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How Leibniz tried to tell the world he had squared the circle

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Abstract

In 1682, Leibniz published an essay containing his solution to the classic problem of squaring the circle: the alternating converging series that now bears his name. Yet his attempts to disseminate his quadrature results began seven years earlier and included four distinct approaches: the conventional (journal article), the grand (treatise), the impostrous (pseudepigraphia), and the extravagant (medals). This paper examines Leibniz's various attempts to disseminate his series formula. By examining oft-ignored writings, as well as unpublished manuscripts, this paper answers the question of how one of the greatest mathematicians sought to introduce his first great geometrical discovery to the world.

Résumé

En 1682, Leibniz publie un essai contenant sa solution au problème classique de la quadrature du cercle : la série convergente alternée qui porte désormais son nom. Pourtant, ses tentatives de diffusion de ses résultats en quadrature ont commencé sept ans plus tôt et comprenaient quatre approches distinctes : la conventionnelle (article de journal), la grandiose (traité), l'imposture (pseudépigraphie) et l'extravagante (médailles). Cet article examine les diverses tentatives de Leibniz pour diffuser sa formule de série. En examinant des écrits souvent ignorés, ainsi que des manuscrits non publiés, cet article répond à la question de savoir comment l'un des plus grands mathématiciens a cherché à introduire sa première grande découverte géométrique dans le monde.

MSC: 01A45; 41A55; 51-03

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0. Introduction

In February 1682, Leibniz published a short essay entitled "De vera proportione circuli ad quadratum circumscriptum in numeris rationalibus ... expressa" [On the True Proportion of The Circle to The Circumscribed Square, Expressed in Rational Numbers], in which he related the key result of his inves-

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tigation into the classic problem of squaring the circle, that is, determining the area of a circle enclosed by a 1×1 square. About halfway through, he revealed to the public for the first time the alternating converging series that now bears his name[1]: "the square of the diameter being 1, the area of the circle is $\frac{1}{1}-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\frac{1}{9}-\frac{1}{11}+\frac{1}{13}-\frac{1}{15}+\frac{1}{17}-\frac{1}{19}$ etc." (Leibniz, 1682a, 44/Leibniz, 2022d).[2] The publication of this essay marked the end of Leibniz's attempts to disseminate his quadrature results, attempts which began seven years earlier and along the way included four distinct approaches that might best be described as the conventional (correspondence and a journal article), the grand (a treatise), the impostrous (pseudepigraphia), and the extravagant (medals). This paper is concerned not with an analysis of Leibniz's quadrature research or its methods,[3] or with its inspiration or genesis, but with these various attempts to disseminate and promote his series formula in the years immediately following its discovery.[4] By examining a number of oft-ignored writings, as well as various unpublished manuscripts, we can tell the story of Leibniz's attempts to make public his solution to the quadrature problem, and so answer the question of how one of the greatest mathematicians sought to introduce his first great geometrical discovery to the world.

The story is worth telling for the insight it provides into the various avenues available for the transmission of ideas in the seventeenth century as well as the strategic and sometimes creative thinking that lay behind Leibniz's decisions to avail or not to avail himself of them. It is worth remembering that when Leibniz discovered the general infinite series for squaring the circle in autumn 1673 (see A VII 4, 749–750; A VII 6, 24, 29–30; A VII 3, 282–288), he was just 27 and enjoyed a blossoming reputation as an inventor, legal scholar, and philosopher. He was at the time in the employ of the court of Mainz, for which he had been tasked to review the legal code. In February 1672 he had been sent to Paris on a diplomatic mission, remaining there until October 1676, following his decision to accept the position of court councillor in Hanover. At the time of his arrival in Paris, Leibniz had a few publications to his name, but none in mathematics.[5] In fact, when he began his Paris sojourn he was a mathematical novice; as he later recalled, he had arrived in Paris with "a superb ignorance of mathematics" (GM III, 71), an ignorance soon rectified by his intense application to the subject under the tutelage of Christiaan Huygens (1629–1695). Moreover, at the time Leibniz devised his quadrature formula he was on unpaid leave from his role in Mainz (see A I 1, 349), and his only other income, from working as tutor to the Boineburg family, came to an end soon afterwards, on 13 September 1674 (see A I 1, 396). The need to improve his prospects and secure suitable employment was not lost on Leibniz, who entertained various ways of exploiting his work on the quadrature to that end, several of which would have struck his contemporaries (as well as us) as quite unorthodox, if not plainly eccentric. Along the way, as we shall see, Leibniz learned a valuable lesson that would shape the way he disseminated his mathematical discoveries thereafter. But let's start at the beginning.

[1] It is sometimes called the Leibniz-Gregory series (or formula), or the Madhava-Leibniz series, in recognition of Leibniz discovering it independently of Mādhava of Sangamagrāma (c.1340–1425) and James Gregory (1638–1675).

[2] Where two references are separated by a /, the first is to the original language source, the second is to a published English translation. If no published translation is cited, the translation is my own. There exist two other English translations of "De vera proportione circuli": Leibniz (1682b) and A Society of Gentlemen (1742, 59–62). Both are dated but passable, though the latter omits certain passages.

[3] For which, see for example Knobloch (1989), Ferraro (2008, 25–44), Lützen (2014), Knobloch (2015), Knobloch's "Nachwort" in Leibniz (2016, 279–291), and Crippa (2019).

[4] Although this paper is focused on Leibniz's efforts to disseminate his quadrature results during the 1670s and 1680s, they were by no means restricted to this timeframe. For example, decades later, in 1710, Leibniz included the quadrature formula in an ornate frontispiece to the first volume of the journal *Miscellanea Berolinensia*, of which he was the editor. See Leibniz (1710, frontispiece).

[5] Though it should be noted that Leibniz's expanded doctoral dissertation, "Dissertatio de arte combinatoria," which was published in 1666, did have some mathematical content; see A VI 1, 163–230.

1. The conventional: correspondence and a journal article

Leibniz's quadrature formula first appears in his writings in a set of draft notes from the first half of 1674 (A VII 6, 74). In September or October 1674, Leibniz sketched a short essay for himself that would serve as the blueprint, at least in terms of its broad strokes, for several later attempts to disseminate his findings. Like its successors, this essay, "The Quadrature of the Circle by a Rational Progression" (A VII 6, 88–91/Leibniz, 2022e) begins with a short history of the quadrature problem and past attempts to solve it before presenting Leibniz's own solution in terms of the infinite series, albeit without proofs or methods (Leibniz simply states that he "had the good fortune at last" to find a simple, regular progression of rational numbers whose sum is equal to the circle; A VII 6, 89/Leibniz, 2022e). The essay concludes with the confident claim that as he had already succeeded in showing that the sum of other infinite series is equal to a finite quantity, for example the reciprocals of triangular numbers $\frac{1}{1} + \frac{1}{3} + \frac{1}{6} + \frac{1}{10} + \frac{1}{15}$ etc. (which sums to 2), the series for the quadrature of the circle would likely prove to be summable also.

With a valuable discovery in hand, Leibniz lost little time informing some of his more eminent correspondents, albeit without divulging his result or his methods. The first dated mention of Leibniz's quadrature is found in his letter of 15 July 1674 to Henry Oldenburg (1619–1677), secretary of the Royal Society. Here Leibniz restricts himself to informing Oldenburg that he has developed an important theorem "by means of which the area of a given circle or sector can be accurately expressed by a series of rational numbers continuously extended to infinity" (A III 1, 120). A follow-up letter three months later offered little by way of further detail.[6] Leibniz was similarly cagey when writing to the physicist Edme Mariotte (1620–1684) in October 1674:

> I had the good fortune of finally finding the transformation of the circle into another, *equivalent rational figure*, by means of which I can give a progression of infinite rational numbers, whose sum is finite and equal to the area of the circle, or the circumference, supposing the radius to be 1. (A III 1, 140/Leibniz, 2022a)

However, when writing a lengthy essay entitled "Arithmetical quadrature" for his mentor, Huygens, the same month, Leibniz was much more forthcoming, divulging not just his formula but also his proof and methods, captured in ten propositions, each with its own demonstration, several of which were based on a version of the versiera curve, or Witch of Agnesi (see A III 1, 154–169). Huygens annotated Leibniz's essay and returned it along with a letter dated 6 November 1674, in which he declared that Leibniz's work on the quadrature, was

