A HISTORY OF THE CONCEPTIONS OF LIMITS AND FLUXIONS IN GREAT BRITAIN

FROM

NEWTON TO WOODHOUSE

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LIMITS AND FLUXIONS

INTRODUCTION

1. EVERY great epoch in the progress of science is preceded by a period of preparation and prevision. The invention of the differential and integral calculus is said to mark a "crisis" in the history of mathematics. The conceptions brought into action at that great time had been long in preparation. The fluxional idea occurs among the schoolmen—among Galileo, Roberval, Napier, Barrow, and others. The differences or differentials of Leibniz are found in crude form among Cavalieri, Barrow, and others. The undeveloped notion of limits is contained in the ancient method of exhaustion; limits are found in the writings of Gregory St. Vincent and many others. The history of the conceptions which led up to the invention of the calculus is so extensive that a good-sized volume could be written thereon. We shall not yield to the temptation of lingering on this pre-history at this time, but shall proceed at once to the subject-matter of the present monograph.

CHAPTER I

NEWTON

2. It was in the year 1734 that Bishop Berkeley made his famous attack upon the doctrine of fluxions, which was the starting-point of all philosophical discussion of the new mathematics in England during the eighteenth century. In what follows we quote from the writings of Newton that were printed before 1734 such parts as bear on his conceptions of fluxions, so that the reader may judge for himself what grounds there were for Berkeley's great assault. To assist us in the interpretation of some of these printed passages, we quote also from manuscripts and letters of Newton which at that time were still unprinted. In the next chapter we give an account of the foundations of fluxions as displayed by other writers in books and articles printed in Great Britain before 1734. It is hoped that the material contained in these first two chapters will enable the student to follow closely and critically the debates on fluxions.

From Newton's Publications printed before 1734

I. Principia

3. Three editions of the *Principia* were brought out in Newton's lifetime; the first in 1687, the

second in 1713, the third in 1726. We give extracts which bear on the theory of limits and fluxions and indicate the changes in phraseology introduced in the second and third editions. We give also translations into English based on the text of the 1726, or third, edition.

Principia, Book I, Section I, Lemma I

First edition :

4. "Quantitates, ut & quantitatum rationes, quæ ad æqualitatem dato tempore constanter tendunt & eo pacto propius ad invicem accedere possunt quam pro data quavis differentia; fiunt ultimo æquales.

5. "Si negas, sit earum ultima differentia D. Ergo nequeunt propius ad æqualitatem accedere quam pro data differentia D: contra hypothesin."

Second and third editions :

6. "Quantitates, ut & quantitatum rationes, quæ ad æqualitatem tempore quovis finito constanter tendunt, & ante finem temporis illius propius ad invicem accedunt quam pro data quavis differentia, fiunt ultimo æquales.

7. "Si negas, fiant ultimò inæquales, & sit earum, etc." [As in the first edition.]

Translation by Robert Thorp:[1]

8. "Quantities, and the ratios of quantities, which, in any finite time, tend continually to equality; and

[1] *Mathematical Principles of Natural Philosophy, by Sir Isaac Newton, Knight. Translated into English, and illustrated with a Commentary, by* ROBERT THORP, M.A., vol. i, London, 1777.

before the end of that time, approach nearer to each other than by any given difference, become ultimately equal.

"If you deny it, let them be ultimately unequal; and let their ultimate difference be D. Therefore, they cannot approach nearer to equality than by that given difference D. Which is against the supposition."

Principia, Book I, Section I, Lemma II

Translation by Motte:[1]

9. "If in any figure A*ac*E, terminated by the right lines A*a*, AE, and the curve *ac*E, there be inscribed any number of parallelograms A*b*, B*c*, C*d*, etc., comprehended under equal bases AB, BC, CD, etc., and the sides B*b*, C*c*, D*d*, etc., parallel to one side A*a* of the figure; and the parallelograms *a*K*bl*, *b*L*cm*, *c*M*dn*, etc., are completed. Then if the breadth of those parallelograms be supposed to be diminished, and their number be augmented *in infinitum*; I say, that the ultimate ratios which the inscribed figure AK*b*L*c*M*d*D, the circumscribed figure A*albmcndo*E, and curvilinear figure A*abcd*E, will have to one another, are ratios of equality.

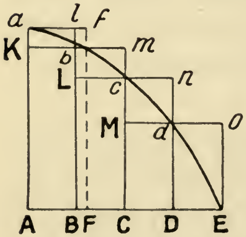

FIG. 1.

[1] *The Mathematical Principles of Natural Philosophy, by Sir Isaac Newton; translated into English by* ANDREW MOTTE, London, 1729. (Two volumes.)

"For the difference of the inscribed and circumscribed figures is the sum of the parallelograms K*l*, L*m*, M*n*, D*o*, that is (from the equality of all their bases), the rectangle under one of their bases K*b* and the sum of their altitudes A*a*, that is, the rectangle AB*la*. But this rectangle, because its breadth AB is supposed diminished *in infinitum*, becomes less than any given space. And therefore (by Lem. I) the figures inscribed and circumscribed become ultimately equal one to the other; and much more will the intermediate curvilinear figure be ultimately equal to either. Q. E. D."

Principia, Book I, Section I, Lemma XI,
Scholium (first part omitted)

10. ". . . Quæ de curvis lineis deque superficiebus comprehensis demonstrata sunt, facile applicantur ad solidorum superficies curvas & contenta. Præmisi vero hæc Lemmata, ut effugerem tædium deducendi perplexas[1] demonstrationes, more veterum Geometrarum, ad absurdum. Contractiores enim redduntur demonstrationes per methodum Indivisibilium. Sed quoniam durior est Indivisibilium hypothesis, & propterea methodus illa minus Geometrica censetur; malui demonstrationes rerum sequentium ad ultimas quantitatum evanescentium summas & rationes, primasque nascentium, id est, ad limites summarum & rationum deducere; & propterea limitum illorum demonstrationes qua potui brevitate præmittere. His enim idem præstatur

[1] In the third edition "longas" takes the place of "perplexas."

quod per methodum Indivisibilium; & principiis demonstratis jam tutius utemur. Proinde in sequentibus, siquando quantitates tanquam ex particulis constantes consideravero, vel si pro rectis usurpavero lineolas curvas; nolim indivisibilia, sed evanescentia divisibilia, non summas & rationes partium determinatarum, sed summarum & rationum limites semper intelligi; vimque talium demonstrationum ad methodum præcedentium Lemmatum semper revocari.

11. "Objectio est, quod quantitatum evanescentium nulla sit ultima proportio; quippe quæ, antequam evanuerunt, non est ultima, ubi evanuerunt, nulla est. Sed & eodem argumento æque contendi posset nullam esse corporis ad certum locum pergentis[1] velocitatem ultimam. Hanc enim, antequam corpus attingit locum, non esse ultimam, ubi attingit, nullam esse. Et responsio facilis est. Per velocitatem ultimam intelligieam,[2] qua corpus movetur; neque antequam attingit locum ultimum & motus cessat, neque postea, sed tunc cum attingit; id est, illam ipsam velocitatem quacum corpus attingit locum ultimum & quacum motus cessat. Et similiter per ultimam rationem quantitatum evanescentium intelligendam esse rationem quantitatum non antequam evanescunt, non postea, sed quacum evanescunt. Pariter & ratio prima

[1] In the second and third editions "pervenientis" takes the place of "pergentis." In the third edition the sentence reads, ". . . ad certum locum, ubi motus finiatur, pervenientis velocitatem ultimam."

[2] In the second and third editions "intelligi eam" takes the place of "intelligieam."

nascentium est ratio quacum nascuntur. Et summa prima & ultima est quacum esse (vel augeri et[1] minui) incipiunt & cessant. Extat limes quem velocitas in fine motus attingere potest, non autem transgredi. Hæc est velocitas ultima Et par est ratio limitis quantitatum & proportionum omnium incipientium & cessantium. Cumque hic limes sit certus & definitus, problema est vere Geometricum eundem determinare. Geometrica vero omnia in aliis geometricis determinandis ac demonstrandis legitime usurpantur.

12. "Contendi etiam potest, quod si dentur ultimæ quantitatum evanescentium rationes, dabuntur & ultimæ magnitudines ; & sic quantitas omnis constabit ex indivisibilibus, contra quam Euclides de incommensurabilibus, in libro decimo Elementorum, demonstravit. Verum hæc objectio falsæ innititur hypothesi. Ultimæ rationes illæ quibuscum quantitates evanescunt, revera non sunt rationes quantitatum ultimarum, sed limites ad quos quantitatum sine limite decrescentium rationes semper appropinquant, & quas propius assequi possunt quam pro data quavis differentia, nunquam vero transgredi, neque prius attingere quam quantitates diminuuntur in infinitum. Res clarius intelligetur in infinite magnis. Si quantitates duæ quarum data est differentia augeantur in infinitum, dabitur harum ultima ratio, nimirum ratio æqualitates nec tamen ideo dabuntur quantitates ultimæ seu maximæ quarum ista est ratio. Igitur in sequentibus,

[1] In the third edition "aut" takes the place of "et."

siquando facili rerum imaginationi[1] consulens, dixero quantitates quam minimas, vel evanescentes vel ultimas; cave intelligas quantitates magnitudine determinatas, sed cogita semper diminuendas sine limite."

Translation by Robert Thorp:

13. "Those things which have been demonstrated of curve lines, and the surfaces which they comprehend, are easily applied to the curve surfaces and contents of solids. But I premised these lemmas to avoid the tediousness of deducing long demonstrations to an absurdity, according to the method of the ancient geometers. For demonstrations are rendered more concise by the method of indivisibles. But, because the hypothesis of indivisibles is somewhat harsh, and therefore that method is esteemed less geometrical, I chose rather to reduce the demonstrations of the following propositions to the prime and ultimate sums and ratios of nascent and evanescent quantities; that is, to the limits of those sums and ratios: and so to premise the demonstrations of those limits, as briefly as I could. For hereby the same thing is performed, as by the method of indivisibles; and those principles being demonstrated, we may now use them with more safety. Therefore, if hereafter I shall happen to consider quantities, as made up of particles, or shall use little curve lines for right ones, I would not be understood to mean indivisible, but evanescent

[1] In the third edition "conceptui" takes the place of "imaginationi."

divisible quantities; not the sums and ratios of determinate parts, but always the limits of sums and ratios: and, that the force of such demonstrations always depends on the method laid down in the preceding lemmas.

14. "It is objected, that there is no ultimate proportion of evanescent quantities; because the proportion, before the quantities have vanished, is not ultimate; and, when they have vanished, is none. But, by the same argument, it might as well. be maintained, that there is no ultimate velocity of a body arriving at a certain place, when its motion is ended: because the velocity, before the body arrives at the place, is not its ultimate velocity; when it has arrived, is none. But the answer is easy: for by the ultimate velocity is meant that, with which the body is moved, neither before it arrives at its last place, when the motion ceases, nor after; but at the very instant when it arrives; that is, that very velocity with which the body arrives at its last place, when the motion ceases. And, in like manner, by the ultimate ratio of evanescent quantities is to be understood the ratio of the quantities, not before they vanish, nor after, but that with which they vanish. In like manner, the first ratio of nascent quantities is that with which they begin to be: and the first or last sum is that, with which they begin and cease to be, or to be augmented or diminished. There is a limit, which the velocity at the end of the motion may attain, but cannot exceed. This is the

ultimate velocity. And there is a like limit in all quantities and proportions that begin and cease to be. And since such limits are certain and definite, to determine the same is a problem strictly geometrical. But whatever is geometrical we may be allowed to use in determining and demonstrating any other thing that is likewise geometrical.

15. "It may be also argued, that if the ultimate ratios of evanescent quantities are given, their ultimate magnitudes will be also given; and so all quantities will consist of indivisibles, which is contrary to what Euclid has demonstrated concerning incommensurables, in the tenth book of his *Elements*. But this objection is founded on a false supposition, for those ultimate ratios with which quantities vanish are not truly the ratios of ultimate quantities, but the limits to which the ratios of quantities, decreasing without end, always converge; and to which they may approach nearer than by any difference, but can never go beyond, nor attain to, unless the quantities are diminished indefinitely. This will appear more evident in quantities indefinitely great. If two quantities, whose difference is given, are augmented continually, their ultimate ratio will be given, to wit, the ratio of equality; but the ultimate or greatest quantities themselves, of which that is the ratio, will not therefore be given. If then in what follows, for the more easy apprehension of things, I shall ever mention quantities *the least possible*, or *evanescent*, or *ultimate*, beware lest you understand quan-

tities of a determinate magnitude; but conceive them to be continually diminished without limit."

Principia, Book II, Section II, Lemma II.

16. "... Has quantitates, ut indeterminatas & instabiles, & quasi motu fluxuve perpetuo crescentes vel decrescentes, hic considero; & earum[1] incrementa vel decrementa momentanea sub nomine momentorum intelligo: ita ut incrementa pro momentis addititiis seu affirmativis, ac decrementa pro subductitiis seu negativis habeantur. Cave tamen intellexeris particulas finitas. Momenta, quam primum finitæ sunt magnitudinis, desinunt esse momenta. Finiri enim repugnat aliquatenus perpetuo eorum incremento vel decremento.[2] Intelligenda sunt principia jamjam nascentia finitarum magnitudinum. Neque enim spectatur in hoc lemmate magnitudo momentorum, sed prima nascentium proportio. Eodem recidit si loco momentorum usurpentur vel velocitates incrementorum ac decrementorum (quas etiam motus, mutationes & fluxiones quantitatum nominare licet) vel finitæ quævis quantitates velocitatibus hisce proportionales. Termini[3] autem cujusque generantis coefficiens est quantitas, quæ oritur applicando genitam

[1] The first edition gives "eorum" instead of "earum."

[2] In the place of "Momenta, quam primum finitæ sunt magnitudinis, desinunt esse momenta. Finiri enim repugnat aliquatenus perpetuo eorum incremento vel decremento," the second and third editions have this: "Particulæ finitæ non sunt momenta, sed quantitates ipsæ ex momentis genitæ."

[3] In the second and third editions "Lateris" takes the place of "Termini."

ad hunc terminum. Igitur sensus lemmatis est, ut, si quantitatum quarumcunque perpertuo motu crescentium vel decrescentium A, B, C, etc., momenta, vel mutationum velocitates[2] dicantur a, b, c, etc., momentum vel mutatio geniti[3] rectanguli AB fuerit $aB+bA$, et geniti[3] contenti ABC momentum fuerit $aBC+bAC+cAB$: . . .

17. "*Cas.* 1. Rectangulum quodvis motu perpetuo auctum AB, ubi de lateribus A & B deerant momentorum dimidia[4] $\frac{1}{2}a$ & $\frac{1}{2}b$, fuit $A-\frac{1}{2}a$ in $B-\frac{1}{2}b$, seu $AB-\frac{1}{2}aB-\frac{1}{2}bA+\frac{1}{4}ab$; & quam primum latera A & B alteris momentorum dimidiis aucta sunt, evadit $A+\frac{1}{2}a$ in $B+\frac{1}{2}b$, seu $AB+\frac{1}{2}aB+\frac{1}{2}bA+\frac{1}{4}ab$. De hoc rectangulo subducatur rectangulum prius, et manebit excessus $aB+bA$. Igitur laterum incrementis totis a et b generatur rectanguli incrementum $aB+bA$. Q.E.D."

English Translation by Andrew Motte:

18. " . . . These quantities I here consider as variable and indetermined and increasing or decreasing as it were by perpetual motion or flux; and I understand their momentaneous increments or decrements by the name of Moments; so that the

[1] In the second and third editions "hoc latus" takes the place of "hunc terminum."

[2] In the third edition "vel his proportionales mutationum velocitates" takes the places of "vel mutationum velocitates."

[3] "geniti" is left out in the first edition.

[4] In this history, the solidus (/) will be used sometimes in printing fractions which come in the line of the text. The reader must remember that this notation is modern; it occurs in none of the passages which we quote from seventeenth- and eighteenth-century books. In some cases the use of the solidus has made it necessary to insert parentheses which do not occur in the original.

increments may be esteemed as added, or affirmative moments; and the decrements as subducted, or negative ones. But take care not to look upon finite particles as such. Finite particles are not moments, but the very quantities generated by the moments. We are to conceive them as the just nascent principles of finite magnitudes. Nor do we in this Lemma regard the magnitude of the moments, but their first proportion as nascent. It will be the same thing, if, instead of moments, we use either the Velocities of the increments and decrements (which may also be called the motions, mutations, and fluxions of quantities) or any finite quantities proportional to those velocities. The coefficient of any generating side is the quantity which arises by applying the Genitum to that side. Wherefore the sense of the Lemma is, that if the moments of any quantities A, B, C, etc., increasing or decreasing by a perpetual flux, or the velocities of the mutations which are proportional to them, be called a, b, c, etc., the moment or mutation of the generated rectangle AB will be $a\mathrm{B}+b\mathrm{A}$; the moment of the generated content ABC will be $a\mathrm{BC}+b\mathrm{AC}+c\mathrm{AB}$: . . .

19. "*Case* 1. Any rectangle as AB augmented by a perpetual flux, when, as yet, there wanted of the sides A and B half their moments $\frac{1}{2}a$ and $\frac{1}{2}b$, was $\mathrm{A}-\frac{1}{2}a$ into $\mathrm{B}-\frac{1}{2}b$, or $\mathrm{AB}-\frac{1}{2}a\mathrm{B}-\frac{1}{2}b\mathrm{A}+\frac{1}{4}ab$; but as soon as the sides A and B are augmented by the other half moments; the rectangle becomes $\mathrm{A}+\frac{1}{2}a$ into $\mathrm{B}+\frac{1}{2}b$, or $\mathrm{AB}+\frac{1}{2}a\mathrm{B}+\frac{1}{2}b\mathrm{A}+\frac{1}{4}ab$. From this rectangle subduct the former rectangle, and there

will remain the excess $aB+bA$. Therefore with the whole increments a and b of the sides, the increment $aB+bA$ of the rectangle is generated. Q.E.D."

II. Wallis's De Algebra Tractatus

20. The Latin edition of John Wallis's *Algebra*, which appeared in 1693, contains on pages 390–396 a treatise on the "Quadrature of Curves" which Newton had prepared many years before, and from which he cited many things in his letter of October 24, 1676. In revised phraseology and with a new Introduction, the "Quadrature of Curves" was republished in 1704, as we shall see presently. Through the researches of Rigaud[1] we know now that what is given in Wallis's *Algebra*, p. 390, line 18, to p. 396, line 19, are Newton's own words, except, no doubt, the word "clarissimus," as applied to himself. From this part we quote as follows:[2]—

21. Page 391: "Per *fluentes quantitates* intelligit indeterminatas, id est quæ in generatione Cuvarum per motum localem perpetuo augentur vel diminuuntur, & per earum *fluxionem* intelligit celeritatem incrementi vel decrementi. Nam quamvis *fluentes quantitates* & earum *fluxiones* prima fronte conceptu difficiles videantur, (solent enim nova difficilius concipi), earundem tamen notionem cito faciliorem evasuram putat, quam sit notio momen-

[1] S. P. Rigaud, *Historical Essay on Sir Isaac Newton's Principia*, Oxford, 1838, p. 22.

[2] Johannis Wallis, S.T.D., *De Algebra Tractatus; Historicus & Practicus*. Oxoniæ, MDCXCIII.

torum aut *partium minimarum* vel *differentiarum infinite parvarum*: propterea quod figurarum & quantitatum generatio per motum continuum magis naturalis est & facilius concipitur, & Schemata in hac methodo solent esse simpliciora, quam in illa partium. . . ."

22. Page 392: "Sint v, x, y, z fluentes quantitates, & earum fluxiones his notis $\dot{v}$, $\dot{x}$, $\dot{y}$, $\dot{z}$ designabuntur respective. Et quoniam hæ fluxiones sunt etiam indeterminatæ quantitates, & perpetua mutatione redduntur majores vel minores, considerat velocitates quibus augentur vel diminuuntur tanquam earum fluxiones, & punctis binis notat in hunc modum $\ddot{v}$, $\ddot{x}$, $\ddot{y}$, $\ddot{z}$, & perpetuum incrementum vel decrementum harum fluxionum considerat ut ipsarum fluxiones, . . ."

23. Page 392: "Sit enim o quantitas infinite parva, & sint $o\dot{z}$, $o\dot{y}$, $o\dot{x}$ Synchrona momenta seu incrementa momentanea quantitatum fluentium z, y, & x: & hæ quantitates proximo temporis momento per accessum incrementorum momentaneorum evadent $z+o\dot{z}$, $y+o\dot{y}$, $x+o\dot{x}$: . . ." After substituting these in $x^3-xyy+aaz=0$, then subtracting the original expression and dividing the remainder by o, he remarks (page 393): "Terminos multiplicatos per o tanquam infinite parvos dele, & manebit æquatio $3\dot{x}x^2-\dot{x}yy-2xy\dot{y}+aa\dot{z}=0$."

Translation:

24. Page 391: "By *flowing quantities* he understands indeterminates, that is, those which, in the

generation of curves by local motion are always increased or diminished, and by their *fluxions* he understands the velocity of increase or decrease. For, however difficult of comprehension *flowing quantities* and their fluxions appear at first sight (for new things are usually perceived with difficulty), yet he thinks a notion of them will be obtained more easily than the notion of *moments* either of *least parts* or of *infinitely small differences* ; because the generation of figures and quantities is more naturally and easily conceived, and the drawings in this method are usually more simple than in that of parts."

25. Page 392: "Let the flowing quantities be designated v, x, y, z, and their fluxions by the marks $\dot{v}$, $\dot{x}$, $\dot{y}$, $\dot{z}$, respectively. And since these fluxions are likewise indeterminate quantities, and by perpetual motion become greater or lesser, he considers the velocities by which they are increased or diminished as their fluxions, and marks them with double dots in this way $\ddot{v}$, $\ddot{x}$, $\ddot{y}$, $\ddot{z}$, and he considers the perpetual increase or decrease of these fluxions as fluxions of themselves. . . ."

26. Page 392: "Let o be an infinitely small quantity, and $o\dot{z}$, $o\dot{y}$, $o\dot{x}$ the synchronous moments or momentaneous increments of the flowing quantities z, y, x: and these quantities at the next moment of time, by the accession of the momentaneous increments become $z+o\dot{z}$, $y+o\dot{y}$, $x+o\dot{x}$: . . ." After substituting these in $x^3-xyy+aaz=0$, then subtracting the original expression and divid-

ing the remainder by o, he remarks (page 393): "Destroy the terms multiplied by o as infinitely small, and there will remain the equation $3\dot{x}x^2 - \dot{x}yy - 2xy\dot{y} + aa\dot{z} = 0$."

III. Quadratura Curvarum,[1] 1704

"Introductio

27. "Quantitates Mathematicas non ut ex partibus quam minimis constantes, sed ut motu continuo descriptas hic considero. Lineæ describuntur ac describendo generantur non per appositionem partium sed per motum continuum punctorum, superficies per motum linearum, solida per motum superficierum, anguli per rotationem laterum, tempora per fluxum continuum, et sic in cæteris. Hæ Geneses in rerum natura locum vere habent et in motu corporum quotidie cernuntur. Et ad hunc modum Veteres ducendo rectas mobiles in longitudinem rectarum immobilium genesin docuerunt rectangulorum.

28. "Considerando igitur quod quantitates æqualibus temporibus crescentes et crescendo genitæ, pro velocitate majori vel minori qua crescunt ac

[1] *Tractatus de Quadratura Curvarum*, published in 1704 in London, as an appendix to Newton's *Opticks*. It was reprinted under the editorship of William Jones in London in the year 1711, in a volume containing also three other papers of Newton, viz., the *De analysi per æquationes infinitas*, *Enumeratio linearum tertii ordinis*, and *Methodus differentialis*. An English translation of the *Quadratura Curvarum*, made by John Stewart, was brought out in 1745 at London, in a volume containing also Newton's *Analysis by Equations of an Infinite Number of Terms*. A German translation of the *Quadratura Curvarum* by Gerhard Kowalewski appeared at Leipzig in 1908 in *Ostwald's Klassiker der exakten Wissenschaften*, Nr. 164.

generantur, evadunt majores vel minores; methodum quærebam determinandi quantitates ex velocitatibus motuum vel incrementorum quibus generantur; et has motuum vel incrementorum velocitates nominando *Fluxiones* et quantitates genitas nominando *Fluentes*, incidi paulatim Annis 1665 et 1666 in Methodum Fluxionum qua hic usus sum in Quadratura Curvarum.

29. "Fluxiones sunt quam proxime ut Fluentium

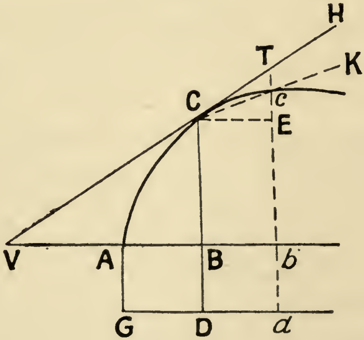

FIG. 2.

augmenta æqualibus temporis particulis quam minimis genita, et ut accurate loquar, sunt in prima ratione augmentorum nascentium; exponi autem possunt per lineas quascunque quæ sunt ipsis proportionales. Ut si areæ ABC, ABDG ordinatis BC, BD super basi AB uniformi cum motu progredientibus describantur, harum arearum fluxiones erunt inter se ut ordinatæ describentes BC et BD, et per ordinatas illas exponi possunt, propterea quod ordinatæ illæ sunt ut arearum augmenta nascentia.

Progrediatur ordinata BC de loco suo BC in locum quemvis novum *bc*. Compleatur parallelogrammum BCE*b*, ac ducatur recta VTH quæ curvam tangat in C ibsisque *bc* et BA productis occurrat in T et V: et abscissæ AB, ordinatæ BC, et lineæ curvæ AC*c* augmenta modo genita erunt B*b*, E*c*, et C*c*; et in horum augmentorum nascentium ratione prima sunt latera trianguli CET, ideoque fluxiones ipsarum AB, BC et AC sunt ut trianguli illius CET latera CE, ET et CT et per eadem latera exponi possunt, vel quod perinde est per latera trianguli consimilis VBC.

30. "Eodem recidit si sumantur fluxiones in ultima ratione partium evanescentium. Agatur recta C*c* et producatur eadem ad K. Redeat ordinata *bc* in locum suum priorem BC, et cœuntibus punctis C et *c*, recta CK coincidet cum tangente CH, et triangulum evanescens CE*c* in ultima sua forma evadet simile triangulo CET, et ejus latera evanescentia CE, E*c* et C*c* erunt ultimo inter se ut sunt trianguli alterius CET latera CE, ET et CT, et propterea in hac ratione sunt fluxiones linearum AB, BC et AC. Si puncta C et *c* parvo quovis intervallo ab invicem distant recta CK parvo intervallo a tangente CH distabit. Ut recta CK cum tangente CH coincidat et rationes ultimæ linearum CE, E*c* et C*c* inveniantur, debent puncta C et *c* coire et omnino coincidere. Errores quam minimi in rebus mathematicis non sunt contemnendi.

31. "Simili argumento si circulus centro B radio BC descriptus in longitudinem abscissæ AB ad

angulos rectos uniformi cum motu ducatur, fluxio solidi geniti ABC erit ut circulus ille generans, et fluxio superficiei ejus erit ut perimeter circuli illius et fluxio lineæ curvæ AC conjunctim. Nam quo tempore solidum ABC generatur ducendo circulum illum in longitudinem abscissæ AB, eodem superficies ejus generatur ducendo perimetrum circuli illius in longitudinem curvæ AC. . . .

32. "*Fluat quantitas x uniformiter et invenienda sit fluxio quantitatis x^n.* Quo tempore quantitas x fluendo evadit $x+o$, quantitas x^n evadet $\overline{x+o}|^n$, id est per methodum serierum infinitarum, $x^n+nox^{n-1}+(nn-n)/2\,oox^{n-2}+$etc. Et augmenta o et $nox^{n-1}+(nn-n)/2\,oox^{n-2}+$etc. sunt ad invicem ut 1 et $nx^{n-1}+(nn-n)/2\,ox^{n-2}+$etc. Evanescant jam augmenta illa, et eorum ratio ultima erit 1 ad nx^{n-1}: ideoque fluxio quantitatis x est ad fluxionem quantitatis x^n ut 1 ad nx^{n-1}.

33. "Similibus argumentis per methodum rationum primarum et ultimarum colligi possunt fluxiones linearum seu rectarum seu curvarum in casibus quibuscunque, ut et fluxiones superficierum, angulorum et aliarum quantitatum. In finitis autem quantitatibus Analysin sic instituere, et finitarum nascentium vel evanescentium rationes primas vel ultimas investigare, consonum est geometriæ veterum: et volui ostendere quod in Methodo Fluxionum non opus sit figuras infinite parvas in geometriam introducere. Peragi tamen potest Analysis in figuris quibuscunque seu finitis seu infinite parvis quae figuris evanescentibus finguntur

similes, ut et in figuris quæ pro infinite parvis haberi solent, modo caute procedas."

Translation by John Stewart:

"*Introduction*

34. "I consider mathematical quantities in this place not as consisting of very small parts; but as describ'd by a continued motion. Lines are describ'd, and thereby generated not by the apposition of parts, but by the continued motion of points; superficies's by the motion of lines; solids by the motion of superficies's; angles by the rotation of the sides; portions of time by a continual flux: and so in other quantities. These geneses really take place in the nature of things, and are daily seen in the motion of bodies. And after this manner the ancients, by drawing moveable right lines along immoveable right lines, taught the genesis of rectangles.

35. "Therefore considering that quantities, which increase in equal times, and by increasing are generated, become greater or less according to the greater or less velocity with which they increase and are generated; I sought a method of determining quantities from the velocities of the motions or increments, with which they are generated; and calling these velocities of the motions or increments *Fluxions*, and the generated quantities *Fluents*, I fell by degrees upon the Method of Fluxions, which I have made use of here in the Quadrature of Curves, in the years 1665 and 1666.

36. "Fluxions are very nearly as the augments of the fluents generated in equal but very small particles of time, and, to speak accurately, they are in the *first ratio* of the nascent augments; but they may be expounded by any lines which are proportional to them.

37. "Thus if the area's ABC, ABDG be described by the ordinates BC, BD moving along the base AB with an uniform motion, the fluxions of these area's shall be to one another as the describing ordinates BC and BD, and may be expounded by these ordinates, because that these ordinates are as the nascent augments of the area's.

38. "Let the ordinate BC advance from it's place into any new place *bc*. Complete the parallelogram BCE*b*, and draw the right line VTH touching the curve in C, and meeting the two lines *bc* and BA produc'd in T and V: and B*b*, E*c* and C*c* will be the augments now generated of the absciss AB, the ordinate BC and the curve line AC*c*; and the sides of the triangle CET are in the *first ratio* of these augments considered as nascent, therefore the fluxions of AB, BC and AC are as the sides CE, ET and CT of that triangle CET, and may be expounded by these same sides, or, which is the same thing, by the sides of the triangle VBC, which is similar to the triangle CET.

39. "It comes to the same purpose to take the fluxions in the *ultimate ratio* of the evanescent parts. Draw the right line C*c*, and produce it to K. Let the ordinate *bc* return into it's former

place BC, and when the points C and *c* coalesce, the right line CK will coincide with the tangent CH, and the evanescent triangle CE*c* in it's ultimate form will become similar to the triangle CET, and it's evanescent sides CE, E*c* and C*c* will be *ultimately* among themselves as the sides CE, ET and CT of the other triangle CET, are, and therefore the fluxions of the lines AB, BC and AC are in the same ratio. If the points C and *c* are distant from one another by any small distance, the right line CK will likewise be distant from the tangent CH by a small distance. That the right line CK may coincide with the tangent CH, and the ultimate ratios of the lines CE, E*c* and C*c* may be found, the points C and *c* ought to coalesce and exactly coincide. The very smallest errors in mathematical matters are not to be neglected.

40. "By the like way of reasoning, if a circle describ'd with the center B and radius BC be drawn at right angles along the absciss AB, with an uniform motion, the fluxion of the generated solid ABC will be as that generating circle, and the fluxion of it's superficies will be as the perimeter of that circle and the fluxion of the curve line AC jointly. For in whatever time the solid ABC is generated by drawing that circle along the length of the absciss, in the same time it's superficies is generated by drawing the perimeter of that circle along the length of the curve AC. . . ."

41. "*Let the quantity x flow uniformly, and let it be proposed to find the fluxion of* x^n.

"In the same time that the quantity x, by flowing, becomes $x+o$, the quantity x^n will become $\overline{x+o}|^n$, that is, by the method of infinite series's, $x^n+nox^{n-1}+(n^2-n)/2\ oox^{n-2}+$etc. And the augments o and $nox^{n-1}+(n^2-n)/2\ oox^{n-2}+$etc. are to one another as 1 and $nx^{n-1}+(n^2-n)/2\ ox^{n-2}$ $+$etc. Now let these augments vanish, and their ultimate ratio will be 1 to nx^{n-1}.

42. "By like ways of reasoning, the fluxions of lines, whether right or curve in all cases, as likewise the fluxions of superficies's angles and other quantities, may be collected by the method of *prime* and *ultimate* ratios. Now to institute an analysis after this manner in finite quantities and investigate the *prime* or *ultimate* ratios of these finite quantities when in their nascent or evanescent state, is consonant to the geometry of the ancients: and I was willing to show that, in the Method of Fluxions, there is no necessity of introducing figures infinitely small into geometry. Yet the analysis may be performed in any kind of figures, whether finite or infinitely small, which are imagin'd similar to the evanescent figures; as likewise in these figures, which, by the Method of Indivisibles, used to be reckoned as infinitely small, provided you proceed with due caution."

43. In the Quadrature of Curves proper, under "Proposition I" the proof of the rule for finding the fluxion of expressions like $x^3-xy^2+a^2z-b^3=0$ contains the following passages which indicate the use made of the symbol "o" and of the term

"moment," and the mode of passing to the limit. We quote :—

"*Demonstratio*

44. "Nam sit o quantitas admodum parva et sunto $o\dot{z}$, $o\dot{y}$, $o\dot{x}$, quantitatum z, y, x, momenta id est incrementa momentanea synchrona. Et si quantitates fluentes jam sunt z, y et x, hæ post momentum temporis incrementis suis $o\dot{z}$, $o\dot{y}$, $o\dot{x}$ auctæ, evadent $z+o\dot{z}$, $y+o\dot{y}$, $x+o\dot{x}$, quæ in æquatione prima pro z, y et x scriptæ dant æquationem . . .

$$3\dot{x}x^2+3\dot{x}\dot{x}ox+\dot{x}^3oo-\dot{x}yy-2x\dot{y}y-2\dot{x}o\dot{y}y-xo\dot{y}\dot{y}-\dot{x}oo\dot{y}\dot{y}+aa\dot{z}=0.$$

Minuatur quantitas o in infinitum, et neglectis terminis evanescentibus restabit $3\dot{x}x^2-\dot{x}yy-2x\dot{y}y+aa\dot{z}=0$. Q.E.D."

Translation by John Stewart :

"Demonstration

45. "For let o be a very small quantity, and let $o\dot{z}$, $o\dot{y}$, $o\dot{x}$ be the moments, that is the momentaneous synchronal increments of the quantities z, y, x. And if the flowing quantities are just now z, y, x, then after a moment of time, being increased by their increments $o\dot{z}$, $o\dot{y}$, $o\dot{x}$: these quantities shall become $z+o\dot{z}$, $y+o\dot{y}$, $x+o\dot{x}$: which being wrote in the first equation for z, y and x, give this equation . . .

$$3\dot{x}x^2+3\dot{x}\dot{x}ox+\dot{x}^3oo-\dot{x}yy-2x\dot{y}y-2\dot{x}o\dot{y}y-xo\dot{y}\dot{y}-\dot{x}oo\dot{y}\dot{y}+aa\dot{z}=0.$$

Let the quantity o be diminished infinitely, and neglecting the terms which vanish, there will remain $3\dot{x}x^2 - \dot{x}yy - 2x\dot{y}y + aa\dot{z} = 0$. Q.E.D."

IV. An Account of the "Commercium Epistolicum"

46. It is now generally accepted that the account [1] of the *Commercium Epistolicum*, published in the *Philosophical Transactions*, London, 1717, was written by Newton. The reasons for attributing it to him are stated by De Morgan[2] and by Brewster.[3] In abstract the account is as follows:—

47. (Pp. 177–178.) In a letter of October 24, 1676, to Oldenburgh, Newton explained that in deducing areas he considered the area as growing "by continual Flux"; "from the Moments of Time he gave the Name of Moments to the momentaneous Increases, or infinitely small Parts of the Abscissa and Area generated in Moments of Time. The Moment of a Line he called a Point, in the Sense of Cavalerius, tho' it be not a geometrical Point, but a Line infinitely short, and the Moment of an Area or Superficies he called a Line, in the sense of Cavalerius, tho' it be not a geometrical Line,

[1] *Philosophical Transactions*, vol. xxix, for the years 1714, 1715, 1716. London, 1717. "An Account of the Book entituled *Commercium Epistolicum Collinii et aliorum, De Analysi promota . . .*," pp. 173–224. This account was translated into Latin and inserted in the edition of the *Commercium Epistolicum* of 1725.

[2] See De Morgan's articles in the *Philosophical Magazine*, S. 4, vol. iii, June, 1852, pp. 440–444; vol. iv, November 1852, p. 323.

[3] Sir David Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 2nd ed., vol. ii, Edinburgh, 1860, pp. 35, 36.

but a Superficies infinitely narrow. And when he consider'd the Ordinate as the Moment of the Area, he understood by it the Rectangles under the geometrical Ordinate and a Moment of the Abscissa, tho' that Moment be not always expressed." Again, p. 179: "And this is the Foundation of the Method of Fluxions and Moments, which Mr. Newton in his Letter dated Octob. 24, 1676, comprehended in this Sentence. *Data æquatione quotcunque fluentes quantitates involvente, invenire Fluxiones; et vice versa.* In this Compendium Mr. Newton represents the uniform Fluxion of Time, or of any Exponent of Time by an Unit; the Moment of Time or its Exponent by the Letter *o*; the Fluxions of other Quantities by any other Symbols; the Moments of those Quantities by the Rectangles under those Symbols and the Letter *o*; and the Area of the Curve by the Ordinate inclosed in a Square, the Area being put for a Fluent and the Ordinate for its Fluxion. When he is demonstrating a Proposition he uses the Letter *o* for a finite Moment of Time, or of its Exponent, or of any Quantity flowing uniformly, and performs the whole Calculation by the Geometry of the Ancients in finite Figures or Schemes without any Approximation: and so soon as the Calculation is at an End, and the Equation is reduced, he supposes that the moment *o* decreases *in infinitum* and vanishes. But when he is not demonstrating but only investigating a Proposition, for making Dispatch he supposes the Moment *o* to be infinitely little, and forbears to write it down, and uses all manner of Approximations which he conceives will produce no Error in the Conclusion." In Newton's *Principia* "he frequently considers Lines as Fluents described by Points, whose Velocities increase or decrease, the Velocities are the first Fluxions, and their Increase the second." The Compendium of his Analysis was written "in or before the year 1669" (p. 180). "And the same Way of working he used in his Book of Quadratures, and still uses to this day" (p. 182). On p. 204 we read: "Mr. Newton used the letter o in his *Analysis* written in or before the Years 1669, and in his Book of *Quadratures*, and in his *Principia Philosophiæ*, and still uses it in the very same Sense as at first. . . . These Symbols o and $\dot{x}$ are put for things of a different kind. The one is a Moment, the other a Fluxion or Velocity as has been explained above. . . . Prickt Letters never signify Moments, unless when they are multiplied by the Moment o either exprest or understood to make them infinitely little, and then the Rectangles are put for the Moments" (p. 204). Further on we read: "It [the method of fluxions] is more elegant [than the Differential Method of Leibniz], because in his Calculus there is but one infinitely little Quantity represented by a symbol, the symbol o. We have no Ideas of infinitely little Quantities, and therefore Mr. Newton introduced Fluxions into his Method, that it might proceed by finite Quantities as much as possible. It is more Natural and Geometrical, because founded upon the *primæ quantitatum nascentium rationes*, which have a Being in Geometry, whilst *Indivisibles*, upon which the Differential Method is founded, have no Being either in Geometry or in Nature. There are *rationes primæ quantitatum nascentium*, but not *quantitates primæ nascentes.* Nature generates Quantities by continual Flux or Increase; and the ancient Geometers admitted such a Generation of Areas and Solids" (p. 205).

From Newton's Correspondence and Manuscripts not in print in 1734

48. Manuscripts of Newton, some of them still unpublished, show that he first thought of fluents and fluxions in 1665 and 1666, when he was in his twenty-third and twenty-fourth years.[1] The notation by dots occurs as early as 1665. As pointed out by De Morgan,[2] these early papers are infinitesimal in character. They were first published in 1838.[3] A manuscript, dated Nov. 13, 1665, gives rules for finding the velocities p, q, r, etc., of two or more lines x, y, z, etc., described by bodies A, B, C, etc., the lines being related to each other

[1] See a list of Newton's manuscripts and publications on fluxional calculus prepared by Philip E. B. Jourdain, in his edition of *Augustus De Morgan's Essays on the Life and Work of Newton*, The Open Court Publishing Co., 1914, pp. 107–112.

[2] Augustus De Morgan, "On the Early History of Infinitesimals in England," *The London, Edinburgh, and Dublin Philosophical Magazine*, 4th S., vol. iv, 1852, pp. 321–330. This article is an important historical contribution, of which extensive use is made in the present history.

[3] See S. P. Rigaud, *Historical Essay on the first Publication of Sir Isaac Newton's Principia*, Oxford, 1838, Appendix, pp. 20–24.

by an equation, such as $x^3 - 2a^2y + zzx - yyx + zyy - z^3 = 0$. "If the body A, with the velocity p, describe the infinitely little line o in one moment, in the same moment B, with the velocity q, will describe the line oq / p," and the body C, with the velocity r, will describe the line or / p. So that, if the described lines be x, y and z "in one moment," they will be $x+o$, $\dot{y}+oq / p$, $z+or / p$ "in the next." He finds that the relation of the velocities p, q, r, in the above example, is $3pxx + pzz - pyy - 2aaq - 2yxq + 2zyq + 2zxr + yyr - 3zzr = 0$. In proving his rules for differentiation, Newton divides by o, and in the resulting expression observes that "those terms in which o is, are infinitely less than those in which it is not. Therefore, blotting them out, there rests" the relation sought. The notation by dots, "pricked letters," occurs on a leaf, dated May 20, 1665, which has never been printed.[1]

49. It is evident that Newton permitted twenty-eight years to pass between the time of his first researches on fluxions and 1693, the date when the earliest printed account of his notation of fluxions appeared from his pen in the Latin edition of Wallis's *Algebra*. Moments and fluxions are mentioned in his *Principia*, as has been shown by our extracts.

50. Of importance in the interpretation of the meanings of "moment" in the second edition of

[1] S. P. Rigaud, *op. cit.*, Appendix, p. 23. Consult also the remarks on this passage made by G. Eneström in *Bibliotheca mathematica*, 3. F., Bd. 11, Leipzig, 1910–1911, p. 276, and Bd. 12, 1911–1912, p. 268, and by A. Witting in Bd. 12, pp. 56–60. See also *A catalogue of the Portsmouth collection of books and papers, written by or belonging to Isaac Newton*, Cambridge, 1889.

the *Principia* (1713) is a letter of May 15, 1714, from Newton to Keill,[1] from which we quote the following :—

51. ". . . altho I use prickt Letters in the first Proposition of the book of Quadratures, yet I do not there make them necessary to the method. For in the Introduction to that book I describe the method at large & illustrate it w^th^ various examples without making any use of such letters. And it cannot be said that when I wrote that Preface I did not understand the method of fluxions because I did not there make use of prickt letters in solving of Problems.[2] The book of Quadratures is ancient, many things being cited out of it by me in my Letter of 24 Octob. 1676. . . .

52. "ffluxions & moments are quantities of a different kind. ffluxions are finite motions, moments are infinitely little parts. I put letters with pricks for fluxions, & multiply fluxions by the letter *o* to make them become infinitely little and the rectangles I put for moments. And wherever prickt letters represent moments & are without the letter *o* this letter is always understood. Wherever $\dot{x}$, $\dot{y}$, $\ddot{y}$, $\dddot{y}$, etc., are put for moments they are put for $\dot{x}o$, $\dot{y}o$, $\ddot{y}oo$, $\dddot{y}o^3$. In demonstrating Propositions I always write down the letter *o* & proceed by the Geometry of Euclide and Apollonius without any

[1] J. Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes*, London, 1850, pp. 176, 177.

[2] John Bernoulli, in the *Acta Eruditorum* for February and March, 1713, had criticised a passage in the *Principia*, and claimed that Newton did not understand the second fluxions when writing that passage.

approximation. In resolving Questions or investigating truths I use all sorts of approximations w^{ch} I think will create no error in the conclusion and neglect to write down the letter *o*, and this do for making dispatch. But where $\dot{x}$, $\dot{y}$, $\ddot{y}$, $\dddot{y}$ are put for fluxions without the letter *o* understood to make them infinitely little quantities they never signify differences. The great Mathematician[1] therefore acts unskilfully in comparing prickt letters with the marks *dx* and *dy*, those being quantities of a different kind."

Remarks

53. The extracts from Newton's writings demonstrate the following :—

(1) At first Newton used infinitesimals (infinitely small quantities), as did Leibniz and other mathematicians of that age. As early as 1665, when Newton was a young man of twenty-three, he used them and speaks of "blotting them out."[2] He uses infinitesimals in the *Principia* of 1687[3] and in his account of the quadrature of curves in Wallis's *Algebra* of 1693, where Newton speaks of himself in the third person.[4] It is worthy of emphasis, in contrast to Leibniz, that Newton uses only infinitesimals of the *first* order. Moreover, as De Morgan remarked long ago,[5] "the early distinction between the systems of the two is this, that Newton, holding to the conception of the *velocity* or *fluxion*,

[1] John Bernoulli. See Edleston, *op. cit.*, p. 171. [2] See our § 48.
[3] See our §§ 10, 13, 16, 18. [4] See our §§ 21, 26.
[5] De Morgan, *Philosophical Magazine*, 4 S., vol. iv, 1852, p. 324.

used the infinitely small increment as a means of determining it; while, with Leibnitz, the relation of the infinitely small increments is itself the object of determination."

(2) As early as 1665, Newton speaks of describing an "infinitely little line" in "one moment," and then uses the expression "in the next" moment.[1] Here "moment" cannot mean a point of time, destitute of duration; it means an infinitely small duration, an infinitesimal of time. Doubtless this use of "moment" with reference to time suggested the more extended and general use of the term "momentum" or "momenta" as found in the *Principia*[2] and later publications.

