JOHN WALLIS AS EDITOR OF NEWTON’S MATHEMATICAL WORK

by

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This paper explores Wallis’s role as editor of Newton’s mathematical work. My objective is to understand how two mathematicians who held different views concerning mathematical method could nonetheless cooperate with one another quite effectively. Most notably, Wallis and Newton pursued different policies as far as the printing of algebra is concerned. In the 1690s Newton held the view that algebra is a heuristic method ‘not worthy of publication’. Wallis, instead, for all his life was keen on making algebraic methods explicit in print. As the analysis of the correspondence between Wallis, Collins and Newton reveals, the methodological tension between Wallis and Newton was resolved in such a way that Newton agreed to print his heuristic methods in Wallis’s English Algebra (1685) and Latin Opera (1693–99). Newton wished to guarantee his priority rights on discoveries in algebra and calculus, yet he also sought to avoid any tight authorial commitment towards them. Wallis, in contrast, received from Newton material that turned out to be useful for the fulfilment of a nationalistic programme aimed at eulogizing British mathematicians as well as his own work.

Keywords: John Wallis; Isaac Newton; correspondence; mathematical method

INTRODUCTION

The extant letters between John Wallis and Isaac Newton (eight from Wallis, four from Newton) date from the summer of 1692 to the early months of 1699 (n.s.). In this period Wallis was busy editing his monumental Opera, which was published in three tomes in 1693, 1695 and 1699. The importance of this correspondence and of Wallis’s Opera for Newton’s career as a mathematical author cannot be overestimated. Indeed, this material has often been referred to by historians examining the calculus controversy between Newton and Gottfried Wilhelm Leibniz. The publication of Newton’s 1676 letters to Leibniz (via Oldenburg)—the so-called epistola prior and epistola posterior—and of extracts from Newton’s mathematical papers in Wallis’s works (first his English Algebra of 1685, and then volumes 2 (1693) and 3 (1699) of his Latin Opera) had a momentous role in the calculus controversy.2

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Much to the frustration of his admirer and correspondent John Collins, in the 1670s Newton rejected Collins’s proposals to print his new method of series and fluxions. Rumours about Newton’s mathematical prowess reached Wallis in the mid 1670s. The correspondence between Wallis and Collins in 1676–77 reveals that Wallis was impatiently waiting to lay his hands on the two great epistolae, and that he was eventually granted permission to reproduce them in part in his English Algebra.

In the 1690s, when Leibniz was circulating his nova methodus in the Acta eruditorum, Wallis repeatedly complained about the fact that the ‘notions of fluxions’ were circulating on the Continent ‘by the name of Leibniz’s calculus differentialis’. Wallis was able to obtain a few precious fragments of Newton’s new analysis, and eagerly included them along with the epistolae in volumes 2 and 3 of Opera.

It is worth considering in more detail the manner in which these two mathematicians interacted. If Newton was so reluctant to print his discoveries about infinite series and fluxions, why did he allow Wallis to print some of his mathematical work on series and quadratures? And how did Wallis exploit the opportunity he was being offered?

The aim of this paper is to show that the correspondence between Newton and Wallis and the editorial work that the latter performed after obtaining information on Newton’s results and permission to publish them reveals that the two men held different views on mathematical method and adopted different publication policies, while finding it feasible and advantageous to cooperate with one another.

NEWTON’S AND WALLIS’S EARLY MATHEMATICAL CAREERS

Before turning to a consideration of the relationship between Wallis and Newton, I shall broadly describe their mathematical careers up to the time their lives intersected in the mid 1670s. Newton’s career began in the anni mirabiles he spent as a young scholar in Cambridge between 1664 and 1669, when he discovered the method of infinite series and fluxions, an algorithm that may be considered ‘equivalent’ to Leibniz’s differential and integral calculus. From his youth, Newton believed that mathematics could yield certain knowledge about the natural world. He took a critical stance against the probabilism and mitigated scepticism endorsed by many eminent contemporaries of his who were particularly influential in the Royal Society, including Robert Boyle. Boyle, Robert Hooke, Thomas Sprat, Joseph Glanvill and many other members of the Society maintained that experimental philosophers were to avoid all ‘confidence’, ‘dogmatism’ and ‘arrogance’: that their aim was to formulate conjectures supported by careful experimentation, rather than truths about natural laws deduced by mathematical reasoning. To the contrary, in 1670 Newton claimed in his Lucasian optical lectures that, by applying geometry to natural philosophy, ‘instead of the conjectures and probabilities that are being blazoned about everywhere, we shall finally achieve a science of nature supported by the highest evidence.’

