Remaking Mathematics: Bolzano reads Lagrange

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In einer mathematischen Schrift das Motto:

Je suis homme, et j'ai fait des livres, j'ai donc fait aussi des erreurs. Exceptons, si l'on veut, les livres de Géometrie et leurs Auteurs. Encore s'il n'y a point d'erreurs dans les propositions mêmes; qui nous assurera qu'il n'y en ait point dans l'ordre de déduction, dans la choix, dans la méthode? Euclide démontre, et parvient à son but, mais quel chemin prend-il? combien n'erre-t-il pas dans sa route? [1]

Bolzano, a philosopher of the first rank, was also a mathematician of considerable ability. This was, in his day as in ours, a rare combination of talents; for the most part, mathematicians and philosophers at that time formed two separate communities, each ignorant of the other's concerns. Hence when Bolzano set out to write on mathematics, he addressed two almost entirely disjoint groups, and his writings had two distinct goals, attempting to raise the level of mathematical culture among philosophers on the one hand and the level of philosophical culture among mathematicians on the other.

On the philosophical side, for example, he devoted many pages to a critical assessment of the remarks of Kant and his followers. This was done not in order to protect mathematicians from a bad influence—for there was little to fear in that direction at the time—but rather because Kant's misunderstandings had important (and unfortunately widespread) consequences for philosophical doctrine. Bolzano pointed out in considerable detail what has since become glaringly evident: *viz.*, that Kant's account of mathematics, however well it might have fit the practice of earlier mathematics (and Bolzano did not think it did so very well), was not at all in touch with developments in the science since the mid-eighteenth century. In the very years that Kant was underlining the central role of construction in mathematics, leading mathematicians like Euler and Lagrange were setting out treatises on analysis and even mechanics which used no constructions at all. Early nineteenth century research, to which Bolzano made decisive contributions, confirmed

[1]Lettres écrites de la montagne par J. J. Rousseau, 1re lettre p. 9. Quoted after Bolzano's notebooks, Miscellanea mathematica 1812-12, ed. B. van rootselaar and A van der Lugt **BBGA** 2B 3/1.124.

the point, serving as a living refutation of Kant's views: the mathematical method employed there was not notably different from the philosophical, as Kant had said it must be; and it was apparent that pure intuitions, apart from the inconvenience of there not being any, were not only not required, but also of no use in the new mathematics.

If philosophers of that time had much to learn from mathematics, so too, Bolzano thought, mathematicians had a good deal to learn from philosophy. Most surprising, shocking even, was the lack of logical culture among mathematicians. As Bolzano showed over and again, mathematicians often did not have a very clear notion of the meaning of their concepts and propositions, nor of what their proofs were supposed to accomplish. The result, too frequently, was a nearly complete incoherence. This did not necessarily lead directly to incorrect results, and indeed the best eighteenth-century mathematicians, by dint of both luck and sound instinct, had had amazing successes despite the defects of their methods. But, Bolzano noted, the further we move from familiar ground, the more likely such confusions were to lead to errors. For this reason and others, mathematicians needed, he claimed, a new philosophy of mathematics, a better-founded notion of the conceptual and deductive structure of science. It was one of the great accomplishments of his life to provide one. It was communicated not only in his philosophical writings, but also in his mathematical treatises, each of which begins with a homily on correct method.

Bolzano's relation to philosophers like Kant is an important subject. Equally or perhaps even more so from a philosophical perspective, in my opinion, are his relations to the mathematicians. I will attempt to illustrate this by means of a discussion of Bolzano's reading of Lagrange, one of the most eminent analysts in Bolzano's formative years.

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With Cauchy, Bolzano was among the most thorough and acute of Lagrange's readers, and it is clear that Bolzano had a good deal of respect for him as a mathematician, going through his treatises pencil in hand as soon as they were published, and occupying himself with many of the same questions. Like Lagrange, Bolzano was dissatisfied with the state of the foundations of analysis; like him he sought to provide an autonomous foundation for this branch of mathematics, one free from appeals to infinitesimals, geometry, and motion. Bolzano also appears to have respected Lagrange's opinion on the contents of analysis. His early papers, on the binomial theorem and the intermediate value theorem, deal with topics treated prominently by Lagrange; and his *Theory of Functions*,[2] written in the 1830's, incorporates (as the title alone might lead us to expect) many features of Lagrangian analysis, notably the centrality of the function concept and a prominent concern with Taylor's theorem.

