On Cauchy's Rigorization of Complex Analysis

by

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Abstract

In this paper, I look at Cauchy's early (1814–1825) rigorization of complex analysis. I argue that his work should not be understood as a step in improving the deductive methods of mathematics but as a clear, innovative and systematic stance about the semantics of mathematical languages. His approach is contrasted with Laplace's "notational inductions," influenced by Condillac's ideas about the language of algebra. Cauchy's opposition is then not to be seen as stemming from a comeback of geometric and synthetic methods, but as a rejection of the key Condillacian doctrines that algebra is about abstract quantities and that its rules provide means of discovering new mathematical truths. He thereby paved the way for the arithmetization of calculus and fruitfully extended his approach to complex analysis like no one before him. I finish by discussing lessons we can draw about how mathematical rigour differs from rigour in other sciences.

Dedication

To my friend Jared

You ask me what's idiosyncratic about philosophers?... There is, for instance, their lack of a sense of history, their hatred for the very notion of becoming, their Egyptianism. They think they're honoring a thing if they de-historicize it, see it sub sepcie aeterni — if they make a mummy out of it. Everything that philosophers have handled, for thousands of years now, has been conceptual mummies; nothing real escaped their hands alive.

Twilight of the Idols, Nietzsche (1889)

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Note on Translation

All translations from French to English are my own. I use some French words for important concepts to make explicit to the reader that I mean them in the sense their relevant 18th–19th century users meant them. Such words will be italicized to indicate they come from a non-English language.

1 How to Ask 'Why is Mathematics Rigorous?'

Mathematics is generally thought of as a discipline that is, in an important sense, more rigorous than any other discipline, from physics to psychology, chemistry to political science, biology to post-colonial marxist critical theory. But in what sense is mathematics rigorous? Tarski, (1936, p. 112) answered without hesitation that

> The view has become more and more widely accepted, that *the deductive method is the only essential feature which distinguishes mathematical disciplines from all other sciences* [.]

Mathematics, accordingly, is done with axioms "stating fundamental properties of concepts" from which one deduces "with a logical argument" more and more theorems Aleksandrov, 1999, p. 3. "Logical rigour" is what sets mathematics apart. This view echoes that of many contemporary mathematicians like Feferman who believe that "99% [of mathematics] can easily be formalized in ZFC and, in fact, in much weaker systems" (2000, p. 403). One also finds this deductivist presentation in many real analysis textbooks, perhaps famously in Rudin, 1953, but also in contemporary ones like Lay, 2014 and Barry, 2015a.

That mathematics is done in a deductivist way is false if interpreted as a claim about the context of mathematical discovery, about how mathematics is actually done, as examples from Lakatos, 1976 suffice to show. Even if the use of axiomatics might date from Euclid's *Elements*, its wide-spread use in mathematics only dates from the second half of the 19^{th} century (Schlimm, 2006), (2013, p. 43–44) in the works of Frege, 1879, Pasch, 1882, Dedekind, 1888, Peano, 1889 and Hilbert, 1899. Seeing earlier mathematicians as modern axiomatizers is "out of line" Grattan-Guinness, 2004, p. 177.

If "[t]he context of discovery is left to psychological analysis" Reichenbach, 1947, p. 2, we end up with the view that mathematics is rigorous because its statements can be deductively justified. Setting aside technicalities arising from results in mathematical logic like Löwenheim, 1915, Skolem, 1922, Gödel, 1931 and Tarski, 1933, this view seems to entail that mathematics was not rigorous before Frege, 1879 gave mathematicians quantified logic, for until then, deductively double-checking their proofs in a formal system was impossible.

Importantly, deductivism seems to see mathematics as a set of theories which, through important steps in their rigorization, become more and more in line with the canons of deductive logic. Some historians have objected that the view seems to "photocopy" modern mathematical and logical notions into past theories Grattan-Guinness, 2004, p. 165, problematically assuming that mathematical concepts remain the same through time Schubring, 2005, p. 1–3, to then illegitimately make the claim that all of mathematics, across epochs, is rigorous because it is deductive. The present paper can be seen as vindicating these criticisms by illustrating how mathematics should not be seen a set of theories but as a *practice* (Dalmedico, 1997, p. 46–47): important steps in the rigorization of mathematics were made by changing not mathematical theories *per se*, but how mathematicians *viewed* and *interpreted* them.

Another approach to the question of rigour is instead to acknowledge that mathematical rigour is itself a historical concept and consequently in progress Bottazzini, 1986, p. 3, (2001, p. 35). Mathematics is and has been made rigorous in many different ways and for many different reasons Segura and Sepulcre, 2016, p. 212–213; it is not a unified theory continuously built through time that has come down to us today Bottazzini and Dalmedico, 2001, p. 3, Ferraro, 2008, p. vii. There is no determinism in mathematics, that is, we could be doing mathematics very differently today had social, political, scientific or even anecdotic contingencies been different Grabiner, 1975, p. 440. The interesting questions then become how and why new techniques, approaches and standards of rigour arise in mathematics Bottazzini, 1990, p. XV–XVI. One upshot of this historicist view is that other disciplines can learn different ways to become more rigorous by looking at how mathematicians made their field more rigourous in light of challenges, debates and discoveries in their own field.

Regardless of who is right, the best way to answer the question 'How is mathematics rigorous?' is to look at the work of mathematicians. Since the 19^{th} century in Western mathematics is regularly called the "age of rigour" Lüzten, 2003, p. 155, the works of key figures who participated in the rigorization of analysis can help us understand why and how mathematical practice often becomes more rigorous. This will help us assess the deductivist claim that progress in mathematical rigour is more than often an improvement in its deductive methods.

To do so, I will look at the work of Cauchy, "[t]he most important figure in the initiation of rigorous analysis" Grabiner, 1981, p. 3, in complex analysis. My aim will be to isolate some key components of what Cauchy's rigorization consisted in and why he engaged in rigourizing this field. As I will argue, Cauchy's rigorization has much to do with clarifying what the subject of mathematics is — *contra* Freudenthal, (1971, p. 377) but perhaps in agreement with Fraser, (1989, p. 332) — for various mathematical reasons. This, I believe, is a serious worry for deductivism.

2 The Issue: The Scope of Mathematics

In the introduction of his *Cours d'analyse de l'école royale polytechnique* (1821), Cauchy warns us of the following grave philosophical mistake.

> But it would be a grave mistake to think that one finds certainty only in geometric demonstrations, or in the testimony of the senses; and while to this day no one has attempted to prove the existence of Augustus or that of Louis XIV[1] using analysis, every sensible man will agree that this existence is as certain for him as the square of the hypothenuse or Maclaurin's theorem. (1821, p. vj–vij).

At the heart of this intriguing remark is Cauchy's conviction that we should not "stretch [mathematical sciences] beyond their domain," for example, "let us not imagine that one can do history with formulas, nor give as sanction to morality some theorems of algebra or integral calculus"[2] (1821, p. vij). This echoes Aristotle's comment in *Nicomachean Ethics* (B.1, §3), who claims "it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician scientific proofs." The scope of mathematical formulas was an issue Cauchy took seriously.

> As for methods, I have sought to give them all the rigour we require in geometry, so to never rely on the reasons drawn from the generality of algebra. Reasons of this kind, although commonly admitted, especially in the passage from convergent to divergent series, and from real quantities to imaginary expressions, can be considered, it seems to me, only as inductions proper for sometimes sensing[3] the truth, but which agree little with the exactness so vaunted of mathematical sciences. (1821, p. ij–iij)

[1] Bradley and Sandifer, (2000, p. 1) and Barany, (2011, p. 380–383) believe Cauchy is referring to Caesar Augustus (63 BCE – 14 CE). Like myself, Bradley and Sandifer note that "Cauchy, whose given name is Augustin-Louis, may be engaging in a rare display of humour by choosing these two particular examples." Barany sees it instead as a carefully chosen rhetorical device.

[2] Bradley and Sandifer, (2000, p. 3) wrongly translate "nor to make moral judgments with theorems of algebra or integral calculus." Cauchy clearly means "justifying morality" and does not even use '*jugement*'.

[3] Bradley and Sandifer, (2000, p. 2) weirdly translate '*pressentir*' as "introducing."

These inductions based on the generality of algebra do not meet the well-known exactitude of mathematics, since they "tend to attribute an undefined[4] scope to algebraic formulas, whereas, in reality, most of these formulas hold only under certain conditions, and for some values of the quantities they contain" (1821, p. iij–iv). Relying on the generality of algebra thus crucially consists in being mistaken about the scope of mathematical formulas.

To better understand why Cauchy took issue with the generality of algebra, I will focus on his innovative rigourous treatment of imaginary numbers in his *Cours* (1821) and in some of his early works in complex analysis (1817; 1817; 1821; 1822; 1822; 1823; 1823; 1825; 1825; 1825; 1826). Cauchy himself noticed the novelty of his approach as it forced him to accept "many propositions which may appear a little rigid at first," for example, that "an imaginary equation is only the symbolic representation of two equations between[5] real quantities" (1821, p. iv). Focusing on this part of his work might be even more pertinent since "[t]he second part of Cauchy's *Cours* has been slighted by historians by comparison to the attention they have paid to the first part of it" while "Cauchy took the ontological problem concerning imaginaries much more seriously than anybody else had done before" Bottazzini, 1990, p. CII.[6] In particular, I will be concerned with these two questions:

(Q_1) Why can one not rely on the passage from the reals to the imaginaries according to Cauchy?

(Q_2) How does Cauchy believe one should deal with imaginaries and their relation to real numbers?

I will suggest that two reasons why Cauchy believed we should not rely on this passage is that real functions have certain algebraic properties which imaginary ones do not have; and that imaginary substitutions in contexts of integration distort the value of definite integrals. Moreover, Cauchy thought that this passage should be done just in case there is a proof that in that context, the relevant manipulations hold of real and imaginary numbers.

[4]Bradley and Sandifer, (2000, p. 2) wrongly translate '*indéfinie*' as "limitless."

[5]Bradley and Sandifer, (2000, p. 2) wrongly use '*involving*', not rendering the characteristic 18$^{\text{th}}$-19$^{\text{th}}$ language of equations and functions "*being made of* quantities."

[6]Dhombres, (1985, p. 86), Smithies, (1986, p. 45–46), Bottazzini, (1986, p. 126) and (2003, p. 217) also make this remark. Grattan-Guinness, (1970, p. 378) even asserts "[t]he initial stimulus for this work was foundational questions concerning the representation of complex numbers."

However, and perhaps circularly, I believe these two answers cannot be understood outside Cauchy's new rigourous complex analysis since his criticisms of the generality of algebra are formulated from his own rigourous perspective. Thus, I will first argue that his rigorization of complex analysis amounts to clearly and systematically specifying the subject of complex analysis. As Cauchy says:

> In determining these conditions and these values [of the application of mathematical formulas], and in fixing the meaning of the notations I use in a precise manner, I make disappear all uncertainty; and then the different formulas represent nothing but relations between real quantities, relations which will always be easy to verify by the substitutions of numbers for the quantities themselves. (1821, p. iij–iv)

In final, Cauchy's style of rigorouzation, I believe, conflicts with deductivism, as I will argue in the conclusion. I will first describe an instance of reasoning based on the generality of algebra to which Cauchy explicitly objects: imaginary substitutions in real definite integrals. Taking Laplace as a canonical example of a practitioner of this reasoning, I will explain why Laplace thought such reasonings were admissible in mathematics. Doing so will bring us in a detour to the French empiricist philosophers Condillac, whose views I believe influenced French mathematicians and Laplace in particular. Then, I will look at Cauchy's approach to complex analysis and contrast it to Argand's more geometric interpretation of imaginary numbers. My goal will be to situate Cauchy's novel perspective in the plethora of algebraic and geometric approaches to calculus in the late 18^{th} and early 19th centuries in France. I will finally turn to mathematical reasons why he objected specifically to the use of the generality of algebra. My main historical conclusion will be that Cauchy rigourized complex analysis by taking a clear, innovative and systematic stance about the meaning of his real and imaginary notations. In the conclusion, I use this important remark about the history of mathematics as an argument in favour of a historicist approach to rigour: assuming *all* mathematics is rigorous *only* because it is deductive leads to a mischaracterization of its practice and an incorrect reconstruction of its history.

3 A Case: From the Reals to the Imaginaries

Cauchy complains about the passage from the reals to the imaginaries as a reasoning grounded in the generality of algebra. Smithies, (1986, p. 47–48) suggests that

Cauchy (1820) identified an instance of this passage in the work of Poncelet, reprinted in his *Traité des propriétés projectives des figures* (1822). This claim is surprising considering that Poncelet saw himself as doing in this treatise a "particular *Géometrie* [...] finally independent from algebraic *Analyse*" (1822, p. xix). In effect, his treatise barely contains any algebraic expressions, which is understandable given that projective geometry does not rely, generally speaking, on absolute or relative measures of length. Moreover, what Cauchy takes issue with in that article (and later (1825)) is not the passage from the reals to the imaginaries in *Analyse* but Poncelet's "principle of continuity" in *Géometrie* (1820, p. 332–333). While Cauchy qualifies this principle as a kind of "induction" having problematic counterparts in *Analyse*, notably in the context of integration, Poncelet immediately pointed out that it was not an inductive form of reasoning (1822, p. xxiv) and that these objections miss the point since "I considered [the principle of continuity], simply, I repeat, in its application in pure *Géometrie*, not in integration formulas" (1866, p. 358).

A better example of the passage from the reals to the imaginaries discussed later by Smithies, 1997 and mentioned by Cauchy, (1814) is Laplace's calculation of some improper integrals of complex functions. To understand what Cauchy objects to, I briefly lay out Laplace's use of the generality of algebra in this section.

3.1 Laplace's "Remarquable Artifice"

Laplace himself qualifies his passage from the real to the imaginaries as a "*remarquable artifice*" (1809, p. 193–194). As an example, he takes the following integral

$$\int_0^\infty \frac{dx e^{x\sqrt{-1}}}{x^\alpha}, \quad \text{where } 0 < \alpha < 1. \tag{3.1}$$

Despite the fact that x in (3.1) ranges over real numbers, Laplace substitutes the imaginary function $t^{\frac{1}{1-\alpha}}\sqrt{-1}$ for x and, after algebraic manipulations, obtains

$$\frac{1}{1-\alpha}(-1)^{\frac{1-\alpha}{2}} \int_0^\infty dt e^{-t^{\frac{1}{1-\alpha}}}, \tag{3.2}$$

where t is a real variable evaluated between 0 and ∞. Letting the integral part of (3.2) be k, we get

$$\int_0^\infty \frac{dx e^{x\sqrt{-1}}}{x^\alpha} = \frac{1}{1-\alpha}(-1)^{\frac{1-\alpha}{2}} k.$$

Since $(-1)^{\frac{1-\alpha}{2}} = \cos\phi + \sqrt{-1}\sin\phi$ for some angle ϕ, Laplace writes

$$\begin{aligned} -1 &= (\cos\phi + \sqrt{-1}\sin\phi)^{\frac{2}{1-\alpha}} \\ &= \cos\frac{2}{1-\alpha}\phi + \sqrt{-1}\sin\frac{2}{1-\alpha}\phi. \end{aligned}$$

From this, he gets that for some integer r,

$$\begin{aligned} \frac{2}{1-\alpha}\phi &= (2r+1)\pi \\ \phi &= (2r+1)(1-\alpha)\frac{\pi}{2} \end{aligned}$$

which entails that

$$-1^{\frac{1-\alpha}{2}} = \cos(2r+1)(1-\alpha)\frac{\pi}{2} + \sqrt{-1}\sin(2r+1)(1-\alpha)\frac{\pi}{2}.$$

Thus,

$$\int_0^\infty \frac{dxe^{x\sqrt{-1}}}{x^\alpha} = \int_0^\infty \frac{dx\cos x}{x^\alpha} + \sqrt{-1}\int_0^\infty \frac{dx\sin x}{x^\alpha}. \tag{3.3}$$

Laplace then compares the real and imaginary quantities of (3.3) in light of (3.2).

$$\int_0^\infty \frac{dx\cos x}{x^\alpha} = \frac{k}{1-\alpha}\cos(2r+1)(1-\alpha)\frac{\pi}{2}, \tag{3.4}$$

$$\int_0^\infty \frac{dx\sin x}{x^\alpha} = \frac{k}{1-\alpha}\sin(2r+1)(1-\alpha)\frac{\pi}{2}. \tag{3.5}$$

By geometric reasoning, he concludes that (3.4) and (3.5) are positive and finite when $\alpha < 1$ (1809, p. 195–196). Letting $\alpha = 1/2$, he gets that

$$k = \int_0^\infty dte^{-t^2} = \frac{1}{2}\sqrt{\pi}$$

and the equations (3.4) and (3.5) become

$$\begin{aligned} \int_0^\infty \frac{dx\cos x}{\sqrt{x}} &= \sqrt{\pi}\cos\frac{2r+1}{4}\pi, \\ \int_0^\infty \frac{dx\sin x}{\sqrt{x}} &= \sqrt{\pi}\sin\frac{2r+1}{4}\pi, \end{aligned}$$

allowing Laplace to calculate (3.1).

3.2 A Practice without Theory?

Laplace's substitution does not solely consist in reading a real variable of integration as also ranging over imaginary quantities. It instead consists in expressing the real variable of integration x with the complex function $t^{\frac{1}{1-\alpha}}\sqrt{-1}$, the resulting integral (3.2) with respect to t being considered from $t = 0$ to $t = \infty$, as $x \to \infty$ when $t \to \infty$. Since the resulting integrals (3.4) and (3.5) with respect to t are constant and finite, he can directly derive the result of his main integral (3.1) with respect to x. This differs from merely attributing real and imaginary quantities to a variable, which was already unproblematically done in some contexts at the time, for example, in the various proofs of the fundamental theorem of algebra given by D'Alembert, 1746, Laplace, 1795, J.-L. Lagrange, 1798, Gauss, 1799, Argand, 1806; Argand, 1814 and Cauchy, 1817a; Cauchy, 1817b; Cauchy, 1820b.

Laplace thought this substitution was permissible since "[w]hen results are expressed in indeterminate quantities, the generality of the notation encompasses all cases, be they real, or imaginary" (1809, p. 193). That Laplace does not go into more detail probably lead Bottazzini and Gray, (2013, p. 126) to describe this manipulation of a complex function (similar, in their and Smithies's view (1997, p. 11), to manipulations by Bernoulli, (1702), L. Euler, (1755), L. Euler, (1775a), L. Euler, (1775b), L. Euler, (1775c), L. Euler, (1777a), L. Euler, (1777b), L. Euler, (1777c), and L. Euler, (1781), D'Alembert, (1752) and D'Alembert, (1761) and J. L. Lagrange, (1762) and J. L. Lagrange, (1781)) as a "practice without theory."

Bottazzini and Gray might be right that Laplace had no "theory," but they still do not explain why he nonetheless thought his substitution was a permissible and useful move. They and Smithies, (1997, p. 16–23) do not explain why other mathematicians like Poisson, (1810a), Poisson, (1810b), Poisson, (1811a), and Poisson, (1811b) and later Cauchy, (1814) expressed doubts about this method.

In the context of his considerable work on probabilities, Laplace once said that "speaking of rigour, [...], almost all of our knowledge is only probable; and in the small number of things we can know with certainty, in the mathematical sciences themselves, the main ways to reach the truth, induction and analogy, are based on probabilities" (1814, p. 1). These inductions, "used for a long time by geometers" have had "a large number of examples which have justified their use" (1814, p. 36). In mathematics as in science, one can thus infer general laws from observed similarities, like Newton, says Laplace, who inferred both his binomial theorem and his universal law of gravitation the same way (1795, p. 40–41), (1814, p. 84).

Laplace frequently qualifies the passage from the reals to the imaginaries as one of these inductions or analogies made possible by the "language of *Analyse*, the most perfect of all, being itself a powerful means of discovery" since "its notations, when they are necessary and happily imagined, are the seeds of new calculations" (1795, p. 156), (1809, p. 304), (1810, p. 360–361), (1814, p. 36). As examples, he later cites Descartes' introduction of the notation 'n^m' for $n \cdot n \cdots n$ (1637), Wallis' '$(\sqrt[m]{l})^n$' for $l^{\frac{n}{m}}$ (1656) and Leibniz's differential notation (where $l, n, m \in \mathbb{N}$) (1814, p. 3–7), (1820, p. xl).

In addition to being a "notational induction," Laplace sees this passage as a special case of his "theory of generative functions," emerging from some of his work in (1774), (1779), (1783) and expanded in (1814). Looking at this theory would carry us too far away. It is only important to notice that Laplace thought the confirmation of his theory for many real cases implied that in special cases where some "change of signs for the coefficients" introduce imaginary quantities, he could inductively infer that it would also hold (1814, p. 87).

One reason why Laplace was sympathetic to notational inductions is that, as Schubring, (2005, p. 365–368) and Ferraro, (2007, p. 72–73) have already noticed, some of his broader views about mathematics fit in a predominant 18$^{\text{th}}$ century *algebraic* approach to mathematics, a view that some (e. g. Grabiner and Laplace himself (1795, p. 11)) see originating in Newton's *Arithmetica Universalis* (1707), then L. Euler, (1748), Leonhard Euler, (1755), and L. Euler, (1770), Maclaurin, (1756), and reaching its apogee in France with early Carnot, (1775), Lacroix, (1797a) and Lacroix, (1797b), Abrogast, (1800), Ampère, (1806), J. L. Lagrange, (1797), J. L. Lagrange, (1801), J. L. Lagrange, (1806), and J. L. Lagrange, (1813), A.-M. Legendre, (1811). Interestingly, Abrogast similarly introduces and justifies his "méthode de séparation des échelles" as a notational induction (1800, p. viij–ix). It is common to read commentators interpreting many aspects of analysis in the second-half of the 18$^{\text{th}}$ century from that perspective, e. g. Grattan-Guinness, (1970, p. 380–381), Grabiner, (1981, p. 16–28), Fraser, (1987) and Fraser, (1989), Ferraro, 2001; Ferraro, 2007, Jahnke, (2003b, p. 107), Schubring, (2005, p. 257–294) and Ferraro and Panzo, 2012.