Much of the polemic between Hooke and Newton over the paper that the latter had published on light in 1672—and in which he illustrated his experimentum crucis—can be viewed as a clash between two different conceptions of method. Much to Hooke and Oldenburg’s disapproval, Newton claimed that his new theory of light was not

Hypothesis but most rigid consequence, not conjectured by barely inferring ‘tis thus because not otherwise or because it satisfies all phaenomena (the Philosophers
Yet, when confronted with the problem of defining what mathematical methods might have such an important role, Newton found himself in the somewhat uncomfortable position of having to critically distance himself from his early algebraic methods, which lacked the certainty and elegance he appreciated in the writings of ancient geometers. Newton, the great algebraist and inventor of calculus, then downgraded his early algorithmic techniques, making them into mere instruments of discovery. In his view, the algorithmic techniques he had discovered in his *anni mirabiles* were not instruments of proof, and thus could not be deployed to inject certainty into natural philosophy. Consequently, in the 1670s Newton devoted himself to geometry hoping to find a substitute for algebraic methods. In attempting to revive ancient geometrical methods he reached important results, but these advancements were never to render algebra and calculus superfluous. In short, Newton could not dispense with algebra, series and fluxions in his mathematical practice. Consider for instance his study of cubics (which was printed in 1704 as an appendix to *Opticks* as *Enumeratio linearum tertii ordinis* (published for S. Smith and B. Walford, London, 1704)) or certain advanced parts of *Principia* where Newton based his propositions on a ‘method for squaring curvilinear figures’. In these cases we have evidence that Newton obtained his results via algorithmic techniques, not geometry. Newton’s predilection for geometrical methods despite his frequent use of symbolical ones generated a tension between his methodological convictions and his mathematical practice—a tension he resolved by engineering a publication strategy that confined algebraic and calculus methods to what is sometimes called ‘scribal publication’. That is, Newton disseminated some of his early discoveries in algebra and calculus via the circulation of his private correspondence and manuscripts. As he explained to David Gregory in 1694: ‘Our specious algebra is fit enough to find out, but entirely unfit to consign to writing and commit to posterity.’

The paradoxical result of all this is that Newton’s printed works, most notably his celebrated *Principia*, lack the evidence he was searching for and present notable gaps in their demonstrative structure. In many sections of *Principia* Newton concealed his use of symbolism, the result being that some of his demonstrations are incomplete. Newton never resolved this paradox. He attempted to come to terms with it by presenting himself as a follower of ancient geometers such as Euclid, who, he opined, had kept their methods of discovery hidden, and by using acolytes and friends—David Gregory, Nicolas Fatio de Duillier, John Keill, James Stirling and Wallis—as proxies. These people were then left with the burden of explicating the hidden symbolic subtext of Newton’s printed geometrical works, most notably his *Principia*—as previously mentioned—and *Enumeratio linearum tertii ordinis*.

By contrast with Newton, Wallis passionately sided with the defenders of the new algebraic methods, as his polemic with Thomas Hobbes clearly reveals. Armed with a solid background in theology, metaphysics and logic, Wallis first embarked on mathematical research in 1647–48, when he came across William Oughtred’s *Clavis mathematicae* (1631), whose third edition (1652) also proved to be an important source for the young Newton. The pronounced symbolism of Oughtred’s work fascinated Wallis, who remained a defender of the heuristic power of algebraic notation all his life. Wallis eventually came into contact with more advanced (continental) ideas in the early 1650s—and weighty results from Wallis soon followed. Most notably, in *De sectionibus conicis*...
Wallis defined conic sections in algebraic terms, and, even though geometry still guided his demonstrations, this work could be understood as supporting the approach to geometry that René Descartes had developed in his *Géométrie* (1637). Wallis’s masterpiece, *Arithmetica infinitorum* (1656), instead deals with a topic that had remained untouched by Descartes: ‘the quadrature of curvilinear figures’. This expression was used by seventeenth-century mathematicians to denote methods for the determination, for example, of the area of a surface bounded by a curve or the volume of a solid bounded by curvilinear surfaces. Nowadays, the methods to solve these problems are considered part of the integral calculus. The quadrature of curvilinear figures was a very important subject in the mid seventeenth century, not least because of its applications to natural philosophy.

The young Newton was deeply indebted to Wallis’s work on quadratures: his inventive annotations to *Arithmetica infinitorum* have been published by Whiteside in volume 1 of Newton’s *Mathematical papers*. Newton made the first great mathematical discovery of his life, that of the binomial series for fractional powers, in the winter of 1664/5 by generalizing Wallis’s work. As mentioned above, in the early 1670s Newton reached the conviction that results such as the binomial series were useful tools of discovery that lacked the certainty of ancient geometry.

