17

The Creation of the Calculus

> Who, by a vigor of mind almost divine, the motions and figures of the planets, the paths of comets, and the tides of the seas first demonstrated. NEWTON'S EPITAPH

1. *The Motivation for the Calculus*

Following hard on the adoption of the function concept came the calculus, which, next to Euclidean geometry, is the greatest creation in all of mathematics. Though it was to some extent the answer to problems already tackled by the Greeks, the calculus was created primarily to treat the major scientific problems of the seventeenth century.

There were four major types of problems. The first was: Given the formula for the distance a body covers as a function of the time, to find the velocity and acceleration at any instant; and, conversely, given the formula describing the acceleration of a body as a function of the time, to find the velocity and the distance traveled. This problem arose directly in the study of motion and the difficulty it posed was that the velocities and the acceleration of concern to the seventeenth century varied from instant to instant. In calculating an instantaneous velocity, for example, one cannot, as one can in the case of average velocity, divide the distance traveled by the time of travel, because at a given instant both the distance traveled and time are zero, and 0/0 is meaningless. Nevertheless, it was clear on physical grounds that moving objects do have a velocity at each instant of their travel. The inverse problem of finding the distance covered, knowing the formula for velocity, involves the corresponding difficulty; one cannot multiply the velocity at any one instant by the time of travel to obtain the distance traveled because the velocity varies from instant to instant.

The second type of problem was to find the tangent to a curve. Interest in this problem stemmed from more than one source; it was a problem of pure geometry, and it was of great importance for scientific applications. Optics, as we know, was one of the major scientific pursuits of the seventeenth century; the design of lenses was of direct interest to Fermat, Descartes, Huygens, and Newton. To study the passage of light through a lens, one

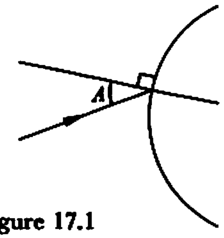

Figure 17.1

must know the angle at which the ray strikes the lens in order to apply the law of refraction. The significant angle is that between the ray and the normal to the curve (Fig. 17.1), the normal being the perpendicular to the tangent. Hence the problem was to find either the normal or the tangent. Another scientific problem involving the tangent to a curve arose in the study of motion. The direction of motion of a moving body at any point of its path is the direction of the tangent to the path.

Actually, even the very meaning of "tangent" was open. For the conic sections the definition of a tangent as a line touching a curve at only one point and lying on one side of the curve sufficed; this definition was used by the Greeks. But it was inadequate for the more complicated curves already in use in the seventeenth century.

The third problem was that of finding the maximum or minimum value of a function. When a cannonball is shot from a cannon, the distance it will travel horizontally—the range—depends on the angle at which the cannon is inclined to the ground. One "practical" problem was to find the angle that would maximize the range. Early in the seventeenth century, Galileo determined that (in a vacuum) the maximum range is obtained for an angle of fire of 45°; he also obtained the maximum heights reached by projectiles fired at various angles to the ground. The study of the motion of the planets also involved maxima and minima problems, such as finding the greatest and least distances of a planet from the sun.

The fourth problem was finding the lengths of curves, for example, the distance covered by a planet in a given period of time; the areas bounded by curves; volumes bounded by surfaces; centers of gravity of bodies; and the gravitational attraction that an *extended* body, a planet for example, exerts on another body. The Greeks had used the method of exhaustion to find some areas and volumes. Despite the fact that they used it for relatively simple areas and volumes, they had to apply much ingenuity, because the method lacked generality. Nor did they often come up with numerical answers. Interest in finding lengths, areas, volumes, and centers of gravity was revived when the work of Archimedes became known in Europe. The method of exhaustion was first modified gradually, and then radically by the invention of the calculus.

2. *Early Seventeenth-Century Work on the Calculus*

The problems of the calculus were tackled by at least a dozen of the greatest mathematicians of the seventeenth century and by several dozen minor ones. All of their contributions were crowned by the achievements of Newton and Leibniz. Here we shall be able to note only the principal contributions of the precursors of these two masters.

The problem of calculating the instantaneous velocity from a knowledge of the distance traveled as a function of the time, and its converse, were soon seen to be special cases of calculating the instantaneous rate of change of one variable with respect to another and its converse. The first significant treatment of general rate problems is due to Newton; we shall examine it later.

Several methods were advanced to find the tangent to a curve. In his *Traité des indivisibles*, which dates from 1634 (though not published until 1693), Gilles Persone de Roberval (1602–75) generalized a method Archimedes had used to find the tangent at any point on his spiral. Like Archimedes, Roberval thought of a curve as the locus of a point moving under the action of two velocities. Thus a projectile shot from a cannon is acted on by a horizontal velocity, PQ in Figure 17.2, and a vertical velocity, PR. The resultant of these two velocities is the diagonal of the rectangle formed on PQ and PR. Roberval took the line of this diagonal to be the tangent at P. As Torricelli pointed out, Roberval's method used a principle already asserted by Galileo, namely, that the horizontal and vertical velocities acted independently of each other. Torricelli himself applied Roberval's method to obtain tangents to curves whose equations we now write as $y = x^n$.

While the notion of a tangent as a line having the direction of the resultant velocity was more complicated than the Greek definition of a line touching a curve, this newer concept applied to many curves for which the older one failed. It was also valuable because it linked pure geometry and dynamics, which before Galileo's work had been regarded as essentially distinct. On the other hand, this notion of a tangent was objectionable on mathematical grounds, because it based the definition of tangent on physical concepts. Many curves arose in situations having nothing to do with motion and the definition of tangent was accordingly inapplicable. Hence other methods of finding tangents gained favor.

Fermat's method, which he had devised by 1629 and which is found in his 1637 manuscript *Methodus ad Disquirendam Maximam et Minimam* (Method of Finding Maxima and Minima),[1] is in substance the present method. Let PT be the desired tangent at P on a curve (Fig. 17.3). The length TQ is called the subtangent. Fermat's plan is to find the length of TQ, from which one knows the position of T and can then draw TP.

1. *Œuvres*, 1, 133–79; 3, 121–56.

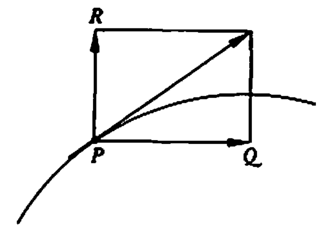

Figure 17.2

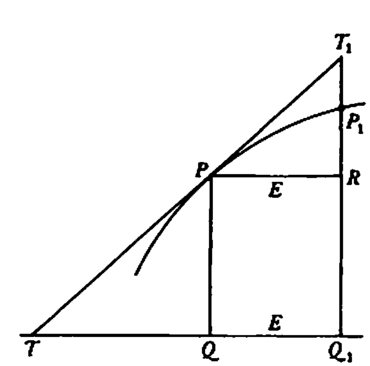

Figure 17.3

Let QQ_1 be an increment in TQ of amount E. Since triangle TQP is similar to triangle PRT_1,

$$TQ:PQ = E:T_1R.$$

But, Fermat says, T_1R is almost P_1R; therefore,

$$TQ:PQ = E:(P_1Q_1 - QP).$$

Calling $PQ, f(x)$ in our modern notation, we have

$$TQ:f(x) = E:[f(x+E) - f(x)].$$

Hence

$$TQ = \frac{E \cdot f(x)}{f(x+E) - f(x)}.$$

For the $f(x)$ Fermat treated, it was immediately possible to divide numerator and denominator of the above fraction by E. He then set $E = 0$ (he says, remove the E term) and so obtained TQ.

Fermat applied his method of tangents to many difficult problems. The method has the *form* of the now-standard method of the differential calculus, though it begs entirely the difficult theory of limits.

To Descartes the problem of finding a tangent to a curve was important because it enables one to obtain properties of curves—for example, the angle of intersection of two curves. He says, "This is the most useful, and the most general problem, not only that I know, but even that I have any desire to know in geometry." He gave his method in the second book of *La Géométrie*. It was purely algebraic and did not involve any concept of limit, whereas Fermat's did, if rigorously formulated. However, Descartes's method was useful only for curves whose equations were of the form $y = f(x)$, where $f(x)$ was a simple polynomial. Though Fermat's method was general, Descartes

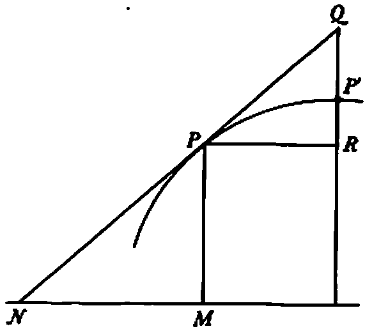

Figure 17.4

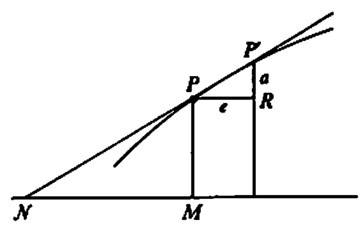

Figure 17.5

thought his own method was better; he criticized Fermat's, which admittedly was not clear as presented then, and tried to interpret it in terms of his own ideas. Fermat in turn claimed his method was superior and saw advantages in his use of the little increments E.

Isaac Barrow (1630–77) also gave a method of finding tangents to curves. Barrow was a professor of mathematics at Cambridge University. Well versed in Greek and Arabic, he was able to translate some of Euclid's works and to improve a number of other translations of the writings of Euclid, Apollonius, Archimedes, and Theodosius. His chief work, the *Lectiones Geometricae* (1669), is one of the great contributions to the calculus. In it he used geometrical methods, "freed," as he put it, "from the loathsome burdens of calculation." In 1669 Barrow resigned his professorship in favor of Newton and turned to theological studies.

Barrow's geometrical method is quite involved and makes use of auxiliary curves. However, one feature is worth noting because it illustrates the thinking of the time; it is the use of what is called the differential, or characteristic, triangle. He starts with the triangle PRQ (Fig. 17.4), which results from the increment PR, and uses the fact that this triangle is similar to triangle PMN to assert that the slope QR/PR of the tangent is equal to PM/MN. However, Barrow says, when the arc PP' is sufficiently small we may safely identify it with the segment PQ of the tangent at P. The triangle PRP' (Fig. 17.5), in which PP' is regarded both as an arc of the curve and as part of the tangent, is the characteristic triangle. It had been used much earlier by Pascal, in connection with finding areas, and by others before him.

In Lecture 10 of the *Lectiones*, Barrow does resort to calculation to find the tangent to a curve. Here the method is essentially the same as Fermat's. He uses the equation of the curve, say $y^2 = px$, and replaces x by $x + e$ and y by $y + a$. Then

$$y^2 + 2ay + a^2 = px + pe.$$

He subtracts $y^2 = px$ and obtains

$$2ay + a^2 = pe.$$

Then he discards higher powers of a and e (where present), which amounts to replacing PRP' of Figure 17.4 by PRP' of Figure 17.5, and concludes that

$$\frac{a}{e} = \frac{p}{2y}.$$

Now he argues that $a/e = PM/NM$, so that

$$\frac{PM}{NM} = \frac{p}{2y}.$$

Since PM is y, he has calculated NM, the subtangent, and knows the position of N.

The work on the third class of problems, finding the maxima and minima of functions, may be said to begin with an observation by Kepler. He was interested in the shape of casks for wine; in his *Stereometria Doliorum* (1615) he showed that, of all right parallelepipeds inscribed in a sphere and having square bases, the cube is the largest. His method was to calculate the volumes for particular choices of dimensions. This in itself was not significant; but he noted that as the maximum volume was approached, the *change* in volume for a fixed change in dimensions grew smaller and smaller.

Fermat in his *Methodus ad Disquirendam* gave his method, which he illustrated with the following example: Given a straight line (segment), it is required to find a point on it such that the rectangle contained by the two segments of the line is a maximum. He calls the whole segment B and lets one part of it be A. Then the rectangle is $AB - A^2$. He now replaces A by $A + E$. The other part is then $B - (A + E)$, and the rectangle becomes $(A + E)(B - A - E)$. He equates the two areas because, he argues, at a maximum the two function values—that is, the two areas—should be equal. Thus

$$AB + EB - A^2 - 2AE - E^2 = AB - A^2.$$

By subtracting common terms from the two sides and dividing by E, he gets

$$B = 2A + E.$$

He then sets $E = 0$ (he says, discard the E term) and gets $B = 2A$. Thus the rectangle is a square.

The method, Fermat says, is quite general; he describes it thus: If A is the independent variable, and if A is increased to $A + E$, then when E becomes indefinitely small and when the function is passing through a maximum or minimum, the two values of the function will be equal. These two values are equated; the equation is divided by E; and E is now made to vanish, so that from the resulting equation the value of A that makes the function a maximum or minimum can be determined. The method is essentially the one he used to find the tangent to a curve. However, the basic fact there is a similarity of two triangles; here it is the equality of two function values. Fermat did not see the need to justify introducing a non-zero E and then, after dividing by E, setting $E = 0$.[2]

The seventeenth-century work on finding areas, volumes, centers of gravity, and lengths of curves begins with Kepler, who is said to have been attracted to the volume problem because he noted the inaccuracy of methods used by wine dealers to find the volumes of kegs. This work (in *Stereometria Doliorum*) is crude by modern standards. For example, the area of a circle is to him the area of an infinite number of triangles, each with a vertex at the center and a base on the circumference. Then from the formula for the area of a regular inscribed polygon, 1/2 the perimeter times the apothem, he obtained the area of the circle. In an analogous manner he regarded the volume of a sphere as the sum of the volumes of small cones with vertices at the center of the sphere and bases on its surface. He then proceeded to show that the volume of the sphere is 1/3 the radius times the surface. The cone he regarded as a sum of very thin circular discs and was able thereby to compute its volume. Stimulated by Archimedes' *Spheroids and Conoids*, he generated new figures by rotation of areas and calculated the volumes. Thus he rotated the segment of a circle cut out by a chord around the chord and found the volume.

The identification of curvilinear areas and volumes with the sum of an infinite number of infinitesimal elements of the same dimension is the essence of Kepler's method. That the circle could be regarded as the sum of an infinite number of triangles was in his mind justified by the principle of continuity (Chap. 14, sec. 5). He saw no difference in kind between the two figures. For the same reason a line and an infinitesimal area were really the same; and he did, in some problems, regard an area as a sum of lines.

