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Source: *Archive for History of Exact Sciences*, Vol. 6, No. 5 (6.VIII.1970), pp. 372-400
Published by: Springer
Stable URL: https://www.jstor.org/stable/41133312
Accessed: 28-10-2019 15:58 UTC

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Bolzano, Cauchy and the "New Analysis" of the Early Nineteenth Century

I. GRATTAN-GUINNESS

Communicated by J. E. HOFMANN

Summary

In this paper[1] I discuss the development of mathematical analysis during the second and third decades of the nineteenth century; and in particular I assert that the well-known correspondence of new ideas to be found in the writings of BOLZANO and CAUCHY is *not* a coincidence, but that CAUCHY had read one particular paper of BOLZANO and drew on its results without acknowledgement. The reasons for this conjecture involve not only the texts in question but also the state of development of mathematical analysis itself, CAUCHY both as personality and as mathematician, and the rivalries which were prevalent in Paris at that time.

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1. Introduction

The central theme of this paper is an historical conjecture concerning the development of mathematical analysis in the early nineteenth century. It is well known that the major event was the publication in 1821 of the *Cours d'Ana-*

[1] This paper is a revised and greatly expanded version of a lecture entitled "Did Cauchy read Bolzano before writing his *Cours d'Analyse?*" given at the *Problemgeschichte der Mathematik* seminar at Oberwolfach, West Germany, on the 26th November, 1969. I wish to thank Professors J. E. HOFMANN and C. J. SCRIBA for their invitation to this seminar.

The text draws frequently on my history of *The Development of the Foundations of Mathematical Analysis from Euler to Riemann* and *Joseph Fourier 1768—1830*, which are both to be published by the M.I.T. Press and are referred to in later footnotes as *Foundations* and *Fourier*, respectively. The latter work was written with the collaboration of Dr. J. R. RAVETZ, and the former with the help of his detailed criticism: I wish to record here my indebtedness to his assistance.

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lyse of AUGUSTIN-LOUIS CAUCHY (1789—1857),[2] in which CAUCHY presented a new type of analytical reasoning far superior to previous ideas for the development of analysis — limits, functions, the calculus, and so on. CAUCHY's achievement was the so-called "arithmeticisation" of analysis, a method whose development and application has been a major interest for mathematicians ever since.

It has been also well-known for some time that CAUCHY had been anticipated in his basic ideas of the new analysis by an obscure pamphlet published in Prague in 1817 by BERNARD BOLZANO (1781—1848). In contrast to the broad programme of CAUCHY's book, BOLZANO devoted his little work to the proof of a theorem which he described in its title: "Purely analytical proof of the theorem, that between any two values [of a function $f(x)$] which guarantee an opposing result [in sign] lies at least one real root of the equation $[f(x) = 0]$."[3] The "pure analysis" which BOLZANO produced in his proof is exactly that which we find greatly developed and extended in CAUCHY'S *Cours d'Analyse* and his later writings on analysis.

I do not believe that we have here an example of a remarkable coincidence of new ideas. Such occurrences are of course well-known in the history of science, but I shall argue for the conjecture that in this case CAUCHY was well acquainted with BOLZANO'S paper and that he drew on its novelties without ever making acknowledgement to him.

The argument for this thesis is not based on new documentary evidence: there is no reference to BOLZANO'S work among the scattered fragments of CAUCHY'S papers and letters, no library record of CAUCHY'S reading or borrowing BOLZANO'S paper, no copy of it in his personal library (which in fact has been dispersed). My reasons for the conjecture are circumstantial and related to intellectual matters, and involve not only the general development of analysis at that time but also that aspect of the growth of science which is ignored all too often by its historians — the social and educational situation of the period, and the personalities of the principal characters.

2. The Common Ideas in Bolzano and Cauchy

We consider first the directly corresponding results in the two works, in each case in its general historical setting.

2.1. Continuity of a Function. Normally the continuity of a function was then identified with its description by a single algebraic expression, and the function was usually thought to be differentiable: in fact, under EULER'S influence the

[2] A.-L. CAUCHY, *Cours d'Analyse de l'Ecole Royale Polytechnique. 1re Partie: Analyse Algébrique* (1821, Paris) = *Oeuvres*, (2) **3**. No further parts of this work were published: it is referred to in later footnotes as *Cours*.

[3] 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—17: publ. 1818), 60 pp. = Ostwald's Klassiker, No. 153 (ed. P. JOURDAIN: 1905, Leipzig), 3—43. French trans. in *Rev. d'Hist. Sci. Appl.*, **17** (1964), 136—164: there have also been various other translations and issues. The paper is referred to in later footnotes as *Beweis*.

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term "continuous" was usually confined to functions which we now call "differentiable".[4] There were efforts to move away from this view, including by EULER himself; but nobody had come at all close to the formulation of continuity given by BOLZANO and CAUCHY:

BOLZANO: "A function $f(x)$ varies according to the law of continuity for all values of x which lie inside or outside certain limits, is nothing other than this: if x is any such value, the difference $f(x+\omega)-f(x)$ can be made smaller than any given quantity, if one makes ω as small as one ever wants to."[5]

CAUCHY: "The function $f(x)$ will remain continuous with respect to x between the given limits, if between these limits an infinitely small increase of the variable always produces an infinitely small increase of the function itself".[6]

One of the most interesting and important features of this formulation of continuity is that it extends the old formulation beyond that of differentiability, for it also encompasses functions with corners. I think that BOLZANO was aware of the extension in 1817, for in later manuscripts he studied the distinction between the new continuity and differentiability to the extent of constructing a continuous non-differentiable function of the type studied later only by the school of WEIERSTRASS in the 1870's.[7] But CAUCHY seems to have seen the new idea only as a reformulation of the old one when he wrote the *Cours d'Analyse*, for the examples he gave there of continuous functions were all of standard differentiable algebraic expressions, with the functions x^a for negative a, and $\frac{a}{x}$, regarded as "discontinuous" at $x=0$ since they then became infinite.[8] In fact, he explicitly discussed the distinction only in a paper of 1844, and then in a way which tried to give the impression that he had known it all along:

"In the works of Euler and Lagrange, a function is called *continuous* or *discontinuous*, according as the diverse values of that function, corresponding to diverse values of the variable ... are or are not produced by one and the same equation .... Nevertheless the definition that we have just recalled is far from offering mathematical precision; for the analytical laws to which functions can be subjected are generally expressed by algebraic or transcendental formulae [that is, by the EULERIAN range of algebraic expressions], and it can happen that various formulae represent, for certain values of a variable x, the same function: then, for other values of x, different functions."

He then quoted the example

$$\sqrt{x^2}=\frac{2}{\pi}\int_0^\infty \frac{x^2}{t^2+x^2}\,dt=\begin{cases} x \text{ if } x\geqq 0 \\ -x \text{ if } x\leqq 0 \end{cases}, \qquad (1)$$

[4] EULER'S classic presentation of his theory of functions was given in the opening sections of both volumes of his *Introductio ad analysin infinitorum* (2 vols: 1748, Lausanne) = *Opera Omnia*, (1) **8**—9.

[5] B. BOLZANO, *Beweis*, preface, part IIa.

[6] A.-L. CAUCHY, *Cours*, 34—35 = *Oeuvres*, (2) **3**, 43.

[7] See B. BOLZANO, *Functionenlehre* (ed. K. RYCHLIK), in his *Schriften*, **1** (1930, Prague), esp. pp. 66—70, 88—89.

[8] A.-L. CAUCHY, *Cours*, 36—37 = *Oeuvres*, (2) **3**, 44—45.

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in which the first two forms are "continuous" in EULER's sense while the third is "discontinuous";

"... but the indeterminacy ceases if for Euler's definition we substitute that which I have given [in the *Cours d'Analyse*]".[9]

2.2. Convergence of a Series. A major innovation of the new analysis was the study of the convergence of a series (or of classes of series) as a general problem separate from and indeed prior to that of its summation; but it *would* be wrong to presume that the problem of convergence had previously been ignored or taken for granted. 17th and 18th century mathematicians were perfectly well aware that a series was to be interpreted as a term-by-term addition of its members, and that individual series (usually series of constant terms or certain power series) could be shown to be convergent, especially if they were associated with some geometrical limiting procedure such as the approximation to a curve by a polygon. But this understanding had been endangered during the 18th century, especially by EULER's great ability to devise complicated new methods of summation of series. Today we understand that some of these methods reduce to orthodox summation for orthodox convergent series and some do not; but EULER and his contemporaries seemed to have regarded *all* methods as legitimate, giving "the" sum of the series rather than its sum relative to the method of summation involved. This more sophisticated understanding began to develop only in the 1890's, under the leadership of BOREL:[10] until then, series considered "divergent" (that is, oscillatory series as well as those with an infinite sum) had been banished from analysis under the influence of CAUCHY's work. But he and BOLZANO were not the first to consider the convergence of a series to be an important property worthy of investigation of its own. GAUSS had even advanced as far as a sophisticated convergence test by 1812[11]: FOURIER had already treated the convergence of particular examples of his series in 1807, in his first paper on the diffusion of heat[12]: LAGRANGE had tried to find expressions for the remainder term of a TAYLOR series, in connection with his long held belief that the series could serve as the foundation of the calculus;[13] and LACROIX was also aware of the need for general formulation of convergence.[14] Both BOLZANO and CAUCHY also stressed that the convergence of a series is to be determined only by the tendency of the n^{th} partial sums to a limiting value s as n tended to infinity;[15]

[9] A.-L. CAUCHY, "Mémoire sur les fonctions continues ou discontinues", *C. R. Acad. Roy. Sci.*, **18** (1844), 116—130 (see pp. 116—117) = *Oeuvres*, (1) **8**, 145—160 (pp. 145—146).

[10] For extended discussion, see my *Foundations*, ch. 4.

[11] K. F. GAUSS, "Disquisites generales ...", *Comm. Soc. Reg. Sci. Göttingen Rec.*, **2** (1811—13: publ. 1813), cl. math., 46 pp. = *Werke*, **3**, 123—162: see art. 16. For a history of convergence tests, see the appendix to my *Foundations*.

[12] J. B. J. FOURIER, "Sur la propagation de la chaleur," MS. 1851, *Ecole Nationale des Ponts et Chaussées*, Paris: see arts. 42—43. The publication of this entire manuscript constitutes the body of my *Fourier:* see there ch. 7 on this point.

