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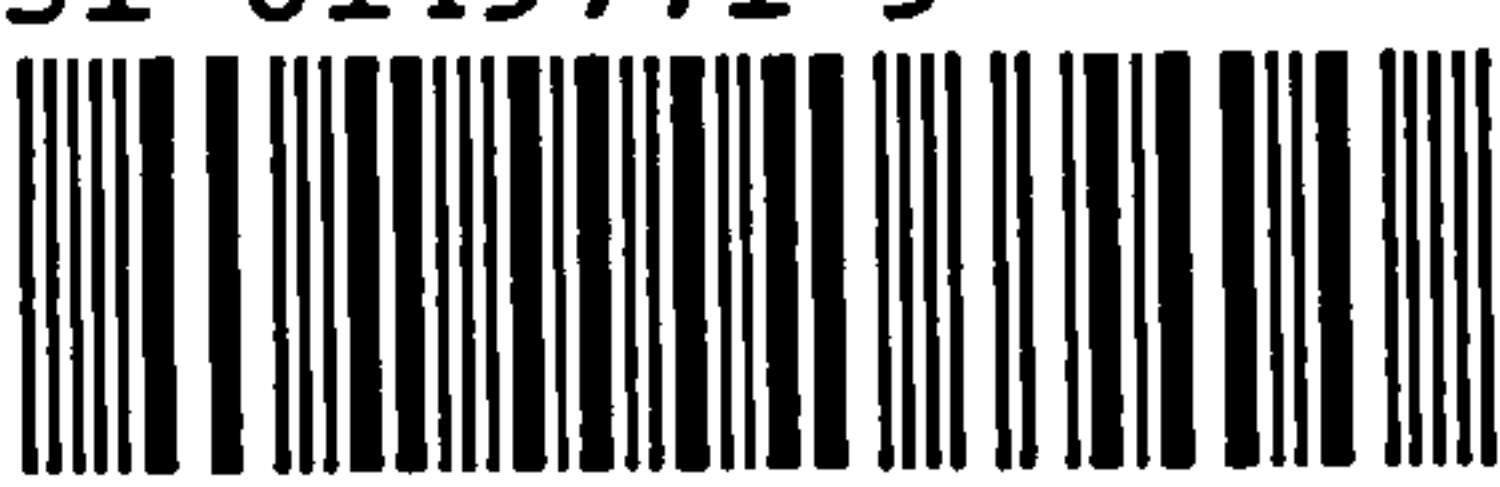
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THE MATHEMATICAL WORKS OF BERNARD BOLZANO

PUBLISHED BETWEEN 1804 AND 1817

by

Stephen Bruce Russ M. A., B. Sc., A. K. C.

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A thesis submitted from the Mathematics Faculty of the  
Open University for the degree of Doctor of Philosophy.

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September 1980

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### Statements

1. No part of this thesis has previously been submitted for any degree or qualification at any university.
  
2. The pages A430 - A489 of the Appendix to the thesis have been published (with minor alterations and a separate introduction) in the article A translation of Bolzano's paper on the intermediate value theorem, *Historia Mathematica* Vol. 7(2)(1980) pp. 156-185. No other part of the thesis has been published.
  
3. The author is willing that this work be made available or photocopied at the discretion of the Librarian of the Open University.

## Abstract

The purpose of the thesis is to assess the mathematical achievements of Bernard Bolzano on the basis of the five early published works. The material is divided into the areas of the foundations of mathematics, geometry and analysis. In making this assessment there have been two principal considerations. Firstly, any judgement of the significance of Bolzano's work should be made in the light of the historical context, so considerable space is devoted to the relevant 18th century sources. Secondly, as a general framework to the thesis there is the question of how Bolzano's general views about mathematical proofs and concepts are related to his achievements. The main claim and conclusion of the thesis is that this relationship was unusually clear and significant in the case of Bolzano's work.

There is an Appendix containing the first English translation of all five of Bolzano's works as well as the German texts of their first editions.

### Acknowledgements

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## Chapter 1 : Introduction

### 1.1 The Aims, Scope and Organisation of the Thesis

The main purpose of the thesis is to investigate, in one particular case, the way in which views about the nature of mathematics have been related to the development of important new mathematical concepts and methods. The case we shall examine is that of the early work of Bernard Bolzano as this is exemplified in his five mathematical works published between 1804 and 1817. These works are concerned with the foundations of mathematics and with geometry and analysis. The papers of 1816 and 1817 contain a number of substantial and original contributions to analysis. Bolzano was also especially interested in examining the concepts and proofs of mathematics in a philosophical spirit. As a Professor of Theology at the University of Prague he was relatively isolated in his mathematical work; he lacked immediate criticism and influence from contemporaries and he was most forthcoming in his writing about the general approach and motivation behind his work. These factors combine to make a study of his work particularly suitable for our purpose.

The relationship of general views to particular results in the work of

an individual is liable to be a rather vague matter. Nevertheless the question of such a relationship offers a useful background for certain preliminary studies which are worthwhile in their own right. These are:

- (a) to state as clearly as possible Bolzano's views on the nature of mathematics, particularly its concepts and proofs.
- (b) to describe and assess Bolzano's early mathematical work in the light of his own time.

Most of the thesis is concerned with achieving these two goals, but as an overall framework we shall make various references to, and finally draw some conclusions about, the wider question of the relationship of (a) to (b). As a general aid to these purposes and for the sake of promoting a wider appreciation of Bolzano's work we have added, in an Appendix, both the texts and full English translations of the five mathematical works concerned.

Any assessment of Bolzano's mathematical work requires some preliminary account of the use and meaning of various terms and concepts at the beginning of the nineteenth century. It is very difficult even to describe any achievement of this period directly in modern terms without thereby giving a misleading impression of the achievement. For this reason we have devoted considerable space to the discussion of important works which would be known to Bolzano. This is particularly necessary for the chapters

on analysis.

The primary source materials for the thesis, those referred to in the title, are the following five works:

Betrachtungen über einige Gegenstände der Elementargeometrie.

(Considerations on some objects of elementary geometry.)

Prague 1804 X + 63 pp. (BG)

Beyträge zu einer begründeteren Darstellung der Mathematik

(Contributions to a more well-founded Presentation of Mathematics.)

Prague 1810 XVI + 152 pp. (BD)

Der binomische Lehrsatz, und als Folgerung aus ihm der polynomische, und die Reihen, die zur Berechnung des Logarithmen und Exponentialgrößen dienen, genauer als bisher erwiesen.

(The binomial theorem, and as a consequence from it the polynomial theorem, and the series which serve for the calculation of logarithmic and exponential quantities, proved more strictly than before.)

Prague 1816 XVI + 144 pp. (BL)

Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege.

(Purely analytic proof of the theorem, that between any two values, which

give results of opposite sign, there lies at least one real root of the equation.)

Prague 1817 60 pp. (RB)

Die Drey Probleme der Rectification, der Complonation und der Cubirung, ohne Betrachtung des unendlich Kleinen, ohne die Annahmen des Archimedes, und ohne irgend eine nicht streng erweisliche Voraussetzung gelöst zugleich als Probe einer gänzlichen Umgestaltung der Raumwissenschaft, allen Mathematikern zur Prüfung vorgelegt.

(The three problems of rectification, complonation and cubature, solved without consideration of the infinitely small, without the hypotheses of Archimedes, and without any assumption which is not strictly provable; at the same time being presented for the scrutiny of all mathematicians as a sample of a complete reorganisation of the science of space.)

Leipzig 1817 XXIV + 80 pp. (DP)

These five works will be referred to throughout the thesis by the abbreviations BG, BD, BL, RB, DP respectively. Further bibliographical information and background to the works is given in 1.3. The full text of their first editions, together with English translations and notes, comprise the Appendix to the thesis.

There is a large amount of other mathematical material which was written by Bolzano during the period we are considering. He kept a kind of mathematical diary containing notes, drafts of articles, etc., but these

remained unpublished until very recently. The first two volumes of this material (covering the years 1803 to 1811) have now appeared in the series Miscellanea Mathematica (edited by Prof. Bob van Rootselaar and Anna van der Lugt) which is currently being published as part of the Bolzano Gesamtausgabe (Complete works) (Bolzano [1]) by Frommann Verlag of Stuttgart. However, although some reference will be made to this material it has not been studied or taken into account here in detail. It would be essential for a thorough understanding of the development of Bolzano's thought in this period but this has not been our first priority. The published works represent Bolzano's most significant mathematical work during the first part of his life and they therefore represent a natural and reasonable way to restrict the material for this thesis.

The organisation of material in the main thesis has already been summarised in the Contents pages. There is a separate page of Contents in the Appendix for details of the arrangement of the translations and their notes. The references are arranged alphabetically by author, the works of each author being numbered in square brackets. Thus specific references will be given in the form "Kant [2] p. 57", unless the work concerned is referred to only once when the page reference may be given in the List of References. When the date of a work is particularly relevant this may be included with the reference thus "Kant [2] (1800) p. 57". Because of the special place of the five works listed already (p. 9) their



bibliographic details appear in 1.3 and will not be repeated in the main List of References.

References to the main thesis will be by page number or, if more appropriate, by section number, as in the previous sentence. References to the texts of the five main works will be given in two ways: the page in the Appendix which has the translation of the relevant passage, and the standard abbreviation for the text followed by the page or paragraph number in the first German edition. The Appendix has been separately paginated with the prefix A to the page numbers. For brevity we shall omit the abbreviation 'p.' in all references to the Appendix. Since the Prefaces of the five German texts have separate Roman numeral paginations every page of these texts has a unique dual reference. Thus the beginning of the main text of BD has reference: (A108;BD,1).

Many of the quotations made throughout the thesis are from works originally published in German, French or Latin. For the most important of these the quotation has been given in the original language and in translation but often quotations have simply been given in an English version. All such translations, as well as translations of titles and headings etc., are by the present author unless they have been otherwise ascribed.

## 1.2 Biographical Remarks on Bolzano

The outline of the first half of Bolzano's life and work which is given here is brief and only intended to provide the context and background for the five mathematical works. The principal sources used are the biography Winter [1] and the autobiography Bolzano [2].

Bernard Bolzano was born on the 5th October 1781 in Prague where he lived and worked for most of his life. He died there in 1848. Thus the background to his childhood and early career was the state of almost un-interrupted war which existed over most of Europe between 1789 and 1815. Prague was the main town of Bohemia which at this time was part of the Habsburg Empire controlled by the Kaiser in Vienna.

Bolzano's father, an Italian art-dealer, had emigrated to Prague in the 1760's and then married Cecilia Maurer. Of their twelve children only two survived to adulthood. Bolzano himself was not a strong child but in spite of headaches and a weak heart he says, "I was a very lively boy who never rested for a moment" (Bolzano [2] p.56). This disposition to incessant activity did not abate as he grew older and is manifest in the huge amount of manuscript material which he worked at continually throughout his life.

At first the young Bolzano was educated at home by a tutor. Then at the age of ten he attended the nearby Piarist Gymnasium where his progress was good but not outstanding. In 1796 he entered the Philosophy Faculty in

Prague University and for four years he followed courses mainly in philosophy and mathematics. Although, on his own account, he found both subjects initially rather difficult he discovered that in pure mathematics there was ample scope for the fundamental, purely conceptual investigations which appealed to him so strongly. He refers specifically to Kästner's important textbook (Kästner [1]) where he,

proved what is generally passed over because everyone already knows it, i. e. he sought to make the reader clearly aware of the basis [Grund] on which his judgements rest. That was what I liked most of all. My special pleasure in mathematics rested therefore particularly on its purely speculative parts, in other words I prized only that part of mathematics which was at the same time philosophy. (Bolzano [2] p. 64)

This particular interest in mathematics seems to have been awakened soon after he started attending the mathematics class of Prof. S. Wydra. The first of Bolzano's mathematical works is dedicated to Wydra (see Note [2] on A87). In the academic year 1799-1800 he attended two classes in "higher mathematics" taught by Prof. F. J. Gerstner. After an outstanding performance in the examination on these course Bolzano was granted permission to borrow an unlimited number of books from the University library and a stipend of sixty Gulden a year.

In philosophy Bolzano's first important study was of Baumgarten's

Metaphysik (Baumgarten [1]). This was a standard university text of the eighteenth century and was largely an exposition of Wolff's philosophy. Although Bolzano speaks in his autobiography of his criticisms of the work he remained generally sympathetic to the rationalist methods and outlook of Leibniz and Wolff. Many of his fellow students at this time were under the spell of Kant. The first edition of the Kritik der reinen Vernunft (Critique of Pure Reason) (Kant [1]) had appeared in the year of Bolzano's birth 1781, and during the 1790's in Prague there was a group of students who spent several hours each day in communal reading of Kant's works. Bolzano studied the Kritik in 1798 and from the outset he disagreed with Kant's central claims concerning knowledge. In particular he regarded Kant's notion of a pure a priori intuition as unintelligible. In the case of mathematical propositions he therefore opposed the view that the synthetic a priori judgements of mathematics are based on the pure intuitions of space and time. He pointed out (presumably referring to preliminary work on BG) that he had already proved several synthetic propositions of geometry "purely from concepts". This is discussed further in Chapter 2. Independence of thought was something to which Bolzano attached great value. It was characteristic of his work on any subject to read the works of his predecessors very carefully but always to proceed with his own independent (and often original) ideas.

In the autumn of 1800 he began three years of theological study. He

was thinking a great deal at this time of whether or not to commit himself to the Church and possibly to take vows in one of the religious orders in Prague. He eventually decided against the exclusively religious life for at least two reasons. Although he was basically a perfectly orthodox Catholic he did not find his rationalist inclinations fitting as comfortably as he had hoped with his theological studies. Secondly, he came to realise that his real vocation and gift was as a teacher and educator, rather than as a pastor. During this time he read numerous works on education, making a particularly close study of Pařizek's Lehrmethode (Pařizek [1]). This interest in education, in practice as well as theory, undoubtedly influenced all his work. For example, his general and overriding concern for the clarity and correct ordering of concepts is likely to have been reinforced by the obvious educational value of ensuring that ideas are introduced clearly and systematically. There are various references, even in the mathematical works, to particular difficulties likely to be experienced by students learning about an idea for the first time.

Education was, however, only subsidiary to the main motive force in Bolzano's life which was not so much a matter of religion as morality. He laid great emphasis on his conception of the "highest moral law" which was always to choose in a given situation that action which was most conducive to the well-being of the whole. An example of the subservience of even religion to morality is the remark made by his teacher and friend Prof. Mika

(quoted in Bolzano [2] p. 67) that, "a doctrine is justified as soon as it can be shown that faith in it assures us of a certain moral benefit". This principle was clearly very significant to Bolzano. For example, with reference to the concept of divine revelation he says that it was not so much a matter of what, "the facts actually were in themselves but rather what kind of idea of them is the most edifying for us". (Bolzano's emphasis in Bolzano [2] p. 67).

There thus arises an interesting tension in Bolzano's thinking. The religious principle just stated seems highly pragmatic and subjective. The mathematical ideal of always proving and presenting the true basis or ground for theorems and definitions seems to be based on a desire to conform to an objective truth or state of affairs. It is unlikely that both principles could co-exist in harmony in Bolzano's thought, at least as we should understand them today. It is perhaps the second one, the objectivity and truth of mathematics which is likely to be misunderstood. We should not regard Bolzano's research in pure mathematics as ultimately being done for the sake of truth or for perfecting mathematics: rather it is an interesting, but serious, exercise in correct thinking. This value and purpose of mathematics is stressed at the opening of the Preface to BG (A12; BG, V). At least part of Bolzano's motivation here was probably that correct thinking should promote correct morality and correct action, i.e. virtue.

While pursuing his theological studies Bolzano was also preparing his

doctoral thesis on geometry. This was accepted and published in 1804 as BG. He was awarded the degree of Doctor of Philosophy on 5th April 1804. On the 7th April he was ordained and on the 19th April he was appointed (provisionally) to the newly-formed professorship in theology at the University of Prague. Such a position had been formed at all the universities in the Empire by the Kaiser, then Franz I, for mainly political reasons. The purpose was ostensibly to curtail the current wave of liberalism and free-thinking. In addition to courses of lectures Bolzano was required to give twice-weekly sermons to the students and citizens of Prague. He performed these duties with great seriousness and enthusiasm and he soon became highly respected and popular in Prague.

However, Bolzano's appointment was viewed from the start with criticism and suspicion by the authorities in Vienna, the main reasons for this being his relatively liberal and supposedly unorthodox views in both theology and politics, and his refusal to use the authorised textbook written by Frint, the chief Chaplain in Vienna. His appointment was eventually only confirmed in 1807. His relations with the authorities were always uneasy and he was finally dismissed by imperial order in 1819, and after much wrangling he went into retirement from 1821 with a State pension but with orders not to teach or publish in any way. Although these publication restrictions were relaxed later it is true to say that the five mathematical works published in the early period of his life (1804 - 1817) were the only

mathematical works published in Bolzano's lifetime.

We conclude these remarks on the first part of Bolzano's life with some comment on his personal afflictions which testify to his remarkable powers of concentration and industry. In the period from 1813 while his teaching position was in constant jeopardy, he suffered the losses of his fifteen-year-old sister Franziska, his father (in 1816) and his younger brother Peter, a successful medical student (in 1818). In the preceding years he had devoted much time and attention to the education of both his brother and sister. Franziska's death brought on tubercular attacks in Bolzano himself which left him unable to lecture for the years 1813 - 1816. It was during this same period that three of the main works discussed here, BL, RB and DP were written and published. This was in addition to numerous other unpublished works and notes on mathematics, logic, theology, ethics, politics and philosophy.



### 1.3 The Primary Sources

The primary source materials for the thesis are the five works listed in 1.1. In this section we shall add some background and bibliographic details on these works. It will be useful first to indicate how the works are related to the thesis chapters in terms of their content.

The subjects, or problems, which Bolzano deals with divide up fairly clearly into three areas: the foundations of mathematics, geometry and analysis. Accordingly these are the subjects of our main chapters. All the works have some significance for Bolzano's views on foundational questions but it is BD that is specifically devoted to this subject. The works RB and BL are clearly on analysis and BG is almost entirely on geometry. The rectification problem, considered in DP, involves analysis applied to geometry and although it is therefore mainly a contribution to analysis Bolzano uses it as an excuse to include various geometrical definitions and ideas. Thus BD and BG are the main subjects of Chapters 2 and 3 respectively. Both BL and RB are dealt with in Chapters 4 and 5 while DP is treated in each of Chapters 3 and 5.

In the case of each of the five works Bolzano himself believed he had made an important and original contribution to a contemporary problem, and to communicate this to the academic world was the immediate purpose of the publication. His first work, BG, which must have been written while Bolzano was completing his theological studies, presented an original

approach to elementary geometry in which the correct arrangement of concepts and theorems was all-important. On the basis of this work, which was warmly commended by Prof. Gerstner, he was awarded the degree of Doctor of Philosophy. There were at least three quite favourable reviews of the work in the contemporary journals but it did not attract the attention for which Bolzano had hoped (see 1.4). Criticism and interest in his work from other mathematicians is something Bolzano particularly desired and never really experienced.

On the title page of BD are the words "Erste Lieferung" ("First Issue"). It was to be the first part of many in what Bolzano intended as a complete re-organisation of all mathematical theories in accordance with his principles for the correct introduction of concepts and proofs. However, this grand project never materialised, apparently because he was so discouraged at the poor response to BD. He explains in RB that BD,

had the misfortune, with all the importance of its contents, of not even being announced and reviewed in some learned journals, and in others only very superficially. This forced me to postpone the continuation of these contributions to a later time and meanwhile just to attempt to make myself better known to the learned world by publishing some papers which, by their titles, would be more suited to arouse attention. (A455; RB, 27)

He goes on to explain that this purpose of arousing attention to his work was to be served by the publication of BL, RB and "some other papers" (he mentioned DP) but that these were waiting for a publisher. The problem of finding a publisher was clearly a difficult one: Bolzano has a different publisher for each of the five works. Although never published (until recently) his manuscripts show that he had prepared drafts of the second issue of BD . . . . as early as 1810. (See Bolzano [1] Vol. 2A/5 Einleitung.) It is difficult to date the writing of the works precisely but it is likely that each of the works BL, RB and DP had been prepared by about 1815. A footnote in BL (A312; BL, 32) refers to his discovery of a proof of the intermediate value theorem, "It is already sketched out in a special paper and should soon be printed." This, of course, was RB which was published in the Abhandlungen der königlichen böhmischen Gesellschaft der Wissenschaften 5th Volume (Prague 1818). In 1817 it had been printed separately although the present author has never seen a copy of this printing. Bolzano was elected as ordinary member of the Royal Bohemian Society of Sciences in 1815. It is remarkable that though RB is brief, very well-presented and highly significant, as well as enjoying the circulation of the Abhandlungen . . . , there seem to be no contemporary reviews of the work.

The work DP, which we have put last, was published in 1817 in Leipzig. This was the only one of the five not to appear in Prague and possibly its publication in Leipzig contributed to its receiving at least two

reviews in the contemporary journals. It is impossible to be certain, but it seems likely that this was written before 1816 and perhaps before RB was written, but published after the publication of RB. On A589;DP, 76 we read, "The foregoing work [i. c. DP] had been ready for printing for a long time when the paper of Dr. A. L. Crelle . . . . appeared, . . . . 1816, . . . .". In all the Prefaces of his five works Bolzano follows a methodical and characteristic pattern of tracing the previous work on the particular problem concerned, showing its inadequacy and outlining his own procedure. In RB this is followed, rather conspicuously, by a personal section mentioning all his other four mathematical works and his hopes for recognition and criticism (A451-456;RB, 23-29). It is plausible, but only a conjecture, that this section was added, perhaps by way of advertisement for his other works, when he was assured of publication in the Abhandlungen. . . It is in this section that he says DP is still waiting for a publisher. This is the only evidence we have, and it is clearly far from conclusive even if this conjecture were true, that DP was published after the first appearance of RB.

Each of the five works, except for BL, has had a second edition produced during this century. These editions are as follows.

BG and DP both appear in Vol. 5 of Spisy Bernard<sup>2</sup>a Bolzana/Oevres de Bernard Bolzano/Bernard Bolzano's Schriften (Bolzano [3]) edited with notes by Dr. Jan Vojtěch, Prague 1948; BG is on pp. 5-49 and DP is on pp. 67-138. It should be noted that both these second editions, although much clearer and easier to read than the first editions, contain errors or misprints which do not occur in the respective first editions. In the case of BG this is

compensated by the greater number of first edition errors that are corrected by Vojtěch. The first edition of DP has very few errors and is more reliable than Vojtěch's edition. However, these are almost all fairly minor misprints which are easily detectable. Vojtěch's notes are particularly useful for the details of all the authors mentioned by Bolzano in these works.

