces (pp. 40–45) decrease as the inverse square of the distances from the centre of every planet, appears by Cor. VI, Prop. IV, Book I; for the periodic times of the satellites of Jupiter are one to another (pp. 401–402), as the $\frac{3}{2}$th power of their distances from the center of this planet.

This proportion has been long ago observed in those satellites; and Mr. *Flamsteed,* who had often measured their distances from Jupiter by the micrometer, and by the eclipses of the satellites, wrote to me, that it holds to all the accuracy that possibly can be discerned by our senses. And he sent me the dimensions of their orbits taken by the micrometer, and reduced to the mean distance of Jupiter from the earth, or from the sun, together with the times of their revolutions, as follows:

The greatest elongation of the satellites from the centre of Jupiter as seen from the sun					The periodic times of their revolutions			
	′	″		″	d	h	m	s
1st	1	48	or	108	1	18	28	36
2d	3	01	or	181	3	13	17	54
3d	4	46	or	286	7	03	59	36
4th	8	13½	or	493½	16	18	5	13

Hence the $\frac{3}{2}$th power of the distances may be easily seen. For example: the $16^d.\ 18^h.\ 05^m.\ 13^s.$ is to the time $1^d.\ 18^h.\ 28^m.\ 36^s.$ as $493\frac{1}{2}'' \cdot \sqrt{493\frac{1}{2}''}$ to $108'' \cdot \sqrt{108''}$, neglecting those small fractions which, in observing, cannot be certainly determined.

Before the invention of the micrometer, the same distances were determined in semidiameters of Jupiter thus:

Distance of the	1st	2d	3d	4th
By Galileo	6	10	16	28
By Simon Marius	6	10	16	26
By Cassini	5	8	13	23
By Borelli, more exactly	$5^2/_3$	$8^2/_3$	14	$24^2/_3$

After the invention of the micrometer:

Distance of the	1st	2d	3d	4th
By Townley	5.51	8.78	13.47	24.72
By Flamsteed	5.31	8.85	13.98	24.23
More accurately by the eclipses	5.578	8.876	14.159	24.903

And the periodic times of those satellites, by the observations of Mr. *Flamsteed,* are $1^d. 18^h. 28^m. 36^s.$ | $3^d. 13^h. 17^m. 54^s.$ | $7^d. 3^h. 59^m. 36^s.$ | $16^d. 18^h. 5^m. 13^s.$ as above.

And the distances thence computed are 5.578 | 8.878 | 14.168 | 24.968, accurately agreeing with the distances by observation.

Cassini assures us (pp. 403–404) that the same proportion is observed in the circumsaturnal planets. But a longer course of observations is required before we can have a certain and accurate theory of those planets.

In the circumsolar planets, Mercury and Venus, the same proportion holds with great accuracy, according to the dimensions of their orbits, as determined by the observations of the best astronomers.

[7.] *The superior planets revolve about the sun, and the radii drawn to the sun describe areas proportional to the times.*

That Mars is revolved about the sun is demonstrated from the phases which it shows, and the proportion of its apparent diameters (pp. 403–406); for from its appearing full near conjunction with the sun, and gibbous in its quadratures, it is certain that it surrounds the sun.

And since its diameter appears about five times greater when in opposition to the sun than when in conjunction therewith, and its distance from the earth is inversely as its apparent diameter, that distance will be about five times less when in opposition to than when in conjunction with the sun; but in both cases its distance from the sun will be nearly the same with the distance which is inferred from its gibbous appearance in the quadratures. And as it encompasses the sun at almost equal distances, but in respect of the earth is very unequally distant, so by radii drawn to the sun it describes areas nearly unifom; but by radii drawn to the earth, it is sometimes swift, sometimes stationary, and sometimes retrograde.

That Jupiter, in a higher orbit than Mars, is likewise revolved about the sun, with a motion nearly uniform, as well in distance as in the areas described, I infer thus.

Mr. *Flamsteed* assured me, by letters, that all the eclipses of the innermost satellite which hitherto have been well observed do agree with his theory so nearly, as never to differ therefrom by two minutes of time; that in the outermost the error is little greater; in the outermost but one, scarcely three times greater; that in the innermost but one the difference is indeed much greater, yet so as to agree as nearly with his computations as the moon does with the common tables; and that he computes those eclipses only from the mean motions corrected by the equation of light discovered and introduced by Mr. *Römer*. Supposing, then, that the theory differs by a less error than that of 2′ from the motion of the outmost satellite as hitherto described, and taking as the periodic time $16^{d}. 18^{h}. 5^{m}. 13^{s}.$ to 2′ in time, so is the whole circle of 360° to the arc 1′ 48″, the error of Mr. *Flamsteed's* computation, reduced to the satellite's orbit, will be less than 1′ 48″; that is, the longitude of the satellite, as seen from the centre of Jupiter, will be determined with a less error than 1′ 48″. But when the satellite is in the middle of the shadow, that longitude is the same with the heliocentric longitude of Jupiter; and, therefore, the hypothesis which Mr. *Flamsteed* follows, viz., the *Copernican,* as improved by *Kepler,* and (as to the motion of Jupiter) lately corrected by himself, rightly represents that longitude within a less error than 1′ 48″; but by this longitude, together with the geocentric longitude, which is always easily found, the distance of Jupiter from the sun is determined; which must, therefore, be the very same with that which the hypothesis exhibits. For that greatest error of 1′ 48″ that can happen in the heliocentric longitude is almost insensible, and quite to be neglected, and perhaps may arise from some yet undiscovered eccentricity of the satellite; but since both longitude and distance are rightly determined, it follows of necessity that Jupiter, by radii drawn to the sun, describes areas so conditioned as the hypothesis requires, that is, proportional to the times.

And the same thing may be concluded of Saturn from his satellite, by the observations of Mr. *Huygens* and Dr. *Halley;* though a longer series of observations is yet wanting to confirm the thing, and to bring it under a sufficiently exact computation.

[8.] *The force which controls the superior planets is not directed to the earth, but to the sun.*

For if Jupiter was viewed from the sun, it would never appear retrograde nor stationary, as it is seen sometimes from the earth, but always to go forwards with a motion nearly uniform (p. 405). And from the very great inequality of its apparent geocentric motion, we infer (by Book I, Prop. III, Cor. IV) that the force by which Jupiter is turned out of a rectilinear course, and made to revolve in an orbit, is not directed to the centre of the earth. The same argument holds good for Mars and for Saturn. Another centre of these forces is therefore to be looked for (by Book I, Prop. II and III, and the Corollaries of the latter), about which the areas described by radii intervening may be uniform; and that this is the sun, we have proved already in Mars and Saturn nearly, but accurately enough in Jupiter. It may be alleged that the sun and planets are impelled by some other force equally and in the direction of parallel lines; but by such a force (by Cor. VI of the Laws of Motion) no change would happen in the situation of the planets one to another, nor any sensible effect follow: but our business is with the causes of sensible effects. Let us, therefore, neglect every such force as imaginary and precarious, and of no use in the phenomena of the heavens; and the whole remaining force by which Jupiter is impelled will be directed (by Book I, Prop. III, Cor. I) to the centre of the sun.

[9.] *The circumsolar force decreases in all planetary spaces inversely as the square of the distances from the sun.*

The distances of the planets from the sun come out the same, whether, with *Tycho,* we place the earth in the centre of the system, or the sun with *Copernicus:* and we have already proved that these distances are true in Jupiter.

Kepler and *Boulliau* have, with great care (p. 404), determined the distances of the planets from the sun; and hence it is that their tables agree best with the heavens. And in all the planets, in Jupiter and Mars, in Saturn and the earth, as well as in Venus and Mercury, the cubes of their distances are as the squares of their periodic times; and therefore (by Cor. VI, Prop. IV, Book I) the centripetal circumsolar force throughout all the planetary regions decreases as the inverse square of the distances from the sun. In examining this proportion, we are to use the mean distances, or the transverse semiaxes of the orbits (by Prop. XV, Book I), and to neglect those little fractions, which, in defining the orbits, may have arisen from the insensible errors of observation, or may be ascribed to other causes which we shall afterwards explain. And thus we shall always find the said proportion to hold exactly; for the distances of Saturn, Jupiter, Mars, the earth, Venus, and Mercury, from the sun, obtained from the observations of astronomers, are, according to the computation of *Kepler,* as the numbers 951000, 519650, 152350, 100000, 72400, 38806; by the computation of *Boulliau,* as the numbers 954198, 522520, 152350, 100000, 72398, 38585; and from the periodic times they come out 953806, 520116, 152399, 100000, 72333, 38710. Their distances, according to *Kepler* and *Boulliau,* scarcely differ by any sensible quantity, and where they differ most the distances calculated from the periodic times fall in between them.

[10.] *The circumterrestrial force decreases inversely as the square of the distances from the earth. This is shown on the hypothesis that the earth is at rest.*

That the circumterrestrial force likewise decreases as the inverse square of the distances, I infer thus.

The mean distance of the moon from the centre of the earth, is, in semidiameters of the earth, according to *Ptolemy, Kepler* in his *Ephemerides,* *Boulliau, Hewelcke,* and *Riccioli,* 59; according to *Flamsteed,* 59⅓; according to *Tycho,* 56½; to *Vendelin,* 60; to *Copernicus,* 60⅓; to *Kircher,* 62½ (pp. 407–409).

But *Tycho,* and all that follow his tables of refraction, making the refractions of the sun and moon (altogether against the nature of light) to exceed those of the fixed stars, and that by about four or five minutes in the horizon, did thereby augment the horizontal parallax of the moon by about the like number of minutes; that is, by about the 12th or 15th part of the whole parallax. Correct this error, and the distance will become 60 or 61 semidiameters of the earth, nearly agreeing with what others have determined.

Let us, then, assume the mean distance of the moon 60 semidiameters of the earth, and its periodic time in respect of the fixed stars $27^d. 7^h. 43^m.$, as astronomers have determined it. And (by Cor. VI, Prop. IV, Book I) a body revolved in our air, near the surface of the earth supposed at rest, by means of a centripetal force which should be to the same force at the distance of the moon inversely as the squares of the distances from the centre of the earth, that is, as 3600 to 1, would (excluding the resistance of the air) complete a revolution in $1^h. 24^m. 27^s.$

Suppose the circumference of the earth to be 123249600 *Paris* feet, as has been determined by the late mensuration of the *French* (p. 425), then the same body, deprived of its circular motion, and falling by the impulse of the same centripetal force as before, would, in one second of time, describe 15 1/12 *Paris* feet.

This we infer by a calculus formed upon Prop. XXXVI of Book I, and it agrees with what we observe in all bodies about the earth. For by the experiments of pendulums, and a computation based thereon, *Huygens* hath demonstrated that bodies falling by all that centripetal force with which (of whatever nature it is) they are impelled near the surface of the earth, do, in one second of time, describe 15 1/12 *Paris* feet.

[11.] *The same proved on the hypothesis that the earth moves.*

But if the earth is supposed to move, the earth and moon together (by Cor. IV of the Laws of Motion, and Prop. LVII, Book I) will be revolved about their common centre of gravity. And the moon (by Prop. LX, Book I) will in the same periodic time, $27^d. 7^h. 43^m.$, with the same circumterrestrial force diminished as the inverse square of the distance, describe an orbit whose semidiameter is to the semidiameter of the former orbit, that is, to 60 semidiameters of the earth, as the sum of both the bodies of the earth and moon is to the first of two mean proportionals between this sum and the body of the earth; that is, if we suppose the moon (on account of its mean apparent diameter 31½′) to be about 1/42 of the earth, as 43 to $\sqrt[3]{(42 \cdot 43^2)}$, or as about 128 to 127. And therefore the semidiameter of the orbit, that is, the distance between the centres of the moon and earth, will in this case be 60½ semidiameters of the earth, almost the same with that assigned by *Copernicus,* which the *Tychonic* observations by no means disprove; and, therefore, the squared ratio of the decrement of the force holds good in this distance. I have neglected the increment of the orbit which arises from the action of the sun as inconsiderable; but if that is subtracted, the true distance will remain about 60 4/9 semidiameters of the earth.

[12.] *The decrease of the forces inversely as the square of the distances from the earth and planets is proved also from the eccentricities of the planets and the very slow motion of the apsides.*

But further (p. 407), this proportion of the decrement of the forces is confirmed from the eccentricity of the planets, and the very slow motion of their apsides; for (by the Corollaries of Prop. XLV, Book I) in no other proportion could the circumsolar planets once in every revolution descend to their least and once ascend to their greatest distance from the sun, and the places of those distances remain immovable. A small error from the squared ratio would produce a motion of the apsides considerable in every revolution, but in many enormous.

But now, after innumerable revolutions, hardly any such motion has been perceived in the orbits of the circumsolar planets. Some astronomers affirm that there is no such motion; others reckon it no greater than what may easily arise from the causes hereafter to be assigned, which is of no moment in the present question.

We may even neglect the motion of the moon's apse (pp. 406–407), which is far greater than in the circumsolar planets, amounting in every revolution to three degrees; and from this motion it is demonstrable that the circumterrestrial force decreases in no less than the inverse square, but far less than the inverse cube of the distance; for if the square were gradually changed into the cube, the motion of the apse would thereby increase to infinity; and, therefore, by a very small mutation, would exceed the motion of the moon's apse. This slow motion arises from the action of the circumsolar force, as we shall afterwards explain. But excluding this cause, the apse or apogee of the moon will be fixed, and the squared ratio of the decrease of the circumterrestrial force in different distances from the earth will accurately take place.

[13.] *The intensity of the forces directed to the individual planets. The powerful circumsolar force.*

Now that this proportion has been established, we may compare the forces of the several planets among themselves (p. 406).

In the mean distance of Jupiter from the earth, the greatest elongation of the outermost satellite from Jupiter's centre (by the observations of Mr. *Flamsteed*) is 8′ 13″; and therefore the distance of the satellite from the centre of Jupiter is to the mean distance of Jupiter from the centre of the sun as 124 to 52012, but to the mean distance of Venus from the centre of the sun as 124 to 7234; and their periodic times are $16\frac{3}{4}^{d}$. and $224\frac{2}{3}^{d}$.; and from hence (according to Cor. II, Prop. IV, Book I), dividing the distances by the squares of the times, we infer that the force by which the satellite is impelled towards Jupiter is to the force by which Venus is impelled towards the sun as 442 to 143; and if we diminish the force by which the satellite is impelled according to the inverse square of the distance 124 to 7234, we shall have the circumjovial force in the distance of Venus from the sun to the circumsolar force by which Venus is impelled as $\frac{13}{100}$ to 143 or as 1 to 1100; therefore at equal distances the circumsolar force is 1100 times greater than the circumjovial.

And, by the like computation, from the periodic time of the satellite of Saturn 15^{d}. 22^{h}. and its greatest elongation from Saturn, while that planet is in its mean distance from us, 3′ 20″, it follows that the distance of this satellite from Saturn's centre is to the distance of Venus from the sun as $92\frac{2}{5}$ to 7234; and from thence that the absolute circumsolar force is 2360 times greater than the absolute circumsaturnal.

[14.] *The small circumterrestrial force.*

From the regularity of the heliocentric and irregularity of the geocentric motions of Venus, of Jupiter, and the other planets, it is evident (by Cor. IV, Prop. III, Book I) that the circumterrestrial force, compared with the circumsolar, is very small.

Riccioli and *Vendelin* have each tried to determine the sun's parallax from the moon's dichotomies observed by the telescope, and they agree that it does not exceed half a minute.

Kepler, from *Tycho's* observations and his own, found the parallax of Mars insensible, even in opposition to the sun, when that parallax is somewhat greater than the sun's.

Flamsteed attempted the same parallax with the micrometer in the perigean position of Mars, but never found it above 25″; and hence concluded the sun's parallax at most 10″.

From this it follows that the distance of the moon from the earth bears no greater proportion to the distance of the earth from the sun than 29 to 10000; nor to the distance of Venus from the sun than 29 to 7233.

From these distances, together with the periodic times, by the method above explained, it is easy to infer that the absolute circumsolar force is at least 229400 times greater than the absolute circumterrestrial force.

And though we were only certain, from the observations of *Riccioli* and *Vendelin,* that the sun's parallax was less than half a minute, yet from this it will follow that the absolute circumsolar force exceeds the absolute circumterrestrial force 8500 times.

[15.] *Apparent diameters of the planets.*

By similar computations I happened to discover an analogy, that is observed between the forces and the bodies of the planets; but, before I explain this analogy, the apparent diameters of the planets in their mean distances from the earth must be determined.

Mr. *Flamsteed* (p. 402), by the micrometer, measured the diameter of Jupiter 40″ or 41″; the diameter of Saturn's ring 50″; and the diameter of the sun about 32′ 13″.

But the diameter of Saturn is to the diameter of the ring, according to Mr. *Huygens* and Dr. *Halley,* as 4 is to 9; according to *Gallet,* as 4 is to 10;

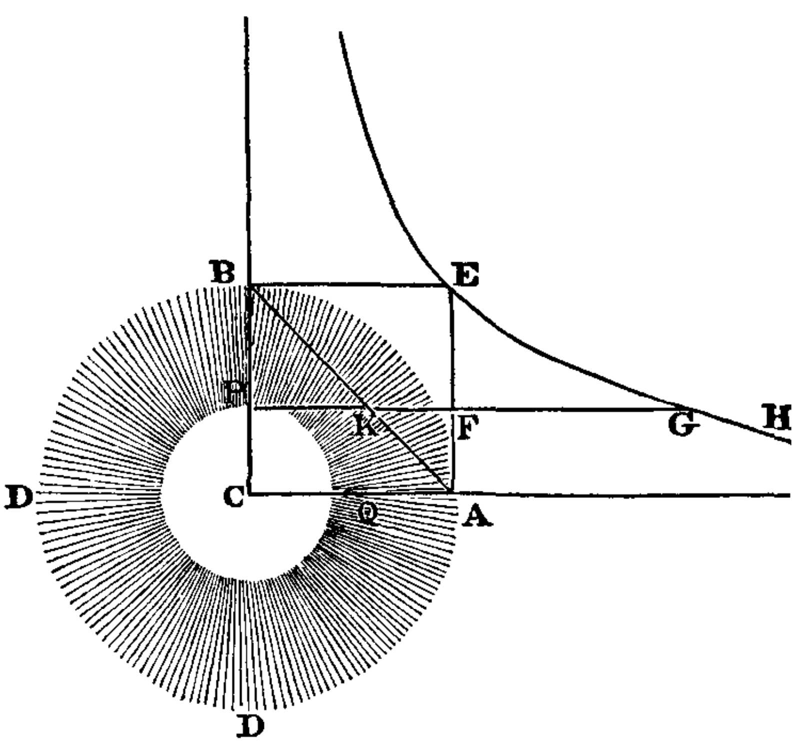
and according to *Hooke* (by a telescope of 60 feet), as 5 to 12. And from the middle ratio, 5 to 12, the diameter of Saturn's body is inferred to be about 21″.

[16.] *Correction of the apparent diameters.*

Such as we have said are the apparent magnitudes; but, because of the unequal refrangibility of light, all lucid points are dilated by the telescope, and in the focus of the object glass cover a circular space whose breadth is about the 50th part of the aperture of the glass.

It is true, that towards the circumference the light is so diffuse as hardly to be visible; but towards the middle, where the light is of greater intensity, and is sensible enough, it makes a small lucid circle, whose breadth varies according to the splendor of the lucid point, but is generally about the 3d, or 4th, or 5th part of the breadth of the whole.

Let ABD represent the circle of the whole light; PQ the small circle of the denser and clearer light; C the centre of both; CA, CB semidiameters of the greater circle containing a right angle at C; ACBE the square comprehended under these semidiameters; AB the diagonal of that square; EGH an hyperbola with the centre C and asymptotes CA, CB; PG a perpendicular erected from any point P of the line BC, and meeting the hyperbola in G, and the right lines AB, AE, in K and F: and the density of the light in any place P will, by my computation, be as the line FG, and therefore at the centre infinite, but near the circumference very small. And the whole light within the small circle PQ is to the whole without as the area of the quadrilateral figure CAKP is to the triangle PKB. And we are to understand the small circle



PQ to be there terminated, where FG, the intensity of the light, begins to be less than what is required for visibility.

Hence it was, that, at the distance of 191382 feet, a fire of 3 feet in diameter, through a telescope of 3 feet, appeared to M. *Picard* of 8″ in breadth, when it should have appeared only of 3″ 14‴; and hence it is that the brighter fixed stars appear through the telescope as of 5″ or 6″ in diameter, and that with a good full light; but with a fainter light they appear to run out to a greater breadth. Hence, likewise, it was that *Hewelcke,* by diminishing the aperture of the telescope, did cut off a great part of the light towards the circumference, and brought the disk of the star to be more distinctly defined, which, though hereby diminished, did yet appear as of 5″ or 6″ in diameter. But Mr. *Huygens,* only by clouding the eyeglass with a little smoke, did so effectually extinguish this scattered light, that the fixed stars appeared as mere points, void of all appreciable breadth. Hence also it was that Mr. *Huygens,* from the breadth of bodies interposed to intercept the whole light of the planets, reckoned their diameters greater than others have measured them by the micrometer; for the scattered light, which could not be seen before for the stronger light of the planet, when the planet is hid, appears in every way the farther spread. Lastly, it is for this reason that the planets appear so small when projected on the disk of the sun, being lessened by the dilated light. For to *Hewelcke, Gallet,* and Dr. *Halley,* Mercury did not seem to exceed 12″ or 15″; and Venus appeared to Mr. *Crabtrie* only 1′ 3″; to *Horrox* but 1′ 12″; though by the mensurations of *Hewelcke* and *Huygens* without the sun's disk, it ought to have been seen at least 1′ 24″. Thus the apparent diameter of the moon, which in 1684, a few days both before and after the sun's eclipse, was measured at the Observatory of *Paris* 31′ 30″, in the eclipse itself did not seem to exceed 30′ or 30′ 05″; and therefore the diameters of the planets are to be diminished when without the sun, and to be augmented when within it, by some seconds. But the errors seem to be less than usual in the micrometer measurements. So from the diameter of the shadow, determined by the eclipses of the satellites, Mr. *Flamsteed* found that the semidiameter of Jupiter was to the greatest elongation of the outermost satellite as 1 to 24.903. Therefore since that elongation is 8′ 13″, the diameter of Jupiter will be 39½″; and, rejecting the scattered light, the diameter found by the micrometer 40″ or 41″ will be reduced to 39½″; and the diameter of Saturn 21″ is to be diminished by the like correction, and to be reckoned 20″, or something less. But (if I am not mistaken) the diameter of the sun, because of its stronger light, is to be diminished somewhat more, and to be reckoned about 32′, 32′ 6″.

[17.] *Why some of the planets are more, others less dense, and the forces in all are proportional to the quantities of matter.*

That bodies so different in magnitude should come so near to a proportionality with their forces, is not without some mystery (pp. 416–417).

It may be that the remoter planets, for want of heat, have not those metallic substances and ponderous minerals with which our earth abounds; and that the bodies of Venus and Mercury, as they are more exposed to the sun's heat, are also harder baked, and more compact.

For, from the experiment of the burning-glass, we see that the heat increases with the density of light; and this density increases inversely as the square of the distance from the sun; whence the sun's heat in Mercury is proved to be sevenfold its heat in our summer seasons. But with this heat our water boils; and those heavy fluids, quicksilver and the spirit of vitriol, gently evaporate, as I have tried by the thermometer; and therefore there can be no fluids in Mercury but what are heavy, and able to bear a great heat, and from which substances of great density may be formed.

And why not, if God has placed different bodies at different distances from the sun, so that the denser bodies always possess the nearer places, and each body enjoys a degree of heat suitable to its condition, and proper for its constitution? From this consideration it will best appear that the weights of all the planets are one to another as their forces.

But I should be glad if the diameters of the planets were more accurately measured; and that may be done, if a lamp, set at a great distance, is made to shine through a circular hole, and both the hole and the light of the lamp are so diminished that the image may appear through the telescope just like the planet, and may be defined by the same measure: then the diameter of the hole will be to its distance from the objective glass as the true diameter of the planet to its distance from us. The light of the lamp may be diminished by the interposition either of pieces of cloth, or of smoked glass.

[18.] *Another analogy between forces and bodies attracted is shown in the heavens.*

Of kin to the analogy we have been describing is another observed between the forces and the bodies attracted (pp. 411–414). Because the action of the centripetal force upon the planets decreases inversely as the square of the distance, and the periodic time increases as the $\frac{3}{2}$th power of the distance, it is evident that the actions of the centripetal force, and therefore the periodic times, would be equal in equal planets at equal distances from the sun; and in equal distances of unequal planets the total actions of the centripetal force would be as the bodies of the planets; for if the actions were not proportional to the bodies to be moved, they could not equally retract these bodies from the tangents of their orbs in equal times: nor could the motions of the satellites of Jupiter be so regular, if it was not that the circumsolar force was equally exerted upon Jupiter and all its satellites in proportion of their several weights. And the same thing is to be said of Saturn in respect of its satellites, and of our earth in respect of the moon, as appears from Cor. II and III, Prop. LXV, Book I. And, therefore, at equal distances, the actions of the centripetal force are equal upon all the planets in proportion to their bodies, or to the quantities of matter in their several bodies; and for the same reason must be the same upon all the particles of the same size of which the planet is composed; for if the action was greater upon some sort of particles than upon others, in proportion to their quantity of matter, it would be also greater or less upon the whole planets, not in proportion to the quantity only, but likewise to the sort of matter more copiously found in one and more sparingly in another.

[19.] *It is found also in terrestrial bodies.*

In such bodies as are found on our earth of very different sorts, I examined this analogy with great care (pp. 351–352).

If the action of the circumterrestrial force is proportional to the bodies to be moved, it will (by the second Law of Motion) move them with equal velocity in equal times, and will make all bodies, let fall, to descend through equal spaces in equal times, and all bodies, hung by equal threads, to vibrate in equal times. If the action of the force was greater, the times would be less; if that was less, these would be greater.

But it has been long ago observed by others, that (allowance being made for the small resistance of the air) all bodies descend through equal spaces in equal times; and, by the help of pendulums, that equality of times may be observed with great exactness.

I tried the thing in gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two equal wooden boxes. I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes, hung by equal threads of 11 feet, made a couple of pendulums perfectly equal in weight and figure, and equally exposed to the resistance of the air: and, placing the one by the other, I observed them to play together forwards and backwards for a long while, with equal vibrations. And therefore (by Cor. I and VI, Prop. XXIV, Book II) the quantity of matter in the gold was to the quantity of matter in the wood as the action of the motive force upon all the gold to the action of the same upon all the wood; that is, as the weight of the one to the weight of the other.

And by these experiments, in bodies of the same weight, one could have discovered a difference of matter less than the thousandth part of the whole.

[20.] *The agreement of those analogies.*

Since the action of the centripetal force upon the bodies attracted is, at equal distances, proportional to the quantities of matter in those bodies, reason requires that it should be also proportional to the quantity of matter in the body attracting.

For all action is mutual, and (pp. 13, 26, by the third Law of Motion) makes the bodies approach one to the other, and therefore must be the same in both bodies. It is true that we may consider one body as attracting, another as attracted; but this distinction is more mathematical than natural. The attraction resides really in each body towards the other, and is therefore of the same kind in both.

[21.] *Their coincidence.*

And hence it is that the attractive force is found in both. The sun attracts Jupiter and the other planets; Jupiter attracts its satellites; and, for the same reason, the satellites act as well one upon another as upon Jupiter, and all the planets mutually one upon another.

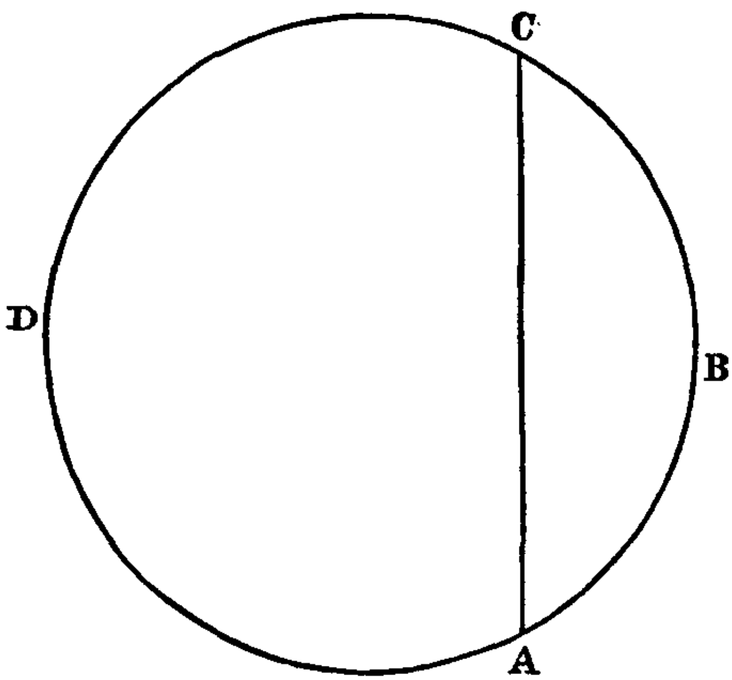
And though the mutual actions of two planets may be distinguished and considered as two, by which each attracts the other, yet, as those actions are between both, they do not make two but one operation between two terms. Two bodies may be attracted each to the other by the contraction of a cord interposed. There is a double cause of action, namely, the disposition of both bodies, as well as a double action so far as the action is considered as upon two bodies; but as between two bodies, it is but one single one. It is not one action by which the sun attracts Jupiter, and another by which Jupiter attracts the sun; but it is one action by which the sun and Jupiter mutually endeavor to approach each the other. By the action with which the sun attracts Jupiter, Jupiter and the sun endeavor to come nearer together (by the third Law of Motion); and by the action with which Jupiter attracts the sun. Likewise Jupiter and the sun endeavor to come nearer together. But the sun is not attracted towards Jupiter by a twofold action, nor Jupiter by a twofold action towards the sun; but it is one single intermediate action, by which both approach nearer together.

Thus iron draws the loadstone (p. 26), as well as the loadstone draws the iron; for all iron in the neighborhood of the loadstone draws other iron. But the action between the loadstone and iron is single, and is considered as single by the philosophers. The action of iron upon the loadstone is, indeed, the action of the loadstone between itself and the iron, by which both endeavor to come nearer together: and so it manifestly appears; for if you remove the loadstone, the whole force of the iron almost ceases.

In this sense it is that we are to conceive one single action to be exerted between two planets arising from the conspiring natures of both; and this action standing in the same relation to both, if it is proportional to the quantity of matter in the one, it will be also proportional to the quantity of matter in the other.

[22.] *The forces due to relatively very small bodies are inappreciable.*

Perhaps it may be objected that, according to this philosophy (p. 415), all bodies should attract one another, contrary to the evidence of experiments in terrestrial bodies; but I answer, that the experiments in terrestrial bodies do not count; for the attractions of homogeneous spheres near their surfaces are (by Prop. LXXII, Book I) as their diameters. Hence a sphere of one foot in diameter, and of a like nature to the earth, would attract a small body placed near its surface with a force 20000000 times less than the earth would do if placed near its surface; but so small a force could produce no sensible effect. If two such spheres were distant but by ¼ of an inch, they would not, even in spaces void of resistance, come together by the force of their mutual attraction in less than a month's time; and lesser spheres will come together at a rate yet slower, namely, in the proportion of their diameters. Nay, whole mountains will not be sufficient to produce any sensible effect. A mountain of an hemispherical figure, three miles high, and six broad, will not, by its attraction, draw the pendulum two minutes out of the true perpendicular; and it is only in the great bodies of the planets that these forces are to be perceived, unless we may reason about smaller bodies in manner following.



[23.] *The forces directed towards all terrestrial bodies are proportional to their quantities of matter.*

Let ABCD (p. 26) represent the globe of the earth cut by any plane AC into two parts ACB and ACD. The part ACB bearing upon the part ACD presses it with its whole weight; nor can the part ACD sustain this pressure and continue unmoved, if it is not opposed by an equal contrary pressure. And therefore the parts equally press each other by their weights, that is, equally attract each other, according to the third Law of Motion; and, if separated and let go, would fall towards each other with velocities inversely as the bodies. All this we may try and see in the loadstone, whose attracted part does not propel the part attracting, but is only stopped and sustained thereby.

Suppose now that ACB represents some small body on the earth's surface; then, because the attractions of this particle, and of the remaining part

ACD of the earth towards each other, are equal, but the attraction of the particle towards the earth (or its weight) is as the matter of the particle (as we have proved by the experiment on pendulums), the attraction of the earth towards the particle will likewise be as the matter of the particle; and therefore the attractive forces of all terrestrial bodies will be as their several quantities of matter.

[24.] *Shown that the same forces are directed towards celestial bodies.*

The forces (p. 413), which are as the matter in terrestrial bodies of all forms, and therefore are not mutable with the forms, must be found in all sorts of bodies whatsoever, celestial as well as terrestrial, and be in all proportional to their quantities of matter, because among all there is no difference of substance, but of modes and forms only. But in the celestial bodies the same thing is likewise proved thus. We have shown that the action of the circumsolar force upon all the planets (reduced to equal distances) is as the matter of the planets; that the action of the circumjovial force upon the satellites of Jupiter observes the same law; and the same thing is to be said of the attraction of all the planets towards every planet: but it follows from this (by Prop. LXIX, Book I) that their attractive forces are as their several quantities of matter.

[25.] *The forces decrease from the surfaces of the planets outwardly, inversely as the square of the distances, and inwardly, directly as the distances from the centres.*

As the parts of the earth attract one another so do those of all the planets. If Jupiter and its satellites were brought together, and formed into one globe, without doubt they would continue to attract one another as before. And, on the other hand, if the body of Jupiter was broken into more globes, surely these would no less attract one another than they do the satellites now. From these attractions it is that the bodies of the earth and all the planets assume a spherical figure, and their parts cohere, and are not dispersed through the ether. But we have before proved that these forces arise from the universal nature of matter (p. 416), and that, therefore, the force of any whole globe is made up of the several forces of all its parts. And from this it follows (by Cor. III, Prop. LXXIV, Book I) that the force of every particle decreases inversely as the square of the distance from that particle; and (by Prop. LXXIII and LXXV, Book I) that the force of an entire globe, reckoning from the surface outwards, decreases inversely as the square of the distance, but reckoning inwards, decreases simply as the first power of the distances from the centres, if the matter of the globe be uniform. And when the matter of the globe, reckoning from the centre towards the surface, is not uniform (pp. 415–417), nevertheless, the decrease from the surface outwards is inversely as the square of the distance (by Prop. LXXVI, Book I), provided that difformity is similar in places round about at equal distances from the centre. And two such globes will (by the same Proposition) attract one the other with a force decreasing inversely as the square of the distance between their centres.

[26.] *The intensities of the forces and the resulting motions in individual cases.*

Therefore the absolute force of every globe is as the quantity of matter which the globe contains; but the motive force by which every globe is attracted towards another, and which, in terrestrial bodies, we commonly call their weight, is as the content under the quantities of matter in both globes divided by the square of the distance between their centres (by Cor. IV, Prop. LXXVI, Book I), to which force the quantity of motion, by which each globe in a given time will be carried towards the other, is proportional. And the accelerative force, by which every globe according to its quantity of matter is attracted towards another, is as the quantity of matter in that other globe divided by the square of the distance between the centres of the two (by Cor. II, Prop. LXXVI, Book I); to which force, the velocity by which the attracted globe will, in a given time, be carried towards the other is proportional. And from these principles well understood, it will now be easy to determine the motions of the celestial bodies among themselves.

