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Vorlesungen über Geschichte der Mathematik, von Moritz Cantor. Dritter (Schluss) Band.

Dritte Abtheilung, 1727-1758.

A REVIEW: with special reference to the *Analyst* Controversy.

By Professor GEORGE A. GIBSON.

With the issue of the Third Part of the Third Volume, Mr Cantor completes his History of Mathematics, in accordance with the plan he sketched out for himself when he undertook the work. That the labour involved in collecting material and in reducing it to shape would be great, Mr Cantor doubtless well knew; but in all probability his most liberal estimate of the demands likely to be made upon his energies has been far exceeded; in any case, one can readily understand the feelings of satisfaction with which he writes the preface to the concluding volume.

It hardly requires to be stated that this history is certain to remain for many years the standard work on the subject with which it deals; in completeness, in accuracy, in clearness of arrangement, it stands unrivalled, and for the period which it covers is bound to be a permanent work of reference. Even the year (1758) with which the history closes, though doubtless somewhat arbitrary, is yet more appropriate than appears at first sight. The most characteristic achievement in pure mathematics that has to be recorded during the period covered by the third volume (1668-1758) is doubtless the introduction of the Infinitesimal Calculus; but in spite of the great impetus that the work of Newton, Leibniz, and the Bernoullis gave to mathematical discovery, there was for many years after the publication of the first discoveries considerable uncertainty as to the philosophical basis on which the Calculus was to be built. The great variety and importance of the results to which the new method led pushed into the background the necessity for a thorough investigation of the fundamental principles of that method; when the method was seriously called in question, appeal was usually made to the principles so clearly expounded by Archimedes. Whether the Calculus

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was to be defended from the Newtonian or the Leibnizian standpoint, it was the usual practice to try and prove its consistency with the ancient geometry. Sometimes, indeed, the correctness of the principle was deduced from the accuracy of the conclusions—more particularly in the case of infinite series; but obviously such a defence as this would break down the moment it was seriously examined by any competent logician.

Now, in the period to which the concluding part of Mr Cantor's third volume is devoted, the basis of the Infinitesimal Calculus was subjected to a most searching examination, with the result, it seems to me, of placing the Calculus on as secure a foundation as the ancient geometry. It is not altogether inappropriate, therefore, that Mr Cantor's work should close when such a definite result had been fairly gained. I do not suppose, however, that it was this consideration that influenced Mr Cantor in his determination of the year to which he should bring down his history, but it was rather the practical difficulty of reducing to order the enormous detail which almost seems to overwhelm the historian of the last hundred and fifty years.

Whatever be the reason for stopping at the particular year adopted, there can be no doubt that this history is a boon to mathematicians that can hardly be overestimated; and the fact that the first volume is already in a second edition must be to Mr Cantor a most gratifying evidence that his labours have been appreciated. There can, besides, be little doubt that his work has very much widened the circle of those who are interested in historical inquiries, and has given a great impetus to historical research.

In directing the attention of the Society to the concluding part of this history, I propose to adopt the same method that I followed on a previous occasion,* and confine myself chiefly to one definite issue, namely, the controversy called forth by the publication of Berkeley's *Analyst*. In taking this course I am influenced, partly by the consideration that criticism of isolated details is in itself less suited to the ends our Society has in view, but mainly because Mr Cantor's account of that controversy is very meagre, and not commensurate, as I think, with the place it occupies in the development of mathematical thought. From a passage on p. 718, it would seem that Mr Cantor has no first-hand acquaintance with the numerous

* *Proc. Edin. Math. Soc.*, Vol. XIV.

rejoinders which the *Analyst* called forth, except what may be found in Berkeley's *Works* and in Maclaurin's *Fluxions*; nor need one be much surprised at this ignorance of the writings of Philalethes Cantabrigiensis, since these are only to be found (so far as I am aware) in extremely rare journals. It is unfortunate, however, that the much more valuable contributions of Benjamin Robins were not known to Mr Cantor, as these would have shown that the state of mathematical learning in England in the years immediately subsequent to the death of Newton was by no means so low as the incompetence of Philalethes would seem to indicate. Robins gave a complete and masterly defence of fluxions, and, in particular, laid down in clear and unambiguous form the doctrine of limits as the basis of the Infinitesimal Calculus. The work of Robins is not so inaccessible as that of Philalethes, since all his important contributions are included in his *Mathematical Tracts,* published, under the editorship of James Wilson, M.D., at London in 1761. To Robins, more than to Maclaurin, I think, is due the credit of expounding in systematic and consistent form the fundamental conception of a limit, and of freeing Newton's statements from the ambiguities which gave plausibility to Berkeley's attack. As this controversy is of real importance in mathematical history, and as it seems to be very imperfectly known, I hope it may be of some interest to relate it with as much detail as the space at my disposal allows.

The facts of Berkeley's life are sufficiently well known to justify me in proceeding at once to state the origin of his famous essay, entitled—*The Analyst; or a Discourse addressed to an Infidel Mathematician* (London, 1734). *The Analyst* was addressed to Dr Halley, and begins with these words:— "Though I am a stranger to your person, yet am I not, sir, a stranger to the reputation you have acquired in that branch of learning which hath been your peculiar study; nor to the authority that you therefore assume in things foreign to your profession; nor to the abuse that you, and too many more of the like character, are known to make of such undue authority, to the misleading of unwary persons in matters of the highest concernment, and whereof your mathematical knowledge can by no means qualify you to be a competent judge." * This introductory sentence indicates what the rest of Berkeley's contributions to the controversy abundantly confirms—that his object in writing the *Discourse* was not so much to assail the

* Berkeley's *Works* (Fraser's Edition), Vol. III., 257–8.

conclusions reached by the new analysis, as to call in question the claim of mathematicians *as such* to be regarded as authorities in matters of religious faith. He does not charge mathematicians as a body with infidelity; but, seeing that mathematical truth is usually considered to be the most logically demonstrated of all truths, he tries to destroy the presumption that the opinions of mathematicians in matters of faith are likely to be more logical and trustworthy than those of divines, by contending that in the much-vaunted fluxional calculus there are mysteries that are accepted without question by mathematicians, but that are at the same time not capable of logical demonstration. He lays special stress on the fact that it is the logic and not the conclusions of the mathematicians that he assails; thus * "I beg leave to repeat and insist that I consider the geometrical analyst as a logician, *i.e.*, so far forth as he reasons and argues; and his mathematical conclusions, not in themselves, but in their premises; not as true or false, useful or insignificant, but as derived from such principles and by such inferences."