[13] See especially his *Théorie des fonctions analytiques* ..., (2nd edition: 1813, Paris) = *Oeuvres*, **9**: part 1, arts. 35—40.

[14] See especially his *Traité du calcul différentiel et du calcul intégral* (1st edition: 1797—1800, Paris), **1**, 4—9.

[15] B. BOLZANO, *Beweis*, art. 5. A.-L. CAUCHY, *Cours*, 123—125 = *Oeuvres*, (2) **3**, 114—115.

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thus this correspondence is not so striking, although the idea was still then very much a new one. But in both works we find a new type of result, not to be found in any other contemporary writing. BOLZANO had defined a class of series:

"... which possess the property that the variation (increase or decrease) which their value suffers through a prolongation [of terms] as far as desired remains always smaller than a certain value, which again can be taken as small as one wishes, if one has already prolonged the series sufficiently far",[16] and then he proved that for series with this property,

"... there always exists a certain constant value, and certainly only one, which the terms of this series always approach the more, and towards which they can come as close as desired, if one prolongs the series sufficiently far."[17] CAUCHY stated that:

"For the series $\left[\sum_{r=1}^{\infty} u_r\right]$ to be convergent it is yet necessary that for increasing values of n the different sums

$$u_n + u_{n+1}$$
$$u_n + u_{n+1} + u_{n+2}$$
$$\&c. \quad . \quad . \quad . \quad .$$

... finish by constantly achieving numerical values smaller than any assignable limit. Reciprocally, when these various conditions are fulfilled, the convergence of the series is assured."[18]

In other words, they both found a general condition for convergence in terms of the behaviour of $(s_{n+r} - s_n)$ as n tended to infinity: a result of quite profound originality. Contrary to general belief, BOLZANO in fact only asserted the sufficiency of the condition in his paper; his proof is very difficult to follow even with the ideas of his new analysis, and in fact is faulty. The necessity of the condition is far easier to recognise and prove: CAUCHY did prove it, but then avoided difficulties by hinting that sufficiency followed as a consequence (which it does not!):

"the sums s_n, s_{n+1}, ... differ from the limit s, *and consequently among themselves*, by infinitely small quantities."[19]

2.3. Bolzano's Main Theorem. The theorem which BOLZANO actually proved in his paper was the following generalisation of the theorem of his title:

Let $f_1(x)$ and $f_2(x)$ be continuous functions for which $f_1(\alpha) < f_2(\alpha)$ and $f_1(\beta) > f_2(\beta)$: then $f_1(a) = f_2(a)$ for at least one value a of x between α and β. (The basic theorem is the case where $f_2(x) \equiv 0$.)

As a theorem it is most untypical of its time: that is, a general theorem concerning the properties of functions was *not* the kind of result then being sought in analysis. BOLZANO himself saw it rather as a theorem in the theory of equations, as a companion to GAUSS's recent proofs of the decomposition of a polynomial

[16] B. BOLZANO, *Beweis*, art. 5.
[17] B. BOLZANO, *Beweis*, art. 7.
[18] A.-L. CAUCHY, *Cours*, 124—125 = *Oeuvres*, (2) **3**, 115—116.
[19] A.-L. CAUCHY, *Cours*, 125 = *Oeuvres*, (2) **3**, 115. My italics.

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into linear and quadratic factors.[20] CAUCHY saw it as a theorem of the new analysis, and put it *twice* into the *Cours d'Analyse* (in its restricted form): firstly with a naive geometrical argument, and later, in the part of his book reserved for those with a special interest in analysis, with a condensation argument which seems very much like an unrigorous version of the intricate proof developed in BOLZANO'S paper.[21]

2.4. Bolzano's Lemma. A crucial lemma required by BOLZANO to establish the existence of the real root was the following lemma:

"If a property M does not apply to all values of a variable quantity x, but to all those which are smaller than a certain u: so there is always a quantity U which is the largest of those of which it can be asserted that all smaller x possess the property M."[22]

With this extraordinary theorem came another new idea into analysis, completely untypical of its time: the *upper limit* of a sequence of values. It 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 ..." when calculating limiting values, especially in connection with the development of tests for convergence of a series.[23] 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: not until the 1860's was it introduced again and properly used, by the WEIERSTRASS school of analysts.[24]

2.5. The Real Number System. Lastly, a point which is less striking than the others but worth mentioning: the considerations given in both works to the real numbers. In the course of proving his lemma as well as in other parts of his paper BOLZANO had recourse to extended considerations of real numbers, especially regarding the rational or irrational limiting values of sequences of certain finite series of rationals.[25] In later manuscripts he extended these remarks into a full theory of rational and irrational numbers of the type which, like continuous non-differentiable functions and the theorem on upper limits, was next investigated

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[20] K. F. GAUSS, "Demonstratio nova altera ..." and "Theorematis de resolubitate ...", *Comm. Soc. Reg. Sci. Göttingen Rec.*, **3** (1814—15: publ. 1816), cl. math., 107—134, and 135—142 = *Werke*, **3**, 31—56, and 57—64.

[21] A.-L. CAUCHY, *Cours*, 43—44 and 460—462 = *Oeuvres*, (2) **3**, 50—51 and 378—380.

[22] B. BOLZANO, *Beweis*, art. 12.

[23] See especially the sections on convergence tests in chs. 6 and 9 of the *Cours*.

[24] 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 (whose every neighbourhood contains members of the sequence), while BOLZANO defined the upper limit (which does not *necessarily* have this property); but we cannot interpret this distinction as intentional in BOLZANO and CAUCHY'S time and I do not know of any recorded awareness of it then. For a brief discussion of the point, see P. E. B. JOURDAIN, "On the general theory of functions," *Journ. rei. ang. Math.*, **128** (1905), 169—210 (pp. 185—188).

[25] B. BOLZANO, *Beweis*, art. 8: see also art. 12.

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only by WEIERSTRASS and his followers.[26] CAUCHY wrote just once on the real number system: it was in the *Cours d'Analyse*, where he gave a superficial formal 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 an irrational number, one can obtain it by rational numbers with values which are brought nearer and nearer to it"[27] — merely a remark on a property of the real numbers and not as a *definition* of the irrational number in the sense of the later work, as has sometimes been thought. 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. The striking feature of this remark, as with his interpretation of continuity and his only incomplete use of the upper limit, is that it is there *at all*, rather than that it appears in a mutilated form.

3. The New Analysis

Thus we find a significant collection of unusual results in the two works: yet there is a much stronger and more profound link between them, which cannot be identified by means of precise quotations or references — namely, a *unity of approach*. We have here a good example of the rule that the whole is greater than the sum of the parts, for it is the *homogeneity* and *general applicability* of these new ideas which is their most significant feature. The term "arithmeticisation of analysis" is given to them, because they operate by means of arithmetical differences and proofs within the analysis are based on the arithmetical manipulation of them; but I do not favour this name, partly because it is identified with the later WEIERSTRASSIAN developments of analysis but principally because the arithmeticisation is only at the service of something more profound: the theory of *limit-avoidance*.

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 for 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 formed in 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, terms which CAUCHY made quite explicit in the introduction to the *Cours d'analyse*:

"When the values successively attributed to a particular variable approach indefinitely a fixed value, so as to finish by differing from it by as little as one wishes, this latter is called the *limit* of all the others."[28]

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[26] These manuscripts were published in K. RYCHLIK (ed.), *Theorie der reellen Zahlen im Bolzanos handschriftlichen Nachlasse* (1962, Prague).

[27] A.-L. CAUCHY, *Cours*, 409 and 415 = *Oeuvres*, (2) **3**, 337 and 341.

[28] A.-L. CAUCHY, *Cours*, 4 = *Oeuvres*, (2) **3**, 19.

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One important aspect of limit-avoidance is that it is *independent* of the continuum of values over which the analysis is conducted. Limit-avoidance can be developed whether an infinitesimal or non-infinitesimal field is being used: the use of the WEIERSTRASSIAN term "arithmeticisation of analysis", applied to the period when WEIERSTRASS excluded infinitesimals from analysis, has led us to forget that its limit-avoiding character was shown also by the earlier period instigated by BOLZANO, who used both types of continuum in his analysis,[29] and CAUCHY, who practiced only infinitesimals throughout his mathematical career. Since WEIERSTRASS's time, we have held a fairly contemptuous view of the infinitesimalists which I regard as unfair. A remarkable amount of pure and applied analysis was developed from the time of NEWTON onwards with the aid of infinitesimals; but there were important foundational difficulties involved in their use, and in fact CAUCHY is a good example of them. These difficulties seem to me to lie especially in the foundations of the calculus, which if we examine from the point of view of limit-avoidance also reveal the attraction that infinitesimals must have had to the founders of the algebraic calculus.

We make our point in the LEIBNIZIAN notation, which not only became the standard system but also contained a key to the difficulties that the infinitesimalists faced. When we calculate the derivative by means of the definition

$$\frac{dy}{dx} =_{\text{Df.}} \lim_{h \to 0} \left[\frac{f(x+h) - f(x)}{h} \right], \tag{2}$$

we may quite easily obtain the value of the derivative involved; but we are left with the important foundational question of *how* that value is obtained in light of the fact that the ratio on the right hand side of (2) becomes $\frac{0}{0}$ when $h = 0$. The virtue of infinitesimals, quantities which obeyed the law

$$a + h = a \tag{3}$$

of addition to the "ordinary" numbers, was that, being non-zero they avoided the limiting value and therefore the difficulty of $\frac{0}{0}$; on the other hand, being smaller than "any assignable quantity" (that is, any non-infinitesimal), they effectively allowed the limit to be taken. This view was of course an inconsistent one, but I think that it lay basically behind infinitesimalist reasoning and was the source of its difficulties. The infinitesimal was either zero or non-zero, according to the needs of the moment: thus it could be added to or withdrawn from any quantity in an equation, with the presumed certainty of leaving the mathematical situation described by that equation undisturbed. We may see this as a double-interpretation for the infinitesimal — a limit-avoiding interpretation as a non-zero quantity, and what we may call by contrast a "limit-achieving" interpretation as an essentially zero quantity allowing the limit to be taken. From this distinction there follows a corresponding double-inter-

[29] In the *Beweis* BOLZANO did not explicitly discuss the possible continua, and seemed to have allowed the use of infinitesimals; but later in the year he published another pamphlet, on *Die drei Probleme der Rectification, der Complanation und die Cubirung, ohne Betrachtung des unendlich Kleinen, ... und ohne irgend eine nicht streng erweisliche Voraussetzung gelöst;* ... (1817, Prague) = *Schriften*, 5 (1948, Prague), 67—138.