(3) The use of dots, "prickt letters," to indicate velocities or fluxions goes back to 1665,[3] but they are not used by Newton in print until 1693 in Wallis's *Algebra*; they are used extensively in Newton's *Quadrature of Curves* of 1704.[4]

(4) Newton first used the word "fluxion" in print in 1687 in the *Principia*.[5]

(5) The first refinement of the doctrine of fluxions is found in Newton's *Principia*, where he speaks of "prime and ultimate ratios"[6] and of "limits."[7]

(6) The high-water mark of Newton's efforts to place the doctrine of fluxions upon a thoroughly logical basis is found in his *Quadrature of Curves*, 1704. It indicates the almost complete exclusion

[1] See our § 48.
[2] See our §§ 16, 18, 21, 24.
[3] See our § 48.
[4] See our §§ 22, 25, 44, 45.
[5] See our §§ 16, 18.
[6] See our §§ 10, 13.
[7] See our §§ 4, 6, 8, 10, 13.

of quantities infinitely little. "I consider mathematical quantities in this place not as consisting of very small parts," says Newton.[1] Also "the very smallest errors in mathematical matters are not to be neglected,"[2] and "in the method of fluxions there is no necessity of introducing figures infinitely small into geometry."[3] In view of these statements the symbol *o* used in the *Quadrature of Curves*, a "quantitas ad modum parva,"[4] must be interpreted as a small *finite* quantity. In this connection De Morgan's remarks are of interest:[5] "In 1704, Newton in the *Quadratura Curvarum* renounced and abjured the infinitely small quantity; but he did it in a manner which would lead any one to suppose that he had never held it. . . . And yet, there is something like a recognition of *some one* having used infinitely small quantities in *Fluxions*, contained in the following words: *volui ostendere quod in Methodo Fluxionum non opus sit figuras infinite parvas in Geometriam introducere*: nothing is wanted except an avowal that the *some one* was Newton himself. The want of this avowal was afterwards a rock of offence. Berkeley, in the *Analyst*, could not or would not see that Newton of 1687 and Newton of 1704 were of two different modes of thought."

We do not interpret Newton's expressions of 1704 as declarations that a logical exposition of

[1] See our §§ 27, 34. [2] See our §§ 30, 39.
[3] See our §§ 33, 41. [4] See our §§ 44, 45.
[5] De Morgan, *Philosophical Magazine*, 4 S., vol. iv, 1852, p. 328.

fluxions cannot be given on the basis of infinitesimals or that infinitely small quantities are impossible; for he says,[1] "the analysis may be performed in any kind of figures whether finite or infinitely small, which are imagined similar to the evanescent figures."

In fact, not even in 1704 did Newton succeed in completely banishing from his doctrine of fluxions the infinitely little. If what he used in 1704 is not the infinitely little, it is so closely related thereto, that it cannot be called either a finite magnitude or an absolute zero.

In 1704, fluxions are "in the *first ratio* of the nascent augments," or "in the *ultimate ratio* of the evanescent parts."[2] Unless the fully developed theory of limits is read into these phrases, they will involve either infinitely little parts or other quantities no less mysterious. At any rate, the history of fluxions shows that these expressions did not meet the demands for clearness and freedom from mysticism. Newton himself knew full well the logical difficulty involved in the words "prime and ultimate ratios"; for in 1687 he said,[3] "it is objected, that there is no ultimate proportion of evanescent quantities; because the proportion, before the quantities have vanished, is not ultimate; and, when they have vanished, is none." How does Newton meet this, his own unanswerable argument? He does so simply by stating the difficulty in another

[1] See our §§ 33, 42. [2] See our §§ 29, 30, 33, 36, 38, 39, 42.
[3] See our §§ 11, 14.

form: "But, by the same argument, it might as well be maintained, that there is no ultimate velocity of a body arriving at a certain place, when its motion is ended: because the velocity, before the body arrives at the place, is not its ultimate velocity; when it has arrived, is none. But the answer is easy: for by the ultimate velocity is meant that . . . at the very instant when it arrives." If "instant," as used here, is not an infinitesimal, the passage would seem to be difficult or impossible of interpretation.

(7) A return to the open use of the infinitely small quantities is seen in writings of Newton after the year 1704. It might be argued that such a return was necessary in the second edition of the *Principia*, 1713, unless the work were largely rewritten. Newton's *Analysis per æquationes numero terminorum infinitas* was first printed in 1711, and might have been rewritten so as to exclude infinitesimals as fully as was done in the *Quadrature of Curves* of 1704. But the infinitely little is permitted to remain.[1] There is no disavowal of such quantities either in the *Commercium Epistolicum*, with the editors of which Newton was in touch, or in Newton's own account of this publication, contributed to the *Philosophical Transactions*.[2]

(8) The theory of limits is involved in the first lemma of the *Principia*,[3] and in the explanation of prime and ultimate ratios as given in that work.

[1] See our § 66. [2] See our § 47.
[3] See our §§ 4, 6, 8, 9, 10, 12, 13, 15.

CHAPTER II

PRINTED BOOKS AND ARTICLES ON FLUXIONS BEFORE 1734

54. THE earliest printed publication in Great Britain on the new calculus was from the pen of John Craig, a Scotsman by birth, who settled in Cambridge and became a friend of Newton. Later he was rector of Gillingham in Dorsetshire. He was "an inoffensive, virtuous man," fond of mathematics. In 1685 he published at London a book entitled, *Methodus figurarum . . . quadraturas determinandi*. At that time nothing could be known about fluxions except through private communication. In 1684 Leibniz published his first ideas of Differential Calculus in the Leipzig *Acts*. Craig used in 1685 the calculus of Leibniz and also the notation of Leibniz. Continental writers call Craig the introducer of the theory of Leibniz into England. On p. 28 of his book, Craig derives $dp = 48nr^4y^2dy$ from $p = 16nr^4y^3$, and arrives at a differential equation (æquationem differentialem). The meanings of dp, dy, dx, etc., are not explained but taken for granted, reference being made to Leibniz. In 1693 Craig published another book in which the notation of Leibniz is used. He con-

tributed also several papers to the *Philosophical Transactions* (London), but never, before 1718, did he use fluxional symbols. In preparing the book of 1685 he had received from Newton the binomial theorem which he used before it had appeared in print, but he had no communication about fluxions. "We have here the singular indifference," says De Morgan, "which Newton at that time, and long afterwards, showed toward his own calculus."[1] Craig wrote a tract in 1693, and articles for the *Philosophical Transactions* in 1701, 1703, 1704, 1708, using the differential calculus all this time. In the issue No. 284, 1703, he employs the Leibnizian sign of integration $\int$. Craig submitted to Newton one of his early manuscripts (probably the one printed in 1693). With regard to this event De Morgan wrote to Hamilton, the inventor of quaternions: "Few of us know that Leibniz was perfectly well known in England before the dispute, and that Newton's first provocative to an imperfect publication was *ds* and infinitely small quantities paraded under his own eyes by an English writer (Craig), who lent him his MSS. to read."[2] Craig's publication of 1718 followed the great controversy on the invention of the calculus; now he uses fluxions exclusively and says not a word on the differential calculus. The book does not discuss fundamentals, and no explanation of $\dot{x}$ is given. As conjectured

[1] De Morgan, *Philosophical Magazine*, 4 S., vol. iv, 1852, p. 326.
[2] *Life of Sir William Rowan Hamilton*, by Robert P. Graves, vol. iii, 1889, p. 415.

by De Morgan, it may have been Craig's manuscript that suggested to Newton the need of making his own fluxions accessible to the public. At any rate, in 1693 there appeared the account of fluxions in Wallis's *Algebra*. [See *Addenda*, p. 289.]

55. Abraham De Moivre, a French mathematician who in 1688, after the revocation of the Edict of Nantes, came to London, contributed in 1695 to No. 216 of the *Philosophical Transactions* (London) an article in which he uses $\dot{x}$, $\dot{y}$, $\ddot{x}$, $\ddot{y}$, and lets both "fluxion" and "moment" stand for things infinitely small. In the same number of the *Transactions*, the astronomer Edmund Halley has an article on logarithms in which he uses infinitely small *ratiunculæ* and *differentiolæ*, but neither the notation of Leibniz nor that of Newton. In 1697, David Gregory used in No. 231 of the *Transactions* $\dot{x}$ and speaks of "fluxio fluxionis" without, however, explaining his terms.

56. Fatio de Duillier, a Swiss by birth, who had settled in London and become member of the Royal Society, wrote in 1699 a treatise, *Lineæ brevissimi descensus investigatio geometrica*, uses fluxions as infinitely small quantities. This publication is noted as containing a statement which started the Newton-Leibniz controversy on the invention of the Calculus.

57. It is remarkable that Roger Cotes, in 1701, when an undergraduate at Trinity College, Cambridge [Newton's own College], wrote a letter on mathematical subjects, in which $\dot{x}$ is used as

"infinitely little."[1] In 1702–3 Humphry Ditton, in vol. xxiii of the *Transactions*, used the fluxional notation, without explanation.

58. Other writings that do not define their terms are the *Fluxionum methodus inversa*, 1704, by the London physician, George Cheyne, and De Moivre's *Animadversiones in D. Georgii Cheynai Tractatum*, London, 1704. However, Cheyne lets $\dot{x} = 1$, from which we infer that, with him, $\dot{x}$ was finite. [See *Addenda*, p. 289.]

59. The next writer on fluxions was John Harris, a voluminous author of books on various subjects. He was at one time Secretary of the Royal Society. In 1702 he published at London *A New Short Treatise of Algebra*, which devotes the last 22 pages, out of a total of 136 pages, to fluxions. It is the first book in the English language in which this subject is treated. The doctrine of fluxions is the "Arithmetick of the *Infinitely small* Increments or Decrements of *Indeterminate* or *Variable Quantities*, or as some call them the *Moments* or *Infinitely small Differences* of such Variable Quantities. These Infinitely small Increments or Decrements, our incomparable Mr. Isaac Newton calls very properly by this name of Fluxions" (p. 115). A few lines further on it says that Newton "calls the *celerity* or *Velocity* of the Augmentation of Diminution of these *Flowing Quantities*, by the name of *Fluxions*." A second edition of this book appeared in 1705. As authors on fluxions, Harris in 1705

[1] J. Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes*, London, 1850, p. 196.

mentions Newton, Wallis, Nieuwentiit, Carré, Leibniz, l'Hospital, de Moivre, and Hayes.

60. John Harris also published a *Lexicon Technicum*, of which the second volume, London, 1710, contains an article, "Fluxions."

"This general Method of finding the Fluxions of all Powers and Roots, I had from the Hon. Fr. Robartes, Esq. If a Quantity gradually increases or decreases, its immediate Increment or Decrement is called its *Fluxion.* Or the Fluxion of a Quantity is its Increase or Decrease indefinitely small. . . . Since $\dot{x}\dot{x}$ . . . is infinitely smaller than $2x\dot{x}$, whereby it can make no sensible Change in that Quantity, it may be laid aside as of no Value. . . . Authors' Names who have written of Fluxions: *D. Bernoulli Tractatus de Principiis Calculi Exponentialis*; *Nieuwentiit's Analysis Infinitorum*, Amster., 1695; *Dr. Cheyne's Fluxions*, with *Moivre's Animadversions* on them, and the Doctor's reply; *Hays's Fluxions*, Lond., 1704; *Analyse des Infiniment Petits. Par l'Hospital*, Fr., Paris, 1696; *Le Calcule Integrale, par M. Carré*, Paris, 1700; *Mr. Abraham de Moivre's* Use of *Fluxions*, in the Solution of Geometrick Problems. See *Philos. Trans.*, N. 216; *Mr. Humphry Ditton's Institution of Fluxions.*"

61. In the above list of writers are Charles Hayes and Humphry Ditton, authors of English texts now demanding our attention. Hayes starts his elucidation of fundamentals (p. 1) as follows:[1]

[1] *A Treatise of Fluxions: or, An Introduction to Mathematical Philosophy*, Charles Hayes, London, 1704.

"Magnitude is divisible in *infinitum*, and the Parts after this infinite Division, being infinitely little, are what Analysts call *Moments* or *Differences*; And if we consider Magnitude as Indeterminate and perpetually Increasing or Decreasing, then the infinitely little Increment or Decrement is call'd the *Fluxion* of that Magnitude or Quantity: And whether they be called *Moments*, *Differences* or *Fluxions*, they are still suppos'd to have the same Proportion to their Whole's, as a Finite Number has to an Infinite; or as a finite Space has to an infinite Space. Now those infinitely little Parts being extended, are again infinitely Divisible; and these infinitely little Parts of an infinitely little Part of a given Quantity, are by Geometers call'd *Infinitesimæ Infinitesimarum* or *Fluxions* of *Fluxions*. Again, one of those infinitely little Parts may be conceiv'd to be Divided into an infinite Number of Parts which are call'd *Third Fluxions*, etc."

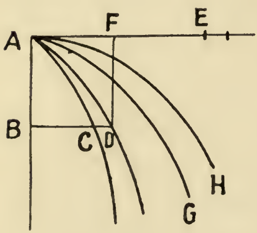

FIG. 3.

He endeavours to justify this doctrine by illustrations. The angle of contact FAC formed by the line AE and the ordinary parabola AC, is less than any rectilineal angle; the angle FAD, formed by AE with the cubical parabola AD, is infinitely less than the angle FAC, and so on. Hayes defines the doctrine of *Fluxions* as the "Arithmetick of infinitely small Increments or Decrements of Indeterminate or variable Quantities." He cautions the reader: "But we must take great heed, not to consider the Fluxions, or Increments, or Decrements as finite Quantities" (p. 4). He rejects $\dot{x}\dot{z}y$ and $\dot{x}\dot{z}\dot{y}$ "as being incomparably less" than $\dot{x}zy$.

The same year in which Hayes wrote this first English book on fluxions which could make any claim to attention, saw the appearance of Newton's *Quadratura Curvarum.* The contrast in the definition of "fluxion" was sharp. Hayes called it "an infinitely small increment"; Newton called it a "velocity," a finite quantity.

62. William Jones, in his *Synopsis Palmariorum Matheseos*, London, 1706, devotes a few pages to fluxions and fluents, using the Newtonian notation. On p. 225 he gives, in substance, Newton's lemma, in these words: "Quantities, as also their Ratio's, that continually tend to an Equality, and therefore that approach nearer the one to the other, than any Difference that can possibly be assign'd, do at last become equal." Then he says: "Hence all Curved Lines may be considered as composed of an Infinite Number of Infinitely little right Lines." He uses "infinitely small" quantities, but defines a fluxion as "the Celerity of the Motion," fluxions being "in the first Ratio of their Nascent Augments." Jones represents here the Newton of the *Principia*, and of the *Quadrature of Curves* as given in 1793.

63. The earliest book exhibiting a careful study

of Newton's tract of 1704 was Humphry Ditton's *Institution of Fluxions*, 1706.[1] Ditton was prominent as a divine as well as a mathematician. Like so many other English writers on fluxions during the eighteenth century, he had not been at either of the great universities. He states in his preface that he has also consulted and drawn from the writings of John Bernoulli and some other Continental writers.

64. The reader of Ditton's book is impressed by the fact that he labours strenuously to make everything plain. He takes the reader fully into his confidence. This is evident in the extracts which follow (pp. 12–21):—

"Suppose any flowing Quantities, . . . as also their Increments . . . which Increment imagine to be generated in equal very small Particles of Time. I conceive we may say without Scruple, that the *Fluxions are the velocities of those Increments, consider'd not as actually generated, but quatenus Nascentia, as arising and beginning to be generated.* As there is a vast difference between the Increments consider'd as Finite, or really and actually generated; and the same considered only as *Nascentia* or in the first Moment of their Generation: So there is as great a difference also between the Velocities of the Increments, consider'd in this two fold respect. . . .

[1] *An Institution of Fluxions: Containing the First Principles, The Operations, with some of the Uses and Applications of that Admirable Method; According to the Scheme prefix'd to his Tract of Quadratures, by (its First Inventor) the Incomparable Sir Isaac Newton.* By Humphry Ditton, London, 1706.

The Reason of that [difference], is this. Because there is (speaking strictly and accurately) an Infinity of Velocities to be consider'd, in the Generation and Production of a *Real Increment* ; . . . So that if we conceiv'd the Fluxion, to be the Velocity of the Increment, *as actually Generated* ; we must conceive it to be an Infinite Variety or Series of Velocities. Whereas the Velocity, with which any sort of Increment *arises*, or *begins to be generated* ; is a thing that one may form a very clear and distinct Idea of, and leaves the Mind in no Ambiguity or Confusion at all. . . . However, if we take those Particles of time exceeding small indeed, and Neglect the Acceleration of the Velocity as inconsiderable, we may say the Fluxions are proportional to those Increments ; remembering at the same time, that they are *but nearly*, and not *accurately* so. . . . If in the *Differential Calculus*, some Terms are rejected and thrown out of an Equation, because they are nothing *Comparatively*, or with respect to other Terms in the same Equation ; that is, because they are infinitely small in proportion to those other Terms, and so may be neglected upon that Score: On the other hand, in the Method of Fluxions, those same Terms go out of the Equation, because they are multiplied into a Quantity, which . . . does at last really vanish. . . . N.B. Speaking here of Infinitely small Quantities, or Infinitesimals as some Authors (and particularly Mr. Neiwentiit) chuse to term them, I cannot but take notice of a notion, which that Excellent and Ingenious Person advances in his *Analysis Infinitorum*.

It is this; That a *Quantity Infinitely Great, a Finite or any given Quantity, an Infinitesimal, and Nihilum Geometricum*, are in Geometrical Proportion. I confess I cannot discover the truth of this. . . . Let m denote an infinite Quantity, d any finite one; then is d / m the Infinitesimal of d, according to Mr. Neiwentiit. Now his Assertion is, that $m : d :: d / m : 0$; therefore since from the nature of Geometrical Proportion, 'tis also $m : d :: d / m : dd / mm$; it follows that dd / mm is $= 0$ . . . then $d / m = 0$. Now Mr. Neiwentiit will hardly allow his Infinitesimal to be nothing; and yet . . . I think it must follow, that $d = 0$." Proceeding geometrically, Ditton explains the fluxions of lines, areas, solids, and surfaces. Next he takes up algebraical expressions. To find the fluxion of x^n, he lets x flow uniformly and represents the augment of x in a given particle of time by the symbol o. While x becomes $x + o$, x^n becomes $(x + o)^n$. Expanding the binomial, he finds that the two augments are as 1 to $nx^{n-1} + (n^2 - n)ox^{n-2} / 2 +$ etc. "And the Ratio of them (making o to vanish) will be that of 1 to nx^{n-1}." According to his notation $\dot{x}$ is a fluxion of x, and x is a fluxion of $\acute{x}$. Taking o as a very small quantity, he lets the expressions $o\dot{z}$, $o\dot{y}$ represent the *moments*, or increments of the flowing quantity z, y generated in a very small part of time. "If therefore *now, at the present Moment*, the flowing Quantities are z, y, x; the next *Moment* (when augmented by these Increments) they will become $x + o\dot{z}$, $y + o\dot{y}$, $x + o\dot{x}$." He expresses the general mode of procedure for finding the fluxion, which coincides with the modern mode of finding a derivative. Ditton considers the increments as *finite* (p. 53). "These *Momenta* are in proportion to one another as the Fluxions of the flowing Quantities respectively, for $o\dot{z}$, $o\dot{y}$, $o\dot{x}$, are as $\dot{z}$, $\dot{y}$, $\dot{x}$; and Mr. Newton had before expresly told us; that the Increments generated in a very small Particle of time were *very nearly*, as the Fluxions." Evidently Ditton does not here overlook that $o\dot{z}$, $o\dot{y}$, $o\dot{x}$ represent the increments only "very nearly." He observes (p. 98) that we may "go on with ease to the second, third, and any other Fluxions; neither are there any new Difficulties to be met with."

A second edition of Ditton's book was brought out in 1726 by John Clarke.

65. Ditton's first edition appeared at a time when the Newton-Leibniz controversy was under way. Leibniz had appealed to the Royal Society for justice. That Society appointed a committee which published a report containing letters and other material bearing on the case, in a book called the *Commercium Epistolicum*,[1] which figures prominently in the lamentable controversy. From this book the early use of infinitely small quantities on the part of Newton is conspicuously evident. The book makes it clear also that some of Newton's warmest supporters were guilty of gross inaccuracy in the use of the word "fluxion."

[1] *Commercium Epistolicum D. Johannis Collins, et aliorum de analysi promota: jussu societatis regiæ in lucem editum.* Londini, MDCCXII.

66. Newton's *Analysis per æquationes numero terminorum infinitas*, which was sent on July 31, 1669, through Barrow to Collins, and which was first published at London in 1711, was reprinted in the *Commercium Epistolicum.* In this *Analysis* infinitely small quantities are used repeatedly, but the word "fluxion" and the fluxional notation do not occur. In a letter to H. Sloane, who was then Secretary of the Royal Society of London, written in answer to a letter of Leibniz dated March 4, 1711, John Keill, professor of astronomy at Oxford, recounts the achievements of Isaac Barrow and James Gregory, and says: "If in place of the letter o, which represents an infinitely small quantity in James Gregory's *Geometriæ pars universalis* (1667), or in place of the letters a or e which Barrow employs for the same thing, we take the $\dot{x}$ or $\dot{y}$ of Newton or the dx or dy of Leibniz, we arrive at the formulas of fluxions or of the differential calculus."[1] Thus Keill, the would-be great champion of Newton, instead of warning the reader against confusing differentials and fluxions, himself comes dangerously close to conveying the erroneous idea that $\dot{x}$ and $\dot{y}$ are infinitely small, the same as dx and dy. He comes so near to this as to be guilty of lack of caution, if not of inaccuracy.

More serious is a statement further on. The en-

[1] "Nam si pro Litera o, quæ in Jacobi Gregorii Parte Matheseos Universali quantitatem infinite parvam repræsentat; aut pro Literis a vel e quas ad eandem designandam adhibet Barrovius; ponamus $\dot{x}$ vel $\dot{y}$ Newtoni, vel dx seu dy Leibnitii, in Formulas Fluxionum vel Calculi Differentialis incidemus" (p. 112).

listment of the services of a clever lawyer would be needed to acquit the editors of the *Commercium Epistolicum* of gross error when, in the final summary of their case against Leibniz, they declare (p. 121), "that the Differential Method is one and the same with the Method of Fluxions, excepting the name and the notation; Mr. Leibniz calling those Quantities Differences, which Mr. Newton calls Moments or Fluxions; and marking them with the letter *d*, a mark not used by Newton."

67. Joseph Raphson, in his *History of Fluxions* (which appeared as a posthumous work at London, in 1715, printed in English, and in the same year also in Latin, the Latin edition containing new correspondence bearing on the Newton-Leibniz controversy), says on p. 5 that Newton "makes use of Points, and denotes those first *Differences* (which by a Name congruous to their Generation, being consider'd as the first Increments or Decrements of a continued Motion, he calls *Fluxions*) thus, viz. $\dot{x}$, $\dot{y}$, $\dot{z}$." This misrepresentation of Newton is the more astonishing when we recollect that Raphson was very partial to Newton, and also meant his History "to open a plain and easy way for Beginners to understand these Matters." Newton never looked upon a *fluxion* as anything different from velocity; with him it was always a *finite* quantity. To make matters worse, Raphson continues: "To these Quantities he adds others of another Gender, and which in relation to Finite ones may be conceiv'd as infinitely great, and denotes them thus $'x$, $'y$, $'z$,

whereof the first or finite Quantities themselves, viz. x, y, z, may be conceiv'd as *Fluxions*." And again, "a Point . . . may be consider'd as the Fluxion of a Line, a Line as the Fluxion of a Plane, and a Plane as the Fluxion of a Solid, and a finite Solid as the Fluxion of a (partially) infinite one, and that again as the Fluxion of one of an higher Gender of Infinity, and so on *ad inf.* which we shall further illustrate in some Dissertations at the end of this Treatise."

68. Brook Taylor brought out at London in 1715 his *Methodus incrementorum directa et inversa*, in which he looks upon fluxions strictly from the standpoint of the Newtonian exposition in the *Quadrature of Curves*, 1704.

69. James Stirling uses $\dot{x}$ and $\dot{y}$ as infinitesimals in his *Lineæ tertii ordinis*, Oxford, 1717. He draws the infinitely small right triangle at the contact of a curve with its asymptote, the horizontal side being "quam minima" and equal to $\dot{x}$, the vertical side being $\dot{y}$. In the appendix to this booklet of 1717, $\dot{x}$ and $\dot{y}$ are again infinitely small. In his *Methodus differentialis*, London, 1730, there is no direct attempt to explain fundamentals, any more than there was in 1717, but on p. 80 he puts the fluxion of an independent variable equal to unity, from which we infer that a fluxion is with him now a finite velocity.

70. For twenty-four years after Ditton no new text appeared. In 1730 Edmund Stone, a self-taught mathematician who had studied De l'Hospital,

sent forth a new book, the first part of which was a translation.[1]

The following extract is from Stone's translation of De l'Hospital's Preface, the words in the square brackets [ ] being interpolated by Stone :—

"By means of this Analysis we compare the infinitely small (Differences or) Parts of finite Magnitudes, and find their Ratio's to each other; and hereby likewise learn the Ratio's of finite Magnitudes, those being in reality so many infinitely great Magnitudes, in respect of the other infinitely small ones. This Analysis may ever be said to go beyond the Bounds of Infinity itself; as not being confined to infinitely small (Differences or) Parts, but discovering the Ratio's of Differences of Differences, or of infinitely small Parts of infinitely small Parts, and even the Ratio's of infinitely small Parts of these again, without End. So that it not only contains the Doctrine of Infinites, but that of an Infinity of Infinites. It is an Analysis of this kind that can alone lead us to the Knowledge of the true Nature and Principles of Curves: For Curves being no other than Polygons, having an Infinite Number of Sides, and their Differences arising altogether from the different Angles which their infinitely small Sides make with each other, it is the Doctrine of Infinites alone that must enable us to determine the Position of these Sides, in order to get the

[1] *The Method of Fluxions, both Direct and Inverse. The former being a Translation from the Celebrated Marquis De l'Hospital's Analyse des Infinements Petits: And the Latter Supply'd by the Translator*, E. Stone, F.R.S. London, MDCCXXX.

Curvature formed by them; and thence the Tangents, Perpendiculars, Points of Inflexion and Retrogression, reflected and refracted Rays, etc., of Curves.

"Polygons circumscribed about or inscribed in Curves, whose Number of Sides infinitely augmented till at last they coincide with the Curves, have always been taken for Curves themselves. . . . It was the Discovery of the Analysis of Infinites that first pointed out the vast Extent and Fecundity of this Principle. . . . Yet this itself is not so simple as Dr. Barrow afterwards made it, from a close Consideration of the Nature of Polygons, which naturally represent to the Mind a little Triangle consisting of a Particle of a Curve (contained between two infinitely near Ordinates), the Difference of the correspondent Absciss's; and this Triangle is similar to that formed by the Ordinate, Tangent, and Subtangent. . . . Dr. Barrow . . . also invented a kind of Calculus suitable to the Method (*Lect. Geom.*, p. 80), tho' deficient. . . . The Defect of this Method was supplied by that of Mr. Leibnitz'z,[1] [*or rather the great Sir Isaac Newton*].[2] He began where Dr. Barrow and others left off: His *Calculus* has carried him into Countries hitherto unknown. . . . I must here in justice own (as Mr Leibnitz himself has done, in *Journal des Sçavans* for August 1694) that the learned Sir Isaac Newton likewise discover'd something like the *Calculus Differentialis*, as appears by his excellent

[1] *Acta Erudit. Lips.*, ann. 1684, p. 467.
[2] See *Commercium Epistolicum.*

Principia, published first in the Year 1687, which almost wholly depends upon the Use of the said *Calculus*. But the Method of Mr. Leibnitz'z is much more easy and expeditious, on account of the Notation he uses. . . ."

In the preface of "The Translator to the Reader" Stone points out that the work he is bringing out "becomes the more necessary, because there are but two English Treatises on the Subject . . . the one being Hay's *Introduction to Mathematical Philosophy*, and the other, Ditton's *Institution of Fluxions*"; the former "too prolix," the latter "much too sparing in Examples" and "too redundant" in the explanation of fluxions, so that "it is next to impossible for one who has not been conversant about Infinites to apprehend it. That of our Author is much easier, tho less Geometrical, who calls a Differential (or Fluxion) the infinitely small Part of a Magnitude." "But," continues Stone, "I would not here be thought in any wise to lessen the Value of Sir Isaac Newton's Definition: When the Learner has made some Progress, I would have him then make himself Master of it." Stone then proceeds to explain the nature of fluxions, following closely Newton's language in his *Quadrature of Curves*.

71. In De l'Hospital's treatise, as translated by Stone, we read:

"The infinitely small Part whereby a variable Quantity is continually increased or decreas'd, is called the *Fluxion* of that Quantity."

Here Stone simply writes "fluxion" where De l'Hospital writes "différence," which is a mischievous procedure, seeing that the two words stand for things totally different. De l'Hospital's wording is "La portion infiniment petite dont une quantité variable augmente ou diminuë continuellement, en est appellée la Différence." Stone also changes from the Leibnizian to the Newtonian notation, by writing $\dot{x}$ instead of dx. Then follow two postulates:

"Grant that two Quantities, whose Difference is an infinitely small Quantity, may be taken (or used) indifferently for each other: or (which is the same thing) that a Quantity, which is increased or decreas'd only by an infinitely small Quantity, may be consider'd as remaining the same.

"Grant that a Curve Line may be consider'd as the Assemblage of an infinite Number of infinitely small right Lines: or (which is the same thing) as a Polygon of an infinite Number of Sides, each of an infinitely small Length, which determine the Curvature of the Line by the Angles they make with each other."

De l'Hospital's "prendre la différence" is rendered by Stone "to find the fluxions." The fluxion of xy is found by taking the product of $x+\dot{x}$ and $y+\dot{y}$, and neglecting $\dot{x}\dot{y}$, "because $\dot{x}\dot{y}$ is a Quantity infinitely small, in respect of the other Terms $y\dot{x}$ and $x\dot{y}$."

72. Further on in Stone's translation (p. 73) we read:

"The infinitely small Part generated by the continual increasing or decreasing of the Fluxion of a variable Quantity, is called the *Fluxion* of the *Fluxion* of that Quantity, or *second Fluxion*." In like manner he defines *third Fluxion*; "fluxion of the second fluxion" taking the place of "différence de la différence seconde."

In the appendix, containing Stone's *Inverse Method of Fluxions*, a *fluent* is defined thus:

"The *fluent* or *flowing Quantity* of a given fluxionary Expression, is that Quantity whereof the given fluxionary Expression is the Fluxion."

Remarks

73. The earliest treatment of the new analysis which became current in England was that of Leibniz. The Scotsman Craig used it for over a quarter of a century before rejecting it in favour of fluxions. Harris, Hayes, and Stone drew their inspiration from French writers who followed Leibniz. A hopeless confusion arose in the use of the term "fluxion." Newton always took it to be a velocity, but many writers, including Newton's friends who prepared the *Commercium Epistolicum*, simply said "fluxion" instead of "differential," thus putting a home label upon goods of foreign manufacture. A strict follower of the Newton of 1704 was Ditton; fluxions are taken as infinitesimals by Fatio de Duillier, Cotes (in 1701), Harris, Hayes, Raphson, Stirling (in 1717), and Stone.

Stone comes out strongly with the view that a circle is a polygon of an infinite number of sides. He also uses the infinitesimal triangle. Hayes and Stone have no hesitation in speaking of "fluxions of fluxions," and "infinitely little parts of an infinitely little part." No writers, unless we except Newton (1704) and Ditton, dispense with the use of infinitely small quantities. The dropping of such quantities from an equation was usually permitted without scruple.

What an opportunity did this medley of untenable philosophical doctrine present to a close reasoner and skilful debater like Berkeley! [See *Addenda*, p. 289.]

CHAPTER III

BERKELEY'S ANALYST (1734); CONTROVERSY WITH JURIN AND WALTON

74. BISHOP BERKELEY'S publication of the *Analyst*[1] is the most spectacular event of the century in the history of British mathematics. The arguments in the *Analyst* were so many bombs thrown into the mathematical camp.

The views expressed in the *Analyst* are foreshadowed in Berkeley's *Principles of Human Knowledge* (§§ 123–134), published nearly a quarter of a century earlier. The "Infidel mathematician," it is generally supposed, was Dr. Halley. Mathematicians complain of the incomprehensibility of religion, argues Berkeley, but they do so unreasonably, since their own science is incomprehensible. "Our Sense is strained and puzzled with the perception of objects extremely minute, even so the Imagination, . . . is very much strained and puzzled to frame clear ideas of the least particles of time, or the least increments generated therein:

[1] *The Analyst: or, a Discourse addressed to an Infidel Mathematician. Wherein it is examined whether the Object, Principles, and Inferences of the Modern Analysis are more distinctly conceived, or more evidently deduced, than religious Mysteries and Points of Faith.* London, 1734.

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 still more difficult to conceive the abstracted velocities of such nascent imperfect entities. But the velocities of the velocities—the second, third, fourth, and fifth velocities, etc.—exceed, if I mistake not, all human understanding" (*Analyst*, § 4). . . .

75. "In the *calculus differentialis* . . . our modern analysts are not content to consider only the differences of finite quantities: they also consider the differences of those differences, and the differences of the differences of the first differences: and so on *ad infinitum*. That is, they consider quantities infinitely less than the least discernible quantity; and others infinitely less than those infinitely small ones; and still others infinitely less than the preceding infinitesimals, and so on without end or limit" (§ 6).

76. "I proceed to consider the principles of this new analysis. . . . Suppose the product or rectangle AB increased by continual motion: and that the momentaneous increments of the sides A and B are *a* and *b*. When the sides A and B are deficient, or lesser by one-half of their moments, the rectangle was $\overline{A - \frac{1}{2}a} \times \overline{B - \frac{1}{2}b}$, *i.e.* $AB - \frac{1}{2}aB - \frac{1}{2}bA + \frac{1}{4}ab$. And as soon as the sides A and B are increased by the other two halves of their moments, the rectangle becomes $\overline{A + \frac{1}{2}a} \times \overline{B + \frac{1}{2}b}$ or $AB + \frac{1}{2}aB + \frac{1}{2}bA + \frac{1}{4}ab$. From the latter rectangle subduct the former, and

the remaining difference will be aB + bA. Therefore the increment of the rectangle generated by the entire increments a and b is aB + bA. Q.E.D. But it is plain that the direct and true method to obtain the moment or increment of the rectangle AB, is to take the sides as increased by their whole increments, and so multiply them together, A + a by B + b, the product whereof AB + aB + bA + ab is the augmented rectangle; whence, if we subduct AB the remainder aB + bA + ab will be the true increment of the rectangle, . . . and this holds universally by the quantities a and b be what they will, big or little, finite or infinitesimal, increments, moments, or velocities" (§ 9). . . . The point of getting rid of ab cannot be obtained by legitimate reasoning." . . .

77. "The points or mere limits of nascent lines are undoubtedly equal, as having no more magnitude one than another, a limit as such being no quantity. If by a momentum you mean more than the very initial limit, it must be either a finite quantity or an infinitesimal. But all finite quantities are expressly excluded from the notion of a momentum. Therefore the momentum must be an infinitesimal. . . . For aught I see, you can admit no quantity as a medium between a finite quantity and nothing, without admitting infinitesimals" (§ 11).

78. Berkeley next premises the following *lemma*, which figures prominently in the debates about fluxions:

"'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.' This is so plain as to need no proof" (§ 12).

79. Berkeley examines now the method of obtaining the fluxion of x^n by writing $x+o$ in the place of x, expanding by the binomial formula, writing down the increments of x and x^n, which are in the ratio of

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

or, when the increment o is made to vanish, in the ratio of 1 to nx^{n-1}. Berkeley argues:

"But it should seem that this reasoning is not fair or conclusive. For when it is said, let the increments vanish, *i.e.* let the increments be nothing, or let there be no increments, the former supposition that the increments were something, or that there were increments, is destroyed, and yet a consequence of that supposition, *i.e.* an expression got by virtue thereof, is retained. Which, by the foregoing lemma, is a false way of reasoning. Certainly when we suppose the increments to vanish, we must suppose their proportions, their expressions, and everything else derived from the supposition of their existence, to vanish with them (§ 13).

. . . All which seems a most inconsistent way of arguing, and such as would not be allowed of in Divinity (§ 14). . . . Nothing is plainer than that no just conclusion can be directly drawn from two inconsistent suppositions (§ 15). . . . It may perhaps be said that [in the *calculus differentialis*] the quantity being infinitely diminished becomes nothing, and so nothing is rejected. But, according to the received principles, it is evident that no geometrical quantity can by any division or subdivision whatsoever be exhausted, or reduced to nothing. Considering the various arts and devices used by the great author of the fluxionary method; in how many lights he placeth his fluxions; and in what different ways he attempts to demonstrate the same point; one would be inclined to think, he was himself suspicious of the justness of his own demonstrations, and that he was not enough pleased with any notion steadily to adhere to it" (§ 17). . . .

80. "And yet it should seem that, whatever errors [in the *calculus differentialis*] are admitted in the premises, proportional errors ought to be apprehended in the conclusion, be they finite or infinitesimal: and that therefore the *ἀκρίβεια* of geometry requires nothing should be neglected or rejected. In answer to this you will perhaps say, that the conclusions are accurately true, and that therefore the principles and methods from whence they are derived must be so too. But . . . the truth of the conclusion will not prove either the form or the matter of a syllogism to be true" (§ 19).

81. Berkeley proceeds to show that correct results are derived from false principles by a *compensation of errors*, a view advanced again later by others, particularly by the French critic L. N. M. Carnot. Taking $y^2=px$, Berkeley says that the subtangent is not ydx / dy if dy is the true increment of y corresponding to dx; the accurate subtangent, obtained by similar triangles, is $ydx / (dy+z)$, where $z=dydy / (2y)$. That is, if dy is the true increment, then in ydx / dy there is an "error of defect." But in ydx / dy, as used in the differential calculus, the dy is not its true value, viz. $dy=pdx / (2y)-dydy / (2y)$ (obtained by writing $x+dx$ for x and $y+dy$ for y, in the equation $y^2=px$), but its erroneous value, $pdx / (2y)$. There is here an "error of excess." "Therefore the two errors being equal and contrary destroy each other (§ 21); . . . by virtue of a twofold mistake you arrive, though not at science, yet at truth." Berkeley gives other illustrations of cases where "one error is redressed by another."

82. "A point may be the limit of a line: a line may be the limit of a surface: a moment may terminate time. But how can we conceive a velocity by help of such limits? It necessarily implies both time and space, and cannot be conceived without them. And if the velocities of nascent and evanescent quantities, *i.e.* abstracted from time and space, may not be comprehended, how can we comprehend and demonstrate their proportions; or consider their *rationes primæ* and *ultimæ*? For, to

consider the proportion or *ratio* of things implies that such things have magnitude; that such their magnitudes may be measured" (§ 31). . . .

83. "If it be said that fluxions may be expounded or expressed by finite lines proportional to them; which finite lines, as they may be distinctly conceived and known and reasoned upon, so they may be substituted for the fluxions, . . . I answer that if, in order to arrive at these finite lines proportional to the fluxions, there be certain steps made use of which are obscure and inconceivable, be those finite lines themselves ever so clearly conceived, it must nevertheless be acknowledged that your proceeding is not clear nor your method scientific" (§ 34).

Berkeley discusses this matter with reference to a geometric figure, and argues that "a point therefore is considered as a triangle, or a triangle is supposed to be formed in a point. Which to conceive seems quite impossible" (§ 34). . . .

84. "And what are these fluxions? The Velocities of evanescent increments. And what are these same evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them the ghosts of departed quantities?" (§ 35). . . .

"And if the first [fluxions] are incomprehensible, what shall we say of the second and third fluxions, etc.?" (§ 44).

"To the end that you may more clearly comprehend the force and design of the foregoing remarks . . ., I shall subjoin the following Queries" (§ 50).

Then follow sixty-seven queries, of which the sixteenth is a good specimen: "*Qu.* 16. Whether certain maxims do not pass current among analysts which are shocking to good sense? And whether the common assumption, that a finite quantity divided by nothing is infinite, be not of this number?"

Jurin's First Reply to Berkeley

85. A reply to Berkeley's *Analyst* was made by the noted physician, James Jurin, at one time a student in Trinity College, Cambridge, who had imbibed Newtonian teachings from Newton himself. Jurin wrote under the pseudonym of "Philalethes Cantabrigiensis." The letter[1] is dated April 10, 1734.

86. Philalethes says that the charge in the *Analyst* "consists of three principal points: (1) Of Infidelity with regard to the Christian Religion. (2) Of endeavouring to make others Infidels, and succeeding in those endeavours by means of *the deference* which is paid *to their judgment, as being*

[1] *Geometry No Friend to Infidelity: or, a Defence of Sir Isaac Newton and the British Mathematicians, In a Letter to the Author of the Analyst. Wherein it is examined, How far the Conduct of such Divines as intermix the Interest of Religion with their private Disputes and Passions, and allow neither Learning nor Reason to those they differ from, is of Honour or Service to Christianity, or agreeable to the Example of our Blessed Saviour and his Apostles.* By Philalethes Cantabrigiensis. Ne Deus intersit, nisi dignus vindice nodus Inciderit. London: Printed for T. Cooper at the Globe in Ivy-Lane. MDCCXXXIV. Price 1s.

presumed to be of all men the greatest masters of reason. (3) Of error and false reasoning in their own science."

87. The early part of Jurin's reply is given to a discussion of the religious side. If there is no more certainty in modern analysis, argues Jurin, than in the Christian religion, this comparison brings no honour to Christianity; it is not true that mathematicians are infidels, leading others to infidelity. If it were true, this fact ought not in prudence to be published. Even if it be shown that the method of fluxions is built upon false principles, will it follow that all other parts of mathematics rest on inaccurate and false reasoning? Your attack, I surmise, is really, not so much in the interest of Christianity, as to demonstrate your superiority as a reasoner, by showing Newton and Barrow, two of the greatest mathematicians, less clear and just than you are. But because a mathematician "is thought to reason well in Geometry," his "decisions against the Christian Religion" will not "pass even upon weak and vulgar minds." "Sir Isaac Newton was a greater Mathematician than any of his contemporaries in France, . . . yet I have not heard that the French Mathematicians are converted to the Protestant Religion by his authority." Your objections against Newton's Fluxions may be "reduced under three heads: (1) Obscurity of this doctrine; (2) False reasoning in it by Sir Isaac Newton, and implicitly received by his followers; (3) Artifices and fallacies used by

Sir Isaac Newton, to make this false reasoning pass upon his followers." Jurin continues: "It must be owned that this doctrine . . . is not without difficulties," but "have you not altered his expressions in such a manner, as to mislead and confound your readers, instead of informing them," thereby increasing the difficulties? "Where do you find Sir Isaac Newton using such expressions as *the velocities of the velocities*, *the second*, *third and fourth velocities*, *the incipient celerity of an incipient celerity*, *the nascent augment of a nascent augment?*" As to the "moment or increment of the rectangle AB," the mathematicians take it to be $a\text{B}+b\text{A}$; you say that the rigorous value is $a\text{B}+b\text{A}+ab$. "Do not they know that in estimating any finite quantity how great soever . . ., a globe, suppose, as big as the earth, . . . or even the orb of the fixed stars . . ., this omission shall not cause them to deviate from the truth so much as a single pin's head, nay not the millionth part of a pin's head?" The operations by fluxions are no more objectionable than those by decimal fractions, where we take ·33333, etc., instead of $\frac{1}{3}$. You say that the Marquis de l'Hospital, in his *Analyse des infiniment petits*, Prop. 2, having found the fluxion of xy to be $xdy+ydx+dxdy$, drops the $dxdy$ "without the least ceremony." But does he not especially require in a postulate, "that a quantity, which is augmented or diminished by another quantity infinitely less than the first, may be considered as if it continued the same, *i.e.* had received no such augmentation or diminution?" As to Newton, he takes (*Principia*, lib. ii, lemma 2, cas. I; our § 17) initially $(A - \frac{1}{2}a)(B - \frac{1}{2}b)$ and finally $(A + \frac{1}{2}a)(B + \frac{1}{2}b)$, thereby deriving $aB + bA$, not as the increment of AB, but as the increment of $(A - \frac{1}{2}a)(B - \frac{1}{2}b)$. ". . . Rigorously speaking, the moment of the rectangle AB is not, as you suppose, the increment of the rectangle AB; but it is the increment of the rectangle $\overline{A - \frac{1}{2}a} \times \overline{B - \frac{1}{2}b}$." A moment may be either an increment or a decrement; you obtain the increment $aB + bA + ab$, the decrement of AB is $aB + bA - ab$. Which of those two will you call the moment of AB? "I apprehend the case will stand thus: $aB + bA + ab + aB + bA - ab$ making twice the moment of the rectangle AB; it follows that $aB + bA$ will make the single moment of the same rectangle";[1] the velocity which the flowing rectangle has, is its velocity "neither before nor after it becomes AB, but at the very instant of time that it is AB." In like manner with the moment of the rectangle. Let me advise you hereafter to "first examine and weigh every word he [Newton] uses." Lastly, I must observe that the moment of AB, namely $aB + bA$, and the increment of the same rectangle, $aB + bA + ab$, "are perfectly and exactly equal, supposing a and b to be diminished *ad infinitum*."

88. As to your second instance of false reasoning, in Newton's book on *Quadratures*, apparently that is "so truly Bœotian a blunder" that I know not how "a Newton could be guilty of it." You

[1] Jurin, *op. cit.*, p. 46.

interpret "*Evanescant jam augmenta illa*" (our § 32), as "let now the increments vanish, *i.e.* let the increments be nothing, or let there be no increments." But "do not the words *ratio ultima* stare us in the face, and plainly tell us that though 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?" You grossly misinterpreted Newton.

89. As to the third head of your objections, since Newton did not reason falsely, "he had no occasion to make use of arts and fallacies to impose upon his followers." "Having now . . . driven you entirely out of your intrenchments . . . I should sally out and attack you in your own." "But as they seem rather designed for shew, than use, . . . to dazzle the imagination . . . [they] will likewise immediately disappear like *the Ghost of a departed quantity*," if you exorcise them "with a few words out of the first section of the *Principia*." You say that the paradox, "that Mathematicians should deduce true Propositions from false Principles" is accounted for by the fact that one error "is compensated by another contrary and equal error." But the two are no errors at all, as is evident from the fact that true results follow when only the first operation is carried out, so that no compensation is possible. Jurin argues that the first supposed fallacy, without the second, gives as the subtangent of $y^2 = ax$, the value $2x(2y + dy) \div (2y)$; the second supposed fallacy,

without the first, gives $2x(2y) \div (2y+dy)$. Both these expressions are equal to $2x$, "which is the result either of two errors, or of none at all." If you claim that $2x(2y+dy) \div (2y) > 2x$, how much greater is it, supposing $2x = 1000$ miles? Not as much as the thousand-millionth part of an inch. Jurin ends with a discussion of Lock on abstract ideas.

Walton's First Reply to Berkeley

90. Little is known about John Walton. He was Professor of Mathematics in Dublin, and participated in this controversy. Otherwise, practically nothing about him has been handed down.

His reply to Berkeley was published in 1735 at Dublin.[1] Berkeley attacked the method of fluxions more particularly as given in Newton's earlier exposition; Walton defended the theory on the basis of the later treatment as given by Newton in his *Quadratura Curvarum* (1704), and in the *Principia*, Book II.

