Did Cauchy Plagiarize Bolzano?

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1. Introduction

1. In an elaborate erudite paper* I. GRATTAN-GUINNESS has put forward a case that CAUCHY plagiarized BOLZANO:

In Section 2, he discusses why *if* CAUCHY plagiarized BOLZANO, he did it so badly,

In Section 3, he presents a new limit concept which he calls "limit avoidance",

In Section 4, he mentions some facts from analysis before CAUCHY's time,

In Section 5 he claims that CAUCHY could not have written a so "utterly untypical" work as his *Cours d'Analyse* of 1821 without having been inspired by somebody else,

In Section 6–7 he analyzes the quarrels among French mathematicians around 1800 and CAUCHY's bad character so as to explain psychologically *why* CAUCHY plagiarized BOLZANO,

In Section 8 he discusses whether CAUCHY could have read BOLZANO,

In Section 9 he deals with the personal relations between CAUCHY and BOLZANO.

Here I wish to discuss the specific question set as the title of this paper, whether CAUCHY plagiarized BOLZANO, a question not considered directly by GRATTAN-GUINNESS.

I have to apologize that I am not well enough acquainted with the *chronique scandaleuse* of the French Academy to follow GRATTAN-GUINNESS there. On the other hand I entirely agree with him that a historian is obliged to read *between* the lines**, though I think it just as important to read the lines themselves. In history of mathematics it is also a good idea to understand the mathematics involved.

The question set as the title of the present paper can be put more precisely by asking

whether CAUCHY read BOLZANO,
whether CAUCHY could have learned new things from BOLZANO,
whether these things were so important that he should have cited BOLZANO.

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* I. GRATTAN-GUINNESS, "Bolzano, Cauchy and the New Analysis of the Early Nineteenth Century", Archive for History of Exact Sciences 6 (1970), 372–400.

** p. 387, 17.

It is no sacrilege to ask such questions, even the last one. False ascriptions are a tradition in mathematics; twice I have met opposition when I refuted such ascriptions*.

2. The Style of Cauchy's Text-Books on Calculus**

CAUCHY is credited with having laid the first solid foundations of what is now called Analysis or Calculus. Though this is true, it is not the whole truth, and in a certain sense it is a misleading statement. It is true that mathematicians learned from CAUCHY's *Cours d'Analyse* and other text-books what continuity and convergence were and how to test for them, how to be careful with TAYLOR series and how to estimate their remainders, how to avoid pitfalls when multiplying and rearranging series, how to deal with multivalued functions, how to define differential quotients and integrals, how to be careful with improper and singular integrals, and that they found there the first example of the powerful method that later became standard in analysis and recently has come to be called "epsilontics".

To know what was new in CAUCHY's textbooks on Calculus, we had better listen to his own words, in the Introduction to his Cours d'Analyse***:

> Quant aux méthodes, j'ai cherché à leur donner toute la rigueur qu'on exige en géométrie, de manière à ne jamais recourir aux raisons tirées de la généralité de l'algèbre. Les raisons de cette espèce, quoique assez communément admises, surtout dans le passage des séries convergentes aux séries divergentes, et des quantités réelles aux expressions imaginaires, ne peuvent être considérées, ce me semble, que comme des inductions propres à faire pressentir quelquefois la vérité, mais qui s'accordent peu avec l'exactitude si vantée des sciences mathématiques. On doit même observer qu'elles tendent à faire attribuer aux formules algébriques une étendue indéfinie, tandis que, dans la réalité, la plupart de ces formules subsistent uniquement sous certaines conditions, et pour certaines valeurs des quantités qu'elles renferment. En déterminant ces conditions et ces valeurs, et en fixant d'une manière précise le sens des notations dont je me sers, je fais disparaître toute incertitude; et alors les différentes formules ne présentent plus que des relations entre les quantités réelles, relations qu'il est toujours facile de vérifier par la substitution des nombres aux quantités elles-mêmes. Il est vrai que, pour rester constamment fidèle à ces principes, je me suis vu forcé d'admettre plusieurs propositions qui paraîtront peut-être un peu dures au premier abord. Par exemple, j'énonce dans le chapitre VI, qu'*une série divergente n'a pas de somme;* dans le chapitre VII, qu'*une équation imaginaire est seulement la représentation symbolique de deux équations entre quantités réelles;* dans le chapitre IX, que, *si des constantes ou des variables comprises dans une fonction, après avoir été supposées réelles, deviennent imaginaires, la notation à l'aide de laquelle la fonc-*

* GRATTAN-GUINNESS remarks (p. 398, 5 f.b.) that his "conjecture has aroused considerable adverse criticism before publication". In his lecture on this subject before an audience of mathematicians rather than historians that I attended, it was his mathematics rather than his thesis on CAUCHY that aroused opposition.

** CAUCHY, Oeuvres (2) **3**—**5**.

*** CAUCHY, Oeuvres (2) **3**.

tion se trouvait exprimée, ne peut être conservée dans le calcul qu'en vertu d'une convention nouvelle propre à fixer le sens de cette notation dans la dernière hypothèse; & c. Mais ceux qui liront mon ouvrage reconnaîtront, je l'espère, que les propositions de cette nature, entraînant l'heureuse nécessité de mettre plus de précision dans les théories, et d'apporter des restrictions utiles à des assertions trop étendues, tournent au profit de l'analyse, et fournissent plusieurs sujets de recherches qui ne sont pas sans importance. Ainsi, avant d'effectuer la sommation d'aucune série, j'ai dû examiner dans quels cas les séries peuvent être sommées, ou, en d'autres termes, quelles sont les conditions de leur convergence; et j'ai, à ce sujet, établi des règles générales qui me paraissent mériter quelque attention.

