# THE
# MATHEMATICAL PAPERS OF
# ISAAC NEWTON
## VOLUME VII
## 1691-1695

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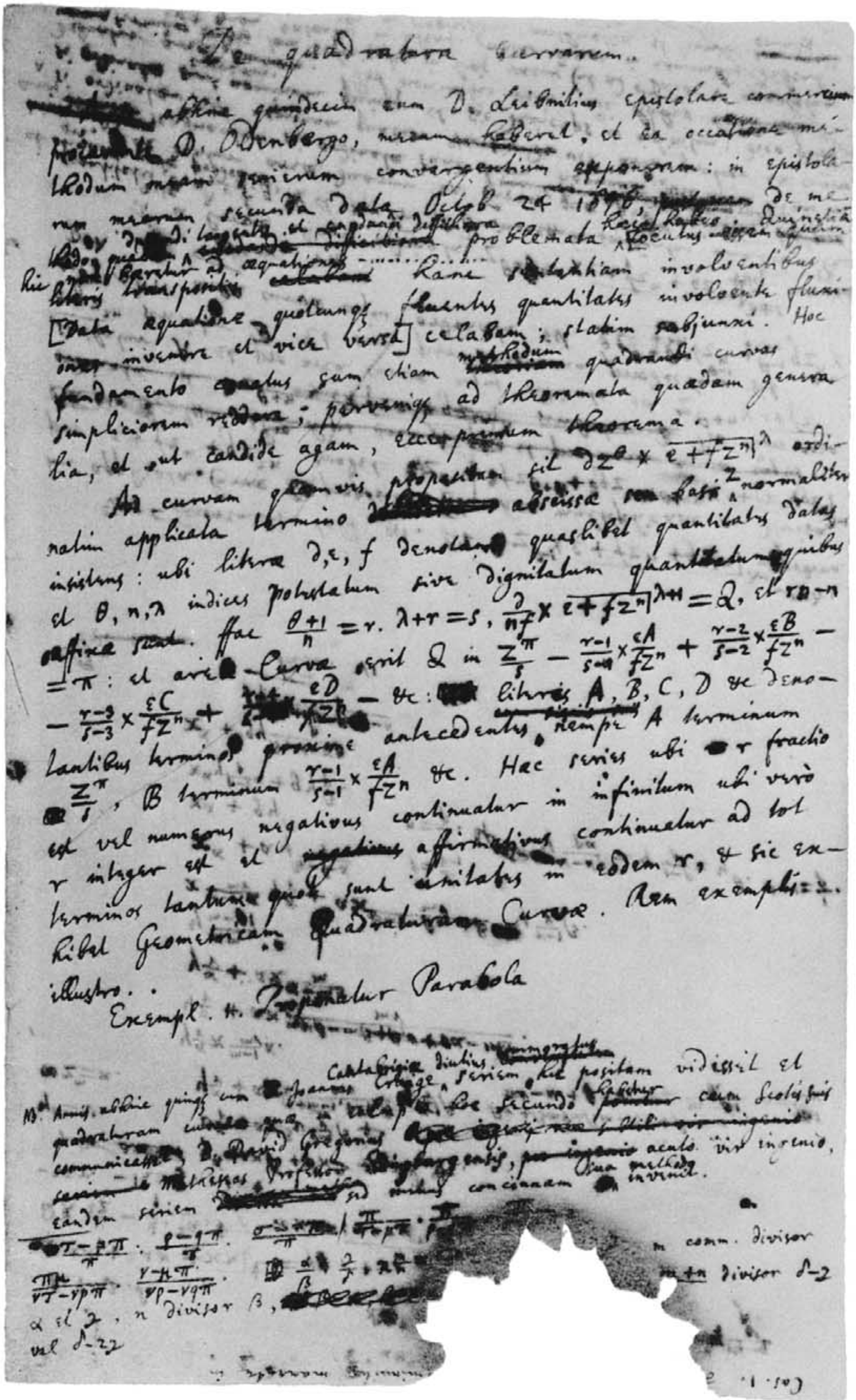

The opening page of Newton's 1691 tract 'De quadratura Curvarum' (**1**, §1).

 

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D. T. WHITESIDE

WITH THE ASSISTANCE IN PUBLICATION OF

A. PRAG

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*TO MICHAEL HOSKIN*
*FOR THE UNSTINTED WARMTH OF HIS*
*ENCOURAGEMENT OVER SO MANY YEARS*

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# PREFACE

The sixth volume of this edition of his extant mathematical papers closed with Newton, freshly returned to professorial routine in the sluggish backwater of Cambridge after a year's heady racing to and fro in the mainstream of political life in London, making a start on his self-imposed task of recasting and refining the text of the mighty *Principia* which he had given out publicly a little while before. In this sequel we draw as ever upon the vast wealth of his surviving worksheets and drafts chronologically to retrace his mathematical output over the four years from November 1691, terminating in the autumn of 1695 a few weeks before he took (in March the next year) his fateful decision to accept the Wardenship of the London Mint and so, after nearly thirty-five years, effectively sever his ties with a University which had intellectually mothered him and for so long quietly sheltered him from the harsh and earthy realities of the outside world.

These last years of his active tenure of the Lucasian chair at Cambridge were, I need not say, in large part passed in consolidating and retrenching familiar ground which Newton had long since gained, but they also saw fundamental advances into such novel terrains as those of the general Taylor expansion of an algebraic function and of the projective classification of curves, notably the component species of the cubic whose earlier equivalent Cartesian enumeration is here, too, given its final polish; and above all they reveal him taking a hard look back at the higher geometry of the 'ancients' as its traces have endured in the works of its exponents Euclid, Archimedes and Apollonius and of their 'synthesizer' Pappus. While those portions of Newton's present writings on the quadrature of curves and the cataloguing of cubics which were afterwards appended by him, as his 'Tractatus De Quadratura Curvarum' and 'Enumeratio Linearum Tertii Ordinis', to the *princeps* edition of his *Opticks* in 1704 will be well known in their essence to the *cognoscenti*, the hitherto unpublished later propositions of his 1691 'De quadratura' and the massive bulk of his ensuing geometrical researches, whose very existence—not to mention their intrinsic excellence—has gone publicly all but unrecorded in the nearly three centuries since Newton penned their script, will here come mint-fresh to all but the favoured few (if indeed there are any) who have previously gained access to the totality of his jumbled mathematical *Nachlass*. The other small pieces, on conchoidal cubics and foliate quartics and on a variety of contrivances *ad hoc* of rules for interpolation and approximate quadrature, which fill the remaining pages are not of momentous importance, but hold some surprises for those interested to pursue their detail. Let it be enough that the autograph manuscripts now reproduced are no mere resurrected historical curiosities fit only once more to gather dust in some forgotten corner, but will require the rewriting of more than one page in the historical textbook.

As ever, for permission to print the texts of papers in their custody, I stand principally indebted to the Librarian and Syndics of the University Library, Cambridge, but my gratitude is again owed to the same private owner who has, desiring only that his anonymity be preserved, once more allowed me freely to publish the complementary folios in his possession which exactly fill the main gaps in the former's collection of Newton's mathematical papers (and which were removed therefrom in Newton's own day by William Jones). While the University of Cambridge has now increasingly taken over the burden of financing the long hours of carrying out preparatory research, collating manuscript and printed material, and producing the finished editings of texts which are here presented, I must continue to acknowledge the monetary support provided, now as for more than ten years past, by the Sloan Foundation of Philadelphia, the Leverhulme Trust and the Master and Fellows of Newton's own Trinity College, Cambridge. What can I find to say in communally thanking them all which time has not unworthily stiffened into cliché? Perhaps that a monument such as this to an intellectual giant of the past is not raised on the shoulders, however broad in the flesh or willing in the spirit, of its editor alone.

It will be seen that my accredited coadjutors are reduced to be but one. This is, as always, to ignore the smaller efforts taken by others in affording me piecemeal factual information and corrections of minor detail: this volume, in particular, wears a kinder and more balanced face to David Gregory through the gentle persistence of Miss Christina Eagles. Adolf Prag, who remains to bedeck the title-page, has long since earned my supreme accolade of being accepted as ever-available and near-omniscient helpmate and prime critic in guiding my volumes through into their published form; so many of their finer nuances of mathematical, verbal and historical detail are his. To my other past helper Michael Hoskin, who has vanished from the title to be properly (I may hope) and worthily reincarnated in my dedication of this volume to him, I convey my warmest thanks for all he has done for me, as mentor and friend, these last twenty years.

To the Syndics of the Cambridge University Press, lastly, let me express my acknowledgement of all the hard work and expertise expended and applied by the staff of the Publishing and Printing Divisions in clarifying and beautifying the disorders and uglinesses of my submitted handwritten copy to be the object of surpassing printer's vertu which it has here become.

