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D'Alembert's proof of the fundamental theorem of algebra

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Abstract

D'Alembert's proof of the fundamental theorem of algebra (FTA), the first published, is still widely misunderstood. Typical of d'Alembert, his work is bold and imaginative but in need of significant repair. The proof is examined in detail, in both the 1746 and 1754 versions, along with commentary over 250 years and recent efforts to revive d'Alembert's reputation. A particular challenge is to work with algebraic equations while avoiding dependence on the FTA itself. A repaired version is offered.

Résumé

La démonstration de d'Alembert du théorème fondamental de l'algèbre (TFA)—la première publiée de ce théorème—est encore largement mal comprise. Typique de d'Alembert, ce travail est plein d'audace et d'imagination, mais il a besoin d'être substantiellement rectifié. On examine en détail cette preuve, dans les deux versions de 1746 et 1754, et l'on commente sa réception depuis 250 ans, y compris les efforts récents pour rétablir la réputation de d'Alembert. Un défi tout particulier résulte de la nécessité de travailler sur les équations algébriques tout en évitant l'utilisation du TFA. Une version rectifiée de la preuve de d'Alembert est donnée.

MSC: 01A50; 12-03; 14H05

Keywords: Fundamental theorem of algebra; D'Alembert; Algebraic equations; Algebraic functions

1. Introduction

The first published proof of the fundamental theorem of algebra (FTA) was by Jean le Rond d'Alembert (1717–1783), in an article "Recherches sur le calcul intégral" [D'Alembert, 1746], sent to Berlin in December 1746 for inclusion in *Memoires de l'Académie Royale, Berlin*, for 1746, and which appeared in 1748. It was based on algebraic equations. The FTA is the claim that every real polynomial has real or complex roots.

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As it turned out, Leonhard Euler (1707–1783) had himself read a proof of the FTA to the Berlin Academy in November 1746 [Euler, 1980, 254]. Euler's proof was algebraic, centered on an argument that if a real polynomial of degree 2^k, $k > 1$, was written as the product of two polynomials of equal degree, then the coefficients of the proposed factors could be found as real roots of a system of equations. His proof was published in the *Memoire* for 1749 [Euler, 1749], actually issued in 1751.

Over the next decades, other algebraic proofs of the FTA were given by Daviet de Foncenex [1759], by J.L. Lagrange [1772], and in 1795 by P.-S. Laplace [1812]. C.F. Gauss offered a proof [Gauss, 1799] based in part on "geometric considerations" [Gauss, 1816, 33], and a second proof, algebraic, of the FTA in 1815 [Gauss, 1816]. An important analytic proof was given by J.-R. Argand [1806], with an improved version in Argand [1814/1815].

All these early proofs—the list could be expanded—relied on claims which lacked adequate justification according to modern standards. All the algebraic proofs mentioned assumed a real root for every real polynomial of odd degree, and Argand's proof assumed that a continuous function achieves a minimum on a closed disk; late 19th-century development of the real numbers and continuity provided justification. All the algebraic proofs through that of Laplace assumed the existence, in some form, of roots of a given polynomial; this too would be justified, principally in the work of Kronecker and Dedekind in the late 19th century [Kiernan, 1971]. The proofs of Euler and Foncenex and Lagrange required Lagrange's theorem on similar functions, stated and proved in Articles 100 to 104 of Lagrange [1770/1771]. (See van der Waerden [1985, 81].) And justification for Gauss's first proof was finally provided in Ostrowski [1920]. The simplest of these proofs, those of Laplace and of Argand, are still presented, as in Samuel [1967] and Fefferman [1967], respectively.

D'Alembert's proof is different. First, there is no broadly accepted understanding of his proof. The reader may consult [Boyer, 1968, 491; Dieudonné and Guérindon, 1978, 68–69; Stillwell, 1989, 196–198] for three very different descriptions of d'Alembert's proof. (I recommend [Gigli, 1925, 189–192; Bottazzini, 1986, 15–16; Gilain, 1991, 113–115] for accuracy.) Second, unlike all the proofs mentioned above, d'Alembert's has not engendered a line of repaired and improved proofs of the FTA. Dieudonné and Guérindon, among others, have said that the major deficiency of d'Alembert's proof can be remedied by "an elementary argument of compactness." The story is not that simple; care is needed, especially with algebraic equations, to avoid dependence on the FTA on the way to its proof. This paper is an attempt to view the history of d'Alembert's proof in that spirit. Introductory material makes up Sections 2 and 3; Section 4 is a detailed summary of d'Alembert's proof; Sections 5 and 6 discuss commentary on d'Alembert's proof over 250 years and repair of the proof.

2. Imaginary roots in the 18th century

Before we look at d'Alembert's proof and its interpretation, it is helpful to consider the 18th-century understanding of "imaginary," used as early as Descartes' *La Géométrie*, 1637. The solution of third and fourth degree equations by square and cube roots had been known since the publication of Cardano's *Ars Magna* in 1545. It was not generally accepted, however, that the nonreal roots produced this way were complex, and those roots were simply called "imaginary," as were the nonreal roots of all polynomials. Thus, for example, Lagrange [1772] carried the title "Sur la forme des racines imaginaires des équations"; the work was a proof that the "imaginary" roots were actually complex. A second meaning is encountered in 18th-century work: expressions of the form $p + q\sqrt{-1}$ are called "imaginary" when p and q are real. D'Alembert's proof of 1746 was followed by a note, disputable, which paired the two meanings of "imaginary": "Il est à remarquer que dans les demonstrations precedentes, on n'a point supposé que la racine imaginaire du multinome, eût ou pût avoir une expression imaginaire, avant de la reduire à $p+q\sqrt{-1}$" [D'Alembert, 1746, Remark 1, Art. X]. Gauss used "imaginary" in [1799] to denote complex numbers; he later coined the name "complex."