The charge, of which so much was made in the first reply of Philalethes, that Berkeley had accused mathematicians, as a body, of infidelity, is totally false and need not further be referred to.

There is, however, another point of view from which Berkeley's *Discourse* has been regarded that may be here briefly referred to. De Morgan † considers it to have been a publication involving the principle of Dr Whately's argument against the existence of Bonaparte. "The Analyst," he says, "is a tract which could not have been written except by a person who knew how to answer it. But it is singular that Berkeley, though he makes his fictitious character nearly as clear as afterwards did Whately, has generally been treated as a real opponent of fluxions. Let us hope that the arch Archbishop will fare better than the arch Bishop." I confess that I have some difficulty in adopting this opinion of De Morgan's, though there is more to be said in its favour than appears at a first view, and it would explain some points in the later developments of the controversy—as, for example, the absence of any reply to Robins. On the whole, however, I consider De Morgan's hypothesis more ingenious than sound.

Berkeley's first argument is directed against the conceiv-

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* *Works*, III., p. 270.

† *Phil. Mag.* (S. 4) 1852, IV, 329, note.

ability of fluxions. Quoting from the introduction to the *Quadratura Curvarum*, he says, "a method hath been found to determine quantities from the velocities of their generating motions. And such velocities are called fluxions; and the quantities generated are called flowing quantities. These fluxions are said to be nearly as the increments of the flowing quantities, generated in the least equal particles of time; and to be accurately in the first proportion of the nascent, or in the last of the evanescent increments. Sometimes, instead of velocities, the momentaneous increments or decrements of undetermined flowing quantities are considered, under the appellation of moments. By moments we are not to understand finite particles. These are said not to be moments, but quantities generated from moments, which last are only the nascent principles of finite quantities. It is said that the minutest errors are not to be neglected in mathematics; that the fluxions are celerities not proportional to the finite increments, though ever so small; but only to the moments or nascent increments, whereof the proportion alone, and not the magnitude, is considered. And of the aforesaid fluxions there be other fluxions, which fluxions of fluxions are called second fluxions. And the fluxions of these second fluxions are called third fluxions; and so on."

The description of *moments* in the above passage is not contained in the *Quadratura,* but is taken from the second Lemma of the second book of the *Principia*; this point should be specially noted, as one object of the introduction to the *Quadratura* is to show that the doctrine of fluxions is independent of infinitesimals, and the use of moments *there* would have endangered the contention. Whether Newton's exposition is really independent of the principle of moments is, of course, a different question.

Now Berkeley objects that the imagination is very much strained and puzzled to frame clear ideas of the least particles of time, or the least increments generated therein; and much more so to comprehend the moments, or those increments of the flowing quantities in *statu nascenti,* in their very first origin or beginning to exist, before they become finite particles. "And it seems much more difficult to conceive the abstracted velocities of such nascent imperfect entities. But the velocities of the velocities—the second, third, fourth, fifth velocities, &c.—exceed, if I mistake not, all human understanding. Certainly, in any sense, a second or third fluxion seems an obscure mystery. The incipient celerity of an incipient celerity, the nascent augment of a nascent augment, *i.e.*, of a thing which hath no magnitude —take it in what light you please, the clear conception of it will, if I mistake not, be found impossible; whether it be so or not, I appeal to the trial of every thinking reader."

Berkeley then makes similar observations on the differentials of the foreign mathematicians, and closes this part of his argument with the assertion that if we penetrate beneath the symbols we shall discover much emptiness, darkness, and confusion, even direct impossibilities and contradictions.

Berkeley's contention here is practically that it is nonsense to speak of velocities as being proportional to moments, since moments are not finite quantities, and therefore a ratio of moments is a ratio of things that have no magnitude. Or, in different language, if the increment of space δx be generated in time δt, then δx and δt are not moments so long as they are finite; the fluxion of x is the ratio of δx to δt when these are no longer finite; but if they are not finite, then there are no magnitudes which can bear a ratio to each other. Hence even a first fluxion, as thus defined, has no real meaning.

It goes without saying that if Berkeley has correctly interpreted Newton, his criticism is just; but that criticism rests on a false interpretation of Newton's conception of prime and ultimate ratios, as will be seen later.

Berkeley then examines the proof for finding the fluxion of a rectangle AB given in the *Principia*, Book II., Lemma 2. When the sides A, B are deficient by half their moments $\frac{1}{2}a$, $\frac{1}{2}b$ respectively, the rectangle is

$$AB - \tfrac{1}{2}bA - \tfrac{1}{2}aB + \tfrac{1}{4}ab;$$

when the sides are greater by half their moments the rectangle is

$$AB + \tfrac{1}{2}bA + \tfrac{1}{2}aB + \tfrac{1}{4}ab.$$

If the former rectangle be subtracted from the latter, there remains $bA + aB$, and this is the increment of AB generated by the entire increments a, b of the sides.

The objection is urged that the increment is really

$$(A + a)(B + b) - AB, \textit{ i.e., } bA + aB + ab,$$

and therefore the term ab has been illegitimately neglected, contrary to Newton's own principles that even the smallest errors are not to be neglected in mathematics. To emphasise the difficulty of getting rid of the term ab by legitimate methods, Berkeley next examines the proof in the *Quadratura* of the fluxion of x^n. His criticism is based on the following lemma:—

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"If, with a view to demonstrate any proposition, a certain point is supposed, by virtue of which certain other points are attained; and such supposed point be itself afterwards destroyed or rejected by a contrary supposition; in that case, all the other points attained thereby, and consequent thereupon, must also be destroyed and rejected, so as from thenceforward to be no more supposed or applied in the demonstration."