In *Two New Sciences* Galileo conceives of areas in a manner similar to Kepler's; in treating the problem of uniformly accelerated motion, he gives an argument to show that the area under the time-velocity curve is the distance. Suppose an object moves with varying velocity $v = 32t$, represented by the straight line in Figure 17.6; then the distance covered in time OA is the area OAB. Galileo arrived at this conclusion by regarding $A'B'$, say, as a typical velocity at some instant and also as the infinitesimal distance covered (as it would be if multiplied by a very small element of time), then arguing that the area OAB, which is made up of lines $A'B'$, must therefore be

2. For the equations that precede his setting $E = 0$, Fermat used the term *adaequalitas*, which Carl B. Boyer in *The Concepts of the Calculus*, p. 156, has aptly translated as "pseudo-equality."

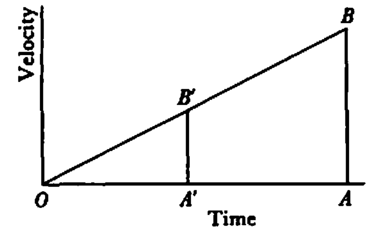

Figure 17.6

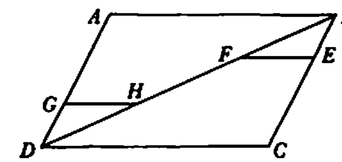

Figure 17.7

the total distance. Since AB is $32t$ and OA is t, the area of OAB is $16t^2$. The reasoning is of course unclear. It was supported in Galileo's mind by philosophical considerations that amount to regarding the area OAB as made up of an infinite number of indivisible units such as $A'B'$. He spent much time on the problem of the structure of continuous magnitudes such as line segments and areas but did not resolve it.

Bonaventura Cavalieri (1598–1647), a pupil of Galileo and professor in a lyceum in Bologna, was influenced by Kepler and Galileo and urged by the latter to look into problems of the calculus. Cavalieri developed the thoughts of Galileo and others on indivisibles into a geometrical method and published a work on the subject, *Geometria Indivisibilibus Continuorum Nova quadam Ratione Promota* (Geometry Advanced by a thus far Unknown Method, Indivisibles of Continua, 1635). He regards an area as made up of an indefinite number of equidistant parallel line segments and a volume as composed of an indefinite number of parallel plane areas; these elements he calls the indivisibles of area and volume, respectively. Cavalieri recognizes that the number of indivisibles making up an area or volume must be indefinitely large but does not try to elaborate on this. Roughly speaking, the indivisibilitists held, as Cavalieri put it in his *Exercitationes Geometricae Sex* (1647), that a line is made up of points as a string is of beads; a plane is made up of lines as a cloth is of threads; and a solid is made up of plane areas as a book is made up of pages. However, they allowed for an infinite number of the constituent elements.

Cavalieri's method or principle is illustrated by the following proposition, which of course can be proved in other ways. To show that the parallelogram $ABCD$ (Fig. 17.7) has twice the area of either triangle ABD or BCD, he argued that when $GD = BE$, then $GH = FE$. Hence triangles ABD and BCD are made up of an equal number of equal lines, such as GH and EF, and therefore must have equal areas.

The same principle is incorporated in the proposition now taught in solid geometry books and known as Cavalieri's Theorem. The principle says that if two solids have equal altitudes and if sections made by planes parallel

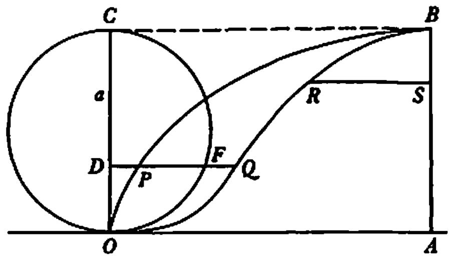

Figure 17.8

to the bases and at equal distances from them always have a given ratio, the volumes of the two solids have this given ratio to each other. Using essentially this principle, Cavalieri proved that the volume of a cone is 1/3 that of the circumscribed cylinder. Likewise he treated the area under two curves, say $y = f(x)$ and $y = g(x)$ in our notation, and over the same range of x-values; considering the areas as the sums of ordinates, if the ordinates of one are in a constant ratio to those of the other, then, says Cavalieri, the areas are in the same ratio. He showed by his methods in *Centuria di varii problemi* (1639) that, in our notation,

$$\int_0^a x^n\,dx = \frac{a^{n+1}}{n+1}$$

for positive integral values of n up to 9. However, his method was entirely geometrical. He was successful in obtaining correct results because he applied his principle to calculate ratios of areas and volumes where the ratio of the indivisibles making up the respective areas and volumes was constant.

Cavalieri's indivisibles were criticized by contemporaries, and Cavalieri attempted to answer them; but he had no rigorous justification. At times he claimed his method was just a pragmatic device to avoid the method of exhaustion. Despite criticism of the method, it was intensively employed by many mathematicians. Others, such as Fermat, Pascal, and Roberval, used the method and even the language, sum of ordinates, but thought of area as a sum of infinitely small rectangles rather than as a sum of lines.

In 1634 Roberval, who says he studied the "divine Archimedes," used essentially the method of indivisibles to find the area under one arch of the cycloid, a problem Mersenne called to his attention in 1629. Roberval is sometimes credited with independent discovery of the method of indivisibles, but actually he believed in the infinite divisibility of lines, surfaces, and volumes, so that there are no ultimate parts. He called his method the "method of infinities," though he used as the title of his work *Traité des indivisibles*.

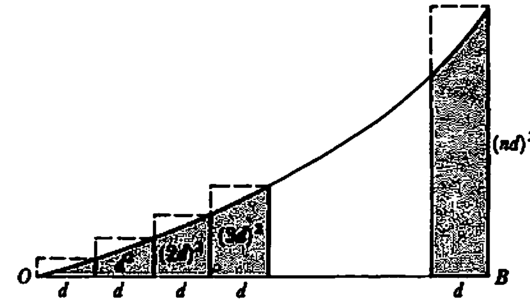

Figure 17.9

Roberval's method of obtaining the area under the cycloid is instructive. Let $OABP$ (Fig. 17.8) be the area under half of an arch of a cycloid. OC is the diameter of the generating circle and P is any point on the arch. Take $PQ = DF$. The locus of Q is called the companion curve to the cycloid. (The curve OQB is, in our notation, $y = a \sin x/a$ where a is the radius of the generating circle, provided the origin is at the midpoint of OQB and the x-axis is parallel to OA.) Roberval affirms that the curve OQB divides the rectangle $OABC$ into two equal parts because, basically, to each line DQ in $OQBC$ there corresponds an equal line RS in $OABQ$. Thus Cavalieri's principle is employed. The rectangle $OABC$ has its base and altitude equal, respectively, to the semicircumference and diameter of the generating circle; hence its area is twice that of the circle. Then $OABQ$ has the same area as the generating circle. Also, the area between OPB and OQB equals the area of the semicircle OFC, since by the very definition of Q, $DF = PQ$, so that these two areas are everywhere of the same width. Hence the area under the half-arch is 1 1/2 times the area of the generating circle. Roberval also found the area under one arch of the sine curve, the volume generated by revolving the arch about its base, other volumes connected with the cycloid, and the centroid of its area.

The most important new method of calculating areas, volumes, and other quantities started with modifications of the Greek method of exhaustion. Let us consider a typical example. Suppose one seeks to calculate the area under the parabola $y = x^2$ from $x = O$ to $x = B$ (Fig. 17.9). Whereas the method of exhaustion used different kinds of rectilinear approximating figures, depending on the curvilinear area in question, some seventeenth-century men adopted a systematic procedure using rectangles as shown. As the width d of these rectangles becomes smaller, the sum of the areas of the rectangles approaches the area under the curve. This sum, if the bases are all d in width, and if one uses the characteristic property of the parabola that the ordinate is the square of the abscissa, is

$$(1) \qquad d\cdot d^2 + d(2d)^2 + d(3d)^2 + \cdots + d(nd)^2$$

or

$$d^3(1 + 2^2 + 3^2 + \cdots + n^2).$$

Now the sum of the mth powers of the first n natural numbers had been obtained by Pascal and Fermat for use in just such problems; so the mathematicians could readily replace the last expression by

(2) $$d^3\left(\frac{2n^3 + 3n^2 + n}{6}\right).$$

But d is the fixed length OB divided by n. Hence (2) becomes

(3) $$OB^3\left(\frac{1}{3} + \frac{1}{2n} + \frac{1}{6n^2}\right).$$

Now if one argues, as these men did, that the last two terms can be neglected when n is infinite, the correct result is obtained. The limit process had not yet been introduced—or was only crudely perceived—and so the neglect of terms such as the last two was not justified.

We see that the method calls for approximating the curvilinear figure by rectilinear ones, as in the method of exhaustion. However, there is a vital shift in the final step: in place of the indirect proof used in the older method, here the number of rectangles becomes infinite and one takes the limit of (3) as n becomes infinite—though the thinking in terms of limit was at this stage by no means explicit. This new approach, used as early as 1586 by Stevin in his *Statics*, was pursued by many men, including Fermat.[3]

If the curve involved was not the parabola, then one had to replace the characteristic property of the parabola by that of the curve in question and so obtain some other series in place of (1) above. Summing the analogue of (1) to obtain the analogue of (2) did call for ingenuity. Hence the results on areas, volumes, and centers of gravity were limited. Of course the powerful method of evaluating the limit of such sums by reversing differentiation was not yet envisaged.

Using essentially the kind of summation technique we have just illustrated, Fermat knew before 1636 that (in our notation)

$$\int_0^a x^n\,dx = \frac{a^{n+1}}{n+1}$$

for all rational n except -1.[4] This result was also obtained independently by Roberval, Torricelli, and Cavalieri, though in some cases only in geometrical form and for more limited n.

Among those who used summation in geometrical form was Pascal. In 1658 he took up problems of the cycloid.[5] He calculated the area of any

3. *Œuvres*, 1, 255–59; 3, 216–19.
4. *Œuvres*, 1, 255–59; 3, 216–19.
5. *Traité des sinus du quart de cercle*, 1659 = *Œuvres*, 9, 60–76.

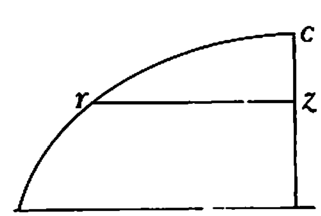

Figure 17.10

segment of the curve cut off by a line parallel to the base, the centroid of the segment, and the volumes of solids generated by such segments when revolved around their bases (YZ in Fig. 17.10) or a vertical line (the axis of symmetry). In this work, as well as in earlier work on areas under the curves of the family $y = x^n$, he summed small rectangles in the manner described in connection with (1) above, though his work and results were stated geometrically. Under the pseudonym of Dettonville, he proposed the problems he had solved as a challenge to other mathematicians, then published his own superior solutions (*Lettres de Dettonville*, 1659).

Before Newton and Leibniz, the man who did most to introduce analytical methods in the calculus was John Wallis (1616–1703). Though he did not begin to learn mathematics until he was about twenty—his university education at Cambridge was devoted to theology—he became professor of geometry at Oxford and the ablest British mathematician of the century, next to Newton. In his *Arithmetica Infinitorum* (1655), he applied analysis and the method of indivisibles to effect many quadratures and obtain broad and useful results.

One of Wallis's notable results, obtained in his efforts to calculate the area of the circle analytically, was a new expression for π. He calculated the area bounded by the axes, the ordinate at x, and the curve for the functions

$$y = (1 - x^2)^0, y = (1 - x^2)^1, y = (1 - x^2)^2, y = (1 - x^2)^3, \cdots$$

and obtained the areas

$$x, x - \frac{1}{3}x^3, x - \frac{2}{3}x^3 + \frac{1}{5}x^5, x - \frac{3}{3}x^3 + \frac{3}{5}x^5 - \frac{1}{7}x^7, \cdots$$

respectively. When $x = 1$, these areas are

(4) $$1, \frac{2}{3}, \frac{8}{15}, \frac{48}{105}, \cdots.$$

Now the circle is given by $y = (1 - x^2)^{1/2}$. Using induction and interpolation, Wallis calculated its area, and by further complicated reasoning arrived at

$$\frac{\pi}{2} = \frac{2\cdot2\cdot4\cdot4\cdot6\cdot6\cdot8\cdot8\cdots}{1\cdot3\cdot3\cdot5\cdot5\cdot7\cdot7\cdot9\cdots}.$$

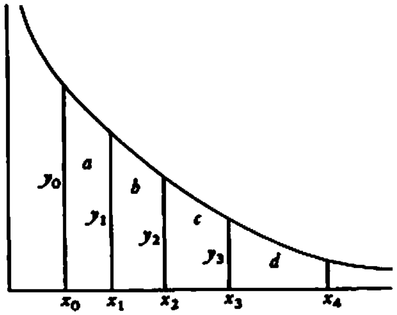

Figure 17.11

Gregory of St. Vincent in his *Opus Geometricum* (1647), gave the basis for the important connection between the rectangular hyperbola and the logarithm function. He showed, using the method of exhaustion, that if for the curve of $y = 1/x$ (Fig. 17.11) the x_i are chosen so that the areas $a, b, c, d, \ldots$ are equal, then the y_i are in geometric progression. This means that the sum of the areas from x_0 to x_i, which sums form an arithmetical progression, is proportional to the logarithm of the y_i values or, in our notation,

$$\int_{x_0}^{x} \frac{dx}{x} = k \log y.$$

This agrees with our familiar calculus result, because $y = 1/x$. The observation that the areas can be interpreted as logarithms is actually due to Gregory's pupil, the Belgian Jesuit Alfons A. de Sarasa (1618–67), in his *Solutio Problematis a Mersenno Propositi* (1649). About 1665 Newton also noted the connection between the area under the hyperbola and logarithms and included this relation in his *Method of Fluxions*. He expanded $1/(1 + x)$ by the binomial theorem and integrated term by term to obtain

$$\log_e (1 + x) = x - \frac{x^2}{2} + \frac{x^3}{3} - \cdots.$$

Nicholas Mercator, using Gregory's results, gave the same series independently (though he did not state it explicitly) in his *Logarithmotechnia* of 1668. Other men soon found series which, as we would put it, converged more rapidly. The work on the quadrature of the hyperbola and its relation to the logarithm function was done by many men, and much of it was communicated in letters, so that it is hard to trace the order of discovery and to assign credit.