BD appeared in a second edition under the title, Philosophie der Mathematik oder Beiträge zu einer begründeteren Darstellung der Mathematik, edited with an introduction and notes by Dr. Heinrich Fels. This was published by Ferdinand Schöningh in Paderborn in 1926. There is also an unaltered reprint of the first edition, with a new introduction by Dr. Hans Wussing, published by Wissenschaftliche Buchgesellschaft in Darmstadt in 1974.

The second edition of RB appeared in the series Ostwalds Klassiker der exakten Wissenschaften Nr. 153 published in Leipzig in 1905. It was edited and supplied with notes by P. E. B. Jourdain. Again, although clearly laid out, there are more misprints introduced in this edition than those corrected from the first edition. RB has been translated into French by J. Sebestik in Bernard Bolzano et son Mémoire sur le théorème fondamental de l'Analyse in Revue d'histoire des Sciences 17 (1964) p. 129.

The author's translation of RB which is given here on A430-489 has been published in Historia Mathematica Vol. 7(2) (1980) pp. 156-185. There have also been translations in Czechoslovakian and Russian for details of which see Bolzano[1] Vol. E2/1p.86.

#### 1.4 Reviews of Bolzano's Works

We shall list here the references to the contemporary reviews that we know about, together with brief summaries of their contents.

##### For BG:

- (1) Neue Leipziger Literaturzeitung Dritter Band 95. Stück  
(1805)

This is just half a page summarising the contents of BG with very little comment from the reviewer.

- (2) Allgemeine Literatur-Zeitung (Halle and Leipzig) Erster  
Band Nr. 26 Feb. 1806.

A very brief and rather patronising review which is unfair in that it fails to indicate the contents of the work and just states the reviewer's lack of sympathy for studying the basic concepts of any science. He disagrees with Bolzano that motion is alien to geometry since it need not presuppose an empirical object.

- (3) Heidelbergsche Jahrbücher der Literatur Erster Jahrgang  
Vierte Abtheilung pp. 156-158. Heidelberg 1808.

The reviewer gives a brief but fair summary of Bolzano's

distinctive approach to elementary geometry but then devotes half the review to the difference between theorems and problems. He criticises Bolzano's classification of theoretical and practical geometry and his claim that all the problems in Euclid really belong to the latter. The reviewer wrongly regards Bolzano's proof of Pythagoras' theorem (by similarity) as original and fails to see the errors in what he describes as the "very easy" theory of parallel lines which Bolzano gives. He commends the work as deserving attention and further study.

For BD:

Revision der Literatur Zweyter Theil Vierter Band p. 313

Heidelberg 1810

This is only one small page and does no justice to Bolzano's work. The reviewer quotes the definition of mathematics and some of the subsequent classifications and explains why he believes Bolzano has misunderstood Kant's distinction between intuition and concept.

For BL and RB:

No reviews known.

For DP:

- (1) Allgemeine Literatur-Zeitung Band 3 Col. 180-184

Jena 1819

This is a fairly detailed summary of the method Bolzano adopts to prove the formula for the rectification of a simple curve. The reviewer's account closely follows Bolzano's own summary in the Preface of DP. However, the review concludes by denying, with virtually no argument, Bolzano's two principle claims for the work that he avoids the use of infinitesimals and that it is a perfectly strict proof.

- (2) Leipziger Literatur-Zeitung No. 175, 176 Col. 1392-1403

1822

This review is substantial but erratic in style and most unfavourable to Bolzano. It begins with a faithful summary of the first part of the Preface of DP but then proceeds to deal at length (and with obvious sarcasm) with the geometrical definitions which are really incidental to the main purpose of the work. Although there are several lengthy verbatim quotations from DP these are mixed up with the reviewer's own summaries without any indication of when a passage is actually quoted from DP. Some of the criticisms of the procedure Bolzano adopts for the



main proofs are fair and useful but the sweeping conclusions which deny the paper any mathematical value at all are quite unjustified and represent either a wilful attack or else a complete failure to appreciate Bolzano's purpose. Certainly this review could have damaged any mathematical reputation or attention Bolzano might have been gaining from his recent publications.

## Chapter 2 : Foundations of Mathematics

### 2.1 Introduction

At the opening of the nineteenth century in Europe it was normal for anyone pursuing an academic interest in mathematics or philosophy to be quite well acquainted with the methods and achievements of both these fields of knowledge. Philosophers have generally held mathematics and its methods in high regard: both as a model of argument and as a body of knowledge of a kind which merits particular attention. And in the wake of the rationalist philosophers and of Kant's Critique of Pure Reason this was particularly true when Bolzano was studying mathematics and philosophy at the University of Prague. However, mathematics was certainly not free from criticism. Bishop Berkeley's attack (The Analyst of 1734) on the use of fluxions and their ratios in the calculus was fully justified. Although Euler, Lagrange and the Bernoullis (among others) were producing a vast amount of mathematics throughout the eighteenth century it was the widespread, successful applications of mathematics (in mechanics, astronomy, military and civil "engineering") which maintained its high reputation and sense of progress, and which justified its methods. In the second half

of the century there were only a few significant new branches of mathematics emerging: the study of differential equations, differential geometry, descriptive geometry and the calculus of variations. Also in this period there were a number of disquietingly pessimistic remarks being made about the future of mathematics. Lagrange wrote to d'Alembert in 1781, "It appears to me that also the mine [of mathematics] is already very deep and that unless one discovers new veins it will be necessary sooner or later to abandon it." (Lagrange [1] p. 368) In a report on the progress of mathematics since 1789 Delambre, secretary of the mathematics section of the Institut de France, was able to write in 1810,

It would be difficult and rash to analyse the chances which the future offers to the advancement of mathematics; in almost all its branches one is blocked by insurmountable difficulties; perfection of detail seems to be the only thing which remains to be done. (Delambre [1], translation as in Kline [1] p. 623)

Whether establishing secure foundations for mathematics was a matter of the "perfection of detail" has doubtless always been a matter of debate among mathematicians. But in the last two centuries there are many examples of what were foundational studies for one generation becoming a well established branch of mathematics for the next generation. Around 1800 the status of Euclid's parallel postulate, the summation of infinite series and the nature of the continuity of a function were among

many "details" which had still not been properly clarified. (It is salutary to consider with hindsight the power and fruitfulness that such fundamental clarifications would unleash in the nineteenth century.) Now the mathematical and philosophical aspects of these problems could really not be separated.

As long as there was no notion of primitive and undefined concepts governed only by an axiom system, the clarification and definition of the fundamental concepts (such as line, surface, solid, the nature of infinity or of an infinite sum), was something which had to be done whether it looks to us now like mathematics or not. Consequently the philosophical remarks in a mathematical work of this period may not always be regarded merely as customary reflections, or speculations, on the mathematics. They may, on occasion, be no more than that, but on the other hand they may be intended as an integral part of the work and essential both for understanding the general development of the theory and for following particular proofs. This applies very much to Bolzano's work because as we have seen from Chapter 1 he was especially interested in this philosophical aspect of mathematics and believed that it was defects in this area that were the source of the difficulties and confusion in the geometry and analysis of his time. His overall purpose was to render mathematical theories clear and correct. This was seen as a worth-while work in itself, an excellent exercise for the mind which would be of great benefit for those learning mathematics and as facilitating the further development of the subject.

To achieve this aim of clarity and correctness Bolzano embarks in the first period of his career on working out a programme of the systematic analysis of proofs and concepts. In all the five published works with which we are concerned here this analysis begins with the criticism of all the previous work on a particular problem with which Bolzano was acquainted. This criticism is usually against method, e. g. the choice and definition of the concepts used or the arrangement and status of the theorems, rather than against defects in logical deduction. It generally leads to a refinement and development of the relevant concepts, such as those of angle and congruence in BG (see 3.2.1,3 ), or of continuity and convergence in BL and RB (see 4.3 and 4.4). Sometimes the analysis of concepts seems to lead Bolzano to entirely new definitions, for example his distinction of distance and length and the neighbourhood definitions of line, surface and solid in DP (see 3.4.2).

This programme of conceptual refinement and enrichment is explained and applied in general terms in BD. Here Bolzano discusses the nature of mathematics itself, its classifications and concepts, and the general requirements for a correct proof. It is therefore the first published work devoted to what we should now describe as foundational aspects of mathematics. Some of the main ideas of BD have already been expressed (albeit more briefly and in application to geometry) in the earlier work BG. Thus it will be these two works which are our main sources for describing and assessing Bolzano's views on the foundations of mathematics. The particular

concepts which arose in the light of these views in geometry and analysis are only mentioned here by way of example and illustration; the details and difficulties of their use are discussed in the appropriate later chapter.

## 2.2 The Nature of Mathematics

### 2.2.1 The Science of Quantity

Although we might describe the leading mathematicians of the eighteenth century as analysts, the majority of those learning mathematics at this time would have regarded pure mathematics as more or less identical with geometry. We say here "pure mathematics" because it was normal in the mathematics textbooks to include subjects such as mechanics, optics, astronomy, geography, surveying, navigation, hydrodynamics, chronology, gnomonics etc. There would be a little arithmetic and algebra in the pure mathematics, but the great contemporary adventures in analysis would be incomprehensible to all but the most devoted scholars. It was therefore geometry that was the cornerstone of mathematics and most "applied mathematics" was really applied geometry. In France mathematicians were generally referred to, even in the nineteenth century, as "les géomètres".

Geometry had for long been regarded as the "science of magnitude" (Mercator [1] 1678) or of "extended quantity" (Wolff [1] 1713, Ch. 1 Def. 1). Thus it was natural for the mathematicians who reflected on the nature of their subject to believe that all its various divisions would be covered by the general definition of mathematics as the "science of quantity" or "science of quantities". For example, J. Schultz (an author to whom Bolzano made frequent reference, see p. 87) gives the definition:

Die Mathesis oder Mathematik heisst die Wissenschaft der

Größen (scientia quantorum).

(The science of quantities (scientia quantorum) is called  
mathesis or mathematics.) (Schultz [1] p.2)

One of the principal German textbooks was a collection of ten volumes under the general title, Die Mathematische Anfangsgründe by A. G. Kästner, the first volume of which went through six editions between 1758 and 1801. Among the "Vorinnerungen" (Preliminaries) of this first volume we read:

Die Mathematik enthält eigentlich solche Lehren, vermitteltst  
derer die Größen sich durch Schlüsse vergleichen lassen...

(Mathematics properly contains those theories by means of which  
quantities can be calculated by deductions.) (Kästner [1] p.3)

By far the most popular university text in French was Bézout's Cours de mathématique (1764 with many later editions) which opens as follows:

1. On appelle, en général, quantité, tout ce qui est susceptible  
d'augmentation ou de diminution.... Tout ce qui est quantité est  
de l'objet des Mathématiques...

(1. In general, everything that is capable of increase or decrease  
is called quantity.... Everything that is quantity is the object of  
mathematics...) (Bézout [1] p.1)

In his survey of algebra in the period 1758 - 1799 F. Cajori writes with regard to this kind of definition of mathematics that, "In no textbook



do we meet with an essentially different definition... it was almost universal with mathematicians"(M.Cantor [1] p. 76).

It was thus natural that Bolzano should begin his consideration of the nature of mathematics with criticism of this definition. He concentrates on what is to be understood by the word "quantity". The distinction of discrete and continuous quantities, of what is countable and what is in some sense measurable, had already been made frequently. For example, in The Mathematical Dictionary of Thomas Walter (1762) mathematics is defined as, "that science which considers magnitudes either as they are computable or measurable" (Walter [1] ). However, Bolzano does not discuss this distinction nor do most of the authors from which he quotes or with which he was familiar. With A. G. Kästner (and, as we have seen, Bézout) quantity is defined abstractly as, "whatever is capable of increase or decrease" (Kästner [1]p.1). In his Anfangsgründe J. Schultz first defines quantity as a kind of predicate of an object, "The determination of how many times an object must be combined with itself in order to produce a similar object is called the magnitude or quantity" (Schultz [1] p.2). Then he continues, "An object in which quantity occurs is called a magnitude or quantity (Quantum)." In contrast to such abstract definitions there is the definition quoted by Bolzano from an anonymous work, "A quantity is something that exists and can be perceived through some sense" (Anon.[1] ). Bolzano himself says he appeals to the ordinary use of language for his definition:

"a whole insofar as it consists of several equal parts, or still more generally, something which can be determined by numbers" (A111;BD, 4). This brings out both the substantive and predicative aspects of the concept of quantity. It is not a predicate but, by directing our attention to a certain view-point, it makes way for a predicate. We should not speak of a "larger or smaller quantity", of a "known or unknown quantity" (in English or German) unless we had first used and understood these expressions with reference to a quantity of something. On the other hand, we are also in the habit of using such expressions abstractly, as though "something" actually was "determined by numbers". Bolzano finds fault with the definition of mathematics as a "science of quantity" on both of these interpretations of "quantity". Quantity, he says is considered

in abstracto in pure general mathesis, i.e. logistics or arithmetic but it does not exhaust the content of even this science. The concept of quantity or of number does not even appear in many problems of the theory of combinations. . . For example, if one puts the question: which permutations - not how many - of the given things a, b, c, . . . are possible?" (A111;BD, 4; on the terms "mathesis" and "logistics" see Notes [9], [10] on A261, A262).

Furthermore, in the applied parts of mathematics which consist of the application of quantity, for example to time and space, there will be axioms or theorems which concern only the object of the theory. For

example, there are the propositions that all moments, or that all points, are similar. Perhaps then, what is really meant, or should be meant, by the "science of quantity" is, "the science of those objects to which the concept of quantity is especially applicable" (A113;DD, 6). But of course this is hardly satisfactory. If the criterion is simply that quantity can be applied to an object for the theory of that object to be part of mathematics, then almost everything becomes mathematics. If it is that such applicability should be frequent and occur in many ways the criterion is subjective and vague.

So far, Bolzano's criticisms are very reasonable and even understated with respect to 18th century mathematics. The "science of quantity" definition can never in fact have been a successful way to delineate the domain of mathematics. (Cajori remarks on the non-quantitative Greek problem of determining whether four given points lie in a plane (McCantor [1] p. 76). It is much more like a rough working definition supplying what is given by etymology in the case of subjects like philosophy, theology and metaphysics, but is lacking in the case of mathematics.

### 2.2.2 Bolzano and Kant

It has in the past often been regarded as a proper task of philosophy to determine and distinguish the true subject-matter of the particular

branches of knowledge. Bolzano certainly believed this can and should be done not only for mathematics (and other subjects) as a whole but also for the classification of its various parts. He quotes approvingly Kant's criticism on the contemporary custom of distinguishing mathematics from philosophy by making their objects quantity and quality respectively: "by this the effect is mistaken for the cause" (A109;BD, 2). Bolzano thus maintains that it is not part of the essence of mathematics that it deals with quantity, this is merely a consequence of its true nature. Although this is as far as Bolzano's approval of Kant goes on this matter he adds an Appendix to BD (A242;BD, 134) which is devoted to Kant's account of mathematics. However, it appears from this more detailed study that Bolzano fundamentally misunderstood Kant, so we shall now give a brief outline of Kant's definition of mathematics. Our purpose here is not to describe the full extent of Bolzano's misunderstanding or account for its reasons but only to indicate the specific influence Kant had on Bolzano's thought on the nature of mathematics.

Kant's account of the nature of mathematics is a highly specialised account in terms of the epistemology extensively elaborated in the Critique of Pure Reason. Mathematics is said to be the science of the construction of concepts. To construct a concept means to exhibit a priori the intuition which corresponds to the concept. Intuitions arise through our sensibility and are characterised by their immediacy in relation to their object. They may be empirical (arising through sensation) or pure and a priori

(representing the form of our sensibility). Concepts, on the other hand, arise from the understanding and are things which are thought. Intuitions and concepts constitute the elements of all our knowledge, so that neither concepts without intuitions, in some way corresponding to them, nor intuitions without concepts, can yield knowledge. Both may be either pure or empirical. Now Kant claims that mathematics and philosophy are the two fields where reason achieves genuine synthetic a priori knowledge and his main concern (in the passages to which Bolzano refers in BD, I, §1, §5 (A109) is to distinguish these fields of knowledge from one another rather than to give a complete definition of either. The distinction does not so much lie in their objects (these overlap to some extent) but exists by virtue of "the mode in which reason handles that object" (Kant [1] p. 578). Philosophy confines itself to universal concepts; mathematics proceeds essentially to intuition in which it considers the concept in concreto, "in an intuition which it presents a priori, that is, which it has constructed, and in which whatever follows from the universal conditions of the construction must be universally valid of the object of the concept thus constructed" (Kant [1] p. 578).

Bolzano rejects the Kantian definition of mathematics on the grounds that he believes the concept of a pure a priori intuition is contradictory (A116; BD, 9). But he nowhere explains why he believes this. It might be reasonable to doubt the existence of pure intuitions in Kant's

sense (he himself, of course, argued that there are two such, space and time), but to claim that they are contradictory suggests either a wilful misinterpretation or only a cursory study of Kant's work. It is extraordinary that in BD Appendix §2 (A245) Bolzano writes:

If we then ask what a pure intuition is meant to be, then it seems to me at least that no other answer is possible than: an intuition which is combined with the necessity that it must be so and not otherwise.

This is an invention of Bolzano's and it does not, as suggested, arise from an omission on Kant's part. On the contrary, Kant repeatedly offers in the Critique of Pure Reason the explanation that a pure intuition is the form of our sensibility by virtue of which appearances (i. e. empirical intuitions arising through sensation) can be ordered in certain relations, for example, in spatial and temporal relations (Kant [1] p. 66, 67, 92). However, the source of Bolzano's misunderstanding in this respect seems to be in the basic idea of an intuition. Again in BD Appendix §2, (A245), he answers the question of what intuitions are by quoting the distinction made in the Logik (Kant [2] (1800)p. 96) between intuitions and concepts; these are contrasted as singular ideas (repraesentio singularis) and general or discursive ideas (repraesentio discursiva). This is simply a logical distinction with respect to quantity and, though it may also be found implicitly in the other main passages describing intuitions and concepts in the Critique of Pure

Reason, it is completely inadequate as a proper characterisation of these central kinds of ideas. A contemporary reviewer of BD (see 1.4 for reference) made the point (perhaps with considerable restraint) that Bolzano would not have mistaken the intuitive nature of mathematical knowledge if he had considered not just one of Kant's explanations of the difference between intuition and concept but had taken all his references on this matter into account. Indeed to interpret an intuition simply as a singular idea or an idea of an individual is fraught with confusion. This is explained with specific reference to mathematics by Hartmann and Schwarz in the Introduction to their translation of the Logik. They write as follows:

In general the uniqueness or singularity of the object of a construction has nothing whatsoever to do with the singular or particular aspect of an abstracted or analytic concept; . . . .

It is thus false to say, as it is sometimes done, that the individual in a mathematical intuition has anything to do with the individual of an abstract concept, . . . or that, 'in a mathematical argument general concepts are considered by means of their representatives.' For what Kant does say is that to construct a concept is the same as to exhibit, that is, show up in space and time, darstellen, a priori an intuition which corresponds to the concept; that this is the construction of a schema. . . . (Kant [2] n. ci. ).

It seems to have been exactly the confusion referred to here that Bolzano makes when he disputes that the certainty of mathematical knowledge can possibly be based on intuitions. He writes in BD Appendix §7 (A251):

Kant seems to want to say, If I combine the general concept, e. g. of a point, or of a direction or distance, with an intuition, i. e. present to myself a single point, a single direction or distance, then I find that this or that predicate belongs to these single objects and feel at the same time that this is also the case with all other objects which belong under this concept.

Bolzano then claims that this feeling cannot arise from what is single and individual in the object (the intuition) but from what is general in it (the concept). Possibly this is why he claims at BD, I§6 (A116) that the concept of a pure a priori intuition is contradictory: that it purports to give universal and necessary knowledge on the basis of an individual. However, it is sufficiently proved that this is based on an inadequate understanding of Kant's notion of intuition from the quotation from the Critique of Pure Reason given on p. 40 and from the quotation of Hartmann and Schwarz above.

In spite of this somewhat cavalier treatment of Kant, it is clear that in composing BD Bolzano was often influenced by him and that this was not always in terms of opposition. The two examples which are most relevant here are the problem of explaining the certainty of mathematical knowledge and the task of correctly separating mathematics from philosophy. It is



quite clear even in the earlier work BG that for Bolzano the purpose or existence of a proof of a proposition is not to assure us of the certainty of that proposition. We do not (or should not) become any more sure of the truth of a proposition the "better" it is proved. There are many elementary propositions for which we are certain of their truth before we consider how, if at all, they may be proved. It is reasonable to expect some explanation of such unusual certainty in our knowledge. Kant had this explanation available in the a priori nature of the intuitions used in the construction of concepts in mathematics. But since this had been misunderstood and rejected by Bolzano he had to seek elsewhere. Instead of resorting, like Descartes, to the clarity and distinctness of mathematical ideas, he appeals to our (supposed) capacity to test mathematics. He says that certainty and obviousness arise

because one can very easily test the results of mathematics by intuition and experience. For example, that the straight line really is the shortest one between two points is proved by everybody by innumerable experiments a long time before we can prove it by deductions. Also the well-known obviousness of mathematics gradually disappears where the experience is lacking. (A257;BD, 150).

This makes it sound as though there is no difference in kind between mathematical knowledge and that of the natural sciences, that it is just a matter of degree and that mathematics has been particularly well confirmed. The certainty of pure or abstract mathematics seems to derive from its

applications or "results" being confirmed. But it would be an anachronism to make Bolzano a thorough-going inductive empiricist. He did not believe the truth of a mathematical proposition is established by any sort of induction, though our confidence in such a proposition may be strengthened by widespread and frequent confirmation. Nor did he regard the "experience" by which mathematical results can be tested to be confined to sense experience. He speaks of "intuition and experience" and by "intuition" Bolzano means (at this stage in his thought, see A255;BD, 148) something like a mental image (albeit of an individual) which need not have its origin in sensation. In principle an a priori "thought experiment" could falsify a mathematical result .

Bolzano agrees with Kant that mathematics and philosophy (or, for Bolzano metaphysics) are the "two main parts of our a priori knowledge" (A120;BD, 13). It is therefore important in defining mathematics to distinguish it as carefully as possible from philosophy. In doing so Bolzano is led to develop his definition into an original and logical characterisation of mathematics which is the forerunner of modern views on mathematics. We shall now consider Bolzano's own definition of mathematics which he presents and discusses in BD, §§7-10(A117-123).