Wallis instead profiled himself as one of the staunchest defenders of the primacy of algebra over geometry, and was happy to publish his heuristic methods in print. His approach to the publication of mathematical heuristics is best revealed in his polemic with Pierre de Fermat and Hobbes on the methods used in *Arithmetica infinitorum* (1656). Wallis claimed that he deserved praise both for having discovered a new heuristic method and for having made this available in print. Against Fermat, who had contrasted the rigour and elegance of the methods used by ancient geometers with the algebraic techniques in *Arithmetica infinitorum*, Wallis expressed admiration for the ancient synthetic method while formulating a defence of the heuristic power of symbolism. Wallis emphasized that he valued the ancient method, but that his work was about a method of discovery, not a method of proof. To criticize his work, as Fermat had, was to miss the point. Wallis wrote:

> To the Elegance and neatness of the Ancients’ way of Construction and Demonstration, I am not Enemy. And that these Propositions might be so demonstrated, I was far from being ignorant. [... Fermat] doth wholly mistake the design of that Treatise [the *Arithmetica infinitorum*]; which was not so much to shew a Method of Demonstrating things already known (which the Method that he commends, doth chiefly aim at,) as to shew a way of Investigation or finding out of things yet unknown: (Which the Ancients did studiously conceal). [... And that therefore I rather deserve thanks, than blame, when I did not only prove to be true what I had found out; but shewed also, how I found it, and how others might (by those Methods) find the like.]

Newton knew about Fermat’s criticism of Wallis, because the letters of the French mathematician had been included in Wallis’s *Commercium epistolicum*, which Newton had read in his early years. Hence, Newton would have found the prospect of being embroiled in a similar dispute devastating for the image he wished to give of himself as a mathematical natural philosopher. Those who, like Wallis and, later, Leibniz, were publishing the modern symbolical methods knew that they had to face criticism from those who sided with the *veteres* against the *recentiores*. An aspect of Newton’s intellectual biography that cannot be overestimated is that in his maturity he shared many
of the values of those critical of Wallis’s heuristics; so, in a way, he became critical of himself, because much of his early work was indebted to *Arithmetica infinitorum*. As we shall see below, this divergence between Wallis and Newton on the matter of mathematical methods and publication strategies emerges in their correspondence.

**Wallis’s use of Newton’s epistolae in his English Algebra**

Wallis belonged to a network of mathematicians who exchanged information on things such as antiquarian findings, new thrilling results and new publications. One of the most active promoters of these epistolary exchanges was John Collins. Wallis and Collins corresponded in the years 1676 and 1677 on one of the most remarkable events that had occurred at that time in the world of mathematics: the exchange of letters between Newton and Leibniz (via Oldenburg). The Wallis–Collins correspondence reveals that Wallis might have had a difficult time acquiring the two great letters that Newton had sent to Leibniz in 1676. The *epistola prior* summarizes the results in quadratures via infinite series contained in the *De analysi per aequationes numero terminorum infinitas*, the famous tract that Newton had completed in 1669 and that circulated via Barrow and Collins among a close circle of English and Continental mathematicians before finally being printed in 1711. The *epistola posterior* deals instead with an amazing array of results, most notably the so-called ‘prime theorem’ on quadratures and other advanced quadrature techniques. This letter is actually a small, carefully written treatise that summarizes most of Newton’s mathematical results.

Collins seems to have been somewhat reluctant to hand over Newton’s material to Wallis, perhaps because of Wallis’s reputation for publishing other people’s work without acknowledgement. Wallis was certainly keen on employing material from manuscripts and private correspondence in his printed works. He adopted a historicist approach, consistent with his role as *custos archivorum* in Oxford. As a historian he was far from unbiased. In writing about the history of mathematics, Wallis had two aims in mind: one was to vindicate the primacy of British mathematicians; the other was to highlight the crucial importance of his own work for the advancement of mathematical learning. Nationalism and self-aggrandizement are prominent features of Wallis’s mathematical works. Wallis often presents himself as an old man gazing with great satisfaction—and a somewhat patronizing air—at the achievements of a new generation of (preferably British) mathematicians who are building new superstructures on Wallisian foundations. In this context, Wallis’s claim of Nicolaus Mercator for the British camp aroused scandalized protests on Leibniz’s part. Eventually (between 22 February 1676/7 and 8 October 1677), Wallis obtained a copy of Newton’s *epistolae*, it seems from Oldenburg to include in his *Algebra*. How did Wallis deploy the much-sought-for Newtonian *epistolae*? The 1685 edition of *Algebra* features English translations of excerpts from these letters: Newton’s *epistola prior* is paraphrased almost in its entirety. Little use is made instead of the *posterior*. Excerpts from the *posterior* are deployed (again freely paraphrased) to provide explanations of statements occurring in the *prior* (for instance, Newton’s explanations, in answer to Leibniz’s request of 17/27 August 1676, of the discovery of the binomial theorem, and the parallelogram method for the resolution of ‘affected’ equations, are included). Indeed, in the *prior* Newton had simply stated the binomial theorem and what
later came to be called the Newton–Raphson method, and in the posterior he described how he found the binomial theorem during the plague (‘Eo tempore pestis ingruens’), and detailed the ways in which he obtained the general form of the coefficients and provided an explanation of the parallelogram rule.