The "generality of algebra" meant that what was true for real numbers, was true for complex numbers, too, what was true for convergent series, was true for divergent ones, what was true for finite magnitudes, held also for infinitesimal ones. Today it is hard to believe that mathematics ever relied on such principles, and since differentials now are only an uneasy remainder of the pre-CAUCHY period, we readily identify CAUCHY's renovation with the progress from "infinitesimal" methods to epsilontics, in spite of CAUCHY's own, much broader, appreciation, by which *all* metaphysics was barred from mathematics. The next generation of mathematicians, who had been brought up with the *Cours d'Analyse*, and the generations after WEIERSTRASS, CANTOR and DEDEKIND, who knew which course the development of analysis was due to take after CAUCHY, put the stress differently than CAUCHY and his generation would have done; at that time, and even more today, people would not properly understand what it meant if you told them that CAUCHY abolished "the generality of algebra" as a foundation stone of mathematics.

I. GRATTAN-GUINNESS has been puzzled by the "untypical" character of CAUCHY's work on Calculus as compared to his production before 1821. It is indeed puzzling. But GRATTAN-GUINNESS might have added that it is untypical even if compared with CAUCHY's work after 1821. The strange thing is that in his research papers CAUCHY never lived up to the standards he had set in his *Cours d'Analyse*. Though he had given a definition of continuity, he never proved formally the continuity of any particular function. Though he had stressed the importance of convergence, he operated on series, on FOURIER transforms, on improper and multiple integrals, as though he had never raised problems of rigor. In spite of the stress he had laid on the limit origin of the differential quotient, he developed also a formal approach to differential quotients like LAGRANGE's. He admitted semi-convergent series and rearrangements of conditionally convergent series if he could use them. He formally restricted multivalued complex functions of x as $\log x$, $\sqrt{x}$, and so on, to the upper half plane, but if he could use them in the lower half plane, he easily forgot about this prescription. CAUCHY looks self-contradictory, but he was simply an opportunist in mathematics, notwithstanding his dogmatism in religious and political affairs. He could afford this opportunism because, with the background of a vast experience, he had a sure feeling for what was true, even if it was not formulated or proved according to the standards of the *Cours d'Analyse*.

Why, then, was the *Cours d'Analyse* so different from his other work? Not because it was more fundamental, but because it was a textbook, in which he not only communicated his results but also made explicit his background experience. CAUCHY was not a lover of foundational research like BOLZANO, but to teach mathematics to beginners, he had to analyze and to present the techniques implicit in his background. A similar situation is common today, when a modern teacher of mathematics will make explicit his logical habits, even though he is not a logician.

There is at least one work of CAUCHY, his theory of determinants of 1812*, which shows the same "untypical" features; it is not to be wondered at that for a long time this was the only textbook on determinants. The most "untypical" CAUCHY of all, however, is found in his marvellous first communication on Elasticity of 1822**, which by its conceptual style towers high above the usual algorithmic swamp in which he moves.

Certainly, one has to be careful with stylistic arguments. If CAUCHY's work had come down to us anonymously, by stylistic arguments we might attribute the *Cours d'Analyse*, the introduction to elasticity, and the remainder of his scientific work to at least three different CAUCHYS; on account of content we might even attribute his work on complex functions also to at least three CAUCHYS, so as to account for the strange phenomenon of periodic amnesia: often he asserts propositions he had recognized as wrong a short time before*** and for 26 years he seems to have forgotten the most important paper he wrote in this field****.

CAUCHY did not live *in vacuo*. He was moved by work of others, and though he made lavish acknowledgements to work of others, we can never be sure whether he cited all sources of his inspiration. By his own testimony we know that LEIBNIZ was inspired to his discoveries in Calculus by work of PASCAL which actually was only weakly related to what LEIBNIZ himself finally achieved; even according to modern standards LEIBNIZ could hardly have been obliged to cite PASCAL on these grounds. In any case from LEIBNIZ' publications we could not guess who among LEIBNIZ' predecessors was the most influential.

To tell from mere stylistic arguments that CAUCHY's *Cours d'Analyse* must have been inspired by essentially other sources than those on complex functions or hydrodynamics, is an utterly dangerous conclusion. I have spent so much time on it because the difference of style between the *Cours d'Analyse* and other work of CAUCHY is indeed striking, and because I. GRATTAN-GUINNESS confesses that this feature was the starting point of his investigation.

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* CAUCHY, Oeuvres (2) **1**, 91–169. (Mémoire sur les fonctions qui ne peuvent obtenir que deux valeurs...) See also Oeuvres (2) **1** 64–90. (Mémoire sur le nombre de valeurs qu'une fonction peut acquérir.)

** CAUCHY, Oeuvres (2) **2**, 300–304.

*** *E.g.* the conditions for development into a series of partial fractions in CAUCHY, Oeuvres (2) **7**, 324–362, and (1) **8**, 55–64, or multivalued functions in CAUCHY, Oeuvres (1), **8**, 156–160 and (1) **8**, 264.

**** A. L. CAUCHY, Mémoire sur les intégrales définies prises entre des limites imaginaires, Paris 1825, 4°, 68 pages. Reprinted in Bull. sci. math. **7** (1874), 265–304; **8** (1875), 43–55, 148–159; due to be reprinted in CAUCHY, Oeuvres (2) **15**.

3. Bolzano's Pamphlet of 1817

The first theorem of BOLZANO's pamphlet* is what is now called CAUCHY's convergence theorem; since a theory of real numbers is lacking, its proof can be nothing but a sham. We will come back to this point.

The next theorem is usually described as the theorem on the existence of the lowest upper bound of a bounded set of real numbers; in fact the only bounded sets considered are lower classes as used in DEDEKIND cuts, so that it would be better to term it the theorem on the *existence of the cut number*. From old times this existence has been used implicitly or explicitly. It was BOLZANO's great idea to prove it. The proof, using a sequence of dichotomies and the "Cauchy convergence criterion", is correct.

The third theorem is about continuous functions f and ϕ with $f(\alpha) < \phi(\alpha)$ and $f(\beta) > \phi(\beta)$; it states the existence of an intermediate x where $f(x) = \phi(x)$. Continuity had been defined in the preface in a perfectly modern way. The theorem is derived by considering the subset of y such that $f(x) < \phi(x)$ for all $x \leqq y$ and by applying the preceding theorem to it. Again it is a merit of BOLZANO to have recognized the idea to prove it.

