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# Why was Wantzel overlooked for a century? The changing importance of an impossibility result

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**Abstract**

The duplication of a cube and the trisection of an angle are two of the most famous geometric construction problems formulated in ancient Greece. In 1837 Pierre Wantzel (1814–1848) proved that the problems cannot be constructed by ruler and compass. Today he is credited for this contribution in all general treatises of the history of mathematics. However, his proof was hardly noticed by his contemporaries and during the following century his name was almost completely forgotten. In this paper I shall analyze the reasons for this neglect and argue that it was primarily due to the lack of importance attributed to such impossibility results at the time.


**Resumé**

Terningens fordobling og vinklens tredeling er to af de mest berømte geometriske konstruktionsproblemer formuleret i det antikke Grækenland. I 1837 beviste Pierre Wantzel (1814–1848) at problemerne ikke kan konstrueres med passer og lineal. I vore dage omtales dette bevis i alle generelle matematikhistorier. Men i hans samtid blev Wantzel's bevis overset, og i det efterfølgende århundrede gik hans navn stort set i glemmebogen. I denne artikel skal jeg analysere hvorfor beviset blev overset, og jeg skal argumentere for at det primært skyldes at den slags umulighedsresultater ikke blev tillagt større betydning i begyndelsen af 1800-tallet.


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## 1. Introduction

This paper has a twofold aim. On the one hand, it tells a story of the reception of one particular mathematical result, namely, the proof of the impossibility of constructing the

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duplication of the cube and the trisection of the angle by ruler and compass. On the other hand, it is a contribution to our understanding of the thorough conceptual changes that mathematics underwent during the 19th century.

I shall begin by establishing that Pierre Wantzel's proof of the impossibility of two of the three classical problems remained virtually unknown for a century after its publication in 1837. The neglect of a mathematical result does not in itself pose a historical problem that needs an explanation. After all, the majority of results published in mathematical journals and books are more or less overlooked and never make it into the treatises of the history of mathematics. However, in the case of Wantzel's proof, there are good reasons that we would have expected his paper to have been noticed by his contemporaries and successors. Though the classical construction problems were not at the center stage of mathematical research during the early 19th century, they were certainly well known. The quadrature of the circle was the most celebrated of the problems, but the duplication of the cube and the trisection of the angle enjoyed so much fame that one would have expected Wantzel's resolution of them to have made an impression on the mathematical community. However, that did not happen until a century later when Wantzel began to be generally acknowledged as the first person to have solved the problems.

Thus, the reception of Wantzel's proof raises two historical problems: 1. Why was Wantzel's proof initially overlooked? and 2. Why was it rediscovered and made prominent later on? I shall mainly discuss the first of these questions, but at the end of the paper I shall address the second question as well.

## 2. Neglect and rediscovery of Wantzel's proof

At the occasion of the centenary of the École Polytechnique in 1894, A. de Lapparent wrote a short biography of the Parisian mathematician Pierre Laurent Wantzel (1814–1848). It began with the words "Wantzel, in the eyes of the world, is forgotten" [Lapparent, 1895, 133]. Indeed, if one consults 19th- and early 20th-century treatises on the history of mathematics or mathematical works discussing the classical problems, one will not find any mention of Wantzel or his impossibility proof.

Some histories of mathematics, such as those of Arneth [1852], Cajori [1893], Cantor [1898–1908], and Smith [1923–1925], discussed the classical problems without mentioning their impossibility with ruler and compass. Other treatises, such as that of Hankel [1874], emphasized the impossibility problem, but they did not refer to any proofs of impossibility. In the works after 1880 one can distinguish two different ways of dealing with the history of the impossibility question: Some writers, such as Fink [1890, 161–162], Rudio [1892, iii], Adler [1906, 267], Ball[1893, 38], Sturm [1917, 15–16], and Sanford [1930], referred to Ferdinand Lindemann's proof [Lindemann, 1882] of the transcendence of $\pi$ and the resulting impossibility of the quadrature of the circle, but left the history of the impossibility of the other two classical problems somewhat vague or ascribed them to Gauss [1801, §365]. A late and extreme example of this type of treatment is Coolidge [1940, 53], who ascribed the impossibility proof of all three classical problems to Lindemann. Another strand of works explicitly or implicitly referred to the impossibility proofs contained in Klein's *Vorträge über ausgewählte Fragen der Elementargeometrie* [1895] or to the only reference regarding the matter given in that work, namely Julius Petersen's algebra book [1877]. Works in this tradition include those of Sturm [1895, 134], Enriques [1907, 129 and its second edition 1923, 140], Smith [1906, 65], and Dickson [1911] [see also Dickson, 1914]. Probably Zeuthen [1896, 75] also implicitly referred to his friend and colleague Petersen when he declared that the impossibility of solving the classical problems with ruler and compass had been proven "in recent times." Even the otherwise very well informed and complete *Encyclopädie der mathematischen Wissenschaften* only mentioned Wantzel once, namely in connection with a number-theoretic result. The paper in the *Encyclopädie* by Sommer [1914] dealing with the classical problems did not mention Wantzel at all.

In the 19th-century literature dealing with the classical problems, I have only found one reference to Wantzel's impossibility proof. This was published in the concluding paragraph of Petersen's doctoral thesis of 1871 [Petersen, 1871, 44], in which he presented his own proof for the first time. I do not know how Petersen found out about Wantzel's paper. His own interest in the problem certainly grew out of his earlier interest in geometric problem-solving, which resulted in his famous book *Methods and Theories* [Petersen, 1866, 1879]. Petersen's knowledge of Wantzel is the more surprising because he was infamous for his neglect of the literature [cf. Lützen et al., 1992, 38]. However, Petersen's reference to Wantzel was overlooked by his contemporaries, probably because the doctoral thesis was written in Danish. As mentioned above, Petersen included an improved version of the proof in his algebra book [Petersen, 1877, 161–177], and this proof became known through Klein's reference to the German translation (the book also appeared in a French translation). However, in this book there was no reference to Wantzel, so even a historically informed mathematician such as Klein seems to have remained unaware of Wantzel's paper.

In 1906 Max Simon published an account "on the development of elementary geometry during the 19th century" in which he explicitly gave credit to Wantzel's "rigorous" proof of the impossibility of angle trisection by ruler and compass [Simon, 1906, 82]. Simon's account was intended as a contribution to the *Encyclopädie der mathematischen Wissenschaften*, but since Simon did not conform to the standards of this work (in particular, the references were not precise enough), Klein refused to include it in the *Encyclopädie*. Instead, Klein arranged for Simon's account to be published as a supplementary volume to the yearbook of the Deutsche Mathematiker-Vereinigung. If Simon's reference to Wantzel had been published in the *Encyclopädie* it might have been noticed. Since it was not, it seems to have been overlooked.

Indeed, in an account of Wantzel's life and work published in 1918 by Cajori, he stated, "Quite forgotten are the proofs given by Wantzel of three other theorems of note, viz., the impossibility of trisecting angles, of doubling cubes, and of avoiding the "irreducible case" in the algebraic solution of irreducible cubics. For these theorems Wantzel appears to have been the first to advance rigorous proofs" [Cajori, 1918, 345]. Cajori's paper published in the *Bulletin of the American Mathematical Society* may well have been the primary origin of the general acknowledgment of Wantzel's priority in the 20th century. Unfortunately Cajori did not reveal how he got the idea of writing about this obscure mathematician.

