Bolzano's *Beyträge* at 200: a quiet revolution in logic and philosophy of mathematics

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Une bonne logique ferait dans les esprits une révolution bien lente, et le tems pourrait seul en faire connaître un jour l'utilité.
Condillac

1 Introduction

Two hundred years ago, a little-known Bohemian priest published a manifesto calling for a revolution in mathematics and logic, promising himself to undertake a complete reconstruction of mathematics from the ground up, in accordance with the principles of his new logic, itself a work in progress. Bernard Bolzano (for this was the priest's name) had no gift for catchy titles. He called his book *Contributions to a Better-founded Presentation of Mathematics*.[1] Then as now, crackpots were common, so perhaps we should not be surprised that Bolzano's work seems to have made little impression on his contemporaries. The difference in this case is that Bolzano—unlike Mesmer, Gall, and so many others—actually succeeded. The transformed mathematics he envisioned and helped to bring about is none other than the classical mathematics of late nineteenth and early twentieth- centuries, many central features of which persist to this day.

The *Contributions* were intended to be but the first installment of a series of publications setting out Bolzano's views on the foundations of the various branches of mathematics, both pure and applied. Lack of interest in the first installment,

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[1] *Beyträge zu einer begründeteren Darstellung der Mathematik* (Prague, 1810), hereafter **BD**; English tr. in S. B. Russ, ed. and tr., *The Mathematical Works of Bernard Bolzano* (Oxford University Press, 2004), hereafter *MW*.

however, led Bolzano to abandon this project, and instead to publish a series of papers he thought more likely to catch the public's attention. These papers are to the first installment what Descartes's *Geometry, Optics,* and *Meteorology* are to the *Discourse*, namely, samples of the promised fruits of the method. Among them are a pair of papers on the foundations of real analysis which achieved decisive results, as well as a more speculative paper which, among other things, contains suggestive fragments of point-set topology.[2] Though these papers did not become as famous as Descartes's *Geometry*, the *Purely analytic proof* was perhaps just as influential, finding avid readers, notably in the circle of mathematicians around Weierstraß, and, as I have argued elsewhere, indirectly influencing through them the development of analytic philosophy.[3]

In this paper, I would like to present some of the central elements of the *Contributions*, with a particular focus on Bolzano's understanding of the *subjective* side of foundational research. I will then discuss his further elaboration of these views in Volume 3 of the *Theory of Science*. I will conclude with a discussion of some of Bolzano's mathematical work from this period, namely, his paper on the binomial theorem, in the light of his views on mathematical method.

2 The objective order

I will begin with a quick survey of what I find to be some of the most striking views put forward in the *Contributions* concerning the objective structure of mathematics.

The second part of the *Contributions* begins with the following bold declaration:

> [T]his much seems to me to be certain: in the realm of truth, i.e., in the collection of all true judgements, a certain *objective connection* prevails which is independent of our accidental and *subjective recognition* of it. As a consequence of this some of these judgments are the grounds of others, and the latter the consequences of the former.[4]

[2] *Der binomische Lehrsatz und als Folgerung aus ihm der polynomische, und die Reihen, die zur Berechnung der Logarithmen und Exponentialgrössen dienen, genauer als bisher erwiesen* (Prague, 1816), hereafter **BL**; *Rein analytischer Beweis des Lehrsatzes, daß zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liegt* (Prague, 1817), hereafter **RB**. *Die drey Probleme der Rectification, der Complanation und der Cubirung usw.* (Leipzig, 1817); English translations in **MW**.

[3] P. Rusnock, "Bolzano and the traditions of analysis," in *Bolzano and Analytic Philosophy* ed. W. Künne, M. Siebel and M. Textor (Amsterdam: Rodopi, 1998); also in *Grazer philosophische Studien* **53** (1997) 61-85.

[4] *Beyträge*, II, §2; **MW**, p. 103.

If we follow Bolzano here, we will have to recognise that, independently of human minds and their capacities, activities etc., some truths, called *consequences*, are dependent upon others, their *grounds*. If, in addition, we suppose with Bolzano that some truths are basic, having consequences but no grounds, there will also be objective notions of *axioms* or *principles* [*Grundsätze*] on the one hand and *theorems* on the other. Truths will have this status in and of themselves, and not relative to how anyone came to know them or may have presented them in some treatise or other.[5] Similar things may also be said about the *definitional* order of mathematics: objectively, regardless of whether anyone is aware of it, some concepts will be composed of others, and some concepts will be indefinable, not from the subjective, human point of view but in and of themselves.

Now Bolzano thought it imperative to develop an account of the mathematical method (or *logic*) which did justice to this objective point of view, and spent many years developing one, culminating the the *Theory of Science* as well as the shorter work "On the Mathematical Method" from the 1830s.

In the first half of the *Contributions*, Bolzano develops and defends a definition of mathematics as the general science of forms, the forms to which "things must conform in their existence."[6] This proposal, vague as it stands, is given more substance in the second, unpublished installment of the *Contributions* on so-called *universal mathematics*.[7] There it becomes clear that mathematics is primarily concerned with what Bolzano calls *systems*, that is (in modern terms), sets of elements possessing various properties and relations. (Later, Bolzano would also call these *Inbegriffe*, or *collections*.)

The next key insight, from which so much else follows, is that mathematical theories (and indeed sciences in general) are themselves systems—at this point, Bolzano thinks of them as structured collections of true *judgments*, not yet having hit upon the concept of a proposition in itself—and thus can be studied in the same way as other mathematical structures, using the same tools and methods. From what was said above, moreover, it is clear that no subjective concepts will appear in the objective theory of mathematical theories. Such concepts would be no more appropriate here than they would be, e.g., in a treatise of geometry or number theory. We don't say, for example, that a circle is a kind of geometric object

[5]Quine ("Two Dogmas of Empiricism" in *From a Logical Point of View*, 2nd ed., p. 35) might seem at first glance to think this approach absurd: "[Given] simply a notation, mathematical or otherwise, and indeed as thoroughly understood a notation as you please in point of the translations or truth conditions of its statements, who can say which of its true statements rank as postulates? Obviously, the question is meaningless—as meaningless as asking which points in Ohio are starting points." Note, however, that Quine's remarks are aimed at a collection of sentences with no deductive structure, but only a division into true and false.

[6]**BD**, I, §8.