91. Walton begins by stating that inasmuch as the credulous may "become infected" by Berkeley's attack on fluxions, it seems necessary to give a short account of the nature of fluxions. "The momentaneous Increments or Decrements of flow-

[1] *A Vindication of Sir Isaac Newton's Principles of Fluxions, against the Objections contained in the Analyst.* By J. Walton.—Siquid novisti rectius istis, candidus imperti: Si non, his utere mecum. *Hor.* In the fulness of his Sufficiency he shall be in Straits: Every Hand of the *Wicked* shall come upon him. *Job.*—Dublin, Printed; and reprinted at London, and sold by J. Roberts in Warwick-Lane. 1735. [Price Six Pence.]

ing Quantities, he [Newton] elsewhere calls by the name of *Moments*, . . : By Moments we may understand the nascent or evanescent Elements or Principles of finite Magnitudes, but not Particles of any determinate Size, or Increments actually generated; for all such are Quantities, themselves generated of Moments."

92. "The magnitudes of the momentaneous Increments or Decrements of Quantities are not regarded in the Method of Fluxions, but their first or last Proportions only; that is, the Proportions with which they begin or cease to exist." . . . "The ultimate Ratios with which synchronal Increments of Quantities vanish, are not the Ratios of finite Increments, but Limits which the Ratios of the Increments attain, by having their magnitudes infinitely diminish'd. . . . There are certain determinate Limits to which all such Proportions perpetually tend, and approach nearer than by any assignable Difference, but never attain before the Quantities themselves are infinitely diminish'd; or 'till the Instant they evanesce and become nothing." "The Fluxions of Quantities are very nearly as the Increments of their Fluents generated in the least equal Particles of Time," and they "are accurately in the first or last Proportions of their nascent or evanescent Increments." "The Fluxions of Quantities are only velocities. . . ." Again, ". . . to obtain the Ratios of Fluxions, the corresponding synchronal or isochronal Increments must be lessened *in infinitum*. For the

Magnitudes of synchronal or isochronal Increments must be infinitely diminished and become evanescent, in order to obtain their first or last Ratios, to which Ratios the Ratios of their corresponding Fluxions are equal." The moment of the rectangle AB is $Ab+Ba$, for consider $Ab+Ba+ab$ and $Ab+Ba$, "under a constant Diminution of the Increments a and b . . . [they] constantly tend to an Equality . . . [and] they become equal, and their Ratio becomes a Ratio of Equality. . . ." Hence $Ab+Ba+ab$ "is not the Moment or Fluxion of the Rectangle AB, except in the very Instant when it begins or ceases to exist." Here fluxions appear to be no longer velocities (finite magnitudes) but moments. Walton next quotes a Latin passage from the *Quadratura Curvarum.* He says that Berkeley seems "to have been deceived by an Opinion that there can be no first or last Ratios of mathematical Quantities," but Walton insists that if quantities are generated together, or if they vanish together, they will do so "under certain Ratios, which are their first or last Ratios." Walton claims that Berkeley's lemma "is in no Way pertinent to the Case for which it was intended"; he explains the Newtonian process of finding the fluxion of x^n, supposing x to increase uniformly, and points out that this is done without rejecting quantities "on account of their exceeding smallness." Commenting on Berkeley's contention that "no geometric Quantity, by being infinitely diminished, can ever be exhausted or become nothing," Walton states that the fluxional calculus assumes that "Quantities can be generated by Motion . . . and consequently they may also by Motion be destroy'd."

93. Walton's *Vindication* follows Newton's exposition closely; Berkeley's claim that Walton followed in Jurin's track and borrowed from him, is, I believe, incorrect. Take the vital question of rejecting infinitesimals: Jurin claims that, being so very small, they do not appreciably affect the result; Walton takes the stand that there is no rejection whatever of infinitesimals. The main criticism to be passed on Walton's first essay consists, in our judgment, in a failure to meet Berkeley's objections squarely and convincingly.

Berkeley's Reply to Jurin and Walton

94. Jurin's and Walton's articles were answered by Berkeley in a publication entitled, *A Defence of Free-Thinking in Mathematics.*[1]

Berkeley restates the purpose he had in writing the *Analyst*: "Now, if it be shewn that fluxions are really most incomprehensible mysteries, and that those who believe them to be clear and scientific do entertain an implicit faith in the author of that method: will not this furnish a fair *argumentum ad hominem* against men who reject that very thing in religion which they admit in human learn-

[1] *A Defence of Free-Thinking in Mathematics. In Answer to a Pamphlet of Philalethes Cantabrigiensis. . . . Also an Appendix concerning Mr. Walton's Vindication. . . . By the Author of "The Minute Philosopher,"* Dublin, 1735.

ing?[1] (§ 3) . . . I say that an infidel, who believes the doctrine of fluxions, acts a very inconsistent part in pretending to reject the Christian religion —because he cannot believe what he doth not comprehend" (§ 7). . . .

[1] Berkeley is not the only one who invoked the aid of the Doctrine of Fluxions in theological discussion. In a criticism (*A Review of the Fiery Eruption*, etc., London, 1752, p. 128) of Bishop William Warburton's *Julian*, concerning earthquakes and fiery eruptions, which, Warburton argued, defeated Julian's attempt to rebuild the temple at Jerusalem, it is stated that a connection (needed in the argument) was established between the preservation of Christianity and the destruction of Judaism by the following clever procedure:—

"The great modern Father of the mathematics had invented a new and curious way of improving that science by a fiction; according to which quantities are supposed to be generated by the continual flux or motion of others. In the application of this method it became necessary to consider these quantities, sometimes in a nascent, and at other times in an evanescent state, by which ingenious contrivance they could be made either continually to tend to and at last absolutely to become nothing, or *vice versa*, according to the intention and occasions of the Artist. Now by extending this noble invention to the two religions, it evidently appeared, that, from the time of the first coming of Christ, Judaism entered into its *evanescent* state, as on the other hand Christianity did into a *nascent* state, by which means both being put into a proper flux, one was seen continually decaying, and the other continually improving, till at last by the destruction of the Temple Judaism actually vanished and became nothing, and the Christian religion then bursted out a perfectly generated Entity. . . . As the great author of the mathematical method of fluxions had for very good reasons studiously avoided giving any definition of the precise magnitude of those moments, by whose help he discovers the exact magnitude of the generated quantities, so our Author [Warburton] by the same rule of application, and under the influence of the same authority, was fairly excused from defining that precise degree of perfection and imperfection in which the two religions subsisted, during the respective *evanescent* and *nascent* state of each, by the help of which he discovered the precise time when Judaism was perfectly abolished, and Christianity perfectly established. But we may well suppose, that the most alluring charm in this extraordinary piece of ingenuity, was the creating of a new character by it: For questionless he may now be justly stiled the great founder and inventor of the *fluxionary method* of theology. . . . This fancy of a necessary connexion between the Temple-edifice, and the being of Christianity, . . . this pretended Christianity which is of such an unsubstantial nature, that it must necessarily vanish at the restoration of the Temple, can be nothing else but a mere *Ghost*, . . . evidently the Ghost of departed Judaism."

95. "I have said (and I venture still to say) that a fluxion is incomprehensible: that second, third, and fourth fluxions are yet more incomprehensible: that it is not possible to conceive a simple infinitesimal: that it is yet less possible to conceive an infinitesimal of an infinitesimal, and so onward. What have you to say in answer to this? Do you attempt to clear up the notion of a fluxion or a difference? Nothing like it" (§ 17).

96. Berkeley quotes from Newton's *Principia* and *Quadrature of Curves*, and then asks, "Is it not plain that if a fluxion be a velocity, then the fluxion of a fluxion may, agreeably thereunto, be called the velocity of a velocity? In like manner, if by a fluxion is meant a nascent augment, will it not then follow that the fluxion of a fluxion or second fluxion is the nascent augment of a nascent augment?" (§ 23).

97. "I had observed that the great author had proceeded illegitimately, in obtaining the fluxion or moment of the rectangle of two flowing quantities. . . . In answer to this you allege that the error arising from the omission . . . is so small that it is insignificant (§ 24). . . . If you mean to defend the reasonableness and use of approximations . . . I have nothing to say. . . . That the method of fluxions is supposed accurate in geometrical rigour is manifest to whoever considers what the great author writes about it . . . *In rebus mathematicis errores quam minimi non sunt contemnendi*" (§ 25; our § 30).

98. Berkeley justifies his use of the expression "increment of a rectangle" by quoting from Newton (our § 17), "*rectanguli incrementum* $aB + bA$."

"You say 'you do not consider AB as lying at either extremity of the moment, but as extended to the middle of it; as having acquired the one half of the moment, and as being about to acquire the other; or, as having lost one half of it, and being about to lose the other.' Now, in the name of truth, I entreat you to tell what this moment is, . . . Is it a finite quantity, or an infinitesimal, or a mere limit, or nothing at all? . . . If you take it in either of the two former senses, you contradict Sir Isaac Newton. And, if you take it in either of the latter, you contradict common sense; it being plain that what hath no magnitude, or is no quantity, cannot be divided" (§ 30).

". . . You observe that the moment of the rectangle determined by Sir Isaac Newton, and the increment of the rectangle determined by me are perfectly and exactly equal, supposing a and b to be diminished *ad infinitum*: and, for proof of this, you refer to the first lemma of the first section of the first book of Sir Isaac's Principles. I answer that if a and b are real quantities, then ab is something, and consequently makes a real difference: but if they are nothing, then the rectangles whereof they are coefficients become nothing likewise: and consequently the *momentum* or *incrementum*, whether Sir Isaac's or mine, are in that case nothing at all. As for the above-mentioned lemma, . . . however that way of reasoning may do in the method of *exhaustions*, where quantities less than assignable are regarded as nothing; yet, 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" (§ 32).

99. As regards Newton's *evanescant jam augmenta illa* (our § 32), Berkeley argues that it means either "let the increments vanish," or else "let them become infinitely small," but the latter "is not Sir Isaac's sense," since on the very same page in the Introduction to the Quadrature of Curves he says that there is no need of considering infinitely small figures. Taking advantage of the fact that the Newton of the *Principia* (1687) differed from the Newton of the *Quadratura Curvarum* (1704), Berkeley broke out into the following philippic: "You Sir, with the bright eyes, be pleased to tell me, 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 (our § 12): *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 (our § 33): *Volui ostendere quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere.* 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 his Principles (our § 17): *Ubi de lateribus A et B deerant momentorum dimidia*, etc.—where the moments are supposed to be divided. I should be very glad a person of such a luminous intellect would be so good as to explain whether by fluxions we are to understand the nascent or evanescent quantities themselves, or their motions, or their velocities, or simply their proportions . . . that you would then condescend to explain the doctrine of the second, third, and fourth fluxions, and show it to be consistent with common sense if you can" (§ 36).

100. In an appendix to the *Defence of Free-Thinking in Mathematics*, Berkeley replies to Walton, stating that the issues raised by him had been previously raised by "the other," that he delivered a technical discourse without elucidating anything, that his scholars have a right to be informed as to the meaning of fluxions and should therefore ask him "the following questions." Then follow many questions, of which we give a few:

"Let them ask him—Whether he can conceive velocity without motion, or motion without extension, or extension without magnitude? . . . Whether nothing be not the product of nothing multiplied by something; and, if so, . . . when ab is nothing, whether $Ab + Ba$ be not also nothing? *i.e.* whether the momentum of AB be not nothing? Let him then be asked, what his momentums are good for, when they are thus brought to nothing? . . . I wish he were asked to explain the difference between a magnitude infinitely small and a magnitude infinitely diminished. . . . Let him be farther asked, how he dares to explain the method of Fluxions, by the *Ratio* of magnitudes infinitely diminished, when Sir Issac Newton hath expressly excluded all consideration of quantities infinitely small? If this able vindicator should say that quantities infinitely diminished are nothing at all, and consequently that, according to him, the first and last *Ratio's* are proportions between nothings, let him be desired to make sense of this. . . . If he should say the ultimate proportions are the *Ratio's* of mere limits, then let him be asked how the limits of lines can be proportioned or divided?"

Walton's Second Reply to Berkeley

101. In a second reply[1] to Berkeley, Walton states that in the Appendix to the *Defence*, Berkeley "has composed a Catechism which he recommends to my Scholars" and which Walton quotes. I am first to be asked, "Whether I can conceive Velocity without Motion, or Motion without Extension. . . .

[1] J. Walton, *Catechism of the Author of the Minute Philosopher Fully answer'd.* Printed at Dublin. Reprinted at London, and sold by J. Roberts, 1735. It is a pamphlet of 30 pages.

I answer, I can conceive Velocity and Motion in a Point of Space; that is, without any assignable Length or Extension described by it . . . for . . . if a cause acts continually upon a given Thing . . . there must be a continual Increase of its Velocity: the Velocity cannot be the same in any two different Points," as in the case of falling bodies. Referring to $Ab+Ba$, Walton continues: "I agree with him that nothing is the Product of nothing multipl'd by something; but must know what he means by the vanishing of the Gnomon[1] and Sum of the two Rectangles . . . before I give him a direct Answer. If by vanishing he means that they vanish and become nothing as Areas, I grant they do; but absolutely deny, upon such an Evanescence of the Gnomon and Sum of the two Rectangles by the moving back of the Sides of the Gnomon till they come to coincide with those of the Rectangle, that nothing remains. For there still remain the moving Sides, which are now become the Sides of the Rectangle, . . . the Motion of the Gnomon is the same with the Sum of the Motions of the Two Rectangles, when they evanesce, and are converted into the two Sides of the Rectangle AB. If a point moves forward to generate a Line, and afterwards the same Point moves back again to destroy the Line with the very same Degrees of Velocity, in all Parts of the Line

[1] If a parallelogram is extended in length and breadth and if the original parallelogram be removed, the remaining figure is called the gnomon.

which it had in those Parts when moving forward to generate it; in the Instant the Line vanishes as a Length . . . the generating point will remain, together with the Velocity it had at the very Beginning of its Motion. And the Case is the very same with respect to the Rectangle increasing by the Motion of its Sides." This point is elaborated with great fullness. After some illustrations, Walton exclaims: "This is a full and clear Answer to this part of the catechism, and shows that its Author has been greatly mistaken in supposing that I explained the Doctrine of Fluxions by the *Ratio of Magnitude's infinitely diminish'd*, or by *Proportions between nothings*. . . . I do not wonder that this Author should have no clear Ideas or Conceptions of second, third or fourth Fluxions, when he has no clear Conceptions of the common Principles of Motion, nor of the first and last Ratios of the isochronal Increments of Quantities generated and destroyed by Motion. . . . In order to prevent my being Catechised any more by this Author," Walton makes a confession "of some Part of my Faith in Religion."

Jurin's Second Reply to Berkeley

102. Jurin brought out a second publication,[1] of 112 pages, which was in reply to Berkeley's *Defence of Free-Thinking*. Passing by unimportant preliminaries, we come to Jurin's definitions of "flow-

[1] *The Minute Mathematician: or, The Free-Thinker no Just-Thinker*. By Philalethes Cantabrigiensis. London, 1735.

ing quantity," "fluxion" ("the velocity with which a flowing quantity increases or decreases"), "increment," "nascent increment" ("an increment just beginning to exist from nothing . . . but not yet arrived at any assignable magnitude how small soever"), "evanescent increment" (similarly defined). He then endeavours to prove the proposition: "The Fluxions, or Velocities of flowing quantities . . . are exactly in the first proportion of the nascent increments, or in the last proportion of the evanescent increments." He insists that "the first ratio of the nascent increments must be the same, whether the velocities be uniform or variable"; hence, "the nascent increments must be exactly as the velocities with which they begin to be generated." In further explanation, Jurin says that, according to Newton, nascent increments are "less than any finite magnitude," "their magnitude cannot be assigned or determined," "the proportion between them . . . being all that is requisite in his Method." In further explanation of the proportion of evanescent increments he says, it "is not their proportion before they vanish," "nor is it their proportion after they have vanished," "but it is their proportion at the instant that they vanish." Jurin then states that Berkeley has "taken as much pains as . . . any man living, except a late Philosopher of our University, to make nonsense of Sir Isaac Newton's principles." There is no occurrence in Newton's writings of "velocity without motion," "motion without extension," which

Berkeley pretends to derive from them. Jurin succeeds, we think, in establishing the contention that there is no greater difficulty in explaining the second or third fluxion, than there is in explaining the first. "The second fluxion is the velocity with which the first fluxion increases." Jurin confesses that his statement in his first reply to Berkeley, to the effect that certain errors were of "no significance in practice," was intended for popular consumption, for men such as one meets in London.

103. "One of them, indeed, could make nothing of what I had said about the length of a subtangent, or the magnitude of the orb of the fixed stars; but was fully satisfied by the information given him by one of his acquaintance to the following effect. The Author of the Minute Philosopher has found out that, if Sir Isaac Newton were to measure the height of St. Paul's Church by Fluxions, he would be out about three quarters of a hair's breadth: But yonder is one *Philalethes* at Cambridge, who pretends that Sir Isaac would not be out above the tenth part of hair's breadth. Hearing this, and that two books had been written in this controversy, the honest gentleman flew into a great passion, and after muttering something to himself about some body's being overpaid, he went on making reflections, which I don't care to repeat, as not being much for your honour or mine."

104. Jurin thereupon takes up the rectangle AB. The terms "moment" and "increment" are involved in the discussion of it. Jurin de-

clares: "I absolutely and fully agree with you that the *incrementum* in the conclusion is the *momentum* in the Lemma," that "the *momentum* in the Lemma" is "the *momentum* of the rectangle AB." Further, Jurin says, "the *incrementum* in the conclusion is manifestly the excess of the rectangle $\overline{A+\frac{1}{2}a}\times\overline{B+\frac{1}{2}b}$, above the rectangle $\overline{A-\frac{1}{2}a}\times\overline{B-\frac{1}{2}b}$, i.e. the increment of the rectangle $\overline{A-\frac{1}{2}a}\times\overline{B-\frac{1}{2}b}$. Therefore we are agreed that the moment of the rectangle AB is the increment of the rectangle $\overline{A-\frac{1}{2}a}\times\overline{B-\frac{1}{2}b}$. Consequently you were mistaken in supposing that the moment of the rectangle AB was the increment of the same rectangle AB. . . . The moment AB is neither the increment nor the decrement of AB," for if it really was the increment of AB, and also its decrement, we would have $Ab+Ba+ab=Ab+Ba-ab$, i.e. $2ab=0$. Hence the rectangle ab "is by his own confession equal to nothing." Jurin concludes that the fluxion of AB is not the velocity with which the increment or decrement of AB is generated, but the "middle arithmetical proportional between these two velocities," this being "in like manner as I had before supposed an arithmetical mean between the increment and decrement of AB, which mean is the moment of AB." Berkeley had considered four definitions of a moment, that of a finite quantity, or an infinitesimal or a mere limit, or nothing at all; and he had found each either to contradict Newton or to contradict common sense. Jurin does not accept "any one of those senses." A moment, says Jurin, is defined by Newton as "nascent increment," its magnitude is "utterly unassignable." Jurin continues:

105. "You seem much at a loss to conceive how a nascent increment, a quantity just beginning to exist, but not yet arrived to any assignable magnitude, can be divided or distinguished into two equal parts. Now to me there appears no more difficulty in conceiving this, than in apprehending how any finite quantity is divided or distinguished into halves. For nascent quantities may bear all imaginable proportions to one another, as well as finite quantities."

106. Near the close Jurin enters upon the discussion of Berkeley's *Lemma*, given in the *Analyst*: "If one supposition be made, and be afterwards destroy'd by a contrary supposition, then everything that followed from the first supposition, is destroyed with it." Not so, says Jurin, when the supposition and its contradiction are made at different times. "Let us imagine yourself and me to be debating this matter, in an open field, . . . a sudden violent rain falls . . . we are all wet to the skin . . . it clears up . . . you endeavour to persuade me I am not wet. The shower, you say, is vanished and gone, and consequently your . . . wetness . . . must have vanished with it." You say that your explanation of the correctness of results as due to a compensation of errors, was intended by you to apply, not to Newton, but to Marquis de l'Hospital; your statements were such that not I

alone, but Mr. Walton as well, inferred that you were charging Newton with committing double errors. The rest of Jurin's ill-arranged article is given either to a renewed and fuller elucidation of his previous contentions or to poetical outbursts. Sure of the soundness of his exposition, he exclaims, "I meet with nothing in my way but *the Ghosts of departed* difficulties and objections."

Berkeley's Second Reply to Walton

107. Walton's *Catechism . . . fully Answered* was followed by Berkeley's *Reasons for not replying to Mr. Walton's Full Answer*, 1735. This last reply has been called "a combination of reasoning and sarcasm," in which "he affects to treat his opponent as a disguised convert." Says Berkeley: "He seems at bottom a facetious man, who, under the colour of an opponent, writes on my side of the question, and really believes no more than I do of Sir Isaac Newton's doctrine about fluxions, which he exposes, contradicts, and confutes, with great skill and humour, under the mask of a grave vindication." Berkeley objects to Walton's motion and velocity "in a point" of space; "consider the reasoning: The same velocity cannot be in two points of space; therefore velocity can be in a point of space. . . . I can as easily conceive Mr. Walton should walk without stirring, as I can his idea of motion without space."[1] Newton calls absolute

[1] Walton is not consistent in his use of the term "motion." In some passages it means translation; in others it means velocity, or else both

motion "a translation from absolute place to absolute place,"[1] and relative motion, "from one relative place to another. Mr. Walton's is plainly neither of these sorts of motion"; hence, he argues against Newton. "When *ab* is nothing, that is, when *a* and *b* are nothing, he denies that A*b*+B*a* is nothing. This is one of the inconsistencies which I leave the reader to reconcile." In his *Vindication* he holds that, "to obtain the last ratio of synchronal increments, the magnitude of those increments must be infinitely diminished"; in his *Catechism . . . fully Answered* "he chargeth me as greatly mistaken in supposing that he explained the doctrine of fluxions by the ratio of magnitudes infinitely diminished."[2] In his *Catechism . . . fully Answered* "he tells us that 'fluxions are measured by the first and last proportion of isochronal increments generated or destroyed by motion.' A little later he says, these ratios subsist when the isochronal increments have no magnitude." Can "isochronal increments subsist when they have no magnitude"? Berkeley

translation and velocity, as when he says, ". . . isochronal increments must be made to vanish by a Retroversion of the Motion before we can obtain the Motions with which they vanish, or begin to be generated; that is, before we can obtain the *Fluxions* of the Quantities, the Name given by Sir Isaac Newton to those Motions." J. Walton, *Catechism . . . fully Answered*, pp. 18, 19.

[1] I. Newton, *Principia*, Definitions, Scholium, def. viii.

[2] What Walton actually wrote was, that Berkeley had been mistaken in supposing that he explained fluxions "by the *Ratios of Magnitudes infinitely diminish'd*, or by *Proportions between nothings*." Three pages earlier Walton had denied that Newton and he measured fluxions "by the Proportions of Magnitudes infinitely small." Evidently Walton meant to exclude the "infinitely small," but used "magnitudes infinitely diminished" at one time as magnitudes "infinitely small," and at another time as signifying something else, namely, "increments" that "vanish."

then quotes from his own *Analyst*: "As it is impossible to conceive velocity without time or space, without either finite length or finite duration, it must seem above the power of man to comprehend even the first fluxions." In the endeavour to explain this matter, Walton's skill has been "vain and impertinent."

The Second Edition of Walton's Second Reply

108. Walton begins[1] by explaining what Newton means by Velocity. It is "the ratio of the Quantity of Motion to the Quantity of Matter in the body"; that is, if V is the velocity, M the quantity of motion, F the force generating the motion, D the density, B the bulk or magnitude, W the weight, then "V is M / Q, and is as F / W, or as F / DB," for, "the Quantity of Motion is the Quantity of Matter and Velocity taken together; that is, M is QV" (p. 35). "The Author [Berkeley] therefore has been grossly mistaken in asserting that *Velocity* necessarily implies both Time and Space, and cannot be conceived without them.—And that there is *no* Measure of Velocity except Time and Space." It appears that "a body in Motion, will have a Velocity inherent in itself during the Whole Time of its Motion: and consequently there must be a Velocity where-ever the Body is, exclusive of Time and Space . . . its

[1] *The Catechism of the Author of the Minute Philosopher fully answer'd. The Second Edition. With an Appendix, in Answer to the Reasons for not replying to Mr. Walton's Full Answer.* By J. Walton . . . Dublin: Printed by S. Powell, for William Smith at the Hercules, Bookseller: in Dame-Street, 1735.

[a point's] Velocity will exist in a Point, and successively will exist in every Point of Space through which the Point moves" (p. 37). Berkeley thinks that "from the generated Velocity not being the same in any two different Points of the described Space it will not follow that Velocity can exist in a Point of Space. But in this he is mistaken. For the continual Action of a Moving Force necessarily preserves a *continual* Velocity; and if the generated Velocity be not the same in any two different Points of the described Space, a Velocity must of Consequence exist in every Point of that Space" (p. 38). This account of velocity "is agreeable to Sir Isaac Newton's Notion of Velocity; who constantly excludes described Space from his Idea of that Term." Motion being measured by QV, "the continual translation of a Body therefore into a new Place is, . . . an *Effect* of this Tendency forward in the Body, and not the Tendency itself; consequently Space described is an *Effect* of Velocity, and not Velocity itself" (p. 47). On the question of first and last ratios it cannot be said that Walton here throws new light. He insists that he explained fluxions not "by the Ratio of Magnitudes infinitely diminish'd, but by the first and last Ratios of Increments generated or destroyed in equal times: that is, by the Ratios of the Velocities with which those Increments begin or cease to exist" (p. 53). To Berkeley's charge that Walton "supposed two Points to exist at the same Time in one Point, and to be moved different

Ways without stirring from that Point," Walton replies that there is no difficulty in supposing two points existing in a given place each having its own velocity, but he never said that they can go in different directions "without stirring from the Point." Berkeley, in his remarks about the fourth fluxion of a cube, did not observe all the conditions which he [Walton] had imposed. "He [Berkeley] intreats me to explain whether Sir Isaac's Momentum be a finite Quantity, or an Infinitesimal, or a mere Limit. I tell him, that Sir Isaac's Momentum is a *finite* quantity; it is a Product contained under the moving Quantity and its Velocity, or under the moving Quantity and first Ratio of that Space described by it in a given Particle of Time." Since both these factors are finite, the product is finite (p. 62). "By Moments therefore he is not to understand generated Increments of Fluents, but certain finite Products or Quantities of very different Nature from generated Increments, expressing only the Motions with which those Increments begin or cease to exist" (p. 63).

Remarks

109. Berkeley's *Analyst* must be acknowledged to be a very able production, which marks a turning-point in the history of mathematical thought in Great Britain.

His contention that no geometrical quantity can be exhausted by division[1] is in consonance with

[1] See our § 79.

the claim made by Zeno in his "dichotomy," and the claim that the actual infinite cannot be realised. The modern reader may not agree with Berkeley on this point, nor in the claim that second or third fluxions are more mysterious than the first fluxion. Nevertheless, a reader of Berkeley feels that he spoke in the *Analyst* with perfect sincerity. Interesting is De Morgan's comment:[1] "Dishonesty must never be insinuated of Berkeley. But the *Analyst* was intentionally a publication involving the principle of Dr. Whately's argument against the existence of Buonaparte; and Berkeley was strictly to take what he found. The *Analyst* 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."

110. Sir William Rowan Hamilton once wrote De Morgan: "On the whole, I think that Berkeley persuaded *himself* that he was in earnest against Fluxions, especially of orders higher than the first, as well as against matter." To this De Morgan replied: "I have no doubt Berkeley knew that the fluxions were sound enough."[2]

[1] A. De Morgan, *Philosophical Magazine*, 4 S., vol. iv, 1852, p. 329, note.

[2] *Life of Sir William Rowan Hamilton*, by R. P. Graves, vol. iii, 1889, p. 581.

111. One is not so easily convinced of the ability and sincerity of Jurin. That at first he should argue that quantities may be dropped because small, and afterwards admit that this argument was intended for popular consumption, is not reassuring.[1] That he should fail to see the soundness of Berkeley's criticism of Newton's proof $(A+\frac{1}{2}a)(B+\frac{1}{2}b)-(A-\frac{1}{2}a)(B-\frac{1}{2}b)$ for the increment of AB is somewhat surprising, even if it must be admitted that neither Walton nor any other eighteenth-century mathematician appears to have seen and admitted the defect. In this connection we quote from a letter which Hamilton wrote De Morgan in 1862 when Hamilton was seeing his *Elements of Quaternions* through the press:[2]

"When your letter arrived this morning, I was deep in Berkeley's 'Defence of Freethinking in Mathematics'; . . . I think there is more than mere plausibility in the Bishop's criticisms on the remarks attached to the Second Lemma of the Second Book of the Principia; and that it is very difficult to understand the *logic* by which Newton proposes to prove, that the *momentum* (as he calls it) of the *rectangle* (or product) AB is equal to $aB+bA$, if the *momenta* of the sides (or factors) A and B be denoted by a and b. His mode of getting rid of ab appeared to me long ago (I must confess it) to involve so much of *artifice*, as to

[1] See our §§ 97, 102, 103.

[2] *Life of Sir William Rowan Hamilton*, by R. P. Graves, vol. iii, p. 569.

deserve to be called *sophistical*; although I should not like to say so publicly. He subtracts, you know, $(A-\frac{1}{2}a)(B-\frac{1}{2}b)$ from $(A+\frac{1}{2}a)(B+\frac{1}{2}b)$; whereby, of course, *ab* disappears in the result. But by *what right*, or *what reason* other than to give an unreal air of *simplicity* to the calculation, does he *prepare* the *products* thus? Might it not be argued similarly that the difference,

$$(A+\tfrac{1}{2}a)^3-(A-\tfrac{1}{2}a)^3=3aA^2+\tfrac{1}{4}a^3,$$

was the moment of A^3; and is it not a sufficient *indication* that the mode of procedure adopted is not the fit one for the subject, that it quite *masks* the notion of a *limit*; or rather has the appearance of treating that notion as foreign and irrelevant, notwithstanding all that had been said so well before, in the First Section of the First Book? Newton does not seem to have cared for being very consistent in his *philosophy*, if he could anyway get hold of *truth*, or what he considered to be such. . . ."

We give also Hermann Weissenborn's objection[1] to Newton's procedure of taking half of the increments a and b; with equal justice one might take, says he, $(A+\frac{2}{3}a)(B+\frac{2}{3}b)-(A-\frac{1}{3}a)(B-\frac{1}{3}b)$, and the result would then be $Ab+Ba+\frac{1}{3}ab$.

112. Walton's two (or three) articles do not seem to have been read much. They are seldom mentioned. The pamphlets are now rare. Pro-

[1] H. Weissenborn, *Principien der höheren Analysis in ihrer Entwickelung von Leibniz bis auf Lagrange*, Halle, 1856, p. 42.

fessor G. A. Gibson had not seen them when he wrote on the *Analyst* controversy.[1] Walton seemed to have a good intuitive grasp of fluxions, but lacked deep philosophic insight. He showed himself inexperienced in the conduct of controversies, and did not know how to protect himself against attack from a skilful adversary.

113. It is worthy of notice that Walton[2] expressed himself on the nature of limits, by claiming that the limit was reached. As to the nature of "variable velocity," it is interesting to see that Berkeley realised the difficulty of the concept, and probably saw that there was no variable velocity as a physical fact, while Walton was compelled to take refuge in less primitive mechanical concepts in order to uphold his side of the argument.[3] Unjustifiable is Walton's identification of Newton's "moment" with "momentum" of mechanics.

114. Berkeley's Lemma[4] was rejected by Jurin and Walton. We shall see that it found no recognition from mathematicians in England during the eighteenth century, but was openly and repeatedly accepted as valid in its application to limits, by Woodhouse at the beginning of the nineteenth century. The Newtonian derivation of the fluxion of x^n (see our §§ 32, 41), accomplished by dividing both o and $(x+o)^n-x^n$ by the finite increment o, and then putting o equal to zero in the quotient, is

[1] G. A. Gibson, "The Analyst Controversy," in *Proceedings of the Edinburgh Math. Soc.*, vol. xvii, 1899, p. 18.

[2] See our § 92. [3] See our § 108. [4] See our §§ 78, 92, 106.

certainly open to the logical objection raised by Berkeley. Eighteenth-century mathematicians did not attach due importance to this point.

115. The existence of infinitesimals (infinitely small quantities) was denied by Berkeley, but, it would seem, not denied by Jurin and Walton. All three finally abjured the philosophy which permits their being dropped because so small. It is well known that many mathematicians of prominence have believed in the reality of such quantities. From Leibniz to Lagrange all Continental writers of note used them. Lagrange headed a small school that was opposed to them, when he published his *Fonctions analytiques*. There followed a reaction against Lagrange. De Morgan once remarked: "Duhamel, Navier, Cournot, are pure infinitesimalists. Some of them say an infinitely small quantity is one which may be made as small *as you please*. This is an evasion; but they do not mean that *dx* is *finite*. . . . By-the-way, Poisson was a believer in the *reality* of infinitely small quantities—as I am."[1]

". . . For myself, I am now fixed in the faith of the *subjective reality* of *infinitesimal quantity*. But *what* an infinitely small quantity is, I know no more than I know what a *straight line* is; but I know it *is*; and there I stop short. But I do not believe in *objectively realised* infinitesimals."

[1] *Life of Sir William Rowan Hamilton*, by Robert P. Graves, vol. iii, pp. 572, 580. Consult also De Morgan's article, "On Infinity; and on the Sign of Equality," in *Trans. of the Cambridge Phil. Society*, vol. xi, Cambridge, 1871 [read May 16, 1864].

116. We must not neglect to express our appreciation of the fact that Berkeley withdrew from the controversy after he had said all that he had to say on his subject. Some of the debates that came later were almost interminable, because the participants continued writing even after they had nothing more to say.

CHAPTER IV

JURIN'S CONTROVERSY WITH ROBINS AND PEMBERTON

Robins's "Discourse," and Review of it

117. Benjamin Robins was a native of Bath and a self-educated mathematician of considerable reputation.

The debate carried on by Bishop Berkeley with Jurin and Walton induced Benjamin Robins to issue a publication, entitled, *A Discourse Concerning the Nature and Certainty of Sir Isaac Newton's Methods of Fluxions, and of Prime and Ultimate Ratios*, 1735.[1] Evidently Robins felt that Berkeley's attacks should be met, and that Jurin was not the man to defend Newton satisfactorily. Robins was a man of mathematical power; his exposition is regarded by Professor G. A. Gibson as very able, and far superior to that of Jurin.[2] Without naming either Berkeley or Jurin, and without referring to their articles, Robins proceeds to his task. The whole foundation of the doctrine of fluxions is

[1] This paper is republished, along with subsequent articles on the same subject, in the *Mathematical Tracts of the late Benjamin Robins, Esq.* . . . in two volumes, edited by James Wilson, M.D. London, 1761, vol. ii, pp. 1–77.

[2] G. A. Gibson, *loc. cit.*, pp. 22–25.

based by Robins upon the following two definitions and certain general propositions annexed to them :

I. Definition : " . . . we . . . define an ultimate magnitude to be the limit, to which a varying magnitude can approach within any degree of nearness whatever, though it can never be made absolutely equal to it."

Here for the first time is the stand taken openly, clearly, explicitly, that a variable (say the perimeter of a polygon inscribed in a circle) can never *reach its limit* (the circumference). The gain from the standpoint of debating is very great ; a regular inscribed polygon whose sides are steadily doubling at set intervals of time, say, every half second, presents a picture to the imagination which is comparatively simple. The hopeless attempt of imagining the limit as reached need not be made. But this great gain is made at the expense of generality. Robins descends to a very special type of variation which is not the variation encountered in ordinary mechanics ; it is an exceedingly artificial variation. According to Robins's definition, Achilles never caught the tortoise. It would not be difficult to assume a time rate in the doubling of the sides of a polygon inscribed in a circle, so that the circumference *is reached.* Thus, let the first doubling of the number of sides take place in 1 second, the second doubling in $\frac{1}{2}$ a second, the third in $\frac{1}{4}$ a second, and so on. It is easy to see that under this mode of variation the polygons *do reach the* *limit*, the circumference. The process here transcends our power of imagination, but lies within the limits of reason. We are dwelling upon this point because of its extreme importance in the history of the theory of limits.

118. Robins constructs upon his first definition the theorem, "that, when varying magnitudes keep constantly the same proportion to each other, their ultimate magnitudes are in the same proportion." As a corollary of this he states "that the ultimate magnitudes of the same or equal varying magnitudes are equal."

II. Definition: "If there be two quantities, that are (one or both) continually varying, either by being continually augmented, or continually diminished; though the proportion, they bear to each other, should by this means perpetually vary, but in such a manner, that it constantly approaches nearer and nearer to some determined proportion, and can also be brought at length in its approach nearer to this determined proportion than to any other, that can be assigned, but can never pass it: this determined proportion is then called the ultimate proportion, or the ultimate ratio of those varying quantities."

Theorem: "To this definition of the sense, in which the term ultimate ratio, or ultimate proportion is to be understood, we must subjoin the following proposition: That all the ultimate ratios of the same varying ratio are the same with each other."

119. Robins remarks thereupon that attempts at the exposition of this method, so far as it depends upon his first definition, were made by Lucas Valerius in a treatise on the centre of gravity, and by Andrew Tacquet in a treatise on the cylindrical and annular solids; but the development involving his second definition was first made by Newton. There are a number of writers, not mentioned by Robins, who might be cited as forerunners in the theory of limits; such, for instance, as Gregory St. Vincent and Stevin.

Newton's definition of momenta as the momentaneous increments or decrements of varying quantities, "may possibly be thought obscure." Robins elucidates thus: "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 those differences is necessary for expressing that ultimate ratio" (§ 154). For instance, $Ab+Ba$ only, and not the whole increment $Ab+Ba+ab$, is called the momentum of the rectangle under A, B.

120. Of this *Discourse*, a long account of twenty-six pages, written by Robins himself, although his name does not appear,[1] was given in *The Present State of the Republick of Letters*, London, October, 1735, in which it is stated that Robins wrote his *Discourse* with the view of removing the doubts which had lately arisen concerning fluxions and

[1] This account is republished in the *Mathematical Tracts of the late Benjamin Robins*, edited by James Wilson, London, 1761, vol. ii, p. 78.

prime and ultimate ratios; that Robins carefully distinguished both these methods from the method of indivisibles and also from each other. After an historical excursion viewing the works of the ancients, of Cavalieri and Wallis, the introduction by Newton of the concept of motion is taken up. "If the proportion between the celerity of increase of two magnitudes produced together is in all parts known," then "the relation between the magnitudes themselves must from thence be discoverable." This is the basis for fluxions. The "method of prime and ultimate ratios proceeds entirely upon the consideration of the increments produced." By it Newton avoids "the length of the ancient demonstrations by exhaustions," on which, according to Robins, the method of fluctions rests. "Newton did not mean, that any point of time was assignable, wherein these varying magnitudes would become actually equal, or the ratios really the same; but only that no difference whatever could be named, which they should not pass." Newton's term momentum is used simply for greater brevity, hence need not be considered. Newton's description is capable of an interpretation too much resembling the language of indivisibles—in fact, he sometimes did use indivisibles at first; Robins has freed the doctrine from this imputation in a manner that "shall agree to the general sense of his [Newton's] description."

Jurin's Review of his own Letters to Berkeley

121. In the November, 1735, number of the *Republick of Letters*, Philalethes Cantabrigiensis (Jurin) appears with an article, *Considerations upon some passages contained in two Letters to the Author of the Analyst.* The two letters in question are the two replies Jurin himself had made to Berkeley. The article is really a reply to Robins, though Robins's name is not mentioned. Jurin claims to have adhered strictly to Newton's language; some other defenders of Newton, says he, are guilty of departing from it. Their objections to his own defence are threefold:

"I. My explication of *Lemma* 1, *Lib.* I, *Princip.*" See our §§ 4, 6, 8.

"II. The sense of the *Scholium ad Sect.* 1, *Libr.* I, *Princ.*, particularly as to,

> "1. The doctrine of Limits, 2. The meaning of the term evanescent, or vanishing." See our §§ 10–15.

"III. The sense of *Lemma* 2, *Lib.* II, *Princip.*" See our §§ 16–19.

122. As to the first objection, Jurin insists that Newton's words *fiunt ultimo æquales* mean that the quantities "do at last become actually, perfectly, and absolutely equal." He takes the hands of a clock between 11 and 12. The arcs traced by the hands "1. Constantly tend to equality, 2. During an hour, 3. And will come nearer to one another than to have any given difference, 4. Before the

end of the hour; . . . at the end of the hour, the two quantities must become equal." Further, "by taking the consideration of a finite time, Sir Isaac Newton is able to assign a point of time, at which he can demonstrate the quantities to be actually equal." Consider, says Jurin, the ordinate to a point of a hyperbola and that ordinate continued to the asymptote: they do not become equal in a finite time; Newton's *Lemma* is, "with great judgment, so worded on purpose, as necessarily to exclude this and such like cases." Thus Newton's inscribed and circumscribed rectangles of *Principia*, *Lib.* I, *Sec.* 1, *Lemma* 2 (fig. 1 in our § 9), were thought by Nieuwentiit and others never to be capable of coincidence with the curve (say the quadrant of a circle); but Jurin *assumes* the variation to be of such a nature that the limit is actually reached, as demanded by Newton's *Lemma*. For, suppose a point to move on the horizontal radius from the circumference to the centre A *in one hour*; suppose also that, when this moving point is at B on that radius, there be two rectangles described upon AB (one inscribed, the other circumscribed), and that upon every other part of the horizontal radius that is equal to AB, namely the parts BC, CD, DE, taken in order, rectangles be similarly erected "at the same point of time," then as the moving point nears the centre, the rectangles diminish in size and increase in number, and they must together become equal to the quadrant at the end of the hour. Jurin points out that he has introduced here all the suppositions of Newton's first *Lemma*, namely that, (1) the two figures tend constantly to equality, (2) during one hour, *i.e.* a finite time, (3) and come nearer to one another than to any given difference, (4) before the end of the hour, *i.e.* before the end of a finite time. Jurin continues :

"If any man shall say, that a right-angled figure, inscribed in a curvilineal one, can never be equal to that curvilineal figure ; much less to another right-lined figure, circumscribed about the curve ; I agree with him. I am ready to own that, during the hour, these figures are one inscribed, and the other circumscribed ; that neither of them is equal to the curvilineal figure, much less one to another. But then, on the other hand, it must be granted me, that, at the instant the hour expires, there is no longer any inscribed or circumscribed figure ; but each of them coincides with the curvilineal figure, which is the limit, the *limes curvilineus*, at which they then arrive."

123. Jurin thereupon proceeds to Lemma 7 of Book I, Section 1 in Newton's *Principia*, which, he says, requires additional consideration. It relates to fig. 4, where ACB is any arc and "the points A and B approach one another and meet." Lemma 7, in Andrew Motte's translation, reads as follows :—

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

Jurin says that here the chord AB, the arch ACB, and the tangent AD come to vanish when B reaches A, and their last ratio is unity. Newton "directs our imagination, not to these vanishing quantities themselves, but to others which are proportional to them, and always preserve a finite magnitude," such as A*b*, the arch A*cb*, A*d*. Since at the instant when A and B coincide, "the angle BAD, or *b*A*d*, will vanish; it is easy to conceive that, . . . the chord A*b* must coincide with the tangent A*d*, . . . consequently, AB, AD must likewise, at the same instant of time, arrive at the same proportion of a perfect equality."

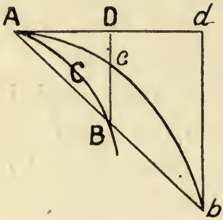

FIG. 4.

124. Proceeding to the last *Scholium* in Book I, Section 1 of the *Principia*, Jurin starts by defining the word *limit*. "I apprehend therefore that, by the limit of a variable quantity, is meant some determinate quantity, to which the variable quantity is supposed continually to approach, and to come nearer to it than to have any given difference, but never to go beyond it. And by the limit of a variable ratio, is meant some determinate ratio, to which the variable ratio is supposed continually to approach, and to come nearer to it than to have any given difference, but never to go beyond it. By arriving at a limit I understand Sir Isaac Newton to mean, that the variable quantity, or ratio, becomes absolutely equal to the determinate quantity, or ratio, to which it is supposed to tend."

With unusual lucidity, for that period, Jurin says on the subject of limits: "Now whether a quantity, or ratio, shall arrive at its limit, or shall not arrive at it, depends entirely upon the supposition we make of the time, during which the quantity, or ratio, is conceived constantly to tend or approach towards its limit." If we assume the approach to be made in a finite time, the limit is reached, otherwise it is not reached. Of a variable which "can never *attingere limitem*" Newton gives one illustration at the end of the *Scholium*: that of two quantities having at first a common difference and increasing together by equal additions, *ad infinitum*. Since they can never be really and in fact increased *ad infinitum*, says Jurin, their ratio cannot arrive at its limit. What Newton wanted to meet was the objection, "that if the last ratio's of evanescent quantities could be assigned, the last magnitudes of those quantities might likewise be assigned." Newton says No, "for those last ratio's, with which the quantities vanish, strictly speaking, are not the ratio's of the last quantities . . . . but they are the limits" which those ratios can never "arrive at," "before the quantities are diminished *ad infinitum.*" As to the sense in which Newton uses the word evanescent or vanishing, in the Scholium under consideration, Jurin inclines to the view that "both imply one single instant, or point of time."

125. In the *Principia*, Book II, Section 2, Lemma 2 (our §§ 16–19), Newton defines *moment* as "a momentaneous increment, or decrement, of a

flowing quantity, proportional to the velocity of the flowing quantities." According to Jurin, Newton puts *a*, *b*, *c* to signify either the moments, or the velocities, of the flowing quantities A, B, C. Leibniz looks upon them as differences. Newton, says Jurin, never used indivisibles, and his method to find the differences of variable quantities is not "rigorously geometrical," but is more rigorous than the treatment given by Leibniz.

Robins's Rejoinder

126. Robins replied in the *Republick of Letters* for December, 1735, in a *Review of some of the Principal Objections that have been made to the Doctrine of Fluxions and Ultimate Proportions; with some Remarks on the different Methods that have been taken to obviate them.* Robins does not here mention Philalethes any more than the latter referred directly to Robins. The objections to fluxions, says Robins, are levelled at Newton's expression, *fluxiones sunt in ultima ratione decrementorum evanescentium vel prima nascentium.* "Which being usually thus translated, that fluxions are in the ultimate ratio of the evanescent decrements, or in the first ratio of the nascent augments, it has from hence been ask'd, what these nascent or evanescent augments are?" There are difficulties of interpretation, whether the augments have quantity or have not. One way out of this difficulty which has been pointed out, is to say: "the limit of the proportion that the decrements bear to each other

as they diminish, is the true proportion of the fluxions" (p. 438). Here a new difficulty arises: Does the varying ratio reach its limit "actually, perfectly, and absolutely," or does it not? On the understanding that it does not reach the limit, "all that has at any time been demonstrated by the ancient method of exhaustions may be most easily and elegantly deduced." Rigour of demonstration does not require ultimate coincidence. "Coincidence of the variable quantity and its limit, could it be always prov'd, would yet bring no addition to the accuracy of these demonstrations" (p. 441). Hence, "why to the natural difficulty of these subjects should the obscurity of so strained a conception be added?" Is this view a correct interpretation of Newton? A literal translation of his Lemma 1, Section 1, Book I, *Principia* (see our §§ 4, 6, 8), is: "Quantities, and the ratios of quantities, that during any finite time constantly approach each other, and before the end of that time approach nearer than any given difference, are ultimately equal." What is the meaning of "given difference"? If it be a "difference first assigned" according to which the degree of approach of these quantities may be afterwards regulated; then . . . ratios, and their limits, tho' they do never actually coincide, will come within the description of this *Lemma*; since the difference being once assign'd, the approach of these quantities may be so accelerated, that in less than any given time the variable quantity, and its limit, shall differ by less than the assign'd difference." Here Robins expresses the idea that the rapidity of approach toward the limit can be arbitrarily altered, yet he does not apparently perceive—certainly he does not admit—that this rapidity may be altered in such a way that the variable actually does reach its limit. On the contrary, he maintains that "where the approach is determin'd by a subdivision into parts," "it is obvious, that no coincidence can ever be obtain'd." A coincidence such as Philalethes explains in the case of rectangles circumscribed and inscribed in a curve, if it could take place, is not a coincidence such as Newton intended, for Newton did not in this instance use motion, but continual subdivision. Robins tries to establish his view of the matter by giving an instance of erroneous results being deduced by letting the variable reach its limit. He takes a hyperbola and revolves its principal axis in the plane of the curve, around the point of intersection of this axis and an asymptote, until the two lines coincide. At the end "the hyperbola coincides with the asymptote," which is "absurd." As a matter of fact there is no absurdity. In $b^2x^2-a^2y^2=a^2b^2$, the slope of the asymptote is $m=b/a$. Robins's process amounts to making $m=o$, which gives a real locus when $b=o$, namely the locus $y^2=o$. The only objection lies in still calling the final curve a "hyperbola."