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pretation of $\frac{dy}{dx}$. Let us take a specific example of a derivative, say for the function

$$y = x^3, \tag{3}$$

whose derivative

$$\frac{dy}{dx} = 3x^2 \tag{4}$$

is calculated from

$$\frac{dy}{dx} =_{\text{Df.}} \lim_{h \to 0} \left[\frac{(x+h)^3 - x^3}{h} \right]. \tag{5}$$

When h achieves its limiting value zero (4) gives us the value of the derivative, and so the denoting symbol $\frac{dy}{dx}$ is in fact just a symbol and is *not* to be taken as an arithmetical ratio "$dy \div dx$". Thus it is not valid to multiply through (4) by dx to obtain

$$dy = 3x^2 dx. \tag{6}$$

(6) follows from (4) by turning from the limit-achieving interpretation of $\frac{dy}{dx}$ as a whole symbol to its limit-avoiding interpretation, where it *is* the ratio "$dy \div dx$". For if we avoid the limiting value by the non-zero infinitesimal quantity dx, then we see from the right hand side of (5) that the situation for the increment dy $(= d(x^3))$ is given by

$$dy = 3x^2 dx + q, \tag{7}$$

where q is a *second-order* infinitesimal obeying the law

$$a + q = a \tag{8}$$

of addition to "ordinary" or first-order infinitesimal quantities a. $\frac{dy}{dx}$ in this kind of situation, if we wish to consider it, could arise by dividing throughout (7) to give:

$$\frac{dy}{dx} = 3x^2 + \frac{q}{dx}, \tag{9}$$

a result of a *fundamentally different kind from* (4). There is a difference between the two far greater than the first order infinitesimal $\frac{q}{dx}$: we see a basic *qualitative* difference, for $\frac{dy}{dx}$ appears in (4) as a limit-achieving symbol but in (9) as a limit-avoiding ratio. Further, the deduction of (9) from an infinitesimal equation (7) is not necessary to the derivation of (4). For let us suppose that we change continua so that in WEIERSTRASSIAN style we reject the use of infinitesimals. Then (4) and (5) still stand (with the limit now of course taken over the non-infinitesimal field); but (7) and all its consequences, such as (9), disappear altogether for (7) itself changes into the identity

$$0 = 0, \tag{10}$$

whether or not it was true in the infinitesimal continuum.

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The ideas that I have presented here are essentially straightforward, and are susceptible of considerable extension; but they are independent of the modern interest in developing a *consistent* theory of infinitesimals.[30] They do not themselves establish a consistent infinitesimalism but at least show that much can be clarified in terms which could have been understood and developed in the infinitesimalist period. Yet they were far from the considerations of the time: in particular, CAUCHY'S treatment of the foundations of the calculus was as incoherent and incompetent as any that were ever offered. In his *Résumé des leçons ... sur le calcul infinitésimal* of 1823, the next instalment of his new analysis after the *Cours d'Analyse*, he explicitly rejected LAGRANGE'S faith in TAYLOR'S series, but he replaced it with an extraordinary theory of the derivative which made simultaneous use of both LAGRANGE'S theory of derived functions $f'(x)$, $f''(x)$, ... and also of CARNOT'S theory of differentials dx, ddx, ...: infinitesimals not only achieved the limit in CAUCHY'S system but they also avoided it, at times by non-infinitesimal amounts, changing their role with every appearance of new and usually unnecessary notation.[31] However, when CAUCHY came to integration he was wonderfully successful, laying out the whole basic structure of the theory of the "CAUCHY integral" (defined in terms of the area as the limit of a sum) in a masterly display of the power of the new analysis of limit-avoidance.

This is what the new analysis was: only in limit-avoidance terms can its full power and subtlety be appreciated, and theorems such as the necessary and sufficient condition for convergence in the diminishing of $(s_{n+r} - s_n)$ — *where the limit s is avoided altogether* — and BOLZANO'S theorem on the existence of upper limits, can be seen to their best advantage. Yet to understand BOLZANO and CAUCHY'S work we must look at the old as well as the new. What sort of analysis had they replaced?

4. The Old Analysis

We have referred earlier briefly to certain features of 18th century analysis, and it is appropriate now to make more detailed remarks about its character. In speaking of the "old analysis", we are referring only to the subject immediately prior to BOLZANO and CAUCHY'S work; and we find that many of its features were the result of problems in other areas of mathematics, especially in the solution of difference and differential equations. Following the leadership of EULER, his contemporaries (mainly D'ALEMBERT, DANIEL BERNOULLI and LAGRANGE) and successors (mainly LAGRANGE, LAPLACE and MONGE) had developed a wide range of solution methods. It is impossible to describe them all in a sentence, but often they involved the construction of exact differentials prior to integration to give functional solutions, or assumptions of particular kinds of solution which led via the conditions of the problem to auxiliary equa-

[30] See A. ROBINSON, *Non-Standard Analysis* (1966. Amsterdam); and also the work initiated by C. SCHMEIDEN & D. LAUGWITZ, "Eine Erweiterung der Infinitesimalrechnung", *Math. Zeitschr.*, **69** (1958), 1—39.

[31] A.-L. CAUCHY, *Résumé des leçons données à l'Ecole Royale Polytechnique sur le calcul infinitésimal. Tome premier* (1823, Paris) = *Oeuvres*, (2) **4**, 5—261. No other volumes were published: see here lecture 5.

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tions. The analytical techniques themselves — which involved not only differentiation and integration, but also summation and rearrangement of series (especially power series), manipulations of algebraic expressions, the taking of limiting cases (in moving from difference to differential equations for example), and so on — were normally used as required without consideration of their validity. This is not intended as a criticism, but merely a general statement of the situation: it led to an enormous range of results in pure and applied mathematics which have remained important ever since. Further, there were cases when questions of rigour and validity *did* arise, of which the most important was the problem of the motion of the vibrating string;[32] but in general the situation at the beginning of the 19th century was that not only were such considerations relatively limited but the techniques themselves were susceptible of, and received, plenty of further development without concern for the rigour involved. This is a matter of great importance when considering the "new analysis" of BOLZANO and CAUCHY. Their new foundations, based on limit avoidance, certainly swept away the old foundations, founded largely on faith in the formal techniques; but it would be a mistake of posterior wisdom to assume that old foundations had been in a serious and comprehensive state of decay and were recognised as such by those who were using them. Historians of science seem to be only too ready to make assumptions of this kind when considering "revolutions" in science: they also tend to identify anticipations of a new system in the old one with that new system instead of what they probably were, something else in the old system which was quite different and also interesting. The historiographical point here is the danger of determinism; that because a body of knowledge developed in a particular way, then it must be viewed historically as having been capable of developing *only* that way, certainly from the intellectual point of view and perhaps even chronologically. Yet in fact any situation is always open to a variety of future developments: we must not allow the intermediate historical processes that actually happened to distort our vision of the situation from which they started.

I have already claimed that the new analysis replaced an old analysis which does not seem to have needed such a radical replacement: from the point of view of the BOLZANO-CAUCHY question, it follows that it is all the more surprising that exactly the same type of replacement began to emerge twice within four years. But we must consider also the anticipations of the new system in the old one. The "new analysis" laid great stress on the rigour of processes: did no "old analyst" try to do the same? Yes, certainly, but not in any way resembling the comprehensive and homogeneous character of the new method: they had other ideas which were quite different and also interesting. EULER tried hard, though with little practical success, to produce a consistent infinitesimalism in his "reckoning with zeros", including consideration of different orders of infinitesimal. D'ALEMBERT tended to distrust infinitesimals altogether, while LAGRANGE tried to avoid all limiting processes by *defining* the derivatives of a function in

[32] For a discussion of foundational questions in the light of this problem, see my *Foundations*, Ch. 1; and for an extended account of the solution of differential equations in this period, see C. TRUESDELL, *The rational mechanics of flexible or elastic bodies 1638—1788*, L. Euleri Opera Omnia, (1) **11**, pt. 2 (1960, Zurich).

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terms of the coefficients of its expansion as a TAYLOR series. This was "limit-avoidance" of a completely different and considerably less successful kind, and it won few supporters. One of them, however, was ARBOGAST, who tried towards the end of the century to reduce the number of distinctions between types of function to a group based on analytical rather than algebraic or mechanical considerations. L'HUILIER offered a thoughtful essay on the taking of limits: I am sure that CAUCHY read it, for he always used the notation "lim" for a limiting value which L'HUILIER introduced there. But I doubt if he learnt much more from it, for the results obtained are severely limited, being concentrated on the derivative and often providing no more than a re-writing of known ideas. L'HUILIER also criticised (with justice) EULER's use of infinitesimals, and CARNOT took it further into a profound essay on orders of the infinitely small and the interpretation of the LEIBNIZIAN notations as infinitesimals. But perhaps the best example, especially from the point of view of anticipations of BOLZANO and CAUCHY, is LACROIX, the principal text-book writer of the day. He was not an important creative mathematician, but he was capable of some measure of appreciation of contemporary work and he read exhaustively among the earlier literature. I referred earlier to his understanding of convergence of series as a general problem, which he learnt from D'ALEMBERT'S vague warnings against divergent series in the 1760's: he also gave in 1806 a formulation of continuity vaguely similar to that of BOLZANO and CAUCHY.[33] Thus we may say that LACROIX anticipated them if we wish; yet it would be more misleading than illuminating to do so, not least to the understanding of LACROIX'S results. For one cannot find in LACROIX'S writings the general aim that BOLZANO and CAUCHY achieved, not even in the new editions of his works that continued to appear after CAUCHY'S text-books were published.

What would have happened if CAUCHY had *not* read BOLZANO? Without doubt, foundational questions would have received discussion, but it seems to me most unlikely that the radical reform that in fact happened would have taken place: rather only parts of that theory would probably have emerged, especially in the convergence of series and the integral as the limit of a sum, while the rest, apparently sound enough, would have received well-meaning but limited examination. But in order to put the old and the new analyses into better perspective we must describe some of the fundamental problems which were current before BOLZANO'S paper; and at the same time we shall pass on to further aspects of the CAUCHY-BOLZANO question, aspects which involve not only analysis itself but also the Paris in which CAUCHY was working and the way in which his mathematical genius was inspired.