To find the fluxion of x^n, suppose x by flowing to become $x+o$, then x^n will become

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

The increments of x and of x^n are therefore to each other

$$\text{as } o \text{ to } nox^{n-1} + \frac{n(n-1)}{2}o^2x^{n-2} + \text{ etc.},$$

$$\text{or as 1 to } nx^{n-1} + \frac{n(n-1)}{2}ox^{n-2} + \text{ etc.}$$

Let now the increments vanish, and their last proportion is 1 to nx^{n-1}, so that the fluxion of x is to that of x^n as 1 to nx^{n-1}.

Berkeley objects that this reasoning is not fair or conclusive because the supposition that the increments vanish, destroys the former supposition that there were increments, and therefore, we must suppose that everything derived from the supposition of their existence should vanish with them. He dilates at considerable length on the want of logic shown in this reasoning, and then proceeds to show that when conclusions, known otherwise to be sound, are obtained by this method, the logic is faulty all the same, as the right conclusion is reached by compensation of errors. The examples he takes are drawn from the finding of tangents by the use of differentials, and his criticism at this part is aimed rather at Leibniz than Newton. These examples are reproduced by Mr Cantor, and I will pass them over with the remark that they are of more than usual interest in view of subsequent attempts to establish the calculus on the principle of compensation.

The *Analyst* is a lengthy pamphlet and contains much acute criticism of mathematical methods; but its contentions are in essence these two :—(1) the conception of fluxions is unintelligible, inasmuch as they suppose the ratio of quantities that have no magnitude, for, in Berkeley's view, prime and ultimate ratios are such; (2) the demonstration of the value of a fluxion, say that of x^n, rests on the violation of an axiomatic canon of sound reasoning.

As may be supposed, such an uncompromising assault on the

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fundamental principles of the new analysis created great excitement in the mathematical world; not only was Berkeley accused of charging mathematicians as a body with infidelity, but he had cast a slur on the genius of Newton, who had emerged, as was universally believed in England, triumphant at every point from the great dispute about the invention of the calculus. For many years every work on fluxions that appeared in England tried, with more or less success, to demolish Berkeley's arguments; the first attempt in that direction, as was right and fitting, came from Cambridge, in the shape of a pamphlet bearing the title *Geometry, no Friend to Infidelity; or, A Defence of Sir Isaac Newton and the British Mathematicians, in a Letter addressed to the Author of the Analyst. . . . By Philalethes Cantabrigiensis.* The pamphlet was published at London in 1734, the letter being dated, "Cambridge, April 10, 1734."

Mr Cantor (p. 718) attributes the authorship to two Cambridge professors, Conyers Middleton and Robert Smith; Philalethes asserts that there was but one author, though he admits consulting Smith on various points. There is little doubt, however, that Philalethes was not Middleton, but Dr Jurin; Middleton's controversies with Berkeley had reference to other subjects than fluxions.

Philalethes reduces the objections of the *Analyst* to three, namely: (1) The obscurity of the doctrine of fluxions; (2) False reasoning in it used by Sir Isaac Newton and implicitly received by his followers; (3) Artifices and Fallacies used by Newton to make this false reasoning pass upon his followers.

In regard to (1) Philalethes admits that the doctrine is not without its difficulties, but denies that these difficulties are insuperable for any "thinking reader" who is well versed in geometry; the appeal to thinking readers who are not so versed is inadmissible.

The essence of the defence is made under head (2), and an extremely weak defence it is. To account for the neglecting of the term $a\,b$ in finding the increment of the rectangle A B, he contends in the first place that mathematicians can very well estimate the effect of omitting that term, and they know that the error is not so much as a pin's head compared with a globe the size of the earth, or of the sun, or even of the orb of the fixed stars. At a later stage of the controversy, indeed, he somewhat resiles from this position by saying that he had introduced this comparison by way of popular exposition. In the second place, however, he maintains that "rigorously speaking, the

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moment of the rectangle AB is not, as you suppose, the increment of the rectangle AB; but it is the increment of the rectangle $(A - \frac{1}{2}a)\ (B - \frac{1}{2}b)$."

To clear up the point he observes that the word moment is used, both by Newton and by Berkeley, to signify indifferently either an increment or a decrement, that the increment of AB is, on Berkeley's showing, $aB + bA + ab$ and that the decrement should, therefore, be $aB + bA - ab$. He then asks which of these expressions is to be considered the moment, and maintains that the moment is the arithmetic mean of the two. This extraordinary contention about the arithmetic mean is apparently a favourite with Philalethes, as he repeats it in his second reply to Berkeley; afterwards, under stress of Robins' criticism, he alleges that the argument was intended to be taken not as rigorous demonstration, but only as *against Berkeley.* As a matter of fact, however, he tries to patch it up in later articles, where he is opposing not Berkeley but Robins—with disastrous results to the doctrine he is defending.

A curious illustration of the blindness of Philalethes to Newton's doctrine, as expounded in the introduction to the *Quadratura* (*volui ostendere quod in Methodo Fluxionum non opus sit Figuras infinite parvas in Geometriam introducere*), occurs in the closing sentence of this section:—"Lastly, to remove all scruple and difficulty about this affair, I must observe that the moment of the rectangle AB determined by Sir Isaac Newton, namely $aB + bA$, and the increment of the same rectangle determined by yourself, namely, $aB + bA + ab$, are perfectly and exactly equal, supposing a and b to be diminished *ad infinitum*, and this by the lemma just now quoted." (*Principia*, Book I., Sect. 1, Lemma 1).

Philalethes then turns to the criticism of the method of finding the fluxion of x^n. He objects to Berkeley's translation of the phrase "*evanescent jam augmenta illa et eorum ratio ultima erit*," namely, "let the increments be nothing," or "let there be no increments." He says "Ought it not to be thus translated :—'Let the augments now become evanescent, let them be upon the point of evanescence.' . . . . Do not the words *ratio ultima* stare us in the face, and plainly tell us that there is a last proportion of evanescent increments, yet there can be no proportion of increments which are nothing, of increments which do not exist!" What precisely he means by the *last* proportion of evanescent increments may be left out of account at present, as this part of the controversy is fully dealt with at a later stage.

Of the third main objection (artifices and fallacies, etc.), little can be said, as Philalethes, in that part of his reply, gives proof of nothing except his command of strong language.