The last theorem asserts the existence of a real root of a polynomial between two points where its values are of opposite sign.

As compared to CAUCHY's work, BOLZANO's pamphlet is clumsily written and partially confused. BOLZANO has no term for convergence, and none for the limit of a sequence; he always circumscribes the convergence to a certain limit by the sentence that defines this property. Of course he has no term for lowest upper bound either. His terminology is unusual; a sequence of functions is called a *veränderliche Grösse*, and a single function a *beständige Grösse*. The CAUCHY convergence criterion is formulated for a sequence, not of numbers, but of functions, and the property that is formulated, is, in fact, uniform convergence although BOLZANO draws no conclusion from it (*e.g.* with respect to continuity); the criterion is actually applied to numerical sequences only**. The proof of this criterion is worse than faulty, it is utterly confused and not at all related to the thing to be proved. At that time it was, indeed, hard to understand that such a theorem could not be proved without an underlying theory of real numbers; recently published papers of BOLZANO show that later he became aware of this fact.

This failure does not prevent the pamphlet from being a marvellous piece of work; the proofs of the other theorems are correct.

4. The Common Ideas in Bolzano and Cauchy

I am borrowing the titles of this section and of the subsections 1–5 from I. GRATTAN-GUINNESS; his remarks in the corresponding section will be analyzed here.

* B. BOLZANO, *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege* (1817), Prague = Abh. Königl. Böhm. Gesell. Wiss. (3) **5** (1814–1817; publ. 1818), 60 p. — Also in: OSTWALD's Klassiker No. 153, ed. Ph. E. B. JOURDAIN.

** This is dissimulated in I. GRATTAN-GUINNESS' quotation, where the hypothesis of the theorem is replaced with a provisional announcement taken from another section of the pamphlet.

4.1. Continuity of a Function. BOLZANO's and CAUCHY's definitions are equivalent. BOLZANO's is far better; it is modern (though instead of δ and ε he uses ω and Ω); the succession of the quantifiers is correct and clear. CAUCHY's definition uses the language of infinitesimals (an infinitely small increase of the variable produces an infinitely small increase of the functions); even the succession of the quantifiers is not clear in this formulation.

It is hard to explain how CAUCHY, if borrowing the definition of continuity from BOLZANO, could have presented it in deteriorated form; later on such occurrences are explained by I. GRATTAN-GUINNESS as instances of CAUCHY's failure to fathom the depth of BOLZANO's thought. There is, however, not the slightest reason to assume that CAUCHY learned the concept of continuous function from BOLZANO, since it was already instrumental in CAUCHY's* treatise of 1814 on complex functions (the Cauchy integral theorem):

> *Solution.* — Si la fonction $\varphi(z)$ croit ou décroit d'une manière continue entre les limites $z=b'$, $z=b''$, la valeur de l'intégrale sera représentée, à l'ordinaire, par
> $$\varphi(b'')-\varphi(b').$$
> Mais, si, pour une certaine valeur de z représentée par Z et comprise entre les limites de l'intégration, la fonction $\varphi(z)$ passe subitement d'une valeur déterminée à une valeur sensiblement différente de la première, en sorte qu'en désignant par ζ une quantité très petite, on ait
> $$\varphi(Z+\zeta)-\varphi(Z-\zeta)=\Delta,$$
> alors la valeur ordinaire de l'intégrale définie, savoir,
> $$\varphi(b'')-\varphi(b')$$
> devra être diminuée de la quantité Δ, comme on peut aisément s'en assurer.

To within a formal definition the full-fledged idea of continuity is presented not only here; it is also the main idea underlying the introduction of the CAUCHY principal value of singular integrals, which provided CAUCHY's approach to his integral theorem. There can be little doubt that here was CAUCHY's point of departure to continuity.

I. GRATTAN-GUINNESS claims that in 1821 CAUCHY did not know that continuity did not imply differentiability, while BOLZANO knew it. There is no proof for the second claim, and in the light of the role continuity plays in CAUCHY's treatise of 1814, the first claim is ridiculous.

4.2. Convergence of a Series. In the case of the Cauchy convergence criterion CAUCHY's formulation is much better than BOLZANO's. If CAUCHY ever read BOLZANO, and even if he did not understand his confused exposition, the possibility can hardly be excluded that he guessed what BOLZANO meant and consequently arrived at an improved version. Of course, this is no proof that it really happened this way. CAUCHY prepares the announcement of his criterion by a fine heuristic approach which, undoubtedly, is his own**; when reading his exposition, one can

* CAUCHY, Oeuvres (1) **1**, 402–403.

** CAUCHY, Oeuvres (2) **3**, 115–116.

imagine him standing at the blackboard, explaining that for a sum $\sum u_n$ to converge, it does not suffice that the u_n converge to 0, nor does it suffice that the $u_n + u_{n+1}$ converge to 0, nor does it suffice that the $u_n + u_{n+1} + u_{n+2}$ converge to 0, and so on, and that in order to get convergence of the sum you have rather to make all these expressions arbitrarily small by choosing n large.

In today's mathematics this is so natural an approach that one feels little need to ask who invented it, yet in the historical setting the CAUCHY convergence criterion looks like a premature discovery. In fact, if we expect a great many applications of the CAUCHY convergence criterion in CAUCHY's work, we are likely to be disappointed. It is applied at essentially two places:

First, to justify the majorant method of convergence proofs (if $|a_n| \leqq |c_n|$ for almost all n, and if $\sum |c_n|$ converges, then $\sum a_n$ converges), which in the particular case of a geometrical series as a majorant, is the foundation of CAUCHY's famous "*Calcul des limites*" in power series and differential equations,

Second, to prove the convergence criterion on alternating series (if the $|a_n|$ are such that $a_n a_{n+1} \leqq 0$, $|a_n| \geqq |a_{n+1}|$, and $\lim a_n = 0$, then $\sum a_n$ converges).

