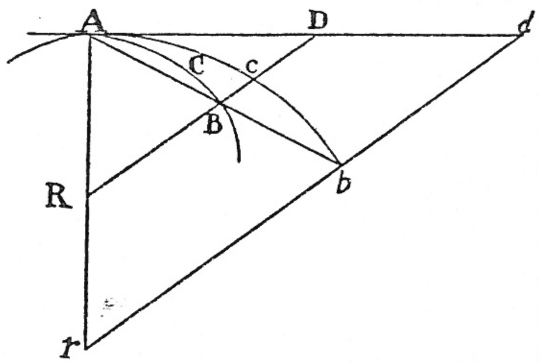
LEMMA V

All homologous sides of similar figures, whether curvilinear or rectilinear, are proportional; and the areas are as the squares of the homologous sides.

LEMMA VI



If any arc ACB, *given in position, is subtended by its chord* AB, *and in any point* A, *in the middle of the continued curvature, is touched by a right line* AD, *produced both ways; then if the points* A *and* B *approach one another and meet, I say, the angle* BAD, *contained between the chord and the tangent, will be diminished* in infinitum, *and ultimately will vanish.*

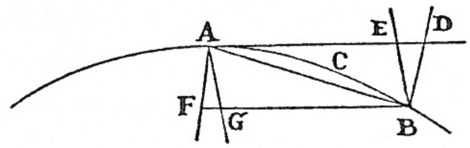
For if that angle does not vanish, the arc ACB will contain with the tangent AD an angle equal to a rectilinear angle; and therefore the curvature at the point A will not be continued, which is against the supposition.

LEMMA VII

The same things being supposed, I say that the ultimate ratio of the arc, chord, and tangent, any one to any other, is the ratio of equality.

For while the point B approaches towards the point A, consider always AB and AD as produced to the remote points *b* and *d*; and parallel to the secant BD draw *bd;* and let the arc A*cb* be always similar to the arc ACB. Then, supposing the points A and B to coincide, the angle *d*A*b* will vanish, by the preceding Lemma; and therefore the right lines A*b*, A*d* (which are always finite), and the intermediate arc A*cb*, will coincide, and become equal among themselves. Wherefore, the right lines AB, AD, and the intermediate arc ACB (which are always proportional to the former), will vanish, and ultimately acquire the ratio of equality. Q.E.D.

Cor. 1. Whence if through B we draw BF parallel to the tangent, always cutting any right line AF passing through A in F, this line BF will be ultimately in the ratio of equality with the evanescent arc ACB; because, completing the parallelogram AFBD, it is always in a ratio of equality with AD.



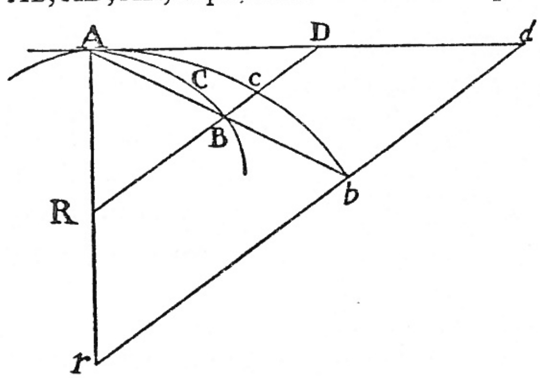
Cor. ii. And if through B and A more right lines are drawn, as BE, BD, AF, AG, cutting the tangent AD and its parallel BF; the ultimate ratio of all the abscissas AD, AE, BF, BG, and of the chord and arc AB, any one to any other, will be the ratio of equality.

Cor. iii. And therefore in all our reasoning about ultimate ratios, we may freely use any one of those lines for any other.