[7]*Allgemeine Mathesis*. In **BBGA**, II.A.5.

with such and such relations to human cognitive faculties, e.g., a figure which can be *seen to have* such and such properties in appropriate conditions; similarly, the central concepts of the objective part of logic should be definable without appeal to anything subjective. What we have, in other words, is a conception of logic as the study of formal systems,[8] an idea almost a century too early if we follow the usual historiography.

Bolzano admits that he is not entirely certain about all the details of the objective order,[9] but he does propose objective criteria a proof must satisfy in order to be successful. Two of these are set out in part II. The first runs as follows:

> If the subject (or the hypothesis) of a proposition is as wide as it can be so that the predicate (or the thesis) can be applied to it, then in any correct proof of this proposition all characteristics of the subject must be used, i.e., must be applied in the derivation of the predicate, and if this does not occur, the proof is incorrect.[10]

If, for example, we intend to justify an equation that holds for all numbers between certain limits, but only for those numbers, we will have to appeal to this restriction in the course of our proof. Otherwise, the proof should establish that the equation holds for all values. Since it can't (the more general claim being false), the proof must be faulty: *it proves too much, and hence proves nothing.* As simple as it may seem, this criterion was enough to show the incorrectness of the almost all previous proofs in the theory of power series, a subject I shall return to later.

The second is an adaptation of Aristotle's ban on crossing from one genus to another (*μεταβασις ἐις ἀλλο γενος*) in the course of a demonstration.[11] Bolzano, for his part, expressed it in extensional terms:

> As well as the characteristics of the subject several other intermediate concepts can also appear in a proof. However, if the proof is to contain nothing superfluous, then for an affirmative proposition there should only appear intermediate concepts which are not narrower than the subject and not wider than the predicate.[12]

Perhaps the most noted application of this principle is Bolzano's claim that proofs

[8]More precisely, of *formalized* systems in Tarski's sense of the term, since the primitive concepts have fixed extensions.

[9]**BD**, II, §2; **MW**, p. 103.

[10]**BD** , II, §28; **MW**, p. 122-123. Cf. **WL**, §375.

[11]*Posterior Analytics*, I, 7.

[12]**BD**, II, §29; **MW**, p. 123.

in analysis should make no use of geometrical concepts or principles.[13] These "foreign elements" are rejected not based upon some vague desire for purity but rather on straightforward logical grounds. For 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 particular kind of three-dimensional continuous quantity. Hence, in proving a theorem of analysis, appeals to geometry are of no value (objectively speaking), since the theorem concerns continuous quantities of all kinds (at that time, most would have thought these to include quantities such as time, force, mass, charge, etc.), not just the spatial ones. An appeal to the properties of spatial quantities, as convincing as it might be subjectively speaking, will only be relevant to the general proposition if they are also properties that belong to all kinds of continuous quantities. But if a property belongs to continuous quantities in general, then this will be the objective reason why it also belongs to, say, spatial quantities. Hence an appeal to a property of spatial quantities would at best produce a circular proof: we appeal to a property of spatial quantities in order to establish that all continuous quantities have the property, but the latter truth is the ground of the former. This would be like trying to prove that the Pythagorean theorem holds of all right triangles by pointing out that it holds for some special kind of them like isosceles right triangles.

Once we have understood Bolzano's principle and its application, we understand immediately why he thought it necessary to formulate a purely analytic definition of continuity, and to prove a variety of "obvious" theorems about continuous functions. A contemporary of Bolzano, if asked to characterize continuity more precisely, might have thought it most natural to fall back on descriptions drawn from geometry or kinematics. A function is continuous, one could say, if its graph has no gaps or jumps, if this graph could be traced without lifting one's pencil, or if the function could be used to represent the trajectory of a moving particle. If meanings are conceived as images, continuity might be thought to be something we grasp directly, something we can simply see.[14] We can anticipate what Bolzano's objection to such characterizations would be, at least if they were offered as proper definitions. In appealing to the graph of a function to define continuity, for instance, it is assumed that we know what it means for a geometrical object to be, er, um, *continuous*. But this property is either the same for all kinds of continuous

[13]Bolzano discusses a particularly interesting case in the *Contributions*, II, 29, Note. I have discussed this in the paper "Remaking mathematics: Bolzano reads Lagrange," *Acta Analytica* **18** (1997) 51-72, pp. 68-70.

[14]Modern readers can easily formulate a further objection to such characterizations. Anyone who has seen a movie can attest that it is all but impossible to tell the difference between simply looking at a moving object and looking at a sequence of still images of the moving object projected at the usual rate of 24 frames per second. That the motion is continuous is equally obvious in the two cases.

quantities or it isn't. If it isn't, the appeal to geometry is irrelevant, and if it is, as we saw before, it is circular. But in any case, what we imagine is irrelevant—for this can at best acquaint us with particular instances of geometrical continuity; it can never serve to define it.

3 Objective and subjective in the *Contributions*

Bolzano has been praised along with Frege for eliminating the psychological elements of logic. While this is justified in a way, it is also potentially misleading. For while Bolzano meticulously eliminated psychological elements from his objective account of mathematical theories (the part of his logic that would eventually grow to constitute the Theory of Elements in the *Theory of Science*), he by no means neglected the subjective side of science (which is dealt with at considerable length in volumes 3 and 4 of the *Theory of Science*). Indeed, Bolzano's account of the subjective side of mathematical, particularly foundational, research is among the richest and most nuanced to be found in the literature. In my view, it was precisely his previous elaboration of the objective account which permitted him to obtain such a clear view of the subjective side.

The prevailing views among philosophers of Bolzano's time, notably Kant and his followers, differed slightly if at all from those put forward a century and a half earlier by Descartes and Pascal.[15] If anything, some of the finesse of these authors may have been lost through transmission. Proof was viewed in subjective terms: a proof is a series of considerations through which the truth of a judgment becomes evident. Obviously, if a proof is to succeed, its premises must themselves be evident. This may occur because the premises have themselves been proved, but clearly this cannot proceed *ad infinitum*. There must thus be some judgments which are evident in and of themselves, to serve as the starting point of proofs, and these are the axioms, postulates, or common notions. Definition, too, was conceived in subjective terms, its purpose being to bring us to a clear understanding of the meaning of a term. Obviously, again, this will only work if the terms used in the definition are themselves clearly understood. Now this may occur because those terms have themselves been defined but, if we are to avoid an infinite regress or a circle, there must be some terms which are clearly understood in and of themselves, and these are the primitives or indefinables.