### 2.2.3 Bolzano's Definition of Mathematics

Bolzano claims that the main outline of his definition and the subsequent classification of mathematics is original but he acknowledges the influence of a review which he quotes from as follows:

Quantity is only an object of mathematics because it is the most general form, to be finite, but in its nature mathematics is a general theory of forms. Thus, for example, is arithmetic, insofar as it considers quantity as the general form of finite things; geometry insofar as it considers space as the general form of Nature; the theory of time insofar as it considers the general theory of forces; the theory of motion insofar as it considers the general form of forces acting in space. (A117;BD,10)

(The reference Bolzano gives for this review is "Leipz. Litteratur-Zeitung (1808 Jul. St. 81)" but the title of the journal should be, Neue Leipziger Litteratur-Zeitung. It is a vigorous and interesting review but there is no indication of the identity of the author.) Bolzano's definition of mathematics is: "a science which deals with the general laws (forms) according to which things are regulated in their existence" (A118;BD,11). By "things" he includes anything "which can be an object of our perception" and he explains in the same passage that this refers to intuitions and concepts as well as things with "objective existence". In speaking of "the laws regulating their existence" he means to indicate that it is not for mathematics to prove

the actual existence of anything, it is only concerned with "the conditions of the possibility" (A119;BD, 12) of anything. Such laws are general because they always concern whole classes of things and never particular individuals. Thus there emerges here, with the reviewer quoted above and with Bolzano, possibly the first printed statement of something like the modern conception of mathematics as being primarily concerned with structure, with the most general features that hold for classes of possible entities.

It is too wide a definition because although recognising that mathematics cannot be confined to quantity Bolzano has admitted, quite deliberately, subjects (such as the theory of causation) which have never been generally accepted as part of mathematics. We must agree with Menninger that this concept of mathematics takes the subject "into the areas of philosophy, metaphysics or epistemology according to our interpretation of the expression 'conditions of the possibility'." (Menninger [1] p. 8). The weakness of the definition, and indeed his whole philosophy of mathematics is traced by Menninger to two main sources (ibid pp. 7-15). Firstly, Menninger says that the boundaries of mathematics can only be seen clearly and defined from within mathematics itself; Bolzano's view and definition of mathematics were "from outside". This is obscure. Secondly, he claims it is the lack of attention on Bolzano's part to any proper consideration of epistemology that leads to his various errors. This is surely a sound and important criticism. Bolzano avoids most of the problems Kant is seeking

to answer in his Critique of Pure Reason by assuming our knowledge a priori of conceptual truths which describe the logical structure governing the reality of things outside us. However, Bolzano was not purporting to write a treatise on philosophy in BD and Menninger is wrong to attribute to him the idea that our knowledge of these conceptual truths is "beyond question".

The task of proving, from a priori concepts the real existence of certain objects is relegated by Bolzano to metaphysics. He makes the distinction clear:

mathematics concerns itself with the question, how must things be made in order that they should be possible? Metaphysics raises the question, which things are real - and indeed (because it is to be answered a priori) - necessarily real? Or still more briefly, mathematics would deal with hypothetical necessity, metaphysics with absolute necessity (A121;BD, 14).

It is explained that this hypothetical form is not always conspicuous because the premisses, or conditions, are often common to the whole subject - for example in geometry - and they are "tacitly assumed". Here then is a logical counterpart of the "structural" definition of mathematics. We are reminded of Benjamin Peirce's definition, "mathematics is the science which draws necessary conclusions" (Peirce [1]), and Russell's definition, "pure mathematics is the class of all propositions of the form, 'p implies q'." (Russell [1] )

Ironically Bolzano now includes Kant among those whose views support and confirm this account of mathematics. He quotes Kant's definition of pure natural science as a science of the laws which govern the existence of things (phenomena); and claims that this easily leads to his definition of mathematics. The point is that pure natural science is virtually mechanics, or at least, as Kant used to call it, "pure mechanics", and this is a part of mathematics. Now if we treat the other parts of mathematics as Kant here treats mechanics, the general features of the resulting definitions are summed up in Bolzano's first definition, thus:

Time and space are also two conditions which govern the existence of appearances, chronometry and geometry which consider the properties of these two forms in abstracto deal likewise, though only indirectly, with the laws which govern the existence of things (i. e. things perceivable by the senses). Finally arithmetic, which deals with the laws of countability, thereby develops the most general laws according to which things must be regulated in their existence, even in their ideal existence. (A122;BD, 15).

### 2.3 The Classification of Mathematics

As the subject has developed the classification of the various branches of mathematics has become increasingly difficult and intractable and yet it is not regarded today as a serious problem, except possibly by librarians and publishers. It is a matter of practical, rather than theoretical, significance. However, for Bolzano and many of his contemporaries such as Kästner and Schultz it was a central issue. For they regarded it as the business of science, and mathematics in particular, not only to discover new truths but to arrange them in a "true and natural order". This classification was not simply a matter of organising existing mathematical theories, rather it was to represent faithfully the true divisions of mathematics. A definition which is intended to express the essential nature of mathematics now assumes an important and determinative role in that it can be used to produce a sort of "ideal" or theoretical classification with which existing theories should match, at least approximately.

The obvious response to an attempted definition of mathematics is to look at various branches of mathematics and their theories to see in what way, and how closely, they conform to the given definition. This is what the reviewer quoted above (from BD, I, §7(A117)) attempts to do, albeit in a very rough and ready way. It is an approach which starts from the actual parts of mathematics and compares them with the definition. Bolzano's approach is in the opposite direction; starting from the general

definition he applies subsidiary concepts and logical processes to descend to what the particular branches of mathematics should be.

He first distinguishes between classifications which are made on "a scientific basis" and those which are simply practical or conventional. Also there is the distinction to be made between a classification of mathematics as a whole and the classification of a particular discipline such as geometry. It is with the latter in mind that he explains in BD, II, §9(A120) what he means by a "proper classification", but there is no reason to suppose that there is any difference in principle between the two kinds of classification. He claims that every genuine classification is a dichotomy. So for a classification of a concept A we require some concept B which can be consistently adjoined to, or excluded from, A to produce a classification of the form [(A cum B), (A sine B)].

The use of classification is one means to aid the achievement of Bolzano's ideal of each part of mathematics being presented in its correct order. He remarks that this is particularly difficult because for a proper classification it is necessary to be clear about the simple concepts and axioms for each part of mathematics.

To obtain the first two main divisions of mathematics Bolzano does proceed exactly as described above by means of dichotomies. However, this is not made very clear in his account in BD, I, §11-13(A123-129) and so we shall summarise it here from this point of view.



Mathematics is the science of the laws which govern the existence of things. The laws which apply to all things of whatever sort will form general mathesis (and include arithmetic, theory of combinations etc.). Those laws which apply only to some things will then be gathered together into appropriate classes according to the kinds of things to which they apply. These theories are parts of particular mathesis and are all subordinate (as species to genus) to the general mathesis. The next division is between things which are necessary in their existence or being and those that are not (i. e. those which are free and not subject to the laws of causality). The latter sort of thing produces no new parts of mathematics because they are subject to no laws except the most general, e. g. concerning number, which are already included in the general mathesis. The things which are necessary may be so simply and in themselves (i. e. God, a subject of metaphysics), or conditionally, presupposing something else (e. g. the speed of a moving body). This conditional, or hypothetical necessity is the occasion for the introduction of Bolzano's very general concept of a "ground" (Grund). A cause (Ursache) is a ground which acts in time. The objective relationship of ground and consequence (Grund und Folge) is one holding between (timeless) propositions. (See later 2.4.2.) The general conditions governing the becoming or being of everything which is produced through some ground is the first part of particular mathesis and is called the theory of grounds or aetiology. (See Bolzano [1] Vol. 2A/5 (1977) for the previously

unpublished work on this part of mathematics which Bolzano intended for the second instalment of BD.)

At this stage Bolzano breaks off the sequence of dichotomies concerning things considered objectively and introduces the notion of our perception of things. Anything we perceive as real is perceived in time and if it is also perceived as outside ourselves it is perceived in space. Time and space are conditions governing all appearances of things, and therefore, says Bolzano, indirectly governing things themselves. Their properties when they are considered in abstracto produce the second and third parts of particular mathematics, chronometry and geometry. When considered not in abstracto but as containing actual things their properties give rise to the theory of causes (temporal aetiology) and mechanics respectively. The resulting classification appears in a table on BD, 37(A144) which may be summarised as follows. We have added the subjects corresponding to each division in square brackets.

- |    |  |  |
|----|--|--|
| A. | General mathesis (things in general)                       | [arithmetic, combinations,<br>algebra, analysis] |
| B. | Particular mathesis (particular things)                    |  |
|    | I. Aetiology (necessary things)                            | [probability]                                    |
|    | II. (necessary things perceivable by the<br>senses)        |  |
|    | a. (their form in abstracto)                               |  |
|    | $\alpha$ .theory of time $\beta$ .theory of space          | [geometry]                                       |
|    | b. (things perceivable by the sense in concreto)           |  |
|    | $\alpha$ .temporal aetiology $\beta$ .pure natural science | [mechanics]                                      |

The introduction of time and space as the forms or conditions of appearances might seem to be taken over from Kant but the origin of introducing time and space in this way goes back at least to Leibniz (for example, in his Metaphysical Foundations of Mathematics, Leibniz [1]). Under general mathesis Bolzano himself only specifies "arithmetic, theory of combinations and several other branches", presumably algebra and analysis should also be included here.

This classification hardly seems to have been a great success. It fails to distinguish - in the general mathesis - between major areas of mathematics which, while perhaps not distinct are regarded as distinguishable, and it introduces subjects such as aetiology and the theory of time which have not yet contributed very significantly to mathematics. There seems little advantage here and considerable complication, compared with the classification of quantity into discrete and continuous, producing arithmetic and allied subjects from the former and geometry and analysis from the latter. Thus it can hardly be claimed (as Bolzano suggests, A123;BD,16) that his classification of mathematics vindicates his definition.

Finally Bolzano considers various other proposals for classifying mathematics. One of his most interesting suggestions here is that the best procedure to distinguish an elementary mathematics from a higher mathematics might be to include in the latter the theories containing the concept of infinity (whether great or small) or that of a differential. These were,

of course, concepts which were then ill-defined and poorly understood.

Bolzano explains,

If in the future it should be decided that the infinite or the differential is nothing but a symbolic expression just like  $\sqrt{-1}$  and such like, and if also it turns out that the method of proving truths by merely symbolic inventions is a method of proof which (although quite special) is always correct and logically admissible, then I believe it would be most expedient to continue to include the concept of infinity and other equally symbolic concepts in the domain of higher mathematics. Elementary mathesis would then be that which accepts only real concepts or expressions in its exposition - higher mathesis that which also accepts merely symbolic ones. (A137, BD, 30).

Here for the first time a definite procedure is suggested for justifying the inclusion in a mathematical theory of symbols which could not be interpreted as denoting. It bears a striking similarity to Hilbert's programme for dealing with infinite number symbols as ideal elements in theories whose consistency would be proved by means of strictly finite methods.

## 2.4 The Concepts and Proofs of Mathematics

### 2.4.1 Conceptual Correctness

In spite of the prevailing interest in the classification of the various branches of mathematics its effect was to be seen mainly in the organisation of the many branches of applied mathematics. For example, Montucla lists about twenty topics under the general division of "physico-mathematics" in his Histoire des mathématiques (Montucla [1]). Classification in this context was essentially a cataloguing exercise which was carried out successfully when theorems and results could be grouped according to their "object" e.g. optics, astronomy, hydrodynamics etc. In pure mathematics this was not so easy and only geometry could clearly be distinguished from the "arithmetic family". Now Bolzano's chief concern and criticism of the pure mathematics of his time was its disorder and confusion. This was not a matter which simply required a better sorting of results into compartments. Bolzano believed that a mathematical theory was not just a collection of associated theorems, it was the representation of "hypothetical necessity". That is, every theorem or true proposition should be presented with its correct ground, which may itself consist of a finite sequence (or "tree") of ground-consequence relations. Therefore a theory consisted essentially of finite proof-sequences which could be broken off at any point to produce theorems. The disorder and confusion referred to above was in these proof-sequences; concepts and methods from one theory were being employed

in another theory. The obvious and most far-reaching example of this was the use of geometrical ideas in analysis. Certainly with Newton, and then for much of the eighteenth century, a function of one variable was identified with a plane curve. The concept of motion, often used in geometry at this time, produced what might be called a "dynamic" limit concept which did not favour the development of an arithmetic concept of limit. Ever since Euclid many algebraic results had been interpreted, proved and developed in purely geometrical terms. Such confusions could, of course, never occur in a formal system or even such a system consistently interpreted according to given rules. But such an idea was not clear to Bolzano, however much we can now, with hindsight, see it inherent in the notion of "hypothetical necessity". For Bolzano the starting points, or axioms, of the proof-sequences in a theory are the propositions containing simple, but always meaningful, concepts. And they are true by virtue of these meanings. In order to claim that the disorder of proofs was not just an aesthetic desire for the uniformity of proof and conclusion, it was therefore necessary to assume that there are genuine conceptual divisions of knowledge, or of truths, rather like the sharp divisions into species that were believed to exist in the organic world. This assumption was central and essential to Bolzano's early mathematical work. The immediate consequence for mathematics was summed up early in the first work, BG, as follows:

I could not be satisfied with a completely strict proof if it were

not even derived from concepts which the thesis to be proved contained, but rather made use of some fortuitous, alien, intermediate concept [Mittelbegriff], which is always an erroneous μεταβασις εἰς ἄλλο γένος [transition to another genus] (A15, BG, VIII)

There are really two ideas conflated here: that of a correct proof and that of a correct concept, the correctness in each case being relative to a given conclusion or theory. The correctness of a concept therefore depends on its context which is often a proof, and the correctness of a proof may depend on the concepts it involves. We shall therefore discuss these ideas together.

According to Bolzano logical or formal correctness is not the sole criterion of an adequate or correct proof: the concepts involved in the deduction are to be appropriate, in some sense, to the conclusion. For example, with respect to the elementary theory of the triangle and parallel lines the concepts of straight line and direction are appropriate, while those of motion and the plane are deemed inappropriate. By considering these last two examples we can distinguish several ways in which concepts can be inappropriate. First, the concept of motion essentially belongs to a different subject from geometry; it requires the empirical concept of an object which occupies different positions in space and this is alien to the science which only studies space. To employ the idea of motion in a

geometry proof is an example of a *μεταβασις εἰς ἄλλο γένος* (transition to another genus). Bolzano uses the phrase again in RB(A434;RB, 6) when distinguishing within mathematics between geometry and analysis. But in the present case of the concept of motion there is a further, related reason for its rejection in geometry: it is conceptually out of order. We are not here concerned with a deductive or logical ordering of propositions but rather an ordering of concepts whereby, if on analysis of concept A it is found to contain a concept B as an essential component, then B is prior to A. This relationship of containment between concepts is metaphorical and ambiguous, we discuss it further later in this section (p.67). But in the sense evidently intended here the concept of space is prior to that of motion and so the use of motion is not merely out of place in geometry in the sense of being alien, it is strictly circular. Any attempt to prove a geometrical proposition in this way requires a proof of the possibility of a suitable motion and this in turn will depend on the truth of the original geometrical proposition to be proved. Now it may be asked here, why should the proof of the possibility of a certain motion be required? It is not (at least in many cases) that intuitive clarity is lacking, nor even that intuition is inadmissible in geometry; rather it is that where-ever possible it is the mathematician's duty to uncover the basis or ground for every judgement occurring in a proof. In this case, it would be claimed, the true ground for the possibility of the motion lies not in the intuition, but in the geometrical proposition.



Bolzano is primarily concerned in BG to find, as he believes for the first time, the correct proofs for elementary geometry, and it is a necessary condition for such proofs that they employ only concepts which are appropriate to the theorems concerned.

The other concept which is rejected as inappropriate is that of the plane. This may appear surprising when many of the results which Bolzano proves are found gathered together into a subject which has usually been referred to as "plane geometry". It is not, of course, that the plane is not a geometric concept; it is just that it is premature to employ the concept of plane when proving theorems which only concern angles, straight lines, triangles and parallels. The claim this time is not that a logical circularity would be involved in using the notion of plane but that through analysis of these concepts and their development from the simpler to the more complex, the plane comes later than all those others just mentioned. Bolzano seems to have in mind here a kind of hierarchy of concepts which proceeds from the simpler to the more complex and whose structure is reflected in the definitions. Then the principle being used here is that for the proof of a result involving concepts at a certain level in the hierarchy we should not presuppose any concepts from a higher level in the hierarchy. To presuppose a concept in a proof or method means to proceed in any way which requires us to think, or to have, the given concept. For example, the use of Euclid's parallel postulate presupposes the plane and so does the treatment of angle

as a quantity (see 3.2.1 ). It may be noted that while a system of two intersecting straight lines, required for the concept of angle itself, undoubtedly determines a plane this fact is irrelevant to the question of the priority of the concepts. It is unnecessary to have or to use the concept of the plane in order to understand what is meant by the system of lines. There are, for example, many surfaces other than the plane which pass through, though are not determined by, a pair of intersecting lines.

Proofs which ignore this hierarchical principle will, of course, often succeed in the sense that they are logically correct and perfectly convincing, but the point of adhering to this principle is that the proof should then follow and reflect the objective dependence between truths. It is this purpose which is Bolzano's central motive in the early mathematical works.

Thus to summarise what we might call the "principle of conceptual correctness", there are three ways in which the introduction of a concept into a proof may be incorrect. Firstly, it may be of an alien kind not belonging to the subject concerned and not being involved in the conclusion to be proved, e.g. motion in geometry. Secondly, it may be "out of order" and this itself can occur in two ways. The introduction of the concept may lead to a proof which is logically circular (again motion is an example). Or the concept may have been drawn from a higher level than any concepts in the conclusion (according to some hierarchical development of concepts in order of increasing complexity), and thus it would be premature, e.g. the plane in elementary geometry.

### 2.4.2. Ground and Consequence

Now we shall consider in more detail the nature of the "objective dependence" which, according to Bolzano, mathematical proofs should follow and represent. The crucial idea is the relationship of ground and consequence [Grund und Folge] already mentioned briefly in Sec. 2.3. Bolzano does not argue for the existence or need of such a relationship. He writes at the beginning of the second main section of BD:

this much seems to me certain: in the realm of truth, i. e. in the sum total 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 judgements are the grounds of others and the latter are the consequences of the former. To represent this objective connection of judgements, i. e. to choose a set of judgements and arrange them one after another so that a consequence is represented as such and conversely, seems to me the proper purpose to pursue in a scientific exposition. (A146;BD, 39)

Throughout BD Bolzano says very little about the nature of this objective connection between truths. Possibly he thought it obvious and generally acknowledged. In the Wissenschaftslehre of 1837 he remarks in a note (Bolzano[4], Vol. II, §198) that he was confirmed in this view of the existence of a real relationship of consequence between truths because "so

many others had been of the same opinion". He mentions there also the Aristotelian distinction between truths which show that something is the case, and those that show why it is the case. However, the distinction made by Aristotle, and what would be "generally acknowledged", is that we can recognise a difference between a fact and an explanation for that fact. Bolzano's claim regarding an objective connection between truths or judgments seems more like a theory to account for this general recognition, much as we might postulate physical objects to account for certain groups of sensations. It is not at all clear that we have any direct apprehension of such an "objective connection" or "objective dependence" between truths. Furthermore, an important criticism which Bolzano nowhere deals with would be that even what is recognised in the above distinctions of "that" and "why", or a fact and its explanation, is not so much a connection between truths but rather a connection between the circumstances or objects referred to by those truths. The only answer we can give to this is to consider now Bolzano's remarks on the difference between the ground-consequence relation and a cause.

The standard example used in the Wissenschaftslehre to explain the ground-consequence relation is the following. Consider the propositions:

- (1) It is warmer at X than at Y.
- (2) The thermometer is higher at X than at Y.

If we know either (1) or (2) then we also know the other, or at least it is a

basis for knowing [Erkenntnisgrund] the other. But objectively (1) is the ground for the consequence (2) and not conversely. We have already mentioned Bolzano's explanation of a cause as a ground which acts in time. It appears that he intends a ground as a kind of propositional counterpart of a cause in the material realm. Now we are quite accustomed to saying with regard to the above propositions, "(1) is the cause of (2)" when what we really mean is that the circumstance described by (1) is the cause of the circumstance described by (2). There is therefore a kind of derived connection between the propositions (1) and (2) by virtue of the connection between the circumstances to which they refer and which is objective in the same derived sense. It might even be useful to give this secondary connection a name like "ground". However, this was certainly not how Bolzano was thinking. His claim is that the ground-consequence relation is a relation sui generis which holds objectively between truths, i. e. it holds whether or not we happen to recognise it, though in at least some instances we do recognise it. It might be thought that a much better example than that given above by Bolzano in the Wissenschaftslehre would be the connection which we apprehend easily enough between the premisses of a valid syllogism and its conclusion. It is true that this is an example of ground (the two premisses) and consequence (the conclusion), but we now have to be very careful to distinguish this from the formal relationship between propositions of derivability [Ableitbarkeit] which also holds here. The latter consists in the fact that every substitution

for the terms involved which yields true premisses will also yield a true conclusion. The ground-consequence relation is one which is more substantial than the purely formal relation of derivability but is less substantial or "material" than that of a cause. Thus it is hardly something which should have been passed over as being well known or beyond doubt as Bolzano seems to do in BD. However, it must be remembered that in order to gain a better understanding of Bolzano's intention we have used a source (the Wissenschaftslehre) that only appeared twenty-seven years later than BD. At the time of BD, in the draft for the second issue of the Beiträge . . . , Bolzano could do no more to explain the terms "ground" and "consequence" than to declare they were incapable of definition and to state four axioms which they would satisfy (see Bolzano [1] Vol. 2A 5, p. 78).

The relationship of ground to consequence does make more sense in the overall context of the later "an sich" realm: that is, the collection of objective propositions, truths and ideas in themselves [an sich] which is elaborated in the Wissenschaftslehre. There is however no mention of the an sich realm in Bolzano's writing up to 1817. So it is the ground-consequence relation that takes priority in Bolzano's thought and it may have been to give a better account of this relation that the an sich objects were eventually postulated.