> very fine and very felicitous. And to my mind, it is no small thing to have discovered, in a problem that has exercised so many minds, a new way which seems to offer some hope of reaching its true solution. (A III 1, 170)

[6] "You know that Viscount Brouncker and the distinguished Nikolaus Mercator produced an infinite series of rational numbers equal to a hyperbolic area. But up to now no one has been able to effect this for a circle, although, indeed, Wallis and the illustrious Brouncker gave rational numbers which approached more and more closely to it: nevertheless no one has given a progression of rational numbers whose sum continued to infinity is exactly equal to a circle. Truly, I have at last happily succeeded in this: for I have found a very simple series of numbers whose sum is exactly equal to the circumference of a circle, taking the diameter to be unity. And this series is further remarkable because it exhibits certain wonderful harmonies of the circle and hyperbola. And thus the problem of the quadrature of the circle, which up to now has been examined in vain, has been now transferred from geometry to the arithmetic of infinitesimals. Therefore it only remains to perfect the theory dealing with the sums of series or progression of numbers. Whoever has hitherto sought the exact quadrature of the circle has not so much as opened a path by which one might hope to be able to arrive at it; I would dare to state that this has now first been done by me. I can show exactly the ratio of the diameter to the circumference, not indeed by the ratio of a number to a number, for that would have been to discover it absolutely, but by the ratio of a number to the whole of a certain very simple and regular series of rational numbers" (A III 1, 130–131; English translation from Oldenburg, 1977, 101–102).

Huygens suspected that given Leibniz's success in summing other infinite series, it should be possible to find the sum of his quadrature series also, but even if this should prove to be impossible, Huygens informed his protégée, "you have nonetheless found a very remarkable property of the circle and one which will be celebrated forever among geometers" (A III 1, 171).

After the exchange with Huygens, Leibniz did not mention his formula to another correspondent for almost two years, next revealing it to Henry Oldenburg at the end of August 1676 (see A III 1, 575–576). Yet while Leibniz was clearly reticent to report his results in letters at this time, he had no such scruples doing so in face-to-face discussions. In the summer and autumn of 1676 he wrote that a number of his friends knew of his circle formula because he had told them about it (see A VII 6, 432, 515; A III 1, 575–576). Leibniz certainly had the opportunity to disseminate his formula verbally to many members of the mathematical community in Paris. Aside from his acquaintances with mathematical expertise, such as the mathematician Jacques Ozanam (1640–1718), the philosopher Nicholas Malebranche (1638–1715) and the physicists Mariotte and Jean Gallois (1632–1707), Leibniz had access to leading mathematicians through the Royal Academy of Sciences. Although not a member himself, Huygens, Mariotte and Gallois were. Moreover, Leibniz addressed the Academy twice in 1675, first on 9 January, to show his calculating machine, and again on 24 April, to show a portable watch of his own design.[7] These presentations would have given him the opportunity to meet such academicians as Pierre de Carcavi (1600–1684), Gilles de Roberval (1602–1675), Jacques Buot (1623–1678), and Philippe de La Hire (1640–1718). How many of these heard about the circle formula first hand from Leibniz himself is unknown, but there is evidence that at least two of them did, namely Mariotte and Ozanam. The circle formula appears on a sheet of scratch paper upon which both Mariotte and Leibniz recorded their ideas in October 1674 (see A VII 1, 882/Strickland and Lewis, 2022, 29), and on another containing an exchange of ideas between Ozanam and Leibniz in July – September 1676 (see A VII 6, 453).[8]

Having started to disseminate the quadrature formula in the latter half of 1674, more than a year elapsed before Leibniz made the first of his concerted attempts to transmit it to an audience wider that his circle of acquaintances in Paris. On 2 November 1675 he informed Gallois of his intention to publish his results in the form of letters (A III 1, 304), which had become a conventional way of disseminating findings since the launch of the periodicals *Journal des Sçavans* and *Philosophical Transactions* in 1665. Leibniz put his plan into action at the end of 1675, sketching a letter to Jean Paul de La Roque (d.1691), then editor of the *Journal des Sçavans*.[9] In the opening paragraph, Leibniz states:

> as I prefer not to write a volume stuffed with a great number of repeated propositions in order to give just one which is new and significant, I have recourse to your journal, which gives us the means of publishing a theorem without writing a book. (A III 1, 337 and 344–345)

[7] The Academy's minutes for 9 January read: "The same day Mr. Leibniz showed his numerical machine, for performing the 4 rules of arithmetic with great ease, the description of which he will give to put in the Register. Mr. Leibniz will make his estimate and take care to send for the workman. We will make a deal with him, and we will pay him, provided the machine succeeds" (Académie royale des sciences, 1675–1679b, 1v). The Academy's brief report on Leibniz's presentation of the portable watch ends: "He will take the trouble to correct it, and compare it with a pendulum clock" (Académie royale des sciences, 1675–1679b, 39v).

[8] See also A VII 5, 299, where the circle formula appears on a sheet of scratch paper upon which both Leibniz and Ehrenfried Walther von Tschirnhaus (1651–1708) wrote their ideas in October 1675.

[9] Two versions of this letter are printed in the Academy edition of Leibniz's writings: A III 1, 337–344, corresponding to a so-called original draft, and A III 1, 344–353, corresponding to a so-called reworking of the original draft. However, there are problems with this division. Leibniz left a single manuscript (LH 35, 2, 1, Bl. 44–45), which was heavily revised, and some of the material that the Academy editors treat as being part of the original draft but not the reworked draft, such as the second passage I quote below ("I call this quadrature *arithmetical*..."), is in fact left intact in the manuscript, suggesting that Leibniz may have intended to retain it in a fair copy, had he prepared one. I therefore treat Leibniz's letter to de La Roque as a single text and do not distinguish between original and reworked draft versions.

After a brief history of unsuccessful attempts to find the quadrature of the circle, Leibniz turns to his own solution, and how it fits into his classification of the various kinds of quadratures:

> I call this quadrature *arithmetical* because it expresses exactly the magnitude of the circle and of its portions by an infinite string of numbers that are rational or commensurate with a given magnitude. If it gave only a means of approximating in lines or numbers, without giving a theorem of equality, it would be *mechanical*. If this range were finite, or if the sum of this range could be expressed by numbers or roots of a finite number, the quadrature would be *analytical*; or if it could be constructed by lines describable in one stroke, it would be *geometrical*, unless these lines suppose the extension of a material curve in a straight line, or something similar; for such a quadrature could rightly be called *physical*. (A III 1, 338)

With the classification laid out, Leibniz presents his theorem:

> The arithmetic quadrature of the circle and of its parts can be understood in this theorem: with the radius of the circle being 1, and the tangent BC of half BD of a given arc BDE being called b, the magnitude of the arc will be: $\frac{b}{1} - \frac{b^3}{3} + \frac{b^5}{5} - \frac{b^7}{7} + \frac{b^9}{9} - \frac{b^{11}}{11}$ etc. Now, once the arcs are found, it is easy to find the areas, and the corollary of this theorem is that when the diameter and its square are 1, the circle is $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11}$ etc. (A III 1, 345, cf. 339)

The letter continues with a presentation of Leibniz's transmutation theorem (by means of which he was able to work out the area of a segment of a cycloid and so derive geometrical demonstrations not just for all of the perfect quadratures so far known but also his quadrature of the circle) and concludes on the hopeful note that his quadrature series may yet prove to have a finite sum, expressible in rational numbers, as is the case with several other infinite series derivable from it.