As soon as these two criteria have been established, the reader of the *Cours d'Analyse* may readily forget about the CAUCHY convergence criterion.

This is not to be wondered at since there was not any other essential use of the CAUCHY convergence criterion up to the rise of the direct methods of the variational calculus at the turn of the 19$^{\text{th}}$ century. The majorant method and the criterion on alternating series as algorithmic tools were just what mathematicians in CAUCHY's time, and even later, needed. The CAUCHY convergence criterion with its much more involved logical structure, lacked this algorithmic appeal. CAUCHY's work in analysis would not have looked different if he had never formulated the CAUCHY convergence criterion and, instead, had accepted the principle of the majorant method and the criterion on alternating series as obvious truths which did not need a proof, just as, for instance, he accepted without argument that the endpoints of a nested sequence of intervals, shrinking to zero, had a limit*.

From CAUCHY's time up to the end of the 19$^{\text{th}}$ century the CAUCHY convergence criterion was an expression of logical profundity rather than a practical tool. This is what I meant when I characterized the CAUCHY convergence criterion as a "premature discovery"—a characterization which at the same time means a praise of its discoverers.

I. GRATTAN-GUINNESS could have made a relatively strong point against CAUCHY out of the argument that the CAUCHY convergence criterion fitted less into CAUCHY's work than anything else. Strangely enough he did not. Though he challenged CAUCHY's originality in much weaker cases, he did not do so in this one, which would have been the strongest.

Though I cannot exclude the possibility that CAUCHY borrowed his convergence criterion from BOLZANO, I stress that I do not see any indication that he actually did so.

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* CAUCHY, Oeuvres (2) **3**, 379; in the proof of the theorem of the intermediate zero of a continuous function.

4.3. Bolzano's Main Theorem. The theorem on the vanishing of a continuous function between two points where its values are of opposite sign is still less fundamental to CAUCHY's Calculus. It is almost self-evident that such a pure existence theorem did not mean much at that time. In CAUCHY's *Cours d'Analyse* it stands in the classical constructive context of solving numerical equations, particularly in connection with a method of LEGENDRE*, cited by CAUCHY**. The theorem itself had long been known. BOLZANO's and CAUCHY's merit is to have proved it. I. GRATTAN-GUINNESS' statement that CAUCHY's proof uses a condensation argument is far off the mark if by "condensation argument" he means what is usually understood by this term. His claim that CAUCHY's proof

> seems very much like an unrigorous version of the intricate proof developed in BOLZANO's paper

is as wrong as can be. The most convincing though somewhat lengthy way to refute this claim is to quote CAUCHY himself***:

> **Théorème I.** — *Soit $f(x)$ une fonction réelle de la variable x, qui demeure continue par rapport à cette variable entre les limites $x=x_0$, $x=X$. Si les deux quantités $f(x_0)$, $f(X)$ sont de signes contraires, on pourra satisfaire à l'équation*
>
> $$f(x)=0 \tag{1}$$
>
> *par une ou plusieurs valeurs réelles de x comprises entre x_0 et X.*
>
> *Démonstration.* — Soit x_0 la plus petite des deux quantités x_0, X. Faisons
>
> $$X-x_0=h,$$
>
> et désignons par m un nombre entier quelconque supérieur à l'unité. Comme des deux quantités $f(x_0)$, $f(X)$, l'une est positive, l'autre négative, si l'on forme la suite
>
> $$f(x_0),\quad f\left(x_0+\frac{h}{m}\right),\quad f\left(x_0+2\frac{h}{m}\right),\quad \ldots,\quad f\left(X-\frac{h}{m}\right),\quad f(X),$$
>
> et que, dans cette suite, on compare successivement le premier terme avec le second, le second avec le troisième, le troisième avec le quatrième, etc., on finira nécessairement par trouver une ou plusieurs fois deux termes consécutifs qui seront de signes contraires. Soient
>
> $$f(x_1),\quad f(X')$$
>
> deux termes de cette espèce, x_1 étant la plus petite des deux valeurs correspondantes de x. On aura évidemment
>
> $$x_0<x_1<X'<X$$
>
> et
>
> $$X'-x_1=\frac{h}{m}=\frac{1}{m}(X-x_0).$$

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* M.-A. LEGENDRE, Essai sur la théorie des nombres. Supplément, février 1816, § III.

** CAUCHY, Oeuvres (2) 3, 381.

*** CAUCHY, Oeuvres (2) 3, 378–380.

Ayant déterminé x_1 et X' comme on vient de le dire, on pourra de même, entre ces deux nouvelles valeurs de x, en placer deux autres x_2, X'' qui, substituées dans $f(x)$, donnent des résultats de signes contraires, et qui soient propres à vérifier les conditions

$$x_1 < x_2 < X'' < X',$$

$$X'' - x_2 = \frac{1}{m}(X' - x_1) = \frac{1}{m^2}(X - x_0).$$

En continuant ainsi, on obtiendra: 1° une série de valeurs croissantes de x, savoir

$$(2) \qquad x_0, \quad x_1, \quad x_2, \quad \ldots;$$

2° une série de valeurs décroissantes

$$(3) \qquad X, \quad X', \quad X'', \quad \ldots,$$

qui, surpassant les premières de quantités respectivement égales aux produits

$$1 \times (X - x_0), \quad \frac{1}{m} \times (X - x_0), \quad \frac{1}{m^2} \times (X - x_0), \quad \ldots,$$

finiront par différer de ces premières valeurs aussi peu que l'on voudra. On doit en conclure que les termes généraux des séries (2) et (3) convergeront vers une limite commune. Soit a cette limite. Puisque la fonction $f(x)$ reste continue depuis $x = x_0$ jusqu'à $x = X$, les termes généraux des séries suivantes

$$f(x_0), \quad f(x_1), \quad f(x_2), \quad \ldots,$$

$$f(X), \quad f(X'), \quad f(X''), \quad \ldots$$

convergeront également vers la limite commune $f(a)$; et, comme en s'approchant de cette limite ils resteront toujours de signes contraires, il est clair que la quantité $f(a)$, nécessairement finie, ne pourra différer de zéro. Par conséquent on vérifiera l'équation

$$(1) \qquad f(x) = 0,$$

en attribuant à la variable x la valeur particulière a comprise entre x_0 et X. En d'autres termes,

$$(4) \qquad x = a$$

sera une *racine* de l'équation (1).