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[33] S. F. LACROIX, *Traité élémentaire du calcul intégral* (2nd edition; 1806, Paris): see art. 60. The other works to which we referred explicitly were L. F. A. ARBOGAST, *Mémoire sur la nature des fonctions arbitraires qui entrent dans les intégrales des équations aux différentielles partielles* (1791, St. Petersburg): S. L'HUILIER, *Exposition élémentaire des principes des calculs supérieures* (1786, Berlin), esp. chs. 1 and 11; and L. N. M. CARNOT, *Reflexions sur la métaphysique du calcul infinitésimale* (1st edition: 1797, Paris. 2nd edition: 1813, Paris). On EULER'S and LAGRANGE'S views on analysis, see A. P. JUSCHKEWITSCH, "Euler and Lagrange über die Grundlagen der Analysis," *Sammelband der zu Ehren des 250. Geburtstages Leonhard Eulers* (ed. K. SCHRODER: 1959, Berlin), 224—244; and on all these and other developments, my *Foundations*, chs. 1 and 3.

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5. Cauchy's Originality as a Mathematician

If CAUCHY came to his new ideas independently of BOLZANO, then he perceived a completely novel approach to analysis and detected its superiority over known techniques which themselves were not lacking in power or generality. This kind of achievement is characteristic of certain mathematicians: it reflects their sensitive "intuition for problems", their ability to see far beyond contemporary work into totally new ways of solving current problems, or even of forming new problems of which others were hardly aware. GAUSS is a prime example of such a thinker, with his notebooks already filled with the seeds of most 19th century mathematics within its first decade: BOLZANO shows this ability, too, and to the extent that he was in fact extremely limited in ability at "orthodox" developments of current and popular methods. Thus in 1816, for example, before the flood of his own new thinking, he published a treatise on the binomial series in the style of the old analysis which is really quite remarkably uninteresting.[34] But CAUCHY is a good example of originality of another kind, lacking such sensitivity and feeling for new problems but, when stimulated by the achievements or especially *lack of success* in some contemporary work, would expand the accomplished fragments into immense generalisations and extensions within the same field of research. His monument in mathematics in his theory of functions of a complex variable and their integration, one of the great achievements of all 19th century mathematics. Its origins are to be found in a large paper of 1814 (his 25th year) on the validity of using complex numbers in the evaluation of definite integrals. The technique had been used for decades from time to time, without much consideration of its validity: in particular, in June 1814, LEGENDRE published an instalment of the second volume of his *Exercises du calcul intégral*, a work containing various methods of evaluating definite integrals whose main aim was towards the development of his theory of elliptic integrals.[35] This instalment concerned itself chiefly with integrals whose integrands were the product of rational and trigonometric functions, and it provided the spark for CAUCHY'S fire, for from LEGENDRE'S work CAUCHY came to the following generalised problem concerning the evaluation of definite integrals: what are sufficient conditions for the validity of using complex variables in such evaluations? His solution was the equality of two mixed partial differentials:

$$\frac{\partial^2}{\partial x\,\partial y}\int f(z)\,dz = \frac{\partial^2}{\partial y\,\partial x}\int f(z)\,dz, \tag{11}$$

where z is a complex function of x and y;

$$z = h(x, y) + ik(x, y) \tag{12}$$

and thus

$$f(z) = u(x, y) + iv(x, y). \tag{13}$$

From this fruitful equation (11) stemmed a variety of general theorems (including the "Cauchy-Riemann equations") and thence hosts of particular integrals,

[34] B. BOLZANO, *Der binomische Lehrsatz und als Folgerung aus ihm der Polynomische und die Reihen, ...* (1816, Prague). The most interesting section is on pp. 27—40.

[35] A.-M. LEGENDRE, *Exercises du calcul intégral sur divers ordres des nombres transcendantes et sur les quadratures* (3 vols: 1811—17, Paris).

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including the evaluation of some of LEGENDRE's. CAUCHY presented his paper in August (1814) to the *Institut de France*, and LEGENDRE was one of its examiners: he rightly praised its many important new results, but had a most interesting and important dispute with CAUCHY over the evaluation of

$$\int_0^\infty \frac{x \cos a x}{\sin b x} \frac{dx}{1+x^2}. \tag{14}$$

Put in modern terms, if we regard the integral as a function of $\frac{a}{b}$ then it has a discontinuity of magnitude π at the odd multiple values of its argument. CAUCHY had by separate equations evaluated the left- and right-hand limiting values of the function for $\frac{a}{b}<1$ and $\frac{a}{b}>1$; but in the 1814 instalment of his book LEGENDRE had used a power series expansion method on a generalisation of (14) to produce in a limiting case the *arithmetic mean* of CAUCHY's two evaluations for $\frac{a}{b}=1$, and he could not understand that this new type of algebraic expression — the integral representation — could in fact give a *discontinuous* function. CAUCHY produced a spurious piece of infinitesimal reasoning to resolve the situation to LEGENDRE's satisfaction;[36] but it must have shown him that there were foundational questions in real variable analysis apart from the use of complex numbers with which he would have to deal.

Let us return, however, to the question of CAUCHY's type of mathematical inspiration. We see in this episode that CAUCHY was directly stimulated by LEGENDRE's attempts at integral evaluation to work in exactly the same field, rather than to intuite from it some more general and abstract kind of problem concerned with the use of functions of a complex variable. In the 1814 paper for example, the theory of singularities and residues which he was to produce in later years was given in a *real variable* integral form, which we may write as:

$$\begin{aligned}\int_{y_1}^{y_2}\int_{x_1}^{x_2} \frac{\partial S}{\partial x}\,dx\,dy - \int_{x_1}^{x_2}\int_{y_1}^{y_2} \frac{\partial S}{\partial x}\,dy\,dx = \int_0^{\varepsilon} [S(X+p,Y+q) \\ -S(X+p,Y-q)-S(X-p,Y+q)+S(X-p,Y-q)]\,dp,\end{aligned} \tag{15}$$

where $\frac{\partial S}{\partial x}$ has an infinity at the point (X, Y) inside the rectangle bounded by the sides, $x=x_1$, $x=x_2$, $y=y_1$ and $y=y_2$.[37] His later fine achievements in the new analysis with the theory of integration may be traced in large part to the issues involved in the profound result (15).

In the following year of 1815 CAUCHY had another large paper ready, this time on the propagation of water-waves.[38] Complex variables were again present,

[36] For a full account of this episode see my *Foundations*, ch. 2.

[37] CAUCHY's paper was "Mémoire sur les intégrales définies", *Mém. prés. Acad. Roy. Sci. div. sav.*, (2) **1** (1827), 601—799 = *Oeuvres*, (1) **1**, 319—506. LEGENDRE's evaluation of the integral (14) is in his [35], **2**, 124.

[38] A.-L. CAUCHY, "Théorie de la propagation des ondes à la surface d'un fluide pesant d'une profondeur indéfinie," *Mém. prés. Acad. Roy. Sci. div. sav.*, (2) **1** (1827), 3—312 = *Oeuvres*, (1) **1**, 4—318.

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as they were to be in all of his mathematical output; and integrals were also to be found, for the prominent new feature here was the use of integral methods to solve linear partial differential equations (and thus to use again the integral representation of a function). The inspiration in this case is not so easy to trace, as it is impossible to say how much of FOURIER's then still unpublished work on heat diffusion he had seen; but he knew of POISSON's (lesser) work in the same field, and doubtless he was aware of some results of LAPLACE which we shall discuss later. At all events, in 1817 his further researches brought him to "Fourier's Integral Theorem":

$$f(x) = \frac{1}{\pi} \int_0^\infty f(p)\, dp \int_0^\infty \cos q(x-p)\, dq, \tag{16}$$

in a short paper whose rushed and excited tone suggests that he had really found the result independently of FOURIER.[39] FOURIER acquainted him with his own prior discovery of the theorem, and then CAUCHY certainly did read his manuscripts: not only did he publish an acknowledgement in 1818,[40] but in all his later work on integral solutions to partial differential equations there was a new confidence and dexterity, and again — *extensions* and *generalisations* (to multiple integral solutions, and so on) of what FOURIER had already done.[41]

And then we come to 1821 and the *Cours d'Analyse*: large numbers of theorems on all aspects of real and complex variable function theory, based on the ideas which we listed in our section 2. From where had the inspiration come this time? From within CAUCHY himself? Perhaps; but it is so utterly untypical of his kind of achievement whereas under the hypothesis of his prior reading of BOLZANO it is so perfect an example of it, that it seems difficult not to accept the latter possibility. Perhaps I can best illustrate the force of this point by describing my own researches into the development of the foundations of analysis during this period. I had started naturally enough with CAUCHY'S *Cours d'Analyse* and his other contributions to analysis, and in the course of reading other of his writings his need for an initial external stimulus to his genius had become clear to me. Thus I wanted to find the source of the new ideas of the *Cours d'Analyse*, and so I made a special search of all of CAUCHY'S work written prior to 1821. I found many important things, especially the 1814 integrals paper and the disagreement over (14) with LEGENDRE, and the affair of 1817 over FOURIER'S Integral Theorem (16): there was clearly plenty of motivation for CAUCHY to try to improve analytical techniques. But of the new ideas that were to achieve that aim — of them, to my great surprise, I could find nothing. Only later did I follow up my knowledge that BOLZANO had done "something" in analysis which no-one had read (or so I thought); and I can remember quite clearly the extraordinary effect of reading BOLZANO'S 1817 pamphlet and seeing the *Cours d'Analyse* emerging from its pages. I then re-read the *Cours d'Analyse* and found the fine details of

[39] A.-L. CAUCHY, "Sur une loi de réciprocité qui existe entre certaines fonctions", *Bull. Sci. Soc. Philom. Paris* (1817), 121—124 = *Oeuvres*, (2) **2**, 223—227.

[40] A.-L. CAUCHY, "Second note sur les fonctions réciproques", *Bull. Sci. Soc. Philom. Paris* (1818), 178—181 = *Oeuvres*, (2) **2**, 228—232.