[27.] *All the planets revolve around the sun.*

From comparing the forces of the planets one with another, we have above seen that the circumsolar does more than a thousand times exceed all the rest; but by the action of a force so great it is unavoidable that all bodies within, nay, and far beyond, the bounds of the planetary system must descend directly to the sun, unless by other motions they are impelled towards other parts: nor is our earth to be excluded from the number of such bodies; for certainly the moon is a body of the same nature with the planets, and subject to the same attractions with the other planets, seeing it is by the circumterrestrial force that it is retained in its orbit. But that the earth and moon are equally attracted towards the sun, we have above proved; we have likewise before proved that all bodies are subject to the said common laws of attraction. Nay, supposing any of those bodies to be deprived of its circular motion about the sun, by having its distance from the sun, we may find (by Prop. XXXVI, Book I) in what space of time it would in its descent arrive at the sun; namely, in half that periodic time in which the body might be revolved at one-half of its former distance; or in a space of time that is to the periodic time of the planet as 1 to $4\sqrt{2}$; as that Venus in its descent would arrive at the sun in the space of 40 days, Jupiter in the space of two years and one month, and the earth and moon together in the space of 66 days and 19 hours. But, since no such thing happens, it must needs be, that those bodies are moved towards other parts (p. 3), nor is every motion sufficient for this purpose. To hinder such a descent, a due proportion of velocity is required. And hence depends the force of the argument drawn from the retardation of the motions of the planets. Unless the circumsolar force decreased as the square of their increasing slowness, the excess thereof would force those bodies to descend to the sun; for instance, if the motion (other things being equal) was retarded by one-half, the planet would be held in its orbit by one-fourth of the former circumsolar force, and by the excess of the other three-fourths would descend to the sun. And therefore the planets (Saturn, Jupiter, Mars, Venus, and Mercury) are not really retarded in their perigees, nor become really stationary, or regressive with slow motions. All these are but apparent, and the absolute motions, by which the planets continue to revolve in their orbits, are always direct, and nearly uniform. But that such motions are performed about the sun, we have already proved; and therefore the sun, as the centre of the absolute motions, is quiescent. For we can by no means allow quiescence to the earth, lest the planets in their perigees should indeed be truly retarded, and become truly stationary and regressive, and so for want of motion should descend to the sun. But further; since the planets (Venus, Mars, Jupiter, and the rest) by radii drawn to the sun describe regular orbits, and areas (as we have shown) nearly and to sense proportional to the times, it follows (by Prop. III, and Cor. III, Prop. LXV, Book I) that the sun is moved with no notable force, unless perhaps with such as all the planets are equally moved with, according to their several quantities of matter, in parallel lines, and so the whole system is transferred in right lines. Reject that translation of the whole system, and the sun will be almost quiescent in the centre thereof. If the sun was revolved about the earth, and carried the other planets round about itself, the earth ought to attract the sun with a great force, but the circumsolar planets with no force producing any sensible effect, which is contrary to Cor. III, Prop. LXV, Book I. Add to this, that if hitherto the earth, because of the gravitation of its parts, has been placed by most authors in the lowermost region of the universe; now, for better reason, the sun possessed of a centripetal force exceeding our terrestrial gravitation a thousand times and more, ought to be depressed into the lowermost place, and to be held for the centre of the system. And thus the true disposition of the whole system will be more fully and more exactly understood.

[28.] *The common centre of gravity of the sun and all the planets is at rest and the sun moves with a very slow motion. Explanation of the solar motion.*

Because the fixed stars are quiescent one in respect of another (pp. 419–420), we may consider the sun, earth, and planets, as one system of bodies carried hither and thither by various motions among themselves; and the common centre of gravity of all (by Cor. IV of the Laws of Motion) will either be quiescent, or move uniformly forwards in a right line: in which case the whole system will likewise move uniformly forwards in right lines. But this is an hypothesis hardly to be admitted; and, therefore, setting it aside, that common centre will be quiescent: and from it the sun is never far removed. The common centre of gravity of the sun and Jupiter falls on the surface of the sun; and though all the planets were placed towards the same parts from the sun with Jupiter, the common centre of the sun and all of them would scarcely recede twice as far from the sun's centre; and, therefore, though the sun, according to the different situations of the planets, is variously agitated, and always wandering to and fro with a slow motion of libration, yet it never recedes one entire diameter of its own body from the quiescent centre of the whole system. But from the weights of the sun and planets above determined, and the situation of all among themselves, their common centre of gravity may be found; and, this being given, the sun's place at any supposed time may be obtained.

[29.] *Nevertheless, the planets revolve in ellipses having foci at the centre of the sun; and the radii drawn to the sun describe areas proportional to the times.*

About the sun, thus librated, the other planets are revolved in elliptic orbits (p. 421), and by radii drawn to the sun, describe areas nearly proportional to the times, as is explained in Prop. LXV, Book I. If the sun were quiescent, and the other planets did not act mutually one upon another, their orbits would be elliptic, and the areas exactly proportional to the times (by Prop. XI, and Cor., Prop. LXVIII, Book I). But the actions of the planets among themselves, compared with the actions of the sun on the planets, are of no moment, and produce no sensible errors. And those errors are less in revolutions about the sun agitated in the manner but now described than if those revolutions were made about the sun quiescent (by Prop. LXVI, Book I, and Cor., Prop. LXVIII, Book I), especially if the focus of every orbit is placed in the common centre of gravity of all the lower included planets; viz., the focus of the orbit of Mercury in the centre of the sun; the focus of the orbit of Venus in the common centre of gravity of Mercury and the sun; the focus of the orbit of the earth in the common centre of gravity of Venus, Mercury, and the sun; and so of the rest. And by this means the foci of the orbits of all the planets, except Saturn, will not be sensibly removed from the centre of the sun, nor will the focus of the orbit of Saturn recede sensibly from the common centre of gravity of Jupiter and the sun. And therefore astronomers are not far from the truth, when they reckon the sun's centre the common focus of all the planetary orbits. In Saturn itself the error thence arising does not exceed 1′ 45″. And if its orbit, by placing the focus thereof in the common centre of gravity of Jupiter and the sun, shall happen to agree better with the phenomena, from thence all that we have said will be further confirmed.

[30.] *The sizes of the orbits and the motion of their aphelions and nodes.*

If the sun was quiescent, and the planets did not act one on another, the aphelions and nodes of their orbits would likewise (by Prop. I, XI, and Cor., Prop. XIII, Book I) be quiescent. And the longer axes of their elliptic orbits would (by Prop. XV) be as the cube roots of the squares of their periodic times: and therefore from the given periodic times would be also given. But those times are to be measured not from the equinoctial points, which are movable, but from the first star of Aries. Put the semiaxis of the earth's orbit 100000, and the semiaxes of the orbits of Saturn, Jupiter, Mars, Venus, and Mercury, from their periodic times, will come out 953806, 520116, 152399, 72333, 38710, respectively. But from the sun's motion every semiaxis is increased (by Prop. LX, Book I) by about one-third of the distance of the sun's centre from the common centre of gravity of the sun and planet (pp. 424–428). And from the actions of the exterior planets on the interior, the periodic times of the interior are somewhat protracted, though scarcely by any sensible quantity; and their aphelions are transferred (by Cor. VI and VII, Prop. LXVI, Book I) by very slow motions forwards. And on the like account the periodic times of all, especially of the exterior planets, will be prolonged by the actions of the comets, if any such there are, without the orbit of Saturn, and the aphelions of all will be thereby carried forwards. But from the progress of the aphelions the regress of the nodes follows (by Cor. XI, XIII, Prop. LXVI, Book I). And if the plane of the ecliptic is quiescent, the regress of the nodes (by Cor. XVI, Prop. LXVI, Book I) will be to the progress of the aphelion in every orbit as the regress of the nodes of the moon's orbit to the progress of its apogee nearly, that is, as about 10 to 21. But astronomical observations seem to confirm a very slow progress of the aphelions, and a regress of the nodes in respect of the fixed stars. And hence it is probable that there are comets in the regions beyond the planets, which, revolving in very eccentric orbits, quickly fly through their perihelian parts, and, by an exceedingly slow motion in their aphelions, spend almost their whole time in the regions beyond the planets; as we shall afterwards explain more fully.[1]

[[1] Appendix, Note 58.]

[31.] *From the foregoing principles are derived all the lunar motions thus far noted by astronomers.*

The planets thus revolved about the sun (pp. 433–435) may at the same time carry others revolving about themselves as satellites or moons, as appears by Prop. LXVI, Book I. But from the action of the sun our moon must move with greater velocity, and, by a radius drawn to the earth, describe an area greater for the time; it must have its orbit less curved, and therefore approach nearer to the earth in the syzygies than in the quadratures, except so far as the motion of eccentricity hinders those effects. For the eccentricity is greatest when the moon's apogee is in the syzygies, and least when the same is in the quadratures; and hence it is that the perigean moon is swifter and nearer to us, but the apogean moon slower and farther from us, in the syzygies than in the quadratures. But further, the apogee has a progressive and the nodes a regressive motion, both unequable. For the apogee is more swiftly progressive in its syzygies, more slowly regressive in its quadratures, and by the excess of its progress above its regress is yearly transferred forwards; but the nodes are quiescent in their syzygies, and most swiftly regressive in their quadratures. But further, still, the greatest latitude of the moon is greater in its quadratures than in its syzygies; and the mean motion swifter in the aphelion of the earth than in its perihelion. More inequalities in the moon's motion have not hitherto been taken notice of by astronomers; but all these follow from our principles in Cor. II-XIII, Prop. LXVI, Book I, and are known really to exist in the heavens. And this may be seen in that most ingenious, and if I mistake not, of all the most accurate, hypothesis of Mr. *Horrox,* which Mr. *Flamsteed* has fitted to the heavens; but the astronomical hypotheses are to be corrected in the motion of the nodes; for the nodes admit the greatest equation or prosthaphaeresis in their octants, and this inequality is most conspicuous when the moon is in the nodes, and therefore also in the octants; and hence it was that *Tycho,* and others after him, referred this inequality to the octants of the moon, and made it menstrual; but the reasons by us adduced prove that it ought to be referred to the octants of the nodes, and to be made annual.

[32.] *Several irregularities of motion are deduced, not hitherto observed.*

Besides those inequalities taken notice of by astronomers (pp. 434, 473–475), there are yet some others, by which the moon's motions are so disturbed, that hitherto by no law could they be reduced to any certain regulation. For the velocities or hourly motions of the apogee and nodes of the moon, and their equations, as well as the difference between the greatest eccentricity in the syzygies and the least in the quadratures, and that inequality which we call the variation, in the progress of the year are augmented and diminished (by Cor. XIV, Prop. LXVI, Book I) as the cube of the sun's apparent diameter. Besides that, the variation is mutable nearly as the square of the time between the quadratures (by Cor. I and II, Lem. X, and Cor. XVI, Prop. LXVI, Book I); and all those inequalities are somewhat greater in that part of the orbit which faces the sun than in the opposite part, but by a difference that is scarcely or not at all perceptible.

[33.] *The distance of the moon from the earth at a given time.*

By a computation (p. 444), which for brevity's sake I do not describe, I also find that the area which the moon by a radius drawn to the earth describes in the several equal moments of time is nearly as the sum of the number $237\frac{3}{10}$, and the versed sine of the double distance of the moon from the nearest quadrature in a circle whose radius is unity; and therefore that the square of the moon's distance from the earth is as that sum divided by the hourly motion of the moon. Thus it is when the variation in the octants is in its mean quantity; but if the variation is greater or less, that versed sine must be augmented or diminished in the same ratio. Let astronomers try how exactly the distances thus found will agree with the moon's apparent diameters.

[34.] *The motions of the satellites of Jupiter and Saturn derived from the motions of our moon.*

From the motions of our moon we may derive the motions of the moons or satellites of Jupiter and Saturn (p. 433); for the mean motion of the nodes of the outermost satellite of Jupiter is to the mean motion of the nodes of our moon in a proportion compounded of the square of the periodic time of the earth about the sun to the periodic time of Jupiter about the sun, and the simple proportion of the periodic time of the satellite about

Jupiter to the periodic time of our moon about the earth, by Cor. xvi, Prop. lxvi, Book i: and therefore those nodes, in the space of a hundred years, are carried 8° 24′ backwards, or forwards. The mean motions of the nodes of the inner satellites are to the (mean) motion of (the nodes of) the outermost as their periodic times to the periodic time of this, by the same Corollary, and are thence given. And the motion of the apse of every satellite orbit forwards is to the motion of its nodes backwards, as the motion of the apogee of our moon is to the motion of its nodes, by the same Corollary, and is thence given. The greatest equations of the nodes and line of the apsides of each satellite orbit are to the greatest equations of the nodes and the line of the apsides of the moon respectively as the motion of the nodes and line of the apsides of the satellites' orbits in the time of one revolution of the first equations is to the motion of the nodes and apogee of the moon in the time of one revolution of the last equations. The variation of a satellite seen from Jupiter is to the variation of our moon in the same proportion as the whole motions of their nodes respectively, during the times in which the satellite and our moon (after parting from) are revolved (again) to the sun, by the same Corollary; and therefore in the outermost satellite the variation does not exceed 5″ 12‴. From the small amount of those inequalities, and the slowness of the motions, it happens that the motions of the satellites are found to be so regular, that the more modern astronomers either deny all motion of the nodes, or affirm them to be very slowly regressive.

[35.] *The planets rotate around their own axes uniformly with respect to the stars; these motions are well adapted for the measurement of time* (p. 423).

While the planets are thus revolved in orbits about remote centres, in the meantime they make their several rotations about their proper axes: the sun in 26 days; Jupiter in $9^h. 56^m.$; Mars in $24\frac{2}{3}^h.$; Venus in $23^h.$; and that in planes not much inclined to the plane of the ecliptic, and according to the order of the signs, as astronomers determine from the spots or maculae that by turns present themselves to our sight in their bodies; and there is a like revolution of our earth performed in $24^h.$; and those motions are neither accelerated nor retarded by the actions of the centripetal forces, as appears by Cor. xxii, Prop. lxvi, Book i; and therefore of all others they are the most uniform and most fit for the measurement of time; but those revolutions are to be reckoned uniform not from their return to the sun, but to some fixed star: for as the position of the planets to the sun is non-uniformly varied, the revolutions of those planets from sun to sun are rendered non-uniform.

[36.] *In like manner the moon rotates around its axis by diurnal motion; hence arises its libration.*

In like manner is the moon revolved about its axis by a motion most uniform in respect of the fixed stars, viz., in $27^d. 7^h. 43^m.$, that is, in the space of a sidereal month; so that this diurnal motion is equal to the mean motion of the moon in its orbit; upon this account the same face of the moon always turns towards the centre about which this mean motion is performed, that is, the exterior focus of the moon's orbit, nearly; and hence arises a deflection of the moon's face from the earth, sometimes towards the east, and other times towards the west, according to the position of the focus towards which it is turned; and this deflection is equal to the equation of the moon's orbit, or to the difference between its mean and true motions; and this is the moon's libration in longitude: but it is likewise affected with a libration in latitude arising from the inclination of the moon's axis to the plane of the orbit in which the moon is revolved about the earth; for that axis retains the same position to the fixed stars, nearly, and hence the poles present themselves to our view by turns, as we may understand from the example of the motion of the earth, whose poles, by reason of the inclination of its axis to the plane of the ecliptic, are by turns illuminated by the sun. To determine exactly the position of the moon's axis to the fixed stars, and the variation of this position, is a problem worthy of an astronomer.

[37.] *The precession of the equinoxes and the libratory motion of the axes of the earth and planets.*

By reason of the diurnal revolutions of the planets, the matter which they contain endeavors to recede from the axis of this motion, and hence the fluid parts rising higher towards the equator than about the poles (p. 424), would lay the solid parts about the equator under water, if those parts did not rise also (pp. 424, 429); upon this account the planets are somewhat thicker about the equator than about the poles; and their equinoctial points (p. 433) thence become regressive; and their axes, by a motion of nutation, twice in every revolution, librate towards their ecliptics, and twice return again to their former inclination, as is explained in Cor. XVIII, Prop. LXVI, Book I; and hence it is that Jupiter, viewed through very long telescopes, does not appear altogether round (p. 428), but having its diameter that lies parallel to the ecliptic somewhat longer than that which is drawn from north to south.

[38.] *The ocean must flow twice and ebb twice, each day, and the highest water occurs at the third hour after the approach of the luminaries to the meridian of the place.*

And from the diurnal motion and the attractions (pp. 435–440) of the sun and moon our sea ought twice to rise and twice to fall every day, as well lunar as solar (by Cor. XIX, XX, Prop. LXVI, Book I), and the greatest height of the water to happen before the sixth hour of either day and after the twelfth hour preceding. By the slowness of the diurnal motion the flood is retracted to the twelfth hour; and by the force of the motion of reciprocation it is protracted and deferred till a time nearer to the sixth hour. But until that time will be more accurately determined by the phenomena, why should we not choose the middle between those extremes, and conjecture the greatest height of the water to happen at the third hour? In this manner the water will rise all that time in which the force of the luminaries to raise it is greater, and will fall all that time in which their force is less; namely, from the ninth to the third hour when that force is greater, and from the third to the ninth when it is less. The hours I reckon from the approach of each luminary to the meridian of the place, as well under as above the horizon; and by the hours of the lunar day I understand the twenty-fourth parts of that time which the moon spends before it comes about again by its apparent diurnal motion to the meridian of the place which it left the day before.

[39.] *The tide is greatest in the syzygies of the luminaries and least in their quadratures, and at the third hour after the moon reaches the meridian; outside of the syzygies and quadratures the tide deviates somewhat from that third hour towards the third hour after the solar culmination.*

But the two motions which the two luminaries raise will not appear distinct, but will make a certain mixed motion. In the conjunction or opposition of the luminaries their forces will be conjoined, and bring on the greatest flood and ebb. In the quadratures the sun will raise the waters which the moon depresses, and depress the waters which the moon raises; and from the difference of their forces the smallest of all tides will follow. And because (as experience tells us) the force of the moon is greater than that of the sun, the greatest height of the water will happen about the third lunar hour. Outside of the syzygies and quadratures, the greatest tide which by the single force of the moon should take place at the third lunar hour, and by the single force of the sun at the third solar hour, by the combined forces of both must happen at an intermediate time that approaches nearer to the third hour of the moon than to that of the sun; and, therefore, while the moon is passing from the syzygies to the quadratures, during which time the third hour of the sun precedes the third of the moon, the greatest tide will precede the third lunar hour, and that by the greatest interval a little after the octants of the moon; and by like intervals the greatest tide will follow the third lunar hour, while the moon is passing from the quadratures to the syzygies.

[40.] *The tides are greatest when the luminaries are nearest the earth.*

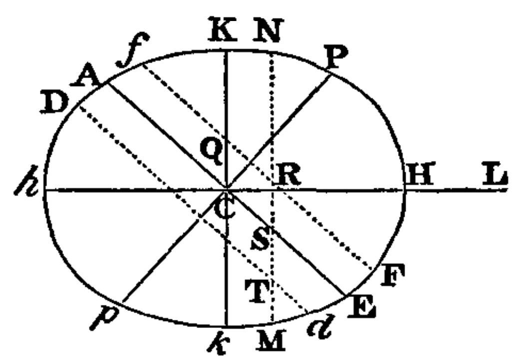
But the effects of the luminaries depend upon their distances from the earth; for when they are less distant their effects are greater, and when more distant their effects are less, and that as the third power of their apparent diameters. Therefore it is that the sun in the winter time, being then in its perigee, has a greater effect, and makes the tides in the syzygies somewhat greater, and those in the quadratures somewhat less, other things being equal, than in the summer season; and every month the moon, while in the perigee, raises greater tides than at the distance of fifteen days before or after, when it is in its apogee. Hence it comes to pass that two highest tides do not follow one the other in two immediately succeeding syzygies.

[41.] *The tides are greatest about the equinoxes.*

The effect of either luminary likewise depends upon its declination or distance from the equator; for if the luminary were placed at the pole, it would constantly attract all the parts of the waters, without any increase or remission of its action, and could cause no reciprocation of motion; and, therefore, as the luminaries decline from the equator towards either pole, they will by degrees lose their force, and on this account will excite lesser tides in the solstitial than in the equinoctial syzygies. But in the solstitial quadratures they will raise greater tides than in the quadratures about the equinoxes; because the effect of the moon, then situated in the equator, most exceeds the effect of the sun; therefore the greatest tides take place in those syzygies, and the least in those quadratures, which happen about the time of both equinoxes; and the greatest tide in the syzygies is always succeeded by the least tide in the quadratures, as we find by experience. But because the sun is less distant from the earth in winter than in summer, it comes to pass that the greatest and least tides more frequently appear before than after the vernal equinox, and more frequently after than before the autumnal.

[42.] *Without the equator the floods are alternately greater and less.*

Moreover, the effects of the luminaries depend upon the latitudes of places. Let A*p*EP represent the earth on all sides covered with deep waters; C its centre; P, *p* its poles; AE the equator; F any place without the equator; F*f* the parallel of the place; D*d* the correspondent parallel on the other side of the equator; L the place which the moon possessed three hours before; H the place of the earth directly under it; *h* the opposite place; K, *k* the places at 90 degrees distance; CH, C*h* the greatest heights of the sea from the centre of the earth; and CK, C*k* the least heights; and if with the axes H*h*, K*k* an ellipse is described, and by the revolution of that ellipse about its longer axis H*h* a spheroid HPK*hpk* is formed, this spheroid will nearly represent the figure of the sea;



and CF, C*f*, CD, C*d* will represent the sea in the places F, *f*, D, *d*. But further; if in the said revolution of the ellipse any point N describes the circle NM, cutting the parallels F*f*, D*d*, in any places R, T, and the equator AE in S, CN will represent the height of the sea in all those places R, S, T, situated in this circle. Wherefore, in the diurnal revolution of any place F the greatest flood will be in F, at the third hour after the approach of the moon to the meridian above the horizon; and afterwards the greatest ebb in Q, at the third hour after the setting of the moon; and then the greatest flood in *f*, at the third hour after the approach of the moon to the meridian under the horizon; and, lastly, the greatest ebb in Q, at the third hour after the rising of the moon; and the latter flood in *f* will be less than the preceding flood in F. For the whole sea is divided into two huge and hemispherical floods, one in the hemisphere KH*k*C on the north side, the other in the opposite hemisphere KH*k*C, which we may therefore call the northern and the southern floods: these floods, being always opposite the one to the other, come by turns to the meridians of all places after the interval of twelve lunar hours; and, seeing the northern countries partake more of the northern flood, and the southern countries more of the southern flood, thence arise tides alternately greater and less in all places without the equator in which the luminaries rise and set. But the greater tide will happen when the moon declines towards the vertex of the place, about the third hour after the approach of the moon to the meridian above the horizon; and when the moon changes its declination, that which was the greater tide will be changed into a lesser; and the greatest difference of the floods will fall out about the times of the solstices, especially if the ascending node of the moon is about the first of Aries. So the morning tides in winter exceed those of the evening, and the evening tides exceed those of the morning in summer; at *Plymouth* by the height of one foot, but at *Bristol* by the height of fifteen inches, according to the observations of *Colepress* and *Sturmy.*

[43.] *By the persistence of the impressed motion the difference of the tides is reduced and the greatest may be the third flood after the syzygies in a month.*

But the motions which we have been describing suffer some alteration from that force of reciprocation which the waters [having once received] retain a little while by their inertia; hence it comes to pass that the tides may continue for some time, though the actions of the luminaries should cease. This power of retaining the impressed motion lessens the difference of the alternate tides, and makes those tides which immediately succeed after the syzygies greater, and those which follow next after the quadratures less. And hence it is that the alternate tides at *Plymouth* and *Bristol* do not differ much more one from the other than by the height of a foot, or of fifteen inches; and that the greatest tides of all at those ports are not the first but the third after the syzygies.

And, besides, all the motions are retarded in their passage through shallow channels, so that the greatest tides of all, in some straits and mouths of rivers, are the fourth, or even the fifth, after the syzygies.

[44.] *The motion of the sea may be retarded by obstructions in the bed.*

It may also happen that the greatest tide may be the fourth or fifth after the syzygies, or occur even later, because the motions of the sea are retarded in passing through shallow places towards the shores; for so the tide arrives at the western coast of *Ireland* at the third lunar hour, and an hour or two after at the ports in the southern coast of the same island; as also at the islands *Cassiterides,* commonly called *Sorlings;* then successively at *Falmouth, Plymouth, Portland,* the isle of *Wight, Winchester, Dover,* the mouth of the *Thames,* and *London Bridge,* spending twelve hours in this passage. But further; the propagation of the tides may be obstructed even by the channels of the ocean itself, when they are not of sufficient depth, for the flood happens at the third lunar hour in the *Canary* islands; and at all those western coasts that lie towards the Atlantic ocean, as of *Ireland, France, Spain,* and all *Africa,* to the *Cape of Good Hope,* except in some shallow places, where it is impeded, and takes place later; and in the straits of *Gibraltar,* where, by reason of a motion propagated from the Mediterranean sea, it flows sooner. But, passing from those coasts over the breadth of the ocean to the coasts of *America,* the flood arrives first at the most eastern shores of *Brazil,* about the fourth or fifth lunar hour; then at the mouth of the river of the *Amazons* at the sixth hour, but at the neighboring islands at the fourth hour; afterwards at the islands of *Bermudas* at the seventh hour, and at port St. *Augustin* in *Florida* at seven and a half. And therefore the tide is propagated through the ocean with a slower motion than it should be according to the course of the moon; and this retardation is very necessary, that the sea at the same time may fall between *Brazil* and *New France,* and rise at the *Canary* islands, and on the coasts of *Europe* and *Africa,* and *vice versa:* for the sea cannot rise in one place but by falling in another. And it is probable that the *Pacific* sea is agitated by the same laws; for in the coasts of *Chili* and *Peru* the highest flood is said to happen at the third lunar hour. But with what velocity it is thence propagated to the eastern coasts of *Japan,* the *Philippine* and other islands adjacent to *China,* I have not yet learned.

[45.] *From the obstructions of beds and shores, various phenomena arise, such as that the sea may flow only once every day.*

Further, it may happen (p. 439) that the tide may be propagated from the ocean through different channels towards the same port, and may pass quicker through some channels than through others, in which case the same tide, divided into two or more succeeding one another, may compound new motions of different kinds. Let us suppose one tide to be divided into two equal tides; the former thereof precedes the other by the space of six hours, and happens at the third or twenty-seventh hour from the approach of the moon to the meridian of the port. If the moon at the time of this approach to the meridian were in the equator, every six hours alternately there would arise equal floods, which, meeting with as many equal ebbs, would so balance one the other, that, for that day, the water would stagnate, and remain quiet. If the moon then declined from the equator, the tides in the ocean would be alternately greater and less, as was said; and from hence two greater and two lesser tides would be alternately propagated towards that port. But the two greater floods would make the greatest height of the waters to occur in the middle time between both, and the greater and lesser floods would make the waters to rise to a mean height in the middle time between them; and in the middle time between the two lesser floods the waters would rise to their least height. Thus in the space of twenty-four hours the waters would come, not twice, but once only to their greatest, and once only to their least height; and their greatest height, if the moon declined towards the elevated pole, would happen at the sixth or thirtieth hour after the approach of the moon to the meridian; and when the moon changed its declination, this flood would be changed into an ebb.

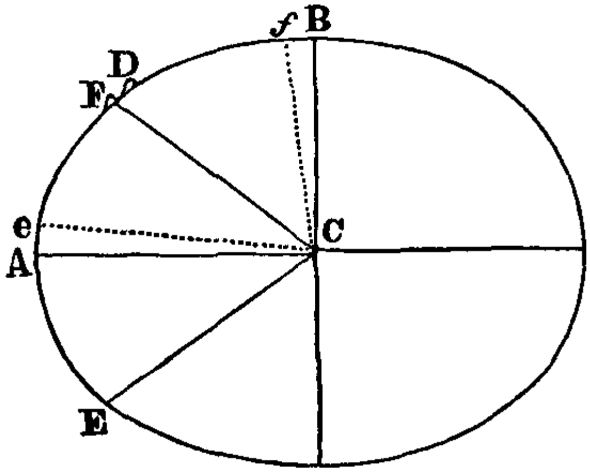
Of all this we have an example in the port of *Batshaw,* in the kingdom of *Tunquin,* in the latitude of 20° 50′ north. In that port, on the day which follows after the passage of the moon over the equator, the waters stagnate; when the moon declines to the north, they begin to flow and ebb, not twice, as in other ports, but once only every day; and the flood happens at the setting, and the greatest ebb at the rising of the moon. This tide increases with the declination of the moon till the seventh or eighth day; then for the seventh or eighth day following it decreases at the same rate as it had increased before, and ceases when the moon changes its declination. After this the flood is immediately changed into an ebb; and thenceforth the ebb happens at the setting and the flood at the rising of the moon, till the moon again changes its declination. There are two inlets from the ocean to this port: one more direct and short between the island *Hainan* and the coast of *Quantung,* a province of *China;* the other round about between the same island and the coast of *Cochim;* and through the shorter passage the tide is propagated more rapidly to *Batshaw.*

[46.] *The times of the tides in channels are more irregular than in the ocean.*

In the channels of rivers the influx and reflux depends upon the current of the rivers, which obstructs the ingress of the waters from the sea, and promotes their egress to the sea, making the ingress later and slower, and the egress sooner and faster; and hence it is that the reflux is of longer duration than the influx, especially far up the rivers, where the force of the sea is less. So *Sturmy* tells us, that in the river *Avon,* three miles below *Bristol,* the water flows only five hours, but ebbs seven; and without doubt the difference is yet greater above *Bristol,* as at *Caresham* or the *Bath.* This difference does likewise depend upon the quantity of the flux and reflux; for the more vehement motion of the sea near the syzygies of the luminaries more easily overcoming the resistance of the rivers, will make the ingress of the water to happen sooner and to continue longer, and will therefore diminish this difference. But while the moon is approaching to the syzygies, the rivers will be more plentifully filled, their currents being obstructed by the greatness of the tides, and therefore will somewhat more retard the reflux of the sea a little after than a little before the syzygies. Upon this account the slowest tides of all will not happen in the syzygies, but precede them a little; and I observed above that the tides before the syzygies are also retarded by the force of the sun; and from both causes conjoined the retardation of the tides will be both greater and sooner before the syzygies. All this I find to be so, by the tide tables which *Flamsteed* has composed from a great many observations.

[47.] *From a greater and deeper ocean arise greater tides, and these are greater at the coasts of continents than at islands in mid-ocean, and greater still in shallow bays which open with wide inlets to the sea.*

By the laws we have been describing, the times of the tides are governed; but the greatness of the tides depends upon the greatness of the seas. Let

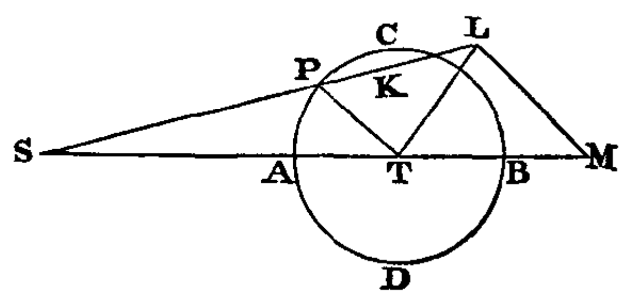


C represent the centre of the earth, EADB the oval figure of the seas, CA the longer semiaxis of this oval, CB the shorter erected at right angles upon the former, D the middle point between A and B, and ECF or *e*C*f* the angle at the centre of the earth, subtended by the breadth of the sea that terminates in the shores E, F, or *e*, *f*. Now, supposing that the point A is in the middle between the points E, F, and the point D in the middle between the points *e*, *f*, if the difference of the heights CA, CB represent the quantity of the tide in a very deep sea surrounding the whole earth, the excess of the height CA above the height CE or CF will represent the quantity of the tide in the middle of the sea EF, terminated by the shores E, F; and the excess of the height C*e* above the height C*f* will nearly represent the quantity of the tide on the shores *f* of the same sea. Hence it appears that the tides are far less in the middle of the sea than at the shores; and that the tides at the shores are nearly as EF (pp. 481–482), the breadth of the sea not exceeding a quadrantal arc. And hence it is that near the equator, where the sea between *Africa* and *America* is narrow, the tides are far less than towards either side in the temperate zones, where the seas are extended wider; or on almost all the shores of the Pacific sea, as well towards *America* as towards *China*, and within as well as without the tropics; and that in islands in the middle of the sea they scarcely rise higher than two or three feet, but on the shores of great continents are three or four times greater, and above, especially if the motions propagated from the ocean are by degrees contracted into a narrow space, and the water, to fill and empty the bays alternately, is forced to flow and ebb with great violence through shallow places; as *Plymouth* and *Chepstow*

Bridge in *England,* at the mount of St. *Michael* and town of *Avranches* in *Normandy,* and at *Cambaia* and *Pegu* in the *East Indies.* In these places, the sea, hurried in and out with great violence, sometimes lays the shores under water, sometimes leaves them dry, for many miles. Nor is the force of the influx and efflux to be broken till it has raised or depressed the water to forty or fifty feet and more. Thus also long and shallow straits that open to the sea with mouths wider and deeper than the rest of their channel (such as those about *Britain* and the *Magellanic Straits* at the eastern entry) will have a greater flood and ebb, or will more increase and remit their course, and therefore will rise higher and be depressed lower. On the coast of *South America* it is said that the *Pacific* sea in its reflux sometimes retreats two miles, and gets out of sight of those that stand on shore. Hence in these places the floods will be also higher; but in deeper waters the velocity of influx and efflux is always less, and therefore the ascent and descent is so too. Nor in such places is the ocean known to ascend to more than six, eight, or ten feet. The quantity of the ascent I compute in the following manner.

[48.] *The force of the sun to disturb the motions of the moon, computed from the foregoing principles.*

Let S represent the sun, T the earth (pp. 440–441), P the moon, PADB the moon's orbit. In SP take SK equal to ST and SL to SK in the squared ratio of SK to SP. Parallel to PT draw LM; and, supposing the mean quantity of the circumsolar force directed towards the earth to be represented by the distance ST or SK, SL will represent the quantity thereof directed towards the moon. But that force is compounded of the parts SM, LM; of which the force LM and that part of SM which is represented by TM, do disturb the motion of the moon (as appears from Prop. XLVI, Book I, and its Corollaries). So far as the earth and moon are revolved about their common centre of gravity, the earth will be liable



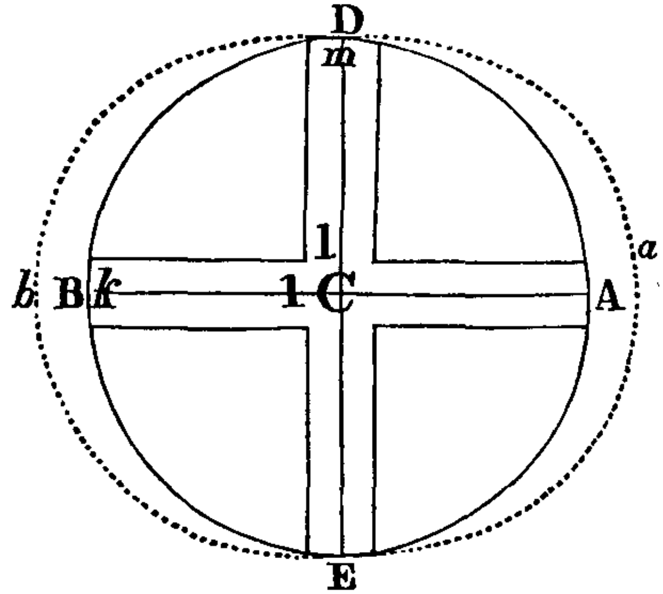
to the action of the like forces. But we may refer the sums, as well of the forces as of the motions, to the moon, and represent the sums of the forces by the lines TM and ML, which are proportional to them. The force LM, in its mean quantity, is to the force by which the moon may be revolved in an orbit, about the earth quiescent, at the distance PT, in the squared ratio of the moon's periodic time about the earth, to the earth's periodic time about the sun (by Cor. XVII, Prop. LXVI, Book I); that is, in the squared ratio of $27^d. 7^h. 43^m.$ to $365^d. 6^h. 9^m.$; or as 1000 to 178725, or 1 to 178 29/40. The force by which the moon may revolve in its orbit about the earth at rest, at the distance PT of 60½ semidiameters of the earth, is to the force by which it may revolve in the same time at the distance of 60 semidiameters as 60½ to 60; and this force is to the force of gravity with us as 1 to 60 · 60, nearly; and therefore the mean force ML is to the force of gravity at the surface of the earth as 1 · 60½ is to 60 · 60 · 178 29/40, or 1 to 638092.6. Hence the force TM will be also given from the ratio of the lines TM, ML. And these are the forces of the sun, by which the moon's motions are disturbed.

