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Gottfried Wilhelm Leibnitz

Gottfried Wilhelm Leibnitz (or Leibniz) was born at Leipzig on June 21 (O.S.), 1646, and died in Hanover on November 14, 1716. His father died before he was six, and the teaching at the school to which he was then sent was inefficient, but his industry triumphed over all difficulties; by the time he was twelve he had taught himself to read Latin easily, and had begun Greek; and before he was twenty he had mastered the ordinary text-books on mathematics, philosophy, theology and law. Refused the degree of doctor of laws at Leipzig by those who were jealous of his youth and learning, he moved to Nuremberg. An essay which there wrote on the study of law was dedicated to the Elector of Mainz, and led to his appointment by the elector on a commission for the revision of some statutes, from which he was subsequently promoted to the diplomatic service. In the latter capacity he supported (unsuccessfully) the claims of the German candidate for the crown of Poland. The violent seizure of various small places in Alsace in 1670 excited universal alarm in Germany as to the designs of Louis XIV.; and Leibnitz drew up a scheme by which it was proposed to offer German co-operation, if France liked to take Egypt, and use the possessions of that country as a basis for attack against Holland in Asia, provided France would agree to leave Germany undisturbed. This bears a curious resemblance to the similar plan by which Napoleon I. proposed to attack England. In 1672 Leibnitz went to Paris on the invitation of the French government to explain the details of the scheme, but nothing came of it.

At Paris he met Huygens who was then residing there, and their conversation led Leibnitz to study geometry, which he described as opening a new world to him; though as a matter of fact he had previously written some tracts on various minor points in mathematics, the most important being a paper on combinations written in 1668, and a description of a new calculating machine. In January, 1673, he was sent on a political mission to London, where he stopped some months and made the acquaintance of Oldenburg, Collins, and others; it was at this time that he communicated the memoir to the Royal Society in which he was found to have been forestalled by Mouton.

In 1673 the Elector of Mainz died, and in the following year Leibnitz entered the service of the Brunswick family; in 1676 he again visited London, and then moved to

Hanover, where, till his death, he occupied the well-paid post of librarian in the ducal library. His pen was thenceforth employed in all the political matters which affected the Hanoverian family, and his services were recognized by honours and distinctions of various kinds, his memoranda on the various political, historical, and theological questions which concerned the dynasty during the forty years from 1673 to 1713 form a valuable contribution to the history of that time.

Leibnitz's appointment in the Hanoverian service gave him more time for his favourite pursuits. He used to assert that as the first-fruit of his increased leisure, he invented the differential and integral calculus in 1674, but the earliest traces of the use of it in his extant note-books do not occur till 1675, and it was not till 1677 that we find it developed into a consistent system; it was not published till 1684. Most of his mathematical papers were produced within the ten years from 1682 to 1692, and many of them in a journal, called the Acta Eruditorum, founded by himself and Otto Mencke in 1682, which had a wide circulation on the continent.

Leibnitz occupies at least as large a place in the history of philosophy as he does in the history of mathematics. Most of his philosophical writings were composed in the last twenty or twenty-five years of his life; and the points as to whether his views were original or whether they were appropriated from Spinoza, whom he visited in 1676, is still in question among philosophers, though the evidence seems to point to the originality of Leibnitz. As to Leibnitz's system on philosophy it will be enough to say that he regarded the ultimate elements of the universe as individual percipient beings whom he called monads. According to him the monads are centres of force, and substance is force, while space, matter, and motion are merely phenomenal; finally, the existence of God is inferred from the existing harmony among the monads. His services to literature were almost as considerable as those to philosophy; in particular, I may single out his overthrow of the then prevalent belief that Hebrew was the primeval language of the human race.

In 1700 the academy of Berlin was created on his advice, and he drew up the first body of statutes for it. On the accession in 1714 of his master, George I., to the throne of England, Leibnitz was thrown aside as a useless tool; he was forbidden to come to England; and the last two years of his life were spent in neglect and dishonour. He died at Hanover in 1716. He was overfond of money and personal distinctions; was unscrupulous, as perhaps might be expected of a professional diplomatist of that time; but possessed singularly attractive manners, and all who once came under the charm of his personal presence remained sincerely attached to him. His mathematical reputation was largely augmented by the eminent position that he occupied in diplomacy, philosophy and literature; and the power thence derived was considerably increased by his influence in the management of the Acta Eruditorum.

The last years of his life - from 1709 to 1716 - were embittered by the long controversy with John Keill, Newton, and others, as to whether he had discovered the differential calculus independently of Newton's previous investigations, or whether he had derived the fundamental idea from Newton, and merely invented another notation for it. The controversy occupies a place in the scientific history of the early years of the eighteenth century quite disproportionate to its true importance, but it so materially affected the history of mathematics in western Europe, that I feel obliged to give the leading facts, though I am reluctant to take up so much space with questions of a personal character.