I will often use "complex" in place of the clumsy "of the form $p+q\sqrt{-1}$." I also use "imaginary," in quotation marks, in the 18th-century sense of "not real and of indeterminate form."

3. Euler and d'Alembert, 1746

In hindsight, the near simultaneous appearance of two proofs of the FTA is only a small surprise. Already in the early years of the calculus, the method of partial fraction expansion posed the question of whether every real polynomial is the product of real factors of degree one and two. In 1702, in papers in the *Acta Eruditorum*, Johann Bernoulli answered in the affirmative while Leibniz took the negative position, claiming that $x^4 + a^4$ could not be written as the product of two real factors [Kline, 1972, 411]. Over the next four decades, growing experience with complex numbers led the majority of mathematicians to side with Bernoulli, but often without a clear grasp of the issue. More than a few simply believed that a process of root extraction such as produced the zeros of the fourth degree polynomial would be found for higher degrees. Thus the closure of the complex numbers under root extraction then guaranteed the FTA. D'Alembert seems to have believed this before 1745, and Euler at least expected it when he wrote [Euler, 1749]; this notion seems to be operating in the Abbé de Gua's work of 1741, where he even answered Leibniz by setting $\sqrt[4]{-a} = m + n\sqrt{-1}$ and solving for (real) m and n [de Gua de Malves, 1741, 480]. (See Gilain [1991].)

With Euler we have finally the clear enunciation and claim for the FTA, the earliest known in a letter to Johann Bernoulli in 1739 [Fauvel and Gray, 1987, 447] and the first published in Euler [1743]. The FTA guaranteed the real factorization needed in Euler's solution of linear differential equations with constant coefficients. It was time for a proof. Euler wrote to Clairaut on 14 August 1742 that the FTA is "indubitable, quoique je ne le puisse démontrer parfaitement" [Euler, 1980, 137]. Through Euler's correspondence with Nicolas Bernoulli over late 1742 and 1743, Bernoulli become convinced of the FTA and an outline of Euler's proof emerged, including ideas in a letter from Bernoulli of 29 November 1743 [Euler, 1998, 596–599]. Again, the issue began with just a fourth degree polynomial.

D'Alembert faced the FTA in his work on integration. This was the subject of his first remarks to the French Royal Academy of Sciences, noted in *Histoire de l'Académie Royale* 1739, of his two earliest mathematical papers, unpublished but registered in 1741 [Hankins, 1970, 239], and the subject of the 1746 paper which included his proof of the FTA. In this last paper he explicitly referred to Bernoulli's 1702 article. A preliminary version of the 1746 "Recherches sur le calcul intégral" was announced before the Royal Academy in December 1744 and read from 6 March to 7 April 1745; an initial fragment survives and has been published [Gilain, 1991, 133–136]. In the fragment, d'Alembert showed that algebraic operations on complex numbers, including taking rational and complex powers, produced complex numbers. Then, in Article 9, the FTA is stated, that every real equation with imaginary roots can be factored into real trinomials, but justified only by the just proven closure of numbers of the form $p+q\sqrt{-1}$. Thus, d'Alembert had little more than a year to comprehend what the FTA entails and to produce a direct proof.

D'Alembert was emerging in 1746 as a leading mathematician and physicist. After treatises on dynamics and hydraulics, in 1746 he won the prize of the Berlin Academy for his work *Réflexions sur la cause générale des vents* [D'Alembert, 1747]. Although naive on the actual causes of wind, this publication included groundbreaking work in partial differential equations [Demidov, 1982], along with demonstrations of the closure of the complex numbers under algebraic operations [Art. 79]. Euler acknowledged him as a master of calculation. Euler wrote d'Alembert (29 December 1746) that his reduction of integrals to the rectification of the ellipse and hyperbola is "comme un chef d'œuvre de votre penetration," and (15 April 1747) "votre superiorité dans les calculs les plus difficiles y eclate partout" [Euler, 1980, 252, 266]. Around that time, d'Alembert submitted to the Berlin Academy his important work on the vibrating string, and he was then beginning works on the movements of the planets and of the moon. His work in celestial mechanics resulted in astonishing success when in May 1749, ahead of Euler and Clairaut, he presented to the Paris Academy an explanation of the nutation of the earth's axis [Euler, 1980, 24].

D'Alembert's limitations are often mentioned [Cantor, 1901, 585; Hankins, 1970, 63–64; Truesdell, 1954, *lviii*]. His work was careless and rushed, not well thought through; "his continual switching from one object to another did not permit him to give all the development and simplicity necessary for making the abstract matters that he treated understandable to the greatest number of readers" [d'Alembert's friend, the Abbé Bossut, quoted in Hankins [1970, 63]]. Nevertheless, he was an intense and imaginative discoverer, and in 1746, in command of the fields of mathematics and physics, he had entered his short period of great mathematical production.