[49.] *Computation of the attraction of the sun which moves the sea.*

If from the moon's orbit (p. 478) we descend to the earth's surface, those forces will be diminished in the ratio of the distances 60½ and 1; and therefore the force LM will then become 38604600 times less than the force of gravity. But this force acting equally everywhere upon the earth, will scarcely produce any change on the motion of the sea, and therefore may be neglected in the explanation of that motion. The other force TM, in places where the sun is vertical, or in their nadir, is triple the quantity of the force ML, and therefore but 12868200 times less than the force of gravity.

[50.] *Computation of the height of the tides under the equator due to solar attraction.*

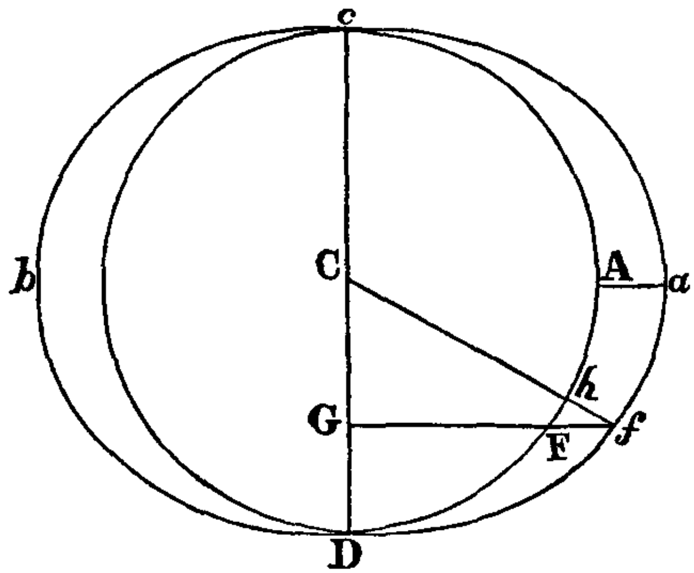
Suppose now ADBE to represent the spherical surface of the earth, *a*D*b*E the surface of the water overspreading it, C the centre of both, A the place to which the sun is vertical, B the place opposite; D, E places at 90 degrees distance from the former; ACE*mlk* a right-angled cylindric canal passing through the earth's centre. The force TM in any place is as the distance of the place from the plane DE, on which a line from A to C stands at right



angles, and therefore in the part of the canal which is represented by EC*lm* is zero, but in the other part AC*lk* is as the gravity at the several heights; for in descending towards the centre of the earth, gravity is (by Prop. LXXIII, Book I) everywhere as the height; and therefore the force TM drawing the water upwards will diminish its gravity in the leg AC*lk* of the canal in a given ratio: upon this account the water will ascend in this leg, till its diminished gravity is compensated by its greater height; nor will it rest in an equilibrium till its total gravity becomes equal to the total gravity in EC*lm*, the other leg of the canal. Since the gravity of every particle is as its distance from the earth's centre, the weight of the whole water in either leg will increase as the square of the height; and therefore the height of the water in the leg AC*lk* will be to the height thereof in the leg C*lm*E as the square root of the ratio of the number 12868201 to 12868200, or in the ratio of the number 25623053 to the number 25623052, and the height of the water in the leg EC*lm* is to the difference of the heights, as 25623052 is to 1. But the height in the leg EC*lm* is of 19615800 *Paris* feet, as has been lately found by the measurement of the *French;* and, therefore, by the preceding proportion, the difference of the heights comes out 9⅕ inches of the *Paris* foot; and the sun's force will make the height of the sea at A to exceed the height of the same at E by 9 inches. And though the water of the canal ACE*mlk* be supposed to be frozen into a hard and solid consistency, yet the heights thereof at A and E, and all other intermediate places, would still remain the same.

[51.] *Computation of the height of the tide at the parallels of latitude, due to solar attraction.*

Let A*a* (in the following figure) represent that excess of height of nine inches at A, and *hf* the excess of height at any other place *h*; and upon DC let fall the perpendicular fG, meeting the globe of the earth in F; and since the distance of the sun is so great that all the right lines drawn thereto may be considered as parallel, the force TM in any place f will be to the same force in the place A as the sine FG to the radius AC. And, therefore, since those forces tend to the sun in the direction of parallel lines, they will generate the parallel heights Ff, Aa, in the same ratio; and therefore the figure of the water $Dfacb$ will be a spheroid made by the revolution of an ellipse about its longer axis ab. And the perpendicular height fh will be to the oblique height Ff as fG to fC, or as FG to AC: and therefore the height fh is to the height Aa in the squared ratio of FG to AC, that is, in the ratio of the versed sine of double the angle DCf to double the radius, and is therefore given. And hence at the different moments of time during the apparent revolution of the sun about the earth we may infer the proportion of the ascent and descent of the waters at any given place under the equator, as well as of the diminution of that ascent and descent, whether arising from the latitude of places or from the sun's declination; viz., that on account of the latitude of places, the ascent and descent of the sea is in all places diminished as the squares of the cosines of latitude; and on account of the sun's declination, the ascent and descent under the equator is diminished as the squares of the cosine of declination. And in places without the equator the half-sum of the morning and evening ascents (that is, the mean ascent) is diminished nearly in the same ratio.



[52.] *The ratio of the tides under the equator in syzygies and quadratures, due to the joint attraction of sun and moon.*

Let S and L respectively represent the forces of the sun and moon placed in the equator, and at their mean distances from the earth; R the radius; T and V the versed sines of double the complements of the sun's and moon's declinations to any given time; D and E the mean apparent diameters of the sun and moon: and, supposing F and G to be their apparent diameters at that given time, their forces to raise the tides under the equator will be, in the syzygies, $\frac{VG^3}{2RE^3}L + \frac{TF^3}{2RD^3}S$; in the quadratures, $\frac{VG^3}{2RE^3}L - \frac{TF^3}{2RD^3}S$. And if the same ratio is likewise observed under the parallels, from observations accurately made in our northern climates we may determine the proportion of the forces L and S; and then by means of this rule predict the quantities of the tides to every syzygy and quadrature.

[53.] *Computation of the lunar attraction causing tides and the height of the water resulting therefrom.*

At the mouth of the river *Avon,* three miles below *Bristol* (pp. 479–482), in spring and autumn, the whole ascent of the water in the conjunction or opposition of the luminaries (by the observation of *Sturmy*) is about 45 feet, but in the quadratures only 25. Because the apparent diameters of the luminaries are not here determined, let us assume them in their mean quantities, as well as the moon's declination in the equinoctial quadratures in its mean quantity, that is, 23½°; and the versed sine of double its complement will be 1682, supposing the radius to be 1000. But the declinations of the sun in the equinoxes and of the moon in the syzygies are zero, and the versed sines of double the complements are each 2000. Hence those forces become $L + S$ in the syzygies, and $\frac{1682}{2000}L - S$ in the quadratures, respectively proportional to the heights of the tides of 45 and 25 feet, or of 9 and 5 paces. And, therefore, multiplying the extremes and the means, we have $5L + 5S = \frac{15138}{2000}L - 9S$, or $L = \frac{28000}{5138}S = 5\frac{5}{11}S$.

But further, I remember to have been told that in summer the ascent of the sea in the syzygies is to the ascent thereof in the quadratures as about 5 to 4. In the solstices themselves it is probable that the proportion may be somewhat less, as about 6 to 5; hence it would follow that L is $= 5\frac{1}{6}S$ [for then the proportion is $\frac{1682}{2000}L + \frac{1682}{2000}S : L - \frac{1682}{2000}S = 6 : 5$]. Till we can more certainly determine the proportion from observation let us assume $L = 5\frac{1}{3}S$;

and since the heights of the tides are as the forces which excite them, and the force of the sun is able to raise the tides to the height of nine inches, the moon's force will be sufficient to raise the same to the height of four feet. And if we allow that this height may be doubled, or perhaps tripled, by that force of reciprocation which we observe in the motion of the waters, and by which their motion once begun is kept up for some time, there will be force enough to generate all that quantity of tides which we really find in the ocean.

[54.] *These forces of the sun and moon can hardly be perceived, except by the tides which they raise in the sea.*

Thus we have seen that these forces are sufficient to move the sea. But, so far as I can observe, they will not be able to produce any other effect sensible on our earth;[1] for since the weight of one grain in 4000 is not sensible in the nicest balance; and the sun's force to move the tides is 12868200 less than the force of gravity; and the sum of the forces of both moon and sun, exceeding the sun's force only in the ratio of 6⅓ to 1, is still 2032890 times less than the force of gravity; it is evident that both forces together are 500 times less than what is required perceptibly to increase or diminish the weight of any body in a balance. And, therefore, they will not sensibly move any suspended body; nor will they produce any sensible effect on pendulums, barometers, bodies swimming in stagnant water, or in the like statical experiments. In the atmosphere, indeed, they will excite such a flux and reflux as they do in the sea, but with so small a motion that no sensible wind will be thence produced.

[55.] *The moon is about six times denser than the sun.*

If the effects of both moon and sun in raising the tides (p. 484), as well as their apparent diameters, were equal among themselves, their absolute forces would (by Cor. XIV, Prop. LXVI, Book I) be as their magnitudes. But the effect of the moon is to the effect of the sun as about 5⅓ is to 1; and the moon's diameter is less than the sun's in the ratio of 31½ to 32⅕, or of 45 to 46. Now the force of the moon is to be increased directly as the ratio of the effect, and inversely as the cube of the ratio of the diameter. Hence the force of the moon compared with its magnitude will be to the force of the

[[1] Appendix, Note 59.]

sun compared with its magnitude in the ratio compounded of $5\frac{1}{3}$ to 1, and inversely as the cube of the ratio of 45 to 46, that is, in the ratio of about $5\frac{7}{10}$ to 1. And therefore the moon, in respect of the magnitude of its body, has an absolute centripetal force greater than the sun in respect of the magnitude of its body in the ratio of $5\frac{7}{10}$ to 1, and is therefore more dense in the same ratio.

[56.] *The moon is denser than our earth in the ratio of about 3 to 2.*

In the time of $27^d.\ 7^h.\ 43^m.$, in which the moon makes its revolution about the earth, a planet may be revolved about the sun at the distance of 18.954 diameters of the sun from the sun's centre, supposing the mean apparent diameter of the sun to be $32\frac{1}{5}'$; and in the same time the moon may be revolved about the earth at rest, at the distance of 30 of the earth's diameters. If in both cases the number of diameters was the same, the absolute circumterrestrial force would (by Cor. II, Prop. LXXII, Book I) be to the absolute circumsolar force as the magnitude of the earth to the magnitude of the sun. Because the number of the earth's diameters is greater in the ratio of 30 to 18.954, the body of the earth will be less in that ratio cubed, that is, in the ratio of $3\frac{28}{29}$ to 1. Therefore the earth's force, for the magnitude of its body, is to the sun's force, for the magnitude of its body, as $3\frac{28}{29}$ to 1; and consequently the earth's density to the sun's will be in the same ratio. Since, then, the moon's density is to the sun's density as $5\frac{7}{10}$ to 1, the former will be to the earth's density as $5\frac{7}{10}$ to $3\frac{28}{29}$, or as 23 to 16. Therefore, since the moon's magnitude is to the earth's magnitude as about 1 to $41\frac{1}{2}$, the moon's absolute centripetal force will be to the earth's absolute centripetal force, as about 1 to 29, and the quantity of matter in the moon is to the quantity of matter in the earth in the same ratio. And hence the common centre of gravity of the earth and moon is more exactly determined than hitherto it has been; from the knowledge of this we may now infer the moon's distance from the earth with greater accuracy. But I would rather wait till the ratio of the bodies of the moon and earth one to the other is more exactly defined from the phenomena of the tides, hoping that in the meantime the circumference of the earth may be measured from more distant stations than anybody has yet employed for this purpose.

[57.] *On the distance of the stars.*

Thus I have given an account of the system of the planets. As to the fixed stars, the smallness of their annual parallax proves them to be removed to immense distances from the system of the planets: that this parallax is less than one minute is most certain; and from this it follows that the distance of the fixed stars is above 360 times greater than the distance of Saturn from the sun. Those who consider the earth one of the planets, and the sun one of the fixed stars, may remove the fixed stars to yet greater distances by the following arguments. From the annual motion of the earth there would happen an apparent transposition of the fixed stars, one in respect of another, almost equal to their double parallax; but the greater and nearer stars, in respect of the more remote, which are only seen by the telescope, have not hitherto been observed to have the least motion. If we should suppose that motion to be only less than 20″, the distance of the nearer fixed stars would exceed the mean distance of Saturn by above 2000 times. Again, the disk of Saturn, which is only 17″ or 18″ in diameter, receives only about $\frac{1}{2100000000}$ of the sun's light; for so much less is that disk than the whole spherical surface of the orb of Saturn. Now if we suppose Saturn to reflect about ¼ of this light, the whole light reflected from its illuminated hemisphere will be about $\frac{1}{4200000000}$ of the whole light emitted from the sun's hemisphere; and, therefore, since light is rarefied inversely as the square of the distance from the luminous body, if the sun was $10000\sqrt{42}$ times more distant than Saturn, it would yet appear as lucid as Saturn now does without its ring, that is, somewhat more lucid than a fixed star of the first magnitude. Let us, therefore, suppose that the distance from which the sun would shine as a fixed star exceeds that of Saturn by about 100000 times, and its apparent diameter will be 7^{v} 16^{vi} and its parallax arising from the annual motion of the earth 13^{iv}: and so great will be the distance, the apparent diameter, and the parallax of the fixed stars of the first magnitude, in bulk and light equal to our sun. Some may, perhaps, imagine that a great part of the light of the fixed stars is intercepted and lost in its passage through so vast spaces, and upon that account pretend to place the fixed stars at nearer distances; but at this rate the remoter stars could be scarcely seen. Suppose, for example, that ¾ of the light is lost in its passage from the nearest fixed stars to us; then ¾ will be lost twice in its passage through a double space, thrice through a triple, and so forth. And, therefore, the fixed stars that are at a double distance will be 16 times more obscure, viz., 4 times more obscure on account of the diminished apparent diameter; and, again, 4 times more on account of the lost light. And, by the same argument, the fixed stars at a triple distance will be 9 · 4 · 4, or 144 times more obscure; and those at a quadruple distance will be 16 · 4 · 4 · 4, or 1024 times more obscure; but so great a diminution of light is no ways consistent with the phenomena and with that hypothesis which places the fixed stars at different distances.

[58.] *That comets, when they come into view, are nearer than Jupiter is shown from their parallax in longitude.*

The fixed stars being, therefore, at such vast distances from one another (pp. 491–493), can neither attract each other perceptibly, nor be attracted by our sun. But the comets must unavoidably be acted on by the circumsolar force; for as the comets were placed by astronomers beyond the moon, because they were found to have no diurnal parallax, so their annual parallax is a convincing proof of their descending into the regions of the planets. For all the comets which move in a direct course, according to the order of the signs, about the end of their appearance become more than ordinarily slow, or retrograde, if the earth is between them and the sun; and more than ordinarily swift if the earth is approaching to a heliocentric opposition with them. Whereas, on the other hand, those comets which move against the order of the signs, towards the end of their appearance, appear swifter than they ought to be, if the earth is between them and the sun; and slower, and perhaps retrograde, if the earth is in the other side of its orbit. This is occasioned by the motion of the earth in different situations. If the earth go the same way with the comet, with a swifter motion, the comet becomes retrograde; if with a slower motion, the comet becomes slower, however; and if the earth move the contrary direction, it becomes swifter; and by ascertaining the differences between the slower and swifter motions, and the sums of the more swift and retrograde motions, and comparing them with the situation and motion of the earth from whence they arise, I found, by means of this parallax, that the distances of the comets at the time they cease to be visible to the naked eye are always less than the distance of Saturn, and generally even less than the distance of Jupiter.

[59.] *This is shown by the parallax in latitude.*

The same thing may be inferred from the curvature of the path of the comets (p. 493). These bodies go on nearly in great circles while their motion continues swift; but about the end of their course, when that part of their apparent motion which arises from the parallax bears a greater proportion to their whole apparent motion, they commonly deviate from those circles; and when the earth goes to one side, they deviate to the other; and this deflection, since it corresponds with the motion of the earth, must arise chiefly from the parallax; and the quantity thereof is so considerable, as, by my computation, to place the disappearing comets a good deal closer than Jupiter. Hence it follows, that, when they approach nearer to us in their perigees and perihelions, they often are within the orbits of Mars and the inferior planets.

[60.] *This is shown otherwise by the parallax.*

Moreover, this nearness of the comets is confirmed by the annual parallax of the orbit, so far as the same is obtained pretty nearly by the supposition that the comets move uniformly in right lines. The method of computing the distance of a comet according to this hypothesis from four observations (first attempted by *Kepler,* and perfected by Dr. *Wallis* and Sir *Christopher Wren*) is well known; and the comets reduced to this regularity generally pass through the middle of the planetary region. So the comets of the years 1607 and 1618, as their motions are defined by *Kepler,* passed between the sun and the earth; that of the year 1664 within the orbit of Mars; and that in 1680 within the orbit of Mercury, as its motion was defined by Sir *Christopher Wren* and others. By a like rectilinear hypothesis, *Hewelcke* placed all the comets about which we have any observations within the orbit of Jupiter. It is a false notion, therefore, and contrary to astronomical calculation, which some have entertained who, from the regular motion of the comets, either remove them into the regions of the fixed stars, or deny the motion of the earth; whereas their motions cannot be reduced to perfect regularity, unless we suppose them to pass through the regions near the earth. And these are the arguments drawn from the parallax, so far as it can be determined without an exact knowledge of the orbits and motions of the comets.

[61.] *From the light of their heads it is shown that comets descend as far as the orbit of Saturn.*

The near approach of the comets is further confirmed from the light of their heads (pp. 493–497); for the light of a celestial body, illuminated by the sun, and receding to remote parts, is diminished inversely as the fourth power of the distance; namely, as the second power, on account of the increase of the distance from the sun; and as another second power, on account of the decrease of the apparent diameter. Hence it may be inferred, that Saturn being at a double distance, and having its apparent diameter nearly half that of Jupiter, must appear about 16 times more obscure; and that, if its distance were 4 times greater, its light would be 256 times less; and therefore would be hardly perceivable to the naked eye. But now the comets often equal Saturn's light, without exceeding him in their apparent diameters. So the comet of the year 1668, according to Dr. *Hooke's* observations, equaled in brightness the light of a fixed star of the first magnitude; and its head, or the star in the middle of the coma, appeared, through a 15-foot telescope, as lucid as Saturn near the horizon; but the diameter of the head was only 25″; that is, almost the same with the diameter of a circle equal to Saturn and his ring. The coma or hair surrounding the head was about ten times as broad; namely, $4\frac{1}{6}'$. Again, the least diameter of the hair of the comet of the year 1682, observed by Mr. *Flamsteed* with a 16-foot tube, and measured with the micrometer, was 2′ 0″; but the nucleus, or star in the middle, scarcely possessed the tenth part of this breadth, and was therefore only 11″ or 12″ broad; but the light and clearness of its head exceeded that of the year 1680, and was equal to that of the stars of the first or second magnitude. Moreover, the comet of the year 1665, in *April*, as *Hewelcke* informs us, exceeded almost all the fixed stars in splendor, and even Saturn itself, as being of a much more vivid color; for this comet was more lucid than that which appeared at the end of the foregoing year, and was compared to the stars of the first magnitude. The diameter of the coma was about 6′; but the nucleus, compared with the planets by means of a telescope, was plainly less than Jupiter, and was sometimes thought less, sometimes equal to the body of Saturn within the ring. To this breadth add that of the ring, and the whole face of Saturn will be twice as great as that of the comet, with a light not at all more intense; and therefore the comet was nearer to the sun than Saturn. From the ratio of the nucleus to the whole head found by these observations, and from its breadth, which seldom exceeds 8′ or 12′, it appears that the stars of the comets are most commonly of the same apparent magnitude as the planets; but that their light may be compared oftentimes with that of Saturn, and sometimes exceeds it. And hence it is certain that in their perihelions their distances can scarcely be greater than that of Saturn. At twice that distance, the light would be four times less, and, on account of its dim paleness, would be as much inferior to the light of Saturn as the light of Saturn is to the splendor of Jupiter: this difference would be easily observed. At a distance ten times greater, their bodies must be greater than that of the sun; but their light would be 100 times fainter than that of Saturn. And at distances still greater, their bodies would far exceed the sun; but, being in such dark regions, they could be no longer visible. It is certainly impossible to place the comets in the middle regions between the sun and fixed stars, accounting the sun as one of the fixed stars; for certainly they would receive no more light there from the sun than we do from the greatest of the fixed stars.

[62.] *They descend far below the orbit of Jupiter and sometimes below the orbit of the earth.*

So far we have gone without considering the obscuration which comets suffer from that plenty of thick smoke which encompasses their heads, and through which the heads always show dull as through a cloud; for the more a body is obscured by this smoke, the nearer must it necessarily approach the sun, that it may vie with the planets in the quantity of light which it reflects; hence it is probable that the comets descend far below the orbit of Saturn, as we proved before from their parallax. But, above all, the thing is confirmed from their tails, which must be due either to the sun's light reflected from a smoke arising from them, and dispersing itself through the ether, or to the light of their own heads.

In the former case we must shorten the distance of the comets, lest we be obliged to allow that the smoke arising from their heads is propagated through such a vast extent of space, and with such a velocity of expansion, as will seem altogether incredible; in the latter case the whole light of both head and tail must be ascribed to the central nucleus. But, then, if we suppose all this light to be united and condensed within the disk of the nucleus, certainly the nucleus will by far exceed Jupiter itself in splendor, especially when it emits a very large and lucid tail. If, therefore, under a less apparent diameter, it reflects more light, it must be much more illuminated by the sun, and therefore much nearer to it. So the comet that appeared *December* 12 and 15, o.s., 1679, at the time it emitted a very shining tail, whose splendor was equal to that of many stars like Jupiter, if their light were dilated and spread through so great a space, was, as to the magnitude of its nucleus, less than Jupiter (as Mr. *Flamsteed* observed), and therefore was much nearer to the sun: nay, it was even less than Mercury. For on the 17th of that month, when it was nearer to the earth, it appeared to *Cassini* through a 35-foot telescope a little less than the globe of Saturn. On the 8th of this month, in the morning, Dr. *Halley* saw the tail, appearing broad and very short, and as if it rose from the body of the sun itself, at that time very near its rising. Its form was like that of an extraordinary bright cloud; nor did it disappear till the sun itself began to be seen above the horizon. Its splendor, therefore, exceeded the light of the clouds till the sun rose, and far surpassed that of all the stars together, as yielding only to the immediate brightness of the sun itself. Neither Mercury, nor Venus, nor the moon itself, are seen so near the rising sun. Imagine all this dilated light collected together, and crowded into the orbit of the comet's nucleus which was less than Mercury; by its splendor, thus increased, becoming so much more conspicuous, it will vastly exceed Mercury, and therefore must be nearer to the sun. On the 12th and 15th of the same month, this tail, extending itself over a much greater space, appeared rarer; but its light was still so vigorous as to become visible when the fixed stars were hardly to be seen, and soon after to appear like a fiery beam shining in a wonderful manner. From its length, which was 40 or 50 degrees, and its breadth of 2 degrees, we may compute what the light of the whole must be.

[63.] *This is confirmed from the signal splendor of their tails in the vicinity of the sun.*

This near approach of the comets to the sun is confirmed from the situation they are seen in when their tails appear most resplendent; for when the head passes by the sun, and lies hid under the solar rays, very bright and shining tails, like fiery beams, are said to issue from the horizon; but afterwards, when the head begins to appear, and is got farther from the sun, that splendor always decreases, and turns by degrees into a paleness like to that of the Milky Way, but much brighter at first; after that vanishing gradually. Such was that resplendent comet described by *Aristotle,* Book I, *Meteor.* 6. "The head thereof could not be seen, because it set before the sun, or at least was hid under the sun's rays; but the next day it was barely visible; for, having left the sun but a very little way, it set immediately after it; and the scattered light of the head obscured by the too great splendor [of the tail] did not yet appear. But afterwards [says *Aristotle*], when the splendor of the tail was now diminished, [the head of] the comet recovered its native brightness. And the splendor of its tail reached now to a third part of the heavens [that is, to 60°]. It appeared in the winter season, and, rising to Orion's girdle, there vanished away." Two comets of the same kind are described by *Justin,* Book XXXVII, which, according to him, "shone so bright, that the whole heaven seemed to be on fire; and by their greatness filled up a fourth part of the heavens, and by their splendor exceeded that of the sun." By these last words is intimated a near position of these bright comets to each other and to the rising or setting sun. We may add to these the comet of the year 1101 or 1106, "the star of which was small and obscure [like that of 1680]; but the splendor arising from it was extremely bright, reaching like a fiery beam to the east and north," as *Hewelcke* has it from *Simeon,* the monk of *Durham.* It appeared at the beginning of *February* about the evening in the southwest. From this and from the situation of the tail we may infer that the head was near the sun. *Matthew Paris* says, "it was about one cubit from the sun; from the third [or rather the sixth] to the ninth hour sending out a long stream of light." The comet of 1264, in *July,* or about the solstice, preceded the rising sun, sending out its beams with a great light towards the west as far as the middle of the heavens; and at the beginning it ascended a little above the horizon: but as the sun went forwards it retired every day farther from the horizon, till it passed by the very middle of the heavens. It is said to have been at the beginning large and bright, having a large coma, which decayed from day to day. It is described in *Append. Matth. Paris, Hist. Ang.* after this manner: "*An. Christi*

1265, there appeared a comet so wonderful, that none then living had ever seen the like; for, rising from the east with a great brightness, it extended itself with a great light as far as the middle of the hemisphere towards the west." The Latin original being somewhat barbarous and obscure, it is here subjoined. *Ab oriente enim cum magno fulgore surgens, usque ad medium hemisphaerii versus occidentem, omnia perlucide pertrahebat.*

"In the year 1401 or 1402, the sun being got below the horizon, there appeared in the west a bright and shining comet, sending out a tail upwards, in splendor like a flame of fire, and in form like a spear, darting its rays from west to east. When the sun was sunk below the horizon, by the lustre of its own rays it enlightened all the borders of the earth, not permitting the other stars to show their light, or the shades of night to darken the air, because its light exceeded that of the others, and extended itself to the upper part of the heavens, flaming," &c., *Hist. Byzant. Duc. Mich. Nepot.* From the situation of the tail of this comet, and the time of its first appearance, we may infer that the head was then near the sun, and went farther from him every day; for that comet continued for three months. In the year 1527, *August* 11, about 4 in the morning, there was seen almost throughout *Europe* a terrible comet in Leo, which continued flaming an hour and a quarter every day. It rose from the east, and ascended to the south and west to a prodigious length. It was most conspicuous to the north, and its cloud (that is, its tail) was very terrible; having, according to the fancies of the vulgar, the form of an arm a little bent holding a sword of a vast magnitude. In the year 1618, in the end of *November,* there began a rumor, that there appeared about sunrise a bright beam, which was the tail of a comet whose head was yet concealed within the brightness of the solar rays. On *November* 24, and from that time, the comet appeared with a bright light, its head and tail being extremely resplendent. The length of the tail, which was at first 20 or 30 deg., increased till *December* 9, when it arose to 75 deg., but with a light much fainter and more dilute than at the beginning. In the year 1668, *March* 5, N.S., about 7 in the evening, *Valentin Estancel,* being in *Brazil,* saw a comet near the horizon in the southwest. Its head was small, and scarcely discernible, but its tail extremely bright and refulgent, so that the reflection of it from the sea was easily seen by those who stood upon the shore. This great splendor lasted but three days, decreasing very remarkably from that time. The tail at the beginning extended itself from west to south, and in a situation almost parallel to the horizon, appearing like a shining beam 23 deg. in length. Afterwards, the light decreasing, its magnitude increased till the comet ceased to be visible; so that *Cassini,* at *Bologna,* saw it (*March* 10, 11, 12) rising from the horizon 32 deg. in length. In *Portugal* it is said to have taken up a fourth part of the heavens (that is, 45 deg.), extending itself from west to east with a notable brightness; though the whole of it was not seen, because the head in this part of the world always lay hid below the horizon. From the increase of the tail it is plain that the head receded from the sun, and was nearest to it at the beginning, when the tail appeared brightest.

To all these we may add the comet of 1680, whose wonderful splendor at the conjunction of the head with the sun was above described. But so great a splendor argues the comets of this kind to have really passed near the fountain of light, especially since the tails never shine so much in their opposition to the sun; nor do we read that fiery beams have ever appeared there.

[64.] *From the light of the heads it appears, under otherwise the same conditions, how much greater this light is in the vicinity of the sun.*

Lastly, the same thing is inferred (pp. 495–497) from the light of the heads increasing in the recess of the comets from the earth towards the sun, and decreasing in their return from the sun towards the earth; for so the last comet of the year 1665 (by the observation of *Hewelcke*), from the time that it was first seen, was always losing of its apparent motion, and therefore had already passed its perigee; yet the splendor of its head was daily increasing, till, being hid by the sun's rays, the comet ceased to appear. The comet of the year 1683 (by the observation of the same *Hewelcke*), about the end of *July,* when it first appeared, moved at a very slow rate, advancing only about 40 or 45 minutes in its orbit in a day's time. But from that time its diurnal motion was continually upon the increase till *September* 4, when it arose to about 5 degrees; and therefore in all this interval of time the comet was approaching to the earth. Which is likewise proved from the diameter of its head measured with a micrometer; for, *August* 6, *Hewelcke* found it only 6′ 5″, including the coma; which, *September* 2, he observed to be 9′ 7″. And therefore its head appeared far less about the beginning than towards the end of its motion, though about the beginning, because nearer to the sun, it appeared far more lucid than towards the end, as the same *Hewelcke* declares. Therefore in all this interval of time, on account of its recess from the sun, it decreased in splendor, notwithstanding its access towards the earth. The comet of the year 1618, about the middle of *December,* and that of the year 1680, about the end of the same month, did both move with their greatest velocity, and were therefore then in their perigees; but the greatest splendor of their heads was seen two weeks before, when they had just got clear of the sun's rays; and the greatest splendor of their tails a little more early, when yet nearer to the sun. The head of the former comet, according to the observations of *Cysat, December* 1, appeared greater than the stars of the first magnitude; and, *December* 16 (being then in its perigee), of a small magnitude, and the splendor or clearness was much diminished. *January* 7, *Kepler,* being uncertain about the head, left off observing. *December* 12, the head of the last comet was seen and observed by *Flamsteed* at the distance of 9 degrees from the sun, which a star of the third magnitude could hardly have been. *December* 15 and 17, the same appeared like a star of the third magnitude, its splendor being diminished by the bright clouds near the setting sun. *December* 26, when it moved with the greatest swiftness, and was almost in its perigee, it was inferior to Os Pegasi, a star of the third magnitude. *January* 3, it appeared like a star of the fourth; *January* 9, like a star of the fifth. *January* 13, it disappeared, by reason of the brightness of the moon, which was then in its increase. *January* 25, it was scarcely equal to the stars of the seventh magnitude. If we take equal times on each hand of the perigee, the heads placed at remote distances would have shone equally before and after, because of their equal distances from the earth. That in one case they shone very bright, and in the other vanished, is to be ascribed to the nearness of the sun in the first case, and his distance in the other; and from the great difference of the light in these two cases we infer its great nearness in the first of them; for the light of the comets tends to be regular, and to appear greatest when their heads move the swiftest, and are therefore in their perigees, except so far as it is increased by their nearness to the sun.

[65.] *The same is confirmed by the large number of comets seen in the region of the sun.*

From these things I at last discovered why the comets frequent so much the region of the sun. If they were to be seen in the regions a great way beyond Saturn, they must appear oftener in those parts of the heavens that are opposite to the sun; for those which are in that situation would be nearer to the earth, and the interposition of the sun would obscure the others: but, looking over the history of comets, I find that four or five times more have been seen in the hemisphere towards the sun than in the opposite hemisphere; besides, without doubt, not a few which have been hid by the light of the sun; for comets descending into our parts neither emit tails, nor are so well illuminated by the sun, as to discover themselves to our naked eyes, till they are nearer to us than Jupiter. But the far greater part of that spherical space, which is described about the sun with so small a radius, lies on that side of the earth which regards the sun, and the comets in that greater part are more strongly illuminated, being for the most part nearer to the sun: besides, from the remarkable eccentricity of their orbits, it comes to pass that their lower apsides are much nearer to the sun than if their revolutions were performed in circles concentric to the sun.

[66.] *This is confirmed also by the greater magnitude and splendor of the tails after the conjunction of the heads with the sun than before.*

Hence also we understand why the tails of the comets, while their heads are descending towards the sun, always appear short and rare, and are seldom said to have exceeded 15 or 20 deg. in length; but in the recess of the heads from the sun often shine like fiery beams, and soon after reach to 40, 50, 60, 70 deg. in length, or more. This great splendor and length of the tails arises from the heat which the sun communicates to the comet as it passes near it. And thence, I think, it may be concluded, that all the comets that have had such tails have passed very near the sun.