LEMMA VIII

If the right lines AR, BR, *with the arc* ACB, *the chord* AB, *and the tangent* AD, *constitute three triangles* RAB, RACB, RAD, *and the points* A *and* B *approach and meet: I say, that the ultimate form of these evanescent triangles is that of similitude, and their ultimate ratio that of equality.*

For while the point B approaches towards the point A, consider always AB, AD, AR, as produced to the remote points *b, d,* and *r,* and *rbd* as drawn parallel to RD, and let the arc A*cb* be always similar to the arc ACB. Then supposing the points A and B to coincide, the angle *b*A*d* will vanish; and therefore the three triangles *r*A*b, r*A*cb, r*A*d* (which are always finite), will coincide, and on that account become both similar and equal. And therefore the triangles RAB, RACB, RAD, which are always similar and proportional to these, will ultimately become both similar and equal among themselves. Q.E.D.

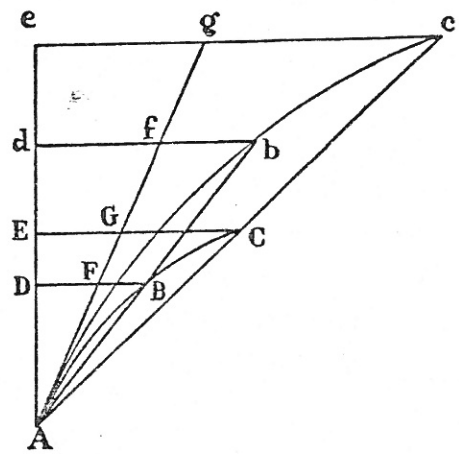


Cor. And hence in all reasonings about ultimate ratios, we may use any one of those triangles for any other.

LEMMA IX

If a right line AE, *and a curved line* ABC, *both given by position, cut each other in a given angle,* A; *and to that right line, in another given angle,* BD, CE *are ordinately applied, meeting the curve in* B, C; *and the points* B *and* C *together approach towards and meet in the point* A: *I say, that the areas of the triangles* ABD, ACE, *will ultimately be to each other as the squares of homologous sides.*

For while the points B, C, approach towards the point A, suppose always AD to be produced to the remote points *d* and *e,* so as A*d,* A*e* may be proportional to AD, AE; and the ordinates *db, ec,* to be drawn parallel to the ordinates DB and EC, and meeting AB and AC produced in *b* and *c.* Let the curve A*bc* be similar to the curve ABC, and draw the right line A*g* so as to touch both curves in A, and cut the ordinates DB, EC, *db, ec,* in F, G, *f, g.* Then, supposing the length A*e* to remain the same, let the points B and C meet in the point A; and the angle *c*A*g* vanishing, the curvilinear areas A*bd,* A*ce* will coincide with the rectilinear areas A*fd,* A*ge*; and therefore (by Lem. v) will be one to the other in the duplicate ratio of the sides A*d,* A*e.* But the areas ABD, ACE are always proportional to these areas; and so the sides AD, AE are to these sides. And therefore the areas ABD, ACE are ultimately to each other as the squares of the sides AD, AE. Q.E.D.



LEMMA X

The spaces which a body describes by any finite force urging it, whether that force is determined and immutable, or is continually augmented or continually diminished, are in the very beginning of the motion to each other as the squares of the times.

Let the times be represented by the lines AD, AE, and the velocities generated in those times by the ordinates DB, EC. The spaces described with these velocities will be as the areas ABD, ACE, described by those ordinates, that is, at the very beginning of the motion (by Lem. IX), in the duplicate ratio of the times AD, AE. Q.E.D.

COR. I. And hence one may easily infer, that the errors of bodies describing similar parts of similar figures in proportional times, the errors being generated by any equal forces similarly applied to the bodies, and measured by the distances of the bodies from those places of the similar figures, at which, without the action of those forces, the bodies would have arrived in those proportional times—are nearly as the squares of the times in which they are generated.

COR. II. But the errors that are generated by proportional forces, similarly applied to the bodies at similar parts of the similar figures, are as the product of the forces and the squares of the times.

COR. III. The same thing is to be understood of any spaces whatsoever described by bodies urged with different forces; all which, in the very beginning of the motion, are as the product of the forces and the squares of the times.

COR. IV. And therefore the forces are directly as the spaces described in the very beginning of the motion, and inversely as the squares of the times.

COR. V. And the squares of the times are directly as the spaces described, and inversely as the forces.

