III.

"HISTORIA ET ORIGO CALCULI DIFFERENTIALIS."

§ 2.

HISTORY AND ORIGIN OF THE DIFFERENTIAL CALCULUS.

It is an extremely useful thing to have knowledge of the true origins of memorable discoveries, especially those that have been found not by accident but by dint of meditation. It is not so much that thereby history may attribute to each man his own discoveries and that others should be encouraged to earn like commendation, as that the art of making discoveries should be extended by considering noteworthy examples of it.

Among the most renowned discoveries of the times must be considered that of a new kind of mathematical analysis, known by the name of the differential calculus; and of this, even if the essentials are at the present time considered to be sufficiently demonstrated, nevertheless the origin and the method of the discovery are not yet known to the world at large. Its author invented it nearly forty years ago, and nine years later (nearly thirty years ago) published it in a concise form; and from that time it has not only been frequently made known in memoirs,[42] but also has been a method of general employment; while many splendid discoveries have been made by its assistance, such as have been included in the *Acta Eruditorum*, Leipsic, and also such as have been published in the memoirs of the Royal Academy of Sciences; so that it would seem that a new aspect has been given to mathematical knowledge arising out of its discovery.

Now there never existed any uncertainty as to the name of the true inventor, until recently, in 1712, certain upstarts, either

[42] It is possible that this may mean "has received high commendation"; for *elogiis* may be the equivalent of eulogy, in which case *celebratus est* must be translated as "has been renowned."

in ignorance of the literature of the times gone by, or through envy, or with some slight hope of gaining notoriety by the discussion, or lastly from obsequious flattery, have set up a rival to him; and by their praise of this rival, the author has suffered no small disparagement in the matter, for the former has been credited with having known far more than is to be found in the subject under discussion. Moreover, in this they acted with considerable shrewdness, in that they put off starting the dispute until those who knew the circumstances, Huygens, Wallis, Tschirnhaus, and others, on whose testimony they could have been refuted, were all dead.[43] Indeed this is one good reason why contemporary prescripts should be introduced as a matter of law; for without any fault or deceit on the part of the responsible party, attacks may be deferred until the evidence with which he might be able to safeguard himself against his opponent had ceased to exist. Moreover, they have changed the whole point of the issue, for in their screed, in which under the title of *Commercium Epistolicum D. Johannis Collinsii* (1712) they have set forth their opinion in such a manner as to give a dubious credit to Leibniz, they have said very little about the calculus; instead, every other page is made up of what they call infinite series. Such things were first given as discoveries by Nicolaus Mercator[44] of Holstein, who obtained them by the process

[43] This is untrue. As has been said, the attack was first made publicly in 1699; at this time, although Huygens had indeed been dead for four years, Tschirnhaus was still alive, and Wallis was appealed to by Leibniz. It is strange that Leibniz did not also appeal to Tschirnhaus, through whom it is suggested by Weissenborn that Leibniz may have had information of Newton's discoveries. Perhaps this is the reason why he did not do so, since Tschirnhaus might not have turned out to be a suitable witness for the defense. Leibniz must have had this attack by Fatio in his mind, for he could hardly have referred to Keill as a *novus homo,* while we know that he did not think much of Fatio as a mathematician. To say that there never existed any uncertainty as to the name of the true inventor until 1712 is therefore sheer nonsense; for if by that he means to dismiss with contempt the attack of Fatio, whom can he mean by the phrase *novus homo*? The sneering allusion to "the hope of gaining notoriety by the discussion" can hardly allude to any one but Fatio. Finally if Fatio is dismissed as contemptible, the second attack by Keill was made in 1708. If it was early in the year, Tschirnhaus was even then alive, though Wallis was dead.

[44] Gerhardt says in a note (G. 1846, p. 22) that his real name was probably Kramer; for what reason I am unable to gather. Cantor says distinctly that his name was Kaufmann, and this is the usually accepted name of the man who was one of the first members of the Royal Society and contributed to its *Transactions*. It seems to me that Gerhardt is guessing; the German word *Kramer* means a small shopkeeper, while *Kaufmann* means a merchant. To Mercator is due the logarithmic series obtained by dividing unity by $(1+x)$ and integrating the resulting series term by term; the connection with the logarithm of $(1+x)$ is through the area of the rectangular hyperbola $y(1+x)=0$. See Reiff, *Geschichte der unendlichen Reihen*.

of division, and Newton gave the more general form by extraction of roots.[45] This is certainly a useful discovery, for by it arithmetical approximations are reduced to an analytical reckoning; but it has nothing at all to do with the differential calculus. Moreover, even in this they make use of fallacious reasoning; for whenever this rival works out a quadrature by the addition of the parts by which a figure is gradually increased,[46] at once they hail it as the use of the differential calculus (as for instance on page 15 of the *Commercium*). By the selfsame argument, Kepler (in his *Stereometria Doliorum*),[47] Cavalieri, Fermat, Huygens, and Wallis used

[45] Newton obtained the general form of the binomial expansion after the method of Wallis, i. e., by interpolation. See Reiff.

[46] We now see what was Leibniz's point; the differential calculus was not the employment of an infinitesimal and a summation of such quantities; it was the use of the idea of these infinitesimals being differences, and the employment of the notation invented by himself, the rules that governed the notation, and the fact that differentiation was the inverse of a summation; and perhaps the greatest point of all was that the work had not to be referred to a diagram. This is on an inestimably higher plane than the mere differentiation of an algebraic expression whose terms are simple powers and roots of the independent variable.

[47] Why is Barrow omitted from this list? As I have suggested in the case of Barrow's omission of all mention of Fermat, was Leibniz afraid to awake afresh the sleeping suggestion as to his indebtedness to Barrow? I have suggested that Leibniz read his Barrow on his journey back from London, and perhaps, tiring at having read the Optics first and then the preliminary five lectures, just glanced at the remainder and missed the main important theorems. I also make another suggestion, namely, that perhaps, or probably, in his then ignorance of geometry he did not understand Barrow. If this is the case it would have been gall and wormwood for Leibniz to have ever owned to it. Then let us suppose that in 1674 with a fairly competent knowledge of higher geometry he reads Barrow again, skipping the Optics of which he had already formed a good opinion, and the wearisome preliminary lectures of which he had already seen more than enough. He notes the theorems as those he has himself already obtained, and the few that are strange to him he translates into his own symbolism. I suggest that this is a feasible supposition, which would account for the marks that Gerhardt states are made in the margin. It would account for the words "in which latter I found the greater part of my theorems anticipated" (this occasion in future times ranking as the first time that he had really read Barrow, and lapse of memory at the end of thirty years making him forget the date of purchase, possibly confusing his two journeys to London); it would account for his using Barrow's differential triangle instead of his own "characteristic triangle." As Barrow tells his readers in his preface that "what these lectures bring forth, or to what they may lead you may easily learn from *the beginnings of each*," let us suppose that Leibniz took his advice. What do we find? The first four theorems of Lecture VIII give the geometrical equivalent of the differentiation of a power of *a dependent* variable; the first five of Lecture IX lead to a proof that, expressed in the differential notation,

$$(ds/dx)^2 = 1 + (dy/dx)^2;$$

the appendix to this lecture contains the differential triangle, and five examples on the *a* and *e* method, fully worked out; the first theorem in Lecture XI has a diagram such that, when that part of it is dissected out (and Barrow's the differential calculus; and indeed, of those who dealt with "indivisibles" or the "infinitely small," who did not use it? But Huygens, who as a matter of fact had some knowledge of the method of fluxions as far as they are known and used, had the fairness to acknowledge that a new light was shed upon geometry by this calculus, and that knowledge of things beyond the province of that science was wonderfully advanced by its use.

Now it certainly never entered the mind of any one else before Leibniz to institute the notation peculiar to the new calculus by which the imagination is freed from a perpetual reference to dia-

diagrams want this in most cases) which applies to a particular paragraph in the proof of the theorem, this portion of the figure is a *mirror image of the figure* drawn by Leibniz when describing the characteristic triangle (turn back to note 30). I shall have occasion to refer to this diagram again. The appendix to this lecture opens with the reference to the work of Huygens; and the second theorem of Lecture XII is the strangest coincidence of all. This theorem in Barrow's words is:

"Hence, if the curve AMB is rotated about the axis AD, the ratio of the surface produced to the space ADLK is that of the circumference of a circle to its diameter; whence, if the space ADLK is known, the said surface is known."

The diagram given by Barrow is as usual very complicated, serving for a group of nine propositions. Fig. F is that part of the figure which refers to the theorem given above, dissected out from Barrow's figure. Now remember that Leibniz always as far as possible kept his axis clear on the left-hand side of his diagram, while Barrow put his datum figure on the left of his axis, and his constructed figures on the right; then you have Leibniz's diagram and the proof is by the similarity of the triangles MNR, PMF, where

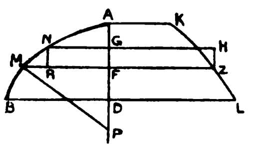


Fig. F.

FZ = PM; and the theorem itself is only another way of enunciating the theorem that Leibniz states he generalized from Pascal's particular case! Lastly, the next theorem starts with the words: "Hence the surfaces of the sphere, both the spheroids and the conoids receive measurement." What a coincidence!

As this note is getting rather long, I have given the full proof of the first two theorems of Barrow's Lecture XII as a supplement, at the end of this section.

The sixth theorem of this lecture is the theorem of Gregory which Leibniz also gives later; I will speak of this when I come to it. As also, when we discuss Leibniz's proof of the rules for a product, etc., I will point out where they are to be found in Barrow ready to his hand.

Yet if all this were so, he could still say with perfect truth that, in the matter of the invention of the differential calculus (as he conceived the matter to consist, that is, the differential and integral notations and the method of analysis), he derived no assistance from Barrow. In fact, once he had absorbed his fundamental ideas, Barrow would be less of a help than a hindrance.

grams, as was made by Vieta and Descartes in their ordinary or Apollonian geometry; moreover, the more advanced parts pertaining to Archimedean geometry, and to lines which were called "mechanical"[48] by Descartes, were excluded by the latter in his calculus. But now by the calculus of Leibniz the whole of geometry is subjected to analytical computation, and those transcendent lines that Descartes called mechanical are also reduced to equations chosen to suit them, by considering the differences dx, ddx, etc., and the sums that are the inverses of these differences, as functions of the x's; and this, by merely introducing the calculus, whereas before this no other functions were admissible but x, xx, x^3, $\sqrt{x}$, etc., that is to say, powers and roots.[49] Hence it is easy to see that those who expressed these differences by 0, as did Fermat, Descartes, and even that rival, in his *Principia* published in 16—,[50] were by that very fact an extremely long way off from the differential calculus; for in this way neither gradation of the differences nor the differential functions of the several quantities can possibly be made out.

There does not exist anywhere the slightest trace of these methods having been practised by any one before Leibniz.[51] With

[48] Apollonian geometry comprised the conic sections or curves of the second degree according to Cartesian geometry; curves of a higher degree and of a transcendent nature, like the spiral of Archimedes, were included under the term "mechanical."