[67.] *The tails arise from the atmospheres of the comets.*

We may conclude from the preceding results that the tails arise from the atmospheres of the heads (pp. 522–525): but we have had three several opinions about the tails of comets; for some will have it that they are nothing else but the beams of the sun's light transmitted through the comets' heads, which they suppose to be transparent; others, that they proceed from the refraction which light suffers in passing from the comet's head to the earth; and, lastly, others, that they are a sort of cloud or vapor constantly rising from the comets' heads, and tending towards the parts opposite to the sun. The first is the opinion of such as are yet unacquainted with optics; for the beams of the sun are not seen in a darkened room, but in consequence of the light that is reflected from them by the little particles of dust and smoke which are always flying about in the air; and hence it is that in air impregnated with thick smoke they appear with greater brightness, and are more faintly and more difficultly seen in a finer air; but in the heavens, where there is no matter to reflect the light, they are not to be seen at all. Light is not seen as it is in the beams, but as it is thence reflected to our eyes; for one cannot see, except by rays falling upon the eyes, and therefore there must be some reflecting matter in those parts where the tails of comets are seen; and so the argument turns upon the third opinion; for that reflecting matter can be nowhere found but in the place of the tail, because otherwise, since all the celestial spaces are equally illuminated by the sun's light, no part of the heavens could appear with more splendor than another. The second opinion is liable to many difficulties. The tails of comets are never seen variegated with those colors which ever tend to be inseparable from refraction; and the distinct transmission of the light of the fixed stars and planets to us is a demonstration that the ether or celestial medium is not endowed with any refractive power. For as to what is alleged that the fixed stars have been sometimes seen by the *Egyptians* environed with a coma because that has but rarely happened, it is rather to be ascribed to a casual refraction of clouds, as well as the radiation and scintillation of the fixed stars to the refractions both of the eyes and air; for upon applying a telescope to the eye, those radiations and scintillations immediately disappear. By the tremulous agitation of the air and ascending vapors, it happens that the rays of light are alternately turned aside from the narrow space of the pupil of the eye; but no such thing can have place in the much wider aperture of the object glass of a telescope; and hence it is that a scintillation is occasioned in the former case which ceases in the latter; and this cessation in the latter case is a demonstration of the regular transmission of light through the heavens without any perceptible refraction. But, to obviate an objection that may be made from the appearing of no tail in such comets as shine but with a faint light, as if the secondary rays were then too weak to affect the eyes, and for this reason it is that the tails of the fixed stars do not appear, we are to consider that by the means of telescopes the light of the fixed stars may be augmented above an hundredfold and yet no tails are seen; that the light of the planets is yet more copious without any tail, but that comets are seen sometimes with huge tails when the light of their heads is but faint and dull; for so it happened in the comet of the year 1680, when in the month of *December* it was scarcely equal in light to the stars of the second magnitude, and yet emitted a notable tail, extending to the length of 40, 50, 60, or 70 degrees, and more; and afterwards, on the 27th and 28th of *January,* the head appeared only as a star of the seventh magnitude; but the tail (as was said above), with a light that was clearly perceptible, though faint, was stretched out to 6 or 7 degrees in length, and with a languishing light that was more difficult to see, even to 12 degrees and more. But on *February* 9th and 10th, when to the naked eye the head appeared no more, I saw through a telescope the tail of 2 degrees in length. But further, if the tail was due to the refraction of the celestial matter, and did deviate from the opposition of the sun, as the figure of the heavens requires, that deviation, in the same places of the heavens, should be always directed towards the same parts: but the comet of the year 1680, *December* 28^d. $8\frac{1}{2}^h$. P.M. at *London,* was seen in Pisces, 8° 41′, with latitude north 28° 6′, while the sun was in Capricorn 18° 26′. And the comet of the year 1577, *December* 29, was in Pisces 8° 41′, with latitude north 28° 40′; and the sun, as before, in about Capricorn 18° 26′. In both cases the situation of the earth was the same, and the comet appeared in the same place of the heavens; yet in the former case the tail of the comet (as well by my observations as by the observations of others) deviated from the opposition of the sun towards the north by an angle of 4½ degrees, whereas in the latter there was (according to the observation of *Tycho*) a deviation of 21 degrees towards the south. The refraction, therefore, of the heavens being thus disproved, it remains that the phenomena of the tails of comets must be explained by some reflecting matter. That vapors sufficient to fill such immense spaces may arise from the comets' atmospheres, may be easily understood from what follows.

[68.] *The air and vapors are extremely rare in celestial spaces, and a very small amount of vapor may be sufficient to explain all the phenomena of the tails of comets.*

It is well known that the air near the surface of our earth possesses a space about 1200 times greater than water of the same weight; and therefore a cylindric column of air 1200 feet high is of equal weight with a cylinder of water of the same breadth, and but one foot high. But a cylinder of air reaching to the top of the atmosphere is of equal weight with a cylinder of water about 33 feet high; and therefore if from the whole cylinder of air the lower part of 1200 feet high is taken away, the remaining upper part will be of equal weight with a cylinder of water 32 feet high. Therefore, at the height of 1200 feet, or two furlongs, the weight of the incumbent air is less, and consequently the rarity of the compressed air greater, than near the surface of the earth in the ratio of 33 to 32. And, having this ratio, we may compute the rarity of the air in all places whatsoever (by the help of Cor., Prop. XXII, Book II), supposing the expansion thereof to be inversely proportional to its compression; and this proportion has been proved by the experiments of *Hooke* and others. The result of the computation I have set down in the following table, in the first column of which you have the height of the air in miles, whereof 4000 make a semidiameter of the

AIR'S

Height	Compression	Expansion
0	33	1
5	17.8515	1.8486
10	9.6717	3.4151
20	2.852	11.571
40	0.2525	136.83
400	0.xvii 1224	26956 xv
4000	0.cv 4465	73907 cii
40000	0.cxcii 1628	20263 clxxxix
400000	0.ccx 7895	41798 ccvii
4000000	0.ccxii 9878	33414 ccix
Infinite	0.ccxii 6041	54622 ccix

earth; in the second the compression of the air, or the incumbent weight; in the third its rarity or expansion, supposing gravity to decrease inversely as the square of the distances from the earth's centre. And the Latin numeral characters are here used for certain numbers of ciphers, as 0.xvii 1224 for 0.000000000000000001224, and 26956 xv for 26956000000000000000.

But from this table it appears that the air, in proceeding upwards, is rarefied in such manner, that a sphere of that air which is nearest to the earth, of but one inch in diameter, if dilated with that rarefaction which it would have at the height of one semidiameter of the earth, would fill all the planetary regions as far as the sphere of Saturn, and a great way beyond; and at the height of ten semidiameters of the earth would fill up more space than is contained in the whole heavens on this side the fixed stars, according to the preceding computation of their distance. And though, by reason of the far greater thickness of the atmospheres of comets, and the great quantity of the circumsolar centripetal force, it may happen that the air in the celestial spaces, and in the tails of comets, is not so vastly rarefied, yet from this computation it is plain that a very small quantity of air and vapor is abundantly sufficient to produce all the appearances of the tails of comets; for that they are indeed of a very notable rarity appears from the shining of the stars through them. The atmosphere of the earth, illuminated by the sun's light, though but of a few miles in thickness, obscures and extinguishes the light not only of all the stars, but even of the moon itself; whereas the smallest stars are seen to shine through the immense thickness of the tails of comets, likewise illuminated by the sun, without the least diminution of their splendor.

[69.] *In what manner the tails may arise from the atmospheres of their heads.*

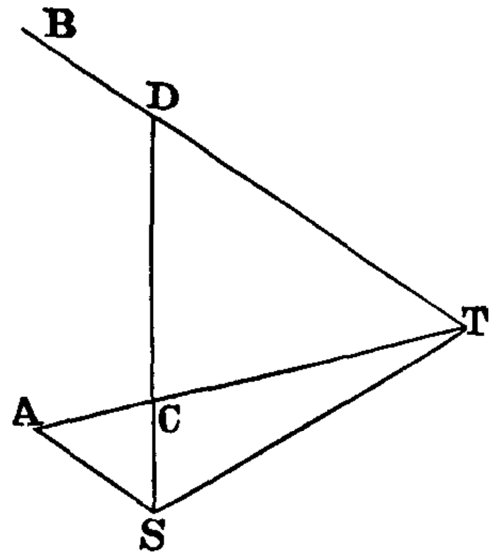
Kepler ascribes the ascent of the tails of comets to the atmospheres of their heads, and their direction towards the parts opposite to the sun, to the action of the rays of light carrying along with them the matter of the comets' tails; and without any great incongruity we may suppose that, in so free spaces, so fine a matter as that of the ether may yield to the action of the rays of the sun's light, though those rays are not able sensibly to move the gross substances in our parts, which are clogged with so palpable a resistance. Another author thinks that there may be a sort of particles of matter endowed with a principle of levity, just as others are with a power of gravity; that the matter of the tails of comets may be of the former sort, and that its ascent from the sun may be due to its levity; but, considering that the gravity of terrestrial bodies varies as the matter of the bodies, and therefore can be neither more nor less in the same quantity of matter, I am inclined to believe that this ascent may rather proceed from the rarefaction of the matter of the comets' tails. The ascent of smoke in a chimney is due to the impulse of the air with which it is entangled. The air rarefied by heat ascends, because its specific gravity is diminished, and in its ascent carries along with it the smoke with which it is engaged. And why may not the tail of a comet rise from the sun after the same manner? For the sun's rays do not act any way upon the mediums which they pervade but by reflection and refraction; and those reflecting particles heated by this action, heat the matter of the ether which is involved with them. That matter is rarefied by the heat which it acquires, and because by this rarefaction the specific gravity, with which it tended towards the sun before, is diminished, it will ascend therefrom like a stream, and carry along with it the reflecting particles of which the tail of the comet is composed; the impulse of the sun's light, as we have said, promoting the ascent.

[70.] *That the tails arise from these atmospheres is evident from their divers appearances.*

But that the tails of comets do arise from their heads (p. 524), and tend towards the parts opposite to the sun, is further confirmed from the laws which the tails observe; for, lying in the planes of the comets' orbits which pass through the sun, they constantly deviate from the opposition of the sun towards the parts which the comets' heads in their progress along those orbits have left; and to a spectator placed in those planes they appear in the parts directly opposite to the sun; but as the spectator recedes from those planes, their deviation begins to appear, and daily becomes greater. And the deviation, other things being equal, appears less when the tail is more oblique to the orbit of the comet, as well as when the head of the comet approaches nearer to the sun. Further, the tails which have no deviation appear straight, but the tails which deviate are likewise bent into a certain curvature; and this curvature is greater when the deviation is greater, and is more perceptible when the tail, other things being equal, is longer; for in the shorter tails the curvature is hardly to be perceived. And the angle of deviation is less near the comet's head, but greater towards the other end of the tail, and that because the lower side of the tail regards the parts from which the deviation is made, and which lie in a right line drawn out infinitely from the sun through the comet's head. And the tails that are longer and broader, and shine with a stronger light, appear more resplendent and more exactly defined on the convex than on the concave side. Upon these accounts it is plain that the phenomena of the tails of comets depend upon the motions of their heads, and by no means upon the places of the heavens in which their heads are seen; and that, therefore, the tails of the comets do not proceed from the refraction of the heavens, but from their own heads, which furnish the matter that forms the tail; for as in our air the smoke of a heated body ascends either perpendicularly, if the body is at rest, or obliquely if the body is moved obliquely, so in the heavens, where all the bodies gravitate towards the sun, smoke and vapor must (as we have already said) ascend from the sun, and either rise perpendicularly, if the smoking body is at rest, or obliquely, if the body, in the progress of its motion, is always leaving those places from which the upper or higher parts of the vapors had risen before. And that obliquity will be less where the vapor ascends with greater velocity, namely, near the smoking body, when that is near the sun; for there the force of the sun by which the vapor ascends is stronger. But because the obliquity is varied, the column of vapor will be incurvated; and because the vapor in the preceding side is something more recent, that is, has ascended somewhat later from the body, it will therefore be somewhat more dense on that side, and must on that account reflect more light, as well as be better defined, the vapor on the other side languishing by degrees, and vanishing.

[71.] *That comets sometimes come within the orbit of Mercury is shown by their tails.*

But it is none of our present business to explain the causes of the appearances of Nature. Let those things which we have last said be true or false, we have at least made out, in the preceding discourse, that the rays of light are directly propagated from the tails of comets in right lines through the heavens, in which those tails appear to the spectators wherever placed; and consequently the tails must ascend from the heads of the comets towards the parts opposite to the sun. And from this principle we may determine anew the limits of their distances in the following manner. Let S represent the sun, T the earth, STA the elongation of a comet from the sun, and ATB the apparent length of its tail; and since the light is propagated from the extremity of the tail in the direction of the right line TB, that extremity must lie somewhere in the line TB. Suppose it in D, and join DS cutting TA in C. Then, since the tail is always stretched out towards the parts nearly opposite to the sun, and therefore the sun, the head of the comet, and the extremity of the tail, lie in a right line, the comet's head will be found in C. Parallel to TB draw SA, meeting the line TA in A, and the comet's head C must necessarily be found between T and A, because the extremity of the tail lies somewhere in the infinite line TB; and all the lines SD which can possibly be drawn from the point S to the line TB must cut the line TA somewhere between T and A. Therefore the distance of the comet from the earth cannot exceed the interval TA, nor its distance from the sun the interval SA beyond, or ST on this side the sun. For instance: the elongation of the comet of 1680 from the sun, *Dec.* 12, was 9°, and the length of its tail 35° at least. If, therefore, a triangle TSA is made, whose angle T is equal to the elongation 9°, and angle A equal to ATB, or to the length of the tail, viz., 35°, then SA will be to ST, that is, the limit of the greatest possible distance of the comet from the sun to the semidiameter of the earth's orbit, as the sine of the angle T is to the sine of the angle A, that is, as about 3 to 11. And therefore the comet at that time was less distant from the sun than by 3/11 of the earth's distance from the sun, and hence either was within the orbit of Mercury, or between that orbit and the earth. Again, on *Dec.* 21, the elongation of the comet from the sun was 32⅔°, and the length of its tail 70°. There-



fore, as the sine of 32⅔° is to the sine of 70°, that is, as 4 is to 7, so was the limit of the comet's distance from the sun to the distance of the earth from the sun, and consequently the comet had not then got without the orbit of Venus. *Dec.* 28, the elongation of the comet from the sun was 55°, and the length of its tail 56°; and therefore the limit of the comet's distance from the sun was not yet equal to the distance of the earth from the same, and consequently the comet had not then got without the earth's orbit. But from its parallax we find that its egress from the orbit happened about *Jan.* 5, and also that it had descended far within the orbit of Mercury. Let us suppose it to have been in its perihelion *Dec.* the 8th, when it was in conjunction with the sun; and it will follow that in the journey from its perihelion to its exit out of the earth's orbit it had spent 28 days; and consequently that in the 26 or 27 days following, in which it ceased to be further seen by the naked eye, it had scarcely doubled its distance from the sun; and by limiting the distances of other comets by the like arguments, we come at last to this conclusion,—that all comets, during the time in which they are visible by us, are within the compass of a spherical space described about the sun as a centre, with a radius double, or at most triple, the distance of the earth from the sun.

[72.] *The comets move in conic sections having one focus in the centre of the sun, and by their radii drawn to that centre describe areas proportional to the times.*

And hence it follows that the comets, during the whole time of their appearance unto us, being within the sphere of activity of the circumsolar force, and therefore agitated by the impulse of that force, will (by Cor. I, Prop. XIII, Book I, for the same reason as the planets) be made to move in conic sections that have one focus in the centre of the sun, and, by radii drawn to the sun, to describe areas proportional to the times; for that force is propagated to an immense distance, and will govern the motions of bodies far beyond the orbit of Saturn.

[73.] *These conic sections are near to parabolas. This is inferred from the velocity of the comets.*

There are three hypotheses about comets (p. 498); for some will have it that they are generated and perish as often as they appear and vanish;

others, that they come from the regions of the fixed stars, and are seen by us in their passage through the system of our planets; and, lastly, others will have it, that they are bodies continually revolving about the sun in very eccentric orbits. In the first case, the comets, according to their different velocities, will move in conic sections of all sorts; in the second, they will describe hyperbolas, and in either of the two will frequent indifferently all quarters of the heavens, as well those about the poles as those towards the ecliptic; in the third, their motions will be performed in ellipses very eccentric, and very nearly approaching to parabolas. But (if the law of the planets is observed) their orbits will not much decline from the plane of the ecliptic; and, so far as I could hitherto observe, the third case obtains; for the comets do, indeed, chiefly frequent the zodiac, and scarcely ever attain to a heliocentric latitude of 40°. And that they move in orbits very nearly parabolic, I infer from their velocity; for the velocity with which a parabola is described is everywhere to the velocity with which a comet or planet may be revolved about the sun in a circle at the same distance in the ratio of $\sqrt{2}$ to 1 (by Cor. VII, Prop. XVI, Book I); and, by my computation, the velocity of comets is found to be much the same. I examined the thing by inferring successively the velocities from the distances, and the distances both from the parallaxes and the phenomena of the tails, and never found the errors of excess or defect in the velocities greater than what might have arisen from the errors in the distances computed after that manner. But I likewise made use of the reasoning that follows.

[74.] *The length of time in which comets describing parabolic orbits pass through the sphere of the earth's orbit.*

Supposing the radius of the earth's orbit to be divided into 1000 parts: let the numbers in the first column of Table I (p. 616) represent the distance of the vertex of the parabola from the sun's centre, expressed by those parts; and a comet in the times expressed in col. 2, will pass from its perihelion to the surface of the sphere which is described about the sun as a centre with the radius of the earth's orbit; and in the times expressed in col. 3, 4, and 5, it will double, triple, and quadruple its distance from the sun.

TABLE I

The distance of a comet's perihelion from the sun's centre	The time of a comet's passage from its perihelion to a distance from the sun equal to			
	The radii of the earth's orbit	To its double	To its triple	To its quadruple
	d h m	d h m	d h m	d h m
0	27 11 12	77 16 28	142 17 14	219 17 30
5	27 16 07	77 23 14		
10	27 21 00	78 06 24		
20	28 06 40	78 20 13	144 03 19	221 08 54
40	29 01 32	79 23 34		
80	30 13 25	82 04 56		
160	33 05 29	86 10 26	153 16 08	232 12 20
320	37 13 46	93 23 38		
640	37 09 49	105 01 28		
1280		106 06 35	200 06 43	297 03 46
2560			147 22 31	300 06 03

The time of a comet's ingress into the sphere of the earth's orbit, or of its egress from the same, may be inferred from its parallax, but with more expedition by the following:

TABLE II

The apparent elongation of a comet from the sun	Its apparent diurnal motion in its own orbit		Its distance from the earth in parts, of which the radius of the earth's orbit contains 1000
	Direct	Retrogressive	
60°	2° 18′	00° 20′	1000
65	2 33	00 35	845
70	2 55	00 57	684
72	3 07	01 09	618
74	3 23	01 25	551
76	3 43	01 45	484
78	4 10	02 12	416
80	4 57	02 49	347
82	5 45	03 47	278
84	7 18	05 20	209
86	10 27	08 19	140
88	18 37	16 39	70
90	Infinite	Infinite	00

[75.] *The velocity with which the comets of 1680 passed through the sphere of the earth's orbit.*

The ingress of a comet into the sphere of the earth's orbit, or its egress from the same, happens at the time of its elongation from the sun, expressed in col. 1, against its diurnal motion. So in the comet of 1681, *Jan.* 4, o.s., the apparent diurnal motion in its orbit was about $3°\ 5'$, and the corresponding elongation $71\frac{2}{3}°$; and the comet had acquired this elongation from the sun *Jan.* 4, about six in the evening. Again, in the year 1680, *Nov.* 11, the diurnal motion of the comet that then appeared was about $4\frac{2}{3}°$; and the corresponding elongation $79\frac{2}{3}$ happened *Nov.* 10, a little before midnight. Now at the times named these comets had arrived at an equal distance from the sun with the earth, and the earth was then almost in its perihelion. But the first table is fitted to the earth's mean distance from the sun assumed of 1000 parts; and this distance is greater by such an excess of space as the earth might describe by its annual motion in one day's time, or the comet by its motion in 16 hours. To reduce the comet to this mean distance of 1000 parts, we add those 16 hours to the former time, and subtract them from the latter; and thus the former becomes *Jan.* 4^d. 10^h. afternoon; the latter *Nov.* 10, about six in the morning. But from the tenor and progress of the diurnal motions it appears that both comets were in conjunction with the sun between *Dec.* 7 and *Dec.* 8; and from thence to *Jan.* 4^d. 10^h. afternoon on one side, and to *Nov.* 10^d. 6^h. of the morning on the other, there are about 28 days. And so many days (by Table 1) the motions in parabolic orbits do require.

[76.] *These comets were not two, but one and the same comet; it is more accurately determined in what orbit and with what velocity the comet traversed the heavens.*

But though we have hitherto considered those comets as two, yet, from the coincidence of their perihelions and agreement of their velocities, it is probable that in effect they were but one and the same; and if so, the orbit of this comet must have either been a parabola, or at least a conic section very little differing from a parabola, and at its vertex almost in contact with the surface of the sun. For (by Table II) the distance of the comet from the earth, *Nov.* 10, was about 360 parts, and *Jan.* 4, about 630. From which distances, together with its longitudes and latitudes, we infer the distance of the places in which the comet was at those times to have been about 280: the half of which, viz., 140, is an ordinate to the comet's orbit, cutting off a portion of its axis nearly equal to the radius of the earth's orbit, that is, to 1000 parts. And, therefore, dividing the square of the ordinate 140 by 1000, the segment of the axis, we find the latus rectum 19.6, or in a round number 20; the fourth part whereof, 5, is the distance of the vertex of the orbit from the sun's centre. But the time corresponding to the distance of 5 parts in Table I is $27^d. 16^h. 7^m$. In this time, if the comet moved in a parabolic orbit, it would have been carried from its perihelion to the surface of the sphere of the earth's orbit described with the radius 1000, and would have spent the double of that time, viz., $55^d. 8\frac{1}{4}^h$. in the whole course of its motion within that sphere: and so in fact it did; for from *Nov.* $10^d. 6^h$. of the morning, the time of the comet's ingress into the sphere of the earth's orbit, to Jan. $4^d. 10^h$. afternoon, the time of its egress from the same, there are $55^d. 16^h$. The small difference of $7\frac{3}{4}^h$. in this rough way of computing is to be neglected, and perhaps may arise from the comet's motion being to a small degree slower, as it must have been if the true orbit in which it was carried was an ellipse. The middle time between its ingress and egress was *Dec.* $8^d. 2^h$. of the morning; and therefore at this time the comet ought to have been in its perihelion. And accordingly that very day, just before sunrise, Dr. *Halley* (as we said) saw the tail short and broad, but very bright, rising perpendicularly from the horizon. From the position of the tail it is certain that the comet had then crossed over the ecliptic, and got into north latitude, and therefore had passed by its perihelion, which lay on the other side of the ecliptic, though it had not yet come into conjunction with the sun; and the comet being at this time between its perihelion and its conjunction with the sun, must have been in its perihelion a few hours before; for in so near a distance from the sun it must have been carried with great velocity, and have apparently described almost half a degree every hour.

[77.] *It is shown by additional examples with what velocity the comets move.*

By like computations I find that the comet of 1618 entered the sphere of the earth's orbit *Dec.* 7, towards sunset; but its conjunction with the sun was *Nov.* 9, or 10, about 28 days intervening, as in the preceding comet; for from the size of the tail of this, in which it was equal to the preceding, it is probable that this comet likewise did come almost into a contact with the sun. Four comets were seen that year, of which this was the last. The second, which made its first appearance *Oct.* 31, in the neighborhood of the rising sun, and was soon after hidden under the sun's rays, I suspect to have been the same as the fourth, which emerged out of the sun's rays about *Nov.* 9. To these we may add the comet of 1607, which entered the sphere of the earth's orbit *Sept.* 14, o.s., and arrived at its perihelian distance from the sun about *Oct.* 19, 35 days intervening. Its perihelian distance subtended an apparent angle at the earth of about 23 degrees, and was therefore of 390 parts. And to this number of parts about 34 days correspond in Table 1. Further, the comet of 1665 entered the sphere of the earth's orbit about *March* 17, and came to its perihelion about *April* 16, 30 days intervening. Its perihelian distance subtended an angle at the earth of about seven degrees, and therefore was of 122 parts: and corresponding to this number of parts, in Table 1 we find 30 days. Again; the comet of 1682 entered the sphere of the earth's orbit about *Aug.* 11, and arrived at its perihelion about *Sept.* 16, being then distant from the sun by about 350 parts, to which, in Table 1, belong 33½ days. Lastly; that memorable comet of *Johann Müller's,* which in 1472 was carried through the circumpolar parts of our northern hemisphere with such rapidity as to describe 40 degrees in one day, entered the sphere of the earth's orbit *Jan.* 21, about the time that it was passing by the pole, and, hastening from thence towards the sun, was hid under the sun's rays about the end of *February;* whence it is probable that 30 days, or a few more, were spent between its ingress into the sphere of the earth's orbit and its perihelion. Nor did this comet truly move with more velocity than other comets, but owed the greatness of its apparent velocity to its passing by the earth at a near distance.

[78.] *It is proposed to determine the orbits of comets.*

It appears, then, that the velocity of comets (p. 505), so far as it can be determined by these rough ways of computing, is that very velocity with which parabolas, or ellipses near to parabolas, ought to be described; and therefore the distance between a comet and the sun being given, the velocity of the comet is nearly given. And hence arises this Problem.

PROBLEM

The relation between the velocity of a comet and its distance from the sun's centre being given, the comet's orbit is required.

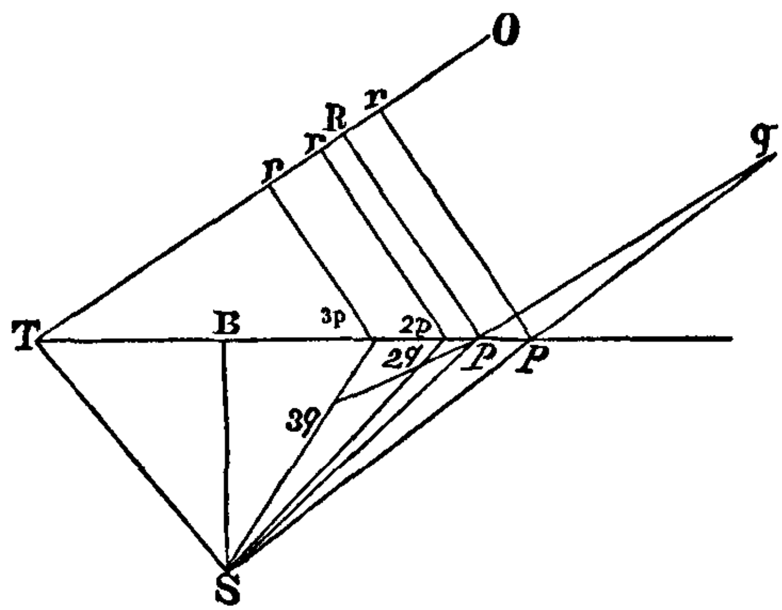
If this Problem were resolved, we should thence have a method of determining the orbits of comets to the greatest accuracy; for if that relation be twice assumed, and from thence the orbit be twice computed, and the error of each orbit be found from observations, the assumption may be corrected by the rule of false position, and a third orbit may thence be found that will exactly agree with the observations. And by determining the orbits of comets after this method, we may come, at last, to a more exact knowledge of the parts through which those bodies travel, of the velocities with which they are carried, what sort of orbits they describe, and what are the true magnitudes and forms of their tails according to the various distances of their heads from the sun; whether, after certain intervals of time, the same comets do return again, and in what periods they complete their several revolutions. But the Problem may be resolved by determining, first, the hourly motion of a comet, at a given time, from three or more observations, and then deriving the orbit from this motion. And thus the determination of the orbit, depending on one observation, and its hourly motion at the time of this observation, will either confirm or disprove itself; for the conclusion that is drawn from the motion only of an hour or two and a false hypothesis, will never agree with the motions of the comets from beginning to end. The method of the whole computation is this.

LEMMA I

To cut two right lines OR, TP, *given in position, by a third right line* RP, *so that* TRP *may be a right angle; and, if another right line* SP *is drawn to any given point* S, *the product obtained from multiplying this line* SP, *by the square of the right line* OR *terminated at a given point* O, *may be a given magnitude.*

It is done graphically thus. Let the given magnitude of the product be $M^2 \cdot N$; from any point r of the right line OR erect the perpendicular rp

meeting TP in p. Then through the point Sp draw the line Sq equal to $\frac{M^2 \cdot N}{Or^2}$. In like manner draw three or more right lines S$2q$, S$3q$, &c.; and a regular line $q2q3q$, drawn through all the points $q2q3q$, &c., will cut the right line TP in the point P, from which the perpendicular PR is to be let fall. Q.E.F.



By trigonometry thus. Assuming the right line TP as found by the preceding method, the perpendiculars TR, SB, in the triangles TPR, TPS, will be thence given; and the side SP in the triangle SBP, as well as the error $\frac{M^2 \cdot N}{OR^2} - SP$. Let this error, designated D, be to a new error, designated E, as the error $2p2q \pm 3p3q$ is to the error $2p3p$; or as the error $2p2q \pm D$ is to the error $2p$P; and this new error added to or subtracted from the length TP, will give the correct length TP $\pm$ E. The inspection of the figure will show whether we are to add to or subtract; and if at any time there should be use for a further correction, the operation may be repeated.

By arithmetic thus. Let us suppose the thing done, and let TP + e be the correct length of the right line TP as found out by delineation; and thence the correct lengths of the lines OR, BP, and SP, will be $OR - \frac{TR}{TP}\, e$, $BP + e$,

$$\text{and } \sqrt{(SP^2 + 2BPe + ee)} = \frac{M^2N}{OR^2 + \frac{2OR \cdot TR}{TP} e + \frac{TR^2}{TP^2} ee.}$$

Hence, by the method of converging series, we have

$$SP + \frac{BP}{SP} e + \frac{SB^2}{2SP^2} ee, \text{ \&c.}, = \frac{M^2N}{OR^2} + \frac{2TR}{TP} \cdot \frac{M^2N}{OR^3} e + \frac{3TR^2}{TP^2} \cdot \frac{M^2N}{OR^4} ee, \text{ \&c.}$$

For the given coefficients

$$\frac{M^2N}{OR^2}-SP, \frac{2TR}{TP}\cdot\frac{M^2N}{OR^3}-\frac{BP}{SP}, \frac{3TR^2}{TP^2}\cdot\frac{M^2N}{OR^4}-\frac{SB^2}{2SP^3},$$

putting F, $\frac{F}{G}$, $\frac{F}{GH}$, and carefully observing the signs, we find

$$F+\frac{F}{G}e+\frac{F}{GH}ee=0, \text{ and } e+\frac{ee}{H}=-G.$$

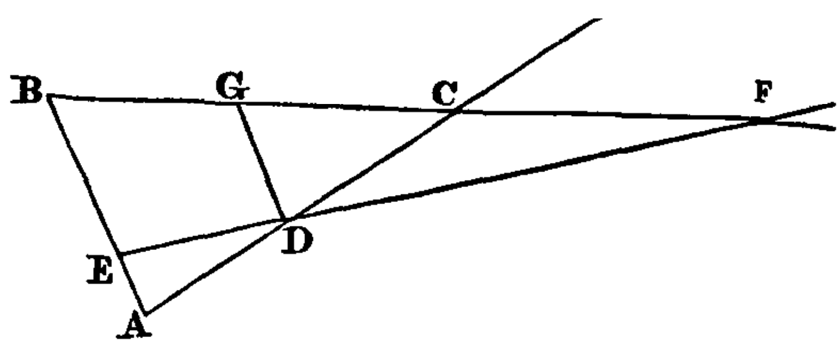
Hence, neglecting the very small term $\frac{e^2}{H}$, e comes out equal to $-G$. If the error $\frac{e^2}{H}$ is not negligible, take $-G-\frac{G^2}{H}=e$.

And it is to be observed that here a general method is hinted at for solving the more intricate sort of problems, as well by trigonometry as by arithmetic, without those involved computations and resolutions of affected equations which hitherto have been in use.

LEMMA II

To cut three right lines given in position by a fourth right line that shall pass through a point assigned in any of the three, and so that its intercepted parts shall be in a given ratio one to the other.

Let AB, AC, BC be the right lines given in position, and suppose D to be the given point in the line AC. Parallel to AB draw DG meeting BC in

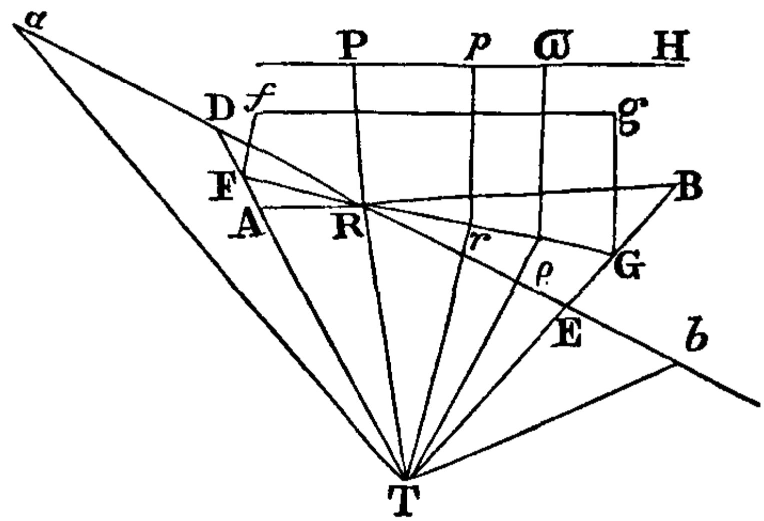


G; and, taking GF to BG in the given ratio, draw FDE; and FD will be to DE as FG to BG. Q.E.F.

By trigonometry thus. In the triangle CGD all the angles and the side CD are given, and from thence its remaining sides are found; and from the given ratios the lines GF and BE are also given.

LEMMA III

To find and represent graphically the hourly motion of a comet to any given time.



From observations of the best credit, let three longitudes of the comet be given, and, supposing ATR, RTB to be their differences, let the hourly motion be required at the time of the middle observation TR. By Lem. II, draw the right line ARB, so as its intercepted parts, AR, RB, may be as the times between the observations; and if we suppose a body in the whole time to describe the whole line AB with an equal motion, and to be in the meantime viewed from the place T, the apparent motion of that body about the point R will be nearly the same as that of the comet at the time of the observation TR.

The same more accurately.

Let T*a*, T*b* be two longitudes given at a greater distance on one side and on the other; and by Lem. II draw the right line *a*R*b* so that its intercepted parts *a*R, R*b* may be as the times between the observations *a*TR, RT*b*. Suppose this to cut the lines TA, TB, in D and E; and since the error of the inclination TR*a* increases nearly as the square of the time between the observations, draw FRG, so that either the angle DRF may be to the angle ARF, or the line DF to the line AF, as the square of the ratio of the whole time between the observations *a*TB to the whole time between the observations ATB, and use the line thus found FG in place of the line AB found above.

It will be convenient that the angles ATR, RTB, *a*TA, BT*b* be no less than ten or fifteen degrees, the times corresponding no greater than eight or twelve days, and the longitudes taken when the comet moves with the greatest velocity; for thus the errors of the observations will bear a less ratio to the differences of the longitudes.

LEMMA IV

To find the longitudes of a comet at any given times.

It is done by taking in the line FG the distances R*r*, Rρ, proportional to the times, and drawing the lines T*r*, Tρ. The way of working by trigonometry is manifest.

LEMMA V

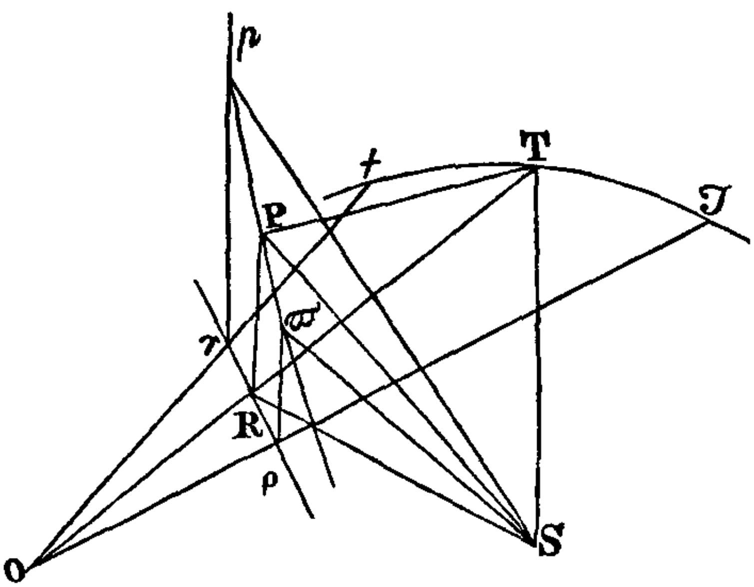
To find the latitudes.