Mathematics, at least in its ideal form, would be structured as follows. We would begin with a set of terms clearly understood in and of themselves, defining

[15] See, e.g., R. Descartes, *Principles*, I, §13; *Discourse*, 2 *Rules*, 3; Pascal, *De l'esprit géométrique*. Kant, *Inquiry concerning the distinctness of the principles of natural theology and morality*, 1; *Critique of Pure Reason*, A A712/B741 ff.

all others in terms of these. We would then set out a number of truths which are evident in and of themselves, and prove all others from this initial stock of premises.

Foundational research, on this conception, would have the following properties: its aim is to produce or reproduce an order which is essentially *subjective*—the ideal order of knowledge described above; it is *individualistic*, because a proof is an activity which unfolds within a single mind; it is *global*, linking any truth which is proved, through a series of evident inferences, to axioms, truths which are evident in and of themselves; and, finally, it is *infallible*, since a clear and distinct apprehension of the primitive basis as well as of the steps of a proof constitutes an absolute guarantee of truth. It would always begin with what is known best, moving on step by step to what was previously unknown, but now becomes evident.

Once Bolzano had come to look upon mathematical theories as autonomous formal systems, and decided that the goal of foundational research should be to discover and display the intrinsic, objective structure of these systems, he abandoned all of the features of the received account of mathematical method I have just enumerated. Since its goal is to discover the objective features of formal systems, it cares nothing for the subjective order of knowledge. Nor is there anything essentially individualistic about it: since, considered objectively, a proof is not a mental process, it is entirely conceivable that the combined efforts of several researchers may be involved in producing or even understanding one. (Objectively speaking, there seems to be no reason why there cannot be proofs that cannot be produced or understood by any collection of humans.) Foundational research need not be global either: even if the ultimate goal is to characterize a formal system in terms of its primitive elements, it may still be useful or indeed indispensable to attempt to accomplish this through *local* advances. Bolzano recognizes precisely this point in the note to II, §11 of the *Contributions*, where he says we may provisionally assume certain propositions in order to prove a given theorem, provided that we are confident that these assumptions may themselves be proved at some future time. Bolzano's proof of the intermediate value theorem has precisely this character: it shows how one result of real function theory may be proved on the basis of certain others, without tracing the latter back to primitives (in particular, no attempt is made in the 1817 paper to provide a theory of real numbers, and to use it to prove the assumptions made about real numbers by Bolzano in the course of his demonstration[16]). Infallibility, finally, is simply laughed out of court, as seems entirely appropriate to anyone familiar with the practice or the history of mathematics.

[16]Bolzano would later tackle this problem in his theory of measurable numbers. See **BBGA**. II.A.8; **MW**, pp. 355 ff.

With perhaps a few exceptions, mathematical theories are not created *ab ovo* within a single mind. Rather, we generally encounter them as going concerns, usually with more than a few problems and generally with incomplete foundations. Foundational research, accordingly, cannot start at the beginning, with perfectly clear primitives and utterly evident axioms. Instead, we may have to work hard to arrive at primitive concepts and propositions capable of supporting a science and, even if we do, there is no guarantee that they will bear the stamp of evidence. As Bolzano noted, we may become convinced of the truth of such a basic proposition not because it is self-evident (it may even, as Bolzano remarks, appear quite dubious[17]), but rather because we understand that we can use it to deduce theorems belonging to our science, but not to deduce any non-theorems.[18] And we may become clear on the meaning of an indefinable term not because this meaning is intrinsically luminous, but rather by observing how the term is used in a variety of sentences which are held to be true.[19] Usually, we start in the middle, sometimes moving outward (seeking new theorems), sometimes inward (attempting to ground already accepted theorems). This should not be thought to be a strict division, however, for, as Bolzano notes, often the search for grounds puts us in a position to discover new theorems.[20]

The contrast between the two approaches can be illustrated from many parts of Bolzano's mathematical work. Consider, by way of example, Bolzano's most famous work, the proof of the intermediate value theorem. He states the theorem as follows:

> Theorem: If two real-valued functions fx and ϕx that are continuous between the values $x = a$ and $x = b$, are such that $fx < \phi x$ for $x = a$ while $fx > \phi x$ for $x = b$, then $fx = \phi x$ for some value of x lying between a and b.

Now consider the following way of (subjectively) proving the proposition, which had been suggested by Lagrange: imagine that two objects are moving along the same path, and one of them is behind the other at time a but ahead at time b. Let fx designate the position of the first object and ϕx the position of the second object at time x. It is clear that the first object must gradually catch up with the second, and finally pass it. Just before doing so, the two will be side by side, and at that moment, $fx = \phi x$.

Bolzano concedes that such considerations work quite effectively to convince us that the proposition is true, but denies that they constitute demonstrations in the

[17]**BD**, II, §21, note; **MW**, p. 119.
[18]*Ibid.*
[19]**BD**, II, §8; **MW**, p. 107-108.
[20]**BD**, II, §2; **MW**, p. 104; cf. **EG**, Preface; **MW**, p. 31.

objective sense of the word. For the theory of quantities is a more general science than kinematics: the theorem is stated for all real-valued functions, not just those associated with positions of moving objects at various times. Now if the proof truly depended on concepts drawn from kinematics, it would be deficient, since it would not establish the validity of the proposition outside of the boundaries of that science. But if the proof is to have wider scope, if it is to apply beyond kinematics, then there must be a core in it that does have the required generality. The use of the concepts of time, space and position in this case would then be superfluous—they could be stripped away to reveal a generally valid proof. The result of doing this, however, is disappointing in the extreme, for once the unnecessary concepts are removed, we are simply left with a restatement of the theorem to be proved:

> The deceptive nature of the whole proof really rests on the fact that the concept of *time* has been involved in it. For if this were omitted it would soon be seen that the proof was nothing but a re-statement in different words of the proposition to be proved. For to say that a function fx, before it passes from the state of being smaller than ϕx to that of being greater, must first go through the state of being equal to ϕx is to say, without the concept of time, that among the values that fx takes, if x is given every arbitrary value between α and β, there is one that makes $fx = \phi x$, which is exactly the proposition to be proved.[21]

As an objective proof, therefore, Lagrange's "demonstration" is worthless, despite its power to convince. In saying this, Bolzano does not mean to say that such subjective proofs have no value at all, since they clearly play an important role in singling out the propositions for which we should seek objective grounds. The point is merely one of distinguishing the very different functions of the two kinds of proof.