[49] The great discovery of Descartes was not simply the application of geometry; that had been done in simple cases ages before. Descartes recognized the principle that every property of the curve was included in its equation, if only it could be brought out. Thus Leibniz's greatest achievement was the recognition that the differential coefficients were also functions of the abscissa. The word function was applied to certain straight lines dependent on the curve, such as the abscissa itself, the ordinate, the chord, the tangent, the perpendicular, and a number of others (Cantor, III, preface, p. v). This definition is from a letter to Huygens in 1694. There is therefore a great advance made by 1714, the date of the *Historia*, since here it is at least strongly hinted that Leibniz has the algebraical idea of a function.

[50] With regard to Newton, at least, this is untrue. Without a direct reference to the original manuscript of Newton it is quite impossible to state whether even Newton *wrote* 0 or o; even then there may be a difficulty in deciding, for Gerhardt and Weissenborn have an argument over the matter, while Reiff prints it as 0. However this may be there is no doubt that Newton considered it as an infinitely small *unit of time*, only to be put equal to zero when it occurred as a factor of terms in an expression in which there also occurred terms that did not contain an infinitesimally small factor. This was bound to be the case, since Newton's $\dot{x}$ and $\dot{y}$ were velocities. In short, expressing Newton's notation in that of Leibniz, we have

$$\dot{x}o \text{ or } \dot{x}0 = (dx/dt).\, dt$$

and therefore $\dot{x}o$ is an infinitesimal or a differential equal to Leibniz's dx.

[51] This is in a restricted sense true. No one seems to have felt the need of a second differentiation of an original function; those, who did, differen-

precisely the same amount of justice as his opponents display in now assigning such discoveries to Newton, any one could equally well assign the geometry of Descartes to Apollonius, who, although he possessed the essential idea of the calculus, yet did not possess the calculus.

For this reason also the new discoveries that were made by the help of the differential calculus were hidden from the followers of Newton's method, nor could they produce anything of real value nor even avoid inaccuracies until they learned the calculus of Leibniz, as is found in the investigation of the catenary as made by David Gregory.[52] But these contentious persons have dared to misuse the name of the English Royal Society, which body took pains to have it made known that no really definite decision was come to by them; and this is only what is worthy of their reputation for fair dealing, in that one of the two parties was not heard, indeed my friend himself did not know that the Royal Society had undertaken an inquiry into the matter. Else the names of those to whom it had entrusted the report would have been communicated to him,[53] so that they might either be objected to, or equipped for their task. He indeed, astounded not by their arguments but by the fictions that pervaded their attack on his good faith, considered such things unworthy of a reply, knowing as he did that it would be useless to defend his case before those who were unacquainted with this subject (i. e., the great majority of readers); also feeling that those who were skilled in the matter under discussion would readily perceive the injustice of the charge.[54] To this was added the reason that he was absent from home when these reports were circulated by his opponents, and returning home after an interval of two years and being occupied with other busi-

tiated once, and then worked upon the function thus obtained a second time in the same manner as in the first case. Barrow indeed considered only curves of continuous curvature, and the tangents to these curves; but Newton has the notation $\ddot{x}$, etc. But the idea had been used by Slusius in his *Mesolabum* (1659), where a general method of determining points of inflection is made to depend on finding the maximum and minimum values of the subtangent. Lastly, it can hardly be said that Leibniz's interpretation of $\int\int$ ever attained to the dignity of a double integral in his hands.

[52] David Gregory is not the only sinner! Leibniz, using his calculus, makes a blunder over osculations, and will not stand being told about it; he simply repeats in answer that he is right (Rouse Ball's *Short History*).

[53] The names of the committee were not even published with their report. In fact the complete list was not made public until De Morgan investigated the matter in 1852! For their names see De Morgan's *Newton*, p. 27.

[54] What then made Leibniz change his mind?

ness, it was then too late to find and consult the remains of his own past correspondence from which he might refresh his memory about matters that had happened so long ago as forty years previously. For transcripts of very many of the letters once written by him had not been kept; besides those that Wallis found in England and published with his consent in the third volume of his works, Leibniz himself had not very many.

Nevertheless, he did not lack for friends to look after his fair name; and indeed a certain mathematician, one of the first rank of our time[55] well skilled in this branch of learning and perfectly unbiased, whose good-will the opposite party had tried in vain to obtain, plainly stated, giving reasons of his own finding, and let it be known, not altogether with strict justice, that he considered that not only had that rival not invented the calculus, but that in addition he did not understand it to any great extent.[56] Another friend of the inventor[57] published these and other things as well in a short pamphlet, in order to check their base contentions. However it was of greater service to make known the manner and reasoning by which the discoverer arrived at this new kind of calculus; for this indeed has been unknown up till now, even to those perchance, who would like to share in this discovery. Indeed he himself had decided to explain it, and to give an account of the course of his researches in analysis partly from memory and partly from extant writings and remains of old manuscripts, and in this manner to illustrate in due form in a little book the history of this higher learning and the method of its discovery. But since at the time this was found to be impossible owing to the necessities of other business, he allowed this short statement of part of what there was to tell upon the matter to be published in the meantime by a friend who knew all about it,[58] so that in some measure public curiosity should be satisfied.

[55] It is established that this was Johann (John) Bernoulli; see Cantor, III, p. 313f; Gerhardt gives a reference to Bossut's *Geschichte,* Part II, p. 219.

[56] This seems to be an intentional misquotation from Bernoulli's letter, which stated that Newton did not understand the meaning of higher differentiations. At least, that is what Cantor says was given in the pamphlet.

[57] It is established that the pamphlet referred to was also an anonymous contribution by Leibniz himself! Is it strange that hard things are both thought and said of such a man?

[58] Again this is Leibniz himself! Had he then no friends at all to speak for him and dare subscribe their signatures to the opinion? Unfortunately Tschirnhaus was dead at the time of the publication of the *Commercium*

The author of this new analysis, in the first flower of his youth, added to the study of history and jurisprudence other more profound reflections for which he had a natural inclination. Among the latter he took a keen delight in the properties and combinations of numbers; indeed, in 1666 he published an essay, *De Arte Combinatoria,* afterward reprinted without his sanction. Also, while still a boy, when studying logic he perceived that the ultimate analysis of truths that depended on reasoning reduced to two things, definitions and identical truths, and that these alone of the essentials were primitive and undemonstrable. When it was stated in contradiction that identical truths were useless and nugatory, he gave illustrative proofs to the contrary. Among these he gave a demonstration that that mighty axiom, "The whole is greater than its part," could be proved by a syllogism of which the major term was

Epistolicum, but he could have spoken with overwhelming authority, as Leibniz's co-worker in Paris, at any time between the date of Leibniz's review of Newton's *De Quadratura* in the *Acta Eruditorum* until his death in 1708, even if he had died before the publication of Keill's attack in the *Phil. Trans.* of that year was made known to him. Does not this silence on the part of Tschirnhaus, the personal friend of Leibniz, rather tend to make Leibniz's plea, that his opponents had had the shrewdness to wait till Tschirnhaus, among others, was dead, recoil on his own head, in that he has done the very same thing? Leibniz must have known the feeling that this review aroused in England, and, Huygens being dead, Tschirnhaus was his only reliable witness. Of course I am not arguing that Leibniz did found his calculus on that of Newton. I am fully convinced that they both were indebted to Barrow, Newton being so even more than Leibniz, and that they were perfectly independent of one another in the development of the *analytical* calculus. Newton, with his great knowledge of and inclination toward geometrical reasoning, backed with his personal intercourse with Barrow, could appreciate the finality of Barrow's proofs of the differentiation of a product, quotient, power, root, logarithm and exponential, and the trigonometrical functions, in a way that Leibniz could not. But Newton never seems to have been accused of plagiarism from Barrow; even if he had been so accused, he probably had ready as an answer, that Barrow had given him permission to make any use he liked of the instruction that he obtained from him. Leibniz, when so accused, replied by asserting, through confusion of memory I suggest, that he got his first idea from the works of Pascal. Each developed the germ so obtained in his own peculiar way; Newton only so far as he required it for what he considered his main work, using a notation that was of greatest convenience to him, and finally falling back on geometry to provide himself with what appealed to him as rigorous proof; Leibniz, more fortunate in his philosophical training and his lifelong effort after symbolism, has ready to hand a notation, almost developed and perfected when applied to finite quantities, which he saw with the eye of genius could be employed as usefully for infinitesimals. De Morgan justly remarks that one dare not accuse either of these great men of deliberate untruth with regard to specific facts; but it must be admitted that neither of them can be considered as perfectly straightforward; and the political similitude, which Cantor speaks of, in which nothing is too bad to be said of an opponent, seems to have applied just as much to the mathematician of the day as to the politician.

a definition and the minor term an identity.[59] For if one of two things is equal to a part of another the former is called the less, and the later the greater; and this is to be taken as the definition. Now, if to this definition there be added the following identical and undemonstrable axiom, "Every thing possessed of magnitude is equal to itself," i. e., $A = A$, then we have the syllogism:

Whatever is equal to a part of another, is less than that other: (by the definition)

But the part is equal to a part of the whole: (i. e., to itself, by identity)

Hence the part is less than the whole. Q. E. D.

As an immediate consequence of this he observed that from the identity $A = A$, or at any rate from its equivalent, $A - A = 0$, as may be seen at a glance by straightforward reduction, the following very pretty property of differences arises, namely:

$$A \quad \underbrace{-A+B}_{+\ L} \quad \underbrace{-B+C}_{+\ M} \quad \underbrace{-C+D}_{+\ N} \quad \underbrace{-D+E}_{+\ P} \quad -E \; = \; 0$$

If now A, B, C, D, E are supposed to be quantities that continually increase in magnitude, and the differences between successive terms are denoted by L, M, N, P, it will then follow that

$$A+L+M+N+P-E=0,$$
$$\text{i. e.,} \qquad L+M+N+P=E-A;$$

that is, the sums of the differences between successive terms, no matter how great their number, will be equal to the difference

[59] This was given in more detail in the first draught of this essay (G. 1846, p. 26): Hitherto, while still a pupil, he kept trying to reduce logic itself to the same state of certainty as arithmetic. He perceived that occasionally from the first figure there could be derived a second and even a third, without employing conversions (which themselves seemed to him to be in need of demonstration), but by the sole use of the principle of contradiction. Moreover, these very conversions could be proved by the help of the second and third figures, by employing theorems of identity; and then now that the conversion had been proved, it was possible to prove a fourth figure also by its help, and this latter was thus more indirect than the former figures. He marveled very much at the power of identical truths, for they were generally considered to be useless and nugatory. But later he considered that the whole of arithmetic and geometry arose from identical truths, and in general that all undemonstrable truths depending on reasoning were identical, and that these combined with definitions yield identical truths. He gave as an elegant example of this analysis a proof of the theorem, The whole is greater than its part.

between the terms at the beginning and the end of the series.[60] For example, in place of A, B, C, D, E, let us take the squares, 0, 1, 4, 9, 16, 25, and instead of the differences given above, the odd numbers, 1, 3, 5, 7, 9, will be disclosed; thus

$$\begin{array}{ccccccccccc} 0 & & 1 & & 4 & & 9 & & 16 & & 25 \\ & 1 & & 3 & & 5 & & 7 & & 9 & \end{array}$$

From which is evident that

$$1+3+5+7+9=25-0=25,$$
$$\text{and}\qquad 3+5+7+9=25-1=24;$$

and the same will hold good whatever the number of terms or the differences may be, or whatever numbers are taken as the first and last terms. Delighted by this easy, elegant theorem, our young friend considered a large number of numerical series, and also proceeded to the second differences or differences of the differences,[61] the

[60] It is fairly certain that Leibniz could not possibly at this time have perceived that in this theorem he has the germ of an integral. The path to the higher calculus lay through geometry. As soon as Leibniz attained to a sufficient knowledge of this subject he would recognize the area under a curve between a fixed ordinate and a variable one as a set of magnitudes of the kind considered, the ordinates themselves being the differences of the set; he would see that there was no restriction on the number of steps by which the area attained its final size. Hence, in this theorem he has a proof to hand that integration as a determination of an area is the inverse of a difference. This does not mean the inverse of a differentiation, i. e., the determination of a *rate*, or the drawing of a tangent. As far as I can see, Leibniz was far behind Newton in this, since Newton's fluxions were founded on the idea of a rate; also Leibniz apparently does not demonstrate the rigor of a method of infinitely narrow rectangles.