On TF, TR, TG, as radii, at right angles erect F*f*, RP, G*g*, tangents of the observed latitudes; and parallel to *fg* draw PH. The perpendiculars *rp*, ρϖ, meeting PH, will be the tangents of the sought latitudes to T*r* and Tρ as radii.

PROBLEM I

From the assumed ratio of the velocity to determine the orbit of a comet.

Let S represent the sun; *t*, T, τ three places of the earth in its orbit at equal distances; *p*, P, ϖ as many corresponding places of the comet in its orbit, so that the distances interposed between place and place may answer to the motion of one hour; *pr*, PR, ϖρ perpendiculars let fall on the plane of the ecliptic, and *r*Rρ the vestige of the orbit in this plane. Join S*p*, SP, Sϖ, SR, ST, *tr*, TR, τρ, TP, and let *tr*, τρ meet in O, TR will nearly converge to the same point O, or the error will be inconsiderable. By the premised Lemmas the angles *r*OR, ROρ are given, as well as the ratios *pr* to *tr*, PR to TR, and ϖρ to τρ. The figure *t*TτO is likewise given both in magnitude and position, together with the distance ST, and the angles STR, PTR, STP. Let us assume the velocity of the comet in the place P to be to the velocity of a planet revolved about the sun in a circle,



at the same distance SP, as V is to 1; and we shall have a line $pP\varpi$ to be determined, of this condition, that the space $p\varpi$, described by the comet in two hours, may be to the space $V \cdot t\tau$ (that is, to the space which the earth describes in the same time multiplied by the number V) as the square root of the ratio of ST, the distance of the earth from the sun, to SP, the distance of the comet from the sun; and that the space pP, described by the comet in the first hour, may be to the space $P\varpi$, described by the comet in the second hour, as the velocity in p to the velocity in P; that is, as the square root of the ratio of the distance SP to the distance Sp, or in the ratio of $2Sp$ to $SP+Sp$; for in this whole work I neglect small fractions that can produce no perceptible error.

In the first place, then, as mathematicians, in the resolution of affected equations, are wont, for the first step, to assume the root by conjecture, so, in this analytical operation, I judge of the sought distance TR as I best can by conjecture. Then, by Lem. II I draw $r\rho$, first supposing rR equal to $R\rho$, and again (after the ratio of SP to Sp is discovered) so that rR may be to $R\rho$ as $2SP$ to $SP+Sp$, and I find the ratios of the lines $p\varpi$, $r\rho$, and OR, one to the other. Let M be to $V \cdot t\tau$ as OR to $p\varpi$; and because the square of $p\varpi$ is to the square of $V \cdot t\tau$ as ST to SP, we shall have, from this, OR^2 to M^2 as ST to SP, and therefore the product $OR^2 \cdot SP$ equal to the given product $M^2 \cdot ST$; whence (supposing the triangles STP, PTR to be now placed in the same plane) TR, TP, SP, PR will be given, by Lem. I. All this I do, first by graphic procedure in a rough and hasty way; then by a new graph with greater care; and, lastly, by an arithmetical computation. Then I proceed to determine the position of the lines $r\rho$, $p\varpi$ with the greatest accuracy, together with the nodes and inclination of the plane $Sp\varpi$ to the plane of the ecliptic; and in that plane $Sp\varpi$ I describe the orbit in which a body let go from the place P in the direction of the given right line $p\varpi$ would be carried with a velocity that is to the velocity of the earth as $p\varpi$ to $V \cdot t\tau$. Q.E.F.

PROBLEM II

To correct the assumed ratio of the velocity and the orbit thence found.

Take an observation of the comet about the end of its appearance, or any other observation at a very great distance from the observations used before, and find the intersection of a right line drawn to the comet, in that observation with the plane $Sp\bar{\omega}$, as well as the comet's place in its orbit at the time of the observation. If that intersection happens in this place, it is a proof that the orbit was rightly determined; if otherwise, a new number V is to be assumed, and a new orbit is to be found; and then the place of the comet in this orbit at the time of that probatory observation, and the intersection of a right line drawn to the comet with the plane of the orbit, are to be determined as before; and by comparing the variation of the error with the variation of the other quantities, we may conclude, by the Rule of Three, how far those other quantities ought to be varied or corrected, so as the error may become as small as possible. And by means of these corrections we may have the orbit exactly, providing the observations upon which the computation was founded were exact, and that we did not err much in the assumption of the quantity V; for if we did, the operation is to be repeated till the orbit is exactly enough determined. Q.E.F.

[END OF THE SYSTEM OF THE WORLD.]

AN HISTORICAL AND EXPLANATORY APPENDIX

BY

FLORIAN CAJORI

1.* Frontispiece. *Portrait of Newton.* The photogravure has been made from a portrait of Newton, which has been gummed in volume 2 of a large work entitled *Heads in Taille Douce* (p. 128). This volume is in the Pepys Library at Cambridge. The Masters and Fellows of Magdalene College graciously consented to have the portrait photographed for reproduction in the present edition of Newton's *Principia.* J. Edleston[1] gives an engraving prepared from this same portrait: but the portrait here shown is a photographic reproduction. The original drawing is in India ink. As to the year when it was made, Edleston concludes (p. xix): "In assigning, therefore, the date of the portrait to the period of a few years on either side of 1691, we shall not perhaps be very wide of the truth. If this supposition be well-founded, this portrait may be considered as the most interesting of all the known portraits of our philosopher, as representing him at a time of his life the least remote from those memorable eighteen months which it cost him to produce the great work that has immortalized his name."

[1] J. Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes,* London, 1850, frontispiece.

2. *Facsimile of the title page of the first edition of the Principia.* A close approach to the date when Newton made alterations in this page may be obtained from the following considerations. Newton's changes in the title page indicate that he was president of the Royal Society of London, but they do not indicate that he had been knighted. In the second edition of the *Principia,* 1713, his knighthood appears in the words "Auctore Isaaco Newtono, Equite Aurato." We know that Newton was elected president of the Royal Society on Nov. 30, 1703; he was knighted Jan. 16, 1705. Therefore the alterations on the title page must have been made in the interval between these two dates. This conclusion is in conformity with a remark of Flamsteed to Pound,[1] Nov. 15, 1704, "The book [Newton's *Opticks*] makes no noise in town, as the Principia did, which I hear he is preparing again for the press with necessary corrections." The alterations were not printed.

[1] Edleston, *op. cit.,* p. xv.

* The numbers refer to corresponding footnotes in the text.

3 (p. xvii). *Preface to the First Edition of the Principia.* This Preface in the first edition has no date and lacks the author's signature. The signature "Is. Newton" and the date "Dabam Cantabrigiæ, e Collegio S. Trinitatis, Maii 8. 1686" first appear in the second edition, 1713. The preface to the first edition of Newton's *Opticks,* 1704, bears no date, while in the second edition, 1718, the date "April 1, 1704" is added. Probably Newton came to recognize the importance of dates in the course of his bitter controversy with Leibniz on the invention of the calculus.

4 (p. xix). *Alterations and corrections made in preparing the second edition of the Principia.* The statement of changes indicated in Newton's short Preface may be supplemented by the following remarks of Ball:[1] "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."

In preparing copy for the second edition of the *Principia,* Cotes took great care to remove errors and imperfections. Newton wrote to him on Oct. 11, 1709: "I would not have you be at the trouble of examining all the Demonstrations in the Principia. Its impossible to print the book w^th out some faults and if you print by the copy sent you, correcting only such faults as occur in reading over the sheets to correct them as they are printed off, you will have labour more then it's fit to give you."[2] In 1713, after the second edition had appeared from the press, Newton sent Cotes a list of errata, perhaps intending it to be printed as a table of errata. To this Cotes replied, Dec. 22, 1713:[3] "I observe You have put down about 20 Errata besides those in my Table. . . . I believe You will not be surpriz'd if I tell You I can send You 20 more as considerable, which I have casually observ'd, and which seem to have escap'd You: and I am far from thinking these forty are all that may be found out, notwithstanding that I think the Edition to be very correct. I am sure it is much more so than the former, which was carefully enough printed; for besides Your own corrections and those I acquainted You with whilst the Book was printing, I may venture to say I made some Hundreds, with which I never acquainted You."

Certain changes occurring in the second edition of the *Principia* are mentioned in Notes 3, 19, 24, 26, 27, 29, 30, 39, 42, 45.

[1] W. W. R. Ball, *An Essay on Newton's Principia,* London, 1893, p. 74.

[2] Edleston, *op. cit.,* p. 5.

[3] Edleston, *op. cit.,* pp. 167, 168.

5 (p. xx). *Cotes's Preface to the Second Edition of the Principia.* It was at the suggestion of Richard Bentley, Master of Trinity College in Cambridge, that Cotes wrote this Preface. "I have S^{r} Isaac's Leave," wrote Bentley, "to remind you of what You and I were talking of, An alphabetical Index, and a Preface in your own Name; If you please to draw them up ready for y^{e} press, to be printed after my Return to Cambridg, You will oblige Yours R Bentley."[1]

Cotes wrote to Newton on Feb. 18, 1712–3, about the Preface: "I think it will be proper besides the account of the Book and its improvements, to add something more particularly concerning the manner of Philosophizing made use of and wherein it differs from that of Descartes and Others, I mean in first demonstrating the Principle it employs. This I would not only assert but make evident by a short deduction of the Principle of Gravity from the Phaenomena of Nature in a popular way that it may be understood by ordinary readers and may serve at y^{e} same time as a specimen to them of the Method of ye whole Book."[2] Then follows a detailed plan which was afterwards somewhat modified. Newton himself prepared a short Preface which made it unnecessary for Cotes to enter into a recital of the "improvements" in the second edition of the *Principia.* Cotes's Preface is therefore confined to "the manner of philosophizing" and an examination of the objections of Leibniz (without mentioning his name) and of the system of vortices. Leibniz, in a letter (April 9, 1716) written under excitement, calls the Preface "pleine d'aigreur."

As stated, the primary object of the Preface was to combat Descartes' theory of vortices. The need of such discussion, twenty-six years after the first appearance of Newton's *Principia,* indicates the great popular attachment to the views of Descartes. Not only was his theory of vortices generally held at this time (1713) on the European continent, but also in England. Cartesian cosmology invaded England soon after Descartes' publication of his theory in 1644. Henry More, of Christ's College, Cambridge, one of the first fellows of the Royal Society of London, in his earlier years had been in correspondence with Descartes and an admirer of his. More's friend, Joseph Glanvill, of Exeter College, Oxford, also a fellow of the Royal Society, wrote appreciatively of Descartes' vortices. The writings of Robert Boyle teem with references to Descartes, "the most acute modern philosopher," yet in Boyle there is only one reference that I could find, to the Cartesian theory of vortices, and that reference was "without allowing this hypothesis to be more than not very improbable."[3] Robert Hooke wrote in criticism of some aspects of the vortex theory.[4]

Descartes' theory of vortices received a popular exposition in the famous textbook on physics, written in French by Rohault. A Swiss physician, Théophile Bonet, made a Latin translation of this text, which appeared in Geneva in 1674 and in London in 1682. Thus England began to use this well written textbook five years before the publication of Newton's *Principia*. The profound divergence of the mechanics of Rohault and Newton stands out glaringly in Rohault's statement that motion in a circle is as natural as in a straight line. The Cartesian doctrine had elements of popular strength. The non-mathematician could understand it. Everyone had seen chips of wood whirled about in eddies of rivers. Everyone had seen a minute whirlwind raise the dust in tiny cyclones. Planets moved like pieces of wood in eddies. These mental pictures carried conviction. On the contrary, Newton's law of inverse squares in gravitational attraction meant nothing to one not accustomed to mathematical thinking.[5] British mathematicians like Halley, David and James Gregory, Keill, Whiston, Cotes, Taylor, Robert Smith, and Saunderson favored Newton's doctrines. Newton himself lectured at Cambridge, certainly as late as 1687,[6] but the details relating to his activity as a lecturer are exceedingly meager. After 1692 he had a long illness. In 1696 he was appointed Warden of the Mint. He was succeeded in the Lucasian Chair at Cambridge about 1701 by Whiston, who lectured on Newtonian philosophy. From these facts alone one might infer that Newton's system easily displaced Cartesianism in British universities. But such was not the fact; the Cartesian system displayed wonderful vitality, even in Cambridge. For about forty years after the first publication of Newton's *Principia* the French system maintained a foothold in England. I offer a few facts in support of this statement. The essayist, Joseph Addison, of Magdalen College, Oxford, delivered an oration in 1693, six years after the publication of Newton's *Principia,* in which he praises Descartes, "who had bravely asserted the truth" against the followers of Aristotle.[7] Whiston[8] refers to David Gregory's teaching Newton at Edinburgh, "while we at Cambridge, poor wretches, were ignominiously studying the fictitious hypotheses of the Cartesian." I have already referred to the publication in England in 1682 of Rohault's physics, containing a popular exposition of Descartes' system. Fifteen years later, in 1697, a new translation of that book into Latin appeared from the pen of Samuel Clarke, of Caius College, Cambridge, whom Whewell describes as a "friend and disciple of Newton." While the translation was in progress, Whiston spoke his mind to Clarke on the fitness of such a translation in the following terms:[9] "Since the youth of the university must have, at present, some System of Natural Philosophy for their studies and exercises; and since the true system of Sir Isaac Newton's was not yet made easy enough for the purpose, it is not improper, for their sakes, yet to translate and use the system of Rohault... but that as soon as Sir Isaac Newton's Philosophy came to be better known, that only ought to be taught, and the other dropped." It should be added that Rohault's was reputed to be by far the best treatise of that time on physics in general. Clarke's translation, in better Latinity, played an important rôle as a textbook, in both English and American colleges. John Playfair[10] says that this new and elegant translation contained additional notes, in which Clarke explained the views of Newton, so that the notes contained virtually a refutation of the text, avoiding, however, all appearance of controversy. Thus, continues Playfair, "the Newtonian Philosophy first entered the University of Cambridge, under the protection of the Cartesian." Playfair's statement needs emendation in one respect. Clarke's edition of Rohault, as printed in 1697, did not contain the additions as footnotes, but as annotations at the end of the volume; they are shorter than in the later editions and refer to ancient writers, and do not refute Descartes' theory of vortices. Clarke's refutation came at a later date. Four editions of Clarke's Latin translation appeared. The third, issued in 1710, differs from the first in having the notes not at the end of the volume, but at the bottom of the pages as footnotes, and greatly enlarged. This third edition (perhaps also the second of 1703, which I have not seen) contains a new annotation which relates to Descartes' vortices and points out conclusively that these vortices do not explain the facts of observation. They do not explain the motion of comets which cut the orbital planes of the planets at all angles; they would make a planet move fastest when farthest from the sun, while as a matter of fact it moves slowest when in that position. On this subject, there is given a long quotation from Newton's *Principia*. The popularity of Clarke's later editions of Rohault may be due largely to the footnotes. Taken as a whole, the text was acceptable to followers of Newton as well as to those of Descartes. Both sides were fairly presented. Professor Playfair directs attention to the fact that tutors in colleges, whose instructions "constitute the real and efficient system" in a British university, sometimes held different views from those of the professors. Thus Professor Keill introduced in his lectures Newtonian philosophy at Oxford, but the Oxford tutors "were not cast in that mold till long afterwards." Ball states that "at Cambridge until recently professors only rarely put themselves into contact with or adapted their lectures for the bulk of the students.... Accordingly if we desire to find to whom the spread of a general study of the Newtonian philosophy was immediately due, we must look not to Newton's lectures or writings, but among proctors, moderators, or college tutors who had accepted his doctrines."[11] Clarke's edition of Rohault suited therefore the needs of tutors, whichever of the two opposing scientific views they favored. That in 1723 Rohault's text was by no means discredited in England is evident from the appearance of an English translation of Clarke's edition, with notes. Other editions of this translation appeared as late as 1729 and 1735. According to Hodlay's life of Samuel Clarke, Rohault was still the Cambridge textbook in 1730, three years after the death of Newton and forty-three years after the appearance of Newton's *Principia*. It looks as if two different practices of instruction had been carried on for many years without open controversy between the two factions, one favoring Descartes as expounded by Rohault, the other favoring

Newton as expounded in Clarke's footnotes, in Whiston's lectures published in 1710 and 1716, and in the teaching of Richard Laughton, a noted tutor at Clare Hall in Cambridge. Desaguliers,[12] who moved from Oxford to London in 1713, informs us that "he found the Newtonian philosophy generally received among persons of all ranks and professions, and even among the ladies by the help of experiments." Somewhat at variance with this statement is that of Voltaire,[13] who visited England in 1727 and declared that though Newton survived the publication of the *Principia* more than forty years, yet at the time of his death he had not above twenty followers in England. But Voltaire[14] said also: "A Frenchman who arrives in London finds a great alteration in philosophy, as in other things. He left the world full, he finds it empty. At Paris you see the universe composed of vortices of subtle matter, in London we see nothing of the kind."

On the European continent, the vortices of Descartes enjoyed a longer life. Attempts were made by Huygens, Perrault, Johann II Bernoulli, and others to remove some of the glaring defects in the original theory of vortices, but by the middle of the eighteenth century the Newtonian system had gained complete ascendancy.

Cotes's Preface is of historical importance in other respects. It is interpreted as advocating the theory of "action at a distance" (see Note 8), and the theory that gravity is an innate property of matter (see Note 6).

[1] Edleston, *op. cit.*, p. 148.
[2] Edleston, *op. cit.*, pp. 151, 154.
[3] *Works of the Honourable Robert Boyle*, vol. 5, London, 1772, p. 403.
[4] Robert Hooke, *Micrographia*, London, 1665, pp. 60, 61.
[5] On the difficulty of understanding the *Principia*, see Ball, *op. cit.*, pp. 114–116.
[6] Edleston, *op. cit.*, p. xcviii.
[7] D. Brewster's *Memoirs of Sir Isaac Newton*, vol. 1, ed. 2, Edinburgh, 1860, pp. 291, 292.
[8] Whiston's *Memoirs of His Own Life*, p. 36, quoted by Brewster, *op. cit.*, vol. 1, p. 291.
[9] Brewster, *op. cit.*, vol. 1, p. 295.
[10] J. Playfair, "Dissertation Fourth," in *Encyclopaedia Britannica*, ed. 8, vol. 1, pp. 609, 610; quoted by Brewster, *op. cit.*, vol. 1, pp. 290, 291.
[11] W. W. R. Ball, *History of the Study of Mathematics at Cambridge*, Cambridge, 1889, p. 74.
[12] J. T. Desaguliers, *Physico-Mechanical Lectures*, London, 1717; quoted by W. Whewell, *History of the Inductive Sciences*, vol. 1, ed. 3, New York, 1875, p. 426.
[13] F. M. A. Voltaire, quoted by Brewster, *op. cit.*, vol. 1, p. 290.
[14] Voltaire, *Eléments de la philosophie de Newton*, 1783; *Œuvres*, vol. 31, 1785, quoted by Whewell, *op. cit.*, vol. 1, 1875, p. 431.

6 (p. xxi). Cotes's Preface. *The nature of gravity.* Cotes's words may have contributed to a misunderstanding of the views of Newton. Cotes says "that the attribute of gravity was found in all bodies" and that "gravity must have a place among the primary qualities of all bodies"; he refers to "the nature of gravity in earthly bodies." In expressions of this sort it might seem implied that gravity is an inherent property of matter. Phrases in Newton's *Principia* (1687) appear to carry a similar implication. Newton says (Book 1, Prop. LX): "If two bodies . . . attracting each other with forces inversely proportional to the square of

their distance"; (Book I, Prop. LXIX) "the absolute forces of the attracting bodies"; (Book I, Prop. LXXII) "the attraction of one corpuscle towards the several particles of one sphere"; (Book I, Prop. LXXV) "the attraction of every particle is inversely as the square of its distance from the centre of the attracting sphere"; (Book I, Prop. LXXVII) "let now the corpuscle P attract the sphere"; (Book III, Prop. V) "Jupiter and Saturn ... by their mutual attractions sensibly disturb each other's motions." In these expressions, the "bodies" or the "corpuscles" are represented as active, as "attracting." They are not passive like a chip of wood carried about by an eddy in a pool, or like a planet passively swept through space by a Cartesian vortex. It was easy, therefore, to jump to the inference that in the Newtonian theory, gravity was an innate, inherent property of matter. Indeed, such an interpretation was made by writers on the European continent, for example by Huygens, Lalande, Bordas-Demoulin and others,[1] and has been generally held by astronomers and physicists. Thus, after the publication of the *Principia* in 1687, Huygens forthwith abandoned the explanation of planetary motion by Descartes' theory of vortices, and published his adherence to Newton's celestial mechanics. But Huygens did not accept the view that gravitation was an innate property of matter, a view which he attributed to Newtonian philosophy. On this point Huygens rejected what he interpreted to be the tenet of Newton, and continued his adhesion to the tenet of Descartes.[2]

While readers of the first edition of the *Principia* had some justification in attributing to Newton the view that gravity was an innate property of matter, they were nevertheless mistaken. In the first edition Newton had made no explicit declaration on this point. We know now that before publishing his great book, as early as Feb. 28, 1678–9, in a letter to Robert Boyle,[3] he speculated on the "cause of gravity" and endeavored to explain attraction by the action of an "aether," consisting of "parts differing from one another in subtility by indefinite degrees." (See Note 55.) It is evident that Newton was no more a believer in gravity as an innate property of bodies than was Descartes. But readers of the first edition of the *Principia* had no means of knowing this. His letter to Boyle was not then made public.

Even Bentley, a great friend and admirer of Newton's, at first entertained the wrong idea of his attitude; in letters to Bentley of 1692–3, Newton strongly opposed the doctrine that gravity was an innate property of matter and also the doctrine of "action at a distance." These letters, like that to Boyle, were not printed until many years later, and could therefore not immediately influence scientific opinion generally. In a letter to Bentley,[4] Newton wrote:

"You some times speak of gravity as essential and inherent to matter. Pray, do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know, and therefore would take more time to consider of it."

In another letter Newton wrote:

"It is inconceivable, that inanimate brute matter, should, without the mediation of something else, which is not material, operate upon and affect other matter without mutual contact, as it must be, if gravitation, in the sense of Epicurus, be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

In the second edition of the *Principia* (1713) Newton made his position clearer by three additions to the text of 1687. In the Scholium following Prop. LXIX of Book I, Newton says: "I here use the word *attraction* in general for any endeavor whatever, made by bodies to approach each other, whether that endeavor arise from the action of the bodies themselves, as tending to each other or agitating each other by spirits emitted; or whether it arises from the action of the ether or of the air, or of any medium whatever, whether corporeal or incorporeal, in any manner impelling bodies placed therein towards each other." Here he maintains an agnostic attitude. In Book III, when discussing the Rules of Reasoning in Philosophy, he adds: "All bodies whatsoever are endowed with a principle of mutual gravitation.... Not that I affirm gravity to be essential to bodies: by their *vis insita* I mean nothing but their inertia." Finally, in the General Scholium at the end of the *Principia,* he said, "I do not frame hypotheses" on the nature of gravity. This was the proper attitude for him to take in a work like the *Principia.* To Boyle he described his notions on this subject to be "so indigested" that he was "not well satisfied" with them.

More positive than in the *Principia* was Newton's statement in the "Advertisement" to the second edition of his *Opticks,* July 16, 1717: "And to shew that I do not take Gravity for an Essential Property of Bodies, I have added one Question [Query 31] concerning its Cause. chusing to propose it by way of a Question, because I am not yet satisfied about it for want of Experiments."

Not only is it a mistake to attribute the doctrine that gravity is an innate quality of bodies to Newton, but it seems to be also a mistake to attribute it to Cotes, notwithstanding some of the phrases that I have quoted from his Preface. That it is a mistake appears from the correspondence between Cotes and Samuel Clarke. Cotes submitted to Clarke his draft of the Preface to the second edition of the *Principia.* He writes to Clarke:[5] "I return You my thanks for Your corrections of the Preface, and particularly for Your advice in relation to that place where I seem'd to assert Gravity to be Essential to Bodies. I am fully of Your mind that it would have furnish'd matter for Cavilling, and therefore I struck it out immediately upon Dr Cannon's mentioning Your Objection to me, and so it never was printed. . . . My design in that passage was not to assert Gravity to be essential to Matter, but rather to assert that we are ignorant of the Essential propertys of Matter and that in respect to our Knowledge Gravity might possibly lay as fair a claim to that Title as the other Propertys which I mention'd. For I understand by Essential propertys such propertys without which no others belonging to the same substance can exist: and I would not undertake to prove that it were impossible for any of the other Properties of Bodies to exist without even Extension."

The question of the nature of gravity has aroused new interest with the advent of Einstein's general theory of relativity, according to which gravity is looked upon not as innate to bodies, but rather as some modification of space. According to Einstein, the earth produces in its surroundings a gravitational field, which, acting on the apple, brings about its motion of fall. In Einstein's gravitational field,[6] in general, a ray of light is propagated curvilinearly. The difference between the new and the old physics is stated by Eddington thus: "Einstein's law of gravitation controls a geometrical quantity curvature in contrast to Newton's law which controls a mechanical quantity force."[7]

1 Edleston, *op. cit.*, p. 159.

2 *Traité de la lumière,* par C. H. D. Z., Leyden, 1690, pp. 125–180; *Discours de la cause de la pesanteur.* As early as 1669 Huygens read before the Paris academy a speculation on the cause of gravity based on a modification of Cartesian vortices. He did not publish on this subject before 1690. When Newton's *Principia* appeared in 1687, Huygens at once accepted Newton's centripetal force varying inversely as the square of the distance, because motions in the solar system were explained with great success by this law. But Huygens rejected Newton's idea that particles of matter of all bodies attract each other, because he could not see how such attraction could be explained on any mechanical principle. Edleston (*op. cit.*, pp. xxxi, lix) makes the interesting statement that the only time Newton and Huygens met, in 1689, at a meeting of the Royal Society of London, Huygens talked on the cause of gravity, while Newton discussed double refraction in Island crystals—each of the two great physicists discoursing on the topic most intimately associated with the other. For details, see also F. Rosenberger, *Isaac Newton und seine physikalischen Principien,* Leipzig, 1895, pp. 234–248.

3 *Isaaci Newtoni Opera* (Horsley's ed.), vol. 4, 1782, pp. 385–394.

4 *Works of Richard Bentley,* vol. 3, London, 1838, pp. 210, 211. Letter of Newton to Bentley, "Trinity College, Jan. 17, 1692–3."

5 Edleston, *op. cit.*, pp. 150, 159.

6 A. Einstein, *Relativity, the Special and General Theory,* tr. R. W. Lawson, New York, 1921, pp. 75, 88.

7 A. S. Eddington, *The Nature of the Physical World,* New York, 1929, p. 133.

7 (p. xxx). Cotes's Preface. Cotes's term for the earth's orbit is *orbis magnus* (the great orbit). It is a term frequently used also by Newton to designate the earth's orbit in its annual revolution around the sun. The term was introduced by Copernicus (*De revolutionibus orbium caelestium,* Lib. 1, Cap. x) and was used by Rhaeticus, Kepler, and others. The path of the earth was called the "great orbit," not, of course, because of its dimension, for the orbits of the superior plan-

ets are greater, but because of its great importance to the practical astronomer, who must take cognizance of it, in explaining the apparent motions of the sun and planets. In all parts of the *Principia* and the *System of the World* where the term *orbis magnus* occurs, I have substituted for it the expression "earth's orbit." I may add that Newton himself uses the name "earth's orbit" in his *Opticks*, Book II, Part III, Prop. XI.

8 (p. xxxi). Cotes's Preface. *Action at a distance.* The doctrine of "action at a distance" in gravitational attraction has been wrongly ascribed to Newton; it is more properly due to Cotes, who, in his Preface to the Second Edition of the *Principia,* argues against Descartes' theory of vortices. Cotes does not use the phrase "action at a distance," nor does he explicitly advocate the view that celestial spaces are void. He does argue that if a celestial fluid exists it "has no inertia, because it has no resisting force." The implicating sentences of his Preface read as follows: "Those who would have the heavens filled with a fluid matter, but suppose it void of any inertia, do indeed in words deny a vacuum, but allow it in fact. For since a fluid matter of that kind can not be distinguished from empty space, the dispute is now about names and not the nature of things." Samuel Clarke is more definite. In one of the footnotes to his later editions of Rohault he refers explicitly to "that immense Space which is void of all matter."

In Note 6 *supra* I quoted from Newton's letters to Bentley passages relating to gravity, where he says: "That one body may act upon another at a distance through a vacuum, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it."

Maxwell[1] says: "We find in his 'Optical Queries' and in his letters to Boyle, that Newton had very early made the attempt to account for gravitation by means of the pressure of a medium, and that the reason he did not publish these investigations 'proceeded from hence only, that he found he was not able, from experiment and observation, to give a satisfactory account of this medium, and the manner of its operation in producing the chief phenomena of Nature.' . . .[2]

"And when the Newtonian philosophy gained ground in Europe, it was the opinion of Cotes rather than that of Newton that became most prevalent, till at last Boscovich propounded his theory, that matter is a congeries of mathematical points, each endowed with the power of attracting or repelling the others according to fixed laws. In his world, matter is unextended, and contact is impossible. He did not forget, however, to endow his mathematical points with inertia."

Although the phrase "action at a distance" appears very simple, it is subtle on closer inspection and some physicists have pointed out "how weak are the grounds on which we deny principal action at a distance."[3]

An important event in the history of the doctrine "action at a distance" was the appearance of Maxwell's electromagnetic theory of light, in which it was held that electromagnetic disturbances travel with *finite* velocities. Previously, electric and magnetic attraction and repulsion had been assumed to take place instantaneously.

The element of time has come to be considered also in gravitation. The phrase "action at a distance," instead of being used in the old sense with reference to the nonexistence of a medium intervening between attracting masses, is employed since the advent of the theory of relativity to indicate an *instantaneous* action at a distance.[4] In place of an agent we now consider the time of action. But even now the view of Newton is misrepresented. Newtonian action at a distance is spoken of as "immediate action." Newton, on the other hand, postulates an agent and gives it time to act. To be sure, in his calculations of gravitational attractions, he assumes, as a necessary approximation (having no experimental data on the speed of propagation of gravitational action), that the action is instantaneous, but not so in his talks on gravity. In a letter to Boyle[5] he considers the cause of gravitation between two approaching bodies. They "make the ether between them begin to rarefy"; and again,[6] in his hypotheses on light, he says, "So may the gravitating attraction of the earth be caused by the continual condensation of some other such like ethereal spirit . . . in such a way . . . as to cause it [this spirit] from above to descend with great celerity for a supply; in which descent it may bear down with it the bodies it pervades, with force proportional to the superficies of all their parts it acts upon."

[1] J. C. Maxwell, *Proceedings of the Royal Institution of Great Britain*, vol. 7, 1873–1875, London, pp. 48, 49.

[2] C. Maclaurin's *Account of Sir Isaac Newton's Philosophical Discoveries*, London, 1748.

[3] A. Schuster, *The Progress of Physics, 1875–1908*, Cambridge, 1911, p. 37.

[4] It is of interest that, in one place, Laplace made the assumption that the transmission of gravity is not instantaneous, and he found that in order to produce the known effects in the secular acceleration of the moon, gravity must travel seven million times faster than light. The moon, with its subtle orbital inequalities, has in this problem, as in others, displayed a treacherous behavior. Laplace's calculation has been found to be incomplete and his velocity of gravity to be illusory. (See Laplace, *Mécanique céleste*, Livre x, the close of Chap. vii.)

[5] *Isaaci Newtoni Opera, op. cit.*, vol. 4, p. 385.

[6] S. P. Rigaud, *Historical Essay on the First Publication of Newton's Principia*, Oxford, 1838, Appendix, pp. 69, 70.

9 (p. xxxv). *The alterations and additions made in the third edition of the Principia* are indicated in Newton's Preface to that edition only in a general way. A detailed list was prepared by the astronomer J. C. Adams, of Pembroke College, Cambridge, and printed in David Brewster's *Memoirs . . . of Sir Isaac Newton* (ed. 2), vol. 2, Edinburgh, 1860, Appendix No. xxx, pp. 414–419.

Certain changes occurring in the third edition of the *Principia* are mentioned in Notes 11, 19, 26, 29, 33, 39, 42.

10 (p. 1). *Translations of the Principia made by Motte and Thorp.* In revising Motte's translation of Cotes's Preface and of the *Principia* of Newton, use has been made of Robert Thorp's translation into English (ed. 2, London, 1802) of Cotes's Preface and the first book of the *Principia*. Occasional aid has been derived also from J. Ph. Wolfers' translation of the *Principia* into German, 1872. The geometrical figures of the *Principia* are taken from the third edition (1726).

Andrew Motte's translation of the *Principia,* from Latin into English, was made in 1729, from the third edition (1726).

11 (p. 1). Definition 1 of the Principia, *Quantity of matter, or mass.* Newton does not define density. His definition of mass, as the product of density and volume, has been variously appraised. Mach[1] says: "As regards the concept of mass, we remark first that Newton's formulation which defines mass as the quantity of matter of a body, determined by the product of volume and density, is unfortunate. Since we can define density only as the mass of unit volume, the circle is obvious." But it is not easy to believe that Newton was guilty of an *argumentum in circulo* so manifest. Crew[2] holds that "in the time of Newton, density and specific gravity were employed as synonymous, and the density of water was taken arbitrarily to be unity. The three fundamental units employed . . . were therefore density, length, time, instead of our mass, length, time. On such a system, it is both natural and logically permissible to define mass in terms of density."

Newton gives a definition of equal densities of bodies in a later passage in the *Principia* (Book III, Prop. VI, Cor. IV), where he says: "If all the solid particles of all bodies are of the same density, and cannot be rarified without pores, then a void, space, or vacuum must be granted. By bodies of the same density, I mean those, whose inertias are in the proportion of their bulks." It is to be observed, also, that in this passage Newton does not say that the small solid particles, which he assumes to be of the same density, are all of the same size. If all were assumed to be of the same size, then the densities of bodies would be proportional to the numbers of such small particles in equal volumes. Hoppe attributes this latter concept of density to Newton, and claims that it is found earlier in the writings of François Lubin, John Kepler, Pierre Gassendi and Robert Boyle.[3]

But Newton's corpuscular idea, as described in his *Opticks,* goes against Hoppe's interpretation of Newton. In his *Opticks* (third edition, 1721, pp. 375-376), he says: "It seems probable to me, that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties and in such proportion to space, as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them; even so very hard, as never to wear or break in pieces: no ordinary power being able to divide what God himself made one in the first creation."