Had Leibniz sent this letter and de La Roque deemed it suitable for publication, it would have been Leibniz's second paper in the *Journal des Sçavans*, following a short extract on portable watches that appeared in the issue dated 25 March 1675 (see Leibniz, 1675). However, the letter was never sent, nor was a fair copy even made, making it unlikely that Leibniz was merely dissatisfied with the exposition: had that been the case, he could simply have redrafted until he was happy. The fact that Leibniz made no attempt to do so suggests he realized his letter was not a good fit for the *Journal des Sçavans*. His lengthy letter, with its technical terminology, algebraic formulae, and busy diagrams of lines and curves, would have sit uneasily alongside the journal's usual content of short reports of recently-published books, mostly on non-science subjects such as religion, history, and classics. The journal did occasionally publish short excerpts of 1–3 pages from letters written to the editor (indeed, all of Leibniz's earliest publications in the journal were of this format; see Leibniz, 1675, 1677a, 1677b, 1678a/2021a, 1678b, 1681), but Leibniz's letter would have proved challenging to excerpt and would have occupied at least 10 journal pages if printed in full. In short, Leibniz had drafted a paper of a sort that the journal had not published before and arguably stood outside its ambit.[10] With its target readership of the educated public rather than specialist mathematicians, the *Journal des Sçavans* was not the right outlet for Leibniz to announce his quadrature formula. A different approach was needed. And it did not take Leibniz long to find one.

2. The grand design: the treatise *De Quadratura arithmetica*

In June 1676, he began work on *De Quadratura arithmetica circuli ellipseos et hyperbolae cujus Corollarium est trigonometria sine tabulis* [On the Arithmetic Quadrature of The Circle, The Ellipse, and The

[10] In terms of its length and subject matter, Leibniz's essay was better suited to the *Philosophical Transactions* of England, though he appears not to have considered submitting it there.

Hyperbola, a Corollary of Which Is Trigonometry Without Tables], a book-length treatise that drew together more than three years of work on the quadrature problem (A VII 6, 521–676).[11] Comprising 51 propositions, almost half as many scholia, as well as various corollaries, definitions, and figures, it is the longest mathematical work Leibniz ever wrote. Propositions 1 to 11 provide justification for the infinitesimal techniques that serve as the methodological foundation of the whole treatise. Subsequent propositions apply these techniques to various curves and figures, such as the cycloid, parabola, and hyperbola, before coming to the circle. In proposition 31, Leibniz derives the arc tan series: in a circle of center A, where BC is the tangent and AB the radius, the length of arc BO whose trigonometric tangent is BC is "BC $- \frac{\boxed{3}BC}{3\boxed{2}AB} + \frac{\boxed{5}BC}{5\boxed{4}AB} - \frac{\boxed{7}BC}{7\boxed{6}AB}$ etc." (A VII 6, 599) (Here, a number enclosed in a box represents the power, hence: BC $- \frac{BC^3}{3AB^2} + \frac{BC^5}{5AB^4} - \frac{BC^7}{7AB^6}$ etc.) In the scholium to this proposition, Leibniz proudly describes this theorem as "the pièce de résistance of our treatise and the reason we have written the rest" (A VII 6, 600), and insists its value is such that

> with the help of this equation, circular arcs and angles can be treated by analytical calculation like straight lines, and when it comes to applying theory to practice, by a great miracle of geometry we will be able to perform trigonometric operations without tables, with an error as small as we like. (A VII 6, 600)

To make good on his claim, in proposition 32 Leibniz considers the special case where the arc is equal to the eighth of the circumference of the circle, yielding the infinite series expressing $\frac{\pi}{4}$:

> Let the arc BO be an eighth part of the circumference of the circle; its tangent BC will be equal to the radius AB or AD. Consequently, by substituting AB for BC in proposition 31, the arc BO will be: $\frac{AB}{1} - \frac{AB}{3} + \frac{AB}{5} - \frac{AB}{7} + \frac{AB}{9} - \frac{AB}{11}$ etc. Therefore, $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11}$ etc. is to 1 as the arc BO of the eighth-circle is to the radius AB, or the complete arc BE of the quarter circle is to the diameter BH. (A VII 6, 601)

In subsequent propositions Leibniz derives other infinite series from his quadrature series, and shows that they have a sum expressible by a rational number (see for example A VII 6, 604–606), though the claim found in earlier writings, that this may prove to be the case also with his quadrature series, is not made, Leibniz by this time having realized his quadrature series could not be summed in this way.[12]

In October 1676, a little over four and a half years since his arrival in Paris and a matter of weeks after finishing his treatise on the quadrature,[13] Leibniz departed for the northern German city of Hanover to take

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[11] The treatise was first published only in 1993; Leibniz (1993) contains the Latin text; Leibniz (2004) contains the Latin text and a French translation; Leibniz (2014, 107–241) a Spanish translation; and Leibniz (2016) the Latin text and a German translation.

[12] For further details on Leibniz's treatise, see Knobloch (1989), Leibniz (2004, 7–32), Rabouin (2015), and Leibniz (2016, 279–291).

[13] At the end of 1693, Leibniz recalled that prior to leaving Paris he had shown his treatise to his friend Ehrenfried Walther von Tschirnhaus (1651–1708), who in turn had shared it with others, including the mathematician Jacques Ozanam (1640–1718). Ozanam (1684, 199) later published a version of Leibniz's quadrature series without indicating who had discovered it, leading Leibniz (1685, 482/2022f) to devote much of a review of Ozanam's book to pointing out his unscholarly behaviour: "[Ozanam] says he wanted to add easier demonstrations to aid practice. Therefore, in order to easily demonstrate the practical dimension of a circle, our author thought he would use that wonderful Leibnizian proportion between a circle and a circumscribed square, namely as $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$ etc. to 1 (which was discovered more than a decade ago, and was finally published in the second month of the first year of our *Acts*) because in this way, without any consideration of polygons and extraction of roots, the size of the circle is expressed perfectly and most simply in rational numbers. And the demonstration of the theorem was so remarkable that it was shared a long time ago, partly by the most illustrious inventor and partly by his friends, to some of the most eminent geometers in Paris and London, and received with great applause; whence it reached this author too. However, it is important for the sciences to obtain an edition of the demonstration by the inventor himself, along with many other new discoveries worthy of posterity." When writing to Tschirnhaus in 1693, Leibniz stated that, in using his quadrature series without attribution, Ozanam had "adorned himself with borrowed plumes" (A VII 5, 588).

up the post of court councillor, a role he held for the remaining forty years of his life. On departing from Paris, Leibniz left behind a manuscript copy of his treatise, which he had entrusted to his friend Soudry to copy and prepare for printing. But this plan did not go smoothly.

On 29 April 1677, Edme Mariotte, who was keeping an eye on Soudry's efforts to publish the treatise, informed Leibniz that Soudry had gone to Flanders and would not return to Paris for two or three months. Mariotte insisted that, once Soudry returned, he would press him to follow through with publication (A III 2, 75). Leibniz heard nothing further until he received Soudry's letter of 22 November 1677, assuring him that he was getting on with his task:

> I am constantly working to make progress with your book. Because you have left it jammed with corrections and additions in some pages, and because the material is already thorny enough in itself to arrest the reader's attention, I have found it appropriate not to put in the hands of Mr. Arnaud and Father Malebranche until it is properly depicted and quite correct; this is why I am finishing copying it out in the same way in which I am writing this line. (A III 2, 276–277)

In terms of Soudry's efforts to find a printer, the news was less cheering:

> I foresee the difficulty I will have in getting some bookseller to undertake the printing of your book at his expense, because books of profound science, particularly mathematics, since few people understand it, do not sell as well as books dealing with unimportant matters. And I know this from the experience of Mr. Huygens, Mr. Bouillard, Mr. de La Hire, and other people. I even consulted a few knowledgeable people. In short, in whatever way the affair may turn out, I intend to make you satisfied very soon, and to undertake myself the printing of your book, which is as close to my heart as it is to yours. (A III 2, 277)

On 7 December 1677, Mariotte informed Leibniz that he had met with Soudry and urged him to publish Leibniz's treatise (A III 2, 290). Nevertheless, progress stalled, and on 6 March 1678 Mariotte told Leibniz that he had called round Soudry's house three or four times but received no answer, so resorted to leaving a note asking Soudry to see him and give him news on the publication of Leibniz's manuscript (A III 2, 351). Although the reports he was receiving from Paris were hardly reassuring, Leibniz's confidence in Soudry apparently did not waver. Indeed, over the course of 1677 and 1678 he started to divulge his quadrature formula to a number of his correspondents, such as Arnold Eckhard (A II 1, 482) and Herman Conring (A II 1, 581), something he had been reluctant to do beforehand,[14] and during the summer and autumn of 1678 he informed others that his treatise on the quadrature would be printed in Paris (see for example A II 1, 635 and A III 2, 504).