The first encyclopedic work I have found that explicitly refers to Wantzel's proof is the new edition of 1934 by Paul Epstein of Heinrich Weber and Josef Wellstein's *Encyclopädie der Elementarmathematik* [Weber and Wellstein, 1934, 443]. The earlier 1909–1915 edition of that work contained a proof of the impossibility of the two classical problems but no references to earlier proofs.[1] In the 1934 edition Epstein generalized the proof of the previous edition to a proof similar to that of Petersen and referred to both Petersen and Wantzel in a footnote. Three years later Johannes Tropfke, in his history of elementary

[1] See Weber and Wellstein [1909, 1915]. Vahlen [1911] dealt with the matter in a similar way.

mathematics, explicitly attributed the priority of the proof of impossibility of the trisection of an arbitrary angle to Wantzel: "The first rigorous proof that the angle trisection cannot be done with ruler and compass was given by Wantzel [1837]" [Tropfke, 1937, 125]. Thus, it was 100 years before Wantzel's proof found its way into general treatises on the history of mathematics. From this time on it became commonplace to attribute the impossibility theorem to Wantzel. For example, Bell [1940, 77–78] ascribed the proofs of impossibility of the classical problems to Wantzel and Lindemann. Still, Børge Jessen apparently did not know about Wantzel's proof as late as [Jessen, 1943].

My search of the literature is obviously not complete, but still it indicates that Wantzel's proof of the impossibility of the two classical problems remained virtually unknown for a century after its publication. This raises two questions: Why was Wantzel's proof forgotten for so long, and why was it rediscovered? I shall turn to the first of these questions.

The history of mathematics is full of contributions that were later acknowledged for their importance but were overlooked at the time of their publication. In many cases such results were overlooked because they were published in obscure places or because they were controversial at the time of publication. These reasons are often mentioned in connection with the initial neglect of Nikolai Ivanovich Lobachevsky's and János Bolyai's work on non-Euclidean geometry or Bernhard Bolzano's proof of the intermediate value theorem. However, in the case of Wantzel's proof, such reasons cannot explain the neglect.

In fact, Wantzel's proof was published in the second volume (1837) of Liouville's *Journal de Mathématiques pures et appliquées*, which along with Crelle's *Journal* was the leading mathematics journal of the time (indeed the only other specialized mathematics journal was *The Cambridge Mathematical Journal*, founded in 1837). Published in Paris, still considered the mathematical capital of the world, Liouville's *Journal* contained papers by most of the leading French mathematicians and many foreigners as well. So Wantzel's paper was in fact published in a place with a very high impact for the day.

And Wantzel's result was far from controversial. Indeed, at least since Pappus' work from late antiquity, the vast majority of mathematicians had believed that the trisection of an arbitrary angle and the duplication of a cube could not be constructed by ruler and compass.

## 3. Was the proof correct? An account of Wantzel's proof

We should also consider the possibility that Wantzel's contribution did not become famous at the time because the proof was considered to be incorrect or incomplete by his contemporaries. So let us have a closer look at Wantzel's proof in order to uncover if it contains elements that would have been objectionable at the time. The following account or reconstruction of Wantzel's proof keeps as close as possible to Wantzel's own terminology, while at the same time filling in the details that he left out. The amount of "filling in" can be judged from the fact that Wantzel's original argument took up about one-third of the space covered by this reconstruction. Let me list some of the additions and clarifications I have made in the following account: I have added two lemmas that Wantzel used without stating. I have made a notational distinction between the value of the principal unknown of the geometric problem (the line segment we want to construct) and the other roots of the final equation. Wantzel denoted all roots by the same letter. This is probably the most confusing of the notational inconveniences in Wantzel's paper. Moreover, I have explicitly written the arguments of the various rational functions whereas Wantzel left out the arguments. I cannot prove that my reconstruction of Wantzel's proof reflects his own train of thought in all details, but it has the advantage that it is in accordance with Wantzel's published text.

Wantzel's paper [1837], as he stated in the title, addressed the general problem: "Recherches sur les moyens de reconnaître si un problème de géométrie peut se résoudre avec la règle et le compas." His main theorem can be stated as follows:

**Main Theorem.** *The irreducible polynomial (with rationally known coefficients) having a constructible line segment $x_n^0$ as its root must have a degree that is a power of 2.*

His proof can be divided into four parts: 1. First he translated the geometric problem into an algebraic one. 2. Then he showed how this leads to an equation of degree $2^n$ $(n \in \mathbb{N})$. 3. The hard part of the proof "establishes" that this equation is irreducible under certain assumptions. 4. Finally he showed that the duplication of the cube and the trisection of the angle lead to irreducible cubic equations. I shall now give a detailed account of these four parts of the argument.

1. In the first part of the proof he argued that if a problem can be solved by ruler and compass then "the principal unknown of the problem can be obtained by the resolution of a series of quadratic equations whose coefficients are rational functions of the givens of the problem and the roots of the previous equations." This is a very clear formulation of the translation from geometry to algebra. Wantzel's proof is brief and, contrary to modern proofs, it appeals to trigonometry. He did not formulate or prove the simpler converse theorem to the effect that successive solvability by quadratic equations implies constructability with ruler and compass, although some of his later arguments depended on this converse.

2. He then proved that if an algebraic number is a solution of such a system of quadratic equations then it is a root in a polynomial of degree $2^n$ $(n \in \mathbb{N})$. To this end he assumed that the principal unknown of the geometrical problem $x_n^0$ is the root of the last of a series of equations

$$x_1^2 + Ax_1 + B = 0 \tag{1}$$

$$x_2^2 + A_1x_2 + B_1 = 0 \tag{2}$$

$$\dots$$

$$x_m^2 + A_{m-1}x_m + B_{m-1} = 0 \tag{$m$}$$

$$x_{m+1}^2 + A_mx_{m+1} + B_m = 0 \tag{$m+1$}$$

$$\dots$$

$$x_{n-1}^2 + A_{n-2}x_{n-1} + B_{n-2} = 0 \tag{$n-1$}$$

$$x_n^2 + A_{n-1}x_n + B_{n-1} = 0, \tag{$n$}$$

where $A$ and $B$ are rational functions of the given quantities $p, q, r, \dots$, or in modern terminology $A, B \in \mathbb{Q}(p, q, r, \dots)$; $A_1, B_1$ are rational functions of the given quantities and a root $x_1^0$ of the first equation, i.e., $A_1, B_1 \in \mathbb{Q}(p, q, r, \dots, x_1^0)$; and in general $A_m, B_m \in \mathbb{Q}(p, q, r, \dots, x_1^0, x_2^0, \dots, x_{m-1}^0, x_m^0)$.

A rational function of $p, q, r, \dots, x_1^0, x_2^0, \dots, x_{m-1}^0, x_m^0$ can always be written in a very simple form. Indeed one can eliminate all powers of $x_m^0$ higher than the first by using Eq. ($m$). Therefore the function can be written in the form

$$\frac{C_{m-1}x_m^0 + D_{m-1}}{E_{m-1}x_m^0 + F_{m-1}},$$

where $C_{m-1}, D_{m-1}, E_{m-1}, F_{m-1} \in \mathbb{Q}(p, q, r, \ldots, x_1^0, x_2^0, \ldots, x_{m-1}^0)$. Multiplying the numerator and denominator of this fraction by[2] $E_{m-1}x_m^0 + (A_{m-1}E_{m-1} - F_{m-1})$ will reduce it to a *standard form* $A'_{m-1}x_m^0 + B'_{m-1}$, where $A'_{m-1}, B'_{m-1} \in \mathbb{Q}(p, q, r, \ldots, x_1^0, x_2^0, \ldots, x_{m-1}^0)$.

