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17TH CENTURY ARGUMENTS FOR THE IMPOSSIBILITY OF THE INDEFINITE AND THE DEFINITE CIRCLE QUADRATURE

Jesper Lützen

Abstract. — The classical problem of the quadrature (or equivalently the rectification) of the circle enjoyed a renaissance in the second half of the 17th century. The new analytic methods provided the means for the discovery of infinite expressions of π and for the first attempts to prove impossibility statements related to the quadrature of the circle. In this paper the impossibility arguments put forward by Wallis, Gregory, Leibniz and Newton are analyzed and the controversies they gave rise to are discussed. They all deal with the impossibility of finding an algebraic expression of the area of a sector of a circle in terms of its radius and cord, or of the area of the entire circle. It is argued that the controversies were partly due to a lack of precision in the formulation of the results. The impossibility results were all part of a constructive problem solving mathematical enterprise. They were intended to show that certain solutions of the quadrature problem were the best possible because simpler (analytic) solutions were impossible.

Résumé (Arguments du xvii^{e} siècle en faveur de l'impossibilité des quadratures du cercle indéfinie et définie)

Le problème classique de la quadrature (ou de la rectification) du cercle a connu un regain d'intérêt pendant la deuxième moitié du xvii^{e} siècle. Les

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Mots clefs. — xvii^{e} siècle, histoire des mathématiques, quadrature du cercle, pi, impossibilité, fonctions transcendentales, Wallis, Gregory, Leibniz, Newton.

nouvelles méthodes analytiques ont permis la découverte d'expressions infinies du nombre π et ont ouvert la voie vers les premières tentatives de démonstration d'assertions d'impossibilité concernant la quadrature du cercle. Dans cet article les arguments d'impossibilité de Wallis, Gregory, Leibniz et Newton sont analysés et les controverses qu'ils ont causées sont discutées. Tous les arguments concernent l'impossibilité de trouver une expression algébrique de l'aire d'un secteur d'un cercle en termes de son rayon et de sa corde ou de l'aire du cercle entier. Les controverses sont en partie dues à l'imprécision de la formulation des résultats. Les résultats d'impossibilité sont tous issus d'une entreprise mathématique constructive. Leur but était de démontrer qu'une certaine solution de la quadrature du cercle est la meilleure possible parce que des solutions plus simples (analytiques) sont impossibles.

1. INTRODUCTION

The quadrature of the circle is one of the most long lived and widely known mathematical problems. Its history is discussed in all general histories of mathematics and it has been the subject of many specialized papers and books. The problem has two aspects: A positive constructive aspect, concerning the various solutions that have been put forward for over two millennia, and a negative aspect concerning the impossibility of finding a solution using prescribed tools. The historical literature usually deals with the positive aspect in a rather continuous way, presenting and discussing the almost uninterrupted series of solutions that have been discovered since the time of Archimedes. The impossibility question on the other hand is usually dealt with in a more discontinuous fashion, often jumping from the Greeks to Lindemann's 1882 proof of the fact that the problem cannot be solved algebraically, and hence cannot be solved by ruler and compass (see e.g Berggren et al. 2004, and Hobson 1913). This difference in the treatment of the constructive aspect and the impossibility aspect of the problem reflects to some extent the importance that mathematicians of the past have attributed to these aspects and the success with which they have dealt with them. Yet, long before 1882, arguments of impossibility concerning the quadrature of the circle had been put forward by various mathematicians. The aim of the present paper is to shed light on the 17th century contributions to the impossibility question.

I shall analyze four 17th century impossibility arguments concerning the quadrature of the circle which were put forward, respectively, by John Wallis (1616–1703), James Gregory (1638–1675), Gottfried Wilhelm Leibniz (1646–1716) and Sir Isaac Newton (1642–1727).

The 17th century contributions, in particular those of Gregory and Newton, have been discussed in earlier papers such as: ([Heinrich 1901]), ([Dehn & Hellinger 1939]), ([Scriba 1983]), ([Arnol'd 1990]), ([Pourciau 2001]) and ([Pesic 2001]). I shall build on and add to these works. In particular, I shall compare the early modern arguments with each other and consider them in the context of impossibility theorems in general. I shall investigate why the impossibility questions concerning the quadrature of the circle was studied at this time at all. In this connection, I shall argue that impossibility results were primarily aimed at casting light on constructive solutions of the quadrature problem.

Considering the lack of earlier treatments of the impossibility question, I shall address questions such as: What allowed the 17th century mathematicians to contribute to the impossibility question? Which methods did they use? What did they (try to) prove to be impossible? Here it is remarkable that construction by ruler and compass did not occupy a central position. Did they distinguish different impossibility statements concerning the quadrature of the circle? Did they agree about the distinctions? Did they accept each other's proofs, and were the proofs generally accepted by their contemporaries? Did later mathematicians (including modern mathematicians and historians of mathematics) accept the proofs (or slightly rigorized versions of them) as valid? What were the strengths and weaknesses of the impossibility proofs put forward by the17th century mathematicians? An answer to the latter question will not only explain why we do not attribute the impossibility theorems to the mathematicians of this period, but also clarify what the successors could build on and what they had to repair or add.

2. HISTORICAL BACKGROUND

The early modern contributions to the impossibility of the quadrature of the circle must be studied against the background of the earlier Greek contributions to the constructive aspects of the problem and the few earlier remarks concerning the impossibility question.

The problem of constructing a square equal in area to a given circle was formulated explicitly in ancient Greece ([Knorr 1986, 25–39]). The Greeks came up with various solutions of the problem, using mechanical (transcendental) curves such as the quadratrix and the spiral, and so did their successors. Moreover, impressively good approximations were found by Archimedes and later mathematicians[1]. As we shall see these positive results were important contexts for the impossibility arguments, but they shall not be treated in this paper.

With the Greeks also began a long tradition, leading all the way to the present day, of publishing (incorrect) methods for solving the problem by ruler and compass or by algebraic procedures, and subsequent refutations of such methods. In the early modern period the most famous incorrect circle quadratures were those of Christen Sørensen Longomontanus and Thomas Hobbes. They were refuted by John Pell and Wallis respectively (see [Maanen 1986] and [Jesseph 1999]). These refutations were only meant to show that particular "solutions" were incorrect but did not pretend to prove the impossibility of finding a solution at all. Therefore I shall not discuss them in this paper.

In order to appreciate the constructions and the impossibility arguments of the 17th century, one particular ancient result is central: In the measurement of the circle Archimedes (287–212 BC) proved rigorously that a circle is equal (in area) to a right angled triangle with sides equal to the radius and the circumference of the circle[2]. Since the theorems of Euclid's Elements show how to square a triangle and how to construct a right angled triangle with one given side and equal to a square Archimedes' result shows that the quadrature of the circle is equivalent to its rectification. Or, put differently, one can construct the circumference, if and only if one can square the circle. This was clear to all later contributors to the problem.

The impossibility aspect of the quadrature of the circle has a thinner history. Though a few late Greek commentators such as Eutocius (c. 480–540 AD) (see [Knorr 1986, 363]) declared that the rectification (and hence the quadrature) of the circle was impossible, most Greek authors including Pappus (c. 290–350 AD) were careful not to commit themselves on this point. This contrasts Pappus' and other commentator's clear statements concerning the impossibility of the duplication of the cube and the trisection of the angle with ruler and compass.[3]

The greater caution concerning the quadrature of the circle may have been due to the obvious connection between the problem of rectification of the circle and the question of the commensurability of the diameter

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1 These important contributions are discussed by Knorr (1986), Hobson (1913), Berggren & Borwein (2004) and others.

2 See e.g., [Archimedes 1960, 127–134].

3 Pappus *Collectio* book III Chapter VII and book IV Chapter XXXVI (p. 39 and 209 in Vol. 1 of Ver Ecke's translation).

and the circumference of a circle mentioned by several Greek commentators ([Knorr 1986, 363]). Clearly commensurability would have implied constructability, but even the commensurability question remained unanswered. An explicit connection between the problem of the quadrature of the circle and earlier proofs of incommensurability was mentioned by Simplicius (6th century AD):

> The reason why one still investigates the quadrature of the circle and the question as to whether there is a line equal to the circumference, despite their having remained entirely unsolved up to now, is the fact that no one has found out that these are impossible either, in contrast with the incommensurability of the diameter and the side (of the square) (Simplicius *In Physica* ed. Diels, 1802, translated by Knorr [Knorr 1986, 364])

This is a remarkable statement for it is, as far as I know, the first time someone came close to suggesting that the impossibility of the quadrature of the circle could and should be dealt with as a mathematical theorem that requires proof, rather than as a meta-statement about the problem solving activity. No such suggestion seems to have been made by Greek mathematicians concerning the other two classical problems (Lützen 2010, 6). It seems to be the proximity of the rectification problem to the problem of commensurability, whose impossibility had been proved many centuries earlier, that suggested the desirability of a *proof* of impossibility also for the quadrature of the circle.

The possibility or the impossibility of the circle quadrature was debated in Europe during the middle ages[4], but the first real attempts to *prove* the impossibility of the quadrature of the circle were provided by mathematicians of the 17th century.

In the last half of the 18th century the problem of the quadrature of the circle entered a new phase with Lambert's (1728–1777) proof (1761)[5] of the irrationality of π. The present paper will not consider these developments.