D.T.W.
*26 February 1976*

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# EDITORIAL NOTE

Once more we need say little to smooth the reader's way into a volume which in its style, layout and conventions closely models itself upon its predecessors. Our aim, as ever, is to be as faithful to the autograph papers which we reproduce as the confines imposed by the linearities of the printed page will permit. To fill out and clarify their meaning—such, at least, as we conceive it to be—we have again not fought shy of making editorial intercalation in Newton's often roughly, hastily drafted and ill-organized texts; particularly so when we present for modern inspection our versions of what in the original are mere calculations, set out without verbal connectives to indicate the sense and logical sequence which Newton himself understood. To the same end we have trivially liberated his words from grammatical illogicalities of gender, case-ending, plurality and mood; but at the same time have taken care to preserve all the significant idiosyncrasies, contractions, superscripts and archaic spellings which, if nothing more, add an authentic touch of the period to the clean, straight and marginally adjusted lines of type which are here the modern facsimiles of Newton's ink-blobbed, much-cancelled and often rudely scrawled manuscripts. Within the proprieties of modern idiom we have kept our facing English renderings of the principal Latin texts deliberately literal, designing our phrases to be but a prop to the fuller understanding of Newton's Latin words, rather than to be read in their own right as polished paraphrases of his intended meaning. In the case of secondary texts of minor importance, we have not hesitated to set these in the many untranslated appendices which here appear in yet greater profusion than before, along with the substance of those cancelled passages which are too long or complicated to attach in a pertinent footnote to their parent text. (As with anyone else, we may add in justification, the older Newton came to be, the more 'wasteful' are his preserved papers in their relative bulk. Is it sacrilegious to suggest that there is no point in making full and exact reproduction of every last one of his increasingly numerous and individually often minimally variant extant preliminary worksheets for, and posterior revises of, an item which itself is of but minor importance?) In both Latin original and our parallel English rendering *en face* a twin vertical rule in the margin alongside remains our signal that the line or passage so distinguished has been cancelled by Newton in the manuscript. Other conventions and notations here used ape those employed in previous volumes: in particular '$\mathrm{fl}(R\hat{B})$' (see page 452, note (21) below) is set by us to denote the 'fl[uxio]' (fluxion) of the arc subtended by the angle $\hat{B}$ in a circle of radius $R$, and the same is done *mutatis mutandis* elsewhere. In the interests of economy we have slightly standardized the considerable variety of subtly (but not, we think, intentionally) varying sizes and shapes in which Newton portrayed his multi-dotted, multiply accented and boxed symbols; in so doing we have nowhere deviated from the printed equivalents which Newton himself chose in delivering these to the world in his 'Tractatus de Quadratura Curvarum' in 1704. May we therefore be forgiven, if not excused. We would finally remind that forwards and backwards reference within this volume is often made by the convention **2**, 3, §2' (by which understand '[Part] **2**, [Section] 3, [Subsection] 2'), while we point to citations in previous volumes by the code 'IV: 219–20' (understand '[Volume] IV: [pages] 219–20').

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# GENERAL INTRODUCTION

The backdrop to the four years of fresh researches and reshapings of past discoveries here reproduced has already been set in our previous volume.(1) In the autumn of 1691 where we take up this new episode in the continuing saga of his mathematical development Isaac Newton, now late in his forty-ninth year, lay intellectually still very much in the shadow of the magisterial *Philosophiæ Naturalis Principia Mathematica* which he had wrought—imperfectly as that may be—half a dozen years before, caged by its very brilliance and originality, unable to transcend its mental confines. In between times, after a year spent in the glare and noisy bustle of political and social life in London, he had returned gloomily in the mid-winter of early 1690 to the slow, dull pace of college and professorial life in Cambridge, rather helplessly to flit moth-like round the bright-burning candle of his master creation, now and then(2) fanning its flame to a yet purer blaze. He must have felt the deep sadness of realizing that whatever else it was still in him to do would not stand comparison with what he had achieved in the crowded months of his *biennium mirabilissimum* in the middle 1680's.

Though his year amid the dazzle and event of the metropolis had (we may see in hindsight) left him for evermore dissatisfied with the puny material rewards of dedicated scholarship and the petty round of day-to-day academic life, Newton had outwardly appeared to slip back smoothly enough into being once more the remote Lucasian Professor denied by statute(3) the opportunity to play an active part in the governing of the University or the teaching of his own Trinity College, the resident Cambridge expert in all things mathematical, scientific and indeed (with his fellow Lincolnshireman Henry More now dead) even theological.(4) In his little lean-to 'elaboratory' tucked under the wall of Trinity's chapel he took up again, where he had left them off in 1688, his unending train of smoky, bubbling chemical and metallurgical experiments(5)

(1) See VI: xxiii-xxiv.

(2) Compare the remodellings of the *Principia*'s 'Liber primus' at this time which are reproduced in VI: 538–608.

(3) As confirmed by Charles II on 18 January 1664; see III: xxvii.

(4) We have primarily in mind the lengthy open letters on 'notable corruptions' of Scriptural texts which Newton passed to John Locke in November 1690 (see *The Correspondence of Isaac Newton*, 3, 1961: 83–122 and 129–42).

(5) The loose sheets in ULC. Add. 3973.7–9 on which he penned the surviving record of these new experiments evidence his undaunted efforts on various dates between March 1691 and February 1696 to fuse new alloys of tin, copper, lead, bismuth and zinc, to attack these compounds with *aqua fortis* and other acids, and to sublimate them with antimony and *sal armoniacum* (on which see V: xiv, note (17)); compare G. D. Liveing's accurate and informative

—as ever to no real profit, it would seem. Though (so far as we know) he made no further deposit in the University Library of any of his new Lucasian lectures, and what their content may have been is anyone's educated guess, he presumably began again from time to time—if no longer with weekly frequency—to address his largely uncaring undergraduate audience from his professorial podium in the schools. After his previous experiences of so often reading thereon for 'want of Hearers' to 'y$^{e}$ Walls' of his lecture room(6) he did not, we may suppose, repeat his earlier mistake of trying to instil an undiluted version of his *Principia* into his listeners, whoever these might have been,(7) but aimed at the simpler goal of applying the principles of mechanics to the heavens with a lighter, more common touch.(8) To more senior acquaintances he showed himself helpful in smoothing their way into comprehending the fundamentals and basic lessons of his published *œuvre maîtresse*. When the young Richard Bentley, floundering badly under the weight of a long list of 'necessary' preparatory reading,(9) applied to Newton himself in late 1691 for

listing of their content in *A Catalogue of the Portsmouth Collection of Books and Papers written by or belonging to Sir Isaac Newton* (Cambridge, 1888): 19–20.

(6) As Humphrey Newton was long afterwards to observe to John Conduitt; see VI: xii, note (3).

(7) We have previously (again see VI: xxii, note (3)) cited William Whiston's testimony that it was with 'no Assistance' that he set himself 'with the utmost Zeal' to study the *Principia* in Cambridge at this time.

(8) There do exist in ULC. Add. 4005 several English fragments which may in some way or other have had to do, if not with Newton's public lectures, then at least with the private instruction in his chamber of students interested enough to seek him out there for which Lucasian statute also provided (see III: xxii–xxiii). These comprehended, notably, (ff. 23$^{r}$–24$^{r}$/25$^{r}$) an outline of 'The Elements of Mechanics' and 'The Mechanical Frame of the world'; and (ff. 21$^{r}$–22$^{r}$) drafts of the first chapter 'Of the Sun & fixt Starrs' and part of the second one 'Of the Earth & Planets' of a tract on 'Cosmography', which was seemingly broken off to be reordered (on ff. 45$^{r}$–50$^{r}$) as a similar account of fifteen astronomical 'Phænomena'. (The uncancelled portions of the texts of these are printed in A. R. and M. B. Hall, *Unpublished Scientific Papers of Isaac Newton* (Cambridge, 1962): 165–9, 374–7 and 378–85 respectively.) We may add that in a contemporary memorandum 'Of educating Youth in the Universities' (ULC. Add. 4005.5: 14$^{r}$–15$^{v}$, first published by W. W. Rouse Ball in *The Cambridge Review*, **31**, No. 763 [for 21 October 1909]: 29–30 [= *Cambridge Papers* (London, 1918): 244–5] as 'A Seventeenth-Century Flysheet', and more recently refurbished by A. R. and M. B. Hall, *Unpublished Papers:* 369–73) Newton laid down the *desiderata* which he felt an introduction to the elements of exact science should strive for, requiring (f. 14$^{r}$) 'The Mathematick Lecturer to read first some easy & useful practical things, then Euclid, Sphericks [solid geometry], the projections of the Sphere, the construction of Mapps, Trigonometry, Astronomy, Opticks, Musick, Algebra, &c. Also to examin &...instruct in the principles of Chronology & Geography'. (On the last compare IV: 10, note (28).)