Eq. $(n-1)$ has two roots, $x_{n-1}^0$ and $x'_{n-1}$. Wantzel substituted each of these roots for $x_{n-1}^0$ in the rational expressions of $A_{n-1}$ and $B_{n-1}$ on the lefthand side of the last equation $(n)$ and multiplied the results by each other. In this way he got a fourth-degree polynomial with coefficients in $\mathbb{Q}(p, q, r, \ldots, x_1^0, x_2^0, \ldots, x_{n-2}^0)$. Indeed, the coefficients will be symmetric in $x_{n-1}^0$ and $x'_{n-1}$ and will therefore, by a theorem[3] due to Waring [1770, 9–18] and Lagrange [1770–1771, 371–372] and certainly well known to Wantzel, be rational functions of the coefficients of the last equation but one $(n-1)$; i.e., they will be rational functions of $p, q, r, \ldots, x_1^0, x_2^0, \ldots, x_{n-2}^0$. In the same way, Wantzel replaced $x_{n-2}^0$ in these rational functions by the two roots of the previous equation and multiplied the resulting polynomials by each other to obtain a polynomial of degree 8 with coefficients in $\mathbb{Q}(p, q, r, \ldots, x_1^0, x_2^0, \ldots, x_{n-3}^0)$. Continuing in this way, he ended up with a polynomial $P(x)$ of degree $2^n$ with coefficients in $\mathbb{Q}(p, q, r, \ldots)$. This equation has $x_n^0$ as a root. $\square$

3. Of course any algebraic number is the root of a polynomial of degree a power of 2, so the above argument does not give any new information as stated. But Wantzel argued that *if one has reduced the number of equations in* (1)–($n$) *to a minimum, then the final polynomial* $P(x)$ *resulting from the above procedure is irreducible over* $\mathbb{Q}(p, q, r, \ldots)$. This is the main theorem of Wantzel's paper that I formulated above.

Until this point Wantzel's argument has been rather easy to follow, but his proof of the main theorem is harder. My "filling in" really begins here. In fact the following considerations until Theorem 3 are not found in Wantzel's paper. I shall for brevity leave out the known quantities $p, q, r, \ldots,$ assuming that they are themselves rational numbers. This changes nothing in the argument.

Let us first consider the roots of the polynomial $P(x)$. They can be characterized by a particular choice $x'_1, x'_2, x'_3, \ldots, x'_n$ of roots of the equations (1)–($n$). First one chooses a root $x'_1$ of Eq. (1). This root is substituted for $x_1^0$ in the expression of the coefficients $A_1$ and $B_1$ of Eq. (2). Then one chooses a root $x'_2$ of the resulting Eq. (2) and so on. The choice $x_1^0, x_2^0, \ldots, x_n^0$ leads to the principal unknown of the geometric problem; other choices lead to the other $2^n$ roots of $P(x)$.

With this in mind we can formulate a lemma that is used repeatedly by Wantzel, but not formulated explicitly in his paper:

**Lemma 1.** *Let $x'_1, x'_2, x'_3, \ldots, x'_n$ be a choice of roots of the system* (1)–($n$) *leading to a root $x'_n$ of $P(x)$ and let $f(x_1, x_2, x_3, \ldots, x_m)$ be a rational function of $m$ variables ($m \leqslant n$). As explained above, this function applied to $x'_1, x'_2, x'_3, \ldots, x'_m$ can be written in the standard form*

$$f(x'_1, x'_2, x'_3, \ldots, x'_m) = A'_{m-1}(x'_1, x'_2, x'_3, \ldots, x'_{m-1})x'_m + B'_{m-1}(x'_1, x'_2, x'_3, \ldots, x'_{m-1}).$$

*Let $x''_1, x''_2, x''_3, \ldots, x''_n$ be another choice of roots. Then $f(x''_1, x''_2, x''_3, \ldots, x''_m)$ can be written in the standard form*

$$f(x''_1, x''_2, x''_3, \ldots, x''_m) = A'_{m-1}(x''_1, x''_2, x''_3, \ldots, x''_{m-1})x''_m + B'_{m-1}(x''_1, x''_2, x''_3, \ldots, x''_{m-1}), \tag{3}$$

[2] Wantzel erroneously wrote $-E_{m-1}(A_{m-1} + D_m) + F_{m-1}$.

[3] The theorem states that any rational symmetric function in a number of variables can be expressed as a rational function of the elementary symmetric functions in those variables.

*where $A'_{m-1}$ and $B'_{m-1}$ are the same rational functions as in the standard form of $f(x'_1, x'_2, x'_3, \ldots, x'_m)$ but applied to the new series of roots.*

**Proof.** This lemma is an easy consequence of the way we arrived at the standard form. We made certain reductions of $f(x'_1, x'_2, x'_3, \ldots, x'_m)$ using only that $x'_m$ is a root of (m) in which $x'_1, x'_2, x'_3, \ldots, x'_{m-1}$ are inserted into the rational functions $A'_{m-1}$ and $B'_{m-1}$. Precisely the same reductions applied to $f(x''_1, x''_2, x''_3, \ldots, x''_m)$ will of course lead to the standard form (3). □

Another trivial lemma that Wantzel's proof also uses is the following:

**Lemma 2.** *If one of the solutions of one of the equations (1)–(n) say, the root $x'_m$ of Eq. (m), is a rational function of the roots $x'_1, x'_2, x'_3, \ldots, x'_{m-1}$ of the previous equations, then the other conjugate root of this equation (with the same coefficients) will also be a rational function of $x'_1, x'_2, x'_3, \ldots, x'_{m-1}$.*

**Proof.** The reason is of course that the only difference between the two solutions is the sign in front of $\sqrt{D}$ where $D$ is the discriminant. □

Wantzel's proof of the irreducibility proceeds in two steps. First he proved the following result, which I shall state as a theorem:

**Theorem 3.** *If the number of equations in the system (1)–(n) is reduced to a minimum, "then any of them, say $x^2_{m+1} + A_m x_{m+1} + B_m = 0$, cannot be satisfied by a rational function of the givens and the roots of the previous equations"* [Wantzel, 1837, 367].

This formulation is not entirely clear, but the subsequent proof indicates what I think Wantzel had in mind. Before giving an account of Wantzel's proof, I shall indicate a line of argument that he did not follow: Assume that $x^0_m$ is a rational function of $x^0_1, \ldots, x^0_{m-1}$. Then Eq. (m) can be left out of the system since the coefficients of the subsequent equation will be rational functions of $x^0_1, \ldots, x^0_{m-1}$.