Cauchy's proof is simply a faithful description of the naive procedure for solving equations numerically (the title of this *Note* is "*Sur la résolution numérique des équations*"). The only sophistication is that the length of the unit interval is replaced by a more general h, and the 10 of our decimal system by a general basis m.

The proof is not a version of Bolzano's and it is as rigorous as a proof can be. The only correct remark I. Grattan-Guinness made is that Bolzano's proof is intricate; it goes by way of the existence of the least upper bound of a bounded set (or rather the existence of the cut number); once this existence is presumed, BOLZANO'S proof is more elegant than CAUCHY'S.

Anyhow there is not the slightest need to suppose that CAUCHY took his proof from BOLZANO. The idea, however, that such a theorem needed a proof and could be proved, may well have come from BOLZANO. The title of BOLZANO'S pamphlet could have been enough to inspire CAUCHY to prove the theorem even if he never read the pamphlet itself.

Of course this does not prove that CAUCHY ever saw BOLZANO'S pamphlet.

4.4. Bolzano's Lemma. The corner stone in I. GRATTAN-GUINNESS' case that CAUCHY plagiarized BOLZANO, is the following argument: In his *Cours d'Analyse,* instead of the limit concept, which would have been sufficient, CAUCHY used the concept of upper limit, which was not needed, simply because he found it in BOLZANO'S pamphlet. If this were true, it would, indeed, prove convincingly that CAUCHY knew BOLZANO'S pamphlet.

It was pointed out to I. GRATTAN-GUINNESS that his statement here rests on a few mathematical errors. In I. GRATTAN-GUINNESS' paper we now find a text (section 2.4), which, mathematically and historically, is wrong, as I will show in all details; further, attached to this text, footnote 24, which in fact invalidates the main text, and which is wrong in itself. I will now analyze this paragon of confusion.

As I explained, BOLZANO proved in his pamphlet the existence of the least upper bound of bounded sets of a special kind (DEDEKIND lower classes). I. GRATTAN-GUINNESS quotes BOLZANO'S text and then continues:

> with this extraordinary theorem came another new idea into analysis, completely untypical of its time: the upper limit of a sequence of values.

Speaking of upper limit rather than of least upper bound could be a terminological deviation, since for a long time usage here was unsettled. It is certain, however, that I. GRATTAN-GUINNESS means "upper limit" since he refers to a sequence rather than to a set or a lower class, and since he continues with a reference to a convergence test of CAUCHY, the $\sqrt{u_n}$-criterion for the convergence of $\sum u_n$ (with positive u_n). Here, indeed, the upper limit (that is, in modern terms, the largest accumulation value) is needed and is used. I. GRATTAN-GUINNESS says that the term of upper limit is

> ... not to be found explicitly in Cauchy's *Cours d'Analyse,* but instead we have there a frequent use of phrases like "...the largest value of the expression..."

This is entirely wrong. At one of the places alluded to by I. GRATTAN-GUINNESS we read*

> Cherchez la limite ou les limites vers lesquelles converge, tandis que n croît indéfiniment, l'expression $(u_n)^{1/n}$ et désignez par k la plus grande de ces limites, ou, en d'autres termes la limite des plus grandes valeurs de l'expression

* CAUCHY, Oeuvres (2) 3, 121.

dont il s'agit. La série (1) sera convergente si l'on a $k<1$, et divergente si l'on a $k>1$.

At another place*:

> Cherchez la limite ou les limites vers lesquelles converge, tandis que n croît indéfiniment, l'expression $(\varrho_n)^{1/n}$. Suivant que la plus grande de ces limites sera inférieure ou supérieure à l'unité, la série (3) sera convergente ou divergente.

The alternative definition is here repeated in the proof of the theorem:

> Considérons d'abord le cas où les plus grandes valeurs de l'expression $(\varrho_n)^{1/n}$ convergent...

It is difficult to say which one of the two definitions was operative, since the proofs do not use the explicit value of the upper limit but only its being <1 (or >1), that is, the existence of an U such that $(u_n)^{1/n}<U<1$ for almost all n ($(u_n)^{1/n}>U>1$ for infinitely many n). Contrary to I. GRATTAN-GUINNESS' statement the term of upper limit *(la plus grande de ces limites)* is explicit in CAUCHY'S text. On the other hand the plural form and the context *"la limite des plus grandes valeurs de l'expressions"* clearly show that this is not CAUCHY'S terminology for the upper limit as suggested by I. GRATTAN-GUINNESS' quotation "the largest value of the expression..." Cut out this way from CAUCHY'S text by I. GRATTAN-GUINNESS, it is meaningless because it does not allow the hidden quantifiers to be traced.

It does not matter too much what artificially isolated pieces of a text mean if the text is globally clear; in the present case it is not far-fetched, and it is in agreement with the global text to assume that "la plus grande valeur" applies to a finite set, to wit the set of $(u_n)^{1/n}, \ldots, (u_{n+k})^{1/n+k}$, and the plural is to indicate that all such sets are considered.

I. GRATTAN-GUINNESS continues:

> As with continuity of a function, CAUCHY was revealingly only partially aware of the significance of the idea; for he used it only as a tool for developing the proofs of his particular theorems and not as a profound device for investigating more sophisticated properties of analysis. Therefore it would be especially surprising if it were CAUCHY'S own invention...