This broad agreement on content, however, was accompanied by sharp disagreements concerning method. Indeed, Bolzano chose his early subjects in part precisely in order to accentuate these differences. For Lagrange's entire approach to analysis was out of harmony with Bolzano's philosophy of science. And as Lagrange's work was in many ways the highest expression of analysis around the beginning of the nineteenth century, Bolzano's criticisms applied quite generally to the state of mathematics at the time. The difficulties which he found were not of the kind that one could hope to resolve by small changes of detail. They were, rather, systemic. What was required, according to Bolzano, was no less than a "complete transformation" of mathematics, at least of those parts which are not to be rejected as completely incorrect.[3] Not one to make such a statement idly, Bolzano had already been working on the task for over a decade, and would spend a good part of the rest of his life attempting to finish the work, rebuilding mathematics from the ground up in line with his methodology. This led to a detailed confrontation with eighteenth-century and notably Lagrangian mathematics; and it is here, in Bolzano's criticisms, and the alternatives he proposes, that we find the unmistakable imprint of his philosophy.

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In his *Théorie des fonctions analytiques* and again in the *Leçons sur le calcul des fonctions*, Lagrange set out his proposal for a new foundation of analysis, one which presented the principles of the calculus in a way which was, as he put it, "freed from any consideration of the infinitely small, of vanishing [quantities], of limits and of fluxions, and reduced to the algebraic analysis of finite quantities."[4] The philosophical background of Lagrange's work is recognizably of Cartesian derivation.[5] He does not doubt, for example, that "one might, by using limits viewed in a special way, rigorously demonstrate the principles of the differential

[2] *Functionenlehre*, ed. K. Rychlik (Prague, 1930).

[3] *Rein analytischer Beweis des Lehrsatzes, daß zwischen je zwey Werthe, die eine entgegengesetzes Resultat gewähren, mindestens eine reele Würzel der Gleichung liege* (Prague, 1817), Preface; English translation by S. B. Russ, *Historia Mathematica* **7** (1980) 156-185.

[4] Sub-title of Lagrange's *Théorie des fonctions analytiques*.

[5] Cf. C. Fraser, "Lagrange's analytical mathematics, its Cartesian origins and reception in Comte's positive philosophy," *Stud. Hist. Phil. Sci.* **21**(1990) 243-256.

calculus,"[6] as indeed some like d'Alembert have already done. But merely arriving at the results is not sufficient: d'Alembert's concept of limit, for instance, "although sound enough, is not sufficiently clear to serve as a principle for a science whose certainty must be founded on evidence."[7] Still further: all previous attempts, he thinks, have failed to engage the real substance of analysis. Talk of limits, of motion or of geometrical configurations is decidedly beside the point for Lagrange. For him, as for Euler, analysis has to do with functions, i.e. "expressions de calcul," and their transformations according to certain rules. The focus, accordingly, should be on the relationships of algebraic form between the analytical expressions. The principles of the science should reflect this by remaining within the bounds of this activity: they should give some purchase on the things which analysts are most directly concerned with. In short: "Analysis should have no other foundation than that which consists in the first principles and operations of calculation."[8] To focus the mind precisely on the matters at hand, and to rid oneself of all extraneous clutter: this is one of the most compelling senses of Cartesian simplicity, and it is perhaps the dominant one in Lagrange's notion of principles. Like Descartes, too, Lagrange found this simplicity in algebra.

This desire for simplicity, it seems to me, is also the primary reason for Lagrange's rejection of geometrical considerations from analysis as "foreign elements":[9] not so much because analysis (the study of magnitudes) is more general than geometry (the study of spatially extended magnitudes); but rather because appeals to geometry obscure what should be clear–*viz.*, underlying relations of algebraic form.[10] The point has some justice. Consider, for example, the various classical applications of the method of exhaustion in the works of Euclid and Archimedes. From an algebraic point of view, most of these bear on simple relationships which can be expressed by means of elementary integrals like $\int ax = \frac{ax^2}{2}$ and $\int ax^2 = \frac{ax^3}{3}$; and the same relationships keep turning up, time after time. The relationships emerge at the end of the day; but the proofs, which focus on idiosyncratic features of the geometrical configurations under consideration, do little to draw our attention to them. The same relations might seem to turn up in different cases more or less by chance. Much better, in geometry and analysis, to put the algebra front and centre.

[6] *Leçons sur le calcul des fonctions*, p. 8:
[7] *Théorie des fonctions analytiques*, p. 16.
[8] *Leçons*, p. 8.
[9] See, for example, "Théorie des fonctions analytiques," p. 17.
[10] Of course, there was a widespread belief at the time that simplicity, clarity and generality went hand in hand. See, for instance, the fifth of Descartes' rules, or the preface to d'Alembert's *Traité de dynamique* (Paris, 1743).