Leibniz's hopes in this regard were finally dashed in December 1678, when he learned that Soudry had died of apoplexy (see A III 2, 564). Although it would be more than a year before he would learn of the fate of the manuscript that had been in Soudry's possession, Leibniz wasted no time in trying to extract maximum value from the mere fact that he had written it in the first place. Writing to the Academician Jean Gallois on 9/19 December 1678, Leibniz first reminded his influential acquaintance of the importance of his work on the quadrature:

> I left my manuscript on the arithmetical quadrature in Paris. The theorems it contains are considerable for theory and very useful in practice. Because, by retaining in memory only the two very simple progressions I give there, and which one can hardly forget once one has learned them, one can easily solve all the problems of trigonometry, without tables, without instruments, and without books, with as much precision

[14] In 1679 and 1680 Leibniz also disclosed his quadrature formula to Antonio Magliabechi (A III 3, 37), François de la Chaise (A III 2, 191), and Adam Adamandus Kochański (A III 3, 243), though to others, such as Pierre Daniel Huet, he merely indicated that he had succeeded in working out an arithmetical quadrature without indicating his results (A II 1, 737).

> as one wishes. This will be of great use for travellers, who cannot always carry their books with them. Having tables is a convenience, but not being able to solve problems without tables is an imperfection of the science, which I claim to have remedied. (A III 2, 569–570)

Leibniz then revealed that he had envisaged his work on the quadrature as a contribution to that of the Royal Academy:

> This invention has seemed momentous to skilled geometers, and I had the ambition to immortalize it by having it published among the much more important discoveries of your Royal Academy, but I do not know if that will be possible now. If only your kindness would one day find some favourable expedient to ensure that all the pains you have taken for me in the past will still succeed in something similar. For I don't know if it is necessary to always be in Paris to have some connection with the Royal Academy, especially since the King has bestowed similar graces on people who had no such connection with the Academy and who did not undertake any work. (A III 2, 570)

By "all the pains you have taken for me in the past" Leibniz is referring to Gallois' efforts to help secure Leibniz membership of the Royal Academy, efforts which dated back to 1675, following the death of Academician Gilles de Roberval on 27 October of that year. The decision on who would take Roberval's place lay with Jean-Baptiste Colbert (1619–1683), France's Controller-General of Finances, who had founded the Academy in December 1666. In an effort to secure a favourable outcome for Leibniz, a plan was hatched between Gallois and Joachim Dalencé (c.1640–1707) for Leibniz to demonstrate his calculating machine to the Duke of Chevreuse, Colbert's son-in-law, in the hope that a successful demonstration would persuade the duke to lobby Colbert on Leibniz's behalf (see A III 1, 303). Some years later, Leibniz recalled that despite concern among some members about admitting another foreigner—the Academy already had two, the Dutchman Huygens and the Italian Giovanni Domenico Cassini (1625–1712)—his membership was close to being secured when he took himself out of the running by accepting the position of court councillor in Hanover (A II 1, 684). Reading between the lines, it is more likely that Leibniz accepted the role in Hanover once he realized that his prospects of joining the Academy had slipped away. Nevertheless, as is clear from his remarks to Gallois at the end of 1678, Leibniz continued to harbour hopes that some position for him in the Academy might be found, despite no longer being resident in Paris.[15]

After receiving no response to his letter to Gallois, or to his next one sent 21 July 1679 (see A III 2, 787–793), Leibniz decided to switch his focus to his old mentor, Christiaan Huygens. Writing on 8/18 September 1679, Leibniz expressed his hope that by sending Huygens a sample of phosphorous, this might be sufficient for both him and Gallois to take up Leibniz's case with Colbert. He continued:

> It is true that I am not in a position to live in France at present, nevertheless I thought you might find it reasonable, and from time to time the Academy could know through me things that deserve to be known. That is, if it could not happen that I be considered an honorary member of the Academy, even though absent, or at least if another similar advantage could not be granted to me in this regard. Perhaps what I have done in other subjects could still seem fit to be one day among the things that belong to the Academy and particularly my arithmetical quadrature, the manuscript of which I even left in Paris for this reason, in which it [the arithmetical quadrature] is demonstrated in the manner of geometers, with many considerable propositions connected to it. (A III 2, 850)

Writing to Huygens on 10/20 October 1679, Leibniz repeated his hope that his "quadrature ...could be adopted by the Academy" (A III 2, 876). Receiving only a lukewarm response, Leibniz pressed the point again in a letter from end November/beginning December 1679:

[15] For further details on Leibniz's efforts in this regard, see Palomo (2021).

> My arithmetical quadrature has been copied out, and demonstrated with several other considerable propositions, and I had saved it for the Royal Academy, in case the author could be made to have any connection with it; for perhaps this treatise might then deserve a small place among the much more important other ones that are being printed there. (A III 2, 898)

In further exchanges, Huygens urged Leibniz to publish his treatise on the quadrature (A III 2, 889), while Leibniz developed a strategy to use his knowledge of phosphorous to get himself admitted to the Academy (see A II 3, 900–901), and then, from January 1680, the correspondence between the two stopped, the hiatus lasting eight years.

Leibniz's claim that his treatise on the quadrature had been "saved" for the Royal Academy deserves some comment, not least on account of its flagrant expediency. For there is nothing in the treatise itself, even a dedication,[16] to suggest Leibniz had had the Academy in mind when writing it, and no hint of any intended connection to the Academy in Leibniz's independent efforts to have it published via Soudry. The first suggestion of such a connection to the Academy occurs at the very point Leibniz is made aware that the man he'd left in charge of publishing the manuscript in Paris had died.

Leaving that aside, what might Leibniz have meant by saying that he wanted his treatise on the quadrature to be "among the things that belong to the Academy" and "adopted by the Academy"? There is no suggestion here that he wanted the Academy to publish his treatise; in fact, he would have known that the Academy did not print or publish books, nor did it have an arrangement with a particular printer or bookseller to distribute and sell books, this instead being left up to individual members to sort out. In expressing his wish for the Academy's endorsement, then, Leibniz presumably hoped that his treatise would, whenever published, be credited to him *as a member of the Academy*, as indeed were most of those published by Academy members. For example, Philippe de La Hire's *La gnomonique* was credited to "Mr. de La Hire of the Royal Academy of Sciences," and Edme Mariotte's *Premier essay de la végétation des plantes* to "Mr. Mariotte of the Royal Academy of Sciences," and so on (see La Hire, 1682 and Mariotte, 1679).[17] In other words, Leibniz sought membership of the Academy. His remarks to Gallois and Huygens about the treatise being saved for or adopted by the Academy were thus more about his ambitions to join the institution than they were about his original plans for the quadrature treatise, plans which had in any case become scuppered by Soudry's death.[18]

What, then, of the fate of Leibniz's manuscript? More than a year after Soudry's demise, Leibniz learned that the manuscript had been passed to Friedrich Adolf Hansen, who in turn had entrusted it to Christoph Brosseau, Hanover's Resident in Paris (see A I 3, 343, cf. 344). On 22 January 1680, Brosseau informed Leibniz that he had passed it to an Isaac Arontz, who was travelling to Hanover. Along the way, however, it was stolen, leading Leibniz to rue that he had always had difficulty with the post from France (see A I 3, 579).

As it happens, the lost manuscript was the copy Soudry himself had made, while Leibniz's original did find its way back to him, possibly through the hands of Arontz.[19] Certainly Leibniz had it back in his possession by the second half of 1680, which is when he enlisted the diplomat Simon de La Loubère (1642–1729) to ask the Dutch publisher Daniel Elzevier (1626–1680) if he would like to print the manuscript. La Loubère

[16] Leibniz had previously dedicated his "Theoria motus abstracti" of 1671 to the Academy in an attempt to curry its favour. See A VI 2, 260.

[17] Note, however, that other Academy members, such as Huygens and Cassini, did not employ this styling on their books.

[18] Leibniz was finally elected as a foreign member of the Academy on 13 March 1700; see LH 41, 8.