3. DIFFERENT QUADRATURES OF THE CIRCLE

To find the square equal to a given circle, or to find a line segment equal to its circumference. This is the general formulation of the problem

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4 See Knorr (1991) and Clagett (1964) in particular pp. 398–432, 576–609.

5 [Lambert 1768].

whose impossibility we are dealing with here. However, one question remains: What does it mean to "find" the said square or line segment? Here we can distinguish many meanings in the 17th century:

1. Geometric constructions (continuing the Greek tradition):

   a. By ruler and compass. Today this is often considered the main issue concerning the classical problems, but it was rarely mentioned by the authors of impossibility proofs of the 17th century, possibly because it was (correctly) believed that the impossibility of this problem would follow from an impossibility of an algebraic solution.

   b. By intersection of algebraic curves other than the straight line and the circle. The other two classical problems were known from Greek time to be solvable by conic sections,

   c. By transcendental curves (mechanical curves as Descartes called them): As mentioned above, Greek solutions of this type were known.

2. Arithmetic or algebraic solutions (the new 17th century methods).

   a. By rational operations (the question of commensurability).

   b. By algebraic operations (this was the chief domain of the 17th century impossibility arguments).

      i. Express the area of the circle or a sector thereof (see below) as an explicit algebraic expression (function) of the radius (and the cord of the sector) using only rational operations and extractions of nth roots.

      ii. Find an algebraic relation $f(A, r, c) = 0$ between the area A and the radius r (and the cord of the sector c).

   c. By an infinite number of rational or algebraic operations (transcendental expressions). The primary 17th century contributions to the quadrature of the circle (according to the actors themselves and according to a modern evaluation) consisted in such solutions: Viète's product, Wallis' product and Leibniz's series.

Another distinction made in the 17th century was between

1. Approximate circle quadratures (several very accurate methods were devised) and
2. Exact solutions.

The impossibility theorems all concerned the exact solutions.

Finally, the most important distinction made by 17th century mathematicians was between the definite and the indefinite circle quadrature:

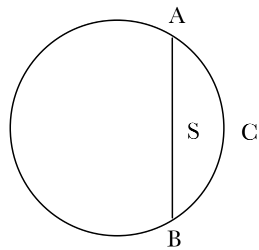

FIGURE 1. The indefinite circle quadrature.

1. The *definite* circle quadrature is the classical problem of determining the area or circumference of the entire circle in terms of its radius (or constructing a square equal to the circle)

2. The *indefinite* circle quadrature asks for the determination of the area or arc length of any sector $S = ABC$ of a circle in terms of the radius of the circle and the length of the cord AB of the sector (Figure 1). This was the preferred problem concerning the quadrature of the circle in the 17th century. I shall refine this case in Section 7.

Some mathematicians, in particular Leibniz (1675–76), made these or similar distinctions very clear[6], whereas other mathematicians did not always formulate which variant of the problem they dealt with. In particular, if a statement of impossibility was made, it was not always clear what exactly was deemed impossible.

The above classification of the geometric methods of solution (1 a,b,c) stems from Descartes (see [Bos 2001]). Descartes only reluctantly used transcendental curves (mechanical curves in his terminology), and in *La Géométrie* he banned them altogether (see [Mancosu 1996, pp. 76–79] and [Jesseph 2007, pp. 418–425]). According to Mancosu and Jesseph, Descartes' clear distinction between algebraic curves and mechanical curves, and his rejection of the latter, was based on his conviction that the quadrature or equivalently the rectification of the circle is geometrically impossible. Indeed, according to Descartes, this impossibility excluded the use of curves like the cycloid, the quadratrix, or the spiral, that could be used for the quadrature of the circle. Thus the impossibility of the quadrature of the circle had deep implications for Descartes' new methodology for the solution of geometric problems, and yet he offered no proof of

6 The classification made here is only a slight variation of Leibniz's classification in ([Leibniz 1675/76]). See Section 12 below.

the impossibility. His only argument was to refer to the Aristotelian claim, that the proportion between the straight and the curved cannot be known ([Mancosu 1996, p. 77]).

4. WALLIS' IMPOSSIBILITY ARGUMENT OF THE DEFINITE ALGEBRAIC CIRCLE QUADRATURE

It is well known that the crowning achievement of Wallis' *Arithmetica Infinitorum* (1656) was an expression of π as an infinite product. More precisely, Wallis found that the ratio of the square of the diameter in a circle to the circle itself (a ratio that we would denote $4/\pi$ and that Wallis denoted $\square$) could be expressed as

$$\square = \frac{4}{\pi} = \frac{3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \cdot 7 \cdots}{2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \cdot 8 \cdots}.$$

It is less well known that Wallis also argued, that this ratio, and thus π, could not be expressed algebraically[7]. He drew this conclusion not from the infinite product expression itself, but from the process that had led him to this expression.

Using bold "induction" and interpolation he filled out an infinite table containing the values of the ratio of a unit square and the area under the curve

$$y = (1 - x^{1/p})^q \tag{1}$$

in the interval $(0, 1)$ for values of p and q equal to $-\frac{1}{2}, \frac{1}{2}, 1, 1\frac{1}{2}, 2, 2\frac{1}{2}, 3, \ldots$ The entry of this table corresponding to $p = q = \frac{1}{2}$ is precisely the ratio $\square$, that he wanted to determine. He argued that the entries of the table having an integer value of either p or q (called the even sequences in the quote below) would be rational fractions, whereas the entries having both p and q half valued would be a rational fraction times $\square$.

Wallis' impossibility argument was based on his expression of the areas in question as ratios of sums. Indeed, if one divides the abscissa interval $(0, 1)$ into n equal parts and sums the ordinates y corresponding to the ordinates of the division points, the sum will according to Wallis represent the area under the curve (1). The ratio of this sum to a sum of an equal number of the longest ordinates (i.e., 1) will thus represent the inverse of

7 Wallis' argument has been discussed by Stedall in her English translation of Wallis' *Arithmetica Infinitorum* ([Wallis 1668]) and by Panza in Chapter 1 of [Panza 2005].

the ratio in question. It can be written in modern notation:

$$\frac{\sum_{i=0}^{n}\left(1-\left(\frac{i}{n}\right)^{1/q}\right)^{p}}{n+1}=\frac{\sum_{i=1}^{n}\left(n^{1/q}-i^{i/q}\right)^{p}}{(n+1)\cdot n^{p/q}}. \tag{2}$$

In order that this ratio shall represent the exact ratio of the areas, n must be taken to be infinite.

With these preliminaries in place we can begin to understand Wallis's argument:

> And indeed I am inclined to believe (what from the beginning I suspected) that this ratio we seek [$\square$] is such that it cannot be forced out in numbers according to any method of notation so far accepted, not even by surds [...] so that it seems necessary to introduce another method of explaining a ratio of this kind, than by true numbers or even by accepted means of surds.
>
> And indeed this, whether opinion or conjecture, seems to be confirmed here, since if we have the appropriate formula, for any even sequence (in the table of Proposition 184) [the above mentioned table] so also we might have obtained a formula of this kind for any odd sequence; then just as for the formulae for the even sequences we have taught how to investigate the ratio of finite series of first powers, second powers, third powers, fourth powers, etc, to a series of the same number terms equal to the greatest of those (in the comment to Proposition 182) so by formulas of the same kind for odd sequences, it would seem there could be investigated similarly the ratio of finite series of second roots of third roots etc. to a series of the same number of terms equal to the greatest of these: why this is not to be hoped for, moreover, we showed in the comment to Proposition 165 ([Wallis 1656, 161]).

In the last mentioned comment Wallis had discussed sums similar to (2) where $p=q=\frac{1}{2}$. He had pointed out that the numerator was a sum of $n+1$ different square roots whereas the denominator is an integer. He explicitly calculated the ratio (2) for $n=6,10,12$ and 20. For example for $n=10$ he got

$$\frac{\sqrt{100-0}+\sqrt{100-1}+\sqrt{100-4}+\sqrt{100-9}+\cdots+\sqrt{100-81}+\sqrt{100-100}}{110} \tag{3}$$

which according to Wallis (p. 123) "cannot be written otherwise more briefly than"

$$\frac{24+3\sqrt{11}+4\sqrt{6}+\sqrt{91}+2\sqrt{21}+5\sqrt{3}+\sqrt{51}+\sqrt{19}}{110}. \tag{4}$$

> And in the same way, as more parts of the radius are taken, so the expression for the ratio necessarily becomes more intricate; and indeed requires repetition of almost all the roots, since they happen little, indeed rarely, and only as if by chance, to be commensurable either with rational numbers or with each other ([Wallis 1656, 123])

So when n grows the expression will contain more and more square roots and thus seems to end up being less and less expressible.

> Therefore if the radius [...] is taken in infinitely many parts (which it seems must be done for our purposes) the ratio of all sines, to the radius taken the same number of times, that is the quadrant [...] circle to the circumscribed square [...], seems wholly inexpressible. ([Wallis 1656, 124])

What Wallis presents in the above quoted comment to Proposition 190 seems to be conceived as a strengthening of this comment to Proposition 165. Wallis seems to argue, that if $\square$ were expressible in terms of a finite number of radicals, then the expression (2) would be thus expressible (for $n = \infty$) not only for $p = q = \frac{1}{2}$, but for all integer or "half" values of p and q. Thus an infinity of different sums of radicals (not just square roots but all other kinds of mth roots) would be reducible to the same finite number of radicals, which is "not to be hoped for".