[61] It is a pity that we are not told the date at which Leibniz read his Wallis; it is a greater pity that Gerhardt did not look for a Wallis in the Hanover Library and see whether it had the date of purchase on it (for I have handled lately several of the books of this time, and in nearly every case I found inserted on the title page the name of the purchaser and the date of purchase). I make this remark, because there arises a rather interesting point. Wallis, in his *Arithmetica Infinitorum,* takes as the first term of all his series the number 0, and in one case he mentions that the differences of the differences of the cubes is an arithmetical series. He also works out fully the sums of the figurate numbers (or as Leibniz calls them the combinatory numbers); the general formulas for these sums he calls their *characteristics.* He also remarks on the fact that any number (see table, p. 32) can be obtained by the addition of the one before it and the one above it (which is itself the sum of all the numbers in the preceding column above the one to the left of that which he wishes to obtain). Thus, in the fourth column 4 is the sum of 3 (to the left) and I (above), i. e., the sum of the two first numbers in column three; 10 is the sum of 6 (to the left) and 4 (above, which has been shown to be the sum of the first two numbers of column three), and therefore 10 is the sum of the first three numbers in column three. Now my point is, assuming it to have been impossible that Leibniz had read Wallis at the time that he was compiling his *De Arte,* we have here another example, free from all suspicion, of that series of instances of independent contemporary discoveries that seems to have dogged Leibniz's career.

third differences or the differences between the differences of the differences, and so on. He also observed that for the natural numbers, i. e., the numbers in order proceeding from 0, the second differences vanished, as also did the third differences for the squares, the fourth differences for the cubes, the fifth for the biquadrates, the sixth for the surdesolids,[62] and so on; also that the first differences for the natural numbers were constant and equal to 1; the second differences for the square, 1.2, or 2; the third for the cubes, 1.2.3, or 6; the fourth for the biquadrates, 1.2.3.4, or 24; the fifth for the surdesolids, 1.2.3.4.5, or 120, and so on. These things it is admitted had been previously noted by others, but they were new to him, and by their easiness and elegance were in themselves an inducement to further advances. But especially he considered what he called "combinatory numbers," such as are usually tabulated as in the margin. Here a preceding series, either horizontal or vertical, always contains the first differences of the series immediately following it, the second differences of the one next after that, the third differences of the third, and so on. Also, each series, either horizontal or vertical contains the sums of the series immediately preceding it, the sums of the sums or the second sums of the series next before that, the third sums of the third, and so on. But, to give something not yet common knowledge, he also brought to light certain general theorems on differences and sums, such as the following. In the series, *a, b, c, d, e,* etc., where the terms continually decrease without limit we have

1	1	1	1	1	1
1	2	3	4	5	6
1	3	6	10	15	21
1	4	10	20	35	56
1	5	15	35	70	126
1	6	21	56	126	252
1	7	28	84	210	462

Terms	a	b	c	d	e	etc.
1st diff.	f	g	h	i	k	etc.
2nd diff.	l	m	n	o	p	etc.
3rd diff.	q	r	s	t	u	etc.
4th diff.	β	γ	δ	ϵ	θ	etc.
etc.	γ	μ	ν	ρ	υ	etc.

[62] The name surdesolid to denote the fifth power is used by Oughtred, according to Wallis. By Cantor the invention of the term seems to be credited to Dechales, who says, "The fifth number from unity is called by some people the quadrato-cubus, but this is ill-done, since it is neither a square nor a cube and cannot thus be called the square of a cube nor the cube of a square: we shall call it supersolidus or surde solidus" (Cantor, III, p. 16). Wallis himself uses "sursolid."

Taking a as the first term, and ω as the last, he found

$$\begin{aligned}
a-\omega &= 1f + 1g + 1h + 1i + 1k + \text{etc.}\\
a-\omega &= 1l + 2m + 3n + 4o + 5p + \text{etc.}\\
a-\omega &= 1q + 3r + 6s + 10t + 15u + \text{etc.}\\
a-\omega &= 1\beta + 4\gamma + 10\delta + 20\epsilon + 35\theta + \text{etc.}\\
&\text{etc.}
\end{aligned}$$

Again we have[63]

$$a-\omega = \begin{cases} +1f \\ +1f-1l \\ +1f-2l+1q \\ +1f-3l+3q-1\beta \\ +1f-4l+6q-4\beta+1\lambda \\ \quad\text{etc.}\quad \text{etc.}\quad \text{etc,} \end{cases}$$

Hence, adopting a notation invented by him at a later date, and denoting any term of the series generally by y (in which case $a=y$ as well), we may call the first difference dy, the second ddy, the third d^3y, the fourth d^4y; and calling any term of another of the series x, we may denote the sum of its terms by $\int x$, the sum of their sums or their second sum by $\int\int x$, the third sum by $\int^3 x$, and the fourth sum by $\int^4 x$. Hence, supposing that

$$1+1+1+1+1+\text{etc.}=x,$$

or that x represents the natural numbers, for which $dx=1$, then

[63] This theorem is one of the fundamental theorems in the theory of the summation of series by finite differences, namely,

$$\Delta^m u_n = u_{n+m} - {}_mC_1 \cdot u_{n+m-1} + {}_mC_2 \cdot u_{n+m-2} - \text{etc.},$$

which is usually called the direct fundamental theorem; for although Leibniz could not have expressed his results in this form since he did not know the sums of the figurate numbers as generalized formulas (or I suppose not, if he had not read Wallis), and apparently his is only a special case, yet it must be remembered that any term of the first series can be chosen as the first term. It is interesting to note that the second fundamental theorem, the inverse fundamental theorem, was given by Newton in the *Principia,* Book III, lemma V, as a preliminary to the discussion on comets at the end of this book. Here he states the result, without proof, as an interpolation formula; (it is frequently referred to as Newton's Interpolation Formula); it may however be used as an extrapolation formula, in which case we have

$$u_{m+n} = u_m + {}_nC_1 \cdot \Delta u_m + {}_nC_2 \cdot \Delta^2 u_m + \text{etc.}$$

In the two formulas as given here, the series are

$$\begin{array}{ccccccc}
u_1 & u_2 & u_3 & u_4 & u_5 & \text{etc.} & \\
& \Delta u_1 & \Delta u_2 & \Delta u_3 & \Delta u_4 & \text{etc.} & \\
& & \Delta^2 u_2 & \Delta^2 u_3 & \Delta^2 u_4 & \text{etc.} & \text{and so on.}
\end{array}$$

$$1 + 3 + 6 + 10 + \text{etc.} = \int x,$$
$$1 + 4 + 10 + 20 + \text{etc.} = \int\int x,$$
$$1 + 5 + 15 + 35 + \text{etc.} = \int^3 x,$$

and so on. Finally it follows that

$$y - \omega = dy.x - ddy.\int x + d^3y.\int\int x - d^4y.\int^3 x + \text{etc.};$$

and this is equal to y, if we suppose that the series is continued to infinity, or that ω becomes zero. Hence also follows the sum of the series itself, and we have

$$\int y = yx - dy.\int x + ddy.\int\int x - d^3y.\int^3 x + \text{etc.}[64]$$

These two like theorems possess the uncommon property that they are equally true in either differential calculus, the numerical or the infinitesimal; of the distinction between them we will speak later.[65]

[64] What are we to understand by the inclusion of this series in this connection? Does Leibniz intend to claim this as his? I have always understood that this is due to Johann Bernoulli, who gave it in the *Acta Eruditorum* for 1694, in a slightly different form, and proved by direct differentiation; and that Brook Taylor obtained it as a particular case of a general theorem in and by *finite differences*. If Leibniz intended to claim it, he has clearly anticipated Taylor. It is quite possible that Leibniz had done so, even in his early days; and as soon as in 1675, or thereabouts, he had got his signs for differentiation and integration, it is possible that he returned to this result and expressed it in the new notation; for the theorem follows so perfectly naturally from the last expression given for $a - \omega$. But it is hardly probable, for Leibniz would almost certainly have shown it to Huygens and mentioned it.

The other alternative is that here he is showing how easily Bernoulli's series could have been found in a much more *general form*, i. e., as a theorem that is true (as he indeed states) for finite differences as well as for infinitesimals; the inclusion of this statement makes it very probable that this supposition is a correct one. This leads to a pertinent, or impertinent, question. Brook Taylor's *Methodus Incrementorum* was published in 1715; the *Historia* was written some time between 1714 and 1716; Gerhardt states that there were two draughts of the latter, and that he is giving the second of these. In justice to Leibniz there should be made a fresh examination of the two draughts, for if this theorem is not given in the original draught it lays Leibniz open to further charge of plagiarism. I fully believe that the theorem will be found in the first draught as well and that my alternative suggestion is the correct one.

In any case, the tale of the *Historia* is confused by the interpolation of the symbolism invented later (as Leibniz is careful to point out). The question is whether this was not intentional. And this query is not impertinent, considering the manner in which Leibniz refrains from giving dates, or when we compare the essay in the *Acta Eruditorum,* in which he gives to the world the description of his method. Weissenborn considers that "this is not adapted to give an insight into his methods, and it certainly looks as if Leibniz wished deliberately to prevent this." Cf. Newton's "anagram" (sic), and the Geometry of Descartes, for parallels.