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Lagrange's proposal for the calculus attempted to do just that. He begins with the notion of a function as an "expression de calcul" involving variable and constant quantities, by which he intends, primarily, the elementary functions (powers of x, the trigonometrical, logarithmic, and exponential functions), and whatever can be generated from these by addition, subtraction, multiplication, division, taking roots, raising to powers, composition, inversion, and by some transcendental operations such as integration and differentiation. The basic assumption—for which Lagrange offers a none too solid proof—is that every function, its variable x being increased by a variable increment i, can be developed as a power series of the following form:

$$f(x+i) = fx + ip + i^2q + i^3r + \cdots \tag{1}$$

This expansion is claimed to be valid "in general"; by this Lagrange means about the same thing we do when we say that in July the weather is, in general, hot. There may, in short, be exceptions; but Lagrange is confident that he can explain such exceptions as due to isolated collapses of algebraic form.[11] The central point is that in most cases we not only can develop functions in a series of the form (1), but also that the development is known. (It is for this reason that Lagrange speaks—bizarrely—of the necessity of an *a priori* proof that every function has such a development;[12] like Euler, who contented himself with an empirical verification,[13] Lagrange thought that this proposition had been abundantly confirmed in practice.)

Then the usual results of the calculus can be obtained, Lagrange claims, using the intrinsic properties of the representation (1), and what he calls the algebra of finite quantities. Instead of defining $\frac{dy}{dx}$ as a ratio of infinitesimals, with all the problems they bring, he defines the derived function $f'(x)$ as the coefficient $p = p(x)$ of i in the expansion (1). After a nifty calculation, again using only algebra and the assumption of representability, we can show that

$$q = 2p' = 2f''(x)$$

[11]E.g. the function $fx = \sqrt{x}$ cannot be developed in this form for $x = 0$: there $f(0+i) = i^{\frac{1}{2}}$. But it is precisely here, when $x = 0$, that the function loses its algebraic form because $\sqrt{0} = 0$. Lagrange discusses such exceptional cases in the 8th of his *Leçons*. For a discussion of exceptional values in Lagrange's analysis, see C. Fraser, "Joseph-Louis Lagrange's algebraic vision of the calculus," *Historia Mathematica* **14**(1987)38-53.

[12]*Théorie des fonctions analytiques*, N^o.2.

[13]*Introductio in analysin infinitorum*, Ch. 4.

twice the derived function of the derived function of f. And so on for the higher terms, which allows us to define the derivatives of all orders in terms of the coefficients of the various powers of i in the expansion (1) according to the schema of Taylor's series:

$$f(x+i) = fx + if'(x) + \frac{i^2}{2}f''(x) + \frac{i^3}{3!}f'''(x) + \cdots + \frac{i^n}{n!}f^{(n)}(x) + \cdots \quad (2)$$

The basic strategy is repeated over and again, with considerable ingenuity, to derive the standard results of analysis. The product rule for differentials,

$$d(uv) = udv + vdu \quad (3)$$

to take one example, is proved as follows.
Since

$$f(x+i) = fx + if'(x) + \frac{i^2}{2}f''(x) + \cdots \quad (4)$$

and

$$g(x+i) = gx + ig'(x) + \frac{i^2}{2}g''(x) + \cdots \quad (5)$$

We have, multiplying these series and collecting terms according to the powers of i:

$$f \cdot g(x+i) = f(x+i)g(x+i) = f(x)g(x) + i(f'(x)g(x) + g'(x)f(x)) + \cdots \quad (6)$$

The coefficient of i, namely $f'(x)g(x) + g'(x)f(x)$, which by definition is $(f \cdot g)'(x)$, gives the desired result.

The modern reader recoils instinctively at Lagrange's proposal, because it seems that the problems of the infinite in the calculus, rather than being avoided, have simply been ignored. The expansion of a function in a power series (1) is often infinite; and at that point, it no longer makes sense to speak of the "algebra of finite quantities". Multiplying infinite series term by term might yield valid results, but again it might not; similarly for all manner of operations (including the manipulation of infinite arrays of infinite series) which Lagrange undertakes, apparently without any worries whatsoever. Still more alarming: the question of convergence for series is never addressed in the general theory; it is relegated to the study of applications.