Wantzel's proof is much more complicated for the following reason: When he says that none of the equations $x^2_{m+1} + A_m x_{m+1} + B_m = 0$ has roots that are rational functions of the roots of the previous equations, he means not only Eq. (m), which must be solved on the way to finding the principal unknown $x^0_n$ of the problem (i.e., where $x^0_1, \ldots, x^0_{m-1}$ have been substituted into the expression of $A_m$ and $B_m$), but all of the Eqs. (m), which must be solved on the way to *all* the roots of $P(x)$ (i.e., where any choice $x'_1, x'_2, x'_3, \ldots, x'_{m-1}$ of roots of the previous equations are substituted into the expression of $A_m$ and $B_m$). The above argument shows that if one of these roots $x'_m$ is a rational function of $x'_1, x'_2, x'_3, \ldots, x'_{m-1}$, one can abbreviate the series of equations leading to $x'_n$. But will the abbreviated system of equations still lead to the principal unknown of the system $x^0_n$? This is not clear, and this seems to be the reason for the more complicated proof given by Wantzel.

**Wantzel's proof of Theorem 3.** The proof is indirect. Wantzel assumes that a certain choice of roots $x'_1, x'_2, x'_3, \ldots, x'_m, x'_{m+1}, \ldots, x'_n$ of Eqs. (1)–(n) contains a root $x'_{m+1}$ of $(m+1)$ that is a rational function of the previous roots

$$x'_{m+1} = g(x'_1, x'_2, x'_3, \ldots, x'_m).$$

If this function is substituted into the lefthand side of $(m+1)$, the result will be a rational function of $x'_1, x'_2, x'_3, \ldots, x'_m$ that can be written in the form

$$A'_{m-1}(x'_1, \ldots, x'_{m-1})x'_m + B'_{m-1}(x'_1, \ldots, x'_{m-1}),$$

where $A'_{m-1}$ and $B'_{m-1}$ are rational functions. The equation itself will be reduced to

$$A'_{m-1}(x'_1,\ldots,x'_{m-1})x'_m + B'_{m-1}(x'_1,\ldots,x'_{m-1}) = 0.$$

Now either $A'_{m-1}(x'_1,\ldots,x'_{m-1}) = 0$ or $A'_{m-1}(x'_1,\ldots,x'_{m-1}) \neq 0$. In the latter case $x'_m = -\frac{B'_{m-1}(x'_1,\ldots,x'_{m-1})}{A'_{m-1}(x'_1,\ldots,x'_{m-1})}$ is a rational function of $x'_1,\ldots,x'_{m-1}$.

Inserting this new rational function into Eq. ($m$) and repeating the procedure, one either reaches a $k$ for which $A'_{k-1}(x'_1,\ldots,x'_{k-1}) = 0$ or else the process ends with an equation of the form

$$A'_0 x'_1 + B'_0 = 0,$$

where $A'_0 \neq 0$ and $B'_0$ are rational numbers. In this case the root $x'_1 = -\frac{B'_0}{A'_0}$ is a rational root of the first equation (1) and thus according to Lemma 2 the other root $x''_1$ of that equation is also rational. Thus $x'_1$ and $x''_1$ can be left out of the expressions of all the remaining roots and coefficients, so the first equation can be left out, contrary to our assumption.

On the other hand, if the process leads to a (first) value of $k$ for which $A'_{k-1}(x'_1,\ldots,x'_{k-1}) = 0$, we are in the following situation: the root $x'_{k+1} = -\frac{B'_k(x'_1,\ldots,x'_k)}{A'_k(x'_1,\ldots,x'_k)}$ is a rational function of $x'_1,\ldots,x'_k$ and this function inserted into Eq. ($k+1$),

$$x_{k+1}^2 + A_k x_{k+1} + B_k = 0, \qquad (k+1)$$

will lead to an equation of the form $A'_{k-1}(x'_1,\ldots,x'_{k-1})x'_k + B'_{k-1}(x'_1,\ldots,x'_{k-1}) = 0$, where $A'_{k-1}(x'_1,\ldots,x'_{k-1}) = 0$. This also implies that $B'_{k-1}(x'_1,\ldots,x'_{k-1}) = 0$.

Wantzel then claimed that both $A'_{k-1}$ and $B'_{k-1}$ vanish identically such that $A'_{k-1}(x''_1,\ldots,x''_{k-1}) = B'_{k-1}(x''_1,\ldots,x''_{k-1}) = 0$ for all choices of roots $x''_1,\ldots,x''_{k-1}$. He did not explain why, but one can prove the result by assuming that $m$ is the smallest value of the index for which a root $x'_{m+1}$ is a rational function of the previous roots $x'_1, x'_2, x'_3, \ldots, x'_m$. Indeed, if $A'_{k-1}$ did not vanish identically, the equation $A'_{k-1}(x''_1,\ldots,x''_{k-1}) = 0$ would make it possible to express one of the roots $x''_1, x''_2, \ldots, x''_{k-1}$ as a function of those of a lower index, contrary to the assumption that $m+1$ was the first index for which that could happen.

Wantzel further claimed that all the roots of the Eq. ($k+1$) are rational functions of the roots of the previous equations. For the root conjugate to $x'_{k+1}$, this follows from Lemma 2 above. Now consider Eq. ($k+1$) corresponding to a different choice of roots of the previous equations, $x''_1, x''_2, x''_3, \ldots, x''_k$. Consider $x''_{k+1} = -\frac{B'_k(x''_1,x''_2,x''_3,\ldots,x''_k)}{A'_k(x''_1,x''_2,x''_3,\ldots,x''_k)}$, where we apply the same rational functions $A'_k$ and $B'_k$ as above to the new sequence of roots of the previous equations. If we insert this value of $x''_{k+1}$ into the lefthand side of Eq. ($k+1$) we can, according to Lemma 1, write the result in the standard form $A'_{k-1}(x''_1, x''_2,\ldots,x''_{k-1})x''_k + B'_{k-1}(x''_1, x''_2,\ldots,x''_{k-1}) = 0$, where $A'_{k-1}$ and $B'_{k-1}$ are the same functions as above. But we know from above that $A'_{k-1} = 0$ and $B'_{k-1} = 0$. Thus the lefthand side of ($k+1$) is equal to 0, so that $x''_{k+1} = -\frac{B'_k(x''_1,x''_2,x''_3,\ldots,x''_k)}{A'_k(x''_1,x''_2,x''_3,\ldots,x''_k)}$ is one of the roots of Eq. ($k+1$) for the choice $x''_1, x''_2, x''_3, \ldots, x''_k$ of the previous roots, and it is rational in these roots. According to Lemma 2, the conjugate root of the equation must also be rational in $x''_1, x''_2, x''_3, \ldots, x''_k$.

Thus, all the roots of ($k+1$) are rational in the roots of the previous equations, and therefore the Eq. ($k+1$) can be left out.

To conclude: if one root of one of the equations (1)–($n$) is a rational function of the roots of the previous equations, then one of the equations can be left out and the system is not minimal. This concludes the proof of Theorem 3. □

Next Wantzel proves the following result:

**Theorem 4.** *If the number of equations in the system (1)–($n$) has been reduced to a minimum and if the resulting polynomial $P(x)$ shares a root with a rational function $F(x)$, then all the roots of $P(x)$ are roots of $F(x)$.*

**Wantzel's proof of Theorem 4.** Any root of $P(x)$ is a root $x'_n$ of Eq. ($n$) for a choice of roots $x'_1, x'_2, x'_3, \dots, x'_{n-1}$ of the previous equations. If this root is inserted into $F(x)$, the result $F(x'_n)$ can be written as

$$F(x'_n) = A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})x'_n + B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}), \tag{4}$$

where $A'_{n-1}$ and $B'_{n-1}$ are rational functions. Similarly, $A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})$ can be written in the form

$$A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) = A^1_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2})x'_{n-1} + B^1_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) \tag{5}$$

and $B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})$ can be written in the form

$$B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) = A^2_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2})x'_{n-1} + B^2_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) \tag{6}$$

and so on. If $x''$ is another root of $P(x)$ corresponding to another choice of roots $x''_1, x''_2, x''_3, \dots, x''_{n-1}, x''_n$ of Eqs. (1)–($n$), $F(x'')$ can similarly be expressed by a series of rational functions. According to Lemma 1, the new rational functions will in fact be equal to

$$A'_{n-1}, B'_{n-1}, A^1_{n-2}, B^1_{n-2}, A^2_{n-2}, B^2_{n-2}, \dots$$

obtained from the root $x'_n$, except the functions should be applied to the roots in the sequence $x''_1, x''_2, x''_3, \dots, x''_{n-1}$.