He then goes on to consider the charge of reaching true conclusions by the compensation of errors. His defence, however, is a mere repetition of the argument for the equality of

$$aB + bA \quad \text{and} \quad aB + bA + ab.$$

There can be little doubt that Philalethes was in blank ignorance of the characteristic features of Newton's doctrine, as expounded, for example, in the introduction to the *Quadratura*. No doubt, the language of Newton was not free from ambiguity, and there is as little doubt that in his first attempts to formulate a doctrine of fluxions, in the *De Analysi per aequationes* for example, he used infinitely little quantities in the sense of Cavallerius and the Leibnizian school. But as he himself expressly says, he used the method of prime and ultimate ratios to avoid the introduction into geometry of infinitely little quantities. The contentions of Philalethes about the equality of $aB + bA$ and $aB + bA + ab$ were enough to make Newton turn in his grave, and his whole manner of treating Berkeley's criticisms was in fact a powerful argument in their favour.

In 1735 Berkeley published *A Defence of Free-thinking in Mathematics, in answer to a Pamphlet of Philalethes Cantabrigiensis . . . Also an Appendix concerning Mr Walton's Vindication of the Principles of Fluxions against the Objections contained in the Analyst.*

Walton's *Vindication*, as well as his other contributions to the controversy, I have not seen; but if one may judge from this *Appendix* and from another tract of Berkeley's, entitled *Reasons for not replying to Mr Walton's Full Answer*, Walton was even less qualified than Jurin to demolish the arguments of *The Analyst.*

In the *Defence* Berkeley takes Philalethes to task for misrepresenting the religious side of the *Analyst*, asserts his right to expose the errors even of the greatest men, admits the great genius of the inventor of fluxions, but somewhat scornfully advises Philalethes to worship truth rather than Newton. He repeats his contention that a fluxion is incomprehensible, and tries so to unveil this mystery as that every reader of ordinary sense and reflection, not the mathematician merely, may be a competent judge of the points in dispute ; he charges Philalethes with apparent ignorance of Newton's attitude towards infinitely small quantities in the method he adopts to get rid of the rect-

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angle *ab*, and directly traverses the interpretation Philalethes gives of the moment of the rectangle AB. He supports his translation of the phrase *evanescant jam augmenta illa* by stating that the expression

$$nx^{n-1} + \frac{n(n-1)}{2}ox^{n-2} + \text{ etc.},$$

can never be brought to nx^{n-1} except by supposing the increment o actually nothing, and further justifies his translation by quoting from the *De Analysi per aequationes*, where *evanescere* is rendered *esse nihil*. He strengthens his allusion to the "various arts and devices used by the great author of the fluxionary method" by describing the various accounts of his momentums and fluxions as inconsistent, and asks Philalethes to tell him "whether Sir Isaac's momentum be a finite quantity, or an infinitesimal, or a mere limit. If you say a finite quantity: be pleased to reconcile this with what he saith in the scholium of the second lemma of the first section of the first book of his Principles:—*Cave intelligas quantitates magnitudine determinatas sed cogita semper diminuendas sine limite.* If you say an infinitesimal: reconcile this with what is said in his introduction to the Quadratures:—*Volui ostendere quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam inducere.* If you should say it is a mere limit: be pleased to reconcile this with what we find in the first case of the second lemma in the second book of the Principles:—*Ubi de lateribus A et B deerant momentorum dimidia, etc.*—where the moments are supposed to be divided." The assertion of Philalethes that the objection based on the compensation of errors had been foreseen and removed by Newton in the *Principia, Book I., Sect.* 1., is characterised as an unquestionable proof of the matchless contempt which Philalethes has for truth. After referring to the use of infinitesimals by the Marquis de l'Hopital, he insists that the question of errors in professed approximations is quite distinct from that of logical errors in the reasoning—a distinction which Philalethes is apparently unable to grasp—and then quotes the diverging conceptions of the principles of the modern analysis held by mathematicians with whom he had conversed.

Under the date "Cambridge, June 13, 1735," Philalethes replies to the *Defence* in a pamphlet entitled *The Minute Mathematician: or the Freethinker no Just Thinker.* After some pages of general invective, Philalethes takes up the challenge, to show that the principles of fluxions may be clearly

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conceived. In several respects this second pamphlet is an improvement on the first, but in spite of occasional glimpses of Newton's real position, Philalethes only confirms the idea that, so far as he had definite conceptions, he stood on quite different ground from Newton, whom he was understood to be defending. Thus he says "A nascent increment is an increment just beginning to exist from nothing, or just beginning to be generated, but not yet arrived at any assignable magnitude how small soever." Again, he is in the regions of the infinitesimals, which Newton had condemned, when he asserts that "the magnitude of a moment is nothing fixed or determinate, is a quantity perpetually fleeting and altering till it vanishes into nothing; in short, that it is utterly unassignable." Or again—"What he (Newton) says, and what I contend for, is this. Though so long as a and b are real quantities their rectangle ab is a real quantity, and there is a real difference between the two quantities $aB+bA$ and $aB+bA+ab$; yet when by a continual diminution *ad infinitum* a and b vanish, their rectangle ab, or the difference between the two quantities $aB+bA$ and $aB+bA+ab$, vanishes likewise, and there is no longer any difference between those quantities, *i.e.*, those quantities are equal."

It is amazing that Philalethes did not see that this argument only proves, if it proves anything, that two magnitudes which are each nothing are equal; he simply confirms Berkeley's contention (*Defence*, sect. 32) that "for a fluxionist writing about momentums to argue that quantities must be equal because they have no assignable difference seems the most injudicious step that could be taken. For, it will thence follow that all homogeneous momentums are equal, and consequently the velocities, mutations, or fluxions, proportional thereto, are all likewise equal."