[65] In reference to the employment of the calculus to diagrammatic geometry, as will be seen later, Leibniz says:

"But our young friend quickly observed that the differential calculus could be employed with figures in an even more wonderfully simple manner

However, the application of numerical truths to geometry, as well as the consideration of infinite series, was at that time at all events unknown to our young friend, and he was content with the satisfaction of having observed such things in series of numbers. Nor did he then, except for the most ordinary practical rules, know anything about geometry;[66] he had scarcely even considered Euclid with anything like proper attention, being fully occupied with other studies. However, by chance he came across the delightful contemplation of curves by Leotaud, in which the author deals with the quadrature of lunules, and Cavalieri's geometry of indivisibles;[67] having given these some slight consideration, he was delighted with the facility of their methods. However, at the time he was in no mind to go fully into these more profound parts of mathematics; although just afterwards he gave attention to the study of physics and practical mechanics, as may be understood from his essay that he published on the *Hypothesis of Physics*.[68]

He then became a member of the Revision Council[69] of the Most Noble the Elector of Mainz; later, having obtained permission from this Most Gracious and Puissant Prince (for he had taken our young friend into his personal service when he was about to

than it was with numbers, because *with figures the differences were not comparable with the things which differed*; and as often as they were connected together by addition or subtraction, being incomparable with one another, the less vanished in comparison with the greater."

[66] This makes what has just gone before date from the time previous to his reading of the work of Cavalieri. See note following.

[67] This is about the first place in which it is possible to deduce an exact date, or one more or less exact. According to Leibniz's words that immediately follow it may be deduced that it was somewhere about twelve months before the publication of the Hypothesis of Physics—if we allow for a slight interval between the dropping of the geometry and the consideration of the principles of physics and mechanics, and a somewhat longer interval in which to get together the ideas and materials for his essay—that he had finished his "slight consideration" of Leotaud and Cavalieri. This would make the date 1670, and his age 24.

[68] This essay founded the explanation of all natural phenomena on motion, which in turn was to be explained by the presence of an all-pervading ether; this ether constituted light.

[69] The dedication of the *Nova methodus* in 1667 to the Elector of Mainz (ancient name Moguntiacum) procured for Leibniz his appointment in the service of the latter, first as an assistant in the revision of the statute-book, and later on the more personal service of maintaining the policy of the Elector, that of defending the integrity of the German Empire against the intrigues of France, Turkey and Russia, by his pen.

leave[70] and go further afield) to continue his travels, he set out for Paris in the year 1672. There he became acquainted with that genius, Christiaan Huygens, to whose example and precepts he always declared that he owed his introduction to higher mathematics. At that time it so happened that Huygens was engaged on his work with regard to the pendulum. When Huygens brought our young friend a copy of this work as a present and in the course of conversation discussed the nature of the center of gravity, which our young friend did not know very much about, the former explained to him shortly what sort of thing it was and how it could be investigated.[71] This roused our young friend from his lethargy, for he looked upon it as something of a disgrace that he should be ignorant of such matters.[72]

Now it was impossible for him to find time for such studies just then; for almost immediately, at the close of the year, he crossed the Channel to England in the suite of the envoy from Mainz, and stayed there for a few weeks with the envoy. Having been introduced by Henry Oldenburg, at that time secretary to the Royal Society, he was elected a member of that illustrious body. He did not however at that time discuss geometry with any one (in truth at that time he was quite one of the common herd as regards this subject); he did not on the other hand neglect chemistry, consulting that excellent man, Robert Boyle, on several occasions. He also came across Pell accidentally, and he described to him certain of his own observations on numbers; and Pell told him that they were not new, but that it had been recently made known by Nicolaus Mercator, in his *Hyperbolae Quadratura,* that the differences of the powers of the natural numbers, when taken continuously, finally vanished; this made Leibniz obtain the work of Nicolaus

[70] This probably refers to the time when his work on the statute-book was concluded, and Leibniz was preparing to look for employment elsewhere.

[71] This is worthy of remark, seeing that Leibniz had attempted to explain gravity in the *Hypothesis physica nova* by means of his concept of an ether. The conversation with Huygens had results that will be seen later in a manuscript (see § 4, p. 65) where Leibniz obtains quadratures "*ex Centrobarycis.*" It also probably had a great deal to do with Leibniz's concept of a "moment."

[72] The use of the word *veterno*—which I have translated "lethargy" as being the nearest equivalent to the fundamental meaning, the sluggishness of old age—coupled with his remark that he was in no mind to enter fully into these more profound parts of mathematics, sheds a light upon the reason why he had so far done no geometry. Also the last words of the sentence give the stimulus that made him cast off this lethargy; namely, shame that he should appear to be ignorant of the matter. This would seem to be one of the great characteristics of Leibniz, and might account for much, when we come to consider the charges that are made against him.

Mercator.[73] At that time he did not become acquainted with Collins; and, although he conversed with Oldenburg on literary matters, on physics and mechanics, he did not exchange with him even one little word on higher geomery, much less on the series of Newton. Indeed, that he was almost a stranger to these subjects, except perhaps in the properties of numbers, even that he had not paid very much attention to them, is shown well enough by the letters which he exchanged with Oldenburg, which have been lately published by his opponents. The same fact will appear clearly from those which they say have been preserved in England; but they suppressed them,[74] I firmly believe, because it would be quite clear from them that up to then there had been no correspondence between him and Oldenburg on matters geometrical. Nevertheless, they would have it credited (not indeed with the slightest evidence brought forward in favor of the supposition) that certain results obtained by Collins, Gregory and Newton, which were in the possession of Oldenburg, were communicated by him to Leibniz.

On his return from England to France in the year 1673,[75] having meanwhile satisfactorily performed his work for the Most Noble Elector of Mainz, he still by his favor remained in the service of Mainz; but his time being left more free, at the instigation of Huygens he began to work at Cartesian analysis (which aforetime had been beyond him),[76] and in order to obtain an insight

[73] We have here a parallel (or a precedent) for my suggestion that Leibniz was mentally confusing Barrow and Pascal as the source of his inspiration for the characteristic triangle. For here, without any doubt whatever, is a like confusion. What Pell told him was that his theorems on numbers occurred in a book by Mouton entitled *De diametris apparentibus Solis et Lunae* (published in 1670). Leibniz, *to defend himself from a charge of plagiarism,* made haste to borrow a copy from Oldenburg and found to his relief that not only had Mouton got his results by a different method, but that his own were more general. The words in italics are interesting.

Of course these words are not italicized by Gerhardt, from whom this account has been taken (G. 1848, p. 19); nor does he remark on Leibniz's lapse of memory in this instance. Further there is no mention made of it in connection with the *Historia,* i. e., in G. 1846. Is it that Gerhardt, as counsel for the defense, is afraid of spoiling the credibility of his witness by proving that part of his evidence is unreliable? Or did he not become aware of the error till afterward? See Cantor, III, p. 76.

[74] An instance is referred to on p. 85 of De Morgan's *Newton,* showing the sort of thing that was done by the committee. This however is not connected with a letter to Oldenburg, but to Collins. It may be taken as a straw that shows the way the wind blew.

[75] Observe that nothing has been said of the fact that Leibniz had purchased a copy of Barrow and took it back with him to Paris.

[76] Cf. the remark in the postscript to Bernoulli's letter, where Leibniz says that the work of Descartes, looked at at about the same time as Clavius, that is, while he was still a youth, "seemed to be more intricate."

into the geometry of quadratures he consulted the *Synopsis Geometriae* of Honoratus Fabri, Gregory St. Vincent, and a little book by Dettonville (i. e., Pascal).[77] Later on from one example given by Dettonville, a light suddenly burst upon him, which strange to say Pascal himself had not perceived in it. For when he proves the theorem of Archimedes for measuring the surface of a sphere or parts of it, he used a method in which the whole surface of the solid formed by a rotation round any axis can be reduced to an equivalent plane figure. From it our young friend made out for himself the following general theorem.[78]

Portions of a straight line normal to a curve, intercepted between the curve and an axis, when taken in order and applied at right angles to the axis give rise to a figure equivalent to the moment of the curve about the axis.[79]

When he showed this to Huygens the latter praised him highly and confessed to him that by the help of this very theorem he had found the surface of parabolic conoids and others of the same sort, stated without proof many years before in his work on the pendulum clock. Our young friend, stimulated by this and pondering on the fertility of this point of view, since previously he had considered infinitely small things such as the intervals between the ordinates in the method of Cavalieri and such only, studied the triangle ${}_1Y\,D\,{}_2Y$, which he called the Characteristic Triangle,[80]

[77] The *libellus* referred to would seem to be the work on the cycloid, written by Pascal in the form of letters, from one Amos Dettonville, to M. de Carcavi.

[78] This theorem is given, and proved by the method of indivisibles, as Theorem I, of Lecture XII in Barrow's *Lectiones Geometricae*; and Theorem II is simply a corollary, in which it is remarked:

"Hence the surfaces of the sphere, both the spheroids, and the conoids receive measurement...."

The proof of these two theorems is given at the end of this section as a supplement. See also Note 46, for its significance.

[79] The whole context here affords suggestive corroboration in favor of the remarks made in Note 31 on the use of the word "moment," though the connection with the determination of the center of gravity is here overshadowed by its connection with the surface formed by the rotation of an arc about an axis.

[80] The figure given is exactly that given by Gerhardt, with the unimportant exception that, for convenience in printing, I have used U instead of Gerhardt's Θ, a V instead of his ח (a Hebrew T), and a Q for his Π. I take it, of course, that Gerhardt's diagram is an exact transcript of Leibniz's, and it is interesting to remark that Leibniz seems to be endeavoring to use T's for all points on the tangent, and P's for points on the normal, or perpendicular, as it is rendered in the Latin.

This diagram should be compared with that in the "postscript" written nine or ten years before. Note the complicated diagram that is given here.

whose sides $D\,{}_1Y$, $D\,{}_2Y$ are respectively equal to ${}_1X\,{}_2X$, ${}_1Z\,{}_2Z$,[81] parts of the coordinates or coabscissae AX, AZ, and its third side ${}_1Y\,{}_2Y$ a part of the tangent TV, produced if necessary.

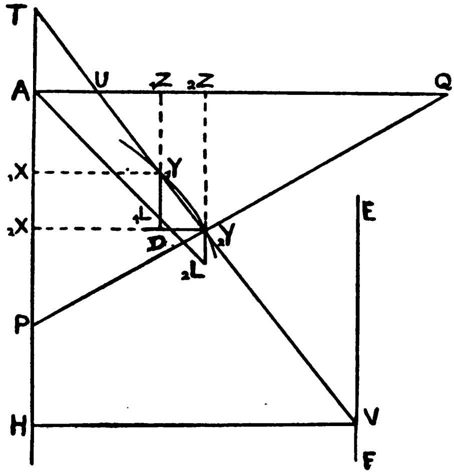


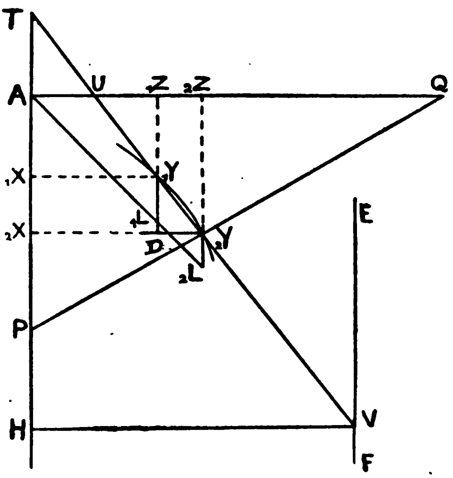
Fig. 3.