The ideas of the infinitesimal calculus can be expressed either in the notation of fluxions or in that of differentials. The former was used by Newton in 1666, but no distinct account of it was printed till 1693. The earliest use of the latter in the note-books of Leibnitz may probably be referred to 1675, it was employed in the letter sent to Newton in 1677, and an account of it was printed in the memoir of 1684 described below. There is no question that the differential notation is due to Leibnitz, and the sole question is as to whether the general idea of the calculus was taken from Newton or discovered independently.

The case in favour of the independent invention by Leibnitz rests on the ground that he published a description of his method some years before Newton printed anything on fluxions, that he always alluded to the discovery as being his own invention, and that for some years this statement was unchallenged; while of course there must be a strong presumption that he acted in good faith. To rebut this case it is necessary to shew (i) that he saw some of Newton's papers on the subject in or before 1675, or at least 1677, and (ii) that he thence derived the fundamental ideas of the calculus. The fact that his claim was unchallenged for some years is, in the particular circumstances of the case, immaterial.

That Leibnitz saw some of Newton's manuscripts was always intrinsically probable; but when, in 1849, C. J. Gerhardt examined Leibnitz's papers he found among them a manuscript copy, the existence of which had been previously unsuspected, in Leibnitz's handwriting, of extracts from Newton's De Analysi per Equationes Numero Terminorum Infinitas (which was printed in the De Quadratura Curvarum in 1704), together with the notes on their expression in the differential notation. The question of the date at which these extracts were made is therefore all important. It is known that a copy of Newton's manuscript had been sent to

Tschirnhausen in May, 1675, and as in that year he and Leibnitz were engaged together on a piece of work, it is not impossible that these extracts were made then. It is also possible that they may have been made in 1676, for Leibnitz discussed the question of analysis by infinite series with Collins and Oldenburg in that year, and it is a priori probable that they would have then shewn him the manuscript of Newton on that subject, a copy of which was possessed by one or both of them. On the other hand it may be supposed that Leibnitz made the extracts from the printed copy in or after 1704. Leibnitz shortly before his death admitted in a letter to Conti that in 1676 Collins had shewn him some Newtonian papers, but implied that they were of little or no value, - presumably he referred to Newton's letters of June 13 and Oct. 24, 1676, and to the letter of Dec. 10, 1672, on the method of tangents, extracts from which accompanied the letter of June 13, - but it is remarkable that, on the receipt of these letters, Leibnitz should have made no further inquiries, unless he was already aware from other sources of the method followed by Newton.

Whether Leibnitz made no use of the manuscript from which he had copied extracts, or whether he had previously invented the calculus, are questions on which at this distance of time no direct evidence is available. It is, however, worth noting that the unpublished Portsmouth Papers shew that when, in 1711, Newton went carefully into the whole dispute, he picked out this manuscript as the one which had probably somehow fallen into the hands of Leibnitz. At that time there was no direct evidence that Leibnitz had seen this manuscript before it was printed in 1704, and accordingly Newton's conjecture was not published; but Gerhardt's discovery of the copy made by Leibnitz tends to confirm the accuracy of Newton's judgement in the matter. It is said by those who question Leibnitz's good faith that to a man of his ability the manuscript, especially if supplemented by the letter of Dec. 10, 1672, would supply sufficient hints to give him a clue as to the methods of the calculus, though as the fluxional notation is not employed in it anyone who used it would have to invent a notation; but this is denied by others.

There was at first no reason to suspect the good faith of Leibnitz; and it was not until the appearance in 1704 of an anonymous review of Newton's tract on quadrature, in which it was implied that Newton had borrowed the idea of the fluxional calculus from Leibnitz, that any responsible mathematician questioned the statement that Leibnitz had invented the calculus independently of Newton. (In 1699 Duillier had accused Leibnitz of plagiarism from Newton, but Dullier was not a person of much importance) It is universally admitted that there was no justification or authority for the statements made in this review, which was rightly attributed to Leibnitz. But the subsequent discussion led to a critical examination of the whole question, and doubt was expressed as to whether Leibnitz had not derived the fundamental idea from Newton. The case against Leibnitz as it appeared to Newton's friends was summed up in the Commercium Epistolicum issued in 1712, and detailed references are given for all the facts mentioned.