In the use of the concept of mass, as distinguished from weight, Newton has forerunners who perceived the difference between mass and weight more or less clearly. Crew finds the earliest quantitative conception of this idea in Huygens' discussion of centripetal force, in 1673, which was fully discussed in his posthumous *De vi centrifuga,* 1703. Huygens states that when particles move with equal speeds along equal circles, the centripetal forces are to each other as "the weights of the particles" or as their "solid quantities"—*sicut mobilium gravitates seu quantitas solidas.* Here the "solid quantity" indicates mass. Hoppe claims the concept of mass for Kepler, who designates it by the word *moles* as in the following quotation from Kepler's *Astronomia nova* (1609): "If two stones were removed to any part of the world, near each other but outside the field of force of a third related body, then the two stones, like two magnetic bodies, would come together at some intermediate place, each approaching the other through a distance proportional to the mass [*moles*] of the other."[4]

[1] E. Mach, *Die Mechanik in ihrer Entwicklung* (ed. 8), Leipzig, 1921, chap. 2, § 3, p. 188.

[2] H. Crew, *The Rise of Modern Physics,* Baltimore, 1928, p. 124.

[3] E. Hoppe, *Archiv für Geschichte der Mathematik, der Naturwissenschaften und der Technik,* n.s., vol. 11, 1929, pp. 354–361. For further statements of Newton on the constitution of matter, consult *Sir Isaac Newton, 1727–1927, A Bicentenary Evaluation of His Work,* Baltimore, 1928, pp. 224, 225.

[4] J. Kepler, Introduction to *Astronomia nova,* 1609, *Opera omnia* (ed. Ch. Frisch), vol. 3, p. 151; *Kepler's Neue Astronomie,* übersetzt von Max Caspar, München-Berlin, 1929, p. 26.

12 (p. 1). Book I, Definition II. *Quantity of motion,* as the expression is used in the *Principia,* is equivalent to the term *momentum* in more modern mechanics, and is measured by the product of mass and velocity.

13 (p. 6). Scholium following Definition VIII. *Absolute motion and absolute time.* Newton pointed out that "the parts of that immovable space, in which those [absolute] motions are performed, do by no means come under the observation of our senses." But he adds, "yet the thing is not altogether desperate; for we have some arguments to guide us, partly from the apparent motions, which are the differences of the true motions; partly from the forces, which are the causes and effects of the true motions, etc." In the light of more recent thought the question arises, in connection with rectilinear motion, whether the existence of "apparent" or relative motion, as revealed to our senses, necessarily carries with it the existence of absolute motion, as vaguely suggested by Newton. Or is it not possible that relative motion is the only rectilinear motion that exists? Take automobiles A, B, and C. Suppose B gains on A with a velocity of 10 kilometers per hour, while C, traveling along the same straight road, in the same direction, gains on A with a velocity of 15 kilometers per hour. From the relative velocity of 5 kilometers per hour, which is the difference of the velocities 15 and 10, we cannot ascertain the velocity of A; A may be at rest on the road, or moving. Of importance in this argument is the circumstance that, from the velocity of A, or from

its state of rest on the road, the inference cannot be drawn by syllogism that such velocity or rest is absolute. "Absolute motion," says Newton, "is the translation of a body from one absolute place into another," and "absolute rest is the continuance of the body in the same part of... immovable space." The existence of absolute motion or rest cannot be established merely from the existence of relative motion or rest. In our illustration of automobile motion, we know that the road itself is in motion, being carried by the earth in its orbit, and so on. Thus, we are forced back to Newton's own admission, that there is no way of bringing absolute motion or absolute space under the observation of the senses. Newton does not mention a universal ether in his discussion of absolute motion, but he might have argued, as has been done since, that motion through such an ether constitutes absolute motion. Here two remarks come to mind: the existence of such an hypothetical ether has been denied in the eighteenth and twentieth centuries; the motion through this ether cannot be said to proceed "under the observation of the senses."

More convincing is Newton's remark on absolute rotation. Two globes are kept by a cord at a given distance apart and are revolved about their common centre of gravity. From the tension of the cord the angular velocity may be determined. Here we have a rotation, resulting from a dynamical experiment more or less familiar through sense-perception, which makes no reference to terrestial, solar, or stellar positions, and seems therefore absolute. It is absolute in somewhat the same sense as Foucault's pendulum may be said to establish the earth's absolute rotation. If this view is correct, then Newtonian dynamics dealt with a rotation which was truly absolute, nevertheless empirical. Does it not follow, one is tempted to ask, that the space in which absolute rotation takes place must itself be absolute? Newton does not draw such an inference, but commentators have declared that the absoluteness of rotation and acceleration compelled Newton to recognize that space could not be relative; for, otherwise, space would have a dual structure, relative for rectilinear motion, absolute for rotation.

Remarks similar to those which I applied to absolute rectilinear motion bear on the discussion of absolute time. It would seem to follow, therefore, that the existence of absolute rectilinear motion and of absolute time are postulates made in Newtonian mechanics; they are not based on experimental evidence and may therefore be said to be metaphysical. There appears to be no *a priori* argument against acceptance as a foundation in mechanics of concepts, some of which are observable and others unobservable or metaphysical. The two types of concepts might form a perfectly solid and coherent structure which yields results in accord with observational data, to a degree of accuracy lying within the probability of experimental error. Indeed, Newton's assumptions satisfied this test in the scientific developments extending over a period of two hundred years. During that time, astronomy and physics made tremendous strides forward. Celestial mechanics flourished; so did engineering and physical science.

On esthetic grounds or on grounds of mistrust of metaphysics, it might be said that an empirical science should be based only on observable phenomena. Religious fears caused Bishop Berkeley, in his *Principles of Human Knowledge* (1710) and in his *Analyst* (1734), to object to absolute space. More recently the desirability of a purely empirical foundation was stressed by Ernst Mach in his *Die Mechanik*.[1]

In the nineteenth century the researches of Faraday and Maxwell on electromagnetism led to experimental results which could only be explained on the assumption of the existence of relative motion. A moving magnet gives rise to a magnetic field and induces an electric current in neighboring conductors which it passes. This is the fundamental phenomenon in dynamos generating currents. Can the velocity of the magnet be considered absolute? "Absolute motion," according to Newton, is "translation of a body from one absolute place to another"; now "place" is absolute when the "space" is absolute, and "absolute space" exists "in its own nature, without regard to anything external." Now a magnet, if in absolute motion, "without regard to anything external" (not even a neighboring conductor which it passes), could not generate an electric current. If, instead of a moving magnet, we consider a moving charge of electricity, similar remarks apply. Plainly, electromagnetic phenomena invoke velocities that are "relative." Such considerations did not, however, rule out "absolute velocity" from physical science, for other phenomena might need the concept of absoluteness, and it was not yet recognized that all atoms and therefore all matter are really electrical.

A more serious situation arose near the close of the nineteenth century. The luminiferous ether of Newton, Huygens, and Hooke in the seventeenth century, which had been discarded by most scientists in the eighteenth century, was reinstated in the nineteenth century. The prevailing belief was that this ether was stagnant, and that the earth could move through it without dragging the ether along. In the minds of many, this stagnant ether constituted a fundamental frame of reference in the explanation of absolute motion. But the stagnant ether was not altogether satisfactory and a few physicists, such as G. G. Stokes, advocated an ether which is dragged as is water by a moving ship. Could this question be settled by experiment? To answer this question, Michelson and Morley in 1887 performed the now famous experiment[2] at Cleveland, Ohio, which Michelson is reported to have called an "unfortunate experiment," for it did not yield itself to satisfactory treatment in the old Newtonian mechanics. If the earth did not drag the ether, there would be an ether wind, the so-called "ether drift." The result of the test showed no such "drift," so that, as interpreted at that time, the earth in the Cleveland cellar dragged the ether along with it. Such a result had not been expected; it seemed to indicate properties of the ether which it was impossible to reconcile with properties required to explain other known phenomena, such as Bradley's aberration of light and the rectilinear path of vertical rays. For nearly twenty years this experiment was a cloud in the scientific firmament.

Perhaps the nature of the Michelson and Morley experiment may be brought to mind best by the statement that, just as a man swimming upstream a given distance and back again requires more time than if swimming in still water, so a ray of light traveling a given distance against an ether wind and back again requires more time than if the ether had been at rest with respect to the apparatus. It is assumed that the swimmer (ray of light) moves always with the same velocity relative to the water (ether). But Michelson and Morley's delicate interferometer indicated no difference of time: hence the inference that there was no "ether drift."

In 1892, G. F. Fitzgerald[3] of Dublin and H. A. Lorentz[4] of Leyden, independently, made the audacious and seemingly arbitrary assumption that a moving body contracts along the line of its motion. A yardstick is shorter when moving in the direction of its length than when it is at rest. On this assumption the Michelson and Morley experiment could be explained, even though the ether was not moving with the earth. But physicists in general did not derive much contentment from this contraction theory. Twelve years passed and then Albert Einstein, at that time in Zurich, advanced his special relativity theory.[5] He built this theory on purely observational foundations, which should explain and coördinate all known phenomena of light, particularly the Michelson and Morley experiment. That trouble-maker, the nineteenth-century luminiferous ether, he cast aside as being purely hypothetical. He discarded also Newton's rectilinear *absolute* motion as having no observational basis. He felt justified in postulating that the velocity of light in a vacuum is constant and independent of the motion of its source. This independence was shown later to exist by Willem de Sitter,[6] by observations on double stars. The second assumption of Einstein was the "principle of relativity" in the restricted sense: If relative to one coördinate system, a second is a uniformly moving coördinate system devoid of rotation, then natural phenomena run their course with respect to the second system according to the same general laws as with respect to the first system. In the dynamics of this theory, the velocity of light plays a leading rôle. A train is traveling on a rectilinear railroad track. Lightning has struck the rails at two places A and B far distant from each other. A man on the track, who happened to be at the midpoint M of the distance AB, perceives the two flashes of lightning at the same time and calls them simultaneous. Let M′ be the midpoint of the distance AB on the moving train. Will an observer on the train, placed at M′, find the two flashes simultaneous? No! For he is traveling on the train toward B, and therefore is moving toward the beam of light coming from B, and away from the beam coming from A. Hence the observer on the train comes to the conclusion that the flash B took place before the one at A. Thus, events simultaneous with reference to the railroad track were not simultaneous with reference to the train. Simultaneity is relative. Every reference body or coördinate system has its own particular time. The statement of the time of an event is not independent of the state of motion of the body of reference; it is not absolute. But in the Newtonian physics a statement of time was given an absolute significance. Einstein's special theory of relativity yields mathematical results in agreement with the Fitzgerald-Lorentz contraction. This is not strange, for all three physicists aimed to make provision for the phenomena revealed by the Michelson and Morley experiment. Lorentz also established equations relating to distances and times of a coördinate system C′ (the uniformly moving train), expressed in terms of the coördinate system C (the rectilinear railroad track). These equations, known as the "Lorentz transformations,"[7] fit into Einstein's special theory of relativity. I give below in parallel columns the values x', y', z', t' of an event with respect to the coördinate system C′ when the values x, y, z, t of the same event with respect to C are given. C′ moves with respect to C with a uniform velocity v. The velocity of light in a vacuum is represented by c. The axes of the two systems C and C′ are respectively parallel. We assume for simplicity the event localized on the x-axis.

The Newton Transformations	The Lorentz Transformations
$x' = x - vt$	$x' = \dfrac{x - vt}{\sqrt{1 - \dfrac{v^2}{c^2}}}$
$y' = y$	$y' = y$
$z' = z$	$z' = z$
$t' = t$	$t' = \dfrac{t - \dfrac{v}{c^2} \cdot x}{\sqrt{1 - \dfrac{v^2}{c^2}}}$

Comparing the two sets of equations, one sees that the more recent is much more complicated. Relativity affords an example of a theory which has grown far more involved in consequence of being founded upon purely empirical data. The two systems merge into one when coördinate systems have relative velocities v that are infinitesimal as compared with the velocity of light. It is this fact that enabled the Newtonian mechanics to represent planetary motions to a high degree of approximation and success. For remarks on Einstein's general theory of relativity of 1915, see Note 6 on the Nature of Gravity.

[1] Mach, *op. cit.*, pp. 216–237.
[2] A. A. Michelson and E. W. Morley, in *Silliman's Journal*, ser. 3, vol. 34, 1887, p. 333.
[3] *Scientific Writings of G. F. Fitzgerald*, Dublin, 1902, pp. lx, 562; O. Lodge, *Philos. Trans.*, A, vol. 184, London, 1894, p. 749.
[4] H. A. Lorentz, *Verlagen d. Zittingen d. K. Akademie van Wetenschappen*, Amsterdam, vol. 1, 1892, p. 74.
[5] Einstein, *op. cit.*, contains a popular exposition.
[6] W. de Sitter, *Physikalische Zeitschrift*, vol. 14, 1913, pp. 429, 1267.
[7] Lorentz, *loc. cit.* For a simple derivation of the Lorentz transformation, see Einstein, *op. cit.*, Appendix 1, p. 139.

14 (p. 13). *Laws of Motion.* Because of their importance, I reproduce here the three laws in the original Latin:

Lex I (in editions of 1687 and 1713). Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum illum mutare.

Lex I (in edition of 1726). Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus illud a viribus impressis cogitur statum suum mutare.

Lex II. Mutationem motis proportionalem esse vi motrici impressae, et fieri secundum lineam rectam qua vis illa imprimitur.

Lex III. Actioni contrariam semper et aequalem esse reactionem: sive corporum duorum actiones in se mutuo semper esse aequales et in partes contrarias dirigi.

The first law is frequently called the "law of inertia." Students of relativity point out that we do not know of any body in Nature which is at absolute rest, not on the earth nor on the sun or stars; that there is rest only with respect to some system of coördinates. Einstein gave a critical examination of what he called a "Galileon system of coördinates," a system in which the law of inertia holds relative to it, and in which no gravitational field exists which is therefore not rigidly attached to the earth. "The visible fixed stars are bodies for which the law of inertia certainly holds to a high degree of approximation. . . . The laws of mechanics of Galileo-Newton can be regarded valid only for a Galileon system of coördinates."[1] See Note 13.

[1] Einstein, *op. cit.*, pp. 12, 13.

I give a few additional references to the laws of motion:

Samuel Horsley (the editor of Newton's *Opera*, 1779–1785) gave a metaphysical discussion of Newton's laws of motion, which is printed in *Lord Monboddo and Some of His Contemporaries* (ed. William Knight), London, 1900, pp. 281–284, 298, 302–305.

J. C. Maxwell, *Matter and Motion*, London, 1876.

Sir W. Thomson and P. G. Tait, *Treatise on Natural Philosophy*, Part 1, Cambridge, 1879.

K. Pearson, *Grammar of Science*, London, 1900, pp. 321–327, 533–536; Appendix, Notes 1 and 2.

E. Mach, *Die Mechanik in ihrer Entwicklung*, ed. 8, Leipzig, 1921.

E. Freundlich, *Grundlagen der einsteinschen Gravitations Theorie*, Berlin, 1920, p. 42.

A. S. Eddington, *Space, Time, and Gravitation*, Cambridge, 1920, Chap. IX.

15 (pp. 13, 45). Second Law of Motion. *Force.* By Newton's second Definition, "quantity of motion" (momentum) arises "from the velocity and quantity of matter conjointly," that is, from mv. By Newton's second Law of Motion, "change of motion," that is, change in the quantity of motion, "is proportional to the motive force impressed." Thus, we have "change of motion" as the measure of the force which produces it. Thus arose the measurement of force by the product of mass and acceleration. This concept of force has played a fundamental rôle in mechanics from the time of Newton to the close of the nineteenth century. It will continue to play a basic rôle in mechanics involving velocities that are small in comparison to the velocity of light. But as a concept in general cosmological mechanics it has faded into the background. A most far-reaching experimental result was obtained in 1901 by W. Kaufmann, namely, that the mass of an electron increases rapidly as its speed nears the velocity of light.[1] The invariance of mass in Newtonian mechanics was thus shown to be incorrect. (See Note 11.) The Newtonian force of gravitational attraction between two bodies varies as the product of their masses, and inversely as the square of the distance. This force was rendered ambiguous by recent research, because (1) mass depends on velocity and (2) distance, according to the theory of relativity, depends upon the location of the observer. Einstein's gravitational theory of 1915 undermines the belief in the reality of gravitation as a "force." But his theory of 1915 does not include similar treatment of electromagnetic forces. Generalizations of Einstein's theory of 1915, to embrace also electromagnetic forces, were made in somewhat different ways by H. Weyl[2] in 1918, by Eddington[3] in 1921, and by Einstein[4] himself in 1929.

[1] W. Kaufmann, *Göttinger Nachrichten,* Nov. 8, 1901; see also the volumes for 1902 and 1903.

[2] H. Weyl, *Sitzungsberichte der Preuss. Akademie d. Wissensch.,* Phys.-Math. Klasse, 1918, p. 465.

[3] E. Eddington, *Proceedings of the Royal Society of London,* A 99, 1921, p. 104.

[4] Einstein, "Zur einheitlichen Feldtheorie," *Sitzungsberichte der Preuss. Akademie d. Wissensch.,* Phys.-Math. Klasse, 1929, I.

16 (pp. 21, 36). Book I, Scholium and Lemma XI. *Obsolete mathematical expressions and notations.* In Newton's Latin editions of the *Principia,* as well as in Motte's translation into English, there occur certain mathematical expressions which are no longer used in mathematics and are therefore not immediately understood by a reader familiar only with modern phraseology. I have altered the translation by substituting for the old, corresponding modern terminology. Most frequent of the obsolete terms are "duplicate ratio," "subduplicate ratio," "triplicate ratio," "subtriplicate ratio," "sesquiplicate ratio," "subsesquiplicate ratio," "sesquialteral ratio." For these I have used, respectively, the terms "square of the ratio," "square root of the ratio," "cube of the ratio," "cube root of the ratio," "3/2th power of the ratio," "2/3th power of the ratio," "ratio of 3 to 2." In a

few rare occurrences, the old usage of the term "proportion" corresponds to the modern "ratio."

I have discarded the vinculum as the sign of aggregation and have introduced round parentheses instead. In some places where the reasoning is unusually involved, I have introduced the modern notation for proportion, $a : b = c : d$, in place of the rhetorical form used in the *Principia*. For the sign of multiplication (×) regularly occurring in the original *Principia,* I have introduced the dot (·), placed halfway up the height of the short letters to distinguish it from the decimal point, which is placed on the lower level. In a few places I have employed the solidus in printing fractions.

17 (p. 22). Book I, Scholium after the Laws of Motion. *Wren and Mariotte.* The reader may wish to consult P. G. Tait's "Note on a Singular Passage in the Principia," *Proceedings of the Royal Society of Edinburgh,* vol. 13, 1886, pp. 72–78, dealing with questions of priority.

18 (p. 63). Book I, Prop. XVI, Cor. IV. By the *mean distance* from the focus of an ellipse is meant the major semiaxis or the distance SB from the focus to an extremity of the minor axis BC. This distance is also the radius of the circle in question. The proof of Cor. IV follows immediately from Cor. VI, Prop. IV, as indicated in the text. The last two sentences in Cor. IV show the consistency of this result with Prop. XVI, according to which the velocity in the elliptic orbit is to that in the circular orbit as $\frac{\sqrt{L}}{BC} : \frac{\sqrt{l}}{SB}$, where L and l are the latera recta of ellipse and circle, respectively. But $BC : SB = \sqrt{(SB \cdot L)} : \sqrt{(SB \cdot l)}$. Hence the velocities as derived from Prop. XVI are inversely as the square roots of the equal distances SB, and are therefore themselves equal.

19 (p. 76). Book I, Lemma XVII. *Trapezium.* Newton used the word "trapezium" in the Euclidean sense as any quadrilateral except a square or parallelogram.

20 (p. 90). Book I, Lemma XXII. *Geometry of conics.* For a statement of the rôle that this Lemma and other parts on the geometry of conics given in the first Book of the *Principia* play in the history of synthetic geometry, consult J. J. Milne, "Newton's Contribution to the Geometry of Conics," in *Isaac Newton, 1642–1727,* London, 1927, pp. 96–114; also Ernst Kötter, *Die Entwicklung der synthetischen Geometry, von Monge bis auf Staudt,* Leipzig, 1901, pp. 8, 9, 30, 39, 41, 109, and in other places indicated in the alphabetical index under "Newton." Consult also Michel Chasles, *Aperçu historique sur l'origine et le développement des méthodes en géométrie;* R. H. Graham, "Newton's Influence on Modern Geometry," *Nature,* vol. 42, 1890, pp. 139–142; C. Taylor, "The *Principia* and Modern Geometry," *Cambridge Philosophical Society Proceedings,* vol. 3, 1880, pp. 359, 360.

Newton's Lemma XXII aims mainly to transfer certain points in the figure to infinity.

21 (p. 110). Book I, Lemma XXVIII. *Oval figures.* Brougham and Routh[1] point out that in this Lemma Newton tried to prove that no figure oval in form, or no continuous curve confined to the finite part of a plane and returning into itself, is capable of definite quadrature. In other words, Newton endeavored to show that the area of such a curve cannot be expressed in terms of rational numbers or of irrationals arising from the solution of algebraic equations with rational coefficients, but can be expressed only in transcendental numbers such as π, which arises as a factor in the area of a circle. Brougham and Routh state that the conclusiveness of Newton's reasoning has been questioned and they cite, as an example, the curve,

$$y^m = x^{(n-1)^m} \cdot (a^n - x^n),$$

where m and n are even positive integers; this curve satisfies Newton's specifications, nevertheless admits of exact quadrature. Consider, for simplicity, the special statement when $m = n = 2$ and a is a positive integer. The curve $y^2 = x^2(a^2 - x^2)$ has, for the segment $o < x \overline{\overline{\lessgtr}} a$, the area,

$$A = \int_o^x 2y\,dx = -\tfrac{1}{3}(a^2 - x^2)^{3/2}\Big|_o^x = \tfrac{1}{3}a^2 - \tfrac{1}{3}(a^2 - x^2)^{3/2}.$$

Removing the radical, one obtains an algebraic equation of the second degree with respect to the required area A. Hence the area of this segment, though in general irrational, is not transcendental for algebraic values of x. If $x = a$, one has as the area of the entire oval $\tfrac{1}{3}a^2$, which is rational.

Newton's passage, "The equation by which any one intersection of two lines is found at the same time exhibits all their intersections by as many roots, and therefore rises to as many dimensions as there are intersections," is open to objection *in its application to the spiral*, a transcendental curve.

[1] H. Brougham and E. J. Routh, *Analytical View of Sir Isaac Newton's Principia*, London, 1855, pp. 72–74. See also Ball, *Essay*, p. 83.

22 (p. 113). Book I, Prop. XXXI and Scholium. *Kepler's problem*. Newton deals here with a famous problem first arising in John Kepler's *Astronomia nova*,[1] 1609. It calls for the determination of x in the equation

$$x - e \sin x = z,$$

where e and z are given. The astronomer J. C. Adams described in an article[2] two rapid methods of approximation, and then points out, "that the first method explained above is exactly equivalent to that given by Newton in the *Principia* at pp. 101, 102 of the second edition, and at pp. 109, 110 [first part of the Scholium] of the third edition, when Newton's expressions are put into the modern analytical form. None of the subsequent authors, however, mention this method as being Newton's, the unusual form in which Newton's solution is given having, no doubt, caused them to overlook it. In the first edition of the *Principia* a modification of the method is given which was, I have no doubt, intended by Newton to be equivalent to the second method given above, but by some inadvertence, instead of the denominator of $\delta x'$ being

$$1 - e\cos(x' + \tfrac{1}{2}\delta z')$$

when expressed in the above notation, he takes it to be what is equivalent to

$$1 - e\cos(x + \tfrac{1}{2}e\sin x'),$$

which is only true for the first approximation when x_0 is taken $= z$."

The first method referred to is described by Adams thus:

"The equation to be solved by successive approximations is

$$x - e\sin x = z,$$

where z is the known mean anomaly, e the eccentricity, and x the eccentric anomaly to be determined.

"Suppose x_0 to be an approximate value of x, found whether by estimation, by graphical construction, or by a previous rough calculation, and let

$$x_0 - e\sin x_0 = z_0.$$

Then if $$\delta x_0 = \frac{z - z_0}{1 - e\cos x_0}$$

and $$x' = x_0 + \delta x_0,$$

x' will be a much more approximate value of x than x_0.

"Similarly, if we put $\quad x' - e\sin x' = z'$

and if $$\delta x' = \frac{z - z'}{1 - e\cos x'}$$

and $$x'' = x' + \delta x'$$

x'' will be a much more approximate value of x than x', and so on. . . ."

Kepler's problem has engaged the attention of many astronomers; 123 papers are listed in a bibliography of the problem in the *Bulletin Astronomique,* Paris, vol. 17, 1900, pp. 37–47. Several tables for solving the problem are extant, the best of which is J. J. Astrand's *Hülfstafeln zur leichten und genauen Auflösung des Kepler'schen Problemes,* Leipzig, 1890.

[1] *Opera,* vol. 3, p. 411; *Kepler's Neue Astronomie,* pp. 358, 414, 52.*

[2] J. C. Adams, "On Newton's Solution of Kepler's Problem," *Monthly Notices of the Royal Astronomical Society,* London, vol. 43, 1882, pp. 43–49.

23 (p. 141). Book I, Prop. XLV, Cor. II. *Motion of the apsides in orbits.* In the second and third editions of the *Principia* occurs the sentence, "The apse of the moon is about twice as swift" (*Apsis lunae est duplo velocior circiter*), that is, twice as swift as the angle 1° 31′ 28″, which marks the forward motion of the apse in one revolution of the body in its elliptical orbit. It is noteworthy that this sen-

tence, "The apse of the moon is about twice as swift," does not occur in the first edition of the *Principia,* and that the angle given there is 1° 31′ 14″. We have here the celebrated discrepancy between the computed and the observed motion of the apsides of the moon. The observed value is about double the computed value. The computation is based upon the addition of 1/357.45th part of the centripetal force, by which the earth attracts the moon, this addition being caused by the solar attraction upon the moon.

The moon problem commanded the attention of Newton for many years.[1] About 1694 he obtained some observational data from the astronomer Flamsteed at Greenwich, for the purpose of verifying the results of his mathematical inquiries, but desired more. Flamsteed, on the contrary, was more inclined to hold back his observations for a while until all should appear in book form, as a monument of his labors as an observational astronomer. This conflict of interests, combined with Newton's irritability at that time in personal matters, and Flamsteed's sensitiveness because of serious physical weakness, led to the regrettable controversy between these two great men, in which many hard words were used.[2]

The discrepancy between theory and observation in the motion of the lunar apsides seems to have been overcome by Newton, but nowhere in any edition of the *Principia* does he explicitly give the explanation, although in Book III, Prop. XXV, he gives figures which would make the annual progression of the line of the apse nearly what it actually is. There, Newton estimates that the total disturbing force of the sun on the moon is to the centripetal force of the earth in the ratio of 1 to $178^{29}/_{40}$; this ratio yields fairly close results for the apsidal motion.

That Newton had overcome the chief difficulty in the motion of the lunar line of the apsides follows also from his remark in Book III, Prop. XXIII: "But the motions of the apsides thus found must be diminished in the proportion of 5 to 9, or of about 1 to 2, on account of a cause which I cannot here stop to explain" *(Diminui tamen debet motus augis sic inventus in ratione 5 ad 9 vel 1 ad 2 circiter ob causam quam hic exponere non vacat)*. Pemberton, who edited the third edition of the *Principia,* suggested that there be added "a brief hint at the principle whence the precept contained in this line was deduced," but Newton did not act on this suggestion. Adams remarks that if Newton had complied with this suggestion, "all the difficulties connected with the motion of the moon's apogee would have been avoided."[3]

In the *Portsmouth Collection of Books and Papers, Cambridge,* 1888, pp. xi-xiii, xxvi-xxx, and in section I, division IX, numbers 7, 12, there are elaborations[4] which according to Ball did not seem to have satisfied Newton, if one may judge by the alterations made in the manuscript, but which nevertheless yield as the mean annual motion of the apogee 38° 51′ 52″, while the observed motion was 40° 41′ 30″.

Astronomers at that time had no means of knowing what results, if any, Newton had reached privately; all they could know were the published results as found in the *Principia*. The motion of the lunar apsides stood out as a problem not fully accounted for, on Newton's law of gravitation. Nearly twenty years passed before any substantial advance was made, but, when finally under way, it came in a sensational manner. The French mathematician, Alexis Claude Clairaut, in 1752 gained a prize of the St. Petersburg Academy for a paper on the theory of the moon, which explained the motion of the apsides.[5] He was the first to apply modern analysis to the theory of the moon. At first the motion of the apsides seemed to Clairaut wholly inexplicable by Newton's law. He started boldly to advance a new hypothesis regarding gravitation, by assuming the centripetal force to be not as $\frac{1}{d^2}$, but as $\frac{1}{d^2}+\frac{1}{d^4}$. But before proceeding far on this new theory he took the precaution to carry his calculation, using Newton's law, to a higher degree of approximation, and, behold, in 1749, he reached results agreeing with observation. The agreement was rightly looked upon as a brilliant triumph for the Newtonian theory. A full investigation of the problem was given by Laplace in his *Mécanique céleste*.[6]

Of interest is a personal remark of Newton's, in connection with lunar theory; he told Halley that the theory of the moon made his head ache and kept him awake so often that he would think of it no more.[7] But we have shown that Newton did continue to give attention to the irregularities of the moon. Lunar theory has caused headaches to astronomers of later date. In our own century, such moon specialists as E. W. Brown and W. de Sitter have found unexpected and unexplained shifts of the moon from its predicted positions. The moon's motion has not yet been exactly explained by gravitational theory. It has been conjectured that the cause of the differences between the prediction and observation lies in the variation of the earth's rate of rotation, probably caused by slight periodic changes in the shape of the earth.[8]

[1] For a historical paper by S. B. Gaythorpe, "On Horrocks's Treatment of the Evection and the Equation of the Centre, with a Note on the Elliptic Hypothesis of Albert Curtz and its Correction by Boulliau and Newton," containing several references to the different editions of Newton's *Principia*, see *Monthly Notices of the Royal Astronomical Society*, London, vol. 85, 1925, pp. 858–865.

[2] See F. Baily, *An Account of the Revd. John Flamsteed*, London, 1835, pp. 69, 217; Brewster, *op. cit.*, pp. 163–183.

[3] Brewster, *op. cit.*, vol. 2, p. 418.

[4] See also Ball, *Essay*, pp. 85, 109.

[5] Clairaut, *Théorie de la lune*, Pétersbourg, 1752; second edition, Paris, 1765, where, on page 5, are given historical notes. For Clairaut's earlier papers see *Mémoires de l'académie des sciences* for the year 1745, Paris, 1749, p. 577, also a still earlier paper on pp. 329–364, and a criticism of this by Buffon on pp. 493–500; Clairaut's reply to Buffon on pp. 529–548 and Buffon's replication, pp. 551–552. Clairaut wrote on the orbit of the moon in these *Mémoires* for the year 1748, Paris, 1752, pp. 421–440. See also Brougham and Routh, *Analytical View . . . Newton's Principia*, pp. 89–98; E. W. Brown, *An Introductory Treatise on the Lunar Theory*, Cambridge, 1896, pp. 2, 6, 127, 237, 238.

[6] P. S. Laplace, *Mécanique céleste*, 1799–1825, Tome III, Livre VII, pp. 169–303; *Œuvres de Laplace*, Tome III, Paris, 1878, pp. 181–323.

[7] Brewster, *op. cit.*, p. 108.

[8] Consult P. A. Hansen, "Darlegung der theor. Berechnung d. in den Mondtafeln angewandten Störungen," *Abhandl. d. math.-phys. Classe der k. sächsisch. Gesellschaft d. Wissenschaften*, Leipzig, vol. 6, 1864, vol. 7, 1865; Simon Newcomb, *Astronomical papers for the use of the American Ephemeries*, vol. 9, Pt. I, p. 18; papers in the *Monthly Notices of the Royal Astronomical Society*, London, by E. W. Brown, vol. 73, 1913, p. 692; vol. 74, 1914, pp. 156, 362, 396, 424, 552; vol. 75, 1915, p. 506; and by A. C. D. Crommelin, vol. 83, 1923, p. 359. Also the article, "Moon," by J. Jackson in the *Encyclopaedia Britannica*, ed. 14.

24 (p. 167). Book I, Prop. LX. *Two mean proportionals.* If $a : x = x : y = y : b$, then x and y are said to be "two mean proportionals" between a and b; x is said to be the "first" of the two mean proportionals. We get $x^2 = ay$ and $y^2 = bx$. Eliminating y, $x = \sqrt[3]{(a^2b)}$. If $a = S + P$ and $b = S$, then, in Prop. LX, the principal axis of P's ellipse when S moves, is to the principal axis of P's ellipse when S is stationary (the periodic time of P remaining the same), as $S + P : \sqrt[3]{\left((S+P)^2S\right)}$.

25 (p. 197). Book I, Prop. LXXV; also Book III, Prop. VIII. *Attraction between spheres.* Newton's determination of the attraction of a solid sphere on an external point is more fully explained, by the aid of modern analysis, in a paper by A. N. Kriloff in the *Monthly Notices of the Royal Astronomical Society,* London, vol. 85, 1925, p. 571.

The theorem that two spheres attract each other by a force varying inversely as the square of the distance between their centers has been shown by James Pierpont to hold also in elliptic space. See Pierpont's paper in the *Bulletin of the American Mathematical Society,* vol. 35, 1929, pp. 351–356.

26 (p. 228). Book I, Prop. XCVI. *Velocity of light.* Newton arrives at the conclusion that the velocity of light in a denser medium is greater than in a rarer medium. But Jean Léon Foucault proved in 1850 that the velocity of light is less in water than in air. This was looked upon by nineteenth-century physicists as an *experimentum crucis* which definitely disproved Newton's emission theory. The twentieth century led to a revival of corpuscular theories of light, and Foucault's experiment came to be no longer considered decisive. Newton had assumed in the *Principia* that the corpuscle of light is "attracted or impelled perpendicularly towards either of those mediums, and not agitated or hindered by any other force." To arrive at a result in accordance with Foucault's experiment, it is only necessary to assume that the corpuscle meets with a different reception. As Alex. Wood[1] pointed out, it may be that the flying corpuscle is not attracted at all; it may be that when it reaches the surface of the water, the component of its velocity parallel to the surface is *diminished* by action of a frictional kind, while the component of its velocity perpendicular to the surface remains unchanged. According to such an assumption, the velocity of the corpuscle in water would be less than in air, as demanded by experiment. In 1924 the theory of "wave mechanics" was advanced, which endeavors to unify the corpuscular theory and the

wave theory of light. For an explanation of Foucault's experiment on the theory of "wave mechanics," see Ludwig Flamm in *Die Naturwissenschaften,* vol. 15, July 15, 1927.

[1] A. Wood, *In Pursuit of Truth,* London, 1927, p. 47.

27 (p. 230). Prop. XCVI, Scholium. *Diffraction of light.* The phenomenon which Newton describes as "inflection" of light is now called "diffraction" of light.

28 (p. 236). Book II, Prop. II. *Resistance proportional to velocity.* If, for the successive equal time intervals, the velocities are v_1, v_2, v_3, v_4, etc., and the resistances are cv_1, cv_2, cv_3, cv_4, etc., then, $v_2 = v_1 - cv_1$, $v_3 = v_2 - cv_2$, $v_4 = v_3 - cv_3$, etc.

Therefore, $1 : c = v_1 : v_1 - v_2 = v_2 : v_2 - v_3 = v_3 : v_3 - v_4 =$ etc.,

and by Lem. I, Book II, $v_1 : v_2 = v_2 : v_3 = v_3 : v_4 =$ etc.,

and $v_1 : v_3 = v_1v_2 : v_2v_3 = 1 : c^2$,

$v_1 : v_4 = v_1v_2v_3 : v_2v_3v_4 = 1 : c^3$, etc.