(9) This list, written out in June 1691 by John Craige for William Wotton (acting on Bentley's behalf), is given in full by David Brewster in his *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton*, **1** (Edinburgh, 1855): 456–9. Substantial excerpts from it, more faithfully transcribed from the original (in Trinity College, Cambridge. R.16.38), are reproduced in *Correspondence*, **3**: 150–1.

a less formidably technical *entrée* to the book, he was rewarded with a much more concise programme of preliminary reading together with the sound advice that 'At y^e^ first perusal of my Book it's enough if you understand y^e^ Propositions w^th^ some of y^e^ Demonstrations w^ch^ are easier then the rest. For when you understand y^e^ easier they will afterwards give you light into y^e^ harder.'(10) And when the next year Bentley came to deliver the first series of commemorative Boyle lectures at St Martin's in London, taking as his theme the natural evidence for the existence of God, he again sought instruction from Newton and again was awarded it: 'When I wrote my treatise about our System', the latter returned on 10 December 1692, 'I had an eye upon such Principles as might work w^th^ considering men for the beleife of a Deity & nothing can rejoyce me more then to find it usefull for that purpose',(11) and over the two ensuing months he went on to elaborate in fine detail his views and beliefs regarding the formation of the visible world and its underlying structure.(12)

During these last four Cambridge years Newton stayed close to home, leaving his University town only on a few brief visits to his native Lincolnshire or to London. If we combine the complementary records of his college buttery accounts and (incomplete) list of signings out of and back into Trinity,(13) and interlard these with what else we can extract from his contemporary letters (few as these are) we may establish more precisely that he passed three weeks in London from the last day or so of December 1691;(14) a fortnight away each in early June and again in early July 1693, both seemingly in trips north to comfort his half-sister Hannah as her husband Robert Barton lay dying from

(10) Joseph Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes* (London, 1850): 274–5 [= *Correspondence of Isaac Newton*, **3**: 156]. Newton added that 'When you have read y^e^ first 60 pages, pass on to y^e^ 3^d^ Book & when you see the design of that you may turn back to such Propositions as you shall have a desire to know, or peruse the whole in order if you think fit'.

(11) *Correspondence*, **3**: 233. We have already cited in I: 9, note (24) Newton's equally often quoted following sentence: 'But if I have done y^e^ publick any service this way 'tis due to nothing but industry & a patient thought.'

(12) *Correspondence*, **3**: 233–40, 244, 246–56; see also A. Koyré, *From the Closed World to the Infinite Universe* (Baltimore, 1957): 179–89 and his 'Newton, Galilée et Platon', *Actes du IX^e^ Congrès International d'Histoire des Sciences* (Barcelona/Madrid, 1959): 165–87 [Englished as 'Newton, Galileo and Plato' in the gathering of his *Newtonian Studies* (London, 1965): 201–20]. We have already sketched in VI: 56–7, note (73) the dynamical substratum of the discerning *critique* of Galileo's 'Platonic' conjecture as to the formation of the solar system which Newton set down for Bentley in his two last letters of 17 January and 25 February 1692/3 (*Correspondence*, **3**: 240, 255–6).

(13) These are collected (from the scattered originals in Trinity College's Muniments Room) on pages lxxxix/xc and lxxxv respectively of Edleston's *Correspondence of Sir Isaac Newton and Professor Cotes* (note (10) above).

(14) See our more detailed comment on this London visit on page 11 below.

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some wasting disease at Brigstock in Northamptonshire,(15) and a further fortnight in London in mid-September following; visited London again briefly at the beginning of September 1694;(16) and made two separate trips to Lincolnshire in September 1695.(17) Except for the troubled late summer of 1693 (to which we shall return in a moment), during these years Newton's life went calmly and uneventfully on, unafflicted by serious illness and disturbed only by the rare arrival of a visitor to talk over his work or an occasional flurry of correspondence. His stay in London in January 1692, probably inspired in the main by his determination to reveal to David Gregory just how far his pretensions to the 'prime' theorem on series-quadrature had been outdistanced by his own newly composed (and yet uncompleted) treatise on the quadrature of curves,(18) led *inter alia* to his renewing personal contact with the Swiss mystic and mathematician Fatio de Duillier,(19) and it is to the latter's vivid ensuing accounts of what in manuscript he was then shown that we know externally so much of Newton's mathematical interests at this time.(20) After Fatio visited Cambridge early the following November their intimacy

(15) In subsequently writing to 'Camebrig' on 24 August seeking Newton's 'advise' on the future settlement of his estate, Hannah observed that 'My Dear Husband ever since his return to Brigstock has been very ill....I find noe hopes of Cure but that hee lossis his flesh and strength very fast' (*Correspondence*, **3**: 278).

(16) See note (56) below.

(17) 'I am newly returned from a journey I lately took into Lincolnshire & am going another journey', Newton wrote to John Flamsteed on 14 September (*Correspondence*, **4**, 1967: 169), adding that he would have no 'time to think of y$^e$ theory of the Moon...this month or above'. The only hint we can trace of the pressing business which twice within a couple of weeks took him north from Cambridge is a letter from his half-brother Benjamin Smith in Colsterworth which, in acknowledging the success of a 'plaster' supplied by Newton to ease the pregnancy pains of Benjamin's wife, spoke 'our thankes for all your trouble and cost' (*ibid.*: 187).

(18) See pages 7–11 below for the context of this. The two principal versions of the treatise 'De quadratura Curvarum' which Newton put together over the early winter of 1691/2 are reproduced in **1**, §§1/2 following.

(19) See VI: xxiii, note (46). In amendment of what we there too vaguely adduced, Newton had met Fatio at least as early as 12 June 1689 when both attended the meeting of the Royal Society at which (see *ibid.*: note (45)) Christiaan Huygens, then on a visit to London, gave an account of his forthcoming *Traité de la Lumière*. Their names are found several times together in the Society's Journal Book over the ensuing months (compare *Correspondence*, **3**: 69, note (1)). In late August following, both Fatio and Huygens (as the latter recorded in his private diary) travelled by barge up the Thames with Newton to hear him plead his case before William for varying the statutes of King's College so that he might be made its Provost (on the failure of which attempted preferment see VI: xxiv, note (48)).

(20) Compare pages 12–13 below. When Newton came to incorporate in his 'De quadratura Curvarum' (as Case 4 of its Proposition XI) a rule whereby, in certain simple cases, a given first-order fluxional equation may be reduced to an exactly quadrable form by means of an appropriate multiplying factor, he paid Fatio the rare honour of explicitly crediting him with its invention (see page 78, note (68) following).

grew apace, despite Fatio's long-lasting moan thereafter about 'a grievous cold, which is fallen upon my lungs' contracted by him on his journey back to London:[21] Newton revealed quite remarkable restraint in pandering to Fatio's hypochondria, prescribing a variety of 'Imperial powders' to cure his malady,[22] and early in 1693 warmed to Fatio's thoughts of continuing to reside in England 'some years, chiefly at Cambridge',[23] but he remained ever cool regarding his *protégé*'s extremist views on the subject of the 'biblical prophesys'.[24] From our present viewpoint it is a pity that what there is of scientific interest in their considerable correspondence with each other at this time has to do with alchemical matters.[25] When in the spring of 1693 Fatio left London for Switzerland to claim a family estate 'such...as will keep me as long as I live, provided I go there again',[26] his close relationship with Newton lapsed and upon his return to England a year or so afterwards, there to spend the remainder of his long life, was resumed only in infrequent, casual encounter.[27]

The year before, to go back, Newton had not allowed his brief *contretemps* with David Gregory over publication of his 1676 series expansion of the area of a curve defined by a binomial equation to spoil the welcome which he

(21) Fatio to Newton, 17 November 1692 (*Correspondence*, **3**: 230, with '9ber' there mistakenly read as 'September', however).

(22) See Fatio's responses to Newton on 17 and 22 November (*Correspondence*, **3**: 230, 231–2).

(23) So Fatio wrote to Newton on 30 January 1692/3 (*Correspondence*, **3**: 242). Newton responded on 14 February that 'When I invited you hither [the previous November] I was contriving how you might subsist here a year or two' (*ibid.*: 245), and added a month later: 'The chamber next me is disposed of; but that which I was contriving was, that since your want of health would not give you leave to undertake your design for a subsistence at London, to make you such an allowance as might make your subsistence here easy to you' (*ibid.*: 263).

(24) To a long paragraph by Fatio on this topic on 30 January 1692/3 (*Correspondence*, **3**: 242) Newton replied succinctly on 14 February that he was 'glad you have taken y^e prophesies into consideration & I believe there is much in what you say about them, but I fear you indulge too much in fansy in some things' (*ibid.*: 245). Fatio was afterwards, of course, to be pilloried at Charing Cross more than once for over-enthusiastically propagating his beliefs along with the fanatical Prophets from the Cévennes.

(25) See especially Fatio's letters to Newton on 4 and 18 May 1693 (*Correspondence*, **3**: 265–6, 268).

(26) So he wrote to Newton on 11 April (*Correspondence*, **3**: 391).