Let me take the opportunity provided by the above example to underline another interesting feature of Bolzano's account of proof. The proposition that Lagrange's (subjective) proof shows to be true, a proposition of the theory of motion, is, objectively speaking, a consequence of the more general proposition concerning all real-valued functions proved in Bolzano's paper. Nevertheless, the objective consequence is in this case more obvious than the more general proposition which is its ground. It, along with similar propositions from geometry and other sciences, may well form the basis of our conviction that the general proposition is true, and thus requires (objective) proof. Thus Bolzano would have no objection to saying that in such cases the consequences prove their grounds (subjectively), which in

[21] **RB**, Preface, II; **MW**, p. 257.

turn are used to prove the consequences (objectively). Once we have distinguished two different senses of proof, such a statement does not endorse circular arguments.[22] It bears repeating that the order of objective dependence need not line up with the order of conviction, and that in particular, the axioms, or unproven starting point of demonstration, need not be obviously true.

Interestingly, Bolzano's rejection of the Cartesian picture has a early eighteenth-century precedent. Recall that Descartes had proposed his method for use not only for mathematics, but for science in general. His hopes for the universal application of his method had fared particularly poorly in physics, where Newton's *Principia* had relegated Descartes' apriorism to the status of an historical curiosity. There was resistance, to be sure. Cartesians complained that Newton's physics had no adequate foundation, because the primitive notion of gravitation, far from being self-evident, was perhaps even unintelligible. In the preface to the second edition of the *Principia*, Roger Cotes answered these critics as follows:

> But shall gravity be therefore called an occult cause, and thrown out of philosophy, because the cause of gravity is occult and not yet discovered? Those who affirm this, should be careful not to fall into an absurdity that may overturn the foundations of all philosophy. For causes usually proceed in a continued chain from those that are more compounded to those that are more simple; when we are arrived at the most simple cause we can go no farther. Therefore no mechanical account or explanation of the most simple cause is to be expected or given; for if it could be given, the cause would not be the most simple. These most simple causes will you then call occult, and reject them? Then you must reject those that immediately depend upon them, and those which depend upon these last, till philosophy is quite cleared and disencumbered of all causes.
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> Some there are who say that gravity is preternatural, and call it a perpetual miracle. Therefore they would have it rejected, because preternatural causes have no place in physics. It is hardly worth while to spend time in answering this ridiculous objection which overturns all philosophy. For either they will deny gravity to be in bodies, which cannot be said, or else, they will therefore call it preternatural because it is not produced by the other properties of bodies, and therefore not by mechanical causes. But certainly there are primary properties of bodies; and these, because they are primary, have no dependence on the others. Let them consider whether all these are not in like manner

[22]Cf. **WL**, §626.

> preternatural, and in like manner to be rejected; and then what kind of philosophy we are like to have.[23]

"Just so," one can imagine Bolzano saying, "and mathematics is no different."

4 Objective and subjective in the *Theory of Science*

Bolzano's new logic promised not only advances in mathematics but also in *logic*. This would surely have sounded not only paradoxical but ridiculous to his German contemporaries, as ridiculous as his admission that he is not entirely clear about the nature of scientific exposition, one of the central parts of logic.[24] For had not the great Kant already established that logic was a completed science? [25]

Bolzano thought such talk most unfortunate:

> It seems to me ... that one of KANT's literary sins was that he attempted to deprive us of a wholesome faith in the perfectibility of logic through an assertion very welcome to human indolence, namely, that *logic is a science which has been complete and closed since the time of Aristotle*. It seems to me that it would be much better to assert as a kind of practical postulate that faith in the perpetual perfectibility not only of *logic* but of *all* science should be maintained. And what is it at bottom other than pride which would lead us to claim that in all future time a certain science will not be presented in a better and more perfect way than it appears at present (namely, through our efforts)?[26]

And, indeed, we find that Bolzano did follow up on the logic of the *Contributions*. Almost every point raised in that work appears anew in the *Theory of Science*, usually having undergone substantial revision. I would like to discuss one example here, centering on Bolzano's claims about *a priori* judgment.

In the appendix to the *Contributions*, Bolzano proposes the following definition of *empirical judgment*:

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[23] Isaac Newton, *Mathematical Principles of Natural Philosophy and his System of the World* tr. A. Motte, revised by F. Cajori (Berkeley and Los Angeles: University of California Press, 1966), Vol. I., p. xxvii.

[24] **BD**, II, §2; **MW**, p. 103.

[25] In one of his lectures, for instance, Kant had said: "We have no one who has exceeded Aristotle or enlarged his logic (which is in itself fundamentally impossible) just as no mathematician has exceeded Euclid.Dohna-Wundlacken Logic, 24.701; *Lectures on Logic* ed. and tr. M. Young (Cambridge: Cambridge University Press, 1992), p. 438. Cf. *Critique of Pure Reason*, Axiv, B viii.

[26] **WL**, §9 [I.40].

> In my view, the distinction between the empirical and *a priori* in our cognition extends originally only to our *judgements*, and it is only through these that it can also be indirectly extended to our concepts or ideas. That is, I am conscious of making judgements of the form, '*I perceive X*'; I call these judgements *empirical judgements* . . . and the *X* in them I call an *intuition*.[27]

He continues:

> . . . the rest of my judgments . . . I call *a priori*.[28]

From the point of view of his later self, he has already made some serious mistakes. But he compounds them immeasurably when he claims, further, that:

> All *a priori* judgments are absolutely certain. . . .[29]

For now it seems that making mistakes in logic and mathematics is ruled out, and this is surely the last thing Bolzano would have wanted to say.

By the time we reach the *Theory of Science*, this tangle has been pretty well cleared up. In the first place, Bolzano now thinks that we must distinguish individual, subjective *acts* of judgment from the objective *content* of these acts. In the *Contributions* as well as many other works of the time, the two were indifferently designated by the term 'judgment', whose use Bolzano now sees to be ambiguous. He will now speak of *propositions in themselves* when he wants to talk about the objective content and reserve the term 'judgment' for the acts of thought in which propositions are taken to be true.[30] Once this distinction has been introduced, it becomes clear that one commits a category mistake in speaking of *a priori* judgments, if 'judgment' is taken in the objective sense (i.e., equivalent to 'proposition in itself'). For while a judgment or cognition can be *a priori*, a proposition in itself cannot be (at best we might speak of the kind of propositions in themselves that *can* be known *a priori*). What he had originally thought to be a single distinction between empirical and *a priori* judgments, Bolzano now sees as two related, but separate distinctions: on the one hand, between judgments arrived at with or without appeal to experience, and on the other, between propositions in themselves that do or do not contain intuitions (the latter being called *purely conceptual* propositions).[31]

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[27]**BD**, Appendix, §4; **MW**, p. 133.