Up to about 1650 no one believed that the length of a curve could equal exactly the length of a line. In fact, in the second book of *La Géométrie*, Descartes says the relation between curved lines and straight lines is not nor ever can be known. But Roberval found the length of an arch of the cycloid. The architect Christopher Wren (1632–1723) rectified the cycloid by showing

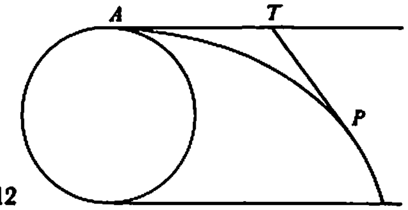

Figure 17.12

(Fig. 17.12) that arc $PA = 2PT$.[6] William Neile (1637–70) also obtained (1659) the length of an arch and, using a suggestion of Wallis, rectified the semicubical parabola ($y^3 = ax^2$).[7] Fermat, too, calculated some lengths of curves. These men usually used an inscribed polygon to approximate the curve, found the sum of the segments, then let the number of segments become infinite as each got smaller. James Gregory (1638–75), a professor at St. Andrews and Edinburgh (whose work was known slightly to his contemporaries but not known generally until a memorial volume, edited by H. W. Turnbull, appeared in 1939), gave in his *Geometriae Pars Universalis* (Universal Part of Geometry, 1668) a method of rectifying curves.

Further results on rectification were obtained by Christian Huygens (1629–95). In particular, he gave the length of arc of the cissoid. He also contributed to the work on areas and volumes and was the first to give results on the areas of surfaces beyond that of the sphere. Thus he obtained the areas of portions of the surfaces of the paraboloid and hyperboloid. Huygens obtained all these results by purely geometric methods, though he did use arithmetic, as Archimedes did occasionally, to obtain quantitative answers.

The rectification of the ellipse defied the mathematicians. In fact, James Gregory asserted that the rectification of the ellipse and the hyperbola could not be achieved in terms of known functions. For a while mathematicians were discouraged from further work on this problem and no new results were obtained until the next century.

We have been discussing the chief contributions of the predecessors of Newton and Leibniz to the four major problems that motivated the work on the calculus. The four problems were regarded as distinct; yet relationships among them had been noted and even utilized. For example, Fermat had used the very same method for finding tangents as for finding the maximum value of a function. Also, the problem of the rate of change of a function with respect to the independent variable and the tangent problem were readily seen to be the same. In fact, Fermat's and Barrow's method of finding tangents is merely the geometrical counterpart of finding the rate of change. But the major feature of the calculus, next to the very concepts of the

6. The method was published by Wallis in *Tractatus Duo* (1659 = *Opera*, 1, 550–69). Wren gave only the result.
7. Neile's work was published by Wallis in the reference in footnote 6.

derivative and of the integral as a limit of a sum, is the fact that the integral can be found by reversing the differentiation process or, as we say, by finding the antiderivative. Much evidence of this relationship had been encountered, but its significance was not appreciated. Torricelli saw in special cases that the rate problem was essentially the inverse of the area problem. It was, in fact, involved in Galileo's use of the fact that the area under a velocity-time graph gives distance. Since the rate of change of distance must be velocity, the rate of change of area, regarded as a "sum," must be the derivative of the area function. But Torricelli did not see the general point. Fermat, too, knew the relationship between area and derivative in special cases but did not appreciate its generality or importance. James Gregory, in his *Geometriae* of 1668, proved that the tangent and area problems are inverse problems but his book went unnoticed. In *Geometrical Lectures*, Barrow had the relationship between finding the tangent to a curve and the area problem, but it was in geometrical form, and he himself did not recognize its significance.

Actually an immense amount of knowledge of the calculus had accumulated before Newton and Leibniz made their impact. A survey of even the one book by Barrow shows a method of finding tangents, theorems on the differentiation of the product and quotient of two functions, the differentiation of powers of x, the rectification of curves, change of variable in a definite integral, and even the differentiation of implicit functions. Though in Barrow's case the geometric formulation made the discernment of the general ideas difficult, in Wallis's *Arithmetica Infinitorum* comparable results were in algebraic form.

One wonders then what remained to be achieved in the way of major new results. The answer is greater generality of method and the recognition of the generality of what had already been established in particular problems. The work on the calculus during the first two thirds of the century lost itself in details. Also, in their efforts to attain rigor through geometry, many men failed to utilize or explore the implications of the new algebra and coordinate geometry, and exhausted themselves in abortive subtle reasonings. What ultimately fostered the necessary insight and the attainment of generality was the arithmetical work of Fermat, Gregory of St. Vincent, and Wallis, the men whom Hobbes criticized for substituting symbols for geometry. James Gregory stated in the preface to *Geometriae* that the true division of mathematics was not into geometry and arithmetic but into the universal and the particular. The universal was supplied by the two all-embracing minds, Newton and Leibniz.

3. *The Work of Newton*

Great advances in mathematics and science are almost always built on the work of many men who contribute bit by bit over hundreds of years; eventually one man sharp enough to distinguish the valuable ideas of his predecessors from the welter of suggestions and pronouncements, imaginative enough to fit the bits into a new account, and audacious enough to build a master plan takes the culminating and definitive step. In the case of the calculus, this was Isaac Newton.

Newton (1642–1727) was born in the hamlet of Woolsthorpe, England, where his mother managed the farm left by her husband, who died two months before Isaac was born. He was educated at local schools of low educational standards and as a youth showed no special flair, except for an interest in mechanical devices. Having passed entrance examinations with a deficiency in Euclidean geometry, he entered Trinity College of Cambridge University in 1661 and studied quietly and unobstrusively. At one time he almost changed his course from natural philosophy (science) to law. Apparently receiving very little stimulation from his teachers, except possibly Barrow, he experimented by himself and studied Descartes's *Géométrie*, as well as the works of Copernicus, Kepler, Galileo, Wallis, and Barrow.

Just after Newton finished his undergraduate work the university was closed down because the plague was widespread in the London area. He left Cambridge and spent the years 1665 and 1666 in the quiet of the family home at Woolsthorpe. There he initiated his great work in mechanics, mathematics, and optics. At this time he realized that the inverse square law of gravitation, a concept advanced by others, including Kepler, as far back as 1612, was the key to an embracing science of mechanics; he obtained a general method for treating the problems of the calculus; and through experiments with light he made the epochal discovery that white light, such as sunlight, is really composed of all colors from violet to red. "All this," Newton said later in life, "was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy [science] more than at any other time since."

Newton said nothing about these discoveries. He returned to Cambridge in 1667 to secure a master's degree and was elected a fellow of Trinity College. In 1669 Isaac Barrow resigned his professorship and Newton was appointed in Barrow's place as Lucasian professor of mathematics. Apparently he was not a successful teacher, for few students attended his lectures; nor was the originality of the material he presented noticed by his colleagues. Only Barrow and, somewhat later, the astronomer Edmond Halley (1656–1742) recognized his greatness and encouraged him.

At first Newton did not publish his discoveries. He is said to have had an abnormal fear of criticism; De Morgan says that "a morbid fear of opposition from others ruled his whole life." When in 1672 he did publish his work on light, accompanied by his philosophy of science, he was severely criticized by most of his contemporaries, including Robert Hooke and Huygens, who had different ideas on the nature of light. Newton was so taken aback that he decided not to publish in the future. However, in 1675 he did publish another paper on light, which contained his idea that light was a stream of particles—the corpuscular theory of light. Again he was met by a storm of criticism and even claims by others that they had already discovered these ideas. This time Newton resolved that his results would be published after his death. Nonetheless he did publish subsequent papers and several famous books, the *Principia*, the *Opticks* (English edition 1704, Latin edition 1706), and the *Arithmetica Universalis* (1707).

From 1665 on he applied the law of gravitation to planetary motion; in this area the works of Hooke and Huygens influenced him considerably. In 1684 his friend Halley urged him to publish his results, but aside from his reluctance to publish Newton lacked a proof that the gravitational attraction exerted by a solid sphere acts as though the sphere's mass were concentrated at the center. He says, in a letter to Halley of June 20, 1686, that until 1685 he suspected that it was false. In that year he showed that a sphere whose density varies only with distance to the center does in fact attract an external particle as though the sphere's mass were concentrated at its center, and agreed to write up his work.

Halley then assisted Newton editorially and paid for the publication. In 1687 the first edition of the *Philosophiae Naturalis Principia Mathematica* (*The Mathematical Principles of Natural Philosophy*) appeared. There were two subsequent editions, in 1713 and 1726, the second edition containing improvements. Though the book brought Newton great fame, it was very difficult to understand. He told a friend that he had purposely made it difficult "to avoid being bated by little smatterers in mathematics." He no doubt hoped in this way to avoid the criticism that his earlier papers on light had received.

Newton was also a major chemist. Though there are no great discoveries associated with his work in this area, one must take into account that chemistry was then in its infancy. He had the correct idea of trying to explain chemical phenomena in terms of ultimate particles, and he had a profound knowledge of experimental chemistry. In this subject he wrote one major paper, "De natura acidorum" (written in 1692 and published in 1710). In the Philosophical Transactions of the Royal Society of 1701, he published a paper on heat that contains his famous law on cooling. Though he read the works of alchemists, he did not accept their cloudy and mystical views. The chemical and physical properties of bodies could, he believed, be accounted for in terms of the size, shape, and motion of the ultimate particles; he rejected the alchemists' occult forces, such as sympathy, antipathy, congruity, and attraction.

In addition to his work on celestial mechanics, light, and chemistry, Newton worked in hydrostatics and hydrodynamics. Beyond his superb experimental work on light, he experimented on the damping of pendulum

motion by various media, the fall of spheres in air and water, and the flow of water from jets. Like most men of the time Newton constructed his own equipment. He built two reflecting telescopes, even making the alloy for the frames, molding the frames, making the mountings, and polishing the lenses.

After serving as a professor for thirty-five years Newton became depressed and suffered a nervous breakdown. He decided to give up research and in 1695 accepted an appointment as warden of the British Mint in London. During his twenty-seven years at the mint, except for work on an occasional problem, he did no research. He became president of the Royal Society in 1703, an office he held until his death; he was knighted in 1705.

It is evident that Newton was far more engrossed in science than in mathematics and was an active participant in the problems of his time. He considered the chief value of his scientific work to be its support of revealed religion and was, in fact, a learned theologian, though he never took orders. He thought scientific research hard and dreary but stuck to it because it gave evidence of God's handiwork. Like his predecessor Barrow, Newton turned to religious studies later in life. In *The Chronology of Ancient Kingdoms Amended*, he tried to date accurately events described in the Bible and other religious documents by relating them to astronomical events. His major religious work was the *Observations Upon the Prophecies of Daniel and the Apocalypse of St. John*. Biblical exegesis was a phase of the rational approach to religion that was popular in the Age of Reason; Leibniz, too, took a hand in it.

So far as the calculus is concerned, Newton generalized the ideas already advanced by many men, established full-fledged methods, and showed the interrelationships of several of the major problems described above. Though he learned much as a student of Barrow, in algebra and the calculus he was more influenced by the works of Wallis. He said that he was led to his discoveries in analysis by the *Arithmetica Infinitorum*; certainly in his own work on the calculus he made progress by thinking analytically. However, even Newton thought the geometry was necessary for a rigorous proof.

In 1669 Newton circulated among his friends a monograph entitled *De Analysi per Aequationes Numero Terminorum Infinitas* (On Analysis by Means of Equations with an Infinite Number of Terms); it was not published until 1711. He supposes that he has a curve and that the area z (Fig. 17.13) under this curve is given by

$$z = ax^m, \tag{5}$$

where m is integral or fractional. He calls an infinitesimal increase in x, the moment of x, and denotes it by o, a notation used by James Gregory and the equivalent of Fermat's E. The area bounded by the curve, the x-axis, the y-axis, and the ordinate at $x + o$ he denotes by $z + oy$, oy being the moment of area. Then

$$z + oy = a(x + o)^m. \tag{6}$$

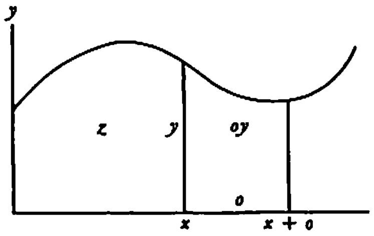

Figure 17.13

He applies the binomial theorem to the right side, obtaining an infinite series when m is fractional, subtracts (5) from (6), divides through by o, neglects those terms that still contain o, and obtains

$$y = max^{m-1}.$$

Thus, in our language, the rate of change of area at any x is the y-value of the curve at that value of x. Conversely, if the curve is $y = max^{m-1}$, the area under it is $z = ax^m$.

In this process Newton not only gave a general method for finding the instantaneous rate of change of one variable with respect to another (z with respect to x in the above example), but showed that area can be obtained by reversing the process of finding a rate of change. Since areas had also been expressed and obtained by the summation of infinitesimal areas, Newton also showed that such sums can be obtained by reversing the process of finding a rate of change. This fact, that summations (more properly, limits of sums) can be obtained by reversing differentiation, is what we now call the fundamental theorem of the calculus. Though it was known in special cases and dimly foreseen by Newton's predecessors, he saw it as general. He applied the method to obtain the area under many curves and to solve other problems that can be formulated as summations.

After showing that the derivative of the area is the y-value and asserting that the converse is true, Newton gave the rule that, if the y-value be a sum of terms, then the area is the sum of the areas that result from each of the terms. In modern terms, the indefinite integral of a sum of functions is the sum of the integrals of the separate functions.

His next contribution in the monograph carried further his use of infinite series. To integrate $y = a^2/(b + x)$, he divided a^2 by $b + x$ and obtained

$$y = \frac{a^2}{b} - \frac{a^2x}{b^2} + \frac{a^2x^2}{b^3} - \frac{a^2x^3}{b^4} + \cdots.$$

Having obtained this infinite series, he finds the integral by integrating term by term so that the area is

$$\frac{a^2x}{b} - \frac{a^2x^2}{2b^2} + \frac{a^2x^3}{3b^3} - \frac{a^2x^4}{4b^4} + \cdots.$$

He says of this infinite series that a few of the initial terms are exact enough for any use, provided that b be equal to x repeated some few times.