4. D'Alembert's proof

There are two complete versions of d'Alembert's proof, together with two theorems from an unpublished manuscript of 15 June 1752 [Euler, 1980, 344]. The first version, mentioned above, is from 1746. The second is found in the long *Introduction* of the *Traité de calcul intégral*, by Louis-Antoine de Bougainville, of 1754. In the preface, Bougainville indicated at the end of his list of sources: "finally, several memoires of M. d'Alembert, not published and which he has kindly wished to communicate to me." He went on to declare that "nothing in this work is mine, if not for the order which I have attempted to place in the various methods, and the form that I give them" [de Bougainville, 1754]. After the proof of the FTA, he wrote that it is taken from the 1746 work of d'Alembert: "I have extended the demonstrations, and put them in a form that I believe is most appropriate to place them within the reach of everyone" [Art. LXXXIV]. The clearest evidence that the 1754 version represented d'Alembert's thinking lies in the manuscript of 1752, apparently sent by d'Alembert to Berlin for publication. The crucial Article LXXIX of 1754 appears virtually word for word as Theorem 1 of the manuscript,[1] very unlikely unless, as noted in the *Preface*, Bougainville saw this or a related unpublished manuscript by

[1] Observation III, of *Observations sur quelques mémoires, imprimés dans le volume de l'académie* 1749, *par M. d'Alembert*, dated 1752: 1er Theorême. Soit a la valeur de l'abscisse x dans une courbe geometrique, lorsque 1'ordonnée y passe du réel à l'imaginaire, je dis qu'on peut supposer à l'abscisse x une valeur $a+b$, telle que l'ordonnée correspondante soit $A+B\sqrt{-1}$, b etant une quantité qui peut etre très petite, mais toujours finie [Euler, 1980, 344].

Article LXXIX Lemme 2, of *Traité de Calcul Intégral, Introduction*, of L.-A. de Bougainville: Soit a la valeur de l'abscisse dans une courbe géométrique ; lorsque l'ordonnée passe du réel à l'imaginaire, on pourra toujours supposer à l'abscisse une d'Alembert. Finally, d'Alembert himself referred to Bougainville's work as containing his argument of 1746, as in the *Encyclopédie* article "Equations" of 1755 [D'Alembert, 1755].

(Note on notation: The two variables of the algebraic equation differ throughout the work of d'Alembert, Gauss, and others. I have chosen to leave the variables as in the original works; however, with the modern $F(v, w)$ to denote the algebraic expression, the first input is always the variable of the polynomial and the second is the undetermined constant, preceded either by $+$ or $-$.)

After his 1746 work, d'Alembert restricted his argument to algebraic functions. In proving the FTA, the problem is to show that the equation

$$F(y,z) = y^m + Ay^{m-1} + By^{m-2} + \cdots + z = 0 \quad \text{(no constant, coefficients real)}$$

has, for every real z, a solution $y(z)$, where y is complex. D'Alembert's plan began with the observations that $y = 0$ is a solution when $z = 0$ and that solutions $y(z)$ vary continuously with z. He argued that we can continue a real or complex solution $y_1(z)$ from $y_1(0) = 0$ as z ranges along the entire real axis.

D'Alembert's proof comprises two major claims, which I present as *Theorem A* (Local) and *Theorem B* (Global).

Theorem A (Local). *If y and z are related by the equation $F(y,z) = 0$, and $y = 0$, $z = 0$ is a solution, then there is a complex solution y for all real z sufficiently small. There is a corresponding result at any real y and z satisfying $F(y,z) = 0$.* (*In the* 1754 *version, F must be a polynomial. The* 1746 *version allowed for $y = \infty$, $z = 0$, but did not develop this case. The same is suggested in Article* LXXVI *of the* 1754 *version.*)

The proof is based on the series expansion of y in z, in which it is assumed that the coefficients are real. D'Alembert argued that for z infinitely small and real, the series produces y that is real or complex.

In D'Alembert [1746] the argument is found in Articles II, III, and IV. In the last, the conclusion was simply extended to small and finite z, a jump which Gauss later criticized.

Art. II. Propos. I.

> *Let TM be a curve so $y = 0$ or ∞ when $z = 0$.* [T = origin] *If one takes z positive or negative, but infinitely small, the value of y in z can always be expressed by a real quantity when z is positive: and, when z is negative, by a real quantity or a quantity $p + q\sqrt{-1}$, where p and q are both real.*[2]

Proof. For z infinitely small, $y = az^{m/n} + bz^{r/s} + cz^{t/u}$, etc., a "serie trés convergente," where the exponents increase.

(1°) If all terms remain positive in making z negative, then y can be expressed as $az^{m/n}$ since "all the other terms are null in ratio to the first."

valeur $a + b$, telle que l'ordonnée correspondante soit $A + B\sqrt{-1}$; b etant une quantité qui peut etre très-petite, mais toujours finie [de Bougainville, 1754].

[2] II. *Propos. I. Soit TM une courbe quelconque dont les coordonnées TP = Z, PM = y,* & *dans laquelle $y = 0$ ou ∞ lorsque $z = 0$. Si on prend z positive ou negative, mais infiniment petite, la valeur de y en z pourra toujours être exprimée par une quantité réelle, lorsque z sera positive* : &, *lorsque z sera negative, par une quantité réelle, ou par une quantité $p + q\sqrt{-1}$, dans laquelle p&q seront l'un* & *l'autre réels* [D'Alembert, 1746].

(2°) If $z^{m/n}$ becomes "imaginary" in making z negative, which occurs if n is even and m odd, then it can always be reduced to the form $p + q\sqrt{-1}$.

"... it is clear that as $-z$ is infinitely small, then one can not only neglect all real terms except one, but also all imaginary terms except one." □

Art. III. Cor. I. If we consider another point on the curve, with real coordinates *AC*, *CT*, an abscissa *AQ* greater than *AC* by infinitely little has an imaginary ordinate only of the form $p + q\sqrt{-1}$.

Proof. Transpose axes and apply Art. II. □

Art. IV. Cor. II.