The 18th century algebraic approach to mathematics involved reading mathematical expressions as stating general relations between *any* objects, as opposed to state particular geometric relations between particular geometric objects (e. g. curves, lines) or particular magnitudes Ferraro, 2001, p. 538–539 Ferraro and Panzo, 2012, p. 100. It is then not surprising that in many cases, these mathematicians thought "that what was true for real numbers was true for complex numbers" Freudenthal, 1971, p. 377. This reading of mathematical expressions came with a distrust of geometrical reasoning and an increased reliance on symbolic or algorithmic reasonings Ferraro, 2001, p. 538–539.

Situating Laplace in the algebraic tradition is a good starting point to understand why he believed in notational inductions. However, it is unclear how the former explains the latter. Did Laplace really hold the naïve view that what is true for reals is also true for imaginaries? If so, why did he bother telling a story about notational inductions? In what sense are they a kind of analogy? In the next section, I will attempt to answer these questions by tracing back some of Laplace's views about mathematics to the French philosopher Condillac.

3.3 Condillac on the Language of Algebra

An important philosophical figure in French mathematics was the philosopher Condillac, as Lacroix already noticed (1805, p. 20–21).[7] As he himself says in his first work of "*Métaphisique*" laying out his broad philosophical project, *Essai sur l'origine des connoissances humaines*, Condillac attempts to pursue Locke's goal in *Essay Concerning Human Understanding* (1690) of empirically studying the human mind "not to discover its nature, but to know its operations" (1746, p. xij). Crucially, Condillac faults Locke for not having paid sufficient attention to how we develop the operations of our minds, especially in relation to language Taylor, 1989, p. 289–290, how "ideas linked themselves with signs" as "it is only by this mean, as I will show, that they link themselves together" [*qu'elles se lient entre elles*] (1746, p. xxij). His idea is that by using signs we voluntarily bring back ideas to our minds which then allows us to reason. All reasoning is done by "the analogy of signs" : "the fewer analogical tricks [with signs] a Language has, the less it helps the memory and the imagination [reasoning]" (1746, p. 203). As an example, Condillac mentions Newton's (or even

[7]See also Jahnke and M. Otte, (1979, p. 79–90), Glas, (1986, p. 250), Dhombres (1992), (1992, par. 9), Schubring, (2005, p. 260–265), Panteki, (1992, p. 63), (2003, p. 286–287), (2008, p. 390).

Racine's) success as being partly achieved by his and his predecessors' choice of notation. Ultimately, one of Condillac's goal was to formulate norms of correct reasoning by a correct use of language (Duchesneau, 1999, p. 56–58).

Of particular interest for us is Condillac's opposition to the use of "*sinthése*" (1746, p. 94–98), by which he means starting from general "principles" or "axioms," and his preference for "*analise*," the composition and decomposition of ideas (1746, p. 101–103). To convince readers of the superiority of *analise*, Condillac suggests mockingly having a look at the work of rationalist philosophers like Descartes, perhaps uncharitably Israel, 1997, p. 7.[8] For him, "the only way to acquire knowledge, is to go back to the origin of our ideas, to follow their generation and to compare them in all possible ways; what I call *analysing*" (1746, p. 94–98). An important way to form new ideas by *analise* is abstraction: "some kind of ideas which are thus nothing else than denominations which we give to things considered in the ways they resemble each other" (1746, p. 214).[9] The "foundation" of abstraction is noticing in the modifications of our mind "a kind of background [*fond*] which always remains the same" (1746, p. 219–220). This "background" has "no reality," but still, our mind continues to look at it "as a being," as if it existed independently of our mind in order to then reason about them.

Condillac slightly modifies his account of sensations in his *Traité des sensations* (1754) and develops his Lockean project in greater details in his later work Rousseau, 1986, p. 32–33. Building on his *Cours d'étude pour l'instruction du prince de Parme* (1775), in his *La logique ou les premiers développemens de l'art de penser* (1780), after restating the importance of *analyse* (now with a different spelling), especially "analyse indéterminée" as practiced by Euler and Lagrange (1780, p. 19–26, 113–121), (1775, p. 67–68); repeating how we make general ideas through abstraction "from known to unknown" (1780, p. 29–39) and how we can reason only through words; Condillac explains that languages themselves are methods *analytiques* (1780, p. 92–100): given

[8] "Descartes, for example, has he not spent more days on his metaphysical Meditations, when he wanted to demonstrate them according to the rules of this method? Can one find worse Demonstrations than those of Spinosa? I could still cite Mallebranche, who used Synthesis a few times. . . " (1746, p. 97). See his *Traité des sistêmes* (1749) for a real engagement with rationalists. Badareu, (1968, p. 340–342) locates Condillac's main worry in *a priori* reasoning; see also Kouřìm, (1974, p. 180–182) for discussion.

[9] Note Condillac's disregard of the use-mention distinction, which, while it might have upset Frege, (1891), can also be found in the works of French mathematicians, e. g. Laplace, (1795, p. 33–34), J. L. Lagrange, (1797), J. L. Lagrange, (1801), J. L. Lagrange, (1806), and J. L. Lagrange, (1813).

their first innate signs, one must simply use analogy to obtain new signs and then de/compose expressions of the language to analyse their corresponding ideas — see also (1775, p. 8–12). Those new ideas can be reached by *analyse* prior to using analogy on signs or after. In the latter case, analogy can lead us astray if we are not careful, but it can also provide us with good reasoning when our language is "well crafted" (1780, p. 108). Analogy, unlike "obviousness [*évidence*]", is a kind of "conjecture" admitting of degrees of certainty (1780, p. 145). In sum, "we will know how to use words, when *analyse* will have given us the habit of looking for their first acceptance in their first usage, and all the other in analogy" (1780, p. 110), even in the case of abstract ideas.

Discussing abstraction and analogy in the context of mathematics in his posthumous *La langue des calculs* (1798), Condillac explains that people first counted specific objects with their fingers before considering numbers as "applicable to all objects of the universe," which amounts to "abstracting them or separating them from these objects, to consider them in isolation" (1798, p. 49). He concludes that the notions of *number* or *quantity* are themselves abstractions which we perceive in the names or signs of numbers. That is, for Condillac, number names like 'two' refer to themselves.

> If you think that abstract ideas are something else than names, tell me, if you can: What is that other thing? In effect, when you will have abstracted from fingers and other objects which can represent numbers; when you will have abstracted from the names which are other signs; in vain you will look for what is left in your mind, you will find nothing, absolutely nothing. (1798, p. 50)

It is quite unclear how by abstracting from all pairs of objects, one could form the idea of *two* consisting of nothing else than 'two'. In what sense is 'two' common to all our ideas of pairs of objects? Moreover, Condillac seems to think that we get ideas before we choose signs for them, which hardly seems possible in the case where our idea of *two* is 'two'. In any case, accordingly, mathematical operations on ideas are operations on signs (1798, p. 223–224). Condillac gives the example of moving from $x + a - b = c$ to $x = -a + b + c$, which one does by manipulating signs "without needing to know what the letters from which it is formed mean." Only when one is done calculating can one interpret numbers as quantities of books, dollars, cents or others (1798, p. 324).

Condillac distinguishes between "arithmetical quantities," those expressed with numerals (1798, p. 240–247), and "literal quantities," those expressed with letters (1798,

p. 274–293). These letters are signs even more indeterminate than numerals "because they mean nothing by themselves, they can each of them mean the quantity we wish" (1798, p. 275). Thus, 'a' in that language "designates in general all possible numbers" (1798, p. 294). They are general names as opposed to particular names. This mirrors D'Alembert's depiction of "*Arithmétique universelle*" in the *Encyclopédie*, an enterprise which partly consists in investigating "the general properties of quantities [*la quantité*], that is to say we consider quantities [*la quantité*] simply as quantities, and not as represented and fixed by any particular expression" (1751). Accordingly, literal quantities obey different grammatical rules which Condillac articulates by explaining their corresponding algebraic operations: while addition, subtraction, multiplication and division for arithmetic quantities are explained in a way that facilitates computation, i. e. in a vertical lay out, for literal quantities, his first approach is one of equation-solving.

For Condillac, the term 'imaginary quantities' should be replaced by "imaginary expressions" since they are expressions which might seem like they stand for an idea like other expressions but are in fact meaningless (1798, p. 401–403). He argues as follows. Defining roots as the fractional exponents, e. g. $\sqrt[n]{a^b} := a^{\frac{b}{n}}$, he easily extends the rules of addition and multiplication of roots "by analogy" with their counterparts for integer exponents (1798, p. 389–394). Then, he notices that if $a < 0$, a^n is positive if n is even and negative if odd; while if $a \geq 0$, a^n is positive for any n. So regardless of what a is, a^n is always positive for even n. But then $\sqrt{-4}$, $\sqrt[4]{-a}$ and $\sqrt{-16}$ cannot be roots since by elevating them with $2, 4, 2$ respectively, we get a negative quantity. So '$\sqrt{-4}$', '$\sqrt[4]{-a}$' and '$\sqrt{-16}$', while they look meaningful, have no idea attached to them.

Interestingly, Condillac's argument is the same as the one D'Alembert gives in the entry "Imaginaire" in the *Encyclopédie* (1751). A puzzling question is why Condillac did not simply extend his claim that numerals refer to themselves to imaginary expressions? A possible answer is that, as a dedicated empiricist, he could not see what imaginary quantities would be an abstraction of. How could one form an idea of an imaginary quantity by composing and decomposing other ideas acquired through sensation? '$\sqrt{-4}$' was then bound to be meaningless, though Condillac still maintained that imaginary expressions ware useful in algebra and that one can find out how to properly manipulate them by analogy. These expressions are useful since "sometimes the calculation goes through these imaginary expressions, which vanish immediately;

and it leads, this way, to real results: this is what we will explain more specifically" (1798, p. 403). Making calculations and discovering truths about imaginaries in and of itself is an enterprise which Condillac never really considered, as they do not exist and their expressions are merely meaningless tools to obtain results about non-imaginary quantities. Sadly, Condillac died before fully developing his views about complex numbers.

In *La langue des calculs*, Condillac also exposes more clearly how analogy works in mathematics. He restates that analogy, a "method of instruction and invention," "of discoveries," is a non-arbitrary way to expand languages by a "relations [*rapport*] of resemblance" which, if chosen carefully, makes a language precise and clear (1798, p. 2–5, 232–233). Mathematics is the most exact science whose language, algebra, is built by analogy (1798, p. 6, 38). Accordingly, "it is the task of analogy to discover all the methods which can be invented," since "when the analogy of mathematicians lead, it leads well, and therefore it is only the task of analogy to lead them" (1798, p. 223). A crucial aspect of analogy is that it allows one to conclude "from the particular to the general" (1798, p. 133–115). For Condillac, we are forced to proceed from particular to general since "general truths are not the first ones to come to our mind [*connaissance*]." However, some more direct demonstrations are sometimes available and preferable.

Condillac gives a few examples of how analogy works in mathematics. He explains how the rules of multiplication are based on how names of numbers were created by analogy (1798, p. 18–20). '*douze*' ('twelve') in Latin means *dix plus deux* (ten plus two) so '*douze fois douze*' means *dix plus deux fois dix plus deux*, from which one can infer

$$12 \times 12 = (12 \times 10) + (12 \times 2) = ((10 \times 10) + (2 \times 10)) + ((10 \times 2) + (2 \times 2)).$$

Another example is $a^0 = 1$ (1798, p. 280–291). '$1a$', '$2a$', '$3a$' are like '1 man', '2 men', '3 men', that is, they indicate that 1, 2 and 3 multiply a. Thus, $1a = 1 \times a$, $2a = 2 \times a$ and $3a = 3 \times a$, and so $aa = a \times a$, $aaa = aa \times a$ and $aaaa = aaa \times a$. As these are all powers, we can then say that $aa = a^2$, $aaa = a^3$, $aaaa = a^4$. One easily sees by analogy that

$$a^3 \times a^4 = aaa \times aaaa = aaaaaa \times a = a^7 = a^{3+4}.$$

Concluding from this particular case to the general, $a^n \times a^m = a^{n+m}$. Since division is the opposite of multiplication, $a^n/a^m = a^{n-m}$. Thus, $a^1/a^2 = a^{1-2} = a^1 = 1/a$. By analogy, $a^0 = a^{1-1} = a^1/a^1 = 1$, even when $a = 0$.

In sum, Condillac's strong empiricist conception of analysis brought about a focus on abstraction in mathematics, departing from Descartes (1641, p. 387–388) and perhaps inspired by Euler's work (1744) (Fraser, 1997). His identification of reasoning with the use of a language made the language of algebra central to his accounts of mathematical reasoning and rigour. By assimilating abstract ideas with names, in particular, arithmetical quantities with numerals and indeterminate quantities with letters, he made algebra its own object, thereby paving the way for a formalistic — even better, an instrumental — understanding of mathematical reasoning. Mathematicians do not make calculations about objects in the world, but rather manipulate general signs in a mechanical way yielding results which, if properly done, can have many different physical interpretations Glas, 1986, p. 252–253. Imaginary quantities, in this picture, are not a subject of inquiry in their own but their expressions are tools of the formal machinery mathematicians use, though they differ from other expressions in that they do not stand for any idea. Moreover, Condillac thought that algebra was such a perfect language that by following its grammatical rules or by designing *new* grammatical rules based on resemblances with older ones, a mathematician could reason and discover new mathematical truths, sometimes from particular to general. In short, for him, mathematics is done in an instrumental way with a formal and general language whose grammar provides known and yet to be discovered inference rules. In retrospect, it is quite astonishing how much weight Condillac put on algebra as a language in his philosophy of mathematics.

3.4 The Mysterious Influence of Condillac on Laplace

Condillac's influence on French intellectuals around the Révolution is partly attested by his explicit impact on scientists like Lavoisier (1789) (Vincent, 2010). His influence also seems to have extended to the formation of the Écoles Normales in 1795, where according to Jahnke and M. Otte, (1979, p. 80) and Panteki, (2003, p. 287) who cite Albury, (1972, p. 27), 1400 copies of his *Logique* were distributed. Importantly, that many mathematicians engaged with some of his theses, from Lacroix's thoughtful discussion (1805, §2) to Gergonne's devastating criticisms against the "philosophical cult" of Condillac (1816; 1818) (Dalmedico, 1986), shows how wide-spread his influence was. More significantly, Condillac's influence on the French *idéologues*, in particular his founder Antoine Destutt de Tracy in his work *Logique* (1805), shows his importance in France at the turn of the century.

Did Laplace read Condillac? I have found no direct citation from Laplace of Condillac's work, which is both surprising and disappointing given that Laplace often cited other mathematicians and philosophers in his work. However, the following similarities between Condillac and Laplace's views and vocabulary suggests that either Laplace read him, or Condillac's influence was so pervasive that mathematicians held some of his views without knowing they came from him. Hypothesizing that Condillac had some influence on Laplace, we can better understand why he thought notational inductions were a means of discovery.

A first important similarity between Laplace and Condillac is their predilection for reasoning from particular cases to more general ones, from known to unknown, as opposed to starting from principles which one then applies to particular cases. While Condillac describes this opposition as between *analyse* and *synthèse*, Laplace, in his famous *Exposition du systême du monde* (1796; 1796), sometimes contrats *inductives* and *analytiques* versus *synthétiques* methods, also using '*analyse*' for what seems to be algebraic methods (1796, p. 286-290$_2$) Hahn, 1990, p. 377–378. Of Newton's *Principia Mathematica* (1686), an extremely important figure for him (Fox, 1974), Laplace says "we see in it the best applications of the method which consists in elevating oneself by a sequence of inductions, from the main phenomena to causes, and to then go back from these causes, to all the details of these phenomena." These general laws are discovered by observing what certain phenomena have in common; they are then verified "either by proofs or by direct experiences when possible, or by examining if they satisfy all known phenomena." As opposed to this "method, the most certain one which can guide us in the investigation of the truth," "ancient philosophers, following an opposing route, and putting themselves at the source of everything, imagined general causes to explain everything." Laplace even praises Bacon who established inductive methods "with all the strength of reason and eloquence, and which Newton has even more strongly recommended by its discoveries" and ridicules the "hopeless systems" of Leibniz, Malebranche and other philosophers, exactly as Condillac did. Clearly, Laplace's rigour or "presentation schema" does not echo the deductive Euclidean model, *contra* Hahn, (1990, p. 363) and Glas, (2002, p. 715–716).

Laplace seems to have rejected Condillac's radical formalist claim that arithmetic is concerned with numerals. But even if they disagreed in what quantities were, they both agreed that algebra is concerned with "abstract combinations" (1796, p. 288-290_2). Laplace reports that Newton might have obtained most of his theorems through analysis but still preferred presenting them in a synthetic manner because of his respect for the "geometry of the ancients." Doing so allowed Newton to link his general investigations about moving bodies in conic sections to the more specific research of the nature of curves done by the Ancients. This illustrates a crucial aspect of Laplace's understanding of the *analyse*/*synthèse* distinction.

> Geometric *synthèse* has in fact, the advantage of never losing sight of its object, and to enlighten the entire route which leads from the first axioms, to their last consequences; instead *analyse* makes us soon forget the main object, to occupy us with abstract combinations; and it is only at the end, that it brings us back to it. (1796, p. 290_1)

Synthèse is then neither general nor abstract since it is always, in some sense, interpreted in a specific subject matter, like physical bodies in space. This is why in doing *synthèse*, one does not loose sight of the subject about which they are reasoning. Laplace's description of *analyse*, on the other hand, is surprisingly similar to Condillac's instrumental depiction of calculation.

> But in isolating itself this way from objects, after having taken from them what was indispensable to arrive at the result we were seeking; in abandoning ourselves to the operations of *analyse*, and keeping all our strengths to overcome the difficulties that arise; we are lead by the power and the generality of this method, to results often inaccessible to *synthèse*. (1796, p. 290_1)

Abstraction, for Laplace, is then a significant advantage of *analyse* over *synthèse*. Delambre, in his *Rapport historique sur les progrès des sciences mathématiques depuis 1789 et sur leur état actuel* (1810, p. 46), highlights the necessity of these various "levels of abstraction" in *analyse* and speaks of Laplace as a mathematician who conciliated the rigour of the ancient geometers and these new methods. Because algebra played a greater role in the mathematical practice of the time, it is more appropriate to see both Condillac and Laplace as believing mathematics is concerned with abstract quantities, unlike Gray, (1992, p. 228–229).

A third similarity is Laplace's praise of the language of *analyse*, a language whose universality is such that "it suffices to translate a particular truth [in it]; to see emerge from their mere expressions, a host of new and unexpected truths" (1796, p. 290_2). Algebra does all of that in the most simple methods, as Condillac emphasized, so that after calculations one can construct a geometric interpretation. Laplace even shares Condillac's belief that algebra has a set of "fundamental expressions" out of which all the other ones are built. Hence, Laplace agrees with Condillac that in mathematics, one can and should move from known cases to general laws in large part with the help of the abstract and formal language of algebra, the best of all scientific languages.

In a more explicitly mathematical context, his *Leçons de mathématiques données à l'école Normale en 1795*, published in 1812, Laplace also clearly expressed his allegiance to this formal algebraic approach in a room full of students who in all likelihood had a copy of Condillac's *Logique*. Laplace states from the start that one goal of his lecture is "the road we ought to follow to elevate ourselves to new discoveries" (1795, p. 11). A clear preference for general methods of solving problems is also at the heart of his lessons, as Dhombres, (1992b, par. 38) also noticed. Especially in pedagogical contexts, Laplace says that one should always teach the most general methods, since "we give to students both knowledge and the method to acquire more knowledge," as a "system of pieces of knowledge linked between each other by a uniform method can better preserve and extend itself" (1795, p. 84).

Given that the object of algebra, for him, is "length conceived in the most abstract way," an "abstraction of the understanding" (1795, p. 11), algebra was bound to occupy an important place in Laplace's mathematics. In effect, Laplace repeats Condillac's point that algebraic quantities are the process of a double abstraction. He even tells a story similar to Condillac's about how people started counting and using numerals (1795, p. 15–17):

> To know the properties of bodies well we have first abstracted from their particular properties, and we have seen in them only a pictured [*figurée*] extension, movable and impenetrable. We have again abstracted from the two last general properties, by considering extension simply as pictured [*figurée*]. The many relations [*rapports*] it presents from this point of view are the object of Geometry. Finally, by an even greater abstraction, we have considered extension as nothing else than a quantity capable of increases and decreases; this is the object of the Science of lengths in general, or of the universal Arithmetic. (1795, p. 78)

Because of its generality, algebra is to be preferred to geometry (1795, p. 103). Moreover, it also reduces reasoning to "operations, which are, in a sense, mechanical" (1795,

p. 35). Laplace even describes the process of restituting these properties to quantities when one does mechanics, astronomy, optics, etc, making mathematics a formal device in which scientific inquiries are to be conducted Glas, 1986, p. 253. This method of "decomposing objects, and recomposing them to perfectly grasp their relations, is named *Analyses*," to which the human mind owes all its knowledge. Laplace even casually asserts Condillac's central doctrine that "knowledge can only perfect itself by the rapprochement of ideas fixed in the memory by signs [...] we have observed, in general, that all complex ideas are composed of simple ideas, combined together, following general modes" (1795, p. 15). As expected, the perfect language accordingly expresses the "greatest number of ideas by the smallest number of words."[10]

For example, take $\frac{a+b}{2}$. In Laplace's own words, this "algebraic quantity" is a "general expression" containing the "general characters" a and b which could be any number (1795, p. 33–34). That he saw a and b as being different from say 1 and 2 is manifest in that he takes time to re-explain addition and multiplication for algebraic quantities. In that vein, Newton's binomial theorem,

$$(a+b)^n = a^n + na^{n-1}b + \frac{n(n-1)}{1\cdot 2}a^{n-2}b^2 + \frac{n(n-1)(n-2)}{1\cdot 2\cdot 3}a^{n-3}b^3 + \cdots$$

states an equality between two abstract quantities, where n can be positive, negative or rational.