It is not entirely clear whether Wallis considered this argument as a proof or whether it was only meant to render plausible, that $\square$ is not an (explicit) algebraic number. In his later critique of Gregory's argument he called his own argument a demonstration:

> ... as having many years since demonstrated the same myself, though he [Gregory] take no notice of it, in my Arithmetica Infinitorum, Proposition 190 with ye Scholium annexed ([Wallis 1668, 285]).

In the *Arithmetica Infinitorum* itself Wallis did not express himself as sharply. As we have seen in the quote above, he referred to the non-algebraic nature of $\square$ as an "opinion or conjecture" and wrote that it "*seems* to be confirmed here".

In fact Wallis himself had already cast doubt on his argument in the comment to Proposition 165, where he pointed to situations, where sums of qth roots do add up to rational numbers. Indeed, while studying the quadrature of curves having an equation

$$y = \sqrt[q]{x}. \tag{5}$$

Wallis had expressed the area under the curve in the interval $(0, 1)$ as the ratio

$$\frac{\sum_{i=0}^{n} \sqrt[q]{i}}{(n+1)\sqrt[q]{n}}. \tag{6}$$

However, Wallis had argued that for $n = \infty$ this ratio is equal to $\frac{q}{q+1}$, "the infiniteness itself indeed (which seems amazing) destroying the irrationality" ([Wallis 1656, 125]). At this point of the book he saw this as a sign that the quadrature of the circle was not out of sight:

> … so clearly not all hope was lacking of eventually finding the ratio of a series of universal roots (of augmented or reduced series) to a series of equals. And indeed if not in every case, at least for those so far set out; and perhaps even in those that touch on the quadrature of the circle itself or the ellipse, or also the hyperbola, something may be gained. ([Wallis 1656, 125])

The reader of Wallis' impossibility argument, who has not forgotten this last remark, may reasonably ask himself, if it could not indeed be the case, that also in the sum (2) for $p = q = \frac{1}{2}$ corresponding to the quadrature of the circle, a similar amazing thing could happen, so that the infinity destroys the irrationality, or just allows the ratio to be expressed in terms of finitely many radicals? Wallis did not explicitly address this possibility, but his reference to the whole table of values of the ratio (2) may be a way of making such a miracle extremely unlikely, or too much to be hoped for, as he put it.

Of course the reader could argue against this that Wallis himself showed, that for integer values of q (the even rows in the table) the ratio actually turns out to be a rational number. And even when q is half valued ($\frac{1}{2}, 1\frac{1}{2}, 2\frac{1}{2}, \dots$) the ratio is also rational when p is an integer. So why could it not happen that the ratio could be rational (or algebraic) in the remaining cases, where both p and q are half-valued? Little wonder that Wallis's contemporaries and followers did not consider his argument as a convincing proof.

5. THE NEW ANALYSIS AND IMPOSSIBILITY PROOFS

New impossibility proofs often come hand in hand with a new technique or method of proof: The ancient proof of the impossibility of measuring the side and the diagonal in a square with a common measure came hand in hand with the method of proof by contradiction, Fermat's number theoretical impossibility proofs came hand in hand with his method of infinite descent, and the proof that it is impossible to prove the parallel postulate came hand in hand with the idea of a model of an axiomatic system. Also in the case of the quadrature of the circle, it was a new method of proof or rather a new mathematical technique that allowed the mathematicians of the 17th century to prove impossibility results, namely the new analytic or algebraic methods of geometry.

It is often stated that in order to prove the impossibility of the classical construction problems, it is necessary to translate them into algebra. Coolidge formulated this conviction thus:

> It is clearly impossible to make an adequate discussion of the possibilities of various geometrical instruments without the aid of analytic geometry. I do not know who first grasped this idea, but it seems evident enough to-day. ([Coolidge 1940, 53])

The idea was already grasped by the mathematicians of the 18th century. For example Nicolas de Condorcet (1743–1794) formulated it as follows in connection with the problem of two mean proportionals:

> … but one did not have, and one could not have had the complete analysis, until one had found the principles of application of algebra to geometry[8] ([Condorcet 1775/78, 61])

A similar opinion can be found in Jean Étienne Montucla's (1725–1799) book on the history of the quadrature of the circle:

> This impossibility is founded on the theory of equations and the nature of geometric curves.[9] ([Montucla 1754, 274–275])

Considering that Descartes was one of the inventors of analytic geometry, one might expect to see him use it in connection with impossibility arguments. And indeed in his discussion ([Descartes 1637]) of the nature of the duplication of the cube and the trisection of the angle, one can find elements of such a view, but in his final impossibility arguments, Descartes did not really appeal to the algebraic translation of the problems (see [Lützen 2010, 21–25]).

The first mathematician who explicitly emphasized the importance and power of the new analytic techniques for the proof of impossibility may have been James Gregory. In the preface to his *Vera circuli et hyperbolae quadraturae* (1667) he wrote:

> But the task of analysis as well as common algebra is not only to solve problems, but also (if this turns out to be the case) to prove their impossibility. ([Gregory 1667, 408])

Like Simplicius before him, Gregory pointed to the similarity of the problem of the quadrature of the circle and the incommensurability of the side and diagonal in a square, but contrary to Simplicius he asserted the impossibility of the first problem:

> For it is the same thing (as I will demonstrate in this treatise) to exhibit the ratio of the circle to the square of its diameter analytically or in a form hitherto

8 "… mais on n'eut & l'on ne put avoir d'analyse complète du Problème, que lorsqu'on eut trouvé les principes de l'application de l'Algèbre à la Géométrie".

9 "Cette impossibilité est fondée sur la théorie des équations & la nature des courbes géométriques".

known, as to exhibit the ratio between the side of a square and its diagonal in a commensurable way. ([Gregory 1667, 410])

As we shall see below, he later insisted that he had indeed proved the impossibility of the algebraic quadrature of the circle, but in the introduction he admitted that he had not given a complete proof in geometric language. In fact,, in a very remarkable continuation of the above quote Gregory stressed, that such a proof would require a general theory of analytic quantities and their incommensurables. He found it surprising that no one had ever written such a work (except for Euclid's Elements book X), since in this vast area one could prove not only when geometric problems could be solved geometrically or analytically or when they needed recourse to mechanical curves, but also why constructions using the mesolabium of Eratosthenes (that constructs mean proportionals or nth roots) cannot always be replaced by constructions by ruler and compass. Moreover, such a theory would show when equations can be reduced to pure equations (extraction of radicals). According to Gregory, such a treatise would "not only be useful for speculative geometry but also very admirable". ([Gregory 1667, 10–11], cf. [Dehn & Hellinger 1939, 474–475])

Since Gregory did not develop such a theory of analytic and incommensurable quantities, it is not entirely clear what he had in mind. However, we may get an idea by considering his own definition of a quantity "composed" of other quantities through the five algebraic operations of addition, subtraction, multiplication, division and extraction of roots as well as any other imaginable operation ([Gregory 1667, Definition 5, p. 413]). In particular, in Definition 6 Gregory called such a composite quantity "analytic", if it was composed using only the five algebraic operations. So what Gregory had in mind was probably a theory of such composites, similar to Euclid's theory of quadratic irrationals in book X of the Elements. His idea could have been to derive properties of a particular type of composites, such that one can show, that a given composite is not of this type. For example one would according to Gregory be able to decide, whether the solution of a particular equation is an analytic composite (presumably of its coefficients). Indeed, this was his line of approach in his purported argument, that the quadrature of the circle is impossible analytically.

Gregory's introduction to ([Gregory 1667]) may be the first publication in the history of mathematics, where impossibility theorems were generally mentioned as an important field of inquiry, that could be pursued by mathematical methods. It is particularly noteworthy that Gregory wanted to investigate *when* equations can be solved by radicals. Here he was much ahead of his contemporaries and immediate successors, for whom the problem was a constructive search for the solution of the equation. Only with Abel and Galois around 1830 do we find an attempt to answer Gregory's question using methods that agree with the general guidelines laid out by Gregory. In a more general way, Gregory's call for a theory of incommensurable quantities may point in the direction of the later theory of transcendental numbers and functions.

6. GREGORY'S ARGUMENT OF IMPOSSIBILITY OF THE ALGEBRAIC INDEFINITE CIRCLE QUADRATURE

Gregory's proof of the impossibility of the definite circle quadrature mentioned in the introduction to his *Vera circuli et hyperbolae quadratura* was only a corollary to a proof of the impossibility of the indefinite algebraic quadrature of the circle. His line of attack was to examine an approximation method similar to that of Archimedes, and from its structure to conclude, that no algebraic "composite" could express the area of a sector exactly in terms of its cord.

Before we analyze Gregory's argument, we must discuss his concept of a composite quantity in a little more detail. Heinrich (1901) stated that Gregory's concept of a quantity composed of other quantities is completely equivalent to Euler's later function concept. However, I think there is a difference between the two concepts, a difference that will be important in the following: Where Euler's function of a variable quantity is an analytical expression composed of this variable and numbers or constant quantities, there are no such numbers or constant quantities in Gregory's formulation. That means that Gregory's analytic composites can contain rational numbers (for example $2/3$ can be written $(x + x)/(x + x + x)$, where x is one of the quantities of which the composite is composed) as well as explicit algebraic numbers. But transcendental numbers cannot figure in Gregory's analytic composites. That means, that where πx is an algebraic (even rational or integer) function of x for Euler, it is not an analytic composite in Gregory's sense. Moreover, Gregory's analytic composites are explicit expressions, whereas Euler's analytic functions can be given implicitly through an equation. In the following, I shall write $f(x, y)$ in places where Gregory would write "a quantity composed algebraically of x and y."