[41] For discussion of these developments, see my *Fourier*, chs. 21 and 22; and BURKHARDT[33], chs. 8—11 *passim*.

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correspondence; but more than that, I could see CAUCHY'S mind at work in its own individual way, taking the fragments of BOLZANO'S thought as he had taken LEGENDRE'S morsels and FOURIER'S substantial achievements earlier, and producing from them whole new systems of mathematical thought.

But if CAUCHY owed so much to BOLZANO, why did he not acknowledge him? To answer this question, we move more fully into the social situation of the time: to Paris, the centre of the mathematical world.

6. The State of Parisian Mathematics

Almost every mathematician of note at this time either lived in or at least visited Paris. One consequence of this galaxy of brilliance was that a state of intense rivalry and sometimes bitter enmity existed almost continuously in the Parisian scientific circles. Everybody was affected by it, although some less than others; and the reasons were not always purely scientific. There were deep and passionate political or religious disagreements, too, heightened by the Napoleonic era and its violent end and brief resurrection in the mid-1810's. These rivalries pose an exciting and difficult problem for the historian of the period, for their detection and description calls for the most careful reading of even the finest point in the most obscure paper, as well as reading *between* the lines of all the scientific literature of the time. Very little work has been done on these rivalries: indeed, most historians have failed to notice them altogether.[42] But perhaps I can give some idea of how they affected the situation and bore especially upon CAUCHY and his *Cours d'Analyse* by describing two of the most important controversies of the time — as fully as I have been able to disclose them.

We have mentioned FOURIER'S name several times, and the first controversy involved his work on heat diffusion. Like GAUSS and BOLZANO, he also had a strong intuition for new problems, and seemingly from about 1802 he began work on the then novel study of the mathematical description of the diffusion of heat in continuous bodies. His early work on the problem proceeded by means of a discrete n-body model, and though he achieved considerable mathematical success a small but vital error in the model itself brought failure to his efforts to obtain a solution for the corresponding continuous bodies by taking n to infinity. Then he had a slight CAUCHY-like inspiration from a small paper of 1804 by BIOT on the propagation of heat in a bar[43] to start again by forming the partial differential equation directly, and in the brief periods of leisure allowed him in the next three years from his duties as Prefect of Isère at Grenoble and from his Egyptological researches he created a genuine revolution of his own: a revolution in mathematical physics, which he took beyond the realm of NEWTONIAN mechanics into a new physical territory of heat diffusion, with its own equations and physical constants and a fresh range of solution methods based on the use of linear equations, the method of separation of variables (then mainly used in solving

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[42] An exception is H. BURKHARDT[33]: for scattered remarks, see ch. 8 *passim*. See also my *Foundations*, esp. chs. 2—5; and *Fourier*, esp. chs. 21 and 22.

[43] J. B. BIOT, "Mémoire sur la propagation de la chaleur," *Bibl. Brit.*, **27** (1804), 310—329 = *Journ. Mines*, **17** (1804), 203—224. FOURIER never acknowledged BIOT'S paper!

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ordinary differential equations) and the superposition of special solutions. FOURIER series were only one consequence of these new methods: another was his creation of the basic theory of the misnamed "Bessel functions", and indeed it was there that he showed his mathematical technique at its greatest. By 1807 he had progressed far; but he was unable to solve the problem of heat diffusion in an *infinite* continuous body, and so he wrote up his theoretical achievements and experimental results in a large monograph submitted to the *Institut de France* in December[44]. LAGRANGE and LAPLACE were the most important of the examiners: for various conceptual reasons LAGRANGE was opposed to the whole approach based on separation of variables, but LAPLACE was very impressed and began to take great interest in FOURIER's work. So a struggle began over the reception of FOURIER's paper, with LAPLACE, FOURIER and MONGE (another examiner, and personally close to FOURIER) in support, and opposition from LAGRANGE and — POISSON.

We must consider POISSON for a moment, for in him more than in any other single person lies the key to the Parisian mathematical rivalries. He graduated brilliantly from the *Ecole Polytechnique* in 1803, and to the aging grand masters of Parisian mathematics — LAGRANGE, LAPLACE, LEGENDRE and MONGE — he must have seemed to be the only heir to their crown: FOURIER was so occupied with administrative work at Grenoble that he could not be expected to be achieving substantial mathematical work, while CAUCHY was still only in his early teens. So POISSON was placed in a position of special favour from the beginning of his career which he exploited to the full, especially by means of influential positions on Parisian scientific journals; but over the next twenty years he gradually but steadily lost favour and reputation to FOURIER and then CAUCHY as they emerged and surpassed him in the quality of their work. The 1807 paper of FOURIER was crucial in this development. By 1805 or 1806 POISSON was already aware of some of FOURIER's results and the type of solution that he was trying to develop: he replied not only by applying to FOURIER's diffusion equation in 1806 the ideas of LAGRANGE and LAPLACE on solutions of partial differential equations using power series of functions,[45] but also by publishing a denigrating five-page review of FOURIER's monograph in 1808 in a journal of which he was mathematical editor.[46] However, LAPLACE, acting in his typical political way, maintained his interest in POISSON (and also in BIOT) while gradually changing his interests towards FOURIER's methods and results. In 1809 he published a miscellany on analysis which — without reference to FOURIER — just happened to contain a treatment of the diffusion equation with initial condi-

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[44] For the references of this manuscript, see [12]; and for a detailed analysis of its contents, see my "Joseph Fourier and the revolution in mathematical physics", *Journ. Inst. Maths. Applics.*, **5** (1969), 230—253. Much new information on FOURIER's life and Prefectural responsibilities is contained in my *Fourier*, ch. 1.

[45] S. D. POISSON, "Mémoire sur les solutions particulières des équations différentielles et des équations aux différences", *Journ. Ec. Polyt.*, cah. 13, **6** (1806), 60—116 (pp. 109—111).

[46] S. D. POISSON, "Mémoire sur la propagation de la chaleur dans les corps solides", *Nouv. Bull. Soc. Philom. Paris*, **1** (1808), 112—116 = FOURIER's *Oeuvres*, **2**, 213—221.

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tions over an infinite interval. His solution

$$v = \frac{1}{\sqrt{\pi}} \int_0^\infty e^{-u^2} f\left(x + 2u\sqrt{Kt}\right) du \tag{17}$$

brought into mathematics a result which later was developed as the "Laplace transform"; it may well have been CAUCHY's inspiration to try integral solutions to partial differential equations.[47] It was certainly FOURIER's inspiration, for it showed FOURIER that an *integral*, rather than a series, solution was applicable in the case of an infinite interval and it led him to "Fourier integrals" and thus to his integral theorem (16). Meanwhile, POISSON had been opposing FOURIER's solution method in favour of functional solutions by means of indirect references in the context of the vibration of elastic surfaces;[48] but FOURIER and his supporters eventually managed to secure a prize problem for heat diffusion in the *Institut de France* for January, 1812. To the revision of the manuscript of 1807 FOURIER added a new section on FOURIER integrals, and also two more new parts on physical aspects of heat which were inspired by discussions with LAPLACE. He won the prize, but the criticisms of LAGRANGE in the examiners' report hurt him for the rest of his life:

"... This work contains the true differential equations of the transmission of heat, both in the interior of the bodies and at their surface, and the novelty of the purpose adjoined to its importance has determined the class [of the *Institut*] to crown this work, observing, however, that the manner of arriving at its equations is not free from difficulties and its analysis of integration still leaves something to be desired, both relative to its generality and on the side of rigour."[49]

LAGRANGE died in 1813; but publication of this second paper was no more likely than its predecessor and so FOURIER wrote his book on heat diffusion as the third version of his work. It did not appear until 1822,[50] having been delayed partly by FOURIER's own difficulties in developing the physical aspects of heat (which he eventually omitted and promised for a sequel which was never written); and the 1812 prize paper did not appear until still later.[51] By this time FOURIER

[47] P. S. LAPLACE, "Mémoire sur divers points d'analyse", *Journ. Ec. Polyt.*, cah. 15, **8** (1809), 229—265 (pp. 235—244) = *Oeuvres*, **14**, 178—214 (pp. 184—193).

[48] See especially the preamble to a prize problem on this topic in *Hist. cl. sci. math. phys. Inst. Fr.* (1808: publ. 1809), 235—241. Obviously written by POISSON, it extols the virtues of functional solutions to the wave equations — in implied contrast to FOURIER series solutions which were then available. In controversial circumstances (described in my *Fourier*, ch. 21), POISSON read his own paper on the subject in 1814, which was published as "Mémoire sur les surfaces elastiques", *Mém. cl. sci. math. phys. Inst. Fr.*, (1812), pt. 2 (publ. 1816), 167—225.

[49] Published in FOURIER's *Oeuvres*, **1**, vii—viii. The manuscript is kept in the Archives of the *Académie des Sciences*, Paris.

FOURIER never allied himself closely to LAPLACE, and gave no acknowledgement to LAPLACE in the prize paper. It may be that LAGRANGE's continued general opposition was supplemented by LAPLACIAN annoyance: the remarkable story of the relations between LAPLACE and FOURIER from 1807 until the 1820's is described in my *Fourier*, chs. 21 and 22.

[50] J. B. J. FOURIER, *Théorie analytique de la chaleur* (1822, Paris) = *Oeuvres*, **1**.

[51] J. B. J. FOURIER, "Théorie du mouvement de la chaleur dans les corps solides", *Mém. Acad. Roy. Sci.*, **4** (1819—20: publ. 1824), 185—555; and **5** (1821—22: publ. 1826), 153—246 = *Oeuvres*, **2**, 3—94.

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had risen to a strong political position, having been appointed *sécretaire perpétuel* of the *Académie des Sciences* in 1821; and then there developed the second of our major controversies, which directly involved CAUCHY's *Cours d'Analyse* — the convergence problem of FOURIER series.