A way to reach general laws in any of these disciplines, including algebra, is of course induction, from particular to general, known to unknown. Laplace, perhaps having in mind Newton's October 24 (1676) letter to Oldenburg, explains how one can inductively infer general claims from particular ones by explaining how Newton discovered that equation in an instructive way, though false in the details but maybe true in spirit (Whiteside, 1961). He first verified it for $n = 2, 4, 8\ldots$ before then realizing the general pattern, thus inferring the "general law" for all positive numbers from "some particular cases" (1795, p. 41). Then, in virtue of the "algebraic language," Newton realized that his formula expressed "truths much more general than those we intended to make it express": it is also true of negative and rational numbers. Dhombres, (1992c, p. 30) has pointed out that L. Euler, 1774 made similar inductive inferences for the same theorem. Other kinds of analogy or induction that Laplace gives are Condillac's explanation of negative exponents, $a^{-3} = \frac{1}{a^3}$ and $a^0 = \frac{a^1}{a^1} = 1$

[10]Weirdly, Dalmedico, (1992, p. 101) wrongly attributes this passage to Lagrange.

(1795, p. 39), perhaps even when $a = 0$, and some forms of causal reasonings (1795, p. 151), again like Condillac. While he might not have been so confident in admitting all "general consequences given by analytical formulas," he believed that experience has shown they usually work, thus justifying notational inductions like the passage from the reals to the imaginaries on the basis of the Condillacian story about reasoning from known to unknown in an abstract and general language.

Importantly, again like Condillac, Laplace still said several times that these inductions needed "direct demonstrations of their results," (1810, p. 361), (1795, p. 41, 152), (1796, p. 286–287), (1814, p. 85). Direct demonstrations avoid "giving to the formulas more generality than they have." Hence, Laplace was also critical of notational inductions which he probably saw more as "means to approach certainty" (1814, p. 84). This echoes Condillac's repeated description of reasoning by analogy as means of discovery. Laplace's interest for discoveries in mathematics was not innocent, *contra* Glas, (1986, p. 258), but reflected his "biggest amusement[:] studying the march of inventors, seeing their genius struggling with the obstacles they faced and overcame" (1777, p. 348). Interestingly, we here witness an 18$^{\text{th}}$ century articulation of the distinction between context of discovery and justification.

Laplace's imaginary numbers are textbook Condillacian. Starting with the equation

$$3x - x^2 = 2, \tag{3.6}$$

Laplace divides both sides by -1, completes the perfect square of the newly obtained left hand side and squares both sides to get

$$x - \frac{3}{2} = \pm\sqrt{\frac{9}{4} - 2}.$$

One then easily gets that $x = \frac{3}{2} \pm \frac{1}{2}$. If one changes slightly (3.6) to

$$3x - x^2 = 3,$$

one gets

$$x - \frac{3}{2} = \pm\sqrt{\frac{9}{4} - 3}, \qquad \text{or} \qquad x = \frac{3}{2} \pm \frac{\sqrt{-3}}{2}.$$

But the quantity $\sqrt{-3}$ is "impossible; because a real number, positive or negative, cannot have as square a negative number; the problem which lead to these values is thus impossible" (1795, p. 45). These values are imaginaries and are extremely useful in analysis: as Condillac also said, "often real lengths present themselves in the form of many imaginaries, in which everything that is imaginary destroys itself mutually."

An example of the usefulness of imaginaries mutually destroying themselves is the following (1795, p. 104–105). Building on the fundamental theorem of Trigonometry,

$$\sin(x+y) = (\sin x)\cdot(\cos y) + (\sin y)\cdot(\cos x),$$

one easily gets that

$$(\cos x \pm \sqrt{-1}\sin x)\cdot(\cos y \pm \sqrt{-1}\sin y) = \cos(x+y) \pm \sqrt{-1}\sin(x+y).$$

Thus, by some kind of induction, if we make y "successively equal to $x, 2x, 3x \ldots$,"

$$(\cos x \pm \sqrt{-1}\sin x)^n = \cos nx \pm \sqrt{-1}\sin nx. \tag{3.7}$$

This formula, "one of the most useful of Analysis, has, like the binomial, the advantage of extending to values of n, integers and fractions, positive and negative, irrational and even imaginary." An example is that any function of sinus and cosinus of an angle x can be developed into a function of sinus and cosinus of x's multiples in this way. Suppose $f(x)$ is such a functions. Replace $\cos x$ in $f(x)$ by

$$\frac{1}{2}\left(\cos x + \sqrt{-1}\sin x\right) + \frac{1}{2}\left(\cos x - \sqrt{-1}\sin x\right)$$

and $\sin x$ by

$$\frac{1}{2\sqrt{-1}}\left(\cos x + \sqrt{-1}\sin x\right) - \frac{1}{2\sqrt{-1}}\left(\cos x - \sqrt{-1}\sin x\right).$$

One can then develop the newly obtained function by multiplying the various products of

$$(\cos x + \sqrt{-1}\sin x) \qquad \text{and} \qquad \cos x - \sqrt{-1}\sin x.$$

For our purpose, Laplace shows how imaginaries can enter a calculation and disappear at the end, like useful tools not contaminating the result who remains real (1795, p. 113–114). Probably on purpose, the example he gives is one of Girolamo Cardano's, a physician (Giglioni, 2013), philosopher and polymath, who published results on cubic functions in his famous *Ars Magna* (1545) already obtained by Niccolò Tartaglia (and thus sparked perhaps one of the most overblown mathematical controversy (Rothman, 2014)) — see Confalonieri, 2015 for Cardano's work and Rommeveaux-Tani, 2016 for its reception. Consider the third degree equation

$$x^3 \pm px + q = 0 \tag{3.8}$$

where p is positive. Supposing $x = r(z \pm \frac{1}{z})$, Laplace gets

$$x^3 \pm px + q = r^3 \left(z^3 \pm \frac{1}{z^3} \right) \pm (3r^3 - pr) \left(z \pm \frac{1}{z} \right) + q = 0.$$

Letting $r^2 = \frac{1}{3}p$ and $-\sqrt{\frac{27q^2}{p^3}} = 2h$,

$$z^3 \pm \frac{1}{z^3} = 2h.$$

If we take $\pm$ to be $-$ and $|h| < 1$, $\frac{1}{4}q^2 - \frac{1}{27}p^3 < 0$ and the equation is irreducible. If $z = \cos u + \sqrt{-1} \sin u$, we get

$$x = \sqrt{\frac{p}{3}} \left(z + \frac{1}{z} \right) = 2\sqrt{\frac{p}{3}} \cos u$$

thus,

$$z^3 + \frac{1}{z^3} = 2 \cos 3u$$

and so $\cos 3u = h$. If A is the smallest angle for which $\cos A = h$, we get three possible values of x.

$$x = 2\sqrt{\frac{p}{3}} \cos \frac{1}{3} A$$
$$x = 2\sqrt{\frac{p}{3}} \cos \left(\frac{2c + A}{3} \right)$$
$$x = 2\sqrt{\frac{p}{3}} \cos \left(\frac{4c + A}{3} \right).$$

In Laplace's own words, "notice here the usage of imaginary quantities to determine real quantities; the value of z expresses the imaginary radical from which the root of the equation of third degree is composed in the irreducible case; but under this form, we clearly see that the imaginaries disappear from the expression of $r(z + \frac{1}{z})$, which is equal to x." Calculations with $\cos x \pm \sqrt{-1} \sin x$ have made it easier for *analystes* to use imaginaries in *analyse* even if these quantities embarrassed them at first. Note that Laplace's strategy of expressing z with the complex trigonometric function is not unlike his debated move of expressing the real variable of integration x by a complex function.

We then see that Laplace shared Condillac's views about how complex ideas and reasoning more generally is performed by de/composing ideas, by *analyse*. Moreover, for him too reasoning is done by manipulating symbols in an instrumental way, linking mathematical computations to the use of algebra. The language of algebra, in its abstractness, generality and simplicity, is then the designated way to get results that can be interpreted in many more specific scientific inquiries, like geometry and astronomy. In this perfect language, inferences from known particular cases to general laws and inferences by analogy are permissible as means of discoveries in need of more direct proofs later. Since Laplace saw a strong connection between mathematical reasoning and the use of the abstract language of algebra, he believed algebraic moves involving the introduction of imaginaries destroying themselves at the end of a calculation, like the ones above, were legitimate and fruitful means of discovery. Moreover, they made imaginaries — these "impossible quantities" — tools for analysis, like for Condillac, as opposed to a proper subject of inquiry. The universality of algebra is thus not the "foundation" of Laplace's analysis Koetsier, 1991, p. 210, but a consequence of his views on the language of mathematics.

In a sense, Freudenthal, (1971, p. 377), Ferraro, (2001, p. 538–539), Schubring, (2005, p. 365–368) and Ferraro, (2007, p. 72–73) have rightly pointed out that Laplace and other algebraists inferred that what held of real numbers would also hold of imaginary numbers because of their view that algebra is carried out in a general language whose object is abstract quantities. But this would be to mistakenly suggest that Laplace was interested in making true claims about imaginaries, which could not *possibly* be the case since these were *impossible* quantities. Similarly, Fraser, (1989, p. 329–330) gets only part of the story by talking only about the abstract "object" of 18$^{\text{th}}$ century mathematics: it is not merely because the object of mathematics is abstract that imaginary substitutions are legitimate. In fact, Laplace's views of algebra and imaginary numbers are even more radical than most commentators seem to realize: it is not that he did not fully appreciate how they differ from real numbers because algebra is about abstract quantities; rather, Laplace thought algebra, being concerned with abstract quantities, was such an abstract and general language that one could use meaningless expressions about impossible quantities in a calculation to discover truths about real quantities.

Laplace did not ignore the question of the scope of mathematical formulas. However, using imaginary numbers as tools forced him to write equations containing them, when he expresses quantities as a complex function, e. g. $z = \cos u + \sqrt{-1}\sin u$, or when he extends a theorem like $(\cos x \pm \sqrt{-1}\sin x)^n = \cos nx \pm \sqrt{-1}\sin nx$ to the imaginary context. Laplace never tells us what these statements mean and rarely gives us good argument for why we should think they are true. We see a key characteristic of Laplace's use of formulas: he was quite liberal in what could figure in their scope, especially in the course of a calculation. This attitude differs greatly from Cauchy, as Belhoste, (1991, p. 219) also remarks: while one has a section of his book titled "Application of the Calculus of Probability to the moral sciences" (1820, p. lxxviii), the other one warns that "let us not imagine that one can attack history with formulas, nor give as sanction to morality some theorems of algebra or integral calculus" (1821, p. vij).

To better contrast Cauchy and Laplace's views, I will now look at Cauchy's early work in complex analysis. Once we better appreciate how they differ, the goal will be to see why Cauchy objects to Laplace's imaginary substitutions. The answer, it turns out, will highlight strikingly different approaches to how mathematicians should read their formulas.

4 The Metaphysics of Imaginaries

4.1 Symbolic Expressions in Analysis

Cauchy's first work in complex analysis figures in *Mémoire sur les intégrales définies* (1814), where he proves amongst other things that the integral of an analytic function of a complex variable along a closed path depends on how the function behaves at points of discontinuity (Ettlinger, 1922). The paper, though it voices a criticism of Laplace's passage, contains no remark or original thoughts about imaginary quantities *per se*; Cauchy even refrains from using them in his reasoning, preferring to separate $f(z)$ where z is complex into $\phi(x)$ and $\chi(y)$, where $f(z) = f(x + y\sqrt{-1}) = \phi(x) +$

$\chi(y)\sqrt{-1}$. As we will see, this is typical of Cauchy's early work in complex analysis.

To compare Cauchy to Laplace, however, first looking at his *Cours* is more instructive since it reveals how Cauchy thought and reasoned with imaginary quantities. At first, there is no striking difference between Laplace and Cauchy's views about imaginary quantities, since the former saw them as "impossible quantities" useful to obtain results about real quantities when they "mutually destroy themselves" (1795, p. 41); and the other did not say what they were but specified that imaginary expressions like '$\alpha + \beta\sqrt{-1}$' are *symbolic expressions*, that is, "combinations of algebraic signs which are meaningless by themselves" (1821, p. 153). Cauchy also states that they work as "a tool, an instrument of calculation" from which one can deduce exact results (1847, p. 94). He thus evades the difficult question of what imaginary numbers are by an usage of the use-mention distinction, differing from Laplace but in clear agreement with Condillac.

However, for Cauchy, equations in which imaginary expressions figure are still meaningful in that they are the "symbolic representation of equations between real quantities" (1821, p. 155). Thus, a statement like $\alpha + \beta\sqrt{-1} = \gamma + \delta\sqrt{-1}$ states the equalities $\alpha = \gamma$ and $\beta = \delta$. In this regard, Cauchy departs significantly from Laplace and Condillac. For at this point, Cauchy can make *true* claims in complex analysis, which was, strictly speaking, impossible for Condillac and Laplace. That Cauchy took time to make this small step suggests a shift from the perspective of imaginary numbers as tools to imaginary numbers as a proper subject of inquiry.

To illustrate the usefulness of imaginary expressions and how imaginary identity claims work, Cauchy also uses the following formulas for the sinus and cosinus of the arc $a + b$

$$\begin{aligned} \cos(a+b) &= \cos a \cdot \cos b - \sin a \cdot \sin b \\ \sin(a+b) &= \sin a \cdot \cos b + \sin b + \cos a. \end{aligned} \tag{4.1}$$

A useful way to obtain (4.1) is by multiplying the two following symbolic expressions

$$M := (\cos a + \sqrt{-1}\sin a) \cdot (\cos b + \sqrt{-1}\sin b),$$

"by operating[11] according to the known rules of algebraic multiplication, as if $\sqrt{-1}$

[11] Bradley and Sandifer, (2000, p. 118) translate '*en opérant*' as "by applying," departing from Cauchy's language for no particular reason.

were a real quantity the square of which is equal to -1." The product resulting from this multiplication will have one real part and one part multiplying $\sqrt{-1}$. The real part will give the value of $\cos(a+b)$ and the coefficient of $\sqrt{-1}$ that of $\sin(a+b)$. Proceeding, we get

$$\begin{aligned} M &= \cos a \cdot \cos b + \cos a \cdot \sqrt{-1}\sin b + \sqrt{-1}\sin a \cdot \cos b + \sqrt{-1}\sin a \cdot \sqrt{-1}\sin b \\ &= \cos a \cdot \cos b + \sqrt{-1}(\cos a \cdot \sin b + \sin a \cdot \cos b) - \sin a \cdot \sin b \\ &= \cos a \cdot \cos b - \sin a \cdot \sin b + \sqrt{-1}(\cos a \cdot \sin b + \sin a \cdot \cos b). \end{aligned}$$

The two components of this last equation give the right hand-side of the equalities (4.1). From these equalities, we can conclude the imaginary equation

$$\begin{aligned} &\cos(a+b) + \sqrt{-1}\sin(a+b) = \\ &\qquad \cos a \cdot \cos b - \sin a \cdot \sin b + \sqrt{-1}(\cos a \cdot \sin b + \sin a \cdot \cos b), \end{aligned} \tag{4.2}$$

but we *cannot* conclude

$$\cos(a+b) + \sqrt{-1}\sin(a+b) =^{*} (\cos a + \sqrt{-1}\sin a) \cdot (\cos b + \sqrt{-1}\sin b), \tag{4.3}$$

for this equation, "taken literally, is not exact and has[12] no meaning."

Hence, imaginary expressions, though meaningless, can somehow lead to correct results. In any case, it is important to realize that in doing all of this, Cauchy has specified, in a sense, the *subject* of complex analysis: while real equations are about real quantities, i. e. increases or decreases in absolute measures of length (1821, p. 2), imaginary ones "represent no more than relations between real quantities" (1821, p. 155). Moreover, his specification is reductive since both imaginary and real expressions have the same subject matter, as Schubring also noticed (2005, p. 449).

One might wonder why Cauchy chose this specific derivation as example of the usefulness of imaginary expressions. As we saw, Laplace mentions it in his leçons 8 (1795, p. 104–105) — see Dhombres, (1980, p. 338–339). Argand, (1806) also makes that deduction in providing a competing account of imaginary quantities; and Servois, (1813), opposing Argand's account, proposes an algebraic proof very close to that of Cauchy. Thus, this result seems to have been a classic of complex analysis at the time.

[12] Bradley and Sandifer, (2000, p. 118) imprecisely translate '*n'a pas de sens*' as "does not make sense."

Most treatments of imaginary numbers prior to Cauchy (except Suremain-Missery (1801), Argand (1806) and Buée, 1806, which I discuss below) focus exclusively on solving polynomials, for example Cousin's *Traité* (1796) and Lacroix's *Compléments* (1799) Bottazzini, 1994, p. 420, Bottazzini and Gray, 2013, p. 126. Bougainville's *Traité du calcul intégral* (1754), intended as a sequel to L'Hospital's *Analyse des infiniments petits* (1696), is the only one I know that fixes identity-conditions for imaginary numbers. As a whole, the *Traité* strikes one for its remarkable organization, with definitions, theorems, lemmas and corollaries in the Eulerian tradition. Its chapter V, "Sur les imaginaires," mostly consists in a version of d'Alembert's proof (1746) of the fundamental theorem of algebra, closely mirroring d'Alembert's reasoning (Baltus, 2004, p. 417–418). Bougainville, like Cauchy, begins by establishing the identity-conditions for imaginary numbers, though he does it with the following argument. If $\alpha+\beta\sqrt{-1}=\gamma+\delta\sqrt{-1}$, then $\alpha=\gamma$ and $\beta=\delta$ (1754, p. 40–41). In effect, this equality entails that $(\alpha+\beta\sqrt{-1})-(\gamma+\delta\sqrt{-1})=0$, so $(\alpha-\gamma)+(\beta-\delta)\sqrt{-1}=0$ and thus $\alpha-\gamma=0$ and $\beta-\delta=0$. Otherwise, $(\alpha-\gamma)=-((\beta-\delta)\sqrt{-1})$, which means a real number is equal to an imaginary number, "which is absurd." Somehow, Bougainville did not think the other direction needed a proof.

After having defined identity-conditions for imaginaries, Bougainville immediately starts proving two important complex theorems of the time: that any imaginary number ζ is equal to another imaginary $\alpha+\beta\sqrt{-1}$, where α and β are real (1754, p. 41); and the fundamental theorem of algebra, that any polynomial without real roots has an imaginary root (1754, p. 52). That he takes time to show the two results independently shows how attuned to the most recent development in mathematics Bougainville was; for the most brilliant mathematicians at that time had only recently clearly distinguished the two, starting from D'Alembert himself in his 1746 proof Gilain, 1991, p. 113–114. Be that as it may, these two theorems were not proved because they were interesting in their own: for D'Alembert, they were a crucial part of his theory of algebraic equations, in relation to the integration of rational fractions Gilain, 1991, p. 113–117. Bougainville himself has his chapter on imaginaries in the introduction of his work, as a tool for solving integrals later on.

Cauchy's statement of identity-conditions for imaginary numbers is, on the other hand, a crucial and important step in the study of imaginary numbers themselves, as we will see. Since his *Cours* is one of the first publications attempting to do so with Argand's *Essai*, I turn to the second to compare them both and better appreciate

Cauchy's approach to complex analysis.

4.2 Argand's Complex Analysis

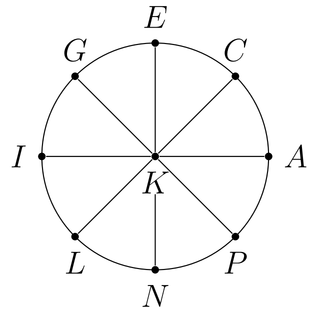
As he himself notices (1847, p. 157), Cauchy's reading of imaginary expressions in his *Cours* is drastically different than that offered earlier by Argand in his *Essai sur une manière de représenter les quantités imaginaires dans les constructions* (1806), which sparked many serious discussions seven years later involving Français, 1813a; Français, 1813b; Français, 1813c, Gergonne, 1813b, Argand, 1813; Argand, 1814 and Servois, 1813.[13] Similarly to Wessel, 1799 and Buée's much more detailed discussion (1806), the main innovation of Argand's account is his reliance on the notion of *direction* (Schubring, 1997), (1998, p. 127), where '$+1$' and '-1' represent movements in opposite directions $-/+$ but of the same absolute length 1. He notices the following equalities of ratios (1806, p. 6–10).

$$+1 : +1 : : -1 : -1$$
$$+1 : -1 : : -1 : +1,$$

Since both the first and the second equalities of ratio agree in their positive or negative sign, Argand then asks whether there is an x such that $+1 : +x :: +x : -1$. To find it, he proposes to still use the notion of *direction* the following way. Let K be a fixed point in the middle of a circle through which four lines AI, CL, EN, GP pass, thereby dividing the circle in eight equal portions. If $\overline{KA}$ is the positive unit, whose absolute length is KA, then $\overline{KI}$ is the negative unit. It follows that there is a $\overline{KE}$ that is perpendicular to $\overline{KA}$ and $\overline{KI}$ such that $KE = KA = KI$ and the direction of $\overline{KA}$ is to $\overline{KE}$ what the direction of $\overline{KE}$ is to $\overline{KI}$. This condition is also satisfied by $\overline{KN}$, which is why we can call them $\overline{KE} = +\sqrt{-1}$ and $\overline{KN} = -\sqrt{-1}$.