### 2.4.3. The Nature of Proof

In describing proofs Bolzano often uses the term "wissenschaftlich" (or "unwissenschaftlich") which we have translated "scientific" in concession to modern usage but it does not imply a special kind of proof for "scientific" statements. It would probably be more accurately rendered "rigorous" or "rational". The nature of a proof is stated very clearly in BD, II, §12(A171):

by the scientific proof of a truth we understand the representation of the objective dependence of it on other truths, i. e. the derivation of it from such truths which are to be considered as the grounds for it - not fortuitously - but in themselves and necessarily, while the truth itself must be considered as the consequence.

Now even if the nature of this dependence is difficult to explain, in order to recognise it and use it, it must be possible to characterise its occurrence among propositions in some way. The question is raised in the same paragraph as before as to, "how many simple, and essentially different, kinds of inference [Schlussarten] there are, i. e. how many ways there are that a truth can be dependent on other truths." This is answered immediately in terms of four purely formal proof patterns and then in later sections there are four conceptual criteria for a correct proof.

The first formal proof structure is the Barbara form of syllogism:

All men are mortal.

Caius is a man.

Therefore, Caius is mortal.

Bolzano remarks that he would prefer to interchange the premisses so that the terms proceeded from the particular to the general in their order of introduction. And he claims, as was quite usual, that every other figure and form of syllogism is either not essentially different from Barbara or else is not simple.

The other kinds of proof which Bolzano lists are given in terms of propositions of the form, "A is (or contains) B". The expression "A contains B" needs some explanation. In a note to BD, II§29(A217) he admits that the statement, "a concept A is contained in another concept B" is ambiguous and can mean that either A or B is the narrower of the two. (The terms "narrower" and wider" are used in their natural extensional sense as applied to concepts) Thus in the active form "A contains B", A is the wider when understood extensionally (the extension of concept A contains the extension of concept B), but A is the narrower if it is interpreted intensionally (the concept B is either part of the meaning of the concept A or as a matter fact belongs to concept A). It is clear from BD. II§26(A208-211) that Bolzano uses the expression in the latter intensional sense. We shall here abbreviate "A is (or contains) B" to "A is B".

The following three schemas are given as valid patterns of proof:

$$\begin{array}{l} \text{(a) } A \text{ is } B \\ \quad A \text{ is } C \\ \hline A \text{ is } (B \text{ et } C) \end{array}$$

$$\begin{array}{l} \text{(b) } A \text{ is } M \\ \quad B \text{ is } M \\ \hline (A \text{ et } B) \text{ is } M \end{array}$$

$$\begin{array}{l} \text{(c) } A \text{ is } M \\ \quad (A \text{ cum } B) \text{ is possible} \\ \hline (A \text{ cum } B) \text{ is } M \end{array}$$



For further clarification of the ground-consequence relation it is worth considering a case where it does not occur. The suggestion of another schema:

A is (B cum C)

A is B

A is C

is rejected by Bolzano because although we can recognise the truth of the two conclusions from the truth of the premiss, the latter cannot objectively be the ground for the former. This is not explained at the time, but in a later section Bolzano says that, "in the proposition, S contains (P cum  $\pi$ ), the proposition, S contains P, is presupposed in such a way that one definitely has to think the latter before the former" (A211;BD, 105). A similar kind of argument would show that in each of the cases (a), (b), (c) above the converse inferences from conclusion to premisses, though possible subjectively, (i. e. as grounds for knowledge) could not be objective. It is evidently regarded as a necessary condition of the ground-consequence relation that it should not be symmetric. All correct proofs are therefore regarded as being formed from combinations of the four simple types of inference listed above.

In BD, II<sup>S</sup>26-29(A208-225) there are four conceptual criteria enunciated which are to apply to all proofs occurring in a "scientific system". In fact the first two do not just apply to proofs but to the general organisation of all

propositions in a theory. They are not original but are put forward because Bolzano regards them as not being sufficiently carefully followed. The first is the principle that one should always proceed from the general to the particular. Or more formally, "If several propositions . . . . have the same predicate, then the proposition with the narrower subject must follow that with a wider subject, and not conversely" (A209;BD, 102). The second criterion (A211-213;BD, 104-106) has already been mentioned and is to the effect that among propositions with the same subject one with a more compound predicate should follow one with a simpler predicate, and not conversely. This can be expressed informally by saying that in a scientific exposition one should prove more, not less, as one proceeds. Here the criterion has deliberately been put in terms of the complexity of the predicate (more compound or simpler) rather than its extension (wider or narrower). It would not work in terms of extension because in the syllogism: S contains M, M contains P, therefore S contains P, M must be narrower than P for otherwise, M contains P could not be true.

The other two rules are specifically for proofs and are still useful today for improving the economy and strength of mathematical theorems.

They are:

1. If the subject of a proposition is as wide as it can be so that the predicate can be applied to it, then all characteristics of the subject must be used in any correct proof (A213-217;BD, 106-110).

2. For an affirmative proposition any intermediate concepts which are introduced, apart from characteristics of the subject, should not be narrower than the subject and not be wider than the predicate. For a negative proposition they should only be wider than the subject or wider than the predicate (A217-255;BD, 110-118).

To illustrate the use and importance of the first of these criteria Bolzano mentions that sometimes a proof is sought without looking for how all the conditions of the conclusion will be used. For example, the parallel postulate only holds if both lines lie in the same plane but, Bolzano adds, few people have considered how this condition is to be used in the proof. And he quotes a proof of Kästner's about the lever which seems to succeed without making use of an essential condition for the conclusion (Kästner [2] I. Abth. (16) and (18)). The proof is therefore clearly false, Bolzano says, because it proves too much.

The second criterion is demonstrated exhaustively by considering each of the four possible simple inferences which could give rise to the conclusion and analysing the relative extents of the intermediate concepts (A217-255;BD, 110-118). Thus it is shown that any proof that is formally correct will also satisfy this conceptual criterion. These forms and criteria for correct proofs are therefore not independent, nor are they sufficient conditions for correctness, but they are necessary conditions and

can easily be used to indicate incorrect proofs. Many proofs ruled out by the principle of conceptual correctness described earlier (p. 61) would also be ruled out by the last-mentioned criterion. For example, an analytic result whose subject would concern all quantities in general should not have a geometric concept, such as a curve, introduced in its proof because this concerns only quantities occurring in space and is therefore narrower than the subject of the conclusion. Generally, though, this sort of diagnosis can only be made after a detailed analysis of the whole proof into its simple inferences and by dividing all its concepts into their simple components. The notion of a simple concept is important so we shall now summarise Bolzano's remarks on it.

#### 2.4.4 Simple Concepts and the Nature of Definition

The distinction of simple and compound concepts is discussed in the context of explaining what a definition should be in BD, II<sup>8</sup>3-5 (A149-157). By a simple concept Bolzano means one which is incapable of further analysis, or as a result, it is one which cannot be produced by combining any two other concepts. A definition is an analysis of a concept, "a statement of the most immediate components out of which a given concept is compounded" (A149;BD, 42). We come to know which concepts are simple and which are compound through the result of our attempt to analyse them.

If when we think of an object we inevitably think of it as compound, then it is so. Thus the concepts of the straight line and the plane cannot be simple - we are immediately aware of the multiplicity of points in these objects and their special arrangement. It is a matter of experience however, that not all concepts are compound, for example the concept of a point is simple.

In his later work Bolzano clearly distinguishes the idea-in-itself (an objective concept existing independently of our thinking it) and the idea which is present in someone's mind at a given time (e. g. Bolzano [4] I, §48). Although it is not clearly formulated here in BD this distinction seems essential to Bolzano's understanding of simple concepts and his subsequent arguments for unique definitions and unique proofs. Simply because we can, or cannot, analyse a given concept into components does not thereby make it compound or simple, this is only how we come to know its nature, and we can make mistakes. Bolzano evidently believes that our apprehension and analysis of concepts will enjoy the same sort of widespread agreement that generally holds for our perception of physical objects. The result of analysing a compound concept is embodied in its definition:

In general if one wishes to ascertain whether a certain concept is simple or divisible then one assumes a genus proximum for it and tries to think of some differentia specifica to add to it which is not itself already identical with the concept to be defined. If this cannot be done in any way, the concept concerned is a simple one. (A155;BD, 48).

Furthermore, if the analysis of a compound concept is continued until all its components are simple then these ultimate components and their relationship in the given compound concept are uniquely determined. "Really nothing is arbitrary in definitions but the word which is chosen for the denotation of the new compound concept." (A159;BD, 52). Thus a correct definition is a true proposition which is not simply about the meanings of words, it describes the structure of a conceptual reality. A mathematical work should not begin with definitions (Euclid is mentioned here as being in error) because they must be seen as introducing "new and genuine concepts" (A160;BD, 53). What needs to come first is the indication of the meaning of the simple concepts of a theory. This is to be done by giving various statements which implicitly define the simple concept by showing its characteristic usage.

For example:

from the propositions: the point is the simple object in space, it is the boundary of a line and itself no part of the line, it has neither extension in length, breadth, nor depth, etc. anyone can derive which concept is denoted by the word "point".

(A162;BD, 55)

To distinguish this way of indicating the meaning of a concept from a proper definition Bolzano calls these statements denotations or descriptions: they may or may not happen to be axioms, they belong to what we should now call the informal metatheory of a particular theory.

#### 2.4.5. Axioms and a Theory of Judgements

It was normal in the eighteenth century to regard as an axiom any proposition which was so intuitively clear or obvious as not to require a proof. Of course, the failure to find a proof would sometimes lead to finding quite obscure propositions "intuitively clear". This attitude was convenient (even if abused) and it was consistent with the view that the purpose of proof was to convince or persuade of the truth of a proposition. Where the truth of a proposition could be recognised immediately from its meaning, a proof would be redundant and to seek one simply pedantic. However, what is regarded as intuitive, obvious or clear is to a large extent subjective and dependent on experience and insight. If a proof is to represent some kind of reality in the structure of concepts or things then its starting point, an axiom, should represent some objectively fundamental state of affairs. This was Bolzano's position. An axiom is not an axiom because we cannot prove it, nor because we see no need to prove it, it is so because it is absolutely unprovable. The point had already been made emphatically at the beginning of BG:

Firstly, I stipulate the rule that the obviousness of a proposition does not absolve me from the obligation still to look for a proof of it, at least until I clearly realise why absolutely no proof could ever be required. (A13;BG, VI)

It is claimed that for every simple concept there is an axiom containing that concept (A196;BD, 89) but the arguments for the existence of axioms in his sense are notably unsuccessful (A176;BD, 69). Instead of saying, as in the medieval theological arguments, that an infinite regress of causes or explanations is impossible (which has a certain plausibility) he says, quite wrongly, that assuming an infinite series does not remove the contradiction that there is in denying a first term to a finite series. He backs this up with some play on the words "ground" and "consequence" and a completely irrelevant allusion to the paradox of Achilles and the tortoise!

The major problem now arises of how we can recognise whether a particular proposition really is unprovable. In order to state the characteristics of an axiom Bolzano first gives, in BD, II, §§14-19(A178-193), a summary of his theory of propositions or judgements. (The word "judgement" [Urteil] begins to be used at §13 where previously "proposition" [Satz] had been used. It is the word Kant used, but in BD, II, §18(A183) Bolzano says a judgement is a proposition which teaches us something new, i. e. a synthetic judgement in Kant's sense.) There is a basic, undefinable act of the mind by which two concepts can be combined to form a compound concept. It is another, different, kind of such act which combines two concepts, a subject and predicate, to yield knowledge in the form of a judgement. In Aristotelian logic this combination was expressed by the copula "is" or "are". Bolzano regards the manner of combining the subject and predicate to be "the most



substantial distinction between judgements" (A180;BD, 73) and accordingly he classifies judgements by means of five possible kinds of combination.

These are:

1. Necessity judgements. This is the inclusion of a thing as individual or kind in a genus. They have the form:
 

"S is a kind of P", or what amounts to the same, "S contains the concept P", or "the concept P belongs to the thing S." (A181;BD, 74). Most mathematical propositions are of this type, an example given is: two lines which cut the arms of an angle in disproportional parts meet when sufficiently produced. This is interpreted as: the concept of two lines which cut the arms of an angle in disproportional parts (=S), is a kind of the concept of two lines which have a point in common (=P).
2. Possibility judgements, with the form: "A can be a kind of B". Bolzano gives the example: "There are equilateral triangles." This is properly expressed, he says, as: "The concept of a triangle (=A) can be a kind of the concept of a figure with equal sides (=B)." (A182;BD, 75).
3. Practical judgements of duty or obligation, with the form: "N should do X."

4. Empirical judgements, with the form: "I perceive X."
5. Probability judgements, for which no examples are given; Bolzano says he is not clear about their proper nature.

The purpose of classifying judgements in terms of five kinds of copula or ways of combining subject with predicate (each presumably corresponding to primitive modes of combination by the mind) is to avoid the necessity of compound concepts in the subject or predicate in, for example, possibility judgements. In BD, II, §20(A193-199) Bolzano shows that all judgements with compound subject or predicate are provable, but since he is convinced that there are unprovable judgements of possibility and obligation (on the grounds of avoiding an infinite regress within each type) he believes it to be more correct to reorganise a proposition like "(A cum B) is possible" with compound subject into, "A can be a kind of B".

The proof given in BD, II, §20(A193-196) is to the effect that if either the subject or predicate of a judgement are compound then its truth will depend on judgements involving the simple components of that compound and will therefore be derivable or provable. This means that no analytic judgement in Kant's sense can be an axiom because the subject must be a compound concept.

We now reach an answer to the question of how we can recognise an axiom. It is a necessary condition that an axiom should contain only simple concepts (A195;BD, 88). It is a sufficient condition for a proposition "A is B",

with A, B simple, to be an axiom that there are no two propositions of the form "A is X" and "X is B" from which it could be inferred. (This essential sufficient condition is over-looked by Bergmann in his detailed discussion of this part of BD, see Bergmann [1] p.165). Now to show that this is the case for any particular proposition will require what Bolzano calls,

a special consideration to which, to distinguish it from a proper proof (or a demonstration) I give the definite name of a derivation (or deduction). Axioms will therefore not be proved, but they will be deduced and these deductions are an essential part of a scientific exposition because without them we should never be certain whether those propositions which are used as axioms really are axioms. (A200;BD, 93)

Unfortunately Bolzano gives no examples (at least in BD) of how these deductions could ever be made.

There is a clear analogy in Bolzano's thought between simple concepts and definable compound concepts on the one hand, and axioms and provable propositions on the other hand. "The domain of the axioms stretches as far as that of the pure simple concepts: where the latter ends and the definitions begin, there also the axioms cease and the theorems begin" (A203;BD, 96). There is an obvious similarity between the role of simple concepts in Bolzano's axioms and primitive concepts in a formal logical theory. However, a primitive concept in the modern sense is a

somewhat arbitrary thing; a simple concept for Bolzano was not arbitrary at all. It was determined, in some sense, by reality and its meaning determined the course of proofs in which it was involved. For an interesting discussion of the relationship between simple and primitive concepts see Bergmann [1] p. 174.

#### 2.4.6. The Uniqueness of Proof

The objectivity throughout Bolzano's approach raises the interesting question of whether there can be essentially different, correct proofs of a theorem. This was first mentioned in analogy with definable concepts:

The question arises here of whether one and the same concept may admit of several definitions. We believe this must be denied in the same way as we deny below (§30) the similar question of whether there are several proofs for one truth.

(A156;BD, 49)

Naturally proofs may differ in the precise order of premisses and even in which premisses are explicitly expressed. These are not "the essential matter" of a proof. The essence of a proof for a certain theorem consists in which judgements the conclusion is based on, in the sense that a consequence is based on its ground. Though there may be several different ways we can come to know a certain truth, there is objectively only one

unique ground for that truth and so essentially only a single unique proof.

(This is the answer given in the discussion beginning on A225;BD, 113)

Yet it has often been characteristic of mathematicians that they have sought to find different proofs of a particular (especially a major) result. The enormous variety of proofs for Pythagoras' theorem or the law of quadratic reciprocity are well known. Sometimes the reasons for producing such different proofs are clear (economy, greater generality, or to avoid some "suspect" principle); sometimes it just seems to be satisfying that a major result may be reached by many different pathways and from intuitively different starting-points. The status of many proofs and what exactly constitutes "different" proofs are still matters of debate.

Bolzano's claim is however quite clear, there are unique grounds, and therefore proofs, for mathematical theorems and so it is the mathematician's duty not only to prove new theorems but also to critically examine established proofs and bring them ever closer to their true pattern, i. e. to correspond exactly with the objective, conceptual reality. This is not just a matter of re-arranging previous proofs: it acts as a programme for new mathematics and was the inspiration and motive behind the works BG and RB. Euclidean geometry needed complete re-organisation and this produced the fruitful refinement and development of the fundamental concepts of distance and equality. Analysis required putting on its own feet rather than continuing to lean on geometric intuitions for support. This

demand was substantially satisfied by the important new clarifications given by Bolzano of the concepts of function, convergence and continuity.

An obvious problem with the idea of an objective ground for all mathematical theorems is the prevalence of the method of reductio ad absurdum or indirect proof. Is there to be an objective, universal contradiction as ground for all such results? Bolzano takes the standard indirect proof, or "apogogic proof" as it used to be called, to be of the following form. To prove that A is B, assume A is not B and derive from this a contradiction with a proposition A is C which has already been proved. Now it is claimed (A230;BD, 123) that for affirmative propositions this indirect method can always be avoided by rearranging the argument thus: whatever is C is always B, A is C so therefore A is B. But of course this analysis breaks down when C is actually the same as B, and why should the contradiction be made with a proposition of the form A is C? In many actual proofs the argument is not of this form nor is it easily reducible to this form. For example, consider the Euclidean proof that a tangent to a circle is perpendicular to the radius. Only negative propositions are regarded as essentially requiring an indirect proof. This is because their direct formulation involves premisses of the form, "what is not M is not N" and Bolzano seems to regard these as peculiarly intractable since they are not the same as, nor derivable from (in the ground-consequence sense) their converse "N is M". It is a major omission in any such objective theory as Bolzano's not to give some special account of negative propositions.

2.4.7. A Note on the Analogy between Concepts and Propositions

There are numerous scattered remarks in BD on the analogy in development between concepts and a certain class of propositions (namely, in a mathematical theory, the axioms and theorems). We gather these together as follows (the paragraph numbers refer to Part II of BD):

<u>Concepts</u>	<u>Propositions</u>
May be simple (undefinable) or compound (definable).  (§4, A150)	There are axioms (unprovable) and theorems (provable).  (§11, A159)
The simple concepts may not be the most vivid or clear ones.  (§8, A161)	The axioms may not be the most obvious or intuitive propositions.  (§21 Note, A200)
Definitions represent the true analysis of a concept into its proper simpler parts.  (§3, A149)	Proofs represent the true pos- ition of a theorem as based on its proper grounds.  (§12, A170)
Definitions are essentially unique.  (§5, A151)	Proofs are essentially unique.  (§30, A225)

## 2.5 The Origins and Significance of Bolzano's Views

In Chapter 1 we referred to the important general influence in the academic world during the eighteenth century of the Leibniz-Wolff school of philosophy. The rigorous and rationalist outlook of this school was by no means universally accepted; it was often the subject of virulent theological attack. However, for one who admired mathematics there was an inevitable attraction in a philosophy which prized this discipline so highly and had been developed by men who in many cases had been very able mathematicians. Rationalism focussed such great attention on mathematics not so much for the sake of the subject itself as for its method. Mathematics was seen to be successfully producing certain knowledge by a method so general that it invited universal application to all areas of knowledge. (See BD, II, §1(A145)). For Wolff the goal of mathematics was the cultivation of the intellect and the preparation of the mind for the study of all other sciences. Each of the great rationalist philosophers modelled at least parts of their philosophy on mathematics. For example, consider Spinoza's Ethics, developed in imitation of Euclid's Elements, and Leibniz's impossibly ambitious plan for a universal characteristic whereby concepts would be associated with characteristic numbers and all argument reduced to computation.

It is hard to point to direct influences on Bolzano's early work from the writings of Leibniz. These writings are notoriously fragmentary with remarks on all sorts of subjects scattered throughout his papers and



correspondence. It is therefore difficult to know to what extent Bolzano had access to these sources. Furthermore, most of the important philosophical work of Leibniz was published posthumously and a substantial amount was still unpublished when Bolzano was writing. There are no references to Leibniz in the five mathematical works up to 1817. (Though there are many such references in the Wissenschaftslehre 1837)

On the other hand, it is clear from the references in BG that Bolzano had read the most important mathematical and philosophical works of Christian Wolff (1679-1754). Wolff was a disciple of Leibniz and had extensive correspondence with him over the last twelve years of Leibniz's life. We shall mention here two themes which were likely to have influenced Bolzano directly. They occur repeatedly in the works of Leibniz and were certainly espoused by Wolff.

Firstly there is the emphasis on logic and foundational studies. If mathematical method is so fruitful then it becomes essential to understand it and its application thoroughly and correctly. Where better to start than in mathematics itself? Thus Leibniz writes:

But it is very important to make explicit all the assumptions which are needed without taking the liberty of accepting them tacitly for granted on the excuse that the thing is self-evident just by an inspection of the diagram or by the contemplation of the idea. In this respect I find that Euclid with all his exactness has sometimes been deficient... (Leibniz [2])

And again on the question of axioms:

The late Roberval planned a new Elements of Geometry in which he was going to demonstrate rigorously several propositions which Euclid took or assumed without proof.... I know that many people ridiculed it, if they had known its importance they would have judged otherwise..... in order to advance the sciences and to pass beyond the columns of Hercules, there is nothing more necessary. (Leibniz [3] ).

Thus mathematics should not only be developed deductively by enlarging the theories from appropriate axioms but the foundations should also be developed by analysing and refining the basic concepts and axioms into absolutely simple forms. This was clearly one of Bolzano's chief aims. We see it in practice in BG where having reorganised elementary geometry on the basis only of the properties of the straight line, he proceeds in the second part to attempt to deduce the theory of the straight line from even simpler concepts such as distance and direction.

Secondly, and related to this theme of foundational study, is the principle of systematically analysing compound concepts into simple concepts on which an entire theory can then be based. To some extent such analysis was, of course, nothing new. It is inherent in the Aristotelian theory of definition in terms of genus and species. What Leibniz brought to the idea was (a) the combinatorial aspect, that all combinations of

concepts are permissible subject only to the resulting compound being possible, i. e. not logically contradictory; and (b) that there are naturally occurring irreducible simple concepts (these are likened to prime numbers, the compound concepts to composite numbers). Both these features are important to Bolzano's account of mathematical method: the first in the systematic hierarchy of concepts presupposed in the principle of conceptual correctness, and in Bolzano's requirement of every defined concept that it should be proved to be possible; the second because all and only simple concepts are components of true axioms.