Many results of the posterior are not included in the English Algebra. In 1693 Wallis explained that the many ‘omissions’ in the English Algebra (that is, most of the results from the posterior) were due to the fact that he ‘hoped that Newton would have published these results’. In the English Algebra Wallis intersperses his own commentary and historical notes. Pride of place is given to the binomial theorem, and to its applications to quadratures, and Wallis spares no words to stress the fact that Newton’s result rests on his own Arithmetica infinitorum. Wallis refers to the binomial theorem as derived from his own results, and he compares both Newton’s and Leibniz’s series for approximating π (stating that the former series converges more quickly). In his reconstruction of the history of mathematics, Wallis praised the new algebraic methods, while interpreting them according to an evolutionary view of mathematical development. In his Algebra Wallis is at pains to show how a process of continuous development and generalization can be traced from Archimedes’s exhaustion techniques and Cavalieri’s geometry of indivisibles down to his own arithmetic of indivisibles. Another aspect of Wallis’s historicist account of mathematics in Algebra is the fundamental place he assigns to his own work. What Wallis was eager to claim is that the achievements of the younger generation of mathematicians to which Newton belonged rested on his own Arithmetica infinitorum. When Wallis presents Newton’s method of numerical approximation (the ‘Newton–Raphson method’ referred to above) paraphrasing from the prior, he underlines the similarity with his method expressed in Commercium epistolicum (1658) (Epist. 17 and Epist. 19).

A continuist approach to history characterizes the Latin Opera as well, to which I turn in the next section. A typical statement—one of the many of this tenor that can be found in volume 2 of Opera—is the one that Wallis made referring to Newton’s method of fluxions. He stated that Newton’s method was similar to Leibniz’s, but also to the more ancient one due to Barrow in his Lectiones geometricae (Godbid, London, 1670). Wallis made it clear that Newton’s greatest result (the method of series and fluxions) was the product of a typically British school, of which Isaac Barrow was also a member. He triumphantly concluded that both Newton’s and Leibniz’s calculi were built on Arithmetica infinitorum. In a way Wallis was right: we should see the invention of the calculus as an achievement that is due to the efforts not only of Newton and Leibniz but also of their predecessors.

In short, Newton provided Wallis with a means of constructing a historical account that exalted the British, as well as Wallis’s role within the British school.

**Wallis’s use of Newton’s epistole in his Latin Opera**

Before we turn to Wallis’s Latin Opera, it is worth mentioning two episodes that occurred when the English Algebra was in press. These two episodes concern two Scots—John Craig and David Gregory—and they both had a remarkable impact on Newton’s strategy for the publication of his mathematics.

In 1684 David Gregory entered Newton’s life in a traumatic way. It was traumatic because in a letter dated 9 June 1684 Gregory announced the publication of Exercitatio geometrica
de dimensione figurarum, a work that contained his uncle James’s results on squaring via infinite series, as well as some results of his own.29 Basically, the Exercitatio contained most of the results on infinite series that Newton had included in his De analysi. After receiving Exercitatio geometrica, Newton began writing two dossiers on quadratures, Matheseos universalis specimena and De computo seriern, which incorporated material from his two letters to Leibniz.30 This manuscript shows that at this juncture Newton considered the possibility of using his two epistolae as evidence to claim priority for the devising of quadrature techniques.31

The second episode that deserves our attention is the following. In 1685 John Craig, a talented Scottish mathematician, visited Newton. Newton showed Craig one of the pillars of his higher quadrature methods, the so-called ‘prime theorem’ that featured as the centrepiece of his epistola posterior. Craig was close to publishing his pioneering treatise on quadratures, Methodus figurarum lineis rectis & curvis comprehensarum quadraturas determinandi (M. Pitt, London, 1685). This short treatise was written using in some propositions Leibnizian differential notation and gave credit to a number of mathematicians. Newton was cited in passing, together with Mercator, Barrow, Wallis, Hendrik van Heuraet, William Neil, David Gregory and Leibniz. Ehrenfried Walther von Tschirnhaus was sharply criticized. Newton’s decision to reveal his prime theorem to Craig finds perhaps its justification in his awareness that Gregory, Craig, Tschirnhaus and Leibniz were all moving towards the discovery of quadrature methods.