[19] There is evidence of another version of the manuscript, now lost. While all surviving manuscripts of the treatise lack pagination, in June 1676 Leibniz refers to a paginated version of the manuscript (see A VII, 6, 153), indicating that the theorems of Ricci and Sluse were to be inserted on pages 42 and 43 respectively. This could indicate that the process of getting his manuscript copied had begun before he had finished writing it.

responded on 10 August 1680: “Mr. Elzevier cannot tell you if he will publish your work without having seen it, that is, the work *de quadratura circuli*, and then he will also tell you if it can be printed in 4°” (A I 3, 415). In the end, nothing came of this approach, Elsevier’s death on 13 October 1680 closing yet another possible door to publication. At this point Leibniz quietly abandoned his plans to publish the treatise, despite receiving offers of assistance from some of his acquaintances. On 5 February 1681, the mathematician and watchmaker Oded Louis Mathion (1620–1700) insisted that all Leibniz had to do was send him the quadrature treatise and he would promptly have it published (see A III 3, 337). There is no evidence that Leibniz responded to Mathion’s letter, let alone his offer of assistance. Then, between June 1682 and May 1683, the Dutch mathematician Johann Jacob Ferguson (c.1630–1691) badgered Leibniz to publish his treatise on the quadrature, even going so far as to approach two possible publishers (Joan Blaeu and Jan Rieuwertsz) on Leibniz’s behalf (see A III 3, 645–646, 672, 800). But by that point, Leibniz had lost interest, and after ignoring many of Ferguson’s entreaties, in mid-May 1683 he finally explained to Ferguson that he no longer wanted to publish a treatise that focused on the quadrature, but rather one on the art of discovery in general, which would treat the quadrature *inter alia* as merely an example or specimen of that art (see A III 3, 813).[20]

3. The impostrous: pseudepigrapha

While Leibniz’s interest in publishing his treatise on the quadrature faded following the loss of the Paris manuscript and the death of Elzevier, and his mathematical and philosophical work took on broader concerns, he did not lose interest in publishing his solution to the quadrature problem. For as we know, he did publish it in February 1682, in the paper “De vera proportione circuli...” However, prior to that, Leibniz concocted another way to disseminate his quadrature result, through a short essay left without a title. Two drafts of the essay exist, the first in Latin, the second (slightly revised) in French. Neither version of the essay has yet been published. The essay starts, as do many of his short writings on the quadrature, with a brief account of the problem and some of the notable attempts to solve it:

> Archimedes determined that the ratio of the circumference to the diameter is a little less than 22 to 7. Metius, coming closer, said that it is as 355 to 113, but Ludolph of Cologne, surpassing all the others by his work, made it as 3.14159265358979323846264338327950289 is to 1.00000000000000000000000000000000. (LH 35, 2, 1 Bl. 225r/Leibniz, 2022c; cf. LH 35, 2, 1 Bl. 226v)

After noting that such approximations may be fine for practice while being ultimately unsatisfying for the mind, the essay prepares the reader for Leibniz’s formula, only with this peculiarity: Leibniz refers to himself in the third person:

> Therefore, instead of a single number, we must have recourse to a progression of rational numbers, continuable to infinity, in order to express exactly the unknown quantity as much as is possible by rational numbers, which we may call here *arithmetical quadrature of the circle*, and this is what Mr. Leibniz has thankfully given us in the following way, which seems all the more considerable since this problem has been the object of detailed investigation. (LH 35, 2, 1 Bl. 225v/Leibniz, 2022c; cf. LH 35, 2, 1 Bl. 226v)

After giving Leibniz’s formula, albeit without proof or methods, the essay then turns to show how infinite series might be summed, focusing on the initial terms of an infinite series of fractions whose denominators are the square numbers minus one (i.e. 3, 8, 15, 24, 35, 48, 63, 80, 99, 120, 168, 195, 224, 255, 288, 323,

[20] Leibniz appears to have entertained such an idea as early as April/May 1680, when he mentioned it to La Loubère; see A III 6, 3–5.

360 etc.). This leads to the following observation, which in the initial Latin draft of the text is again credited to Leibniz in the third person, while in the revised French draft, Leibniz just inserts dots as a placeholder for his own name:

> But ...... has found something that seems very remarkable, namely that the sum of the sequence of all $\frac{1}{3}+\frac{1}{8}+\frac{1}{15}+\frac{1}{24}+\frac{1}{35}+\frac{1}{48}+\frac{1}{63}+\frac{1}{80}+\frac{1}{99}+\frac{1}{120}$ etc. to infinity is findable, and makes $\frac{3}{4}$, and leaving out every other term in the series, $\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+\frac{1}{63}+\frac{1}{99}$ etc. makes $\frac{2}{4}$ or $\frac{1}{2}$. But $\frac{1}{8}+\frac{1}{24}+\frac{1}{48}+\frac{1}{80}+\frac{1}{120}$ etc. makes $\frac{1}{4}$. And again leaving out every other term in the second series, we have $\frac{1}{3}+\frac{1}{35}+\frac{1}{99}$ etc., whose sum is the semi-circle, supposing the square of diameter $\frac{1}{1}$, or else this sum is the whole circle, supposing the inscribed square is $\frac{1}{4}$. (LH 35, 2, 1 Bl. 226r/Leibniz, 2022c; cf. LH 35, 2, 1 Bl. 227v)

A comparison between this untitled pseudonymous essay and “De vera proportione circuli” reveals that the former is an early draft of the latter; indeed, when composing the latter, Leibniz evidently went back to the original Latin draft of the former, retaining not just the basic structure but also many of the sentences verbatim as well as three of the figures. But while the later “De vera proportione circuli...” was intended to be published in Leibniz’s own name, the earlier untitled pseudonymous essay evidently was not. Yet it is unquestionably Leibniz’s own work, his distinct handwriting unmistakable in both the Latin and French manuscripts.

How might we best decipher the enigma of this essay? Let’s start with the date. While neither the original Latin nor revised French manuscript has a watermark, both were filed among Leibniz’s mathematical papers of 1681, a date which is plausible for another reason. For since the untitled pseudonymous essay’s successor, “De vera proportione circuli...,” was written in November or December 1681, the pseudonymous essay must have been written earlier, probably after he had abandoned plans to publish his quadrature treatise at the end of 1680. So a likely date of composition would be between January and November 1681.

As for what Leibniz intended to do with the essay, we should note first that it is clearly intended to be an announcement of Leibniz’s quadrature formula, but also that there is no information in the text itself or in his wider Nachlaß to indicate where the announcement would be made or by whom. Two conjectures can be framed: (1) that the essay was intended for the *Journal des Sçavans*, (2) that it was intended for the Royal Academy, perhaps for inclusion in its weekly minutes (procès-verbaux). Let us consider each conjecture in turn.

First, if we suppose that the untitled pseudonymous essay was intended for publication in a journal, it must have been intended for the *Journal des Sçavans*, since there was no other French-language journal available at the time (*Nouvelles de la republique des lettres* did not start publication until 1684). While it might seem odd that Leibniz would seek to have someone else announce his results in the *Journal des Sçavans*, there is another example of his planning exactly that, in the form of a letter he composed in the name of Academy luminary Cassini which, as its title indicates, was intended for that journal. The letter begins thus:

> Extract from a letter from Mr. Cassini to the author of the *Journal des Sçavans* concerning Mr. Leibniz’s rules for performing the operations of trigonometry without tables.
>
> I have obtained from Mr. Leibniz the rules he has found for the arithmetic quadrature of the circle and the hyperbola, by means of which one can dispense with tables of sines and logarithms if necessary. (LH 35, 15, 5 Bl. 6r)[21]

[21] A preliminary transcription of the Cassini letter has been published online as part of a planned addendum to A III. See Leibniz (2019, 6–8).