Now assume that $x'_n$ is also a root of $F(x)$, i.e., that $F(x'_n) = 0$. From (4) we conclude that

$$A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})x'_n + B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) = 0.$$

If $A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) \neq 0$ we could find $x'_n$ as a rational function,

$$x_n = -\frac{B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})}{A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1})},$$

of the roots of the previous equations. However, from Theorem 3, we know that this is impossible, so we conclude that $A'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) = 0$ and consequently that $B'_{n-1}(x'_1, x'_2, x'_3, \dots, x'_{n-1}) = 0$. From (5) we conclude in the same way that

$$A^1_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) = 0 \quad \text{and} \quad B^1_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) = 0$$

and from (6) we conclude that

$$A^2_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) = 0 \quad \text{and} \quad B^2_{n-2}(x'_1, x'_2, x'_3, \dots, x'_{n-2}) = 0.$$

Continuing this way we arrive at rational functions $A^i_1$ and $B^i_1$ $(i = 1, 2, 3, \dots, 2^{n-2})$ of one variable such that

$$A^i_1(x'_1) = A^i x'_1 + B^i = 0, \quad (i = 1, 2, 3, \dots, 2^{n-2}) \quad \text{and}$$
$$B^i_1(x'_1) = A^{2^{(n-2)}+i} x'_1 + B^{2^{(n-2)}+i} = 0, \quad (i = 1, 2, 3, \dots, 2^{n-2}).$$

Here $A^i$ and $B^i$ $(i = 1, 2, 3, \ldots, 2^{n-1})$ are rational numbers and, using the above argument a last time, we conclude that they are all equal to zero.

If we make the same reduction of $F(x_n'')$ for another root $x_n''$ of $P(x)$ we will, according to the argument above, end up with the same rational numbers $A^i$ and $B^i$ $(i = 1, 2, \ldots, 2^{n-1})$, and since they do not depend on the choice of the roots of the system of equations, they must also be equal to zero. But then $A_1^i(x_1'') = A^i x_1'' + B^i = 0$ and $B_1^i(x_1'') = A^{2^{(n-2)}+i} x_1'' + B^{2^{(n-2)}+i} = 0$ $(i = 1, 2, \ldots, 2^{n-2})$. Going back up through the rational functions, we end up concluding that $F(x_n'') = 0$.

Thus, if a rational function $F(x)$ has one root $x_n'$ in common with $P(x)$, then all roots of $P(x)$ will also be roots of $F(x)$. □

**Wantzel's "Proof" of the Main Theorem.** In particular, the function $F(x)$ in Theorem 4 could be a polynomial, so Wantzel concluded that a polynomial "equation $F(x) = 0$ cannot admit a root of $[P(x)]$ without admitting them all" [Wantzel, 1837, 369]. This result is correct. But he immediately continued with the formulation of the main theorem: "Thus, the equation $[P(x)]$ is irreducible" [Wantzel, 1837, 369]. □

As pointed out by Hartshorne [2007], this last conclusion is incorrect when we attribute the modern meaning to the concept of irreducibility.[4] And there is no doubt that Wantzel used the word "irreducible" in the same way we do today. Indeed, he wrote, "irreducible, that means that it cannot have roots in common with an equation of lower degree" [Wantzel, 1837, 368].

Wantzel's conclusion regarding the irreducibility of $P(x)$ is obviously correct if the polynomial $P(x)$ has only simple roots. But it is false in general. For example, if $P(x) = [Q(x)]^r$, where $Q(x)$ is irreducible, Theorem 4 holds, but $P(x)$ is reducible.

Wantzel provided no proof that $P(x)$ has only simple roots. He may have overlooked the problem or he may have believed that it followed from the minimality of the system (1)–($n$). However, that is not the case, at least if one interprets the minimality as meaning simply that none of the equations of the system can be left out. This is a reasonable interpretation, since it is the only property of a minimal system used in the proof. One might also interpret the minimality more strongly as saying that there does not exist a shorter system leading to the desired root. In that case Wantzel's conclusion is correct, but his argument does not provide a proof of it.

It is rather easy to complete Wantzel's arguments: Assume that $Q(x)$ is the normed irreducible polynomial having the constructible line segment $x_n^0$ as a root. We want to prove that its degree is a power of 2. According to Theorem 4, all of $P$'s roots are also roots of $Q$. Let $x_1 = x_n^0, x_2, x_3, \ldots, x_k$ denote the *different* roots of $P(x)$. $Q(x)$ has no roots other than $x_1, x_2, x_3, \ldots, x_k$ because otherwise the largest common divisor of $P(x)$ and $Q(x)$ would be a non-trivial divisor of $Q(x)$, which is impossible, since $Q(x)$ was assumed to be irreducible. Since an irreducible polynomial has no multiple roots, we conclude that $Q(x) = (x - x_1)(x - x_2)(x - x_3) \cdots (x - x_k)$.

Thus $Q(x)$ divides $P(x)$, so $P(x) = Q(x)P_1(x)$. If $P_1(x)$ is not a constant, it must have one of the $x_i$'s as a root and so, according to Theorem 4, it has all the $x_i$'s as roots, which means

[4] In Section 28 of [Hartshorne, 2000], Hartshorne gave a modern proof using Galois theory of Wantzel's main theorem and remarked in a note (p. 490) that Wantzel's proof "has a gap." Since 1999 he has given talks about Wantzel's proof and its connection to the work of Descartes, Gauss, Petersen, and later writers.

that it is divisible by $Q(x)$. Continuing in this way, we conclude that $P(x) = C[Q(x)]^r$, where $C$ is a rational constant. From Wantzel's argument we know that the degree of $P(x)$ is a power of 2 and therefore the degree of $Q(x)$ is also a power of 2. $\square$

This completion of Wantzel's proof is due to Petersen [1877, 164] and is spelled out in greater detail by Klein [1895, 19, 20].

4. Having proved the Main Theorem, Wantzel turned to the classical problems. He observed without proof that the equation $x^3 - 2a^3 = 0$, corresponding to the duplication of the cube, is always irreducible. More generally, he addressed the problem of finding two mean proportionals between $a$ and $b$, that is, finding $x$ and $y$ such that

$$\frac{a}{x} = \frac{x}{y} = \frac{y}{b}.$$

This leads to the equation $x^3 - a^2b = 0$, which, according to Wantzel, is irreducible unless $b/a$ is a cube. Since these equations have a degree that is not a power of 2, the problems cannot be solved by ruler and compass except when $b/a$ is a cube.