(27) It is rare, indeed, that the contemporary record registers their meeting at all. (But see note (70) below.) In his last letter to Newton on 18 May Fatio made a parting request that 'If it was in your way to come to town I should be very glad to see You and to confer with You' (*Correspondence*, **3**: 270). The Trinity buttery accounts (see note (13) above) record an absence by Newton from Cambridge for ten days in late May, and it is natural to suppose that he acceded to Fatio's wish to visit him in London. What happened so abruptly to terminate their intimacy can only be conjectured, but we need not go to such extremes of speculation as F. E. Manuel presents in psycho-sexual explanation thereof in Chapter 9 of his *A Portrait of Isaac Newton* (Cambridge, Massachusetts, 1968), where he dubs Fatio 'Newton's ape'.

afforded Gregory's friend (and first publisher in 1688 of the quadrature series in dispute[28]), the Edinburgh physician Archibald Pitcairne, when the latter visited Cambridge at the beginning of March 1692 *en route* to take up the chair of medicine at Leyden. On that occasion, as is well known, Pitcairne persuaded out of Newton an autograph 'De natura Acidorum' which, by way of a transcript of its content made by Gregory, was eventually published eighteen years later (in its original Latin and a somewhat differing English version) in the second volume of John Harris' *Lexicon Technicum*.[29] In the same vein Newton wrote a long letter to John Locke the following August, passing comment on two (al)chemical 'recipes' which Locke had transcribed for him out of the papers of the lately deceased Robert Boyle.[30] Locke had been a frequent correspondent of Newton's since December the previous year,[31] and on 3 May the latter had written: 'Now the churlish weather is almost over I was thinking w^th^in a Post or two to put you in mind of my desire to see you here where you shall be as welcome as I can make you....You may lodge conveniently either at y^e^ Rose Tavern or Queen's Arms Inn'.[32] But, so far as we know, nothing came of Newton's proposal and there was a lull in their exchange of letters during a whole year from August 1692. That same month there arrived by post from Oxford a suggestion of much more lasting import: a plea from John Wallis that Newton should contribute something to the news expanded Latin edition of his *Algebra* which Wallis was then about to put to press in the second volume—but first to appear—of his collected *Opera Mathematica*. Wallis' letter itself is lost, but in the draft of his reply on 27 August returning his 'hearty thanks for giving me opportunity of adding or altering what may concern me in your book' Newton not only gave permission, should Wallis think it 'of any moment', for his infinite series for $\pi/2\sqrt{2}$[33] to be inserted 'where you speak of that [for $\pi/4$] of M^r^ Leibnitz', along with the

(28) See page 6, note (15) below.

(29) There is a facsimile reprint in (ed.) I. B. Cohen and R. E. Schofield, *Isaac Newton's Papers and Letters on Natural Philosophy* (Cambridge, 1958): 256–8; the original autograph of the 'De natura Acidorum', completed by a passage in David Gregory's transcription, is reproduced in *Correspondence*, **3**: 205–9.

(30) See *Correspondence*, **3**: 217–19; Locke's transcription of the two (al)chemical recipes had been included with his letter to Newton on 26 July (*ibid.*: 216–17). An unpublished variant draft of the latter's reply exists on ULC. Add. 3965.13: 469^r^. On 7 July Newton had written to Locke that he 'should be glad to assist...all I can, having a liberty of communication allowed me by M^r^ B[oyle]' (*ibid.*: 215).

(31) See his letters of 13 December 1691 and 26 January/16 February 1691/2 (*Correspondence*, **3**: 185–6, 192–3, 195).

(32) *Correspondence*, **3**: 214.

(33) The pairwise alternating one which he had discovered in 1676 (see IV: 208, 211–12) and soon after transmitted to Leibniz in his *epistola posterior* of 24 October of that year (see *Correspondence*, **2**, 1960: 120).

decoding of the anagram in which he had in his *epistola posterior* of 1676 to Leibniz scrambled the bare enunciation of his dual method for extracting the fluent 'root' out of a given fluxional equation,(34) but added that 'The two methods you desire depend upon a third mentioned neare the beginning of the Letter [of 24 October 1676] w^ch ought therefore to be first explained'.(35) Newton then went on to expound this primary method of squaring algebraic curves by series, instancing it not merely in the case of the *primum Theorema* which had been its sole exemplification in 1676, but also by a yet more general theorem of quadrature by expansion into series which he would appear to have newly found.(36) In a second (lost) letter of 17 September he adjoined a lengthy worked example of his method of extracting the fluent 'root' of a given fluxional equation term by term as an ascending or descending infinite series. Wallis' minimal reshaping of Newton's *ipsissima verba* in Chapter XCV of his Latin *Algebra* the next year (37) has its deserved niche in history as the first public account of the intertwined methods of fluxions and infinite series.

(34) See *Correspondence*, **2**: 129 and IV: 673. We have in II: 190–1, note (25) already quoted the unravelling of this anagram as Newton set it down at the time in his Waste Book (ULC. Add. 4004: 81^v).

(35) *Correspondence*, **3**: 219.

(36) See pages 70–1, note (49) following.

(37) In his *Opera Mathematica*, **2** (Oxford, 1693): 390–6, reproduced in **1**, Appendix 3 on pages 170–80 below, there incorporating the few small corrections which Newton entered in his library copy of the volume (now Whipple Science Museum, Cambridge. WS 1305). There exists, we would add, the yet unpublished draft (ULC. Add. 3977.7) of a letter from Newton to Wallis in about January of 1693, communicating his 'hearty thanks for the sheets of your book [which] is so neare finishing' and communicating these 'amendments'—too late for them to be set right in the published volume, even in its *errata*. We may be grateful that a printer's deadline prevented Wallis from taking note of Newton's letter (if indeed it was ever sent), since it also acted to stop the broadcasting of an unfortunate failure of memory on the latter's part when he went on to insist that 'The plague was in Cambridge in both y^e years 1665 & 1666 but it was in 1666 y^t I was absent from Cambridge & therefore I have set down [at *Opera*, **2** 'pag. 368 lin. ult.' where he had a few lines above directed '*pro* 1665 *lege* 1666'] an amendm^t of y^e year'. 'I wrote to you lately', he continued—evidently referring to one or other of his lost letters of 27 August and 17 September 1692—'that I found y^e method of converging series in the winter between y^e years 1665 & 1666. For that was y^e earliest mention of it I could find then amongst my papers. But meeting since w^th the notes [ULC. Add. 4000: 2^r–20^v, reproduced in I: 47–121] w^ch in y^e year 1664 upon my first reading of Vieta's works Schooten's Miscelanies & yo^r Arithmetica Infinitorum I took out of those books & finding among these notes [ff. 18^r–19^v = I: 104–11] my deduction of the series for the circle out of yours in yo^r Arithmetica Infinitorum: I collect y^t it was in y^e year 1664 that I deduced th[is] out of yo^rs. There is also among these notes [ff. 20^r/20^v = I: 112–15] Mercators series for squaring the Hyperbola found by y^e same method w^th some others. But I cannot find y^t I understood y^e invention of these series by division & extraction of roots [as in ULC. Add. 3958.3: 72^r/70^r–71^r, reproduced in I: 122–34] or made any further progress in this business before the winter w^ch was between y^e years 1665 & 1666. But in the winter & y^e spring following by y^e use of Division & extraction of roots I brought the method to be general, & then the plague made

And so to Newton's 'black year' of 1693, as his most recent general biographer[38] has dubbed it. What fresh is there here to say? Certainly, where scholars have, from the pedestals of their own stand-points, bickered ceaselessly this past century and a half over the possible causes and long-term after-effects of Newton's undeniable breakdown in health in the late summer of that year, we would be foolish to attempt any definitive assessments when the extant record offers but a blurred glimpse of the past reality. Between the abandoned draft of a business reply to Otto Mencke, publisher of the *Acta Eruditorum*, at the end of May[39]—a letter from Leibniz[40] (the only one he ever wrote directly to Newton) went similarly unanswered till the autumn—and the taut, melancholic, self-deprecating outbursts to Pepys and Locke nearly four months later to which we shall come in a moment, his extant correspondence (whatever might have perished) consists solely of the already mentioned plea addressed to him by his half-sister Hannah Barton for 'advise' on settling the estate of her dying husband Robert.[41] Fresh from spending much of the two preceding months away (or so we may assume) in Brigstock comforting his sister, Newton suddenly reappears to our modern eyes on 13 September, 'extremely troubled at the embroilment I am in' and having 'neither ate nor slept well this past twelve month, nor...my former consistency of mind',[42] temporarily resident in London 'At the Bull [ Inn ] in Shoreditch' whither the proffered bait of new possibilities of employment in the metropolis had lured him. But the mood to be gone from Cambridge had left him as rapidly as it had taken hold. To Samuel Pepys, who (through John Millington) had 'pressed' him to come to town, he sent off on that day a sorry, hang-tail,

me leave Cambridge. But I do not think it requisite that you should make a particular mention of these things. I believe you have said enough in y$^{e}$ beginning of y$^{e}$ 91$^{th}$ Chapter'—where, that is, Wallis had made extract of the pertinent portion of Newton's 1676 *epistola posterior* (*Correspondence*, **2**: 111–14).