Likewise, to integrate $y = 1/(1 + x^2)$ he uses the binomial expansion to write

$$y = 1 - x^2 + x^4 - x^6 + x^8 - \cdots$$

and integrates term by term. He notes that if, instead, y is taken to be $1/(x^2 + 1)$, then by binomial expansion one would obtain

$$y = x^{-2} - x^{-4} + x^{-6} - x^{-8} + \cdots$$

and now one can integrate term by term. He then remarks that when x is small enough the first expansion is to be used; but when x is large, the second is to be used. Thus he was somewhat aware that what we call convergence is important, but had no precise notion about it.

Newton realized that he had extended term by term integration to infinite series but says in the *De Analysi*:

> And whatever the common Analysis performs by Means of Equations of a finite Number of Terms (provided that can be done) this can always perform the same by Means of infinite Equations so that I have not made any question of giving this the name of Analysis likewise. For the reasonings in this are no less certain than in the other; nor the equations less exact; albeit we Mortals whose reasoning powers are confined within narrow limits, can neither express, nor so conceive all the Terms of these Equations, as to know exactly from thence the quantities we want.

Thus far in his approach to the calculus Newton used what may be described as the method of infinitesimals. Moments are infinitely small quantities, indivisibles or infinitesimals. Of course the logic of what Newton did is not clear. He says in this work that his method is "shortly explained rather than accurately demonstrated."

Newton gave a second, more extensive, and more definitive exposition of his ideas in the book *Methodus Fluxionum et Serierum Infinitarum*, written in 1671 but not published until 1736. In this work he says he regards his variables as generated by the continuous motion of points, lines, and planes, rather than as static aggregates of infinitesimal elements, as in the earlier paper. A variable quantity he now called a fluent and its rate of change, the fluxion. His notation is $\dot{x}$ and $\dot{y}$ for fluxions of the fluents x and y. The fluxion of $\dot{x}$ is $\ddot{x}$, etc. The fluent of which x is the fluxion is $\acute{x}$, and the fluent of the latter is $\acute{\acute{x}}$.

In this second work Newton states somewhat more clearly the fundamental problem of the calculus: Given a relation between two fluents, find the relation between their fluxions, and conversely. The two variables whose relation is given can represent any quantities. However, Newton thinks of them as changing with time because it is a useful way of thinking, though, he points out, not necessary. Hence if o is an "infinitely small interval of time," then $\dot{x}o$ and $\dot{y}o$ are the indefinitely small increments in x and y or the moments of x and y. To find the relation between $\dot{y}$ and $\dot{x}$, suppose, for example, the fluent is $y = x^n$. Newton first forms

$$y + \dot{y}o = (x + \dot{x}o)^n,$$

and then proceeds as in the earlier paper. He expands the right side by using the binomial theorem, subtracts $y = x^n$, divides through by o, neglects all terms still containing o, and obtains

$$\dot{y} = nx^{n-1}\dot{x}.$$

In modern notation this result can be written

$$\frac{dy}{dt} = nx^{n-1}\frac{dx}{dt},$$

and since $dy/dx = (dy/dt)/(dx/dt)$, Newton, in finding the ratio of dy/dt to dx/dt or $\dot{y}$ to $\dot{x}$, has found dy/dx.

The method of fluxions is not essentially different from the one used in the *De Analysi*, nor is the rigor any better; Newton drops terms such as $\dot{x}\dot{x}o$ and $\dot{x}\dot{x}o\dot{x}o$ (he writes $\dot{x}^3oo$) on the ground that they are infinitely small compared to the one retained. However, his point of view in the *Method of Fluxions* is somewhat different. The moments $\dot{x}o$ and $\dot{y}o$ change with time o, whereas in the first paper the moments are ultimate fixed bits of x and z. This newer view follows the more dynamic thinking of Galileo; the older used the static indivisible of Cavalieri. The change served, as Newton put it, only to remove the harshness from the doctrine of indivisibles; however, the moments $\dot{x}o$ and $\dot{y}o$ are still some sort of infinitely small quantities. Moreover, $\dot{x}$ and $\dot{y}$, which are the fluxions or derivatives with respect to time of x and y, are never really defined; this central problem is evaded.

Given a relation between $\dot{x}$ and $\dot{y}$, finding the relation between x and y is more difficult than merely integrating a function of x. Newton treats several types: (1) when $\dot{x}$, $\dot{y}$, and x or y are present; (2) when $\dot{x}$, $\dot{y}$, x, and y are present; (3) when $\dot{x}$, $\dot{y}$, $\dot{z}$, and the fluents are present. The first type is the easiest and, in modern notation, calls for solving $dy/dx = f(x)$. Of the second type, Newton treats $\dot{y}/\dot{x} = 1 - 3x + y + x^2 + xy$ and solves it by a successive approximation process. He starts with $\dot{y}/\dot{x} = 1 - 3x + x^2$ as a first approximation, obtains y as a function of x, introduces this value of y on the right side of the original equation, and continues the process. Newton describes what he does but does not justify it. Of the third type, he treats $2\dot{x} - \dot{z} + \dot{y}x = 0$. He assumes a relation between x and y, say $x = y^2$, so that $\dot{x} = 2\dot{y}y$. Then the equation becomes $4\dot{y}y - \dot{z} - \dot{y}y^2 = 0$, from which he gets $2y^2 + (y^3/3) = z$. Thus, if the third type is regarded as a partial differential equation, Newton obtains only a particular integral.

Newton realized that in this paper he had presented a general method. In a letter to John Collins, dated December 10, 1672, wherein he gives the facts of his method and one example, he says,

> This is one particular, or rather corollary, of a general method, which extends itself, without any troublesome calculations, not only to the drawing of tangents to any curved lines, whether geometrical or mechanical... but also to resolving other abstruser kinds of problems about the crookedness, areas, lengths, centres of gravity of curves, etc.; nor is it... limited to equations which are free from surd quantities. This method I have interwoven with that other of working in equations, by reducing them to infinite series.

Newton emphasized the use of infinite series because thereby he could treat functions such as $(1 + x)^{3/2}$, whereas his predecessors had been limited on the whole to rational algebraic functions.

In his *Tractatus de Quadratura Curvarum* (Quadrature of Curves), a third paper on the calculus, written in 1676 but published in 1704, Newton says he has abandoned the infinitesimal or infinitely small quantity. He now criticizes the dropping of terms involving o for, he says,

> in mathematics the minutest errors are not to be neglected.... I consider mathematical quantities in this place not as consisting of very small parts, but as described by a continual motion. Lines are described, and thereby generated, not by the apposition of parts, but by the continued motion of points; superficies by the motion of lines; solids by the motions of superficies; angles by the rotation of the sides; portions of time by continued flux....
>
> Fluxions are, as near as we please, as the increments of fluents generated in times, equal and as small as possible, and to speak accurately, they are in the prime ratio of nascent increments; yet they can be expressed by any lines whatever, which are proportional to them.

Newton's new concept, the method of prime and ultimate ratio, amounts to this. He considers the function $y = x^n$. To find the fluxion of y or x^n, let x "by flowing" become $x + o$. Then x^n becomes

$$(x + o)^n = x^n + nox^{n-1} + \frac{n^2 - n}{2}o^2x^{n-2} + \cdots.$$

The increases of x and y, namely, o and $nox^{n-1} + \frac{n^2 - n}{2}o^2x^{n-2} + \cdots$ are to each other as (dividing both by o)

$$1 \text{ to } nx^{n-1} + \frac{n^2 - n}{2}ox^{n-2} + \cdots.$$

"Let now the increments vanish and their last proportion will be"

$$1 \text{ to } nx^{n-1}.$$

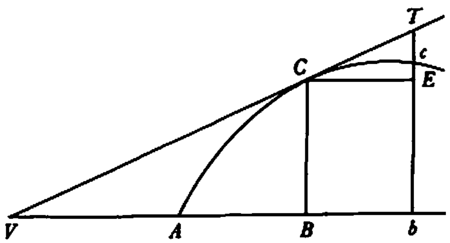

Figure 17.14

Then the fluxion of x is to the fluxion of x^n as 1 to nx^{n-1} or, as we would say today, the rate of change of y with respect to x is nx^{n-1}. This is the prime ratio of the nascent increments. Of course the logic of this version is no better than that of the preceding two; nevertheless Newton says this method is in harmony with the geometry of the ancients and that it is not necessary to introduce infinitely small quantities.

Newton also gave a geometrical interpretation. Given the data in Figure 17.14, suppose bc moves to BC so that c coincides with C. Then the curvilinear triangle CEc is "in the last form" similar to triangle CET, and its "evanescent" sides will be proportional to CE, ET, and CT. Hence the fluxions of the quantities AB, BC, and AC are, in the last ratio of their evanescent increments, proportional to the sides of the triangle CET or triangle VBC.

In the *Method of Fluxions* Newton made a number of applications of fluxions to differentiating implicit functions and to finding tangents of curves, maxima and minima of functions, curvature of curves, and points of inflection of curves. He also obtained areas and lengths of curves. In connection with curvature, he gave the correct formula for the radius of curvature, namely,

$$r = \frac{(1 + \dot{y}^2)^{3/2}}{\ddot{y}}$$

where $\dot{x}$ is taken as 1. He also gave this same quantity in polar coordinates. Finally, he included a brief table of integrals.

Newton did not publish his basic papers in the calculus until long after he had written them. The earliest printed account of his theory of fluxions appeared in Wallis's *Algebra* (2nd ed. in Latin, 1693), of which Newton wrote pages 390 to 396. Had he published at once he might have avoided the controversy with Leibniz on the priority of discovery.

Newton's first publication involving his calculus is the great *Mathematical Principles of Natural Philosophy*.[8] So far as the basic notion of the

8. The third edition was translated into English by Andrew Motte in 1729. This edition, revised and edited by Florian Cajori, was published by the University of California Press in 1946.

calculus, the fluxion, or, as we say, the derivative, is concerned, Newton makes several statements. He rejects infinitesimals or ultimate indivisible quantities in favor of "evanescent divisible quantities," quantities which can be diminished without end. In the first and third editions of the *Principia* Newton says, "Ultimate ratios in which quantities vanish are not, strictly speaking, ratios of ultimate quantities, but limits to which the ratios of these quantities, decreasing without limit, approach, and which, though they can come nearer than any given difference whatever, they can neither pass over nor attain before the quantities have diminished indefinitely."[9] This is the clearest statement he ever gave as to the meaning of his ultimate ratio. Apropos of the preceding quotation, he also says, "By the ultimate velocity is meant that with which the body is moved, neither before it arrives at its last place, when the motion ceases, nor after; but at the very instant when it arrives. . . . And, in like manner, by the ultimate ratio of evanescent quantities is to be understood the ratio of quantities, not before they vanish, nor after, but that with which they vanish."

In the *Principia* Newton used geometrical methods of proof. However, in what are called the Portsmouth Papers, containing unpublished work, he used analytical methods to find some of the theorems. These papers show that he also obtained analytically results beyond those he was able to translate into geometry. One reason he resorted to geometry is believed to be that the proofs would be more understandable to his contemporaries. Another is that he admired Huygens's geometrical work immensely and hoped to equal it. In these geometrical proofs Newton uses the basic limit processes of the calculus. Thus the area under a curve is considered essentially as the limit of the sum of the approximating rectangles, just as in the calculus today. However, instead of calculating such areas, he uses this concept to compare areas under different curves.

He proves that, when AR and BR (Fig. 17.15) are the perpendiculars to the tangents at A and B of the arc ACB, the ultimate ratio, when B approaches and coincides with A, of any two of the quantities chord AB, arc ACB, and AD, is 1. Hence he says in Corollary 3 to Lemma 2 of Book I, "And therefore in all our reasoning about ultimate ratios, we may freely use any one of these lines for any other." He then proves that when B approaches and coincides with A, the ratio of any two triangles (areas) RAB, $RACB$, and RAD will be 1. "And hence in all reasonings about ultimate ratios, we may use any one of these triangles for any other." Also, (Fig. 17.16) let BD and CE be perpendicular to AE (which is not necessarily tangent to arc ABC at A). When B and C approach and coincide with A, the ultimate ratio of the areas ACE and ABD will equal the ultimate ratio of AE^2 to AD^2.

The *Principia* contains a wealth of results, some of which we shall note.

9. Third edition, p. 39.

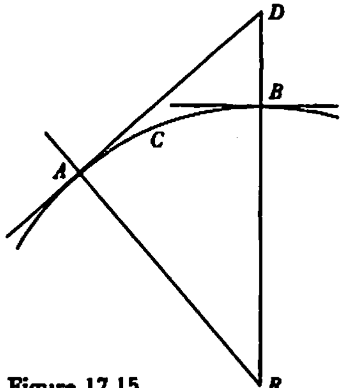

Figure 17.15

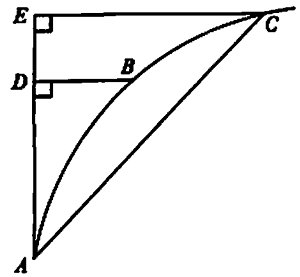

Figure 17.16

Though the book is devoted to celestial mechanics, it has enormous importance for the history of mathematics, not only because Newton's own work on the calculus was motivated in large part by his overriding interest in the problems treated therein, but because the *Principia* presented new topics and approaches to problems that were explored during the next hundred years in the course of which an enormous amount of analysis was created.

The *Principia* is divided into three books.[10] In a prefatory section Newton defines concepts of mechanics such as inertia, momentum, and force, then states the three famous axioms or laws of motion. In his words, they are:

Law I. Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.

Law II. The change [in the quantity] of motion is proportional to the motive power impressed; and is made in the direction of the right line in which that force is impressed.

By quantity of motion Newton means, as he has explained earlier, the mass times the velocity. Hence the change in motion, if the mass is constant, is the change in velocity, that is, the acceleration. This second law is now often written as $F = ma$, when the force F is in poundals, the mass m is in pounds, and the acceleration a is in feet per second per second. Newton's second law is really a vector statement; that is, if the force has components in, say, three mutually perpendicular directions, then each component causes an acceleration in its own direction. Newton did use the vector character of force in particular problems, but the full significance of the vector nature of

10. All references are to the edition mentioned in note 8.

the law was first fully recognized by Euler. This law incorporates the key change from the mechanics of Aristotle, which affirmed that force causes velocity. Aristotle had also affirmed that a force is needed to maintain velocity. Law I denies this.