[28]*Ibid.*

[29]**BD**, Appendix, §7, note; **MW**, p. 134.

[30]**WL**, §§19, 34.

[31]**WL**, §133, note. The concept of an intuition has also been given a definition in objective terms, namely, as a simple idea with precisely one object (**WL**, §72).

In light of these clarifications, the definition of empirical judgment in the *Contributions* now appears to be too narrow, since not only judgments of the form "I perceive X" (where 'X' designates an intuition) contain intuitions. (Judgments of this form, along with certain others, are now given the special name of *immediate* judgments of perception.[32]) Now any judgment which contains an intuition (but not only these, as we shall see) is to be called empirical.[33]

The notion of *a priori* judgment, on the other hand, is further analysed in §306 of the *Theory of Science*. Now it is not just the content of the judgment in question that is looked at, but the content of the entire collection of judgments which mediate that one (its *mediation* pedigree, if you will). Only if a given judgment, along with all those that mediated it, are purely conceptual, should it be called *a priori*:

> If the propositions from which a judgment M is deduced, as well as those from which the former follow, down to the immediate judgments, are all purely conceptual propositions, then judgment M can be called a *judgment from pure concepts*, or *pure*, or *a priori*. In all other cases it could be said to be *drawn from experience* or *a posteriori*.[34]

Thus if even a single intuitional judgment makes it into the mediation pedigree, the resulting judgment is to be declared *empirical*.[35] Thus there is no absurdity of speaking of a purely conceptual judgment, e.g., a mathematical judgment, which is empirical. Indeed, as we shall see, Bolzano seems to have thought that most of our mathematical judgments should, strictly speaking, be classed as empirical.

This concerns actual judgments. But one can also ask about the distinction between judgments which *can* be known *a priori* and those which cannot. It might have seemed tempting to think that a truth can be known *a priori* if and only if it is purely conceptual. We have already seen that Bolzano maintained that *only* purely conceptual truths could be thus known. Interestingly, however, he consistently

[32]**WL**, §300 [III.131].

[33]I point out in passing that this applies even to logically analytic cognitions, e.g., "Either Bolzano is wise or it is not the case that Bolzano is wise." This seems perfectly reasonable to me, since, according to Bolzano, the truth of such propositions requires that they be objectual, and this can only be established, it seems reasonable to suppose, by empirical means. Cf. **WL**, §369 [III.454].

[34]**WL**, §306, no. 12 [III.202].

[35]This seems reasonable given Bolzano's assumptions about the objects of (humanly attainable) intuitions, namely, that they are all changes which occur "just now" in the mind of the individual who forms the intuition (**WL**, §286 [III.89]). When someone's mind registers such a change by forming an intuition, he already has a primitive kind of experience, or at least proto-experience. Thus a proposition containing an intuition cannot be thought, nor *a fortiori* taken to be true, without experience, and the designation of intuitional judgments and the judgments derived from them as empirical is justified at least to this extent.

resists asserting the converse. While he takes the highly optimistic view that *most* purely conceptual truths can be known *a priori*, he never says that this holds for *all*.[36]

Bolzano's refusal to claim that all purely conceptual propositions can be known *a priori* can be seen as an obvious consequence of his other commitments. He says, for example, that he is persuaded that there are pure concepts with infinitely many parts. At the same time, such concepts cannot be thought by humans.[37] Thus any purely conceptual proposition containing such a concept cannot be thought, still less known, by humans, *a priori* or otherwise.[38] On the other hand, one might take Bolzano's reluctance as a sign of his usual prudence. Absent a proof that all conceptual truths can be known *a priori*, we should not assert this.

A final interesting feature of Bolzano's account is that, since all purely conceptual truths are held to be necessary,[39] there are necessary truths that cannot be known *a priori*.

Now what of mathematical infallibility, to which Bolzano seems unwittingly to have committed himself in the *Contributions*? Once again, the more careful account of the *Theory of Science* is a vast improvement. The source of all error, Bolzano now speculates, is that, due to the finitude of our cognitive powers, we find ourselves compelled to make merely probable inferences, inferences which, though fully rational, may nevertheless fail to be truth-preserving:

> According to what was said in §§300 and 301 there are *inferences of probability*, i.e., inferences where the conclusion merely states that a certain proposition M has a greater or lesser degree of probability. Moreover, our soul, on account of its finitude, is obliged to hold this proposition to be true, i.e., to form the judgment M, with a degree of confidence proportionate to the probability. But as a proposition which is probable need not always be true, it is clear how the soul, in elevating all the propositions with a high degree of probability to judgments, can sometimes err. Error arises whenever it happens that something which we have recognized as probable, following perfectly

[36]**WL**, §133, note [II.36], emphasis added: "The ancients already knew, but did not pay much attention to, a division of our cognitions into those whose correctness we ascertain merely through experience, and those that do not require any experience. LEIBNIZ and KANT emphasized this distinction as extremely important; as it happens, it *nearly* coincides with the division of propositions into conceptual and empirical ones, since the truth of *most* conceptual propositions can be decided by pure thought, while propositions which contain an intuition can be judged only by experience."

[37]See, e.g., **WL**, §78 [I.356]; §116 [I.543].

[38]It seems to me that a similar argument would also work for propositions consisting of a sufficiently large, but still finite, number of parts.

[39]In the broad sense; in the strict sense, Bolzano thinks necessity always qualifies actual existence; see **WL**, §182.

> correct rules of inference, and which we expect and take to be true, turns out to be false.[40]

Although immediate judgments of perception are not mediated by such inferences (and hence are infallible), most ordinary empirical judgments are. It follows immediately that the ordinary sort of empirical judgments are fallible:

> It is well known that the first judgments for which we inevitably make use of judgments of probability are the so-called *judgments of experience*. The realm of experience is accordingly the domain where error first and inevitably dwells.[41]

On Bolzano's much richer account of knowledge in the *Theory of Science*, it also turns out that most mathematical judgments turn out to be empirical in the strict sense and thus fallible. Getting things wrong is entirely normal, and is to be expected. This is obvious in cases where our mathematical beliefs are formed with the aid of others' testimony, as when we consult their publications or attend their lectures, etc., as well as when we make use of signs.[42] It is also obvious when garden-variety induction is used in mathematical arguments, as was common practice in the eighteenth and early nineteenth century.[43]

But fallibility is normal even in cases where a proof is conducted purely mentally, since probable inferences are involved whenever we call on memory:

> When we form a judgment merely because we recall having formed it before, I will call it a judgment of *memory*. In calculation and any deduction of a judgment from a long series of inferences, where the first inference is lost to consciousness before the last is reached, such judgments from memory are inevitable. But as every proposition expressing a memory is by its very nature empirical, all judgments drawn from memory, and hence all those derived from them, must strictly speaking be said to be judgments of experience.[44]

Hence:

> [E]rrors may also creep into purely *conceptual judgments*, when we are somehow necessitated or prompted to make use of probability

[40] **WL**, §309 [III.212-213].