The contention of Philalethes is professedly based on the doctrine of prime and ultimate ratios expounded in Sect. 1, Book I. of the *Principia*, but he utterly misconceives that doctrine, interpreting it, it seems to me, exactly as Berkeley had done, so that an ultimate ratio is not the *limit* of a varying ratio, but the *last value* of a ratio. Berkeley very properly maintains that there is no last value of the augments except zero, so that the phrase "the ratio with which they vanish," used by Newton himself, and so often repeated by his expounders, does not represent any mathematical operation, and so far from explaining anything, requires explanation. No doubt the ordinary notions of what is meant by the value at a given

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instant of a varying velocity furnish a starting point for the measure of a fluxion and of the ratios of fluxions, and it is on these notions that Maclaurin bases his exposition in the first volume of his *Fluxions*, the measure of a varying velocity as a limit, being with him a *theorem* rather than a definition (*Fluxions*, vol. I., sect. 67). But a reference to the measure of a varying velocity does not clear up the logical difficulty of the mathematical procedure for finding a fluxion as urged by Berkeley in the cases of the rectangle AB and of x^n. Maclaurin himself seems to recognise this when he comes to treat of *The Computations in the Method of Fluxions*, for he says (*Fluxions* Sect. 702) in the case where A increases at a constant rate by equal differences but B increases by differences that are always varying, "it is not so obvious how, the fluxion of A being supposed equal to its increment A, the variable fluxion of B is to be determined."

Newton's method is the thoroughly sound one of limits, and Berkeley's criticism was really based on a misinterpretation of Newton's terminology. For this misinterpretation there was some excuse on his part; Newton, as Robins conclusively proved, used at first the methods and the language of indivisibles, but after discovering the method of prime and ultimate ratios he discarded that of indivisibles, though he often used language borrowed from the older writings. The terminology of first and last ratios was unfortunate, as it lent itself too readily to an interpretation in the sense of indivisibles; and it was this interpretation that Berkeley and Philalethes alike proceeded upon. Were that interpretation correct, then Berkeley's contentions would in the main be fully justified.

To the second pamphlet of Philalethes, Berkeley made no reply, so that his disputes about fluxions are limited to the three tracts, *The Analyst*, *The Defence*, and *The Reasons for not replying to Mr Walton's full answer* with the *Appendix*. It would be interesting to know why he offered no criticism on the next publication that the controversy produced, namely, *A Discourse concerning the Nature and Certainty of Sir Isaac Newton's Methods of Fluxions and of Prime and Ultimate Ratios. By Benjamin Robins, F.R.S. (London,* 1735*)*. Whatever the reason might be, Berkeley now retired from the contest, and Robins and Philalethes began that long struggle in which Robins proved his immense superiority to his antagonist, alike in temper, in general mathematical learning, and in special knowledge of Newton's fluxionary methods.

Robins, born at Bath, in 1707, of a poor Quaker family, was

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mainly self-taught in mathematics, and for some years supported himself in London as a teacher of mathematics. He is now best known by his great work, *New Principles of Gunnery.* In 1749 he was appointed Engineer-in-General to the East India Company, but his first undertaking, the planning of the defences of Madras, was no sooner accomplished than he was seized with a fever. Though he recovered, he never regained good health, and he died at Fort St David, July 29, 1751.

The *Discourse* is a masterpiece of its kind; in knowledge of the ancient geometry, in grasp of fundamental principles, in strength of logic, and in facility of expression, it is a splendid testimony not only to the intellectual power of its author, but to the profound logical difference between the method of indivisibles and the method of prime and ultimate ratios. As regards the interpretation of Newton's language, it should be borne in mind that Robins, as he more than once remarks in the course of the dispute with Philalethes, reached his conclusions through study of the ancient geometers and of Newton's writings, and he was thus thoroughly fitted to give the right interpretation of phrases that were in themselves ambiguous, or that were used in a different sense by the writers on indivisibles. This remark is of some importance, as Philalethes harped perpetually on a literal acceptation of words, and apparently could not or would not allow himself to see that his literal interpretation made sheer nonsense of Newton's doctrine.

Robins distinguishes between the doctrine of fluxions and that of prime and ultimate ratios. To avoid the imperfections of the method of indivisibles, Newton, he says, considered magnitudes as generated by a continued motion, and discovered a method to compare together the velocities wherewith homogeneous magnitudes increase; on the other hand, to facilitate the demonstrations, he invented the method of prime and ultimate ratios. The foundation of the method of fluxions is this principle, that "if the proportion between the celerity of increase of two magnitudes produced together is in all parts known, then the relations between the magnitudes themselves must from thence be discoverable." It is by means of proportions only that fluxions are applied to geometrical uses, for no determinate degree of velocity ever requires to be assigned for the fluxion of any one fluent.

Robins first proves by the method of exhaustions that the fluxion of x^n/a^{n-1} is to that of x as nx^{n-1}/a^{n-1} is to 1, and also finds the fluxion of a rectangle. The proofs, as might be expected, are somewhat long, but they possess all the evidence

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of an Archimedean demonstration; beyond that there was no appeal. He afterwards gives Newton's own proof, by the method of prime and ultimate ratios, stating at this point that it is equally just with that by the method of exhaustions.

He next goes on to show how fluxions are to be applied to the drawing of tangents to curve lines, and to the mensuration of curvilinear spaces, the proofs being by the method of exhaustions, and then gives a very clear description of the higher orders of fluxions, the manner of determining them, and some of the uses to which they may be applied.

In passing to the consideration of prime and ultimate ratios, he gives a short account of the ancient method of measuring curvilinear spaces, shows that the area of the circle may be determined in the Archimedean manner by the use of one polygon only, and points out that Newton instituted upon this principle (of comparison with one polygon only) a briefer method of conception and expression for demonstrating this sort of propositions than was used by the ancients. He allows that "the concise form, into which Sir Isaac Newton has cast his demonstrations, may very possibly create a difficulty of apprehension in the minds of some unexercised in these subjects," but not of those who are versed in "the ancients," while the method is just and free from any defect in itself. (Sects. 89—94),

He states his interpretation of the first Lemma of Book I. of the *Principia* thus:—"In this method any fixed quantity, which some varying quantity, by a continual augmentation or diminution, shall perpetually approach, but never pass, is considered as the quantity, to which the varying quantity will at last or ultimately become equal; provided the varying quantity can be made in its approach to the other to differ from it by less than by any quantity how minute soever, that can be assigned" (Sect. 95). Again, "Ratios also may so vary, as to be confined after the same manner to some determined limit, and such limit of any ratio is here considered as that with which the varying ratio will ultimately coincide. From any ratio's having such a limit, it does not follow, that the variable quantities exhibiting that ratio have any final magnitude, or even limit, which they cannot pass" (Sects. 98, 99).