As Archimedes had done in his *Measurement of the Circle*, Gregory approximated the circular area with inscribed and circumscribed polygons and investigated what happens, when the number of sides of these approximating polygons is successively doubled. But Gregory departed from the Archimedean procedure in two respects: First, he considered the

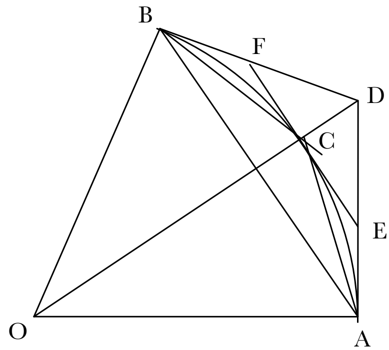

FIGURE 2. Gregory's procedure.

areas rather than the circumferences of these figures; and second, he considered an arbitrary sector of the circle, rather than the whole circle.[10]

Let OAB be a sector of a circle (Figure 2). The first inscribed polygon I_1 is the triangle OAB. The second inscribed polygon I_2 is the quadrangle $OACB$, where C is the midpoint of the arc AB. The third inscribed polygon I_3 is similarly obtained by bisecting each of the arcs AC and BC etc. The first circumscribed polygon C_1 is the quadrangle $OADB$, where AD and BD are tangents to the circle, and the second C_2 is the pentagon $OAEFB$, where EF is the tangent of the circle through C, etc. In Proposition I-VI Gregory proved that

$$(7) \qquad I_{n+1} = \sqrt{C_n I_N}, \quad C_{n+1} = \frac{2C_n I_{n+1}}{C_n + I_{n+1}} = \frac{2C_n I_n}{I_n + \sqrt{C_n I_n}}$$

This double series of inscribed and circumscribed polygons form what Gregory called a convergent series (the origin of this term). Since the area S of the sector lies between I_n and C_n for all n Gregory concluded, that the limit ("termination" in his formulation) of the series must be equal to S. He considered S as a composite of I_1 and C_1, and thought of the operation

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10 Huygens had already in (1654) shown how inscribed and circumscribed polygons could be used more effectively than Archimedes had done, to approximate the area and circumference of a circle and of a circular sector. For example given a regular polygon circumscribed around a circle and a similar inscribed polygon, Huygens could prove, that if one determines two similar regular polygons that are the two mean proportionals between the two given ones, then the circumference of the smaller will be greater than the circumference of the circle, and the larger of the means will exceed the (area of) the circle. This theorem allowed Huygens to tease out twice as many decimals in the approximation of π as a simple comparison would give. Other ingenious theorems allowed him to increase the accuracy even more.

that determined S from I_1 and C_1 as a sixth type of operation in addition to the five analytic or algebraic operations. He wanted to show that this new operation is not analytic.

In order to achieve his goal Gregory pointed out, that if one can find an analytic (algebraic) composite f of two variables such that

$$f(I_n, C_n) = f(I_{n+1}, C_{n+1}) \tag{8}$$

and if K denotes the constant value of $f(I_n, C_n)$ and we let n tend to infinity, we get

$$f(S, S) = K = f(I_1, C_1). \tag{9}$$

This is an algebraic equation from which S can be determined as a composite[11] of I_1 and C_1:

$$S = F(I_1, C_1). \tag{10}$$

Gregory gave an example of a different converging double sequence where this method could be used to find the limit, but then went on to argue, that in the case of the double sequence of inscribed and circumscribed polygons an analytic composite f satisfying (8) does not exist.

In order to carry through this argument, he introduced two new parameters u, v such that

$$I_1 = u^2(u+v) \text{ and } C_1 = v^2(u+v). \tag{11}$$

It is not hard to see that conversely u and v can be expressed as algebraic composites of I_1 and C_1. Thus the area of the sector S is an algebraic composite of I_1 and C_1 if and only if it is an algebraic composite of u and v. From the recursion relations (7) Gregory concluded that

$$I_2 = uv(u+v) \text{ and } C_2 = 2uv^2. \tag{12}$$

Inserting the expressions (11) and (12) into (8) Gregory concluded that the composite f must satisfy the identity

$$f(u^2(u+v), v^2(u+v)) = f(uv(u+v), 2uv^2). \tag{13}$$

He claimed that such algebraic expression f could not exist[12] and gave two arguments for it:

[11] Here Gregory seems to imply that S can be found as an explicit algebraic composite which does not agree with his statement, that general algebraic equations may not be solvable by radicals.

[12] In Huygens' words: "il n'y a aucune quantité qui puisse estre composée analytiquement & de mesme manière, des termes $a^3 + aab$, $abb + b^3$ & des termes $aab + bba$, $2bba$" ([Huygens 1668a, 229]). As we shall see below, Huygens did not believe Gregory's statement.

1. As a result of the u^3 term, the left hand side $f(u^2(u+v), v^2(u+v))$ will always contain higher powers of u than the right hand side $f(uv(u+v), 2uv^2)$, which only contains u to the second power.

2. $f(u^2(u+v), v^2(u+v))$ will contain more terms than $f(uv(u+v), 2uv^2)$.

Hence Gregory concluded that one cannot express the area of a sector as an algebraic composite of its inscribed triangle and its circumscribed quadrangle.

It is easy to see that the inscribed triangle I_1 and the circumscribed quadrangle C_1 are algebraic functions of the radius r of the circle and the cord $c = AB$ of the sector and conversely. The question of the indefinite algebraic circle quadrature as formulated in Section 2 is thus equivalent to the question, whether the area of the sector is an algebraic composite of I_1 and C_1. Gregory therefore claimed to have proven the impossibility of the indefinite algebraic circle quadrature.

7. HUYGENS' CRITIQUE AND GREGORY'S ANSWERS

Already a year after its first appearance, Gregory's book was criticized by Huygens in a review in the *Journal des Sçavans* of July 2. 1668 ([Huygens 1668b, 228–230]). The review soon led to an extensive correspondence ([Huygens 1895, 228–399], [Huygens 1940, 259–315]) between Huygens and his fellow members of the Royal Society. Huygens' first objection was that Gregory had not given sufficient demonstration of the impossibility of finding an algebraic composite f satisfying (13). Although Huygens' counterexample was incorrect (see [Heinrich 1901, 84]), we would agree with Huygens that the two arguments mentioned above are hardly convincing and Gregory's attempts to repair the arguments never came much closer to a proof. For example, in letters to Oldenburg and Collins ([Gregory 1668c, 308], see also [Gregory 1939, 63–64]) he argued, that if (8) is satisfied then the equation

$$f(a,x) = f\left(\sqrt{ax}, \frac{2ax}{a+\sqrt{ax}}\right) \tag{14}$$

would have infinitely many solutions, namely: (a,x), $(\sqrt{ax}, \frac{2ax}{a+\sqrt{ax}})$, ... But since an algebraic equation can only have finitely many solutions, the composite f cannot be algebraic.

As we shall see below, arguments using that an algebraic equation can only have finitely many roots, were applied successfully by Newton, but Gregory's argument seems to miss the point by considering pairs a, x as solutions to one equation. The argument is also surprising, because, if valid, it would exclude the examples, where Gregory had successfully found the limit of a convergent double series using this method.

Secondly, Huygens pointed out, that even if Gregory succeeded in proving that an algebraic composite f satisfying (13) does not exist, this would only prove that the indefinite algebraic circle quadrature could not be obtained using the successive method suggested by Gregory. Huygens maintained that other methods might still lead to the goal. Indeed, Gregory's initial argument is vulnerable to this objection, and also in this case he tried to repair the problem. His counterargument was directed to the president of the Royal Society Henry Oldenburg and published in its Philosophical transactions of July 13. 1668 ([Gregory 1668b, 240–243]). In this first defense of his methods Gregory tried to argue, that if the area can be expressed algebraically as a composite F of the inscribed triangle and the circumscribed quadrangle as in (10), then this composite must itself necessarily satisfy the identity

$$F(I_n, C_n) = F(I_{n+1}, C_{n+1}) \tag{15}$$

which is of the form (8). Thus if the indefinite quadrature of the circle is possible, then Gregory's method would lead to it and Huygens' counterargument would be invalid.

Gregory only tried to establish (15) in the case of $n = 1$, implying apparently that this would establish the identity for all n. The argument is indirect. He assumed that

$$|F(I_1, C_1) - F(I_2, C_2)| = \alpha > 0. \tag{16}$$

Due to the convergence he could determine an n such that

$$C_n - I_n < \alpha. \tag{17}$$

Since $S = F(U_1, C_1)$ we know that

$$I_n < F(I_1, C_1) < C_n \tag{18}$$

and since $F(I_1, C_2)$ is composed from I_2, C_2 just as $F(I_1, C_1)$ is composed from I_1, C_1 Gregory concluded that

$$I_{n+1} < F(I_2, C_2) < C_{n+1} \tag{19}$$

and so a fortiori

$$I_n < F(I_2, C_2) < C_n. \tag{20}$$

But (18), (20) and (17) show that

$$|F(I_1, C_1) - F(I_2, C_2)| < \alpha \tag{21}$$

which clearly contradicts (16).