The Debate Continued

127. Robins's article was followed in the January, 1736, number of the *Republick of Letters* by Philalethes's *Considerations occasioned by a Paper in the last Republick of Letters, concerning some late Objections against the Doctrine of Fluxions, and the different Methods that have been taken to obviate them.* Jurin denies having said that there was an "intermediate state" between augments being "any real quantity" and being "actually vanished"; he says he gave Newton's declaration that "their magnitude *cannot be assigned or determined.*" Such intermediate magnitudes, in Jurin's opinion, cannot be "represented to the mind," but their ratio can be represented to the mind, by contemplating the ratio, "not in the vanishing quantities themselves, but in other quantities permanent and stable, which are always proportional to them" (p. 76). As to Newton's Lemma 1 in Section 1, Book I of the *Principia*, if the great author meant to *conclude*, that the quantities "approach nearer than any given difference," then he first supposed what he would prove, and proved only what he had before supposed. Of this he could not be guilty. Besides, Newton's words,[1] "fiunt æquales," do absolutely subvert such an interpretation. Jurin says that he does not claim that coincidence is *necessary* for rigorous proof; he admits that in Robins's treatment of prime and ultimate ratios, coincidence is

[1] Newton's words are "fiunt ultimo æquales." See our § 4.

not necessary; only, Robins's method is not that of Newton. To establish this last point, Philalethes quotes Newton's *lemma* in Latin, then prints Robins's and his own translation of it. In case of variation, the upper line is Robins's translation, the lower is Jurin's:—

Quantities, and { *the* / *also* } *ratio's of quantities, that during any finite time constantly* { *approach each other,* / *tend to equality,* } *and before the end of that time approach nearer* { *than any given difference, are ultimately equal.* / *to one another than to have any given difference, do at last become equal.* }

It is not clear to Jurin what Robins means by "are ultimately equal," nor can Jurin conceive "how quantities, which do never *become actually equal*, . . . can come within the description of a Lemma, which Lemma expressly affirms, that they *become equal.*" *Fiunt ultimo æquales* means "become at last equal." Jurin quotes different places in the *Principia* which support his point. He denies that Newton proceeds, in the case of inscribed and circumscribed rectangles, by continual divisions of the base of the figure, and gives references in support of his contention. Of interest are the following admissions made by Jurin (p. 87): "This equality therefore we are obliged to acknowledge, although we should not be able, by stretch of imagination, to pursue these figures, and, as it were, to keep them in sight all the way, till the very point of time that they arrive at this equality. For a demonstrated truth must be owned, though we do not perfectly see every step by which the thing is brought about."

"We have therefore no occasion for *the delineation of a line less than any line that can be assigned.* We acknowledge such delineation to be utterly impossible; as likewise the *conception* of such a line, as an actually existing, fixed, invariable, determinate quantity." Jurin here begins to disavow infinitesimals. "I am very free to own that Sir Isaac Newton does not *always* consider this coincidence, or rather equality, of the variable quantity, or ratio and its ultimate, as necessary in his method."

128. The debate between Jurin and Robins had reduced itself by this time, not so much to the discussion of the logical foundations of fluxions, as to the discussion of what Newton's own views on the subject had been. Robins prepared a long paper on the subject for the April, 1736, issue of the *Republick of Letters*, under the title: *A Dissertation shewing, that the Account of the doctrine of Fluxions, and of prime and ultimate ratios, delivered in a treatise entitled, 'A discourse concerning the nature and certainty of Sir Isaac Newton's methods of fluxions, and of prime and ultimate ratios,' is agreeable to the real sense and meaning of their great inventor.* The paper covers 45 pages. Robins repeats the fundamental definitions and historical statements given in his earlier papers, and directs

some attacks against Berkeley. To set forth the views of Newton, quotations are made from his works. He quotes from the Introduction to the *Quadratura Curvarum* (see our §§ 27–42). From the *Quadratura Curvarum* itself he quotes:

"Quantitates indeterminatas, ut motu perpetuo crescentes vel decrescentes, id est, ut fluentes vel defluentes, in sequentibus considero, designoque literis z, y, x, v, et earum fluxiones, seu celeritates crescendi noto iisdem literis punctatis. Sunt et harum fluxionum fluxiones, sive mutationes magis aut minus celeres, quas ipsarum z, y, x, v fluxiones secundas nominare licet," etc.

Robins quotes also from the anonymous account of John Collins's *Commercium Epistolicum*, which figures so prominently in the controversy between the followers of Newton and of Leibniz. This account was published in the *Philosophical Transactions*, vol. xxix, for the years 1714, 1715, 1716, of the Royal Society of London, of which Robins was a member. Robins goes on the assumption that the anonymous article was written by Newton himself, an assumption denied by no one at that time or since, though Jurin in a reply wants to know on what authority Newton's authorship is asserted. Robins quotes as follows (see our § 47):

"When he [Newton] considers lines as fluents described by points, whose velocities increase or decrease, the velocities are the first fluxions, and their increase the second."

129. Robins says that Berkeley, "for the support

of his objections against this doctrine [of fluxions], found it necessary to represent the idea of fluxions as inseparably connected with the doctrine of prime and ultimate ratios, intermixing this plain and simple description of fluxions with the terms used in that other doctrine, to which the idea of fluxions has no relation: and at the same time by confounding this latter doctrine with the method of Leibniz and the foreigners, has proved himself totally unskill'd in both. These two methods of Sir Isaac Newton are so absolutely distinct, that their author had formed his idea of fluxions before his other method was invented, and that method is no otherwise made use of in the doctrine of fluxions, than for demonstrating the proportion between different fluxions. For, in Sir Isaac Newton's words [see our §§ 29, 36], as the fluxions of quantities are nearly proportional to the contemporaneous increments generated in very small portions of time, so they are exactly in the first ratio of the *augmenta nascentia* of their fluents. With regard to this passage the writer of the *Analyst* has made a two-fold mistake. First, he charges Sir Isaac Newton, as saying these fluxions are very nearly as the increments of the flowing quantity generated in the least equal particles of time. Again, he always represents these *augmenta nascentia*, not as finite indeterminate quantities, the nearest limit of whose continually varying proportions are here called their first ratio, but as quantities just starting out from non-existence, and

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yet not arrived at any magnitude, like the infinitesimals of differential calculus. But this is contrary to the express words of Sir Isaac Newton, who after he had shewn how to assign by his method of prime and ultimate ratios the proportion, that different fluxions have to one another, he thus concludes. *In finitis autem quantitatibus Analysin sic instituere et finitarum nascentium vel evanescentium rationes primas vel ultimas investigare consonum est geometriæ veterum: et volui ostendere, quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere.*" (See our §§ 33, 41.)

130. Robins proceeds to explain that the method of prime and ultimate ratios is "no other than the abbreviation and improvement of the form of demonstrating used by the ancients on the like occasions." It has nothing to do with infinitely small quantities, which have led into error not only Leibniz in studying the resistance of fluids and the motion of heavenly bodies, but also Bernoulli likewise in the resistance of fluids and in the study of isoperimetrical curves. Such infinitely small quantities led Parent to make wrong deductions. It was argued that because a heavy body descends through the chord of a circle terminating at its lowest point in the same time as along a vertical diameter, "the time of the fall through the smallest arches must be equal to the time of the fall through the diameter." To relieve Newton of the suspicion of not being free from the obscure methods of indivisibles, Robins says he [Robins] defined an "ultimate

magnitude" and "ultimate ratio" as limits. This exposition Robins had given in full in his *Discourse.* The difference of interpretation of Newton's Lemma I in the *Principia* (Book I, Section I), given by himself and by Jurin, arises from Jurin's misinterpretation of Newton's word *given.* He "supposes it to stand for *assignable*; whereas it properly signifies only what is actually assigned." Jurin claims that by our interpretation, "Newton is rendred obnoxious to the charge of first supposing what he would prove" (p. 307). Robins says in reply that the statement, quantities which "are perpetually approaching each other in such a manner, that any difference how minute soever being given, a finite time may be assigned, before the end of which the difference of those quantities or ratios shall become less than that given difference," is an obvious but not an identical proposition. Robins argues, "that Sir Isaac Newton had neither demonstrated the actual equality of all quantities capable of being brought under this lemma, nor that he intended so to do" (p. 309); when quantities "are incapable of such equality, the phrase of ultimately equal must of necessity be interpreted in a somewhat laxer sense," as in *Principia*, Book I, Prop. 71, "pro æqualibus habeantur, are to be esteemed equal." When Newton says that the number of inscribed parallelograms should be augmented *in infinitum*, he does not mean that the number becomes infinitely great, but that they are augmented *endlessly*. The nature of the motion assumed by Jurin to explain how the limit may be reached is excessively complex. Moreover, "to assert that any collection of these inscribed and circumscribed parallelograms can ever become actually equal to the curve, is certainly an impropriety of speech, . . . the essence of indivisibles consists in endeavouring to represent to the mind such inscribed or circumscribed figure, as actually subsisting, equal to the curve" (p. 312). Our interpretation "thus removes this doctrine quite beyond the reach of every objection" (p. 315). Robins argues that Newton's *ultimæ rationes, quibuscum quantitates evanescunt* are not *rationes quantitatum ultimarum*; but only limits, to which the ratios of these quantities, which themselves decrease without limit, continually approach; and to which these ratios can come within any difference, that may be given, but never pass, nor even reach those limits" (p. 316). "Newton has expressly told us, that the quantities, he calls *nascentes* and *evanescentes*, are by him always considered as finite quantities" (p. 321).

131. The *momenta* of quantities occur in Newton's *De analysi per æquationes numero terminorum infinitas*, drawn up in 1666. Newton says "that he there called the moment of a line a point in the sense of Cavalerius, and the moment of an area a line in the same sense" (see our § 47), that "from the moments of time he gave the name of moments to the momentaneous increases, or infinitely small parts of the abscissa and area generated in moments of time . . .

because we have no ideas of infinitely little quantities, he introduced fluxions into his method, that it might proceed by finite quantities as much as possible." Prime and ultimate ratios he introduced later. Newton says in that place that in his proofs he uses *o* for a finite moment of time, though sometimes, for brevity, he supposes *o* infinitely little. Thus Newton used *o* in two senses; in the fluxions published in 1693 in Wallis's algebra, *o* is used in the sense of indivisibles; in 1704 he gave it a second signification in the *Quadratura Curvarum*. Robins sums up his dissertation thus: "Hence it is very manifest, that as Sir Isaac Newton used at first indivisibles, so he soon corrected those faulty notions by his doctrine of fluxions, and afterwards by that of prime and ultimate ratios. And all the absurdity of expression, and all the inconsistency with himself charged on him by the author of the *Analyst*, arises wholly from mis-representation." This paper was badly arranged and below the level of Robins's earlier contributions.

132. Robins's long paper in the *Republick of Letters* was followed in the July and August (vol. xxviii, 1736) numbers by *Considerations upon some passages of a Dissertation concerning the Doctrine of Fluxions, published by Mr. Robins in the Republick of Letters for April last*, by Philalethes Cantabrigiensis. The paper extends over 136 pages, and could not be easily accommodated in a single number. From now on the disputants, particularly Jurin, are no longer in a calm frame of mind. The

debate is one over words and ceases to be illuminating. Their judgments were perverted by the heat of controversy. Even theological or political controversies could not easily surpass the verbosity and haze exhibited here.

Jurin's first objection to Robins's last analysis is the statement that the method of fluxions has no relation to the method of first and last ratios; Jurin quotes from Newton in support of his contention. The charge that he (Jurin) represents *augmentia nascentia* not as finite, but as just starting out of non-existence, "like infinitesimals of the differential calculus," Jurin denies, saying: Leibniz's differentials "are fixed, determinate, invariable"; he himself has represented the nascent augments as "quantities just starting out from non-existence, and yet not arrived at any magnitude, and not as finite quantities" (p. 52), and quotes Newton in support of this view. According to the article in the *Philosophical Transactions*, No. 342, attributed to Newton, moments are represented "by the rectangles under the fluxions and the moment *o*"; "in his calculus there is but one infinitely little quantity represented by a symbol, the symbol *o*: it is also said, Prick'd letters never signify moments, unless when they are multiplied by the moment *o* either exprest or understood to make them infinitely little, and then the rectangles are put for moments." Jurin charges that Robins has now published four different interpretations of Newton's much-discussed lemma. Newton's phrase, *fiunt ultimo æquales*, the use of the words "perpetually" and "endlessly," "the last difference," are again discussed at length. Jurin quotes from Robins a passage which appears to show that "Mr. Robins is now of opinion, that Sir Isaac's demonstration is applicable to such quantities, as at last become actually equal, as well as to quantities, which only approach without limit to the ratio of equality" (p. 67); therefore, the lemma, "by Mr. Robins's own confession, may be taken in the sense I have always understood it in" (p. 68). However, this is in direct conflict with Robins's earlier assertions. In the discussion about the inscribed rectangles, both Robins and Jurin agree that if the "base of the curve" (our abscissa) be continually subdivided as in Euclid I 10 or V 10, it is manifest "that such subdivision can never be actually finished" (p. 78); but Newton proceeds differently—he supposes a line to be described by a moving point. Jurin thereupon repeats exactly the argument in Zeno's "Dichotomy," though he does not mention Zeno, to show that a point moving across the page in, say, one hour passes over 1 / 2 of the distance, then over 1 / 4 of it, then over 1 / 8, 1 / 16, etc., and insists that "all the possible subdivisions of the line" will be "actually finished" and "brought to a period at the end of the hour." This is given in support of his previous argument that the rectangles inscribed in a curve may reach the limit. "If Mr. Robins will tell me, that the imagination cannot pursue these parallelograms to the very end of the hour, I may ask him, whether the imagination can any better pursue the subdivision of the line, or even of the hour itself, to the end of the hour, which subdivisions he must own to *be brought to a period* by the end of the hour. But there is no need to *strain our imagination, to labour in every case*, or indeed in any case, *after some idea of motion however intricate*; *no subtle inquiry is at all necessary*, since we are obliged to own the conclusion to be true and certain. . . ."

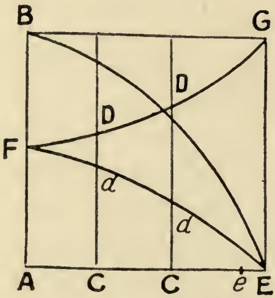

FIG. 5.

"However, since Mr. Robins is pleased to talk so much about straining our imagination, . . . let us see, if we cannot find some plain and easy way of representing to the imagination, that actual equality, at which the inscribed and circumscribed figures will arrive with each other, and with the curvilineal figure, at the expiration of the finite time" (p. 111).

Let the curvilineal figure ABE equal in area the rectangle with sides EA and AF. When the moving point describing the base EA in a finite time is at C, let the rectangle with the base EA and height C*d* be equal to the sum of the parallelograms inscribed in ABE (not drawn in the figure) which stand on CA and upon as many other adjoining parts of EA as can be taken equal to CA. Let E*dd* be the curve traced by the moving point *d*.

Let the area of the rectangle with EA as base and CD as height = sum of the circumscribed parallelograms (not drawn in the figure) standing on CA and upon as many other parts of EA as can be taken equal to CA and adjoining to it; also let GDD be the curve traced by the movable point D. Then as the curvilineal, the inscribed, and the circumscribed figures are respectively equal to EA × AF, EA × Cd, EA × CD, these figures must be proportional to AF, Cd, and CD. These three lines will "be equal to one another at the end of the finite time." Now since Cd and CD approach each other, during a finite time, within less than any given distance before the end of that time, these three lines will, by that *Lemma*, be equal to one another at the end of the finite time. The limit is reached (p. 114).

133. As a further illustration, Jurin takes a rectilinear figure, the right triangle ABE, where EA = AB = a, AF = $\frac{1}{2}a$, EC = x, the point C moving from E to A as before. Upon AC as a base, imagine an inscribed rectangle (height CH), and a circumscribed rectangle (height CK). As in the previous figure, imagine other inscribed and circumscribed rectangles, standing upon as many other parts of EA as can be taken equal to CA, and adjoining to it in order. When CA is an aliquot part of AE, then $a \times$ Cd is the sum of the inscribed rectangles and $a \times$ CD is the sum of the circumscribed rectangles, where C$d = x / 2$, and CD $= a - x / 2$. Let Kd = CD. The ordinate Kd,

drawn to the base BG, will be terminate by EF. When CA is not an aliquot part of AE, if we divide the base into as many parts as may be, there will be left a portion Ee, which, let us call r. Then $Cd = \overline{x+r} \times \overline{a-r} \div (2a)$ and all these ordinates will be bounded by EddF. In the same way, $Kd = a - \overline{x+r} \times \overline{a-r} \div (2a)$, and the ordinate will be bounded by EddF. When $x=a$, r vanishes, $Cd = \frac{1}{2}a$ and $Kd = \frac{1}{2}a$. Hence the inscribed and circumscribed figures do then become equal to each other, and to the triangle ABE; again, the limit is reached.

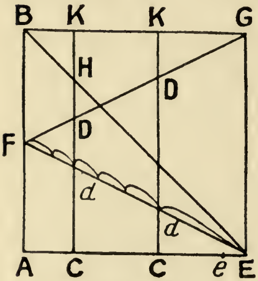

FIG. 6.

Jurin takes Robins to task for asserting that "equality can properly subsist only between figures distinct from each other." To Robins's query, "Does Philalethes here suppose the truth of Sir Isaac Newton's demonstrations to depend on this actual equality of the variable quantity and its limit?" Jurin answers, "I do . . . In the manner Mr. Robins defines, and treats of prime and ultimate ratios, I allow his demonstrations to be just without this actual equality. But Sir Isaac Newton does not define and treat of prime and ultimate ratios, in the same manner with Mr. Robins; nor are Mr. Robins's demonstrations at all like Sir Isaac Newton's demonstration" (p. 128). The inability of our imagination to pursue the rectangles in reaching the limit is no valid argument against the contention that the limit is reached; even in the ancient geometry there are demonstrated truths that lie beyond the reach of the imagination, as for instance, that three cones may equal a cylinder, all of the same base and height (p. 130). The meaning of *moment*, a truly difficult concept, is discussed again, Jurin holding that Newton took it as "a momentaneous increment, . . . less than any finite quantity whatsoever, and proportional to the velocity of the flowing quantity," while Robins seemingly claimed that Newton meant them to be finite quantities (p. 151). With respect to Newton's early use of the infinitely little, Jurin and Robins were in disagreement, and Robins was in our opinion nearer the truth. Robins claimed that Newton at first used infinitely little quantities; that afterwards he improved his method by discarding them; Jurin claimed that Newton's alleged absurdity of expression and inconsistency with himself, as charged by Berkeley and others, "arises wholly from misinterpretation, or misunderstanding him" (p. 179).

134. Jurin's article appeared in the July and August numbers, 1736, of the *Republick of Letters.* Robins could not wait in patience until the entire article of Jurin had been printed. In the August number he replies to the part of Jurin's article that had appeared in the July number. The August number was given up to Jurin and Robins, to the entire exclusion of all other articles and of the usual

book reviews. On the last page of the August number, the editor apologises to the readers and assures them "they shall hereafter have no occasion to complain upon this head." In Robins's reply,[1] both "Robins" and "Philalethes" appear in the third person, as if the writer were some outsider. Robins says: "Newton does not intermix his simple and plain description of fluxions with the terms used in the doctrine of prime and ultimate ratios; for his description of fluxions is contained in the two first paragraphs of his Introduction to the *Quadratures*, in which no terms of the other doctrine occur" (p. 89). The *Lemma* is, of course, taken up again, Robins claiming his interpretation legitimate, "for two quantities may constantly tend to equality during some finite space of time, and before the end of that time come nearer together than to have any difference, which shall be given; and yet at the end of that time have still a real difference," while Jurin's interpretation was not "any difference that shall be given," but "any assignable difference," which would mean that the limit must be reached. Mr. Robins says (p. 97): "It is not difficult to assign a very probable reason, which led Sir Isaac Newton to the use of this expression [fiunt ultimo æquales], for before him it had not been unusual for geometers to speak of the last sums of infinite progressions, which is an ex-

[1] "Remarks on the *Considerations relating to Fluxions*, etc., that were published by Philalethes Cantabrigiensis in the *Republick of Letters* for the last month," *Republick of Letters*, August, 1736, pp. 87–110.

pression something similar to this. Surely here no one will pretend, that an infinite number of terms can in strict propriety of speech, and without a figure, be said to be capable of being actually summed up and added together." Robins makes the only direct reference that was made in this debate to Zeno's paradoxes. He mentions Achilles and the Tortoise, but in a manner devoid of interest. Referring to the line which Jurin supposes traced in one hour, Robins says: "Perhaps it may be easiest understood by comparing the present point with the old argument against motion from Achilles and the Tortoise. It is impossible to pursue in the imagination their motion by the means proposed in that argument to the point of their meeting, because the motion of each is described by the terms of an infinite progression." Robins does not seem fully to realise that Achilles and the Tortoise present a case in which a variable reaches its limit.

135. The editor of the *Republick of Letters* permitted the two disputants to continue their wranglings in an *Appendix* to the September issue.[1]

Philalethes's attempt to represent to the imagination the actual equality at which the inscribed and circumscribed figures will arrive with each other, and with the curvilinear figure, is criticised by Robins

[1] *An Appendix to the Present State of the Republick of Letters for the Month of September*, 1736. *Being Remarks on the Remainder of the Considerations relating to Fluxions, etc., that was published by Philalethes Cantabrigiensis in the Republick of Letters for the last Month. To which is added by Dr. Pemberton a Postscript occasioned by a passage in the said Considerations.* London, 1736.

on the ground that the continued curve "is not to be described, but by an endless number of parabola's" (of which the curve is the envelope); thus, Philalethes gave "as an equation expressing the nature of a single curve, one which in reality includes an infinite series." "Philalethes supposed a last form of the inscribed figures, that was equal to the curve." Robins observed "that equality implies the things, which have that property, to be distinct from each other. For to say a thing is equal to itself is certainly no proper expression." But "there is no such last form distinct from the curve," as Philalethes admits; hence Philalethes "gives up the point."

136. In the *Principia*, Newton does not deliver the doctrine of fluxions, but the doctrine of prime and ultimate ratios. "The understanding of this book is attended with difficulty." The expression *ultima summa* is defective: "Can any sum of a set of quantities, whose number is supposed infinite, in strict propriety of speech be called their last sum?" Later, Robins says: "Let Philalethes reconcile the actual arrival of these quantities to the ratio supposed, and at the same instant vanishing away. Is not this saying, that the two quantities become nothing, and bear proportion at the same instant of time?" (p. (14)). Philalethes "has thought himself unjustly accused by Mr. Robins of supposing a nascent increment to be some intermediate state of that increment between its finite magnitude, and its being absolutely nothing. To have proved this

assertion groundless he ought to have shown, that this definition does not attempt at describing such an intermediate state" (p. (15)). Robins asserts: "Whoever has read Sir Isaac Newton's *Lectiones Opticæ*, and will deny, that he has at any time made use of indivisibles, must be very much a stranger to that doctrine, and to the style of those writers who follow it" (p. (19)). "What reflexion is it upon Sir Isaac Newton to suppose, that he made use of the methods he had learned from others before he had invented better of his own: or that in an analysis of a problem for dispatch he still continued to make use of such methods, when he conceived it would create no error in the conclusion? Has not Sir Isaac Newton said this of himself, and has Mr. Robins said anything more?" (p. (15)). "Does Philalethes here mean, that a quantity can become less than any finite quantity whatsoever, before it vanishes into nothing? If not, then the point is given up to Mr. Robins, who only contends, that vanishing quantities can never by their diminution be brought at last into any state or condition, wherein to bear the proportion called their ultimate: if otherwise, since Philalethes supposes . . . that it is nonsense, that it implies a contradiction to imagine a quantity actually existing fixed, determinate, invariable, indivisible, less than any finite quantity whatsoever; because this imports as much as the conception of a quantity less than any quantity, that can be conceived: how can a quantity supposed to be less than any finite quantity whatsoever be rendered more the object of the conception by being understood to be brought into this condition by a constant diminution from a variable divisible quantity ?" (p. (20)). "Sir Isaac Newton has introduced into use the term moment throughout the whole second book of the *Principia*, and for no other purpose than for the sake of brevity ; for his doctrine of prime and ultimate ratios had been before fully explained, and every proposition of the second book might have been treated on without the use of this term, though perhaps with a somewhat greater compass of words" (p. (23)). "Mr. Robins has endeavored to defend Sir Isaac Newton both against the accusation of the author of the *Analyst*, and the misrepresentation of Philalethes. He has shown, that Sir Isaac Newton's doctrine of prime and ultimate ratios has no connexion with indivisibles, and that, if he ever allowed himself in the use of indivisibles, he knew that he did so, and did not confound both the methods together, as the author of the *Analyst* accuses him, and Philalethes without knowing it has owned" (p. (27)). "Had Philalethes been versed in the ancients, and in the later writers who have imitated them, he could have been at no loss about the true sense of *data quavis differentia* used by Sir Isaac Newton in his first *Lemma*. For this expression is borrowed from the writers, that made use of exhaustions" (p. (29)). "What separates the doctrine of prime and ultimate ratios from indivisibles is the declaration made in the *Scholium* to the first Section of the *Principia*, that Sir Isaac Newton understood by the ultimate sums and ratios of magnitudes no more than the limits of varying magnitudes and ratio's; and he puts the defence of his method upon this, that the determining any of these limits is the subject of a problem truly geometrical. To insist, that the variable magnitudes and ratio's do actually attain, and exist under these limits, is the very essence of indivisibles" (p. (34)).

Robins's reply in the August and September, 1736, numbers of the *Republick of Letters* is condensed in form, yet covers 61 pages. It is impossible for us to convey an adequate idea of the amount of detail entering in the discussion. Altogether Robins shows greater willingness to admit that Newton's views were different at different periods in his career, and that even Newton may be guilty of modes of expression that are not free from obscurity. Moreover, Robins speaks in general with greater sincerity than his opponent. But Jurin proves himself the superior of Robins in adhering to a broader and more comprehensive conception of variables and limits.

Pemberton enters the Debate

137. At this stage a new party enters the debate —Henry Pemberton, who had studied medicine and mathematics at Leyden and Paris, had been a friend of Newton, and had edited the third edition of the *Principia*. In an article following the one of Robins in the "Appendix" (August and September 1736),

Pemberton says: "I . . . am fully satisfied, that Mr. Robins has expressed Sir Isaac Newton's real meaning." Pemberton quotes from Newton's Introduction to the *Quadrature of Curves* about prime and ultimate ratios (see our §§ 33, 42), and then remarks; "Here Sir Isaac Newton expressly calls the quantitates *nascentes* and *evanescentes*, whose prime and ultimate ratios he investigates, by the appellation of finite. Now I desire Philalethes to reconcile this passage with his notion of a 'nascent quantity being a quantity not yet arrived at any assignable magnitude how small soever.' And I must farther ask Philalethes, whether he has not here attempted to define a non-entity."

138. Robins's last article and Pemberton's rash challenge led to another flow of words, covering 77 pages in the "Appendix" to the *Republick of Letters* for November, 1736, in an article by Jurin, entitled *Observations upon some Remarks relating to the Method of Fluxions, published in the Republick of Letters for August last, and in the Appendix to that for September.*

Jurin insists that "the method of fluxions, as it is drawn up by Sir Isaac Newton, could not possibly be *formed before* the method of first and last ratio's *was invented*" (p. (6)).

Robins "takes no notice of the letter *o* being used in the book of *Quadratures*, in the very same sense as in the *Analysis*" (p. (8)). "That symbol never denotes any quantity, but what, by a continual decrease, becomes infinitely little, *i.e.* less than any quantity, and at last vanishes into nothing" (p. (8)).

"He is grossly mistaken in thinking, that quantities, which, before the end of a finite time, come nearer together than to have any assignable difference, will therefore become equal before the end of that time" (p. (12)). "I have clearly proved in November and January last, that Sir Isaac Newton designed no quantities or ratio's to be comprehended within the sense of this lemma, which do not become actually equal" (p. (13)).

"Has then Mr. Robins, . . . offered to shew, that any quantities or ratio's incapable of an actual equality are compared in this lemma? I think not" (p. (22)). In January, "I use the following words, 'This determinate proportion of the finite quantities *a* and *e*, is what I understand by the proportion of the evanescent augments.' This, I say, ought to have been attended to, before this charge against me was renewed" (p. (24)). As regards the ratio between the inscribed and circumscribed figures, "have not I truly expressed it? If my expression be too complex, let these great Geometers shew me a simpler, if they can, and I will make use of that" (p. (34)). Robins's argument about the last form of parallelograms differing from the limiting curve is defective in the minor of the syllogism: "Things which are equal are distinct from each other." "Is it," says Jurin, "the part of a candid and ingenious adversary, to insist always upon the word *equal*, when a more proper expression, as that of *coinciding*, has been used by his antagonist?" If his argument is sound, "it will hold against my expression, that the figures inscribed and circumscribed do at last *coincide* with the curvilinear figure." Jurin claims "that if Mr. Robins's interpretation of the first *Lemma* be admitted, Sir Isaac's demonstrations, as they now stand, will not be accurate, nor geometrically rigorous," for, "as they now stand, the examples he has given in the several *Lemmata* of the first Section, are of such quantities and ratio's only, as do actually arrive at their respective limits" (pp. (42) and (43)). "Mr. Robins and I have been disputing some time, whether Sir Isaac Newton used indivisibles. That Gentleman maintains that he used them; and grounds his charge upon the term *infinitely little*, which is sometimes to be met with in Sir Isaac Newton's writings: but he does not explain the meaning of that term, when used either by Sir Isaac, or by the writers of indivisibles. I, on the contrary, distinctly explain what I apprehend to be meant by it, both when used by Sir Isaac Newton, and when used by the writers of indivisibles. . . . I supposed the writers upon indivisibles, by an infinitely little quantity, to mean a quantity actually existing, fixed, determinate, invariable, indivisible, less than any finite quantity whatsoever" (p. (73)). Robins quotes Pascal and Barrow as using the term *indefinite* in place of *infinite*, but the writers I quoted use *infinite* and *infinitely little*. There is difference of usage among followers of Cavalieri. "It is not denied, but that Sir Isaac Newton, by the term *infinitely little*, meant a quantity variable, divisible, that, by a constant diminution, is conceived to become less than any finite quantity whatsoever, and at last to vanish into nothing. By which meaning all that is faulty in the method of indivisibles, is entirely avoided; and that being allowed, the rest is only a dispute about a word" (p. (74)).

Jurin declares in a "Postscript" that "to carry on two controversies at once is more than I have leisure for"; later "I intend to accept of Dr. Pemberton's invitation"; meanwhile Jurin inserts an attestation of "his learned friend *Phileleutherus Oxoniensis*" to the effect that this friend is "fully satisfied, Mr. Philalethes has expressed Sir Isaac Newton's real meaning." The language of this attestation follows exactly the language of Pemberton, except that Philalethes, and not Robins, is now declared the correct interpreter of Newton.

139. In the December issue, 1736, of the *Republick of Letters*, Robins says in an "Advertisement" that "since Philalethes has given loose to passion," he "cannot think anything farther necessary for the satisfaction of impartial readers" (p. 492), and now takes "leave of Philalethes," but cannot resist a few parting shots. Nor could Philalethes resist making reply to this "Advertisement" in an "Appendix" to the December number, 1736, of the *Republick of Letters*, in which he expresses regret "that so long a correspondence should end in discontent or ill humour." Jurin justifies the practice

he exercised in this controversy of offering poetry (usually in Latin) for the sake of readers who are under necessity "of exercising their faith, rather than their reason in this dispute," for "A verse may catch him, who a sermon flies," and for the sake of enlivening the subject for others, "who are judges of the dispute."

140. In this December "Appendix" Jurin then contributes *A Reply to Dr. Pemberton's Postscript*, which takes up 31 pages. Referring to Newton's *Lemma* 1, Jurin says that in his former expression, the quantities "come nearer to equality than to have any assignable difference between them," it never was his intention to assert "that during the time of the approach, the difference between the quantities is not always assignable"; he meant "that, though they shall always have a difference during the finite time, yet, before the end of that time, their difference shall become less than any quantity that can be assigned. And if my words are taken in this sense, the Dr.'s objection immediately falls to the ground" (p. (24)). Mr. Jurin then gives a "demonstration" of the following proposition: "If two lines (1) tend constantly to equality with each other, (2) during any finite time, as, for instance, an hour; (3) and thereby, their difference become less than any quantity that can be assigned, (4) before the end of the hour; then, at the end of that finite time, or at the end of the hour, the lines will be equal." As to Dr. Pemberton's charge that Jurin misinterprets Newton's

nascent and evanescent increments, Jurin says that he discussed this question with Robins. Newton's words in the *Quadratura Curvarum*, viz. *finitarum nascentium vel evanescentium*, may mean "(1) finite nascent or evanescent quantities, or (2) finite quantities when they begin to be, or when they vanish. But the former sense contradicts the second *Lemma* of the second Book of the *Principia*, where Sir Isaac Newton says, *cave intellexeris particulas finitas* . . . and indeed it is contrary to the whole tenor of his doctrine." The second interpretation is "perfectly conformable to all the rest of Sir Isaac Newton's works" (p. (32)). Jurin repeats that a nascent increment is "an increment not yet arrived at any assignable magnitude, how small soever." To Dr. Pemberton's query, whether Jurin "has not here attempted to define a non-entity," Jurin replies that it "ought not to be called simply a *non-entity*, nor simply an *entity*. It is a *non-entity* passing into entity, or entity arising from non-entity, a beginning entity, something arising out of nothing" (p. (37)).

141. The discussion is carried on from this time in a journal called *The Works of the Learned*, into which the *Republick of Letters* and another journal had merged. In the February, 1737, issue Dr. Pemberton appears with *Some Observations on the Appendix to the Present State of the Republick of Letters for December*, 1736, which enjoys the merit of brevity, being limited to only two pages. Pemberton declares that in Newton's passage in the

Quadratura Curvarum, "Philalethes cannot remove my objection by straining one or two of the words to fit his sense"; Newton meant there that vanishing quantities should not be "otherwise than finite quantities" (p. 157). Moreover, "what kind of nothings they must be, which with any propriety can be said to pass into somethings, and for this reason can be capable of bearing proportions, before they are become anything, certainly requires explanation."

A reply by Jurin in *The Works of the Learned* for March, 1637, is kept within the very moderate compass of 10 pages. The title of his contribution is *The Contents of Dr. Pemberton's Observations published the last month.* Nothing here is of interest in the interpretation of Newton.

Dr. Pemberton's reply in the April issue refers to Jurin's phrase, "they come nearer to equality than to have any assignable difference between them": "My objection to the interpretation of Philalethes [in the *Minute Mathematician*, p. 88] is, that these words, which compose the third article of that interpretation, in conjunction with the fourth article can have no other signification, than that the quantities come nearer to equality than to have any difference between them before that point of time, wherein they are supposed by the second article to become equal; all which amounts to this inconsistency, that there is a time, when the quantities have no difference, and yet are not equal" (p. 306). Dr. Pemberton again gives his endorsement of Robins's interpretation of Newton.

Jurin appears with a 12-page article in the May, 1737, number of *The Works of the Learned*, saying: "He still ascribes to my words a meaning, which I have again and again utterly disavowed; not only so, but he changes the words themselves, putting *any difference* instead of *any assignable difference*" (p. 388). As to the Introduction to Newton's *Quadratura Curvarum*, "in that very *Introduction* Sir Isaac Newton has made use of infinitely little quantities, in the sense I understand them, that is, quantities which being at first finite, do by a gradual diminution at last vanish into nothing and consequently must, during their diminution, become less than any quantity that can be assigned" (p. 389). As to evanescent quantities being entities or non-entities, "If this page were divided from top to bottom into two equal parts, one black, and the other white,[1] and Dr. Pemberton were to ask me, whether the middle line, which divides the two parts, were black or white, I apprehend it would be a direct answer to say, it is neither; it cannot properly be called either a black line, or a white line; it is the end of the white and beginning of the black, or the end of the black and beginning of the white" (p. 389). "I was apprised that Mr. Robins had all along expressed the sentiments of Dr. Pemberton" (p. 393). Dr. Pemberton still refuses to give his interpretation of Newton's

[1] As far as I know, Jurin is the first to use colour devices to illustrate subtle points in evanescent quantity or in number. Jules Tannery, in his *Leçons d'Algèbre et d'Analyse*, Paris, 1906, p. 14, uses colour imagery to illustrate the discussion of irrational numbers.

Lemma. "Every body will be satisfied that the true reason of his backwardness, is the fear he is under, that I shall make good my promise, in shewing, that his explanation is either a false one, or, in case it be true, is to all intents and purposes the very same with mine" (p. 396).

In June, 1737, Dr. Pemberton replies again, by repeating his previous assertion against Philalethes's explanation of Newton's *Lemma*, given in the *Minute Mathematician*, but does not permit himself to be drawn into giving an explanation of his own of Newton's *Lemma*.

In Jurin's article in the July issue, 1737, we read: "I did indeed take notice of the prudence Dr. Pemberton used, in passing by my second interpretation, which was so clear and plain, and was so fully illustrated by examples, that there was no possibility of perverting the sense of it" (p. 70). "But since this dispute, which began upon matters of science, . . . unless Dr. Pemberton shall see fit to revive it by giving his so long demanded explication, I shall not judge it worth while to take notice of what he may hereafter write."

Dr. Pemberton followed with some *Observations* in the August, 1737, number, while in the September number there appears "the last reply of Philalethes," and in the October number the final answer by Pemberton. Thus ended a dispute which had for some time ceased to contain much of scientific and historic value.

Debate over Robins's Review of Treatises written by Leonhard Euler, Robert Smith, and Jurin

142. Being in a somewhat combative mood, Robins made attacks upon Euler's treatise on motion, Dr. Robert Smith's optics, and Jurin's essay on vision.[1]

Robins's criticisms of Euler concern mainly the philosophy of the Calculus. Robins quotes Euler's third proposition, "That in any unequal motion the least element of the space described may be conceived to be passed over with an uniform motion," and then says, this "is not universally true," as, for instance, "when those spaces are compared together, which a body accelerated by any force described in the beginning of its motion; for the ultimate proportion of the first of two contiguous spaces, thus described in equal times to the second, is not that of equality, but the ratio of 1 to 3, as is well known to every one acquainted with the common theory of falling bodies" (p. 2). In another place (p. 4) Robins argues that the path assigned by Euler to a certain body "is false even on the confused principles of indivisibles." Some passages in Robins involve the Leibnizian notation in the calculus, and look quite odd in an eighteenth-century publication prepared by a Briton in Great Britain. Robins concludes that most of Euler's errors "are owing to so strong an attachment to the principles, he had imbibed under that inelegant

[1] *Remarks on Mr. Euler's Treatise of Motion, Dr. Smith's Compleat System of Opticks, and Dr. Jurin's Essay upon Distinct and Indistinct Vision.* By Benjamin Robins, London, 1739.

computist, who was his instructor, that he was afraid to trust his own understanding even in cases, where the maxims, he had learnt, seemed to him contradictory to common sense" (p. 30). This master was John Bernoulli.

143. Never losing an opportunity to engage in controversy, Jurin wrote a treatise in reply.[1] We refer only to such parts of this pamphlet, and the ones which followed it, as bear on fluxions or the parties engaged in the discussions on fluxions.

In the preface Jurin says: "I, it seems, am the Reputed Author of the late dissertations under the name of Philalethes Cantabrigiensis, and the other Gentleman [Dr. Robert Smith] is . . . suspected of being my associate. . . . If Dr. Smith were to tell Mr. Robins, what he has often professed to other persons, that he had no hand in those papers; if to confirm this he were to remind him, that Philalethes has declared more than once, he wrote alone and unassisted; if I—But what signifies pleading, when the execution is over? Mr. Robins has already vented his Resentment to the utmost. . . ."

144. Not without interest is the following reference to young Euler in St. Petersburg, whose scientific achievements have been so very extraordinary. Jurin says that to make no reply to Robins's criticisms "might be such a discouragement to the hopeful young writer, whose name is prefixed

[1] *A Reply to Mr. Robins's Remarks on the Essay upon Distinct and Indistinct Vision Published at the End of Dr. Smith's Compleat System of Opticks.* By James Jurin, M.D., London, MDCCXXXIX.

to their common labours, and who possibly, when he comes to study *suo Marte*, and to see with his own eyes, or to meet with abler instructors, may make some figure in the Learned World, that pure humanity induces me to oblige them with this one Reply" (p. 54).

145. Of course, Robins wrote a tract in reply,[1] but only the preface of this tract demands our attention. In answer to the charge made by Jurin, that he (Robins) had conducted the controversy "with passion and abuse," Robins proceeds to explain their past relations to each other.

"About six years since a pamphlet was publish'd under the title of the *Analyst*; in which the author endeavors to shew, that the doctrine of fluxions invented by Sir Isaac Newton is founded on fallacious suppositions. As that writer had a false idea of this doctrine, . . . I thought the most effectual method of obviating his objections would be to explain . . . what Sir Isaac Newton himself had delivered with his usual brevity. . . . And with this view I published a Discourse on Sir Isaac Newton's method of fluxions, and of prime and ultimate ratios. But in the mean time a controversy was carrying on between the author of the *Analyst* and another, who under the name of Philalethes Cantabrigiensis had undertaken the defence of Sir Isaac Newton: and as I at last perceived, both by the concessions

[1] *A Full Confutation of Dr. Jurin's Reply to the Remarks on his Essay upon Distinct and Indistinct Vision.* By Benjamin Robins, London, 1740.

of Philalethes, and the avowed opinions of others, that the erroneous conceptions of the writer of the *Analyst* on this head were more prevalent even amongst those, who approved of the method of fluxions, than I had at first believed ; I thought, it might be no unacceptable task more particularly to shew those, who were thus misled, how irreconcileable their opinions were with the tenets of Sir Isaac Newton, and how impossible it would be to defend the accuracy of his doctrine on these their mistaken suppositions ; and it was with this intention, that in an account of my book inserted in the *Present State of the Republick of Letters*, some of the errors contained in the writings of Philalethes Cantabrigiensis were endeavoured to be obviated.

"But tho' this discourse was written with great caution, and only mentioned the principles objected to without so much as naming or even insinuating the treatises, from whence they were taken ; yet, as Dr. Jurin, who was generally reputed the author of them, was one, that I often conversed with ; at my request, before this paper was printed, a common friend carried to him the manuscript, and, without pretending to suppose, whether he was, or was not Philalethes, desired him to read it, and asked him if he thought, Philalethes could be displeased with any thing contained in it ; he was also told at the same time, that if he believed any part of it could give offence to that gentleman, whoever he were, it should be struck out, or that I would even let the whole design fall, if he desired it.

"My friend brought me the Doctor's answer importing, that he could not believe, my paper would displease any one, since, if the tenets, I excepted to, were really erroneous, it was reasonable, they should be exposed; and if otherwise, it was the business of Philalethes to defend them . . . it was however added, that I had in two places censured doctrines, which, if I supposed them to be the opinions of Philalethes, I must have misapprehended him. Now . . . I immediately expunged them, and published the remaining part in the *Republick of Letters* for October 1735, as an account of my book on Sir Isaac Newton's method of fluxions, and of prime and ultimate ratios.

"To this Philalethes answered in the following month, and I again replied, till five papers were successively written in this controversy, that is, three by me, and two by him. And all this time so very desirous was I on my part of avoiding irritating circumstances . . . that I thought even the most intimate friend . . . could not be offended with it. . . . But alas . . . Philalethes in his reply, part of which was published in the July following, and the rest in the succeeding month, runs out into the most extravagant heats of passion . . . charging me with dishonestly writing against the convictions of my own judgment. . . . After so gross and unprovoked an abuse, . . . I should surely have been acquitted of any breach of decency, if . . . I had sharply exposed his ignorance in the subjects, he had attempted. But I chose, if possible, to avoid the ridicule of quarreling on a matter of mere speculation . . . I again requested my friend to speak to Dr. Jurin, and to represent to him the inconveniencies, that would arise from the perseverance of Philalethes in his rash and groundless calumny. My friend accordingly went to Dr. Jurin, and carried with him an answer to so much of Philalethes's paper, as was then published, and told the Doctor, that he came to propose to him a method, that might prevent the controversy betwixt me and Philalethes from degenerating into a passionate personal altercation . . . that therefore, if Dr. Jurin thought it expedient, my paper should be given to a certain gentleman, to whose impartiality and knowledge of the subject in debate no exception could be taken on either side; and that if, when that gentleman had perused it, he should believe, I had in any instance changed my opinion from my first entering into this dispute, I did then promise to submit patiently and without reply to any censures of unfairness and dishonesty, that Philalethes . . . should hereafter think proper, . . . [otherwise] it would then be but common justice, that Philalethes should moderate the remaining part of his performance. . . . But this proposal was rejected. . . . It was immediately given out, that my friends had confessed me to have been foiled in the argument; and were now only sollicitous to support me from the charge of unfairness. . . . The reader will not wonder, if I resolved for the future to treat him with that freedom, which his unskilfulness authorised. . . ."