Everybody who is not a stranger to calculus knows that there is no other use of upper limits than just those theorems where CAUCHY used them. Even today they provide an unusual and ineffective device. The conclusion that it was not CAUCHY'S invention because he used it too little is consequently mistaken. I. GRATTAN-GUINNESS still suggests that CAUCHY took this tool from BOLZANO. When he wrote that sentence, he certainly believed that this tool was in BOLZANO'S pamphlet. Probably he was misled by the so-called BOLZANO-WEIERSTRASS Theorem on the existence of an accumulation point for an infinite bounded set of numbers, which can be proved by showing the existence of the upper limit.

* CAUCHY, Oeuvres (2) **3**, 235.

BOLZANO's name in this context, however, is an honorific rather than an historic epithet as is HEINE's name in "HEINE-BOREL theorem"*.

CAUCHY did not use the notion of upper limit more often than he did, because he could not**, and he did not take it from BOLZANO, because it was not in BOLZANO's pamphlet. There is no doubt that I. GRATTAN-GUINNESS now knows these facts, but instead of cancelling the whole section, he has nullified it in a footnote:

> There is a distinction between BOLZANO's introduction of an upper limit and CAUCHY's "largest value of the expression..." in that CAUCHY actually used the *Limes* of a sequence... while BOLZANO defined the upper limit... but we cannot interpret this distinction as intentional in BOLZANO's and CAUCHY's time...

First, neither did CAUCHY use the term "largest value of the expression" nor did BOLZANO speak of upper limits. According to modern terminology the terms are upper limit (or limit superior) and least upper bound (or cut number), respectively. Second, CAUCHY does not use the limit but the upper limit—I. GRATTAN-GUINNESS seems still not to grant that these are different things. Third: Both BOLZANO's and CAUCHY's concepts of least upper bound and upper limit, respectively, were introduced on purpose because in the given context neither of them could use any other concept.

The fact that at first I. GRATTAN-GUINNESS did not notice this distinction, does not entitle him to claim that BOLZANO and CAUCHY could not make it. They did not have to, because they were confronted with different situations, and it is no use asking whether they would have made the distinction if there had been some need to do so.

To summarize, at this point there is no influence of BOLZANO on CAUCHY visible.

4.5. The Real Number System. I. GRATTAN-GUINNESS says:

> In the course of proving this Lemma as well as in other parts of his paper BOLZANO had recourse to extended considerations of real numbers regarding the rational or irrational limiting values of sequences of certain finite series of rationals...

On the contrary:

> CAUCHY wrote just once on the real number system: it was in the *Cours d'Analyse*, where he gave a superficial exposition of the real number system. The initial stimulus for this work was foundational questions concerning the representation of complex numbers; but he took the development of the ideas well into BOLZANO's territory, twice including the remark that "when B is

* HEINE first recognized the importance of uniform convergence, but he did not formulate covering properties.

** Even a concept like the least upper bound was not of any importance for the mathematics of the CAUCHY era. Such concepts become instrumental only with the direct methods of the variational calculus at the end of the 19th century, in particular after HILBERT's salvation of DIRICHLET's principle.

> an irrational number one can obtain it by rational numbers with values which are brought nearer and nearer to it"—merely a remark on a property of the real numbers and not as a *definition* of the irrational number... Once again CAUCHY did not fully appreciate the depth of BOLZANO's thought; and yet it is clear from his partial success that he was aware of BOLZANO's ideas rather than from his partial failure that he was ignorant of them.

It is hard to believe, but the truth is just the other way round. It is true that neither BOLZANO nor CAUCHY defined real numbers (in later investigations BOLZANO tried to do so). There is, however, nothing in BOLZANO's pamphlet that justifies the sentence quoted. There are no "extended considerations on real numbers...", there is not *any* consideration of real numbers and not even anything that could be misunderstood as such by somebody unaccustomed to reading mathematics. What I. GRATTAN-GUINNESS writes is a pure invention. The terms "rational" and "irrational" do occur once, in § 8, when, using as an example the decimal development of $\frac{1}{9}$, BOLZANO warns the reader against believing that the limit of a sequence of different rational numbers must be irrational.

On the contrary, CAUCHY's occupation with real numbers in the *Cours d'Analyse* is hatefully misrepresented. CAUCHY, though not defining real numbers, at least defines the algebraic and exponential operations on real numbers; starting from the rational numbers, where they had been defined directly, he extends the definitions to the real numbers by continuity. In this context he twice uses the fact that real numbers can be obtained as limits of rational ones. These are not isolated remarks as I. GRATTAN-GUINNESS claimed, but rather a deliberate use of this property in a meaningful context.

In any case CAUCHY wrote in the *Cours d'Analyse* much more on real numbers than BOLZANO did in his pamphlet (which was nothing). What could CAUCHY learn at this point from BOLZANO? What was the "depth of BOLZANO's thought" that CAUCHY could not fathom? The bare Nothing or the fact that 0.111... is rational? Where did he trespass into BOLZANO's territory, if this territory consisted of Nothing or of the fact that 0.111... was rational?

4.6. Summary as to the Common Ideas in Bolzano and Cauchy.

1. The idea of continuity, common to them both, was arrived at by each of them independently.

2. The CAUCHY convergence criterion was formulated by each of them; it is possible that CAUCHY took it from BOLZANO, though it can easily be explained as an original invention of CAUCHY's.

3. The theorem on the intermediate value of a continuous function had long been known as a more or less obvious proposition. The idea to prove it may have come to CAUCHY when he read the title of BOLZANO's pamphlet if he ever did. His proof is different from BOLZANO's.

4. As regards upper limits and least upper bounds, there is no common element.

5. On real numbers BOLZANO's pamphlet contains nothing, while CAUCHY in his *Cours d'Analyse* developed a theory of operations with real numbers.