It is perhaps tempting to suggest a connection between Bolzano's ground-consequence relation and the Leibnizian principle of sufficient reason. They are not, in fact, directly comparable but to maintain any kind of direct influence of the one notion on the other would be misguided. For Bolzano the ground-consequence relation pervades all mathematical and scientific theories. Leibniz specifically says that his principle of sufficient reason is not required in arithmetic or geometry (Leibniz [4]). Then again the principle of sufficient reason depends on the predicate-in-notion principle that even in a contingent truth such as, "Caesar crossed the Rubicon", the predicate "crossed the Rubicon" is actually contained in the complete notion of the subject "Caesar". This is explicitly denied in Bolzano's theory of judgement in BD, II§15(A180-183). The underlying reason for this lack of contact between these notions is a fundamental

difference in the account of mathematical truth. For Bolzano a theorem is true because it represents an objective dependence or state of affairs correctly. For Leibniz it is true because its denial is contradictory.

Wolff developed Leibniz's philosophy into a more coherent and systematic form but there does not seem to be anything in his writing which corresponds clearly to Bolzano's relation of ground-consequence. Perhaps the best we can say is that it was a relationship developed in the spirit of rationalism but original to Bolzano and primarily in response to the peculiarly objective nature of mathematical truth.

Some remarks have already been made (1.2) on three works which it is known that Bolzano studied particularly carefully. These were Baumgarten's Metaphysica (Baumgarten [1]) which he read critically at the age of sixteen and which must have been his first serious introduction to the Leibniz-Wolff philosophy. Then there was Kästner's great compendium of ten volumes Die mathematische Anfangsgründe (Kästner [1]) which Bolzano annotated extensively and which clearly inspired his interest in method and rigour. While agreeing with Kästner's intention Bolzano soon believed he had surpassed him since most of the references in our five works are critical of Kästner for not being sufficiently rigorous. Finally there was the pedagogic work Pařizek [1].

An author whose influence has been rather neglected so far in the literature on Bolzano is Johann Schultz (1739-1805). In some of his works

there are some striking parallels to be found with Bolzano's foundational aims. Schultz was a Professor of mathematics at Königsberg and a friend of Kant. His large work on pure mathematics, the Anfangsgründe der reinen Mathesis (published 1790), was written "in response to the needs of our critical-philosophical age" (Schultz [1] Preface). He is intent on being as systematic, general and rigorous as possible and his model is Euclid's Elements in that definitions are duly followed by axioms, postulates, theorems and problems. We quote here some passages from the Preface:

Therefore I have not treated arithmetic merely as the science of numbers but, as its status requires, as the foundation of all special mathesis, also as a general theory of quantity.

Geometry I have treated according to the strict Euclidean method purified from all alien concepts and presented in its characteristic form.

In a science which is to be completely demonstrative the demonstrations must proceed with as much strictness as possible.

The eye has no voice here. A proposition may already appear very clear in itself but as long as a higher reason [Grund] for its correctness can be conceived this must be sought out and stated. This is not pedantry or empty conceit but a considered necessity because the knowledge of mathematical propositions is not a mere mechanism but actual insight.

.....

It seems to me that great care is required in the correcting of the basic concepts. It is clear how detrimental any confusions in these are for science.

.....

Finally one of my most desirable, and most difficult, efforts has been to present the material in its true natural order.

Pure mathematics has always been criticised because theories of completely different kinds are mixed up with one another and it therefore does not seem capable of any orderly classification. I hope this objection has now been fully removed so making the study of mathematics easier. (Schultz [1] Preface)

Most of the basic aims of Bolzano's foundational work are expressed in these extracts. There is the importance of a proper classification, the fundamental nature of arithmetic, the need to remove alien concepts and in the interests of absolute strictness the need to avoid empirical intuition ("The eye has no voice here."). Then, most typical of Bolzano himself, there is the emphasis on correcting the basic concepts and presenting mathematics in its true natural order.

Instead of developing these ideas theoretically as Bolzano does, Schultz says no more about them and seeks, in the main work, to put them into practice immediately. We do not know when Bolzano first read Schultz's work but it was presumably soon after he entered Prague University in 1796.

His several references in BG to the geometrical work of the Anfangsgründe show he was certainly familiar with it by 1804. In BD, I§5(A116) he says that Schultz, "deserves much credit for the foundation of pure mathematics in his Anfangsgründe".

We have emphasised this work of Schultz not to suggest that Bolzano borrowed his ideas from it (in fact there is little more in Schultz's brief Preface than the bald statements we have quoted), but rather to point out that though Bolzano was rather isolated in Prague he was certainly not alone in the spirit and aims of his foundational work.

Finally, it is important to keep in mind that the views outlined in this chapter were not simply a static description of an ideal of mathematics. They represented in Bolzano's mind a practical programme for the transformation and redevelopment of all mathematical theories. The works considered in this thesis are only a fragment of what he hoped and intended to produce. They can only be understood properly in the light of this larger project to which they belong and as products of the attempt to approach more closely to Bolzano's idea of the uniquely correct definitions and proofs of mathematics.

In the paper Johnson [1] (which contains an excellent survey of Bolzano's early geometric work) there is clearly a tension felt between "Bolzano's old-fashioned essentialist theory of definition" and the fruitfulness of many of his ideas. In reference to the geometrical definitions of DP he writes:

The surprising feature of Bolzano's work is that he was able to obtain topologically interesting insights and results in spite of his restrictive theory of definitions.

(Original emphasis in Johnson [1] p. 295).

It is part of the purpose of this thesis to discuss how surprising (if at all) we should find this feature of Bolzano's work. On the one hand there are many remarkably good mathematical ideas (especially in analysis) worked out in detail by someone who was never professionally a mathematician. On the other hand he whole-heartedly espoused a philosophy which emphasised the static and unchangeable aspects of conceptual knowledge. Many of Bolzano's views were restrictive but this is not incompatible with insight and progress. Restricting development in many directions may be highly conducive to development in the few remaining directions. It all depends on a wise choice of restrictions.

Naturally Bolzano himself found his "success" (the little that he knew of it) to be entirely expected since it followed from the working out of what he regarded as true and essential general principles. The cynic will say he was just very lucky. It would be foolish to isolate any one factor (such as a philosophy) in the manifold of contributions to intellectual creativity in an individual as the cause of his success. Nevertheless, what we hope to show in the next three chapters is that the general views outlined here actually contributed to his various specific mathematical achievements rather than detracting from them or being irrelevant.



## Chapter 3 : Geometry

### 3.1 Introduction

#### 3.1.1. Outline of the Geometrical Work

The geometrical work of the early period of Bolzano's life was published in BG (1804) and DP (1817). The former is entirely concerned with geometry although it contains a fair amount of what would now be regarded as philosophical remarks on the kinds of concepts and proofs to be permitted in geometry. The latter work has the purpose of giving strict proofs for the analytic formulae for the three general mensuration problems of length, area and volume. The geometrical material in DP (which mainly consists of definitions) appears in rather disjointed instalments which are logically unnecessary to the proofs but are nonetheless relevant to the main subject. These two published works span the period covered by this thesis but they by no means represent the total of Bolzano's work on geometry at that time. From the mathematical diaries which have been published in Bolzano [1] (Vol. 2B 2/1,2 which cover the years 1803-1811) it is clear that he was giving considerable attention to geometry throughout this period. There is

extensive detailed revision and development of material in BG as well as preliminary ideas for DP. But it is also fair to say the main outlines and achievements of the early work are contained in the contemporary publications which are our primary sources for this chapter. Our purpose here is to present the broad themes of Bolzano's general views and geometrical achievements with due regard to their historical context.

Geometry was a natural subject for Bolzano's first study. Many of the concepts were difficult and easily susceptible to philosophical discussion and criticism. But the structure of proofs was relatively simple, so it was a good area for seeking perfectly rigorous proofs. Moreover, Euclid's parallel postulate had defied all attempts at proof so what better way to vindicate a new approach to geometry than by showing that it led to a "complete theory of parallels" (A35;BG, 13)?

It is clear from the first few pages of BG that, whatever other reasons there may have been, Bolzano's primary purpose is methodological. We have already seen in 2.4 that Bolzano excluded the concept of motion altogether and, more dramatically, he postponed the use of the plane for all the elementary results on triangles and parallels.

The material of BG is divided into two parts. Part I is a complete reorganisation of Euclidean geometry. Most of the main theorems of Book I of the Elements are proved, but the definitions, axioms and proofs are in most cases changed out of all recognition. These changes arise chiefly, as

we shall show in detail in 3.2, from various requirements and refinements of geometrical concepts. Part II is logically presupposed for Part I and contains the "theory of the straight line"; it is very incomplete but contains some interesting ideas on defining a straight line and the concept of distance which are developed further in the results of DP. From the point of view of the geometry the main content of DP is a remarkable series of set-theoretic definitions of line, surface and solid which pre-figure the recursive definitions of dimension given very much later in the century by Poincaré and Brouwer.

As a prelude to assessing the achievements of BG, and to provide some perspective from which to judge the radical conceptual requirements made there, we shall consider the treatment in geometry of the concepts of motion and of the plane by authors before and after Bolzano. Far from attempting a comprehensive survey of the subjects the intention here is simply to provide sufficient historical context to understand clearly what Bolzano achieved in BG. Consequently our references are mainly to authors quoted or studied by Bolzano himself and to those authors mentioned in Heath [1] and Enriques [1] as having made major contributions to the subsequent history of the concepts of motion and the plane.

### 3.1.2. The Concept of Motion

Although the concept of motion has been an ever-present component of geometrical thought there seems to have been no time at which its use has gained universal approval. It was accepted, perhaps reluctantly, by Euclid in his method of superposition while it was rejected by Aristotle as having nothing to do with mathematical objects (Aristotle [1]). Throughout the nineteenth century it was still being accepted by some authors and rejected by others. Helmholtz regarded it as essential to geometry (Helmholtz [1]) which for him thereby became dependent on mechanics, and it was adopted in Peano [1] as a primitive notion in Peano's axiomatic geometry. On the other hand, Veronese gives clear and conclusive objections to the intuitive use of "motion without deformation" in elementary geometry (Veronese [1]).

Euclid's method of superposition is well known. It arises from his common notion 4: Things which coincide with one another are equal to one another. The phrase, "things which coincide" seems to mean, "things which can be moved so as to coincide". Thus the two notions of motion and coincidence are at the basis of the fundamental criterion of equality between geometric objects. It is fundamental because it is used to prove (Elements I, 4) that triangles with two sides and their included angles equal, are themselves equal. This proposition is referred to frequently for proofs in the remainder of Book I. It is Heath's view that Euclid disliked the method and

avoided it when possible (Heath, [1] p. 225). Be that as it may, we have no knowledge of why he may have disliked it, nor has there survived any doubt in the writings of ancient geometers as to its legitimacy. The precise and substantial objection to this use of the concept of motion is clearly formulated by Veronese, but we shall quote here from Heath's summary of the argument:

We must distinguish the intuitive principle of motion in itself from that of motion without deformation. Every point of a figure which moves is transferred to another point in space. "Without deformation" means that the mutual relations between the points of the figure do not change, but the relations between them and other figures do change (for if they did not the figure could not move). Now consider what we mean by saying that, when the figure A has moved from the position  $A_1$  to the position  $A_2$  the relations between the points of A in the position  $A_2$  are unaltered from what they were in the position  $A_1$ , are the same in fact as if A had not moved but remained at  $A_1$ . We can only say that judging of the figure (or the body with its physical qualities eliminated) by the impressions it produces in us during this movement, the impressions produced in us in the two different positions (which are distinct in time) are equal. In fact, we are making use of the notion of equality between two distinct figures. Thus, if we say that two bodies are equal when

they can be superposed by means of motion without deformation, we are committing a petitio principii. The notion of the equality of spaces is really prior to that of rigid bodies or of motion without deformation.... The method of superposition, depending on motion without deformation, is only of use as a practical test; it has nothing to do with the theory of geometry. (original emphasis in Heath [1] p. 227)

This expresses, though more clearly and in greater detail, one of Bolzano's objections to the use of motion in proving a geometrical theorem:

... if one had to prove the possibility of a certain motion which had been assumed with reference to a geometrical theorem, then one would have to have recourse to just this geometrical proposition. (A18;BG, XI).

If the notions of congruence or coincidence are to be used in a criterion of equality for geometric objections they can only be justified by an implicit appeal to some kind of motion. In this connection Heath describes the following as an "acute observation" of Schopenhauer:

I am surprised that, instead of the eleventh axiom, the Parallel Postulate, the eighth is not rather attacked: Figures which coincide (sic! decken) are equal to one another. For coincidence (das Sichdecken) is either mere tautology or something entirely empirical, which belongs not to pure intuition (Anschauung), but to

external sensuous experience. It presupposes in fact the mobility of figures; but that which is movable in space is matter and nothing else. Thus this appeal to coincidence means leaving pure space, the sole element of geometry, in order to pass over to the material and empirical. (Schopenhauer [1]).

How strikingly similar this is to what Bolzano had said regarding congruence exactly forty years earlier:

... the concept of congruence itself is both empirical and superfluous. Empirical: for if I say A is congruent to B, I think of A as an object which I distinguish from B by the space which it occupies. Superfluous: one uses the concept of covering (Decken) to deduce the equality of two things if they are shown to cover each other (sich decken) in a certain position, according to the axiom, "spatial things which cover each other are equal to each other" .... Now one could never conclude that two things are congruent, i. e. that their boundaries are identical, until one had shown that all their determining pieces are identical. But if one proves this, one can also deduce without covering that these determining things are identical. (A51;BG, 29).

The concept of motion has been used in geometry in many ways and for several purposes. So far we have considered its most important role: in relation to congruence for the establishment of a criterion of equality. It

was also used in definitions of geometric objects such as the line and angle.

For example, Christian Wolff in 1717 defines a line thus:

If a point A moves to another B, it describes a line. (Wolff [1] Def. 5).

In his discussion of angle Bolzano quotes from an anonymous work of 1796 as follows:

Angle is the concept of the relationship of a uniform motion of a straight line about one of its points to a complete turn. (Anon. [2]).

In 1812 Bézout defines an angle as:

The amount of rotation which brings one of its arms into the position of the other. (Bézout [1])

Naturally Bolzano's strictures on what concepts are allowed in a theory would prohibit such definitions from appearing in geometry. But many mathematicians of the eighteenth and early nineteenth centuries clearly preferred a "physical" approach and supplied definitions which are primarily guides to the intuition rather than logical compositions of primitive concepts or analyses of the essence of the object concerned. These three views of a definition are not of course mutually exclusive and although Bolzano constantly sought for the "essential" definition of all mathematical concepts, he thoroughly approved of providing (for heuristic and illustrative reasons) guidance for the intuition and he certainly acknowledged that one had to adopt some concepts as undefined (the simple concepts of 2.4.4). The



modern axiomatic geometries dating from the late nineteenth century with their success, efficiency and invulnerability have perhaps made us unduly satisfied with the conceptual agnosticism in which they rejoice. With regard to the concept of motion we can hardly imagine Bolzano's purism allowing him to do anything but condemn the adoption by Peano (1889) of motion as a primitive concept in his axiomatic presentation of Euclidean geometry. On the other hand, a modern view of this is that of Kline:

The inclusion of motion seems somewhat surprising in view of the criticism of Euclid's use of superposition: however, the basic objection is not to the concept of motion but to the lack of a proper axiomatic basis if it is to be used. (Kline [1] p. 1010).

Bolzano would doubtless have wanted to object to both the concept of motion and to any lack of a proper basis. The modern view, so often associated with the axiomatic method, but certainly no part of that method, is that it is largely a matter of taste, or at best expediency, which concepts are chosen as primitive. But because two concepts cannot be distinguished within a theory (e. g. ordinary straight lines and great circles of a sphere in the theory determined by Euclid's axioms 1 - 4), this does not mean there may be no good reasons for preferring one to the other.

As far as the influences on Bolzano are concerned in respect of his repudiation of motion in geometry, we must first consider those authors whom Bolzano refers to himself in BG. Among these, Johann Schultz is

perhaps the most significant. His Anfangsgründe der reinen Mathesis (Schultz, [1]) is referred to three times (A18, 26, 52; BG, XI, 4, 30); and his Entdeckte der Theorie Parallellinien ... (Schultz [2]) is mentioned on A64; BG, 42. In addition there are references to Gensichen [1], Bestätigung der Schultzschen Theorie ... and the anonymous work, Bemerkungen über die Theorien der Parallelen des Hr. Hofpr. Schultz ... (1796). Although the works by Schultz follow the Euclidean model of definitions being followed by axioms, postulates, theorems and problems, we have seen in 2.5 that he shared many of the general views and principles adopted by Bolzano. But the interpretation of these principles is widely different in the two authors, at least in geometry. For example, Schultz adopts the use of the plane at the outset and defines angle in terms of the infinite surface between two intersecting lines (see 3.2.1). Even when Bolzano appeals to Schultz as being in support of some of his ideas he seems apt to overstate the case. This is true in particular of his views on motion and congruence.

In the Preface to BG Bolzano claims (A18; BG, XI) that Schultz must have agreed with the repudiation of motion because he assumes no idea of motion in the Anfangsgründe. It is true that motion is not involved at all in the definitions of line or angle where they were, as we have seen, not uncommon at this time. But in a Note (Anfangsgründe p. 246) we read:

The solid things which occur in space can, as experience shows, change their place in space, i. e. move, but not the space itself in

which they are. If one therefore thinks of a part or boundary of space as movable this is merely something imaginary.

It is not clear whether the consideration of such "imaginary motion" is to be allowed in geometry or not. Such motion would be similar to the sort of motion implied by the common use of congruence mentioned earlier. Bolzano is wrong in BG I§49(A52) to say the concept of "covering" is omitted throughout the Anfangsgründe. In fact, Schultz acknowledges that the usual German expression for congruence is in terms of "covering" or "fitting" and he does not, like Bolzano, condemn such terms. Whether his understanding of them implies motion of any sort is left obscure by the explanation: "congruent extended quantities ... are different merely in their place, so if one wanted to imagine them in the same place at the same time they would be one and the same thing." (Schultz[1] p. 251).

We conclude that although Schultz makes little or no use of the concept of motion and that this seems to be deliberate restraint on his part, there is not the categorical exclusion of the concept that we find in Bolzano's geometrical work (3.2.3).

In the Anfangsgründe of A. G. Kästner which, as mentioned in Chapter 1, was a major influence on Bolzano, we again find considerable reserve in the use or implication of motion in geometry. He gives the "boundary" definitions of solid, surface and line (see next section) which he attributes to Occam, but seems to tolerate the expression "a line arises from the

motion of a point" as an appropriate façon de parler to the effect that a line does not consist of points next to one another but that everywhere on a line there is a point (Kästner[1] p. 347). However, he does not escape Bolzano's criticism (A18;BG, XI) which is directed to his use of rotation in the following axiom:

Axiom of the plane. A straight line of which two points are in a plane, is completely in this plane . . . . But since the plane which contains the straight line can turn about it as an axis, three points determine the position of a plane; and therefore every triangle and every plane angle is in a plane. (Kästner [1] p. 350)

The other two authors mentioned by Bolzano in connection with the use of motion are N. Mercator and Kant. For Mercator (whose work, Mercator[1] is not specified but certainly intended), geometry is the study of magnitude abstracted from all matter. The three principles which constitute magnitude are infinity, point and motion. A point which is set in motion describes a line, and similarly a line in motion describes a surface. Now the motion which Mercator refers to here is perhaps not the motion of physical matter, i. e. not empirical. It is motion apprehended by the mind, motion abstracted from that of a physical particle. If a concept of point can be formed from the spatial material approximations which we call particles, then a concept of motion (of a point) can be formed from the spatio-temporal approximation we call the motion of a particle. This motion has no separate word for its

designation but its distinction from empirical motion corresponds exactly to (and may be understood by means of) the distinction of point and particle.

Certainly it is correct to say that Mercator took motion as "an essential concept" (A16;BG, IX) but since Bolzano's objection to motion is partly based on the empirical nature of the concept it appears that it may, to this extent, be misplaced. We are not concerned here with whether Mercator had such a non-empirical concept of motion as we have outlined above, but only whether such a concept makes sense. If it does, then the wholesale rejection of motion in geometry is misguided, at least the rejection on empirical grounds.

In the Critique of Pure Reason Kant makes the following distinction:

Motion of an object in space does not belong to a pure science and consequently not to geometry. For the fact that something is movable cannot be known a priori, but only through experience. Motion, however, considered as the describing of a space, is a pure act of the successive synthesis of the manifold in outer intuition, in general by means of the productive imagination, and belongs not only to geometry, but even to transcendental philosophy. (Kant [1] p. 167 note).

Without knowing precisely what Kant meant by "a pure act of the successive synthesis of the manifold in outer intuition", it is at least clear that he believed in the possibility of motion as a pure concept free of empirical characteristics and so properly belonging to geometry. However,

although Bolzano actually refers to the note just quoted, he has no patience with the alleged distinction and insists that all motion either presupposes an empirical object distinguished from space, or at least, "a thing distinguished from space", and this is "alien to a subject which merely deals with space itself" (A17;BG, X).

It would be rash to conclude that in these various criticisms Bolzano was simply being naive or obstinate. He may even have acknowledged the soundness of an abstract concept of motion and of its possible use in geometry. We have already seen (2.4 and earlier this section) Bolzano's rejection of motion in geometry was actually based on two claims: (i) it was empirical; (ii) it was logically out of place because any use of motion in a proof would require a proof of the possibility of the motion. The second of these objections is the more fundamental: it is independent of whether the motion concerned is empirical or not. It is also narrower because it really only applies to axioms. It offers no objection to first proving (without motion) the possibility of a certain motion and then using this motion later on for proving some other theorem. An example of such a legitimate introduction of motion might be the definition of a curve (having proved its possibility) by means of the classical notion of locus: the motion of a point following a specified (conceptual) rule. The use of motion here evidently has no empirical implication so long as it is strictly equivalent to the definition as a set of points which satisfy the rule. However, although the introduction of an abstract, conceptual kind of

motion may be logically permissible the danger is obvious. Our mind may revert to interpreting such motion as empirical motion and make inferences relying on experience since our intuition of such motion is much stronger and more accessible than that of the conceptual variety. This is surely exactly what happens in the process of understanding and giving our assent to Kästner's "Axiom of the plane" which we quoted above. In this situation it is entirely consistent with Bolzano's heuristic and pedagogic interests that he condemns, on empirical grounds, these various uses of motion in geometry by previous authors. It does not mean he believed all motion to be empirical, it only means he condemns the empirical misuse of motion in geometry. Some mathematicians of the late eighteenth century, like Kästner and Schultz, had, out of concern for the strictness of proofs, been showing a marked restraint in their use of motion in geometry. But perhaps no one before Bolzano had shown such exceptional thoroughness in searching out, removing and replacing every instance and implication of motion in elementary geometry.