146. The above preface constitutes what we may call Robins's *apologia pro vita sua*. It seems only fitting that Jurin should appear with a similar document. This he did in a long *Letter*.[1]

We make the following quotations from Jurin (p. 8):

"About five years ago some passages in a paper of Mr. Robins, were shown to me . . . and a question was put to me, whether I should take it ill, if those passages were printed, it being intimated, that Philalethes, against whom they were designed, might possibly be some friend of mine: and indeed, several persons were then guessed at, all of which happened to be my friends. To this . . . I gave answer, that I should not at all take it ill. But I added, that as I had read the controversy between Philalethes and the Author of the *Analyst*, with some attention, it seemed to me that in one or two passages Mr. Robins imputed opinions to Philalethes, which . . . that gentleman did not hold. . . . Also, I took notice, that Mr. Robins did not rightly explain Sir Isaac Newton's first Lemma. . . . But when I desired to talk with Mr. Robins about the Lemma, before the papers went to the press, as imagining I could convince him that he was in the wrong, answer was made, that the question was

[1] *A Letter to . . . Esquire, In Answer to Mr. Robins's Full Confutation of the Reply to his Remarks on the Essay upon distinct and indistinct Vision.* By James Jurin, M.D., London, 1741.

not whether I thought him in the right or in the wrong, but only whether I should take anything amiss; to which I replied as before. Upon talking with another friend of Mr. Robins a day or two after, I repeated my desire to talk with Mr. Robins about his explanation of the Lemma, before his papers went to the press: but was told that could not be, for that the part of the papers where the Lemma was spoke of, was to go to the press that afternoon. . . . I do not remember, that any offer was made to me of 'letting the whole design fall, if I desired it.' Had any such offer been made, I had at that time so much regard for Mr. Robins, that I think I should at least have desired him to stop the design, till he and I had examined the Lemma together, in order to prevent his exposing himself in the manner he has since done. As to the second application made to me near a year after, it may easily be judged, that I, who gave these gentlemen no reason to think I had any influence over Philalethes, or so much as knew who he was, could neither comply with nor reject their proposal" (p. 9).

Remarks

147. The debate between Jurin and Robins is the most thorough discussion of the theory of limits carried on in England during the eighteenth century. It constitutes a refinement of previous conceptions.

Jurin possessed the more general conception of a limit in insisting that there are variables which reach their limits. His interpretation of Newton on this point appears to us more nearly correct than that of Robins; Jurin's geometric illustrations of limit-reaching variable, intended to aid the imagination, though as he admits incapable of exhibiting the process "all the way," are nevertheless interesting (see our §§ 124, 132, 133). The imagination is subject to limitations where the reason is still free to act.

Robins, and after him Pemberton, deserve credit in clearly, openly, and completely breaking away from infinitely little quantities, and from prime and ultimate ratios. Robins's conception of a limit was narrow, but this narrowness had certain pedagogical advantages, since it did not involve a mode of advance to the limit which altogether transcended the power of the imagination to follow all the way (see our §§ 117, 118, 129, 130).

It is interesting to observe that both Jurin and Robins disavow belief in the possibility of a subdivision of a line into parts so as to reach a point—they assert "that such subdivision can never be actually finished" (see our §§ 126, 132).

Robins discarded the use of Newton's *moments* in developing the theory of fluxions (see our §§ 119, 120).

Toward the end of his long debate with Robins, Jurin begins to disavow infinitely small quantities. He brings out the difference between infinitesimals as variables, and infinitesimals as constants. He rejects all quantity "fixed, determinate, invariable, indivisible, less than any finite quantity whatsoever," but he usually admits somewhat hazily a quantity "variable, divisible, that, by a constant diminution, is conceived to become less than any finite quantity whatever, and at last to vanish into nothing." (See our §§ 132, 138, 141.)

While Berkeley's *Analyst* and Berkeley's replies to Jurin and Walton involved purely destructive criticism, the present controversy between Jurin and Robins brought forth valuable constructive results. Jurin's papers against Robins are decidedly superior to those he wrote against Berkeley, though here too they contained much that was not pertinent to the subject and was intended merely to amuse the general reader.

CHAPTER V

TEXT-BOOKS IMMEDIATELY FOLLOWING BERKELEY'S ATTACK

148. The *Analyst* was published in 1734; two years later appeared four books on fluxions. Thus, more British text-books on this subject were published in 1736 than in all the thirty years preceding. That the *Analyst* controversy was largely the cause of this increased productivity there can be no doubt. We proceed to give an account of the books which preceded the publication of Maclaurin's *Treatise of Fluxions*, 1742.

John Colson, 1736

149. Newton's *Method of Fluxions*,[1] said to have been written in 1671, was translated and first published in 1736 by John Colson. Colson had been a student at Christ Church, Oxford, which he left without taking a degree. He was appointed

[1] *The Method of Fluxions and Infinite Series; with its Application to the Geometry of Curve-Lines. By the Inventor, Sir Isaac Newton, Kt., Late President of the Royal Society. Translated from the Author's Latin Original not yet made publick. To which is subjoined, A Perpetual Comment upon the Whole Work*, . . . By John Colson, M.A. and F.R.S., Master of Sir Joseph Williamson's free Mathematical-School at Rochester. London, M.DCC.XXXVI. This book was reissued in 1758.

master of a new mathematical school founded at Rochester, and, in 1739, Lucasian professor of mathematics at Cambridge, in succession to Nicholas Saunderson. Colson was a man of great industry but only ordinary ability.

In his preface, Colson refers to the controversies on fluxions, and says that the defenders as well as their opponents were little acquainted with Newton's own exposition, that this book now published for the first time is "the only genuine and original Fountain of this kind of knowledge. For what has been elsewhere deliver'd by our Author, concerning this Method, was only accidental and occasional" (p. x). Colson accompanies Newton's book "with an ample Commentary" and "particularly with an Eye to the fore-mention'd Controversy" (p. x). Colson in this preface represents Newton as holding the principle "that Quantity is infinitely divisible, or that it may (mentally at least) so far continually diminish, as at last, before it is totally extinguish'd, to arrive at Quantities that may be call'd vanishing Quantities, or which are infinitely little, and less than any assignable Quantity. Or it supposes that we may form a Notion, not indeed of absolute, but of relative and comparative infinity" (p. xi). Colson opposes "indivisibles," as also the "infinitesimal method" and "infinitely little Quantities and infinite orders and gradations of these, not relatively but absolutely such" (p. xii). He argues against "imaginary Systems of infinitely great and infinitely little Quantities, and their several orders and properties, which, to all sober Inquirers into mathematical Truths, must certainly appear very notional and visionary" (p. xii), for "Absolute Infinity, as such, can hardly be the object either of our Conceptions or Calculations, but relative Infinity may, under a proper regulation" (p. xii). Newton "observes this distinction very strictly, and introduces none but infinitely little Quantities that are relatively so." Colson answers Berkeley's criticism in the *Analyst* of Lemma 2, Book II, in the *Principia* in the following manner :—

"Let X and Y be two variable Lines. . . . Let there be three periods of time, at which X becomes $A - \frac{1}{2}a$, $A, A + \frac{1}{2}a$; and Y becomes $B - \frac{1}{2}b$, B, and $B + \frac{1}{2}b$ . . . Then . . . the Rectangle XY will become . . . $AB - \frac{1}{2}aB - \frac{1}{2}bA + \frac{1}{4}ab$, AB, and $AB + \frac{1}{2}aB + \frac{1}{2}bA + \frac{1}{4}ab$. Now in the interval from the first period of time to the second . . . its whole Increment during that interval is $\frac{1}{2}aB + \frac{1}{2}bA - \frac{1}{4}ab$. And in the interval from the second period of time to the third, . . . its whole Increment during that interval is $\frac{1}{2}aB + \frac{1}{2}bA + \frac{1}{4}ab$. Add these two Increments together, and we shall have $aB + bA$ for the compleat Increment of the Product XY" (p. xiii), called the "Moment of the Rectangle" when a and b are infinitely little.

Another mode of procedure is this: "the Fluxions or Velocities of increase, are always proportional to the contemporary Moments." "When the Increments become Moments, that is, when a and b are so far diminish'd, as to become infinitely less than A and B; at the same time ab will become infinitely less than either aB or bA (for[1] $a\mathrm{B} \,.\, ab :: \mathrm{B} \,.\, b$, and $b\mathrm{A} \,.\, ab :: \mathrm{A} \,.\, a$), and therefore it will vanish in respect of them. In which case the Moment of the Product or Rectangle will be $a\mathrm{B}+b\mathrm{A}$, as before" (p. xv). Newton, however, prefers the more direct way previously explained.

Proceeding to Newton himself, we find (on p. 24) the following definition: "The Moments of flowing Quantities (that is, their indefinitely small Parts, by the accession of which, in infinitely small portions of Time, they are continually increased) are as the Velocities of their Flowing or Increasing. Wherefore if the Moment of any one, as x, be represented by the Product of its Celerity $\dot{x}$ into an indefinitely small Quantity o (that is, by $\dot{x}\ o$), the Moments of the others v, y, z, will be represented by $\dot{v}o$, $\dot{y}o$, $\dot{z}o$; because $\dot{v}o$, $\dot{x}o$, $\dot{y}o$, and $\dot{z}o$, are to each other as $\dot{v}$, $\dot{x}$, $\dot{y}$, and $\dot{z}$." On p. 25 terms containing o as a factor "will be nothing in respect of the rest. Therefore I reject them."

150. Colson appended extensive annotations to Newton's treatise. In these annotations, p. 250, Colson speaks of "smallest particles," but the term "smallest" does not occur in Newton's definition. However, Colson says that he does not mean "atoms" nor "definite and determinate magnitude, as in the Method of Indivisibles," but things "indefinitely small; or continually decreasing, till

[1] Here $a\mathrm{B} \,.\, ab :: \mathrm{B} \,.\, b$ means $a\mathrm{B} : ab :: \mathrm{B} : b$.

they are less than any assignable quantities, and yet may then retain all possible varieties of proportion to one another. Becoming still more deeply involved in the metaphysics of the subject, Colson adds "that these Moments are not chimerical, visionary, or merely imaginary things, but have an existence *sui generis*, at least Mathematically and in the Understanding, is a necessary consequence from the infinite Divisibility of Quantity, which I think hardly anybody now contests" (p. 251). This he qualifies, "perhaps the ingenious Author of . . . *The Analyst* must be excepted, who is pleased to ask, in his fifth Query, whether it be not unnecessary, as well as absurd, to suppose that finite Extension is infinitely divisible" (p. 251). By *ultimate ratio* Colson means the ratio when the arguments "become Moments" (p. 255). Fearing that *moments*, *infinitely little quantities*, and the like, "may furnish most matter of objection," he says (p. 336) that the symbol *o* at first represents a finite quantity, which then diminishes continually till "it is quite exhausted, and terminates in mere nothing." But "it cannot pass from being an assignable quantity to nothing at once; that were to proceed *per saltum*, and not continually"; hence "it must be less than any assignable quantity whatsoever, that is, it must be a vanishing quantity. Therefore the conception of a Moment, or vanishing quantity, must be admitted as a rational Notion" (p. 336). Again: "The Impossibility of Conception may arise from the narrowness and imperfection of our Faculties, and not from any inconsistency in the nature of the thing"; these quantities "escape our imagination." Referring to imaginaries, $a\sqrt{-1}$ in the solution of cubic equations, Colson says (pp. 338–9): "These impossible quantities . . . are so far from infecting or destroying the truth of these Conclusions, that they are the necessary means and helps of discovering it. And why may we not conclude the same of that other species of impossible quantities, if they must needs be thought and call'd so? . . . Therefore the admitting and retaining these Quantities . . . 'tis enlarging the number of general Principles and Methods, which will always greatly contribute to the Advancement of true Science. In short, it will enable us to make a much greater progress and proficience, than we otherwise can do, in cultivating and improving what I have elsewhere call'd The *Philosophy of Quantity.*"

151. A review[1] of this book contains the following historical exposition. Sir Isaac Newton, 1665, "found the Proportions of the Increments of indeterminate Quantities. These Increments or *Augmenta Momentanea* he called *Moments*, which others called Particles, infinitely small Parts, and Indivisibles; and the Velocities by which the Quantities increased he called Motions, Velocities of Increase, and Fluxions. He considered Quantities not as composed of Indivisibles, but as generated by local Motion, after the manner of the Ancients . . . and represented such Moments [of Time] by the Letter *o*,

[1] *Republick of Letters*, Art. XI, pp. 223–235, 1736.

or by any other Mark drawn into an Unit" (p. 228). "Fluxions are not Moments, but finite Quantities of another kind." "When Mr. Newton is demonstrating any Proposition, he considers the Moments of Time in the Sense of the Vulgar, as indefinitely small, but not infinitely so; and by that means performs the whole work, in finite Figures, by the Geometry of *Euclid* and *Apollonius*, exactly without any Approximation: and when he has brought the work to an Equation, and reduced the Equation to the simplest Form, he supposes the Moments to decrease and vanish; and from the terms which remain he deduces the Demonstration. But when he is only investigating any Truth, or the Solution of any Problem, he supposes the Moment of Time to be infinitely little, in the Sense of Philosophers, and works in Figures infinitely small."

James Hodgson, 1736

152. James Hodgson, a mathematical teacher and writer, and a fellow of the Royal Society of London, is the author of a book, *The Doctrine of Fluxions.*[1]

Hodgson says in his Introduction that "it is now some years since the greatest Part of this Book was prepared for the Press." There is no direct reference in the book to the *Analyst* controversy, but the declaration is made that the principles upon which fluxions rest need "fear no Opposition."

[1] *The Doctrine of Fluxions, founded on Sir Isaac Newton's Method, Published by Himself in his Tract upon the Quadrature of Curves.* By James Hodgson, London, MDCCXXXVI.

Hodgson also says in his Introduction that most books on fluxions that have hitherto appeared proceeded on the same principles as the Differential Calculus, so that "by calling a *Differential* a *Fluxion*, and a second *Differential* a second Fluxion, etc., they have . . . confusedly jumbled the Methods together," although the principles are really "very different." "The *Differential* Method teaches us to consider Magnitudes as made up of an infinite Number of very small constituent Parts put together; whereas the *Fluxionary* Method teaches us to consider Magnitudes as generated by Motion . . . ; so that to call a *Differential* a *Fluxion*, or a *Fluxion* a *Differential* is an Abuse of Terms." In the method of fluxions, "Quantities are rejected, because they really vanish"; in the differential method they are rejected "because they are infinitely small." Hodgson adds that he always used the differential method "'till I became acquainted with the *Fluxionary* Method." He considers fluxions of quantities (p. 50) "in the first Ratio of their *nascent Augments*, or in the last ratio of their *evanescent Decrements*," and gives an able and faithful exposition of Newton's ideas as found in his *Quadrature of Curves*. He cannot think "there is any more difficulty in conceiving or forming an adequate Notion of a nascent or evanescent Quantity, than there is of a Mathematical Point" (p. xi). In explaining the derivation of the fluxion of the product $xy=z$ he apparently permits (p. xv) the small quantity o to "vanish," and thereupon divides both sides of the equation $\dot{x}\dot{y}o+\dot{y}xo=\dot{z}o$ by o. However, in the exposition given on p. 50 he is more careful and divides by o while o is an increment, and obtains $y\dot{x}+\dot{x}y+\dot{y}\dot{z}o=\dot{v}$. Then he says: "Imagine the Quantity o to be infinitely diminished, or, which is the same thing, the Quantity xy to return back again into its arising State; then the Quantity $\dot{x}\dot{y}o$, in this Case, into which o is multiplied, will vanish; whence we shall have $x\dot{y}+y\dot{x}=\dot{v}$ for the Fluxion of the Quantity proposed." Hodgson follows Newton closely and permits the variable to reach its limit.

Thomas Bayes, 1736

153. An anonymous pamphlet of 50 pages, on the *Doctrine of Fluxions*,[1] has been ascribed to Rev. Thomas Bayes. This author contributed in 1763 to the *Philosophical Transactions* a meritorious article on the doctrine of chances.

The pamphlet of 1736 represents a careful effort to present an unobjectionable foundation of fluxions. "The fluxion of a flowing quantity is its rate or swiftness of increase or decrease." Let a, b, x, and y be flowing quantities, let A and B be permanent quantities; if $a:b=A\mp x : B\mp y$, during any time T, and at the end of that Time, a, b, x, y all vanish; then . . . the ratio of A to B is the last ratio of the vanishing quantities a and b (p. 13). This definition is "in effect the same" as that given by Newton.

[1] *Introduction to Doctrine of Fluxions and Defence of the Mathematicians against . . . the Analyst*, 1736.

The author speaks of "that most accurate definition of the ultimate ratio's of vanishing quantities; which we have at the latter end of *Sch. Lemma* XI *Princip.* [see our §§ 10–15], and which is so plain, that I wonder how our author [Berkeley] could help understanding it; which had he done, I am apt to think that all his *Analyst* says concerning the proportion of quantities vanishing with the quantities themselves, had never been heard: For according to this definition, we are not obliged to consider the last ratio as ever subsisting between the vanishing quantities themselves. But between other quantities it may subsist, not only after the vanishing quantities are quite destroyed, but before when they are as large as you please. And the reason why we consider quantities as decreasing continually till they vanish, is not in order to make, but to find out, this last ratio. Sir Isaac Newton does not indeed say that this last ratio is the ratio with which the quantities themselves vanish; but whether he herein speaks with the utmost propriety or not, is a mere nicety on which nothing at all depends" (p. 16, note).

Velocity "signifies the degree of quickness with which a body changes its situation in respect to space"; the fluxion of a quantity "signifies the degree of quickness with which the quantity changes its magnitude." "And when our author asserts, that in order to conceive of a second fluxion, we must conceive of a velocity of velocity, and that this is nonsense; he plainly appeals to the sound and not the sense of words . . . if . . . you make it synonymous to the word *Fluxion*, then the velocity of velocity . . . is nothing but plain common sense" (p. 19). Moments are not used by the author. The author says that, were he to write a treatise on fluxions, "in order to understand equations where Fluxions of different orders are jumbled together; it would be convenient to represent all Fluxions not as before, but as quantities of the same kind with their Fluents. . . . The Fluxion of a quantity anyhow flowing at any given instant is a quantity found out by taking it to the Fluxion of an uniformly flowing quantity in the ultimate proportion of those synchronal changes which then vanish" (pp. 34, 35). The variables x and x^n have the synchronal augments o and $nox^{n-1}+\frac{(n^2-n)}{2}o^2x^{n-2}+$, etc., which are to one another as $1 : nx^{n-1}+\frac{(n^2-n)}{2}ox^{n-2}+$, etc. "Let now these arguments vanish, and their last ratio will be $1 : nx^{n-1}$." "This our author says is no fair and conclusive reasoning, because when we suppose the 'increments to vanish, we must suppose their proportions, their expressions, and everything else derived from the supposition of their existence to vanish with them.' To this I answer, that our author himself must needs know thus much, viz. That the lesser the increment o is taken, the nearer the proportion of the increments of x and x^n will arrive to that of 1 to nx^{n-1}, and that by supposing the increment *o* continually to decrease, the ratio of these synchronal increments may be made to approach to it nearer than by any assignable difference, and can never come up with it before the time when the increments themselves vanish. . . . For tho', strictly speaking, it should be allowed that there is no last proportion of vanishing quantities, yet on this account no fair and candid reader would find fault with Sir Isaac Newton, for he has so plainly described the proportion he calls by this name, as sufficiently to distinguish it from any other whatsoever: So that the amount of all objections against the justice of this method in finding out the last proportion of vanishing quantities can arise to little more than this, that he has no right to call the proportions he finds out according to this method by that name, which sure must be egregious trifling. However, as on this head our author seems to talk with more than usual confidence of the advantage he has over his opponents, and gives us what he says is the amount of Sir Isaac's reasoning, in a truly ridiculous light, it will be proper to see on whom the laugh ought to fall, for I am sure somebody must here appear strangely ridiculous, . . . I readily allow whatever consequence he is pleased to draw from it, if it appears that Sir Isaac, in order to find the last ratios proposed was obliged to make two inconsistent suppositions. To confute which nothing more need be said than barely to relate the suppositions he did make.

"1. Then he supposes that x by increasing becomes $x+o$, and from hence he deduces the relation of the increment of x and x^n.

"2. Again, in order to find the last ratio of the increments vanishing, he supposes o to decrease till it vanishes, or becomes equal to nothing. . . . These are evidently no more inconsistent and contradictory, than to suppose a man should first go up stairs, and then come down again. To suppose the increment to be something and nothing at the same time, is contradictory; but to suppose them first to exist, and then to vanish, is perfectly consistent; nor will the consequences drawn from the supposition of their prior existence, if just, be any ways affected by the supposition of their subsequent vanishing, because the truth of the latter supposition no ways would have been an inconsistency; but to suppose them first unequal, and afterwards to become equal, has not the shadow of difficulty in it. . . . must confess there seems to be some objection against considering quantities as generated from moments. What moments, what the *principia jamjam nascentia finitarum quantitatum*, are in themselves, I own, I don't understand. I can't, I am sure, easily conceive what a quantity is before it comes to be of some bigness or other; and therefore moments considered as parts of the quantities whose moments they are, or as really fixed and determinate quantities of any kind, are beyond my comprehension, nor do I indeed think that Sir Isaac Newton himself did thus consider them" (pp. 35–41).

John Muller, 1736

154. John Muller, a German by birth, dates his *Mathematical Treatise*,[1] 1736, from the Tower of London, and dedicates it to the master-general of the ordnance. He was appointed in 1741 head-master of the Royal Military Academy, Woolwich. He was "the scholastic father of all the great engineers this country employed for forty years."

The author's method of explaining fluxions is somewhat unique. "I make no use of infinitely small quantities nor of nascent or evanescent velocities; and yet I think to have explained those Principles, so that any Person of a moderate capacity . . . may be fully convinced of the Truth thereof" (Preface). He begins his conic sections with the postulate: "Grant that two infinite quantities, differing from each other by a finite quantity, may be esteemed equal." He then explains that this postulate "is here of use only to shew the connection of the Conic-Sections," and hastens to assure the reader that "whenever we make use of it in the demonstration of any Proposition, we shall give always another Demonstration independent on it."

In the *Republick of Letters*, June, 1736, occurs the following comment:

"He introduces this [Conic-Sections] by a Postulatum that sounds very absurdly to those that are

[1] *A Mathematical Treatise: Containing a System of Conic-Sections; with the Doctrine of Fluxions and Fluents, Applied to various Subjects.* By John Muller. London, 1736.

not vers'd in mathematical Speculations. 'Grant,' says he, 'that two infinite Quantities, differing from one another by a finite Quantity, may be esteemed equal.' Such would imagine that there could not be two infinite Quantities; or that if there could, they must necessarily be absolutely and not only reputedly equal. But however Hobbes or Berkeley may talk of geometrical Fallacies, or these unexperienced People think, the Adepts in this Science very well know, that more infinite Quantities than two are possible, and that one Quantity may be infinitely greater than an infinite one, and yet be itself infinitely less than a third. But enough of these *Ludibria* Scientiæ, that I may inform the Publick of the more useful Theorems . . ." (pp. 422, 423).

Muller considers in his text a curve generated by a point "urged by two powers acting in two different directions, the one parallel to the Abscisses and the other parallel to the Ordinates. I prove from thence, that if this point (when arrived at a given place) did continue to move with the velocity it has there, it would proceed in a right line touching the Curve in that place . . . So that the three Directions being known in each place, the proportion between the velocities of the urging powers is likewise known." Fluxions are defined as velocities. To find the fluxion of y^2, he puts $y^2=x$; the subtangent of the parabola is $2y^2$ Since the subtangent is to the ordinate as the velocities along the abscissa and ordinate, he has $2y^2 : y : : \dot{x} : \dot{y}$, or $\dot{x}=2y\dot{y}$, and $2y\dot{y}$ is the required fluxion. Similarly, to find the fluxion of y^3, let $x=y^3$. Take $u=z^3$, the $u-x=\overline{z-y} \times \overline{zz+zy+yy}$, or $z-y : u-x = 1 : zz+zy+yy$. If now y and z approach continually until they coincide with an intermediate ordinate, then $z=y$ and the chord through the extremities of the ordinates y and z will likewise coincide with the tangent. Therefore, the ordinate is to the subtangent as 1 is to $3yy$. Hence the proportion $1 : 3yy = \dot{y} : \dot{x}$, or $\dot{x} = 3yy\dot{y}$, the fluxion required. The same argument is applied to y^m. In these demonstrations appeal is made to a geometric figure, and no attention is directed to the ratio $z-y : u-x$ for the difficult case when $y=z$. The author remarks that "though we commonly say that . . . $m\dot{y}y^{m-1}$ is the Fluxion of y^m; yet that expression is not sufficiently accurate: Therefore, the sense in which we desire to be understood is, that $1 : my^{m-1} :: \dot{y} : m\dot{y}y^{m-1}$, that is, unity is to my^{m-1}, or $\dot{y}$ is to $m\dot{y}y^{m-1}$, as the fluxion or velocity with which y is generated, is to the fluxion, or contemporary velocity with which y^m is generated, and so for the rest" (p. 79). Thus, the emphasis is placed upon the *ratios* of velocities.

Anonymous Translation[1] *of Newton's "Method of Fluxions,"* 1737

154*a*. Colson's translation from the Latin of Newton's *Method of Fluxions*, published in 1736, was followed in 1737 by a second translation, which

[1] *A Treatise of the Method of Fluxions and Infinite Series, With its Application to the Geometry of Curve Lines. By Sir Isaac Newton, Kt. Translated from the Latin Original not yet published. Designed by the Author for the Use of Learners.* London, MDCCXXXVII.

was anonymous. In it no mention is made of Colson's edition. The anonymous translator says in the preface: "We have reason to believe that what is here delivered, is wrought up to that Perfection in which Sir Isaac himself had once intended to give it to the Publick. The ingenious Dr. Pemberton has acquainted us that he had once prevailed upon him to complete his Design and let it come abroad. But as Sir Isaac's Death unhappily put a stop to that Undertaking, I shall esteem it none of the least Advantages of the present Publication, if it may prove a means of exciting that Honourable Gentleman, who is possessed of his Papers, to think of communicating them to some able Hand; that so the Piece may at last come out perfect and entire." As remarked by G. J. Gray,[1] the two translations were made "from copies of the same manuscript," and differ from each other only "in the mode of expressing the work in English."

James Smith, 1737

155. In his *New Treatise of Fluxions*,[2] Smith says (Preface): "What I call here the New Method, and the Six Propositions immediately following,

[1] *A Bibliography of the Works of Sir Isaac Newton.* By George J. Gray. Second edition, Cambridge, 1907, p. 47.

[2] *A New Treatise of Fluxions, containing, I. The Elements of Fluxions, demonstrated in Two easy Propositions, without first or last Ratios, II. A Treatise of Nascent and Evanescent Quantities, first and last Ratios, III. Sir Isaac Newton's Demonstration of the Fluxions enlarged and illustrated: IV. Answers to the Principal Objections in the Analyst.* By James Smith, A.M., London, 1737.

are entirely New . . . Our common Definition of Motion, *translatio corporis de loco in locum* is certainly imperfect, and I am inclined to think, that Aristotle's old exploded Definition of Motion will, some time or other, come into Vogue again. *Actus entis in potentia, quatenus in potentia est.* Motion is an Effect, and every Effect has a coinstaneous Existence with the Action by which it is produced."

The definitions with which Smith starts out are not very reassuring. "The fluxion of a surface is the Velocity of the generating Line." "The velocity of a generating Line is the Sum of the Velocities of all the Points of that Line, whether these Points move with equal, or unequal Velocities." The rectangle xy "flows or increases by the flowing of both its contiguous Sides" together; but it "flows into Length" by the velocity $y\dot{x}$, and "it flows into Breadth at the very same Instant of Time" by the velocity $x\dot{y}$. "Therefore the Velocity with which it flows into Length and Breadth is the Sum of the synchronic Velocities," $x\dot{y}+y\dot{x}$.

Nor is the second topic displayed with illumination. "A nascent Quantity is a Quantity in the Instant of its commencing to exist." Similar to this is the definition of "evanescent Quantity," as are also the definitions of first and last ratios. Interesting is the following proof that if "two Quantities begin and cease to exist in any finite Time T, . . . they have a first and a last Ratio," for, "if they have not a first Ratio, they have not a second nor a third Ratio, etc. Therefore they have no Ratio in the Time T; but in the Time T they are Quantities," and "two quantities of the same kind, as soon or so long as they have any Quantity, Being or Existence [*i.e.* are not absolutely nothing], have a Ratio the one to the other," that is, "they have a Ratio and they have not a Ratio in the Time T, which is absurd." Smith argues also that since two quantities "cannot be in their first Ratio, neither before nor after the Beginning of the Time T, they must have been in their first Ratio at the very Beginning of the Time T, just as they began to exist." Near the close of this part of his book, Smith reveals some of the subtleties of his topic by stating an "Objection" and the "Answer" to it. The Objection: "Nascent and evanescent Quantities are Something or Nothing; for, *Inter ens et non-ens non datur medium.* If Something, then the Ratio of evanescent Quantities is the same with the Ratio before they were evanescent, or when they had any finite Magnitude. . . . If they are mere Nothing, or Non-quanta; then $Bb / Ee = 0 / 0 = 0$; . . . which is absurd." In the "Answer" Smith says: "Evanescent Quantities are really nothing, or Non-quanta; for it is evident . . . that upon *b*'s coinciding with B, and *e*'s coinciding with E, the Increments B*b* and E*e* are annihilated, and evanescent Quantities are never accurately evanescent, but upon this or the like Coincidence. And yet it does not follow that their last Ratio, or the Ratio they nihilesce with, is Nothing. For B*b* / E*e* is neither B*b* nor E*e*, nor B*b* and E*e*, but a Mark or Expression of their Ratio, which may be expressed as well by any other Character. . . . The Increments are indeed annihilated and gone, but their last Ratio remains, and is as real as any Ratio they ever had"; . . . they have as real a Ratio at the last Instant of their Existence; that is, when they are ceasing to be Something, and commencing to be Nothing, as they had at any instant preceding the last Instant of their Existence." . . . "There is, sometimes, something very strange in the Nature of these evanescing Augments, and it is literally true of them, what Juvenal figuratively says of Man.

—Mors sola fatetur,
Quantula sunt hominum corpuscula—

We know nothing of them till they be dead and gone."

Of Part III, in which Smith "demonstrates" Newton's Method of Fluxions, we quote only the last sentence: "I have made use of infinitely little Quantities, and of a second Point as being next to a first Point; but this was only for Illustration sake. There is not the least Occasion for any of these Notions in the Demonstration."

In the last part of Smith's book, Berkeley's contention, "No just Conclusion can be drawn from two contrary Suppositions," is answered by the statement, "This is certainly true, *in sensu composito*, but *in sensu diviso* is intirely false."

We are tempted to make the remark that in 1737 Smith left the subject even more mysterious than he found it.

Thomas Simpson, 1737

156. Thomas Simpson, the son of a weaver, was a self-taught mathematician, and acquired a knowledge of fluxions through Stone's translation of De L'Hospital's *Analyse des infiniment petits*. Simpson was a mathematician of marked power, and influenced considerably the teaching of mathematics in England. In 1737 he brought out his *New Treatise of Fluxions*,[1] which contains some novel features.

"The Fluxions of variable Quantities are always measured by their Relation to each other; and are ever expressed by the finite Spaces that would be uniformly described in equal Times, with the Velocities by which those Quantities are generated."

He finds it easy to show that the fluxion of a rectangular area of constant height and uniformly variable base is as the height drawn into the velocity with which the base changes; also that the fluxion of a curvilinear area generated by an abscissa moving with uniform velocity is at a given point, as the ordinary y for this point, multiplied by that velocity. This last result is applied to finding the fluxion of xy.

Avoiding infinitely small quantities, Simpson finds

[1] *A New Treatise of Fluxions: wherein the direct and inverse Method are demonstrated after a new, clear and concise Manner, with their Application to Physics and Astronomy.* By Thomas Simpson, London, 1737.

the ratio of the fluxions of x and x^2 thus: Let the points m and n move so that the distance h described by n shall always equal the square of the distance g described by m in the same time. Then $(AR)^2 = CS$, $(AR - Rr)^2 = Cs$, and $sS = 2AR \times Rr - (Rr)^2$. But sS is described with accelerated velocity when m moves uniformly, hence sS will be "less than that which would be uniformly described in the same time with the Velocity at the point S, and greater than that which would be described with the Velocity at the point s; and therefore must be equal to the Distance that would be uniformly described with the Velocity at another point e posited somewhere between S and s, in the same Time that the other point m is moving over the Distance rR; therefore $rR : 2AR \times Rr - (Rr)^2 :: g : g(2AR - Rr)$, the Distance that would be described with the Velocity of n, at the point e, in the same Time that m is moving over the Distance g: Now therefore when the points r and s coincide with R and S, then will e coincide with S; . . . and consequently $(2AR - Rr)g$ will then . . . become $2AR \times g$, equal to h the required Distance." The critical part of this proof is "when the points r and s coincide with R and S, then will e coincide with S." A modification of this proof is applied to x^n.

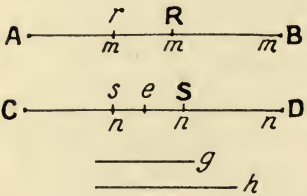

FIG. 7.

Simpson's text marks a departure from Newton in the definition of fluxion. Newton makes it a velocity, Simpson makes it a finite distance. On the necessity and wisdom of this change there can readily be difference of opinion. But there can be no denying that Simpson developed his theory of fluxions in a manner almost, though not entirely, free from the objections against fluxions that had been advanced by Berkeley ; infinitely small quantities are nowhere used. A short but appreciative review of this text appeared in *The Works of the Learned* for July, 1737.

Benjamin Martin, 1739, 1759

157. Benjamin Martin was a mathematician, an optical instrument maker, and a general compiler. He was a self-educated man, and at one time taught reading, writing, and arithmetic. His exposition of fluxions, as found in his *Elements of all Geometry*[1] and in a later work, is below the standard usually reached by him in mathematical writing.

This book, intended as an introduction to modern mathematics, contains in an Appendix an epitome of the doctrine of fluxions. "Since Fluxions are the very small *Increments* and *Decrements* of the Flowing Quantities, or the Velocities of the Motions whereby they increase or decrease, 'tis plain that those Fluxions, or Velocities, themselves may be consider'd as Flowing Quantities, and their Fluxions are call'd Second Fluxions . . ." It would seem

[1] ΠΑΝΓΕΩΜΕΤΡΙΑ ; *or the Elements of all Geometry*. By B. Martin, London, M.DCC.XXXIX.

that in this statement a fluxion is "very small" and at the same time a "velocity." A little later the author refers to fluxions as "in the first Ratio of *Augmenta nascentia.*" Evidently, in this Appendix, covering twelve pages, the author has not succeeded in presenting a consistent theory of fluxions.

A fuller exposition was given twenty years later in the *System of Mathematical Institutions, agreeable to the Present State of the Newtonian Mathesis*, by Benjamin Martin, vol. i, London, MDCCLIX. The theory is still confusing. "Indefinitely small Spaces" (p. 362) are represented by $\dot{x}$ and $\dot{y}$, which are called the fluxions of x and y, and said to represent the velocities of moving points. Newton is reported to have at first delivered the idea of what Martin calls a fluxion, under the name of *momentum*, "a Term used in Mechanics to denote the Quantity of Motion generated by a given Quantity of Matter (A), and the Velocity (a) with which it moved conjointly. This *Momentum* therefore was properly represented by (Aa). . . . But instead of this mechanical Notation, we now use $x\dot{x}$ and $y\dot{y}$ for the *Momenta*, or Fluxions. . . ." It is seldom that one encounters a more grotesque conglomeration of unrelated ideas than is presented here. Martin gives John Rowe's mode of deriving the fluxions of xy and xyz.

An Anonymous Text, 1741

158. *An Explanation of Fluxions in a Short Essay on the Theory.* London: Printed for W. Innys, at the

West-End of St. Paul's, MDCCXLI. This anonymous publication of 16 pages was reprinted in 1809 in the fourth edition of John Rowe's *Doctrine of Fluxions*; it constitutes a real contribution to the logic of fluxions. The pamphlet is offered "as an Explanation of the Doctrine itself, and not of Sir Isaac's Manner of delivering it." "About that," he says, "I don't mean, nor pretend to take a Part in any Controversy." He defines fluxions thus: "The word Fluxion properly apply'd always supposes the Generation of some Quantity (term'd Fluent or Flowing Quantity) with an equable, accelerated, or retarded Velocity, and is itself the Quantity which might be uniformly generated, in a constant Portion of Time, with the Amount or Remainder of that Velocity, at the Instant of finding such Fluxion." "Hence, it will appear that the first Fluxions of Quantities are as the Velocities with which those Quantities are increas'd; that second Fluxions are as the Increase or Decrease of such Velocities; and that by second, third, fourth, etc., Fluxions are meant Fluxions, whose Fluents are themselves Fluxions to other proposed Quantities; and the manner of considering and determining them is the very same as tho' they were first Fluxions, they being actually so to the Quantities from which they are immediately derived" (p. 7).

Then follows the lemma:

"The Fluxion of the Area ABC, whether triangular or curvilinear, is the Rectangle $\dot{x}y$."

Suppose B to move along AF while the ordinate

y terminates in the curve AC; "And, at any proposed Position BC, conceive y to become constant," while B "moves uniformly any constant Time, *mn*, with the Velocity at B, over the Distance $\dot{x}$ or BD; for then will y in the Time *mn* uniformly generate the Rectangle $\dot{x}y$, which Rectangle is plainly the Fluxion of ABC in this Position (*per Definit.*)." Then follows the illuminating scholium: "It has been commonly objected to the Accuracy of Fluxions, that the Trapezium or curvilinear Space BC*de*D, not the Rectangle $\dot{x}y$, is the Fluxion geometrically exact. But, this Objection is built, I apprehend, upon a false Idea of the Thing. It supposes a Fluxion a complete Part of a flowing Quantity, and an Infinity of Fluxions to constitute the flowing Quantity,

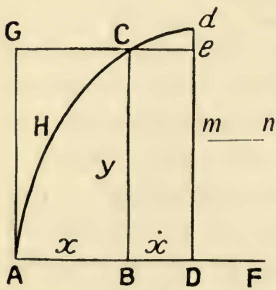

FIG. 8.

which are Mistakes (*per* Definition and Lemma) . . . if $\dot{x}$ be imagined infinitely little, an Infinity of Increments may constitute the Area ABC. But, in Fluxions, our Reasoning is quite different: a Fluxion can no more be called a Part of the Fluent, than an Effect a Part of the Cause. For Instance; from the Fluxion given we know the Fluent, and *vice versa*, just as when a Cause is known to produce a certain Effect, we can infer the one from a Knowledge of the other."

We shall find that later this reference to cause and effect figured in a controversy carried on against Simpson.

As regards the lemma given above, we shall see that the same idea is elaborated in detail by Maclaurin in his work and that a short and even more convincing statement than the one given here is found in the later, revised, text of John Rowe.

From the above lemma, the derivation of the fluxion of xy becomes easy by considering the rectangle ABCG as made up of two parts AHCB and AHCG, and applying the lemma to each part.

John Rowe, 1741, 1757, 1767

159. The first edition (1741) of John Rowe's *Doctrine of Fluxions*[1] appeared anonymously. A copy in the British Museum has the following added by hand after the preface: "This is the first edition of John Rowe's Fluxions. The second came out with his name in 1757 with alterations and additions, and the third came out in 1767 much improved."

In the first edition Rowe begins by stating his programme: "To render the Doctrine of Fluxions plain and easy" by explaining their nature "as deliver'd both by Sir Isaac Newton and by Leibniz." According to Newton, "Fluxion is the same as velocity."

"Definition II [Foreigners Definition]. Quantities are here suppos'd to be generated by a continual Increase, as before; and the indefinitely small Particles

[1] *An Introduction to the Doctrine of Fluxions. Revised by several Gentlemen well skill'd in the Mathematics.* Felicibus inde Ingeniis aperitur Iter—*Claudian.* London, M.DCC.XLI.

whereby they are continually increas'd, are call'd the *Fluxions* of these Quantities" (p. 3). "This is the Notion of Fluxions as deliver'd by Leibnitz and his Followers. But these Fluxions, we shall, in the following Sheets, call by the Names of *Moments*, *Increments* and *Decrements*; that is, *Moments* or *Increments* when the variable Quantities are increasing, and *Decrements* when they are decreasing" (p. 4).

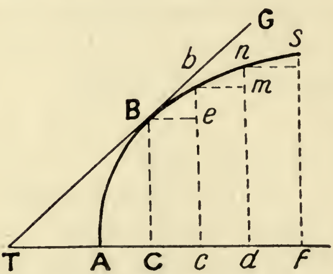

FIG. 9.

"As the Point *b* is continually nearer to a Coincidence with the Tangent TBG the nearer it approaches the Point of Contact B; so if we conceive the Ordinate *cb* to be moved on till it concides with CB; the very first moment before its Coincidence, the Curve B*b*, and Right line BG will be infinitely, or rather indefinitely near a Coincidence with each other; and consequently, in that Case, the Increments B*e*, and *eb* will come indefinitely near to measure the Ratio of the Fluxions of the Absciss and Ordinate AC, and CB, or the Velocities with which they flow at the Point B . . . and therefore (because when any Quantity is increas'd or decreas'd, but by only an infinitely or indefinitely small Particle, that Quantity may be consider'd as remaining the same as it was before;) these Increments may be taken as Proportional to, or for the Fluxions in all Operations; and, on the contrary, the Fluxion for the Increment" (pp. 5, 6). Accordingly, he deduces the rules of operation by the use of increments, and in the result substitutes the fluxion for the increment.

In finding the fluxion of xy he lets x' and y' be the increments, then the "increase in its nascent state" is such that $x'y'$ "bears no assignable Ratio to either $x'y$ or xy' (for as $x'y' : x'y :: y' : y$ and y' by Supposition is infinitely less than y," and can be "expunged or rejected."

160. The third edition (1767) was commented upon by J. Stubbs, Fellow of Queen's College, Oxford, as follows: "I received your valuable present, and was much surprised to find it so prodigiously improved. Indeed, it so much resembles a New Work, when compared with the First Edition, that I almost wish you had made no mention of its being the Third; but left the two former to be forgotten."

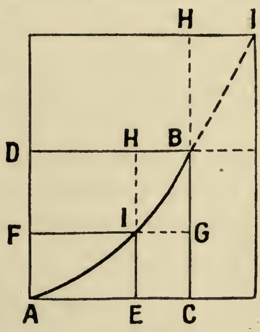

FIG. 10.

The fluxion of xy is now deduced thus: "The fluxion of the curvilinear space AEI is less than the fluxion of the rectangle (of constant altitude) AH before EH reaches BC, and greater after EH passes BC; hence at BC the two fluxions are alike and equal to $y\dot{x}$. Similarly, it follows that the rectangle AG (of constant base) has the same fluxion $x\dot{y}$ at DB as has the curvilinear space AFI. Hence the rectangle of variable base and altitude AEIF with the vertex I moving along the curve through B has the fluxion $x\dot{y}+y\dot{x}$."

In a footnote Rowe expressed the belief that this mode of deriving the rule is not open to criticism as was the method of using increments which in 1734 was "smartly attacked by the late acute Dr. Berkeley."

Rowe proves by a geometrical method similar to the above that the fluxion of a pyramid of fixed vertex and slant edges, whose variable base xy moves parallel to itself and whose variable altitude is z, is $xy\dot{z}$. Taking a parallelopipedon as equal to three pyramids, he finds the fluxion of xyz to be $\dot{x}yz+x\dot{y}z+xy\dot{z}$. This new way of deriving the fluxion of xyz was copied by "his friend" Benjamin Martin in the *Mathematical Institutions*.

At the end of the third edition of Rowe's *Fluxions* is a bibliography of English works on this subject, and he "particularly refers to the Works of his two celebrated Friends, Mr. Emerson and the late Mr. Simpson."

Berkeley Ten Years After

161. Berkeley, in his *Siris*[1] of 1744, expressed himself as follows: "Concerning absolute space, that phantom of the mechanic and geometrical philosophers (§ 250), it may suffice to observe that it is neither perceived by any sense, nor proved by any reason, and was accordingly treated by the greatest of the ancients as a thing merely visionary.

[1] George Berkeley's *Works*. Edition by A. C. Fraser, vol. ii, Oxford, 1871, p. 468 and note.

From the notion of absolute space springs that of absolute motion . . ." He continues in a footnote: "Our judgment in these matters is not to be overborne by a presumed evidence of mathematical notions and reasonings, since it is plain the mathematicians of this age embrace obscure notions, and uncertain opinions, and are puzzled about them, contradicting each other and disputing like other men: witness their doctrine of Fluxions, about which, within these ten years, I have seen published about twenty tracts and dissertations, whose authors being utterly at variance, and inconsistent with each other, instruct by-standers what to think of their pretensions to evidence."

Remarks

162. In these publications no reference is made to the Jurin-Robins controversy, though Berkeley's *Analyst* is frequently discussed. Excepting only in Benjamin Martin, the definition of a fluxion as a "differential" nowhere appears. Therein we see a step in advance.

The influence of Newton's *Quadrature of Curves* (1704) is evident almost everywhere. An improvement in the mode of deriving the fluxion of a "product" appears in the anonymous *Explanation of Fluxions* and in the revised text by John Rowe (our §§ 158, 160).

Noteworthy is Thomas Simpson's new definition of fluxions; this new definition plays an important rôle during the rest of the century.

163. We quote Sir William Rowan Hamilton's remarks on the lemma of the anonymous *Explanation of Fluxions* (1741) and the derivation of the fluxion of xy, based upon it. Hamilton knew this proof as it is given in a later edition of Simpson's fluxions. Says Hamilton:[1] "I notice that Thomas Simpson treats fluxions as *finite* . . . Thomas Simpson's conceptions appear to have been very clear and distinct, and I do not venture to *say* that the geometrical investigation which he gives of the *fluxion* of a rectangle, avowedly supplied to him by a young but unnamed friend, is *insufficient* in itself, but it fails to convince me, perhaps because I was not early accustomed to fluxions. Certainly there is no *neglecting* of ab, or $\dot{x}\dot{y}$, as *small*; for in fact that *rectangle of the fluxions* is not represented at all in his Figure . . . He conceives the *varying rectangle* xy to be the *sum of two mixtilinear triangles*, of which the *two separate fluxions* are $y\dot{x}$ and $x\dot{y}$. This is very ingenious, but I do not feel sure to what degree I could *rely* on it and build upon it any superstructure, if I were now coming, for the first time, as a *learner*, to the subject. However, I suppose that a pupil, if reasonably modest or even prudent, will take, *for a while*, his teacher's statements upon trust; reserving to himself to *return* upon them, and to examine closely their truth and logic when he shall have acquired some degree of familiarity with the subject taught."

[1] *Life of Sir William Rowan Hamilton*, by R. P. Graves, vol. iii, p. 571.

CHAPTER VI

MACLAURIN'S TREATISE OF FLUXIONS, 1742

164. Colin Maclaurin was educated at the University of Glasgow, and through the influence of Newton was elected professor at the University of Edinburgh. Maclaurin's book on fluxions has been considered the ablest and most rigorous text of the eighteenth century. It was pronounced by Lagrange "le chef d'œuvre de géométrie qu'on peut comparer à tout ce qu' Archimède nous a laissé de plus beau et de plus ingénieux."[1]

In the preface to his *Treatise of Fluxions*[2] Maclaurin says: "A Letter published in the Year 1734, under the Title of the *Analyst*, first gave Occasion to the ensuing Treatise. . . . In the mean Time the Defence of the Method of Fluxions, and of the great Inventor, was not neglected. Besides an Answer to the *Analyst* that appeared very early under the Name of Philalethes Cantabrigiensis . . . a second by the same Hand in Defence of the first, a Discourse by Mr. Robins, a

[1] *Mém. de l'Acad. de Berlin*, 1773; quoted in the art. "Maclaurin" in Sidney Lee's *Dict. of National Biography*.

[2] *A Treatise of Fluxions in Two Books.* By Colin MacLaurin, A.M., Professor of Mathematics in the University of Edinburgh, and Fellow of the Royal Society. Edinburgh, MDCCXLII.