In section 2 I explained how the *Cours d'Analyse* rested on a much broader basis of ideas than the few CAUCHY could have borrowed from BOLZANO's pamphlet. Therefore I. GRATTAN-GUINNESS' insinuating question*

What would have happened if CAUCHY had *not* read BOLZANO?

is irrelevant. The present section shows that there is even little if any cause to ask the other insinuating question**

But if CAUCHY owed so much to BOLZANO, why did he not acknowledge him?

Before analyzing his answer on this question, we shall cast a glance at his section 3.

5. Limit-Avoidance

I quote I. GRATTAN-GUINNESS' new limit definition***:

> When we speak of "introducing the concept of a limit" into analysis, we are actually introducing limit-avoidance, where the limiting value is *defined* by the property that the values in a sequence avoid that limit by an arbitrarily small amount when the corresponding parameter [the index n or the sequence s_n of n-th partial sums, say, or the increment α in the difference $(f(x+\alpha)-f(x))$ for continuity] avoids its own limiting value (infinity and zero in these examples). The new analysis of BOLZANO's pamphlet and developed in CAUCHY's text-books was nothing else than a complete reformulation of the whole of analysis in limit-avoidance terms...

No, no, and no. BOLZANO and CAUCHY knew better than I. GRATTAN-GUINNESS what was convergence and what was continuity. It is true there are bad 19^{th} century textbooks where you can find such silly definitions, but this was neither BOLZANO's fault nor CAUCHY's****.

6. Cauchy's Character

To explain *why* CAUCHY plagiarized BOLZANO, I. GRATTAN-GUINNESS writes a story about what he calls the Paris clique of mathematicians. No doubt he has studied that *chronique scandaleuse* better than anybody else. But if the secrets of that society are as relevant to understanding the history of mathematics as he suggests, why does he wrap himself in veils of mystery rather than disclose them? Why does he concoct a pompous story from plain historical facts and unfathomable allusions?

Whoever has studied CAUCHY's work knows how chaotic it is. A proposition is stated, then refuted, only to be stated once more; a procedure is severely criticized, only to be applied successfully at the next opportunity; for no reason

* p. 383, 12 f.b.

** p. 387, 5.

*** p. 378, 13 f.b. — 5 f.b.

**** When I. GRATTAN-GUINNESS lectured at the Utrecht Mathematical Colloquium everybody protested. An hour later people thought they had convinced him. It is a pity they had not done so.

notations are changed back and forth. No, I. GRATTAN-GUINNESS says, stating a certain apparently wrong theorem was a strategic move in the secret game of the Paris clique. As long as I do not know the secret information on which such conclusions must be based, I cannot challenge them*.

A critic is on a safer ground when I. GRATTAN-GUINNESS gives his sources. To prove that CAUCHY took sides in the quarrels of the "Paris clique" (which is utterly improbable) he mentions, in the same work, "fawning references to the powerful secrétaire perpétuel (FOURIER)" and "attacks on the declining POISSON"**. Any one who checks the sources will find that neither is the reference to FOURIER fawning nor is POISSON attacked. The first reads

> si l'on désigne avec M. FOURIER avec $\int\limits_{x'}^{x''} f(x)\,dx$ l'intégrale définie, prise entre les limites $x=x'$, $x=x''$...

and it is the style in which such acknowledgements have been made a thousand times by mathematicians. At the second place quoted we find CAUCHY, rather than attacking POISSON, explaining why he had overlooked certain consequences of his theory which had meanwhile been discovered by POISSON.

To understand what citations mean for mathematicians, it would be worthwhile to make a statistical study of them, say around CAUCHY. Isolated examples are of little value. At the very period when, according to I. GRATTAN-GUINNESS, CAUCHY had reasons to fawn FOURIER and to attack POISSON, he used the introduction to his *Cours d'Analyse* to extend his thanks to LAPLACE and POISSON, who had advised him to publish his courses, and at the end of the same introduction he acknowledged the good counsel he had received from POISSON, AMPÈRE and CORIOLIS. Should we interpret these acknowledgments, too, as attacks?

It is well known that CAUCHY was a strange fellow, and to prove it, there is no need to invent strange stories about him. The strangest is his quixotic conduct after the July revolution of 1830, when as a lone paladine he followed his king to his exile court in Prague. He was a religious and political dogmatic who often exhibited an appalling lack of human relations.

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* A characteristic pomposity is the remark in footnote 85 that the *Procès verbaux des séances de l'Académie tenues depuis la fondation jusqu'au mois d'août* 1835 (10 vols; 1910–22, Hendaye) "are an invaluable source of historical insight into the period 1795–1835, when the rivalries were at their height. They give the minutes of all the private meetings of the *Académie des Sciences*, which the participants can hardly have expected to be published!"

In fact, there is little that might be regarded as sensational to be found in the *Procès verbaux*. The style is the same as that of the later *Comptes Rendus*; the greater part is routine business. The meetings were not private but public. All spontaneous remarks were afterwards carefully edited or omitted; the oral text is better reflected by the newspaper reports.

** CAUCHY, Oeuvres (1) **1**, 340 and 189–191; another source mentioned is not accessible to me.

The adjectives "powerful" and "declining" are melodramatic stereotypes. There has never been any *secrétaire perpétuel* who was not powerful, but I doubt whether FOURIER was more so than his predecessors or successors. Facing a powerful *secrétaire perpétuel*, POISSON, too, needed an adjective though it is a pity that I. GRATTAN-GUINNESS hit on one that is so trivially mistaken as is "declining".

There is a story about CAUCHY and a manuscript of ABEL. In 1826, when his first important work had yet to appear, ABEL visited Paris. A few times he met CAUCHY, who at that period was interested only in mathematical physics. In Paris ABEL wrote the famous work he presented to the French Academy in October 1826. In 1829 he died. In the late thirties the editor of his *Oeuvres*, who knew about the manuscript, tried to get it back from the Academy, but it could not be found. Suddenly, in 1841, the text of the manuscript appeared in print in a publication of the Academy, though, strangely enough, the manuscript itself was still lost.