Similarly, he claimed without proof that the trisection of the angle depends on the equation $x^3 - \frac{3}{4}x + \frac{1}{4}a = 0$. He correctly pointed out that this equation is irreducible[5] when it has no root that is a rational function of $a$. Moreover, he stated that this is the case whenever $a$ is algebraic. This last statement is clearly false, and it is not clear to me what Wantzel had in mind. However, it is true that the equation is irreducible for some values of $a$, for example, the value corresponding to the trisection of the constructible angle of 120°. Thus the trisection cannot be constructed in general by ruler and compass. As Wantzel put it,

> It seems to us that it has not been demonstrated rigorously until now that these problems, so famous among the ancients, are not capable of a solution by the geometric constructions they valued particularly [Wantzel, 1837, 369].

Wantzel finally gave a short and elegant argument for the converse of Gauss's theorem about the constructability of regular polygons (see below):

> The division of the circle into $N$ parts cannot be done by ruler and compass unless the prime factors of $N$ different from 2 are of the form $2^n + 1$ and they enter only in the first power in this number [Wantzel, 1837, 369].

In the last part of the paper, Wantzel tried to investigate how one can decide if a root of an irreducible equation of degree $2^n$ can be constructed by ruler and compass. He described some procedures for deciding the question, but admitted in the concluding paragraph that "These procedures are in general difficult to use" [Wantzel, 1837, 372].

As the above discussion shows, there were in fact some lapses in clarity and some holes in Wantzel's proof, and these shortcomings were of a kind that could have been noticed at the time. In particular, the problems concerning the proof of irreducibility were not entirely trivial. Still, if we use these shortcomings as reasons for the neglect of Wantzel's paper, we must establish that they were pointed out by 19th-century mathematicians. However, there is no evidence that the shortcomings were discovered at all. Wantzel's result was neglected rather than criticized. Indeed, Hartshorne [2007] has been unable to locate anyone who discovered the main hole in the proof before he pointed it out recently.

So although there were problems in Wantzel's proof, they do not seem to be responsible for the neglect of his contribution.

[5] He apparently meant irreducible over $\mathbb{Q}[x]$.

## 4. Was Wantzel a well-known mathematician?

Today, Wantzel is not a household name in the history of mathematics. If at all, he is mentioned in general histories of mathematics for the contribution we discuss in this paper. In specialized works about the history of algebra he may be mentioned in passing for his improvement of Niels Henrik Abel's proof of the unsolvability of the quintic by radicals.

In his own day, he attained some renown in mathematical circles in Paris when, in 1832, he was ranked first in the entrance examination at both the École Polytechnique and the École Normale Supérieure. No student before him had had such success in these prestigious exams. He chose the Polytechnique, and two years later he continued his studies at the École des Ponts et Chaussées. He was still a student at the school when, in 1837, he published the impossibility proof of the two classical problems. The same year he decided that he wanted to devote himself to science rather than engineering and asked for a leave from his studies. However, the head of the school asked him to continue and gave him the opportunity to serve his time by analyzing some German scientific works rather than working in the field. He accepted the offer and graduated as an engineer in 1840. Already two years earlier he had been appointed assistant (répétiteur) in the analysis course at the École Polytechnique, and in 1843 he was promoted to examiner for the school's entrance examination. In 1844 he also became assistant in the course of applied mechanics at the École des Ponts et Chaussées.

To this point, his career followed the path of most famous French mathematicians of the time. However, his research did not measure up to his apparent aptitude for mathematics. His list of publications [Saint-Venant, 1848, 328–331] contains 21 papers and notes, but except for one experimental physics paper of 37 pages on the flow of air, written together with Barré de Saint-Venant [Saint-Venant and Wantzel, 1839], they are all less than 10 pages long, and most are less than 1 page. They had essentially no impact. Saint-Venant, whose obituary [Saint-Venant, 1848] seems to be the source of all later biographical notes on Wantzel, including the present account, wondered why this ingenious young mathematician did not achieve more. Others ascribed it to "the metaphysical form of his mind," but Saint-Venant blamed his bad working habits, his inability to remain with a subject long enough, his great facility with the subjects, and his great workload as a teacher. According to Saint-Venant, Wantzel had a feverish spirit. He did not take care of his health, slept very little, ate irregularly, and misused both coffee and opium.

For these reasons and because of his early death at the age of 33, he was not elected a member of the Académie des Sciences and seems to have remained virtually unknown outside of Parisian circles. The relative obscurity of the author probably contributed to the neglect of his result concerning the classical problems.

## 5. Was the proof considered new?

The reason we now credit Wantzel with the impossibility result is of course that we think he was the first who came up with a proof of it. However, there are indications that, at the beginning of the 19th century, the problem of constructing the two classical problems with ruler and compass was not only considered to be in principle impossible, but the impossibility may even have been considered a well-established and proven fact.

René Descartes had claimed that all constructible problems could ultimately be reduced to the solution of a quadratic equation, and he had pointed out that the two classical problems led to cubic equations [Descartes, 1637, 302, 396].[6] He admitted that this did not give a reason that the two problems were not constructible [Descartes, 1637, 401], but went on to provide a geometric argument. He pointed out that both problems ask for the construction of two points (the two mean proportionals and the two points of trisection). So "inasmuch as the curvature of the circle depends only upon a simple relation between the center and all its points on the circumference, the circle can only be used to determine a single point between two extremes…" [Descartes, 1637, 402]. He thus concluded that the two problems cannot be solved by lines and circles.

About a century later, Jean Étienne Montucla gave an interesting twist to Descartes's arguments, raising the numbers occurring in the argument by one. In an appendix to his *Histoire des recherches sur la quadrature du cercle* [Montucla, 1754], he discussed the other two classical problems and claimed that, with the aid of modern analytic geometry (and only with its aid), one can "prove the impossibility" of the problems with ruler and compass. His proof rested on two principles: 1. An $n$th degree equation has $n$ roots, and 2. A geometric solution of an equation must lead to all its roots and must be performed by the intersection of two curves. Now, since the two classical problems lead to irreducible cubics (irreducibility is claimed but not proved), they cannot be solved by "curves that are only capable of giving less than three points of intersection" [Montucla, 1754, 273–285].

In a subsequent paper I shall return to Descartes's and Montucla's arguments. Here it suffices to point out that Montucla repeated the impossibility claim in his influential *Histoire des mathématiques* of 1758. About the two mean proportionals he declared that "one *proves* today, and the ancients were not ignorant about it, that one cannot solve it with the aid of the ordinary geometry" [Montucla, 1758, vol. 1, 188, my italics]. About the trisection of the angle or the division of the angle in a given ratio he wrote: "It was in vain that one tried to solve one of these problems by plane geometry. Of the same nature as the duplication of the cube, they require the help of a higher kind of geometry or the use of some instruments other than the ruler and the compass" [Montucla, 1758, vol. 1, 193]. He repeated these claims in the second edition [Montucla, 1799, 175, 177–178].

At the end of his book on the history of the quadrature of the circle, Montucla also stated that he had become convinced that James Gregory had proved the impossibility of this third problem by ruler and compass [Montucla, 1754, 293]. However, when he appended a condensed version of this book to the fourth volume of the second edition of his *Histoire des mathématiques,* he admitted that this impossibility proof was generally considered unconvincing, and the publisher of the volume, Joseph-Jérôme Lalande, added in a footnote: "Nevertheless, the author [Montucla] thought that Gregory was right about the quadrature, even the definite one…. But probably he had changed his opinion after 1754" (Lalande in [Montucla, 1802, 633]). Lalande made no similar retraction of Montucla's claim concerning the alleged proof of the impossibility of the other two classical problems.