SCHOLIUM

If in comparing with each other indeterminate quantities of different sorts, any one is said to be directly or inversely as any other, the meaning is, that the former is augmented or diminished in the same ratio as the latter, or as its reciprocal. And if any one is said to be as any other two or more, directly or inversely, the meaning is, that the first is augmented or diminished in the ratio compounded of the ratios in which the others, or the reciprocals of the others, are augmented or diminished. Thus, if A is said to be as B directly, and C directly, and D inversely, the meaning is, that A is augmented or diminished in the same ratio as $B \cdot C \cdot \frac{1}{D}$, that is to say, that A and $\frac{BC}{D}$ are to each other in a given ratio.

Book Two

THE MOTION OF BODIES

(IN RESISTING MEDIUMS)

SECTION I

The motion of bodies that are resisted in the ratio of the velocity.

PROPOSITION I. THEOREM I

If a body is resisted in the ratio of its velocity, the motion lost by resistance is as the space gone over in its motion.

For since the motion lost in each equal interval of time is as the velocity, that is, as the small increment of space gone over, then, by composition, the motion lost in the whole time will be as the whole space gone over. Q.E.D.

COR. Therefore if the body, destitute of all gravity, move by its innate force only in free spaces, and there be given both its whole motion at the beginning, and also the motion remaining after some part of the way is gone over, there will be given also the whole space which the body can describe in an infinite time. For that space will be to the space now described as the whole motion at the beginning is to the part lost of that motion.

LEMMA I

Quantities proportional to their differences are continually proportional.

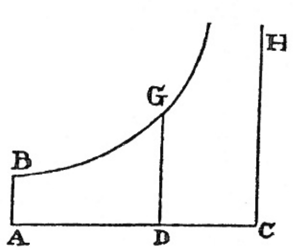
Let $$A : A-B = B : B-C = C : C-D = \&c.;$$
then, by subtraction,
$$A : B = B : C = C : D = \&c.$$ Q.E.D.

PROPOSITION II. THEOREM II

If a body is resisted in the ratio of its velocity, and moves, by its inertia only, through an homogeneous medium, and the times be taken equal, the velocities in the beginning of each of the times are in a geometrical progression, and the spaces described in each of the times are as the velocities.

Case 1. Let the time be divided into equal intervals; and if at the very beginning of each interval we suppose the resistance to act with one single impulse which is as the velocity, the decrement of the velocity in each of the intervals of time will be as the same velocity. Therefore the velocities are proportional to their differences, and therefore (by Lem. 1, Book II) continually proportional. Therefore if out of an equal number of intervals there be compounded any equal portions of time, the velocities at the beginning of those times will be as terms in a continued progression, which are taken by jumps, omitting everywhere an equal number of intermediate terms. But the ratios of these terms are compounded of the equal ratios of the intermediate terms equally repeated, and therefore are equal. Therefore the velocities, being proportional to those terms, are in geometrical progression. Let those equal intervals of time be diminished, and their number increased *in infinitum,* so that the impulse of resistance may become continual; and the velocities at the beginnings of equal times, always continually proportional, will be also in this case continually proportional. Q.E.D.[1]

Case 2. And, by division, the differences of the velocities, that is, the parts of the velocities lost in each of the times, are as the wholes; but the spaces described in each of the times are as the lost parts of the velocities (by Prop. 1, Book I), and therefore are also as the wholes. Q.E.D.



Cor. Hence if to the rectangular asymptotes AC, CH, the hyperbola BG is described, and AB, DG be drawn perpendicular to the asymptote AC, and both the velocity of the body, and the resistance of the medium, at the very beginning of the motion, be expressed by any given line AC, and, after some time is elapsed, by the indefinite line DC; the time may be expressed by the area ABGD, and the space described