In November 1691 David Gregory wrote to Newton about the prime theorem on quadratures. In his letter Gregory claimed the theorem as his own invention.32 It is unclear whether in doing so he was telling a blatant lie or whether he really believed that he had found the theorem on his own, perhaps by deriving it from his uncle’s work. Be that as it may, we know that Craig complained about Gregory’s theft of the prime theorem in a letter to Colin Campbell dated 30 January 1688/9 and, later, in De calculo fluentium libri duo (ex Officina Pearsoniana, London, 1718). According to Craig, the theorem had been communicated by Craig to Gregory on his return to Scotland after the visit to Newton. Indeed, Gregory had printed the theorem, three years before approaching Newton, as his own discovery in Archibald Pitcairn’s Solutio problematis de historicis seu inventoribus (J. Reid, Edinburgh, 1688).33 Further, on 21 July 1692 Gregory sent Wallis the prime theorem for inclusion in his Opera.34 Gregory was claiming that the theorem was invented by him (‘illa Methodus quam a me inventam’35 but he conceded that after sending it to the press he came to know that Newton had discovered it ‘by a different method’ (‘ad eam diversa metodo ante, ut opinor, prevenerat’ (ibid.)). In the second volume of Opera, Wallis was at pains to give an historical account of the events after Craig’s visit to Newton and the publication of Gregory’s theorem in Pitcairn’s book, an account designed to be both truthful and not too offensive for his ‘Collega dignissimus’ (ibid., p. 391).

Probably informed by Wallis of these exchanges between Wallis and Gregory, Newton’s reaction was one of panic and rage. After receiving a request of elucidation on some methods of series expansion and quadrature in a letter sent him by Wallis on 13 August 1692,36 Newton immediately set himself the task of writing extensively on quadratures, and did so with his characteristic forensic style—his aim being that of showing his priority in these important matters.37 The first drafts of what became Tractatus de quadratura curvarum, the treatise published in 1704 as an appendix to Opticks, were written in this context. The next thing that Newton did was to send Wallis extracts illustrating his higher quadrature
methods. He did so, at Wallis’s own request, on 27 August and 12 September 1692. These extracts were included in pp. 390–396 of volume 2 of Opera, which was published in 1693, two years before the first volume. It is here that the first printed instance of fluxional notation occurs. In volume 2 of his Latin Algebra, Wallis provided not only an almost complete reproduction of Newton’s epistola prior, which had already appeared in English in 1685, but also more paraphrases from his epistola posterior. He included Newton’s deciphering of the two string anagrams, an explanation—missing in the epistola posterior—of the dotted notation for fluxions (including higher-order fluxions) and of the rules of the direct method of fluxions (equivalent to the basic rules of the differential calculus that Leibniz had published in 1684) (ibid., pp. 391–393), an explanation of the theorema primum (ibid., pp. 390–391), and more theorems on quadratures, not in the posterior, on which Newton was working in the early 1690s (ibid., pp. 392–396). All this new material was of course sent to Wallis by Newton.

The full text of the two epistolae was eventually printed in the third volume of Opera. According to Turnbull, the copies sent to Wallis were transcribed in Collins’s hand and reached Wallis in December 1696, or shortly afterwards, via Gregory. By then, the confrontation with Leibniz was becoming more and more menacing. All these events were to have a momentous role in the controversy with Leibniz. Here I briefly mention that in the Ad lectorem praefatio to the first volume of Opera (1695) Wallis pointed out that the two epistolae had been communicated to Leibniz, expressing himself in a way that gave the impression that this also applied to the account on fluxions added in the second volume of Opera or that in any case Newton had explained the method of fluxions to Leibniz in 1676. It must be added that the rich correspondence between Wallis and Leibniz was always carried on with great courtesy, even though it is clear from the correspondence between Wallis and Newton—as will appear in the next section—that the former had all the intentions of defending the rights of his younger English colleague against the schemings of the Continentals.

THEMES CONCERNING MATHEMATICAL METHOD IN THE CORRESPONDENCE BETWEEN WALLIS AND NEWTON

The extant letters that Wallis and Newton exchanged in the 1690s contain many references to the publication of Newton’s results in Opera. These letters are also highly revealing of Wallis’s nationalism, his different approach to the publication of heuristic mathematical results, and his combative willingness to withstand criticism addressed against the new methods.

This is how Wallis addressed Newton in an exceptionally intimate letter dated the 30 April 1695:

It hath been an old complaint, that an Englishmen never knows when a thing is well... I owe that Modesty is a Vertue; but too much Diffidence (especially as the world now goes) is a Fault.