In the *Histoire des mathématiques*, Montucla's claim that the impossibility of the two classical problems had been proved has the appearance of a common opinion, and its explicit statement in his influential book may have contributed further to the acceptance of this opinion. Thus, the year after Wantzel's paper had appeared, A.-F. Montferrier, in his *Dictionnaire des Sciences Mathématiques Pures et Appliquées* [Montferrier, 1838, vol. 1, 488], declared that all three classical problems were impossible to solve with ruler and compass. He did not refer to Wantzel and seemed simply to have passed on a well-known fact.

[6] For a masterful discussion of Descartes' geometric problem-solving methods see Bos [2001].

Moreover, at the beginning of the 19th century, there was another and better reason to think that the impossibility of the two classical problems was an established fact. In his influential *Disquisitiones Arithmeticae* of 1801, Gauss had shown how to construct all regular $n$-gons for $n$ of the form $2^{\alpha}p_1p_2\ldots p_n$, where the $p_i$'s are different Fermat primes, i.e., primes of the form $2^{2^k}+1$ $(\alpha, k \in \mathbb{N})$ [Gauss, 1801, §365]. Gauss's proof was a striking application of the algebraic and number-theoretic ideas developed in the book. He also claimed that one cannot construct regular $n$-gons with any other number of sides. He included this claim in the *Disquisitiones* in order to save others the trouble of trying to construct the impossible, but he did not include the proof of the impossibility statement, allegedly due to lack of space.

Gauss later became notorious for such claims of priority and in some cases his claims were rejected by influential mathematicians such as Adrien-Marie Legendre. However, as far as I know, his claim to have proved the impossibility of the above-mentioned constructions was not challenged, and from a modern perspective there is little doubt that Gauss knew how to prove it. Indeed, as demonstrated by Wantzel, the proof is much simpler than the constructive proofs found in Gauss's *Disquisitiones* and uses only the techniques found in this book and in Lagrange's paper on equations [Lagrange, 1770–1771].

The impossibility result mentioned by Gauss is of the same kind as the impossibility of the two classical problems, and the same techniques can easily apply to prove the latter. Even more, the impossibility of the trisection of the angle is a *consequence* of Gauss's postulated impossibility result. Indeed, according to the latter, it is impossible to construct a regular 9-gon because 9 is the product of two equal Fermat primes. Thus, it is impossible to construct its central angle of 40° and therefore impossible to trisect the constructible angle of 120°. Thus, for a mathematician who believed Gauss's claim, Wantzel's paper would not contain a new truth; it would rather contain a hitherto unpublished proof of it.

With hindsight we even know that the central part of Wantzel's argument had already been known and proved by Abel several years before Wantzel published his proof. In fact, in an unfinished paper from about 1828, Abel proved the following theorem:

> The degree of the irreducible equation which is satisfied by an algebraic expression is a product of a certain number of the radical exponents that enter into the algebraic expression [Abel, 1839, 200, 232].

In the special case where all the radicals are square roots, this theorem states that the irreducible equation of an irrational number that is expressed by square roots is a power of 2. As we have seen, this was the central, difficult and not entirely satisfactory part of Wantzel's proof. However, Abel did not apply the theorem to the problem of geometric constructions, and the fragment containing the theorem was not published until 1839, so it was certainly unknown to Wantzel. Moreover, I have not found any public reference to Abel's priority in connection with the impossibility of the two classical construction problems. Ludvig Sylow called Petersen's attention to Abel's priority in a letter written in 1870 [Lützen, 1992, 447], but this claim seems to have remained unknown to the public.

## 6. Was the proof considered too trivial?

But even if Wantzel's proof was considered a novelty, is it possible that it was considered too simple to constitute a noteworthy breakthrough? Indeed, Wantzel's proof is a relatively straightforward use of the methods found in Gauss's *Disquisitiones* and of the more recent methods used by Abel in his proof of the impossibility of the solution of the quintic by radicals. For example, the algebraic characterization of the problems constructible with ruler and compass were lifted directly from Gauss, and the discussion of irreducibility followed ideas in Abel's work. However, these novel algebraic techniques were probably not so well assimilated in Wantzel's day that his readers would consider his proof as trivial. And later in the century, when such techniques were more well known, the proof of impossibility of the classical construction problems was certainly considered as interesting. Thus we can conclude that it was probably not the relative simplicity of Wantzel's proof that made it an overlooked contribution.

## 7. Was the result considered important? The changing paradigms

While some of the above reasons may have contributed to the lack of appreciation of Wantzel's proof, they are not in themselves serious enough to explain the neglect. Instead, I shall argue that Wantzel's result was not considered particularly important at the time and that this was the main reason that it was overlooked. This may at first seem strange. To us Wantzel provided a proof of a famous 2000-year-old problem, a contribution that should have made a splash among his contemporaries. Indeed, when Lindemann, half a century later, proved the transcendence of $\pi$, his result was immediately hailed as the final settlement of the problem of the quadrature of the circle.

However, I will argue that most of Wantzel's contemporaries would not have viewed his proof in this light. Indeed, during the 18th century, mathematics was a constructive enterprise consisting of finding solutions to problems, and this view of mathematics was still shared by most of Wantzel's contemporaries. The classical problems fit nicely into this paradigm, but Wantzel's proof does not provide a solution. A solution would have been a construction, but all Wantzel showed was that a particular type of solution does not exist. This type of result is not really a mathematical result in the constructive paradigm, but a metaresult saying that there is no reason to continue to look for a solution because there is none. If a result is not a mathematical theorem proper, it is not clear that one should ask for a proof. To be sure, impossibility results had been formulated and proved earlier. For example, Pierre de Fermat formulated several theorems of this type in number theory, but as pointed out by Catherine Goldstine [1995, 134–135], they were not well received by his contemporaries.

Ever since Pappus it had generally been believed that the two classical problems were not constructible by ruler and compass, and many mathematicians including Pappus had believed that this was due to the nature of the problems. However, until Descartes, no one had suggested that this was a theorem that could or even should be proved. And even after Descartes the problem seems to have attracted little attention. A similar tendency can be seen in the investigation of the solution of the quintic by radicals. Lagrange [1770–1771, 355, 357, 403] explicitly indicated that this problem might be unsolvable, but while he expressed the hope that his methods would be of help for later attempts at finding a solution, he did not indicate that they could be used in a proof of the impossibility. And when this was successfully attempted by Paolo Ruffini (1799 and later) [Ruffini, 1915], his result was hardly noticed.[7]

Gauss's way of dealing with the impossibility result concerning regular polygons also suggests that he did not attach much importance to it, or at least that he thought that

[7] See Ayoub [1980]. As pointed out by Ayoub, Ruffini was only rediscovered toward the end of the 19th century by Burkhardt [1892].

his contemporaries would not value it highly. His excuse for not including the proof was that the book was already long enough. Of course, if he had thought that the result was as important as the positive construction of the possible regular $n$-gons (as we tend to think to day), he would have been able to add the necessary 10–20 pages to the book. He did not even bother to formulate the impossibility result explicitly. He merely stated that for the values of $n$ that are not of the Gaussian form, he could show that one could not avoid equations of degree higher than the second [Gauss, 1801, §365]. He let his reader infer the result that one cannot construct regular $n$-gons by ruler and compass unless $n$ is of the Gaussian form. His stated reasons for including the claim at all was that it would prevent people from wasting their time trying to construct the impossible.