$\sqrt{-1}$ then becomes a "unit" with algebraic properties like '$\pm$' indicating a direction in expressions of the form $\pm a\sqrt{-1}$ (1806, p. 13). Defining addition and multiplication for these lines, Argand even derives the series expansions for trigonometric functions (1806, p. 26–27). Interestingly, Argand also derives (4.1) in the following way (1806,

[13]The story behind this fascinating dialogue is amusing. Français' initial publications end with the confession that some key ideas of his mémoire were mentioned in a letter Legendre sent to his deceased brother. Argand then noticed that he was probably the author of this letter, which Gergonne and François acknowledged after receiving a copy of his *Essai*. See Schubring, (1997, p. 11–12) for Legendre's letter to Français.

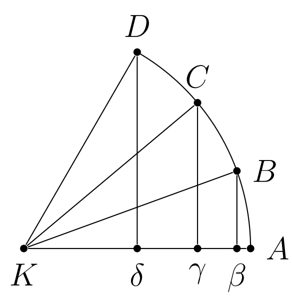


Argand's Imaginaries

p. 28). Let A, B, C, D be on an arc of center K such that $AB = a, AC = b$ and $CD = AB$, where δ, γ, β are the points where D, C, B would respectively meet KA perpendicularly. We get $\overline{KD} = \overline{KB} \times \overline{KC}$, as Argand has already proven. Since

$$\overline{KD} = \overline{K\delta} + \overline{\delta D} = \cos(a+b) + \sqrt{-1}\sin(a+b)$$
$$\overline{KB} = \overline{K\beta} + \overline{\beta B} = \cos a + \sqrt{-1}\sin a$$
$$\overline{KC} = \overline{K\gamma} + \overline{\gamma C} = \cos b + \sqrt{-1}\sin b$$

Argand deduces Cauchy's "nonsensical" statement (4.3), from which he infers (4.2) and then easily gets (4.1).



Argand's Derivation

While Cauchy presents his derivation as a trick to remember (4.1), Argand sees his own as a proof of it. One important reason why that is is that Cauchy sees imaginary expressions as meaningless but useful, while Argand tells a story of what '$\sqrt{-1}$' means, which allows him to explain *why* (4.1) holds. This has led some to criticize Cauchy as treating imaginary expressions like meaningless symbols which "lead, by a short and certain, but obscure and mysterious way, to the results which one could have reached, by the use of proper quantities only" Hoüel, 1874, p. ix–x. A century before Houël, Playfair had already discussed this "paradox" about imaginary quantities (1778, p. 318–321).

Thus, an important difference is that Cauchy interprets imaginary equations as stating *algebraic* properties of pairs of real numbers, while Argand interprets them as stating *geometric* relations between lines in a two-dimensional setting. The rigour of Cauchy's complex analysis in his *Cours* is not geometric in the sense of being built on geometric notions or intuitions like Argand's; neither is his real analysis from which he draws, as Boyer, (1949, p. 273) argued, *contra* Jourdain, (1905, p. 206), Sinaceur, (1973, p. 107), Barany, (2011, p. 374–375), (2013) and Segura and Sepulcre, (2016, p. 209). Curiously, Jourdain, Sinaceur and Barany only cite Cauchy's proof of the intermediate value theorem (1821, p. 51–52) as evidence for their claim. But on the same page, Cauchy explicitly refers the reader to his "purely analytical" and clearly non-geometric proof of the same theorem in Note III — see Grabiner, (1981, p. 69–75) for discussion of that proof. Similarly, in his *Mémoire sur la détermination du nombre des racines réelles dans les équations algébriques* (1815), Cauchy first describes the general ideas of his proofs in geometric terms before carrying them purely algebraically to obtain general results; and a report by Legendre (1814) suggests that was also Cauchy's strategy in 1813.

But given that Argand's discussion of positive and real numbers being directions of absolute measures is strikingly similar to Cauchy's; and that they both derived of (4.1); historians like Dalmedico, (1997, p. 30) have naturally wondered if Cauchy had read Argand and, if so, why he did not interpret imaginary numbers in this geometric setting. The question is even more pressing since Schubring, (2005, p. 446) is right that Cauchy's understanding of quantities is the same as Buée, 1806 and, as Lacroix, 1813 pointed out, Buée himself came up with an account of imaginary numbers similar to Argand's the same year!

One main difference between Argand and Cauchy is that the former sees his treatment merely as a "way to represent" imaginary quantities, as a "*means of research*" (1806, p. 60) Lewis, 1994, p. 724. As he explained later, the goals of Argand's *Essai* were to firstly "give an intelligible meaning to expressions which we were forced to accept in Analysis, but which we did not think possible until now to reduce [*rapporter*] to any known and evaluable quantity;" and secondly, "offer a method of calculation or, if one wants, a notation of a particular kind, which employs geometric signs and ordinary algebraic signs at the same time" (1814).

Around the time of his *Cours*, Cauchy, on the other hand, *is not* concerned with interpreting '$\sqrt{-1}$' but with developing codifying complex reasonings and developing complex notions, e. g. limits, functions, series, differentials, integrals, etc. The general orientation he would take is that of reducing them to combinations of their real counterparts. So their different goals partly explain their different methods. It is in that light that we should distinguish them, as I will illustrate in the following section.

5 Beyond Metaphysics: Mathematics

5.1 Two Proofs of the Fundamental Theorem of Algebra

To really appreciate the orientation and rigour that Cauchy's early work displayed, we need to look closely at how he uses in his proofs the various concepts he defines. The following two proofs illustrate even more the different orientations Argand and Cauchy took in developing their complex analysis. Argand gave a first version of his proof in (1806) and then an improved version (1814). For Petrova, (1974, p. 259) and Remmert, (1991, p. 108–109), Argand "simplifies astonishingly the application of D'Alembert's basic idea" in (1746): minimizing the absolute value of the polynomial by carefully choosing its argument. The proof clearly illustrates his geometric reasoning with imaginary quantities. Take the following polynomial

$$y_x = x^n + ax^{n-1} + bx^{n-2} + \cdots + f + g,$$

where n is an integer and $a, b, \ldots, f, g$ are imaginary. Argand proposes to show that there is an x for which $y_x = 0$. In his geometric setting, the polynomial expresses a line $\overline{KP}$ where P moves depending on x. Thus, Argand's proofs amounts to show that for a value of x, "P coincides with K."

If there were no such x, then of all values of KP, there is one, given by z, that is smaller than all others. Which means that it cannot be the case that $y'_{(z+i)} < y'_z$. Developing $y_{(z+i)}$, Argand gets

$$\begin{aligned} y_{(z+i)} = y_z &+ \left[nz^{n-1} + (n-1)az^{n-2} + \cdots + f\right] \cdot i \\ &+ \left[\frac{n}{1} \cdot \frac{n-1}{2} \cdot z^{n-2} + \cdots\right] \cdot i^2 + \cdots + (nz+a) \cdot i^{n-1} + i^n. \end{aligned} \tag{5.1}$$

More generally, (5.1) can be seen as

$$y_{(z+i)} = y_z + Ri^r + Si^s + \cdots + Vi^v + i^n. \tag{5.2}$$

If all coefficients are 0, then we are done. Supposing at least three of them are not 0, we get

$$\overline{KP} = y_z, \quad \overline{PA} = Ri^r, \quad \overline{AB} = Si^s, \quad \ldots, \quad \overline{FG} = Vi^v, \quad \overline{GH} = i^n.$$

Clearly, $y_{(z+i)}$ can be represented as $\overline{KPAB\ldots FGH}$ or $\overline{KH}$. Argand needs to show that $KH < KP$ so that $y_{(z+i)} < y_z$, contradicting his assumption.

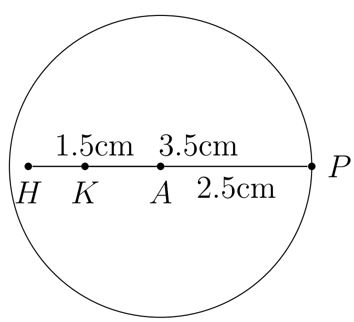
Argand then varies i "in a way such that the point A falls in between K and P." He changes i's direction so that $\overline{PA}$ runs opposite to $\overline{KP}$ and then its length so that $PA < KP$. By reducing i's length even more, he gets the inequality

$$Si^s + \cdots + Vi^v + i^n < Ri^r$$

which is possible since $r < s < \cdots < v < n$. It follows that

$$AH \leq AB + \cdots + EF + GH < PA.$$

Thus, $AH < AP < KP$. If we draw a circle with center A and radius AP, H will be in it, which means that $KH < KP$. As $\overline{KH}$ runs opposite to $\overline{KP}$, H is closer to K than P, contradicting our assumption that P cannot get arbitrarily close to K. Argand even invites us to draw a figure to follow his proof.



Argand's Proof

Servois, (1813, p. 231) objected to Argand that "it is not enough, it seems, to find values of x which give to the polynomial decreasing values; the law of the decreasing [*décroissemens*] needs to bring necessarily the polynomial to zero." *Contra* Remmert, (1991, p. 109), Argand was aware of this criticism and guessed that Servois had something like $y = 1/x$ in mind (1814, p. 208). He answered by citing an argument by Gergonne, (1813a, p. 355) and pleading that "this circumstance does not hold in our demonstration; since, it is clearly not by an infinite value of x that we will make null the polynomial y_x." In modern terminology, Argand presupposed that a continuous function attains its minimum on a compact set (Remmert, 1991, p. 109), (Bottazzini and Gray, 2013, p. 129).

Cauchy gave two proofs of the same theorem: one (1817) which he himself acknowledges is similar to Gauss, 1799 and one (1817; 1820) some have claimed is similar to Argand's, which Cauchy "had only reproduced later in a purely analytical form, but less striking" according to Hoüel, (1874, p. viii) (and perhaps Remmert, 1991, p. 109). While Cauchy, (1820b, p. 411) (1821, p. 291) credits A. M. Legendre, (1797, chap. XIV), not Argand, Gilain, (1997, p. 57) points out that Legendre's proof was similar to Argand's, whose manuscript we know he read as described in a footnote above.

Nonetheless, Cauchy's proof is very different. He motivates the proof by pointing out that in earlier proofs "we payed a special attention to the degree of the equation given [...] [t]hese considerations seeming to me foreign to the question, I thought the theorem in question would depend only on the form of the two real functions produced by the substitution of an imaginary value of the variable in a polynomial" (1817, p. 217), (1820, p. 411). To better appreciate the difference between Cauchy and Argand, here is a similar version of Cauchy's second proof he gave in his *Cours* (1821, p. 275–281). Let $f(x)$ denote the above polynomial. A root would be of the form $f(u + v\sqrt{-1}) = 0$ and thus

$$\phi(u, v) + \sqrt{-1}\chi(u, v) = 0,$$

where ϕ and χ are real functions. This is equivalent to saying that $[\phi(u, v)]^2 + [\chi(u, v)]^2 = 0$, thus to find suitable real numbers u, v such that

$$F(u, v) = [\phi(u, v)]^2 + [\chi(u, v)]^2 = 0.$$

To determine $F(u, v)$, Cauchy expresses the coefficients of f in their corresponding module[14] expression.

$$
\begin{aligned}
a_0 &= \rho_0(\cos\theta_0 + \sqrt{-1}\sin\theta_0),\\
a_1 &= \rho_1(\cos\theta_1 + \sqrt{-1}\sin\theta_1),\\
&\dots\\
a_{n-1} &= \rho_{n-1}(\cos\theta_{n-1} + \sqrt{-1}\sin\theta_{n-1}),\\
a_n &= \rho_n(\cos\theta_n + \sqrt{-1}\sin\theta_n),\\
u + v\sqrt{-1} &= r(\cos t + \sqrt{-1}\sin t).
\end{aligned}
$$

It follows that

$$
\begin{aligned}
f(u+v\sqrt{-1}) =& \rho_0 r^n \left[\cos(nt+\theta_0) + \sqrt{-1}\sin(nt+\theta_0)\right]\\
&+ \rho_1 r^{n-1}\left[\cos((n-1)t+\theta_1) + \sqrt{-1}\sin\left((n-1)t+\theta_1\right)\right]\\
&+ \cdots\\
&+ \rho_{n-1} r\left[\cos(t+\theta_{n-1}) + \sqrt{-1}\sin\left(t+\theta_{n-1}\right)\right]\\
&+ \rho_n\left(\cos\theta_n + \sqrt{-1}\sin\theta_n\right).
\end{aligned} \tag{5.3}
$$

From (5.3), Cauchy expresses the corresponding equalities for $\phi(u,v), \chi(u,v)$ and $F(u,v)$.

$$
F(u,v) = r^{2n}\left[\rho_0^2 + \frac{2\rho_0\rho_1\cos(t+\theta_0-\theta_1)}{r} + \frac{\rho_1^2 + 2\rho_o\rho_2\cos(2t+\theta_0-\theta_2)}{r^2} + \dots\right]
$$

Since $F(u,v)$ is the product of two factors, one being $r^{2n} = (u^2+v^2)^n$, it increases indefinitely as u or v also increases. It is also clearly continuous. Cauchy supposes that A is the lower limit of F for u_0 and v_0. Thus, for α being an infinitely small quantity,

$$
0 < F(u_0+\alpha h, v_0+\alpha k) - F(u_0,v_0).
$$

With this in mind, Cauchy sets out to find A. He writes the expansion of $f(u_0 + \alpha h, v_0 + \alpha k)$ this way

$$
f(u_0+\alpha h, v_0+\alpha k) := R(\cos T + \sqrt{-1}\sin T)
$$

[14]Smith, (1958, p. 267) wrongly believes Cauchy is the first in his *Cours* to use the word 'module' in that sense: Argand did earlier (1814, p. 208) Cartan, 1908, p. 346. See also Bradley and Sandifer, (2000, p. 123).

$$
\begin{aligned}
&+ \alpha R_1 \rho \left[\cos(T_1 + \theta) + \sqrt{-1} \sin(T_1 + \theta)\right] \\
&+ \cdots \\
&+ \alpha^n R_n \rho^n \left[\cos(T_n + n\theta) + \sqrt{-1} \sin(T_n + n\theta)\right]
\end{aligned}
$$

which gives him $\phi(u_0 + \alpha h, v_0 + \alpha k)$, $\phi(u_0 + \alpha h, v_0 + \alpha k)$ and thus

$$
\begin{aligned}
&F(u_0 + \alpha h, v_0 + \alpha k) \\
&\qquad = \left[R \cos T + \alpha R_1 \rho \cos(T_1 + \theta) + \cdots + \alpha^n R_n \rho^n \cos(T_n + n\theta)\right]^2 \\
&\qquad + \left[R \sin T + \alpha R_1 \rho \sin(T_1 + \theta) + \cdots + \alpha^n R_n \rho^n \sin(T_n + n\theta)\right]^2 .
\end{aligned} \tag{5.4}
$$

If $\alpha = 0$, Cauchy notices that $F(u_0, v_0) = A = R^2$, which allows him to rewrite (5.4) by replacing R for $\sqrt{A}$ and to then conclude

$$
\begin{aligned}
&F(u_0 + \alpha h, v_0 + \alpha k) - F(u_0, v_0) \\
&= 2\sqrt{A} \alpha \rho \left[R_1 \cos(T_1 - T + \theta) + \cdots + \alpha^{n-1} \rho^{n-1} R_n \cos(T_n - T + n\theta)\right] \\
&+ \alpha^2 \rho^2 \{\left[R_1 \cos(T_1 + \theta) + \cdots + \alpha^{n-1} \rho^{n-1} R_n \cos(T_n + n\theta)\right] \\
&\quad + \left[R_1 \sin(T_1 + \theta) + \cdots + \alpha^{n-1} \rho^{n-1} R_n \sin(T_n + n\theta)\right]\}
\end{aligned} \tag{5.5}
$$

Since $0 < F(u_0 + \alpha h, v_0 + \alpha k) - F(u_0, v_0)$, the second factor of (5.5) must not be smaller than 0, thus, its first non-negative coefficient R_m must not be smaller than 0, for α decreases as its exponent increases. For that R_m, the first and second factors will be

$$
2\sqrt{A} \alpha^m \rho^m R_m \cos(T_m - T + m\theta), \qquad \alpha^{2m} \rho^{2m} R_m^2.
$$

Since we can make $\cos(T_m - T + m\theta) < 0$ and thus the first factor smaller than 0, to prevent $0 < F(u_0 + \alpha h, v_0 + \alpha k) - F(u_0, v_0)$, we are forced to conclude that $A = 0$, which means that $F(u_0, v_0) = 0$. Thus, taking $x = u_0 + v_0\sqrt{-1}$, we conclude that $f(u_0 + v_0\sqrt{-1}) = 0$.

Cauchy's argument also relies on the assumption that since $F(u, v)$ is continuous, it attains its minimum on a compact set. But it differs from Argand's proof in an important way. Cauchy's strategy, trying to solve the functions $F(u, v)$ built with two real functions $\phi(u, v)$ and $\chi(u, v)$ obtained from $f(u + v\sqrt{-1})$ is different than Argand's attempt to make the point P coincide with K. Sure, both attempt to minimize the absolute value of the polynomial by manipulating its arguments after expanding for $f(z + i)$ or $F(u_0 + \alpha h, v_0 + \alpha k)$. But the way they accomplish this are very different. Cauchy sets the problem in algebraic terms, while Argand in his usual geometric setting. Accordingly, the mathematical objects they use for their arguments differ: Argand uses geometric constructions while Cauchy uses functions. He quickly expresses $f(u+v\sqrt{-1})$'s coefficients in their module form to get $F(u+v\sqrt{-1})$ and then reasons on real modules ρ_i, *thereby eliminating any reliance on* $\sqrt{-1}$. This key move of putting imaginary numbers in their module form to work with real modules is central to Cauchy's early work in complex analysis, as we will see. Argand, on the other hand, keeps $\sqrt{-1}$ in his reasoning, as he relies on the notion of angle and direction to make H fall in the circle with center A and radius AP.

One might downplay the importance of these differences between Cauchy and Argand since, after all, Cauchy somehow treats '$\alpha+\beta\sqrt{-1}$' as referring to the pair of real numbers α, β, which is simply a point in Argand's plane. But this would be reading too much into Cauchy, who takes '$\alpha+\beta\sqrt{-1}$' to be meaningless strictly speaking. Following Cartan, 1908 and Gilain, (1997, p. 69–71), we can distinguish between a strong and a weak geometrical interpretation of complex numbers. A geometrical interpretation is weak if any complex number $z = \alpha+\beta\sqrt{-1} = \rho(\cos\theta + \sqrt{-1}\sin\theta)$ is associated with the point M in a plane whose cartesian coordinates are (α, β) and whose polar coordinates are (ρ, θ). A strong interpretation associates z with the vector $\overline{OM}$, where O is the origin, and interprets arithmetical operations as operations on these vectors. Argand exemplifies the strong version with his "directed lines," even if, as this notion was slowly making its way in French mathematics (Schubring, 1997), Mourey, (1861) might be a clearer example Cartan, 1908, p. 342. Cauchy, on the other hand, qualifies at best for the weak interpretation, though he hardly ever mentions z as a point in a plane. The weak and strong interpretations differ significantly: for example, only with the latter one can one use properties of vectors to justify steps in a proof as Argand does. Servois' skepticism for Argand's *Essai* illustrates the significance of these two interpretations in the early 19^{th} century in somewhat Condillacian terms.

> Geometers, often expressing the position of a point on a plane, by a *vectorial radius* and an *anomaly*, have certainly not ignored [the consequences of Argand's definition] [. . .] [and] being happy to consider separately the *length* and the *position* of a line on a plane, they had not yet formed the *composed idea* of these two *simple ideas* or, if one wishes, they had not created the new *geometrical being*, combining, at the same time, *length* and *position*. (1813, p. 232)

Cauchy's proof illustrates his general orientation of reducing complex reasonings to real reasonings, contrary to Argand's attempt to give an interpretation of imaginary reasonings. Given his more algebraic approach in real contexts, it is natural that Cauchy extended it in complex contexts. Cauchy would later use this general strategy in his *Cours* to define many complex notions and easily extend many results from real into complex analysis. This, I believe, captures nicely the rigour Cauchy displays in the second part of his *Cours*: an algebraic complex analysis systematically reducing complex notions to combinations of their real counterparts.

5.2 From Real to Imaginary Notions

After his remarks on imaginary expressions and equations, Cauchy spends several pages defining various operations on imaginary expressions and proving theorems about these operations: addition, subtraction, multiplication and division (1821, p. 177–178); powers and roots (with fractional exponents) of imaginary quantities (1821, p. 178–182, 217–230); modules of imaginary numbers with the above operations (1821, p. 182–196); integer and fractional roots of 1 and −1 (1821, p. 196–217); imaginary polynomial representations of trigonometric functions (1821, p. 230–239); and some other concepts. Cauchy then moves on to his chapter VII where he discusses imaginary variables and functions.

Cauchy first defines an imaginary variable as an expression

$$u + v\sqrt{-1}$$

where 'u' and 'v' are real variables (1821, p. 204). Real variables, for Cauchy, are quantities which "we consider as successively having to receive many values different from one and another" (1821, p. 4). That complex variables are made of quantities with multiple real values is made manifest in Cauchy's explanation of what

$$a + x, \qquad a - x, \qquad ax, \qquad \frac{a}{x} \tag{5.6}$$

mean when x is an imaginary variable. To convince that these notations have a fixed meaning, Cauchy, (1821a, p. 205) asks us to "let us suppose, to fix[15] our ideas, that

[15] Bradley and Sandifer, (2000, p. 159–160) use 'clarify' for '*fixer*', obscuring the fact that Cauchy sees himself more as stipulating at definition.

the constant a remaining real, the variable x receives the imaginary value

$$\alpha + \beta\sqrt{-1} = \rho(\cos\theta + \sqrt{-1}\sin\theta)."$$

In virtue of how he defined these operations in the earlier chapter, Cauchy tells us that in the case of that specific value of x, the expressions in (5.6) respectively "designate"

$$a + \rho\cos\theta + \rho\sin\theta\sqrt{-1}, \qquad a - \rho\cos\theta - \rho\sin\theta\sqrt{-1}$$
$$a\rho\cos\theta + a\rho\sqrt{-1}\sin\theta, \qquad \frac{a}{\rho}\cos\theta + \frac{a}{\rho}\sqrt{-1}\sin\theta.$$

Hence, as Ferraro, (2008, p. 348) pointed out, Cauchy seems to see expressions like (5.6) as operations on all possible values of the real variables. In the sense that imaginary variables are made of two real variables and that real variables are quantities with multiple values, Cauchy has just reduced imaginary variables to two real quantities with multiple values. This illustrates what Cauchy says in his introduction, that imaginary expressions represent relations between real quantities "which is always easy to verify by the substitution of the numbers for the quantities themselves" (1821, p. iv).