Law III. To every action there is always opposed an equal reaction. . . .

We shall not digress into the history of mechanics except to note that the first two laws are more explicit and somewhat generalized statements of the principles of motion previously discovered and advanced by Galileo and Descartes. The distinction between mass, that is, the resistance a body offers to a change in its motion, and weight, the force gravity exerts on the mass of any object, is also due to these men; and the vector character of force generalizes Galileo's principle that the vertical and horizontal motions of a projectile, for example, can be treated independently.

Book I of the *Principia* begins with some theorems of the calculus, including the ones involving ultimate ratios cited above. It then discusses motion under central forces, that is, forces that always attract the moving object to one (fixed) point (the sun in practice), and proves in Proposition 1 that equal areas are swept out in equal time (which encompasses Kepler's law of areas). Newton considers next the motion of a body along a conic section and proves (Props. 11, 12, and 13) that the force must vary with the inverse square of the distance from some fixed point. He also proves the converse, which contains Kepler's first law. After some treatment of centripetal force, he deduces Kepler's third law (Prop. 15). There follow two sections devoted to properties of the conic sections. The principal problem is the construction of conics that satisfy five given conditions; in practice these are usually observational data. Then, given the time an object has been in motion along a conic section, he determines its velocity and position. He takes up the motion of the apse lines, that is, the lines joining the center of attraction (at one focus) to the maximum or minimum distance of a body moving along a conic that is itself rotating at some rate about the focus. Section 10 is devoted to the motion of bodies along surfaces with special reference to pendulum motion. Here Newton gives due acknowledgment to Huygens. In connection with the accelerating effect of gravity on motions, he investigates geometrical properties of cycloids, epicycloids, and hypocycloids and gives the length of the epicycloid (Prop. 49).

In Section 11 Newton deduces from the laws of motion and the law of gravitation the motion of two bodies, each attracting the other in accordance with the gravitational force. Their motion is reduced to the motion of one around the fixed second body. The moving body traverses an ellipse.

He then considers the attraction exerted by spheres and spheroids of uniform and varying density on a particle. He gives (Sec. 12, Prop. 70) a geometrical proof that a thin homogeneous spherical shell exerts no force on a particle in its interior. Since this result holds for a thin shell, it holds for a sum of such shells, that is, for a shell of finite thickness. (He proves later [Prop. 91, Cor. 3] that the same result holds for a homogeneous ellipsoidal shell, that is, a shell contained between two similar ellipsoidal surfaces, similarly placed.) Proposition 71 shows that the attraction of a thin homogeneous spherical shell on an *external* particle is equivalent to the attraction that would be exerted if the mass of the shell were concentrated at the center, so that the shell attracts the external particle toward the center and with a force varying inversely as the square of the distance from the center. Proposition 73 shows that a solid homogeneous sphere attracts a particle inside with a force proportional to the particle's distance from the center. As for the attraction that a solid homogeneous sphere exerts on an external point, Proposition 74 shows that it is the same as if the mass of the sphere were concentrated at its center. Then if two spheres attract each other, the first attracts every particle of the second as if the mass of the first were concentrated at its center. Thus the first sphere becomes a particle attracted by the distributed mass of the second; hence the second sphere can also be treated as a particle with its mass concentrated at its center. Thus both spheres can be treated as particles with their masses concentrated at their respective centers. All these results, original with Newton, are extended to spheres whose densities are spherically symmetric and to other laws of attraction in addition to the inverse square law.

Newton next takes up the motion of three bodies, each attracting the other two, and obtains some approximate results. The problem of the motion of three bodies has been a major one since Newton's time and has not as yet been solved exactly.

The second book of the *Principia* is devoted to the motion of bodies in resisting media such as air and liquids. It is the beginning of the subject of hydrodynamics. Newton assumes in some problems that the resistance of the medium is proportional to the velocity and in others to the square of the velocity of the moving body. He considers what shape a body must have to encounter least resistance (see Chap. 24, sec. 1). He also considers the motion of pendulums and projectiles in air and in fluids. A section is devoted to the theory of waves in air (e.g., sound waves) and he obtains a formula for the velocity of sound in air. He also treats the motion of waves in water. Newton continues with a description of experiments he made to determine the resistance fluids offer to bodies moving in them. One major conclusion is that the planets move in a vacuum. In this book Newton broke entirely new ground; however, the definitive work on fluid motion was yet to be done

Book III, entitled *On the System of the World*, contains the application of the general theory developed in Book I to the solar system. It shows how the sun's mass can be calculated in terms of the earth's mass, and that the mass of any planet having a satellite can be found in the same way. He

calculates the average density of the earth and finds it to be between 5 and 6 times that of water (today's figure is about 5.5).

He shows that the earth is not a true sphere but an oblate spheroid and calculates the flattening; his result is that the ellipticity of the oblate spheroid is 1/230 (the figure today is 1/297). From the observed oblateness of any planet, the length of its day is then calculated. Using the amount of flattening and the notion of centripetal force, Newton computes the variation of the earth's gravitational attraction over the surface and thus the variation in the weight of an object. He proves that the attractive force of a spheroid is not the same as if the spheroid's mass were concentrated at its center.

He then accounts for the precession of the equinoxes. The explanation is based on the fact that the earth is not spherical but bulges out along the equator. Consequently the gravitational attraction of the moon on the earth does not effectively act on the center of the earth but forces a periodic change in the direction of the earth's axis of rotation. The period of this change was calculated by Newton and found to be 26,000 years, the value obtained by Hipparchus by inference from observations available to him.

Newton explained the main features of the tides (Book I, Prop. 66, Book III, Props. 36, 37). The moon is the main cause; the sun, the second. Using the sun's mass he calculated the height of the solar tides. From the observed heights of the spring and neap tides (sun and moon in full conjunction or full opposition) he determined the lunar tide and made an estimate of the mass of the moon. Newton also managed to give some approximate treatment of the effect of the sun on the moon's motion around the earth. He determined the motion of the moon in latitude and longitude; the motion of the apse line (the line from the center of the earth to the maximum distance of the moon); the motion of the nodes (the points in which the moon's path cuts the plane of the earth's orbit; these points regress, that is, move slowly in a direction opposite to the motion of the moon itself); the evection (a periodic change in the eccentricity of the moon's orbit); the annual equation (the effect on the moon's motion of the daily change in distance between the earth and the sun); and the periodic change in the inclination of the plane of the moon's orbit to the plane of the earth's orbit. There were seven known irregularities in the motion of the moon and Newton discovered two more, the inequalities of the apogee (apse line) and of the nodes. His approximation gave only half of the motion of the apse line. Clairaut in 1752 improved the calculation and obtained the full 3° of rotation of the apse line; however, much later John Couch Adams found the correct calculation in Newton's papers. Finally Newton showed that the comets must be moving under the gravitational attraction of the sun because their paths, determined on the basis of observations, are conic sections. Newton devoted a great deal of time to the problem of the moon's motion because, as we noted in the preceding chapter, the knowledge was needed to improve the method of determining longitude. He worked so hard on this problem that he complained it made his head ache.

4. *The Work of Leibniz*

Though his contributions were quite different, the man who ranks with Newton in building the calculus is Gottfried Wilhelm Leibniz (1646–1716). He studied law and, after defending a thesis on logic, received a Bachelor of Philosophy degree. In 1666, he wrote the thesis *De Arte Combinatoria* (On the Art of Combinations),[11] a work on a universal method of reasoning; this completed his work for a doctorate in philosophy at the University of Altdorf and qualified him for a professorship. During the years 1670 and 1671 Leibniz wrote his first papers on mechanics, and, by 1671, had produced his calculating machine. He secured a job as an ambassador for the Elector of Mainz and in March of 1672 went to Paris on a political mission. This visit brought him into contact with mathematicians and scientists, notably Huygens, and stirred up his interest in mathematics. Though he had done a little reading in the subject and had written the paper of 1666, he says he knew almost no mathematics up to 1672. In 1673 he went to London and met other scientists and mathematicians, including Henry Oldenburg, at that time secretary of the Royal Society of London. While making his living as a diplomat, he delved further into mathematics and read Descartes and Pascal. In 1676 Leibniz was appointed librarian and councillor to the Elector of Hanover. Twenty-four years later the Elector of Brandenburg invited Leibniz to work for him in Berlin. While involved in all sorts of political maneuvers, including the succession of George Ludwig of Hanover to the English throne, Leibniz worked in many fields and his side activities covered an enormous range. He died neglected in 1716.

In addition to being a diplomat, Leibniz was a philosopher, lawyer, historian, philologist, and pioneer geologist. He did important work in logic, mechanics, optics, mathematics, hydrostatics, pneumatics, nautical science, and calculating machines. Though his profession was jurisprudence, his work in mathematics and philosophy is among the best the world has produced. He kept contact by letter with people as far away as China and Ceylon. He tried endlessly to reconcile the Catholic and Protestant faiths. It was he who proposed, in 1669, that a German Academy of Science be founded; finally the Berlin Academy was organized in 1700. His original recommendation had been for a society to make inventions in mechanics and discoveries in chemistry and physiology that would be useful to mankind; Leibniz wanted knowledge to be applied. He called the universities "monkish" and charged that they possessed learning but no judgment and were absorbed in trifles.

11. Published 1690 = *Die philosophische Schriften*, 4, 27–102.

Instead he urged the pursuit of real knowledge—mathematics, physics, geography, chemistry, anatomy, botany, zoology, and history. To Leibniz the skills of the artisan and the practical man were more valuable than the learned subtleties of the professional scholars. He favored the German language over Latin because Latin was allied to the older, useless thought. Men mask their ignorance, he said, by using the Latin language to impress people. German, on the other hand, was understood by the common people and could be developed to help clarity of thought and acuteness of reasoning.

Leibniz published papers on the calculus from 1684 on, and we shall say more about them later. However, many of his results, as well as the development of his ideas, are contained in hundreds of pages of notes made from 1673 on but never published by him. These notes, as one might expect, jump from one topic to another and contain changing notation as Leibniz's thinking developed. Some are simply ideas that occurred to him while reading books or articles by Gregory of St. Vincent, Fermat, Pascal, Descartes, and Barrow or trying to cast their thoughts into his own way of approaching the calculus. In 1714 Leibniz wrote *Historia et Origo Calculi Differentialis*, in which he gives an account of the development of his own thinking. However, this was written many years after he had done his work and, in view of the weaknesses of human memory and the greater insight he had acquired by that time, his history may not be accurate. Since his purpose was to defend himself against an accusation of plagiarism, he might have distorted unconsciously his account of the origins of his ideas.

Despite the confused state of Leibniz's notes we shall examine a few, because they reveal how one of the greatest intellects struggled to understand and create. By 1673 he was aware of the important direct and inverse problem of finding tangents to curves; he was also quite sure that the inverse method was equivalent to finding areas and volumes by summations. The somewhat systematic development of his ideas begins with notes of 1675. However, it seems helpful, in order to understand his thinking, to note that in his *De Arte Combinatoria* he had considered sequences of numbers, first differences, second differences, and higher-order differences. Thus for the sequence of squares

$$0, 1, 4, 9, 16, 25, 36,$$

the first differences are

$$1, 3, 5, 7, 9, 11$$

and the second differences are

$$2, 2, 2, 2, 2.$$

Leibniz noted the vanishing of the second differences for the sequence of natural numbers, the third differences for the sequence of squares, and so on.

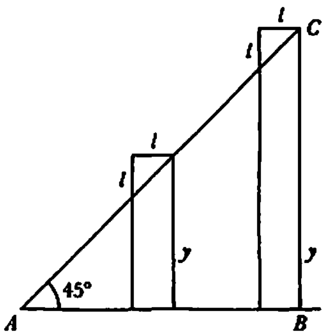

Figure 17.17

He also observed, of course, that if the original sequence starts from 0, the sum of the first differences is the last term of the sequence.

To relate these facts to the calculus he had to think of the sequence of numbers as the y-values of a function and the difference of any two as the difference of two nearby y-values. Initially he thought of x as representing the order of the term in the sequence and y as representing the value of that term.

The quantity dx, which he often writes as a, is then 1 because it is the difference of the orders of two successive terms, and dy is the actual difference in the values of two successive terms. Then using omn. as an abbreviation for the Latin *omnia*, to mean sum, and using l for dy, Leibniz concludes that omn. $l = y$, because omn. l is the sum of the first differences of a sequence whose terms begin with 0 and so gives the last term. However, omn. yl presents a new problem. Leibniz obtains the result that omn. yl is $y^2/2$ by thinking in terms of the function $y = x$. Thus, as Figure 17.17 shows, the area of triangle ABC is the sum of the yl (for "small" l) and it is also $y^2/2$. Leibniz says, "Straight lines which increase from nothing each multiplied by its corresponding element of increase form a triangle." These few facts already appear, among more complicated ones, in papers of 1673.

In the next stage he struggled with several difficulties. He had to make the transition from a discrete series of values to the case where dy and dx are increments of an arbitrary function y of x. Since he was still tied to sequences, wherein x is the order of the term, his a or dx was 1; so he inserted and omitted a freely. When he made the transition to the dy and dx of any function, this a was no longer 1. However, while still struggling with the notion of summation he ignored this fact.

Thus in a manuscript of October 29, 1675, Leibniz starts with

(7) $$\text{omn.}\, yl = \overline{\text{omn.}\overline{\text{omn.}\, l}\,\frac{l}{a}},$$

which holds because y itself is omn. l. Here he divides l by a to preserve dimensions. Leibniz says that (7) holds, whatever l may be. But, as we saw in connection with Figure 17.17,

(8) $$\text{omn.}\, yl = \frac{y^2}{2}.$$

Hence from (7) and (8)

(9) $$\frac{y^2}{2} = \overline{\text{omn.}\overline{\text{omn.}\, l}\,\frac{l}{a}}.$$

In our notation, he has shown that

$$\frac{y^2}{2} = \int\left\{\int dy\right\}\frac{dy}{dx} = \int y\,\frac{dy}{dx}.$$

Leibniz says that this result is admirable.