Treatise of Sir Isaac Newton's with a Commentary by Mr. Colson, and several other Pieces were published on this Subject. After I saw that so much had been written upon it to so good Purpose; I was the rather induced to delay the Publication of this Treatise, till I could finish my Design. . . . The greatest Part of the first Book was printed in 1737; But it could not have been so useful to the Reader without the second. . . . In explaining the Notion of a Fluxion, I have followed Sir Isaac Newton in the first Book . . . ; nor do I think that I have departed from his Sense in the second Book; and in both I have endeavoured to avoid several Expressions, which, though convenient, might be liable to Exceptions, and, perhaps, occasion Disputes. I have always represented Fluxions of all Orders by finite Quantities, the Supposition of an infinitely little Magnitude being too bold a Postulatum for such a Science as Geometry. But, because the Method of Infinitesimals is much in use, and is valued for its Conciseness, I thought it was requisite to account explicitly for the Truth, and perfect Accuracy, of the Conclusions that are derived from it . . ."

165. In the Introduction to his *Fluxions* Maclaurin says: ". . . When the certainty of any part of geometry is brought into question, the most effectual way to set the truth in a full light, and to prevent disputes, is to deduce it from axioms or first principles of unexceptionable evidence, by demonstrations of the strictest kind, after the manner of the

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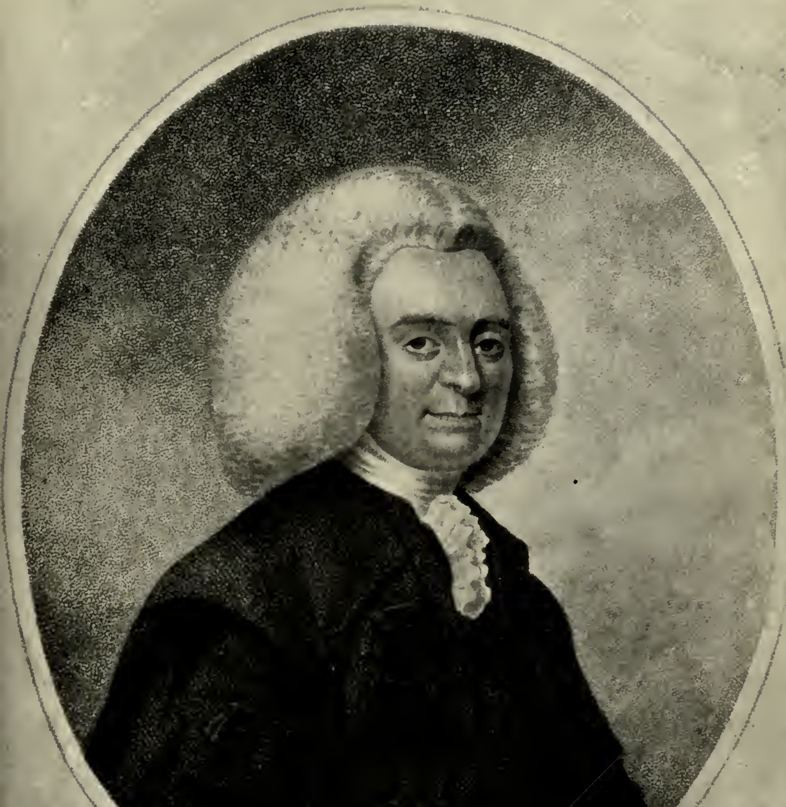

COLIN MACLAURIN, A. M.

antient geometricians. This is our design in the following treatise; wherein we do not propose to alter Sir Isaac Newton's notion of a fluxion, but to explain and demonstrate his method, by deducing it at length from a few self-evident truths, in that strict manner: and, in treating of it, to abstract from all principles and postulates that may require the imagining any other quantities but such as may be easily conceived to have a real existence. We shall not consider any part of space or time as indivisible, or infinitely little; but we shall consider a point as a term or limit of a line, and a moment as a term or limit of time . . . [p. 41]. If we are able to join infinity to any supposed idea of a determinate quantity, and to reason concerning magnitude actually infinite, it is not surely with that perspicuity that is required in geometry. In the same manner, no magnitude can be conceived so small, but a less than it may be supposed; but we are not therefore able to conceive a quantity infinitely small . . ."

166. In the posthumous work, *An Account of Sir Isaac Newton's Philosophical Discoveries*, by Colin Maclaurin, 2nd ed., London, 1750, there is printed a life of Maclaurin, from which we glean the following (pp. viii, ix, and xviii) relating to Berkeley's attack in the *Analyst*:

"Mr. Maclaurin found it necessary to vindicate his favourite study, and repel an accusation in which he was most unjustly included. He began an answer to the bishop's book; but as he proceeded, so many discoveries, so many new theories and problems occurred to him, that, instead of a vindicatory pamphlet, his work came out a complete system of fluxions, with their application to the most considerable problems in geometry and natural philosophy. This work was published at Edinburgh in 1742. . . . His demonstrations had been, several years before, communicated to Dr. Berkeley, and Mr. Maclaurin had treated him with the greatest personal respect and civility: notwithstanding which, in his pamphlet on tar-water,[1] he renews the charge, as if nothing had been done; for this excellent reason, that different persons had conceived and expressed the same thing in different ways. . . . Mr. Maclaurin found it necessary, in demonstrating the principles of fluxions, to reject altogether those exceptionable terms [*infinite* and *infinitesimal*], and to suppose no other than finite determinable quantities, such as Euclid treats of in his geometry."

167. In Chapter I, p. 57, Maclaurin defines a fluxion: "The velocity with which a quantity flows, at any term of the time while it is supposed to be generated, is called its *Fluxion* which is therefore always measured by the increment or decrement that would be generated in a given time by this motion, if it was continued uniformly from that term without any acceleration or retardation: or it may be measured by the quantity that is generated in a given time by an uniform motion which is equal to the generating motion at that term."

[1] In the second edition Berkeley gave the article the name of *Siris*.

The term velocity had been under dispute, particularly in the controversy between Berkeley and Walton. Maclaurin evidently perceived the difficulty in arguing that variable velocity is a physical fact; he says (p. 55), "the velocity of a variable motion at any given term of time is not to be measured by the space that is actually described after that term in a given time, but by the space that would have been described if the motion had continued uniformly from that term. If the action of a variable power, or the velocity of a variable motion, may not be measured in this manner, they must not be susceptible of any mensuration at all"—an argument not likely to convince or silence hostile critics. He quotes Barrow's definition of velocity—"the power by which a certain space may be described in a certain time." In discussing "power" Maclaurin brings in the consideration of "cause" and "effect" in a way that sounds odd in a work laying the foundations to the abstract doctrine of fluxions. Maclaurin uses the word "limit," without giving it a formal definition. Theorem XII reads: "The velocity of a motion that is accelerated or retarded perpetually, is, at any term of the time, to the velocity of an uniform motion, in a ratio that is always a limit between the ratio of the spaces described by these motions in any equal times before that term, and the ratio of the spaces described by them in any equal times after it."

168. In the *Philosophical Transactions*, vol. xlii, for the years 1742–43, London, 1744, Maclaurin

gives an account of his *Treatise of Fluxions*. On p. 330 of these *Transactions* it is pointed out that "the Theory of Motion is rendered applicable to this Doctrine with the greatest Evidence, without supposing Quantities infinitely little or having recourse to prime or ultimate Ratios." Again (p. 336): "There is, however, no Necessity for considering Magnitude as made up of an infinite Number of small Parts; it is sufficient, that no Quantity can be supposed to be so small, but it may be conceived to be diminished further; and it is obvious, that we are not to estimate the Number of Parts that may be conceived in a given Magnitude, by those which in particular determinate Circumstances may be actually perceived in it by Sense; since a greater Number of Parts become visible in it by varying the Circumstances in which it is perceived." Of importance is the following (p. 336): "We shall therefore observe only, that after giving some plain and obvious Instances, wherein a Quantity is always increasing, and yet never amounts to a certain finite Magnitude (as, while the Tangent increases the Arc increases but never amounts to a Quadrant)." That a variable need not reach its limit is also emphasised in other passages, as for instance (pp. 337, 338): "In like manner a curvilineal Area . . . may increase, while the base is produced, and approach continually to a certain finite Space, but never amount to it. . . . A Spiral may in like manner approach to a Point continually, and yet in any Number of Revolutions never arrive at it. . . . The Author insists on these Subjects, the rather that they are commonly described in very mysterious Terms, and have the most fertile of Paradoxes of any Parts of the higher Geometry."

The ideal of mathematical rigour, as entertained by eighteenth-century writers, was reached by the Greek geometricians, Euclid and Archimedes. To derive the rules of fluxions by the rigorous methods of the ancients was the ambition of Maclaurin. Barring some obvious slips that are easily remedied, Maclaurin certainly reached the ideal he had set. Nor is this so very strange. Fluxions involve questions concerning limits; the ancients overcame the difficulties of such questions by their *method of exhaustion*. It was a rigorous method, but dreadfully tedious. Maclaurin secured his aim at a tremendous sacrifice. His work on fluxions consists of 763 good-sized pages; the first 590 pages do not contain the *notation* of fluxions at all; they deal with the derivation of the fluxions of different geometric figures, of logarithms, of trigonometric functions, also with the discussions of maxima and minima, asymptotes, curvature, and mechanics, in a manner that the ancients might have followed, and with the verbosity of which the ancients are guilty. The consequence was that the work was not attractive reading.

Maclaurin was fully aware of the value of a good notation and ease of operation, for he says of the doctrine (p. 575): "The improvements that have been made by it, either in geometry or in philosophy, are in a great measure owing to the facility, conciseness and great extent of the method of computation, or algebraic part. It is for the sake of these advantages that so many symbols are employed in algebra." But to Maclaurin it seemed "worth while to demonstrate the chief propositions of this method in as clear and compleat a manner as possible, if by this means we can preserve this science from disputes" (p. 102). We shall see that Maclaurin's book did not stop disputes. Had the book been read more, it might have been more effective in this respect. Our studies have led us to the conclusion that Maclaurin was not widely read. A second edition of his *Fluxions* did not appear until 1801. His work was praised highly, but seldom used and digested. We might say of Maclaurin what has been said of the German poet Klopstock :—

> "Wer wird nicht einen Klopstock loben?
> Doch wird ihn jeder lesen?—Nein.
> Wir wollen weniger erhoben,
> Und fleissiger gelesen seyn."

Remarks [1]

169. To what extent, if any, Maclaurin may have been influenced by Robins in the mode of treating

[1] In 1745 there appeared an anonymous publication on fluxions which we have not had the opportunity to examine; it was entitled, *The Harmony of the Ancient and Modern Geometry asserted.* In A. C. Fraser's edition of Berkeley's *Works*, vol. iii, Oxford, 1871, p. 301, it is referred to as follows: "This last and forgotten tract consists of papers given in to the Royal Society in 1742, and treats fluxions as a particular branch of an alleged more general reasoning, called the doctrine of maximinority and minimajority."

fluxions it is difficult to say. Later we shall see what James Wilson states on this point. Certain it is that Maclaurin's views agree much more closely with those of Robins than those of Jurin. Maclaurin stood for the narrower view of limits—limits none of which are reached by the variable. However, the conception of limits does not receive as great a degree of emphasis with Maclaurin as it does in the *Discourse* of Robins.

Of Maclaurin's *Fluxions*, Professor Kelland has remarked: "The Analyst did good service to science, if in no other way, at least by giving occasion to this last work. The principles of the method had been previously exhibited in a concise and obscure manner; Maclaurin developed them after the manner of ancient geometers."

In 1749, Maclaurin's *Treatise of Fluxions* was translated into French by Esprit Pezenas, director of the observatory at Avignon.

As we look back, we see that the eight years immediately following Berkeley's *Analyst* were eight great years, during which Jurin, and especially Robins and Maclaurin, made wonderful progress in the banishment of infinitely small quantities and the development of the concept of a limit. Both before and after that eight-year period there were published books in Great Britain containing a mixture of Continental and British conceptions of the new calculus, a superposition of British symbols and phraseology upon the older Continental concepts.

CHAPTER VII

TEXT-BOOKS OF THE MIDDLE OF THE CENTURY

John Stewart, 1745

170. John Stewart, professor of mathematics at Marischal College in Aberdeen, is known as the translator into English, with commentaries, of Newton's *Quadrature of Curves* and *Analysis by Equations of an Infinite Number of Terms.*[1]

The translator spares no pains in the endeavour to remove any obscurities which the ordinary reader might encounter therein. Newton's Quadrature of Curves takes up 33 pages in John Stewart's volume; Stewart's explanations thereof fill 287 pages. Referring to the controversy between Berkeley and Jurin, Stewart says that "because the Doctrine of prime and ultimate Ratios has been so much controverted of late, I shall here enquire whether we have any distinct Idea thereof." He quotes Newton's *Lemma* 1 in Book I, Section 1 of the *Principia*, also the proof of it, and then argues that the limit *is reached*, for "a Difference less than

[1] *Sir Isaac Newton's Two Treatises of the Quadrature of Curves, and Analysis by Equations of an Infinite Number of Terms, explained.* By John Stewart. London, 1745.

any Thing assignable, is the same Thing as no Difference at all: for repeat it as often as you please, it can never be equal to any finite Quantity: and therefore can bear no Ratio to it, by Def. 4, Bk. 5 [of Euclid's] Elements" (p. 37). Stewart gives definitions of ultimate ratio of quantities and of evanescent quantities, also definitions of prime ratio of quantities and of nascent quantities. The following is a specimen: "The ultimate Ratio of evanescent Quantities is the Limit to which the Ratio of variable Quantities diminishing without Bound, continually approaches, to come nearer to it than by any given Difference; but which never goes beyond; yet no sooner attains to, than the Quantities being diminished infinitely, vanish." The following additional statement follows closely the language of Newton (p. 39): "If any one should object that there can be no ultimate Ratio of continually diminishing and at last evanescent Quantities: because before they vanish it is not the last; and after they vanish, they have no Ratio. The Answer is, that the ultimate Ratio is neither the Ratio of them before they vanish; nor after they vanish; but the Ratio wherewith they vanish, or the Limit to which their varying Ratio no sooner arrives, than they vanish; . . . that Ratio they have that very Instant they vanish. . . . It signifies nothing to say ultimate Quantities cannot be assigned, in regard Quantity is divisible without End: for it is not the Quantities themselves that are hereby determined, but only their Ratio:

which is capable of being determined." This matter, says Stewart, has been so clearly explained by Newton, "that the great Dust which has been raised of late about the Whole of this Doctrine, must be owing to Weakness, or some worse Principle" (p. 40).

William Emerson, 1743 (?), 1757, 1768

171. William Emerson was a self-taught mathematician ; he wrote many mathematical texts which indicate a good grasp of existing knowledge, but not great originality. His *Doctrine of Fluxions* appeared at London in 1743 (?). We have before us the third edition, 1768. From it we quote as follows :

"The Velocity of the Increase of any generated Quantity, or the Degree of Quickness (or Slowness) wherewith the new Parts of it continually arise, is called its *Fluxion.*"

"The indefinitely small Portions of the Fluent which are generated in any indefinitely small Portions of Time are called *Moments* or *Increments.*"

". . . The Moments and Fluxions ought not to be confounded together, since the Moments (being generated by Fluxions) are as different from the Fluxions, as any Effect is different from its Cause."

The following is given as an axiom :

"Quantities, which in any finite Time continually converge to Equality, and *before* the End of that Time, approach nearer to one another than by any given Difference, do at last become equal."

"If any should think this not clear enough to pass for an Axiom, he may consider it thus; let D be their ultimate Difference, therefore they cannot approach nearer to equality, than by that given Difference D, contrary to the Hypothesis; which Supposition is absurd in all Cases except when D is nothing."

To find the fluxion of $bx^m y^n$, he lets $o\dot{x}$, $o\dot{y}$ be moments, expands the powers of $x+o\dot{x}$ and $y+o\dot{y}$, and finds the increments. Then he divides "by the indefinite Quantity *o*." "But since the (Velocity or) Fluxion is required wherewith that Moment first arises, in this Case the Moments $o\dot{x}$ and $o\dot{y}$ will also be just arising and therefore nothing, and consequently *o* will be nothing, and therefore all the Terms wherein it is found will be nothing." The final result then follows. In his Preface Emerson claims that "Velocity must be looked upon as the proper efficient Cause of the Space described; and the Space described the adequate Effect of that Cause." . . . "No increment can be taken so small, but it is still further divisible ad infinitum; and since the Velocity is by Supposition continually variable, it is plain, there can be no two Points of the Increment in both of which the Velocity is accurately the same. It is therefore most manifest, that the Velocity here enquired after is peculiar to one only indivisible Point; . . . that the Velocity in any given Point of the Line described . . . has a certain, fixed, determinate Value. . . . Here a metaphysical Disputant may demand, how it comes to pass, that any Velocity which continues for no Time at all, can possibly describe any Space at all; or whether its Effect be absolutely nothing, or an infinitely small Quantity, or what it is. Here then it is, that our Reason is at a Stand, and the human Faculties are quite confounded, lost, and bewildered. . . . Now whether such subtile Questions will be ever determined, or not, yet there is one Refuge for us, viz. that it is nothing at all to our Purpose what they are: . . . The Method of Fluxions has no Dependence on these mysterious Disquisitions. What I apprehend the Method of Fluxions to be concerned in, is . . . what a . . . variable Velocity can produce in the whole. And here I think no Reason can be assigned, why a variable Cause should not produce a variable Effect, . . . though we have no Ideas at all of the perpetually arising Increments, or their Magnitude in their nascent or evanescent State, that have so much, and to so little Purpose, confounded and puzzled the mathematical World."

Thomas Simpson, 1750

172. Simpson's *Treatise of Fluxions* of 1737 has already been noticed (our § 156). His text of 1750, *The Doctrine and Application of Fluxions*, London, is new, not only in the title, but to some extent also in the mode of exposition. He says in his Preface (1750) that he has used a tract entitled *An Explanation of Fluxions in a Short Essay on the Theory*, printed by W. Innys and written by one of

his friends who was too modest to put his name to it. (See our §§ 158, 160, 163.) Simpson used his friend's manner of determining the fluxion of a rectangle and of illustrating fluxions of higher order. Simpson defines a fluxion as follows:

"The Magnitude by which any Flowing Quantity would be uniformly increased, in a given Portion of Time, with the generating Celerity at any proposed Position, or Instant (was it from thence to continue invariable), is the Fluxion of the said Quantity at that Position, or Instant."

The derivation of the fluxion of xy is explained after the manner adopted by John Rowe, both authors being indebted for it to the author of *An Explanation of Fluxions in a Short Essay on the Theory.* The same definitions and explanations of the fundamentals are given by Thomas Simpson in the last part of his *Select Exercises for Young Proficients in the Mathematicks*, 1752. In the preface to his *Fluxions* of 1750, Simpson touches some points of philosophic interest. He says:

"By taking Fluxions as *meer Velocities*, the Imagination is confin'd, as it were, to a Point, and without proper Care insensibly involv'd in metaphysical Difficulties: But according to our Method of conceiving and explaining the Matter, less Caution in the Learner is necessary, and the higher Orders of Fluxions are render'd much more easy and intelligible—Besides, tho' Sir Isaac Newton defines Fluxions to be *the Velocities of Motions*, yet he hath Recourse to the Increments, or

Moments, generated in equal Particles of Time, in order to determine those Velocities ; which he afterwards teaches us to expound by finite Magnitudes of other Kinds : Without which (as is already hinted above) we could have but very obscure Ideas of higher Orders of Fluxions : For if Motion in (or at) a Point be so difficult to conceive, that *Some* have, even, gone so far as to dispute the very Existence of Motion, how much more perplexing must it be to form a Conception, not only, of the Velocity of a Motion, but also infinite Changes and Affections of It, in one and the same Point, where all the Orders of Fluxions are to be considered.

"Seeing the Notion of a Fluxion, according to our Manner of defining It, supposes an Uniform Motion, it may, perhaps, seem a Matter of Difficulty, at first View, how the Fluxions of Quantities, generated by Means of accelerated and retarded Motions, can be rightly assigned ; since not any, the least, Time can be taken during which the generating Celerity continues the same : Here, indeed, we cannot express the Fluxion by any Increment or Space, *actually* generated in a given Time (as in uniform Motion). But, then, we can easily determine, what the contemporary Increment, or generated Space *would be*, if the Acceleration, or Retardation, was to cease at the proposed Position in which the Fluxion is to be found : Whence the true Fluxion, itself, will be obtained, without the Assistance of infinitely small Quantities, or any metaphysical Considerations."

Nicholas Saunderson, 1756

173. At the age of twelve months Saunderson lost his eyesight by small-pox; nevertheless, he rose to prominence. He studied at Christ's College, Cambridge, and in 1711 succeeded Whiston as Lucasian professor of mathematics at Cambridge. His *Fluxions* [1] is a posthumous work.

We read (p. 1): "Let AB represent any Moment of Time, whether finite or infinitely small it matters not, terminated by the two Instants A and B. Let x be the Value of any flowing or growing Quantity at any Instant A, whose Velocity at that Instant is such, that if it was to flow during the whole Moment AB with this Velocity, it would gain a certain Increment represented by $\dot{x}$; then is this Quantity $\dot{x}$ called the Fluxion of x at the Instant A, when the Value of the flowing Quantity was x." In the scholium which follows, it is explained that if the velocity is variable, then the increment of the velocity "gained in the time AB will not be the same with its Fluxion above defined, . . . but if the Time AB be infinitely small, then though the Velocity of x at the Instant B be not the same, mathematically speaking, with the Velocity at the Instant A; yet the Difference being infinitely small in Respect of the whole Velocity, it may safely be neglected, where the finite Ratios of Fluxions are only considered; and so this Increment

[1] *The Method of Fluxions Applied to a select Number of Useful Problems. . . .* By Nicholas Saunderson, Late Professor of Mathematics in the University of Cambridge. London, MDCCLVI.

and the Fluxion above defined may be taken for one another, i.e. the Quantity x for so small a Time, may be looked upon as flowing uniformly" (p. 2). Later we read (p. 4) that if the times are infinitely small, the quantity $\dot{v}\dot{x}$ will be "infinitely less" than $v\dot{x}$ or $x\dot{v}$. Here the fluxions $\dot{x}$, $v\dot{x}$, are looked upon as infinitely small.

In the account of the life of Nicholas Saunderson, printed in the first volume of his *Elements of Algebra*, Cambridge, 1740, p. xv, we read: "Our Professor would not be induced by the Desires and Expectations of any, to engage in the war that was lately waged among Mathematicians, with no small Degree of Heat, concerning the *Algorithm or Principles of Fluxions*. Yet he wanted not the greatest Respect for the Memory of Sir Isaac Newton, and thought the whole Doctrine entirely defensible by the strictest Rules of geometry. He owned indeed that the great Inventor, never expecting to have it canvassed with so much trifling Subtility and Cavil, had not thought it necessary to be guarded every where by Expressions so cautious as he might have otherwise used."

John Rowning, 1756

174. A graduate of Magdalene College, Cambridge, and a Fellow there, Rowning interested himself chiefly in natural philosophy, but wrote also *A Preliminary Discourse*[1] on fluxions, with the intention of writing

[1] *A Preliminary Discourse to an intended Treatise on the Fluxionary Method.* By John Rowning, M.A. London, 1756.

a full treatise. But the treatise in question never appeared. After a popular exposition of the ideas of fluxion and fluent, and of Leibniz's infinitely little quantities and their summation, showing how these methods yield important results in natural philosophy, he refers to Berkeley's attacks and the defence made by Philalethes Cantabrigiensis, Walton, and Robins, also Maclaurin, who "declined entering the Combat," but endeavoured to treat the subject "in a Manner less exceptionable." "But no Body, that I know of," continues Rowning, "has explained it in so easy and familiar a Way as I apprehend the Subject capable of." Moreover, Jurin and Walton "carry things . . . no farther than Sir Isaac had done before. They leave them, as to the Objections made by the *Analyst*, exactly as they found them." The difficulties do not lie in the idea of a first fluxion—a velocity. "In this there is Nothing either infinitely great or infinitely little: Nothing obscure." As to higher fluxions, "these Things indeed elude our Senses; but they do not surpass the Understanding" (p. 85). Berkeley's objection to "infinitely small Quantities" is not fatal, "because finite Measures might have been made use of." His other objection, that "such Quantities are in some Cases retained and made use of for a while, and afterwards, to use his own Expression, like Scaffolds to a Building, are rejected as of no Significancy," may be met by the proof that those quantities "are always such as ought by no means to be retained." In further explanation of his position Rowning says (p. 88), "that the Velocity of any Body is the same at any one Point, or at any one Time, whether the Body moves with an uniform, accelerated, or with a retarded Motion at that Point or Time." This is elucidated by reference to geometric figures, and amounts, in the main, to the explanation given by Rowe in finding the fluxion of xy. One objection to such explanations, which had been raised by Berkeley, was that one could not speak of the velocity a body had *at a point* of space. That such a phraseology is admissible is tacitly assumed by Rowning. What the latter emphasises is that no use is made of the concept of the "infinitely little." As to Berkeley's second objection, that the supposition which is made at the beginning of the process is later displaced by its contrary, as when the symbol o is at first made an actual increment and later in the same process taken as no increment, Rowning argues that terms involving factors oo, ooo, etc., "do arise in consequence of the Acceleration wherewith the Power of x flows, when x itself flows uniformly; and consequently that they arise from the second and higher Fluxions of that Power; and that, therefore, when the first Fluxion of that power is only inquired after . . . they are to be left out and rejected, as appertaining to another Account." It can hardly be claimed that Rowning made a contribution to the theory of fluxions. However, he has a pleasant way of expressing himself. His book was favourably reviewed in the *Monthly Review* (vol. xiv, p. 286).

Israel Lyons, 1758

175. Lyons was a mathematician and botanist. His *Treatise of Fluxions*, London, 1758, is dedicated to Robert Smith, Master of Trinity College, Cambridge, "being the first Essay of a young and unpractised Writer" which "owed its first rude Beginning to the early Encouragement" received from the Master, as the author modestly states. His treatment is geometric. He says: "I reject no Quantities as infinitely smaller than the rest, nor suppose different Orders of Infinitesimals and infinitely great Quantities. But consider the Ratio of the Fluxions as the same as that of the contemporaneous Increments, and take Part of the Increment before and Part after the Fluent is arrived at the Term, where we want the Fluxion, since it is not the Increment after, or the Increment before that we want, but at the very instant, which can no otherwise be found but by considering Part of the Increment before and Part after" (Preface). Fluxions are defined as velocities. "The moments of quantities are the indefinitely small parts, by the addition or subtraction of which, in equal particles of time, they are continually increased or diminished." The author proves the proposition: "The indefinitely small spaces described in equal indefinitely small times are as the velocities," since, "when the time is diminished *ad infinitum*, the difference of the velocities at the beginning and ending of that time will vanish." If two flowing quantities

x and y are to each other in a given ratio, then in $xy=z$ it is argued that $2y=$ incr. of $z\div$ incr. of $x=\dot z\div\dot x$; hence $\dot z=2y\dot x$. When $y=x$, this becomes $\dot z=2x\dot x$; one has also, fluxion $\overline{x+y}^2=2\,\overline{x+v}\times\overline{\dot x+\dot y}$ $=2\times\overline{x\dot x+y\dot x+x\dot y+y\dot y}$.

From this is derived the fluxion of any rectangle xy, thus: The fluxion of $\overline{xy}^2$, or $x^2+2xy+y^2$, is also equal to $2x\dot x+2y\dot y+$ fluxion of $2xy$. Hence fluxion of $2xy=2\dot xy+2x\dot y$.

"In the same manner as the quantities x, y, z, are conceived to flow, and to have their fluxions, so may the quantities $\dot x$, $\dot y$, $\dot z$, be supposed to be variable, and therefore have their fluxions, which are thus represented $\ddot x$, $\ddot y$, $\ddot z$, and are called the second fluxions of x, y, z" (p. 11). "The fluent of any quantity as $x^m\dot x$ is represented thus $|\overline{x^m\dot x}|$."

William West, 1762

176. William West's *Mathematics*[1] is a posthumous work; the author died in 1760. Fluxions are treated from the earlier Newtonian standpoint, infinitely little quantities being used. Some novelty is claimed for this text in the treatment of maxima and minima.

James Wilson, 1761

177. In 1761 Wilson collected some of Benjamin Robins's mathematical tracts in a two-volume book,

[1] *Mathematics*. By the late Rev. Mr. Wm. West of Exeter. Revised by John Rowe, London, 1762. There appeared a second, corrected, edition in 1763.

entitled *Mathematical Tracts of the late Benjamin Robins.* In an Appendix, Wilson inserts some matters of historical interest regarding certain manuscripts of Newton; Wilson also defends Robins against criticisms passed by a French writer, and states his views of Maclaurin's indebtedness to Robins.

Newton's *Method of Fluxions* (see our § 149) was brought out in Paris in 1740 by George Louis Le Clerk, Comte de Buffon, under the title, *La méthode des Fluxions, et des suites infinies.* Buffon prepared a historical Preface, in which he criticised severely Berkeley and Robins for presuming to take exception to anything Newton had written on fluxions or to modify Newton's mode of exposition. Buffon praises Jurin, and then speaks of Robins thus (pp. xxvii–xxix):

". . . il commence par le censurer & par désaprouver sa maniere trop bréve de présenter les choses; ensuite il donne des explications de sa façon, & ne craint pas de substituer ses notions incomplettes aux Démonstrations de ce grand homme. Il avouë que la Géometrie de l'Infini est une science certaine, fondée sur des principes d'une vérité sûre, mais enveloppée, & qui *selon lui n'a jamais été bien connuë*; Newton n'a pas bien lû les Anciens Géometres, son Lemme de la Méthode des Fluxions est obscur & mal exprimé . . . : malheursement les Mathematiciens ont été plus incrédules que jamais, il n'y a pas eu moyen de leur faire croire un seul mot de tout cela, de sorte que

Philalethes comme défenseur de la vérité, s'est chargé de lui signifier qu'on n'en croyoit rien, qu'on entendoit fort bien Newton sans Robin, que les pensées & les expressions de ce grand Philosophes sont justes & très-claires . . ., ce sont des piéces d'une mauvaise critique. . . ."

Buffon presents no argument against the views expressed by Robins, but abuses him for presuming to think independently. This doting attitude toward Newton is justly attacked by James Wilson, in his Appendix to the *Mathematical Tracts of the late Benjamin Robins*, vol. ii, London, 1761, pp. 325–327. Wilson rightly says that if it was a crime for Robins to make mention of the great brevity with which Sir Isaac Newton wrote, Robins was followed in it by Maclaurin and Saunderson. "The truth is," says Wilson, "Sir Isaac Newton at first made the same use of indivisibles, others had done: in his *Analysis per æquationes numero terminorum infinitas*, he expressly says, 'Nec vereor loqui de unitate in punctis, sive lineis infinite parvis[1];' and in his *Lectiones Opticæ* he demonstrated by indivisibles." Wilson contends furthermore that Buffon is wrong in claiming that the mathematicians paid no regard to what Robins had said, that in fact "the best writers soon after trod in Mr. Robins's steps." In fairness to Buffon it should be said, however, that he printed his Preface in 1740, and that Maclaurin, Saunderson, de Bougainville, and d'Alembert, whom

[1] *Comm. Epist*, p. 85.

Wilson mentions as following Robins, wrote at a later date.

178. James Wilson claims[1] that Maclaurin in his *Fluxions* "conformed himself entirely to Mr. Robins's sentiments in regard to Sir Isaac Newton's doctrine," and "has even expressly followed his plan in treating the subject." Jurin had contended (says Wilson) "that Sir Isaac Newton's method, by proving the varying quantities came up to their limits, was more perfect than that of the ancients. Whereas Sir Isaac Newton never claimed such superiority; . . . The coincidence contended for, and thus highly praised by Philalethes, is the very essence of indivisibles." Wilson rightly insists that Buffon's criticisms of Robins are unfair. "When he talks of the obscurity of Mr. Robins's ideas, the insignificancy of his phrases, and the unintelligibleness of his style; he gives the most certain proof, that he had never carefully read his writings, . . . for Mr. Robins is much admired here for the contrary excellencies, on whatever subjects he has employed his pen."

179. Wilson represents Philalethes (Jurin) as championing the use of the infinitely little and of indivisibles. This is putting the case too strongly. In his papers against Berkeley, Jurin uses quantities infinitely little. But toward the end of his debate with Robins he begins to disavow them. Never did Jurin use indivisibles. Few eighteenth-century

[1] *Mathematical Tracts of the late Benjamin Robins*, vol. ii, London, 1761, pp. 312, 315, 320.

writers have brought out as distinctly and clearly as has Jurin the difference between infinitesimals as variables, and indivisibles; Jurin disavowed all quantity "fixed, determinate, invariable, indivisible, less than any finite quantity whatsoever," but he usually did admit somewhat hazily a quantity "variable, divisible, that, by a constant diminution, is conceived to become less than any finite quantity whatever, and at last to vanish into nothing."

Remarks

180. None of the works mentioned in this chapter are great works. Those of William Emerson and Thomas Simpson were the best and the ones most widely used. The first edition of Simpson is of earlier date (1737).

CHAPTER VIII

ROBERT HEATH AND FRIENDS OF EMERSON IN CONTROVERSY WITH JOHN TURNER AND FRIENDS OF SIMPSON

181. The principals, Simpson and Emerson, do not themselves appear in this controversy. During the period of this debate, Robert Heath was editor of *The Ladies' Diary*, which appeared once every year as an almanac. We begin with one of his articles.

Robert Heath, 1746

182. In an article, *Of the Idea, and Nature of Fluxions*,[1] Heath says:

"The Distinction betwixt the *Increments* and Fluxions of Magnitudes, has been this; that the *former* approach in Ratio infinitely near the latter, so that their Difference is unassignable. . . . What we call the *Fluxions*, or *Velocities* of Magnitudes, are only the Fluxions in Chief, or in Part, with which they are born; the Part neglected in the Ratio exactly corresponding with what is rejected in the finite Ratio of the infinitely small Increments, which is therefore the same as the Ratio of our

[1] *The Ladies' Diary: or, the Woman's Almanack, for* 1746.

Fluxions. And hence, whether we call those finite Ratios, Fluxions, or Increments, their Idea, Nature, and Original appear to be the very same thing. For all Things are relative. . . ."

He argues that while we consider a line or plane, generating an area or solid, as of no thickness in the mind, in our notation we represent them as of unit thickness, "and consequently each Line or Plane should be express'd by $o \times \mathrm{L}$, and $o \times \mathrm{P}$, to denote them as they are in the Mind. But $\mathrm{L} \times o$ to o, and $\mathrm{P} \times o$ to o, are in the same Ratio with L to 1, and P to 1, by equal Division by o; and those again in the same *Ratio* with $\mathrm{L}\dot{x}$ to $\dot{x}$, and $\mathrm{P}\dot{x}$ to $\dot{x}$, by equal Multiplication by $\dot{x}$, for the Ratio of Fluxions. But, this finite Notation of *Line* or *Plane*, which we consider of no *Breadth*, or *Thickness*, and yet denote by Unity, each, at the same Time, makes the *Practice* and our *Comprehension* disagree. . . . So that it will be an Error to conclude that the Ratio of the Fluxions of Quantities generated by the Motion of *Lines*, or *Planes*, is arrived at this Way, without the previous Consideration of an Increment; for the very *Lines* and *Planes* must be *Increments*, or *Somethings* next to *Nothings* themselves, before they were what we finitely express them by Notation, or Quantities could never increase or be generated thereby: For to carry a *Line* or *Plane* of no Breadth or Thickness forward, is the same in Terms as to carry Nothing forward. And therefore the Distinction between the Ratio of Increments, and that of Fluxions, is only what the Conception of the

Thing differs from that of its Notation in Practice. . . . Those who desire further Satisfaction as to the Nature of Fluxions, of their noble Use and transcendant Excellence, may consult Mr. Emerson's *Doctrine* of the *whole Art*, which is . . . the best of any. . . . Those writers will find themselves mistaken, who pretended to derive the finite Ratios of Motion, or Fluxions producing Magnitudes, without the previous Consideration of Increments, which include the very Notion of what a Fluxion is. This *some* have attempted by multiplying Quantities into their Velocity, and some by other Means, the Result of which originally depends on *incremental Principles*, if they would consider the Matter as far as it will go." The paper is brought to a finish in the *Ladies' Diary* for 1747.

Main Articles in the Controversy

183. Over the pseudonym of "Cantabrigiensis" there appeared in 1750 an unfriendly review of Simpson's *Doctrine and Application of Fluxions.*[1] The reviewer contended that the definition of a fluxion as the "magnitude by which any flowing quantity would be uniformly increased" (see our § 172) is very "odd"; for, "in quantities uniformly generated, the fluxion must be the fluent itself, or else a part of it." Simpson's endeavour to exclude "velocity" "cannot be made intelligible without introducing velocity into it." "Again, he

[1] *Monthly Review; or, New Literary Journal*, vol. iv, London, 1750, pp. 129–131.

mistakes the effect for the cause; for the thing generated must owe its existence to something, and this can only be the velocity of its motion; but it can never be the cause of itself, as his definition would erroneously suggest." Moreover, it is strange that Simpson "should still stick in the mud and run himself into the old exploded method used by foreigners; and which is subject to all the cavils that have ever been raised against that science."

184. This criticism originated a small tempest. In a journal called *Mathematical Exercises*,[1] its editor, John Turner, makes certain "Observations on certain invidious Aspersions on Mr. Simpson's *Doctrine and Application of Fluxions*, published in the *Monthly Review* for December last, by Cantabrigiensis." Mr. Simpson is there charged as having "mistaken the Effect for the Cause"; Mr. Simpson, says Turner, "builds upon his own Definition;

[1] *Mathematical Exercises* No. III (1751), p. 34. Six numbers of this journal appeared in London in 1750–1752. No. V bears the date 1752; No. VI has no date. Readers are invited "to send their Performances (whether new Problems, Paradoxes, Solutions, etc.) Post paid, to be left with Mr. James Morgan, at the Three-Cranes, in Thames-street . . ." In this connection a statement made by Charles Hutton, in his Memoirs of the Life and Writings of the Author [Thomas Simpson], printed in Thomas Simpson's *Select Exercises in the Mathematics*, new edition, London, 1792, p. xviii, is of interest:

"It has also been commonly supposed that he [Thomas Simpson] was the real editor of, or had a principal share in, two other periodical works of a miscellaneous mathematical nature; viz. the *Mathematician*, and Turner's *Mathematical Exercises*, two volumes, in 8vo, which came out in periodical numbers, in the years 1750 and 1751, etc. The latter of these seems especially to have been set on foot to afford a proper place for exposing the errors and absurdities of Mr. Robert Heath, the then conductor of the *Ladies' Diary* and the *Palladium*; and which controversy between them ended in the disgrace of Mr. Heath, and expulsion from his office of editor to the *Ladies' Diary*, and the substitution of Mr. Simpson in his stead, in the year 1753."

which, he tells us, himself, is not exactly the same as that of Sir Isaac Newton." Mr. Simpson is also charged with plagiarism from Cotes's *Estimatio Errorum.* John Turner says:

"Here his Remarks on the Author's Definition of a Fluxion first demand our Consideration: Mr. Simpson makes it to be, 'the Magnitude by which a flowing Quantity *would be* uniformly increased in a given Time.' This Definition the Critic represents as a very old one; and with regard thereto advances the two following, extraordinary, Positions:

"1. That, in Quantities uniformly generated, the Fluxion must (according to the said Definition) be the Fluent itself, or else a Part of it.

"2. And that, in other Quantities generated by a variable Law, the Fluxion will not be a real, but an imaginary Thing.

"To the first of these Objections I answer, that the Fluxion is neither the Fluent itself nor a Part of it: it is a Quantity of the same Kind with the Fluent; but the Fluent being the Quantity already produced by the generating Point, Line or Surface, supposed still in Motion, and the Fluxion what will arise, hereafter, from the Continuation of that Motion; the latter can no more be denominated a Part of the former than the ensuing Hour a Part of the Time past.

"But his second Observation is a still more glaring Instance of his Disingenuity, and Want of Judgment. Does it follow, because a Body, really, moves over a certain Distance, in a given Time, with an accelerated, or a retarded Velocity, that there is no Distance over which it might pass in the same Time, with its first Velocity uniformly continued. The Space over which a Body would uniformly move with such, or such, a proposed Velocity, is no less real because no Part of it is actually described with that Velocity" (pp. 36, 37).

185. Then follows an article reprinted from the *Daily Gazetteer* for December 4, last [1750], in which one who signs himself "Honestus" (said to have been John Turner himself) charges that the compiler of the *Ladies' Diary* (Robert Heath) is also the compiler of the *Palladium*, and the best material designed by contributors for the *Diary* are reserved by him for the *Palladium*; that the latter publication is owned by the compiler, while the former is not. Robert Heath wrote a reply in the *Daily Gazetteer* of December 6; four letters follow on this subject.

186. John Turner's defence of Simpson led to the publication of what Turner called a "scurrilous Pamphlet." This pamphlet is without doubt the *Truth Triumphant: or Fluxions for the Ladies*,[1] London, 1752, or else those parts in that pamphlet which appear over the pseudonyms "$\dot{X}$ Primus"

[1] The fuller title of the pamphlet is thus: *Truth Triumphant: or, Fluxions for the Ladies. Shewing the Cause to be before the Effect, and different from it; That Space is not Speed, nor Magnitude Motion. With a Philosophic Vision, Most humbly dedicated to his Illustrious High and Serene Excellence, the Sun. For the Information of the Public, by $\dot{X}$, $\dot{Y}$, and $\dot{Z}$, who are not of the Family of x, y, z, but near Relations of x', y', and z'. . . .* London. Printed for W. Owen, M.DCC.LII.

and "$\dot{Y}$ Secundus." These documents evidently emanated from the pen of Robert Heath, assisted possibly by some other adherents of William Emerson. At the risk, perhaps, of not observing strict chronological sequence, we proceed to the consideration of all parts of *Truth Triumphant.* In the dedication "to the Sun," it is stated that "the Family of the Wou'd-be's in this Island is become very numerous, by *uniformly* continuing in their Errors." Thus, both the title-page and the dedication play on Simpson's definition of fluxions and its alleged defects. In the Preface one reads: "Fluxions, then, Ladies, that have so puzzled our wise Mathematicians to define, are the respective Degrees of Motion, at any Instant of Time, of any two things or Bodies that continually flow, or move on, over Space." Four pages are devoted to the explanation of fluxions.

187. Then follow the two criticisms of John Turner's defence of Simpson, signed "$\dot{X}$ Primus" and "$\dot{Y}$ Secundus," to which we have alluded above. In the former of these articles John Turner is treated with contempt. "Who this John Turner is, whether he is Mr. Simpson's Clerk, or his Pupil, or some Dependant on him; or whether he be Mr. Simpson himself, is not very material to the Reader . . ." Turner is continually referred to as "John." To Turner's reply to the first criticism on Simpson's text, "$\dot{X}$ Primus" makes rejoinder: "*John* says, the Fluent being the Quantity already produced—Pray how was this Quantity produced,

by some *magic* Art, without any Fluxion? I believe not. . . . For my Part, I know of no Body that ever said, that the Parts of the Fluent that went before were generated by the Fluxion that is to come after, but every Part by its proper Fluxion. . . ." To Turner's reply to the second criticism, "$\dot{X}$ Primus" makes rejoinder:

"If there be no Magnitude by which the flowing Quantity is really increased, such a Magnitude is not real, but an imaginary Thing only . . . But *John* thinks, that every Thing that exists in his *Imagination*, really exists in *Nature* . . . Sir Isaac Newton defines Fluxions by the Velocities of the Motions. But Mr. Simpson declares against this, and likewise tells us, that by taking Fluxions for mere Velocities, the Imagination is confin'd, as it were, to a Point. How *his* Imagination is confin'd I don't know; but Sir Isaac Newton chused to define it thus, as very well knowing, that this is the only *solid* Foundation upon which it could be defended against all the impertinent Cavils of ignorant or weak Pretenders." The parting shot by "$\dot{X}$ Primus" is—your Great Master will not "think you a fit *Champion* to engage in his Cause for the future; so, good Night, John."

188. The reply made by "$\dot{Y}$ Secundus" is to the effect that the defender of Simpson is "equally in the Dark" with Simpson himself, "otherwise he would not have gone about to defend so defenceless a Cause, as to vindicate an Absurdity, by representing a Fluxion *to be of the same Kind with the*

Fluent, uniformly generated; when the one is a Quantity of *actual Velocity*, and the other a Quantity of *Space*, described by that Velocity, which can be only proportional to it."

189. After some poetry "To the Family of the the Wou'd-be's," follow "Animadversions on Mr. Simpson's Fluxions," "By Ż Tertius," who quotes a criticism of Simpson from the pen of J. Landen. Where Landen's review first appeared we do not know. As quoted here, Landen objects to the definition of fluxions "as *faulty*, by the Author's different *Idea* given of them to that by the Inventor"; Landen disapproves of "denoting all Quantities whatsoever by Lines, to bring them to one Denomination, and those Lines, to be described by Bodies in Motion." In criticism of fluxions in general, Landen says that the finding, from the velocities, the spaces passed over, and *vice versa*, "may be managed by common *Algebra*, without the least Obscurity. The Business had always been better considered in that Light, without ever making Use of the Term *Fluxions*, as if a new Kind of *Analysis*, tho', in *Fact*, only the *Doctrine of Motion* improved, and applied to Purposes before unthought of."

190. The next article in *Truth Triumphant* is a reprint of the first criticism of Simpson, contributed in 1750 by Cantabrigiensis to the *Monthly Review*. Eight more articles concerning motion, fluxions, and mechanics bring the pamphlet to a close; they make no reference to Simpson. "Heliocentricus"

explains higher fluxions in a way that cannot be called illuminating.

191. Then "Amicus" speaks "of the Use of the Algebraic Cypher, in finding the Fluxions of Algebraic Quantities," letting x increase or decrease, and become $x \pm o$, where the "Increments or Decrements are seen to be $\pm o$," and "dividing by algebraic o," thereupon "algebraically considering o of insensible Value, as before it was consider'd of real sensible Value." Taking a reminiscent mental attitude, "Amicus" says:

"This *algebraic Ratio* of the Fluxions of Quantities, to which the diminishing Value of the *algebraic* Increments or Decrements, from their limited State or Value, tend together, to their geometrical vanishing (by *supposing the variable Value less and less*) has been misconceived, as vanishing together with the *real* geometrical Increments or Decrements they are the *Value of*; whence o has been denominated a *departed*, instead of an *algebraic* Quantity, by a famous B—p, tho' it's Reality and Presence still existed before his Eyes; but if o, the Cypher-Value, or *algebraic* Quantity, call'd also *Nothing*, be made to signify *Nothing*, because it is so call'd, the Word *Nothing* with as much Propriety, may be called *no Word*, be allowed to have no Signification among other Words, and be deem'd a mere Blank, as *no* Subject capable of Consideration." Further on in the pamphlet, the query whether there can be "*real* Motion in *no* Time," for "any *one* Point of intermediate Space gone over; especially since an infinite

Number of Points can never actually constitute real Magnitude," and whether "Motion, or Fluxion, can *actually* exist, and be known, but by the next Increment of Space gone over, in some real and next Moment of Time"? These are fundamental problems indeed. Zeno is not mentioned in the pamphlet, but the query involves Zeno's subtle paradox of the "arrow." Nor is the answer given devoid of interest. "But Time, and Motion, flowing over Space, . . . (*since no Quantity can be assign'd, or imagin'd so small, but there will still be smaller*) the respective Degree of Velocity of Motion, or Fluxion (i.e. instantaneous velocities) of that Flowing at any Instant of Time, and Point of Space gone over, will be everywhere assignable by the immediate Increments, as Effects of those preceding Velocities, as has been shewn. Whence it will follow, that certain Degrees of Motion, Fluxion, or Velocity, exist at every instant of Time taken, and Point of Space respectively described; contrary to the *differential* Notion that Foreigners have of this Matter." The weak spot here resides in the words "immediate Increments"; do immediate increments exist in view of the statement in the above parenthesis? The lack of a satisfactory arithmetical continuum comes to view more fully in the antagonism between geometric increases and algebraic increases exhibited in the following passage taken from the pamphlet:

"All the Values of the geometrical Increases flow'd over, in finite Time, can never be algebraically express'd in infinite Time; in which Sense the *algebraic Increases* being again diminish'd, are said never to converge to the Limits of their geometrical Magnitudes in Motion, but will still have sensible Value; yet supposing the geometric Increases, and their algebraic Values to flow and decrease alike, to something determinate, then *o*, and *o*, and it's Powers into a Variable Quantity, and it's Powers, will accurately express the Limits of variable Quantities, or Beginnings of their Increases; which *Limits*, or *Beginnings of Increases* of Quantities, are accurately, as the Fluxions of those Quantities in general."