FOURIER series contain many of the problems which we tackle by means of the new analysis, but we have not yet described any of FOURIER's work in that field. The reason is that, although he understood all the basic analytical problems — convergence, the possibility of discontinuous functions, the integral as an area — before both BOLZANO and CAUCHY had begun their work, he was not strongly attracted to pure analysis as a study and so did not develop his own understanding to the extent of that which he was capable.[52] Doubtless CAUCHY was aware of this fact, for in the *Cours d'Analyse* he put the following theorem:

"When the different terms of the series $\left[\sum_{r=1}^{\infty} u_r\right]$ are functions of the same variable x, continuous with respect to that variable in the vicinity of a particular value for which the series is convergent, the sum of the series is also a continuous function of x in the vicinity of that particular value."[53]

The theorem is remarkable for its falsehood: it was known in its day to be false, and indeed CAUCHY knew it was refuted when he put it in his book. But to find the reasons why it was included, we must examine the type of counter-examples which were then known. They were in fact FOURIER series:

$$f(x) = \tfrac{1}{2} a_0 + \sum_{r=1}^{\infty} (a_r \cos rx + b_r \sin rx), \tag{18}$$

where

$$a_0 = \frac{1}{\pi} \int_{-\pi}^{+\pi} f(u)\, du, \tag{19}$$

$$a_r = \frac{1}{\pi} \int_{-\pi}^{+\pi} f(u) \cos ru\, du, \quad r = 1, 2, \ldots \tag{20}$$

$$b_r = \frac{1}{\pi} \int_{-\pi}^{+\pi} f(u) \sin ru\, du, \quad r = 1, 2, \ldots . \tag{21}$$

The trigonometric functions are continuous, and so the series on the right hand side of (18) is covered by CAUCHY's theorem: thus if $f(x)$ is discontinuous, the series cannot be convergent to it. But FOURIER had produced several series of discontinuous functions, and had shown by direct consideration of their n^{th} partial sums that they were convergent; and since 1815 POISSON had found that he had had to abandon his belief in functional and power series solutions in favour of FOURIER series solutions, and he had found similar examples also. So what was CAUCHY's purpose in stating his theorem? There was of course an intellectual aspect to it, for CAUCHY did have a proof: suffice it to say for now that the

[52] In the 1807 manuscript[12], see arts. 42—43, 64—74: in the 1811 paper[51], see part 1, 269—273 and 304—316: in the book[50] (mostly written by 1815), see arts. 177—179 and 222—229.

[53] A.-L. CAUCHY, *Cours*, 131—132 = *Oeuvres*, (2) **3**, 120.

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distinctions between modes of uniform and non-uniform convergence which resolve the difficulty were not noticed by anybody until the 1840's, that CAUCHY's theorem had some role to play in their development, and that shortly afterwards, in his last years, he wrote a pathetic paper of his own on the subject presenting the same type of idea without any reference to recent work.[54] But on the personal side, there was a message to FOURIER and POISSON between the lines of his theorem: "your trigonometric series may be very interesting, but do you have a general convergence proof for them? Do your series not affront the results of the new analysis?"

The later developments of this rivalry read almost like a novel.[55] Briefly, POISSON had already published a general proof in 1820 based on rather crude manipulations of the "Poisson integral"

$$\int_{-\pi}^{+\pi} \frac{(1-p^2)\,f(\alpha)}{1-2p\cos(x-\alpha)+p^2}\,d\alpha;\ ^{56} \tag{22}$$

but, while he never abandoned it, it impressed few of his contemporaries. If CAUCHY knew it when he wrote the *Cours d'Analyse*, then his theorem was already a comment on it; but in a short paper of 1826 on the convergence problem he certainly showed his awareness of it. For he began that paper with a version of POISSON's convergence proof based on (22) to produce the FOURIER series (18); and then he remarked:

"The preceding series [(18)] can be very usefully employed in many circumstances. But it is important to show its convergence."[57]

CAUCHY's own proof followed; and while it was of considerably better mathematical calibre than POISSON's, it contained one vital flaw — the false assumption that if $u_n \to v_n$ as n tends to infinity, then $\sum_{r=1}^{\infty} u_r$ and $\sum_{r=1}^{\infty} v_r$ converge together. That this assumption *is* false was pointed out in a paper of 1829 on the convergence problem by the young DIRICHLET. In this masterpiece DIRICHLET showed the power of the new analysis in producing the famous sufficient "Dirichlet conditions" for the convergence of a FOURIER series to its function: that it may have

[54] A.-L. CAUCHY, "Note sur les séries convergentes ...", *C. R. Acad. Roy. Sci.*, **36** (1853), 454—459 = *Oeuvres*, (1) **12**, 30—36. For a detailed account of the introduction of modes of convergence, see my *Foundations*, ch. 6. The relevance of CAUCHY's theorem in the *Cours* is especially connected with one paper important in the development of modes of convergence: P. L. SEIDEL's "Note über eine Eigenschaft der Reihen, welche discontinuirlichen Functionen darstellen", *Abh. Akad. Wiss. Münich*, **7** (1847—49), math.-phys. Kl., 381—393. This paper (by a pupil of DIRICHLET!) dealt explicitly with that theorem in the light of discontinuous FOURIER series, and is more than likely to have been the (unmentioned) inspiration of CAUCHY's paper of five years later.

[55] A detailed description is given in my *Foundations*, ch. 5.

[56] S.-D. POISSON, "Mémoire sur la manière d'exprimer les fonctions ..." *Journ. Ec. Polyt.*, cah. 16, **11** (1820), 417—489 (pp. 422—424).

[57] A.-L. CAUCHY, "Mémoire sur les développements des fonctions en séries périodiques", *Mém. Acad. Roy. Sci.*, **6** (1823: publ. 1827), 603—612 (p. 606) = *Oeuvres*, (1) **2**, 12—19 (p. 14).

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a finite number of discontinuities and turning values in an otherwise continuous and monotonic course.[58] And his proof was a development of a sketched argument in FOURIER's book of 1822: he took an alternative form to FOURIER's for the n^{th} partial sum of the series and applied to it a precise version of the proof that FOURIER had outlined.[59] Yet there was more than mathematics in DIRICHLET's paper, too, for during his visit to Paris in 1826 he formed such a close personal attachment to FOURIER that his work on the convergence problem was a personal homage in FOURIER's last years. However, he formed no close relationship to CAUCHY: as well as pointing out the error in CAUCHY's 1826 proof and finding general convergence conditions which, in allowing discontinuities in the function refuted CAUCHY's 1821 theorem, he reported in his paper a presumably verbal remark of CAUCHY's on his 1826 paper that:

"The author of this work himself acknowledges that his proof is defective for certain functions for which, however, convergence is incontestable."[60]

One can find CAUCHY's reaction to DIRICHLET's results if one looks carefully: in 1833 CAUCHY published in French at Turin a summarised version of all his 1820's text-books (based on the lectures that he had been giving there in Italian), and was careful to include his theorem from the *Cours d'Analyse* word for word.[61]

And so we return to BOLZANO and his Prague pamphlet. Is it any wonder that in an atmosphere like this CAUCHY made no acknowledgement to him? References were often not made (apart from honorific citations of the great names of the past), either between members of the Paris cliques or outside them; and even then they were some times double-meant. For example, when CAUCHY finally managed to get his 1814 paper on definite integrals and the 1815 paper on water-waves published in 1827 he introduced in 1825 some extra notes and footnotes to the texts and introduced fawning references to the powerful *secrétaire perpétuel* (FOURIER), especially with regard to his invention of the notation $\int_a^b$ to represent the definite integral; he also inserted attacks on the declining POISSON.[62] But there seems to me to be more specific reasons for CAUCHY's failure to acknowledge BOLZANO. He had appreciated the qualities of BOLZANO's work, and I think that he deliberately excluded references to an obviously obscure work in order to *prevent* its acquaintance by rivals such as POISSON and FOURIER (and perhaps others such as AMPÈRE). This is perhaps not a nice remark to make about CAUCHY but it is all too justified, and indeed CAUCHY's personality is worth our separate attention.

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[58] P. G. LEJEUNE-DIRICHLET, "Sur la convergence des séries trigonometriques ...", *Journ. rei. ang. Math.*, **4** (1829), 157—169 = *Werke*, **1**, 117—132. DIRICHLET's contributions to the new analysis in this and other works (described in my *Foundations*, ch. 5), surpass in my view any other of CAUCHY's successors — including ABEL.

[59] See J. B. J. FOURIER[50], esp. art. 423.

[60] See P. G. LEJEUNE-DIRICHLET[58], 157 = *Werke*, **1**, 119.

[61] A.-L. CAUCHY, *Résumés Analytiques* (1833, Turin), 46 = *Oeuvres*, (2) **10**, 55—56.

[62] For CAUCHY's acknowledgements to FOURIER, see [37], 623 = *Oeuvres*, (1) **1**, 340; and [38], 194 (omitted from *Oeuvres*, (1) **1**, 197). For the attacks in [38] on POISSON, see pp. 187—188 = *Oeuvres*, (1) **1**, 189—191.

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7. Cauchy's Personality

If CAUCHY was one of the greatest mathematicians of his time, he was one of the most unpleasant personalities of all time: a fanatic for Catholic and Bourbonist causes to the point of perversion, he had to prove his superiority at all times over even the weakest of his contemporaries and to publish a virtually continuous stream of work. He also wrote articles on education, the rights of the Catholic and Bourbon causes, and the reform of criminals, to supplement his mathematical output; but he never helped and even at times hindered his younger colleagues in their careers and work. A good example of this concerns a young man who wrote the following of him:

"Cauchy is a fool, and one can't find any understanding with him, although he is the mathematician who at this time knows how mathematics should be treated ... he is extremely catholic and bigoted ... ."

The writer was ABEL, in a letter sent to his friend HOLMBOE when, like DIRICHLET, he visited Paris in October, 1826.[63] Poor ABEL: he cannot have known how right he was, just as he did not understand the Parisian political situation. While in Berlin during the previous January, he had written a paper on convergence tests and their application to the binomial series which made important use of the new analysis: he had also spotted the weakness in CAUCHY's theorem of the *Cours d'Analyse* and made the first public mention of the point in a footnote to the paper.[64] Later in the same letter to HOLMBOE he remarked:

"I have worked out a large paper on a certain class of transcendental functions to present to the *Institut*. I am doing it on Monday. I showed it to Cauchy: but he would hardly glance at it. And I can say without bragging that it is good. I am very curious to hear the judgement of the *Institut* ... ."[65]

This was the paper which ushered in the transformation of LEGENDRE's theory of elliptic integrals into his own theory of elliptic functions; and the story of its fate is only too characteristic of Parisian science and of CAUCHY. CAUCHY and LEGENDRE were the examiners: CAUCHY took it and, perhaps because of ABEL's footnote against his theorem, ignored it entirely: only after ABEL's death in 1829 did he fulfil a request to return it to the *Académie des Sciences*. It was finally published in 1841, when the manuscript vanished in sensational circumstances, to be rediscovered only in the 1950's. This story is well-known;[66] however, there is one aspect of it which has been little remarked upon but which shows the depths to which CAUCHY could sink. When ABEL's paper was in the press another Norwegian mathematician presented a paper to the *Académie des Sciences*

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[63] *Niels Hendrik Abel. Mémorial publié à l'occasion du centenaire de sa naissance* (1902, Christiana), *Correspondance d'Abel* ..., 135 pp. (pp. 45 and 46) = *Texte original des lettres* ..., 61 pp. (pp. 41 and 42). Also in *Oeuvres* (ed. L. SYLOW & S. LIE), **2**, 259.