Of course, this view of the famous lemma at once demolishes great part of Berkeley's contentions. Robins is particularly emphatic in confuting the notion that the last ratio of two varying quantities is necessarily a ratio which the quantities themselves can ever bear to each other, and repeats the illus-

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tration given by Newton at the end of the Scholium (*Prin.*, *Bk. I.*, *Sect.* 1), besides giving others in which the limit of each varying quantity is zero. He spends some time in explaining the meaning to be attached to the Latin words *evanescens* and *nascens*, and certainly his explanation is the most reasonable one, the only one, indeed, that is consistent with the general sense of Newton.

After his explanation of the conception upon which the doctrine is built, he proceeds "to draw out its first principles into a more diffusive form"; all that need be said is that his exposition is extremely clear, and is better than we often find in works of the present day. His explanation is followed by a statement and defence of Newton's own method of finding fluxions.

The *conclusion* of the *Discourse* is devoted to an explanation of the term *momentum*—a term not previously used in the tract. His explanation is that "in determining the ultimate ratios between the contemporaneous differences of quantities, it is often previously required to consider each of these differences apart, in order to discover how much of these differences is necessary for expressing that ultimate ratio. In this case Sir Isaac Newton distinguishes by the name of momentum so much of any difference as constitutes the term used in expressing this ultimate ratio." (Sect. 154). [It may be noted in passing that this separation of a difference into two parts is a favourite proceeding of Maclaurin.] Thus if o be the momentum of x, $nx^{n-1}o$ is the momentum of x^n; and $a\text{B}+b\text{A}$, not the whole increment $a\text{B}+b\text{A}+ab$, is called the momentum of AB, because so much only of the increment is required for determining the ultimate ratio of the increment of AB to the increment of MA, where M is any constant. He adds—"It must always be remembered that the only use which ought ever to be made of these momenta is to compare them with one another, and for no other purpose than to determine the ultimate or prime proportion between the several increments or decrements from whence they are deduced. Herein the method of prime and ultimate ratios essentially differs from that of indivisibles; for, in the method of indivisibles, momenta are considered absolutely as parts whereof their respective quantities are actually composed. But these momenta have no final magnitude which would be necessary to make them parts capable of compounding a whole by accumulation; yet their ultimate ratios are as truly assignable as the ratios between any quantities whatever." He justifies his explanation by the quotation *from the Principia, Book II., Lem. 2 :—neque spectatur magnitudo momentorum, sed prima nascentium proportio.*

As he not only admits but contends in subsequent papers that Newton did not always avoid the method of indivisibles, it may be well to add that Robins offers the above explanation as one that "shall agree to the general sense of his (Newton's) description." In spite of his great admiration for Newton, he saw quite as clearly as Berkeley the absurdity of the exposition of Philalethes, and claimed that Newton himself explicitly allowed the defects of his earlier work and the possibility of interpreting his language in the sense of indivisibles.

The *Discourse* was not ostensibly directed against Berkeley, as Robins was averse to entering into controversy with Philalethes; he thought the best method of elucidating the truth was to make no reference to either disputant, but to re-state the Newtonian doctrine. He soon found, however, that direct conflict with Philalethes was impossible. In the number for October 1735 of a magazine published in London, and called *The Present State of the Republic of Letters*, Robins gave an account of the *Discourse*, and expressed himself more freely than in the tract regarding the possible misconceptions of Newton's position through Newton's own injudicious concessions in the matter of language to the prevalent method of indivisibles. He also explicitly discussed Newton's early use of that method of demonstration and the difficulty involved in his use of *momenta*.

The November number of the same magazine brought out an article by Philalethes, entitled *Considerations upon some Passages contained in two Letters to the Author of the Analyst, written in Defence of Sir Isaac Newton and the British Mathematicians.* After a moderately-worded introduction, he reduces Robins's objections to his representation of Newton's doctrines to these three:—

I. His explication of *Principia*, Book I., Lemma 1.

II. The sense of the Scholium to Book I., Sect. 1, particularly as to (1) the doctrine of Limits, (2) the meaning of the term evanescent or vanishing.

III. The sense of *Principia*, Book II., Lemma 2.

As to I., Philalethes maintains that Newton means not that the quantities or ratios are merely to be *considered* as ultimately equal, but that they do at last become "actually, perfectly, and absolutely equal." Thus the inscribed and circumscribed parallelograms of *Principia*, Book I., Sect. 1, Lemma 2, do ultimately

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each coincide with the curvilinear figure which is the limit at which they arrive. In the same way in regard to II. (1) he holds that it is necessary that the variable quantity should *actually reach* the *limit*. The discussion of objection III. is much more guarded than in the corresponding passages of his criticism of Berkeley. He allows that the course taken by Newton (in his interpretation naturally) to find the difference of variable quantities is not rigorously geometrical in the case of higher products than two, of ABC for example, yet that it approaches nearer to geometric rigour than the method used by Leibniz. Incidentally he denies that Newton ever admitted of indivisibles.

Robins published in the December number of the *Republic* a *Review of some of the principal Objections, &c., with some Remarks on the different Methods that have been taken to obviate them.* He states very clearly, as I think, the view that Philalethes takes of Newton's expression *Fluxiones sunt in ultima ratione decrementorum evanescentium vel prima nascentium*, when he says that that explanation "endeavours to show how this imagined difficulty (of a ratio between nascent or evanescent magnitudes) may be avoided, not by considering these evanescent decrements and nascent augments as being actually vanished, in which case they can have no proportion, nor yet as being of any real magnitude, when their proportion cannot be the same with the proportion of the fluxions, but by supposing that there can be represented to the mind some intermediate state of these augments or decrements at the very instant in which they vanish."

Robins discusses at considerable length the contention that the varying quantity must reach its limit, maintaining that it rests on a misconception of the phrase "given difference," and that even if absolute coincidence of a varying magnitude and its limit could occur, that circumstance is quite irrelevant.