[41] **WL**, §309 [III.214].

[42] **WL**, §309, no. 6 [III.214-218]; §344, no. 8 [III.372].

[43] Cf. "On the mathematical method," §7, note. Eng. tr. in B. Bolzano, *On the Mathematical Method and Correspondence with Exner* (Amsterdam: Rodopi, 2004), p. 53.

[44] **WL**, §306, no. 13 [III.202].

> judgments in their derivation. This is the case in particular for all judgments which draw on memory ...; consequently, the danger of error extends to all judgments based upon long series of inferences, no matter of what kind. For we arrive at these by means of premises, some of which are merely drawn from memory.[45]

We now have a theoretical justification for what Bolzano had been doing all along, namely, carefully examining proofs (others' as well as his own) in the search for errors and room for improvement.

In the second half of Volume 3 of the *Theory of Science*, he reveals some of his techniques of mathematical criticism.[46] Just as in the *Contributions*, he points out several indicators of the incorrectness of proofs. One of them is simple enough: if the conclusion is false, something must be wrong with the proof.[47] Though obvious, I think it fair to say that Bolzano's contemporaries, especially the philosophers, were not aware of how widely this could be applied within mathematics. For, unlike Kant and his followers, Bolzano saw that it was not only mathematical bunglers, but also the masters of the art, who stated and offered proofs for false propositions. Accordingly, we find that the search for counterexamples, so characteristic of the mathematics of the late nineteenth century, is already fully developed with Bolzano, so much so that his *Theory of Functions* can easily seem to belong to a later time.

There are other criteria as well, though I do not have the space to discuss them here.[48] Instead, I would like to point to Bolzano's reasons for developing such criteria in the first place.

Imre Lakatos, in his delightful book *Proofs and Refutations*, claimed, with some exaggeration in my view, that most if not all early nineteenth-century mathematicians believed that genuine or proper mathematics is infallible, so that there is no such thing as a flawed proof. This assumption, he maintained, stifled mathematical creativity:

> As long as ... there were only proofs or non-proofs, but no sound proofs with weak spots, mathematical criticism was barred. It was the infallibilist philosophical background of Euclidean method that bred the authoritarian traditional patterns in mathematics, that prevented publication and discussion of conjecture, that made impossible the rise of mathematical criticism. Literary criticism can exist because

[45] **WL**, §309 [III.214].

[46] I borrow this term from Imre Lakatos (*Proofs and Refutations*, ed. J. Worrall and E. Zahar (Cambridge University Press, 1976), p. 139.

[47] **WL**, §373.

[48] **WL**, §§371-376.

> we can appreciate a poem without considering it to be perfect; mathematical or scientific criticism cannot exist while we only appreciate a mathematical or scientific result if it yields perfect truth. A proof is a proof only if it proves; and it either proves or it does not.[49] The idea—expressed so clearly by Seidel—that a proof can be respectable without being flawless, was a revolutionary one in 1847, and, unfortunately, still sounds revolutionary today.[50]

Lakatos, unfortunately, died just before Bolzano's mathematical manuscripts started to appear in the *Gesamtausgabe*. Had he lived to see them, I think he might have agreed that Bolzano's work was very much in the critical spirit he favoured. For Bolzano, criteria which permit us to determine that proofs is flawed before entering into their details are not to be used to enable us to commit them to the flames wholesale. On the contrary, proofs that can be shown to be faulty by the application of such obvious criteria are often of great interest and utility, since careful study can reveal plausible yet incorrect principles or modes of inference, gaps which need to be filled, and perhaps even suggestions about how to fill them.

> Since . . . there are indicators the presence of which allows us to detect the incorrectness either of a premise or an inference without knowing exactly where the flaw lies, it will also be advantageous to learn and apply these indicators. For it is certainly an advantage to learn that a given proof is untenable, even when we do not know where the fault lies. Closer examination will in most cases reveal just where that is. Indeed, it may be useful to undertake the last-mentioned investigation before the others. For if we have discovered that the proof has one of the indicators, we already know that it is faulty, and thus will work with even greater assurance and diligence to discover where the flaw lies. In this case, we will not be annoyed when we consider the propositions and inferences both individually and in combination, nor when we check the whole procedure for the various flaws that may be committed in proofs until we have discovered the problem.[51]

And again:

> [I]t is worth recalling that the examination of proofs is one of those problems for our reflection that often results (according to the rule of

[49] This view was challenged by the former Prime Minister of Canada, Jean Chrétien, who famously observed that: "A proof is a proof. What kind of a proof? It's a proof. A proof is a proof. And when you have a good proof, it's because it's proven."

[50] I. Lakatos, *Proofs and Refutations* (Cambridge University Press, 1976), p. 139.

[51] **WL**, §370 [III.460].

> §325) in our learning of truths that we did not seek, and not only when a proof is correct, but also when it is not. For even in a flawed proof many new truths, or new combinations of known truths, or new, useful forms of inference may occur, and even the faults that one encounters may teach us something; and if the proof does not prove what it is supposed to, it may still serve to prove something else.[52]

5 Application: Proving the Binomial Theorem

In 1816, Bolzano published a little book on the binomial theorem, one of the first rigorous treatments of the theory of power series. I would like to present some of the main points of this work, and to discuss how they illustrate Bolzano's approach to foundations.