Also, $v_2 = v_1(1-c),\ v_3 = v_1(1-c)^2, \ldots, v_{n+1} = v_1(1-c)^n$.

29 (p. 247). Book II, Prop. VII. *Resistance of spheres.* Let the initial velocity of a body be V and its subsequent velocity v, also let the initial resistance be R and its subsequent resistance be r, and t the time. For a second body, let the corresponding symbols be V_1, v_1, R_1, r_1, and t_1. By supposition,

$$V^2 : v^2 = R : r$$

$$V_1^2 : v_1^2 = R_1 : r_1$$

$$t : t_1 = \frac{V}{R} : \frac{V_1}{R_1}.$$

Then the loss in velocity, $-dv$, of the first body in time, dt, is

$$-dv = r\,dt = \frac{Rv^2}{V^2}dt.$$

Dividing by v^2 and integrating,

$$v^{-1} = \frac{R}{V^2}t + c.$$

When $t = 0$, $v = V$; hence, $C = V^{-1}$.

Simplifying, $$v = \frac{V^2}{Rt + V}.$$

Hence the loss of velocity is,

$$V - v = \frac{VRt}{Rt + V}.$$

For the second body,

$$V_1 - v_1 = \frac{V_1R_1t_1}{R_1t_1 + V_1} = \frac{V_1Rt}{Rt + V}.$$

Therefore the losses in velocities of the two bodies in times t and t_1 are to each other as their initial velocities V and V_1.

To prove the second part, relating to the spaces s, use the value given above for v, in $ds = vdt$; integrating,

$$s = \frac{V^2}{R}\left\{\log(Rt+V) - \log V\right\}.$$

Writing the corresponding formula for the second body, and simplifying, one obtains

$$s : s_1 = Vt : V_1 t_1. \qquad \text{Q.E.D.}$$

30 (p. 249). Book II, Lemma II. *Fixed infinitesimals.* For the benefit of readers interested in the early history of the calculus in England, I quote certain parts of Augustus De Morgan's article "On the Early History of Infinitesimals in England," published in the *Philosophical Magazine*, ser. 4, vol. 4, 1852, pp. 321–330. De Morgan proves that in Newton's early papers and in the first edition of the *Principia*, infinitely small quantities (that is, fixed infinitesimals) are freely used: "Up to the year 1704, and so far as algebraical calculus was concerned, Newton himself used infinitely small quantities; and nothing else in any document yet published. The prime and ultimate ratios, or limits, appear in the *Principia*, but are abandoned in those places in which fluxions are alluded to. I proceed to establish these assertions in detail." I premise that a fluxion, as used by Newton, is a *velocity*, or a time-derivative. The fluxion which Newton denoted by $\dot{x}$ is now written $\frac{dx}{dt}$. In the *Principia* Newton's dot-notation for fluxions does not occur. I quote further from De Morgan: "In the first edition of the *Principia* (1687) the description of the fluxions is founded on infinitesimals, and in the second (1713) this foundation is somewhat altered. In the first, moments are *infinitely small quantities;* in the second, it is not clear what else they are. As in the following extract from the first edition, with its substitute in the second:

First Edition (Book II, Lemma II)

"Cave tamen intellexeris particulas finitas. *Momenta, quam primum finitae sunt magnitudinis, desinunt esse momenta. Finiri enim repugnat aliquatenus perpetuo eorum incremento vel decremento.* Intelligenda sunt principia jamjam nascentia finitarum magnitudinum."

Second Edition (ditto)

"Cave tamen intellexeris particulas finitas. *Particulae finitae non sunt momenta sed quantitates ipsae ex momentis genitae.* Intelligenda sunt principia jamjam nascentia finitarum magnitudinum."

I venture the following translations:

But take care not to look upon finite particles as such. Moments, as soon as they are of finite magnitude, cease to be moments. To be given finite bounds is in some measure contrary to their continuous increase or decrease. We are to conceive them as the just nascent principles of finite magnitudes.

But take care not to look upon finite particles as such. Finite particles are not moments, but the very quantities generated by the moments. We are to conceive them as the just nascent principles of finite magnitudes.

De Morgan comments: "Through the difficulty of the phrases in both extracts this much distinctly appears, that in the first edition the moments, or momentaneous increments, are infinitely small quantities: and this is what I assert.... Newton commenced with the infinitesimal system in as absolute form as did Leibniz, so far as infinitely small quantities of the first order are concerned. Further than these he did not go; and the early distinction between the systems of the two is this, that Newton, holding to the conception of the velocity or fluxions, used the infinitely small increment as a means of determining it; while, with Leibniz, the relation of the infinitely small increments is itself the object of determination."

In 1704 appeared Newton's *Quadratura curvarum* with an Introduction explaining the foundations of fluxions. Here Newton tried to avoid all use of infinitely small constants. We behold in this publication the high-water mark of Newton's efforts to place the doctrine of fluxions upon a thoroughly logical basis. He said, "The very smallest errors in mathematical matters are not to be neglected." De Morgan remarks, "In 1704 Newton in the *Quadratura curvarum* renounced and abjured the infinitely small quantity; but he did it in a manner which would lead one to suppose that he had never held it."[1]

In all three editions of the *Principia* which appeared within Newton's lifetime, quantities that are infinitely small in comparison to others, are dropped. See, for example, Book I, Prop. XXXIX, Cor. III; Prop. XLV, Exam. 2 and 3; also Book II, Prop. X and XV.

[1] For fuller discussion see F. Cajori, *A History of the Conceptions of Limits and Fluxions in Great Britain*, Chicago, 1919, pp. 2–32.

31 (p. 250). Book II, Lemma II. *Fluxions in the Principia.* At this place also in Book I, Lemmas I, II, XI (Scholium), Newton makes a brief general statement of the principles of fluxions and fluents (that is, of the differential and integral calculus), but he does not give his notation. No one is known to have obtained from the *Principia* alone a working knowledge of fluxions. It is generally supposed that he deduced many of his theorems in the *Principia* with the aid of his theory of fluxions and fluents, and afterwards translated his results into the syn-

thetic form. It is interesting to note that in the *Principia* itself there is a confirmation of this supposition. In Lemma II of Book III, he twice gives "a fluent quantity, whose fluxion is ...."

32 (p. 251). Book II, Lemma II, Cor. I. If A, B, C, D, E are *continually proportional,* then, by definition,

$$A : B = B : C = C : D = D : E.$$

Suppose C is given, and suppose $D = CX$, then

$$A = CX^{-2},\ B = CX^{-1},\ D = CX,\ E = CX^2.$$

The moments are

$$a = 2CX^{-3}x = -2A \cdot \frac{x}{X}$$

$$b = -CX^{-2}x = -B \cdot \frac{x}{X}$$

$$d = Cx = D \cdot \frac{x}{X}$$

$$e = 2CXx = 2E \cdot \frac{x}{X}.$$

Hence, $a : b : d : e = -2A : -B : D : 2E$.

33 (p. 251). Book II, Prop. VII, Scholium. *Newton and Leibniz on the invention of the calculus.* In the first two editions of the *Principia,* the Scholium to Prop. VII is wholly different from the Scholium given in the third edition (1726) and translated by Motte. These changes are important in the history of the controversy on the priority and independence of the invention of the differential calculus by Newton and Leibniz. The Scholium in the first edition was as follows:

"In letters which went between me and that most excellent geometer, G. W. Leibniz, ten years ago, when I signified that I was in the knowledge of a method of determining maxima and minima, of drawing tangents, and the like, and when I concealed it in transposed letters involving this sentence (Data aequatione quotcunque; fluentes quantitates involvente, fluxiones invenire, et vice versa; that is, Having any given equation involving never so many flowing quantities, to find the fluxions, and vice versa) that most distinguished man wrote back that he had also fallen upon a method of the same kind, and communicated his method, which hardly differed from mine, except in his forms of words and symbols."

It seems to me that Newton's statement, "which hardly differed from mine, except in the forms of words and symbols," overstates the identity of the Newtonian and Leibnizian systems; there was a difference in the fundamental concepts. Newton, at the time when he wrote the *Principia,* used infinitely small quantities for the purpose of finding *fluxions* (velocities or time-derivative); Leibniz made infinitely small quantities themselves the fundamental concepts, in his *differentials* of different orders. In 1713, when the second edition of the *Principia* appeared, the controversy on the invention of the calculus was well under way; the *Commercium epistolicum* of John Collins, stating the English side of the dispute, had appeared in 1712. Nevertheless, the Scholium of the first edition of the *Principia*, quoted above, was allowed to stand in the second edition, the addition of the words "and the concept of the generation of quantities" (*et idea generationis quantitatum*) excepted, so that the last clause now read: "which hardly differed from mine, except in the forms of words and symbols, and the concept of the generation of quantities." This addition appears to me to add greater precision to the statement.

The difference between Newton and Leibniz in "the concept of the generation of quantities" was greater in 1704 when Newton's *Quadratura curvarum* appeared in print than in 1687. In the Introduction to this publication, Newton said: "I consider mathematical quantities in this place not as consisting of very small parts, but as described by a continuous motion." Thus Newton's exposition of fundamentals in 1704 was somewhat different from that in 1687; no use was made of infinitely small constant quantities in 1704. Therefore Newton's addition to the Scholium in the *Principia* of 1713, "and the concept of the generation of quantities," was very much to the point.

As the controversy progressed, Leibniz, in a letter (April 9, 1716) to an Italian priest, A. S. Conti, then residing in London, reminded Newton of the admission he had made in the Scholium, which Newton seemed now desirous of disavowing. Leibniz died in 1716. Soon after, on November 14, 1716, Newton published in Raphson's *Fluxions* (1716, date of title, 1715) the following statement: "He [Leibniz] pretends that in my book of *Principia* I allowed him the invention of the *Calculus differentialis*, independently of my own; and that to attribute this invention to myself, is contrary to my own Knowledge. But in the paragraph there referred unto I do not find one word to this purpose." With reference to Newton's admission in the Scholium, De Morgan remarks that Newton was weak enough, "first to deny the plain and obvious meaning, and secondly, to omit it entirely from the third edition of the *Principia*."

When in 1726 the third edition of the *Principia* was issued, there appeared in place of the Scholium of the previous editions the new Scholium, which has been printed in all later editions of the *Principia*. This new Scholium makes no mention of Leibniz. On the controversy over the invention of the calculus, see De Morgan, *Essays on the Life and Work of Newton* (ed. P. E. B. Jourdain), Chicago, 1914; M. Cantor, *Vorlesungen über Geschichte der Mathematik*, vol. 3, ed. 2, Leipzig, chap. 92, pp. 233–261; also other histories of mathematics.

34 (p. 257). Book II, Prop. X. *Corrections by Johann and Nicolaus Bernoulli.* On October 14, 1712, Newton wrote to Cotes: "There is an error in the tenth

Proposition of the second Book, Probl. III, which will require the reprinting of about a sheet and an half. I was told of it since I wrote to you, and am correcting it. I will pay the charge of reprinting it, and send it to you as soon as I can make it ready."[1]

Edleston gives additional information on that matter. This error in the first edition of the *Principia* arose in finding the value of the resistance to the motion of a projectile in the air; it was pointed out to Newton by Nicolaus Bernoulli (1687–1759), the nephew of Johann Bernoulli (1667–1748); Nicolaus was on a visit to England during September and October, 1712. Newton's result, when the curve described is a circle, had been previously shown to be erroneous by Johann Bernoulli in a letter to Leibniz of August, 1710, and in a communication to the French Academy, published in its *Mémoires* for 1711, Paris, 1714, pp. 50–56.[2] Newton sent to Johann Bernoulli, by Nicolaus, a copy of his *Analysis per quantitatum series, fluxiones, ac differentias,* published in 1711, and proposed him (Johann Bernoulli) as a member of the Royal Society, into which he was accordingly elected on the first of the December following.

[1] Edleston, *op. cit.*, p. 142.

[2] See also the second edition of the *Histoire de l'académie royale des sciences, Mémoires,* année 1711, pp. 50–54; and the paper by Nicolaus Bernoulli, pp. 54–56.

35 (p. 333). Third edition, Book II, Prop. XXXIV, Scholium. *Surface of least resistance.* In the first edition of the *Principia* there are nine Corollaries to Prop. XXXIII; in the second and third editions, only six Corollaries, and the sixth is rewritten. After Prop. XXXIII, up to Prop. XLI, there is much revision in the second edition, and the results of new experiments are introduced. Prop. XXXV of the first edition becomes Prop. XXXIV of the second and third editions. The Scholium following Prop. XXXV in the first edition, dealing with the surface of least resistance, is the same (unimportant minute changes excepted) as the Scholium which follows Prop. XXXIV in the second and third editions.

In this Scholium Newton gives without proof the geometrical conditions which a surface of revolution must satisfy, that it may move with the least resistance in the direction of its axis through a resisting medium. This is the earliest problem of the kind that is now solved by the calculus of variations. In the Portsmouth Collection,[1] there is the draft of a letter to a correspondent at Oxford, probably David Gregory, and written about 1694, in which Newton explains his method of solution. I reproduce that letter, making only slight changes in notation such as $(\mathrm{N}n)^2$ for "$\mathrm{N}n^{quad}$," $\frac{dq}{dt}$ for "$\dot{q}$," and the use of parentheses in place of the vinculum.

"Sir, I now thank you heartily both for the very kind visit you made me here and for the errata you gave me notice of in my book.... The figure which feels the least resistance in the Schol. of Prop. XXXIV, Lib. II, is demonstrable by these steps.

"1. If upon BM be erected infinitely narrow parallelograms BG*hb* and MN*om* and their distance M*b* and altitudes MN, BG be given, and the semi-sum of their bases $\frac{\text{M}m+\text{B}b}{2}$ be also given and called S and their semi-difference $\frac{\text{M}m-\text{B}b}{2}$ be called x: and if the lines BG, *bh*, MN, *mo*, butt upon the curve *n*N*g*G 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 *mn*N*g*GB 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 summ of the resistances of the two surfaces generated by the infinitely little lines G*g*, N*n* shall be least when $(g\text{G})^4$ is to $(n\text{N})^4$ as BG $\times$ B*b* to MN $\times$ M*m*.

"For the resistances of the surfaces generated by the revolution of G*g* and N*n* are as $\frac{\text{BG}}{(\text{G}g)^2}$ and $\frac{\text{MN}}{(\text{N}n)^2}$, that is, if $(\text{G}g)^2$ and $(\text{N}n)^2$ be called p and q, as $\frac{\text{BG}}{p}$ and $\frac{\text{MN}}{q}$ and their summ $\frac{\text{BG}}{p}+\frac{\text{MN}}{q}$ is least when the fluxion thereof

$$-\frac{\text{BG}}{p^2}\cdot\frac{dp}{dt}-\frac{\text{MN}}{q^2}\cdot\frac{dq}{dt} \text{ is nothing, or } -\frac{\text{BG}}{p^2}\cdot\frac{dp}{dt}=\frac{\text{MN}}{q^2}\cdot\frac{dq}{dt}.$$

"Now $p=(\text{G}g)^2=(\text{B}b)^2+(gh)^2=s^2-2sx+x^2+c^2$ and therefore

$$\frac{dp}{dt}=-2s\frac{dx}{dt}+2x\frac{dx}{dt},$$

and by the same argument $\frac{dq}{dt}=2s\frac{dx}{dt}+2x\frac{dx}{dt}$ and therefore

$$\frac{\text{BG}\times\left(2s\frac{dx}{dt}-2x\frac{dx}{dt}\right)}{p^2}=\frac{\text{MN}\left(2s\frac{dx}{dt}+2x\frac{dx}{dt}\right)}{q^2},$$

or $\frac{\text{BG}(s-x)}{p^2}=\frac{\text{MN}(s+x)}{q^2}$ and thence p^2 is to q^2 as $\text{BG}(s-x)$ to $\text{MN}(s+x)$, that is, $(g\text{G})^4$ to $(n\text{N})^4$ as BG $\times$ B*b* to MN $\times$ M*m*.

"2. If the curve line D*n*N*g*G 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 *n*N and] G*g* is less than if the intermediate solid *bg*NM be removed [along CB without altering M*b*, until *bg* comes] to BG [supposing as before that *on* is equal to *hg* and by consequence is the least that can be], and therefore $(g\text{G})^4$ is to $(n\text{N})^4$ as BG [$\times$ B*b* is to MN $\times$ M*m*].

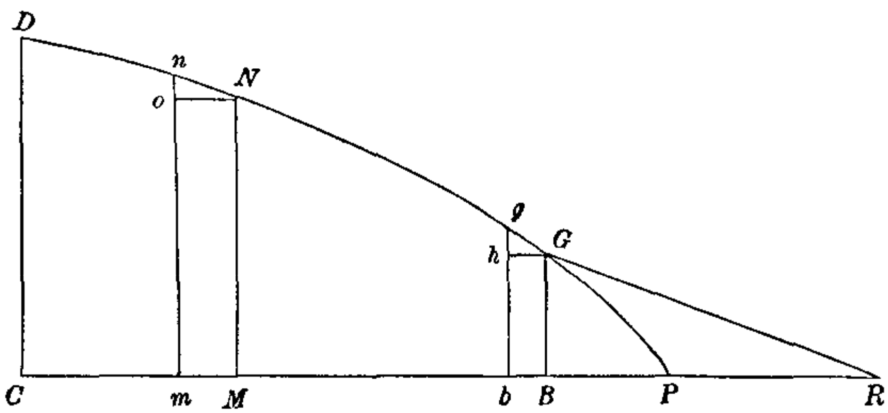
"*[Also if] gh be equal to hG so that the angle [gGh is 45 degr] then will $4(Bb)^4$ be [to $(nN)^4$ as BG × Bb is to] MN × Mm, and by consequence $4(BG)^4$ is to $(GR)^4$ as $(BG)^2$ is to MN × BR or $4(BG)^2 \times BR$ is to $(GR)^3$ [as GR to MN].

"*If the altitude of the frustum of the cone spoken of in the preceding paragraph be infinitely small, the semi-angle of the cone becomes equal to 45°. Hence when the total resistance is a minimum, the curve meets the extreme ordinate GB at an angle of 45°.

"Whence the proposition to be demonstrated easily follows."

The foregoing is Newton's draft of the letter, kept in the Portsmouth Collection, as amended in some defective parts of the manuscript by the interpolation of words placed in brackets. These interpolations were made by the editors of the *Catalogue* of the Portsmouth Collection. The brackets as printed in the *Catalogue* are not all properly inserted; I have followed the placing of them as determined by Oskar Bolza of Freiburg in Baden, who had secured from Arthur Berry of Cambridge (England) a facsimile of the original manuscript.

Bolza made an intensive study of Newton's solution[2] and arrived at the conclusion that the words "until bg comes," inserted by the editors of the *Catalogue* of the Portsmouth Collection, are not quite satisfactory. I reproduce here in translation part of Bolza's article in which the figure and the assumptions made in Newton's letter are followed:



"Then displace the arc Ng by an infinitely small distance K parallel to the axis CB to the new position N′g′ and draw the straight lines N′n, Gg′. Then, if in general, for any arc A lying in the plane of the figure, we designate by (A) the resistance of the surface generated by the revolution of A about the axis BC, we obtain

$$(DnN'g'GB) > (DnNgGB) \quad \ldots\ldots\ldots\ldots (1)$$

Since

$$(N'g') = (Ng),$$

there follows from (1)

$$(nN') + (g'G) > (nN) + (gG)^{I} \quad \ldots\ldots\ldots (2)$$

But the expression $(nN') + (g'G)$, being a function of K, must have a minimum for $K = o$. This is an ordinary problem in minima which Newton solves in the first part of his proof by the procedure still common at the present time, it being only necessary to remark that in the expressions for $(n\mathrm{N})$ and $(g\mathrm{G})$ he neglects at the outset the magnitude $hg = on$ as infinitely small in comparison with BG and MN.

"By solving the minimal problem, Newton obtains the relation

$$\frac{\mathrm{BG}\cdot\mathrm{B}b}{(\mathrm{G}g)^4} = \frac{\mathrm{MN}\cdot\mathrm{M}m}{(\mathrm{N}n)^4} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

which, according to modern phrasing, is nothing else than the well-known first integral of the Eulerian differential equation for the variation problem before us. For, if one designates

$$\angle g\mathrm{G}h = \mathrm{S}_o, \qquad \angle n\mathrm{N}_o = \mathrm{S},$$
$$cotg\mathrm{S}_o = q_o, \qquad cotg\mathrm{S} = q,$$
$$\mathrm{BG} = y_o, \qquad \mathrm{MN} = y,$$

and expresses the distance $\mathrm{B}b$, $\mathrm{G}g$, $\mathrm{M}m$, $\mathrm{N}n$ in terms of $hg = on$, then equation (3) becomes

$$\frac{yq}{(1+q^2)^2} = \frac{y_o q_o}{(1+q_o^2)^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

or

$$\frac{yq}{(1+q^2)^2} = \text{constant}, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

and this is in fact the first integral of the Eulerian differential equation for our problem,

$$\int_{y_o}^{y_1} \frac{ydy}{1+q^2} = \text{extreme} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (6).$$

But Newton advances an important step by determining the slope of the curve at the point G and thereby the constant of integration in (5).

"For in the Scholium referred to above, he had given the following theorem: To obtain the frustum of the cone of given radius OC of the base and of given altitude OD which meets the least resistance in the direction of the axis OD, bisect the altitude OD in Q, produce OQ to S so that QS = QC. Then S is the vertex of the required frustum of the cone.

"When OD becomes infinitely small, then angle SCO approaches the value 45°. From this Newton concludes, what appears as self-evident on the conception of a curve as a polygon of an infinite number of infinitely small sides, that for the solid of revolution of least resistance, the tangent at the point G makes with the axis BC an angle of 45°, that therefore $\phi_o = 45°$. Equation (4) becomes therefore

$$\frac{yq}{(1+q^2)^2} = \frac{y_o}{4} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7)$$

But this is the theorem in Newton's *Principia:*

$$\frac{MN}{GB} = \frac{(GR)^3}{4BR \cdot (GB)^2} \qquad (8)$$

(where GR is parallel to the tangent to the curve DG, at N), translated from geometrical to analytical form.

"Interesting in the calculus of variations is the peculiar mode of variation of the curve DG, used by Newton, which reminds one of the thoughts of Du Bois-Reymond[3] on the fundamental lemma of the calculus of variations.

"The success of this mode of variation lies in the absence of the variable x under the integral (6)."

Bolza proceeds to show how this process may be extended to a more general problem in the calculus of variations.

An elaborate discussion of the solid of least resistance, including much historical material, is found in an article written by A. R. Forsyth.[4]

[1] *A Catalogue of the Portsmouth Collection of Books and Papers Written by or Belonging to Sir Isaac Newton,* Cambridge, 1888, pp. xxi-xxiii.
[2] Oskar Bolza, in *Bibliotheca mathematica,* ser. 3, vol. 13, 1913, pp. 146–149.
[3] Du Bois-Reymond, in *Mathematische Annalen,* vol. 15, 1879, p. 313.
[4] A. R. Forsyth, in *Isaac Newton, 1642–1727, A Memorial Volume,* ed. W. J. Greenstreet, London, 1927, pp. 75–86.

36 (p. 337). Third edition, Book II, Prop. XXXVI. *Water running out of small orifices.* Of this difficult problem, Brougham and Routh state:[1] The investigation of this question, as given by Newton in his first edition, was very erroneous. He had totally neglected the contraction of the vein after the fluid had passed the orifice; hence he had deduced that the velocity of the efflux was that due to *half* the height of the water in the vessel. This mistake he afterwards corrected, but the investigation still remains open to very serious objections."

[1] *Op. cit.,* p. 264; see also p. 380.

37 (pp. 361, 362). Book II, Prop. XL, Scholium, Exper. 13. *Time and angular measure.* In the measure of time, 1 sec. = 60 thirds; 1 third = 60 fourths. In angular measure, $1° = 60'$, $1' = 60''$, $1'' = 60'''$, $1''' = 60^{iv}$.

38 (p. 378). Book II, Prop. XLVIII–L. *Velocity of sound.* In Newton's theoretical deduction of the velocity of sound, he came to the conclusion that the velocity varied directly as the square root of the "elastic force" and inversely as the square root of the "density of the medium," and obtained as the velocity through air 979 feet per second, the experimental value being about 1142 feet. Newton threw out conjectures as to the cause of the discrepancy between the experimental and theoretical values, which rested on assumptions far from convincing. The true explanation was given, over a century later, by Laplace, who showed that, in respect of air, the "elastic force" must be multiplied by the factor 1.41, which is the ratio

of the specific heat of air at constant pressure and the specific heat at constant volume. By this correction, due allowance is made for the changes in elasticity arising from the heat compression and the cold rarefaction.

Notwithstanding Newton's failure to consider these variations of elasticity, his theoretical discussion is a masterpiece of theoretical deduction. Few have recognized this fact more fully than Laplace, who completed the theory. Laplace first published the correction, without demonstration, in the *Annales de physique et de chimie,* vol. 3, 1816, pp. 238–241, and remarked, "La manière dont il y parvient est un des traits les plus remarquables de son génie." In his *Mécanique céleste,* 1798–1825, Livre XII, p. 95, he says: "Sa théorie, quoique imparfaite, est un monument de son génie."[1]

[1] Consult also I. Todhunter and K. Pearson, *A History of the Theory of Elasticity,* vol. I, 1886, pp. 161, 162; remarks on Newton's treatment of elastic bodies are found on pp. 13, 14, 26, 93.

39 (p. 402). Book III, Phenomenon I. *Huygenian telescope.* Newton refers here and in Book III, Prop. XIX, to an instrument, a "123-foot telescope" used by the Reverend James Pound in 1719. Such long telescopes were used, before the invention of the achromatic lens by Dollond about the middle of the eighteenth century. They were designed by Christiaan Huygens, and several were constructed by him and his brother Constantijn. By increasing the length of the telescope, the blurring effect of images with colored edges was greatly reduced and at the same time the magnifying power was increased. Newton states in his *Opticks,* Book I, Part I, Prop. VII, Exp. 16, that "a telescope of 64 feet in length, with an aperture of 2⅓ inches, magnifies about 120 times, with as much distinctness as one of a foot in length, with ⅓ of an inch aperture, magnifies 15 times." Instruments were made as long as 300 feet. The magnifying power of a telescope is measured by the focal length of the object glass, divided by the focal length of the eyepiece. As it was not found possible at that time to construct serviceable eyepieces of very short focal length, the focal length of the objective was increased. Huygens presented to the Royal Society of London three lenses having focal lengths of 123, 170, and 210 feet, respectively. An account of a recent examination of these lenses is given in *Nature,* vol. 123, 1929, p. 655, also p. 575. It being impractical to construct telescopic tubes of such great lengths, Huygens discarded their use and adopted for nocturnal observations the "aerial" telescope. The objective was mounted on a high pole and set in line with the eyepiece by means of cords, but it was found difficult to get the eye and object glasses "married" or brought parallel to each other, and at the same time in line with the object to be observed.

The Royal Society of London loaned the 123-foot objective to the Reverend James Pound, who mounted it in Wanstead Park, Essex, on a Maypole which had been removed from the Strand and which had been procured by Newton himself for mounting this telescope. Verses were affixed to the pole, by a local wit, which began:

Once I adorned the Strand
But now have found
My way to pound
In Baron Newton's land.

40 (p. 407). Book III, Prop. IV. *The earth-moon test of the law of gravitation.* Except the experimental constants used, this computation given in the *Principia* is probably the same as Newton's first computation of 1665 or 1666. This famous Prop. IV is the same in the three editions of the *Principia* (1687, 1713, 1726), except the list of astronomical authorities cited. New in the third edition is the Scholium which follows Prop. IV. In the first two editions there was no scholium at this place.

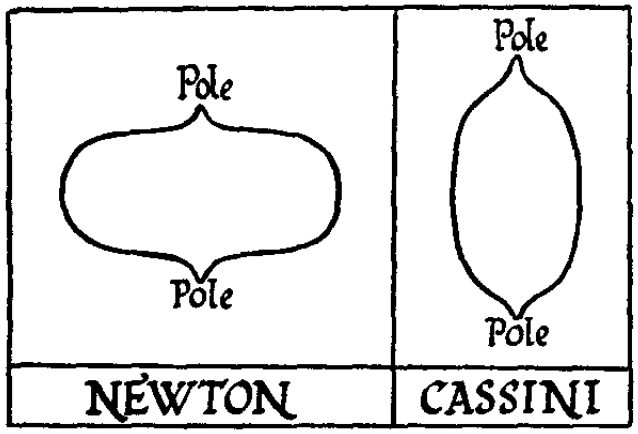
It is well known that in 1665 or 1666 Newton made his first test of the validity of the law of gravitation, and that he did not announce the law until about twenty years later. What was the cause of this long delay? The old reply to this question was that in 1665 or 1666 Newton had a very inaccurate value for the size of the earth, namely 60 miles to a degree of latitude, that, consequently, his computed and experimental values for the intensity of gravity on the earth's surface did not agree, and that the validity of the law did not appear until J. Picard's measurements of the earth in France, which yielded the value of 69⅓ miles to a degree of latitude. This explanation of Newton's delay in announcing the law of gravitation has recently been shown to be incorrect. It has been shown that fairly accurate values for the size of the earth were known since Snell's measurements of 1617 (yielding 66⅔ English statute miles to a degree of latitude) and Norwood's of 1636 (yielding 69½ such miles).

Did Newton in 1666 know Snell's or Norwood's values for the size of the earth? Strangely, Newton nowhere in his writings or letters mentions any particular numerical value previous to the appearance of his *Principia* (1687) in which he adopted the determination of Picard. Nevertheless, it is now definitely settled that Newton was familiar with Snell's value at least as early as 1672, and very probably in 1666. That Newton knew this value in 1672 is evident from his edition of Varen's *Geography*, published in that year. Varen gives in that book a small table of distances at which a mountain peak of known altitude can be seen at sea, disregarding refraction. Varen's computation is erroneous and is corrected by Newton. *That correction was made by the use of Snell's value*. Thus, a mountain height of one German mile could be seen at a distance of 29¼ German miles according to Varen, and of 41½ German miles according to Newton.[1]

What really delayed Newton for twenty years were theoretical questions of great difficulty relating to the attraction of a sphere upon an external point—a problem which he did not solve before 1684 or 1685, and which was first explained in the *Principia*, Book I, Prop. LXXV and LXXVI, and Corollaries. The facts which bear on this question are fully set forth in an article in *Sir Isaac Newton, 1727–1927*, Baltimore, 1928, pp. 127–187. See also the *Principia*, Book III, Prop. VIII.

[1] See *Mathematical Gazette*, vol. 14, 1929, p. 415.

41 (p. 427). Book III, Prop. XIX. *Figure of the earth.* In France Jacques Cassini was misled, by somewhat inaccurate geodetic measurements taken over a comparatively short meridian, to the belief that the earth was elongated at the poles. The opposing theories of Newton and Cassini are strikingly shown in the following old caricature:



Later geodetic measurements made in Lapland by Maupertuis and Clairaut, when taken with those previously made in France, afforded data extending over a much longer meridian distance and showed a flattening of the earth at the poles, as demanded by Newton's theory of gravitation. Voltaire wittily designated Maupertuis as "Aplatisseur du monde et de Cassini." Later Voltaire and Maupertuis became involved in a heroic-comic controversy, and Voltaire (in his *Discours sur la modération*) wrote,

> Vouz avez confirmé dans des lieux pleins d'ennui
> Ce que Newton connut sans sortir de chez lui.

Detailed historical information is found in Isaac Todhunter's *Theories of Attraction and Figure of the Earth,* London, 1873, and in A. D. Butterfield's *History of the Determination of the Figure of the Earth from Arc Measurements,* Worcester, Mass., 1906.

On theoretical grounds, Newton found the diameter of the earth at the equator to be to the diameter from pole to pole as 230 is to 229, on the supposition that the matter of the earth was all "uniformly dense." The compression of the earth is accordingly $(230-229) \div 230$, or $1/230$. The latest experimental determination of the earth's compression, derived from pendulum observations on the intensity of gravity, yields the value $1/297.4$. See *United States Coast and Geodetic Survey, Its Work, Methods and Organization,* Special Publication No. 23, Washington, 1928, p. 127.

42 (p. 430). Book III, Prop. XX. *The toise.* 1 toise = 1.949+ meters = 6.394+ English feet.

43. (p. 433). Book III, Prop. XXII, XXV–XXXV. *The problem of three bodies.* In studying the lunar orbit under the influence of the earth and sun, the attractive forces acting according to the law of gravitation, Newton originated a particular statement of a famous general problem which came to be known in celestial mechanics as the "problem of three bodies." While the determination of the motion of two bodies considered as particles, when their mutual attractions are the only force considered, is a comparatively simple problem in mechanics, the addition of a third body gives rise to unexpected difficulties. No perfectly general solution of this problem has yet been found. Associated with the early history of the "problem of three bodies" after Newton are the names of D'Alembert, Clairaut, Euler, Lagrange, Laplace.[1]

[1] For an outline history of this problem, including the nineteenth century, see E. O. Lovett, "The Problem of Three Bodies," *Science,* vol. 29, 1909, pp. 81–91.

44 (p. 435). Book III, Prop. XXIII. *A satellite of Jupiter.* By the "outmost satellite" of Jupiter is meant the outmost moon of the four moons discovered by Galileo; it is called Callisto. There are now nine moons of Jupiter known. One of them is inside the four moons found by Galileo; the remaining four moons of recent discovery have their orbits more remote from Jupiter than Callisto.

45 (p. 439). Book III, Prop. XXIV. *Time of the highest tide.* That the highest tide does not occur at the syzygy, but about three days later, Newton attributes to the "force of reciprocation" which the water once moved retains a little while by its inertia. But Laplace showed (*Mécanique céleste,* Livre XIII, Chap. 1) that this is not true, that the greatest full tides should occur exactly at the syzygies, and the least exactly at the quadratures, and that therefore the explanation of the delay must be sought in other circumstances; it is caused by friction.[1]

[1] Brougham and Routh, *op. cit.,* p. 293; see also p. 397.
The reader will find much information in an article by J. Proudman, "Newton's Work on the Theory of the Tides," in *Isaac Newton, 1642–1727 . . . ,* pp. 87–95.

46 (p. 445). Book III, Prop. XXVIII. The "radii" in question are like Ad in the figure of Lemma XI, Book I, the curvilinear angle dAB being the angle of contact. If gA or gB be taken as the radius of curvature, we have very nearly $(gb+bd)^2 = g\mathrm{A} + \mathrm{A}d^2$, or the curvature $\frac{1}{g\mathrm{A}} = \frac{2bd}{\mathrm{A}d^2}$. If bd be taken as the evanescent tangent of the angle of contact, then the curvatures of two curves, for equal infinitesimal values of Ad, are to each other as the tangents of those angles.

47 (p. 488). Book III, Lemma III. By "motion" is meant here *momentum, mv.* Assuming, for simplicity, unit angular velocity and unit density, Newton's first

statement in the proof, that the sphere and the circumscribed cylinder have momentums in the ratio of $3\pi:16$, may be shown as follows:

The momentum of a material circular area of radius R, rotating about an axis perpendicular to it and passing through its center, is (taking r as the variable radius of a flat ring, $2\pi r$ in circumference, and dr in width),

$$2\pi\int_0^R r^2dr = \tfrac{2}{3}\pi R^3.$$

The momentum of the cylinder in question is therefore $\tfrac{4}{3}\pi R^4$.