Philalethes took up his defence in the number for January 1736, in an article entitled *Considerations occasioned by a Paper in the last Republic of Letters.* He begins by acknowledging in handsome terms that Robins has established beyond all doubt or cavil the truth of Newton's rule for finding fluxions, but he maintains that Robins misrepresents Newton, his own exposition being in accordance with the words of their common master. He accepts the description quoted above from the *Review*, provided the *intermediate state* of the augments is not interpreted to mean the *quantity* or *magnitude* of the augments. He thinks that *state* "may be represented to the mind, and

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conceived by contemplating this proportion, not in the vanishing quantities themselves, but in other quantities permanent and stable, which are always proportional to them." Philalethes is here, I think, at his best; but he almost immediately, and one might say necessarily, involves himself in the quite gratuitous difficulty of undertaking to prove that Lemma I, Sect. 1. Book I of the *Principia*, necessarily requires that the variable must reach its limit. His contention rests on the literal interpretation of the words *fiunt ultimo aequales*, and on the idea that the words *data quavis differentia* do not mean *a difference first assigned*, according to which the degree of approach of the varying quantity may be afterwards regulated. He repeats from the November paper an illustration designed to show that the inscribed figure Lemma 2 does ultimately coincide with the curvilinear figure, but, as may be supposed, it is impossible to represent to the mind the *last* form of the inscribed figure which is to be equal to the curve. In any case, the whole conception is identical with that on which the method of indivisibles is founded and quite distinct from that of limits. Had Philalethes been able to see that the word *equal*, when associated with the restrictive adverb *ultimately*, was used in a wider meaning than in elementary mathematics, he might have been spared the trouble of all the controversy, and would have left to posterity a better estimate of his capacity than he has actually done.

The next contribution by Robins is a paper in the number of the *Republic* for April, 1736, entitled *A Dissertation showing that the Account of the Doctrines, &c., is agreeable to the real Sense and Meaning of their great Inventor.* The *Dissertation* is a somewhat lengthy document; it goes over the whole field, comparing in clear and interesting form the methods of exhaustions, indivisibles, and prime and ultimate ratios; points out Newton's early use of indivisibles, but shows how his discovery of the method of prime and ultimate ratios furnished a solid foundation for his demonstrations; and proves, as I think, beyond all possibility of doubt that his own account of Newton's aim and methods is thoroughly accurate. He fairly faces the objections Berkeley had raised, and shows with great skill that these are due to misconceptions of Newton's terminology, which disappear when that terminology is examined in the light of Newton's demonstrations and explicit cautions. He fully acknowledges the imperfection of certain phrases, taken by themselves, as, for example, when he says towards the close, "the ultimate ratio of variable quantities, the ratio with which quantities vanish, are, in strict propriety

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of speech, figurative expressions: nay, the last form of a figure, and the form wherewith a figure vanishes, might be interpreted upon the foot of indivisibles. But here these phrases only signify the limits, &c." In the course of the work he departs from his previous practice by explicitly naming Philalethes, and quoting from that gentleman's writings passages to which he objects.

It is perhaps worth noting that he frequently quotes from the account of the *Commercium Epistolicum* in the *Philosophical Transactions* as a document written by Newton himself. Philalethes subsequently asks the authority for attributing that account to Newton, and though Robins gives no authority, both he and Wilson, the editor of his *Tracts*, continue to cite it as Newton's own work.

The controversy between these two disputants might well have closed with the *Dissertation*, but the pugnacity of Philalethes, which was worthy of a better cause, could not brook such a termination. He returns to the attack in two long articles contributed to the numbers of the *Republic* for July and August, 1736, with the title, *Considerations upon some passages of a Dissertation, &c.* These articles display considerable ingenuity of the kind usually associated with a pettifogging lawyer, and great facility in the composition of Latin verse; but they equally reveal his ignorance of mathematical history, and the poverty of his mathematical attainments, while they are disfigured by personalities that verge at times on indecency. They really contribute nothing to the elucidation of the points in dispute. Robins replied in the numbers for August and September (Appendix) in articles with the title *Remarks on the Considerations, &c.*, keeping clear of personalities, and confining himself to answering the *Considerations* in short notes to its chief paragraphs.

To the *Remarks* of Robins there is a long rejoinder by Philalethes in an Appendix to the *Republic* for November, but on this rejoinder Robins offers no criticism. He inserts, however, an *Advertisement* (a word which must not be taken in its modern meaning) in the December number, in which he says: "I think it a very ill compliment from me to the publick to undertake a serious answer to the unadvised speeches of an angry man." He "takes leave of Philalethes with this observation only upon the two points whereon our controversy does indeed solely depend; his definition of nascent and evanescent quantities, and his interpretation of Sir Isaac Newton's first Lemma relating to the doctrine of prime and ultimate ratios."

On the first point, he says Philalethes has relinquished those passages where the terms are expressly discoursed of, while in relation to the Lemma, he maintains that his opponent tries to defend his interpretation by taking refuge in the language of the first edition, altered by Newton himself in the later editions. He leaves him to explain himself on this Lemma to Dr Pemberton.

To understand this reference, it is necessary to explain that the Appendix to the September *Republic*, which contained Robins's *Remarks*, had a *Postscript* from Pemberton, occasioned by a passage in the August article of Philalethes, in which the latter had stated that Newton "by some means or other" had been "prevailed upon to change the word *perplexas* into *longas*" (*Principia*, Book I., Sect. 1. Scholium at end, *praemisi haec lemmata*, &c.) Pemberton resented the remark as an insinuation against himself as editor of the third edition, in which the change was made, and after defending his action as fully sanctioned by Newton, he took occasion to state that "he had the very best opportunity of knowing Sir Isaac's true mind," and he was "fully satisfied that Mr Robins (had) expressed Sir Isaac Newton's real meaning" in regard to the Lemma.

This Postscript was the signal for the renewal of the controversy, but now Pemberton took the place of Robins. It would be unprofitable to follow it further, so far as Philalethes is concerned, and it may be sufficient to state that the contributions of the two disputants will be found in an Appendix to the *Republic* for December 1736, and in the numbers from February to October 1737 of the "*History of the Works of the Learned*"—a monthly magazine which was formed from the amalgamation of the *Republic* and another journal *The Literary World*.

An interesting article by Pemberton in the number for January 1741 of the *Works of the Learned* may be referred to. In that article he interprets the famous Lemma in the sense of Robins, and quotes a passage from Gregory of St Vincent (*Def.* 3 *Libri de Progressionibus Geometricis*) as a probable origin for Newton's terminology of *ultimate ratio*—that is, it was borrowed from the language used in geometrical progressions. The language of Newton's Lemma is strikingly similar to that of Gregory.