What has struck many modern readers as typical eighteenth-century carelessness is in fact the expression of a reasoned position. For Lagrange, universal validity of a formula involving variable quantities does not amount to or even necessarily entail the validity of the formula for all particular values. Rather, the notion is syntactic: a formula is universally valid if it has been correctly derived according to the rules of analysis. Thus, for example, an equation like

$$\frac{1}{1-x} = 1 + x + x^2 + x^3 + \cdots \tag{7}$$

is said to be universally valid precisely because the series expansion may be obtained using the rules of analysis: for instance, by using long division. That the series gives bizarre results for some values is not an issue. As Lagrange wrote in a paper of 1761,

> Now I ask whether whenever, say, an infinite series like $1 + x + x^2 + x^3 + \cdots$ occurs in an algebraic formula, one will not have the right to substitute $\frac{1}{1-x}$ in its place, although this quantity is only truly equal to the sum of the given series if one assumes the last term x^∞ to be zero. It seems to me that one could not dispute the exactitude of such a substitution without overturning the most common principles of analysis.[14]

This, he underlines, is no eccentric opinion. Responding to a proposed counterexample of d'Alembert, he adds:

> ... with similar reasoning, one could also claim that $\frac{1}{1+x}$ is not the sum of the infinite series $1 - x + x^2 - x^3 + \cdots$ because by setting $x = 1$, one obtains $1 - 1 + 1 - 1 + \cdots$, which is either 0 or 1, depending on whether the number of terms one takes is even or odd, while the value of $\frac{1}{1+x}$ is $\frac{1}{2}$. Now I don't believe that any mathematician would want to accept that conclusion.[15]

In contrast with Euler, Lagrange did not hold that the generality of algebra entailed validity for individual values; indeed, he frequently warns against substituting particular values in general formulæ, since this can yield incorrect results.[16] "[T]he development of $f(x+i)$ is," as he says, "only generally true insofar as one does not give x specific values."[17] It is possible, it seems, to think of the series developments as "simple analytic transformations" of functions, as tools useful for "the generation of derived functions."[18] At this level of theory we are quite close to

[14]"Addition aux premières recherches sur la nature et la propogation du son," *Misc. Turin.*, II (1761); reprinted Lagrange, *Oeuvres*, I.319-332, 323.

[15]*Ibid.*

[16]E.g., *Leçons*, p. 25.

[17]*Leçons*, p. 43.

[18]*Leçons*, p. 85.

an approach which treats analysis as an uninterpreted formal system, one dealing with the analytic transformations of formulae according to certain rules.

This could not be the whole story, however. For Lagrange also wanted to apply his analysis, notably to geometry and mechanics. And at that level, questions of specific numerical values arise constantly and insistently. Thus we require theorems linking the general theory, which systematically ignored such questions, with the applications. Lagrange's attempts to come to grips with these problems are among the most fertile and influential parts of his work in analysis. At the same time, as I will discuss in a moment, they are among the most conceptually incoherent parts of his work. Before discussing them, however, let me return to Bolzano.

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Bolzano, for his part, had no prima facie objection to scientific presentations of the sort favored by Lagrange. For heuristic or pedagogical purposes, he acknowledged, this kind of presentation can be of great value. But, he added, if this kind of work is to be done properly, it must be preceded by another, radically different kind of development, a kind which he calls "strictly scientific."[19] Bolzano's conviction was that there is an intrinsic order of deductive dependence in sciences, and that, with suitable diligence, we can discover and set out this order. He wrote in 1810:

> This much . . . seems to me to be certain: in the realm of truths, i.e. in the collection of all true judgments, there is an objective connection, independent of our subjective recognition of it; and that, as a result, some of these judgments are the grounds for others, and the latter the consequences of the former. To set out this objective connection of judgments, i.e. to select a collection of judgments and to present them in such an order that each judgment which is a consequence is cited as such, and conversely, seems to me the true goal which we pursue in scientific expositions.[20]

The elimination of reference to the subject is characteristic of Bolzano's methodology: with it go the requirements of evidence and (Cartesian) simplicity for principles. For Bolzano, a principle, an axiom, is just a logically primitive proposition, an unprovable truth; a fundamental concept one which, having no parts, is indefinable. Proofs are not considerations aimed at convincing us something is true; rather they indicate the objective dependence of a given truth on certain other truths. There

[19] *Beyträge zu einer begründeteren Darstellung der Mathematik* (Prague, 1810), I, §19.
[20] *Beyträge*, II, §2.

is no reason to expect that logically primitive propositions should be evident, nor that the primitive concepts should be, as Pascal had put it, those things known by the natural light. Stripped of everything save their logical properties, Bolzano's concepts and propositions can contribute to the structure of science only in virtue of these. Mathematics (and science in general) itself thus becomes a mathematical object. To know the meaning of a concept or a proposition is to be able to describe, with mathematical precision, the structure of the science in which it is embedded. And this understanding is central to good method, as he wrote in 1816. For "[i]f one has not set out the meaning of a proposition with complete precision, then it is to be expected in advance that the proofs of it will also be more or less unsuccessful."[21]