### 3.1.3. The Concept of the Plane

It seems to have been an original idea of Bolzano's to develop the elementary geometry of triangles and parallels without any use of the concept of the plane. His reasons for doing so have already been explained in 2.4.1. It is a bold idea because it prevents him adding or subtracting angles which

naturally causes fundamental changes from the Euclidean methods. In fact, Bolzano denies that angle is a quantity at all and so angles cannot be greater than each other, or even equal in a quantitative sense. Heath points out that there has always been great difficulty with the definition and development of the plane and its properties (Heath [1] p. 172, 173). Bolzano and Euclid avoided this difficulty in almost opposite ways. While Euclid assumes the existence of the plane from the outset of the Elements, along with points and lines, Bolzano puts his definition of the plane as the final paragraph in BG(A85;BG, 63). The only other published work which seems to have anything in common with Bolzano's approach is Ingrami[1] in which the theory of the plane is developed from that of a triangle considered simply as a "frame" or "3-side".

The postponement of the concept of plane as this is done in BG raises several questions. In what sense is the concept of surface (in particular, the plane) "higher" or more complex than that of line or curve (in particular, straight line)? In order to treat angle as a quantity is it in fact necessary to presuppose the concept of plane? What exactly is meant by "presupposing" the concept of plane?

We have considered the third of these questions to some extent in 2.4.1 and the second question is dealt with in 3.2.1. It is the first question that is really central to the organisation of BG. We have referred to an ordering of concepts through their analysis and development from the simpler



to the more complex (2.4). Bolzano seems to have had in mind a hierarchical arrangement of concepts partly described by his definitions. In this ordering the concept of space for example, is prior to that of motion because it is contained in the latter as an essential component. It is by no means clear that the concept of a straight line is an essential component of the concept of the plane. This would be the case if a plane is defined as a surface generated by the uniform motion of a straight line but this was not an option for Bolzano. In the Preface to BG(A20;BG, XIII) he says that if the plane is used for the elementary theory of triangles it will require axioms of the plane, and as he quaintly puts it, "if one had to prove them [it] would require just that theory of triangles". (Regarding the "proof" of an axiom see 2.4.5). So to be precise it is not the theory of the straight line but the theory of triangles that Bolzano claims to have priority over that of the plane.

The definitions of straight line and plane are given in terms of certain collections of points:

The plane of the angle ras is that object which contains all and only those points which can be determined by their relationship (their angles and distances) to the two directions R, S. (A85;BG, 63)

An object which contains all and only those points which are between the two points a and b is called a straight line between a and b.

(A79;BG, 57)

A point  $m$  has been defined to be between  $a$  and  $b$  if the directions  $ma$ ,  $mb$  are opposite directions, a term which again has been defined in an earlier paragraph (see 3.3.1). Hence the definition of plane does not explicitly contain the concept of straight line but it contains the same components of point and direction as the straight line. Thus there is certainly a partial ordering of the concepts in which point and direction are each prior to plane and straight line. But merely on this criterion of "containment in definition" it is not conclusive how the straight line and the plane should themselves be related, if at all. The development of Part I of BG suggests, however, that increasing complexity of definition corresponds to increasing dimension. (This becomes perfectly clear in the recursive and general definitions of DP, see section 3.4.2). Possibly the increase in dimension was sufficient in Bolzano's view to ensure the priority of line over surface. This ordering of the geometric objects was perhaps not so natural in Bolzano's time as it is today. It was a common practice then to define the notions of solid, surface, line and point in that order, each being the boundary of the previous one, except for solid which was given to the intuition in material objects. Priority was therefore given to sensory perception, the geometrical point being the ultimate abstract concept. For example, Kästner gives the mediaeval definitions:

a geometrical solid (*solidum*, *corpus*) is an extension that exists inside its boundaries, surrounded on all sides. The extension of a

solid at its boundaries is called a surface (superficies), and the extension of a surface at its boundaries is a line (linea). (Kästner [1] p. 178)

The same, or similar, definitions are given in Schultz [1] p. 246. Bolzano says they are improper because we can, and do, perfectly well imagine a surface, line or point without a solid which they bound. Nonetheless he retains the idea that the concept of a solid is special because it can be "given adequately" as something in intuition; lines and surfaces are both "pure objects of thought". (A70;BG, 48)

It is worth noting that at this stage in Bolzano's work he makes no attempt to give general definitions of line and surface. In BG he only defines straight line and plane. It is with the later general definitions in DP that there is clear justification, on the grounds of the definitions, for the priority of the theory of straight lines and triangles over the concept of the plane.

#### 3.1.4 Summary of BG Part I

Part I of BG contains the elementary results about triangles and parallel lines; it is intended to presuppose only one axiom (A32;BG§19) and the "theory of the straight line". In the second part there is an attempt to derive this theory of the straight line from simpler concepts such as point, distance and direction. This part is tentative and unfinished; Bolzano says of it that he has not reached "the proper basis" and that he regards

this as the most difficult part of geometry.

The general development of Part I may be seen from the following summary:

Paragraphs of BG Part I	Contents
§§1-6 (A23-27)	Definition of angle, principle that things which are determined in the same way are equal. Adjacent and vertically opposite angles. Errors of Euclid and discussion of angle as quantity.
§§7-15 (A27-31)	Definition of triangle, equality of triangles. Equality and identity. SAS determines a triangle.
§§16-24 (A31-39)	Similarity of triangles. Things which are determined by similar parts are similar. Axiom about distance. Two proportional sides with equal included angles give similar triangles. Discussion of Wolff and Kant on similarity and the motivation for the axiom.

§§25-49 (A39-52)

Isosceles triangles. Possibility of right angles. Existence and construction. Perpendiculars in isosceles triangles. ASA determines triangle. Equal angles imply similar triangles. Pythagoras' theorem, SSS determines triangle, discussion of coincidence and congruence.

§§50-67 (A52-65)

Intercepts, rectangles, parallels, corresponding and alternate angles. The parallel postulate, parallelograms, discussion of various geometries.

We have seen that Bolzano rejects, for various reasons, the introduction of the concepts of motion and of the plane into elementary geometry. This has far-reaching consequences for his treatment of the subject. The concept of angle is completely revised; congruence is replaced by a general concept of equality between geometric objects based on the Leibnizian notion of "determination" which also serves to support a generalisation of the concept of similarity; finally there is a proof of the parallel postulate. We shall now discuss these geometric consequences in detail.

## 3.2 Main Topics of BG Part I

### 3.2.1. The Concept of Angle

Where definitions of the concept of angle have not been tautologous they have generally involved the concepts of the plane or of motion.

According to Schotten (Schotten, [1] p. 94ff) the definitions of angle may be divided, with few exceptions, into three groups:

- (i) angle is the difference of direction between two straight lines.
- (ii) angle is the quantity or amount (or measure) of the rotation necessary to bring one of its sides from its own position to that of the other side without its moving out of the plane containing both.
- (iii) angle is the portion of the plane included between two straight lines in the plane which meet in a point (or two rays issuing from the point).

We shall give some examples of these types of definition. The definitions already quoted on p. 99 are obviously of type (ii).

Euclid:

A plane angle is the inclination of two lines to one another in a plane which meet together but are not in the same direction. (Elements I Def. 8). (This is exactly the definition given in Kästner [1]).

Legendre:  
(1794)

Lorsque deux lignes droites AB, AC, se rencontrent, la quantité plus ou moins grande dont elles sont écartées l'une de l'autre, s'appelle angle. [28].

(When two straight lines AB, AC meet each other, the quantity, of greater or lesser magnitude, by which they are spread apart, the one from the other, is called the angle). (Legendre [1] Def. IX).

The ideas of "inclination" and "spread" either already contain the concept of angle making the definitions tautologous, or else they indicate the difference of direction of Schotten's group (i). As an example of the group (iii) there is:

Bertrand:  
(1778)

Quand deux droites AB, CD se coupent un plan, elles le partagent les quatre parties ASD, DSB, BSC, CSA dont chacune s'appelle un angle: Enforte qu'un angle est une portion de superficie plane contenue entre deux lignes droites qui se coupent, et sont terminées a leur point de section.

(When two straight lines AB, CD cut each other on the plane, they divide it into four parts ASD, DSB, BSC, CSA each of which is called an angle: more precisely an angle is a region of the plane surface contained between two straight lines which intersect and which end at their point of intersection.)

(Bertrand [1])

The discussion by Heath on the concept of angle is impressively comprehensive but contains a strange comment on Schotten's classification. On p. 179 of Heath [1] he says that when a definition is given which does not come under the group (ii) mentioned above some note is usually added to indicate the connection between angle and rotation. This is claimed to be "remarkable" and a "striking indication that the essential nature of angle is closely



connected with rotation". (It would surely be remarkable if this obvious and natural connection was not usually pointed out.) But why should such a connection be an indication of the essential nature of angle rather than that of rotation? For Bolzano it would be the case that to understand or use the concept of rotation we must already have the concept of angle, and not conversely.

Naturally the definitions of both groups (ii) and (iii) involving the notions of the plane and motion are rejected by Bolzano. In a note to BG, [§6(A25-27) he explains what he regards as the two major defects in the traditional Euclidean treatment of angle. Firstly, angles can only be added on the assumption that they are in the same plane, so the concept of plane is implicitly required for this operation. Secondly, such arithmetic operations also make the assumption that angles are quantities. A quantity is understood to be something measurable by a number of units. The objection to angles as quantities is itself twofold. It again requires the concept of the plane for the addition of single unit angles. Also Bolzano seems to believe that the concept of angle as quantity involves the concept of the area between the arms of the angle. This is a somewhat confused section (A26;BG, 4) and there follows a comment that, "the true origin of all ideas of angles as quantities is the empirical concept of motion". However, though he may not be clear exactly why angle is not a quantity there is no doubt at all in his mind that, "angle in its essence is not a quantity" (A27, BG, 5). This seems ultimately to be

based on the fact that we can conceive a system of two intersecting lines without necessarily involving or assuming any other concept at all.

Accordingly Bolzano begins BG with his own original definition:

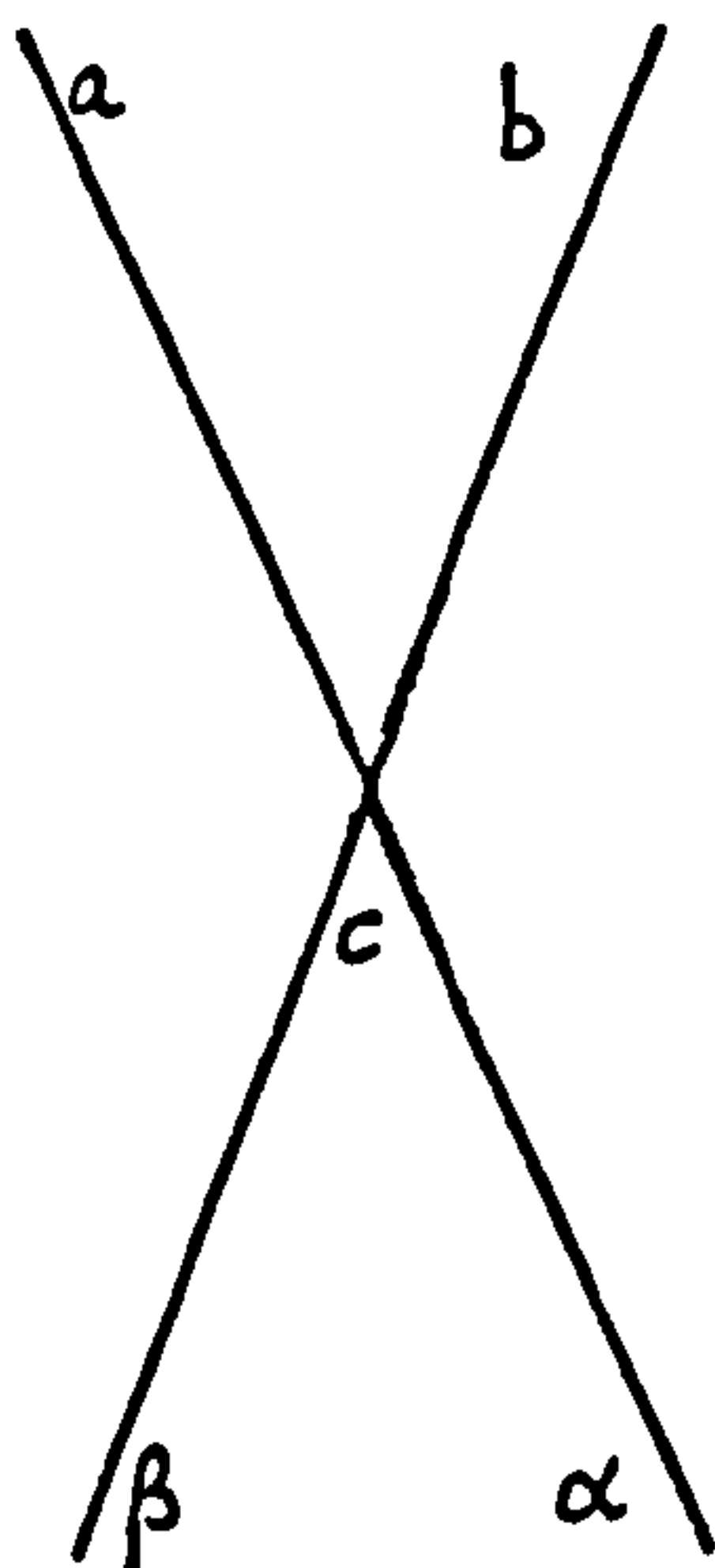
Angle is that predicate of two straight lines  $ca$ ,  $cb$  (Fig. 1) which have one of their extreme points  $c$  in common, which belongs jointly to every other system of two lines  $c\alpha$ ,  $c\beta$  which are parts of the former with the same initial point  $c$ . (A23;BG, 1, Fig. 1 is on A86).

In Part II the definition is refined in two ways: (i) angle is said to be really a predicate of the directions of two lines, (ii) angle is identified with the system of intersecting directions (A74;BGII§12). Thus the definition becomes remarkably sophisticated for the time. It is very similar to the one in Hilbert's Grundlagen der Geometrie nearly one hundred years later:

Definition: Let  $\alpha$  be a plane and  $h$ ,  $k$  any two distinct rays emanating from  $O$  in  $\alpha$  and lying on distinct lines. The pair of rays  $h$ ,  $k$  is called an angle and is denoted by  $\sphericalangle(h, k)$  or by  $\sphericalangle(k, h)$ . (Hilbert[1] p. 11).

In Hilbert's work this is soon followed by a theorem to establish the quantitative comparison of angles. But this is not, of course, done by Bolzano who is deliberately avoiding the quantitative aspect. Naturally such a procedure involves fundamental changes in the usual development of geometry. Angles can be equal or unequal but not greater or smaller. And here equality cannot be determined numerically as quantities but requires a new criterion. This appears in the following form: "Things which are determined in the same

way are equal."(A25;BG, 3). This is simply quoted as though it was a well known and self-evident principle. It is rather similar to the Leibnizian criterion for congruent structures (see the next two sections). It is applied throughout BG to various geometrical objects but particularly to angles and triangles. As an example of its use, and as introduction to the next section on the concept of "determination", we shall consider the proof that vertically opposite angles are equal (IS<sup>S</sup>5,A25). It is first proved as follows that any angle determines its adjacent angle (A24;BG, 2). From the definition, an angle is determined when its arms are determined and the arms of an adjacent angle consist of one of the arms of the given angle and an extension of the other arm of the given angle. Thus assuming, from what Bolzano would call "the theory of the straight line" that every straight line can be extended uniquely, then certainly any angle determines its adjacent angle.



Then  $ac\beta$  is an adjacent angle to  $acb$

and  $bc\alpha$  is an adjacent angle to  $bca$ .

So if  $acb = bca$  then  $ac\beta = bc\alpha$ .

Bolzano himself was dissatisfied at

not having a proof that  $acb = bca$ ,

see II<sup>S</sup>14(A75).

In I§6(A25) it is said that the "usual" way of proving the equality of two things is to deduce from the given information the equality of their "determining pieces". It is clearly regarded as a major defect in method that Euclid does not do so in his proof of this theorem (Elements I Prop. 15). The Euclidean proof is as follows (using the lettering of the above diagram). The angles  $ac\beta$  and  $acb$  are equal to two right angles (I, 13), also angle  $acb$  and  $bc\alpha$  are equal to two right angles. So  $ac\beta$  and  $acb$  are equal to  $acb$  and  $bc\alpha$ , (Post. 4 and Common notion 1), subtract  $acb$  from each and the remaining angles  $ac\beta$  and  $bc\alpha$  are equal (Common notion 3).

In Euclid's proof, treating angles as quantities has led to a kind of "arithmetisation" of the argument which Bolzano wholly rejects. He avoids it by means of his purely conceptual use of "determination". If we were to say that vertically opposite angles are equal because they are both equal to  $(180 - \theta)$  where  $\theta$  is one of the other angles, this would be more in the spirit of Bolzano's method than that of Euclid but with the notion of determination translated into arithmetic terms.

### 3.2.2. Determination

From the comparison of the method of proof indicated at the end of the previous section we can make two observations which will help to analyse Bolzano's method in the later work. Using the notion of "determining pieces"

or "determining parts" can contribute to our understanding of a result. We are forced, in the above, to think of how the vertically opposite angles arise and we thereby gain insight into the structure of their relationship. To be precise, instead of simply finding by arithmetical manipulation that two quantities are numerically equal, we are led to understand how this equality arises from having a common adjacent angle, which in turn arises from the particular arrangement of angles, i. e. that each arm of each angle is an extension of an arm of the other angle.

On the other hand the use of "determine" in a non-quantitative sense raises considerable problems. In Bolzano's principle, "things which are determined in the same way are equal" it is not clear what "in the same way" means. The comparable principle for Leibniz raises the same problem: "if the determining elements are congruent then also the things determined from them in the same way will be congruent." (*Si determinantia sunt congrua, talis erunt etiam determinata posito scilicet eodem determinandi modo*) The phrase "in the same way" could refer to the sources, the manner, or the result of the determination. If it is the result, the principle is a tautology. If the same components can determine (objects) in different ways, i. e. the determining process can take place in different ways with different results, either all the components (determining pieces) have not been given or else "determine" is being used in such a peculiar sense that it is not the correct word. Also it seems clear that unequal objects, e. g. straight lines of

different lengths, are determined in the same sort of way. This leaves the conclusion that "in the same way" refers to the equality (or identity) of the determining pieces. Equality in this respect would then have to be taken as a primitive or "given" concept.

In the geometrical example above the vertically opposite angles are indeed determined by the angle  $acb$  between them. But it is not so clear that they are determined in the same way. Bolzano believed that he should prove that  $acb$  and  $bca$  are equal but he could surely not have been pressed far on what equality means in this context. The angles have identical parts but are "thought of" differently. However, since the angle definition of  $IS_1(A23)$  takes no account of the order of specification of the two arms we shall regard their equality as part of the definition and the proof as sound. We shall here regard equality as an undefined relation and being determined in the same way as meaning being determined by equal parts.

The idea of determination can be interpreted in a logical way as a uniqueness statement. So to say that SAS determines a triangle means that given a certain SAS there is a unique triangle containing that data. This is how the idea or the term is usually used now and it is at least implied by any reasonable meaning. However, when we consider some other important occurrences such as the principles that the equality of determining parts implies equality of objects ( $A25;BG, IS_6$ ), and the similarity of determining parts implies the similarity of objects ( $A31;BG, IS_{17}$ ), it appears that the

intention is to identify components which are essential to the existence or being of the total object in a sense which other components are not. It is this essential, or existential, sense of determine which was likely to be most important to Leibniz and Bolzano and which is perhaps most difficult for us to grasp today. The idea is not that the components will, in themselves, determine a particular object of a particular kind. The kind of object to be determined must already be known and specified. Two points determine a straight line but they also determine a parabola through a third given point and infinitely many other totalities or kinds of object. Furthermore, there is no question of the determining parts being unique to a particular object; - there are many sets of determining parts for a particular triangle. So the determining parts are not special in themselves, but only a combination of two or more parts may have this property of determining a whole of some kind.

Determination may thus be considered in a dynamic sense as an act which, like cause and effect in the physical world, inevitably reaches a conclusion whether we happen to consider the result or not. For example, the two-point system can be considered, as Bolzano does in *HS6(A70)*, in itself alone with its properties of distance and direction. Now in this dynamic sense determine means something like "produce". A certain line can be produced or generated in this way. It does not depend however, for its existence on those points. The "movement" in this sense of determine is

psychological: the two points indicate a possible route for our minds to reach the concept of a particular line. But the determining is not a psychological matter because there is at the same time a static sense of determine which concerns both existence and uniqueness. There actually is, as a matter of fact, a unique straight line through the two points. These geometric objects are, for Bolzano, neither physical representations, nor mind-dependent ideas, nor abstractions. They have an objective existence as conceptual objects. The relation of determining is sui generis, a relation between conceptual objects. It may be compared to the relation of ground to consequence between propositions in themselves developed in BD and in much greater detail in Bolzano [4]. The logical consequence of a statement involving "determine" is a uniqueness statement, but it is at the same time more than that. It tells us about the composition and existence of the whole being determined. And we have already indicated (p.120) how this kind of information can be valuable in mathematical proof, both for discovering a proof and for making it clear.

### 3.2.3. Equality and Congruence

In the Elements Euclid used the notion of equality in a quantitative sense; (I, 4) is exceptional for the phrase, "the triangles are equal". Only in the solid geometry of Books XII and XIII did he use the expression "equal



and similar" for congruent structures, i. e. ones which would occupy the same space. This term "equal and similar" is used for congruence in the eighteenth century German writings (for example in Schultz [1] p. 251 and Kästner [1] p. 177). The verb "sich decken" (literally "to cover itself") was still used for congruence well into the nineteenth century (see BG I§49(A51) and the quotation of Schopenhauer above (p. 97 ). In Latin works "congruere" was used, and "Kongruenz" and cognates became common in German in the early nineteenth century.