Besides the controversy on Fluxions above described, there was another between Robins and Jurin, occasioned by a review which the former had published of an essay by Jurin *Upon*

Distinct and Indistinct Vision, appended to Smith's *Compleat System of Opticks* (1738). The only occasion for referring to this second controversy is that in the preface to a rejoinder published by Robins in 1740, Robins gives a full statement of the reasons which induced him to confine his first publications on Fluxions to an impersonal statement of its principles. Suspecting from the first that Philalethes was Dr Jurin, he did not wish to appear as an opponent of one whom he considered as a personal friend, and he had even submitted his earlier manuscripts to Jurin, through a common acquaintance, with the object of suppressing anything that might seem to reflect on Philalethes. The extravagance, however, of the later articles of Philalethes had obliged Robins to refer to him explicitly, and in this preface it is established beyond all reasonable doubt that Jurin and Philalethes were the same person.

It is impossible to pass from this stage of the controversy evoked by the *Analyst*, without referring to the contemporary estimate of the merits of Jurin and Robins. It is usually Jurin who obtains the credit of refuting Berkeley, and when Robins is mentioned at all, his criticism is put alongside that of Jurin. Thus, Maclaurin, in the Preface to his *Fluxions*, names Philalethes and Robins, as two who had undertaken the defence of the method of Fluxions, but nowhere so far as I know, has he tried to reconcile their divergent interpretations. John Stewart, Professor of Mathematics in Aberdeen, published in 1745, a translation of the *Quadratura*, and the *Analysis per aequationes*, accompanying the translation with a voluminous and in many respects extremely able commentary; yet, he quotes Philalethes as having demolished the arguments of the author of the *Analyst*, but never, so far as I can discover after a diligent study of his work, even names Robins. Again, Wilson, in an Appendix to his edition of Robins's *Tracts* (1761) refers to a French translation of Newton's *Fluxions* by M. de Buffon, in which the translator in his Preface abuses Robins in violent terms, and represents Philalethes as having completely triumphed over him.

It seems to me beyond doubt, that if Philalethes has correctly interpreted Newton, the latter has no claim to be considered as the one who first established the Calculus on the basis of Limits, and that it is Robins who should get the credit of so founding the Calculus. From first to last, Philalethes uses the language of Newton in the sense of the writers on indivisibles, and is totally unable to comprehend the utter inconsistency that he thus introduces into Newton's writings.

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The protests that Newton made in the *Principia*, in the *Quadratura*, and throughout the Priority Controversy, are absolutely meaningless in the view Philalethes takes of Newton's doctrine. There can be no question, that there is a profound difference of conception in the views of Philalethes and Robins, and I confess myself at a loss quite to understand the favour shown to the work of Philalethes, and the comparative neglect of the brilliant essays of Robins. It is impossible by means of extracts to convey a sufficient sense of the extreme vagueness and want of precision on the part of Philalethes when treating the crucial points of a theory of limits; his language is largely figurative, and can have definite meaning assigned to it only by a re-interpretation based on a clear conception of a limit. Robins, on the other hand, is a ripe student of the masterpieces of antiquity, and is as precise and definite as Archimedes himself. His admiration for Newton is great, but it is sane, and the defence he makes of Newton's work is perhaps the best in existence.

As has been already stated, the arguments of the *Analyst* were, in a more or less explicit form, discussed by most English writers on Fluxions for several years after the publication of that work. There is, however, only one treatise that may here be noted as a direct fruit of the strictures of Berkeley, namely the great treatise by Maclaurin. This work was published at Edinburgh in 1742, but the greater part of the First Book was printed in 1737. Maclaurin's *Fluxions* is too well known to justify me in doing more than refer to it. The introductory chapters of Book I. are, I think, rendered rather tedious through his desire to stick closely to the ancient geometric method, but there can be no question of the immense power of logical exposition they display. It may be noted, as an instance of his breadth of mind, that he has even a good word to say for the Leibnizian calculus, and he shows how the demonstrations by that method may be made thoroughly rigorous.

Berkeley did a great service to sound reasoning in mathematics by the publication of the *Analyst*. The rapid accumulation of results, due to the introduction of the new analysis, had tended to throw into the background the logical principles on which any truly scientific knowledge of mathematics can alone be based, and the controversy the *Analyst* called forth is favourably distinguished from that on the invention of the Calculus by the comparative absence of the grosser personalities. Were it for nothing else than the *Discourse* and *Dissertation* of Robins, and the *Fluxions* of Maclaurin, Berkeley's name should be had in reverence of mathematicians.

One may almost regret that no equally gifted critic of the theory of Infinite Series stood out to challenge the work of mathematicians in that department of their science. Mr Cantor's volume records the multitudinous results that were rapidly finding their way into publicity, * but it shows at the same time how badly needed was a logical analysis of the nature of an infinite series. But, after all, it was perhaps better in the end that this formal period ran its course, for when, at a later time, the necessary revision came, there was no lack of instances with which to illustrate the necessity for the restrictions to which the employment of such series is subject.

As it is to the Edinburgh Mathematical Society this paper is being read, I may be allowed to make a personal reference. My interest in the *Analyst* controversy was originally awakened by the lamented and gifted A. Y. Fraser, the first Secretary of the Society. His interest in it had been aroused, when he was an undergraduate at Aberdeen, by current unsatisfactory demonstrations of the leading propositions in the calculus, and he always attributed to Robins his first acquaintance with the true meaning of the theory of limits. How thoroughly he had profited by his study of Robins may be seen by the article on the Calculus, which he contributed to Chambers's *Encyclopaedia*. It was, I know, his intention to contribute to our *Proceedings* an account of the *Analyst* controversy, and at the time of his death he was engaged in arranging the materials; so far as I have been able to ascertain, however, there has not been found among his papers any record of his work in this field. How great a loss we have thus sustained, those who knew Mr Fraser's brilliant powers of exposition can best appreciate.

* At p. 654 Mr Cantor expresses the opinion that in a certain memoir of Euler the idea on which the method of undetermined coefficients is founded is first clearly expressed. The method of undetermined coefficients was certainly well known to Newton, and is frequently applied in the *Quadratura*.

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