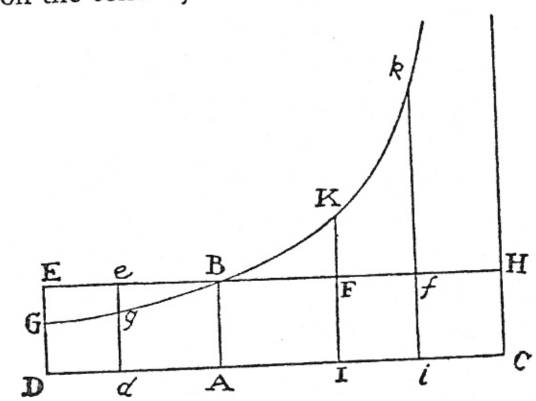
[[1] Appendix, Note 28.]

in that time by the line AD. For if that area, by the motion of the point D, be uniformly increased in the same manner as the time, the right line DC will decrease in a geometrical ratio in the same manner as the velocity; and the parts of the right line AC, described in equal times, will decrease in the same ratio.

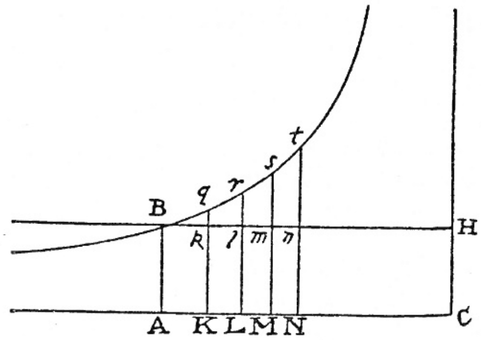
PROPOSITION III. PROBLEM I

To define the motion of a body which, in an homogeneous medium, ascends or descends in a right line, and is resisted in the ratio of its velocity, and acted upon by an uniform force of gravity.

The body ascending, let the gravity be represented by any given rectangle BACH; and the resistance of the medium, at the beginning of the ascent, by the rectangle BADE, taken on the contrary side of the right line AB. Through the point B, with the rectangular asymptotes AC, CH, describe an hyperbola, cutting the perpendiculars DE, *de* in G, *g;* and the body ascending will in the time DG*gd* describe the space EG*ge*; in the time DGBA, the space of the whole ascent EGB; in the time ABKI, the space of descent BFK; and in the time IK*ki* the space of descent KF*fk*; and the velocities of the bodies (proportional to the resistance of the medium) in these periods of time will be ABED, AB*ed*, o, ABFI, AB*fi* respectively; and the greatest velocity which the body can acquire by descending will be BACH.



For let the rectangle BACH be resolved into innumerable rectangles A*k*, K*l*, L*m*, M*n*, &c., which shall be as the increments of the velocities produced in so many equal times; then will o, A*k*, A*l*, A*m*, A*n*, &c., be as the whole velocities, and therefore (by supposition) as the resistances of the medium in the beginning of each of the equal times. Make AC to AK, or ABHC to AB*k*K, as the force of gravity to the resistance in the beginning of the second time; then from the force of gravity subtract the resistances, and ABHC, K*k*HC, L*l*HC, M*m*HC, &c., will be as the absolute forces with which the body is acted upon in the beginning of each of the times, and therefore (by Law I) as the increments of the velocities, that is, as the rectangles A*k*, K*l*, L*m*, M*n*, &c., and therefore (by Lem. I, Book II) in a geometrical progression. Therefore, if the right lines K*k*, L*l*, M*m*, N*n*, &c., are produced so as to meet the hyperbola in *q*, *r*, *s*, *t*, &c., the areas AB*q*K, K*qr*L, L*rs*M, M*st*N, &c., will be equal, and therefore analogous to the equal times and equal gravitating forces. But the area AB*q*K (by Cor. III, Lem. VII and VIII, Book I) is to the area B*kq* as K*q* to ½*kq*, or AC to ½AK, that is, as the force of gravity to the resistance in the middle of the first time. And by the like reasoning, the areas *q*KL*r*, *r*LM*s*, *s*MN*t*, &c., are to the areas *qklr*, *rlms*, *smnt*, &c., as the gravitating forces to the resistances in the middle of the second, third, fourth time, and so on. Therefore since the equal areas BAK*q*, *q*KL*r*, *r*LM*s*, *s*MN*t*, &c., are analogous to the gravitating forces, the areas B*kq*, *qklr*, *rlms*, *smnt*, &c., will be analogous to the resistances in the middle of each of the times, that is (by supposition), to the velocities, and so to the spaces described. Take the sums of the analogous quantities, and the areas B*kq*, B*lr*, B*ms*, B*nt*, &c., will be analogous to the whole spaces described; and also the areas AB*q*K, AB*r*L, AB*s*M, AB*t*N, &c., to the times. Therefore the body, in descending, will in any time AB*r*L describe the space B*lr*, and in the time L*rt*N the space *rlnt*. Q.E.D. And the like demonstration holds in ascending motion.