Pseudo-Cassini then states that he will gives the rules "in the way Mr. Leibniz communicated them to me. Here are his own words." With that, Pseudo-Cassini's voice becomes that of Leibniz, who proceeds to give the rule for finding the sine of the complement (i.e. cosine) c from the given arc a, which is expressed in a power series thus: $c = 1 - \frac{\mathrm{a}^2}{2} + \frac{\mathrm{a}^4}{24} - \frac{\mathrm{a}^6}{720}$ etc.[22] Although pseudo-Cassini claims he will give three of Leibniz's trigonometric rules, only the above rule is given, suggesting either the letter was left unfinished or its second page has been lost.[23] This letter, like the untitled pseudonymous essay announcing Leibniz's formula for the quadrature, is clearly in Leibniz's hand. Moreover, both texts present parts of his work on the quadrature: the rule found in the pseudo-Cassini letter is also found in proposition 50 of Leibniz's treatise, *De quadratura arithmetica circuli ellipseos et hyperbolae* (see A VII 3, 661–664). Assuming that both the pseudo-Cassini letter and the untitled pseudonymous essay on his quadrature formula were written around the same time, namely 1681, we could surmise that despite having abandoned plans to publish the treatise *De quadratura arithmetica circuli*, Leibniz still wanted to get some of its key ideas into the public domain, and considered doing so using the names of others.

Moreover, one can see why his idea of others announcing key results of his quadrature research through essays he had written on their behalf might have appealed to him. In 1681, our presumed date for both writings, Leibniz was certainly more of an established scholar than he had been seven years earlier, when he first devised his quadrature formula, but he still had little standing as a mathematician. Indeed, at that point he had published just two short essays on mathematics (both two pages in length) in the *Journal des Sçavans* in 1678.[24] As a relative unknown in the field, Leibniz may have thought that there was benefit to be had in having his work sponsored—and by extension, vindicated—by luminaries in the Republic of Letters, especially by members of the Academy, the institution he still wished to join.

How likely is it that the untitled pseudonymous essay on the quadrature was, like the pseudo-Cassini letter, intended for the *Journal des Sçavans*? While there is no direct evidence either way, one could certainly build a circumstantial case in favour of the hypothesis. The existence of the pseudo-Cassini letter reporting other parts of Leibniz's work on the quadrature is certainly suggestive in this regard. Perhaps just as importantly, there were precedents for scholars to have their ideas featured in the *Journal des Sçavans* through the pen of others. For example, the 7 August 1679 issue of the *Journal des Sçavans* contains a short "Letter written to Monsignor the Duke of ....." which reports some new ideas of the inventor Jean de Hautefeuille (1647–1724). The author of the letter is not identified, but it was evidently not Hautefeuille, who is referred to in the third person in the letter, but rather the editor of the journal, or one of his assistants, given the reference to the new microscopes "about which we have spoken in our journals this past year" (Anon., 1679a, 215). What is printed in the journal, then, is not the original letter presumably from Hautefeuille to an unnamed duke, but the editor's account thereof. A month later, in the issue dated 4 September 1679, there is a short report about a calculating machine invented by René Grillet de Rouen (fl. 17th century), the phrasing in the report again indicating that it was written not by the inventor himself but by the journal's editor ("We have spoken in another [issue of the] journal of a small arithmetic machine of his [Grillet's] invention..."; Anon., 1679b, 249).[25] Two months after that, the journal included a report on a letter to the journal written by Jean-Baptiste Panthot (1640–1707), dean of the college of medicine in Lyon, about a burning mirror

[22] "In the proposed right-angled triangle AFB, let the hypotenuse AB be equal to 1. Let the arc BD described from the centre A, and from the radius AB be called a, and AF or BM, sine of the complement of arc BD (that is, sine of arc BL, which is the complement of arc BD at 90 degrees), being called c, I say that c is equal to $1 - \frac{\mathrm{a}^2}{1,2} + \frac{\mathrm{a}^4}{1,2,3,4} - \frac{\mathrm{a}^6}{1,2,3,4,5,6}$ etc., that is, c is equal to $1 - \frac{\mathrm{a}^2}{2} + \frac{\mathrm{a}^4}{24} - \frac{\mathrm{a}^6}{720}$ etc." (LH 35, 15, 5 Bl. 6v).

[23] The extant manuscript fills both sides of a single sheet, so it cannot be ruled out that the letter continued on a different sheet.

[24] Namely, Leibniz (1678a/2021a) and (1678b).

[25] Unhelpfully, the title of the report follows that of a book Grillet had published five years earlier, though it is not an account of that book. See Grillet (1673).

invented by François Villette (1621–1698) (see Anon., 1679c).[26] Whether Hautefeuille, Grillet, or Villette played any part in getting their work featured in the journal in this way is not known, nor even whether the reports were printed with their knowledge or approval. Nevertheless, in each of these cases, the journal printed, from a third party source, its own report of an invention while giving full credit to the relevant inventors. From these articles Leibniz would have learned that there was more than one way to have one's work featured in the journal: in addition to writing a letter to the editor (an approach he had considered and abandoned in the case of his quadrature result, as we saw in section 1), one could have someone else do it.

Of course, such a stratagem works best with the consent of the person in whose name one seeks to publish one's work. Yet there is no evidence that Leibniz ever sought or obtained Cassini's permission to send a letter in his name to the *Journal des Sçavans*,[27] making it likely that the pseudo-Cassini letter was little more than a partially-executed (because drafted) flight of fancy on Leibniz's part.[28] The same can be said of the untitled pseudonymous essay announcing the quadrature result which, if likewise planned for the *Journal des Sçavans*, was probably intended to be submitted by one of Leibniz's friends in Paris, perhaps Mariotte (Leibniz was not in active correspondence with Huygens or Gallois in 1681, making it unlikely he would have had either of them in mind). But there are no traces in what survives of Leibniz's correspondence of him discussing the idea with Mariotte or anyone else.

We turn now to the second of our two hypotheses: that the untitled pseudonymous essay may have been intended for the Royal Academy of Sciences. This hypothesis might seem implausible at first blush, but is difficult to dismiss entirely. While the Academy's meetings were typically used by members to read or report upon their own work, in the handwritten minutes (procès-verbaux) of these meetings one does occasionally find memoirs, i.e. short academic papers, written by members and otherwise unpublished, usually draft works-in-progress.[29] Occasionally, letters received by the Academy's members from non-members were included in the minutes if they contained interesting and pertinent material.[30] So there was

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[26] The source of this report may have been posthumously published as a short 16-page book; see Anon. (1715).

[27] There is no extant correspondence between Leibniz and Cassini, though they are known to have had a brief exchange in 1677, mediated through Christoph Brosseau, Hanover's Resident in Paris. On 13 February 1677, Leibniz instructed Brosseau to repay a debt of 12 crowns to Cassini, to whom Leibniz had also addressed a note or letter for Cassini that he asked Brosseau to pass on. Cassini is known to have replied in March 1677, though the Akademie editors surmise that Cassini's reply was probably nothing more than a note of acknowledgement (see A III 2, 43 and 50).

[28] Two further things are worth noting about Leibniz's plan to send the pseudo-Cassini letter, and perhaps also his untitled pseudonymous essay on the quadrature, to the *Journal des Sçavans*. First, Leibniz was well aware that the *Journal des Sçavans*, under de La Roque's stewardship, was not keen on printing mathematics papers. In May 1678, the journal had printed a short letter of Leibniz's concerning the quadrature of a portion of the roulette (Leibniz, 1678b). A month later, de La Roque cautioned Leibniz not to expect free rein in publishing his mathematical work in the journal: "As you know that the number of mathematicians is smaller than that of those who are merely curious, I must take care of what I give in the *Journal*" (A III 2, 456). Duly warned, Leibniz did not submit another mathematics paper to the journal until 1692 (see Leibniz, 1692), five years after de La Roque had stepped down as editor. Second, it is surprising that Leibniz was prepared to trust the *Journal des Sçavans* with his mathematical work again, after the way it had handled his short paper on the quadrature of the roulette. On 9/19 December 1678, Leibniz explained to de La Roque that the way his demonstration had been printed in the journal would make it "pass for an enigma" since the words were arranged completely differently from how he had written them. His demonstration, which had originally been "so short, and nevertheless so intelligible," had been mangled so badly by the printer that its arrangement made Leibniz laugh (A III 2, 563). While an experienced mathematician like Philippe de La Hire might just about manage to make sense of what was printed, Leibniz insisted, others would be able to do so only with great difficulty. Almost twenty years later, Leibniz was still complaining about the printer's erroneous typesetting of this paper (see A III 6, 625).