A *binomial* in the sense I am concerned with here is an expression of the form $(x+y)^n$. If n is a natural number, the expansion of the binomials $(x+y)^n$ gives rise to the following pattern for $n = 0, 1, 2, 3, \ldots$:

$$\begin{gathered}
(x+y)^0 = 1 \\
(x+y)^1 = x + y \\
(x+y)^2 = x^2 + 2xy + y^2 \\
(x+y)^3 = x^3 + 3x^2y + 3xy^2 + y^3 \\
(x+y)^4 = x^4 + 4x^3y + 6x^2y^2 + 4xy^3 + y^4 \\
(x+y)^5 = x^5 + 5x^4y + 10x^3y^2 + 10x^2y^3 + 5xy^4 + y^5 \\
\vdots
\end{gathered}$$

The coefficients of these expansions give rise to Pascal's triangle, with all its beautiful properties:

$$\begin{array}{ccccccccc}
 & & & & 1 & & & & \\
 & & & 1 & & 1 & & & \\
 & & 1 & & 2 & & 1 & & \\
 & 1 & & 3 & & 3 & & 1 & \\
1 & & 4 & & 6 & & 4 & & 1
\end{array}$$

And so on

We can write the above expansions as follows:

$$(x+y)^n = \sum_{k=0}^{n} \binom{n}{k} x^{n-k} y^k$$

[52] Cf. **WL**, §370 [III.462-3].

where

$$\binom{n}{k} = \frac{n(n-1)(n-2)\cdots(n-k+1)}{k!}$$

When n is a positive integer, this reduces to:

$$\binom{n}{k} = \frac{n!}{k!(n-k)!}$$

It simplifies things to set $x = 1$. In this case, we get:

$$(1+y)^n = \sum_{k=0}^{n} \binom{n}{k} y^k$$

Around 1665, Newton discovered that the expansion also worked for other values of the exponent n, in particular for negative and fractional values, though in these cases new complications emerge, since the expansion results in an infinite series.

For $n = -1$, for example, we obtain:

$$(1+x)^{-1} = \frac{1}{1+x} = 1 - x + x^2 - x^3 + x^4 - \cdots$$

While for $n = \frac{1}{2}$, we have:

$$(1+x)^{\frac{1}{2}} = \sqrt{1+x} = 1 + \frac{1}{2}x + \frac{\frac{1}{2}(\frac{1}{2}-1)}{2!}x^2 + \frac{\frac{1}{2}(\frac{1}{2}-1)(\frac{1}{2}-2)}{3!}x^3 + \cdots$$

which yields:

$$\sqrt{1+x} = 1 + \frac{1}{2}x - \frac{1}{8}x^2 + \frac{1}{16}x^3 - \frac{5}{128}x^4 + \cdots$$

Through much honest toil, Newton discovered that, for suitable values of x, these series got closer and closer to the value on the left hand side of the equation as more and more terms were taken. It was tempting to conclude that the result held for all values of the exponent, real as well as complex, and other mathematicians did just that. (As noted above, such inductive arguments were actually fairly common in 18th century mathematics). The fact that no one at the time really knew what exponentiation meant when the exponent was irrational or complex did not slow them down in the least.

All the same, Bolzano could rightly observe in 1816, based upon his customary thorough search of the literature, that no one so far had offered an acceptable proof of the binomial theorem for any case other than the most simple one, where the exponent is a positive integer. He found many attempted proofs of the general binomial theorem, but could tell, almost at a glance, that none of them was satisfactory.

This is why: in the difficult cases of the binomial theorem, the expansion results in an infinite series. Depending on the value of x, however, the series may or may not approach the value on the left hand side of the equation as more terms are taken. Consider what happens for $x = -2$ and $n = -1$: here we have:

$$(1-2)^{-1} = -1 = 1 + 2 + 4 + 8 + 16 + \cdots$$

a series for which the partial sums are:

$$1, 3, 7, 15, 31, \ldots$$

Similarly, we have:

$$-\tfrac{1}{2} = 1 + 3 + 9 + 27 + \cdots$$

$$-\tfrac{1}{3} = 1 + 4 + 16 + 64 + \cdots$$

$$-\tfrac{1}{4} = 1 + 5 + 25 + 125 + \cdots$$

And so on.

Strange as it may seem, some eminent eighteenth-century mathematicians, notably Euler and Lagrange, attempted to defend the correctness of the binomial formula even in these cases.[53] Their defense involved a qualification: though the above series were still claimed to be correct, they were said to be "unsuitable for calculation". Even if one were prepared to adopt this approach (Bolzano, along with Cauchy, Abel and others, wasn't[54]), a proof that the series gives the correct result when values that are suitable for calculation are chosen would still be required. But it was precisely this that was *obviously* lacking in Bolzano's opinion.

All mathematicians who attempted to prove the theorem for the cases where the expansion gives rise to an infinite series, Bolzano noted, admitted that we must have $|x| < 1$ if x is to be suitable for calculation. If a proof is to be correct, then, it will have to appeal to this restriction somewhere. But no mention of it was to be

[53] L. Euler, "De seriebus divergentibus,"(1755) *Leonhardi Euleri opera omnia*, (Bern, 1911-1975)(1)14,585-617; J.-L. Lagrange, "Addition aux premières recherches sur la nature et la propogation du son," *Misc. Turin.*, II (1761); reprinted Lagrange, *Oeuvres*, I.319-332, 323. See also E. J. Barbeau, "Euler Subdues a Very Obstreperous Series," *The American Mathematical Monthly*, Vol. 86, No. 5 (May, 1979), pp. 356-372.

[54] Bolzano, **BL**, Preface, II, i; **MW**, p. 159f.; Cauchy, *Cours d'analyse* (Paris, 1821), introduction, p. iv; N. H. Abel, *Œuvres complètes*, ed. Sylow et Lie (Christiana, 1881), Vol. 1, p. 219.

found in any of the proofs Bolzano had read. If they were correct, then, (according to the first criterion set out in the *Contributions*) the formula should hold for all values of x without restriction. Since it doesn't, we can know in advance that the proofs fail. *They prove too much, and hence prove nothing*.

The reason why previous proofs had failed, he explains, was that mathematicians had not reflected sufficiently on the *meaning* of the theorem. In the general case, we have an infinite series, which is claimed to have a sum. But what, exactly, is an infinite series, and what does it mean for such a series to have a sum? Moreover, what is meant by *equality* when infinite power series are involved, as, e.g., in the equation:

$$(1+x)^n = \sum_{k=0}^{\infty} \binom{n}{k} x^k$$

Without becoming clear on what is actually claimed by such statements, we have no hope of proving them. Bolzano accordingly sets out to give appropriate characterizations of these notions, in effect defining the modern notions of a limit and the convergence of an infinite series. Only then does he proceed to prove the binomial theorem by cases, depending upon whether the exponent is a positive integer, a negative integer, a proper fraction, or a rational number. Though somewhat plodding, this part of Bolzano's paper is a genuine triumph, constituting one of the first rigorous proofs in the theory of power series.