As for the rotating sphere, take any section of it perpendicular to the axis of rotation, the momentum of which is $\tfrac{2}{3}\pi R^3 \sin^3 \alpha$ times an infinitesimal thickness $R \sin \alpha \cdot d\alpha$, or $\tfrac{2}{3}\pi R^4 \sin \alpha^4 \cdot d\alpha$, α being the angle between R and the abscissa. The momentum of the whole sphere is therefore

$$\tfrac{4}{3}\pi R^4 \int_0^{\frac{\pi}{2}} \sin^4 \alpha \cdot d\alpha = \tfrac{1}{4} R^4\pi^2.$$

We have, therefore,

momentum of the sphere : momentum of the cylinder =

$$\tfrac{1}{4}\pi^2R^4 : \tfrac{4}{3}\pi R^4 = 3\pi : 16, \quad \ldots\ldots\ldots\ldots \quad (1)$$

Newton's second statement in his proof, that the momentums of the cylinder and thin ring, both taken about the axis of the cylinder, are as double the matter in the cylinder to triple the matter in the ring, is evident from the proportion,

$$\tfrac{4}{3}\pi R^4 : 2\pi R^2 \cdot dR = 2(2\pi R^3) : 3(2\pi R \cdot dR), \quad .. \quad (2)$$

To obtain the momentum of this ring about its own diameter, multiply its infinitesimal arc, $R \cdot d\alpha$, by the distance, $R \cos \alpha$, of that arc from the vertical diameter, and the momentum of the ring about its diameter becomes

$$4R^2 \cdot dR \int_0^{\frac{\pi}{2}} \cos\alpha \cdot d\alpha = 4R^2 \cdot dR.$$

Hence, Newton's third statement in the proof is evident from the proportion,

$$2\pi R^2 \cdot dR : 4R^2 \cdot dR = 2\pi R : 4R, \quad \ldots\ldots\ldots\ldots \quad (3)$$

Multiplying together corresponding terms of the proportions (1), (2), and (3), and simplifying, we obtain,

momentum of the sphere : momentum of the ring about its diameter =

$$\pi^2R^2 : 32 \cdot dR = 3\pi^2(\tfrac{4}{3}\ \pi R^3) : 32(2\pi R \cdot dR) =$$
$$.925275(\tfrac{4}{3}\pi R^3) : 1000000(2\pi R \cdot dR).$$

48 (p. 498). Book III, Props. XL–XLII. *Comets.* Some additions and changes occur in the second and third editions. Thus, new in the second and third editions, are what follows Cor. III of Prop. XL, also the Corollary and Scholium of

Lemma VIII, the data of the comet of 1681 following "Nov. 20" in Prop. XLI, and finally the part of Prop. XLII which follows the "Q.E.I." The third edition differs from the second in the closing part of Prop. XLII.

49 (p. 499). Book III, Lemma V, following Prop. XL. *Newton's formulas of interpolation.* This famous Lemma deals with these formulas. Newton discusses this subject in four other places: (1) in his *Methodus differentialis,* published in 1711; (2) in a letter of May 8, 1675, to John Smith, giving detailed instructions for the calculation of certain tables; (3) in a manuscript first published in 1927, contained in the Portsmouth Collection of Newtonian manuscripts; (4) in his letters of June 13, 1676, and October 24, 1676, first published in the *Commercium epistolicum* edited by John Collins in 1712. All the contributions of Newton to the theory of interpolation are brought together in a booklet by Duncan C. Fraser, under the title, *Newton's Interpolation Formulas,* London, 1927.[1] Fraser reproduces the *Methodus differentialis* and the manuscript in the Portsmouth Collection, each in facsimile, also in an English translation. He presents much interesting historical material. Newton's interpolation process, as explained in some modern treatises and called "Newton's formula of interpolation" or "Newton's original formula of interpolation," is not Newton's general formula, but only his special formula for values of the argument that are equidistant. This attachment of Newton's name to the special use, without mention of his more general treatment, is seen, for example, in Simon Newcomb, *Logarithmic and other Mathematical Tables,* New York [1882], p. 62; in Herbert L. Rice, *The Theory and Practice of Interpolation,* Lynn, Mass., 1899, pp. 41–61; in H. C. Plummer, *An Introductory Treatise on Dynamical Astronomy,* Cambridge (England), 1918, p. 325. On the contrary, in T. N. Thiele, *Interpolationsrechnung,* Leipzig, 1909, p. 3, there is an explanation of "divided differences" which lead to Newton's general formula, and this explanation is applicable to values of the argument which are not necessarily equidistant.

[1] Fraser's booklet is reprinted from the *Journal of the Institute of Actuaries,* vol. 51, pp. 77–106 (October, 1918) and pp. 311–232 (April, 1919); vol. 58, pp. 53–95 (March, 1927). A more recent contribution to this subject by Fraser is a detailed article, entitled "Newton and Interpolation," in *Isaac Newton, 1642–1727 . . . ,* pp. 43–69.

50 (p. 502). Book III, Lemma X. *A theorem of Newton's.* Kriloff[1] has drawn attention to the fact that the relation between the time of description of an arc of a parabolic orbit of comets, the two radii vectores drawn to the extremities of that arc, and the chord, is called in modern treatises on theoretical astronomy "Euler's theorem,"[2] "Lambert's theorem,"[3] or "Euler-Lambert's theorem," even though Lagrange, in his *Mécanique analytique,* Pt. II, § VII, p. 26, pointed out that it is only the analytical expression of Newton's Lemma X in Book III. Kriloff

shows that the required algebraic transformations can be performed in a very simple manner.

[1] *Monthly Notices of the Royal Astronomical Society,* London, vol. 84, 1924, p. 392.

[2] L. Euler, *Miscellanea Berlinensia,* vol. 7, 1743, "Determinatio orbitae cometae anno 1742 observati."

[3] J. H. Lambert, *Nouveaux mémoires de l'académie royale des sciences et belles-lettres,* année 1771, Berlin, 1773, pp. 352–364.

51 (p. 508). Book III, Prop. XLI, Example. *Newton's astronomical observations.* While it is generally known that Newton carried on experiments on light, which were of high importance, also on falling bodies and the motion of pendulums in resisting media, it is not widely known that he refers in this place in the *Principia* to astronomical observations which he himself had made in 1681. He used a "telescope of 7 feet," apparently not a reflector.

52 (p. 544). Book III, General Scholium to Prop. XLII. *Newton's idea of God.* In the first edition of Newton's *Principia* (1687) no statement is made on the nature of God. Nevertheless, criticism was passed upon the *Principia,* on theological grounds, by two prominent thinkers, Bishop Berkeley, who in 1710 published his *Principles of Human Knowledge,* and Leibniz, who on February 10, 1711, wrote a letter to Hartsoeker, a Dutch physician at Düsseldorf, which was published on May 5, 1712, in the *Memoirs of Literature,* a weekly sold in London.[1]

Berkeley attacks Newton's exposition in the *Principia* of the notion of absolute space, absolute time, and absolute motion. "This celebrated author," says Berkeley, "holds there is an *absolute Space,* which, being unperceivable to sense, remains in itself similar and immoveable." As to absolute motion, "I must confess it does not appear to me that there can be any motion other than *relative,* so that to conceive motion, there must be at least conceived two bodies.... But the chief advantage arising from it [relative space as advocated by Berkeley] is that we are freed from that dangerous dilemma,... of thinking either Real Space is God, or else that there is something besides God which is eternal, uncreated, infinite, indivisible, unmutable. Both which may justly be thought pernicious and absurd notions."[2] Thus the absolute space, time, and motion of Newton was attacked as an atheistic conception.

Leibniz, the second distinguished critic, does not mention Newton or the *Principia* in his letter to Hartsoeker, but the reference is obvious. Leibniz says:

"Thus the ancients and moderns who avow that gravity is an *occult quality,* are right if they mean thereby that there is a certain mechanism unknown to them, by which bodies are impelled toward the center of the earth. But if their notion is that this transpires without any mechanism, by a simple *primitive property* [qualité primitive], or by a law of God which brings about this effect without using any intelligible means [moyens intelligibles], then it is a senseless occult quality, which is so very occult that it can never be cleared up, even though a Spirit, not to say God himself, were endeavoring to explain it."

Neither Newton nor Cotes, who edited the second edition (1713) of the *Principia,* made a direct reference to Berkeley. But Cotes, writing to Newton on March 18, 1713, refers to the letter of Leibniz to Hartsoeker, and says: "I think it will be proper [to] add somethings by which your Book may be cleared from some prejudices which have been industriously laid against it. As that it deserts Mechanical causes, is built upon Miracles, and recurrs to Occult qualities. That You may not think unnecessary to answer such Objections You may be pleased to consult a Weekly Paper called *Memoires of Literature* and sold by Ann Baldwin in Warwick-Lane.... You will find a very extraordinary Letter of Mr Leibnitz to Mr Hartsoeker which will confirm what I have said. I do not propose to mention Mr. Leibnitz's name, twere better to neglect him, but the Objections I think may very well be answered and even retorted upon the maintainers of Vortices."[3] Cotes made a spirited reply to Leibniz toward the end of his Preface to the Second Edition of the *Principia.*

That Newton was misinterpreted by Leibniz with reference to the nature of gravity, is evident. Newton did not believe in action at a distance without the aid of an intervening medium. (See Note 6.) That he was interested in theological questions even before he wrote the *Principia,* is evident from his annotations of Henry More's book, *On the Prophet Daniel and the Apocalypse,* found in Newton's own copy of this book, "ex dono Reverendi Authoris." These annotations have not yet received due attention in the study of Newton's theological and cosmological concepts at different periods of his career. Referring to the *Principia,* Newton said in a letter to Richard Bentley (Dec. 10, 1692), "When I wrote my treatise about our system, I had an eye on such principles as might work with considering men for the belief of a Deity; and nothing can rejoice me more than to find it useful for that purpose."

And so, in 1713, twenty-six years after the first appearance of the *Principia,* Newton, then seventy-one years old, prepared the famous General Scholium, printed at the end of the second edition of the *Principia.*

The sentence, "And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy," is of interest for two reasons. First, it was not in the original draft of the General Scholium; Newton added it, as the second edition was going through the press.[4] Secondly, Newton here gives his justification for treating of this subject in the *Principia.* To obtain an idea of God "from the appearances of things, does certainly belong to Natural Philosophy."

In the third edition (1726) of the *Principia,* there are six additions or changes made to the General Scholium. These alterations are as follows:

(1) New in the 1726 edition is the sentence, "And lest the systems of the fixed stars should, by their gravity, fall on each other, he hath placed those systems at immense distances from one another."

(2) New in the 1726 edition following the words "God of Israel" are the words, "the God of Gods, and Lord of Lords."

(3) In the 1726 edition the passage, "the Eternal of Gods; we do not say, my Infinite, or my Perfect," takes the place of "we do not say, my Infinite, your Infinite, the Infinite of Israel; we do not say my Perfect, your Perfect, the Perfect of Israel," in the second edition.

(4) In the 1726 edition, "He is not eternity and infinity" takes the place of "He is not eternity or infinity" in the second edition.

(5) The four sentences in the 1726 edition, "Every soul that has perception is ... God is the same God, always and everywhere," are an addition to the Scholium as given in the second edition.

(6) The four sentences in the 1726 edition, "For we adore him as his servants; ... by a certain similitude which, though not perfect, has some likeness, however," are an addition to the Scholium in the second edition. But substantially this addition Newton prepared long before 1726, in fact, only six months after the publication of the second edition in 1713. It is given in a list of corrections and additions[5] to the second edition, which he sent to Cotes; but this was not printed at the time.

It is to be noted that Newton's idea of God in the second (1713) edition of the *Principia* is drawn largely from the "appearances of things," while the interpolations printed in the third edition (1726) are taken more particularly from the "ways of mankind."

[1] This letter of Leibniz' is found in Leibniz, *Opera omnia,* vol. 2, Geneva, 1768, Pt. II, p. 60; *Philosophische Schriften von Leibniz* (ed. C. I. Gerhardt), vol. 3, Berlin, 1887, p. 519. See also Brewster, *Memoirs of Sir Isaac Newton,* vol. 2, chap. 22, pp. 219–222; Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes,* p. 153. Leibniz attacks the theory of gravity also in his *Essais de théodicée sur la bonté de Dieu,* 1710.

[2] G. Berkeley, *Principles of Human Knowledge,* Part I, Par. 111, 117.

[3] Edleston, *op. cit.,* p. 153.

[4] See Edleston, *op. cit.,* p. 155.

[5] Edleston, *op. cit.,* p. 165.

53 (p. 546). Book III, General Scholium to Prop. XLII. *Final causes.* Newton used the term "final cause" as did Aristotle, who distinguished four kinds of cause: "material," "formal," "efficient," and "final"; Aristotle's final cause was the purpose, aim, or end for which a thing is made. Thus: "For to sojourn in the land, is cause we come to thee"; a pen is made to serve in writing.

54 (p. 546). Book III, General Scholium to Prop. XLII. *Statement of the law of gravitation.* The modern general statement of this law, namely, that "every particle of matter attracts every other particle with a force varying directly as the

product of their masses and inversely as the square of the distance between them," is nowhere found in the *Principia* and the *System of the World*. Perhaps the nearest approach to it is given in the General Scholium at the end of the *Principia,* where Newton states that "gravity . . . operates . . . according to the quantity of the solid matter which they [the sun and planets] contain, and propagates its virtue on all sides to immense distances, decreasing always as the inverse square of the distances." For other approximations to the modern form of statement, see Book I, Prop. LXXVI, Cor. III and IV, and the *System of the World,* Par. 26.

55 (p. 547). Book III, General Scholium to Prop. XLII. *Newton's use of hypotheses.* "I frame no hypotheses" (*hypotheses non fingo*) is the expression of Newton, occurring in the General Scholium at the end of his *Principia*—an expression frequently quoted to indicate his contempt for reckless speculation and his absolute reliance upon observation and experiment. No doubt readers of Newton's *Principia,* of his early published papers on light, and of his *Opticks,* will be puzzled by this absolute declaration, "hypotheses non fingo," for surely Newton himself framed many hypotheses—as many, perhaps, as any other scientist of note. How is this statement of his position to be reconciled with his actual practice?

In the first place, it should be noted that Newton does not advance "hypotheses non fingo" as a general proposition, applying to all his scientific endeavor; it is used by him in connection with a public statement relating to that special, that difficult and subtle subject, the real nature of gravitation, which was mysterious then and has remained so to our day. Moreover, this "hypotheses non fingo" is to be taken, not as his private practice, nor his individual habit of thought, but as the position which he took in public print, on the occasion of placing before the scientific world the positive results of his mathematical thinking, which were primarily based on observation and experimentation. Newton's "hypotheses non fingo" disrupted from its context is a complete misrepresentation of Newton.

An examination of the various passages in Newton's writings, relating to the use of hypotheses, discloses the rule that experimental facts must invariably take precedence over any hypothesis in conflict with them. Secondly, hypotheses which seem incapable of verification by experiment are to be viewed with suspicion. In any event, one should observe the distinction between exact experimental results and mere suggestions derived from hypotheses.

It may be of some interest to give in chronological order some of the leading hypotheses advanced by Newton, as well as his comments made at different times of his career, on the proper use of hypotheses.

In 1666, when about twenty-four years of age, Newton cogitated on gravity, framed the hypothesis of its reaching to the moon and of its variation inversely as the square of the distance; he then proceeded to test it by comparing the implications of that hypothesis with the results of actual experiment.[1]

In 1672 were published Newton's experiments on the dispersion of light.[2] To explain why the length of the spectrum obtained from light entering the room through a small circular opening was five times its width, Newton set up different hypotheses, or "suspicions" as he called them, but removed every one of them as untenable, and was finally led to his *experimentum crucis*. One of these untenable hypotheses was that the solar rays after leaving the prism forming the spectrum moved in curved lines, some more curved than others—"I had often seen a Tennis-ball, struck with an Oblique Racket, describe such a Curve Line. . . . If the Rays of Light should possess Globular Bodies, and by their Oblique passage, out of one Medium into another, acquire a Circulating Motion, they ought to feel the greater resistance from the ambient Aether, on that side, where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion . . . I could observe no such Curvity in them." In this research, the young man of thirty indulged with perfect freedom in the formulation of hypotheses. Two of them were no other than the corpuscular theory of light and that of the luminiferous ether! These were hypotheses as bold and far-reaching as have ever been advanced in physics. Nor were they of the type that are readily verifiable by experiment. However, his sub-hypothesis of the curved path of these light particles was easily eliminated through experimental test.

In 1672, in reply to criticisms made by Robert Hooke, Newton explained how Hooke's hypothesis that light is caused by pulses or, better, by wave motions of the ether, could be used to explain the phenomena of spectral colors, and the colors of thin plates. But Newton found Hooke's hypothesis unable to explain the fact that light travels in straight lines.[3] If light consisted of vibrations, he argued, it would, like sound, spread "into the Shadowed Medium." We see here that Newton has no hesitation in entertaining the hypothesis of the wave motion of light in the ether.

However, toward the close of Newton's reply to Hooke there appears the first foreshadowing of impatience with hypotheses. Hooke would explain the spectrum caused by a prism, not by the different refrangibility of the rays, but by the splitting and rarefying of ethereal pulses. Hooke's hypothesis was one of pulses in the ether, rather than of actual waves in the ether. Newton considers Hooke's hypothesis, but finds it insufficient, and finally says, "But whatever be the Advantages or Disadvantages of this Hypothesis, I hope I may be excused from taking it up, since I do not think it needful to explicate my Doctrine by any Hypothesis at all." Farther on Newton says:[4]

"You see therefore how much it is beside the business in hand, to dispute about Hypotheses. For which reason I shall now, in the last place, proceed to Abstract the difficulties in the Animadversor's Discourse, and without having regard to any Hypothesis, consider them in general Terms. And they may be reduced to these three Queries:

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

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

"3. Whether Whiteness be a Mixture of all Colours?"

The interesting revelation in this last quotation is that Newton dismisses the two major hypotheses of light as wave motion and as flying particles, in order to consider three minor hypotheses, more easily disposed of by experimental test, but nevertheless hypotheses, although he does not designate them by that name.

When another critic, Gaston Pardies, Professor of Mathematics in the Parisian College of Clermont, called the refrangibility of light a hypothesis, Newton replied with emphasis and warmth, that his theory "seemed to contain nothing else than certain properties of light, which I have discovered and regard it not difficult to prove; and if I had not perceived them to be true, I would have preferred to reject them as futile and inane speculation, rather than acknowledge them as hypotheses."[5]

In reply to further objections of Pardies, Newton says:[6]

"For the best and safest method of philosophizing seems to be, first diligently to investigate the properties of things and establish them by experiment, and then to seek hypotheses to explain them. For hypotheses ought to be fitted merely to explain the properties of things and not attempt to predetermine them except in so far as they can be an aid to experiments. If any one offers conjectures about the truth of things from the mere possibility of hypotheses, I do not see how any thing certain can be determined in any science; for it is always possible to contrive hypotheses, one after another, which are found rich in new tribulations. Wherefore I judged that one should abstain from considering hypotheses as from a fallacious argument, and that the force of their opposition must be removed, that one may arrive at a maturer and more general explanation." This passage is important; it is the fullest single statement that Newton has made on the use of hypotheses. He recognizes the rôle they can be made to play in research, and he places the proper restrictions to their use.

In 1673, in a reply to criticisms of Christiaan Huygens, Newton said:[7] "Nor is it easier to frame an Hypothesis by assuming only two Original Colours, rather than an indefinite Variety; unless it be easier to suppose that there are but two Figures, Sizes, and Degrees of Velocity or Force of the Aethereal Corpuscles or

Pulses, rather than an indefinite Variety; which certainly would be a harsh Supposition.... But to examine how Colours may be explained Hypothetically, is beside my purpose. I never intended to show wherein consists the Nature and Difference of Colours, but...to leave it to others to Explicate by Mechanical Hypotheses, the Nature and Difference of these Qualities; which I take to be no difficult matter." Here, for a moment, Newton allows himself the luxury of considering ethereal corpuscles or pulses of different forms, sizes, and velocities; he does not reprimand himself for it, nor state that he is indulging in unscientific procedure, but finally he decides to leave these hypotheses to others. He saw that often the same experimental fact could be explained by different hypotheses,[8] hence the futility of arguing about them in heated discussion.

My quotations from Newton suggest the motive which induced him to take a stand against the use of hypotheses, namely, the danger of becoming involved in disagreeable controversies. This guess is confirmed by Newton's own statement contained in a communication of December 9, 1675, to the Royal Society:[9] "I had formerly purposed never to write any hypothesis of light and colours, fearing it might be a means to engage me in vain disputes: but I hope a declared resolution to answer nothing, that looks like a controversy, unless possibly at my own time upon some by-occasion, may defend me from that fear." And then Newton proceeds to elaborate more fully his explanations of refraction and reflection of light, involving corpuscles of different sizes, causing in their flight compressions and rarefactions of the ether at a refracting surface. This is his famous hypothesis of "fits" of easy reflections and refractions, which is of interest to twentieth-century physicists as bearing considerable resemblance to the modern theory of "wave mechanics." Newton could no more dispense with hypotheses in his own cogitations than an eagle can dispense with flight. Nor did Newton succeed in avoiding controversy.

The years 1672–6 were years of conflict, and Newton became more and more disinclined to make any of his speculations public. By nature Newton was very sensitive to criticism. He loathed controversy.

Did his disagreeable experiences of 1672–6 stop his private indulgence in the practice of framing hypotheses? Not at all. His mind remained as free, bold, and imaginative as ever. To friends like Robert Boyle, he felt free to disclose his innermost thoughts, both in conversation and by letter. To be convinced of this fact one needs only to read Newton's letter[10] to Boyle on the cause of gravitation, written on February 28, 1678/9. (See also Note 6.) This was about eight years before the publication of the *Principia*. On the very topic which in the *Principia* led to the declaration, "hypotheses non fingo," there was in this letter to Boyle a very revelry of unverifiable hypotheses. But he puts forth the caution, "my notions about things of this kind are so indigested, that I am not well satisfied myself with them." I quote:

"And, first, I suppose that there is diffused through all space an aethereal substance, capable of contraction and dilation, strongly elastic; and, in a word, much like air in all respects, but far more subtle.... When two bodies, moving toward one another, come nearer together, I suppose the aether between them to grow rarer than before.... When two bodies, approaching one another, come so near together as to make the aether between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another,... but at length, when they come so near together, that the excess of pressure of the external aether, which surrounds the bodies, about that of the rarefied aether, which is between them, is so great, as to overcome the reluctance which the bodies have from being brought together, then will that excess of pressure drive them with violence together.... Now, hence I conceived it is, chiefly, that a fly walks on water without wetting its feet, and consequently without touching the water";

"I shall set down one conjecture more,... it is about the cause of gravity. For this end I will suppose aether to consist of parts differing from one another in subtility by indefinite degrees: that in the pores of bodies, there is less of the grosser aether in proportion to the finer, than in open spaces; and consequently, that in the great body of the earth there is much less of the grosser aether, in proportion to the finer, than in the regions of the air: and that... from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the aether is insensibly finer and finer. Imagine, now, any body suspended in the air, or lying on the earth; and the aether being, by the hypothesis, grosser in the pores which are in the upper parts of the body, than in those which are in the lower parts; and that grosser aether, being less apt to be lodged in those pores, than the finer aether below; it will endeavour to get out, and give way to the finer aether below, which cannot be, without the bodies descending to make room above for it to go out into."

These speculations on gravity were too extravagant to find a place in a work like the *Principia.* Hence Newton's exercise of control over himself in his public declaration, "hypotheses non fingo." He pressed the distinction between hypotheses and experimental law, which he himself carefully observed. In his *Principia,* Book III, Rule 3, "of reasoning in [natural] philosophy," he says,[11] "We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which is wont to be simple, and always consonant to itself."

The classic pronouncement of Newton against the improper use of hypotheses is found at the end of his *Principia:* "Whatever is not deduced from the phenomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered."

In 1704 was published Newton's book entitled *Opticks*. It contained some earlier papers and also some new matter. The book could not avoid hypotheses, but an effort was made to exclude the more fanciful of them from the main body of the work. The parts which seemed to Newton over-highly speculative, or which represented investigations left incomplete, were introduced at the close in the form of "Queries." In these Queries Newton apparently allowed himself full freedom of speech. They afford a wonderful insight into the operations of Newton's mind on questions of physical inquiry.

In general, we may say that Newton gave his scientific imagination free scope and made very extensive use of hypotheses. But hypotheses were always scrupulously dethroned whenever they were found to be in irreconcilable conflict with experimental fact. In his publications Newton frequently avoided full statement of his hypotheses, hoping thereby to escape much-dreaded controversies.

[1] The test was most probably in general outline the same as the one given in the *Principia*, Book III, Prop. IV.

[2] *Philosophical Transactions Abridged* (J. Lowthrop), vol. I, London, 1705, pp. 128–130.

[3] *Philosophical Transactions Abridged*, vol. 1, 1705, p. 146; *Philosophical Transactions*, No. 88, 1672, p. 5086.

[4] *Philosophical Transactions Abridged*, vol. 1, 1705, p. 148; *Philosophical Transactions*, No. 88, 1672, p. 5086.

[5] *Philosophical Transactions Abridged*, vol. 1, 1705, p. 141; *Philosophical Transactions*, No. 84, 1672, p. 4091.

[6] *Isaaci Newtoni Opera*, vol. 4, pp. 314, 315. Newton's letter to Oldenburg, June 2, 1672. This part of the letter was not printed in the *Philosophical Transactions*.

[7] *Philosophical Transactions Abridged*, vol. 1, 1705, p. 157; *Philosophical Transactions*, No. 97, 1673, p. 6108.

[8] *Philosophical Transactions Abridged*, vol. 1, 1705, p. 145; *Philosophical Transactions*, No. 88, 1672, p. 5086.

[9] T. Birch, *History of the Royal Society of London*, vol. 3, 1757, p. 248.

[10] *Isaaci Newtoni Opera*, vol. 4, pp. 385–394.

[11] Present volume, pp. 398–399.

56 (p. 547). Book III, General Scholium to Prop. XLII. *Causality*. Questions of cause and effect have interested philosophers of all times—Aristotle, Hume, Kant, Mill. Among the earliest scientists to instill confidence in the existence of a comprehensive system of causality in physical phenomena was Newton. Kepler's three laws of planetary motion were brilliant discoveries, but they did not satisfy the requirements of causality. It was Newton who showed that the three apparently independent laws were the logical consequence of one fundamental law of Nature. At the opening of Book III of the *Principia*, Newton discusses relations of cause and effect in the Rules of Reasoning in Philosophy.

But in his *Principia* Newton did not venture to make suggestions on the cause of gravity. (See Note 6.) Such ultra-speculative writing is found in his letters to Richard Bentley and Robert Boyle (see Note 55 on Hypotheses), but would have been inappropriate in the *Principia,* a work aiming at close adherence to experimental facts and rigid mathematical deduction from them. Hence, Newton's famous passage in the General Scholium at the end of the second and third editions of the *Principia,* "Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses." Relations of cause and effect have engaged the attention of scientists since the time of Newton. Famous is Robert Mayer's dictum—*causa aequat effectum*—a metaphysical tenet which guided him in arriving at the principle of the conservation of energy.

While Newton endeavored to subject the phenomena of physical Nature to mechanical law, he realized more fully than did his immediate followers that he had not succeeded in doing so altogether. In fact, he did not believe that there was a "world machine" which kept on running according to the law of gravitation without supervision of God,[1] but rather that irregularities in the solar system caused by action of planets and comets on each other are regulated by God whenever emergencies arise. "This most beautiful system of the sun, planets, and comets," says Newton in his *Principia* (Book III, Prop. XLII, General Scholium), "could only proceed from the counsel and dominion of an intelligent and powerful Being." The sufficiency of mechanical law to explain the mechanical phenomena of the solar system was entertained by Laplace, the great successor of Newton in elaborating mathematically the consequences of the law of gravitation. Laplace said in his *Essai philosophique sur les probabilités:*[2] "If an intelligence, for one given instant, recognizes all the forces which animate Nature, and the respective positions of the things which compose it, and if that intelligence is also sufficiently vast to subject these data to analysis, it will comprehend in one formula the movements of the largest bodies of the universe as well as those of the minutest atom: nothing will be uncertain to it, and the future as well as the past will be present to its vision. The human mind offers in the perfection which it has been able to give to astronomy, a modest example of such an intelligence." In these words we have a bold assertion of the belief in determinism, causal relation. Laplace believed that on the law of gravitation he and others had succeeded in showing what Newton did not do, namely, that the solar system is stable. In answer to a question whether it was true that in his *Mécanique céleste* he had never mentioned the Creator, Laplace told Napoleon, "Je n'avais pas besoin de cette hypothèse-la." Laplace had indeed succeeded in explaining, for example, all the inequalities in the observed motions of Jupiter and Saturn by Newton's law; every difficulty had led him to a new triumph.[3]

But even in classical mechanics Laplace's proof of the stability of the solar system is no longer considered conclusive. The nineteenth century came to demand greater mathematical rigor than had the eighteenth century. K. Weierstrass, in the last years of his life, gave much attention to this question of the stability of the solar system. So did H. Poincaré. But thus far no rigorous answer has been given. Referring to this problem, H. F. Baker said at the meeting of the British Association in 1913: "For those who can make pronouncements in regard to this I have a feeling of envy; for their methods, as yet, I have a quite other feeling. The interest of this problem alone is sufficient to justify the craving of the pure mathematician for powerful methods and unexceptional rigour."[4]

Laplace's determinism has lost its place in more recent physical science on still other grounds, namely, the rôle that statistical science and probability have come to play in some of the most fundamental inquiries. These sciences relate to the behavior of large assemblies. We acquire a new electric globe. How long will it last? We do not know. It cannot be predicted mathematically. But we do know that, by a statistical study of large numbers of that make of globes, the mean life of such a globe is a certain number of days. There is no individual relation of causality in this probable duration of our new globe. We are dealing here with a statistical mean, in which the ordinary relation of cause and effect does not prevail. If the glass were heated in the ordinary manner above its melting point, the relation of causality would hold directly, for we are certain the glass would melt. According to Kant, the law of causality is, "Everything that happens (begins to be) presupposes something upon which it follows according to some rule." No such rule exists for the length of service of our individual globe.

Eddington says, "In recent times some of the greatest triumphs of physical prediction have been furnished by admittedly statistical laws which do not rest upon a basis of causality."[5] According to modern views, the second law of thermodynamics[6] has a statistical foundation. The absence of determinism is seen in present-day atomic physics. Planck's derivation of his original quantum theory[7] rested on probability. The new quantum mechanics developed by W. Heisenberg[8] in 1927 emphasizes indeterminism, or the principle of uncertainty in microscopic processes. He points out that we cannot simultaneously measure the position and the velocity of an individual particle. Thus we cannot find both the position and the velocity of an electron, in an exact sense, at any one moment. Suppose we had a microscope so powerful as to reveal to us atomic and intra-atomic events. An isolated electron could not be seen, for we cannot see an object unless it emits or reflects light. But if light comes from an electron, the electron has been interacting with a light quantum or photon. This interaction changes the velocity of the electron just as surely as the velocity of a billiard ball is changed when hit by another, or the position of a gun changes when it recoils from the emission of a projectile. In the act of sending out light, the electron jumps. The ray of light may tell where the electron was, but it does not tell with what velocity the electron was moving, before the jump. Stating the principle somewhat more generally, if we can measure where the microscopic particle is, we cannot tell how fast it is moving, and if by certain effects we can measure how fast it is moving, we cannot at the same time tell where it is. Prediction of its status for the future cannot be made with exactness, but only with limited accuracy, expressed in terms of a certain probability.

This principle of indeterminism cuts at the root of the older physics, in which position and velocity were fundamental. Laplace's general determinism falls to ruins. According to this principle of indeterminism, we must abandon the conception of a chain of precise causal relations connecting the history of the physical universe, both past and future, with the exact state of the universe at a given instant.

Future research may afford important modification of this principle. Meanwhile, Eddington points out that this principle provides the believer in free will with an escape from the conclusions of the mechanistic philosophy of Laplace and the nineteenth-century classical science.

Great have been the advances since Newton. But the march of science is not always unidirectional. After two hundred years of wandering, we are returning to the teaching of Newton in some of our general viewpoints. Newton saw no conflict between religion and science. Present-day thought, as Eddington remarks, tends to remove some of the obstacles to a reconciliation of religion and science.

[1] Consult A. J. Snow, *Matter and Gravity in Newton's Philosophy*, Oxford, 1926, p. 204.

[2] *Œuvres complètes*, Tome VII, p. vi.

[3] Laplace, *Mécanique céleste*, Livre XV, Chap. I, p. 324.

[4] H. F. Baker, *Report of the British Association for 1913*, London, 1914, p. 371.

[5] Eddington, *The Nature of the Physical World*, p. 298.

[6] Consult H. Weyl, *Philosophie der Mathematik und Naturwissenschaften*, München, 1927, pp. 154, 155.

[7] Consult W. Schottky, "Das Kausalproblem der Quantentheorie als eine Grundfrage der modernen Naturforschung überhaupt," in *Die Naturwissenschaften*, vol. 9, 1921, pp. 492–496; E. Mach, *Analyse der Empfindungen*, ed. 3, 1902, Chap. V.

[8] W. Heisenberg of Kopenhagen in *Zeitschrift für Physik*, vol. 43, 1927, pp. 172–198. For comments on Heisenberg, see N. Bohr in *Die Naturwissenschaften*, vol. 16, 1928, p. 246.

57 (p. 549). *Translator of the System of the World.* An English translation of the *System of the World* was published in 1728, but the name of the translator was withheld. There is a long Latin passage in the *System of the World* which is virtually identical with a Latin passage in Book III of the *Principia*. The translations of these passages into English are so nearly alike, that they most probably are attributable to one and the same translator. Consider, in particular, the critical clause in that passage (*Principia*, Book III, Prop. XLI, Example; *System of the World*, Par. 37), "Nam quod dicitur Fixas ab Aegyptiis comatas nonnun-

quam visas fuisse." The translation of "comatas" caused trouble and was rendered in both books by the use of three words, "coma or capillitium," the whole clause being translated, "For as to what is alleged that the fixed stars have been sometimes seen by the Egyptians environed with a coma or capillitium." Such singular coincidence in a free translation makes it virtually certain that both books are put into English by the same translator—namely, Andrew Motte. In this revision of Motte's translation I have omitted the words "or capillitium."

58 (p. 576). *System of the World,* Paragraph 30. *A remote planet predicted?* J. Ph. Wolfers, in his German edition of Newton's *Principia,* Berlin, 1872, p. 659, finds this passage (particularly the part "Hence it is probable that there are comets ... which ... spend almost their whole time in regions beyond the planets") extremely interesting, as pointing to the existence of the planet Uranus, which at that time had not been observed, and which was first seen by William Herschel in 1781. Newton considered planets and comets to be closely related heavenly bodies, a view quite in accordance with modern ideas.

59 (p. 594). *System of the World,* Paragraph 54. *Terrestial tidal effects.* Tidal effects on land, which Newton considered insensible, were detected by Michelson and Gale in the solid earth, on the grounds of the Yerkes Observatory, in 1919, by the application of very delicate methods of observing interference fringes of monochromatic light. The results were used in a determination of the rigidity of the earth.[1]

[1] A. A. Michelson and Henry G. Gale, *Physical Review,* ser. 2, vol. 15, 1920, p. 144; *Science,* n.s., vol. 50, 1919, p. 327; also vol. 39, 1914, p. 927.