Second, Cauchy defines an imaginary function as an expression "in which the real part and the coefficient of $\sqrt{-1}$ are necessarily real functions of the variables $x, y, z \ldots$" (1821, p. 209). For example,

$$\phi(x) + \chi(y)\sqrt{-1}, \qquad \omega(x) := \phi(x) + \chi(x)\sqrt{-1}$$

are both imaginary functions, where a function is a strong relation between two real variables (1821, p. 31).

With these notions in hand, Cauchy can then easily proceed to define the notions of continuous functions (1821, p. 212) (restricted to imaginary functions of one variable, as Bottazzini, (2003, p. 218) and Bottazzini and Gray, (2013, p. 115) noticed), infinitely small imaginary quantities (1821, p. 212)[16] and series (1821, p. 230–233). Other than providing one of the first clear definitions of these complex notions,

[16]See Robinson, 1967, Fisher, 1978; Fisher, 1979, Lakatos, 1978, Grabiner, 1981, Laugwitz, 1987, Koetsier, (1991, chap. 3), Lüzten, 2003, Schubring, 2005, K. U. Katz and M. G. Katz, 2011 and Borovik and M. G. Katz, 2012 for different views about Cauchy's use of real infinitely small quantities in his *Cours*.

Cauchy's reductive approach allows him to easily extend many of his real proofs into complex contexts. Here are some examples: his theorems about the limits of multi-variable complex functions (1821, p. 213–214); about factors of vanishing functions (1821, p. 216–220); about function equations (1821, p. 220–229)[17]; about convergence of series of continuous functions and various other series (1821, p. 234–238); about the radius of convergence of complex series (1821, p. 240–256).

An illuminating example for our discussion of the passage from the reals to the imaginaries in the case of complex integration is Cauchy's later work (1822; 1823; 1823; 1825; 1825; 1825; 1826). Letting $y = f(x)$ be a continuous function with respect to x between the two limits $x = x_0$ and $x = X$, Cauchy considers the following sum, where $x_0 < x_1 < \cdots < x_{n-1} < X$ (1823, p. 81–84).

$$S := (x_1 - x_0)f(x_0) + (x_2 - x_1)f(x_1) + \cdots + (X - x_{n-1})f(x_{n-1}). \tag{5.7}$$

As he notices, S varies depending on the number n of x_i and their values, i. e. "adopted mode of division" (partition of $[x_0, X]$). By an algebraic argument, Cauchy concludes that "if we decrease indefinitely the numerical values of these elements, by increasing their number, the value of S will in the end become roughly constant, or, in other words, it will reach a certain limit which will depend solely on the form of the function $f(x)$, and of the extreme values x_0, X given to the variable x. This limit is what we call a *definite integral*." Letting each $\Delta x_i := (x_i - x_{i-1})$, Cauchy expresses (5.7) the following way

$$S := \sum f(x)\Delta x.$$

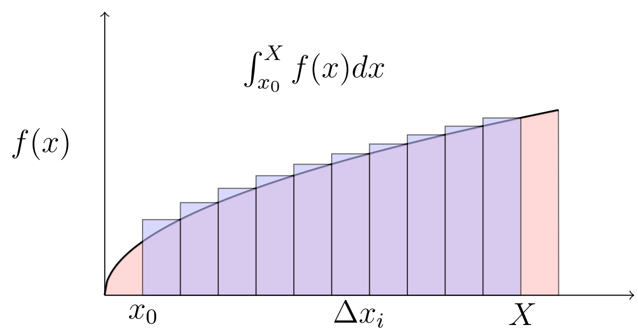
The definite integral to which S converges as all Δx_i become infinitely small is written

$$\int_{x_0}^{X} f(x)dx, \tag{5.8}$$

"in which the letter $\int$ substituted to the letter $\sum$ indicates, not a sum of product similar to $f(x)dx$ anymore, but the limit of a sum of this kind." Cauchy's return to a Leibnizian conception Guicciardini, 1994, p. 311 differs from that of many of his immediate predecessors who had, perhaps since the Bernoulli brothers (1742,

[17] Bradley and Sandifer, (2000, p. 173) rightly point out that there are specific complications arising from the definition of complex multiplication. See Dhombres, 1992c for an insightful discussion of these equations.

p. 387), defined it as the inverse of differentiation Grabiner, 1981, p. 141, Grattan-Guinness, 1994b, p. 360, Domingues, 2008, chap. 5, Boyer and Merzbach, 2011, p. 456. Later examples include l'Huilier, (1786, p. 32, 143–144), Cousin, (1796, p. 128–150), Bézout, (1796, p. 97) and Lacroix (1798, p. 1–2), (1802, p. 187–188). Relying on his new notion of continuity, the definition explicitly puts a condition for $f(x)$ to be integrable (which would be weakened by Riemann) and raises the question of the existence of a given integral, a thing that was not possible before Hawkins, 1975, p. 9–12. The modern fundamental theorem of calculus and not the Leibnizian definition became a substantial result on its own (Smithies, 1986, p. 54). Schubring, forgetting Riemann, believes this to be "the principal foundation until further refinements such as Lebesgue's and Stieltjes's integrals" (2005, p. 479).



Leibniz Integral

Cauchy himself justifies his definition (1822, p. 297) in part by noticing that some integrals might not have antiderivatives (*fonction primitive*) or, even, can often have more than one primitive, as Smithies, (1986, p. 54) pointed out. For example,

$$\int_{-1}^{-2} \frac{dx}{x} = \int_{-1}^{+2} \frac{xdx}{x^2} = \int_{-1}^{+2} \frac{\frac{1}{2}d(x^2)}{x^2}.$$

Accordingly, one could take $\log(x)$ as antiderivative in the first case, which would be imaginary, or the real function $1/2\log(x^2)$. Another advantage is that "it easily allows separating easily any imaginary equation in two real equations" (1822, p. 296) Grabiner, 1981, p. 161–162. In effect, a year later (1823, p. 89), Cauchy notes that if $f(x) = \phi(x) + \chi(x) + \psi(x) + \cdots$, then

$$S = \sum f(x)\Delta x = \sum \phi(x)\Delta x + \sum \chi(x)\Delta x + \sum \psi(x)\Delta x + \cdots ,$$

so "going to the limits"

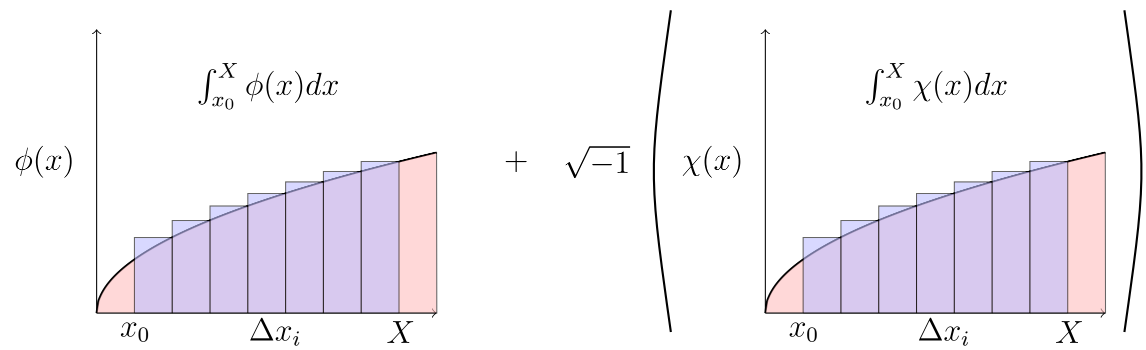
$$\int f(x)dx = \int \phi(x)dx + \int \chi(x)dx + \int \psi(x)dx + \cdots . \tag{5.9}$$

From (5.9), Cauchy easily infers that if $f(x) := \phi(x) + \chi(x)\sqrt{-1}$, then

$$\int f(x)dx = \int \left(\phi(x) + \chi(x)\sqrt{-1}\right) dx = \int \phi(x)dx + \sqrt{-1} \int \chi(x)dx. \tag{5.10}$$

Again, we see that Cauchy defines a complex notion in terms of a combination of its real counterparts. He even extends this general approach to the case of integration in the complex plane two years later in his famous *Mémoire sur les intégrales définies* (1825).

$$\int_{x_0}^{X} f(x)dx :=$$



$f : \mathbb{R} \mapsto \mathbb{C}$ **Integral**

Cauchy starts his *Mémoire* highlighting that Laplace obtained some "results of interest" with these integrals, probably referring to cases like the one discussed above. Questions about the difference between integrating in the real vs. complex plane were raised for example by Legendre in his *Exercises de calcul intégral* (1811, p. 352–354). He noticed that "$\Gamma'(\frac{1}{2}) = 2\sqrt{\pi}$ and $\Gamma(\frac{1}{2}) = \sqrt{\pi}$ both represent the integral $\int x^{-\frac{1}{2}}e^{-x}dx$; but the first is taken between the imaginary limits which make $x^{-\frac{1}{2}}e^{-x}$ equal zero, and the second one is taken between the real limits $x = 0, x = \infty$; thus it is not surprising that they are both unequal." But Legendre never explained what integration between imaginary limits amounted too. Even considering the works of

Brisson and Ostrogradsky, Cauchy regrets that "neither this work, nor any published *Mémoire* to this day, on the various branches of integral calculus, have fixed the degree of generality that a definite integral has, taken between imaginary limits, and the number of values they can admit" (1825, p. 2). Recognizing the usefulness of these integrals, Cauchy expresses the same worry about the scope of mathematical formulas he had in his *Cours*. His definition answers that worry by again making complex formulas about algebraic relations of pairs of real numbers. Cauchy expresses an integral in the complex plane as

$$\int_{x_0+y_0\sqrt{-1}}^{X+Y\sqrt{-1}} f(z)dz. \tag{5.11}$$

(5.11) refers to "the limit or one of the limits towards which the sum of the products of the form"

$$\begin{gathered}((x_1 - x_0) + (y_1 - y_0)) \cdot f(x_0 + y_0\sqrt{-1}),\\ ((x_2 - x_1) + (y_2 - y_1)) \cdot f(x_1 + y_1\sqrt{-1}),\\ \vdots\\ ((X - x_{n-1}) + (Y - y_{n-1})) \cdot f(x_{n-1} + y_{n-1}\sqrt{-1})\end{gathered}$$

converges when all Δx_i and Δy_i get infinitely small. But Cauchy does not stop there. For each $\Delta_i x$ and Δy_i to become infinitely small, one can express them with the two increasing continuous real function $x = \phi(t)$ and $y = \chi(t)$ from $t = t_0$ to $t = T$, where

$$\begin{aligned} \phi(t_0) &= x_0, \qquad & \chi(t_0) &= y_0,\\ \phi(T) &= X, \qquad & \chi(T) &= Y. \end{aligned} \tag{5.12}$$

We can then express any intervals Δx_i and Δy_i this way

$$\Delta x_i = (t_i - t_{i-1})\phi'(t_{i-1}), \qquad \Delta y_i = (t_i - t_{i-1})\chi'(t_{i-1}).$$

With these two functions (5.12), one can express "very roughly" (5.11) as the sum of all Δx_i and Δx_i multiplied by the corresponding value of f.

$$\int_{x_0+y_0\sqrt{-1}}^{X+Y\sqrt{-1}} f(z)dz \approx \sum_i (\Delta t_i)\left(\phi'(t_i) + \sqrt{-1}\chi'(t_i)\right) f\left(\phi(t_i) + \sqrt{-1}\chi(t_i)\right).$$

So going to the limit,

$$\int_{x_0+y_0\sqrt{-1}}^{X+Y\sqrt{-1}} f(z)dz := \int_{t_0}^{T} \left(\phi'(t) + \sqrt{-1}\chi'(t)\right) f\left(\phi(t) + \sqrt{-1}\chi(t)\right) dt. \tag{5.13}$$

Even after naturally extending his definition of integration for complex values, Cauchy still reduces integrals between imaginary limits to a real integral of a complex function Smithies, 1997, p. 89. As Bottazzini, (1986, p. 153), Bottazzini and Gray, (2013, p. 137) explain, the two functions ϕ and ψ can be seen as defining a curve in the complex plane which joins the points (x_0, y_0) to (X, Y) within the rectangle these points determine. From this, Cauchy first proves the theorem which now bears his name (though Gauss had already proved it (1811)) in a very different from than its current presentation (1825, p. 5–6).

Theorem 5.1. *Let us conceive that the function $f(x + y\sqrt{-1})$ remains finite and continuous, any time x remains within the limits x_0, X and y between the limits y_0, Y. In this particular case, one can easily prove that the value of the integral (5.13) [...] is independent from the nature of the functions $x := \phi(t)$, $y = \chi(t)$.*

Proof. If ϵ is an infinitely small quantity, u, v are two functions of t vanishing at $t = t_o$ and $t = T$, then Cauchy replaces x by $x + \epsilon u$ and y by $y + \epsilon v$. He then develops this increment of (5.13) in power series of ϵ, whose coefficients will be

$$\epsilon \int_{t_o}^{T} \left((u + v\sqrt{-1})(x' + y'\sqrt{-1})f'(x + y\sqrt{-1}) + (u' + v'\sqrt{-1})f(x + y\sqrt{-1})\right) dt.$$

Integrating by part, Cauchy obtains

$$\int_{t_o}^{T} (u' + v'\sqrt{-1})f(x + y\sqrt{-1})dt = -\int_{t_o}^{T} (u + v\sqrt{-1})(x' + y'\sqrt{-1})f(x + y\sqrt{-1})dt,$$

which means that the coefficients of the power series equal 0. This means that if x and y receive infinitely small increments whose sum is finite, the resulting increment of the integral will be of first order, that is, 0. ♦

Cauchy proves more results in the rest of his *Mémoire* and in another published a few months later (1825). Smithies, (1997, p. 99, 111) and Bottazzini and Gray, (2013, p. 137–138) noticed that Cauchy uses a geometrical language to describe his integrals later in his *Mémoire* but in a far less developed way than Argand. As Smithies said, "it does not appear, however, that he was yet thinking in terms of integration around a closed curve." This illustrates how committed to his view that complex analysis is about pairs of real numbers Cauchy was. We also see how his rigour manifested itself again in his reductive algebraic approach to integration in the complex plane. Cauchy would carry this reductive project even in his latter work, where he explicitly says that algebraic theories would be of course clearer "if we succeeded in getting rid of imaginary expressions completely, by reducing the letter i to being nothing more than a real quantity," which he thinks he has done with his theory of algebraic and arithmetic equivalences (1847, p. 93–94).

5.3 Cauchy's Rigour: Between Laplace and Argand

A first main difference between Cauchy and Laplace is of course that only the former was concerned with developing the study of imaginary numbers as an independent subject Fraser, 2015, p. 41. This phrasing might be misleading since, strictly speaking, Cauchy did not think there were such things as imaginary numbers. Instead, one could say that he was interested in developing and studying imaginary notions analogous to real ones like functions, series, derivatives and integrals. This ambivalent attitude towards imaginaries has led some like Cartan, (1908, p. 337) to mis-characterize Cauchy as having a mere instrumental and formal approach to them in the same way Laplace did. It also explains why though he believed that '$\alpha+\beta\sqrt{-1}$' was non-sensical, Cauchy maintained that '$\alpha + \beta\sqrt{-1} = \delta + \gamma\sqrt{-1}$' was not, since it meant $\alpha = \delta$ and $\beta = \gamma$. Laplace did not have to bother with this distinction since he was not concerned with making true complex claims. After all, imaginaries were mere tools which destroy themselves at the end of a calculation.

Another key difference is that Cauchy did not read mathematical formulas as being about algebraic quantities. This is a significant difference since many mathematicians at the time distinguished between arithmetic and algebra in a similar way to Laplace: from Clairaut in his *Éléments d'algèbre* (1746, p. 33) who distinguishes between "numerical quantities" and "literals," to Lagrange, who was probably also influenced by Condillac Dalmedico, 1992, par. 46, stating that algebra is concerned with quantities "which we consider in a general manner" in his *Leçons données à l'École normale* (1795), and Lacroix's own *Éléments d'algèbres* (1797, p. 34). Even Poncelet, who preferred geometry over algebraic analysis, explained algebra's generality by its "abstract signs" which "leave to this [absolute] length all the indetermination possible" (1822, p.xi–xii). It was important for Cauchy to have equations which "represent no more than relations between real quantities, relations which are always easy to verify by the substitution of numbers to real quantities themselves" (1821, p. iij–iv). Cauchy's analysis, from functions to power series, differentials and integrals, complex and real, is about quantities, i. e. variations in absolute measure of lengths.

This contrast between Cauchy and his predecessor does not reside in the methods of algebra but in its objects. Dalmedico, (1992, par. 97–99) has suggested we read Lagrange as saying that algebra has no object but is a method to gain arithmetical or geometrical knowledge, in the same sense that analysis is a method for Condillac. Ferraro and Panzo, (2012) have argued instead that abstract quantities were the object of Lagrange's algebra. While Lagrange's wording points in favour of the second reading, this disagreement might be more terminological than substantive. As Peacock, (1834, p. 185) pointed out, algebra can be seen from two perspectives: as an independent science or as a means of investigating other sciences by the application of its results. Clairaut's *Éléments* (1746), Condillac's *Logique* (1780, p. 127–133), Laplace's *Leçon* 3 (1795, p. 34–36), Lagrange *Leçon* 2 (1795), Lacroix's *Éléments* (1797) and Bézout's *Cours* (1799, § 1) show that algebra can of course be a method to solve equations. As an independent science, though, all of them emphasize that algebra has its own language which is to be interpreted as being about abstract quantities or quantities in general. One cannot use algebra to reason about anything non-quantitative. On the other hand, analysis, for Condillac, Laplace and Lagrange, is a method of composing and decomposing ideas, moving from known to unknown, sometimes inductively, which one can use in many different languages and apply to any field of inquiry. Rejecting abstract quantities thus came with a rejection of these forms of inductive reasoning.

It is worth noticing that Cauchy did not define addition, subtraction, multiplication and division for both arithmetic and algebra, like Laplace, Lagrange and Condillac did. For him, like for Bézout, (1799, § 1), "[i]n algebra, we represent, not only numbers, but also quantities, by letters" (1821, p. 18, 334). If for Lagrange "algebra surfs so to speak equally on arithmetic and on geometry; its object is not to find the values themselves of the quantities one is looking for, but the system of operation to be performed on the given quantities to deduce the values of the quantities looked for" (1808, p. vj); for Cauchy, while one uses letters only in algebra, there are no distinction betweens the object of algebra and that of arithmetic.

Rejecting abstract quantities did not mean that Cauchy went so far as to interpret his work in a geometric way, like Argand and Buée did. Neither does Cauchy seem to agree with Monge or Poncelet, important figures in descriptive geometry. The former pleaded for geometry and analysis to be "cultivated together: descriptive geometry would carry in the most complicated analytical operations the evidence that is its character, and, in its turn, analysis would carry in geometry the generality that is proper to it [analysis]" (1795, p. 16). In his lectures at l'École Normale and after at l'École centrale des travaux publiques, he also highlighted the correspondence between particular geometric constructions and the general equations they represent (Belhoste and Taton, 1992, par. 85). One of Poncelet's important goal in his *Traité* was to perfect and generalize geometry by developing descriptive geometry, "a general geometry," to finally make it independent from the more general algebraic analysis (1822, p. x–xi). If Cauchy's *Cours* signaled a return to geometry, as some have suggested, it was not in its reliance on geometric constructions or any form of geometric reasoning one finds in Poncelet (Lorenat, 2015). While Cauchy might have shared Monge and Laplace's common interest in general methods for finding solutions Belhoste, 2001, p. 22, he was not attracted to either Laplace's abstract algebra nor Monge's descriptive geometry. This shows how limited the usefulness of the adjectives 'algebraic' and 'geometrical' is in explaining Cauchy's new rigour, as Dalmedico, (1997, p. 30–32) rightly pointed out.

As Belhoste, (1991, p. 215–216) has suggested, Cauchy's rigour is better understood within the traditions of Euler and Lagrange. This can be seen in how divorced from geometrical reasonings and focused on general solutions Cauchy's work in analysis is. While he had done substantive work in physics, Cauchy mostly kept his analysis separate from its applications, though they of course influenced each other. However, one must be careful in concluding that Cauchy's analysis was abstract as Belhoste and Richards, (1991, p. 313) do. It was abstract insofar as it was general and divorced from application, not in that it relied on abstract ideas (other than perhaps, that of *quantité*, as Condillac pointed out) like Laplace and Lagrange did Glas, 1986, p. 256. From within the Eulerian and Lagrangian traditions, Cauchy proposed a change in the methods by rejecting abstract ideas and Condillacian analysis.