[29] For example, Denis Dodart's "Memoire des travaux chymiques que l'on pourroit faire sur les plantes" on 10 January 1675 (Académie royale des sciences, 1675–1679b, 5r-7r); Jacques Buot's "Le limacon de Monsieur Pascal" on 26 June 1675 (Académie royale des sciences, 1675–1679b, 58a–g); and Philippe de La Hire's "Propositions sur les Foyers des sections de coniques nouvellement decouvertes, et demonstrées" on 23 July 1678 (Académie royale des sciences, 1675–1679a, 176r–184v).

[30] For example, a letter of 16 January 1675 from a Mr. Piar to Academy member Denis Dodart was included in the 30th January entry of the Academy's minutes; Académie royale des sciences (1675–1679b, 8–10v).

scope for a non-member like Leibniz to have short pieces read out in the Academy and even included in the Academy's minutes. Leibniz was well aware of this. In fact, two of his letters to Mariotte, on the inertia of bodies, were read at the Royal Academy on 28 April 1680.[31] Although having a mathematical discovery debut in the Academy's handwritten minutes would not have given it much exposure, in his keenness to join the Academy Leibniz may have deemed it the right kind of exposure, which in any case would not have prejudiced any future publication plans for his work.

While there is no reason to think that Leibniz ever did send his pseudonymous essay to the Royal Academy, he did later claim—to Colbert no less—that he had notified the institution about his quadrature result. In the first half of October 1682, Leibniz wrote to Colbert to request the Academy's support for a project to study minerals *in situ* rather than in cabinets of curiosities. Before requesting this support, Leibniz reminded Colbert of what he had done for the Academy thus far:

> But I don't know if I dare say that I had the good fortune to communicate to your Royal Academy two things that can be taken into account. The first is the composition of a light that can be called perpetual, which chance gave to a chemist of my acquaintance, to which I have nevertheless contributed something since; and the other is the true proportion, such as it is in effable numbers, between the circle and its square, which has been sought from the time of Archimedes down to us, and which I have found and demonstrated. (A III 3, 720)

While Leibniz certainly did inform the Academy of a method of making phosphorous,[32] there is no evidence either in Leibniz's Nachlaß or in the Academy's own minutes that he informed it of his quadrature result also.[33] In claiming to have done so, he may have been referring, rather obliquely, to the letter he had written to Academy member Huygens back in October 1674 reporting his quadrature result. Alternatively, he may have been alluding to one of his friends, Ehrenfried Walther von Tschirnhaus (1651–1708), communicating the result to the Academy, something Leibniz later implied had happened in 1682 (see A II 2, 87, 492), though there is no record of any such communication in the Academy's minutes. Needless to say, neither means of dissemination would qualify as the sort of formal notification Leibniz implied he had given.

Although Leibniz opted not to pursue publication or dissemination of the pseudonymous essay on the quadrature (or the pseudo-Cassini letter for that matter), he was clearly not dissatisfied with it; as mentioned above, he reused large parts of the Latin draft verbatim for "De vera proportione circuli...," along with several of the figures. The transformation of the untitled pseudonymous essay into "De vera proportione circuli...," which is likewise devoid of proofs or methods, likely occurred in November or December 1681. On 19/29 October 1681, Christoph Pfautz, one of the co-founders of the soon-to-be-launched German periodical *Acta eruditorum*, asked Leibniz for a contribution to the journal (A III 3, 506). The other co-founder, Otto Mencke, wrote a week later with the same request (A I 3, 506). In December 1681, Leibniz obliged, enclosing "De vera proportione circuli..." in a letter to Pfautz (A III 3, 524). The essay was published in the second issue of the *Acta eruditorum*, in February 1682.

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31 "Two letters from Mr. Leibniz were read concerning some new discoveries he has made to perfect geometry and physics." See Académie royale des sciences (1679–1683, 50v). The letters in question are also available in A III 3, 79–80 and 80.

32 This occurred via his friend Tschirnhaus, who on 4 July 1682 read to the Academy parts of a letter Leibniz had sent him the end of the previous month. The Academy's minutes for that meeting state that Tschirnhaus "gave the manner of making phosphorous that Mr. Leibniz had sent to him" and then reproduce the relevant part of Leibniz's letter (under the title "L'operation pour faire le phosphore") translated from German to French; Académie royale des sciences (1679–1683, 165–166).

33 The only mention of the quadrature in the Academy's minutes from 1678 to 1682 occurs in the minutes of the meeting of 18 January 1681, in which is written out a short unpublished essay by Gilles de Roberval on measuring the circle. Roberval died in 1675, and his essay has nothing to do with Leibniz's work on the quadrature. See Académie royale des sciences (1679–1683, 93v–95r).

4. The extravagant: medals

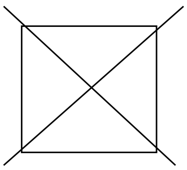
For most, the publication of results in a journal would mark the end of one's transmission efforts, but in this case for Leibniz it did not. And his final idea for dissemination was also the most extravagant: he envisaged his quadrature formula commemorated on a medal. In fact, the idea had sufficient appeal that Leibniz sketched two manuscripts containing different draft designs for a commemorative quadrature medal.

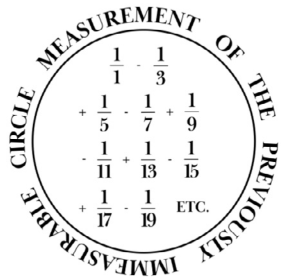
It is well known that Leibniz sought to have another of his mathematical discoveries, binary arithmetic, captured on a medal.[34] Several designs for a binary medal were made in early 1697 (see A I 13, 123–125/Strickland and Lewis, 2022, 103), and circulated among correspondents (see LBr 683 Bl. 6), ensuring that they were published soon after Leibniz's death (see for example Leibniz, 1720, 103–112; Nolte, 1734, 1–16; Ludovici, 1737, I: 132–138). The binary medal designs have been often reproduced ever since (see for example Zacher, 1973, 52; Glaser, 1981, 32–34; Ching and Oxtoby, 1992, 70; Strickland and Lewis, 2022, 103). By comparison, Leibniz's sketches for a quadrature medal are not well known. This is not surprising; neither has yet been published in full, with one not yet published at all.

Both manuscripts seek to pair the quadrature formula with another of Leibniz's innovations: binary arithmetic in one case, the arithmetic machine in the other. Let's start with the one featuring the quadrature formula and binary arithmetic. What follows is the complete manuscript, with notable deletions marked by ~~strike-through text~~ or a large X over the material Leibniz crossed out:

MEASUREMENT OF THE CIRCLE
~~LEIBNITIANA~~
PUBLISHED 1682.

WE ENCOMPASS THE IMMEASURABLE
WORLD IN OUR SOUL
The mind grasps the immeasurable





0	0	1000	9
1	1	1001	10
10	2	1010	11
11	3	1010	12
100	4	1011	13
101	5	1	14
110	6		15
111	7		16
1000	8		

0
1
10
11
100
101
110
111
1000

0	0	1001	9
1	1	1010	10
10	2	1011	11
11	3	1100	12
100	4	1110	13
101	5	1111	
110	6		
111	7		
1000	8		

0	0	1000	8
1	1	1001	9
10	2	1010	10
11	3	1011	11
100	4	1100	12
101	5	1101	13
110	6	1110	14
111	7	1111	15
		10000	16

THE IMMEASURABLE
WORLD OBEYS NEW LAWS

The immeasurable world is a circle not yet measured. There are *new laws* by which an infinite series is established. There is, however, a deeper idea about the laws of nature promulgated first by me.

(LBr F 15 Bl. 22r/Leibniz, 2022g)[35]

In this manuscript, the obverse of the medal features the Leibniz series; the same figure, without any indication that it was envisaged for a medal, is to be found in the untitled pseudonymous essay on the quadrature as well as "De vera proportione circuli..." The inclusion of the table of decimal numbers 0 – 16 in binary, which took Leibniz four attempts to get right, could suggest that this was intended to go on the reverse of

[34] For further details, see Bredekamp (2020, 98–104).