Another theorem of the same kind, which Leibniz derived from a geometrical argument, is

(10) $$\text{omn.}\, xl = x\,\text{omn.}\, l - \text{omn.omn.}\, l,$$

where l is the difference in values of two successive terms of a sequence and x is the number of the term. For us this equation is

$$\int x\, dy = xy - \int y\, dx.$$

Now Leibniz lets l itself in (10) be x, and obtains

$$\text{omn.}\, x^2 = x\,\text{omn.}\, x - \text{omn.omn.}\, x.$$

But omn. x, he says, is $x^2/2$ (he has shown that omn. yl is $y^2/2$). Hence

$$\text{omn.}\, x^2 = x\frac{x^2}{2} - \text{omn.}\,\frac{x^2}{2}.$$

By transposing the last term he gets

$$\text{omn.}\, x^2 = \frac{x^3}{3}.$$

In this manuscript of October 29, 1675, Leibniz decided to write $\int$ for omn., so that

$$\int l = \text{omn.}\, l \quad \text{and} \quad \int x = \frac{x^2}{2}.$$

The symbol $\int$ is an elongated S for "sum."

Leibniz realized rather early, probably from studying the work of Barrow, that differentiation and integration as a summation must be inverse processes; so area, when differentiated, must give a length. Thus, in the same manuscript of October 29, Leibniz says, "Given l and its relation to x, to find $\int l$." Then, he says, "Suppose that $\int l = ya$. Let $l = ya/d$. [Here he puts d in the denominator. It would mean more to us if he wrote $l = d(ya)$.] Then just as $\int$ will increase, so d will diminish the dimensions. But $\int$ means a sum, and d, a difference. From the given y we can always find y/d or l, that is, the difference of the y's. Hence one equation may be transformed into the other; just as from the equation

$$\overline{\int c \int \overline{l^2}} = \frac{c \int \overline{l^3}}{3a^3},$$

we can obtain the equation

$$c \int \overline{l^2} = \frac{c \int \overline{l^3}}{3a^3 d}."$$

In this early paper Leibniz seems to be exploring the *operations* of $\int$ and d and sees that they are inverses. He finally realizes that $\int$ does not raise dimension nor d lower it, because $\int$ is really a summation of rectangles, and so a sum of areas. Thus he recognizes that, to get back to dy from y, he must form the difference of y's or take the differential of y. Then he says, "But $\int$ means a sum and d a difference." This may have been a later insertion. Hence a couple of weeks afterwards, in order to get from y to dy, he changes from dividing by d to taking the differential of y, and writes dy.

Up to this point Leibniz had been thinking of the y-values as values of terms of a sequence and of x usually as the order of these terms, but now, in this paper, says, "All these theorems are true for series in which the differences of the terms bear to the terms themselves a ratio that is less than any assignable quantity." That is, dy/y may be less than any assignable quantity.

In a manuscript dated November 11, 1675, entitled "Examples of the inverse method of tangents," Leibniz uses $\int$ for the sum and x/d for difference. He then says x/d is dx, the difference of two consecutive x-values, but apparently here dx is a constant and equal to unity.

From barely intelligible arguments such as the above, Leibniz asserted the fact that *integration as a summation process is the inverse of differentiation.* This idea is in the work of Barrow and Newton, who obtained areas by antidifferentiation, but it is first expressed as a relation between summation and differentiation by Leibniz. Despite this outright assertion, he was by no means clear as to how to obtain an area from what one might loosely write as $\sum y\,dx$—that is, how to obtain an area under a curve from a set of rectangles. Of course this difficulty beset all the seventeenth-century workers. Not possessing a clear concept of a limit, or even clear notions about area, Leibniz thought of the latter sometimes as a sum of rectangles so small and so

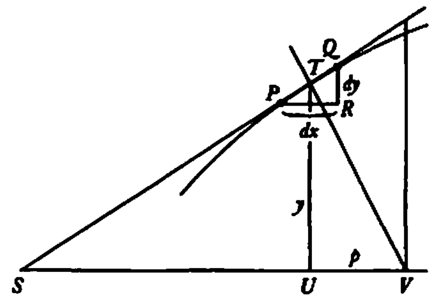

Figure 17.18

numerous that the difference between this sum and the true area under the curve could be neglected, and at other times as a sum of the ordinates or y-values. This latter concept of area was common, especially among the indivisibilists, who thought that the ultimate unit of area and the y-value were the same.

With respect to differentiation, even after recognizing that dy and dx can be arbitrarily small quantities, Leibniz had yet to overcome the fundamental difficulty that the ratio dy/dx is not quite the derivative in our sense. He based his argument on the characteristic triangle, which Pascal and Barrow had also used. This triangle (Fig. 17.18) consists of dy, dx, and the chord PQ, which Leibniz also thought of as *the curve between* P *and* Q *and part of the tangent at* T. Though he speaks of this triangle as indefinitely small, he maintains nevertheless that it is similar to a definite triangle, namely, the triangle STU formed by the subtangent SU, the ordinate at T, and the length of tangent ST. Hence dy and dx are ultimate elements, and their ratio has a definite meaning. In fact, he uses the argument that, from the similar triangles PRQ and SUT, $dy/dx = TU/SU$.

In the manuscript of November 11, 1675, Leibniz shows how he can solve a definite problem. He seeks the curve whose subnormal is inversely proportional to the ordinate. In Figure 17.18, the normal is TV and the subnormal p is UV. From the similarity of triangles PRQ and TUV, he has

$$\frac{dy}{dx} = \frac{p}{y}$$

or

$$p\,dx = y\,dy.$$

But the curve has the given property

$$p = \frac{b}{y},$$

where b is the proportionality constant. Hence

$$dx = \frac{y^2}{b}\,dy.$$

Then

$$\int dx = \int \frac{y^2}{b}\,dy$$

or

$$x = \frac{y^3}{3b}.$$

Leibniz also solved other inverse tangent problems.

In a paper of June 26, 1676, he realizes that the best method of finding tangents is to find dy/dx, where dy and dx are differences and dy/dx is the quotient. He ignores $dx \cdot dx$ and higher powers of dx.

By November of 1676, he is able to give the general rules $dx^n = nx^{n-1}\,dx$ for integral and fractional n and $\int x^n = x^{n+1}/n + 1$, and says, "The reasoning is general, and it does not depend upon what the progressions of the x's may be." Here x still means the order of the terms of a sequence. In this manuscript he also says that to differentiate $\sqrt{a + bz + cz^2}$, let $a + bz + cz^2 = x$, differentiate $\sqrt{x}$, and multiply by dx/dz. This is the chain rule.

By July 11, 1677, Leibniz could give the correct rules for the differential of sum, difference, product, and quotient of two functions and for powers and roots, but no proofs. In the manuscript of November 11, 1675, he had struggled with $d(uv)$ and $d(u/v)$, and thought that $d(uv) = du\,dv$.

In 1680, dx has become the difference of abscissas and dy the differences in the ordinates. He says, "... now these dx and dy are taken to be infinitely small, or the two points on the curve are understood to be a distance apart that is less than any given length...." He calls dy the "momentaneous increment" in y as the ordinate moves along the x-axis. But PQ in Figure 17.18 is still considered part of a straight line. It is "an element of the curve or a side of the infinite-angled polygon that stands for the curve...." He continues to use the usual differential form. Thus, if $y = a^2/x$, then

$$dy = -\frac{a^2}{x^2}\,dx.$$

He also says that differences are the opposite to sums. Then, to get the area under a curve (Fig. 17.19), he takes the sum of the rectangles and says one can neglect the remaining "triangles, since they are infinitely small com-

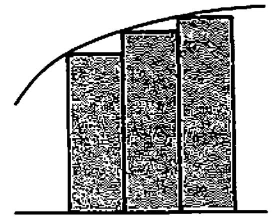

Figure 17.19

pared to the rectangles ... thus I represent in my calculus the area of the figure by $\int y\,dx$...." He also gives, for the element of arc,

$$ds = \sqrt{dx^2 + dy^2};$$

and, for the volume of a solid of revolution obtained by revolving a curve around the x-axis,

$$V = \pi \int y^2\,dx.$$

Despite prior statements that dx and dy are small differences, he still talks about sequences. He says, "Differences and sums are the inverses of one another, that is to say, the sum of the differences of a series [sequence] is a term of the series, and the difference of the sums of a series is a term of the series, and I enumerate the former thus, $\int dx = x$, and the latter thus, $d\int x = dx$." In fact, in a manuscript written after 1684, Leibniz says his method of infinitesimals has become widely known as the calculus of differences.

Leibniz's first publication on the calculus is in the *Acta Eruditorum* of 1684.[12] In this paper the meaning of dy and dx is still not clear. He says in one place, let dx be any arbitrary quantity, and dy is defined by (see Fig. 17.18)

$$dy : dx = y : \text{subtangent}.$$

This definition of dy presumes some expression for the subtangent; hence the definition is not complete. Moreover, Leibniz's definition of tangent as a line joining two infinitely near points is not satisfactory.

He also gives in this paper the rules he had obtained in 1677 for the differential of the sum, product, and quotient of two functions and the rule for finding $d(x^n)$. In this last case he sketches the proof for positive integral n but says the rule is true for all n; for the other rules he gives no proofs. He makes applications to finding tangents, maxima and minima, and points of inflection. This paper, six pages long, is so unclear that the Bernoulli brothers called it "an enigma rather than an explication."[13]

12. *Acta Erud.*, 3, 1684, 467–73 = *Math. Schriften*, 5, 220–26.
13. Leibniz: *Math. Schriften*, 3, Part 1, 5.

In a paper of 1686[14] Leibniz gives

$$y = \sqrt{2x - x^2} + \int \frac{dx}{\sqrt{2x - x^2}}$$

as the equation of the cycloid. His point here is to show that by his methods and notation some curves can be expressed as equations not obtainable in other ways. He reaffirms this in his *Historia* where he says that his *dx*, *ddx* (second difference), and the sums that are the inverses of these differences can be applied to all functions of x, not excepting the mechanical curves of Vieta and Descartes, which Descartes had said have no equations. Leibniz also says that he can include curves that Newton could not handle even with his method of series.

In the 1686 paper as well as in subsequent papers,[15] Leibniz gave the differentials of the logarithmic and exponential functions and recognized exponential functions as a class. He also treated curvature, the osculating circle, and the theory of envelopes (see Chap. 23). In a letter to John Bernoulli of 1697, he differentiated under the integral sign with respect to a parameter. He also had the idea that many indefinite integrals could be evaluated by reducing them to known forms and speaks of preparing tables for such reductions—in other words, a table of integrals. He tried to define the higher-order differentials such as *ddy* (d^2y) and *dddy* (d^3y), but the definitions were not satisfactory. Though he did not succeed, he also tried to find a meaning for $d^\alpha y$ where α is any real number.

With respect to notation, Leibniz worked painstakingly to achieve the best. His *dx*, *dy*, and *dy*/*dx* are, of course, still standard. He introduced the notation log x, d^n for the nth differential, and even d^{-1} and d^{-n} for $\int$ and the nth iteration of summation, respectively.

In general Leibniz's work, though richly suggestive and profound, was so incomplete and fragmentary that it was barely intelligible. Fortunately, the Bernoulli brothers, James and John, who were immensely impressed and stirred by Leibniz's ideas, elaborated his sketchy papers and contributed an immense number of new developments we shall discuss later. Leibniz agreed that the calculus was as much theirs as his.

5. *A Comparison of the Work of Newton and Leibniz*

Both Newton and Leibniz must be credited with seeing the calculus as a new and general method, applicable to many types of functions. After their work, the calculus was no longer an appendage and extension of Greek geometry, but an independent science capable of handling a vastly expanded range of problems.

14. *Acta Erud.*, 5, 1686, 292–300 = *Math. Schriften*, 5, 226–33.
15. *Acta Erud.*, 1692, 168–71 = *Math. Schriften*, 5, 266–69; *Acta Erud.*, 1694 = *Math. Schriften*, 5, 301–6.

Both also arithmetized the calculus; that is, they built on algebraic concepts. The algebraic notation and techniques used by Newton and Leibniz not only gave them a more effective tool than geometry, but also permitted many different geometric and physical problems to be treated by the same technique. A major change from the beginning to the end of the seventeenth century was the algebraicization of the calculus. This is comparable to what Vieta had done in the theory of equations and Descartes and Fermat in geometry.

The third vital contribution that Newton and Leibniz share is the reduction to antidifferentiation of area, volume, and other problems that were previously treated as summations. Thus the four main problems—rates, tangents, maxima and minima, and summation—were all reduced to differentiation and antidifferentiation.

The chief distinction between the work of the two men is that Newton used the infinitely small increments in x and y as a means of determining the fluxion or derivative. It was essentially the limit of the ratio of the increments as they became smaller and smaller. On the other hand, Leibniz dealt directly with the infinitely small increments in x and y, that is, with differentials, and determined the relationship between them. This difference reflects Newton's physical orientation, in which a concept such as velocity is central, and Leibniz's philosophical concern with ultimate particles of matter, which he called monads. As a consequence, Newton solved area and volume problems by thinking entirely in terms of rate of change. For him differentiation was basic; this process and its inverse solved all calculus problems, and in fact the use of summation to obtain an area, volume, or center of gravity rarely appears in his work. Leibniz, on the other hand, thought first in terms of summation, though of course these sums were evaluated by antidifferentiation.

A third distinction between the work of the two men lies in Newton's free use of series to represent functions; Leibniz preferred the closed form. In a letter to Leibniz of 1676, Newton stressed the use of series even to solve simple differential equations. Though Leibniz did use infinite series, he replied that the real goal should be to obtain results in finite terms, using the trigonometric and logarithmic functions where algebraic functions would not serve. He recalled to Newton James Gregory's assertion that the rectification of the ellipse and hyperbola could not be reduced to the circular and logarithmic functions and challenged Newton to determine by the use of series whether Gregory was correct. Newton replied that by the use of series he could decide whether some integrations could be achieved in finite terms, but gave no criteria. Again, in a letter of 1712 to John Bernoulli, Leibniz objected to the expansion of functions into series and stated that the calculus should be concerned with reducing its results to quadratures (integrations) and, where necessary, quadratures involving transcendental functions.