Schubring, (2005, chap. V) has linked Cauchy to "a broader epistemological transformation in France after the first years of the Revolution combined with a reappraisal of geometry, a revived dominance of the synthetic method, and an increased shift toward empiricism in epistemology in general" (2005, p. 320). He illustrates this shift through the evolution of Carnot, at first a strong defender of Condillac's preference for analysis and a *langue bien faite* in his *Dissertation sur la théorie de l'infini mathématique* (1775, p. 253–255). Schubring rightly points out the influence of Condillac in pre-Révolution Carnot but wrongly suggests that one major divergence from Condillac was that "Carnot thinks that mathematics always remains bound to the empirical concept of *quantité*" (2005, p. 319). As we saw, Condillac clearly thought quantities, be they arithmetical or literal/abstract, were the objects of mathematics. Moreover, Carnot also believed algebra was concerned with less determinate quantities than arithmetic. However, in his latter work, perhaps as early as *Réflexions sur la métaphysique du calcul infinitésimal* (1797) but certainly in *De la corrélation des figures de géométrie* (1801) and more vividly in *Géométrie de position* (1803), Carnot relies more and more on geometric constructions and becomes suddenly critical of algebraic methods. An illustration of this is his attempt to justify the analytic use of infinitesimals with geometric constructions in his *Réflexions* Gillispie and Pisano, 2013, p. 122-124.

But Carnot, even in his *Géométrie*, still conceives of algebra as an "indeterminate arithmetic," a form of reasoning involving unintelligible characters or "hieroglyphs, which more than often designate beings of reason [*être de raison*]," "some non-real objects" (1803, p. 12). Notably, Carnot in this work denies that negative quantities exist, since a quantity is "a real object the mind can grasp": for example, $-a$ (where $a > 0$) means that one needs to subtract a from 0, "which is absurd, since there is nothing below 0" (1803, p. 7). Algebra relies on these "algebraic forms" or "symbolic expressions" like $-a$ and $\sqrt{-a}$ in its calculation and then gets rid of them in its results, thereby enjoying a significant advantage over synthesis (1803, p. 9). Reminding Laplace's claim that imaginaries are tools destroying themselves at the end of a calculation, Carnot extends this idea even further to negative quantities.

Carnot also departs from Laplace in portraying analysis more as the use of symbolic expressions than as composing and decomposing ideas; and in being skeptical about its applications to fields other than mathematics (1803, p. 10). This different depiction of the analytic/synthetic distinction would prompt Gergonne to criticize it on different grounds than that of Condillac, comparing Carnot to a stranger living in France hearing Clairaut, Laplace and Lagrange using '*analyse*' in their papers which he could not understand and then inferring that it must mean the usage of incomprehensible symbols (1816, p. 363–372). Gergonne's own views aligned more with Arnaud's *La logique ou l'art de penser* (1662). So whether or not post-Revolutionary France saw a revival of synthetic methods hinges upon who's understanding of the analytic/synthetic distinction one uses. It is false if meant in Carnot's sense as Cauchy's conscient use of symbolic expressions shows; and unclear if meant in Gergonne's.

However, if meant in Laplace's sense, as a distinction between algebraic methods in a general and abstract language and more geometric methods in a more specific language about points and lines, then Cauchy does not fit this narrative. Still, one can find evidence of pressures to make the education at the École polytechnique more (Laplacian) *synthétique*, e. g. the 1811 recommendations by the École de Metz and the Comité des Fortifications that "*synthèse* be employed concurrently with *analyse*" and that instructors "do not neglect to indicate the geometric construction which represent certain formulas" Fourcy, 1828, p. 294–295. But Cauchy did not use geometric constructions in his textbook and research papers like Carnot did, even in his *Leçons sur les applications du calcul infinitésimal à la géométrie* (1826; 1828). The only papers I know of where he uses drawings and geometric reasonings is some of his early work on polygons and polyhedra (1813; 1813).

Schubring seems to identify a comeback to geometry in French mathematics not in its use in reasoning but as a foundational role, citing Antoine Destut Tracy, (1804, p. 203) saying that mathematics is concerned with measurement as an example. In that sense, Cauchy, who defines numbers as absolute measures of lengths and quantities as their increases and decreases, departs from Condillac's more arithmetical conception. But since Cauchy's definition hardly plays any important role in his *Cours*; and since he only spends two pages on the matter, qualifying his work as geometric for these reasons is more confusing than illuminating. Ironically, Carnot's *Géometrie* does not count as geometric in that sense since he defines quantities as "the thing itself of which we are investigating the properties, or its absolute value, that is to say, in abstraction from the sign" (1803, p. 7); and Laplace's claim that arithmetic is concerned with lengths would make his approach geometric.

It is also hard to understand Schubring's claim that French mathematicians turned away from analytical methods to empiricist concepts at the beginning of Napoleon's rule, since a wide-spread understanding of analysis around the Revolution stemmed from Condillac's empiricist philosophy. Since he contrasts Condillac's discussion of abstraction with the *Idéologues*' concepts "predominantly in close relation to the sensations, and understanding them as being equipped with an empiricist substance," one could conclude that Schubring linked the rejection of abstraction with empiricism. But many empiricists like Locke and Condillac believed in abstraction. In fact, the matter becomes a controversial question for empiricists in Hume's *Treatise of Human Nature* (1739). Not only is the transition that Schubring discusses obscure; it is also hard to see in what sense Cauchy is an empiricist, as he never really discussed the matter.

Hence, a first part of Cauchy's new approach to analysis consists in getting rid of the idea that algebra is about abstract quantities and maintaining that it is about the same quantities than arithmetic is. Since his use of algebra is nonetheless central in his analysis, a better way to capture his rigour is in his reducing the difference between algebra and arithmetic: the arithmetization of algebra. Note that this is an arithmetization primarily of the object, not language, of algebra, thus differing from Jahnke and M. Otte, (1979, p. 84–85), who perhaps refer to a latter period of what Klein, (1895) has called "the arithmetization of analysis." In Janhke and Otto's later words,

> During the 18$^{\text{th}}$ century, numbers, in their inseparable linkage of the quantity concept, represented the actual field of mathematics, and algebra, and the symbolic calculi of mathematics were regarded merely as a language permitting an easy and suggestive manner of representing relationships between number or quantities. This status became precisely the reverse in the 19$^{\text{th}}$ century. Algebra was now to directly include the actual mathematical relationships, which constitute the subject matter under study, while arithmetics, for its parts, became the language of algebra resp. of the entire mathematics, by means of which, and in which, all mathematical facts must ultimately be expressible. (1981, p. 28–29)

I believe Cauchy is one of the first, if not the first, to start this transition, which is why some commentators like Gilain qualified the *Cours* as "an analysis less algebraic than arithmetical" (1989, par. 133). An immediate contemporary of Cauchy who noticed his stance that "arithmetic provides a sufficient basis for symbolical algebra" in both its object and its operations is Peacock, (1834, p. 192–194). Peacock would object to this approach, also advocated by Frend, (1796) and Frend, (1799), and suggest a clear separation between the two in his *A Treatise on Algebra* (1830) (Richards, 1980,

p. 347–350), (Pycior, 1981, p. 33–37), (Richards, 1991), (Fisch, 1999, p. 155–171), (Lambert, 2013, p. 283–286).

A second equally important feature of Cauchy's approach consists in logically developing all of analysis on this arithmetization, from variables to functions, limits, infinitesimals, series, derivatives and integrals, both real and complex, about relations between quantities Grabiner, 1981, p. 164. This careful edifice contrasts with Clairaut and Lacroix's textbooks by its clear reliance on definitions, lemmas, theorems and corollaries.

These two steps are not innocent reinterpretations of mathematical languages without any consequences. A significant consequence of this is a constant worry that his notation refers to relations between quantities "which are always easy to verify," as he himself says in the introduction of his *Cours*. This desired criterion or approach to the scope of mathematical formulas ensured that any equation was in principle "numerically verifiable" Smithies, 1986, p. 59. Such an attitude explains why he adopts what Grattan-Guiness has called a "limit-avoidance" approach (1970, p. 378–379), i. e. a reliance on sequences to define limits and then infinitesimals. It is also why Cauchy, more than anyone else before, worried about proofs of existence for many of his concepts Laugwitz, 1994, p. 325, from his *Cours* to his early work in complex analysis and even his *Cours inédit: Équations différentielles ordinaires* (1981) as Gilain discusses in the preface. His proof of Rolle's theorem, of the existence of many differentials and integrals reflect this important shift in analysis.

Carrying this arithmetic of variations in absolute measure of lengths in the study of imaginary numbers, a study worth pursuing for its own sake, captures nicely the rigour Cauchy displays in his complex analysis. Prior to Cauchy, the belief that imaginaries were both useful and impossible was widespread amongst mathematicias, who nonetheless used them in many calculations. This confirms Crowe's second law of change in mathematics that "many new mathematical concepts, even though logically acceptable, meet forceful resistance after their appearance and achieve acceptance only after an extended period of time" (1975, p. 162–163). What Crowe had perhaps not expected was that mathematicians often investigate these impossible entities like Cauchy did. Moreover, if I am right that Cauchy is one of the first to seriously develop complex analysis as a field of research in its own, then we have a counterexample to Crowe's law 4, which states that "[t]he rigour permeating textbook presentations of many areas of mathematics was frequently a late acquisition in the historical development of those areas and was frequently forced upon, rather than actively sought by, the pioneers in those fields." The narrative that mathematicians first provided a sound geometrical basis of complex numbers before developing key notions of complex analysis (Struik, 1981, p. 16) is without a doubt erroneous.

Cauchy's rigour required a change in how one interprets complex sentences and in the common beliefs about which fields of research are worth pursuing. Drawing on the famous *The Structure of Scientific Revolutions* (1962) and Kuhn's later work (1970; 1970; 1970), Mehrtens, (1976, p. 31–35) develops the notion of *disciplinary matrix* — the common commitments of the members of a community — in the case of mathematics by listing five of its important elements. Amongst those are *beliefs in particular models*, referring to philosophical views mathematicians attached to their work which highlight the multiple layers of mathematical knowledge Crowe, 1975, Law 5. The former change Cauchy brought can be seen as a change in belief model having consequences in the accepted methods. A second element is the *values*, shared opinions about what and how a given research should be done in addition to how valuable it is. The latter change illustrates how Cauchy pushed for change in the values of the Eulerian and Lagrangian disciplinary matrix.

Perhaps, Cauchy's challenges to the 18^{th} century algebraic approach amounts to a "revolution", a complete, powerful, comprehensive solution to problems sometimes opening entirely new theories Dauben, 1984, p. 63–64. Or perhaps it is an "epistemological shift" restructuring mathematical ways of knowing Mehrtens, 1992, p. 44–45. For Dauben, Cauchy's new rigour was a revolution in the methods it brought: precise definitions leading further to new discoveries and applications of mathematical concepts like uniform convergence, continuity, summability, asymptotic expansions (1992, p. 72–74). However, his argument relies on a caricatured understanding of 18^{th} century mathematicians who "were interested primarily in results;" a misleading attribution of the epsilon-delta calculus to Cauchy (Borovik and M. G. Katz, 2012); and the overblown claim that Cauchy's rigorous calculus "not only included a precise definition of limits, but aspects (if not all) of the modern theories of convergence, continuity, derivatives and integrals."

It is also important to realize that to a certain extent, Cauchy's new rigour is conservative in that he works from within the Eulerian and Lagrangian program: imaginary numbers are still impossible but useful quantities, whose study Cauchy brought to a whole new level. If as Dunmore, (1992, p. 215–218) and Gray, (1992,

p. 229–236) believe, a revolution in complex analysis happened only when a shift in the ontology of numbers occurred, e. g. from quantities to sets or structures, thereby including imaginaries; then Cauchy's rigorization is in fact a philosophically conservative first step in the development of complex analysis Gray, 2008, p. 62–68. Hence, far from settling the case of whether or not there are revolutions in mathematics, we see that Cauchy's rigour in complex analysis is both hard to downplay and easy to overstate.

6 Against a Remarkable Artifice

Why did Cauchy bring new standards of rigour? This is a very important question that is notoriously difficult to answer. Some have looked at the broader sociological context of the French Revolution and the Restauration (Hodgkin, 1981), (Struik, 1981); some have looked at Cauchy's strong catholic and royalist views (Dhombres, 1978), (Belhoste, 1991). Far from disregarding these explanations, in this section, I give two mathematical reasons which might have led Cauchy to adopt this new approach to complex analysis. As Cauchy's work consists more in devising a meaningful mathematical discourse to develop the field of complex analysis than providing a sophisticated philosophical story justifying the use of imaginaries, in this section, I illustrate Kitcher's thesis that foundational work is not usually undertaken for philosophical but mathematical reason (1983, p. 246).

6.1 Algebraic Problems for the Passage

In fixing the "sense" of expressions with complex variables, Cauchy mentions that if x, y, z, u, v, w are variables, imaginary or real, the following operations have the following properties (1821, p. 242).

$$\begin{aligned} x + y + z - (u + v + q) &= x + y + z - u - v - w \\ xy &= yx \\ u(x + y + z) &= ux + uy + uz \\ \frac{x + y + z}{u} &= \frac{x}{u} + \frac{y}{u} + \frac{z}{u} \\ \frac{x}{u} \cdot \frac{y}{v} \cdot \frac{z}{w} &= \frac{xyz}{uvw} \\ \frac{x}{\left(\frac{u}{v}\right)} &= \frac{vx}{y} = \frac{v}{y} \cdot x. \end{aligned} \tag{6.1}$$

Similarly, an expression of the form x^a has the same properties if x is a real or imaginary quantity when a is an integer (1821, p. 244). However, if a, b, c are fractions and x, y, z are imaginary variables, the following properties hold only in specific cases.

$$\begin{aligned} x^a x^b x^c &= x^{a+b+c} \\ x^a y^a z^a &= (xyz)^a \\ (x^a)^b &= x^{ab}. \end{aligned} \tag{6.2}$$

The first equality of (6.2) "subsists only every time the real part α of the imaginary expression x is positive" (1821, p. 246). The second equality holds only when $\alpha, \alpha', \alpha''$ are positive and

$$\arctan \frac{\beta}{\alpha} + \arctan \frac{\beta'}{\alpha'} + \arctan \frac{\beta''}{\alpha''} \in \left[-\frac{\pi}{2}, \frac{\pi}{2} \right]$$

while the last one only when

$$\alpha \arctan \frac{\beta}{\alpha} \in \left[-\frac{\pi}{2}, \frac{\pi}{2} \right].$$

These show that one needs to be careful when doing algebraic manipulations with imaginary variables, for not all algebraic properties of real numbers hold for all their values. A similar case arises for the imaginary functions

$$A^z, Lz, \sin z, \cos z, \arcsin z, \arccos z.$$

Moreover, "[t]he conventions made in the chapter VII^e do not suffice yet to fix in a precise manner the meaning [of theses] notations" (1821, p. 246). Not only does Cauchy not make inferences by analogy or induction; he also refuses to define some complex notions by simple analogy or induction from their real counterparts.

To illustrate Cauchy's new approach of developing complex analysis from real analysis and carefully looking for when one can move freely from reals to imaginaries, here is his "rigorous though cumbersome procedure" to define A^x Bottazzini, 1994, p. 421. The easiest way to do so, for Cauchy, is to use imaginary series. Having proven that A^x can be developed in the following convergent series when x is real,

$$A^x = 1 + \frac{xlA}{1} + \frac{x^2(lA)^2}{1 \cdot 2} + \cdots + \frac{x^n(lA)^n}{n!} + \tag{6.3}$$

Cauchy looks at whether the series converges when we replace x for the imaginary variable z (1821, p. 257). He answers positively on the basis of a corollary to this theorem.

Theorem 6.1. *An imaginary series of ascending powers of an imaginary variable is a series of the form*

$$\sum_{n=0}^{\infty} \left(a_n + b_n\sqrt{-1}\right) z^n,$$

where a_n and b_n are real quantities and z an imaginary variable. When b_n vanishes, we have a series of the for $\sum a_n z^n$, which, if we express z as $x(\cos\theta + \sqrt{-1}\sin\theta)$ where x is real, can be written

$$\sum_{n=0}^{\infty} a_n x \left(\cos\theta + \sqrt{-1}\sin\theta\right).$$

Any series of this form is convergent for all values of x such that $x \in (-\frac{1}{A}, \frac{1}{A})$, where A is the biggest limit of the n^{th} root of a_n, i. e. $A := \limsup (a_n)^{\frac{1}{n}}$.

Proof. Cauchy proves this by using results reached earlier in his *Cours* (1821, p. 121–122, 131–132, 235–236).

Lemma 6.1.1. *Let u_n be a real sequence and A be the largest limit of the sequence $(u_n)^{\frac{1}{n}}$.*

$$\textit{If } \lim_{n\to\infty} \sup\,(u_n)^{\frac{1}{n}} := A < 1, \ \sum_{n=o}^{\infty} u_n \textit{ converges},$$

and

$$\textit{If } \lim_{n\to\infty} \sup\,(u_n)^{\frac{1}{n}} := A > 1, \ \sum_{n=o}^{\infty} u_n \ \textit{diverges.}$$

Proof. The proof is strikingly the same as the one one finds in modern textbooks, e. g. Rudin's. Assume $A < 1$. There is a number U such that $A < U < 1$. Then, "the largest values of $(u_n)^{\frac{1}{n}}$ will not get indefinitely close to the limit $[A]$, without at one point being constantly inferior to U." Thus, we can find a value N of n such that for any $m \geq N$,

$$(u_n)^{\frac{1}{m}} < U, \qquad u_m < U^m.$$

If we let $1, U, U^2, \ldots, U^n, U^{n+1}, \ldots$ be terms of a sequence, then $\sum U_n$ converges since $U < 1$. Thus, we get that $\sum u_n$ also converges, since all its terms after a certain N are smaller than those of U_n.

Assume $A > 1$ and let U be such that $A > U > 1$. As n increases, the values of $(u_n)^{\frac{1}{n}}$ will get closer to A and will eventually become larger than U. Thus, there is an N such that for any $m \geq N$,

$$(u_m)^{\frac{1}{m}} > U, \qquad u_m > U^m.$$

Thus, our series $\sum U^n$ similarly formed is clearly divergent and will have terms after n that are always smaller than those of $\sum u_n$, so $\sum u_n$ diverges. ♦

Lemma 6.1.2. *If for all $n \in \mathbb{N}$ $\rho_n \geq 0$, $\sum \rho_n$ converges, then $\sum \rho_n \cos\theta_n$ and $\sum \rho_n \sin\theta_n$ converge.*

Proof. Since $\cos\theta_n \leq 1$ and $\sin\theta_n \leq 1$, by multiplying the terms of $\sum \rho_n$ by any of them, "we can only increase the convergence by diminishing the numerical values of these same terms, and by changing the signs of a few of them." ♦

Lemma 6.1.3. *If $p_n + q_n\sqrt{-1} = \rho_n \left(\cos\theta_n + \sqrt{-1}\sin\theta_n\right)$ for any $n \in \mathbb{N}$, then*

$$\textit{If } \lim_{n\to\infty} \sup\,(\rho_n)^{\frac{1}{n}} < 1, \ \sum_{n=0}^{\infty}(p_n + q_n\sqrt{-1}), \ \textit{converges}$$

and

$$\textit{If } \lim_{n\to\infty} \sup\,(\rho_n)^{\frac{1}{n}} > 1, \ \sum_{n=0}^{\infty}(p_n + q_n\sqrt{-1}), \ \textit{diverges.}$$

Proof. Assume $\limsup(\rho_n)^{\frac{1}{n}} < 1$. By **Lem 6.1.1** $\sum \rho_n$ converges and by **Lem 6.1.2** $\sum \rho_n \cos\theta_n$ and $\sum \rho_n \sin\theta_n$ converge. Thus,

$$\sum_{n=1}^{\infty} \left(p_n + q_n\sqrt{-1}\right) = \sum_{n=1}^{\infty} \rho_n \left(\cos\theta_n + \sqrt{-1}\sin\theta_n\right) \text{ converges.}$$

Assume $\limsup(\rho_n)^{\frac{1}{n}} > 1$. By **Lem 6.1.1** $\sum \rho_n$ diverges, so $\sum(p_n^2 + q_n^2)^{\frac{1}{2}}$ diverges, which means either $\sum p_n$ or $\sum q_n$ diverges. In both cases, $\sum(p_n + q_n\sqrt{-1})$ diverges. ♦

Take any series of the form $\sum a_n z$ where z is an imaginary variable. $\sum a_n z = \sum a_n x(\cos\theta + \sqrt{-1}\sin\theta)$ where x is real. Let $A := \limsup(a_n)^{\frac{1}{n}}$. Clearly, $\limsup(a_n x) = Ax$. By **Lem 6.1.3**, if $\limsup(a_n x)^{\frac{1}{n}} = Ax < 1$, the series converges; if $\limsup(a_n x)^{\frac{1}{n}} = Ax > 1$, the series diverges. That is, the series converges when $x < 1/A$ and diverges when $x > 1/A$. In other words, when $x \in (-\frac{1}{A}, \frac{1}{A})$. ♦

Two crucial corollaires follow from **Thm 6.1** (1821, p. 240).

Corollary 6.1.1. *Suppose* $a_n \in \mathbb{R}$ *for all* $n \in \mathbb{N}$ *and* $\sum a_n x$ *converges for all* $x \in X \subseteq \mathbb{R}$. *Define* $Z \subseteq \mathbb{C}$ *this way:* $Z := \{z = \rho(\cos\theta + \sqrt{-1}\sin\theta) : \rho \in X\}$. *Then* $\sum a_n z$ *converges for all* $z \in Z$.

Corollary 6.1.2. *In particular, if* $\sum a_n x$ *converges for all* $x \in \mathbb{R}$, *then* $\sum a_n z$ *converges for all* $z \in Z$.