These events are in agreement with the constructive paradigm that considers the only real solution to a problem to be a construction of a solution and considers an impossibility result as a metastatement.

In the history of the quintic, Abel explicitly tried to change this view of impossibility results. As emphasized by Sørensen [2004, 174–175], Abel suggested that one ought to rephrase the problem as a problem that always has an answer. Instead of asking for a solution by radicals, one should first ask if the quintic has a solution of this kind, and only if the answer turns out to be yes would one proceed to the question of finding such a solution constructively [Abel, 1839, 217]. By formulating the existence question as a mathematical problem, Abel had allowed the impossibility result to be a solution of a problem, not just a statement about the impossibility of finding an answer to a problem. Thus impossibility statements became proper theorems in mathematics.

Several younger mathematicians continued along the same line as Abel. Evariste Galois is the most obvious example, but also Joseph Liouville's investigations of integrability of functions and differential equations in finite form follow this lead. Indeed, Liouville was the first to show that certain integrals cannot be expressed in finite terms [Lützen, 1990, Chapter IX]. Wantzel's result fits this trend. In fact Wantzel seems to have been particularly fond of impossibility results. He published a simplified proof of Abel's theorem concerning the impossibility of solving the quintic by radicals [Wantzel, 1845a]. According to Cajori [1918, 339], this proof became quite well known because it was reproduced by Joseph Serret in his widely read *Algèbre Supérieure* ([Serret, 1849, 292–296] and later editions). As mentioned in the quote above from Cajori's paper, Wantzel also showed that it is impossible to avoid the irreducible case in the solution of cubic equations. More precisely, he proved the following theorem: "It is impossible to express the roots of a cubic equation by real radicals when the roots are all real" [Wantzel, 1843, 127]. Moreover, Wantzel published a paper about the "impossibility of expressing the roots of an algebraic equation by transcendental functions" [Wantzel, 1845b] as well as a note about Fermat's last theorem [Wantzel, 1847].

These new tendencies relative to impossibility results went hand in hand with a new focus on existence theorems, and can be viewed as two sides of the same coin. In particular, Cauchy insisted that before finding the sum of an infinite series or the solution of a differential equation, one should prove their existence. And both trends were parts of a change from a constructive to a conceptual paradigm in mathematics. Another instance of this change was the emergence in the 1830s of Sturm–Liouville theory where the (qualitative) properties of solutions to certain differential equations were investigated without solving the equation in analytic form [Lützen, 1990, Chapter X].

However, in the 1830s and 1840s, these new ideas were mostly a young man's game. The majority of the mathematicians of the older generation seem to have continued in the more constructive paradigm during the first half of the 19th century. And although some of the results, such as Abel's impossibility result, received some attention, other of the theories were mostly overlooked at the time. For example, it took until about 1870 before Sturm–Liouville theory was developed further.

Wantzel's proof may have been overlooked by his contemporaries because it was not a proof of an important theorem in the still prevailing constructive paradigm. It did not construct a solution of the two classical problems, but only stated that a solution was out of reach. In this sense, it was not a mathematical theorem but a metamathematical result. This at least suggests a reason that the settlement of the two classical problems remained anonymous until the 20th century.

## 8. The rediscovery of the impossibility proofs

Towards the end of the 19th century impossibility results became an integrated part of mathematics. In his famous talk on mathematical problems in 1900, David Hilbert stated: "In recent mathematics (der neueren Mathematik) the question as to the impossibility of certain solutions plays a preeminent role" [Hilbert, 1901, 297]. In particular, Hilbert emphasized that the problems of the proof of the parallel axiom, the squaring of the circle, and the solution of the quintic by radicals "have finally found fully satisfactory and rigorous solutions, although in another sense than that originally intended" [Hilbert, 1901, p. 297 of collected works]. When Hilbert spoke of a new "sense" of the concept of solution, he clearly referred to a reformulation of the problems in the vein suggested by Abel, where instead of asking "find the solution," one asks "does a solution exist?" It is this reformulation that allowed Hilbert to declare that in mathematics all problems can be solved: there is no ignorabimus. In the later editions of the *Grundlagen der Geometrie* [e.g., Hilbert, 1922, 111], Hilbert repeated his remarks about the importance of impossibility results in modern mathematics. For Hilbert such results were important because they show how far one can get with a prescribed method or within a given axiomatic system.

The quotes reveal that Hilbert was aware that the importance he attributed to impossibility results was of a recent date. Indeed as the previous account of the literature on the classical problems reveals, the next persons after Wantzel to publish a proof of the impossibility of the two classical problems may have been Petersen [1871, 77] followed by Klein [1895]. To be sure, Saint-Venant, in his biography of Wantzel, mentions that "Since then (i.e., since 1837), M. Sturm has simplified this type of demonstration, but he has published nothing on it" [Saint-Venant, 1848, 329]. This simplification is also mentioned in the biography of Charles François Sturm in *The MacTutor History of Mathematics Archive* [O'Connor and Robertson, 2008], but nothing seems to be known about it. It seems to have vanished from view, together with Wantzel's published contribution. Indeed, at the end of the 19th century, when the impossibility proofs began to take their place among the important theorems of mathematics, Wantzel was so completely forgotten that even a historically interested mathematician such as Klein did not know of his name, or at least was ignorant of its connection with the classical problems.

After 1880 there was an increased interest in the constructability of the two classical problems in the mathematical and historical literature. As I pointed out above, the priority question was at first dealt with in a somewhat vague way, but it is clear that the new prominence of the problems raised the question. Therefore it is not surprising that Wantzel's name eventually resurfaced. As I indicated, we may be indebted to Cajori [1918] for the rediscovery and dissemination of Wantzel's proof.

## 9. Conclusions

Wantzel's proof of the impossibility of constructing the trisection of the angle and the duplication of the cube by ruler and compass was forgotten for almost a century after its publication in 1837. The obscurity of the author, and the fact that some of his contemporaries considered the result to be known, or even demonstrated, may have contributed to this neglect, but of even greater importance was that the result was not considered an important mathematical result at the time it was published. In the constructive and quantitative paradigm, which still dominated large parts of mathematics during the first half of the 19th century, an impossibility result such as the one proved by Wantzel was not considered an important result *within* mathematics but rather a meta-result *about* mathematics, in this case about the impotence of a particular method of construction.[8] In Gauss's words, such results were of interest only in so far as they would prevent mathematicians from wasting their time trying to do impossible mathematics.

Together with Abel's proof of the impossibility of the solution of the quintic by radicals, Augustin-Louis Cauchy's work on analysis, and other works on qualitative and conceptual questions, Wantzel's result heralded a new qualitative and conceptual paradigm in mathematics. However, when this paradigm prevailed towards the end of the 19th century, Wantzel's name had been so completely forgotten that even when his impossibility results were re-proved and obtained their place at the center of mathematics, it took several decades before Wantzel's name was attached to them.

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[8] My distinction between mathematics and metamathematics corresponds to Leo Corry's distinction between the body and the image of mathematics [Corry, 1996, 3–5].

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