"Visionarius" closes with a philosophic vision in which four candidates for honors appear before the Goddess of Science. Rejected are the first three, viz. the author of *Mathematical Exercises* (John Turner), the one holding to the motto "A cypher is no Algebraic Quantity" (Bishop Berkeley), and a Grand Magnifier of Fluxions (Thomas Simpson); crowned is the author of the incomparable treatises of *Fluxions* and *Trigonometry* (William Emerson), in whose interests *Truth Triumphant* appears to be mainly written.

192. The probability is that the "scurrilous pamphlet" to which John Turner made reply in an issue of his *Mathematical Exercises* was only a part of what is given in *Truth Triumphant.* The latter is probably a later and enlarged publication. In that reply Turner argues that "it must appear to everyone that, what Mr. Simpson defines as Fluxions,

are exactly such finite Quantities, proportional to the Velocities as Sir Isaac Newton here[1] speaks of; since it is well known that the Quantities produced, or the Distances described, in any given Time, by Motions uniformly continued, are, accurately, as the Velocities of the said Motions."

193. In No. V (1752) of the *Mathematical Exercises* appears another article in the controversy, written by John Turner. There is little in it requiring our attention. It is a reply to two pamphlets, the *Lady's Philosopher* and a new *Palladium*, both publications from the pen of Robert Heath.

Ladies' Diary, 1751, 1752

194. The *Ladies' Diary* for 1751 has an article on *The Nature and Use of the Algebraic Cypher, or Quantity* 0, "by Fluxioniensis"; 0 / 0 is proved to signify "any Value at Pleasure by considering $(a^n - x^n) \div (a - x)$ for $n = 1, 2, 3, 4$, etc., when $x = a$." This "confutes the Notion of some Mathematicians" that 0 / 0 expresses "a Ratio of Equality." Next it is argued that $0^0 = 1$. "Hence," says a second anonymous critic, "all Cypher-Paradoxes, and Mysteries of Ultimate Ratios, or Ratios of Least Increments or Decrements of Quantities, vanish and Day appears. . . ."

"Waltoniensis, making a Distinction between o signifying *some* Quantity, and o signifying *no* Quantity, or absolute Nothing, says that when x

[1] *Principia*, Bk. II, *Lemma* 2. See our §§ 16–19.

converges to o, in the *ultimate* State before it vanishes, $x^x = 1$; but says, when x entirely vanishes, or becomes absolutely of no Value, that then $x^x = o^0 = o$: But o being supposed no Quantity is contradicted by *Algebraic* Computation, which is general and retains o, in a Mathematical Sense, for a Quantity in the Scale, as much as any other Figure or Literal by which Quantity is denoted and compared. . . . Waltoniensis farther observes that *Fluxions* are the Limits to which the *Ratios* of the Increments or Decrements of Quantities converge, and are assignable from the Principles of Motion only (*uniform*, *accelerated*, and *retarded*); and thinks the Doctrine has nothing to do with infinitely small Quantities, First and Last *Ratios*; and that only finite Quantities need be introduced—'to avoid Disputes, and the dark *Mists* spread over the Process, different to the demonstrative Lights of the Antients.' But Motion refers to the Spaces passed over, by which it is comprehended, measured, and compared: And tho' Mr. *Simpson* has pretended to deduce the *Ratios* of Fluxions of Quantities without the use of *indefinitely small Quantities* (see his *New Doctrine and Application of Fluxions*) yet the Motion of his *Points* along the *Lines* answers to them by the indefinitely small Spaces described together, and are to the same Effect as Quantities taken indefinitely small; which Sir Isaac Newton himself introduced to illustrate the Quantity of relative Motion by. *Fluxions*, as instantaneous Velocities, are also as the Increases or Quantities of Space passed over together by those *instantaneous Velocities*, uniformly continued; but are not the Spaces or Quantities themselves that *would be* described by them according to Mr. *Simpson's* new Theory, and Application. See *Emerson's* Doctrine and Application of Fluxions. Price 6s. only."

195. In the *Ladies' Diary* for 1752 the reader is amused by satirical remarks on mathematicians. There is also a continuation of the discussion "Of the Cypher-Value and Office of the Algebraic Quantity *o*." "Nihil Maximus says that ·9999, etc. *ad inf.* will never converge to 1, nor yet 1 / (10,000, etc. *ad inf.*) to *o*; because any Quantity infinitely increased or diminished will be still greater or less, and never numerically arrive at Infinity, and *o* Value. . . . That a Distinction should be carefully made between what are called *infinite* and *indefinite* great and small Quantities (*the former of which being impossible*); for what is of *indefinite* Value has Equality, tho' it may be sometimes unassignable; while what is *infinite* is *never* determinable, and has *never* Equality. Hence the numerical Value of *o* and 1 / (1000, etc.), will be for ever different; one being a Quantity of *no* sensible Value, but yet significant, and the other of indefinite small Value. . . . *Infinite* Quantity, or infinite numerical Value, expressed by Authors, is neither practicable nor comprehensible. . . . An infinite *Series* can never precisely converge to a finite or determinate Value; because it *for ever runs on.* The finite Value, taken for that of an *infinite*

Series, is only the Value from whence that *infinite* Series is or may be derived. Mr. Landen thinks that the Value *o* is no algebraic Quantity; but calls it a *mere Blank*, or absolute Nothing . . .; he says, that its peculiar Office is only in arithmetic Notation; while we see it applied to other Use and Office in Treatises of Algebra and Fluxions, as also by himself, for an algebraic Character or Quantity *o*, its own Value."

This discussion of *o* is continued at great length. Confusion arises from the double use of the symbol and from the difficulties surrounding *o* as the limit of variables or sequences. Reference is made twice in this *Ladies' Diary* (1752) to the pamphlet *Truth Triumphant, or Fluxions for the Ladies*, where the nature and office of *o* are discussed, and exception is taken to Landen's views on *o*. "Fluxioniensis" says: "And therefore I should not stick to rank this excellent Reasoner with the great Master of Reason he mentions, the B—p of Cloyne, as he clearly appears to be of the same class."

Popular Impression of the Nature of Fluxions

196. A reviewer of Richard Jack's *Euclid's Data Restored*[1] quotes from Jack's preface what appears to be the opinion of a non-specialist:

"Others, who claim the honour of extending their principles, treat of what they call *Fluxions*, *calculus differentialis*, *infiniment petit's*, *extreme and*

[1] *Monthly Review*, vol. xvi, London, 1757.

ultimate ratios, etc., and with so much obscurity, that no distinct idea of the thing treated, is communicated to the mind of the Reader. From their want of that precision and perspecuity which the Ancients carefully observed in all their writings, the mind becomes clouded with confusion, begins to doubt, which terminates in a disbelief of their principles; for which reason they have been often called upon to demonstrate them: but no demonstration has appeared."

To this the anonymous reviewer of Jack's book replies:

"That the principles of Fluxions stand in need of demonstration, especially since the publication of Maclaurin's works, is certainly a mere pretence, made only to cover the ignorance of the objector . . ."

Remarks

197. In this chapter we have given views held by writers representing the rank and file of mathematical workers. In several passages the need of an adequate theory of a linear continuum makes itself strongly felt.

Some curiosity attaches to the following contemporaneous opinion of *Truth Triumphant*:[1]

"This is an odd assemblage of controversial scraps, chiefly relating to some disputes concerning Mr Emerson's treatise on fluctions, and Mr. Simpson's on the same subject. This most unimportant

[1] *Monthly Review*, vol. v, London, 1751, p. 462.

controversy was first occasioned by the few observations on Mr Simpson's book published in the *Monthly Review*. . . . The author writes in a manner that can do little honour to any party or opinion. And why he chose to give this strange and insignificant production so odd a title, is a mystery that none but himself can clear up."

CHAPTER IX

ABORTIVE ATTEMPTS AT ARITHMETISATION

John Kirkby, 1748

198. In the preface to his *Doctrine of Ultimators* [1] the author states that his doctrine "depends upon scarce any Thing else but a due Application of the Cypher *o* to the analogous Office in Universal Arithmetic, which it is always known to occupy in Common Arithmetic." He argues that "the superlative impropriety of the Word Fluxion, when applied to this Purpose, will fully appear; when we come to consider, that it is put to express an Idea, which arises from the Contemplation of Quantities purely as Quantities: that is, in the same abstract Manner, as they are the proper Subject of Algebra, exclusive of every other Consideration; and con-

[1] *The Doctrine of Ultimators. Containing a new Acquisition to Mathematical Literature, naturally resulting from the Consideration of an Equation, as reducible from its variable to its ultimate State: Or, a Discovery of the true and genuine Foundation of what has hitherto mistakenly prevailed under the improper Names of Fluxions and the Differential Calculus. By means of which we now have that Apex of all Mathematical Science entirely rescued from the blind and ungeometrical Method of Deduction which it has hitherto laboured under; and made to depend upon Principles as strictly demonstrable, as the most self-evident Proposition in the first Elements of Geometry.* By the Reverend Mr. John Kirkby, Vicar of Waldershare in Kent. London, MDCCXLVIII. Pp. 144.

sequently have not the least Regard to Time or Motion, which are necessarily implied in a Fluxion. And the essential Property of a Fluxion is certainly excluded, after the most singular Manner, in the Idea of Quantity considered at its *Ne plus ultra*: that is, in other Terms, when it is in a State, where all Possibility of such imaginary Flux is taken from it. So that the Term Fluxion, when used to this Purpose, if it have any Meaning at all, is as contrary to the true, as Darkness is to Light." He takes an algebraic equation $Aa^0 \pm Ba' \pm Ca^2 \pm Da^3 \pm \ldots \pm Za^n = 0$, assumes the coefficients Y and Z of the two highest terms as fixed, and declares (without proof) that the absolute term A is a maximum when the n roots of the equation are equal. When such an equality exists, the equation is reduced "to its ultimate." When the roots are equal he represents them by $+c$ or $-c$. To reduce the trinomial $A \pm Ba \pm Za = 0$ to its ultimate, "we must make $B / Z = \pm nc^{n-1}$ in the n Power of $c \pm a = 0$. That is (because $c = a$) $B / Z = na^{n-1}$. Therefore the Ultimate required is $B \pm nZa^{n-1} = 0$, or $Ba^0 \pm nZa^{n-1}a^0 = 0$." To be observed here is that Kirkby connects, though only in an obscure way, his ultimate with the coefficient of a in the second term of the binomial expansion of $(c \pm a)^n$. He then pretends to prove "that the Ultimate of the Sum of never so many Equations is the same with the Sum of their respective Ultimates"; hence, the Ultimate of the above general equation is $o \pm Ba^0 \pm 2Caa^0 \pm 3Da^2a^0 \pm \ldots \pm nZa^{n-1}a^0 = 0$. He gives the rule for finding the "ultimate" or "ultimator" of a^n; this ultimator is $na^{n-1}a^0$; he also gives the rule for writing down "the subject of every Ultimator"; the subject of the ultimator $na^{n-1}a^0$ being a^n+c. He applies these rules when the exponents are fractional.

The "ultimator" of the product of two variables, ae, is found thus. "Put $ae=bee$, and $ae=caa$. Whence $a=be$, and $e=ca$, and $ae=\frac{1}{2}\,bee+\frac{1}{2}\,caa$. The Ultimator of which last is . . . bee^0+caa^0, and consequently is equal to the Ultimator of ae. But $be=a$, and $ca=e$. Therefore these substituted for their Equals in that Ultimator give ae^0+ea^0 for the Ultimator of ae" (p. 43). It will, of course, be noticed that special limitations are placed upon the variables a and e, when the coefficients b and c are tacitly assumed to be constants. Kirkby proceeds to the derivation of the ultimators of fractions and logarithms. He explains the necessity of retaining in the Ultimator each variant (variable) under its o Power. "Without this we cou'd have no Means from the Nature of the thing itself, whereby to distinguish an Ultimator from a Subject." The functions of a^0, e^0 are more than simply to represent unity; just what they are is not very clear, although to the author "it is evident then, as often as any Subject consists of different Variants Ex gr. x, y, z, that the Expressions x^0, y^0, z^0, in the Ultimator have the same Difference in Power with the same Variants under any other common Exponent x^n, y^n, z^n. . . . Therefore the Expression x^0, y^0, z^0, I conceive may be each fitly called the *Peculiar Unit* of its respective Scale of Powers. Hence every *Ultimator* may be defined to be, *The proper Reference of each Subject in a given Equation to the Peculiar Units of the Powers of all its Variants, in Order to discover the Ratios of those Variants to one another in their Ultimate State.* Which I take to be the true Definition of what has been hitherto most improperly and unintelligibly called a *Fluxion* by some, and a *Differential* by others" (page 49).

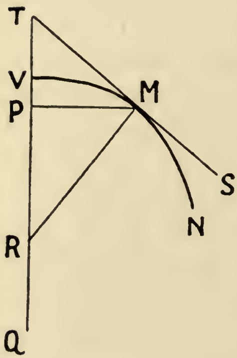

FIG. 11.

199. Kirkby's doctrine may perhaps become plainer by the study of one of his applications. In any curve with the concave side to TQ, the greater abscissa VP (or v) has always the greater "semi-ordinate" PM (or s), "and each are the greatest that they possibly can be to the same Arch VM, or to the same intercepted axis VR. Therefore the Subnormal PR (or $r-v$), and consequently the Normal MR ($=c$) are each the least that they possibly can be to the same Arch VM, or the same intercepted Axis VR (or r). Therefore, if in the last Equation [$r^2-c^2=2rv-v^2-s^2$], c and r be invariable, we have r^2-c^2 an *Ultimum*. Consequently, the Ultimate of that Equation . . . is $2rv^\circ-2vv^\circ-2ss^\circ=0$, or (dividing by 2) $ss^\circ=\overline{r-v}\times v^\circ$. Whence $v^\circ : s^\circ = s : r-v$. That is in all Curves, as

the Ultimator of the Abscissa is to the Ultimator of the Semiordinate; so is the Semiordinate itself to the Subnormal" (p. 51).

The author has occasion to use second and third ultimators and to consider ultimators as variable or invariable. He lets (p. 60) $\dot{x}^\circ$ be the invariable of the first ultimator x°, $\overset{\circ}{\dot{x}}$ the invariable of the second ultimator $\overset{\circ}{\overset{\circ}{x}}$, etc., and warns the reader that his dot does not mean a fluxion. In the more involved applications to curves he lets an infinitely small arch equal $x^\circ = \dot{x}^\circ$. Our impression of the book is that the author's intentions were good when he attempted an arithmetisation. But there is a total lack of clear and rigorous exposition.

200. The *Ladies' Diary*, London, 1750, p. 45, contains a hostile criticism of the *Doctrine of Ultimators* by an anonymous writer (probably the editor, Robert Heath), in which the author of this doctrine is said to declare that fluxions, as explained by Newton, are "absurd and unintelligible," and to place confidence in "the Authority of a certain Irish B—p, a Mathematician as wise as himself. For you must know that this pious B—p (the sagacious Author of the *Analyst*, as he stiles him) out of his religious Zeal against Mathematical Learning, had been engaged in the same senseless Attempt with himself, of degrading the noblest Science. . . . Having thus, as he [the author of "Ultimators"] thinks, overturn'd the Doctrine of Fluxions . . . he has given us instead of it . . . a new Science of

his own, whose Foundation, it seems, depends on *Cyphers*, and Nought Powers full of conceited Expressions. . . . He expresses his Ultimators by the Help of x°, y°, z°, etc., calls them peculiar Units, and of different Values, all of which is absurd. . . . I pass over . . . his using x°, y°, z°, for the same End as others use $\dot{x}$, $\dot{y}$, $\dot{z}$."

John Petvin, 1750

201. In a *Sketch of Universal Arithmetic*,[1] brought out as a posthumous booklet, we encounter a curiosity. Its philosophy of mathematics and of fluxions in particular is set off by the following quotations:

(Page 156) "I do not then consider it [mathematical quantity] as generated or produced, but as that which is. Time and Motion produce nothing of the Kind, and have no Place here. Nor do I consider it as continuous, nor as consisting of very small or infinitely little Parts, but as consisting of Parts in general. These Parts therefore I consider as discrete: And by x, y, z, etc., I understand Multitude. The Ones or Monads, of which x is many, I call x; . . . Nor do I consider x, y, z, etc., barely as many; but as a certain many. So that x, y, z, etc., are Wholes; $\underset{\cdot}{x}$, $\underset{\cdot}{y}$, $\underset{\cdot}{z}$, etc., their respective Parts. These Parts may be considered again as Wholes, consisting of another Order of

[1] *Letters concerning Mind. To which is added, a Sketch of Universal Arithmetic; comprehending the Differential Calculus, and the Doctrine of Fluxions.* By the late Reverend Mr. John Petvin, A.M., Vicar of Ilsington in Devon, London, 1750.

Parts," designated as $\underset{\cdot}{x}$, etc. "Such Things as an Instant, a Point, a Fluxion, she [arithmetic] has nothing to do with. . . . I have joined Fluxion with Point and Instant, because Fluxion seems to be to Motion, as an Instant is to Time; which I suppose to be as a Point is to a Line. Motion cannot be conceived without Time and Space; and when the former runs into an Instant, and the latter into a Point, then it is (as I understand it) that Motion becomes Fluxion. . . . In this Sense Fluxion is no more a Part of Motion than a Point is a Part of a Line." His "parts" are finite increments. The part of xy is $\underset{\cdot}{x}y+\underset{\cdot}{y}x+\underset{\cdot}{x}\underset{\cdot}{y}$. "This Doctrine of Wholes and Parts proceeds upwards from Parts to Wholes, as well as downwards from Wholes to Parts universally" (p. 159). "The Ordinate therefore being x^m, when $x^m\dot{x}$ expresses the Fluxion of it, the only Meaning I have for $\dot{x}$ is, that it is the Proportion of a Point to an Instant. And to my Apprehension, a Point may as well be called a last Line, as this called a Velocity." "I have lately deduced some arithmetical Theorems from arithmetical Principles, which other Mathematicians have drawn from Fluxions of Fluxions, etc., and these Theorems fell in with my Design." Just how these deductions were made is not explained by the author.

John Landen, 1758

202. John Landen was a self-educated mathematician of real mathematical power. Had he had the benefits of University training he might have

occupied a much higher rank as a mathematician. Foreigners place him high among his English contemporaries. He wrote *Mathematical Lucubrations*, 1755, and *Residual Analysis*, 1764. We shall consider only his *Discourse concerning Residual Analysis*,[1] 1758. From it we quote as follows:

"Yet, notwithstanding the method of fluxions is so greatly applauded, I am induced to think, it is not the most natural method. . . . The operations therein being chiefly performed with algebraic quantities, it is, in fact, a branch of the algebraic art, or an improvement thereof, made by the help of some peculiar principles borrowed from the doctrine of motion. . . . We may indeed very naturally conceive a line to be generated by motion; but there are quantities . . . which we cannot conceive to be so generated. It is only in a figurative sense, that an algebraic quantity can be said to increase or decrease with some velocity. Fluxions therefore are not immediately applicable to algebraic quantities. . . . It therefore, to me, seems more proper, in the investigation of propositions by algebra, to proceed upon the *anciently-received* principles of that art. . . . That the borrowing principles from the doctrine of motion, with a view to improve the analytic art, was done, not only without any necessity, but even without any peculiar advantage, will appear by showing, that whatever can be done by the method of computa-

[1] *A Discourse Concerning Residual Analysis: A new Branch of the Algebraic Art.* By John Landen. London, 1758.

tion, which is founded on those borrowed principles, may be done as well, by another method founded entirely on the *anciently-received* principles of algebra. . . . It is by means of the following theorem [p. 5], viz.

$$\frac{x^{\frac{m}{n}} - v^{\frac{m}{n}}}{x - v} = x^{\frac{m}{n}-1} \times \frac{1 + \frac{v}{x} + \left(\frac{v}{x}\right)^2 + \left(\frac{v}{x}\right)^3 \quad (m)}{1 + \left(\frac{v}{x}\right)^{\frac{m}{n}} + \left(\frac{v}{x}\right)^{\frac{2m}{n}} + \left(\frac{v}{x}\right)^{\frac{3m}{n}} \quad (n)},$$

(where m and n are any integers) that we are enabled to perform all the principal operations in our said Analysis."

His Residual Analysis is a method involving vanishing fractions and therefore not free from controversial questions. That the fluxion of x^3 is $3x^2$ is explained according to the Residual Analysis by the consideration that $(y^3 - x^3) \div (y - x) = x^2 + xy + y^2$, which is equal to $3x^2$ when $y = x$. We proceed to give an application in Landen's own words:

203. (Page 5) "Fluxionists, in determining the limit of the ratio of the increments of x and $x^{\frac{m}{n}}$, commonly have recourse to the binomial theorem (which is much more difficult to investigate than the limit they are seeking): But how easily may that limit be found, without the help of that theorem, by the equation exhibited in page 5! Thus, the increment of x being denoted by x', the increment of $x^{\frac{m}{n}}$ is $\overline{x+x'}^{\frac{m}{n}} - x^{\frac{m}{n}}$, and the ratio of those increments is

$$\frac{\overline{x+x'}^{\frac{m}{n}}-x^{\frac{m}{n}}}{x'}=\frac{\overline{x+x'}^{\frac{m}{n}}-x^{\frac{m}{n}}}{\overline{x+x'}-x}=\overline{x+x'}^{\frac{m}{n}-1}$$

$$\times\frac{1+\frac{x}{x+x'}+\overline{\frac{x}{x+x'}}\Big|^{2}+\overline{\frac{x}{x+x'}}\Big|^{3} \quad (m)}{1+\overline{\frac{x}{x+x'}}\Big|^{\frac{m}{n}}+\overline{\frac{x}{x+x'}}\Big|^{\frac{2m}{n}}+\overline{\frac{x}{x+x'}}\Big|^{\frac{3m}{n}} \quad (n)},$$

which, when x' vanishes, is manifestly equal to $\frac{m}{n}x^{\frac{m}{n}-1}$, the limit of the said ratio."

The explanation of the method of drawing tangents is too long for quotation, and we shall limit ourselves to the following outline of it, as given by Landen :

"I consider the curve as already described, without any regard to its generation, and find the value of a certain line (terminated by the curve and its tangent), in algebraic terms involving (s) the subtangent with other quantities ; which algebraic expression I observe, from an obvious property of the line it is found to denote, must have a certain property with respect to being positive or negative in certain cases. I therefore assume that expression equal to another which is known to have that very property ; and from thence, by means of the theorem mentioned in page 5, readily find the required value of s" (p. 10).

204. Landen's *Discourse* was attacked by an anonymous writer in the *Monthly Review* for June 1759, who claims that the Residual Analysis "is no

other than Sir Isaac Newton's method of differences; and it is well known, that if the differences are diminished so as to vanish, their vanishing ratio becomes that of fluxions" (p. 560), "that his pretended Residual Analysis renders the investigations more tedious and obscure than any other." Landen wrote a reply in the July number, from which we quote only the part relating to the word "function." Says Landen: "He objects to *prime number, function*, etc., as terms never heard before.—Alas! how egregiously does he betray his ignorance!"

James Glenie, 1793

205. James Glenie graduated from the University of St. Andrews, and became a military engineer. He was a prominent Fellow of the Royal Society of London. In his *Antecedental Calculus*,[1] 1793, he begins with the statement, "Having, in a Paper, read before the Royal Society, the 6th of March, 1777, and published in the Philosophical Transactions of that Year, promised to deliver, without any consideration of Motion or Velocity, a Geometrical Method of Reasoning applicable to every purpose, to which the much celebrated Doctrine of Fluxions of the illustrious Newton has been or can

[1] *The Antecedental Calculus, or a Geometrical Method of Reasoning, without any Consideration of Motion or Velocity applicable to every Purpose, to which Fluxions have been or can be applied.* By James Glenie, Esq., M.A. and F.R.S. London, 1793. According to G. Vivanti (see M. Cantor's *Vorlesungen über Geschichte der Mathematik*, vol. iv, Leipzig, 1908, p. 667), James Glenie (1750–1817) was an artillery officer in the war of the American Revolution, later professor of mathematics in the military school of the East India Company.

be, applied; and having taken notice of the same Method, in a small Performance, written in Latin, and printed the 16th of July, 1776, I now proceed to fulfil my promise with as much conciseness as perspicuity and precision will admit of." In his *Antecedental Calculus*, p. 10, he says of Newton: 'I am perfectly satisfied, that had this great Man, discovered the possibility of investigating a general Geometrical Method of reasoning, without introducing the ideas of Motion and Time, . . . he would have greatly preferred it, since Time and Motion have no natural or inseparable connection with pure Mathematics. The fluxionary and differential Caculi are only branches of general arithmetical proportion."

Glenie speaks (p. 3) of "the excess of the magnitude, which has to B a ratio having to the ratio of A + N to B the ratio of R to Q (when R has to Q any given ratio whatever), above the magnitude, which has to B a ratio having to the ratio of A to B the same ratio of R to Q, is geometrically expressed by" a complicated fraction whose denominator is $B^{(R-Q)/Q}$, and whose numerator is the result of expanding by the binomial theorem $(A+N)^{R/Q}$ and then subtracting $A^{R/Q}$ therefrom.

A similar expression is given for the case in which A − N takes the place of A + N: "The excess of the magnitude, which has to B a ratio, having to the ratio of A to B the ratio of R to Q, above the magnitude, which has to B a ratio, having to the ratio of A − N to B the ratio of R to Q, is geometrically expressed by" a fraction whose denominator is $B^{(R-Q)/Q}$, and whose numerator is obtained by expanding and simplifying $A^{R/Q}-(A-N)^{R/Q}$. "But if $A+N$ and $A-N$ stand to B in relations nearer to that of equality than by any given or assigned magnitude of the same Kind, these general expressions become $R/Q \cdot A^{(R-Q)/Q} \cdot N \div B^{(R-Q)/Q}$. This I call the antecedental of the magnitude which has to B such a ratio as has to the ratio of A to B the ratio of R to Q. Now if N the antecedental of A be denoted by $\dot{A}$ or $\overset{a}{\dot{A}}$ . . . [and] if $Q=1$ and $R=2$, 3, 4, 5, etc., this expression gives $\frac{2A\dot{A}}{B}$, $\frac{3A^2\dot{A}}{B^2}$ . . . respectively." For the "antecedent" $A\frac{A}{B}$ he finds the "antecedental" $\frac{2A\dot{A}}{B}$ or $2\dot{A} + \frac{2M\dot{A}}{B}$ (putting M for $A-B$). Glenie shows that at a point of a curve the antecedentals of the abscissa, ordinate and curve, are as the sub-tangent, the ordinate and the tangent, respectively.

Glenie's calculus involves extremely complicated identities of ratios and examines the antecedents of ratios having given consequents. The style of exposition is poor. In deriving the antecedentals, Glenie quietly drops out all the terms in the numerator that involve powers of N higher than the first power. As this calculus plays no part in the later history of fluxions, we shall give only one more quotation; it relates to the Binomial Theorem (not used by him in the development of his fundamental formulas). He says (p. 11): "It may not perhaps be improper to add, that, if to the expressions delivered above for the excess of the magnitude, which has to B a ratio, having to the ratio of A + N to B, the ratio of R to Q, above the magnitude, which has to B a ratio, having to the Ratio of A to B the same ratio of R to Q; and for the excess of the magnitude, which has to B a ratio, having to the ratio of A to B the ratio of R to Q, above the magnitude, which has to B a ratio, having to the ratio of A − N to B the ratio of R to Q, be prefixed the magnitude, which has to B a ratio, having to the ratio of A to B the ratio of R to Q, we get a geometrical Binomial, of which, when it is supposed to become numerical, the famous Binomial Theorem of Sir Isaac Newton is only a particular case."

Remarks

206. The classic treatment of fluxions in Great Britain, during the eighteenth century, rests primarily on geometrical and mechanical conceptions. Attempts to found the calculus upon more purely arithmetical and algebraical processes are described in this chapter. All these attempts are either a complete failure or so complicated as to be prohibitive. Easily the ablest among these authors was John Landen. De Morgan says of his Analysis[1]: "It is the limit of D'Alembert supposed to be *attained*,

[1] *Penny Cyclopædia*, Art. "Differential Calculus."

instead of being a *terminus* which can be attained as near as we please. A little difference of algebraical suppositions makes a fallacious difference of form: and though the residual analysis draws less upon the disputable part of algebra than the method of Lagrange, the sole reason of this is that the former does not go so far into the subject as the latter."

In the same article De Morgan speaks of Kirkby's Ultimators thus:

"A something between Landen and D'Alembert, as to principle, published in 1748, was called the 'Doctrine of Ultimators, containing a new Acquisition, etc., or a Discovery of the true and genuine Foundation of what has hitherto mistakenly prevailed under the improper names of Fluxions and the Differential Calculus.'"

CHAPTER X

LATER BOOKS AND ARTICLES ON FLUXIONS

Encyclopædia Britannica, 1771, 1779, 1797

207. The article "Fluxions" in the first edition of the *Encyclopædia Britannica*, Edinburgh, 1771, gives this definition: "The fluxion of any magnitude at any point is the increment that it would receive in any given time, supposing it to increase uniformly from that point; and as the measure will be the same, whatever the time be, we are at liberty to suppose it less than any assigned time." The fluxion of a rectangle is the increment, with the small rectangle at the corner omitted; the latter "is owing to the additional velocity wherewith the parallelogram flows during that time and therefore is no part of the measure of the fluxion." "The increment a quantity receives by flowing for any given time, contains measures of all the different orders of fluxions; for if it increases uniformly, the whole increment is the first fluxion; and it has no second fluxion. If it increases with a motion uniformly accelerated, the part of the increment occasioned by the first motion measures the first fluxion, and the

part occasioned by the acceleration measures the second fluxion. . . ."

The same article is reprinted in the second edition (1779) and the third edition (1797).

Robert Thorp, 1777

208. Thorp made a translation of part of Newton's *Principia.*[1]

In the "advertisement" we read: "The doctrine of prime and ultimate ratios . . . is established, so as to remove the various objections which have been raised against it, since it was first published. To the relations of finite quantities alone the reasoning on this subject is confined." The translation of *quantitates quam minimæ*, *evanescentes*, *ultimæ*, *infinité magnæ*, and the like, has not been literal, yet they are "explained in that sense under which the author cautions his readers to understand them. This is the more necessary, as the terms *infinite*, *infinitesimal*, *least possible*, and the like, have been grossly misapplied and abused."

209. In the Commentary to Lemma I in Sect. I of Bk. I in the *Principia*, Thorp says: "The prime and ultimate ratios of magnitudes . . . are investigated by observing their finite increments or decrements, and thence finding the limits of the ratios of those *variable* magnitudes; not the ratios to which the magnitudes ever actually arrive (for

[1] *Mathematical Principles of Natural Philosophy. By Sir Isaac Newton, Knight. Translated into English, and illustrated with a Commentary*, by Robert Thorp, M.A., vol. i, London, 1777.

they are never, strictly speaking, either prime or ultimate in fact), but those limits to which the ratios of magnitudes perpetually approach; which they can never reach, nor pass beyond; but to which they appear nearer than by any assignable difference." . . . "We now proceed to explain this Lemma more particularly than perhaps might seem necessary, had it not been much controverted, misrepresented, and misunderstood." As one of the conditions of the proposition, Thorp states, is "that quantities and the ratios of quantities must *continually* tend to equality. The one must never become equal to, nor pass beyond the other: their difference must never either vanish to nothing, or become negative." In this restriction Thorp goes even further than had Robins. The following passage from Thorp's commentary is thoroughly in the spirit of Robins: ". . . That we may not be led, from the expression *ultimately* equal, to suppose, that there is an *ultimate* state, in which they are actually equal, we are cautioned in the scholium at the end of this Section [of *Principia*, Bk. I, Sect. 1] in these words, *The ultimate ratios, in which quantities vanish, are not in reality the ratios of ultimate quantities; but the limits to which the ratios of quantities continually decreasing always approach; which they never can pass beyond, nor arrive at, unless the quantities are continually and indefinitely diminished.* According to Thorp, the inscribed or circumscribed polygon can never arrive at the curve. He quotes from Saunderson's Fluxions. By the doctrine of indivisibles there "has been introduced into mathematical reasoning all that absurd jargon concerning quantities infinitely great, and infinitely little, which has been so much objected to by mathematicians. And, though it has often been elegantly applied by some able geometers to the demonstration of many noble theorems ; yet in the hands of less accurate reasoners, it has often led to false conclusions" (p. 71).

F. Holliday, 1777

210. In a somewhat lengthy preface to his *Introduction to Fluxions*[1] the author tells that, when in 1745 he was in London, in company with W. Jones and De Moivre, they expressed great approbation of Emerson's *Fluxions*, with regard to the method of treatment, but thought his book too high for beginners. The author tries to be more diffuse in the laying down of first principles. He derives the fundamental results in two ways : first, by the aid of nascent or evanescent quantities, as suggested by Newton's *Principia* ; second, "without using any infinitely small quantities, or vanescent Parallelograms, which perhaps will be more acceptable to many of my Readers." Holliday explains at great length the Scholium (see our §§ 10–15) on prime and ultimate ratios, and gives a short account of the invention of fluxions as given in the review of

[1] *An Introduction to Fluxions, Designed for the Use, and Adapted to the Capacities of Beginners.* By the Reverend F. Holliday, Vicar of West Markham and Bothamsall, Nott's. London, 1777.

Collins's *Commercium Epistolicum* in the *Philosophical Transactions*, 1717. Though following Newton closely, variations were bound to arise. Thus, Holliday says (p. 73), "Fluxions are not magnitudes but the *velocities* with which magnitudes, varying by a continual motion, increase or decrease." It cannot be claimed that Holliday made any contribution to the philosophy of fluxions, nor even that he profited as much as he might by the refinements in the logic which had been made by English writers since the time of Newton.

Charles Hutton, 1796, 1798

211. In his *Mathematical Dictionary*, London, 1796, Charles Hutton makes reference to the advantage of Simpson's definition of a fluxion as a magnitude uniformly generated in a finite time, the imagination being now no longer confined to a single point and to the velocity at that point; moreover, "higher orders of Fluxions are rendered much more easy and intelligible."

212. From the part on fluxions in Hutton's *Course of Mathematics*[1] we take the following:

"The rate or proportion according to which any flowing quantity increases, at any position or instant, is the Fluxion of the said quantity, at that position or instant: and it is proportional to the magnitude by which the flowing quantity would be uniformly increased, in a given time with the

[1] *A Course of Mathematics.* By Charles Hutton. London, 4th ed., 1803–1804, vol. ii, p. 279. [First ed., 1798–1801.]

generating celerity uniformly continued during that time."

". . . If the motion of increase be accelerated, the increment so generated, in a given finite time, will exeeed the fluxion: . . . But if the time be indefinitely small, so that the motion be considered as uniform for that instant; then these nascent increments will always be proportional, or equal, to the fluxions, and may be substituted instead of them in any calculation."

The fluxion of xy is derived in two ways: the first by the method of considering the rectangle composed of two parts, as previously expounded by Rowe.

The second method finds algebraically the increment $xy' + yx' + x'y'$, "of which the last term $x'y'$ is nothing, or indefinitely small, in respect of the other two terms, because x' and y' are indefinitely small in respect of x and y. . . . Hence, by substituting $\dot{x}$ and $\dot{y}$ for x' and y', to which they are proportional, there arises $x\dot{y} + y\dot{x}$ for the true value of the fluxion of xy."

S. Vince, 1795, 1805

213. Vince's *Principles of Fluxions* appeared in 1795 as the second volume of the *Principles of Mathematics and Natural Philosophy in Four Volumes*,[1] which were brought out under the general editorship of James Wood. A second

[1] *The Principles of Mathematics and Natural Philosophy in Four Volumes. Vol. II, The Principles of Fluxions: Designed for the Use of Students in the University.* By the Rev. S. Vince, A.M., F.R.S., Cambridge, 1795.

edition of Vince was printed in 1805. From this second edition we quote:

P. 1: "The velocities with which flowing quantities increase or decrease at any point of time, are called the *fluxions* of those quantities at that instant.

"As the velocities are in proportion to the increments or decrements *uniformly* generated in a given time, such increments or decrements will represent the fluxions."[1]

Vince also quotes Newton on the generation of quantities by motion: "Sir I. Newton, in the Introduction to his *Quadrature of Curves*, observes that 'these geneses really take place in the nature of things, and are daily seen in the motion of bodies. And after this manner, the ancients, by drawing moveable right lines along immoveable right lines, taught the geneses of rectangles.'"

Vince gives no formal definition of a *limit*; but his philosophy of this subject is disclosed by the two following quotations (pp. 4 and 5): "By keeping the ratio of the vanishing quantities thus expressed by finite quantities, it removes the obscurity which may arise when we consider the quantities themselves; this is agreeable to the reasoning of Sir I. Newton in his *Principia*, Lib. I, Sect. 1, Lem. 7, 8, 9."

"It has been said, that when the increments are

[1] "This is agreeable to Sir I. Newton's ideas on the subject. He says: 'I sought a method of determining quantities from the velocities of the motions or increments with which they are generated; and calling these velocities of the motions or increments, *fluxions*, and the generated quantities *fluents*, I fell by degrees upon the method of fluxions.'—Introd. to *Quad. Curves*."

actually vanished, it is absurd to talk of any ratio between them. It is true; but we speak not here of any ratio then existing between the quantities, but of that ratio to which they have approached as their *limit*; and that ratio still remains. Thus, let the increments of two quantities be denoted by ax^2+mx and bx^2+nx; then the *limit* of their ratio, when $x=0$, is $m:n$; for in every state of these quantities, $ax^2+mx : bx^2+nx :: ax+m : bx+n ::$ (when $x=0$) $m:n$. As the quantities therefore approach to nothing, the ratio approaches to that of $m:n$ as it's limit. We must therefore be careful to distinguish between the ratio of two evanescent quantities, and the *limit* of their ratio; the former ratio never arriving at the latter, as the quantities vanish at the instant that such a circumstance is about to take place."

By aid of the binomial theorem, Vince finds the fluxion of x^n, when the fluxion of x is given; he then finds the fluxion of xy by considering $(x+y)^2=x^2+2xy+y^2$, by which the fluxion of $2xy$ can be found in terms of the fluxions $(x+y)^2$, x^2 and y^2.

Agnesi—Colson—Hellins, 1801

214. The *Analytical Institutions*[1] is the first calculus that was written by a woman. The authoress

[1] *Analytical Institutions, in four books: Originally written in Italian, by Donna Maria Gætana Agnesi, Professor of the Mathematicks and Philosophy in the University of Bologna. Translated into English by the late Rev. John Colson, M.A., F.R.S., and Lucasian Professor of the Mathematicks in the University of Cambridge. Now first printed, from the Translators Manuscript, under the inspection of the Rev. John Hellins, B.D., F.R.S.* Vols. i and ii. London, 1801.

is the noted Maria Gætana Agnesi, of the University of Bologna.

The Italian original was first published at Milan in 1748. The two volumes of the translation were printed at the expense of Baron Maseres. In an introduction, Hellins points out that Colson hoped to interest the ladies of England in the study of fluxions by his translation of the work of the great Italian lady, "And, in order to render it more easy and useful to the Ladies of this country, . . . he [Colson] had designed and begun a popular account of this work, under the title of *The Plan of the Lady's System of Analyticks*; explaining, article by article, what was contained in it. But this he did not live long enough to finish."

215. Colson dealt with Agnesi's work somewhat as Stone had dealt with that of De L'Hospital, inasmuch as both translators substituted the notation of Newton in place of that of Leibniz. The word fluxions ("flussioni") occurs in the original Italian of Agnesi's masterly work. How Colson's conscience may have troubled him, when a fluxion stood out in his translation as something "infinitely little," may be judged when we consider that in 1736 he brought out an English translation, with an extensive comment, of Newton's *Method of Fluxions*. With Newton a fluxion always meant a *velocity*.

We quote a few passages from Colson's Agnesi (vol. ii, pp. 1, 2):

"The Analysis of infinitely small Quantities, which is otherwise called the *Differential Calculus*, or the *Method of Fluxions*, is that which is conversant about the differences of variable quantities, of whatever order those differences may be."

"Any infinitely little portion of a variable quantity is called it's *Difference* or *Fluxion*; when it is so small, as that it has to the variable itself a less proportion than any that can be assigned; and by which the same variable being either increased or diminished, it may still be conceived the same as at first."

On p. 3 we read that certain lines in a figure "will be quantities less than any that can be given, and therefore will be *inassignable*, or *differentials*, or *infinitesimals*, or, finally, *fluxions*. Thus, by the common Geometry alone, we are assured that not only these infinitely little quantities, but infinite others of inferior orders, really enter the composition of geometrical extension."

"These propositions," says a reviewer[1] of the translation, "may appear exceptionable, in point of language, to the *rigorists* in geometry; but they are nevertheless founded on good principles, and furnish rules for the comparison of evanescent quantities, which will prove safe guides in investigation. The demonstrations appear to us to be perfectly sound (if the word *infinite* be taken in its true sense, as denoting merely the absence of any limit), with the exception, perhaps, of the first theorem, which (as is not a little curious to remark) is liable to the

[1] *Edinburgh Review*, vol. iii, 1805, p. 405.

same objection that has been made of the lemma of Newton's Principia. In both instances, also, the error is rather apparent than real." The first theorem in question states that the two intersecting perpendiculars to a curve drawn at the ends of "an infinitely little portion of it of the first order," "may be assumed as equal to each other." We wonder what Robins and Maclaurin would have thought, had they been alive in 1801 and 1805, and read these definitions and comments! What horrible visions would these ghosts of departed quantities have brought to Bishop Berkeley, had he been alive! But the nineteenth century was destined to bring back to British soil still greater accentuations of infinitesimals.

T. Newton, 1805

216. The Rev. T. Newton says in the preface of his *Illustrations of Sir Isaac Newton's Method*:[1]

"Every Mathematician now considers the whole doctrine of Prime and Ultimate Ratios in no other light, than as a Doctrine of Limits." Young readers of Sir Isaac Newton's *Principia* encounter difficulties because commentators have made "use of the terms of Indivisibles, in their explanations; . . . Newton expressly says, that by the ultimate ratios of quantities he means the ratios of their limits.[2] And when he wants to infer the

[1] *An Illustration of Sir Isaac Newton's Method of Reasoning. By Prime and Ultimate Ratios.* By the Rev. T. Newton, Rector of Tewin, Herts; late Fellow of Jesus College, Cambridge. Leeds, 1805.

[2] See our §§ 12, 15.

equality of inequality of those limits from some relation of the variable quantities, which are never supposed absolutely to reach their limits, it certainly requires something more than a definition to shew this. . . . It is not my intention to detain the reader, with answering the objections of the *Analyst* and his followers, because it has been already done by others in a satisfactory manner. . . . Notwithstanding the assertions of some modern writers, the method of ultimate ratios is extremely perspicuous, strictly logical, and more concise than any other of modern invention; . . . it neither involves the strange notion, that a straight line may be a part of a curve, and a plane superficies a part of a concave or convex one; nor the unintelligible idea of adding and subtracting indivisibles, or inconceivably small magnitudes. Whatever magnitudes are compared, according to this method, they are always supposed to be finite."

T. Newton begins with the following two definitions (p. 1):

"If a variable quantity, either increasing or decreasing, approaches to a fixed quantity, the difference between them being continually diminished, so as at length to become less than any assignable quantity; the fixed quantity is called the Limit of the variable quantity."

"If the ratio of two variable quantities continually approaches to a fixed ratio, so as to come nearer to it than by any assignable difference; the fixed ratio is called the Limiting Ratio of the variable quantities."

William Dealtry, 1810, 1816

217. In the preface of Dealtry's *Principles of Fluxions*[1] (1816) we read:

"The method of Fluxions rests upon a principle purely analytical; namely, the theory of limiting ratios; and the subject may therefore be considered as one of pure mathematics, without any regard to ideas of time and velocity. But the usual manner of treating it is to employ considerations resulting from the theory of motion. This was the plan of Sir Isaac Newton in first delivering the principles of the method; and it is adopted in the following Work, from the belief, that it is well adapted for illustration."

Dealtry defines a "fluxion of a quantity at any point of time" as "its increment or decrement, taken proportional to the velocity with which the quantity flows at that time." . . .

"When a quantity increases with a velocity which continually varies, the quantity, which measures the fluxion, is a limit between the preceding and succeeding increments, and is ultimately equal to either of them." He explains that "the word ultimately is intended to denote that particular instant, when the time is diminished *sine limite*,"

[1] *The Principles of Fluxions: Designed for the Use of Students in the Universities.* By William Dealtry, B.D., F.R.S., late Fellow of Trinity College, Cambridge. 2nd ed., Cambridge, 1816.

and quotes Newton's Scholium, Sect. 1, in the *Principia.* He points out, also, that if x increases uniformly, x^2 increases with accelerated velocity, and the part of the increment x'^2 is the effect of the acceleration, and therefore, by his definition of fluxion, to be "omitted in taking the fluxions" (p. 8).

New Editions, 1801–1809

218. William Davis, who was a bookseller in London and editor of the *Companion to the Gentleman's Diary*, appears also as the editor of new editions of three different texts on fluxions. In 1801 he saw through the press the second edition of Maclaurin's *Treatise of Fluxions*; in 1805 the third edition of Thomas Simpson's *Doctrine and Application of Fluxions.* In 1809 appeared the fourth edition of John Rowe's *Doctrine of Fluxions*, revised "by the late William Davis."

Remarks

219. Among some of the authors of this period there is less concern than among writers of former years about the attainment of the rigour of the ancients. Perhaps the effects of the revival of the ideals of Euclid and Archimedes which followed the publication of the *Analyst* were gradually subsiding. It would not be fair to this age to judge its mathematical status altogether by the authors which we have selected. There was a movement under way

at this time which is reflected in the literature that will be under consideration in the next chapter.