(38) F. E. Manuel in his *Portrait of Isaac Newton* (note (27) above), where he so titles his Chapter 10 (pages 213–25).

(39) See *Correspondence*, **3**: 270–1; Mencke's letter of 1 February to which Newton began here to respond is lost. He did not come to review his unfinished first reply till some six months afterwards, and then on 22 November sent off to Mencke a considerably reorganized letter (*ibid.*: 291–2).

(40) *Correspondence*, **3**: 257–8. While Leibniz put this in the post in early March, it probably did not arrive in Cambridge—if we may go by the transit times between England and Germany of other letters at this time—till some weeks (or even months) later. We will examine in note (46) below the detailed answer Newton at length rendered in October to Leibniz' 'great expectation' of him to apply his utmost skill 'tum ut problemata quæ ex data tangentium proprietate quærunt lineas, reducantur optimè ad quadraturas; tum ut quadraturæ ipsæ (quod valde vellem) reducantur ad curvarum rectificationes' (*ibid.*: 257).

(41) See note (15) above.

(42) As he excused himself in his letter to Pepys on that day (see *Correspondence*, **3**: 279).

withdrawn refusal of some unspecified offer of a (Government?) position: 'I never designed to get anything by your interest, nor by King James's[!] favour, but am now sensible that I must...see neither you nor the rest of my friends any more, if I may but leave them quietly.'(43) And on the morrow he penned a celebrated outburst to Locke, one whose detail he could not recall a month later, or so he claimed:

'Being of opinion that you endeavoured to embroil me w^th^ woemen & by other means I was so much affected with it as that when one told me you were sickly & would not live I answered twere better if you were dead. I desire you to forgive me this uncharitableness. For I am now satisfied that what you have done is just & I beg your pardon for my having hard thoughts of you for it &...also for saying or thinking that there was a designe to sell me an office, or to embroile me.'(44)

When his more balanced sanity returned, Newton excused himself to Locke by remarking that 'The last winter by sleeping too often by my fire I got an ill habit of sleeping & a distemper w^ch^ this summer has been epidemical put me further out of order, so that when I wrote to you I had not slept an hour a night for a fortnight together & for 5 nights together not a wink'.(45)

What 'office' it was that Newton here refused to purchase—if it was on sale at all—and what feminine entanglements he sought desperately to evade—did they exist outside of his own imagination—we shall probably never know. The exact rôles in conditioning his reactions to these played severally by his repressed deep-seated puritanism, his less well documented psycho-sexual frustration (coupled maybe with the totally putative onset of male menopause), his recent close brush with death within his immediate family and the inevitably attendant thoughts of his own mortality, his wider neuroses and quirks of personality, the physical effects of his recent 'epidemical distemper' and the mental strain of its debilities and of being deprived of sleep for so long, even the poison of the noxious fumes from his fire (that in his chamber or of the furnaces in his chemical elaboratory?): these will be argued for ever more. What we would here insist firmly upon is that we can trace no enduring influence of this short-lived breakdown in altering the course of his contem-

(43) Again cited from *Correspondence*, **3**: 279. Pepys was more than a little taken aback at receiving a letter which was, he wrote to Millington on 26 September, 'so surprising to me for the inconsistency of every part of it, as to be put into great disorder by it,...lest it should arise from...a discomposure in head, or mind, or both' (*ibid.*: 281).

(44) *Correspondence*, **3**: 280.

(45) Newton to Locke, 15 October 1693 (*Correspondence*, **3**: 284). He had given a similar account of the onset of his malady to Millington when he met him at Huntingdon on 28 September, there excusing himself, as Millington made haste to inform Pepys, 'upon his own accord, and before I had time to ask him any question' for 'a distemper that much seized his head, and that kept him awake for above five nights together' and asked so to be pardoned by Pepys, 'he being very much ashamed he should be so rude to a person from whom he hath so great an honour' (*ibid.*: 282).

b-2

porary scientific pursuits and practices, or more than momentarily affecting their rate of development. Having speedily made his peace with both Pepys and Locke, Newton passed in mid-October 1693 to make a long-delayed reply to Leibniz' letter of the previous spring, cheerfully unknotting for him the fluxional anagram which he had transmitted seventeen years before, and also answering Leibniz' request to explain how (by means of a curve's evolute) a reduction of the problem of rectification is made to one of the quadrature of curves.(46) And he was equally quick to oblige when in late November there arrived in Cambridge John Smith, Writing Master of Christ's Hospital at London, bearing a letter from Pepys(47) requesting that he test Smith's solution

(46) Newton to Leibniz, 16 October 1693 (*Correspondence*, **3**: 285). The general mode which he there presents for relating the problems of quadrature and rectification is essentially that, discovered by him long before in May 1665 (see I: 263) and afterwards elaborated in Proposition 9 of his October 1666 tract (see I: 432–4), whereby the length of an evolute curve is determined as the difference in length between the radii of curvature drawn to touch it at its two end-points from any of the family of involutes which the 'unwrapping' of its tangent generates; except that this tangent is now referred to the semi-intrinsic coordinates compounded of the distance $x$ of its instantaneous meet with an arbitrarily fixed straight line measured from some given point in the latter, and of the difference $z$ between its length at that point and its length when $x = 0$. At once $\dot{z}/\dot{x} = y/a$ is the cosine of the angle which the evolving tangent makes with the fixed line; whence, once the function $y \equiv f_x$ is determined from the given property of the curve whose arc-length is to be found, the latter's rectification is yielded by the quadrature of the area $az = \int_0^x y.dx$. Conversely, given the relationship between $x$ and $y$ and thence the cosine $y/a = \dot{z}/\dot{x}$ of the angle of slope of the corresponding evolute tangent, the family of tangents so constructed will envelop a curve whose related general arc-length is $z-b$. It does not follow, however, as Newton would have it, that such a rectification 'semper fieri potest Geometrice ubi fluxionum $\dot{x}$ et [$\dot{z}$] relatio geometrica est' since all but a few functions $z$ having 'geometrical' fluxional derivatives will not themselves be algebraic. (Here we would warn, incidentally, that several slips of Newton's pen in writing $y$ and $\dot{y}$ in place of $z$ and $\dot{z}$ stand mostly uncorrected in all the several printings which the letter has received since the middle nineteenth century.) There Newton leaves it for Leibniz, but of course where in particular the evolving tangent is normal to the fixed line when $x = 0$—there having length $b$, say—and we take the evolute curve to have the general point $(r, s)$ in the system of perpendicular Cartesian coordinates in which the fixed line is the abscissa $s = 0$ and its given point is the origin $(0, 0)$, then at once $x = s.(dr/ds)-r$ and likewise

$$z = s\sqrt{[1+(dr/ds)^2]} - \int_s^b \sqrt{[1+(dr/ds)^2]}.ds + b.$$

When—in apparent ignorance of Newton's earlier investigation—Jakob Hermann independently proposed the problem 'Invenire curvam vel curvas algebraicas, quarum rectificatio indefinita dependeat a quadratura cujusvis curvæ algebraicæ' in the *Acta Eruditorum* in August 1719, a solution was given in this form four years later by Hermann himself (*Acta* (April 1723): 174–9) and further extended the next year by Johann Bernoulli (*Acta* (August 1724): 356 ff. = *Opera Omnia*, **2** (Lausanne/Geneva, 1742): 582–92).

(47) Who commended 'y^e Bearer...noe less for what I personally know of his general Ingenuity...than for y^e general Reputation he has in this Towne (inferiour to none, but superiour to most) for his Maistery in the two Points of his Profession, namely, Faire–Writeing and Arithmetick, soe farr (principally) as is subservient to Accountantship' (*Correspondence*, **3**:

to a problem of dice currently the topic of conversation 'in this Towne, ...among Men of Numbers', namely: 'How much more or lesse Expectation *A* may (w^th^ equal Lucke) reasonably have; of throwing at one or every Throw one Sice at least with Six Dyes, than *B* two Sices with Twelve, or *C* three with Eighteen Dyes?'(48) In reply on 16 December Newton laid out for Pepys a series of 'progressions of numbers' from which he drew, for 1, 2, 3, ..., 6 dice in succession the 'number of chances without sixes', the 'chances for one six & no more' and the 'chances for two sixes & no more',(49) and thence deduced that the probability, 31031/46656, of there ensuing at least one six with 6 throws of dice is greater than that, 1346704211/2176782336, of there falling at least two sixes with 12 throws, 'And so by producing the progressions to the number of eighteen dice...you will have the proportion of [the players'] stakes upon equal advantage'.(50) That there should be an odds-on chance of throwing at least one six (or any other face) in 6 throws of dice still appeared paradoxical to Pepys, and Newton patiently spelled out in yet a further letter to him on 22 December 1693 how it is that such 'chances' are not mutually

293). It would seem that Smith was responsible at least for organizing the 'blue-coat boys' usually allotted the task of drawing the winning tickets in London lotteries of this period, and may well have been himself paid for administering their running, if not helping to frame their rules and gauge their expectations of profit.