Cor. I. Therefore the greatest velocity that the body can acquire by falling is to the velocity acquired in any given time as the given force of gravity which continually acts upon it to the resisting force which opposes it at the end of that time.

Cor. II. But the time being augmented in an arithmetical progression, the sum of that greatest velocity and the velocity in the ascent, and also their difference in the descent, decreases in a geometrical progression.

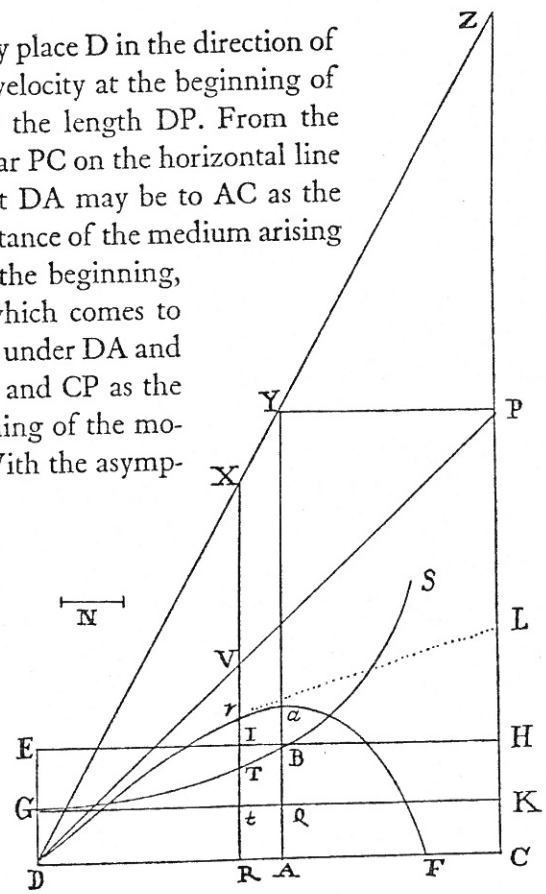
COR. III. Also the differences of the spaces, which are described in equal differences of the times, decrease in the same geometrical progression.

COR. IV. The space described by the body is the difference of two spaces, whereof one is as the time taken from the beginning of the descent, and the other as the velocity; which [spaces] also at the beginning of the descent are equal among themselves.

PROPOSITION IV. PROBLEM II

Supposing the force of gravity in any homogeneous medium to be uniform, and to tend perpendicularly to the plane of the horizon: to define the motion of a projectile therein, which suffers resistance proportional to its velocity.

Let the projectile go from any place D in the direction of any right line DP, and let its velocity at the beginning of the motion be represented by the length DP. From the point P let fall the perpendicular PC on the horizontal line DC, and cut DC in A, so that DA may be to AC as the vertical component of the resistance of the medium arising from the motion upwards at the beginning, to the force of gravity; or (which comes to the same) so that the rectangle under DA and DP may be to that under AC and CP as the whole resistance at the beginning of the motion, to the force of gravity. With the asymptotes DC, CP describe any hyperbola GTBS cutting the perpendiculars DG, AB in G and B; complete the parallelogram DGKC, and let its side GK cut AB in Q. Take a line N in the same ratio to QB as DC is in to CP; and from any point R of the right line DC erect RT perpendicular to it, meeting the hyperbola in T,



and the right lines EH, GK, DP in I, t, and V; in that perpendicular take Vr equal to $\frac{t\mathrm{GT}}{\mathrm{N}}$, or, which is the same thing, take Rr equal to $\frac{\mathrm{GTIE}}{\mathrm{N}}$; and the projectile in the time DRTG will arrive at the point r, describing the curved line DraF, the locus of the point r; thence it will come to its greatest height a in the perpendicular AB; and afterwards ever approach to the asymptote PC. And its velocity in any point r will be as the tangent rL to the curve. Q.E.I.