[64] N. H. ABEL, "Untersuchungen über die Reihe ...", *Journ. rei. ang. Math.*, **1** (1826), 311—329 (p. 316) = *Oeuvres* (ed. B. HOLMBOE), **1**, 66—92 (p. 71) = *Oeuvres* (ed. L. SYLOW & S. LIE), **1**, 219—250 (p. 225).

[65] In addition to the references in [63], we may add for this passage ABEL's *Oeuvres* (ed. B. HOLMBOE), **2**, 269—270.

[66] For a detailed account of this affair, see O. ORE, *Niels Hendrik Abel — mathematician extraordinary* (1957, Minneapolis), 246—261.

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on elliptic functions. CAUCHY was again an examiner, and his report contains the following words:

"Geometers know the beautiful works of Abel and of Mr. Jacobi on the theory of elliptic transcendentals. One knows that of the important papers ... one of them in particular was approved by the *Académie* in 1829, on the report of a commission of which Mr. Legendre was a part [CAUCHY himself having been the other!], then crowned by the *Institut* in 1830, and that the value of the prize was remitted to Abel's mother. In fact this illustrious Norwegian, whom a project of marriage had determined to undertake a voyage in the depth of winter, unfortunately fell ill towards the middle of January 1829 and, in spite of the care that had been lavished on him by his fiancée's family, he died of phthisis on the 6th April, having been confined to bed for three months ....

"Before completing this report where we have often had to recall the works of Abel, it appears to us proper to dispel an error which is already quite widespread. It has been supposed that Abel died in misery, and this supposition has been the occasion for violent attacks directed against scholars from Sweden and from other parts of Europe. We would want to believe that the authors of these attacks will regret that they expressed themselves with such vehemence, when they read the Preface of the ... *Oeuvres d'Abel*, recently published in Norway by Mr. Holmboe, the teacher and friend of the illustrious geometer. They will see there with interest the flattering encouragements, the expressions of esteem and admiration that Abel received from scholars during his life, particularly from those who occupied themselves at the same time as he with the theory of elliptic transcendentals ...."[67]

In fact CAUCHY must have known that, while preparing his 1839 edition of ABEL'S works, HOLMBOE had tried without success to obtain the 1826 manuscript from the *Académie des Sciences* and that its publication in 1841 was due only to the fact that he had raised the matter to governmental level. 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 acknowledgement.

But how unknown *was* BOLZANO'S paper?

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[67] A.-L. CAUCHY, "Rapport sur un mémoire de M. Broch, relatif à une certaine classe d'intégrales," *C. R. Acad. Roy. Sci.*, **12** (1841), 847—850 = *Oeuvres*, (1) **6**, 146—149. ABEL'S paper was then appearing as "Mémoire sur une propriété générale d'une classe très-étendue de fonctions transcendantes", *Mém. prés. Acad. Roy. Sci. div. sav.*, (2) **7** (1841), 176—264 = *Oeuvres* (ed. L. SYLOW & S. LIE), **1**, 145—211. BROCH'S paper appeared as "Mémoire sur les fonctions de la forme

$$\int x^{s-\nu\varrho-1} f(x^{\varrho})\, R(x^{\varrho})^{+\frac{s}{x\varrho}}\, \partial x",$$

Journ. rei. ang. Math., **23** (1846), 145—195 and 201—242: we note the five-year delay, and the fact that its publication was *not* in the journal of the *Académie* to which it had been assigned. CAUCHY'S report (with LIOUVILLE as co-signatory but certainly not author!) prefaced the paper on pp. 145—147: he was referring in the above quotation to the "Notice sur la vie de l'auteur" that HOLMBOE put in his edition of ABEL'S *Oeuvres*, **1**, v—xiv. At the end of that edition HOLMBOE included a selection of his letters from ABEL, and we note from [63] and [65] that he did *not* include ABEL'S remark on CAUCHY.

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8. The Availability and Familiarity of Bolzano's Work

We have mentioned several times that BOLZANO's achievements anticipated specifically the work of the WEIERSTRASS school in the 1860's, and it was they who first brought BOLZANO's mathematical publications[68] to general attention at that time. DU BOIS REYMOND, CANTOR, HANKEL, HARNACK, HEINE, SCHWARZ, STOLZ — they formed perhaps the most talented group ever to work on foundational problems in analysis, and they all had a deep interest in the history of their subject. I do not know which of them first came across BOLZANO's writings: the first to make a reference in print was HANKEL in 1871,[69] but SCHWARZ was the one most interested in these questions and it was he who around that time named WEIERSTRASS's theorem on the existence of a limiting value of an infinite closed sequence of values the "BOLZANO-WEIERSTRASS theorem", in view of BOLZANO's theorem on the existence of an upper limit in his 1817 pamphlet which we quoted in section 2.4.[70] WEIERSTRASS's group were then studying continuous non-differentiable functions, rational and irrational numbers, and the early ideas of set theory, on all of which BOLZANO had preceded them; and so it had tended to be assumed (posterior wisdom again) that in his own day BOLZANO was not read at all. Without any doubt his works were not widely available — for proof of this, we need only mention that it is today extremely difficult to find copies of any of them. But it would be a mistake to assume that because they appeared as pamphlets they could not have become widely familiar. On the contrary, at that time the publication of pamphlets was a common method of issuing scientific literature and indeed avoided the notorious delays of academy journals: CAUCHY for example, always anxious for rapid publication, put some very important work into pamphlets and lithographs, and even published his own mathematical journal during two periods of his life.[71] There seems to have been a well organised trade in the sale of such material, based on the catalogues of book shops designed especially for scientific and intellectual circles: it was by these means, for example, that BOLZANO in Bohemia managed to learn of and obtain the current literature. So we may presume that the work was in reasonably fluid circulation — and surely especially in Paris, the scientific centre of the age. CAUCHY himself reveals this in his own writings. Although his refer-

[68] Apart from the *Beweis* and the works listed in [29] and [34], BOLZANO also published *Betrachtungen über einige Gegenstände der Elementargeometrie* (1804, Prague) = *Schriften*, **5** (1948, Prague), 9—49; and *Beiträge zu einer begründeten Darstellung der Mathematik. 1. Lieferung* (1810, Prague) = (1926, Paderborn). (No other parts published.) BOLZANO's friend F. PŘIHONSKÝ posthumously published his *Paradoxien des Unendlichen* (1851, Leipzig): there have been various re-issues and translations of this work, including an English edition (1950, London).

[69] H. HANKEL, "Grenze", *Allg. Enc. Wiss. Künste*, sect. 1, pt. 90 (1871, Leipzig), 185—211: see pp. 189, 209—210. The first major study was by STOLZ, as "B. Bolzanos Bedeutung in der Geschichte der Infinitesimalrechnung", *Math. Ann.*, **18** (1881), 255—279 (and corrections in **22** (1883), 518—519).

[70] See K. SCHWARZ, "Zur Integration der partiel Differentialgleichung $\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0$", *Journ. rei. ang. Math.*, **74** (1872), 218—253 (p. 221) = *Abhandlungen*, **2**, 175—210 (p. 178).

[71] See his *Exercises des Mathématiques* (4 vols. and 1 instalment: 1826—30, Paris), and *Exercises d'Analyse et de Physique Mathématique* (4 vols: 1840—47, Paris). They appear respectively in his *Oeuvres*, (2) **6—9**; and (2) **11—14**.

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ences were often not always given, they show that he was abreast of current writings in all European languages, and not only the most prominent authors, books and journals: there are also references to little known material. In the *Cours d'Analyse*, for example, he referred to a pompous little tract of 1820 published in London on rules of signs in the theory of equations,[72] which was at least as obscure as BOLZANO's pamphlet. In fact, BOLZANO had given his paper two opportunities for publication, for not only did he issue it as a pamphlet in 1817, but — with the same printing — inserted it into the 1818 volume of the Prague Academy *Abhandlungen*.[73] That journal was available in Paris: indeed, the *Bibliothèque Impériale* (now the *Bibliothèque Nationale*) began to take it with *precisely the volume containing Bolzano's pamphlet*.[74] So here is at least one plausible possibility for CAUCHY to have found a copy of BOLZANO's paper, quite apart from the book-trade: he could have noticed a new journal in the library's stock and examined it as a possible course of interesting research.

We turn now from the availability to the familiarity of BOLZANO's works. We have seen that they were not widely circulated, although probably more so than might be imagined; but apart from that I feel that an important factor in the apparent indifference of his contemporaries was a *lack of understanding* of what he had achieved. Since his important results were so far ahead of its time, only a genius of CAUCHY's type and magnitude could bring them to the realisation they deserved (and of which their creator was probably incapable). We can appreciate this point better if we return to ABEL. There is no reference to any of BOLZANO's works in ABEL's writings, and seemingly no direct influence either, even though they had both written on the binomial series; but ABEL had certainly read some BOLZANO, for he expressed great admiration for him in a notebook and hoped to meet him in Prague during his European tour.[75] I suspect that several mathematicians were in ABEL's position: impressed by BOLZANO's work, but unable to take it further themselves.[76] But without doubt there were, unfortunately, many who never discovered it at all. This, therefore, is a situation in marked contrast to CAUCHY's works, which were read by everybody — including BOLZANO.

[72] P. NICHOLSON, *Essay on involution and evolution; containing a new accurate and general method of ascertaining the numerical value of any function* ... (1820, London). CAUCHY's reference is in the *Cours*, 500 = *Oeuvres*, (2) **3**, 409: he also wrote a number of papers on this subject in the 1810's, but with an interest towards structural properties (permutations, *etc.*) rather than in the foundations of analysis. For commentary, see H. WUSSING, *Die Genesis des abstrakten Gruppenbegriffes* (1969, Berlin) esp. pp. 61—66.