Just ten days earlier an alarmed Wallis had written to Newton:

I wish you would also print the two large Letters of June & August [sic!] 1676. I had intimation from Holland, as desired there by your friends, that something of that kind were done; because your Notions (of Fluxions) pass there with great applause by the
Wallis as editor of Newton

name of Leibnitz’s Calculus Differentialis. . . . You are not so kind to your Reputation (& that of the Nation) as you might be, when you let things of worth ly by you so long, till others carry away the Reputation that is due to you. I have endeavoured to do you justice in that point; and am now sorry that I did not print those two letters verbatim.\(^{47}\)

Wallis was suspicious of the machinations of the Continentals and was eager to exploit Newton’s great letters of 1676 as a weapon against them. Already on 16 September 1676 he had written, referring to the French, the following words to Collins regarding the *epistola prior*:

I would be content to have a copy of Mr. Newton’s letter, which you mention, having a very good esteem of the person, though no acquaintance with . . . I had rather see it in print . . . As to our correspondence with the French, I like it best when it is done in print; they being apt to be disingenuous, in claiming for their own which they have from hence, without owning whence they have it.\(^{48}\)

On 8 October 1677, referring to both the *epistolae*, Wallis wrote:

I am still of opinion that Mr. Newton should perfect his notions, and print them suddenly. These letters, if printed, will need a little review by himself; for there be some slips in hasty writing them.\(^{59}\)

At a time when publication practices were drastically changing—mainly because of the founding of scientific societies and launching of periodicals—Wallis showed keen awareness of the fact that scribal publications were not sufficient evidence in themselves for those wishing to claim priority for mathematical discoveries. The risk of plagiarism was lurking behind Newton’s excessively relaxed approach to the scribal circulation of his private correspondence and manuscripts.

From Newton’s viewpoint, printing new mathematical methods posed a different kind of risk, however: not so much one of plagiarism—as Wallis feared—as one of criticism from classicists. Newton, who attributed great importance to the imitation of the ancients as part of his philosophical agenda, would have found it difficult to put up with such criticism. In contrast, Wallis—a well-known ruthless polemicist—regarded this risk as a mere nuisance that was not worth worrying about. He seems to have shown little understanding of the subtleties of Newton’s anxieties regarding the philosophical dimension of the debates concerning mathematical method as well as the theory of colours. Referring to *Opticks*, in a letter dated 30 April 1695, Wallis wrote to Newton: ‘when published that trouble will be over. You think, perhaps, it may occasion some Letters (of exceptions) to you, which you shal be obliged to Answer. What if so? ’twill be at your choise whether to Answer them or not.’\(^{50}\)

The ‘trouble’ here, we may surmise, would have been a recurrence of the polemic that Newton had first been engaged in with Hooke on the matter of his *experimentum crucis*, and which had put his patience to a hard test in the 1670s. In many places in his correspondence we find Wallis reassuring Newton about the predictable controversies that the publication of his new mathematical methods and *Opticks* were bound to engender.

What Wallis, the great controversialist, could not understand is that the ‘letters of exceptions’ he was willing to regard as mere nuisances would have proven lethal for Newton, who sought to present himself as the author of mathematical works that stood above all dispute, just as much as Apollonius’s *Conics*. 
CONCLUSION

The print publication of Newton’s new and controversial symbolic methods entailed precisely the kind of authorial commitment he sought to avoid. But the advancement of mathematics in the 1680s in Britain and on the Continent compelled Newton to revise his scribal publication practices. In the mid 1680s, David Gregory was promoting his uncle’s achievements on infinite series. In 1684 Leibniz published his *Nova methodus* in *Acta eruditorum*, and in 1685 Craig published his results on quadratures in Leibnizian notation giving little credit to Newton. Making his two *epistolae* available to Wallis for publication enabled Newton to affirm his own achievements more visibly. At the same time, by publishing his results in Wallis’s work Newton managed to keep a distance from the unsafe and modern symbolic manipulations on which they rested. One might say that Wallis was rendering available in print mathematical results expressed in the register of scribal publication. Indeed, in his 1685 *Algebra* and 1693 *Opera*, Wallis printed a paraphrased *commercium epistolicum* between Newton, Oldenburg and Leibniz, interpolated with historical and mathematical commentaries. And this is the epistolary register that Newton considered as acceptable for his symbolic, heuristic, methods.

This is not the only example of the way in which Newton used the printed works of his acolytes and friends to promote ideas he was uncomfortable at explicitly authoring. An analogy can be drawn here with the policy Newton adopted in his so-called ‘classical scholia’, the long lubrication on the wisdom of the ancients he composed in the 1690s and which he considered adding as *scholia* to some propositions of book 3 in a new edition of *Principia*. Actually, knowledge about these theologically controversial *scholia* was circulating among Newton’s acolytes, and the *scholia* first appeared in print in the Praefatio to David Gregory’s *Astronomiae physicae & geometricae elementa* (e Theatro Sheldoniano, Oxford, 1702). Gregory had learnt about Newton’s idea of *prisca sapientia* during an extended visit he had paid him in May 1694. Newton allowed his classical *scholia* to be printed in the work of one of his acolytes, thus avoiding a too explicit authorial commitment to the theologically controversial theses these contained.