In his 1810 essay on mathematical method, Bolzano had pointed out some ways in which things could go wrong, setting out in particular two criteria for the correctness of proofs. The first he expressed as follows:

> If the subject (or the hypothesis) of a proposition is as wide as it can possibly be so that the predicate (or the thesis) can still be applied to it, then in every correct proof of the proposition all of the characteristics of the subject must be used, i.e. must be adduced in the derivation of the predicate; and when this does not occur, the proof is incorrect.[22]

If, for example, one wants to prove that equilateral triangles possess a given property, a property which does not belong to triangles in general, then it is necessary to use the hypothesis of equal sides in the proof. If it is not used, then the proof should apply not only to equilateral but rather to all triangles. Such a proof is thus defective because it proves *too much*. Logically, this observation borders on the trivial. Bolzano himself describes it as simple and obvious. But despite this, he adds, this small point has been all too frequently ignored in practice.[23]

Consider, for instance, the theory of power series, the basis of Lagrange's foundation of the calculus. An important type of proposition here asserts the equality of two expressions, one finite, the other infinite, as for example:

$$\frac{1}{1-x} = 1 + x + x^2 + x^3 + \cdots \tag{8}$$

Lagrange's approach to such statements, as I have said, was to interpret them as reports of applications of the rules of analysis. The equation meant that the series development had been derived from the finite expression through correct use of the

[21] *Der binomische Lehrsatz usw.* (Prague, 1816), p. vii.
[22] *Beyträge*, II, §28.
[23] *Beyträge*, II, §28, Anm.

rules of analysis. As such, the universal validity of an equation of this sort by no means entails that the infinite series will converge for any given value of x or, if it does, that it will converge to the value of the finite expression for the given value of x. Considerations of this kind are simply beyond the scope of the proof.

This clearly leaves important questions unanswered. Infinite series were of interest to eighteenth-century analysts not simply as uninterpreted mathematics, but also, and even primarily, because they yielded precise numerical estimates under certain conditions. We need to know both what these conditions are and why the series yields precise estimates when are they are fulfilled. But Lagrange's method of attacking the problem does not even allow these questions to be asked.

For Bolzano, this was the result of a muddle, of an insufficiently clear grasp of the meaning of the proposition. In his paper on the binomial theorem, he pointed out that before setting out to prove a proposition it was a good idea to know what it meant. In the case of the binomial theorem, one has an equation asserting that a certain finite expression is equal to the sum of an infinite series:

$$(1+x)^n = 1 + nx + n \cdot \frac{(n-1)}{2}x^2 + \dots + n \cdot \frac{(n-1)(n-2)(n-3)\cdots(n-r+1)}{2 \cdot 3 \cdot 4 \cdot \cdots \cdot r} \cdot x^r + \cdots ad\, inf. \tag{9}$$

In order to understand this proposition, we need, as a bare inspection of the statement indicates, to know what the sum of an infinite series is, and what equality means in this context. Bolzano defines the first of these in the now standard way, employing the modern notion of convergence without using the word. Thus defined, it is clear that divergent series, as Cauchy would write a few years later, have no sum. When the series does have a sum for a given value of x, it still remains to be shown that the the sum is equal to the value of the finite expression for that value of x. This Bolzano does, in the case of the binomial theorem, for all real values of the exponent and for all values of x less than unity in absolute value.

Bolzano noted that from the time of Newton, who discovered the binomial expansion, everyone knew that, for n not a natural number, the series converged only for these values of x.[24] Only in such a case, he claimed, could one meaningfully speak of the equality of the two expressions. But no proof (and Bolzano had surveyed dozens of them[25]) had actually used this condition. The proofs could not,

[24] Actually, the series converges for $|x| = 1$ for some values of n. But Bolzano failed to notice this due to a subtle error in his proof strategy.

[25] Not, however, that offered by Gauss in the course of his investigation of the hypergeometric series, where the problem is dealt with satisfactorily. Gauss's paper was published in 1814, and apparently Bolzano did not then know of it.

therefore, be correct according to the stated criterion, and thus "...it is already decided in advance that there must be some error or other waiting to be discovered in each of the previous proofs."[26]

Things were the same, he underlined in his *Theory of Functions*, for the proofs given by Lagrange and others after him of Taylor's theorem:

> The incorrectness of most of these is clear simply from the circumstance that they—as the saying goes—prove too much: i.e. one permits oneself to make inferences which, were they allowed, would have as a result that this formula must be valid for cases in which it most decidely is not. The proof given by Lagrange (in the *Théorie des fonctions analytiques* and in the *Leçons sur le calcul des fonctions*) is considered to be one of the best, and many others ... still retain its essential points. The weakness of this proof consists ... in the assumption that $f(x+i) - fx$ may be represented in the form $Ai^{\alpha} + Bi^{\beta} + \cdots$ which, since $f(x+i) - fx$ can designate any arbitrary function, is at bottom nothing other than the assumption that every function of a variable x must be contained in the form $Ax^{\alpha} + Bx^{\beta} + \cdots$. This assumption in such a general form is, however, not only as yet unproven, but decidedly false. One knows full well, for example, that the function $\log x$ ... cannot be represented by any such series. This, too, is admitted: but it is said that the cases where such a development does not work only occur as exceptions, only for certain values of the variable. As long as one takes x *in its full generality*—as it is called—the assumption $f(x+i) - fx = Ai^{\alpha} + Bi^{\beta} + \cdots$ will hold. I won't reprimand here the rather improper manner of speaking according to which one takes x in its full generality when one takes it in such a way that it cannot represent any arbitrary number. Precisely this means not to take an expression in its full generality. One should much rather say that the equation $f(x+i) - fx = Ai^{\alpha} + Bi^{\beta} + \cdots$ holds not for every value of x, nor for every value of i, but certainly always for certain values, even (if you will) for infinitely many.[27]

One might, he continues, attempt to prove the hypothesis in question by using a very restricted concept of function. "Only we must not forget," he reminds us, "that the proposition which we demonstrate in this way under the name of Taylor's theorem is a truth of very limited scope and application, the putting forward of which could not satisfy us when we consider the current standpoint of [analysis]."[28]

[26] *Der binomische Lehrsatz*, p. vii.

[27] *Functionenlehre*, ed. K. Rychlik (Prague, 1930), II, §91.

[28] *Ibid.*

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Lagrange, as I have remarked, did not rely on his series to yield precise numerical values at the level of basic theory, as he was confident that he could secure them when needed for applications. Here, however, he ran into more difficulties, in particular into a mistake warned against by the second of Bolzano's criteria for correct demonstrations. This criterion is on the surface a mere repetition of tradition. Assimilated directly to Aristotle's ban on genus crossing in demonstration, Bolzano even calls it by the same name: a *metabasis eis allo genos*. Aristotle had used this phrase to describe the error of adducing a principle which is not proper to a given genus in order to prove that a necessary attribute belongs to the genus.[29] Bolzano, for his part, expressed it in extensional terms:

> Besides the characteristics of the subject several other intermediate concepts may also occur in a proof; however, if the proof is to contain nothing superfluous, these should only be such as, for an affirmative proposition, are no narrower than the subject and no wider than the predicate....[30]

The principle has for him quite a different sense, and markedly different application than the traditional. Most significantly, Bolzano uses it to confirm a Lagrangian doctrine, namely, that proofs in analysis should not make use of geometrical concepts or principles. These "foreign elements" are rejected, however, not on Cartesian but on semantic grounds. For like Lagrange Bolzano held that many geometrical theorems were simple applications of theorems of analysis, derivable from the latter by means of the proposition that space is a kind of three-dimensional continuous quantity. To appeal to a geometrical principle in the proof of a result of analysis is thus to make an inference from the special to the general. But this is not the worst: for such proofs can and often do turn into mere circles, the geometrical proposition being invoked to prove the more general theorem of analysis of which it is a special case. It is in this sense that Bolzano's ban on crossing genera in demonstration has the greatest weight and novelty: through semantic analysis, he showed the empty promise of the appeal to geometrical intuition in real function theory.

He was particularly eloquent on this subject in his best known work in analysis, the 1817 proof of the intermediate value theorem. Among the previous proofs he criticizes is that of Lagrange.[31] Lagrange had said that the result was customarily

[29] E.g. *An. post*, I, 6-7.

[30] *Beyträge*,II, §29.

[31] Given in the *Traité de la résolution des équations numériques de tous les degrés* (2^e ed. Paris: 1808).

proved by the theory of curved lines. But given his rejection of geometrical principles in analysis, we are not surprised to find him offering one and even two analytic proofs. The first, given in the text,[32] he himself admits as not fully convincing; so in an appendix he gives another, one designed to "avoid all difficulties."[33] It runs as follows. Suppose $f(x)$ is a polynomial with $f(a) < 0 < f(b)$. We may suppose $0 < a < b$ without loss of generality. We now take the sum of the terms of f with positive sign and call them $P(x)$, the sum of the terms preceded by a negative sign $Q(x)$, so that $f(x) = P(x) - Q(x)$. Then for $x = a$, we have $P(x) < Q(x)$, while for $x = b$, $P(x) > Q(x)$. He continues:

> Now from the form of the quantities P and Q, which contain only positive terms and positive, integral exponents, it is obvious that these quantities will necessarily increase as x does, and that, in making x increase through all the insensible degrees, they will also increase by insensible degrees, but in such a way that P will increase more than Q, since from being smaller it becomes the greater of the two quantities. Therefore there will necessarily be a term between the two values a and b where P will equal Q, just as two bodies which one assumes to be moving in the same direction along the same trajectory and which, leaving at the same time from two different points, arrive at the same time at two other points, but in such a way that the one which was behind at first is afterwards ahead of the other, must necessarily meet on their path.[34]

Bolzano's criticism of this proof is striking in its clarity.[35] First, he observes, taking up a Lagrangian point, the concepts of motion and time are certainly foreign to analysis, and thus can have no essential role to play in a correct proof. The most charitable interpretation we can give, then, is to treat their use here as metaphorical. But at this point, the emptiness of the proof becomes obvious; once again, the *metabasis* turns out to mask an underlying circularity:

> The deceptiveness of the whole proof rests mainly on the inclusion of the concept of *time*. For if this were omitted, it would be seen immediately that the proof was nothing but a repetition, in different words, of the proposition to be proved. For to say that a function Px, before it passes from the state of being smaller than Qx to that of being greater, must first go through the state of being equal to Qx is to say,

[32] Ch. 1.
[33] *Op. cit.*, p. 133.
[34] *Op. cit.*., note 1.
[35] *Rein analytischer Beweis*, Preface, II.

> without the concept of time, that among the values that Px takes, if one puts in every value of x between a and b, there is one that makes $Px = Qx$. This is exactly the proposition to be proved.[36]

In his lovely essay on the *esprit géometrique*, Pascal had warned us not to "undertake to prove any of those things which are so evident of themselves that there is nothing which is more clear to prove them with."[37] Otherwise, one risks falling into "inexplicable confusions," as did a certain unnamed acquaintance of Pascal who, he tells us, went to the absurdity of explaining a word by the word itself, defining light as "a luminary movement of luminous bodies."[38] Among those propositions too obvious to prove he had singled out a geometrical version of the intermediate value theorem;[39] Lagrange seems to have fulfilled his prediction of futility.

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Bolzano pointed out the same flaw in a proof from the *Théorie des fonctions analytiques*, one which was of considerable importance for Lagrange, as it was supposed to provide a link between the general theory (where numerical values were not an issue) and applications (where they were). The result which Lagrange seeks to prove—promising both the convergence of the series on an interval and an error-estimate—is the following:

> In the series
> $$f(x) + pi + qi^2 + ri^3 + \cdots$$
> which comes from the development of $f(x + i)$, one can always take i small enough so that any given term is greater than the sum of all the terms that follow it, and so that this will also hold for all smaller values of i.[40]

His proof, however, disappoints even on his own terms, making an obvious appeal to geometry. Here are its basic points: Let us take, for example, the first term of the series, $f(x)$. According to the statement of the theorem, we should be

[36] *Rein analytischer Beweis*, Preface, II (tr. S. B. Russ). I have changed the symbols used by Bolzano to make his text uniform with that of Lagrange.

[37] "De l'esprit gómétrique et de l'art de persuader," *Œuvres complètes de Pascal*, ed. J. Chevalier (Paris: éditions de la Pléiade, 1954), p. 597.

[38] *Op. cit.*, p. 579-80.

[39] *Op. cit.*, p. 603.

[40] *Théorie des fonctions analytiques*, N^o.6.

able to find a value of i sufficiently small so that $f(x)$ will be smaller than the sum of all the following terms, i.e. $ip + i^2q + i^3r + \cdots$. For the sake of abbreviation, he writes this sum as iP. Lagrange now asks us to consider the curve with i as abscissa and iP the ordinate. Now, he says

> ...since the remainders $iP, iQ, iR\ldots$ are functions of i which become zero, by the very nature of the development, when $i = 0$, it follows that ...this curve will cut the axis at the origin of the abscissae and ...the course of the curve will necessarily be continuous from this point on; therefore it will approach the axis little by little before cutting it, and will approach it, consequently, by a quantity less than any given quantity, so that one can always find an abscissa i corresponding to an ordinate less than a given quantity, while to each smaller value of i will also correspond ordinates less than the given quantity.[41]

One can in particular, he claims, take i so that iP will be less than fx, which was to be shown.