[73] See the references in [3].

[74] The present call mark of this volume is R. 15200 in the *Département des Imprimés*. There is no record of its readers, neither does it contain any annotated markings or corrections. The only other copy of the work known to me in Paris is in the holding of the journal by the *Muséum Nationale d'Histoire Naturelle* — a source hardly likely to have been used by CAUCHY. The copy has no revealing annotations on it.

[75] See L. SYLOW, "Les études d'Abel et ses découvertes," [62], 59 pp. (pp. 6 and 13); and K. RYCHLIK, "Niels Hendrik Abel a Čechy", *Pok. mat. fys. astron.*, **9** (1964), 317—319.

[76] LOBACHEWSKY also knew BOLZANO's 1817 pamphlet on the roots of a continuous function: see B. L. LAPTIEV, "О библиотечных записах книг и журналов, выданных Н. И. Лобачевскому", Усп. Мат. Наук, **14** (1959), pt. 5, 153—155.

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9. The Personal Relations between Bolzano and Cauchy

That CAUCHY read BOLZANO's 1817 pamphlet is the subject of our conjecture; but that BOLZANO read CAUCHY's *Cours d'Analyse* is beyond question, for in an important manuscript of the 1830's on analysis he referred to CAUCHY's as one of the recent formulations of continuity in his own style.[77] By then of course, BOLZANO's ideas had gained much publicity through CAUCHY's book, which itself had been published at Königsberg in 1828 in a German translation which may well have been the version that BOLZANO read. Yet there was never a priority row between the two over their common ideas. This is, however, not surprising. In the first place, BOLZANO was no CAUCHY, incessantly anxious for publication and his "rights"; and in addition he was already a controversial figure in Bohemia on account of his progressive views on society and religion. Thus, even if he had wanted to stage a priority row from Bohemia against the great CAUCHY in Paris, he would have found it especially difficult. But I would suggest that there is still another reason why BOLZANO did not promote such a row; namely, that he probably *never noticed* the correspondence of ideas — or at least their significance — when he read the *Cours d'Analyse*. For the *Cours* is a large book, nearly 600 pages in length; and almost all of it is CAUCHY, applying BOLZANO's germinal ideas to one analytical problem after another. But the ideas themselves and the direct points of correspondence appear only here and there in its course, and could easily be missed in the general context.

This view is strengthened when we consider their personal relations. There was no meeting between the two in the 1810's or 1820's, for CAUCHY was in France and BOLZANO in Bohemia; but after the fall of the Bourbons in 1830 CAUCHY exiled himself, firstly to Italy, and then, between 1833 and 1835, to Prague to assist in the education of the son of the dethroned King CHARLES X. The tone of BOLZANO's reaction to CAUCHY's visit to Prague, in a letter he sent to his friend PŘIHONSKÝ in August, 1833, indicates quite clearly that he had had no contact with CAUCHY of any sort and that he suspected no *direct* use of his results by CAUCHY:

"The news of Cauchy's presence [in Prague] is uncommonly interesting for me. Among all living mathematicians today he is the one whom I esteem the most and to whom I feel the most akin; I owe to his inventive spirit some of the most important proofs. I ask you very much to recommend me to him and to say that I would have travelled now straight to Prague to make his personal acquaintance, if I — after what you tell me of his appointment — could not hope for certain that I will meet him at the end of September, ..."[78]

There were in fact a few meetings, for BOLZANO described them in a letter of December, 1843 to FESL:

"Cauchy, the mathematician, was ... in the years 1834 or 1835 ... in Prague, where we met a few times during the few days that I was accustomed to spend at that time (at Easter and in the autumn) in Prague. After my departure I let

[77] B. BOLZANO[7], in *Schriften*, **1** (1930, Prague), 15: see also p. 94.

[78] See E. WINTER (ed.), "Der böhmische Vormärz in Briefen B. Bolzanos an F. Přihonský (1824—1848)", *Veröff. Inst. Slav., Dtsch. Akad. Wiss. Berlin.*, **11** (1958), 306 pp. (p. 156).

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Kulik deliver to him (1834) an essay filling a single quarto sheet which I had drafted for Cauchy sometime in French, on the famous mathematical problem of the rectification of curves, because I rightly feared that he would find the "Paper on the three problems of rectification, planing and cubing" published in 1817[79] too comprehensive and difficult. Early last year, as I was looking through some issues of Cauchy's writings[80] bound with the usual coloured wrappers, and [turned to] the lists of works announced on the back, I noticed with astonishment a small note by him on the same subject, that he had published as a lithograph in Paris in 1834 (therefore presumably only after he had read my little essay). Naturally I would be very eager to read the note ... ."[81]

Eventually BOLZANO managed to obtain a copy of the paper: in fact it came through FESL who pointed out to him that it had been written in 1832 rather than 1834 and so could not be related to his essay, and that it treated the subject in a quite different way. BOLZANO admitted this in an acknowledgement to FESL in May, 1844,[82] and it is quite clear that in this case there was not even a correspondence of ideas; but on the foundations of analysis a very different situation seems to have applied. One would dearly like to know the content of their conversations; but if BOLZANO ever wondered even for a moment that CAUCHY had read his 1817 paper before writing the *Cours d'Analyse*, I imagine that he would have been pleased rather than annoyed. For when he wrote that paper, he had known then that it was a significant work which would probably not reach the audience that it deserved; and so he had ended its preface with a plea to the scientific community which I believe CAUCHY accepted:

"... I must request ... that one does not overlook this particular paper because of its limited size, but rather examine it with all possible strictness and make known publicly the results of this examination, in order to explain more clearly what is perhaps unclear, to revoke what is quite incorrect, but to let succeed to general acceptance, the sooner the better, what is true and right."[83]

10. Epilogue

My conjecture has aroused considerable adverse criticism before publication, and will doubtless receive much more now: thus to minimise the possibility of misunderstandings of this paper, a few points may be worth stressing.

1. Part of my purpose has been to describe some of the extra-intellectual aspects of Parisian mathematics; and whether or not my conjecture is correct

[79] The reference for this work is given in [29].

[80] Presumably the *Exercises d'Analyse* listed in [71].

[81] See I. SEIDERLOVÁ, "Bemerkung zu den Umgängen zwischen B. Bolzano und A. Cauchy," *Čas. pěst. mat.*, **87** (1962), 225—226.

[82] See [81]. CAUCHY's paper, read to the *Académie des Sciences* on the 22nd October, 1832, was the "Mémoire sur la rectification des courbes et la quadrature des surfaces courbes", *Mém. Acad. Roy. Sci.*, **22** (1850), 3—15 = *Oeuvres*, (1) **2**, 167—177; but in the publisher's lists in the *Exercises d'Analyse* it is described as an 11-page lithograph of 1832, which was its first publication. I do not understand why BOLZANO thought that it had been published in 1834.

[83] B. BOLZANO, *Beweis*, end of preface.

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I am firmly convinced that rivalries of the type of which I have given some examples played an important role in Parisian mathematics, and so I have tried to bring to the attention of historians of this period the kinds of historical problem that they will have to face in interpreting its literature. In addition, the theory of "limit-avoidance" is an historical tool which appears to be some use in one form or another in investigating the development of analysis and the calculus in this and other periods.

2. I cannot stress too strongly that in characterising Cauchy's genius as responsive to exterior stimuli I am trying to *describe* rather than *decry* the depth and extent of his originality. Without any question he and GAUSS were the major mathematicians of the first decades of the nineteenth century: thus his work has to be given especial attention by historians. It is of course not my position that CAUCHY would *never* give references without intending a double meaning, but I do think that in his writings, *and equally in those of his "colleagues"*, questions of this type do need to be borne very carefully in mind. With regard to BOLZANO's pamphlet, it is possible that CAUCHY, the busy and active research mathematician and professor at three Paris colleges, simply did not bother to mention it or even forgot that he had read it (though personally I would not regard this explanation as sufficient). My case would be much strengthened by documentary evidence of some kind: CAUCHY did leave a *Nachlass* containing mathematical manuscripts and correspondence, for it was used by VALSON when preparing his excessively admiring biography of CAUCHY,[84] but unfortunately it was kept in the family and there is reason to think that, like his library, it has now been lost.

3. I remarked that CAUCHY was familiar with European languages: in the case of German, it is perhaps worth mentioning explicitly (from a number of examples) that he examined in 1817 a manuscript in German sent in to the *Académie des Sciences*,[85] and that he reviewed MÖBIUS's *Der barycentrische Calcul* in 1828.[86] We may also record another "coincidence of ideas" with obscure German writing strikingly similar to the case of BOLZANO's pamphlet. In April 1847, GRASSMANN, then a schoolmaster at Stettin, sent to CAUCHY two copies of his 1844 *Ausdehnungslehre*, but he never received any acknowledgement; however between late 1847 and 1853 CAUCHY published a number of papers on a theory of "clefs algébriques" which basically used the same sort of ideas and even some almost identical notation.[87] I offer no judgement here on the matter:

[84] C.-A. VALSON, *La vie et les travaux du Baron Cauchy* (2 vols.: 1868, Paris): see esp. vol. **2**, viii—x.

[85] See *Procès-Verbaux des séances de l'Académie tenues depuis la fondation jusqu'au mois d'août, 1835* (10 vols: 1910—22, Hendaye), **6**, 210. I may remark here that these volumes 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!

[86] A.-L. CAUCHY, *Bull. Univ. Sci. Ind.* [Ferrusac], *Sci. math. phys. chim.*, **9** (1828), 77—80. Not in the *Oeuvres*.

[87] For the references and some discussion of the affair, see M. J. CROWE, *A history of vector analysis* (1967, Notre Dame and London), 82—85 and 106. CROWE's last reference in his [63] is inaccurate and in fact misleading; it should be "Mémoire sur les clefs algébriques", *Exercises d'Analyse et de Physique Mathématique*, **4** (1847, Paris), 356—400 = *Oeuvres*, (2) **14**, 417—460.

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I merely record it as another example of the kind of historical problem which surrounds the great achievements of the Parisian mathematicians of the time, when Paris was the centre of the scientific world and CAUCHY's achievements among its principal adornments.

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(Received February 17, 1970)

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