The basic congruence theorems are usually expressed in terms of two triangles each with sufficient data respectively equal for the conclusion to be drawn that the remaining sides or angles are equal. This use of congruence has doubtless been strongly influenced by the superposition method of Euclid adopted by him for I, 4 in the case of two triangles with two sides and their included angles equal (SAS). The corresponding sides are placed over each other and then the other parts of the triangles "must" also coincide and so be equal. But this is a theoretical "must" which would not be refuted by the failure of any practical test. The claim that they must coincide vitiates the need for any superposition. The point is that the data given in either triangle is sufficient to determine its other sides and angles. Both the method of superposition and the notion of congruence are redundant for the proof of the conclusion: they merely illustrate the conclusion with reference to two triangles. Now it is entirely consistent with Bolzano's views

on geometry considered so far that he should regard the concept of congruence, and the various associated terms and methods, as alien and superfluous. (We have already quoted the passage in I§49 (A51) rejecting congruence on p. 98.) His own proof of the SAS criterion for equality is obtained by proving that the data are sufficient to determine a triangle (A30:BG, I§12). This comes directly from the definitions (A23:BG, I§1) and (A27:BG, I§7). The "congruence" theorem (A30:BG, I§14) then follows since two triangles with SAS equal have equal determining parts. This is surely the essence of the purely geometrical result concerned here: that certain conceptual objects determine, through their relationships, certain other conceptual objects. For Bolzano then the crucial relationship is the many-one relation of determination between several instances of physical or conceptual triangles and their one abstract counterpart. This is in contrast to the view of congruence as a binary relation on the field of physical or conceptual triangles.

In reference to the use made of the Leibnizian criterion for the equality of geometric objects Vojtěch suggests that it represents a purely logical standpoint and is therefore insufficient (Vojtěch [1] p.188, Note 11). By "insufficient" Vojtěch was evidently meaning "lacking in modern axiomatic rigour" and in this sense he is certainly right. But two points need to be made in this connection. On the one hand we must remember Bolzano's purpose - to provide conceptually correct and correctly ordered proofs of elementary geometry. The axiomatic model of the time was still the Elements and this work was, in Bolzano's view, conceptually chaotic and

insufficient to provide a sound foundation for geometry. Equally insufficient to him would appear the clinical modern ideal of a complete and consistent theory with minimal independent sets of axioms and primitive concepts. The meaning of "sufficiency" with regard to a mathematical principle or theory has changed profoundly since Bolzano's time.

On the other hand (and now as a corollary to the above) it would be a complete mistake to regard Bolzano's use of the equality criterion, things whose determining pieces are equal are themselves equal, as representing a purely "logical standpoint". It could be no such thing. The concepts of "determining pieces" and "equality" may be undefined but they have a geometrical significance. The objects of geometry are viewed as laid out in a definite progression of complexity: points, the straight line, configurations of lines (triangles and parallels), the plane. For each stage correct definitions are sought which to some extent take the place of axioms. When the terms "determine" and "equal" occur in such definitions or axioms they are to be understood in their geometrical context. It is contrary to the whole spirit of Bolzano's approach to regard these terms as entirely formal or logical. They would have a different meaning and role when used in analysis.

### 3.2.4. Similarity

Similarity is a weaker or restricted form of equality which has been used in geometry to identify properties of shape and ratio in contrast to properties of magnitude. Equality is a general relationship which was taken into elementary geometry through the notion of congruence and applied to objects such as triangles. Bolzano believed that such a transition, or special application, of a general concept to a particular subject was necessary and useful, but he objected fundamentally, for the various reasons we have examined, to how this had been achieved in the Euclidean tradition. It is not hard to see why Bolzano would also be dissatisfied with Euclid's treatment of similarity. It is defined in Book 6 of the Elements (Definition1):

Similar rectilinear figures are such as have their angles severally equal and the sides about the equal angles proportional.

Apart from using equality of angles in a quantitative sense this has simply isolated, in an ad hoc manner, sufficient properties to ensure that geometrical figures satisfying the definition will be similar in the general sense of having the same shape. And mathematically this is perfectly adequate and is justified by its usefulness. However, Bolzano prefers to try and embed the mathematical notion of similarity directly into a more general notion of similarity. This is not just to satisfy some metaphysical principle, he believes the definition will thereby be more useful because it will have greater mathematical generality (see end of A38; BG. IS24). Also, being more "correct" it should render the elementary theory more complete; in particular his hope is

"to complete the known gaps in the theory of parallels by means of the theory of the similarity of triangles" (A35;BG,13 ). Bolzano's definition is as follows:

Two spatial things are called similar if all characteristics which arise from the comparison among themselves of the parts of each one of them, are equal in both, or if by every possible comparison among themselves of the parts of each, no unequal characteristic can be observed. (A31;BG, I§16)

The principle (A31;BG, I§17) that, "objects whose determining pieces are similar are themselves similar" is "proved" by use of the meaning of the word "determining", and so should better be regarded as an axiom governing the use of this concept. The idea is to relate similarity and determination in the natural way and to provide an analogy with the equality principle (I§4, A24). The definition itself is a kind of compromise between the specific proportionality of sides in Euclid, and the very general intrinsic quality defined by Leibniz: those things are similar in which it is not possible to discover, by consideration of themselves alone, whether they are to be distinguished (similia sunt, in quibus per se singulatim consideratis inveniri non potest, quo discernantur). In BG, I§24(A34) Bolzano comments at length on his work on similarity. He feels obliged to stress the originality of his treatment because he says Wolff "already sets forth in detail the same theory in his Philosophia prima seu Ontologia Sect. III Cap. I de Identitate et Similitudine,

and also in the Elementis Matheseos Universae". This is rather an exaggeration. Wolff's definition of similarity in the Philosophia is almost the same as that of Leibniz: "those things are similar in which the things by which they should be distinguished from one another are equal..." (Wolff [2] §195). He makes no mathematical application of the concept but elsewhere he says it "conforms to the practice of mathematicians" (Wolff [3] §222). In the Elementis again there is little that could be called a theory of similarity. There is the general definition in the Elementa Arith. (Definition 12) and then its application in the Elementa Geometriae where he acclaims "the most ingenious Leibniz" for using the notion of similarity to facilitate many proofs for which Euclid used only congruence. However, there are very few examples of such mathematical improvements actually produced; the proofs are mostly very crude. For example, for the congruence of triangles the SAS condition is deduced as a corollary to the corresponding construction problem and the proof of the SSS condition consists of the comment, "one being laid on another, they will perfectly agree" (Wolff [1] ). Bolzano's treatment is vastly superior from a modern (and from a Greek) point of view. The references to Wolff may be therefor two reasons. He was a well-known and respected scholar in the eighteenth century German-speaking world with whom it might be beneficial to be associated and Bolzano was particularly attracted by the idea that two or more independent discoveries of a theory was strong evidence for its veracity. (See DP§31, A556) Secondly,

However insubstantial the mathematical resemblance of their work Bolzano was undoubtedly strongly influenced by the general ideas about similarity originating from Leibniz and Wolff.

We shall give one further definition of similarity, due to Wolff, which while not quoted in  $\text{IS}24(\text{A}34)$  would probably be known to Bolzano and is particularly helpful for understanding his axiom  $\text{IS}19(\text{A}32)$ . It is to be found in Wolff's Anfangsgründe aller mathematischen Wissenschaften:

Similarity is the correspondence by which things are distinguished from one another by the mind, and further, similar things cannot be distinguished from one another without, for example, the help of a measure. (Wolff [3] )

The one explicit axiom of BG appears near the beginning of the section on similarity,  $\text{IS}19(\text{A}32)$ , and is as follows:

There is no special idea given to us a priori of any determinate distance (or absolute length of a line), i. e. of a determinate kind of separation of two points.

This is the beginning of a closely connected sequence of five paragraphs ending with the important result ( $\text{A}34$ ; BG,  $\text{IS}23$ ) that in similar triangles corresponding angles are equal. The immediate use of the axiom is to prove that all straight lines are similar and from this that triangles with proportional sides around equal included angles are also similar. One might attempt to prove that all straight lines are similar directly from the definition

as follows. The two end-points are determining parts of a straight line and these afford no comparison between them so no "unequal characteristics" can arise and so, by IS17(A31), the straight lines themselves are similar. It depends on what is to be counted as a "comparison" of "parts". If it simply refers to ratios of distances then our direct proof without the axiom would surely hold. This is evidently too much of a modern and technical interpretation of "comparison". By simply comparing the two points of a two-point system the only characteristic that can arise, or that the mind can perceive, is that of apartness or separation. And although we understand that separateness admits of degrees, greater or less, the mind alone cannot distinguish or compare degrees of separateness. This is what the axiom says. Another way to put the matter would be to say that the distance associated with a two-point system is not an intrinsic property of the system, i. e. there is no way to measure or compare it without going outside the system. This is perhaps even clearer if we compare distance with angle in the usual quantitative sense. There is a natural "absolute" measure of angle, namely a complete revolution, to which there is nothing comparable for distance.

It is clear from the quotations above that by the term "similar" the Leibniz-Wolff school wished to indicate the equality of the intrinsic, structural properties of any system. The concept of distance can be understood a priori, but the comparison of arbitrary distances is an essentially empirical matter and not part of pure geometry. Thus Bolzano's axiom is being



applied here to supplement his definition of similarity and to preserve its domain among those things which can be known a priori. Indeed, rather than an axiom in the modern sense it is more like a principle that prescribes the a priori character of geometrical knowledge. It is not included in the definition of similarity because, "it applies usefully in all parts of mathematics" (A38;BG, 16). In this section its function is to make a very general concept of similarity applicable to geometry.

These philosophical surroundings to the concept of similarity should not obscure the considerable significance of its early introduction in this account of elementary geometry. From a modern point of view this is perhaps the most worthwhile and lasting achievement of BG Part I. One of Bolzano's pupils, R. Zimmermann, points out that this is what makes Bolzano's presentation of geometry so very different from the usual ones:

The important concept of the similarity of spatial figures appears, in Euclid's exposition, much later than the theory of parallels, but in Bolzano's exposition it is among the first concepts of geometry.

(Zimmermann [1] p.170)

From a pedagogic point of view this change certainly makes for an easier grasp of the main outlines of the subject (e. g. the similarity proof of Pythagoras' Theorem (A48;BG, 1845) is simple and instructive). This aspect of Bolzano's reorganisation of Euclid is still vindicated by the way elementary geometry is taught today.

### 3.2.5. The Theory of Parallels

There is some confusion in the literature on the significance of Bolzano's work on parallels in BG. In BG, I§59(A57) there occurs the theorem, "Through the same point  $o$  outside the straight line  $xy$  there is only one parallel to  $xy$ ." This has the appearance of the Playfair equivalent of Euclid's fifth postulate, the parallel postulate, and has led some writers to claim that Bolzano had erroneously proved this postulate while not realising (like Wallis) that it was equivalent to his earlier assumption of the existence of unequal similar figures. (Examples of this are found in Folta [1]p.93, Folta [2] and Kolman [1]p.44) Others deem the proof a highly significant application of Bolzano's concept of similarity and recount it in detail (e.g. Bergmann [1] p.190). Perhaps the most appropriate reaction (as we shall show in this section) is that of van Rootselaar in his excellent detailed commentary on BG in the Introduction to Bolzano [1] Vol.2B 2/2(p.13) where he makes little more comment than "that Bolzano, almost incidentally, proved the parallel axiom".

There can be no doubt that Bolzano never intended to "prove" Euclid's parallel postulate in the sense that this is usually meant, i.e. to prove it from the other four postulates. As we have already sufficiently shown, Bolzano's whole approach to elementary geometry was totally different from that of Euclid. There is no reference anywhere in BG to Euclid's form of his fifth postulate. When Bolzano does refer to it in BD, 108(A215) he says

that, "it only holds under the condition that both lines lie in the same plane". His final comment on his own work (A63;BG, 41) is that, "These are perhaps the most important propositions of the theory of parallels which are here expressed without the concept of the plane." Thus Bolzano did not intend in BG, at any stage, to solve this rather notorious problem of Euclidean geometry although it was topical at this time. The answer to Folta's question of "Why Bolzano did not try to get acquainted with Klügel's thesis?" (Folta [1] p. 96 referring to the important work Klügel [1] which had been inspired by Kästner) is that he was probably just not interested in investigating further a system in which he believed he had diagnosed far deeper errors than this mere symptom of the unproved parallel postulate. Bolzano was busy developing his own kind of "non-Euclidean" geometry for which he could deduce the theory of parallels which appears in BG, I§§50-66(A52-65).

In another sense, however, Bolzano's BG was more "Euclidean" than Euclid's Elements. And this was because of the assumptions made in BG Part II on the "theory of the straight line" on which Part I of BG was based. In Part I Bolzano assumes several results which are equivalent, in the plane, to Euclid's parallel postulate. For example, in BG, I§24(A34) there is the existence of similar unequal triangles, and in the definition of parallel lines (A55;BG, I§34) there is the assumption that all points equidistant from the straight line do themselves form a straight line. Now it would be Bolzano's claim that these can all be proved from his definition and

theory of the straight line in Part II. This is a deeper reason why BG cannot be compared with the Elements. Euclid did not incorporate the intuitive idea of the "straightness" of his straight lines into his first four postulates. Nor is his attempted definition of straight line used anywhere in actual proofs. Bolzano's definition of straight line in BG, II§26(A79) is stronger than Euclid's postulates 1 and 2 and could, if developed further, have been axiomatised to provide a rigorous foundation for BG Part I, if not, with suitable modification, for most of Euclidean geometry. In fact, van Rootselaar has shown in some detail how this might have been done in the above-mentioned Introduction (Bolzano [1] Vol. 2B, 2/2) where there is also detailed reference to the various revisions and further efforts to complete work started in BG which are contained in the volume itself.

Bolzano's references to the theory of parallels are rather obscure, they are to Schultz, Gensichen, Bendavid and Langsdorf (A64; BG, 42). Schultz seemed to believe he was the first to prove the postulate in Schultz [2] where he mentioned the work Klügel [1]. There is no mention in BG of important contributions such as those of Saccheri, Klügel and Lambert but this is possibly because of their Euclidean stand-point.

There is an interesting connection between Lambert's work on parallels and Bolzano's axiom I§19(A32). In the main work Lambert [1] he uses a quadrilateral with three right angles and considers the three hypotheses on the nature of the fourth angle, it may be: (i) a right angle, (ii) an obtuse

angle, or (iii) an acute angle. Assuming the infinite extent of the straight line disproves (ii). But on (iii) we can consistently adopt an absolute measure for line segments\*. Consequently if we deny the existence of an absolute unit for distance we can reject the third hypothesis. Now Bolzano does assume the infinite extent of the straight line (e.g. I§30(A41) and I§39(A46)) and in the axiom I§19(A32) he denies, for philosophical reasons, the existence of an absolute measure. (Evidently in the belief that a priori knowledge of spatial things requires this denial) So the way would have been open to him, had he wished, to deduce the parallel postulate in Euclidean terms. Legendre gives a proof of the postulate much later using this discovery by Lambert (see Legendre[2]). Lambert himself did not deny an absolute measure and proceeded further to seek a contradiction.

The theory of parallels which appears in BG remained very important in Bolzano's estimation throughout this early period. Given the assumptions of BG Part I the main results are correctly deduced and, because of the originality of his approach, it represents a considerable achievement. On BD, XIV(A105) he explains why he feels BG received such little attention,

the small extent of the pamphlet, its uninformative title, the far too laconic style, the anonymity of the author and many other circumstances were certainly not favourable for securing attention.

He also refers on the same page to, "an obvious mistake in the theory of parallels". As we say in Note [4] on A260 this probably refers to the

\*For details of this see Bonola [1].

theorem BG, IS30 (A41) where "gleich" (equal) occurs instead of "ähnlich" (similar). Although it is not in the main body of results on parallels it is an important theorem used for those results. A further reason for supposing he was referring to such a trivial error is that in a later reference to BG in RB, 22(A455) he claims that one reason why his "new theory of parallels" deserves attention is precisely that, "no obvious error has been detected" ! The second reason given there for the importance of his theory is that he regards Legendre (in Legendre [1]) as having "hit upon just the same view of things quite independently of me". And it is true that in Book I of that work Legendre develops a theory of parallels on the basis of similarity but he assumes the plane and is very much in the Euclidean tradition. (See further on this Zimmermann [1] and Bergmann [1] p. 196) Also relevant here is a short fragment written by Bolzano in 1813 entitled Neue Theorie der Parallelen (New Theory of Parallels) which is reproduced in Bolzano [1] Vol. 2A5, p. 135.

### 3.3. BG Part II and Assumptions of BG

#### 3.3.1. General Outline of BG Part II

The title of BG Part II is an accurate description: Thoughts concerning a prospective theory of the Straight Line. It is a rather disorganised jumble of ideas about the assumptions made in Part I concerning the properties of the straight line. It is incomplete and tentative - just a sketch put forward, "to find out whether I should continue on this path" (A21;BG, XV). The quality and significance of these ideas are very mixed but there are some which make important distinctions and are developed usefully later in DP. The material divides roughly into the following three sections:

§§ 1 - 5 (A66 - 70)	Identity and equality. Determination and possibility. Definitions and nature of geometric objects.
§§ 6 - 24 (A70 - 79)	The system of two points, distance and direction. Various properties, concept of opposite direction.
§§ 25 - 43 (A79 - 85)	Definition of straight line. Relations of order. Mid-point and definition of plane.

We shall comment on the first of these sections here and on the remainder in 3.3.2. It is in these paragraphs 1 - 5 that Bolzano most justifies the title "champion of rationalism in geometry" which Kerry confers equally on Bolzano and Leibniz (Kerry [1] p. 476). He discusses the purely conceptual nature of geometric objects, emphasises the necessity of proving the possibility of any spatial object (in contrast to proving constructibility in Euclid), and rejects the traditional definition of geometric objects which gives priority to the concept of solid.

The assumption that points exist, or that they are possible is not considered. The concept of point is said to be indispensable to geometry and is defined as a "characteristic of space (σημειον) that is itself no part of space" (A69;BG, 47). In the same passage we read that a point is "a purely imaginary object" (ein bloss imaginärer Gegenstand). Presumably this means it is an object which can be conceived by the mind but of which we have no direct intuition or sense experience. This does not imply that it is an abstraction constructed by the mind but rather, to be consistent with Bolzano's outlook, it is an objective entity apprehended by the mind. Possibly he thought of such objects as constituting an intelligible realm similar to the propositions and ideas "an-sich" (in themselves) described later in the Wissenschaftslehre (Bolzano [4]). Lines, surfaces and geometrical solids would be of the same sort, but the last is distinguished as being adequately presented "in intuition". Bolzano concludes:



Accordingly every pure intuition of lines and surfaces which is attempted (e. g. by the motion of a point) must be impossible. The definitions in this paper of the straight line, §26, and the plane, §43, are made on the assumption that both are pure objects of thought (Gedankendinge). (A69;BG, 47)

Because of this purely conceptual nature of spatial objects Bolzano seems to regard their possibility as a sufficient criterion for their existence, or at least for their use in geometry. It is not always clear whether possibility meant more for Bolzano than consistency of definition, he certainly did not require actual construction. He says that one of the purposes of theoretical geometry is to show the possibility of this or that spatial thing (A40;BG, I§27). To quote from that paragraph: "the theoretician must be allowed to assume certain spatial objects without showing the method of their actual construction, provided he has proved their possibility".

Such possibility is also to be proved of relations before they can be applied to spatial things. In BG, II§3(A68) he suggests that the possibility of equal things could be demonstrated from the axiom BG, I§19(A32) since it states (in full generality) that we have no a priori idea of any determinate spatial thing and so more than one equal spatial thing of any kind must be possible. But at the introduction to the axiom and at I§24, BG, (A34) "determinate" had the meaning "determinate size" so that we could conclude precisely that there is more than one spatial thing possible of different (unequal)

sizes and so unequal to each other. But "determinate" now appears to be interpreted as "determinate position" for in the last sentence of BG, II§3(A68) we read: "If therefore any spatial thing A is possible at a point a, then also an equal spatial thing B (=A) must be possible at the different point b".

One further point arises on this matter of proving the possibility of certain objects. Vojtěch suggests that Bolzano "forgot" to require the possibility of similar structures or to derive it from other postulates (Vojtěch[1] p. 190 Note 16). It is more likely that he regarded his axiom in I§19;BG, (A32) as specifically postulating similar spatial objects and so their possibility needed no demonstration. The denial of a special idea or concept does seem to have implied for Bolzano that the whole range of possible concepts (i. e. possible to be thought) would correspond to "objects". This is confirmed by the attempted use of the axiom described above where "determinate" means "determinate position".

### 3.3.2. Definition of Straight Line

A relatively systematic sequence of ideas now leads up to the definition of straight line in BG, II§26(A79). This begins with the important analysis of the simplest non-trivial geometric structure, a system of two points. The concept of the relation between the two points a and b is divided into the two components of distance and direction as follows:

I. That which so belongs to point b in relation to a that it is independent of the definite point a (which is precisely this one and not another), and which consequently can be present equally in relation to another point, e. g.  $\alpha$ . This is called the distance of point b from a.

II. That which so belongs to point b in relation to a that it is dependent solely on the definite point a, where we have now separated off what already lies in the concept of distance, i. e. what can belong to point b in respect of another point. This is called the direction in which b lies from a. (A70;BG, 48).

As far as being a correct (or "essential") definition of the relation of a to b, or of the components of this relation, this is obviously unsatisfactory. Distance is no more "independent" than direction. The two concepts could have been interchanged without making any difference to this definition. The paragraph II<sup>S</sup>7(A71) is intended to show the possibility of both concepts but it is hopelessly confused because in the proof for each concept he requires the possibility of the other. Instead of "possibility" he actually shows the necessity of each concept by showing that neither concept, of itself, exhausts the content of the relation. This is hardly surprising since the first component of the relation (distance) is something independent of the particular point a. Even here there is confusion: "independent" (unabhängig) does not seem the appropriate term; what is evidently meant is, "that which is not

uniquely determined by the particular point  $\underline{a}$ ". In spite of these muddles the analysis does yield something positive which is summarised in II§8(A72), namely that the relation of two different points  $\underline{a}$  and  $\underline{b}$  can be divided into two primary concepts, distance and direction, such that neither of these alone determines one from the other. Given both the distance and direction of  $\underline{b}$  from  $\underline{a}$  they determine the point  $\underline{b}$ , and given two points they determine a distance and direction.

No such systematic analysis of these relations had been given before and it shows how the concept of determination can be used to define the simplest geometric object - the two point system - in terms of distance and direction. Next, a triangle is defined as a three point system (A76;BG, II§18), no mention need to be made of lines (which are as yet undefined). It is regarded as being only a concession to convenience, in Part I, to use the heterogeneous concept of straight line in reference to triangles.