No such summary (with facts, dates, and references) of the case for Leibnitz was issued by his friends; but John Bernoulli attempted to indirectly weaken the evidence by attacking the personal character of Newton; this was in a letter dated June 7, 1713. The charges were false, and when pressed for an explanation of them, Bernoulli most solemnly denied having written the letter. In accepting the denial Newton added in a private letter to him the following remarks, which are interesting as giving Newton's account of why he was at last induced to take any part in the controversy. "I have never," said he, "grasped at fame among foreign nations, but I am very desirous to preserve my character for honesty, which the author of that epistle, as if by the authority of a great judge, had endeavoured to wrest from me. Now that I am old, I have little pleasure in mathematical studies, and I have never tried to propagate my opinions over the world, but I have rather taken care not to involve myself in disputes on account of them."

Leibnitz's defence or explanation of his silence is given in the following letter, dated April 9, 1716, from him to Conti. "Pour répondre de point en point à l'ouvrage publié contre moi, il falloit entrer dans un grand détail de quantité de minutiés passées il y a trente à quarante ans, dont je ne me souvenois guère: il me falloit chercher mes vieilles lettres, dont plusiers se sont perdus, outre que le plus souvent je n'ai point gardé les minutes des miennes: et les autres sont ensevelies dans un grand tas de papiers, que je ne pouvois débrouiller qu'avec du temps et de la patience; mais je n'en avois guère le loisir, étant chargé présentement d'occupations d'une toute autre nature."

The death of Leibnitz in 1716 only put a temporary stop to the controversy which was bitterly debated for many years later. The question is one of difficulty; the evidence is conflicting and circumstantial; and every one must judge for himself which opinion seems most reasonable. Essentially it is a case of Leibnitz's word against a number of suspicious details pointing against him. His unacknowledged possession of a copy of part of one of Newton's manuscripts may be explicable; but the fact that on more than one occasion he deliberately altered or added to important documents (ex. gr. the letter of June 7, 1713, in the Charta Volans, and that of April 8, 1716, in the Acta Eruditorum), before publishing them, and, what is worse, that a material date in one of his manuscripts has been falsified (1675 being altered to 1673), makes his own testimony on the subject of little value. It must be recollected that what he is alleged to have received was rather a number of suggestions than an account of the calculus;

and it is possible that as he did not publish his results of 1677 until 1684, and that as the notation and subsequent development of it were all of his own invention, he may have been led, thirty years later, to minimize any assistance which he had obtained originally, and finally to consider that it was immaterial. During the eighteenth century the prevalent opinion was against Leibnitz, but to-day the majority of writers incline to think it more likely that the inventions were independent.

If we must confine ourselves to one system of notation then there can be little doubt that that which was invented by Leibnitz is better fitted for most of the purposed to which the infinitesimal calculus is applied than that of fluxions, and for some (such as the calculus of variations) it is indeed almost essential. It should be remembered, however, that at the beginning of the eighteenth century the methods of the infinitesimal calculus had not been systematized, and either notation was equally good. The development of that calculus was the main work of the mathematicians of the first half of the eighteenth century. The differential form was adopted by continental mathematicians. The application of it by Euler, Lagrange, and Laplace to the principles of mechanics laid down in the Principia was the great achievement of the last half of that century, and finally demonstrated the superiority of the differential to the fluxional calculus. The translation of the Principia into the language of modern analysis, and the filling in of the details of the Newtonian theory by the aid of that analysis, were effected by Laplace.

The controversy with Leibnitz was regarded in England as an attempt by foreigners to defraud Newton of the credit of his invention, and the question was complicated on both sides by national jealousies. It was therefore natural, though it was unfortunate, that in England the geometrical and fluxional methods as used by Newton were alone studied and employed. For more than a century the English school was thus out of touch with continental mathematicians. The consequence was that, in spite of the brilliant band of scholars formed by Newton, the improvements in the methods of analysis gradually effected on the continent were almost unknown in Britain. It was not until 1820 that the value of analytical tools was fully recognized in England, and that Newton's countrymen again took any large share in the development of mathematics.

Leaving now this long controversy I come to the discussion of the mathematical papers produced by Leibnitz, all the more important of which were published in the Acta Eruditorum. They are mainly concerned with various questions on mechanics.

The only papers of first-rate importance which he produced are those on the differential calculus. The earliest of these was one published in the Acta Eruditorum for October, 1684, in which he enunciated a general method for finding maxima and minima, and for drawing tangents to curves. One inverse problem, namely, to find the curve whose subtangent is constant, was also discussed. The notation is the same as that with which we are familiar, and the differential coefficients of xn and of products and quotients are determined. In 1686 he wrote a paper on the principles of the new calculus. In both of these papers the principle of continuity is explicitly assumed, while his treatment of the subject is based on the use of infinitesimals and not on that of the limiting value of ratios. In answer to some objections which were raised in 1694 by Bernard Nieuwentyt, who asserted that dy/dx stood for an unmeaning quantity like 0/0, Leibnitz explained, in the same way that Barrow had previously done, that the value of dy/dx in geometry could be expressed as the ratio of two finite quantities. I think that Leibnitz's statement of the objects and methods of the infinitesimal calculus as contained in these papers, which are the three most important memoirs on it that he produced, is somewhat obscure, and his attempt to place the subject on a metaphysical basis did not tend to clearness; but the fact that all the results of modern mathematics are expressed in the language invented by Leibnitz has proved the best monument of his work. Like Newton, he treated integration not only as a summation, but as the inverse of differentiation.