There are differences in their manner of working. Newton was empirical, concrete, and circumspect, whereas Leibniz was speculative, given to generalizations, and bold. Leibniz was more concerned with operational formulas to produce a calculus in the broad sense; for example, rules for the differential of a product or quotient of functions, his rule for $d^n(uv)$ (u and v being functions of x), and a table of integrals. It was Leibniz who set the canons of the calculus, the system of rules and formulas. Newton did not bother to formulate rules, even when he could easily have generalized his concrete results. He knew that if $z = uv$, then $\dot{z} = u\dot{v} + v\dot{u}$, but did not point out this general result. Though Newton initiated many methods, he did not stress them. His magnificent applications of the calculus not only demonstrated its value but, far more than Leibniz's work, stimulated and determined almost the entire direction of eighteenth-century analysis. Newton and Leibniz differed also in their concern for notation. Newton attached no importance to this matter, while Leibniz spent days choosing a suggestive notation.

6. *The Controversy over Priority*

Nothing of Newton's work on the calculus was published before 1687, though he had communicated results to friends during the years 1665 to 1687. In particular, he had sent his tract *De Analysi* in 1669 to Barrow, who had sent it to John Collins. Leibniz visited Paris in 1672 and London in 1673 and communicated with some of the people who knew Newton's work. However, he did not publish on the calculus until 1684. Hence the question of whether Leibniz had known the details of what Newton did was raised, and Leibniz was accused of plagiarism. However, investigations made long after the deaths of the two men show that Leibniz was an independent inventor of major ideas of the calculus, though Newton did much of his work before Leibniz did. Both owe much to Barrow, though Barrow used geometrical methods almost exclusively. The significance of the controversy lies not in the question of who was the victor but rather in the fact that the mathematicians took sides. The Continental mathematicians, the Bernoulli brothers in particular, sided with Leibniz, while the English mathematicians defended Newton. The two groups became unfriendly and even bitter toward each other; John Bernoulli went so far as to ridicule and inveigh against the English.

As a result, the English and Continental mathematicians ceased exchanging ideas. Because Newton's major work and first publication on the calculus, the *Principia*, used geometrical methods, the English continued to use mainly geometry for about a hundred years after his death. The Continentals took up Leibniz's analytical methods and extended and improved them. These proved to be far more effective; so not only did the English mathematicians fall behind, but mathematics was deprived of contributions that some of the ablest minds might have made.

7. *Some Immediate Additions to the Calculus*

The calculus is of course the beginning of that most weighty part of mathematics generally referred to as analysis. We shall be following the important developments of this field in succeeding chapters; we might note here, however, some additions that were made immediately after the basic work of Newton and Leibniz.

In his *Arithmetica Universalis* (1707) Newton established a theorem on the upper bound to the real roots of polynomial equations. The theorem says: A number a is an upper bound of the real roots of $f(x) = 0$ if, when a is substituted for x, it gives to $f(x)$ and to all its derivatives the same sign.

In his *De Analysi* and *Method of Fluxions*, he gave a general method of approximating the roots of $f(x) = 0$, which was published in Wallis's *Algebra* of 1685. In his tract *Analysis Aequationum Universalis* (1690), Joseph Raphson (1648–1715) improved on this method; though he applied it only to polynomials, it is much more broadly useful. It is this modification that is now known as Newton's method or the Newton-Raphson method. It consists in first choosing an approximation a. Then calculate $a - f(a)/f'(a)$. Call this b, and calculate $b - f(b)/f'(b)$. Call this last result c, and so forth. The numbers $a, b, c, \ldots$ are successive approximations to the root. (The notation is modern.) Actually the method does not necessarily give better and better approximations to the root. J. Raymond Mourraille showed in 1768 that a must be chosen so that the curve of $y = f(x)$ is convex toward the axis of x in the interval between a and the root. Much later Fourier discovered this fact independently.

In his *Démonstration d'une méthode pour résoudre les égalitéz de tous les dégrez* (16191), Michel Rolle (1652–1719) gave the famous theorem now named after him, namely, that if a function is 0 at two values of x, say, a and b, then the derivative is 0 at some value of x between a and b. Rolle stated the theorem but did not prove it.

After Newton and Leibniz the two most important founders of the calculus were the Bernoulli brothers, James and John. James (= Jakob = Jacques) Bernoulli (1655–1705) was self-taught in mathematics and so matured slowly in that subject. At the urging of his father he studied for the ministry, but eventually turned to mathematics, and in 1686 became a professor at the University of Basle. His chief interests thereafter were mathematics and astronomy. When, in the late 1670s, he began to work on mathematical problems, Newton's and Leibniz's work was still unknown to him. He too learned from Descartes's *La Géométrie*, Wallis's *Arithmetica Infinitorum* and Barrow's *Geometrical Lectures*. Though he took much from

Barrow, he put it into analytical form. He gradually became familiar with Leibniz's work, but because so little of the latter appeared in print, much of what James did overlapped Leibniz's results. Actually he, like the other mathematicians of the time, did not fully understand Leibniz's work.

James's activity is closely linked with that of his younger brother John (= Johann = Jean, 1667–1748). John was sent into business by his father but turned to medicine, while learning mathematics from his brother. He became a professor of mathematics at Groningen in Holland and then succeeded his brother at Basle.

Both James and John corresponded constantly with Leibniz, Huygens, other mathematicians, and each other. All these men worked on many common problems suggested in letters or posed as challenges. Since results, too, were in those days often communicated in letters with or without subsequent publication, the matter of priority is complicated. Sometimes credit was claimed for a result that was announced even though no proof was given at that time. The question is further complicated by the peculiar relationships that developed. John was extremely anxious to secure fame and began to compete with his brother; soon each was challenging the other on problems. John did not hesitate to use unscrupulous means to appear to be the discoverer of results he got from others, including his brother. James was very sensitive and reacted in kind. Each published papers that owed much to the other without acknowledging the origins of their ideas. John actually became a vitriolic critic of his brother, and Leibniz tried to mediate between the two. Though James had said earlier, while praising Barrow, that Leibniz's work should not be depreciated, he became more and more distrustful of Leibniz. Moreover, he resented Leibniz's superior insights and thought Leibniz was arrogant in pointing out that he had done things James thought were original with himself. He became convinced that Leibniz sought only to belittle his work and was favoring John in the disputes between the brothers. When Nicholas Fatio de Duillier (1664–1753) gave Newton credit for creating the calculus and became embroiled in controversy with Leibniz, James wrote letters to Fatio opposing Leibniz.

As to the Bernoullis' work in the calculus, they, too, tackled problems such as finding the curvature of curves, evolutes (envelopes of the normals to a curve), inflection points, the rectification of curves, and other basic calculus topics. The results of Newton and Leibniz were extended to spirals of various sorts, the catenary, and the tractrix, which was defined as the curve (Fig. 17.20) for which the ratio PT to OT is a constant. James also wrote five major papers on series (Chap. 20, sec. 4), which extended Newton's use of series to integrate complicated algebraic functions and transcendental functions. In 1691 both James and John gave the formula for the radius of curvature of a curve. James called it his "golden theorem" and wrote it as

$$z = dx\, ds : ddy = dy\, ds : ddx$$

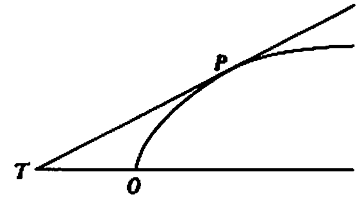

Figure 17.20

where z is the radius of curvature. If we divide numerator and denominator of each ratio by ds^2 we get

$$z = \frac{dx/ds}{d^2y/ds^2} = \frac{dy/ds}{d^2x/ds^2},$$

which are more familiar forms. James also gave the result in polar coordinates.

John produced a now-famous theorem for obtaining the limit approached by a fraction whose numerator and denominator approach 0. This theorem was incorporated by Guillaume F. A. l'Hospital (1661–1704), a pupil of John, in an influential book on the calculus, the *Analyse des infiniment petits* (1696), and is now known as L'Hospital's rule.

8. *The Soundness of the Calculus*

From the very introduction of the new methods of finding rates, tangents, maxima and minima, and so forth, the proofs were attacked as unsound. Cavalieri's use of indivisible ultimate elements and his arguments shocked those who still respected logical rigor. To their criticism Cavalieri responded that the contemporary geometers had been freer with logic than he—for example, Kepler, in his *Stereometria Doliorum*. These geometers, he continued, had been content in their calculation of areas to imitate Archimedes' method of summing lines, but had failed to give the complete proofs that the great Greek had used to make his work rigorous. They were satisfied with their calculations, provided only that the results were useful. Cavalieri felt justified in adopting the same point of view. He said that his procedures could lead to new inventions and that his method did not at all oblige one to consider a geometrical structure as composed of an infinite number of sections; it had no other object than to establish correct ratios between areas or volumes. But these ratios preserved their sense and value whatever opinion one might have about the composition of a continuum. In any case, said Cavalieri, "rigor is the concern of philosophy and not of geometry."

Fermat, Pascal, and Barrow recognized the looseness of their work on summation but believed that one could make precise proofs in the manner of Archimedes. Pascal, in *Letters of Dettonville* (1659), affirmed that the infinitesimal geometry and classical Greek geometry were in agreement. He concluded, "What is demonstrated by the true rules of indivisibles could be demonstrated also with the rigor and the manner of the ancients." Further, he said the method of indivisibles must be accepted by any mathematicians who pretend to rank among geometers. It differs only in language from the method of the ancients. Nevertheless, Pascal, too, had ambivalent feelings about rigor. At times he argued that the heart intervenes to assure us of the correctness of mathematical steps. The proper "finesse," rather than geometrical logic, is what is needed to do the correct work, just as the religious appreciation of grace is above reason. The paradoxes of geometry as used in the calculus are like the apparent absurdities of Christianity; and the indivisible in geometry has the same relation to the finite as man's justice has to God's.

The defenses Cavalieri and Pascal offered applied to the summation of infinitely small quantities. As to the derivative, early workers such as Fermat and Roberval thought they had a simple algebraic process that had a very clear geometric interpretation and so could be justified by geometrical arguments. Actually Fermat was careful not to assert general theorems when he advanced any idea he could not justify by the method of exhaustion. Barrow argued only geometrically and, despite his attacks on the algebraists for their lack of rigor, was less scrupulous about the soundness of his geometrical arguments.

Neither Newton nor Leibniz clearly understood nor rigorously defined his fundamental concepts. We have already observed that both vacillated in their definitions of the derivative and differentials. Newton did not really believe that he had departed from Greek geometry. Though he used algebra and coordinate geometry, which were not to his taste, he thought his underlying methods were but natural extensions of pure geometry. Leibniz, however, was a man of vision who thought in broad terms, like Descartes. He saw the long-term implications of the new ideas and did not hesitate to declare that a new science was coming to light. Hence he was not too concerned about the lack of rigor in the calculus.

In response to criticism of his ideas, Leibniz made various, unsatisfactory replies. In a letter to Wallis of March 30, 1690[16] he said:

> It is useful to consider quantities infinitely small such that when their ratio is sought, they may not be considered zero but which are rejected as often as they occur with quantities incomparably greater. Thus if we have $x + dx$, dx is rejected. But it is different if we seek the difference between $x + dx$ and x. Similarly we cannot have $x\,dx$ and $dx\,dx$ standing together. Hence if we are to differentiate xy we write $(x + dx)(y + dy) - xy = x\,dy + y\,dx + dx\,dy$. But here $dx\,dy$ is to be rejected as incomparably less than $x\,dy + y\,dx$. Thus in any particular case, the error is less than any finite quantity.

16. Leibniz: *Math. Schriften*, 4, 63.

As to the ultimate meanings of dy, dx and dy/dx, Leibniz remained vague. He spoke of dx as the difference in x values between two infinitely near points and of the tangent as the line joining such points. He dropped differentials of higher order with no justification, though he did distinguish among the various orders. The infinitely small dx and dy were sometimes described as vanishing or incipient quantities, as opposed to quantities already formed. These indefinitely small quantities were not zero, but were smaller than any finite quantity. Alternatively he appealed to geometry to say that a higher differential is to a lower one as a point is to a line[17] or that dx is to x as a point to the earth or as the radius of the earth to that of the heavens. The ratio of two infinitesimals he thought of as a quotient of inassignables or of indefinitely small quantities, but one which could nevertheless be expressed in terms of definite quantities such as the ratio of ordinate to subtangent.

A flurry of attacks and rebuttals was initiated in books of 1694 and 1695 by the Dutch physician and geometer Bernard Nieuwentijdt (1654–1718). Although he admitted that in general the new methods led to correct results, he criticized the obscurity and pointed out that sometimes the methods led to absurdities. He complained that he could not understand how the infinitely small quantities differed from zero and asked how a sum of infinitesimals could be finite. He also challenged the meaning and existence of differentials of higher order and the rejection of infinitely small quantities in portions of the arguments.

Leibniz, in a draft of a reply to Nieuwentijdt, probably written in 1695, and in an article in the *Acta Eruditorum* of 1695,[18] gives various answers. He speaks of "overprecise" critics and says that excessive scrupulousness should not cause us to reject the fruits of invention. He then says his method differs from Archimedes' only in the expressions used, but that his own are better adapted to the art of discovery. The words "infinite" and "infinitesimal" signify merely quantities that one can take as large or as small as one wishes in order to show that the error incurred is less than any number that can be assigned—in other words, that there is no error. One can use these ultimate things—that is, infinite and infinitely small quantities—as a tool, much as algebraists used imaginary roots with great profit.

Leibniz's argument thus far was that his calculus used only ordinary mathematical concepts. But since he could not satisfy his critics, he enunciated a philosophical principle known as the law of continuity, which was practically the same as one already stated by Kepler. In 1687, in a letter to Pierre Bayle,[19] Leibniz expressed this principle as follows: "In any supposed transition, ending in any terminus, it is permissible to institute a general reasoning, in which the final terminus may also be included." To support

17. *Math. Schriften*, 5, 322 ff.
18. *Acta Erud.*, 1695, 310–16 = *Math. Schriften*, 5, 320–28.
19. *Math. Schriften*, 5, 385.