Bolzano cited this proof as an example of one which contained a *metabasis* which "is obvious at first glance".[42] And here, too, the appeal to geometry covers a vicious circle, "because the proof that every equation of the form $y = fx$ yields a continuous curved line is only possible if one already presupposes precisely the purely arithmetic claim which one is seeking to prove."[43]

To see this more clearly, consider a little more closely just what this proof says. Lagrange claims that the remainders $iP, iQ, \ldots$ must become zero for $i = 0$. Evidently, he supposed that the remainder iP could be expressed either by $f(x+i) - fx$ or by the infinite series $ip + i^2q + i^3r + \cdots$. But he had by no means shown that this series is convergent. And how could he, at this level of generality? The best hope which we have left would be to define iP as $f(x+i) - fx$. At this point one appeals to the continuity of the curve $y(i) = f(x+i) - fx$ at $i = 0$ in order to show... what, exactly? That "one can always find an abscissa i corresponding to an ordinate less than a given quantity, while to each smaller value of i will also correspond ordinates less than the given quantity." But this is just a purely analytic statement which says that the function $y(i)$ is continuous at $i = 0$ with $y(0) = 0$: a perfect circle.

The failure of this proof should not surprise us: having put no reference to numerical values in his general theory, Lagrange was in no position to secure them

[41] *Ibid.*.
[42] *Beyträge*, II, §29, Anm.
[43] *Ibid.*.

here or elsewhere. His proposition is thus more an expression of faith than something which one might hope to prove. The necessary tools are simply not to hand, something also apparent in the case of the intermediate value theorem. Bolzano's reworking of these results thus had to begin with more basic concepts. His own presentation of Taylor's theorem, for instance, comes very close to the end of his *Theory of Functions*, after he had set out a theory of real numbers, given much more general definitions of function, continuity, and derivatives of all orders, and proved the necessary lemmas. Only then could he proceed to a proof that, under certain specified conditions, a function can be represented (in a precise, numerical sense) on a domain by its Taylor series.[44] The starting point of Lagrange's proposed foundation thus occurs near the end of Bolzano's *Theory of functions*: just one indication among many of the radical transformation Lagrange's analysis underwent in his hands.

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Leibniz wrote in the *Nouveaux essais*: "Having . . . considered both the old and the new, I have found that most accepted doctrines can bear a sound sense."[45] This was very much Bolzano's attitude towards Lagrange's analysis. Lagrange had, in his characteristic way, unified large areas of analysis, providing many valuable insights and results along the way. With good reason, his works had great influence on Bolzano's contemporaries. The faults in method, however glaring, were never reason for Bolzano to abandon Lagrange entirely. Rather, he consistently sought to provide a sound sense for Lagrange's doctrines. As he wrote in the *Paradoxes of the Infinite*:

> Although we may, as I believe, rightly accuse the manner in which the theory of the infinite has been presented until now of having many grave faults, it is nevertheless known that for the most part one obtains completely correct results if one uses the rules which are generally introduced into the calculation with the infinite with appropriate care. Such results could never have presented themselves if there were not a truly irreproachable manner of conceiving and handling this method

[44] I won't discuss Bolzano's reconstruction in any detail here, in part because of limitations of space, in greater part, however, because of its familiarity. For Bolzano's approach to real function theory is now quite widely shared, in general terms as well as in some important details—so much so that one could still quite easily use Bolzano's *Theory of Functions*, despite its (often instructive) errors, as an introductory text.

[45] Leibniz, *New Essays*, I.ii.§§21-23, tr. Remnant and Bennett.

> of calculation. And I will gladly believe that it was at bottom just this which the acute inventors of this method had in mind, even if they were not in a position to set out their thoughts with complete distinctness, something which, in difficult cases, generally succeeds only after repeated attempts.[46]

What may seem like unwise or even affected charity here is in fact a reflection of the seriousness of Bolzano's enterprise. Accepted doctrines must be respected, both because they often contain a good deal of truth and because they endure in the present. Giving them a solid meaning and foundation insofar as possible is important not just for the sake of recovering the elements of truth, but also for raising the level of philosophical culture.

Bolzano's attempts to remake Lagrangian analysis in his *Theory of Functions* have been of relatively minor interest to historians of mathematics. Partly this is because, remaining unpublished until 1930, the work most probably had no influence on the development of analysis. In part, too, because others like Cauchy, Abel and Dirichlet had, for their own reasons, taken up many of the same foundational problems. But these features of his work are purely accidental, due mainly to the stupidities of the Austrian administration and by the delays which followed from Bolzano's decision to write a major treatise of logic in the 1820s. Setting these considerations aside, we can see in Bolzano's reconstruction of Lagrangian analysis what is perhaps the first conscious, systematic and sustained application of what would later come to be called analytic philosophy. It remains to this day one of the most successful.

[46]Bolzano, *Paradoxien des Unendlichen* ed. A. Höfler (Hamburg, 1921), §37.