The crucial and problematic point in this argument is the step from (18) to (19). The conclusion is clearly not correct for an arbitrary function F and an arbitrary doubly convergent series. For example, if

$$C_{n+1} = \sqrt{C_n}, \quad I_{n+1} = \sqrt{I_n}, \quad C_1 = 2, \quad I_1 = \frac{1}{2}$$
$$F(x, y) = 4x/y$$

then (18) holds for n=2, but (19) does not hold. However, Gregory's conclusion is correct for the series that he has in mind if it is assumed, that the area of *any* circular sector (smaller than a quarter circle for example) can be expressed algebraically by its inscribed triangle and its circumscribed quadrangle by *the same* function (10). This is a crucial assumption which Gregory did not explicitly refer to in his argument.

The argument, that Gregory did not give, could go as follows:

Consider a circular sector S with inscribed triangle I_1 and circumscribed quadrangle C_1 as above. If one draws a sector similar to S but a times larger in area (a being any positive real number) then it is clear, that its inscribed triangle will also be a times as large as I_1 and its circumscribed quadrangle will be a times larger than C_1. Therefore, since we assumed (10) to hold for all sectors we have the general identity

$$aS = F(aI_1, aC_1). \tag{22}$$

Now, if the angle of the given sector S is halved, the sector is divided into two congruent sectors S'. Let I'_1 and C'_1 denote the inscribed triangle and the circumscribed quadrangle of the half sector S'. From the way the inscribed and circumscribed sectors were formed, it is obvious that

$$2I'_1 = I_1, \quad 2C'_1 = C_2. \tag{23}$$

By assumption we also have

$$S = F(I_1, C_1), \quad S' = F(I'_1, C'_1). \tag{24}$$

Combining (22) (for $a = 2$ and S replaced by S'), (23) and (24), we get the identity:

$$F(I_2, C_2) = F(2I'_1, 2C'_1) = 2S' = S = F(I_1, C_1). \tag{25}$$

This argument (let us call it A) can easily be generalized to prove the identity (15) for all n.

Thus I have shown that Gregory was in fact correct when he concluded, that if his method could not solve the algebraic indefinite quadrature of the circle, then no other method would lead to it either. The argument A also shows that Gregory's conclusion from (18) to (19) is indeed correct, but at the same time it makes Gregory's argument superfluous. Indeed the argument A led to (15) directly without the use of the indirect proof suggested by Gregory. Thus it seems unlikely that Gregory had the above proof in mind, when he concluded from (18) to (19).

8. GREGORY AND THE DEFINITE CIRCLE QUADRATURE REINTERPRETATION OF THE INDEFINITE CIRCLE QUADRATURE

I have already mentioned, that Gregory in the introduction to his *Vera Circuli…* claimed to have demonstrated, that the definite circle quadrature is impossible algebraically. However, this theorem was nowhere formulated or proved explicitly in Gregory's book, and as we have also seen he admitted, that he had not provided a proof in geometric language. Thus it is not surprising, that an attentive reader such as Wallis at first did not get the impression, that Gregory claimed to have proven it ([Wallis 1668, 283]). Yet, Gregory reaffirmed in later communications that this was his intent. His argument seems to have been that since he had shown in general that any sector could not be squared algebraically, then in particular the quarter circle (and thereby the full circle) cannot be squared algebraically. Huygens, in his critique of Gregory's book, pointed out that this conclusion is invalid, and in a report written on November 14. 1668 to Lord Brouncker concerning the discussion between Gregory and Huygens, Wallis agreed with Huygens ([Wallis 1668]). He pointed out that one can show, that the trisection of the angle is impossible in general[13], but that there are nevertheless special trisectable angles such as the right angle. Similarly, he argued, the indefinite circle quadrature can be impossible while some cases (as for example the entire circle) could be analytically squared.

13 Descartes claimed that he had proven this (See Lützen 2010). Wallis' formulation of the impossibility of the indefinite angle trisection is strange. Having written the cubic equation determining the cord of the third of an angle in terms of the cord of the angle itself he wrote that this solution "cannot be universally designed by those he (Gregory) calls Analyticall operations" ([Wallis 1668, 285]) and he referred to Charles (perhaps Descartes?) and Schoten. In view of Cardano's formulas this may sound surprising, but perhaps he referred to the unavoidability of complex numbers in this irreducible case. He nowhere mentioned ruler and compass.

The reason for the disagreement seems to be at least partially due to an ambiguity in the formulation of the indefinite algebraic circle quadrature. There are two subtly but importantly different formulations in play:

1. There exists an algebraic function in two variables, such that the area of any sector of a circle is equal to this function applied to its radius and cord. In modern terminology this (almost) means that arcsine is an explicit algebraic function

2. For each given sector of a circle there exists an algebraic function whose value evaluated in the radius r and the cord c of the sector is equal to its area. This corresponds to the statement that the area is explicitly algebraic over $\mathbb{Q}(r,c)$ and almost to the statement that arcsin v or equivalently $\sin v$ is (explicitly) algebraic over $\mathbb{Q}(v)$.

There is a much weaker formulation of the indefinite circle quadrature, that does not seem to have been contemplated by the actors, but which should be mentioned, because its converse may arguably be the impossibility statement Gregory believed to have proved:

3. There exists a sector of a circle (different from the zero sector) whose area is an algebraic function of its radius and cord.

It is clear that if the circle is indefinitely squarable in the first sense, then the circle is indefinitely squarable in the second sense too (for this to hold it is important that the algebraic function only contains algebraic constants as implied by Gregory's definition). It is also clear that the second claim implies the third. However, the converses are not obvious. [14]

Corresponding to these three types of indefinite algebraic circle quadratures we can consider three impossibility claims.

4. There exists no algebraic function in two variables such that the area of any sector of a circle is equal to this function applied to its radius and cord.

5. There exist a sector of a circle for which there exists no algebraic function whose value evaluated in the radius and the cord of the sector is equal to its area.

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14 The first statement is now known to be false because Arcsin and sin are transcendental functions. The second statement is also false. Indeed it follows from Lindemann-Hermite's theorem that v and $\sin v$ cannot be algebraic simultaneously. Thus constructible points on the unit circle correspond to transcendental arcs and areas. The last statement is true. Indeed for each rational value of q the equation $\sin v = v + q$ has a solution v (a different solution for each value of q). For these countably many values of v $\sin v$ is even in $Q(v)$ and thus a fortiori algebraic over this field.

6. For any given sector (different from zero) of a circle there exist no algebraic function whose value evaluated in the radius and the cord of the sector is equal to its area.

They are the converses of 1, 2 ,3 respectively, and thus $6 \Rightarrow 5 \Rightarrow 4$ where the last implication rests on Gregory's convention, that algebraic expressions only contain rational constants. Statement 6 is false and 5 and 4 are true.

It is not so easy to determine if Gregory claimed to have proven impossibility 4 or 6. He formulated the theorem in the following way:

> Prop. XI. Theorem: I say that the circular, elliptic or hyperbolic sector *ABIP* is not analytically composed from the triangle *ABP* and the trapezoid *ABFP*[15]. ([Gregory 1667, 419])

Since he did not chose any particular sector we can rather safely say that he did not have impossibility statement 5 in mind.

For the hypothesis that Gregory had formulation 4 in mind speaks the fact, that his argument (16)-(21) only applies under this circumstance. However, this only becomes clear in my attempt (22)-(25) to complete Gregory's proof, so the argument is far from conclusive.

For the hypothesis that Gregory had the (incorrect) impossibility statement (6) in mind speaks his conviction, that the impossibility of the indefinite circle quadrature implies the impossibility of the definite circle quadrature as a special case. Indeed, the formulation 6 would have this obvious implication, whereas 4 or 5 do not have this implication. However, if he had formulation 6 in mind his argument for the impossibility of the indefinite circle quadrature is vulnerable to Huygens' second objection. Indeed, in that case my amplification of the second part of his argument does not work.

Huygens and Wallis both read Gregory as saying either 4 or 5. In both cases they were right in pointing out, that the impossibility of the definite circle quadrature does not follow from it.

Huygens' reading of Gregory seems to be close to impossibility statement 4 above:

> Thus in order to conclude that the ratio of the circle to the square of its diameter is not analytic, one needs to demonstrate not only that the sector of the

[15] "Prop. XI. Theorema: Dico sectorum circuli ellipseos vel hyperbolae ABIP non esse compositum analyticè à triangulo ABP & trapezio ABFP". Gregory dealt with all the three conic sections at once.

circle is not indefinitely analytical...but that it is also true in all definite cases.[16] ([Huygens 1668b, 273])

Wallis on the other hand seems to have believed, that formulation 4 and 5 were equivalent:

> That his 11th proposition[17], though ever so well demonstrated, shews onely yt ye Sector *indefinitely considered* can not be so compounded as is there sayd: Or, (which is equivalent) not every Sector. Notwithstanding which it might well inough be possible, that *some* Sector (if not all) might be analyticall to its Triangle or Trapezium: (And I think he doth allow it so to bee, or even commensurable...) ([Wallis 1668, 284])

The quote shows Wallis' uncertainty about the exact meaning of Gregory's statements, and the entire correspondence shows, that it was difficult for the correspondents to formulate the subtle differences between the different impossibility statements. In particular this seems to explain the differences in opinion about the relation between the impossibility of the definite circle quadrature and the indefinite one.