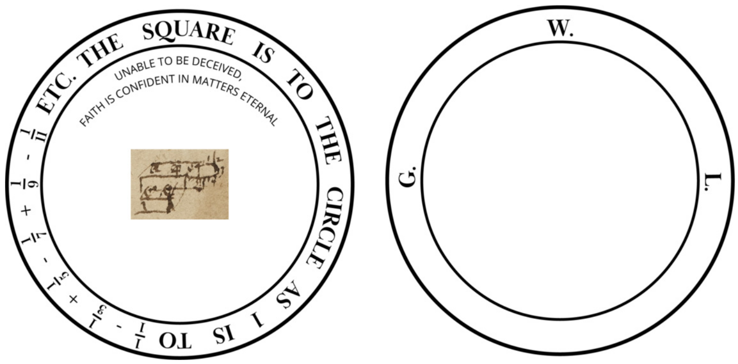
[35] Parts of this manuscript have been published in A IV 6, 626–627.

the medal. Leibniz certainly saw the two discoveries as connected, at least around 1679 to 1680, when he was starting to experiment with binary. In a very early writing on binary, probably from the first quarter of 1679, Leibniz sketches out binary-decimal conversion and ends with "It is to be demonstrated whether tetragonism [i.e. the quadrature] is possible" (LH 35, 3 B 2 Bl. 6/Strickland and Lewis, 2022, 33). The idea is explored further in "De progressione dyadica" [On binary progression] written 15/25 March 1679. There, Leibniz converts the decimal fractions from $\frac{1}{2}$ to $\frac{1}{9}$ into binary, and spots that those with odd-numbered denominators are all periodic (LH 35, 3 B 2, Bl. 3v/Strickland and Lewis, 2022, 58). For example, $\frac{1}{3}$ in binary is represented by 0.01010101_2, where the two-digit string 01 repeats ad infinitum, and $\frac{1}{7}$ is represented by 0.001001001001_2, where the three-digit string 001 repeats ad infinitum. The repeating patterns in binary representations of fractions with odd-numbered denominators, the very same fractions that exclusively featured in Leibniz's quadrature formula, suggested to him that investigating the periodicity of these fractions in binary could yield a rule that would enable one to work out the digits of π: "Having obtained the period from a given number, one will also obtain the method of expressing by a certain rule the sequence of digits expressing the size of a circle, that is, $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$ etc." (LH 35, 3 B 2, Bl. 4r/Strickland and Lewis, 2022, 59). As such a rule eluded him, in August 1680 Leibniz tried out a different approach in a manuscript entitled "Attempted Expression of the Circle in Binary Progression" (LH 35, 12, 2 Bl. 97/Strickland and Lewis, 2022, 61–62). Here, Leibniz considers the powers of 2^{-1} geometric sequence namely: 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$ etc., comparing various terms of this sequence with sequences of terms from his circle formula. However, a calculation error early on led him to incorrectly conclude that $\frac{1}{2} + \frac{1}{4}$ is greater than the area of the circle (which, if true, would make π less than 3). Failing to spot the mistake, which subsequently gets compounded, Leibniz eventually gave up.

By 1682, the earliest date that Leibniz's first quadrature medal design could have been sketched, he was no longer pursuing the links between the periodicity of binary fractions and his quadrature formula, so it is unlikely he would have seen them as two sides of the same coin, so to speak, as he had several years earlier. But it is not implausible that Leibniz wanted to capture two of his mathematical innovations, the quadrature formula and binary arithmetic, on one medal.

Indeed, that was the aim of the second medal design too, which features both Leibniz's quadrature formula and his arithmetic machine. This is the complete manuscript:

Memorial Medal of my Tetragonism and Arithmetic Machine



Demonstrations about numbers and motions are infallible symbols of the divine faith.
And infinite series are symbols of eternal things.

(LH 35, 2, 1 Bl. 2r/Leibniz, 2022b)[36]

[36] Leibniz's rough sketch is reproduced with permission of the Gottfried Wilhelm Leibniz Bibliothek – Niedersächsische Landesbibliothek, Hanover. A preliminary transcription of this manuscript has been made available online in Leibniz (2021b, 15–16).

The design is likely unfinished, as the reverse of the medal is left blank, aside from Leibniz's initials spaced around the outside edge. The sketch of the arithmetic machine is rough and difficult to make out, and likely would have been sharpened up had further drafts been made. However, just like Leibniz's planned binary medal of 1697, neither of the two planned quadrature medals ever got off the drawing board.

Because of the territorial fragmentation of Germany at the time, mints were under the control of the various duchies or free imperial cities, so to get a medal minted Leibniz would have had to secure approval from a relevant authority. This would not have been easy. In the seventeenth century, medals were invariably minted to commemorate people (usually royalty), biblical scenes, or key events such as victorious battles, royal births, marriages, and deaths etc., rather than scientific or mathematical innovations. So Leibniz would have needed to find someone who had not just the power to order his medal be made but also sufficient interest in his mathematical advances to want them commemorated. In 1697, he thought he had identified someone both willing and able to produce a binary medal: Rudolph August, Duke of Brunswick and Lüneburg. The year before, the duke had expressed deep interest in Leibniz's binary arithmetic (see A I 12, 65/Strickland and Lewis, 2022, 88), leading Leibniz to take the step of pitching him the idea of a binary medal (A I 13, 116–121/Strickland and Lewis, 2022, 99–103). (The pitch was unsuccessful because by the time the duke received it he had already had a number of wax seals made to commemorate binary; see A I 13, 127–129/Strickland and Lewis, 2022, 106.) By contrast, Leibniz appears not to have approached anyone with either of his designs for a quadrature medal. There is no mystery here: quite simply, no duke or prince-bishop had shown any interest in Leibniz's quadrature result, making it an uphill battle, at best, to convince one to commemorate it in on a medal. Had Leibniz managed to get either of the quadrature medals produced, it is likely he would have given them to the great and the good: royal figures, nobles, distinguished scholars, i.e. those best placed to advance his career. Leibniz was never above attempting to curry favour with such people, much to the distaste of Bertrand Russell (1937, xv, cf. 2), who would later castigate Leibniz for putting the "admiration of Princes and (even more) of Princesses" over his scholarship, apparently overlooking in the process that advancement for someone like Leibniz, who was not of the nobility himself, depended on winning precisely this sort of admiration.

5. Conclusion

In focusing on Leibniz's various efforts to disseminate his quadrature results, we ought not get the impression that the promulgation of his other mathematical discoveries was without incident or interest.[37] Nevertheless, the many, often colourful twists and turns that characterized Leibniz's various attempts to disseminate his quadrature results were not, as far as we can tell, replicated in later attempts to make public his mathematical discoveries. In part this was because the dissemination of his quadrature work was for a time intimately bound up with his efforts to secure a position at the Royal Academy. Once Leibniz had settled in Hanover (or rather, resigned himself to remaining there), the need to leverage his mathematical discoveries to advance his position evaporated, and his willingness to pursue dissemination strategies such as lengthy treatises or pseudonymous essays followed suit. That aside, we may suppose that Leibniz also learned a valuable lesson from his diverse and often abortive attempts to disseminate his work on the quadrature, namely: keep it simple. Indeed, all of the other mathematical contributions he saw fit to give the world were published by the conventional route of journal articles, most often in the *Acta eruditorum*, which became his vehicle of choice, especially for his mathematical and scientific work, with no fewer than 55 of his papers appearing there and dozens elsewhere.[38] Leibniz fully embraced the format of the journal

[37] For example, Leibniz was prompted to publish his first paper on the differential calculus in October 1684 (see Leibniz, 1684) because a year earlier Tschirnhaus (1683) had published a paper in the *Acta eruditorum* employing methods very close to those found in Leibniz's as-yet unpublished calculus.

[38] See the list in Leibniz (2005, 1289–1306).

article: it suited his working patterns, which afforded him little time to write lengthy works, as well as his preference for short, punchy pieces that would give others the opportunity to develop his ideas further. Or as Fontenelle (1718, 108–109) aptly put it in his eulogy of Leibniz delivered to the Academy of Sciences, the institution Leibniz was so desperate to join as a young man:

> He [Leibniz] did not publish any body of work on mathematics, but only many loose pieces, of which he would have made books if he had wanted... He said he liked to see the plants for which he had provided the seeds growing in the gardens of others. These seeds are often more valuable than the plants themselves; the art of discovery in mathematics is more precious than the majority of things that have been discovered.

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