From **Cor 6.1.2**, Cauchy deduces that since the real expansion of A^x expressed in (6.3) is convergent for any real value of x, it also converges for any imaginary number. Thus we can consider this series "as ways to fix, even when the variable becomes imaginary, the meaning of the notations" (1821, p. 257). But instead of defining these expressions as infinite series, Cauchy adds a few more steps to obtain them "in a finite form." If we let $A = e$, we get

$$e^x = 1 + \frac{x}{1} + \frac{x^2}{1 \cdot 2} + \frac{x^3}{1 \cdot 2 \cdot 3} + \cdots . \tag{6.4}$$

As Cauchy has shown, the series still converges for any imaginary values since it converges for all reals. Thus, we can write

$$\begin{aligned} e^{xla} &= 1 + \frac{xlA}{1} + \frac{x^2(lA)^2}{1\cdot 2} + \frac{x^3(lA)^3}{1\cdot 2\cdot 3} + \cdots \\ e^{x\sqrt{-1}} &= 1 + \frac{x}{1}\sqrt{-1} - \frac{x^2}{1\cdot 2} - \frac{x^3}{1\cdot 2\cdot 3} + \cdots \\ e^{-x\sqrt{-1}} &= 1 - \frac{x}{1}\sqrt{-1} - \frac{x^2}{1\cdot 2} + \frac{x^3}{1\cdot 2\cdot 3} + \cdots \end{aligned} \tag{6.5}$$

He then infers safely, as all the following are known to be convergent series, that

$$\begin{aligned} e^{xlA} &= A^x \\ e^{x\sqrt{-1}} &= \cos x + \sqrt{-1}\sin x \\ e^{-x\sqrt{-1}} &= \cos x - \sqrt{-1}\sin x. \end{aligned} \tag{6.6}$$

Finally, he suggests the following definitions for an imaginary variable z.

$$\begin{aligned} A^z &:= e^{zlA} \\ \cos z &:= \frac{e^{z\sqrt{-1}} + e^{-z\sqrt{-1}}}{2} \\ \sin z &:= \frac{e^{z\sqrt{-1}} - e^{-z\sqrt{-1}}}{2}. \end{aligned}$$

Cauchy also shows how one can calculate the values of these expressions when we make $z = \alpha + \beta\sqrt{-1}$:

$$\begin{aligned} A^z &:= e^{zlA} = e^{(\alpha+\beta\sqrt{-1})lA} = e^{\alpha lA}e^{\beta lA\sqrt{-1}} = A^\alpha(\cos\beta lA + \sqrt{-1}\sin\beta lA) \\ \cos z &= \frac{e^\beta + e^{-\beta}}{2}\cos\alpha - \frac{e^\beta - e^{-\beta}}{2}\sin\alpha\sqrt{-1} \\ \sin z &= \frac{e^\beta + e^{-\beta}}{2}\sin\alpha + \frac{e^\beta - e^{-\beta}}{2}\cos\alpha\sqrt{-1} \\ &= \cos\left(\frac{\pi}{2} - \alpha - \beta\sqrt{-1}\right) \end{aligned}$$

He finally defines $Lz, \arcsin z, \arccos z$ by taking them as inverses of the above.

Cauchy's approach to new definitions for $A^z, \cos z, \sin z$ vividly illustrates what he says in his introduction,

> if some constants or variables taken in a function, after having been sup-

> posed real, become imaginary, the notation by which the function was expressed, can only be preserved in the calculation by virtue of a new convention which fixes[18] the sense of this notation in the last hypothesis. (1821, p. iv)

Instead of simply assuming by analogy that the series in (6.3) could be used to define A^z when z is an imaginary variable, Cauchy first proved that the series was also convergent in the imaginary case. This is a very different approach to the language of Analysis than Laplace's. It shows a conscious distinction between real and imaginary variables and a careful discussion of when one can switch from one to the other, instead of casually expressing real variables with complex functions. Notice also Cauchy's elimination of reasoning with $\sqrt{-1}$ by focusing on the module of imaginaries and building on his convergence theorems for real series.

From these definitions, Cauchy shows that the following properties hold in cases where x is a real or imaginary variable (1821, p. 272):

$$\begin{aligned} A^x \cdot A^y \cdot A^z &= A^{x+y+z} \\ A^x \cdot B^x \cdot C^x &= (ABC)^x \\ \cos(x+y) &= \cos x \cdot \cos y - \sin x \sin y \\ \sin(x+y) &= \sin x \cos y + \sin y \cos x. \end{aligned} \tag{6.7}$$

However, the same does not hold for $lx, \arccos x, \arcsin x$. As lx is the inverse operation of Ax, Cauchy, (1821a, p. 320) defines lx as

$$l(x) := \frac{1}{2} l\left(\alpha + \beta^2\right) + \left(\arctan \frac{\beta}{\alpha}\right)\sqrt{-1},$$

which has "a precise meaning determined by the equation [above], in the case where the real part of the imaginary expression represented by x is positive, while the notation

$$l((x))$$

has in all possible cases an infinity of values determined by the following equations":

[18] Bradley and Sandifer, (2000, p. 2) again translate '*fixer*' by a word ('keeping') which does not capture the fact that Cauchy sees himself stipulating definitions.

(1821, p. 266)

$$l((\alpha+\beta\sqrt{-1}))=l(\alpha+\beta\sqrt{-1})+l((1))$$
$$l((\alpha+\beta\sqrt{-1}))=l(-\alpha-\beta\sqrt{-1})+l((-1)).$$

Because of these "branching" problems, many properties of lx which hold for real variables fail for imaginary ones, for example, the formula

$$l(x)+l(y)+l(z)=l(xyz) \tag{6.8}$$

hold only when the real parts $\alpha, \alpha', \alpha''$ of the values of x, y, z are positive and

$$\arctan\frac{\beta}{\alpha}+\arctan\frac{\beta'}{\alpha'}+\arctan\frac{\beta''}{\alpha''}\in\left[-\frac{\pi}{2},\frac{\pi}{2}\right].$$

Or again,

$$L(x^{\mu})=\mu L(x)$$

holds only when

$$\mu\arctan\frac{\beta}{\alpha}$$

is in the same limits (1821, p. 272–273).

We thus see that the passage from the reals to the imaginaries, when it involves reading a real variable x as an imaginary variable, i. e. as having imaginary values, is perilous since some algebraic manipulations like (6.2) and (6.8) fail to hold for all imaginary values of an imaginary variable, even if some like (6.1) and (6.7) still hold for real and imaginary variables. Cauchy probably saw the latter cases as conditions when one can extend the scope of a mathematical formula beyond the cases for which it holds, as he mentions in his introduction. Since this is what Laplace's inductions discussed above involve, we have found one reason why Cauchy objects to his inductions. Moreover, proofs like **Cor 6.1.1** and **Cor 6.1.2** illustrate how seriously Cauchy takes the distinction between real and imaginary variable and how their different corresponding mathematical objects like series relate to each other.

6.2 On Integrals Going through the Infinite

In the second part of his "Suite du mémoire sur les intégrales définies" (1820), Poisson looks at definite integrals where "the function one integrates becomes infinite between the limits of the integration." Previous geometers have found finite values for these integrals by assuming that "the infinite elements of the integral destroy themselves by the opposition of the signs + and −." This trick is unsatisfactory for Poisson since some integrals like

$$\int_{-1}^{+1} \frac{dx}{x^2} \tag{6.9}$$

are positive between the two limits of integration while he nonetheless thinks their value is −2. Another puzzling case is

$$\int_{-1}^{+1} \frac{dx}{x}, \tag{6.10}$$

since $f(x)$ is real for all values between −1 and +1 while the value of the integral is allegedly $-\log(-1)$. As Smithies points out (1997, p. 36), one can find discussions of such integrals as early as by D'Alembert, (1768). To clarify this issue, Poisson suggests "going back to the origins of definite integrals" (1820, p. 319–320). Let $a, b \in \mathbb{R}$ and f be a function of x considered between a, b such that

$$\int f(x)dx = Fx + c,$$

where c is an arbitrary constant. If we make c such that $Fa + c = 0$, then

$$\int_a^b f(x)dx := Fb - Fa.$$

Referring to Lacroix's second edition of his *Traité* (1814, art. 417), Poisson states that the following used to be the definition of definite integrals but is now considered to be "the fundamental proposition of the theory of definite integrals." For any α, we can find a sequence of points $a = a_0 < a_1 < \ldots, a_n = b$ such that $a_i - a_{i+1} \leq \alpha$ and a corresponding sequence $f(a_0), f(a_1), \ldots, f(a_{n-1})$.

$$\int_a^b f(x)dx = \lim_{\alpha \to 0} \sum_{i=0}^{n} \alpha f(a_i). \tag{6.11}$$

For both Lacroix and Poisson (1820, p. 319–320), who also cite Joseph-Louis Lagrange, (1804, p. 69), this results holds only when f remains finite as x moves between a and b, which might explain why they preferred defining integrals as inverses of differentials. Otherwise, "the definite integral has no necessary relation with the sum of the differential values," which prompts Poisson to use other techniques to find the integrals of such functions. For example, in evaluating (6.10), Poisson makes the imaginary substitution $x = -(\cos z + \sin z\sqrt{-1})$ (1820, p. 320–321). Accordingly,

$$dx = \sin z - \cos z\sqrt{-1} = -\sqrt{-1}(\cos z + \sin z\sqrt{-1})$$

thus

$$\begin{aligned}\int_{-1}^{+1} \frac{dx}{x} &= \int_0^{(2n+1)\pi} \frac{-\sqrt{-1}(\cos z + \sin z\sqrt{-1})}{-(\cos z + \sin z\sqrt{-1})} \cdot dz \\ &= \int_0^{(2n+1)\pi} \sqrt{-1}dz \\ &= z\sqrt{-1}\,\Big|_0^{(2n+1)\pi} \\ &= -(2n+1)\pi\sqrt{-1}.\end{aligned}$$

Thus, Poisson clearly thought imaginary substitutions were acceptable in cases where the integrand has infinite values. To stress the difference between these two kinds of integrals, Poisson even argues that if f is integrable between $[a, b]$ and $a < c < b$,

$$\int_a^b f(x)dx = \int_a^c f(x)dx + \int_c^b f(x)dx$$

holds only when f is finite over $[a, b]$ (1820, p. 324–325). In general, Poisson thought that any integral whose integrand becomes infinite between the limits of integration only once would be imaginary (1820, p. 324–325). Interestingly, he also points out that real integrals with a finite integrand, those "being seen as the sum of the values of the differential, between the limits of integration," can have different values if the variable of integration is given imaginary values. As an example, he considers

$$y := \int_{-\infty}^{+\infty} \frac{\cos axdx}{b^2 + x^2}, \tag{6.12}$$

where $a, b \in \mathbb{R}$ (1820, p. 330–333). Substituting x for $t + k\sqrt{-1}$ where t is a real variable and $k \in \mathbb{R}$, he obtains after algebraic manipulations

$$y = \frac{\pi}{2b}\left(e^{-ab} - e^{ab}\right), \quad \text{if } k > b$$
$$y = \frac{\pi}{b}e^{-ab}, \quad \text{if } k < b.$$

For Poisson, "[t]his remark, which, it seems to me, had not been made yet, was necessary to prevent the difficulties which could present the usage of imaginary quantities in the theory of definite integrals." From this, Poisson seems to conclude that imaginary substitutions are to be banned when evaluating integrals of finite integrands. But he does not conclude that the problem has something to do with the infinitudes in the integrands as Grattan-Guinness, (1990, p. 734) too quickly concludes. Not only does (6.12) have no infinities; a few pages later, Poisson notices that the same applies to

$$y := \int_{-\infty}^{+\infty} \frac{x \tan ax}{x^2 + b^2} \cdot dx \tag{6.13}$$

whose integrand has infinite values infinitely many times. When one does $x = t + k\sqrt{-1}$ for $k \in \mathbb{R}$: if $k > b$, $y = \pi$ and if $b > k$, $y = 2\pi/(e^{2ab} + 1)$ (1820, p. 334–336). Hence, imaginary substitutions, while useful in dealing with infinite integrands, can lead us astray with both kinds of integrals. Poisson does not propose an alternative method and even continues to use them to solve other integrals in the same paper. He indeed "had discovered *something*, without being in full control of it" Grattan-Guinness, 1990, p. 734. Poisson would make a similar comment later (1823, p. 460). His remarks illustrate the potential problems created by imaginary substitutions in the context of integration. Cauchy might have been aware of these specific problems as early as 1814 when he published his *Mémoire*; but he certainly mistrusted them.

Poisson still suggested using imaginary substitutions, more by necessity, for integrals whose integrands have infinite or indeterminate values at certain points. Since he thought they could not be seen as infinite sums of differentials and that the "fundamental theorem of calculus" did not hold for them, an imaginary substitution was probably for him nothing else than a formal algebraic trick to compute integrals. Crucially then, a more severe blow to their use would be to extend the Leibnizian conception of integrals to these problematic cases; for then, under this reading of '$\int$', making the variable of integration go through imaginary values could very likely affect the infinite sum of differentials. This is what Cauchy had started doing as early as 1814 in his *Mémoire*, *contra* Guitard, (1987, p. 216), by using what he called "singular integrals" (1814, p. 394) definite integrals between infinitely small limits of integration. This technique was positively welcomed by A.-M. Legendre and Lacroix, (1814, p. 325) and met with skepticism by Poisson, who thought "these integrals are not presented here for the first time" and Cauchy's "curious usage" was a "indirect way of obtaining them [known results] which should not be preferred to ordinary methods" (1814, p. 187). Cauchy's handling of such integrals prompted him to say that it "shows once and for all that we should not reject integrals in which the functions under the sign $\int$ goes through the infinite" (1814, p. 434).

Finding acceptable ways to deal with the integration of functions reaching the infinite between two limits was also a concern of Cauchy in two notes on "reciprocal functions" (1817; 1818), functions f and ϕ who satisfy equations like

$$f(x) = \left(\frac{2}{\pi}\right)^{\frac{1}{2}} \int_0^\infty \phi(\mu)\cos(\mu x)d\mu, \tag{6.14}$$

and who, as him and Poisson showed, also verify the equations obtained by replacing simultaneously f with ϕ and ϕ with f. As he shows, these reciprocal functions can be used to solve definite integrals while getting around "any objections one could make against [this] method, in the case where [the integrand] becomes infinite for real values of x" (1817, p. 226).

More importantly, later, in the addition to his "Mémoire sur l'intégration des équations linéaires aux différentielles partielles et à coefficients constants" (1823), Cauchy restates his definition of integral as the sum between two limits of infinitely small differential values (1823, p. 333–334). The usual worry is that "this value will be, in many cases, infinite or indeterminate." To solve this issue he proposes again using singular integrals, referring to his (1814) *Mémoire* and to another one delivered at the *Académie des Sciences*, possibly this one published later (1844). Having argued that methods of evaluating integrals based on the development of series are insufficient (1822, p. 280), notably with his counterexample $e^{-\frac{1}{x^2}}$ to Lagrange's claim that any function can be developed into its Maclaurin series, Cauchy makes the following proposal (1822, p. 284–290). If $f(x)$ becomes infinite or indeterminate for $x = x_0$, k is an infinitely small number, $f_0 := k(fx_0 + k)$, α', α'' two positive constants, the singular integral of $f(x)$ at x_0 is

$$\int_{x_0+k\alpha'}^{x_0+k\alpha''} f(x)dx \tag{6.15}$$

and is equal to

$$f_0 \log\left(\frac{\alpha''}{\alpha'}\right). \tag{6.16}$$

Cauchy notes that

$$\int_{x_0+k\alpha'}^{x_0+k\alpha''} f(x)dx = \int_{x_0-k\alpha'}^{x_0-k\alpha''} f(x)dx$$

except when $kf(x_0+k)$ and $-kf(x_0-k)$ converge to two different limites. In other words, except when

$$\lim_{x\to x_0^-} f(x) \neq \lim_{x\to x_0^+} f(x).$$

More generally, if $f(x)$ becomes indeterminate on $[x', x'']$ at $x_0, x_1, x_2, \ldots, x_{n-1}$, k is infinitely small, $\alpha', \alpha'', \beta', \beta'', \ldots, \epsilon', \epsilon''$ are positive quantities, the general value of the integral of $f(x)$ between x' and x'' is defined as

$$\int_{x'}^{x''} f(x)dx := \int_{x'}^{x_0-k\alpha'} f(x)dx + \int_{x_0+k\alpha''}^{x_1-k\beta'} + \cdots + \int_{x_{n-1}+k\epsilon''}^{x''} f(x)dx. \tag{6.17}$$

If we let $\alpha' = 1, \alpha'' = 1, \beta' = 1, \beta'' = 2, \ldots, \epsilon' = 1, \epsilon'' = 1$, we get a particular value of the integral of $f(x)$ which Cauchy calls "valeur principale"

$$\int_{x'}^{x''} f(x)dx = \int_{x'}^{x_0-k} f(x)dx + \int_{x_0+k}^{x_1-k} + \cdots + \int_{x_{n-1}+k}^{x''} f(x)dx. \tag{6.18}$$

If (6.17) := A and (6.18) := B, we get

$$A = B + f_0 \log\left(\frac{\alpha'}{\alpha''}\right) + f_1 \log\left(\frac{\beta'}{\beta''}\right) + \cdots + f_{n-1}\left(\frac{\epsilon'}{\epsilon''}\right).$$

Cauchy's handling of integrands being indeterminate or infinite between the limits of integration is summed up in that theorem.

Theorem 6.2. *If $f(x)$ becomes indeterminate on $[x', x'']$ at $x_0, x_1, x_2, \ldots, x_{n-1}$, k, $\alpha', \alpha'', \beta', \beta'', \ldots, \epsilon', \epsilon''$ are positive quantities, A the general value of the integral of*

$f(x)$ between x' and x'' and B its principale value, then

$$A \text{ is finite and determinate} \quad \text{if and only if} \quad \lim_{k \to 0}(A - B) = 0.$$

Cauchy then shows many applications of this theory for complex functions (see Bottazzini and Gray, (2013, p. 119–121) and Smithies, (1997, p. 73–76)), some of which had previously been obtained by Poisson, (1822, p. 138–139). He also justifies his approach to definite integrals by citing Poisson's result (1820, p. 328–329) that

$$\int_0^\infty \frac{\cos ax}{x^2 - b^2} \cdot dx \tag{6.19}$$

is equal to

$$-\frac{\pi}{2b} \cdot \sin ab - \frac{1}{2b} \cdot \cos ab \cdot \log(-1).$$

Cauchy thought that Poisson's reliance on primitive functions had been wrong somewhere since the real integral (6.19) had an imaginary value, which Poisson justified not by an imaginary change of variable Smithies, 1997, p. 75, but by the fact that $\cos ax/(x^2 - b^2)$ becomes infinite only once between 0 and ∞, when $x = b$. Instead, Cauchy proposes that (6.19)'s general value is given by

$$\int_0^\infty \frac{\cos ax}{x^2 - b^2} \cdot dx = \int_0^{1-k\alpha'} \frac{\cos ax}{x^2 - b^2} \cdot dx + \int_{1+k\alpha''}^\infty \frac{\cos ax}{x^2 - b^2} \cdot dx.$$

This gives the following formula where $m = \alpha'/\alpha''$

$$\frac{\pi}{2b} \left(\cos ab \log m - \sin ab\right).$$

Since the principal value of (6.19) is obtained when $\alpha' = \alpha'' = 1$, we get $m = 1$ so $\log m = 0$ and thus its principal value is

$$-\frac{\pi \sin ab}{2b}.$$

We then see how Cauchy opposed the passage from reals to imaginaries in contexts of integration, the specific case we were concerned with in this paper. Imaginary substitution sometimes give imaginary values to real integrals and often give contradictory results depending on which imaginary function one chooses when substituting for x.

These difficulties arising in all kinds of integrals, as Poisson noticed, illustrated why mathematicians should feel uneasy at the time using them. Extending the Leibnizian conception of integrals to real and complex integrals, even ones whose integrand has infinite or indeterminate values, was a decisive step for Cauchy in casting doubt on their use.

7 Concluding Remarks

In this discussion, my two specific questions were

(Q_1) Why can one not rely on the passage from the reals to the imaginaries according to Cauchy?

(Q_2) How does Cauchy believe one should deal with imaginaries and their relation to real numbers?

As an answer to (Q_1), we have seen that in carrying the passage from reals to imaginaries, one might mistakenly assume algebraic properties of real functions hold for imaginary functions; and one might end up with multiple values for the same definite integral depending on which imaginary function they use for their substitution. Moreover, Cauchy dealt with imaginaries by clearly distinguishing real from complex analysis; reducing many imaginary notions to their real counterparts; eliminating as much as possible $\sqrt{-1}$ from his reasonings; and insisting that one could read a variable x as having imaginary values only if they had a proof that the relevant manipulations hold of real and imaginary values, which answers (Q_2).

Let us return to our question in the introduction: does Cauchy's early rigour in complex analysis lend support to deductivism, the idea that mathematics is rigorous mainly because it is deductive? In a sense, one could expect the answer to be negative. For as Hilbert said (1925, p. 370), 17^{th}–18^{th} century mathematicians' uneasiness with imaginary numbers arose not because they entailed a contradiction, but because imaginaries were impossible objects. Accordingly, an important rigorization of the field at the time would have to specify in some way what imaginary expressions were about, the subject of complex analysis, in a satisfactory manner. In other words, rigorization had to be *semantical*, not *syntactical*.

This is what Cauchy did by stipulating that '$\alpha + \beta\sqrt{-1} = \gamma + \delta\sqrt{-1}$' means $\alpha = \gamma$ and $\beta = \delta$. However, it is important not to misrepresent the attitudes Cauchy's predecessors had towards imaginaries. Laplace saw them as meaningless but useful tools which enter calculations in a formal language and destroy themselves at the end of it. Like many others, he had no problem using imaginary expressions while at the same time believing that they could not be read like real expressions. Concerns with imaginary numbers in France at the time were more about how and when they could legitimately be used — as the back-and-forth between Laplace and Poisson on imaginary substitutions in definite integrals illustrates — than about their ontology.

As we saw, these methodological, not ontological, issues with imaginaries prompted Cauchy to develop complex analysis as a separate field of inquiry and to explore its relation with real analysis. Doing so required first giving some kind of foundation for both field and then investigate how they relate. Following Marquis, 1995, we can distinguish six relations 'being a foundation of' can express. If S and T are two systems, **Found**(S, T) can mean one of the following:

LogFound(S, T) S is a *logical* foundation of T, S is an axiomatization of T.