9. LEIBNIZ'S ARGUMENT OF IMPOSSIBILITY OF THE ALGEBRAIC INDEFINITE CIRCLE QUADRATURE

In a "Preface to a small work on the arithmetic quadrature of the circle" from 1675–1676 Leibniz gave an argument for the impossibility the indefinite algebraic circle quadrature. This manuscript was published for the first time by Knobloch in 1993/2007 ([Leibniz 1675/76]), so it had no influence on other arguments at the time. Still, it is interesting to compare it to Gregory's argument and to the later argument by Newton, as well as with similar arguments put forward in the 18th century.

Leibniz' argument is brief, clear and simple ([Leibniz 1675/76, 7]). It is a proof by contradiction: Assume that the indefinite quadrature (or equivalently the indefinite rectification) of the circle were possible algebraically, i.e., that there were an algebraic equation

$$P(\sin v, v) = 0 \tag{26}$$

relating the arc v and its sine, where P is a polynomial of degree m.

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16 "Pour conclure donc que la raison du Cercle au Quarré de son diametre n'est pas analytique, il falloit demontrer non seulement que le Secteur de cercle n'est pas analytique indefinitè...; mais que cela est vray aussi *in omni casu definito*".

17 See above. Wallis accepted Gregory's argument that no function satisfying (2) exists, but like Huygens he argued that that only means "that ye converging series cannot his way be determined, not that it can no way be determined analytically ([Wallis 1668, 284]). So in fact he did not accept the conclusion of Proposition XI.

> Under this assumption one can draw a curve of the same degree so that when the abscissa expresses the sine, the ordinate will express the arc and conversely. Thus using this curve one can divide a given angle or arc in a given ratio, or find the sine of an arc that has a given ratio to a given arc. Thus the problem of the universal division of the angle will be of a definite degree ([Leibniz 1675/76, 7]).

Here Leibniz referred to the problem of dividing an angle in a given ratio. This generalization of the classical problem of trisection of an angle asks for the construction of an angle which is $\frac{m}{n}$ of a given angle. Since addition of angles is a simple matter, the problem boils down to dividing an angle into n parts.

However, according to Leibniz it was known, that the division of an angle into n parts depends on an equation of degree n (at least when n is odd). But according to Leibniz this contradicts that the problem can be solved by an equation of a definite degree independent of n.

Leibniz was much clearer than Gregory about what he believed he had shown:

> But to present the relation between the arc to (its) sine in general by an equation of a certain degree is impossible ([Leibniz 1675/76, 6]).

Even clearer is the following quote.

> Except that through this rule (Leibniz's series for Arctan) not only the whole circle but also an arbitrary part of it, and not only the whole circumference but also any arc of it can be found, *which is impossible by a fixed analytic expression.* (Leibniz, 1675/6, 5)

As formulated here, the impossibility statement clearly corresponds to 4 above, except Leibniz took the radius of the circle to be equal to 1 and formulated his impossibility primarily in terms of the rectification rather than the area. Moreover, as opposed to Gregory, he did not insist on an explicit algebraic expression in terms of radicals, but simply asked if there could be an algebraic equation relating the arc and its sine. Thus, as formulated by Leibniz, the impossibility statement undoubtedly says that sin (or equivalently arcsin) is not an algebraic function.

Leibniz also quite clearly stressed that this impossibility of what he called the full quadrature (Quadratura plena) (what we have called the indefinite quadrature) did not imply the impossibility of the definite algebraic circle quadrature

> That this is impossible (the algebraic definite circle quadrature) was asserted by the very ingenious Scotsman Gregory in his book De Vera Circuli Quadratura, but he has not provided a proof there, if I am not mistaken. I still do not see what prevents the circumference itself or a determined part of it to

> be measured, and that the ratio of a certain arc to its sine can be expressed by an equation of a certain degree. ([Leibniz 1675/76, 6])

In other words Leibniz did not believe it had been proved that π was transcendental

10. ANALYSIS OF LEIBNIZ' IMPOSSIBILITY ARGUMENT

When Leibniz invoked the equation corresponding to the division of an angle into n parts, he probably meant an equation expressing $\sin \frac{v}{n}$ in terms of $\sin v$ or more generally a polynomial equation of the form

$$Q\left(\sin\left(\frac{v}{n}\right), \sin v\right) = 0 \tag{27}$$

where Q is a polynomial of a degree that may depend on n (and v).

Leibniz claimed that this equation was of degree n at least when n is odd. He did not supply a proof, but stated that it was admitted by the analysists and could easily be proved, space permitting. He probably referred to the formula

$$\sin v = \sum_{k=0}^{n} \binom{n}{k} \cos^k\left(\frac{v}{n}\right) \sin^{n-k}\left(\frac{v}{n}\right) \sin\left(\frac{1}{2}(n-k)\pi\right) \tag{28}$$

that had been found already by Viète. Applied to an odd value of n all the terms in the sum containing odd powers of $\cos \frac{v}{n}$ will vanish. The even powers can be rewritten in terms of sines of the same or lower powers using the formula $\cos^2 \frac{v}{n} = 1 - \sin^2 \frac{v}{n}$. In this way the right hand side of (28) can be rewritten as a polynomial of degree n in $\sin \frac{v}{n}$. Thus, given $\sin v$ one needs to solve an nth degree equation of the form

$$P\left(\sin\left(\frac{v}{n}\right)\right) = \sin v \tag{29}$$

to determine sine of the nth part of the angle (at least when n is odd). Here P is a polynomial of degree n with integer coefficients. This argument shows why Leibniz restricted his claim to odd values of n.

The argument that Leibniz in the style of the 17th century formulated in terms of curves can now be phrased as follows in terms of functions: If there is a polynomial relation of the form (26), $\sin x$ is an algebraic function of the arc x, and conversely the arc is an algebraic function $\sin^{-1}$ of its sine. And then $\sin \frac{v}{n}$ is an algebraic function of $\sin v$ namely:

$$\sin\left(\frac{v}{n}\right) = \sin\left(\frac{1}{n}\sin^{-1}(\sin v)\right). \tag{30}$$

This gives an algebraic relation of the form

$$Q\left(\sin\left(\frac{v}{n}\right), \sin v\right) = 0 \tag{31}$$

where Q is a polynomial of fixed degree independent of n. However, according to Leibniz this contradicts the fact that the problem of dividing the angle is of degree n, which is not bounded by a fixed value. So he concluded that an algebraic relation of the form (26) is impossible, i.e., the indefinite circle quadrature is impossible algebraically.

From a modern perspective this proof by contradiction is valid if one can prove, that the sine of the nth part of the angle cannot be obtained from the solution of an equation of degree lower than n, or at least that there is no upper bound to the possible degrees when n ranges over all natural numbers. This requires an irreducibility argument, and there is no reason to believe that Leibniz supplied or even could have supplied such an argument.[18]

11. LEIBNIZ'S PUBLISHED PAPERS

The impossibility argument analyzed above was not published during Leibniz's own life time. It constitutes the conclusion of a preface that Leibniz wrote for his small work on the arithmetical quadrature of the circle, a work that he never published. Instead he published a number of papers on the subject (see [Leibniz 1858, 83–132]). In these published papers Leibniz repeatedly claimed that the indefinite quadrature of the circle was impossible algebraically (e.g., Leibniz 1858, 92, 120, 124). In the paper "De dimensionibus figuram inveniendis" that was published in the *Acta Eruditorum* in 1684, he mentioned that this impossibility "can be proved easily in many ways" ([Leibniz 1684, 124]) but he did not reveal any of the ways. He emphasized that this impossibility did not imply that the definite circle quadrature was algebraically impossible as well. In order to bring out this non-sequitur he gave an example of an algebraic curve, whose quadratrix (area curve) was non-algebraic but whose total area was algebraic. However, he only sketched his arguments for these claims. He claimed that if there were an algebraic quadratrix he could prove that it must be of a particular form, and he further argued that no curve of this form would do as a quadratrix ([Leibniz 1684, 125–126]).

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18 In Lützen 2010 I have argued that Descartes had a similar problem in *La Géométrie*. Irreducibility arguments only became part of algebra around 1800 with the works of Gauss and Abel.

12. NEWTON'S ARGUMENT OF IMPOSSIBILITY OF THE ALGEBRAIC INDEFINITE OVAL QUADRATURE

In Lemma 28 of book 1 of the *Principia* Newton (1687) proved that an arbitrary oval cannot be squared indefinitely analytically. This theorem has the impossibility of the indefinite algebraic quadrature of the circle as a simple corollary. Newton formulated his theorem as follows:

> No oval figure exists whose area, cut off by straight lines at will, can in general be found by means of equations finite in the number of their terms and dimensions ([Newton 1687, 511]).

Newton's theorem and its proof have been discussed at great length by his contemporaries as well as by modern mathematicians and historians of mathematics. Arnol'd has given a mathematically exhaustive discussion of Newton's theorem in the book ([Arnol'd 1990]) and in the paper ([Arnol'd & Vasil'ev 1989]). Pourciau (2001) and Pesic (2001) have dealt with the matter in a more historical fashion. The following discussion is based on their detailed work.

Newton did not specify explicitly what he meant by an oval, but it is rather clear that it must be a closed convex algebraic curve. In an addition to the second edition of the Principia Newton stated that he was "speaking of ovals that are not touched by conjugate figures extending out to infinity" ([Newton 1687, 512]). I shall return to Newton's notion of an oval when I have discussed Newton's proof of the theorem.

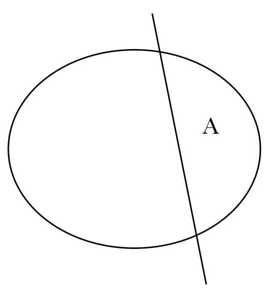

FIGURE 3.