In 1686 and 1692 he wrote papers on osculating curves. These, however, contain some bad blunders, as, for example, the assertion that an osculating circle will necessarily cut a curve in four consecutive points: this error was pointed out by John Bernoulli, but in his article of 1692 Leibnitz defended his original assertion, and insisted that a circle could never cross a curve where it touched it.

In 1692 Leibnitz wrote a memoir in which he laid the foundation of the theory of envelopes. This was further developed in another paper in 1694, in which he introduced for the first time the terms "co-ordinates" and "axes of co-ordinates."

Leibnitz also published a good many papers on mechanical subjects; but some of them contain mistakes which shew that he did not understand the principles of the subject. Thus, in 1685, he wrote a memoir to find the pressure exerted by a sphere of weight W placed between two inclined planes of complementary inclinations, placed so that the lines of greatest slope are perpendicular to the line of the intersection of the planes. He asserted that the pressure on each plane must consist of two components, "unum quo decliviter descendere tendit, alterum quo planum declive premit." He further said that for metaphysical reasons the sum of the two pressures must be equal to W. Hence, if R and R' be the required pressures, and □ and 1/2π - □ the inclinations of the planes, he finds that

$$R = 1/2\ W(1 - \sin \square + \cos \square)$$

and

$$R\square = 1/2\ W(1 - \cos \square + \sin \square).$$

The true values are R = W cos □ and R’ = W sin □. Nevertheless some of his papers on mechanics are valuable. Of these the most important were two, in 1689 and 1694, in which he solved the problem of finding and isochronous curve; one, in 1697, on the curve of quickest descent (this was the problem sent as a challenge to Newton); and two, in 1691 and 1692, in which he stated the intrinsic equation of the curve assumed by a flexible rope suspended from two points, that is, the catenary, but gave no proof. This last problem had been originally proposed by Galileo.

In 1689, that is, two years after the Principia had been published, he wrote on the movements of the planets which he stated were produced by a motion of the ether. Not only were the equations of motion which he obtained wrong, but his deductions from them were not even in accordance with his own axioms. In another memoir in 1706, that is, nearly twenty years after the Principia had been written, he admitted that he had made some mistakes in his former paper, but adhered to his previous conclusions, and summed the matter up by saying “it is certain that gravitation generates a new force at each instant to the centre, but the centrifugal force also generates another away from the centre.... The centrifugal force may be considered in two aspects according as the movement is treated as along the tangent to the curve or as along the arc of the circle itself.” It seems clear from this paper that he did not really understand the principles of dynamics, and it is hardly necessary to consider his work on the subject in further detail. Much of it is vitiated by a constant confusion between momentum and kinetic energy: when the force is “passive” he uses the first, which he calls the vis mortua, as the measure of a force; when the force is “active” he uses the latter, the double of which he calls the vis viva.

The series quoted by Leibnitz comprise those for ex, log (1 + x), sin x, vers x and tan-1x; all these had been previously published, and he rarely, if ever, added any demonstrations. Leibnitz (like Newton) recognised the importance of James Gregory’s remarks on the necessity of examining whether infinite series are convergent or divergent, and proposed a test to distinguish series whose terms are alternately positive and negative. In 1693 he explained the method of expansion by indeterminate coefficients, though his applications were not free from error.

To sum the matter up briefly, it seems to me that Leibnitz’s work exhibits great skill in analysis, but much of it is unfinished, and when he leaves his symbols and attempts to interpret his results he frequently commits blunders. No doubt the demands of politics, philosophy, and literature on his time may have prevented him from elaborating any problem completely or writing a systematic exposition of his views, though they are no excuse for the mistakes of principle which occur in his papers. Some of his memoirs contain suggestions of methods which have now become valuable means of analysis, such as the use of determinants and of indeterminate coefficients; but when a writer of manifold interests like Leibnitz throws out innumerable suggestions, some of them are likely to turn out valuable, and to enumerate these (which he did not work out) without reckoning the others (which are wrong) gives a false impression of the value of his work. But in spite of this, his title to fame rests on a sure basis, for by his advocacy of the differential calculus his name is inseparably connected with one of the chief instruments of analysis, as that of Descartes - another philosopher - is similarly connected with analytical geometry.