> Now if *AC* is augmented by a finite quantity *CQ* up to a certain size, then the ordinate can be assumed to be $p + q\sqrt{-1}$. [Proof:] For if there is no finite value *CQ* such that the corresponding ordinate can be expressed $p + q\sqrt{-1}$ then this ordinate could not be expressed by $p + q\sqrt{-1}$ for *CQ* infinitely small. ...[3]

The 1754 proof included a similar argument in Articles LXXVI and LXXIX. This time, in Article LXXVI Lemma 1, a specific algorithm, "the parallelogram of M. Newton, or on the Analytic Triangle of M. l'Abbé de Gua, in the manner taught by M. Cramer (*Analyse* Chapt. III p. 54)" [Cramer, 1750] was cited to derive from equation $z^m + bz^{m-1}u + \cdots + Kz + gu + F = 0$ the series (assume $F = 0$)

$$z = Du^k + Cu^{k+p} + \cdots.$$

The argument then continued as in 1746, but without the infinitely small: when u is negative and "tres-petite, quoique finie," the individual terms of the series have form $A + B\sqrt{-1}$, as do finite sums of those terms, and A and B decrease since they are "of the same number of dimensions as Du^k," so the series "is the true value of z" and is of the form $A + B\sqrt{-1}$. [Article LXXVI.]

For Article LXXIX Lemma 2, we imagine the real abscissa increasing to a certain value a at which the ordinate "passe du réel à l'imaginaire"—we must understand "imaginaire" to mean "of indeterminate form." Then, by Article LXXVI, "one can always suppose for the abscissa a value $a + b$ such that the ordinate will be $A + B\sqrt{-1}$, b a quantity which can be very small [trés petite], but always finite."

Despite his denial in Art. X, d'Alembert did seem to assume, as Gauss accused, that every real u has a corresponding root z, real or "imaginary," which he then argued must be real or complex.

Theorem B (Global). *Let $F(x, y) = 0$ be the* (*algebraic*) *equation of a curve $x(y)$ in the complex plane, where y ranges along a segment of the real axis. Then ordinate $x(y)$ can be continued* taking on only real and complex values *as abscissa y ranges along the* entire real axis. (*The FTA follows immediately.*)

In the 1746 proof, the argument is found in Articles V and VI.

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[3] IV. Cor. II. Donc si on augmente l'abscisse *AC* d'une quantité finie *CQ*, au moins jusqu'à un certain terme, l'ordonnée correspondante pourra etre supposée $= p + q\sqrt{-1}$. Car s'il n'y avoit aucune valeur finie de *CQ*, telle que $p + p\sqrt{-1}$ [sic] pût exprimer l'ordonnée correspondante, cette ordonnée ne pourroit pas non plus etre exprimée par $p + q\sqrt{-1}$, *CQ* etant infiniment petite... [D'Alembert, 1746].

Art. V. Cor. III. For any finite *CQ* that augments abscissa *AC* the "imaginary" ordinate that corresponds is of the form $p + q\sqrt{-1}$.

Proof. For if not, there is a greatest value of *CQ*, namely α, whose corresponding ordinate has the form $p + q\sqrt{-1}$. Now apply Articles II, III, and IV to p and q as separate functions. In adding to α an infinitely small quantity then the value of p can be supposed $t + i\sqrt{-1}$, and the value of $q = \beta + \delta\sqrt{-1}$. "So in augmenting α by an infinitely small quantity and, consequently (art. 4) by a finite quantity, the corresponding ordinate can be supposed $t + i\sqrt{-1} + (\beta + \delta\sqrt{-1})\sqrt{-1} = \cdots e + f\sqrt{-1}$ where e and f are real." And this contradicts the hypothesis. □

Art. VI. Propos. II.

> *Let $x^m + ax^{m-1} + bx^{m-2} + \cdots + fx + g$ be a polynomial which vanishes for no real number in place of x; then I say that there is always a quantity $p + q\sqrt{-1}$ which makes the polynomial equal to zero.*[4]

Proof. (1°) The last term g [assume it gives an "imaginary" solution] can be changed so that there is a real solution: if we take any real h and then take $h^m + ah^{m-1} + \cdots + fh$ as the real number K, then

$$x^m + ax^{m-1} + bx^{m-2} + \cdots + fx - K$$

has a real root, h.

(2°) On line *BAD* [*A* is assumed to be the origin], *B* corresponding to $-K$ and *D* to g, erect as perpendiculars, real or "imaginary," the corresponding quantities which make the polynomial vanish. It is evident that the real ordinates form a curve; by Art. V the "imaginary" ordinate corresponding to abscissa *AD* can always be supposed equal to $p + q\sqrt{-1}$. □

In the 1754 version, Article LXXX Theorem 2 is essentially the same as Article VI of 1746. Its proof depends on Article LXXIX as Art. VI of 1746 depends on Art. V, although the details are different. In the equation $x^m + ax^{m-1} + bx^{m-2} + \cdots + fx + g = 0$, the g is replaced by real y, and we let $x = p + q\sqrt{-1}$, where p and q are indeterminate, of a form "tout-à-fait inconnue," yielding eventually—the details are sketchy—a pair of equations, one of p in y and a second of q in y. Now p and q, real for real y in at least some interval, can only cease to have real values at a point $y = L$ if, by Art. LXXIX, they become complex on an interval about $y = L$; but then $x = p + q\sqrt{-1}$ is still complex, meaning p and q stay real. So L cannot exist. Thus for all real y there is x of form $m + n\sqrt{-1}$.