(48) Or so Pepys rephrased it for Newton on 9 December 1693 (*Correspondence*, **3**: 297). In his introductory letter on 22 December Pepys had queried more vaguely whether it is 'as easy a Taske' to 'fling a 6' with 6 dice, as '2 Sixes' with 12 dice, or as '3 Sixes' with 18 dice (*ibid.*: 294); and Newton rightly responded four days later that 'y^e^ Question...seemed to me at first to be ill stated & in examining Mr Smith about y^e^ meaning of some phrases in it he put the case of y^e^ Question y^e^ same as if *A* plaid with six dyes till he threw a six & then *B* threw as often w^th^ 12 & *C* w^th^ 18[,] the one for twice as many sixes [&] the other for thrice as many...' (*ibid.*: 295).

(49) See *Correspondence*, **3**: 299. In modern abridged notation Newton there tabulates in succession the chances, namely $\binom{j}{i}5^{j-i}$ in $6^j$, of turning up $i$ sixes, $i = 0, 1, 2$, in $j$ dice, $j = 1, 2, 3, \ldots, 6$, understanding that the full number of throws are allowed to each player even after another has thrown his pertinent number of sixes in fewer 'flings' than he would (on average) need. He had, of course, imbibed the simple notion of equi-probability of chances to which he here appeals long before when, as an undergraduate at Trinity, he had made careful study of Huygens' tract *De Ratiociniis in Ludo Aleæ* (Leyden, 1657); see I: 58–62.

(50) *Correspondence*, **3**: 300. It will be clear from the previous note that the probability 'upon equal advantage' (as Newton put it) of throwing at least $n$ sixes in $6n$ 'flings' of the dice will be $P(n) = \sum_{n\leqslant i\leqslant 6n}\binom{6n}{i}5^{6n-i}/6^{6n}$. Since the (normal) distribution of the expansion of $\left(\frac{5}{6}+\frac{1}{6}\right)^{6n}$ is spread with uniform deviation round its largest term $\binom{6n}{n}\left(\frac{5}{6}\right)^{5n}\left(\frac{1}{6}\right)^{n}$, for sufficiently large $n$ the probability $P(n)$ will come to differ minimally from $\frac{1}{2}$. Explicit proof that $P(n)$ indeed tends monotonically to $\frac{1}{2}$ as $n \to \infty$ is given by T. W. Chaundy and J. E. Bullard in their analysis of 'John Smith's problem' in *Mathematical Gazette*, **44**, 1960: 253–60. A more comprehensive survey of the historical background—with the suggestion that the problem owed more

exclusive.(51) The next year in May he went to an immense amount of trouble, no doubt again at Pepys' initial wish, in casting an alternative scheme of instruction for the 'blue-coat boys' of the Mathematical School at Christ's Hospital, but it would take us too far from our purpose to go into its detail.(52)

Above all, the considerable bulk of the mathematical papers reproduced in the pages here following which (so we assert and can many times solidly prove) derive from the two years after the autumn of 1693 will demonstrate that there was no sudden drop, in quantity or in quality, in Newton's technical output. As we have more than once observed in the previous volume(53) and as may many times more be seen below,(54) when David Gregory paid an extended visit to Cambridge early in May 1694 and was allowed virtually free run of Newton's private scientific papers, he found there a veritable treasure-house

to Pepys than to Smith in its origin—is presented by F. N. David in 'Mr Newton, Mr Pepys & dyse: A historical note', *Annals of Science*, **13**, 1957: 137–47 (summarized in her *Games, Gods and Gambling* (London, 1962): 125–9).

(51) *Correspondence*, **3**: 302–3, answering Pepys' letter of the previous day on the point (*ibid.*: 301–2).

(52) Much of the documentary material which supports this statement remains unpublished. The Mathematical Master at this time, Newton's former Trinity *confrère* Edward Paget (on whom see VI: xviii–xx), had drawn up a revised, more forward-looking curriculum for his School; and the Treasurer of Christ's Hospital, Nathaniel Hawes, was at a Committee meeting on 2 May 'desired when he goes to Cambridge on Friday next [4 May] to take with him a copy of the old and new schemes, and advise with the Professor and other Mathematicians in the University concerning them, and get their opinions in writing which of the two schemes they judge best' (see *Correspondence*, **3**: 366, note (2)). The secretary copy of Paget's scheme sent to Newton (ULC. Add. 4005.16: 85r–86r) and the draft (*ibid.*: 89v–88v) of his preliminary response to Hawes a few days later, raising 'a few Questions about ye new scheme of Learning proposed for yor foundation', both remain in manuscript. Likewise unprinted is the covering letter to Paget *and Professor Cotes* (the draft exists on ULC. Add. 3965.12: 330r) which Newton sent to London on 25 May along with his fuller reply to Hawes (Edleston, *Correspondence of Sir Isaac Newton* (note (10) above): Appendix: 280–92 = *Correspondence*, **3**: 357–65) and his own preferred 'New Scheme of Learning proposed for the Mathematical Boys in Christ's Hospital' (Edleston: 292–4 = *Correspondence*, **3**: 365–6; a number of variant preliminary castings of this in Newton's own hand exist at [in order of the sequence of their composition] ULC. Add. 4005.16: 100r/100v, 91r, 88r, 90r/90v, 87r/87v and Trinity College, Cambridge. R.5.4[22], the last a revision of the version first published by Edleston from the secretary copy in Christ's Hospital Court Book of that sent to Hawes). There followed two further letters of Newton to Hawes on 26 May (Edleston: 294–5 = *Correspondence*, **3**: 367–8) and 14 June (Edleston: 296–7), but yet again unprinted is a final letter to him on the topic in (?) July 1694 (the draft is ULC. Add. 4005.16: 93r) where Newton urges the advantage gained 'If Mr Stones Foundation [that is, the Mathematical School] be conjoyned wth ye Kings in a subservient way so that as often as any of the King's places become vacant by death they may be filled up not out of the grammar school but out of Mr Stones children of like standing in Mathematical learning'.

(53) See especially VI: 568–9, note (1); 578–9, note (21); 583, note (34); and 601, note (2).

(54) Particularly pages 208, note (28); 221, note (1) and 222, note (10); 269–70, note (55); 508–9, note (2).

of all things mathematical, some of which Newton was even then in the course of further elaborating and refining.(55) While the true depth and extent of these predominantly geometrical researches has never hitherto been made public, and we may accordingly forgive those who have previously seen only derivative sterility in all but a few isolated mathematical sorties made by him after 1693, or the rare resparking of his youthful fire in response to an occasional submitted problem, let us henceforth (if the tenacity of received opinion permits) correct this blindness to the past truth. All which is, conversely, not to deny that in Newton's scientific papers and correspondence of the early 1690's we may trace a slow but accelerating decrease in his elasticity to absorb fresh findings and his hitherto matchless capacity to attack novel problems and evolve new techniques of solution. Though we should not exaggerate what is at first a barely perceptible trend, his mathematical writings from 1693 onwards do indeed come more and more to look back to the glories of yester-year, their explicitly announced purpose less to create anew than to finish and polish earlier investigations left rough and incomplete. But this is the inevitable relentless attrition of old age, not the sudden and permanent debility of a mental storm or physical breakdown in health in the summer of that year.

(55) On pages 196–7 below we cite the detailed impression which Gregory took away with him from Cambridge of the grand mathematical project on which Newton was working at this time, his multi-volume treatise of *Geometria*, cast in both ancient analytical and modern fluxional moulds, whose surviving drafts are reproduced in our present Part 2 following. Upon his return to Oxford—having in July (see *Correspondence*, **3**: 380–2; and VI: 470–7) drawn from Newton a simplified step-by-step demonstration of the form of the 'figure w$^{ch}$ feels y$^{e}$ least resistance in y$^{e}$ Schol. of Prop. XXXV Lib II' of his *Principia*—David Gregory began to organize what else he had gleaned during his Cambridge visit, first in individual mathematical memoranda and then in a full-blown elaboration, in 47 propositions, of a treatise on 'Isaaci Newtoni Methodus Fluxionum; ubi Calculus Differentialis Leibnitij, et Methodus Tangentium Barrovij explicantur, et exemplis plurimis omnis generis illustrantur. Auctore Davide Gregorio M.D. Astronomiæ Professore Saviliano Oxoniæ'. (Gregory's preliminary scheme, now Royal Society. Gregory MS: 64: 'Describenda et Chartis consignanda Mense Septembri MDCXCIV'—listed in his later catalogue of his papers as C79: 'Adumbratio nostræ [!] de fluxionibus methodi'—is reproduced in *Correspondence*, **4**, 1967: 15–16. He went on to draft the full compendium in late October, as several dates entered by him in his original manuscript, now St Andrews. QA 33G8/D12, establish beyond surmise.) While this loose collection of calculus problems was fairly widely circulated in its day—apart from Gregory's fair copy of its text (now in Christ Church, Oxford) there exist transcripts of it in the hand of William Jones and of John Keill, the latter (now ULC. Lucasian Papers [Res. 1894]: Packet No. 13) once in Newton's possession it would seem—it has never been printed, justly so since its content is all but wholly derivative from the researches of Newton and the published articles of such creative mathematicians as Leibniz and Jakob Bernoulli. For all that we may, here as elsewhere, praise Gregory's sincere aim of opening up Newton's close-held mathematical findings to the world at large, our reaction to such feebly wrought endeavours to do so must ever be one of sorrow that Newton could attract no more able and gifted a disciple to widen and extend the deep inroads into future mathematical discovery which he had himself wrought over the past three decades.