Even though this triangle is indefinite (being infinitely small), yet he perceived that it was always possible to find definite triangles similar to it. For, suppose that AXX, AZZ are two straight lines at right angles, and AX, AZ the coabscissae, YX, YZ the coordinates, TUV the tangent, PYQ the perpendicular, XT, ZU the subtangents, XP, ZQ the subnormals; and lastly let EF be drawn

and the introduction of the secant that is ultimately the tangent, which does not appear in the first figure. From what follows, this is evidently done in order to introduce the further remarks on the similar triangles. It adds to the confusion when an effort is made to determine the dates at which the several parts were made out. For instance, the remark that finite triangles can be found similar to the characteristic triangle probably belongs approximately to the date of his reply to the assertions of Nieuwentijt, which will be referred to later.

[81] The notation introduced in the lettering should be remarked. His early manuscripts follow the usual method of the time in denoting different positions of a variable line by the same letter, as in Wallis and Barrow, though even then he is more consistent than either of the latter. He soon perceives the inconvenience of this method, though as a means of generalizing theorems it has certain advantages. We therefore find the notation C, (C), ((C)), for three consecutive points on a curve, as occurs in a manuscript dated (or it should be) 1675. This notation he is still using in 1703; but in 1714, he employs a subscript prefix. This is all part and parcel with his usual desire to standardize and simplify notations.

parallel to the axis AX; let the tangent TY meet EF in V, and from V draw VH perpendicular to the axis. Then the triangles ${}_1YD\,{}_2Y$, TXY, YZU, TAU, YXP, QZY, QAP, THV, and as many more of the sort as you like, are all similar. For example, from the similar triangles ${}_1YD\,{}_2Y$, ${}_2Y\,{}_2XP$, we have $P\,{}_2Y \,.\, {}_1YD = {}_2Y\,{}_2X \,.\, {}_2Y\,{}_1Y$; that is, the rectangle contained by the perpendicular $P\,{}_2Y$ and ${}_1YD$ (or the element of the axis, ${}_1X\,{}_2X$) is equal to the rectangle contained by the ordinate ${}_2Y\,{}_2X$ and the element of the curve, ${}_1Y\,{}_2Y$, that is, to the moment of the element of the curve about the axis. Hence the whole moment of the curve is obtained by forming the sum of these perpendiculars to the axis.



Also, on account of the similar triangles ${}_1YD\,{}_2Y$, THV, we have ${}_1Y\,{}_2Y : {}_2YD = TV : VH$, or $VH \,.\, {}_1Y\,{}_2Y = TV \,.\, {}_2YD$; that is, the rectangle contained by the constant length VH and the element of the curve, ${}_1Y\,{}_2Y$, is equal to the rectangle contained by TV and ${}_2YD$, or the element of the coabscissa, ${}_1Z\,{}_2Z$. Hence the plane figure produced by applying the lines TV in order at right angles to AZ is equal to the rectangle contained by the curve when straightened out and the constant length HV.

Again, from the similar triangles ${}_1YD\,{}_2Y$, ${}_2Y\,{}_2XP$, we have ${}_1YD : D\,{}_2Y = {}_2Y\,{}_2X : {}_2XP$, and thus ${}_2XP \,.\, {}_1YD = {}_2Y\,{}_2X \,.\, D\,{}_2Y$, or the sum of the subnormals ${}_2XP$, taken in order and applied to the axis, either to ${}_1YD$ or to ${}_1X\,{}_2X$, will be equal to the sum of the products of the ordinates ${}_2Y\,{}_2X$ and their elements, ${}_2YD$, taken in order. But straight lines that continually increase from zero, when each is multiplied by its element of increase, form altogether a triangle. Let then AZ always be equal to ZL, then we get the right-angled triangle AZL, which is half the square on AZ; and thus the figure that is produced by taking the subnormals in order and applying them perpendicular to the axis will be always equal to half the square on the ordinate. Thus, to find the area of a given figure, another figure is sought such that its subnormals are respectively equal to the ordinates of the given figure, and then this second figure is the quadratrix of the given one; and thus from this extremely elegant consideration we obtain the reduction of the areas of surfaces described by rotation[82] to plane quadratures, as well as the rectification of curves; at the same time we can reduce these quadratures of figures to an inverse problem of tangents. From these results,[83] our young friend wrote down a large collection of theorems (among which in truth there were many that were lacking in elegance) of two kinds. For in some of them only definite magnitudes were dealt with, after the manner not only of Cavalieri, Fermat, Honoratus Fabri, but also of Gregory St. Vincent, Guldinus, and Dettonville; others truly depended on infinitely small magnitudes, and advanced to a much greater extent. But later our young friend did not not trouble to go on with these matters, when he noticed that the same method had been brought into use and perfected by not only Huygens, Wallis, Van Huraet, and Neil, but also by James Gregory and Barrow. However it did not seem to me to be altogether useless to explain at this juncture, as is plain from what I have given,[84] the steps by which he attained to greater things, and also the manner in which, as if led by the hand, those who are at present but beginners[85] with regard

[82] This sentence conclusively proves that Leibniz's use of the moment was for the purposes of quadrature of surfaces of rotation.

[83] "From these results"—which I have suggested he got from Barrow—"our young friend wrote down a large collection of theorems." These theorems Leibniz probably refers to when he says that he found them all to have been anticipated by Barrow, "when his Lectures appeared." I suggest that the "results" were all that he got from Barrow on his first reading, and that the "collection of theorems" were found to have been given in Barrow when Leibniz referred to the book again, after his geometrical knowledge was improved so far that he could appreciate it.

[84] The use of the first person is due to me. The original is impersonal, but is evidently intended by Leibniz to be taken as a remark of the writer, "the friend who knew all about it." The distinction is marked better by the use of the first personal pronoun than in any other way.

[85] Query, all except Leibniz, the Bernoullis, and one or two others.

to the more abstruse parts of geometry may hope to rise to greater heights.

Now Leibniz worked these things out at Paris in the year 1673 and part of 1674. But in the year 1674 (so much it is possible to state definitely), he came upon the well-known arithmetical tetragonism;[86] and it will be worth while to explain how this was accomplished. He once happened to have occasion to break up an area into triangles formed by a number of straight lines meeting in a

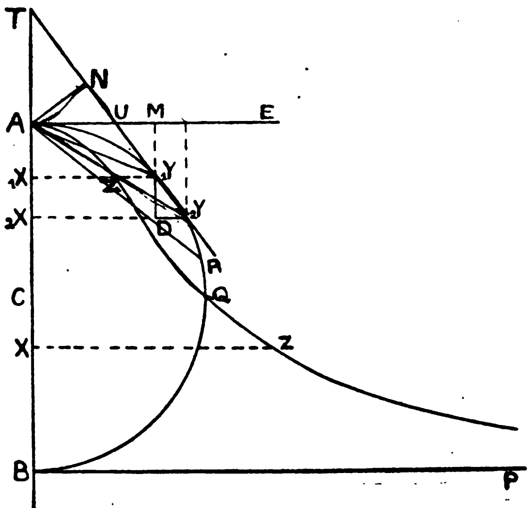


Fig. 4.

point, and he perceived that something new could be readily obtained from it.[87]

In Fig. 4, let any number of straight lines, AY, be drawn to the curve AYR, and let any axis AC be drawn, and AE, a normal or coaxis to it; and let the tangent at Y to the curve cut them in T and U. From A draw AN perpendicular to the tangent; then

[86] Tetragonism = quadrature; the arithmetical tetragonism is therefore Leibniz's value for π as an infinite series, namely,

"The area of a circle, of which the square on the diameter is equal to unity, is given by the series

$$\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \text{etc.''}$$

[87] This is clearly original as far as Leibniz is concerned; but the consideration of a polar diagram is to be found in many places in Barrow. Barrow however forms the polar differential triangle, as at the present time, and does not use the rectangular coordinate differential triangle with a polar figure; nor does Wallis. We see therefore that Leibniz, as soon as ever he follows his own original line of thinking, immediately produces something good.

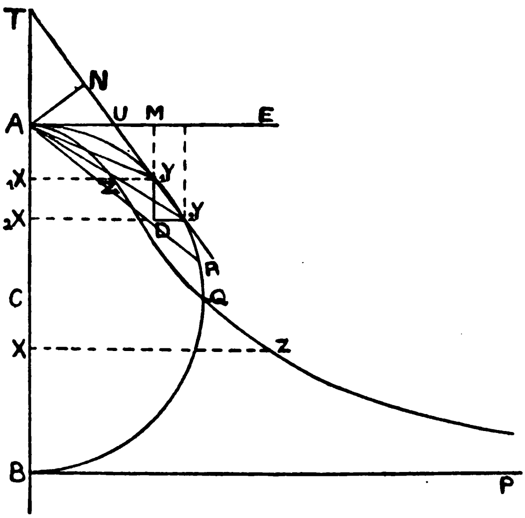
it is plain that the elementary triangle $A\,{}_1Y\,{}_2Y$ is equal to half the rectangle contained by the element of the curve ${}_1Y\,{}_2Y$ and AN. Now draw the characteristic triangle mentioned above, ${}_1YD\,{}_2Y$, of which the hypotenuse is a portion of the tangent or the element of the arc, and the sides are parallel to the axis and the coaxis. It is then plain from the similar triangles ANU, ${}_1YD\,{}_2Y$, that ${}_1Y\,{}_2Y : {}_1YD = AU : AN$, or $AU \,.\, {}_1YD$ or $AU \,.\, {}_1X\,{}_2X$ is equal to $AN \,.\, {}_1Y\,{}_2Y$, and this, as has been already shown, is equal to double the triangle $A\,{}_1Y\,{}_2Y$. Thus if every AU is supposed to be transferred to XY, and taken in it as AZ,[88] then the trilinear space AXZA so formed will be equal to twice the segment AY⌣A,[89] included between the straight line AY and the arc AY. In this way are obtained what he called the figures of segments or the proportionals of a segment. A similar method holds good for the case in which the point is not taken on the curve, and in this manner he obtained the proportional trilinear figures for sectors cut off by lines meeting in the point; and even when the straight lines had their extremities not in a line but in a curve (which one after the other they touched), none the less on that account were useful theorems made out.[90] But this is not a fit occasion to follow out such matters; it is sufficient for our purpose to consider the figures of segments, and that too only for the circle. In this case, if the point A is taken at the beginning of the quadrant AYQ, the curve AZQZ will cut the circle at Q, the other end of the quadrant, and thence descending will be asymptotic to the base BP (drawn at right angles to the diameter at its other end B); and, although extending to infinity, the whole

[88] This is evidently a misprint; it is however curious that it is repeated in the second line of the next paragraph. Probably, therefore, it is a misreading due to Gerhardt, who mistakes AZ for the letters XZ, as they ought to be; and has either not verified them from the diagram, or has refrained from making any alteration.