5. Commentary on D'Alembert's proof

D'Alembert's proof met a mixed reception in the 18th century. The first criticisms, by Euler, Foncenex, and Lagrange, were aimed at the series development of a root, x. All felt that some claims were not justified.

Euler wrote d'Alembert on 29 December 1746

[4] VI. *Propos. II. Soit un multinome quelconque $x^m + ax^{m-1} + bx^{m-2} + \cdots + fx + g$, tel qu'il n'y ait aucune quantité réelle qui etant substituée à la place de x, y fasse evanouir tous les termes, je dis qu'il y aura toujours une quantité $p + q\sqrt{-1}$ à substituer à la place de x, & qui rendra ce multinome egal à zero* [D'Alembert, 1746].

> I have read with as much profit [fruit] as satisfaction your last piece with which you have honored our Academy [Berlin]. The manner in which you prove that every expression $x^n + Ax^{n-1} + \text{etc.} = 0$ which has no real roots must have them of the form $p \pm q\sqrt{-1}$: and consequently that it should have a factor of the form $xx + \alpha x + b$ fully satisfies me; but as it proceeds from the resolution of the value of x in an infinite series, I do not know if everyone will be convinced. [Euler, 1980, 252]

In his own proof of the FTA, Euler is more direct; no one had yet "with sufficient rigor" shown the truth of the FTA [Euler, 1749, Art. 7].

The exact reason for Euler's doubt about the development of the root, x, as a series in y, is unclear, but the breadth of d'Alembert's claim was good reason. Gauss later provided counterexamples [Gauss, 1799, Art. 5]. D'Alembert soon admitted in a letter of 20 July 1749 [Euler, 1980, 302] that he should restrict his claim to "geometric" [algebraic] curves.

In his proof of the FTA of 1759, Daviet de Foncenex questioned d'Alembert's claim that the series for root x in y guaranteed a complex number x when y is negative.

> Since the imaginary value that he finds by this method is only approached, one might suspect that the neglected quantity, however small it might be, could be precisely that which makes impossible the finite expression of the unknown.
>
> ...it often happens that a term which one believed could be neglected in a series is, however, that which changes its nature.[5] [Foncenex, 1759, 115]

J.L. Lagrange wrote in the second paragraph of his first proof of the FTA [Lagrange, 1772] that d'Alembert's "demonstration is very ingenious and leaves, it seems to me, nothing to be desired in exactitude; but it is indirect, being drawn from the consideration of curves and of infinite series...." By 1798, Lagrange was more hesitant:

> This proof is incomplete, for, although in an equation of two indeterminates one can always express one by a series of ascending powers of the other, it can happen that the coefficients of this series depend themselves on equations which do not have real roots and which introduce into the series other imaginaries besides those which come from the powers of the indeterminate. [Lagrange, 1798, Art. 7]

C.F. Gauss (1777–1855) presented, in his doctoral dissertation [Gauss, 1799], a penetrating and highly influential analysis of d'Alembert's proof. The same paper is widely considered to contain the first "substantial" [Kline, 1972, 598] proof of the FTA. Gauss carried his criticism to *Theorem B*.

Gauss accurately described d'Alembert's proof. In Article 5 of [1799], Gauss wrote [concerning the equation $p(x) - X = 0$]:

> Finally, d'Alembert declared that if X is supposed to be able to run through the entire interval between two real values R, S [inclusive]...where x always has the form $p + q\sqrt{-1}$, then function X can be increased or diminished...by a real finite quantity holding x always in the form $p + q\sqrt{-1}$.[6] [Gauss then presented d'Alembert's justification from 1746, Art. 5.]

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5 Puisque la valeur imaginaire qu'il trouve par cette méthode n'étant qu'approchée, on pourrait soupçonner que la quantité que l'on néglige, quelque petite qu'elle soit, ne fût précisément celle qui empêcheroit qu'on ne pût exprimer l'inconnue par une expression finie : ...il arrive souvent qu'un terme qu'on croyoit pouvoir négliger dans une série, est cependant celui qui la fait changer de nature [Foncenex, 1759, 115].

6 Tandem affirmat ill. D'ALEMBERT, si X totum intervallum aliquod inter duos valores reales R, S percurrere posse supponatur (i.e., tum ipsi R, tum ipsi S, tum omnibus valoribus realibus intermediis aequalis fieri) ; tribuendo ipsi x valores semper in forma $p+q\sqrt{-1}$ contentos ; functionem X quavis quantitate finita reali adhuc augeri vel diminui posse (prout $S > R$ vel $S < R$), manente x semper sub forma $p+q\sqrt{-1}$ [Gauss, 1799, Art. 5].

Gauss had opened his paper with a detailed summary and criticism of proofs by d'Alembert, Foncenex, and Euler (and Lagrange). All were accused of assuming what they were proving, namely, that every polynomial has roots. This was the first of four objections in Article 6 leveled at d'Alembert's argument.

Gauss's second objection was that the series development for x is not possible for all transcendental functions—he offered the counterexample $y = e^{1/x}$, with $x = 1/\log y$—although the claim is true when only algebraic functions are considered, as in de Bougainville [1754]. And even then, d'Alembert gave no proof.

The third objection was to the free use of the infinitely small and to the unjustified jump from the infinitely small to the finite. This objection also loses strength for the 1754 version of d'Alembert's proof.

The fourth objection is the most serious, although Gauss did not emphasize it. It refers to the crucial Art. V of d'Alembert's 1746 proof and the corresponding material, in Arts. LXXIX and LXXX, of 1754. Referring to the proof of Article V, Gauss pointed out that the limiting value, α, of the set of abscissas that produce complex ordinates $p + q\sqrt{-1}$ need not itself produce such an ordinate. He went on to observe that for algebraic functions this case of a limiting value α that is not in that set of abscissas would not occur; "nevertheless without proof, which is not possible in this case, the method must be held as incomplete."