CogFound(S, T) S is a *cognitive* foundation for T, S states the cognitive processes required for understanding T.

EpiFound(S, T) S is an *epistemological* foundation for T, S guarantees the truth of T.

SemFound(S, T) S is a *semantical* foundation for T, S specifies what the language of T refers to.

OntFound(S, T) S is an *ontological* foundation for T, S states what kind of entities and their modes of being T has.

MetFound(S, T) S is an *methodological* foundation for T, S gives the methods to create and prove things about entities in T.

Treating each of these as *ideal types* (Weber, 1904), we can ask what kind of foundation for complex analysis best characterizes Cauchy's new rigour. It is certainly neither logical, nor epistemological and nor cognitive, which would have amounted more to the kind of story of the genesis of mathematics Condillac attempted to give. Marquis' distinction between semantical and ontological foundations is hard to fully understand; but one emphasizes carefully specifying the meaning of a mathematical language, while the other a detailed metaphysical discussion of the subject of a mathematical discipline, independently of the language its practitioners use. In that light, Cauchy's foundational stance is more semantical than ontological. That he takes time to distinguish between the two equations

$$\cos(a+b) + \sqrt{-1}\sin(a+b) \\ = \cos a \cdot \cos b - \sin a \cdot \sin b + \sqrt{-1}(\cos a \cdot \sin b + \sin a \cdot \cos b)$$

and

$$\cos(a+b) + \sqrt{-1}\sin(a+b) \\ = (\cos a + \sqrt{-1}\sin a) \cdot (\cos b + \sqrt{-1}\sin b),$$

only the first one being meaningful since it is of the form $\alpha+\beta\sqrt{-1} = \delta+\gamma\sqrt{-1}$, illustrates that his worry was more semantical than ontological. His "rigorous though cumbersome procedure" explained above to define A^z, Lz, $\sin z$, $\cos z$, $\arcsin z$, $\arccos z$, when z is complex, also illustrates that point. Cauchy was clear in his introduction (1821, p. iij): "by fixing in a precise manner the meaning of the notations I use, I make all uncertainty disappear."

Cauchy's semantical foundation of complex analysis had consequences for how one can use imaginary numbers. For one, imaginary substitutions in real definite integrals had now to be justified with a proof. More significantly, once a clear distinction between real and imaginary mathematical objects, e. g. variables, functions, series, integrals, was made, then assuming that what holds of one holds of the other could no longer be done uncritically. Also, a new methodological principle Cauchy stressed was to never introduce any notation that had not been previously carefully defined on the basis of other definitions.

Still, the impressive systematization and organization Cauchy displays in his *Cours*, *Résumé* and later work in complex integration is an essential part of his new rigour Sinaceur, 1973, p. 109. Since he defined real notions on the basis of the notion of limit; and complex notions on the basis of their real counterpart; Cauchy's edifice of definitions carefully built one on the top of the other might be seen as improving the deductive methods of mathematics at the time. Similarly, that he frequently attempted to prove general theorems in order to obtain new ones, build definitions or apply them to specific problems, certainly reflects a key element of the axiomatic method. Why can one not see these definitions, coupled with the few axioms he discusses in his Note I at the end of his *Cours*, as evidence that Cauchy's rigorous approach is to a certain extent logical and axiomatic? Was Cauchy's rigour a kind of axiomatics without explicit use of axioms Koetsier, 1991, p. 218?

Even if we use a broad understanding of axioms and their roles (Schlimm, 2013), it would be more misleading than enlightening to qualify Cauchy's work as axiomatic, precisely because it lacks explicit use of axioms or principles. The use of axioms from the 17$^{\text{th}}$ to the early 19$^{\text{th}}$ century seems confined to geometry: even Newton's axioms for calculus were deeply rooted in his geometrical interpretation of the concepts of calculus, as they stated conditions under which geometrical figures could be constructed by the movement of bodies Sepkoski, 2002, p. 251, (2005, p. 51–55), Guicciardini, 2003, p. 417–418. My suspicion is that while mathematicians moved away from Newton's geometrical calculus to Leibniz's formal calculus, the explicit use of axioms in calculus disappeared.

A better example of a logical or deductive rigorization happening roughly at the same time as Cauchy, also driven by issues arising with imaginary numbers, would perhaps be Peacock's *A Treatise on Algebra* (1830), probably inspired by Babbage's unpublished collection of essays *The Philosophy of Analysis* (1821) (Dubbey, 1977) or even in Woodhouse's *The Principles of Analytical Calculation* (1803) (Becher, 1980). Peacock even criticizes Cauchy's axioms as presented in the *Cours*, arguing that Cauchy unjustifiably generalizes arithmetical axioms, definitions and theorems to their algebraical versions and does not distinguish between axioms, assumptions and theorems in a satisfactory manner (1834, p. 192–193). Part of why Peacock and Cauchy disagree is because the former saw algebra more like an uninterpreted calculus in need of quasi-arbitrary rules for manipulation, while the latter saw it as a more general arithmetic of quantities. Comparing the two figures on axioms is beyond the scope of this paper.

In sum, there are no axioms in the sense of Aristotle (), Euclid, (n.d.) or in the modern sense (Hintikka, 2011) in Cauchy. Instead, we need to appreciate his definitions *qua* definitions. Why were Cauchy's definitions good definitions? Perhaps because they were or were tending to the "natural" or "proper" (Tappenden, 2008a; Tappenden, 2008b) definition of real and complex integrals. This presupposes that there is *one* proper definition of integration and that Cauchy found it or was on its way to. Not only does this explanation seem guilty of the "foundationalist filter" of screening off past mathematics as not-yet-achieved proto-mathematics Corfield, 2003, p. 8; the immense gap between Cauchy's definitions and the current set theoretical ones shows it to be at best doubtful.

For example, Rudin, 1953, p. 121–122, if $[a, b] \subseteq \mathbb{R}$, a partition P of $[a, b]$ is a finite set $P \subseteq [a, b]$ of points $x_0, x_1, \ldots, x_n$ such that $a = x_0 \leq x_1 \leq \cdots \leq x_{n-1} \leq x_n = b$. For any $i = 1, \ldots n$, we let $\Delta x_i := x_i - x_{i-1}$. Granted that f is bounded on $[a, b]$, we define

$$
\begin{aligned}
M_i &:= \sup f\left([x_{i-1}, x_i]\right) & m_i &:= \inf f\left([x_{i-1}, x_i]\right) \\
U(P, f) &:= \sum_{i=1}^{n} M_i \Delta x_i & L(P, f) &:= \sum_{i=1}^{n} m_i \Delta x_i \\
\overline{\int_a^b} f dx &:= \inf U(P, f) & \underline{\int_a^b} f dx &:= \sup L(P, f).
\end{aligned}
$$

If the two last upper and lower integrals are equal, then we say that f is Riemann integrable and their common value is the value of $\int_a^b f dx$. If $f(x) = \phi(x) + \chi(x)\sqrt{-1}$ and ϕ, χ are Riemann integrable on $[a, b]$,

$$
\int_a^b f dx := \int_a^b \phi(x) dx + \sqrt{-1} \int_a^b \chi(x) dx. \tag{7.1}
$$

The notion of integration along a path in the complex plane can be done in many ways, all of which are even further removed from Cauchy's, e. g. Rudin, 1970, p. 202, Barry, 2015b, p. 40–43. If $\Omega \subseteq \mathbb{C}$ is open and connected, a *curve* in Ω is a continuous function $\gamma : [a, b] \mapsto \Omega$. A *path* on $[a, b] \subset \mathbb{R}$ for $a < b$ is a piecewise continuously differentiable (or smooth) function on $[a, b]$, that is, there is a partition P of $[a, b]$ such that γ has a continuous derivative on $[x_{i-1}, x_i]$ (γ may be discontinuous at any x_i). Given a partition P of $[a, b]$ and a curve γ,

$$
\ell_P(\gamma) := \sum_{i=0}^{n-1} \left|\gamma(t_{i+1}) - \gamma(t_i)\right|. \tag{7.2}
$$

Letting $\ell(\gamma) := \sup P \ell_P(\gamma)$, if $\ell(\gamma) < \infty$, one can show that

$$\ell(\gamma) = \lim_{n\to\infty} \sum_{i=0}^{n-1} \left| \gamma\left(\frac{i+1}{n}\right) - \gamma\left(\frac{i}{n}\right) \right|.$$

Letting f be continuous on the range of γ, the integral of $f(z)$ along the curve γ is defined as follow:

$$\begin{aligned} \oint_\gamma f(z)dz &:= \int f(\gamma(t))d\gamma(t) \qquad (7.3) \\ &= \lim_{n\to\infty} \sum_{i=0}^{n-1} f\left(\gamma\left(\frac{i}{n}\right)\right)\left[\gamma\left(\frac{i+1}{n}\right) - \gamma\left(\frac{i}{n}\right)\right]. \end{aligned}$$

If γ is a path, we have

$$\oint_\gamma f(z)dz = \int_a^b f(\gamma(t))\gamma'(t)dt. \qquad (7.4)$$

As one can see, (5.13) and (7.4) differ in many, many ways: use of supremums and infimums, reliance on topological notions, a broader understanding of paths, etc. But when Cauchy distanced himself from the formalist definition of integrals as the opposite of differentials, was that not an important step towards our current understanding? Maybe, but it depends what we mean by 'towards': it might have been an important event in the history of integration which sparked more work that culminated in (7.4); but not much of our modern conception prefigures in Cauchy's definition, (5.13) does not naturally lead to (7.4) or even makes it necessary. It is hard to see how Cauchy's definitions are a step towards this more "natural" or "proper" definition of integration along a path. Moreover, this perspective on Cauchy's work is problematic in that it assumes some kind of determinism in mathematics, i. e. that complex integration was inevitably moving in that specific direction.

A classic example of an alternative way of doing analysis is Robinson's non-standard analysis (1966). Robinson's idea is to show that "Leibniz's ideas [of infinitesimals] can be fully vindicated and that they lead to a novel and fruitful approach to classical Analysis and to many other branches of mathematics" (1966, p. 2). He does so by building a non-standard model $\mathbb{R}^* \supsetneq \mathbb{R}$ which contains infinite numbers a such that for all $b \in \mathbb{R}$, $b < a$. If a, c are such numbers, then for any $b \in \mathbb{R}$, $1/a < b$,

$1/c < b$, thus $1/a$ and $1/c$ are *infinitesimals* and infinitely close to each other, (written $1/a \simeq 1/c$). His strategy is then to prove for any definition or theorem in standard analysis that one can rephrase it in non-standard analysis using infinitesimals. For example, if '$\mathbb{R} \vDash s$' stands for s is true of the standard real field and '$\mathbb{R}^* \vDash s$' s is true in the non-standard real field,

$$\begin{aligned}
\mathbb{R} \vDash \lim s_n = s \quad &\Leftrightarrow \quad \mathbb{R}^* \vDash s_n \simeq s, \text{for all } n \in \mathbb{N} * -\mathbb{N}, \\
\mathbb{R} \vDash f \text{ is continuous at } x_0 \quad &\Leftrightarrow \quad \mathbb{R}^* \vDash f(x) \simeq f(x_0) \text{ for all } x \simeq x_0, \\
\mathbb{R} \vDash \frac{df}{dx} = c \quad &\Leftrightarrow \quad \mathbb{R}^* \vDash \frac{f(x) - f(x_0)}{x - x_0} \simeq c \text{ for all } x \sim x_0.
\end{aligned}$$

Integration is done in a similar way (1966, p. 71–72). Let f be a standard function continuous on $[a, b]$ where $a < b$. A fine partition P^* of $[a, b]$ is a sequence $\{x_\omega\}$ for $\omega \in \mathbb{N}^*$ where $a = x_0 < x_1 < \cdots < x_\omega = b$ and $x_i - x_{i-1} := \Delta x_i$ is infinitely small for all $i = 1, 2, \ldots, \omega$. Let $\{\zeta\}$ be such that $x_i \leq \zeta_{i+1} \leq x_{i+1}$ for $i = 0, 1, \ldots, \omega - 1$. Then the infinitesimal sum relative to $[a, b]$, f, P^* and ζ is defined

$$S_a^b(f(x), P^*, \zeta) := \sum_{i=1}^{\omega} f(\zeta_i)\Delta_i. \tag{7.5}$$

As expected, we get the following.

Theorem 7.1. *If f is Riemann integrable on $[a, b]$, then for any finite partition P^* and suitably chosen ζ,*

$$\mathbb{R} \vDash \int_a^b f(x)dx = S_a^b(f(x), P^*, \zeta),$$

where '$S_a^b(f(x), P^*, \zeta)$' interpreted in $\mathbb{R}$ is suitably chosen by a Robinson-procedure on the basis of how '$S_a^b(f(x), P^*, \zeta)$' is interpreted in $\mathbb{R}^*$

Proof. Let f be Riemann integrable on $[a, b]$. Then, for any non-zero $\epsilon \in \mathbb{R}$, there is a non-zero $\delta \in \mathbb{R}$ which determines partition P of $[a, b]$ where $\Delta x_i \leq \delta$ for $i = 1, \ldots, n$ such that

$$\mathbb{R} \vDash \left| \int_a^b f(x)dx - \sum_{i=1}^{n} f(\zeta_i)\Delta_i \right| < \epsilon.$$

Clearly, the same statement holds in $\mathbb{R}^*$. Thus, if we let P^* be a fine partition such that $P \subset P^*$, we obtain

$$\mathbb{R}^* \models \left| \int_a^b f(x)dx - \sum_{i=1}^{\omega} f(\zeta_i)\Delta_i \right| < \epsilon.$$

Thus, we have $\int_a^b f(x)dx \simeq S_a^b(f(x), P^*, \zeta)$ in $\mathbb{R}^*$, which entails

$$\mathbb{R} \models \left| \int_a^b f(x)dx - S_a^b(f(x), P^*, \zeta) \right| \epsilon,$$

showing that the two are equal since ϵ was arbitrary. ♦

Though I have not found papers dealing specifically with complex non-standard integration, it can easily be done in a similar way, where $S_b^a(f(x), P^*, \zeta) = S_a^b(\phi(x), P^*, \zeta) + \sqrt{-1}S_a^b(\chi(x), P^*, \zeta)$ if $f(x) = \phi(x) + \chi(x)\sqrt{-1}$.

Theorem 7.2. *If $f(x) = \phi(x) + \chi(x)\sqrt{-1}$, then the following holds by **Thm 7.1**.*

$$\mathbb{C} \models \int_a^b f(z)dz = S_b^a(f(z), P^*, \zeta).$$

Proof. In $\mathbb{C}$ we have

$$\begin{aligned}\int_a^b f(z)dz &= \int_a^b \phi(x)dx + \sqrt{-1}\int_a^b \chi(y)dy \\ &= S_a^b(\phi(x), P^*, \zeta) + \sqrt{-1}S_a^b(\chi(y), P^*, \zeta) \\ &=: S_b^a(f(z), P^*, \zeta).\end{aligned}$$

♦

One can then define non-standard integration along a path this way:

$$S_\gamma(f, P^*, \zeta) := S_a^b(f\gamma(t)\gamma'(t), P^*, \zeta). \tag{7.6}$$

To make sure that the definition is justified, we prove the following theorem.

Theorem 7.3. *If $\Omega \subseteq \mathbb{C}$, γ a curve in Ω for some $[a, b] \subseteq \mathbb{R}$ where $a < b$, f is continuous on $\gamma([a, b])$, P^* a fine partition of $[a, b]$ and ζ appropriately chosen, then,* *by* ***Thm 7.2***,

$$\mathbb{C} \vDash \oint_\gamma f(z)dz = S_\gamma(f, P^*, \zeta)$$

Proof. In $\mathbb{C}$ we have

$$\begin{aligned}\oint_\gamma f(z)dz &= \int_a^b f(\gamma(t))\gamma'(t)dt. \\ &= S_a^b(f\gamma(t)\gamma'(t), P^*, \zeta) \\ &=: S_\gamma(f, P^*, \zeta).\end{aligned}$$

♦

Our question, then, becomes: was Cauchy's definition of integral between imaginary limits a step toward standard complex integration along a path or its non-standard equivalence. That is, when Cauchy said

$$\int_{x_0+y_0\sqrt{-1}}^{X+Y\sqrt{-1}} f(z)dz := \int_{t_0}^{T} \left(\phi'(t) + \sqrt{-1}\chi'(t)\right) f\left(\phi(t) + \sqrt{-1}\chi(t)\right) dt,$$

did his definition prefigured, or naturally lead to, the proper natural definition

$$\mathbb{C} \vDash \oint_\gamma f(z)dz := \int_a^b f(\gamma(t))\gamma'(t)dt.$$

or

$$\mathbb{C}^* \vDash S_\gamma(f, P^*, \zeta) := S_a^b(f\gamma(t)\gamma'(t), P^*, \zeta) \quad ?$$

The question does not only depend on which of (7.4) or (7.6) is the *natural* definition: it also depends on which one is more similar in spirit to Cauchy's, in its construction, in the model he had in mind ($\mathbb{C}$ or $\mathbb{C}^*$), in its use, etc, a highly debated topic (see footnote 16). While this might not be the most historically illuminating question (Fraser, 2015), even if it turned out that one or the other was closer to Cauchy's, we would still be hard pressed to say why it is its natural offspring.

A more natural (!) way to explain the significance of Cauchy's definitions is to look not at how they relate to the work of mathematicians 200 years later, but to that of his immediate predecessors and peers. As Grabiner noticed (1978, p. 381) in the case of his definition of the derivative, Cauchy's achievement lies not so much in his definitions but in the way he uses them to prove an extended body of important old and new results. We saw this in the case of his definition of integration in the complex plane, from which he easily proved the Cauchy integral theorem and many more results. This was (and still is (Parameswaran, 2010)) an important factor in justifying definitions.

An other virtue of Cauchy's definitions, which he himself notices for his definition of real integration, is that it easily carries into other emerging fields. This important feature has been usually ignored by historians and philosophers. If the integral of a function is simply the opposite of its derivative, its primitive function, then it is not obvious how one defines the integral of a complex function $f : \mathbb{R} \mapsto \mathbb{C}$ because functions can have multiple primitives. Suppose $f(x) = \phi(x) + \psi(x)\sqrt{-1}$ and F, Φ, Ψ are respectively primitive functions of f, ϕ, ψ, then we get

$$\int f(x)dx = F(x) + c. \tag{7.7}$$

On the other hand,

$$\begin{aligned}\int f(x)dx &= \int \left(\phi(x) + \psi(x)\sqrt{-1}\right) dx \\ &= \int \phi(x)dx + \sqrt{-1} \int \psi(x)dx \\ &= \Phi(x) + a + \sqrt{-1}\left(\Psi(x) + b\right).\end{aligned} \tag{7.8}$$

So our question is whether or not

$$F(x) + c = \Phi(x) + a + \sqrt{-1}\left(\Psi(x) + b\right), \tag{7.9}$$

which is not obvious given the multiplicity of primitive functions a function can have, especially when we make the integral definite. Moreover, which one of (7.7) or (7.8) would be the correct definition of $\int f(x)dx$? As Cauchy's definition relies on summations, the summation of f can easily be split into the two summations of ϕ and ψ which gives us the definiens of $\int f(x)dx$. And as we saw, it was also then easy to extend this definition to $\int f(z)dz$.

Werndl, (2009) has listed three other different ways in which a definition can be justified: because it is equivalent in an interesting way to another good definition; because it eliminates a redundant clause in an already good definition; and, importantly for our purpose, because it captures a pre-formal idea valuable to describe the world [or some models]. This last one is often explained in terms of a concept definition capturing a concept image (Tall and Vinner, 1981). Cauchy seems to have thought it important that integrals of real function capture the idea of representing the area under a curve, even in the case of indeterminate or infinite integrands. He probably felt uneasy with Poisson's view that some real definite integrals can have complex values for such integrands, which prompted him to develop his theory of singular integral. Putting the Leibnizian integral back on the table with new methods to deal with problematic cases is an important part of his new approach to real and complex analysis.

In sum, the new rigor Cauchy displays in his early work in complex analysis is, while multi-faceted, first and foremost a precise stance about the semantics of real and complex discourses, with the help of a structured edifice of proof-generating definitions extending in new fields of mathematics. None of this amounts to improving the deductive methods of mathematics strictly speaking, even if it had an impact in improving mathematical reasoning. A particular attention to the meaning of mathematical formulas undoubtedly results into a greater care in their scope of application. Similarly, a conscious effort in distinguishing real and imaginary notions results in a more sophisticated treatment of how they can be applied and how they relate to each other. That is, by being clear about what mathematical languages are about, by fixing the *subject* of mathematics, Cauchy's reasoning displayed a new level of precision not only in its inferences and proof techniques, but also in its methods and in the kinds of questions and theorems he deemed important.

This was all done without specifying axioms, inference rules and hidden assumptions. Perhaps, one could explain Cauchy's approach in a more syntactic way, by listing statements of his predecessors he rejected (those expressing the generality of algebra?) and those which he accepted as more basic given his new approach to mathematical languages. But that would be to totally miss how him and his predecessors *practiced* mathematics, which includes not merely the results they obtained and proofs they made but also how they interpreted them. Recasting Cauchy's rigour deductively obscures his achievement and our main finding that his mathematics was rigorous because it was made of a precise language and had a simple object, namely, quantities or variations in absolute measure of length. Similarly, as this episode in the rigorization of analysis illustrates, depicting mathematics as deductive, or as rigorous in virtue of its being deductive, obscures the many ways in which it can be practiced, its many distinctive aspects, and, crucially, its historical development. Mathematics *is not* a deductive discipline, i. e. it is not a discipline characterized by its being deductive, though it certainly *can be done* deductively.

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