For $\mathrm{N}:\mathrm{QB}=\mathrm{DC}:\mathrm{CP}=\mathrm{DR}:\mathrm{RV}$,

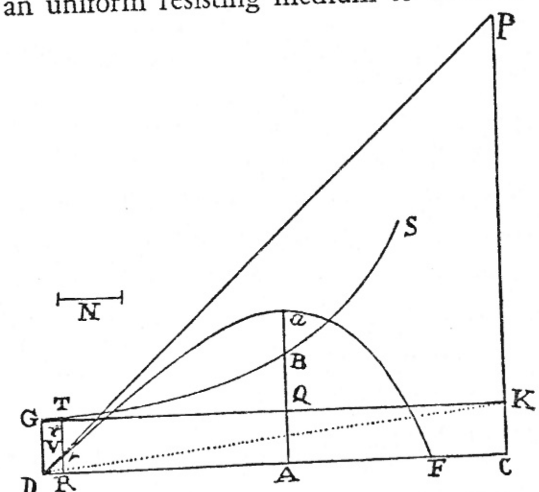
and therefore RV is equal to $\frac{\mathrm{DR}\cdot\mathrm{QB}}{\mathrm{N}}$, and R$r$ $\left(\text{that is, RV}-\text{V}r\text{, or } \frac{\mathrm{DR}\cdot\mathrm{QB}-t\mathrm{GT}}{\mathrm{N}}\right)$ is equal to $\frac{\mathrm{DR}\cdot\mathrm{AB}-\mathrm{RDGT}}{\mathrm{N}}$. Now let the time be represented by the area RDGT, and (by Laws, Cor. II) distinguish the motion of the body into two others, one of ascent, the other lateral. And since the resistance is as the motion, let that also be distinguished into two parts proportional and contrary to the parts of the motion: and therefore the length described by the lateral motion will be (by Prop. II, Book II) as the line DR, and the height (by Prop. III, Book II) as the area DR · AB − RDGT, that is, as the line Rr. But in the very beginning of the motion the area RDGT is equal to the rectangle DR · AQ, and therefore that line Rr $\left(\text{or } \frac{\mathrm{DR}\cdot\mathrm{AB}-\mathrm{DR}\cdot\mathrm{AQ}}{\mathrm{N}}\right)$ will then be to DR as AB − AQ or QB to N, that is, as CP to DC; and therefore as the motion upwards to the motion lengthwise at the beginning. Since, therefore, Rr is always as the height, and DR always as the length, and Rr is to DR at the beginning as the height to the length, it follows, that Rr is always to DR as the height to the length; and therefore that the body will move in the line DraF, which is the locus of the point r. Q.E.D.

Cor. I. Therefore Rr is equal to $\frac{\mathrm{DR}\cdot\mathrm{AB}}{\mathrm{N}}-\frac{\mathrm{RDGT}}{\mathrm{N}}$; and therefore if RT be produced to X so that RX may be equal to $\frac{\mathrm{DR}\cdot\mathrm{AB}}{\mathrm{N}}$, that is, if the parallelogram ACPY be completed, and DY cutting CP in Z be drawn, and

RT be produced till it meets DY in X; X*r* will be equal to $\frac{RDGT}{N}$, and therefore proportional to the time.