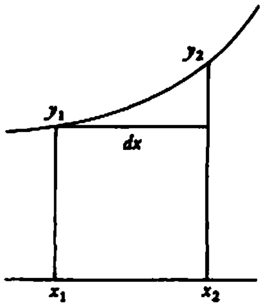

Figure 17.21

this principle he gives, in an unpublished manuscript of about 1695, the example of including under one argument ellipses and parabolas, though the parabola is a limiting case of the ellipse when one focus moves off to infinity. He then applies the principle to the calculation of $dy{:}dx$ for the parabola $y = x^2/a$. After obtaining

$$dy{:}dx = (2x + dx){:}a,$$

he says, "Now, since by our postulate it is permissible to include under the one general reasoning the case also in which [Fig. 17.21] the ordinate x_2y_2 is moved up nearer and nearer to the fixed ordinate x_1y_1 until it ultimately coincides with it, it is evident that in this case dx becomes equal to 0 and should be neglected...." Leibniz does not say what meaning should be given to the dx that appears at the left side of the equation.

Of course, he says, things that are absolutely equal have a difference that is absolutely nothing; therefore a parabola is not an ellipse.

> Yet a state of transition may be imagined, or one of evanescence, in which indeed there has not yet arisen exact equality or rest.... but in which it is passing into such a state that the difference is less than any assignable quantity; also that in this state there will still remain some difference, some velocity, some angle, but in each case one that is infinitely small....
>
> For the present, whether such a state of instantaneous transition from inequality to equality ... can be sustained in a rigorous or metaphysical sense, or whether infinite extensions successively greater and greater, or infinitely small ones successively less and less, are legitimate considerations, is a matter that I own to be possibly open to question....
>
> It will be sufficient if, when we speak of infinitely great (or, more strictly, unlimited) or of infinitely small quantities (i.e., the very least of those within our knowledge), it is understood that we mean quantities that are indefinitely great or indefinitely small, i.e., as great as you please, or as small as you please, so that the error that any one may assign may be less than a certain assigned quantity.

> On these suppositions, all the rules of our algorithm, as set out in the *Acta Eruditorum* for October 1684, can be proved without much trouble.

Leibniz then goes over these rules. He introduces the quantities $(d)y$ and $(d)x$ and carries out the usual processes of differentiation with them. These he calls assignable or definite nonvanishing quantities. After obtaining the final result, he says, we can replace $(d)y$ and $(d)x$ by the evanescent or unassignable quantities dy and dx, by making "the supposition that the ratio of the evanescent quantities dy and dx is equal to the ratio of $(d)y$ and $(d)x$, because this supposition can always be reduced to an undoubtable truth."

Leibniz's principle of continuity is certainly not a mathematical axiom today, but he emphasized it and it became important later. He gave many arguments that are in accordance with this principle. For example, in a letter to Wallis,[20] Leibniz defended his use of the characteristic triangle as a form without magnitude, the form remaining after the magnitudes had been reduced to zero, and challengingly asked, "Who does not admit a form without magnitude?" Likewise, in a letter to Guido Grandi,[21] he said the infinitely small is not a simple and absolute zero but a relative zero, that is, an evanescent quantity which yet retains the character of that which is disappearing. However, Leibniz also said, at other times, that he did not believe in magnitudes truly infinite or truly infinitesimal.

Leibniz, less concerned with the ultimate justification of his procedures than Newton, felt that it lay in their effectiveness. He stressed the procedural or algorithmic value of what he had created. Somehow he had confidence that if he formulated clearly the rules of operation and these were properly applied, reasonable and correct results would be obtained, however doubtful might be the meanings of the symbols involved.

It is apparent that neither Newton nor Leibniz succeeded in making clear, let alone precise, the basic concepts of the calculus: the derivative and the integral. Not being able to grasp these properly, they relied upon the coherence of the results and the fecundity of the methods to push ahead without rigor.

Several examples may illustrate the lack of clarity even among the great immediate successors of Newton and Leibniz. John Bernoulli wrote the first text on the calculus in 1691 and 1692. The portion on the integral calculus was published in 1742;[22] the part on the differential calculus, *Die Differentialrechnung*, was not published until 1924. However, the Marquis de l'Hospital did publish a slightly altered French version (already referred to) under his own name in 1696. Bernoulli begins the *Differentialrechnung* with three postulates. The first reads: "A quantity which is diminished or increased by an

20. *Math. Schriften*, 4, 54.
21. *Math. Schriften*, 4, 218.
22. *Opera Omnia*, 3, 385–558.

infinitely small quantity is neither increased nor decreased." His second postulate is: "Each curved line consists of infinitely many straight lines, these themselves being infinitely small." In his reasoning he followed Leibniz and used infinitesimals. Thus to obtain dy from $y = x^2$, he uses e for dx and gets $(x + e)^2 - x^2$, or $2xe + e^2$, and then just drops e^2. Like Leibniz, he used vague analogies to explain what differentials were. Thus, he says, the infinitely large quantities are like astronomical distances and the infinitely small are like animalcules revealed by the microscope. In 1698 he argued that infinitesimals must exist.[23] One has only to consider the infinite series 1, 1/2, 1/4, ... If one takes 10 terms, then 1/10 exists; if one takes 100 terms, then 1/100 exists. Corresponding to the infinite number of terms there is the infinitesimal.

A few men, Wallis and John Bernoulli among them, tried to define the infinitesimal as the reciprocal of ∞, the latter being a definite number to them. Still others acted as though what was incomprehensible needed no further explanation. For most of the seventeenth-century men, rigor was not a matter of concern. What they often said could be rigorized by the method of Archimedes could actually not have been rigorized by an Archimedes; this is particularly true of the work on differentiation, which had no parallel in Greek mathematics.

Actually the new calculus was introducing concepts and methods that inaugurated a radical departure from earlier work. With the work of Newton and Leibniz, the calculus became a totally new discipline that required foundations of its own. Though they were not fully aware of it, the mathematicians had turned their backs on the past.

Germs of the correct new concepts can be found even in the seventeenth-century literature. Wallis, in the *Arithmetica Infinitorum*, advanced the arithmetical concept of the limit of a function as a number approached by the function so that the difference between this number and the function could be made less than any assignable quantity and would vanish ultimately when the process was continued to infinity. His wording is loose but contains the right idea.

James Gregory in his *Vera Circuli et Hyperbolae Quadratura* (1667) explicitly pointed out that the methods used to obtain areas, volumes, and lengths of curves involved a new process, the limit process. Moreover, he added, this operation was distinct from the five algebraic operations of addition, subtraction, multiplication, division, and extraction of roots. He put the method of exhaustion into algebraic form and recognized that the successive approximations obtained by using rectilinear figures circumscribed about a given area or volume and those obtained by using inscribed rectilinear figures both converged to the same "last term." He also noted that

23. Leibniz: *Math. Schriften*, 3, Part 2, 563 ff.

this limit process yields irrationals not obtainable as roots of rationals. But these insights of Wallis and Gregory were ignored in their century.

The foundations of the calculus remained unclear. Adding to the confusion was the fact that the proponents of Newton's work continued to speak of prime and ultimate ratios, while the followers of Leibniz used the infinitely small non-zero quantities. Many of the English mathematicians, perhaps because they were in the main still tied to the rigor of Greek geometry, distrusted all the work on the calculus. Thus the century ended with the calculus in a muddled state.

Bibliography

Armitage, A.: *Edmond Halley*, Thomas Nelson and Sons, 1966.

Auger, L.: *Un Savant méconnu: Gilles Persone de Roberval (1602–1675)*, A. Blanchard, 1962.

Ball, W. W. R.: *A Short Account of the History of Mathematics*, Dover (reprint), 1960, pp. 309–70.

Baron, Margaret E.: *The Origins of the Infinitesimal Calculus*, Pergamon Press, 1969.

Bell, Arthur E.: *Newtonian Science*, Edward Arnold, 1961.

Boyer, Carl B.: *The Concepts of the Calculus*, Dover (reprint), 1949.

———: *A History of Mathematics*, John Wiley and Sons, 1968, Chaps. 18–19.

Brewster, David: *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton*, 2 vols., 1855, Johnson Reprint Corp., 1965.

Cajori, Florian: *A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse*, Open Court, 1919.

———: *A History of Mathematics*, Macmillan, 1919, 2nd ed., pp. 181–220.

Cantor, Moritz: *Vorlesungen über Geschichte der Mathematik*, 2nd ed., B. G. Teubner, 1900 and 1898, Vol. 2, pp. 821–922; Vol. 3, pp. 150–316.

Child, J. M.: *The Geometrical Lectures of Isaac Barrow*, Open Court, 1916.

———: *The Early Mathematical Manuscripts of Leibniz*, Open Court, 1920.

Cohen, I. B.: *Isaac Newton's Papers and Letters on Natural Philosophy*, Harvard University Press, 1958.

Coolidge, Julian L.: *The Mathematics of Great Amateurs*, Dover (reprint), 1963, Chaps. 7, 11, and 12.

De Morgan, Augustus: *Essays on the Life and Work of Newton*, Open Court, 1914.

Fermat, Pierre de: *Œuvres*, Gauthier-Villars, 1891–1912, Vol. 1, pp. 133–79, Vol. 3, pp. 121–56.

Gibson, G. A.: "James Gregory's Mathematical Work," *Proc. Edinburgh Math. Soc.*, 41, 1922/23, 2–25.

Huygens, C.: *Œuvres complètes*, 22 vols., Société Hollandaise des Sciences, Nyhoff, 1888–1950.

Leibniz, G. W.: *Œuvres*, Firmin-Didot, 1859–75.

———: *Mathematische Schriften*, ed. C. I. Gerhardt, 7 vols., Ascher-Schmidt, 1849–63. Reprinted by Georg Olms, 1962.

More, Louis T.: *Isaac Newton*, Dover (reprint), 1962.

Montucla, J. F.: *Histoire des mathématiques*, Albert Blanchard (reprint), 1960, Vol. 2, pp. 102–77, 348–403; Vol. 3, pp. 102–38.

Newton, Sir Isaac: *The Mathematical Works*, ed. D. T. Whiteside, 2 vols., Johnson Reprint Corp., 1964–67. Vol. 1 contains translations of the three basic papers on the calculus.

———: *Mathematical Papers*, ed. D. T. Whiteside, 4 vols., Cambridge University Press, 1967–71.

———: *Mathematical Principles of Natural Philosophy*, ed. Florian Cajori, 3rd ed., University of California Press, 1946.

———: *Opticks*, Dover (reprint), 1952.

Pascal, B.: *Œuvres*, Hachette, 1914–21.

Scott, Joseph F.: *The Mathematical Work of John Wallis*, Oxford University Press, 1938.

———: *A History of Mathematics*, Taylor and Francis, 1958, Chaps. 10–11.

Smith, D. E.: *A Source Book in Mathematics*, Dover (reprint), 1959, pp. 605–26.

Struik, D. J.: *A Source Book in Mathematics, 1200–1800*, Harvard University Press, 1969, pp. 188–316, 324–28.

Thayer, H. S.: *Newton's Philosophy of Nature*, Hafner, 1953.

Turnbull, H. W.: *The Mathematical Discoveries of Newton*, Blackie and Son, 1945.

———: *James Gregory Tercentenary Memorial Volume*, Royal Society of Edinburgh, 1939.

Turnbull, H. W. and J. F. Scott: *The Correspondence of Isaac Newton*, 4 vols., Cambridge University Press, 1959–1967.

Walker, Evelyn: *A Study of the* Traité des indivisibles *of Gilles Persone de Roberval*, Columbia University Press, 1932.

Wallis, John: *Opera Mathematica*, 3 vols., 1693–99, Georg Olms (reprint), 1968.

Whiteside, Derek T.: "Patterns of Mathematical Thought in the Seventeenth Century," *Archive for History of Exact Sciences*, 1, 1961, pp. 179–388.

Wolf, Abraham: *A History of Science, Technology and Philosophy in the 16th and 17th Centuries*, 2nd ed., George Allen and Unwin, 1950, Chaps. 7–14.

Abbreviations

Journals whose titles have been written out in full in the text are not listed here.

Abh. der Bayer. Akad. der Wiss. Abhandlungen der Königlich Bayerischen Akademie der Wissenschaften (München)
Abh. der Ges. der Wiss. zu Gött. Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen
Abh. König. Akad. der Wiss., Berlin Abhandlungen der Königlich Preussischen Akademie der Wissenschaften zu Berlin
Abh. Königlich Böhm. Ges. der Wiss. Abhandlungen der Königlichen Böhmischen Gesellschaft der Wissenschaften
Abh. Math. Seminar der Hamburger Univ. Abhandlungen aus dem Mathematischen Seminar Hamburgischen Universität
Acta Acad. Sci. Petrop. Acta Academiae Scientiarum Petropolitanae
Acta Erud. Acta Eruditorum
Acta Math. Acta Mathematica
Acta Soc. Fennicae Acta Societatis Scientiarum Fennicae
Amer. Jour. of Math. American Journal of Mathematics
Amer. Math. Monthly American Mathematical Monthly
Amer. Math. Soc. Bull. American Mathematical Society, Bulletin
Amer. Math. Soc. Trans. American Mathematical Society, Transactions
Ann. de l'Ecole Norm. Sup. Annales Scientifiques de l'Ecole Normale Supérieure
Ann. de Math. Annales de Mathématiques Pures et Appliquées
Ann. Fac. Sci. de Toulouse Annales de la Faculté des Sciences de Toulouse
Ann. Soc. Sci. Bruxelles Annales de la Société Scientifique de Bruxelles
Annali di Mat. Annali di Matematica Pura ed Applicata
Annals of Math. Annals of Mathematics
Astronom. Nach. Astronomische Nachrichten
Atti Accad. Torino Atti della Reale Accademia delle Scienze di Torino
Atti della Accad. dei Lincei, Rendiconti Atti della Reale Accademia dei Lincei, Rendiconti
Brit. Assn. for Adv. of Sci. British Association for the Advancement of Science
Bull. des Sci. Math. Bulletin des Sciences Mathématiques
Bull. Soc. Math. de France Bulletin de la Société Mathématique de France
Cambridge and Dublin Math. Jour. Cambridge and Dublin Mathematical Journal
Comm. Acad. Sci. Petrop. Commentarii Academiae Scientiarum Petropolitanae
Comm. Soc. Gott. Commentationes Societatis Regiae Scientiarum Gottingensis Recentiores
Comp. Rend. Comptes Rendus
Corresp. sur l'Ecole Poly. Correspondance sur l'Ecole Polytechnique
Encyk. der Math. Wiss. Encyklopädie der Mathematischen Wissenschaften
Gior. di Mat. Giornale di Matematiche
Hist. de l'Acad. de Berlin Histoire de l'Académie Royale des Sciences et des Belles-Lettres de Berlin