Gauss concluded, in his final paragraph on d'Alembert, in Art. 6, "For these reasons I deny that d'Alembert's proof can be held satisfactory." But then he added, "Nevertheless it seems to me possible that this can be the true nerve of a proof unaffected by all the objections."[7] Thus, Gauss suggested that the circular reasoning that was a fatal flaw in the other proofs considered (Arts. 6–12) could be avoided in d'Alembert's. Gauss finished with a promise of a proof on a later occasion and told readers to "compare [conf.], meanwhile, Article 24 below." The proof never appeared, but the point was taken up by later commentators.

It should be noted that Gauss's opinion of d'Alembert could be harsher than his 1799 judgment. He wrote in Gauss [1815, 106], while reporting on his own second proof of the FTA, that the charge of circular reasoning applied to d'Alembert's proof as much as to those of Euler, Foncenex, Lagrange, and Laplace.

At the opening of the 19th century, Gauss was not accorded primacy with the FTA. Cauchy [1817, 217], for example, listed Lagrange, Laplace, and Gauss as having established the theorem, without emphasizing Gauss's place. However, by the end of the 19th century, the history of the FTA generally followed the outline provided by Gauss. Gauss's evaluation of his predecessors was taken up by the most respected historians, including Gino Loria, Moritz Cantor, Florian Cajori [Cajori, 1908, 139], and Eugene Netto. They joined in crediting to Gauss the first rigorous proof of the FTA [Loria, 1891, 203], or at least "stronger claims to a satisfactory proof" than his predecessors [Netto, 1898, 234]. They accepted Gauss's opinion that the algebraic proofs of Euler, Lagrange, and Laplace were fatally flawed by circular reasoning; Loria emphasized Gauss's comment that d'Alembert's offered the possibility of a sound proof.

Loria's influential article (see Gilain [1991, 121]) argued that d'Alembert's proof was essentially correct, only lacking justification for some of its (true) claims, while the other early proofs were essentially flawed [Loria, 1891]. Loria said that Gauss, in the final Article 24 of his 1799 paper,

[7] Propter has rationes demonstrationem D'ALEMBERTianam pro satisfaciente habere nequeo. Attamen hoc non obstante verus demonstrationis nervus probandi per omnes obiectiones infringi mini videtur, ... [Gauss, 1799, Art. 6].

> traced the general course of a proof modeled on that of the geometer of the Encyclopedia [d'Alembert], a sign which—although leaving considerable labor to one who wishes to transform it into a rigorous and complete argument—is sufficient to serve as confirmation of the favorable judgment pronounced by Gauss on the inquiry of d'Alembert. [Loria, 1891, 189]

Loria's reading of Gauss's Article 24 is hard to accept. Gauss did say in Article 23 that his argument of Article 24 is "nevertheless in particular essential aspects like the d'Alembertian." But Gauss was arguing (in modern notation) that if $f(x+y\sqrt{-1})$ is represented as $T(x,y)+U(x,y)\sqrt{-1}$, then one can continuously follow the value of $U(x,y)$ along the curves $T(x,y)=0$ to a point where $U(x,y)=0$. There is something d'Alembertian in that one follows a curve, but this is not a proof following the course set out by d'Alembert. (Variations on this argument are found in Le Vavasseur [1907, 192] and Dieudonné and Guérindon [1978, 68–69].)

Cantor provided a very good short account of d'Alembert's proof, one not so generous as Loria's. After an unflattering description of d'Alembert's mathematical style, Cantor gave an outline of d'Alembert's 1746 proof. Unlike Loria, he did not argue that d'Alembert's argument could be repaired. His comments, on both Euler [Cantor, 1901, 602] and d'Alembert's proofs, recognized the influence of Gauss.

> Gauss had remarked about this that even if all d'Alembert's other objectives were granted, the assumption could not be justified that if a function $\phi(x)$ takes a value S and does not take a value U, then there must be a value T between S and U which $\phi(x)$ achieves but does not surpass. It is more likely that $\phi(x)$ approaches T without reaching it. [Cantor, 1901, 587]

And Netto's brief evaluation of d'Alembert's proof is no more than a reference to Gauss: "Gauss, who showed [d'Alembert's proof] to be inadequate in several points, declared at the same time that it could be converted to a fuller rigor" [Netto, 1898, 236].

In recent decades, several historians have reexamined the early proofs of the FTA and Gauss's objections to those proofs. Where Cauchy, in [1817], traced the history of the FTA without even mentioning d'Alembert, some modern evaluations give d'Alembert's proof a special place among the early efforts, even before Gauss in the case of [Stillwell, 1989, 195–198]: "We can now fill the gaps in d'Alembert (1746) by appeal to standard methods and theorems, whereas there is still no easy way to fill the gap in Gauss (1799)." [Bottazzini, 1986, 15–16, 40–41; Dieudonné and Guérindon, 1978; Gigli, 1925, 189–192; Gilain, 1991; Houzel, 1989; Petrova, 1974], and Juŝkeviĉ and Taton [Euler, 1980, 253] all present d'Alembert's argument as containing unproven claims but fundamentally sound. I accept the conclusion but believe that the justifications must be examined with care.