This trackless manuscript has always been an exciting feature in the melodramatic life of ABEL, who according to the stories died in misery, oblivion, and disappointment. (It has long been known that this story is untrue*.)

In such a story a villain is needed. According to old LEGENDRE, ABEL's paper was illegible, so the referees, CAUCHY and himself, could not read it. Even today it is commonly believed that the manuscript was lost by CAUCHY's neglect. In 1922 a copy of CAUCHY and LEGENDRE's report on ABEL's paper, dated 29 June 1829, was discovered**; it proved that CAUCHY's account of his role in the story was correct. It is obvious that CAUCHY had no further business with ABEL's manuscript, since after the July revolution of 1830 he went abroad and did not return before 1838. The academician LIBRI, however, who to annoy other people, had invented the main facts in ABEL's melodramatic life, got some business with ABEL's paper; in any case he read the proofs, though according to him without the manuscript. LIBRI was a mediocre mathematician who became famous by his sudden departure to London in 1848, when he was accused of having over many years stolen from the French public libraries a million's worth of rare books and manuscripts. Thus it was not too far-fetched to look into LIBRI's estate in the Moreniana library in Florence. Finally, in 1952, VIGGO BRUN did so, and he found ABEL's manuscript***. A written explanation of it by LEGENDRE had been published in World War II**** but had not been noticed. It reads†:

> Ce Mémoire a été mis d'abord entre les mains de M. Le Gendre qui l'a parcouru, mais voyant que l'écriture étoit peu lisible et les caractères algébriques souvent mal formés, il le remit entre les mains de son confrère, M. Cauchy avec prière de se charger du rapport. M. Cauchy distrait par d'autres affaires et n'ayant reçu nulle provocation pour s'occuper du Mémoire de M. Abel, attendu que celui-ci n'était resté que peu de jours à Paris après la présentation de son Mémoire à l'Académie, et n'avait chargé personne de suivre cette affaire auprès des commissaires, M. Cauchy, dis-je, a oublié pendant très long temps le Mémoire de M. Abel dont il étoit dépositaire. Ce n'est que vers

* Read VIGGO BRUN's debunking paper in Journal r. u. angew. Math. **193** (1954), 239–249.

** D. E. SMITH, *Amer. Math. Monthly* **29** (1922), 394–5. Among my autographs, 29. Legendre and Cauchy sponsor Abel. — It is in agreement with the Procès verbaux (*cf.* footnote *, p. 389).

*** See footnote *.

**** G. CANDIDE, Sulla mancata pubblicazione, nel 1826 della celebre Memoria di Abel. Tip. Editr. "Marra" di G. Bellone, Galatina 1942, XX.

† Journ. r. u. angew. Math. **193**, 244–245.

le mois de mars 1829, que les deux Commissaires apprirent, par l'avis que l'un d'eux réçut** d'un savant d'Allemagne, que le Mémoire de M. Abel, qui avait été presenté à l'Académie, contenait ou devait contenir des résultats d'analyse fort interessants, et qu'il était étonnant qu'on n'en eût pas fait de rapport à l'Académie. Sur cet avis M. Cauchy rechercha le Mémoire, le trouva et se disposait à en faire son rapport; mais les Commissaires furent retenus par la considération que M. Abel avait déjà publié dans le Journal de Crelle une partie de son Mémoire présenté à l'Académie, qu'il continuerait probablement à faire paraître la suite, et qu'alors le rapport de l'Académie, qui ne pouvait être que verbal, deviendrait intempestif*.

Dans cet état de choses nous apprenons subitement la mort de M. Abel, perte très fâcheuse pour les sciences, et qui paraît maintenant rendre le rapport nécessaire pour conserver s'il y a lieu, dans le receuil des savants étrangers, un des principaux titres de gloire de son auctor**.

This unveils the mystery around ABEL's manuscript. It is not unusual that referees neglect their task, in particular, if they are not interested in the subject or if it is the work of a virtually unknown author, though I agree that CAUCHY was usually more careful. Delays of 10–15 years in printing treatises accepted by the French Academy were not unusual either; every publication needed a royal authorization. In ABEL's case it may have played a role that the essential part of the manuscript had already been published in "Crelle's Journal".

I. GRATTAN-GUINNESS' report on this event is a distortion of the story as it is known now. He omits all evidence that is in favour of CAUCHY, and he falsifies two points***:

First he claims that the neglected manuscript

> ...was the paper which ushered in the transformation of LEGENDRE's theory of elliptic integrals into his own theory of elliptic functions...

to add one more melodramatic feature. The paper on elliptic functions was published in Crelle's Journal. The manuscript in question was about "ABEL's theorem"; an extract also appeared in Crelle's Journal.

Second, he claims:

> CAUCHY took it and, perhaps because of ABEL's footnote against him, ignored it entirely: only after ABEL's death in 1829 did he fulfil a request to return it to the *Académie des Sciences.*

The reader can check that this is in all essentials contrary to LEGENDRE's report. If I. GRATTAN-GUINNESS is in the possession of secret information that refutes LEGENDRE's report, he should reveal his sources. Meanwhile I am entitled to consider LEGENDRE's report as correct.

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* The procedure of a formal report was applied only to manuscripts; printed pieces submitted to the Academy were given a *rapport verbal.*

** *Sic.*

*** p. 393.

I. GRATTAN-GUINNESS continues:

> ... there is one aspect of it which has been little remarked upon but which shows the depths to which CAUCHY could sink.

The evidence I. GRATTAN-GUINNESS produces for CAUCHY's moral downfall is an exposé of 1841, where CAUCHY first praises ABEL and then refutes the story that ABEL died in misery. We now know that CAUCHY's exposé is correct.

I. GRATTAN-GUINNESS does not explain in what CAUCHY's downfall consisted, but anyhow it was a downfall and

> ... anyone capable of writing in this manner, knowing the negative role played by himself in the matter under discussion, would hardly think twice about borrowing from an unknown paper published in Prague without acknowledgment.

Anyone? Maybe. But CAUCHY was someone.

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