Wallis and Newton differed in many respects in their mathematical agendas and publication practices, but they were nonetheless able to cooperate quite effectively. Newton used Wallis for his own purposes. He printed his heuristic methods in Wallis’s works in such a way as to avoid any tight authorial commitment to them. In turn, Wallis used Newton: Newton equipped Wallis with important ammunition for his nationalist programme and for his self-aggrandizement as the father of new symbolic British mathematics.

APPENDIX 1: A LIST OF THE EXTANT LETTERS BETWEEN WALLIS AND NEWTON

(1) Wallis to Newton, 13 August, 1692. The Centre for Kentish Studies, at County Hall in Maidstone, Kent (MS U120 F15.6).
(3) Newton to Wallis, 27 August 1692. Original lost. Translation of an extract from Wallis, *op. cit.* (note 17), pp. 390–391 in Newton, *op. cit.* (note 1), vol. 3, pp. 220–221. Whiteside suggests it is of a later date (winter 1692–93) and that it was drafted in
reply to Wallis's request for comments on the proofs of Wallis, *op. cit.* (note 17), pp. 390–396. See Newton, *op. cit.* (note 1), vol. 7, p. 392. However, Philip Beeley (personal communication) teaches me that 'We also have the beginning of what appears to be a further letter to Newton, dated 1 September 1692. It reads “Sir, Your letter (which I received here last night (the carrier being <illegible>) was very wellcom’d.” The fact that the letter came with a carrier makes clear that it was too large to come with the post. The editors of Newton [*op. cit.* (note 1)] re-dated Newton’s letter to Wallis to Winter 1692/3, but the date 27 August would have to be seen as correct for Newton’s reply in which he provided more information on the method of fluxions. By this time, the proofs of Volume 2 of Wallis’s *Opera mathematica* [*Wallis, op. cit.* (note 17)] were not ready. Newton’s corrections to those proofs probably are to be dated end of 1692.'

4) Wallis to Newton, 1 September 1692. Incomplete, page is cut at the bottom. The Centre for Kentish Studies, at County Hall in Maidstone, Kent (MS U120 F15.7).


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NOTES

1 Ten letters are easily accessible in volumes 3, 4 and 7 of Isaac Newton, The correspondence of Isaac Newton (Cambridge University Press, 1959–77). Two letters in Wallis’s hand have recently been discovered in the library of the Centre for Kentish Studies, at County Hall in Maidstone, Kent. I thank Philip Beeley for sharing this information with me. In Appendix 1 I provide a list of these letters.


3 Wallis to Newton, 10 April 1695. Newton, op. cit. (note 1), vol. 4, p. 100.


5 Newton, op. cit. (note 1), vol. 1, pp. 96–97.


11 Newton, op. cit. (note 2).


14 See especially Wallis to Collins, 16 September 1676, Cambridge University Library, Macclesfield Collection (MS Add. 9597/13/6/251–253); 22 February 1676/77 (MS Add. 9597/13/6/254); 8 October 1677 (MS Add. 9597/13/6/256). Rigaud, Correspondence of scientific men of the Seventeenth Century (Oxford University Press, 1841), vol. 2, pp. 600, 604–605 and 608–609.


16 Slightly modernizing the notation, one can say that the ‘primum Theorema’ in the epistola posterior (Newton, op. cit. (note 1), vol. 2, p. 115) concerns the calculation of the area under
the curve $y = x^p(a + bx^q)\), \(a \text{ and } b \text{ constants, } p, q \text{ and } r \text{ rational numbers, in terms of a power series. For a discussion of the prime theorem see Guicciadini, op. cit. (note 7), pp. 202–204. '… and Mr. Nicholas Mercator, with us'. Wallis, op. cit. (note 12), p. 346.

17 In the Latin edition Wallis replaced a generic 'which I have seen' (Wallis, op. cit. (note 12), p. 330) with 'quas [Epistolae] inde mihi impertivit Oldenburgius'. John Wallis, De algebra tractatus, historicus & practicus, anno 1685 Anglice editus, nunc auctus Latine (e Theatro Sheldoniano, Oxford, 1693), p. 368. Information on the transmission of Newton’s 1676 epistolae to Wallis can be gathered from the following letters. Wallis to Collins, 16 September 1676: ‘Quid Newtonus praestitit ne vidi quidem, nec (ante tuas receptas literas) de Leibnittii hac in re meditatis quidquam….I would be content to have a copy of Mr. Newton’s letter, which you mention, having a very good esteem of the person, though no acquaintance with him.' Rigaud, op. cit. (note 14), vol. 2, pp. 598 and 600. In February the epistolae had not yet been delivered to Wallis. Wallis to Collins, 22 February 1676/7: ‘[I am] expecting from you an extract of Mr. Newton’s letters.’ Rigaud, op. cit. (note 14), vol. 2, p. 605. In October 1677 the epistolae were in Wallis’s hands. Wallis to Collins, 8 October 1677: ‘I am still of opinion that Mr Newton should perfect his notions, and print them suddenly. These letters, if printed, will need a little review by himself; for there be some slips in hasty writing them.’ Rigaud, op. cit. (note 14), vol. 2, p. 609.