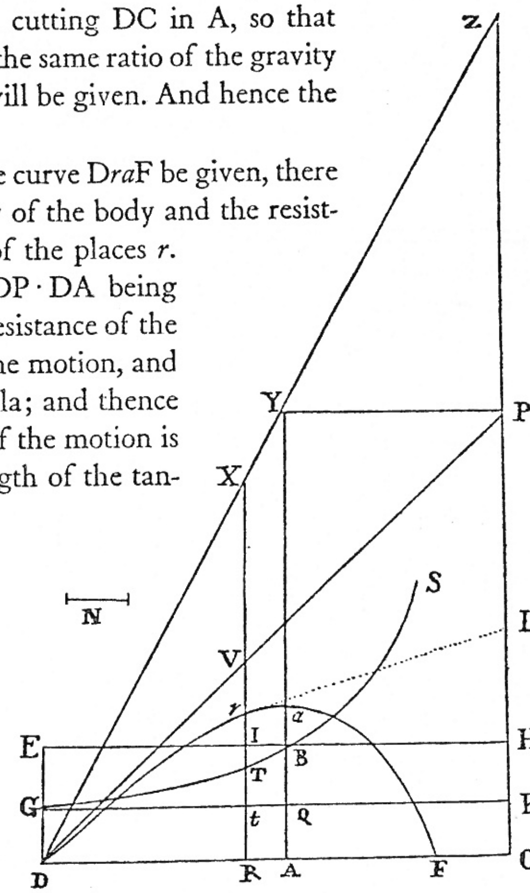
Cor. II. Whence if innumerable lines CR, or, which is the same, innumerable lines ZX, be taken in a geometrical progression, there will be as many lines X*r* in an arithmetical progression. And hence the curve D*ra*F is easily delineated by the table of logarithms.

Cor. III. If a parabola be constructed to the vertex D, and the diameter DG produced downwards, and its latus rectum is to 2DP as the whole resistance at the beginning of the motion to the gravitating force, the velocity with which the body ought to go from the place D, in the direction of the right line DP, so as in an uniform resisting medium to describe the curve D*ra*F, will be the same as that with which it ought to go from the same place D in the direction of the same right line DP, so as to describe a parabola in a nonresisting medium. For the latus rectum of this parabola, at the very beginning of the motion, is $\frac{DV^2}{Vr}$; and V*r* is $\frac{tGT}{N}$ or $\frac{DR \cdot Tt}{2N}$. But a right line which, if drawn, would touch the hyperbola GTS in G, is parallel to DK, and therefore T*t* is $\frac{CK \cdot DR}{DC}$, and N is $\frac{QB \cdot DC}{CP}$. And therefore V*r* is equal to $\frac{DR^2 \cdot CK \cdot CP}{2DC^2 \cdot QB}$, that is (because DR and DC, DV and DP are proportionals), to $\frac{DV^2 \cdot CK \cdot CP}{2DP^2 \cdot QB}$; and the latus rectum $\frac{DV^2}{Vr}$ comes out $\frac{2DP^2 \cdot QB}{CK \cdot CP}$, that is (because QB and CK, DA and AC are proportional), $\frac{2DP^2 \cdot DA}{AC \cdot CP}$, and therefore is to 2DP as DP · DA to CP · AC; that is, as the resistance to the gravity. Q.E.D.



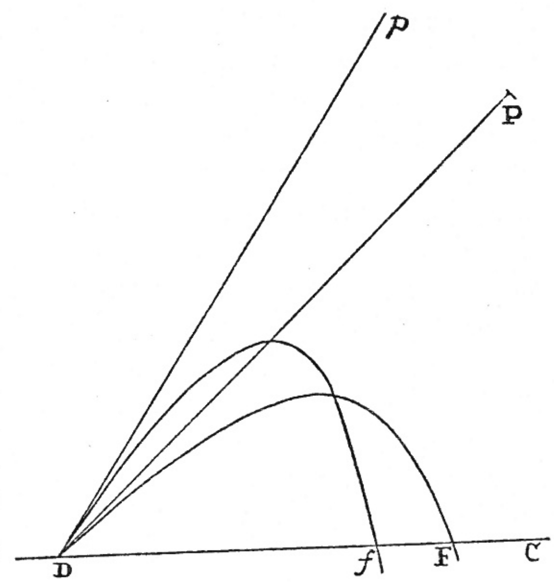
COR. IV. Hence if a body be projected from any place D with a given velocity, in the direction of a right line DP given by position, and the resistance of the medium, at the beginning of the motion, be given, the curve D*ra*F, which that body will describe, may be found. For the velocity being given, the latus rectum of the parabola is given, as is well known. And taking 2DP to that latus rectum, as the force of gravity to the resisting force, DP is also given. Then cutting DC in A, so that CP · AC may be to DP · DA in the same ratio of the gravity to the resistance, the point A will be given. And hence the curve D*ra*F is also given.