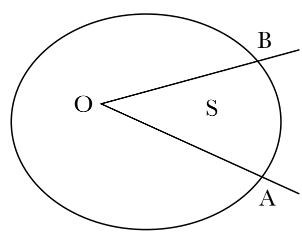

FIGURE 4.

From the formulation of Newton's theorem and his subsequent proof we can infer that the theorem was intended to say, that there is no polynomial equation $P(A, a, b, c)$ that determines the area A in a given oval cut off by an arbitrary line, where a, b, c are the coefficients of the equation of the line[19]. Newton selected an arbitrary point O inside the oval, and a fixed half line OA that intersects the oval in A. We can reformulate the claim of the theorem into the following equivalent claim: If S denotes the sector AOB cut out of the oval between OA and another (variable) line OB (B on the oval) then there is no algebraic relation between S and B, or more precisely, there exist no polynomial equation

$$P(S, x) = 0 \tag{32}$$

relating S and the x-coordinate of B.[20] In a slightly modernized way we can formulate the idea of Newton's argument as follows: Assume that (32) holds. If $\bar{A}$ designates the area of the entire oval and if S is the sector corresponding to a particular point $B = (x, y)$, then $P(S + n\bar{A}, x) = 0$ for all natural numbers n. Indeed, if we let OB rotate one full rotation, B and thus x will return to its former value but the area S will have increased by $\bar{A}$. After n rotations x will return to the same value and the area will have increased by $n\bar{A}$. But that means that the equation (32), considered as an equation in S for fixed x, will have infinitely many solutions namely $S + n\bar{A}$ for any natural n. However, this is not possible if P is a polynomial of finite degree.

Newton formulated the proof somewhat differently, probably in order to bring out the multivaluedness more clearly[21], and in order to formulate the contradiction as a geometric contradiction concerning intersections of curves. He generated a new spiral shaped curve (Figure 5) as the trajectory of a point C moving on the half line OB while this half line rotates around O in such a way, that the distance OC is everywhere proportional to (say equal to) the area of the sector AOB swept out in the oval by the half line OB. Newton correctly remarked, that if the area of the sector can be found "by means of a finite equation", then "all the points of the spiral can be found by means of a finite equation" i.e., it is an algebraic curve. And thus "the intersection of any straight line, given in position, with the spiral can also be found by means of a finite equation" ([Newton 1687, 511]). But it is clear that any straight line (in particular a line through O) will intersect the spiral in infinitely many points. And that cannot be, because an algebraic equation can only have finitely many roots.

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19 In his formulation Newton also seems to include the coefficients of the equation of the oval, but since they are constants we can leave them out.

20 Or equivalently the cord AB.

21 Today we would use Riemann surfaces.

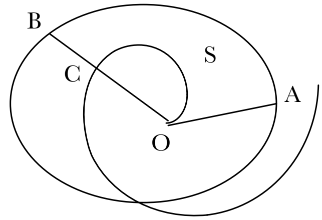

FIGURE 5.

13. EVALUATIONS OF NEWTON'S ARGUMENT

Newton's argument has been hotly debated both by his contemporaries and by modern historians and mathematicians. The theorem is very general for its time and the proof is strikingly qualitative and topological in nature. Of course, there are some algebraic arguments behind the scene, ensuring that an algebraic determination of the area will indeed imply that the spiral curve is algebraic. However, these considerations were left out by Newton. Some of Newton's contemporaries, such as Huygens and Leibniz, considered the theorem to be incorrect, and put forward various "ovals" that they claimed would be counterexamples. The counterexamples were either non-algebraic curves (such as a triangle, mentioned by Huygens) or algebraic curves with self intersections as the lemniscate (mentioned by Leibniz). The lemniscate is a figure eight shaped curve with the property, that the total area swept out in one complete turn of the half line OB is zero, so that the contradiction in Newton's argument does not arise. These counterexamples surfaced because Newton did not sufficiently clearly specify, what he meant by an oval. He would clearly have rejected that the examples of Bernoulli and Leibniz were true ovals. Surprisingly, the editor of Newton's Collected Papers, Whiteside, also believed that the theorem was false and came up with a new kind of counterexample (see note 126 on p. 307 of Newton 1974). Also this example can be rejected on various grounds (see e.g., Pourciau 2001, p. 291). All these authors have tried to discredit Newton's theorem by giving counterexamples. A deeper critique of Newton's theorem must naturally also evaluate the proof.

Even if we supply all the suppressed algebraic details of Newton's proof it seems to show only, that there is no algebraic relation between x and S that holds after any number of turns of the half line OB. To a modern reader the question naturally arises, if there could be a locally valid algebraic relation between x and S? Could it for example be possible to have an algebraic expression that holds, when x increases between its minimal value and its maximal value? If there exists such a local relation, it would serve most purposes. Here Arnol'd has shown ([Arnol'd 1990, 88–89]), that "no analytic oval is algebraically integrable, even locally", and he has shown that any infinitely smooth algebraic curve is analytical. In this way he has shown, that Newton's theorem is correct even in the local version for a large class of algebraic curves. He even maintained that Newton knew of these theorems and their proofs, and found only one problem in Newton's formulation, namely that he did not specify sufficiently accurately what he meant by an oval. Pourciau is less inclined to attribute an entirely satisfactory and clear proof of the theorem to Newton, and here I side with Pourciau.

Still, of the proofs put forward by 17th century mathematicians for the impossibility of the algebraic indefinite circle quadrature, Newton's stand out. As Leibniz' proof it can be made correct, but contrary to Leibniz's proof it was published at the time, even in a prominent place. Its striking generality and qualitative nature also set Newton's theorem and its proof apart from the other arguments put forward in the 17th century. And despite the criticisms put forward by some of Newton's contemporaries and immediate successors, Newton's argument was widely accepted by the mathematicians of the 18th century (see below and Whitesidet's note 126 p.306 of Newton 1974).

14. WHY PROVE IMPOSSIBILITY

Today impossibility theorems and their proofs are an integral and important part of mathematics. Already in 1882 Lindemann became instantly famous, when he proved the transcendence of π and thus the impossibility of the definite algebraic quadrature of the circle and consequently its quadrature by ruler and compass. However, as I have argued in [Lützen 2009, 388–390] impossibility arguments have not always been considered important. For Example Wallis and other early modern mathematicians did not think highly of Fermat's impossibility statements in number theory ([Goldstein 1995, 134–135]). For 17th century geometers mathematics primarily dealt with positive solutions of mathematical problems. And indeed the impossibility statements and -proofs mentioned above were all additions to positive solutions of the quadrature of the circle, either approximate or given by an infinite expression. But if the positive solutions were the main purpose of the works, what then was the purpose of the impossibility theorems?

This is probably seen most clearly in Leibniz's text with the constructive sounding title: "Preface to a small work on the arithmetical quadrature of the circle". The main purpose of the paper was to present what Leibniz called the arithmetical circle quadrature, (what we call Leibniz's series)

$$\frac{\pi}{4} = \frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots \tag{33}$$

giving the area of a circle of diameter one, or more generally what Leibniz called the full quadrature (what we have called the indefinite quadrature)

$$\frac{b}{1} - \frac{b^3}{3} + \frac{b^5}{5} - \frac{b^7}{7} + \cdots \tag{34}$$

expressing the arc in a unit circle (less than a quarter circle) with tangent equal to b. The impossibility proof served the purpose of proving the "corollary": "There cannot be found a more perfect full analytic quadrature whose dimensions of the terms are rational numbers, than the one we have given" [referring to the series (34)] ([Leibniz 1675/76, 6]).

In other words, the purpose of the impossibility proof was to show, that Leibniz's own solution to the problem of the quadrature of the circle was the simplest possible, i.e., to highlight the importance of his positive achievement.

In order to make this clear Leibniz made a classification similar to the one given in Section 3 above of different ways of squaring the circle. He first distinguished between empiric and rational quadratures. The former can be obtained for example by winding a thread around a cylinder, or by filling a cylinder with water and emptying it into a box shaped container. The rational solutions are subdivided into exact and approximate solutions, each of which are subdivided further into linear (using curves) or arithmetical solutions. The latter solutions are subdivided into the finite (analytic) ones and those that involve infinite calculations, and each of them are divided into those that involve only rational numbers and those that involve also irrational numbers.

This classification also implies rather obvious value judgments: rational is better than empirical; exact is better than approximate, finite is better than infinite, and expressions using only rational numbers are better than expressions involving irrational numbers. According to Leibniz's own classification, his solution of the circle quadrature is rational, exact, arithmetical and based only on rational numbers. The only way it could be perfected was by finding a solution that had the same classification except being finite rather than infinite. The impossibility theorem stated, that such a solution does not exist. This leaves Leibniz's solution as the simplest and most perfect one, at least in the case of the indefinite (full) quadrature. In the case of the definite quadrature Leibniz was less assertive. As pointed out above he admitted, that it had not been proven that this problem could not be solved by a finite equation or even by rational numbers. However, he argued, that even if it turned out to be possible to find such a solution, it was doubtful that it could be as beautiful as his series (33) that possesses a "wonderful simplicity" ([Leibniz 1675/76, 5–7]).

In this example we see clearly how impossibility proofs have a place even in a constructive paradigm of mathematics. In fact if constructive solutions are classified and if this classification is normative in the sense that certain solutions are judged better than others, an impossibility theorem can serve the purpose of showing, that a solution is the best possible, because solutions of a "better" kind are impossible. In this way impossibility results can highlight the importance of constructive solutions.