[89] The symbol ⌣ is here to be read as "and then along the arc to."

[90] Probably refers to Leibniz's work on curvature, osculating circles, and evolutes, as given in the *Acta Eruditorum* for 1686, 1692, 1694. It is to be noted that with Leibniz and his followers the term evolute has its present meaning, and as such was first considered by Huygens in connection with the cycloid and the pendulum. It signified something totally different in the work of Barrow, Wallis and Gregory. With them, if the feet of the ordinates of a curve are, as it were, all bunched together in a point, so as to become the *radii vectores* of another curve, without rupturing the curve more than to alter its curvature (the area being thus halved), then the first curve was called the evolute of the second and the second the involute of the first. See Barrow's *Lectiones Geometricae,* Lecture XII, App. III, Prob. 9, and Wallis's *Arithmetica Infinitorum,* where it is shown that the evolute, in this sense, of a parabola is a spiral of Archimedes.

figure, included between the diameter AB, the base BP...., and the curve AZQZ.... asymptotic to it, will be equal to the circle on AB as diameter.



But to come to the matter under discussion, take the radius as unity, put AX or UZ = x, and AU or AZ = z, then we have $x = 2zz :,\ 1 + zz$;[91] and the sum of all the x's applied to AU, which at the present time we call $\int x\, dz$, is the trilinear figure AUZA, which is the complement of the trilinear figure AXZA, and this has been shown to be double the circular segment.

The author obtained the same result by the method of transmutations, of which he sent an account to England.[92] It is required to form the sum of all the ordinates $\sqrt{(1 - xx)} = y$; suppose $y = \pm 1 \mp xz$, from which $x = 2z :,\ 1 + zz$, and $y = \pm zz \mp 1, :,\ zz + 1$; and thus again all that remains to be done is the summation of rationals.

This seemed to him to be a new and elegant method, as it did to Newton also, but it must be acknowledged that it is not

[91] The colon is used as a sign of division, and the comma has the significance of a bracket for all that follows. It is curious to notice that Leibniz still adheres to the use of xx for x^2, while he uses the index notation for all the higher powers, just as Barrow did; also, that the bracket is used under the sign for a square root, and that too in addition to the vinculum. For an easy geometrical proof of the relation $x = 2z^2/(1 + z^2)$, see Note 94.

[92] See Cantor, III, pp. 78-81. Also note the introduction of what is now a standard substitution in integration for the purpose of rationalization.

of universal application. Moreover it is evident that in this way the arc may be obtained from the sine, and other things of the same kind, but indirectly. So when later he heard that these things had been derived in a direct manner by Newton with the help of root-extractions,[93] he was desirous of getting a knowledge of the matter.

From the above it was at once apparent that, using the method by which Nicolaus Mercator had given the arithmetical tetragonism of the hyperbola by means of an infinite series, that of the circle might also be given, though not so symmetrically, by dividing by $1+zz$, in the same way that the former had divided by $1+z$. The author, however, soon found a general theorem for the area of any central conic. Namely, the sector included by the arc of a conic section, starting from the vertex, and two straight lines joining its ends to the center, is equal to the rectangle contained by the semi-transverse axis and a straight line of length

$$t \pm \frac{1}{3}t^3 + \frac{1}{5}t^5 \pm \frac{1}{7}t^7 + \ldots\ldots,$$ [94]

where t is the portion of the tangent at the vertex intercepted between the vertex and the tangent at the other extremity of the arc, and unity is the square on the semi-conjugate axis or the rectangle contained by the halves of the latus-rectum and the transverse axis, and $\pm$ is to be taken to mean + for the hyperbola and − for the circle or the ellipse. Hence if the square of the diameter is taken to be unity, then the area of the circle is

$$\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \text{etc.}$$

[93] This term represents what is now generally known as the method of inversion of series. Thus, if we are given

$$x = y + ay^2 + by^3 + cy^4 + \text{etc.},$$

where x and y are small, then $y = x$ is a first approximation; hence since $y = x - ay^2 - by^3 - cy^4 - \text{etc.}$, we have as a second approximation

$$y = x - ax^2;$$

substituting this in the term containing y^2, and the first approximation, $y = x$, in the term containing y^3, we have

$$y = x - a(x - ax^2)^2 - bx^3 = x - ax^2 + (2a^2 - b)x^3,$$

as a third approximation; and so on.

[94] The relation $x = 2z^2/(1+z^2)$ can be easily proved geometrically for

When our friend showed this to Huygens, together with a proof of it, the latter praised it very highly, and when he returned the dissertation said, in the letter that accompanied it, that it would be a discovery always to be remembered among mathematicians, and that in it the hope was born that at some time it might be possible that the general solution should be obtained either by exhibiting its true value or by proving the impossibility of expressing it in recognized numbers.[95] There is no doubt that neither he nor the discoverer, nor yet any one else in Paris, had heard anything at all by report concerning the expression of the area of a circle by means of an infinite series of rationals (such as afterward it became known had been worked out by Newton and Gregory). Certainly Huygens did not, as is evident from the short

the circle; hence, by using the orthogonal projection theorem, Leibniz's result for the central conic can be immediately derived.

Thus suppose that, in the diagrams below, AC is taken to be unity, then $AU = z$ and $AX = x$.

Then, in either figure, since the $\triangle$s BYX, CUA are similar,

$$AX : XB = AX . XB : XB^2 = XY^2 : XB^2 = AU^2 : CA^2;$$

hence, for the circle, we have

$$AX : AB = AU^2 : AC^2 + AU^2, \text{ or } x = 2z^2/(1+z^2);$$

and similarly for the rectangular hyperbola

$$AX : AB = AU^2 : AC^2 - AU^2, \text{ or } x = 2z^2/(1-z^2).$$

Applying all the x's to the tangent at A, we have (by division and integration of the right-hand side, term by term, in the same way as Mercator)

$$\text{area AUMA} = 2(z^3/3 \mp z^5/5 + z^7/7 \mp \text{etc.})$$

Now, since the triangles UAC, YXB are similar, $UA.XB = AC.XY$; hence $2\triangle AYC = 2UA.AC \mp UA.AX = 2UA.AC \mp AUMA \mp 2$ seg. AYA, for Leibniz has shown that $AXMA = 2$ seg. AYA; hence it follows immediately that

$$\text{sector ACYA} = z \mp z^3/3 + z^5/5 \mp \text{etc.}$$

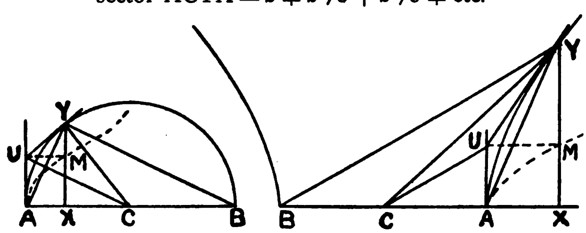


Fig. G. Fig. H.

If now, keeping the vertical axis equal to unity, the transverse axis is made equal to a, Leibniz's general theorem follows at once from the orthogonal projection relation.

Note that z is, from the nature of the diagrams, less than 1.

[95] Wallis's expression for π as an infinite product, given in the *Arithmetica* (or Brouncker's derived expression in the form of an infinite continued fraction), or the argument used by Wallis in his work, could not possibly be taken as a proof that π could not be expressed in recognized numbers.

letter from him that I give herewith.[96]... Thus Huygens believed that it was now proved for the first time that the area of a circle was exactly equal to a series of rational quantities. Leibniz (relying on the opinion of Huygens, who was well versed in such matters), believed the same thing and so wrote those two letters to Oldenburg in 1674, which his opponents have published, in which he announces it as a new discovery;[97] indeed he went so far as to say that he, before all others, had found the magnitude of the circle expressed as a series of rational numbers, as had already been done in the case of the hyperbola.[98] Now, if Oldenburg had already communicated to him during his stay in London the series of Newton and Gregory,[99] it would have been the height of impudence for him to have dared to write in this way to Oldenburg; and either forgetfulness or collusion on the part of Oldenburg in not charging him with the deceit. For these opponents publish the reply of Oldenburg, in which he merely points out (he says "I do not wish you to be unaware....") that similar series had been noted by Gregory and Newton; and these things also he communicated in the year following in a letter (which they publish) written in the month of April.[100] From which it can be seen that they are blinded with envy or shameless with spite who dare to pretend that Oldenburg had already communicated those things to him in the preceding year. Yet there may be some blindness in their spite, because they do not see that they publish things by which their lying statements are refuted, nor that it would have been far better to have suppressed these letters between him and Oldenburg, as they have done in the case of others, either wholly or in part. Besides, from this time onwards he begins to correspond with Oldenburg about geometry; that is, from the time when he, who up till then had been but a

[96] The letter that is missing would no doubt have been given, in the event of the *Historia* being published. According to Gerhardt it is to be found in *Ch. Hugenii....exercitationes,* ed. Uylenbroeck, Vol. I, p. 6, under date Nov. 7, 1674.

[97] Collins wrote to Gregory in Dec. 1670, telling him of Newton's series for a sine, etc.; Gregory replied to Collins in Feb. 1671, giving him three series for the arc, tangent and secant; these were probably the outcome of his work on *Vera Circuli* (1667).

[98] By Mercator; query, also an allusion to Brouncker's article in the *Phil. Trans.,* 1668.

[99] Quite conclusive; no other argument seems required.

[100] This date, April 12, 1675, is important; it marks the time when Leibniz first began to speak of geometry in his correspondence with Oldenburg, as he says below.

beginner in this subject, first found out anything that he considered worthy to be communicated; and former letters written from Paris on March 30, April 26, May 24, and June 8, in the year 1673, which they say they have at hand but suppress, together with the replies of Oldenburg, must undoubtedly have dealt with other matters and have nothing in them to render those fictitious communications from Oldenburg the more deserving of belief. Again, when our young friend heard that Newton and Gregory had discovered their series by the extraction of roots,[101] he acknowledged that this was new to him, nor at first did he understand it very much; and he confessed as much quite frankly and asked for information on certain points, especially for the case in which reciprocal series were sought, by means of which from one infinite series the root was extracted by means of another infinite series. And from this also it is evident that what his opponents assert, that Oldenburg communicated the writings of Newton to him, is false; for if that were the truth, there would have been no need to ask for further information. On the other hand, when he began to develop his differential calculus, he was convinced that the new method was much more universal for finding infinite series without root-extractions, and adapted not only for ordinary quantities but for transcendent quantities as well, by assuming that the series required was given; and he used this method to complete his short essay on the arithmetical quadrature; in this he also included other series that he had discovered, such as an expression for the arc in terms of the sine or the complement of the sine, and conversely he showed how, by this same method, to find the sine or cosine when the arc was given.[102] This too is the reason why later he stood in no need of other methods than his own; and finally, he published his own new way of obtaining series in the *Acta Eruditorum*. Moreover, as it was at this time, just after he had published the essay on the Arithmetical Quadrature in Paris, that he was

[101] Newton obtained the series for arcsin x from the relation $\dot{a}:\dot{x} = 1:\sqrt{(1-x^2)}$, by expansion and integration, and then the series for the sine by the "extraction of roots." See Note 93, and, for Newton's own modification, Cantor, III, p. 73.