Both before the time of Berkeley's *Analyst* and after the time of Maclaurin's *Fluxions* there appeared in Great Britain texts which superposed British symbols and phraseology upon the older Continental concepts. The result was a system, destitute of scientific interest. Newton's notation was poor and Leibniz's philosophy of the calculus was poor. That result represents the temporary survival of the least fit of both systems. The more recent international course of events has been in a diametrically opposite direction, namely, not to superpose Newtonian symbols and phraseology upon Leibnizian concepts, but, on the contrary, to superpose the Leibnizian notation and phraseology upon the limit-concept, as developed by Newton, Jurin, Robins, Maclaurin, D'Alembert, and later writers.

CHAPTER XI

CRITICISMS OF FLUXIONS BY BRITISH WRITERS UNDER THE INFLUENCE OF D'ALEMBERT, LAGRANGE, AND LACROIX

Review of Lagrange's "Fonctions analytiques," 1799

220. Important is a review[1] of Lagrange's *Théorie des fonctions analytiques*, which, as is well known, is an attempt to deduce the principles of the calculus, diverted of all reference to infinitely small or evanescent quantities, limits or fluxions, and reduced to the algebra of finite quantities. The reviewer gives a general criticism of the methods of fluxions and the differential calculus. He discusses the principle of motion: "It will not be denied that this principle is introduced purely for the purpose of illustration, . . . on the ground of convenience. . . . The mathematical principle, on which the doctrine of fluxions depends, is a definition . . . and fluxions were defined to be velocities. . . . Now velocity is nothing real, but is only the relation between the space described and the time of describing it;—of which relation we have a clear idea when the motion is uniform." The reviewer continues: "In variable

[1] *Monthly Review*, London, vol. xxviii, 1799, Appendix.

motion, however, we inquire what velocity is; and here it is defined to be the relation between the space which *would* be described were the motion continued uniform from any point, and the time. Still difficulties remained; this definition might convey to the mind a general idea of the nature of velocity, but was of no *mathematical* use, since the *space which would be described* could not be immediately ascertained and determined. Another step was therefore to be made, and which was made by establishing this proportion; if V be the velocity, S the space, which would be described, and T the time, S′ the space really described, and T′ the corresponding time; then $V = \frac{S}{T} =$ ultimate ratio of $\frac{S'}{T'}$, when S′ and T′ are indefinitely diminished."

Again he says:

"On the ground of perspicuity and evidence, the understanding is not much assisted by being directed to consider all quantity as generated by motion; . . . when such quantities as weight, density, force, resistance, etc., become the object of inquiry . . . then the true end of the figurative mode of speech, *illustration*, is lost. . . . That which happened to Aristotle has happened to Newton; his followers have bowed so implicitly to his authority, that they have not exercised their reason. The method of fluxions had never so acute, so learned, and so judicious a defender as Maclaurin:—yet whoever consults it . . . finds the author speaking of 'causes and effects,' of 'the springs and principles of things,' and proposing to deduce the 'relation of quantities by comparing the powers which are conceived to generate them';—will be convinced that this could only happen from so able a mathematician having failed to seize the right principles." "If English mathematicians first adopted Newton's method from veneration to him, . . . they have since persevered in it (we may almost say) against conviction." The reviewer claims that the criticisms of D'Alembert, Torelli, and Landen have shown that the use of motion is unnecessary and unreal. We have given citations from Landen in an earlier chapter (see our §§ 202, 203). D'Alembert is quoted as saying fifty years previous:

"Introduire ici le mouvement, c'est y introduire une idée étrangère, et qui n'est point nécessaire à la démonstration : d'ailleurs on n'a pas d'idée bien nette de ce que c'est que la vîtesse d'un corps à chaque instant, lorsque cette vîtesse est variable. La vîtesse n'est rien de réel ; . . . c'est le rapport de l'espace au tems, lorsque la vîtesse est uniforme ; . . . Mais lorsque le mouvement est variable, ce n'est plus le rapport de l'espace au tems, c'est le rapport de la différentielle de l'espace à celle du tems ; rapport dont on ne peut donner d'idée nette, que par celle des *limites*. Ainsi, il faut nécessairement en revenir à cette dernière idée, pour donner une idée nette des *fluxions*."[1]

[1] Art. "Fluxion" in *Encyclopédie, ou Dictionnaire raisonné des sciences*, etc., t. 6, Paris, 1756.

221. The reviewer states that foreign mathematicians have written treatises in which motion is entirely excluded, "and in some of these treatises, the principles of the doctrine in question have been laid down with a considerable degree of evidence and exactness." The Residual Analysis of Landen rests on "a process purely algebraical: but the want of simplicity . . . is a very great objection to it." The reviewer is of the opinion that Euler and D'Alembert give "the most clear and precise notions of the principles on which the differential calculus is established." He refers to Euler's *Institutiones calculi differentialis*, 1755. D'Alembert, says the reviewer, "observes that the method is really founded on that of prime and ultimate ratios, or of limits, which latter method is only an algebraical translation of the former; that, in fact, there are no such things as infinitely small quantities; and that, when such quantities are mentioned, it is by the adoption of a concise mode of speech for the purpose of simplifying and abridging the reasoning;—that the true object of consideration is the limit of the ratio of the finite differences of quantities."

The reviewer continues: "The explanations given by Euler and D'Alembert, beyond all doubt, deserve much consideration, yet their method of considering the doctrine of fluxions is not completely satisfactory, but is objectionable on two grounds: first, that we have no clear and precise notion of the ratio of quantities, when those quantities are in their vanishing state, or cease to be quantities;

secondly, the connection and natural order of the sciences are interrupted, if we give a distinct and independent origin to that which in fact, is a branch of analysis derived from the same common stock, whence all the other branches are deduced." Then follows a sympathetic account of the foundations for the calculus laid by Lagrange in his *Théorie des fonctions analytiques*, 1798. In passing, the reviewer remarks that "Emerson, Stone, Simpson, Waring, etc., have published treatises on fluxions; in none of which, however, are the principles clearly laid down."

Review of a Memoir of Stöckler, 1799

222. In the same journal[1] there is a review of a memoir on fluxions written by the Portuguese mathematician, Garçao Stockler, who modifies the explanation of fundamentals by the introduction of a "hypothetical fluxion" (a uniform velocity that generates a quantity equal to the real increment generated during the actually variable motion), which is always contained between the proper fluxions at the first and second instant under consideration. By diminishing the interval of time, the hypothetical fluxion approaches the true fluxion more nearly than by any assignable quantity. Here also, the real object of consideration is the *limit*. The reviewer argues that the fundamental principles are not new, and that the objections to Newton's fluxions apply equally to those of Stockler. In a reply to

[1] *Monthly Review*, vol. xxviii, London, 1799, p. 571.

the *Monthly Review*, Stockler denies the reviewer's allegation that he [Stockler] supposed quantity to be generated by motion. "The idea of motion, and the idea of velocity, are too particular to be admitted into a general theory of fluent quantities."[1]

Review of Lacroix's "Calcul différentiel," 1800

223. A review of S. F. La Croix's *Traité du calcul différentiel*[2] served as the occasion of further comments and criticisms of fundamental concepts:

"Who would direct his ridicule against the refinements, subtleties, and trifling of the schoolmen, if he read what has been written by some men who were presumed to be the greatest masters of reason, and whose employment and peculiar privilege consisted in deducing truth by the justest inferences from the most evident principles? The history of the differential calculus, indeed, shows that even mathematicians sometimes bend to authority and a name, are influenced by other motives than a love of truth, and occasionally use (like other men) false metaphysics and false logic. No one can doubt this, who reads the controversial writings to which the invention of fluxions gave rise: he will there find most exquisite reasonings concerning quantities which survived their grave, and, when they ceased to exist, did not cease to operate; concerning an infinite derivation of velocities,—and a progeny of

[1] *Monthly Review*, vol. xxxii, p. 497.
[2] *Monthly Review*, vol. xxxi, London, 1800, p. 493.

infinitesimals smaller than the 'moonshine's wat'ry beams,' and more numerous than

> 'Autumnal leaves that strow the brooks
> In Vallombrosa.' (Milton, *Par. Lost*, i, 302.)

"The contemporaries and partizans of Newton were men infinitely inferior to him in genius, but they had zeal, and were resolved to defend his opinions and judgments. Hence they undertook the vindication of fluxions, according to the principles and method of its author; although it may be fairly inferred, from the different explanations given of that doctrine by Newton in different parts of his works, that Newton himself was not perfectly satisfied of the stability of the grounds on which he had established it."

The reviewer quotes (p. 497) from Lacroix's preface:

"These notions [velocities, motions], although rigorous, are foreign to geometry, and their application is difficult. . . . Properly speaking, fluxions were to him [Newton] only a means of giving a sensible existence to the quantities on which he operated. The advantage of the method of fluxions over the differential calculus in point of metaphysics, consists in this; that, fluxions being finite quantities, their moments are only infinitely small quantities of the first order, and their fluxions are finite; by these means, the consideration of infinitely small quantities of superior orders is avoided. . . . I can only mention a method which Landen gave in 1758, to avoid consideration of infinity of motions, or of fluxions, since it rests on a very elegant algebraic theorem which cannot be given in a work of this nature. The freedom with which Landen divests himself of national prejudice stamps a remarkable character on his work; he is perhaps the only English mathematician, who has acknowledged the inconvenience of the method of fluxions." . . .

"We can always descend from the function to the differential coefficient or from the primitive function to the derived function: but, generally speaking, the reverse step is attended with the greatest difficulty."

"The rivals of Newton thought and invented for themselves; had they been influenced by his authority, and devoted their talents to the perfection of synthesis, science must have been considerably retarded. To the improvement of the algebraical analysis, is to be attributed the amazing advances of physical astronomy."[1]

Review of Carnot's "Réflexions," 1801

224. In the *Monthly Review*[2] (London) for 1801 there is a short and unimportant account of Lazare N. M. Carnot's new book, *Réflexions sur la métaphysique du calcul infinitésimal*, 1797. Carnot explains the correctness of results obtained by the infinitesimal calculus of Leibniz on the theory of compensation of errors—a theory which had been

[1] *Monthly Review*, vol. xxxii, p. 491.
[2] *Monthly Review*, vol. xxxiv, 1801, p. 463.

advanced much earlier by Berkeley in his *Analyst.* Mr Philip E. B. Jourdain has found clear indications of this theory in Maclaurin's *Fluxions* and in Lagrange's *Théorie des fonctions analytiques.* The method of limits is explained by Carnot in the manner of D'Alembert. "Of fluxions, indeed," says the reviewer, "as founded on the strange basis of velocity, there is no account."

Robert Woodhouse, 1803

225. In 1803, Robert Woodhouse published his *Principles of Analytical Calculation.*[1] Woodhouse had graduated B.A. at Caius College, Cambridge, in 1795, as senior wrangler. He then held a scholarship and a fellowship at Caius College, devoting himself to mathematics. He has the distinction of being the first to strongly encourage the study in England of the mathematical analysis which had been created on the Continent by Swiss and French mathematicians. In his *Principles of Analytical Calculation* he discussed the methods of infinitesimals and limits, and Lagrange's theory of function, pointing out the merits and defects of each. "By thus exposing the unsoundness of some of the Continental methods, he rendered his general support of the system far more weighty than if he had appeared to embrace it as a blind partisan."[2]

226. The ideas set forth in this book are, on the

[1] *The Principles of Analytical Calculation*, by Robert Woodhouse, A.M., F.R.S. Cambridge, 1803.

[2] Art. "Woodhouse, Robert," in Sidney Lee's *Dictionary of National Biography.*

whole, in such close agreement with those advanced in the preceding reviews, that the query naturally arises, whether Woodhouse is not the author of those reviews. We have reached no final decision on this point.

In the preface Woodhouse passes in review the different methods of establishing the foundations of the calculus. He criticises the use of motion in the proof of the binomial and other related theorems. "It required no great sagacity to perceive, that a principle of motion, introduced to regulate processes purely algebraical, was a foreign principle." If the binomial theorem and related theorems for the development of a function be established by algebra, independently of motion, then "from the second term of this expansion, the fluxion or differential of a quantity may be immediately deduced, and in a particular application, it appears to represent the velocity of a body in a motion. The fluxionists pursue a method totally the reverse; they lay down a principle of motion as the basis of their calculus, thence deduce some of the first processes, and establish the binomial theorem, by which it is said, the extraction of roots may be effected. . . . The project of extracting the square and cube roots of algebraical quantities by a principle of motion, is surely revolting to the common sense."

"Of his own method, Newton left no satisfactory explanation: those who attempted to explain it, according to what they thought the notions of its author, and . . . by reasoning which fairly may be called tedious and prolix. Of the commentators on the method of fluxions, Maclaurin is to be esteemed most acute and judicious, but his Introduction exhibits rather the exertions of a great genius struggling with difficulties, than a clear and distinct account of the subject he was discussing." To remove this prolixity, it was proposed, conformably to the notions of Newton, to call in the doctrine of prime and ultimate ratios or of limits. Euler and D'Alembert, on the other hand, rejected motion, but retained limiting ratios, failing, however, in supplying a satisfactory explanation therefor. Woodhouse is the earliest English mathematician who speaks in respectful and appreciative terms of the services to mathematics rendered by Bishop Berkeley. In fact, Woodhouse admits as valid some of Berkeley's objections which had been declared invalid. The methods of treating the calculus "all are equally liable to the objection of Berkeley, concerning the fallacia suppositionis, or the *shifting of the hypothesis.*" Thus, in fluxions and the method of limits, x is increased by i, and, in the case of x^m, the increment of the function, divided by i, is $mx^{m-1}+\frac{m(m-1)}{z}x^{m-2}i+$, etc.; then, putting, $i=0$, there results mx^{m-1}. But since the expansion of $(x+i)^m$ was effected "on the express supposition, that i is some quantity, if you take $i=0$, the hypothesis is, as Berkeley says, shifted, and there is a manifest sophism in the process" (p. xii).

227. As another objection to limits, or prime and ultimate ratios, Woodhouse declares that "the method is not perspicuous, inasmuch as it considers quantities in the state, in which they cease to be quantities."

Moreover, "the definition of a limit, is neither simple nor concise" (p. xvii). "The name of Berkeley has occurred more than once in the preceding pages: and I cannot quit this part of my subject without commending the *Analyst* and the subsequent pieces, as forming the most satisfactory controversial discussion in pure science, that ever yet appeared: into what perfection of perspicuity and of logical precision, the doctrine of fluxions may be advanced, is no subject of consideration: But, view the doctrine as Berkeley found it, and its defects in metaphysics and logic are clearly made out. If, for the purpose of habituating the mind to just reasoning . . . I were to recommend a book, it should be the *Analyst.*" "The most diffuse and celebrated antagonists of Berkeley, are Maclaurin and Robins, men of great knowledge and sagacity: but the prolixity of their reasonings confirms the notion, that the method they defend is an incommodious one."

"Landen, I believe, first considered and proposed to treat the fluxionary calculus merely as a branch of Algebra: After him, M. Lagrange, a name ever to be celebrated, in the Berlin Acts for 1772, laid down its analytical principles; and subsequently in his *Théorie des fonctions analytiques*, 1796, he has resumed the subject: in this treatise, the author expressly proposes, to lay down the principles of the differential calculus, independently of all consideration of infinitely small, or vanishing quantities, of limits, or of fluxions" (p. xviii). While Woodhouse considers Lagrange's discussion as very valuable, he does not find it free from logical faults.

William Hales, 1804

228. As a protest against the new movement and a vindication of Newton from the attacks upon fluxions in the *Monthly Review*, William Hales prepared a book, the *Analysis Fluxionum*, which was published in Maseres' *Scriptores Logarithmici*, vol. v, London, 1804. Hales endeavours to show that the doctrine of prime and ultimate ratios is really the same as the doctrine of the limits of the ratios. Hales's fundamental definitions are:

"Rationes ultimæ sunt limites, ad quos quantitatum sine fine decrescentium rationes, 1, semper appropinquant; et, 2, quas propiùs assequi possunt quàm pro datâ quâvis differentiâ; 3, nunquam verò transgredi; 4, nec priùs attingere, quam quantitates ipsæ diminuuntur in infinitum."

"Momentum est fluentis augmentum aut *decrementum momentaneum*; id est, tempore quam minimo genitum. Estque fluxioni proportionale."

After Hales's work had gone to press, he became acquainted with Benjamin Robins's *Discourse* of 1735, and published in appendices[1] numerous extracts from

[1] Maseres, *Scriptores Logarithmici*, vol. v, pp. 848, 854, 856.

it. Says Hales: "It is far superior indeed to the subsequent explanations of professed commentators; and it is a high gratification to myself to find, that the mode of explanation, which I adopted of the Doctrine of Limits, is precisely the same as Robins's; long before I had seen his admirable treatise, which did not fall into my hands until lately, a considerable time after the publication of the *Analysis Fluxionum.*" Maseres calls the *Discourse* of Robins "the ablest tract that has ever been published on the subject." Hales's text and the appendices to it contain considerable historical material, consisting mainly of references to and quotations from earlier writings. In view of the testimony of Laplace, Legendre, and Lacroix on the superiority of the method of fluxions, "how was it possible," asks Hales, that the eyes of the Monthly Reviewers "could still be so holden . . . as still to assert, that Newton himself was not perfectly satisfied of the stability of the ground on which he had established his Method of Fluxions!" Hales's motive in opposing Continental ideas was probably partly theological. D'Alembert, considered by him a hostile critic of Newton, is called "a philosophizing infidel," one "of the original conspirators against Christianity," "at once the glory and disgrace of the French Academy of Sciences," whose last words were "a terrific contrast to the death of the Christian Philosopher," Colin Maclaurin.[1]

The publication of Hales's Fluxions in large

[1] Maseres, *Scriptores Logarithmici*, vol. v, pp. 176-182.

quarto form and in the Latin language, the inclusion in the Appendix of matters foreign to the subject of the book, together with the attempt to maintain a system of notation and mode of exposition that was beginning to be considered provincial, caused the book to "fall still-born from the press."

Encyclopædia Britannica, 1810

229. In the fourth edition of the *Encyclopædia Britannica*, Edinburgh, 1810, the article "Fluxions" is wholly rewritten, and is much more extensive than the article in former editions. There is a lengthy historical introduction, and emphasis is placed upon work done on the Continent. It observes "that there is no work in the English language that exhibits a complete view of the theory of fluxions, with all the improvements that have been made upon it to the present time." Mention is made then of "several excellent works in the French language," mentioning Cousin, Bossut, La Croix, L'Huilier.

Letting u be "any function" of x, the *limit* of the ratio $(u'-u)/h$ is defined as "a quantity to which the ratio may approach nearer than by any assignable difference, but to which it cannot be considered as becoming absolutely equal." The article asserts that the method of fluxions "rests upon a principle purely analytical, namely the theory of limiting ratios; and this being the case, the subject may be treated as a branch of pure mathematics, without having occasion to introduce any ideas foreign to geometry. Sir Isaac Newton, however, in first delivering the principles of the method, thought proper to employ considerations drawn from the theory of motion. But he appears to have done this chiefly for the purpose of illustration, for he immediately has recourse to the theory of limiting ratios, and it has been the opinion of several mathematicians of great eminence (such as Lagrange, Cousin, La Croix, etc., abroad, and Landen in this country) that the consideration of motion was introduced into the method of fluxions at first without necessity, and that succeeding writers on the subject ought to have established the theory upon principles purely mathematical, independent of the ideas of time and velocity, both of which seem foreign to investigations relating to abstract quantity." "By the fluxions then of two variable quantities having any assigned relation to each other, we are in the following treatise always to be understood to mean *any indefinite quantities which have to each other the limiting ratio of their simultaneous increments* (we . . . mean the ratio of the *numerical* values of the increments, which may always be compared with each other, whether the variable quantities be of the same kind, as both lines, or both surfaces, etc., or of different kinds, as the one a line, and the other a surface). The Newtonian notation is used in the article exclusively."

Lacroix's "Elementary Treatise," 1816

230. The translation of Lacroix's *Elementary Treatise on the Differential and Integral Calculus*[1] in 1816 marks an important period of transition.

From the "Advertisement" we quote:

This work of Lacroix "may be considered as an abridgement of his great work on the Differential and Integral Calculus, although in the demonstration of the first principles, he has substituted the method of limits of D'Alembert, in the place of the more correct and natural method of Lagrange, which was adopted in the former. The first part of this Treatise, which is devoted to the exposition of the principles of the Differential Calculus, was translated by Mr. Babbage. The translation of the second part, which treats of the Integral Calculus, was executed by Mr. G. Peacock, of Trinity College, and by Mr. Herschel, of St. John's College, in nearly equal proportions."

On p. 2 the process of differentiation of $u=ax^2$ is explained, so that $2ax$ "is the limit" of the ratio $(u'-u)/h$, or it is "the value towards which this ratio tends in proportion as the quantity h diminishes, and to which it may approach as near as we choose to make it."

Thus Lacroix's definition, like D'Alembert's, does not prohibit the limit to be reached. In Note A, added by the translators, we read:

[1] *An Elementary Treatise on the Differential and Integral Calculus.* By S. F. Lacroix. Translated from the French. Cambridge. 1816.

"A limit, according to the notions of the ancients, is some fixed quantity, to which another of variable magnitude can never become equal, though in the course of its variation it may approach nearer to it than any difference that can be assigned." Thus, the method of limits is here ascribed by the translators to the ancients, which is an act of reading into the ancient expositions a theory not actually there. The ancient "Method of Exhaustions" is merely a prelude to the theory of limits. Peacock gives in Note A a history of the theory of limits, in which researches on the Continent are dwelled upon and the contribution made by Newton is explained, but no reference is made to Jurin, Robins, and Maclaurin. In Note B Peacock states that the method used by Lacroix in this treatise "was first given by D'Alembert, in the *Encyclopédie*" article "Différentiel." Evidently Peacock was not altogether friendly toward this method, for in Note B he proceeds "directly to show in what manner this calculus may be established upon principles which are entirely independent of infinitesimals or limits," and then informs the reader "that we are indebted for the principal part of the contents of this note, to the Calcul des Fonctions of Lagrange and the large treatise by our author, on the Differential and Integral Calculus." Peacock proceeds to give an account of Lagrange's calculus of functions and of the method of fluxions. Attention is called to "the difficulty of denoting the operations of finding the different orders of fluxions" according to the Newtonian notation, "when for *u* we put the function itself, which it represents."

231. The attitude of some British mathematicians of the early part of the nineteenth century toward the discussions of the fundamental concepts of the calculus carried on during the eighteenth century is exhibited in the following passage from John Leslie's *Dissertation* on the progress of mathematical and physical science :[1]

"The notion of flowing quantities, . . . appears on the whole, very clear and satisfactory ; nor should the metaphysical objection of introducing ideas of motion into Geometry have much weight. Maclaurin was induced, however, by such cavelling, to devote half a volume to an able but superfluous discussion of this question. As a refinement on the ancient process of Exhaustions, the noted method of Prime and Ultimate Ratios . . . deserves the highest praise for accuracy of conception. It has been justly commended by D'Alembert, who expounded it copiously, and adapted it as the basis of the Higher Calculus. The same doctrine was likewise elucidated by our acute countryman Robins ; . . . Landen, one of those men so frequent in England whose talents surmount their narrow education, produced in 1758, a new form of the Fluxionary Calculus, under the title of Residual Analysis, which, though framed with little elegance,

[1] Dissertation Fourth, in the *Encyclopædia Britannica*, 7th ed., vol. i, 1842, pp. 600, 601.

may be deemed, on the whole, an improvement on the method of ultimate ratios."

Remarks

232. The first part of the nineteenth century marks a turning-point in the study and teaching of mathematics in Great Britain. Attention has been directed to the efforts of Woodhouse to introduce the higher analysis of the Bernoullis, Euler, Clairaut, and Lagrange. His efforts were strongly and ably seconded by three other young men at Cambridge, John Frederick William Herschel, Charles Babbage, and George Peacock, who used to breakfast together on Sunday mornings, and in 1812 founded the "Analytical Society at Cambridge," for the promotion, as Babbage humorously expressed it, of "the principles of pure D-ism in opposition to the *Dot*-age of the University." The translation into English of Lacroix's *Elementary Treatise* and the publication, in 1820, of *Examples* with their solutions, brought the more perfect notation of Leibniz and the refined analytical methods to the attention of young students of mathematics in England.[1]

[1] Before the nineteenth century, the use in England of the Leibnizian notation *dz* and *∫ydz* is exceedingly rare. In our § 54 we saw that about the beginning of the eighteenth century these symbols were used by John Craig in articles published in the London *Philosophical Transactions*. When criticising Euler, Benjamin Robins once used the Leibnizian notation; see our § 142. Mr. Philip E. B. Jourdain has brought to my attention the fact that the sign of integration ∫ occurs also in a book, entitled, *Second Volume of the Instructions given in the Drawing School established by the Dublin Society. . . . Under the Direction of Joseph Fenn, heretofore Professor of Philosophy in the University of Nantes. Dublin*, MDCCLXXII. De Morgan refers to this work in a letter to Hamilton. See Graves' *Life of Sir William Rowan Hamilton*, vol. iii, p. 488. See also our *Addenda*, p. 289.

As usually happens in reformations, so here, some meritorious features were discarded along with what was antiquated. William Hales, in 1804, referred to the much neglected *Discourse* of Benjamin Robins (1735), with its full and complete disavowal of infinitesimals and clear-cut, though narrow, conception of a limit. By a curious turn in the process of events, Robins was quite forgotten in England, and D'Alembert's definition was recommended and widely used in England. Now Robins and D'Alembert had the same conception of a limit; both held to the view that variables cannot reach their limits. However, there was one difference between the two men: Robins embodied this restriction in his definition of a limit; D'Alembert omitted it from his definition, but referred to it in his explanatory remarks. D'Alembert says:[1]

"On dit qu'une grandeur est la *limite* d'une autre grandeur, quand la seconde peut approcher de la première plus près que d'une grandeur donnée, si petite qu'on là puisse supposer, sans pourtant que la grandeur qui approche, puisse jamais surpasser la grandeur dont elle approche; ensorte que la différence d'une pareille quantité à la *limite* est absolument inassignable." Further on in the same article we read: "A proprement parler, la *limite* ne coïncide jamais, ou ne devient jamais égale à la quantité dont elle est la *limite*; mais celle-ci s'en

[1] Art. "Limite" in the *Encyclopédie, ou dictionnaire raissonné des Sciences des arts et des métiers, publié par M. Diderot, et M. D'Alembert.* Paris, 1754. See also the later edition of Geneva, 1772.

approche toujours de plus en plus, & peut en différer aussi peu qu'on voudra. . . . On dit que la somme d'une progression géométrique décroissante dont le premier terme est a & le second b, est $(a-b) / (aa)$; cette valeur n'est poit proprement la somme de la progression, c'est la *limite* de cette somme, c'est-à-dire la quantité dont elle peut approcher si près qu'on voudra, sans jamais y arriver exactement."

233. That even the best expositions of limits and the calculus that the Continent had to offer at that time were recognised in England to be imperfect, is shown by a passage in a letter which William Rowan Hamilton wrote in 1828 to his friend John T. Graves:[1]

"I have always been greatly dissatisfied with the phrases, if not the reasonings, of even very eminent analysts, on a variety of subjects. . . . An algebraist who should thus clear away the metaphysical stumbling-blocks that beset the entrance of analysis, without sacrificing those concise and powerful methods which constitute its essence and its value, would perform a useful work and deserve well of Science."

[1] *Life of Sir William Rowan Hamilton*, by Robert P. Graves, vol. i, 1882, p. 304.

CHAPTER XII

MERITS AND DEFECTS OF THE EIGHTEENTH-CENTURY BRITISH FLUXIONAL CONCEPTIONS

Merits

234. There are, perhaps, no intuitional conceptions available in the study of the calculus which are clearer and sharper than motion and velocity. There is, therefore, a certain advantage in approaching the first study of the differential calculus or of fluxions by the consideration of motion and velocity. Even in modern teaching of the elements to beginners, we cannot afford to ignore this advantage offered by the eighteenth-century British mode of treating the calculus.

A second point of merit lies in the abandonment of the use of infinitely little quantities. Not all English authors of the eighteenth century broke away from infinitesimals, but those who did were among the leaders: Robins, Maclaurin, Simpson, Vince, and a few others. The existence of infinitesimals (defined as infinitely small constants) was looked upon by philosophers and by many mathematicians as doubtful. Their subjective existence was hardly more probable than their objective existence. These mystic creations occupied a hypothetical twilight zone between finite quantity and no quantity. Their abandonment added to the clearness and logical rigour of mathematics. From the standpoint of rigour, the British treatment of the calculus was far in advance of the Continental. It is certainly remarkable that in Great Britain there was achieved in the eighteenth century, in the geometrical treatment of fluxions, that which was not achieved in the algebraical treatment until the nineteenth century; it was not until after the time of Weierstrass that infinitesimals were cast aside by many mathematical writers on the Continent.

235. There is a perversity in historic events exhibited in the fact that after infinitesimals had been largely expelled in the eighteenth century from Great Britain as undesirable, unreal, and mischief-making, they should in the nineteenth century be permitted to return again and to flourish for a time as never before. About 1816 the Leibnizian notation of the calculus and the vast treasures of mathematical analysis due to the Bernoullis, Euler, D'Alembert, Clairaut, Lagrange, Laplace, Legendre, and others, which were all expressed in that notation, found their way into England. This influx led to enrichment and advancement of mathematics in England, but also to a recrudescence—this return of the infinitely small. How thoroughly the infinitesimal invaded certain parts of British territory is seen in Price's large work on the *Infinitesimal Calculus*, a work which in many ways is most admirable and useful.

236. After the development of the theory of limits by the English mathematicians and by such Continental writers as D'Alembert and Lacroix, it would hardly seem necessary even for the sake of brevity to reintroduce the old-time infinitesimal which could be "dropped" whenever it was very small, yet stood in the way. But at all times, and particularly in the eighteenth and beginning of the nineteenth centuries, there have been mathematicians who cared little for the logical foundations of their science. Fascinated by the ease with which the calculus enabled them to dispose of difficult problems in the theory of curves, ordinary mechanics, and celestial motions, they felt more like poets, and held the sentiments toward logic that a distinguished bard entertained toward pure intellectualism when he contemplated the beauties of the rainbow :

> "Triumphal arch that fill'st the sky,
> When storms prepare to part,
> I ask not proud philosophy
> To teach me what thou art."

Defects

237. All the eighteenth-century expositions of the foundations of the calculus—even the British—are defective. Without attempting an historical treatment or a logical exposition of later developments, we desire to point out briefly what some of these defects were.

In the first place, the doctrine of fluxions was so closely associated with geometry, to the neglect of analysis, that, apparently, certain British writers held the view that fluxions were a branch of geometry. In the preface to the *Gentleman's Diary* of London, the new editor, Mr Wildbore, said at the commencement of his editorship in 1781, "the doctrine of fluxions depends on principles purely geometrical, as is very satisfactorily demonstrated by that incomparable geometer, the late Dr Robert Simson of Glasgow in his Opera posthuma."

In the second place, as pointed out by Landen and Woodhouse, there was an unnaturalness in founding the calculus upon the notions of motion and velocity. In a real way, these notions seem to apply only to a limited field in the applications of the calculus, namely, to dynamics. In other fields, motion and velocity are wholly foreign concepts which, if applicable at all, are so only in a figurative sense.

238. Newtonian writers lay great stress upon such conceptions as a line generated by the motion of a point, a surface generated by the motion of a line, and a solid generated by the motion of a surface. We have already referred to the pedagogical advantages of this view, in teaching beginners. But as a final logical foundation this view is inadequate. Not all continuous curves can be conceived as traceable by the motion of a point. An example frequently quoted, in discussions of this sort, is the curve

$$y = 0 \text{ for } x = 0, \ y = x \sin\frac{1}{x} \text{ for } x \neq 0.$$

Let us try to trace this curve by the motion of a point starting from the origin of co-ordinates. In which direction must the point move from the origin? To answer this question we differentiate, and find $dy / dx = \sin(1/x) - (1/x)\cos(1/x)$. At the origin we have $x=0$ and $y=0$. No value can be assigned to dy / dx, because $1/x$ has no meaning when $x=0$; moreover, the equation $y = x \sin(1/x)$ is expressly stated above to apply only when x is not zero. There is, therefore, no way of ascertaining the direction in which the point must depart from the origin. Perhaps we can do better if the moving point is started at another part of the curve. An attempt to plot the curve reveals the fact that it lies between two right lines, of which one makes with the x-axis an angle of $45°$, the other an angle of $-45°$. As the point moves along the curve toward the origin, the curve is found to oscillate with ever-increasing rapidity. When we try to determine the direction by which it jumps into the origin, we encounter the same difficulty as before. As long as x is finite, the direction of motion is determinable. But as soon as we try $x=0$, the determination is impossible. This conclusion must be accepted, in spite of the fact that the curve is continuous in all its parts, including the origin. This example illustrates the inadequacy of motion as a fundamental concept.

239. Difficulties are encountered in the notion of velocity. Is variable velocity an objective reality? Take a body falling from rest. We say that its

velocity is $ds / dt = gt$. At the end of the first second, the velocity is g. If we ask ourselves, How far does the body move with the velocity g? we must admit that no distance can be assigned. We cannot say that the body moves from a certain point to the point immediately beneath; there is no such point immediately beneath. For, as soon as we try to locate such a point, it occurs to us that we can imagine at least one point located *between* the two points under consideration. This intermediate point serves our purposes no better, for a fourth point located between it and the initial point is easily detected, and so on, without end. Thus it is seen that no distance, however small, can be assigned through which a body falls with a given velocity. We are thus compelled to reject variable velocity as a physical fact. What, then, is $ds / dt = gt$? Clearly it is merely a limit, a mathematical concept, useful in mathematical analysis, but without physical reality. To say that ds / dt represents the distance a body *would* fall in unit time after the instant indicated by t, is to assign it merely hypothetical meaning, destitute of concreteness. While these considerations in themselves may not debar the use of velocity as a mathematical concept upon which to build the calculus, they show that the concept is not as simple as it would seem to be at first approach.

The reader will have observed that in all discussion of limits during the eighteenth century the question of the *existence of a limit* of a convergent sequence was never raised; no proof was ever given that a limit actually exists. In this respect the treatment was purely intuitive.

240. Another defect in the British exposition of fluxions was in the use of the word "quantity." No definition of it was given, yet quantities were added, subtracted, multiplied, and divided. It is possible to treat *quantities* or *magnitudes* without the use of number. The fifth and tenth books of Euclid's *Elements* contain such treatment. We may speak of the ratio of one *magnitude* to another *magnitude*, or we may speak of the ratio of one *number* to another *number*. Which was meant in the treatment of fluxions? Straight lines were drawn and the ratios of parts of these lines were written down. What were these the ratios of? Were they the ratios of the line-segments themselves, or the ratios of the numbers measuring the lengths of these line-segments? No explicit answer to this was given. Our understanding of authors like Maclaurin, Rowe, and others is that in initial discussions such phrases as "fluxions of curvilineal figures," "fluxion of a rectangle," are used in a non-arithmetical sense; the idea is purely geometrical. When later the finding of the fluxions of terms in the equations of curves is taken up, the arithmetical or algebraical conception is predominant. Rarely does a writer speak of the difference between the two. Perhaps

> "His notions fitted things so well,
> That which was which he could not tell."

241. Analytical geometry practically identified geometry with arithmetic. It was tacitly assumed

that to every distance corresponds a number and to every number there corresponds a distance. Number was thus given a geometrical basis. This situation continued into the nineteenth century. This metrical view involved the entire theory of measurement, which assumed greater difficulties with the advent of the non-Euclidean geometries. The geometrical theory of number became less and less satisfactory as a logical foundation. Hence the attempts to construct purely arithmetical theories.[1]

A good share of those difficulties arose from irrational numbers, which could not be avoided in analytical geometry. This occurrence is not merely occasional; irrational ratios are at least as frequent as rational ones. What is an irrational number? How do we operate with irrational numbers? What constitutes the sum, difference, product or quotient, when irrational numbers are involved? No explicit answer was given to these questions. It was tacitly assumed without fear, that it is safe to operate with irrational numbers as if they were rational. But such assumptions are dangerous. They might lead to absurdities. Even if they do not, this matter demands attention when mathematical rigour is the aim.

242. Perhaps it may be worth while to recall to the reader's mind illustrations of the danger resulting from taking operations known to yield consistent

[1] For a historical account of the number concept and the founding of the theory of transfinite numbers during the nineteenth century, read Philip E. B. Jourdain's "Introduction" to Cantor's *Transfinite Numbers*, The Open Court Publishing Co., 1915.

results when a certain limited class of numbers is involved, and applying them to numbers of a more general class. Suppose a and b to be rational, positive numbers, not zero; we find, let us agree, consistent results in the operation $a+b$, $a-b$ when $a>b$, and $a\times b$, and $a\div b$. Let us now consider the class composed of rational numbers, both positive and negative; suppose, moreover, that we introduce 0 in order to give interpretation to the operation $a-a$. If in this extended class of numbers we admit the four operations $a+b$, $a-b$, $a\times b$, $a\div b$, trouble arises even after due consideration has been given to the negative numbers. There may arise the following well-known paradox. Let $a=b=1$, then $a^2-b^2=a-b$. Divide both sides of the last equation by $a-b$, and we have $a+b=1$, or $2=1$. Where is the difficulty? The answer is known to every schoolboy: We have used $a-b$, or 0, as a divisor; we have extended the operation of division to the larger class of numbers, and to zero, without first assuring ourselves that such an extension is possible in every case; division by zero is inadmissible.

243. A less familiar example is the following. Let us suppose that, for real exponents, it is established that $(A^x)^y = A^{xy}$. When we apply this process to imaginary exponents, trouble arises. Take the equation $e^{2m\pi i}=e^{2n\pi i}$, where m and n are distinct integers, $i=\sqrt{-1}$, $\pi=3\cdot14159\ldots$, and $e=2\cdot718\ldots$ That this equation holds is evident, for $e^{2m\pi i} = \cos 2m\pi + i \sin 2m\pi = \cos 2n\pi + i \sin 2n\pi = e^{2n\pi i}$. If

both sides of $e^{2m\pi i} = e^{2 \iota\pi i}$ are raised to the power i / 2, we obtain $e^{-m\pi} = e^{-n\pi}$. Here all the letters stand for real numbers; since m and n are not equal to each other, this last equation is an absurdity. The assumption that a rule of operation valid for real exponents was valid also for imaginary exponents, has led to papable error.

Examples of this sort emphasise the need of caution when operations, known to be valid for a certain class of numbers, are applied to numbers belonging to a larger class. Special examination is necessary. These remarks are pertinent when operations applicable to rational numbers are extended to a class which embraces both rational and irrational numbers. What are the numbers called irrational? It is hardly sufficient to say that an irrational number is one which cannot be expressed as the ratio of two rational numbers. A negative definition of this sort does not even establish the existence of irrational numbers. Considerable attention has been paid to the definition of irrational numbers as limits of certain sequences of rational numbers. Thus, $\sqrt{2}$ may be looked upon as the limit of the sequence of rational fractions obtained by the ordinary process of root-extraction, namely, the sequence, 1, 1·4, 1·41, 1·414, 1·4142, . . . This attempt to establish a logical foundation for irrational numbers was not successful. We endeavour, in what follows, to make this matter plainer.

244. Let us agree that in building up an arith-

metical theory we have reached a development of rational numbers (integers and rational fractions). We wish, next, to define *limit* and also *irrational number.* An early nineteenth-century definition of limit was: "When the successive values attributed to a variable approach a fixed value indefinitely so as to end by differing from it as little as is wished, this fixed value is called the limit of all the others." Since, according to our supposition, we are still in the field of rational numbers, this limit, unless it happens to involve only rational numbers and to be itself only a rational number, is, in our case, non-existent and fictitious.

If now, as stated above, an irrational number is defined as the limit of certain sequences of rational fractions, trouble arises. The existence of such a limit is often far from evident. But aside from that general consideration, the difficulty of the situation in our case is apparent: Irrational numbers are limits, but limits themselves are non-existent or fictitious, unless they are rational numbers. To avoid this breakdown in the logical development, it was found desirable to define irrational number without using limits.

245. With the view of avoiding the use of limits in the definition of irrational number, and at the same time avoid inelegant and difficult assumptions, involving complicated considerations relating to the nature of space,[1] devices were invented by several

[1] On this point consult the article "Geometry" in the *Encyclopædia Britannica*, 11th edition, the part on Congruence and Measurement.

logicians independently, which freed the number concept from magnitude and established number theory on the concept of order. Chief among the workers in this field were Méray, Weierstrass, Dedekind, and Georg Cantor. It is to them that we owe representations of number, both rational and irrational, which have yielded a much more satisfactory theory of limits, and in that way have vastly improved the logical exposition of the differential calculus. These theories have brought about the last stages of what is called the *arithmetisation* of mathematics. As now developed in books which aim at extreme rigour, the notion of a limit makes no reference to quantity and is a purely ordinal notion. Of this mode of treatment the eighteenth century had never dreamed.

ADDENDA TO §§ 54, 58, 73

246. ADDITIONAL data on the fundamental conceptions relating to fluxions and on the use of the Newtonian and the Leibnizian notations in England during the lifetime of Newton are contained in George Cheyne's *Philosophical Principles of Religion*, Part II, London, 1716. Part I of this book appeared first in 1705. Like Berkeley's *Analyst*, which was written later, Cheyne's book, Part I and Part II, had for its primary purpose the refutation of atheism. Cheyne says in his preface to the third edition of Part I, "that Atheism, may be eternally confounded, by the most distant Approaches to the true Causes of natural Appearances. And that if the Modern Philosophy demonstrates nothing else, yet it infallibly proves Atheism to be the most gross Ignorance."

Part II of Cheyne's book consists of three chapters and of seven pages of "Additions." He says in his preface to this part that, excepting one short note, the third chapter and the "Additions" are "what the reverend and ingenious Mr. John Craig sent me about seven years ago, when I desired him . . . to write me down his Thoughts on, correct or alter, what I had formerly published on this Head in the first Edition of this Work."

Cheyne uses in Part II, p. 20, the notation $\dot{x}$ to denote a distance Bb when he supposes "b infinitely near to B." In § 58 we pointed out that in 1704 Cheyne wrote once $\dot{x} = 1$, but nowhere in the present book does $\dot{x}$ denote a finite quantity. He argues that $1 / \dot{x} = \infty 1$, that $1 / 0 = \infty$; hence that $\dot{x} = o$, or "relative nothing," which is "the least Part of the Finite, to which it is related or compared." On p. 21 he calls $\dot{x}$ "an infinitely little Part of x." On p. 12 he speaks of the "absolute infinite" as "admitting of neither Increase, nor Diminution, or of any Operation that mathematical Quantity is subjected to," while (p. 13) "absolute nothing" is "neither capable of increasing nor diminishing, nor of any wise altering any Mathematical Quantity to which it is apply'd, but stands in full opposition to absolute Infinite." On the other hand, "indefinite" or "relative infinite" quantities (p. 29) "are not properly either Finite or Infinite, but between both." The "relative nothing" (p. 8) "is an infinitely little Quantity, as it stands related to a given Finite, by the perpetual Subtraction of which from it self it is generated. Let o stand for relative nothing. Thus $o1$ is a *relative infinitely little* Quantity, as it stands related to Unity, by the perpetual Subtraction of which from it self, it is generated; that is $o1 = 1 - 1 + 1 - 1 + 1 - 1 + 1 - 1$ ec., and oa is an infinitely little Quantity, as it stands related to the given Finite a, by the perpetual Subtraction of which from it self, it is generated; that is $oa = a - a + a - a + a - a$ ec." In the "Advertizement" following p. 190 this is further explained thus: "Relative Nothing is said here to be generated by a perpetual Subtraction, tho' the Signs by alternately + and −. For these Reasons, because relative Infinite, was said to be generated by a perpetual Addition, and because that after the first Term, every two succeeding ones in relative Nothing 1 is equivalent to $o1$ thus $1 - 1 + 1 - 1 + 1 - 1$, &c. . . . $= 1 - o1 - o1 - o1$ &c."

247. These explanations are intended by Cheyne merely as introductions to the later chapters, particularly that by John Craig, who (p. 167) declares that o cannot be an absolute nothing, "for an infinite Number of absolute Nothings cannot make 1, but by o is understood an infinitely small part, as in the *calc. diff.* dx is an infinitely small part of x, so that dx is as o to x: Not that dx is absolutely nothing, for it is divisible into an infinite Number of Parts, each of which is ddx." To make the point still plainer, John Craig continues (p. 168): "But then it may be inquir'd what is the Quotient that arises from the Division of 1 by absolute Nothing. I say there is no Quotient because there is no Division: Therefore it is a Mistake to say the Quotient is 1 or Unity undivided, which is demonstrably false, neither is the Quotient $= o$. For properly speaking there is no Quotient, and therefore it is an Error to assign any. In like manner, it is an Error to say, that $o \times a$ makes the Product

o; for properly speaking there is no Product. It is true, this of Multiplication has no influence upon Practice, but that of Division has. From hence it appears, that a Curve is said to meet with its Asymptote, when the Ordinate is infinitely little." Then follows a startling view which had been held about sixty years before by John Wallis in his *Arithmetica Infinitorum*, 1655,[1] but Craig makes no reference to him. Craig argues (p. 169): "This same Notion does explain how it comes to pass that 1 divided by a negative Number gives a Quotient greater than Infinite." Curiously, he represents the logarithmic curve $y = \log x$ as crossing the y-axis at $y = -\infty$, for since the curve approaches the y-axis infinitely near when positive x approaches zero, "we may conceive the Logarithmic Curve continued as intersecting" the y-axis, so as to form "one continued Curve." Accordingly negative numbers have logarithms that are real and negative. His further argument amounts to this: For values of x that are equal to 1 divided by a negative number, y in $y = \log x$ is negative and is less than its value $-\infty$ arising when $x = o$ (presumably in the sense that $-2 < -1$). "Ergo x is a Number greater than infinite." Considering the approach of the logarithmic curve towards its asymptote, Craig says (p. 170) that "here it is observable, that there are affirmative Numbers less than nothing denoted by the several Powers of dx, as dx^2, dx^3, ec., or by the second, third, ec. Differences, and these Numbers may be aptly

[1] Wallis, *Opera*, I, p. 409, Prop. CIV.

represented by the Ordinates of the logarithmic Curve," continued from $y = -\infty$ away from the origin when dx^n is affirmative, or towards the origin when dx^n is negative. In the "Additions," p. 185, Craig devotes six pages to "An Answer to Mr. Varignon's Reflections upon Spaces greater than infinite," in which Craig uses the Leibnizian symbol $\int$ five times, as in $\int: x^{-e}dx = \frac{x^{1-e}}{1-e}$. Nowhere in the book under consideration does Craig use the notation of Newton. The "Additions" are dated "September 23d, 1713."

248. George Cheyne was a pupil of the Scotch physician, Archibald Pitcairne (1652–1713), who is the author of two books on fluxions (which we have not seen), viz. *Fluxionum Methodus inversa; Sive Quantitatum Fluentium Leges generaliores. Ad celeberrimum virum, Archibaldum Pitcarnium, Medicum Edinburgensem*; and *Rudimentorum Methodi Fluxionum inversæ Specimina adversus Abr. de Moivre.* Pitcairne's mathematical bent more or less influenced his medical theories. He liked to ridicule others, and was himself ridiculed in a publication, *Apollo Mathematicus; or, the Art of curing Diseases by the Mathematicks, according to the Principles of Dr. Pitcairne*, 1695.

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