Where assuming the existence of roots of some form was regarded as circular reasoning by Gauss, subsequent construction of the splitting field lets us view this defect as a lacuna. (See Bachmacova [1960, 211].) Christian Gilain takes the question of existence of roots in another direction. He argues that d'Alembert's proof is "a true theorem on the existence of roots" [p. 117], in that way superior to the proofs of Euler and Lagrange, who had assumed that any polynomial could be written $(x-\alpha)(x-\beta)(x-\gamma)\cdots$.

One interpretation of d'Alembert's proof, found, for example, in [Delone, 1956, 281; Petrova, 1974; Stillwell, 1989]; centers on a "Lemma of d'Alembert." From Stillwell:

> ... The key to d'Alembert's proof is a proposition now known as d'Alembert's lemma: if $p(z)$ is a polynomial function and $p(z_0)\neq 0$, then any neighborhood of z_0 contains a point z_1 such that $|p(z_1)|<|p(z_0)|$....
>
> A simple elementary proof of d'Alembert's lemma was given by Argand (1806).... [pp. 196–197]

Stillwell finishes the proof of the FTA as Argand did [1814/1815].

Now, d'Alembert's Lemma, without that name, is the basis of a proof of the FTA in Argand [1806] and, successfully (except for a result of continuity), in Argand [1814/1815], and later by Cauchy [1817]. Argand used the series for p about z_0 to argue that for some nearby z_1, $|p(z_1)| < |p(z_0)|$, when $p(z_0) \neq 0$. But d'Alembert, in arguing for a complex solution, z, of $p(z) = y$ for all real y, had only represented z by the inverse series $q(y)$. D'Alembert was not concerned with $|p(z)|$ or $|q(y)|$ or with inequalities. He had just argued that if $p(z) = y$ gives a real or complex z corresponding to a real y, then z stays real or complex as y increases or decreases by some small real number.

The modern works which correctly report d'Alembert's proof still provide little help in understanding the repairs that are required.

One difficulty lies in justifying, for $p(z) = y$, the expansion of z in fractional powers of y. Puiseux [1850] is often cited, for example in Dieudonné and Guérindon [1978]. However, Puiseux, the pioneer in this topic, simply observed [Articles 17–24] that each complex y outside the finite set of singular points gives as many roots $z_1(y), z_2(y), \ldots,$ as the degree of $p(z)$, and then argued that each root $z_j(y)$ is analytic away from the singular points and that the roots are represented by a fractional power series about each of the singular points. Thus, Puiseux very openly assumed the FTA; he was not proving it.

Likewise, analytic continuation and compactness—called on by various writers—are typically employed with algebraic equations only after one has established, usually by the FTA, a collection of complex pairs y and z satisfying $p(z) = y$. Even Gigli's generally excellent discussion calls on the *permanence of functional equations*, without first justifying the existence of an analytic continuation [Gigli, 1925, 192].

6. A d'Alembertian proof of the fundamental theorem of algebra

We take up the FTA in this form:

The fundamental theorem of algebra. *Given $F(z, y) = p(z) - y$, where $p(z)$ is a real polynomial of degree n with $p(0) = 0$. Then for each real value y^* of y there is a complex solution z of $F(z, y^*) = 0$.*

A d'Alembertian proof of the FTA requires a real starting point y_1, or interval of such points, to which corresponds a complex z_1 so $F(z_1, y_1) = 0$, together with an appropriate path from y_1 to y^*. We then show that y can move on this path all the way from y_1 to y^* while keeping a corresponding complex z which satisfies $F(z, y) = 0$.

The greatest difficulty concerns *singular points*:

Definition. y_1 is a *singular point* of polynomial $F(z, y)$ iff $F(z, y_1)$ has a multiple zero in z. An equivalent condition is that $F(z, y_1)$ and $(\mathrm{d}/\mathrm{d}z)F(z, y_1)$ have a nontrivial common factor.

Lemma. *If y^* is singular, then $p(z) - y^* = F(z, y^*)$ has a complex root z.*

Proof. The Euclidean algorithm on $p(z) - y^*$ and $p'(z)$ produces a nontrivial real polynomial factor of $p(z) - y^*$, and so by induction on the degree of $p(z)$ there is a complex root of $p(z) - y^*$. □

The *First Proof* employs a path which avoids singular points but may have to venture into the complex plane off the real axis. The *Second Proof*, which assumes the first, holds to d'Alembert's plan of a path along the real axis.

First proof. If y^* is not singular, then we can choose real y_1 and the entire path (in the complex plane) from y_1 to y^* avoiding the finite set of singular points. y_1 is selected from the interval of values $p(z)$ for z on a real interval.

There is a bound B on $|y|$ on the path, and a bound C [Hille, 1959, 208] on the modulus of any complex roots of $p(z) = y$ for $|y| < B$. Then for any y_2 on the path from y_1 to y^* which yields a real or complex root $z = z_2$, there is a convergent series expressing a root z of $p(z) = y$ as a function of y for each y in a disk $|y - y_2| < r$, *where r depends on B, C, and the coefficients of $p(z)$ but not on y_2*. A "majorant" argument, as found in Goursat [1904, 394–401] or Hille [1962, Ch. 9], can produce such an r, as can the *Implicit Function Theorem* [Hille, 1962, Ch. 9]. (See the *Majorant Argument* in Appendix A, below.) Either approach depends on the boundedness of a continuous function on a compact set.