[102] It would appear from this that Leibniz could differentiate the trigonometrical functions. Professor Love, on the authority of Cantor, ascribes them to Cotes; but I have shown in an article in *The Monist* for April, 1916, that Barrow had explicitly differentiated the tangent and that his figures could be used for all the other ratios. Note the word "later" in the next sentence.

recalled to Germany, having perfected the technique of the new calculus he paid less attention to the former methods.

Now it is to be shown how, little by little, our friend arrived at the new kind of notation that he called the differential calculus. In the year 1672, while conversing with Huygens on the properties of numbers, the latter propounded to him this problem:[103]

To find the sum of a decreasing series of fractions, of which the numerators are all unity and the denominators are the triangular numbers; of which he said that he had found the sum among the contributions of Hudde on the estimation of probability. Leibniz found the sum to be 2, which agreed with that given by Huygens. While doing this he found the sums of a number of arithmetical series of the same kind in which the numbers are any combinatory numbers whatever, and communicated the results to Oldenburg in February 1673, as his opponents have stated. When later he saw the Arithmetical Triangle of Pascal, he formed on the same plan his own Harmonic Triangle.[104]

Arithmetical Triangle

in which the fundamental series is an arithmetical progression

1, 2, 3, 4, 5, 6, 7, . . .

1

1 1

1 2 1

1 3 3 1

1 4 6 4 1

1 5 10 10 5 1

1 6 15 20 15 6 1

1 7 21 35 35 21 7 1

[103] Probably only to test Leibniz's knowledge.

[104] Gerhardt states that in the first draft of the *Historia*, Leibniz had bordered the Harmonic Triangle, as given here, with a set of fractions, each equal to 1/1, so as to correspond more exactly with the Arithmetical Triangle.

Harmonic Triangle

in which the fundamental series is a harmonical progression;

$$
\begin{array}{ccccccccccccc}
 & & & & & & \frac{1}{1} & & & & & & \\
 & & & & & \frac{1}{2} & & \frac{1}{2} & & & & & \\
 & & & & \frac{1}{3} & & \frac{1}{6} & & \frac{1}{3} & & & & \\
 & & & \frac{1}{4} & & \frac{1}{12} & & \frac{1}{12} & & \frac{1}{4} & & & \\
 & & \frac{1}{5} & & \frac{1}{20} & & \frac{1}{30} & & \frac{1}{20} & & \frac{1}{5} & & \\
 & \frac{1}{6} & & \frac{1}{30} & & \frac{1}{60} & & \frac{1}{60} & & \frac{1}{30} & & \frac{1}{6} & \\
\frac{1}{7} & & \frac{1}{42} & & \frac{1}{105} & & \frac{1}{140} & & \frac{1}{105} & & \frac{1}{42} & & \frac{1}{7}
\end{array}
$$

where, if the denominators of any series descending obliquely to infinity or of any parallel finite series, are each divided by the term that corresponds in the first series, the combinatory numbers are produced, namely those that are contained in the arithmetical triangle. Moreover this property is common to either triangle, namely, that the oblique series are the sum- and difference-series of one another. In the Arithmetical Triangle any given series is the sum-series of the series that immediately precedes it, and the difference-series of the one that follows it; in the Harmonic Triangle, on the other hand, each series is the sum-series of the series following it, and the difference-series of the series that precedes it. From which it follows that

$$\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\frac{1}{5}+\frac{1}{6}+\frac{1}{7}+\text{etc.}=\frac{1}{0}$$

$$\frac{1}{1}+\frac{1}{3}+\frac{1}{6}+\frac{1}{10}+\frac{1}{15}+\frac{1}{21}+\frac{1}{28}+\text{etc.}=\frac{2}{1}$$

$$\frac{1}{1}+\frac{1}{4}+\frac{1}{10}+\frac{1}{20}+\frac{1}{35}+\frac{1}{56}+\frac{1}{84}+\text{etc.}=\frac{3}{2}$$

$$\frac{1}{1}+\frac{1}{5}+\frac{1}{15}+\frac{1}{35}+\frac{1}{70}+\frac{1}{126}+\frac{1}{210}+\text{etc.}=\frac{4}{3}$$

and so on.

Now he had found out these things before he had turned to Cartesian analysis; but when he had had his thoughts directed to this, he considered that any term of a series could in most cases be denoted by some general notation, by which it might be referred to some simple series. For instance, if the general term of the series of natural numbers is denoted by x, then the general term of the series of squares would be x^2, that of the cubes would be x^3, and so on. Any triangular number, such as 0, 1, 3, 6, 10, would be

$$\frac{x.\overline{x+1}}{1.2} \text{ or } \frac{xx+x}{2},$$

any pyramidal number, such as 0, 1, 4, 10, 20, etc., would be

$$\frac{x.\overline{x+1}.\overline{x+2}}{1.2.3} \text{ or } \frac{x^3+3xx+2x}{6},$$ 9 36

and so on. 4, 12, 28, 46

From this it was possible to obtain the difference-series of a given series, and in some cases its sum as well, when it was expressed numerically. For instance, the square is xx, the next greater square is $xx+2x+1$, and the difference of these is $2x+1$; i. e., the series of odd numbers is the difference-series for the series of squares. For, if x is 0, 1, 2, 3, 4, etc., then $2x+1$ is 1, 3, 5, 7, 9. In the same way the difference between x^3 and $x^3+3xx+3x+1$ is $3xx+3x+1$, and thus the latter is the general term of the difference-series for the series of cubes. Further, if the value of the general term can thus be expressed by means of a variable x so that the variable does not enter into a denominator or an exponent, he perceived that he could always find the sum-series of the given series. For instance, to find the sum of the squares, since it is plain that the variable cannot be raised to a higher degree than the cube, he supposed its general term z to be

$$z = lx^3 + mxx + nx, \text{ where } dz \text{ has to be } xx;$$

we have $dz = l\,d(x^3) + m\,d(xx) + n$, (where dx is taken $= 1$); now $d(x^3) = 3xx+3x+1$, and $d(xx) = 2x+1$, as already found; hence

$$dz = 3lxx + 3lx + l + 2mx + m + n \simeq xx;^{105}$$

therefore $l = \frac{1}{3}$, $m = -\frac{1}{2}$, and $\frac{1}{3} - \frac{1}{2} + n = 0$, or $n = \frac{1}{6}$;

[105] The sign here used appears to be an invention of Leibniz to denote an identity, such as is denoted by $\equiv$ at present.

and the general term of the sum-series for the squares is

$$\frac{1}{3}x^3 - \frac{1}{2}xx + \frac{1}{6}x \text{ or } 2x^3 - 3xx + x, : 6.$$ [106]

As an example, if it is desired to find the sum of the first nine or ten squares, i. e., from 1 to 81 or from 1 to 100, take for x the values 10 or 11, the numbers next greater than the root of the last square, and $2x^3 - 3xx + x, : 6$ will be $2000 - 300 + 10, : 6 = 285$, or $2.1331 - 3.121 + 11, : 6 = 385$. Nor is it much more difficult with this formula to sum the first 100 or 1000 squares. The same method holds good for any powers of the natural numbers or for expressions which are made up from such powers, so that it is always possible to sum as many terms as we please of such series by a formula. But our friend saw that it was not always easy to proceed in the same way when the variable entered into the denominator, as it was not always possible to find the sum of a numerical series; however, on following up this same analytical method, he found in general, and published the result in the *Acta Eruditorum,* that a sum-series could always be found, or the matter be reduced to finding the sum of a number of fractional terms such as $1/x$, $1/xx$, $1/x^3$, etc, which at any rate, if the number of terms taken is finite, can be summed, though hardly in a short way (as by a formula); but if it is a question of an infinite number of terms, then terms such as $1/x$ cannot be summed at all, because the total of an infinite number of terms of such a series is an infinite quantity, but that of an infinite number of terms such as $1/xx$, $1/x^3$, etc., make a finite quantity, which nevertheless could not up till now be summed, except by taking quadratures. So, in the year 1682, in the month of February, he noted in the *Acta Eruditorum* that if the numbers 1.3, 3.5, 5.7, 7.9, 9.11, etc., or 3, 15, 35, 63, 99, etc., are taken, and from them is formed the series of fractions

$$\frac{1}{3} + \frac{1}{15} + \frac{1}{35} + \frac{1}{63} + \frac{1}{99} + \text{etc.},$$

then the sum of this series continued to infinity is nothing else but $1/2$; while, if every other fraction is left out, $1/3 + 1/35 + 1/99 + \text{etc.}$

[106] This, and other formulas of the same kind, had been given by Wallis in connection with the formulas for the sums of the figurate numbers. Wallis called these latter sums the "characters" of the series.

expresses the magnitude of a semicircle of which the square on the diameter is represented by 1.[107]

Thus, suppose $x = 1, 2, 3$, etc.[108] Then the general term of

$$\frac{1}{3} + \frac{1}{15} + \frac{1}{35} + \frac{1}{63} + \text{etc. is } \frac{1}{4xx + 8x + 3};$$

it is required to find the general term of the sum-series.

Let us try whether it can have the form $e/(bx + c)$, the reasoning being very simple; then we shall have

$$\frac{e}{bx + c} - \frac{e}{bx + b + c} = \frac{eb}{bbxx + bbx + bc + 2bcx + cc} \backsim \frac{1}{4xx + 8x + 3};$$

hence, equating coefficients in these two formulas, we have

$$b = 2, \; eb = 1, \text{ or } e = \tfrac{1}{2},$$
$$bb + 2bc = 8, \text{ or } 4 + 4c = 8, \text{ or } c = 1;$$

and finally we should have also $bc + cc = 3$, which is the case. Hence the general term of the sum-series is $(1:2)/(2x + 1)$ or $1/(4x + 2)$, and these numbers of the form $4x + 2$ are the doubles of the odd numbers. Finally he gave a method for applying the differential calculus to numerical series when the variable entered into the exponent, as in a geometrical progression, where, taking any radix b the term is b^x, where x stands for a natural number. The terms of the differential series will be $b^{x+1} - b^x$, or $b^x(b-1)$; and from this it is plain that the differential series of the given geometrical series is also a geometrical series proportional to the given series. Thus the sum of a geometrical series may be obtained.

But our young friend quickly observed that the differential calculus could be employed with diagrams in an even more wonderfully simple manner than it was with numbers, because with diagrams the differences were not comparable with the things which differed; and as often as they were connected together by addition or subtraction, being incomparable with one another, the less vanished in comparison with the greater; and thus irrationals could be differentiated no less easily than surds, and also, by the aid of logarithms, so could exponentials. Moreover, he observed that the infinitely small lines occurring in diagrams were nothing else but the

[107] This sentence, in that it breaks the sense from the preceding sentence to the one that follows, would appear to be an interpolated note.