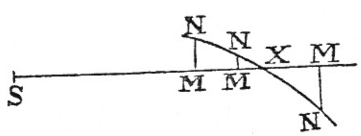
COR. V. And conversely, if the curve D*ra*F be given, there will be given both the velocity of the body and the resistance of the medium in each of the places *r*. For the ratio of CP · AC to DP · DA being given, there is given both the resistance of the medium at the beginning of the motion, and the latus rectum of the parabola; and thence the velocity at the beginning of the motion is given also. Then from the length of the tangent *r*L there is given both the velocity proportional to it, and the resistance proportional to the velocity in any place *r*.



COR. VI. But since the length 2DP is to the latus rectum of the parabola as the gravity to the resistance in D, and, from the velocity augmented, the resistance is augmented in the same ratio, but the latus rectum of the parabola is augmented as the square of that ratio, it is plain that the length 2DP is augmented in that simple ratio only; and is therefore always proportional to the velocity; nor will it be augmented or diminished by the change of the angle CDP, unless the velocity be also changed.

Cor vii. Hence appears the method of determining the curve D*ra*F nearly from the phenomena, and thence finding the resistance and velocity with which the body is projected. Let two similar and equal bodies be projected with the same velocity, from the place D, in different angles CDP, CD*p*; and let the places F, *f*, where they fall upon the horizontal plane DC, be known. Then taking any length for DP or D*p* suppose the resistance in D to be to the gravity in any ratio whatsoever, and let that ratio be represented by any length SM. Then, by computation, from that assumed length DP, find the lengths DF, D*f*; and from the ratio $\frac{Ff}{DF}$, found by calculation, subtract the same ratio as found by experiment; and let the difference be represented by the perpendicular MN. Repeat the same a second and a third time, by assuming always a new ratio SM of the resistance to the gravity, and collecting a new difference MN. Draw the positive differences on one side of the right line SM, and the negative on the other side; and through the points N, N, N, draw a regular curve NNN, cutting the right line SMMM in X, and SX will be the true ratio of the resistance to the gravity, which was to be found. From this ratio the length DF is to be found by calculation; and a length, which is to the assumed length DP as the length DF known by experiment to the length DF just now found, will be the true length DP. This being known, you will have both the curved line D*ra*F which the body describes, and also the velocity and resistance of the body in each place.





SCHOLIUM

However, that the resistance of bodies is in the ratio of the velocity, is more a mathematical hypothesis than a physical one. In mediums void of all tenacity, the resistances made to bodies are as the square of the velocities. For by the action of a swifter body, a greater motion in proportion to a greater velocity is communicated to the same quantity of the medium in a less time; and in an equal time, by reason of a greater quantity of the disturbed medium, a motion is communicated as the square of the ratio greater; and the resistance (by Law II and III) is as the motion communicated. Let us, therefore, see what motions arise from this law of resistance.

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.

no limit to a broken rule?

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 $\frac{3}{2}$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 $\frac{3}{2}$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	$5\frac{2}{3}$	$8\frac{2}{3}$	14	$24\frac{2}{3}$	*Semi-diameter of Jupiter*
Townly *by the micrometer*	5.52	8.78	13.47	24.72	
Cassini *by the telescope*	5	8	13	23	
Cassini *by the eclipse of the satellites*	$5\frac{2}{3}$	9	$14\frac{23}{60}$	$25\frac{3}{10}$	
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 $37\frac{1}{8}''$, and from the transit of the third $37\frac{3}{8}''$. 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 $37\frac{1}{4}''$, 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 $\frac{3}{2}$th 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.]

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 wth 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 Sr 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 ye 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 ye 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 I, 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. I, 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 I 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 1, 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 de-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.

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[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