The last case considered by Bolzano is when the exponent is irrational, e.g.:

$$(1+x)^\pi = 1 + \pi x + \frac{\pi(\pi-1)}{2!}x^2 + \frac{\pi(\pi-1)(\pi-2)}{3!}x^3 + \cdots$$

or

$$(1+x)^{\sqrt{2}} = 1 + \sqrt{2}x + \frac{\sqrt{2}(\sqrt{2}-1)}{2!}x^2 + \frac{\sqrt{2}(\sqrt{2}-1)(\sqrt{2}-2)}{3!}x^3 + \cdots$$

Although in some ways the proof Bolzano sketched for this case might be viewed as a notable failure of his paper, I would like nevertheless to point to it as a confirmation of the essential correctness of his views on foundational research.

The idea of the proof is relatively straightforward. Bolzano begins with the fact that any irrational number can be approximated to any desired degree of accuracy in terms of rational numbers. Thus if y is irrational, it should be able to approximate the binomial:

$$(1+x)^y$$

to any desired degree of accuracy in terms of binomials of the form:

$$(1+x)^{y_i}$$

where the y_i are rational numbers which approach y as i tends towards infinity.

Now the expansion has already been proved correct for rational values of the exponent, whence:

$$(1+x)^{y_i} = \sum_{k=0}^{\infty} \binom{y_i}{k} x^k$$

At the same time, the partial sums

$$\sum_{k=0}^{n} \binom{y_i}{k} x^k$$

can be brought arbitrarily close to:

$$\sum_{k=0}^{n} \binom{y}{k} x^k$$

by taking i sufficiently large. Thus it seems we should be able to prove that the partial sums:

$$\sum_{k=0}^{n} \binom{y}{k} x^k$$

get as close as desired to

$$(1+x)^y$$

provided n is taken large enough.

It seems to me that Bolzano's argument, as stated, contains a gap. If I have understood him correctly, we could formulate his proof in modern terms as follows: Let y be irrational, and suppose $\{y_i\}$ is a sequence of rational numbers such that $\lim_{i\to\infty} y_i = y$.

Since $\lim_{i\to\infty} y_i = y$, we have:

$$(1+x)^y = \lim_{i\to\infty} (1+x)^{y_i} \tag{1}$$

Also, for any n, where the sums are finite, we will also have:

$$\lim_{i\to\infty} \sum_{k=0}^{n} \binom{y_i}{k} x^k = \sum_{k=0}^{n} \binom{y}{k} x^k \tag{2}$$

Now it has already been proved that, for all y_i:

$$(1+x)^{y_i} = \lim_{n\to\infty} \sum_{k=0}^{n} \binom{y_i}{k} x^k \tag{3}$$

Hence:

$$(1+x)^y = \lim_{i\to\infty}(1+x)^{y_i} = \lim_{i\to\infty}\lim_{n\to\infty}\sum_{k=0}^{n}\binom{y_i}{k}x^k \tag{4}$$

Whence (???)

$$(1+x)^y = \lim_{n\to\infty}\lim_{i\to\infty}\sum_{k=0}^{n}\binom{y_i}{k}x^k = \lim_{n\to\infty}\sum_{k=0}^{n}\binom{y}{k}x^k \tag{5}$$

The dodgy step occurs between (4) and (5), where the limits are exchanged. Bolzano did not see this, perhaps because his notation obscured the complexity of the inferences involved.[55]

Cartesians might be tempted to draw comfort from this: Bolzano's predecessors failed to prove the binomial theorem for the case where the expansion is infinite because they were not operating with clear and distinct ideas of infinite sums, etc. Bolzano succeeded where they did not precisely because he began with a clear and distinct idea of an infinite sum. Moreover, he had failed precisely when his ideas were not distinct enough.

For my part, I do not think that this is at all the moral that should be drawn from this story. For Cartesianism as usually understood tells us not to attempt to prove anything until we have clearly and distinctly grasped the concepts and principles involved. On this account, Bolzano should never even have started a proof. He should instead have begun by trying to attain a clear and distinct understanding of the notions and principles involved. This sounds nice, but is it possible? Is it not rather the case that the best and perhaps sometimes the *only* means of getting an increasingly clear and distinct understanding of the principles and notions required to prove a given truth is to formulate, criticize, and revise proofs of that very proposition, or at least of other, related ones? In tuning a violin, we usually move back and forth across the sought pitch, now a little flat, now a little sharp, finally just right. Many have thought mathematics is or at least should be like this: tune first (get your principles clear and distinct), then play (prove). Bolzano reminds us that the analogy breaks down: attempted proofs are the hand that moves the tuning peg. And Bolzano's proof for the irrational case, deficient though it may be, nonetheless contains elements that can be used in a correct proof.

It would take the better part of a century to arrive at a satisfactory proof of the binomial theorem for all real (and complex) values of the exponent. This should

[55] He writes $x_n = L + \Omega$ instead of the modern $\lim_{n\to\infty} x_n = L$. Usually there are no subscripts on the Ω which link them to specific variables. Thus Bolzano had no way of reflecting an exchange of limits in his customary symbolism, and might easily have been unaware that his proof involved such an inference.

not surprise, since such a proof required not only that methods be developed for dealing with the thorny problems Bolzano unwittingly encountered in his proof, but also that suitable definitions be found for the concept of real number as well as for exponentiation where the exponent can take on any real (or even complex) value. Bolzano would indeed go on to make several contributions that could have been applied to the solution of these problems, hitting upon the concept of a uniform limit[56] (useful for negotiating some cases of limit exchange), and developing an arithmetical theory of real (or measurable) numbers.[57] In the end, however, it was only the combined efforts of many mathematicians, extending, criticizing, and refining each other's work, that resulted in a reasonably solid proof. Bolzano, I feel sure, would have seen this as both normal and fitting. As with Catholicism, he believed not in the perfection, but rather the *perfectibility* of mathematics.

[56]See *FL*, §144; **MW**, p. 515; see also **FL**, §49; **MW**, p. 456. It seems likely to me that Bolzano made this discovery by spotting a gap in a proof by Lagrange. See P. Rusnock, "Philosophy of mathematics: Bolzano's responses to Kant and Lagrange," *Revue d'histoire des sciences* **52** (1999) 399-427, p. 422-424.

[57]**BBGA**, II.A.8; **MW**, pp. 355 ff.