22 Newton, op. cit. (note 1), vol. 2, p. 113.


Exercitatio on James Gregory, *Geometriae pars universalis* (Typis Heredum Pauli Frambotti, Padua, 1668) and *Exercitationes geometricae* (M. Pitt, London, 1668) and added some results on the squaring of curves via infinite series.

Collated from Cambridge University Library (MS Add. 3964.3, ff. 7r–20v), and other manuscripts by Whiteside in Newton, *op. cit.* (note 2), vol. 4, pp. 526–617.

See the edited version of the *epistola posterior* that Newton planned to include in *Matheseos universalis specimina* in Newton, *op. cit.* (note 2), vol. 4, pp. 618–633.


Craig complained about Gregory’s theft of the prime theorem in a letter to Colin Campbell dated 30 January 1688/9 and in the *Praefatio ad lectorem* in John Craig, *De calculo fluentium libri duo; quibus subjunguntur libri duo de optica analytica* (ex Officina Pearsoniana, London, 1718). See Newton, *op. cit.* (note 2), vol. 7, p. 6, n. 16.


Wallis to Newton, 13 August 1692, Centre for Kentish Studies, at County Hall in Maidstone, Kent (MS U120 F15.6). Wallis writes, referring to chapter 95 of *Algebra*, which is devoted to the application of series expansions to quadratures, rectification of curves, cubatures of solids, and so on: ‘Sir, I thought fit to acquaint you, that I am now printing my Algebra in Latine, here at Oxford. Above three score sheets are already printed, & we go on apace, about 5 sheets a week. We shall quickly come to that part which concerns you; wherein I shall be willing to do you all the right I can. What you think fit to have added, altered or amended, in what concerns you: I shall therein observe your directions. Particularly, I desire [you] favour me with the two Methods intimated, chap. 95, pag. 345, the one more Ready, the other more General; which, with your leave, I shall insert in their due place. And, if the Printer have by mistake committed some Errors (of which you may be sooner aware than I) you may please to signify them, & what else you think fit to (as soon as with convenience you can, lesst they come too late.).’ I thank Philip Beeley for providing this information to me.

See Newton’s draft reply to David Gregory in Newton, *op. cit.* (note 2), vol. 7, pp. 21–23.


Wallis, *op. cit.* (note 18), pp. 391 and 393.

The excerpts from a draft version of *De quadratura* communicated to Wallis and printed in *Opera* are edited and commented by Whiteside in Newton, *op. cit.* (note 2), vol. 7, pp. 170–182.

The *epistolae* were printed in the *Epistolarum Collectio* in John Wallis, *Operum mathematicorum, volumen tertium* (e Theatro Sheldoniano, Oxford, 1699), pp. 622–629 and 634–645.

A transcript made by Collins of the *epistola posterior* was sent to Wallis via David Gregory and reached him in December 1696, or shortly afterwards. It is now held in the University Library in St Andrews. See Newton, *op. cit.* (note 1), vol. 2, p. 149.


‘Quae in Secundo Volumine habentur; in Praefatione eidem praefixa dicitur. Ubi (inter alia) habetur Newtoni Methodus, de Fluxionibus (ut ille loquitur) consimilis naturae cum Leibniti (ut hic loquitur) Calculo Differentiali (quod, qui utramque methodum contulerit, satis animadvertat, ut ut sub loquendi formulis diversis) quam ego descripsi (Algebrae Cap. 91 &c. praeerem Cap. 95) ex binis Newtoni literis (aut earum alteri) Junii 13. & Augusti [sic!] 24 1676, ad Oldenburgium datis, cum Leibniti tum communicandis (iisdem fere verbis, saltem
leviter mutatis, quae in illis literis habentur,) ubi methodum hanc Leibnitio exponit tum ante
decem annos, nedum plures, ab ipso excogitatam. Quod moneo, nequis causetur, de hoc
Calculo Differentiali nihil à nobis dictum esse.' John Wallis, Opera mathematica, volumen
primum (e Theatro Sheldoniano, Oxford, 1695), Ad lectorem.

45 For further information, the reader should consult Hall, op. cit. (note 2).
46 Newton, op. cit. (note 1), vol. 4, p. 117.
47 Newton, op. cit. (note 1), vol. 4, p. 100.
50 Newton, op. cit. (note 1), vol. 4, p. 116.