In a slightly less obvious way Gregory's and Newton's impossibility results exemplify the same thing. The "true circle quadrature" that the title of Gregory's book refers to, is a very accurate approximation procedure, that he had discovered and that he published in the book. In fact in the correspondence between Gregory, Huygens and their colleagues this constructive procedure was discussed in a more aggressive tone than the impossibility result. Huygens directly accused Gregory of plagiarism, claiming that the Scottish geometer had copied a method that he had himself discovered previously and communicated to the Royal Society[22]. So also in Gregory's book, the impossibility result served the purpose of showing that one could not hope for a better solution of the indefinite circle quadrature, than the approximate one found by Gregory himself.

Newton formulated his impossibility result in connection with the solution of the Kepler problem. The problem is to determine the position of a planet at a given time from Kepler's laws. The first law states, that the planet rotates around the sun in an ellipse with the sun in one of the foci, and the second law states, that the area traversed by the radius vector from the sun to the planet is proportional to time. Thus when the time is given, the area of the appropriate sector of the ellipse is known, and the problem is to determine the position of the planet from this area. Newton's impossibility result shows that the rectangular coordinates of the planet cannot be determined from the area (and thus from the time) by way of a polynomial equation or by the use of a geometric rational curve. "Therefore I cut off an area of an ellipse proportional to the time by a geometrically irrational

22 See [Huygens 1654] and footnote 10.

curve as follows" Newton continued ([Newton 1687, 513]) and then went on to present his own solution of the Kepler problem. Again we see, that the impossibility problem did not stand alone, but was an integral part in an argument showing, that a certain constructive solution of a problem was the simplest, because a simpler one did not exist.

As pointed out by Guicciardini, Newton's impossibility result can be seen in a wider context as one among several arguments put forward by the mature Newton to show the inadequacy of Descartes' methods ([Guicciardini 2009, 305–308]).

One may question the adequacy of Newton's impossibility proof as a means of rejecting an algebraic solution of the Kepler problem. Indeed, what the astronomer wants to find is not the rectangular coordinates of the planet, but the angle between the radius vector of the planet and a fixed direction. Newton's impossibility result does not exclude an algebraic relation between this angle and the area of the sector (or equivalently the time). In fact, if for example the oval is a circle and the chosen inner point is its center, the angle is a linear function of the area or the time.

The reason why Newton's proof does not apply to the relation between angles and the area of the sector, is that contrary to the rectangular coordinates, angles do not return to the same value after a whole revolution. It will be enlarged by a constant 2π just as the area is enlarged by $\bar{A}$, and therefore the contradiction in Newton's proof disappears.

In the case where the oval is an ellipse with eccentricity ϵ and the chosen inner point is one of its foci, the determination of the angle from the area depends on the so-called Kepler equation

$$M = E - \varepsilon \sin E \tag{35}$$

where M is the mean anomaly, which is a linear function of time and E is the eccentric anomaly. In this case, it can be shown, that E cannot be determined as an algebraic function of M. It cannot even be determined as an elementary transcendental function written as an algebraic combination of exponential, logarithmic and trigonometric functions. These results were proved by Liouville (1837/38, 539) (see Lützen 1990, 362, 394), but they do not follow from Newton's argument.[23]

23 Arnol'd has another view on this subject ([Arnol'd 1990, 85]).

15. 18TH CENTURY EVALUATIONS

The impossibility arguments put forward by Gregory and Newton informed the 18th century views of the quadrature of the circle. Even Leibniz's argument played a role. To be sure, Leibniz's own version of the argument remained unpublished, but slight variants of the argument were put forward by Joseph Saurin (1722) and by Thomas Fantet De Lagny (1729)[24]. By the middle of the century a general "enlightened" opinion on the subject was expressed in three influential publications:

1. Montucla's *Histoire des recherches sur la quadrature du cercle* from 1754
2. D'Alembert's entry on the quadrature of the circle in the *Encyclopédie* (1765)
3. The pronouncement written by Condorcet motivating why the Académie des Sciences would no longer investigate purported circle quadratures (1775/1778).

According to the three authors, the general opinion was, that Gregory and in particular Newton had proved the impossibility of the indefinite algebraic quadrature of the circle, whereas the possibility of the definite algebraic circle quadrature was still an open question. However, both Montucla and d'Alembert expressed somewhat diverging personal opinions. In an appendix to his book Montucla declared, that upon reflection he considered Gregory's argument to constitute a proof of the impossibility of the definite circle quadrature:

> ... for if it is true, as it seems one cannot contest him [Gregory], that in general the ratio of a segment [of an arc] or a sector to the inscribed or circumscribed polygon cannot be expressed by a finite function then it is evident that this holds equally true of the entire circle and of any arbitrary particular sector.[25] ([Montucla 1754, 193])

This shows that at least when he wrote the Appendix Montucla interpreted Gregory's proof as having established version 6 in Section 7 above. D'Alembert, on the other hand, cast doubt on the rigor of Newton's argument without rejecting it entirely ([D'Alembert 1768])[26].

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24 On Lagny see [Costabel 2008].

25 "Car s'il est vrai, comme il semble qu'on ne peut le lui contester, qu'en général le rapport d'un segment ou d'un secteur au polygone inscrit ou circonscrit ne peut être exprimé par une fonction finie, il est évident que cela aura également lieu à l'égard du cercle entier, & de quelque segment ou secteur particulier que ce soit".

26 [Jacob 2005] and [2006] gives an interesting analysis of the enlightenment view of the quadrature of the circle. Panckoucke's impossibility proofs have been discussed by Loveland [2004].

While drawing on the 17th century impossibility arguments, the three mentioned 18th century discussions differed in an essential way from those of the 17th century : Where the 17th century impossibility arguments had been put forward in order to highlight a positive solution of the problem, the purpose of the three mentioned treatments was to enlighten the public in order to dissuade amateurs from wasting their time and fortune trying to do the impossible[27]. In this sense the impossibility problem began to obtain a value of its own.

16. CONCLUSION

During the second half of the 17th century the problem of the quadrature of the circle experienced a renaissance. The new analytic methods provided a means to get to grips with the ancient problem. The most celebrated results were the different infinite expressions of π but proofs of impossibility were also put forward. The most widely discussed impossibility questions concerning the quadrature of the circle were the definite and the indefinite algebraic circle quadrature. Impossibility arguments for the indefinite algebraic circle quadrature were put forward by Gregory, Newton and Leibniz. The arguments by the two former were published and hotly debated; Leibniz's argument on the other hand, remained unpublished, but it was rediscovered in the 18th century.

Gregory's proof was a two-step argument. First he argued, that if an algebraic expression having a particular property exists, then any circular sector could be squared by a method similar to Archimedes' method. Second he argued, that such an analytic expression does not exist. His critics led by Huygens maintained that Gregory had not proved the non-existence of an analytic expression with the required property, and further pointed out that even if such an expression does not exist, it only shows that the circle cannot be squared by the particular method suggested by Gregory. Still, Gregory's proof convinced many of his successors. For example it was accepted by Montucla a century later.

Newton's proof was the most widely accepted of the 17th century proofs, and is the only one that is still considered essentially correct, although there is some debate about its completeness. It rests on the observation, that if the indefinite circle quadrature had an algebraic solution, there would exist a polynomial with infinitely many roots. This was known to be false.

[27] See e.g., [Condorcet 1775/78, 65].

Leibniz reduced the impossibility of the indefinite circle quadrature to the problem of the division of an angle. Indeed if the indefinite circle quadrature could be solved algebraically, the division of an angle in n equal parts could be solved by an equation of a fixed degree independent of n. However, Leibniz pointed out that it was known, that solving this question depended on equations whose degree increases beyond all limits when n grows beyond all limits.

The definite circle quadrature was also "proved" to be algebraically impossible by Wallis and Gregory, but their arguments were not accepted by their contemporaries. Wallis showed that if the unit circle had an algebraic area, this would require that many series of fractions containing an increasing number of radicals would approximate an expression with only finitely many radicals. This he found too much to hope for. Gregory's argument was less developed but seems to be based on the argument, that if the indefinite circle quadrature is impossible, then no circular sector can be squared and hence the whole circle cannot be squared. The argument, which was accepted by Montucla,, was rejected by most of his contemporaries. It shows a profound lack of clarity concerning the meaning of the impossibility of the indefinite circle quadrature and its connection to the definite one.

So, while there seems to have been a general conviction, that the new analytic techniques enabled a new approach to impossibility questions regarding the quadrature of the circle, there was less agreement about the way in which the analytic method could be applied. The analytic approach also changed the problem itself. While earlier mathematicians had been concerned with the geometric constructability of the quadrature of the circle, the 17th century impossibility proofs dealt with the analytic expression of the circle area. The connection to the geometric constructability, for example with ruler and compass, was probably believed to have been explained by Descartes, but it was not investigated in detail.

The availability of the new analytic method suggested, that the impossibility of the quadrature of the circle could be proved by mathematical means rather than being dealt with as a meta-question. This change of the nature of the impossibility question was an important advance made by the 17th century mathematicians we have studied. Yet, they all considered the impossibility question as secondary to the constructive problem of solving the circle quadrature. Their proofs of impossibility all served the purpose of showing, that their constructive solutions were the simplest ones, because simpler solutions do not exist. During the following century impossibility statements and proofs began to be considered important in their own right.

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