Now, starting with a disk centered at y_1, then a disk centered at y_2, where $|y_1 - y_2| = r/2$, etc., one builds a chain of overlapping disks of radius r until y^* is reached, with complex z^* so $p(z^*) = y^*$. □

Second proof. We now allow for a singular point y_2 on the path, and thus keep the y-path on the real axis. As in the case, above, of singular y^*, $p(z) - y_2$ has a complex root z_2 (of multiplicity $k > 1$). We can suppose $y_2 = z_2 = 0$, so

$$p(z) - y_2 = p(z) = az^k\bigl[1 + q(z)\bigr], \quad a \neq 0,\ q(0) = 0.$$

Let $h(z)$ be a kth root of $p(z)$. Then

$$h(z) := a^{1/k}z\bigl[1 + q(z)\bigr]^{1/k} = a^{1/k}z\left[1 + \sum_{j=1}^{\infty} c_j z^j\right] \quad \text{for } |z| < K,\ a^{1/k} \text{ any } k\text{th root of } a,$$

K a positive constant. $z = 0$ is nonsingular for $h(z)$, so the relation $h(z) = w$ can be inverted [see *First Proof*] in an analytic function of w in a neighborhood of the origin:

$$z = g(w) = d_1 w + d_2 w^2 + d_3 w^3 + \cdots \quad \text{if } |w| < \delta_1.$$

Since $p(z) = [h(z)]^k$, then $p(g(w)) = w^k$. Let w be a kth root of y. Then $p(g(y^{1/k})) = y$. So

$$z = g\bigl(y^{1/k}\bigr) = d_1 y^{1/k} + d_2 y^{2/k} + d_3 y^{3/k} + \cdots \quad \text{in the disk } |y| < \delta_1^k,$$

where $y^{1/k}$ denotes any of the kth roots of y. (A different choice of the kth root of a produces the same k different values of z. See Hille [1962, Theorem 9.4.3].)

Therefore as y, on the real path from y_1 to y^*, approaches the singular point y_2, one of the disks of radius r or smaller must overlap the disk just found of radius δ_1^k about y_2, without meeting y_2. Thus we have a continuous path of complex values z corresponding to the y-path right through and beyond y_2. At a nonsingular real point of the y-path beyond y_2 we can again build the chain of disks of radius r or smaller.

In this way, we follow d'Alembert's original plan of a y-path on the real axis from initial point y_1 to the given y^*, to which there is a corresponding real or complex z-path of solutions of $p(z) = y$. This proves the Fundamental Theorem of Algebra. □

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Appendix A. Majorant argument

Let $p(z)$ be a real polynomial of degree n with $p(0) = 0$, y complex, C a bound on any complex roots of $p(z) - y$, where $|y| < B$. For a given real value y_2 of y, $|y_2| < B$, suppose $z = z_2$ is a simple complex root of $p(z) - y_2 = 0$. We then claim that there is a positive r, depending only on B, C, and the coefficients of $p(z)$, such that when $|y - y_2| < r$, then there is a complex root z of $p(z) = y$ expressed as a convergent series in y.

Proof. Assume $z_2 = 0 = y_2$. Rewrite $p(z) = y$ in terms of a series around $z = 0$:

$$z = G(z, y) = c_0 y + c_2 z^2 + c_3 z^3 + \cdots + c_n z^n,$$

which is possible since y is not singular. Let (formally)

$$z = b_1 y + b_2 y^2 + b_3 y^3 + \cdots, \tag{A.1}$$

and then by substitution into the series $z = G(z, y)$, we can formally solve for b_1, then for b_2, then for b_3, etc.

We also consider the equation

$$z = d_0 y + d_2 z^2 + d_3 z^3 + \cdots + d_n z^n, \quad \text{where } d_j := |c_j|. \tag{A.2}$$

Set $z = b_1^* y + b_2^* y^2 + b_3^* y^3 + \cdots$, substitute into Eq. (A.2), and solve for b_1^*, b_2^*, b_3^*, etc. By induction,

$$b_1^*, b_2^*, b_3^*, \text{ etc. are all nonnegative and } |b_j| \leqslant b_j^* \text{ for all } j. \tag{A.3}$$

In $|z_2| \leqslant C$, $|y_2| < B$, there is a bound M on the coefficients of $G(z, y)$, since the coefficients are based on (continuous) partial derivatives of $G(z, y)$. Then

$$|z| = \bigl|G(z, y)\bigr| \leqslant M\bigl[|y| + |z|^2 + |z|^3 + \cdots + |z|^n + \cdots\bigr]. \tag{A.4}$$

Note that $|c_j| \leqslant M$.

We can assume (justified below) $|z| < 1$. Replacing $|z|$ by t, and $|y|$ by s, inequality (A.4) is

$$t \leqslant M\left[s + \frac{t^2}{1 - t}\right]. \tag{A.5}$$

Inequality (A.5) is satisfied by $t = 0$. The corresponding equality gives, by the quadratic formula,

$$t = \frac{1 + Ms - \sqrt{(Ms + 1)^2 - 4(M + 1)Ms}}{2(M + 1)}, \tag{A.6}$$

choosing the minus over the plus so that $t \to 0$ as $s \to 0$. The radical can be expanded as a convergent series in (nonnegative) s iff $-1 < 2Ms + M^2s^2 - 4Ms - 4M^2s < 1$. Both inequalities are satisfied iff

$$0 \leqslant s < r := \frac{1 + 2M - \sqrt{(1 + 2M)^2 - 1}}{M}. \tag{A.7}$$

If $r > 1$, set $r := 1$. For s satisfying $0 \leqslant s < r$, the series development of $t = |z|$ as a function of $s = |y|$, given by Eq. (A.6), converges. Because of inequality (A.3), and since $|c_j| \leqslant M$, the series (A.1) also converges for $|y| < r$. And since the series formally solves $z = G(z, y)$, it is an analytic solution of that equation. $\square$

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