[108] There is an unimportant error here. The first value of x evidently should be 0, and not 1.

momentaneous differences of the variable lines. Also, in the same way as quantities hitherto considered by analytical mathematicians had their functions such as powers and roots, so also such quantities as were variable had new functions, namely, differences. Also, that as hitherto we had x, xx, x^3, etc., y, yy, y^3, etc., so now it was possible to have dx, ddx, d^3x, etc., dy, ddy, d^3y, and so forth. In the same way, that it was possible to express curves, which Descartes had excluded as being "mechanical," by equations of position, and to apply the calculus to them and thus to free the mind from a perpetual reference to diagrams. In the applications of the differential calculus to geometry, differentiations of the first degree were equivalent to nothing else but the finding of tangents, differentiations of the second degree to the finding of osculating circles (the use of which was introduced by our friend); and that it was possible to go on in the same fashion. Nor were these things only of service for tangents and quadratures, but for all kinds of problems and theorems in whch the differences were intermingled with integral terms (as that brilliant mathematician Bernoulli called them), such as are used in physico-mechanical problems.

Thus it follows generally that if any series of numbers or lines of a figure have a property that depends on two, three or more consecutive terms, it can be expressed by an equation involving differences of the first, second, third, or higher degree. Moreover, he discovered general theorems for any degree of the differences, just as we have had theorems of any degree, and he made out the remarkable analogy between powers and differences published in the *Miscellania Berolinensia.*

If his rival had known of these matters, he would not have used dots to denote the degrees of the differences,[109] which are useless for expressing the general degree of the differences, but would have used the symbol d given by our friend or something similar, for then d^e can express the degree of the difference in general. Besides everything which was once referred to figures, can now be expressed by the calculus.

[109] Why not? Newton's dotted letters still form the best notation for a certain type of problem, those which involve equations of motion in which the independent variable is the time, such as central orbits. Probably Leibniz would class the suffix notation as a variation of his own, but the D-operator eclipses them all. For beginners, whether scholastic or historically such (like the mathematicians that Barrow, Leibniz and Newton were endeavoring to teach), the separate letter notation has most to recommend it on the score of ease of comprehension; we find it even now used in partial differential equations.

For $\sqrt{(dxdx+dydy)}$[110] is the element of the arc of a curve, ydx is the element of its area; and from that it is immediately evident that $\int y\,dx$ and $\int x\,dy$ are the complements of one another, since $d(xy) = x\,dy + y\,dx$, or conversely, $xy = \int x\,dy + \int y\,dx$, however these figures vary from time to time; and from this, since $xyz = \int xy\,dz + \int xz\,dy + \int yz\,dx$, three solids are also given that are complementary, every two to the third. Nor is there any need for him to have known those theorems which we deduced above from the characteristic triangle; for example, the moment of a curve about the axis is sufficiently expressed by $\int x\sqrt{(dxdx+dydy)}$. Also what Gregory St. Vincent has concerning *ductus*, what he or

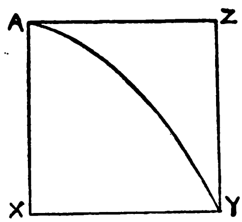


Fig. 5.

Pascal had concerning *ungulae* and *cunei*,[111] every one of these is immediately deduced from a calculus such as this. Thus Leibniz saw with delight those discoveries that he had applauded in others obtained by himself, and thereupon he left off studying them at all closely, because all of them were contained in a calculus such as his.

For example, the moment of the figure AXYA (Fig. 5) about the axis is $\frac{1}{2}\int yy\,dx$, the moment of the figure about the tangent at the vertex is $\int xy\,dx$, the moment of the complementary trilinear figure AZYA about the tangent at the vertex is $\frac{1}{2}\int xx\,dy$. Now these two last moments taken together yield the moment of the circumscribed rectangle AXYZ about the tangent at the vertex, and are complementary to one another.

However, the calculus also shows this without reference to any figure, for $\frac{1}{2}d(xxy) = xy\,dx + \frac{1}{2}xx\,dy$; so that now there is need

[110] Leibniz does not give us an opportunity of seeing how he would have written the equivalent of $dxdxdx$; whether as dx^3 or $\overline{dx}^3$ or $(dx)^3$.

[111] *Ductus* and *ungulae* have already been explained in Notes 28, 29; *cuneus* denotes a wedge-shaped solid; cf. "cuneiform."

for no greater number of the fine theorems of celebrated men for Archimedean geometry, than at most those given by Euclid in his Book II or elsewhere, for ordinary geometry.

It was good to find that thereafter the calculus of transcendent quantities should reduce to ordinary quantities, and Huygens[112] was especially pleased with this. Thus, if it is found that

$$2\int\frac{dy}{y}=3\int\frac{dx}{x},$$

then from this we get $yy=x^3$, and this too from the nature of logarithms combined with the differential calculus, the former also being derived from the same calculus. For let $x^m=y$, then $mx^{m-1}\,dx=dy$. Hence, dividing each side by equal things, we have

$$m\frac{dx}{x}=\frac{dy}{y}.$$

Again, from the equation, $m\log x=\log y$, we have

$$\log x:\log y=\int\frac{dx}{x}:\int\frac{dy}{y}.$$ [113]

By this the exponential calculus is rendered practicable as well. For let $y^x=z$, then $x\log y=\log z$, $dx\log y+x\,dy:y=dz:z$.

In this way we free the exponents from the variable, or at other times we may transpose the variable exponent with advantage under the circumstances. Lastly, those things that were once held in high esteem are thus made a mere child's-play.

Now of all this calculus not the slightest trace existed in all the writings of his rival before the principles of the calculus were

[112] This is peculiar. The demonstration that follows was beyond the powers of Leibniz in June, 1676 (see pp. 121, 122), probably so until Nov., 1676, when he was in Holland, and possibly later still. Hence the result would have been communicated to Huygens by letter, and there would be an answer from Huygens. I have been so far unable to find such a letter.

[113] This only proves the proportionality, enabling Leibniz to convert the equation $2\int dy/y=3\int dx/x$ into $2\log y=3\log x$. It will hardly suffice as it stands to enable him to deal with such an equation as $2\int dy/y=3\int x\,dx$; and it is to be noted that Leibniz does not notice at all the constant of integration. Although Barrow has in effect differentiated (and therefore also has the inverse integral theorems corresponding thereto) both a logarithm and an exponential in Lecture XII, App. III, Prob. 3, 4, yet these problems are in such an ambiguous form that it may be doubted whether Barrow was himself quite clear on what he had obtained. Hence this clear statement of Leibniz must be considered as a great advance on Barrow.

published by our friend;[114] nor indeed anything at all that Huygens or Barrow had not accomplished in the same way, in the cases where they dealt with the same problems.

But how great was the extent of the assistance afforded by the use of this calculus was candidly acknowledged by Huygens; and this his opponents suppress as much as ever they can, and straightway go on with other matters, not mentioning the real differential calculus in the whole of their report. Instead, they adhere to a large extent to infinite series, the method for which no one denies that his rival brought out in advance of all others. For those things which he said enigmatically, and explained at a much later date, are all they talk about, namely, fluxions and fluents, i. e., finite quantities and their infinitely small elements; but as to how one can be derived from the other they offer not the slightest suggestion. Moreover, while he considers nascent or evanescent ratios, leading straight away from the differential calculus to the method of exhaustions, which is widely different from it (although it certainly also has its own uses), he proceeds not by means of the infinitely small, but by ordinary quantities, though these latter do finally become the former.

Since therefore his opponents, neither from the *Commercium Epistolicum* that they have published, nor from any other source, brought forward the slightest bit of evidence whereby it might be established that his rival used the differential calculus before it was published by our friend; therefore all the accusations that were brought against him by these persons may be treated with contempt as beside the question. They have used the dodge of the pettifogging advocate[115] to divert the attention of the judges from the matter on trial to other things, namely to infinite series. But even in these they could bring forward nothing that could impugn the honesty of our friend, for he plainly acknowledged the manner in which he had made progress in them; and in truth in these also, he finally attained to something higher and more general.

[114] Almost seems to read as a counter-charge against Newton of stealing Leibniz's calculus. Note the tardy acknowledgement that Barrow has previously done all that Newton had given.

[115] The whole effect that this *Historia* produces in my mind is that the entire thing is calculated to the same end as the *Commercium Epistolicum*. The pity of it is that Leibniz could have told such a straightforward tale, if events had been related in strict *chronological* order, without any interpolations of results that were derived, or notation that was perfected, later. A tale so told would have proved once and for all how baseless were the accusations of the *Commercium*, and largely explained his denial of any obligations to Barrow.

SUPPLEMENT.

Barrow, *Lectiones Geometricae,* Lect. XII, Prop. 1, 2, 3.

[Page 105, First Edition, 1670.]

General foreword. We will now proceed with the matter in hand; and, in order that we may save time and words, it is to be observed everywhere in what now follows that AB is some curved line, such as we shall draw, of which the axis is AD; to this axis all the straight lines BD, CA, MF, NG are applied perpendicular; the arc MN is indefinitely small; the straight line $\alpha\beta =$ arc AB, the straight line $\alpha\mu =$ arc AM, and $\mu\nu =$ arc MN; also lines applied to $\alpha\beta$ are perpendicular to it. On this understanding:

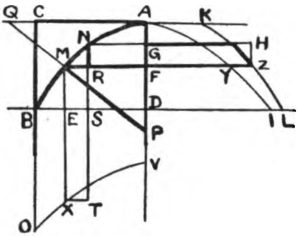


Fig. 6.

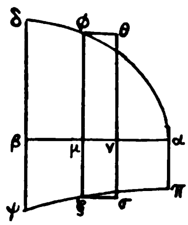


Fig. 7.

1. Let MP be perpendicular to the curve AB, and the lines KZL, $\alpha\phi\delta$ such that $FZ = MP$, $\mu\phi = MF$. Then the spaces $\alpha\beta\delta$, ADLK are equal.

For the triangles MRN, PFM *are similar,* $MN : NR = PM : MF$,

$$MN . MF = NR . PM;$$

that is, on substituting the equal quantities,

$$\mu\nu . \mu\phi = FG . FZ, \text{ or rect. } \mu\theta = \text{rect. } FH.$$

But the space $\alpha\beta\delta$ only differs in the slightest degree from an infinite number of rectangles such as $\mu\theta$, and the space ADLK is equivalent to an equal number of rectangles such as FH. Hence the proposition follows.

2. Hence, *if the curve* AMB *is rotated about the axis* AD, *the ratio of the surface produced to the space* ADLK *is that of the circumference of a circle to its diameter*; whence if the space ADLK is known, the said surface is known.

Some time ago I assigned the reason why this was so.

3. Hence, *the surfaces of the sphere, both the spheroids, and the conoids receive measurement.* For if AD is the axis of the conic section, etc.

NOTE. In the above figure, I have "lined in" the part of the diagram (which serves for about ten theorems) especially used for the first two theorems. If this is compared with Leibniz's figure on p. 15, further comment is needless.