In yet one more change of scene, to pursue our brief chronological conspectus' we find Newton on 1 September 1694 conversing with John Flamsteed at Greenwich, optimistically claiming the moon's theory to be 'in his power' and stating that he needed only '5 or 6 equations' of its motion fully to capture it.(56) But though Flamsteed over the next few months sent him a hundred and fifty and more unprecedentedly accurate observations of the moon's passage which he himself had made over nearly twenty years from his small but efficient observatory atop Greenwich Park, Newton's confident initial expectation of being able to use these to determine the numerical parameters in an improved, dynamically based lunar theory—one whose basic structure was derived theoretically by assessing the disturbing action of the sun's gravitational pull on the simple Keplerian motion of a 'planetary' moon orbiting in an ellipse round the earth at a focus—came slowly to be eroded during the course of an extensive correspondence which he maintained almost without break with Flamsteed over the next year till it petered out in the late summer of 1695. Let us here forbear to cite any details.(57) For all his continuing show of hope that he had it in his grasp to achieve 'this Theory so very intricate & the Theory of Gravity so necessary to it, that I am satisfied it will never be perfected but by somebody who understands y^e Theory of gravity as well or better then I do',(58) and despite some success in afterwards mocking up a modified Horrocksian kinematic model of lunar orbit both in a scholium appended to David Gregory's *Astronomia* in 1702(59) and in a recast scholium to Proposition XXXV of the *Principia*'s third book in its second edition in 1713,(60) when Halley qualified his published propositions on the moon as

(56) Or so David Gregory noted in a contemporary memorandum (C58, now Royal Society. Gregory MS: 26, reproduced in *Correspondence*, 4: 7) where he wrote: 'D. Newtonus primo Septembris die 1694 Grenovici Flamstedium adiit, ubi locutus est de nova editione suorum *Principiorum*. Credit Theoriam lunæ esse in potestate: ad illius locum inveniendum opus erit 5 vel 6 æquationibus'.

(57) For an up-to-date introduction to the tangled mass of primary and secondary literature on this knotty technical topic we may refer to our own survey of 'Newton's Lunar Theory: From High Hope to Disenchantment' (*Vistas in Astronomy*, **19**, 1976: 317–28).

(58) As he described it to Flamsteed on 16 February 1694/5 (*Correspondence*, **4**: 87).

(59) *Astronomiæ Physicæ & Geometricæ Elementa. Auctore Davide Gregorio* (Oxford, 1702): 332–6, especially 333. Newton's preliminary English drafts in ULC. Add. 3966.10: [in sequence] 76, 77, 74, 88–9 and 82–3 (the last of which—except for the omission of its title 'The Theory of the Moon'—is reproduced in *Correspondence*, **4**: 322–6) do not differ in essence from Gregory's published Latin version (now available in facsimile in I. B. Cohen, *Isaac Newton's 'Theory of the Moon's Motion' (1702). With a bibliographical and historical introduction* (London, 1975): 123–8, along with reproductions of its three separate contemporary retranslations back into English, *Newtono inscio*).

(60) *Philosophiæ Naturalis Principia Mathematica* (Cambridge, $_2$1713): 421-5; Newton's drafts of this exist in three successive states in ULC. Add. 3966.9: 61, 67/68 and 65/63 respectively. The scholium reappeared in the *editio ultima* (London, $_3$1726: 459–65) unchanged except for one major excision.

'all sagacity' he wore, as we have previously remarked,[(61)] a thin smile, and when anyone threatened to utter any printed claim that he had indeed mastered the moon's motion he was downright upset.[(62)]

What effect this failure to construct a viable theory of the moon's path had on his wider confidence in his ability to go on making a fruitful contribution to scientific knowledge, we can only guess. One might almost entertain the hypothesis that if he had succeeded in 1695 in contriving an accurate 'Theory of y^e Moon' then Newton would have gained strength to stay on in Cambridge, making sustained effort further to widen and deepen the mechanical and astronomical researches which he had begun to shape so magnificently in dynamical form in the 1680's and continued more recently to promote. As it was, however, from the early months of 1695 onwards he became more and more undecided and unsettled at Cambridge. In April John Wallis had, in sending along the newly printed *Volumen primum* of his collected works, renewed his entreaty that Newton should publish more of his hoard of mathematical and scientific papers—not least because, he wrote, he had just 'had intimation from Holland [that] your Notions (of *Fluxions*) pass there with great applause, by the name of *Leibnitz's Calculus Differentialis*'[(63)]—but Newton

(61) See VI: 27.

(62) If indeed not downright, tetchily angry. The most celebrated outburst of such wrath on Newton's part came in December 1698 when Flamsteed thought to add a paragraph to his forthcoming account of 'y^e parallax of y^e Pole star' which he (mistakenly) thought he had observed—this John Wallis, ever sharp-eyed to the saleability of a 'newsy' discovery, had at once collared for the third volume of his *Opera Mathematica* (then in press)—where he mildly reminded that it was he who had 'accommodated' Newton with accurate observations of the moon 'in ordine ad emendationem Theoriæ lunaris Horroccianæ[,] qua in re spero eum successus consecuturum expectationi suæ pares' (so he quoted his phrasing to Newton on 2 January 1698/9 after Gregory had informed the latter of its general content; see *Correspondence*, **4**: 293). Newton blazed back four days later: 'Upon hearing occasionally that you had sent a letter to D^r Wallis about y^e parallax of y^e fixt starrs to be printed & that you had mentioned me therein with respect to y^e Theory of y^e Moon I was concerned to be publickly brought upon y^e stage about what perhaps will never be fitted for y^e publick & thereby the world put into an expectation of what perhaps they are never like to have. I do not love to be printed upon every occasion much less to be dunned & teazed by forreigners about Mathematical things or to be thought by our own people to be trifling away my time about them when I should be about y^e Kings business. And therefore I desired Dr Gregory to write to Dr Wallis against printing that clause w^ch related to that Theory & mentioned me about it' (*Correspondence*, **4**: 296; as is so often done, we have earlier quoted the penultimate sentence *hors de contexte* in V: xiv, note (14)).

(63) Wallis to Newton, 10 April 1695 (Edleston, *Correspondence of Newton and Cotes* (note (10)): Appendix: 300 = *Correspondence*, **4**: 100). Newton's reply on 21 April is lost, but its gist is readily gatherable from Wallis' further letter on 30 April where he kept up his pressure to publish: 'Consider, that 'tis now about Thirty years since you were master of those notions about *Fluxions* and *Infinite Series*; but you have never published ought of it to this day....' Tis true, I have endeavoured to do you right in that point. But if I had published the same or like

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dithered, even when Wallis went on to propose that he himself be permitted to have printed at Oxford (where 'we have most of the Cutts allready, & furniture fit for it'(64)) the full texts of the two letters of 13 June and 24 October 1676 to Leibniz whose content he had earlier summarized in his *Algebra*.(65) In the outcome, full publication of the *epistolæ prior et posterior* had to await the appearance of Wallis' third volume of *Opera Mathematica*(66) four years later, although maybe his persistence was the necessary goad which in June stimulated Newton privately to begin honing and augmenting the prior drafts of his researches into the species and properties of cubic curves,(67) to be effectively the 'Enumeratio Linearum tertij Ordinis' which he was in in 1704 to append to the *editio princeps* of his *Opticks*. A visit from Edmond Halley in August, 'about a designe of determining the Orbs of some Comets for me'(68) produced little harvest comparable to that of his momentous first trip to Cambridge eleven years before, though he was—a rare privilege!—permitted to carry Newton's 'Quadratures of Curves' back with him to London for transcription,(69) having doubtless urged (as he was again later

notions, without naming you; & the world possessed of anothers *Calculus differentialis*, instead of your *fluxions:* How should this, or the next Age, know of your share therein?' (*Correspondence*, **4**: 117). Wallis could not have foreseen, of course, the mighty industrial complex of recent Newtonian scholarship.

(64) Wallis to Newton, 30 May 1695 (*Correspondence*, **4**: 129). 'Mr Caswell or I', he added, 'will see to the correcting of the Press'.

(65) See IV: 672, note (54). A prod from Wallis on 3 July to correct a 'Transcript [now ULC. Add. 3977.1] of your two letters' which he had earlier sent, if only that 'so corrected... I might at lest leave them reposited in the Savilian Library amongst other Manuscript Papers; which will...confirm to you the reputation of your having discovered these notions so long ago' (*Correspondence*, **4**: 139), would appear to have gone unanswered by Newton, though there does exist the unfinished draft of such a response (ULC. Add. 3977.3, reproduced in *Correspondence*, **4**: 140–1) where he began by thanking Wallis for 'your pains in transcribing my two Letters of 1676' and then passing once more (compare note (37) above) faultily to recall that it was 'in ye beginning of the year 1666...I retired from the University into Lincolnshire to avoyd the plague'. The following November Wallis informed Halley that Newton still did not seem 'forward' for having his two 1676 letters printed in Oxford (see *Coreespondence*, **4**: 186).

(66) Where the full Latin texts of Newton's two letters are set in prime place in its appended 'Epistolarum Collectio' (*Opera Mathematica*, **3**, Oxford, 1699: 622–9, 634–45).

(67) Those reproduced on II: 10–88 and IV: 354–404. With their algebraic reductions by linear transformation recast into a rather less readily graspable appeal to the geometrical lie of the diametral (conic) hyperbola which shares a real asymptote with the general cubic, their content is subsumed to be the backbone of the final 1695 enumeration of the species of cubic curve which is set out on pages 588–644 below.

(68) So Newton reported to Flamsteed on 14 September 1695 (*Correspondence*, **4**: 169). 'He has', Newton went on—quoting from Halley's letter to him of a week earlier (on which see the next note), 'since determined ye orb of ye Comet of 1683 by my Theory & finds by an exact calculus that it answers all your Observations & his own to a minute'.

(69) As we have previously remarked in III: 12, note (29), some weeks after his return to London Halley wrote apologizing to Newton that 'I have not yett returned you your Quadra-

to do[70] that the treatise should be published. He did, it is true, momentarily revive Newton's interest in cometary orbits, the accurate construction of whose conical paths he had only roughly and readily achieved nine years before,[71] and there are in fact to be found among the latter's astronomical papers of this period[72] several new computations of elements of the orbit of the 'great' comet of 1680/1. In a lost letter of 1 October Newton communicated to Halley (his own?) observations of the comet of 1682 so that, by constructing their separate orbits, he might test—as a rough consideration suggested—'if it were not the same with that of 1607'.[73] But he could not match Halley's enthusiasm for such tedious numerical computations as were necessary to 'limitt the Orbs of all the Comets that have been hitherto observed'[74] and their corre-

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tures of Curves, having not yet transcribed them, but no one has seen them, nor shall, but by your directions; and in a few days I will send you them' (E. F. MacPike, *Correspondence and Papers of Edmond Halley* (Oxford, 1932): 91 = *Correspondence*, 4: 165). 'Since I left you', Halley had begun his letter, 'I have been desirous to make triall how I could obtain the position of the Orb of the Comet of 1683, and after having gotten some little direction from a course [coarse] Construction, I took the pains to examine and verifie it by an accurate Calculus, wherin I have exceeded my expectation, finding that a parabolick orb limited according to your Theory will most exactly answer all the Observations Mr Flamsteed and my self formerly made of that Comett, even within the compass of one minute'.

(70) Some seven years afterwards David Gregory entered in his private diary that 'On Sunday 15 Nov. 1702 He [Newton] promised M$^{r}$ [Francis] Robarts, M$^{r}$ Fatio, Capt. Halley & me to publish his Quadratures, his treatise of Light [*sc. Opticks*], & his treatise of the Curves of the 2$^{d}$ Genre' (W. G. Hiscock, *David Gregory, Isaac Newton and their Circle. Extracts from David Gregory's Memoranda, 1677–1708* (Oxford, 1937): 14). He had noted rather differently but two days before that 'Mr Newton is to republish his [*Principia*]; & therein give us his methode of Quadratures' (*ibid.*: 13).

(71) See VI: 481–507; and compare A. N. Kriloff, 'On Sir Isaac Newton's Method of Determining the Parabolic Orbit of a Comet' (*Monthly Notices of the Royal Astronomical Society*, **85**, 1925: 640–56).

(72) Notably those now in ULC. Add. 3965.11/14/18. An unfinished 'Constructio orbis Cometæ qui annis 1680 & 1681 apparuit...ex Observationibus...tr[ibus] quas Flamsteedius habuit Dec 21, Jan 5 & Jan 25' (Add. 3965.11: 170$^{r}$) is reproduced at *Correspondence*, **4**: 167. On our pages 682–8 below we present the edited text of two complementary worksheets (Add. 3965.14: 586$^{r}$ and 3965.11: 165$^{r}$) where Newton computes in two separate ways the slope to the meridian of the same comet's apparent path on 30 December.

(73) As Halley phrased it in his reply on 7 October (*Correspondence*, **4**: 173), adding his entreaty to Newton, 'when your more important business [in Lincolnshire] is over', that he should 'consider how far a Comets motion may be disturbed by the Centers of Saturn and Jupiter, particularly in its ascent from the Sun, and what difference they may cause in the time of the Revolution of a Comett in its very Elliptick Orb'.

(74) Halley to Newton, 21 October 1695 (*Correspondence*, **4**: 182). 'I have', [he there wrote,] 'almost finished the Comet of 1682 and the next you shall know whether that of 1607 were not the same, which I see more and more reason to suspect. I am now become so ready at the finding a Cometts orb by Calculation, that...I think I can make a shift without [rulers].'

spondence tailed off in late autumn as Halley lost his own interest in checking the identity of the two most recent apparitions of 'his' comet.(75)

A last letter from Flamsteed on 11 January 1695/6 querying the truth of a rumour he had heard that Newton had 'finished yᵉ Theory of yᵉ Moon *on uncontestable principles*'(76)—this went unanswered, of course—and then Newton wrote to Halley on 14 March to stop a 'report...sometime spreading among yᵉ Fellows of yᵉ Royal Society as if I was about yᵉ Longitude at Sea' and likewise to obviate a 'rumour of preferment for me in the Mint'.(77) But the latter was true, despite his denial: Newton had been secretly arranging with Charles Montague to put in for just such a post, and within a week the latter wrote that the newly vacated post of Warden of the Mint, with its salary of 'five or six hundred pounds per An' and 'not too much bus'nesse to require more attendance than you can spare',(78) was his for the asking. Within another month the Royal Warrant confirming the appointment(79) was through and Newton, papers and belongings packed away in his trunks, was off on the road to London, never again to return to Cambridge except on the briefest of visits.

And there, since our concern is here only with Newton's last university years, we must leave him. Forgive our prejudiced sigh for the passing of a uniquely talented man from the environs, restrictive as in many ways they were, of the small Fenland town where he had passed the prime of his age for creative invention amid his books and the smoke of his laboratory fire. Many have it in them to be hard-headed businessmen, successful politicians, able organizers of people and administrators of government, even efficient Masters

(75) A final, undated letter from Halley on the topic relates that he 'could not get time to finish the account of the two Comets I promised you' (*Correspondence*, **4**: 190). We would add that the draft (ULC. Add. 3965.14: 605ʳ) of a following letter from Newton to Halley, reproduced in *Correspondence*, **4**: 184–5 under the date 'late October 1695', is in fact—as its revised version (Add. 3982.7, printed in E. F. MacPike, *Correspondence of Halley* (note (69) above): 199) makes yet clearer—of about January 1725.

(76) *Correspondence*, **4**: 192. Whoever Flamsteed's informant was (may be Halley?), he was plausible—and accurately informed?—enough to credit Newton with having discovered '6 severall Inæqualitys [of the moon's motion] &...nevertheless yᵉ Calculation will not be much more troublesome or difficult then formerly'. A few years later Newton was to set seven such inequalities down in the 'Lunæ Theoria Newtoniana' which he allowed Gregory to publish in 1702 (see note (59) above).

(77) *Correspondence*, **4**: 193. The undercurrents of well-founded rumour which began in the winter of 1695/6 to put Newton in the running for a place in the Mint may have had their source in the report on the deterioration of the nation's silver coinage (now in London. Goldsmith's Company MS 62 'Recoinage of 1696') which Newton had prepared some while before, where *inter alia* he 'proposed a Price Control Board...to reduce prices...or at least limit their increase [which] was to operate...on the Chartered Companies of London' (J. Craig, *Newton at the Mint* (Cambridge, 1946): 9).

(78) Montague to Newton, 19 March 1695/6 (*Correspondence*, **4**: 195).

(79) The Mint's record of this, dated 13 April 1696, is reproduced in *Correspondence*, **4**: 200.

of the Mint and forceful Presidents of the Royal Society; only too few have ever possessed the intellectual genius and surpassing capacity to stamp their image upon the thought of their age and that of centuries to follow. Watching over the minting of a nation's coin, catching a few counterfeiters, increasing an already respectably sized personal fortune, being a political figure, even dictating to one's fellow scientists: it should all seem a crass and empty ambition once you have written a *Principia*.... But it did not to Newton. So quickly on to the final mathematical texts which he penned at Cambridge, and to our exegeses thereof in introduction and in footnote.

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