The concept of opposite direction is then defined as follows. Remembering that Bolzano is working in space rather than the plane there is a whole "cone" of directions which form a given angle with a given direction R. But there is a certain value of this angle (namely  $180^\circ$ ) such that this cone degenerates to a single direction, this is the opposite direction to R (A75;BG, II§15). Bolzano points out that from his definition it neither follows that there exists such an opposite direction nor that it is unique (A75, 78;BG, II§16, §24). Now a point m is defined to be between a and b if the directions

ma, mb are opposite, so finally,

An object which contains all and only those points which are between the two points a and b is called a straight line between a and b.

(A79;BG, II§26).

It has generally been easier in mathematics to develop a theory deductively from intuitive concepts rather than to work in the other direction and attempt a logical analysis by which to define and give a foundation for our first concepts. To see the significance of Bolzano's definition here of the straight line let us compare it with those definitions given in the two major eighteenth century works with which Bolzano was certainly familiar. First, that of Baumgarten in his Metaphysik:

A line (linea) is a series of points which are between separate points and which are uninterruptedly next to one another.

.....

That line in which there are as few points as possible between the fixed separate points is the shortest line between those points or the straight line.

(Baumgarten [1] p. 83)

Then from Kästner's Die mathematische Anfangsgründe:

An extension which is such that it is surrounded on all sides and is entirely contained within its boundaries is called a solid extension or a geometrical solid. The extension of a solid at its boundaries

is called a surface (superficies) and the extension of a surface at its boundaries is called a line (linea) . . . . . a straight line is one whose points all lie evenly [nach einer Gegend] .

(Kästner [1] p. 178)

In this passage Kästner specifically rejects the idea in Baumgarten's definition that a line consists of points next to one another.

Bolzano's contribution to the problem was clearly much more precise and subtle than these efforts. His achievement was impressive, original and important for his later work. Yet from a modern point of view Johnson is quite right to say of BG Part II that, "we would judge Bolzano's theory of the straight line to be all but worthless. It was the result of investigating a pseudoproblem." (Johnson [1] p. 288). It would be useless for anyone now to take over Bolzano's problem or method. Simply because the logical possibilities in the intuitive concept of line have been thoroughly explored axiomatically. But it was highly important in Bolzano's time to begin such an exploration, albeit in terms of definitions. It is clear from our account above that it was his general views about the nature of mathematics and the need for purely conceptual foundations that led Bolzano to see the problem of the definition of the straight line as significant and to deal with it in such an abstract manner. His deliberate distinction between distance and direction leads Folta to make a rather exaggerated claim, "Bolzano's ideas are the basis on which linear vector algebra is built up in a purely synthetic,

geometrical form." (Folta[2]p.227). Though this is certainly not historically accurate it is quite true that the abstract concept of a free vector, developed much later in the century by Grassmann, requires the distinctions that were first made in BG by Bolzano.

### 3.3.3. The Assumptions of BG

In Part I of BG there is only the one explicit axiom at BG, I§19(A32) to which reference has been made several times. Most of the other assumptions in Part I are said to be derived from the "theory of the straight line". It may well have been part of Bolzano's ambitious and rationalist hopes that he would be able to prove all these assumptions from the correct definition of the straight line in Part II, thus leaving only one true axiom in his geometry. This does not, of course, happen and Bolzano is quite candid about the problems. In BG, II§24 he lists the assumptions which he is still unable to prove, even from his analysis of distance and direction. These are as follows:

- (i) that the distances  $ab$  and  $ba$  are equal (A73;BG, II§11);
- (ii) that the angles  $ras$  and  $sar$  are equal (A75;BG, II§14);
- (iii) to a given direction there is a unique opposite direction (A75;BG, II§16);
- (iv) in a system of three points consider the relations of the

direction in which every two lie from the third: if these directions are the same or opposite at one point then they are the same at two points and opposite at one point. (A78;BG, IIS<sup>S</sup>24).

The assumption most frequently used in Part I is that two given points determine the straight line which lies between them. This follows from the definition of a straight line and the assumption (iii) above. The existence of a mid-point is assumed at IS<sup>S</sup>26(A39) and IS<sup>S</sup>58(A57), and this is proved at IIS<sup>S</sup>30(A80). At IS<sup>S</sup>39(A46) and IS<sup>S</sup>57(A56) there are various assumptions about the order of points on a line and these all follow from assumption (iv) above which foreshadows the axioms of order put forward later by Pasch. At several places the unique and indefinite extension of a straight line is required (e. g. IS<sup>S</sup>3(A24), IS<sup>S</sup>30(A41). It is even quoted in the Preface as an example of a proposition in the theory of the straight line, but it is not mentioned in Part II. However, assuming arbitrary distances (as mere numbers of units) are possible it could, rather loosely, be deduced from the fact that (IIS<sup>S</sup>39, A84) a point, distance and direction determine another point. For a detailed discussion of possible axiomatisations of BG we refer again to van Rootselaar's Introduction in Bolzano [1] Vol. 2B2/2.



### 3.4. The Geometrical Work in DP

#### 3.4.1. Summary of Main Topics

The principle theme of DP is the provision of what is regarded as a proper proof of the correctness of the usual formulae for the length of a line, the area of a surface and the volume of a solid. Consequently we would expect it to be a mainly analytic work and it does involve the use of Taylor's theorem expansions for functions of one or more variables and the determination of functions by a general kind of similarity principle. But this is interspersed with various paragraphs concerning pure geometry. In particular there appear here for the firsttime "topological" definitions of line, surface and solid. There are also various conceptual distinctions such as those between length and distance, and space and position, and also some applications of the general notion of similarity established in BG. Bolzano himself admits (A534;DP, 21) that most of this geometric work is logically unnecessary for the theory and results of DP: it was a convenient place to record some relevant results of his "reorganisation of geometry" at which he had now been working for many years.

Thus it will not be inappropriate here to consider the purely geometric work in isolation from the context of the rectification problems and instead to regard it as illustration and development of the ideas in BG. This is further justified by the fact that Bolzano seems to have regarded the geometry in DP as of at least as much significance as the analysis. It is

reported in Berg [1] p. 27 that,

It is a curious fact that the copy of Bolzano (6) [= DP] extant in the University Library of Göttingen (sign. "Mathemat. III, 8973") contains a commentary in Bolzano's hand written on two empty pages after the printed text and emphasising these definitions:

There then follows a summary in German by Bolzano of the geometrical results that occur in DP. We shall give an English translation here of the German quotation in Berg [1] as it provides a convenient summary of the geometrical work of DP. Bolzano's page numbers correspond to our DP pagination and we have added the relevant Appendix page numbers as usual.

In this paper the following are comprehensible even for a beginner:

- a) the definitions of line, surface and solid which appear on p. 20ff. (A533), p. 51ff. (A564) and p. 66 (A579).
- b) the definitions of a straight line p. 29 (A542), of a plane surface p. 53 (A566), and several other definitions which appear on p. 53, 54, 67 etc. (A566, 567, 580).
- c) the definition of the concept of the length of a line p. 34 (A547), of the area of a surface p. 57 (A570) and volume of a solid p. 68 (A581).
- d) the definition of the concept of space p. 41 (A554).
- e) of similarity p. 38 (A551).
- f) of geometrical equality p. 37, 38 (A550, 551).
- g) the proof of the theorem that the lengths of similar lines are in

proportion to the lengths of other lines derived from them in a similar way, p. 41(A554), cf. §49, §60.

- h) the definitions of the concepts of a simple line, of a self-returning line, of a bounded line, of a boundary point p. 21(A534); of a simple surface, a self-returning surface, a bounded surface p. 52(A565); of a point which is enclosed by a line on a surface p. 53(A566); of a surface figure p. 54(A567); of a connected solid, of inner or boundary points on a solid p. 66(A579); of a prism p. 67(A580); etc.
- l) Preface p. I - VI (A492-495).
- k) Criticism of various proofs that the straight line is the shortest, etc. Preface p. XIff. (A500).
- l) the Appendix p. 76ff. (A589).
- m) the definitions of the concepts of speed and force p. 16ff. (A529).
- n) the proof of the theorem that every particle whose speed does not alter describes a straight line p. 26(A539).
- o) something about my theory of parallel lines p. 43, 44(A556) etc.

### 3.4.2. The Geometrical Definitions

In the Preface to BD Bolzano complains that "precise definitions are still lacking for the important concepts of line, surface and solid" and that there was still not even agreement on the definition of a straight line, "which

could perhaps be given before the concept of a line in general" (A97;BD, VI). (Bolzano had, of course, as described in 3.3.2, produced such a definition of the straight line: it re-appears substantially unaltered in DP<sup>S</sup>15, A542) Now beginning at §11(A533) we find one of Bolzano's most interesting conceptual achievements: the definitions of these geometrical objects in terms of point sets and neighbourhoods. First there is the definition of the concept of line in general, then its various subsidiary concepts: connected, simple, closed and bounded lines.

The main definition is as follows:

A spatial object at every point of which, beginning from a certain distance and for all smaller distances, there is at least one, and at most only a finite set of points as neighbours, is called a line in general. (Fig. 1-7)(A533;DP, 20)

A spatial object, it is explained in a footnote, is a system or collection, finite or infinite, of points. Bolzano is well aware that examples can be constructed (e.g. containing circular arcs) in which for certain points and for a certain distance there are infinitely many neighbours. So the phrase "beginning from a certain distance" does not mean one such distance can necessarily be specified for all points of the line, but that for each point of the line a suitable "initial" distance can be found. The idea of using neighbouring points, determined by distance, in order to define a geometric object appears here in the mathematical literature for the first time. The

property of possessing neighbours at arbitrarily small distances from any given member point is described in the classical terminology as the genus proximum of all three geometric extensions, line, surface and solid. The differenta specifica for the general line is the property that a distance can always be found for which there are only a finite number of neighbours at each smaller distance (A535;DP, 22).

Some indications of how Bolzano arrived at this definition and why he regarded it as more correct than others are suggested by the way he deals with two possible objections. These are:

- (i) that by this definition the line is reduced to a mere composition of points;
- (ii) the definition must really be a theorem, it makes no reference to the original (or intuitive) meaning of the word "line" as a spatial thing described by the motion of a material point.

The second of these objections is easily dealt with: no concept of motion or a material particle can have any part in pure geometry. It is, instead, this usual empirical definition of a line (which Bolzano calls the "mechanical" concept of a line) which is the theorem provable from the more primitive and properly geometrical definition Bolzano has given. He attempts to illustrate this claim with a proof (A539;DP, 26) that the path of a particle in which "the cause of its motion does not change" is actually a straight line. (The phrase "the cause of its motion" means for Bolzano, "the velocity throughout the

motion"). But the supposed proof succeeds neither in supporting nor illustrating this claim because instead of showing that the locus of such a particle satisfies his geometric definition of a straight line, Bolzano shows it is a spatial object such that "every part of it is similar to the whole". It is claimed then that "geometers know" that only the straight line has this property. It is trivial from the straight line definition (e. g. A542;DP, 29) that it will have the property that every part is similar to the whole because every (connected) part of a straight line will continue to satisfy the definition of a straight line, and (A32;BG, I§20) all straight lines are similar to one another. But it is the converse proposition that is required here and not only is it not proved, it is not clear that it can be proved using Bolzano's notion of similarity. However, this is not to say that the mechanical concept of a straight line may not be proved to satisfy the geometrical definition of DP<sup>S</sup>11(A533) in some other way. Our main concern here is the appearance and origin of this definition.

The first point to emerge unaffected by the above problem is the familiar prohibition of the empirical concept of motion from geometry and the reversal of the intuitive order of dependence. Mechanics depends, as far as proof is concerned, on geometry, never vice versa. This negative principle only leads to the rejection of possible definitions and concepts without any indication of the existence or source of a replacement.

Bolzano's comment in response to the first objection, (that the

definition reduces a line to a mere composition of points) is much more suggestive of the positive considerations leading to his definition. He does not deny that the line is composed of points but he is at pains to avoid this idea being misinterpreted. Three such misinterpretations which he rejects are: (i) that the line is the arithmetic sum of its points (instead "we must look not only at the set of points but also at the way they are put together" A536;DP, 23); (ii) that a finite set of points might be sufficient for a line; (iii) that each point of a line borders directly on the next one. The ideas of (i) and (ii) are related. In BG, II§27(A79) Bolzano said, with reference to the straight line, that "this object contains an infinite number of points, therefore it must be something qualitatively different from a mere system of points". He seems to have believed that moving from the finite to the infinite (in any collection of objects) inevitably involves (or reveals) relationships between the objects which do not exist in the finite case. It is the description of these relationships that causes the difficulty in reducing an object to its elements or constructing it out of these elements. For the definition of a line, the ideas rejected in (i), (ii), (iii) above can be summed up by saying that while a line is an infinite set of points it cannot simply be viewed as a concatenation or "string" of points. (Compare Baumgarten's definition in 3.3.2.) However, how else could we think of it? These considerations reveal the inadequacy of our "constructive" intuition in this case, (and incidentally the prevalence of the definition of a line via the concept of

motion). Consequently it is reasonable to suppose that it was in this way that Bolzano was led to abandon the attempt to construct the geometrical continua out of sets of points. Instead he seeks an essential characteristic of such continua in terms of their constituent points. And here the fact that there is to be no "next" in a continuous extension can quite naturally be expressed by requiring for each point that there are other points at arbitrarily small distances. The fact that the line is, as we should say, one-dimensional, is then guaranteed by requiring that there are only a finite number of neighbouring points at arbitrarily small distances. The concepts of surface and solid are then defined recursively as follows:

A spatial object at each point of which, beginning from a certain distance and for all smaller distances, there is at least one and at most only a finite set of separate lines full of points is called a surface in general (A564;DP, 51).

A spatial object at each point of which, starting from a certain distance and for all smaller distances, there exists at least one absolutely connected surface full of points, is called a solid in general.  
(A579;DP, 66)

By "an absolutely connected surface" Bolzano means a surface each part of which that is itself a surface has at least one line in common with the other parts that are surfaces. For a close comparison of these definitions with the work of Menger in the 1920's see Johnson [1] p. 298ff.



### 3. 4. 3. The Origin of the Definitions

In order to understand the emergence of this sequence of definitions there are two considerations which seem to be of fundamental importance. The definitions have a logical form which was unusual at this time, and they make use of a rather abstract concept of distance. The usual form of mathematical definition (of an object rather than relation) was as an abbreviation; the application of a symbol, word or phrase to a new combination of known objects (or symbols). In this sense it is the naming of the result of a construction. For example, there is the definition of a triangle as a construction from three straight lines (A27;BG. I§7) or the definition of straight line in terms of points (A79;BG, II§26). The existence of the new object defined thus depends on the existence of the known constituent objects and the possibility of the construction. In the case of the line, however, no construction or relationship on a set of points seemed to be successful. Instead the definition proceeds by regarding the line as a completed entity (which is composed of points but we cannot say how), and stating a property of it which acts as a defining characteristic. This approach is logically distinct from the former because now the existence of an entity satisfying the definition requires a new assumption or insight. It is a simple kind of implicit definition and could be regarded from a modern point of view as an axiom governing the primitive terms "point" and "line". Unfortunately Bolzano does not seem to realise the existence problem which such a definition raises. At least he nowhere mentions the need to assume that such an object as he defines a

line to be does actually exist. It is not altogether clear (see remarks in 2.4.4 and p.123) whether or not he believed we have a kind of direct apprehension of such conceptual objects as lines which might obviate the need for such assumptions. (The same distinctions in form, and criticisms over existence, apply also to the definitions of surface and solid as well as to their subsidiary types). The use Bolzano made in the definitions of the classical terms "genus proximum", "differentia specifica" seems to have been rather artificial and conventional. The terminology is certainly compatible with both sorts of definition mentioned here and does not seem to have motivated the one chosen.

The development of implicit definitions was highly important in the subsequent history of mathematics. Consider the definition of a topology as a set of sets with certain properties, or even the algebraic definitions of group, field etc. now more conveniently expressed in terms of sets of axioms. The progression from implicit definition to the more flexible schema of primitive terms and axioms is quite natural. The crucial logical (and psychological) distinction was the move from the simple, constructive definition to the more abstract implicit definition. Thus the transition made by Bolzano from his definition of the straightline in BG to the general geometric definitions in DP is a development of considerable historical significance. Van Rootselaar says in Bolzano [1] (Vol. 2B 2/1, p.14) that although the definition of line in general is usually considered an original achievement of Bolzano, "I have reason to suppose the definition was then fairly common." But he does not say where it is to be found.

#### 3.4.4. Concept of Distance

It is reasonable to suppose that such implicit definitions arise from some insight or abstraction which produces a new concept or the refinement of an old concept. This is then used in the formulation of the defining characteristics. In the present case of Bolzano's definitions of geometric objects it is the refinement of the concept of distance which is fundamental to their formulation. At DP§20(A547) he distinguishes, in reference to his definition of length in the previous paragraph, between distance and length saying that the former is simpler than the latter because any system of two points has a distance associated with it, while we need to consider a line joining the points before we can apply the concept of length. Of course, the idea of a distance between two points is easy and familiar but we can see how, having separated this concept from that of length, it would be much easier to arrive at the more abstract viewpoint by which one can consider the class of points (finite or infinite) at a certain distance from a given point, i. e. what we now call a spherical neighbourhood. This concept of a neighbourhood defined by a distance, and subsequently an arbitrarily small distance, is the central component in all these definitions in DP. The definitions themselves represent the first major insight into the nature of abstract continuous extension. It is an insight which has been generalised and shown itself to be enormously fruitful in the subjects of topology and functional analysis. It is not claimed here that there is any psychological (or other) priority between the appearance

of the neighbourhood concept and the gaining of the insight expressed in the definitions. It is sufficient for our purpose only to point out the undeniable positive contribution to this insight that was made by Bolzano's refinement and use of the concept of distance.

## Chapter 4: Analysis I

### 4.1. General Introduction

#### 4.1.1. The Meaning of "Analysis"

The word "analysis" is used today as a generic term to denote several branches of mathematics which have in common their use of ideas and methods based on the concept of limit. For the proper exploitation of this limit concept a suitable domain for the values of the variables needs to have been specified and defined (e. g. real or complex numbers). Thus the simplest kind of modern analysis (that of a real variable) would not have been possible or even comprehensible at the outset of the 19th century; its two fundamental concepts had not been properly established. The limit concept was still highly controversial and often ill-formulated, and the construction or definition of the real numbers was not to begin for at least another thirty years.

Nevertheless the term "analysis" was widely used in European mathematics throughout the 17th and 18th centuries. Its mathematical meaning early in the 17th century derived from two main sources: (i) the original Greek usage as part of a technique for finding proofs or solutions (usually for geometrical problems), and (ii) the particular success at that time of algebraic methods applied to geometrical problems

(for example, by Vieta and Descartes). In Greek mathematics the term "analysis" referred to the process of working backwards deductively from a theorem until something known or assumed to be true was reached, and then (the synthesis) trying to work forwards along the same path. (See, for example, the description in Pappus [1].) Algebra was well suited to this method for, if a problem could be expressed by means of an equation, the deduction of a solution or an identity from the equation could often successfully be reversed. Vieta was deliberately reviving the Greek term when he called his algebra textbook (1591) In Artem Analyticam Isagoge (Introduction to the analytic art).

Algebra was typically an "art" at this time, it was a procedure for discovery and an aid to doing geometry which was still the main substance of mathematics. Although Vieta's example of referring to algebra as the "analytic art" or as "analysis" was not generally followed in the 17th century these terms were increasingly used in mathematics (in the general sense of a "method") and were associated with algebra, especially the use of equations. An example is in the title of the first formulation of Newton's discovery of the calculus (written 1671, published 1711), De Analysi per Aequationes Numero Terminorum Infinitas (On Analysis by means of equations with an infinite number of terms).

The family of problems which were ancestral to what became known simply as "calculus" consisted of geometrical problems: for example, finding tangents and areas associated with various curves. Many of these

problems had already been solved (in some cases with the aid of the new co-ordinate geometry) by men such as Wallis, Barrow, Fermat and Huygens. But with the general methods and the direct relationship of the tangent and area problems discovered by Newton and Leibniz the techniques of algebra (not only equations but the development of expressions in infinite series) began to take on a new status. Algebra was no longer simply a means to an end (the solution of geometric problems), it was becoming established as an end in itself. Accordingly the status of analysis was enhanced, associated as it was with the success of the algebraic methods in resolving the tangent and quadrature problems.

In the 18th century the word "analysis" was in common use in English, German, Latin and French mathematical writings though its meaning varied considerably during the century and from one country or language to another. The important new factor in the use of the word was the impact and development of calculus. From its origins it was natural that calculus was regarded as an extension of algebra, or at least of "algebraic" geometry, a kind of "algebra of the infinite" as it has been described (Kline [1] p. 324). In works written in English, following Newton's Methodus Fluxionum... (Newton [1] 1671, published 1736), the calculus was usually just referred to as the "method of fluxions". In Latin and French however, the three components of being a powerful mathematical method, being algebraic, and seeming to require infinitesimals, made the use of some equivalent to "infinitesimal

analysis" most appropriate. For example, there were the works by L'Hôpital, Analyse des infiniments petits (1691) and by Euler, Introductio in Analysin Infinitorum (1748).

The several meanings of the word "analysis" in English are illustrated by the entries in Thomas Walter's Mathematical Dictionary (Walter [1] 1762). There are five such entries. First there is the quite general sense of "resolving a thing into its component principles, in order to discover the nature of the thing"; then as a mathematical method in the Greek sense mentioned above; then "analysis of infinities" with the instruction to see "Fluxions"; then the "analysis of powers" meaning the extraction of roots. Finally there is the entry "Analytics" defined as "algebra, or the doctrine of analysis". Concerning this last identification Kline says, "In the famous eighteenth century Encyclopédie, d'Alembert used algebra and analysis as synonyms" (Kline [1] p. 323). Apart from the existence of the several senses of "analysis" already mentioned such a straightforward identification could only be made in a rather loose sense. D'Alembert actually writes as follows:

Analyse..... est proprement la methode de résoudre les problèmes mathématiques, en les réduisant à des equations.

L'Analyse, par résoudre les problèmes, employe le secours de l'Algèbre, ou calcul des grandeurs en général: Aussi ces deux mots, Analyse, Algèbre sont souvent regardés comme synonymes.

(D'Alembert [1]).



Rather than identify them, D'Alembert distinguishes analysis and algebra (there are long separate articles on each). In fact the brief summary quoted here is a fair generalisation of the mathematical use of the term "analysis" in the mid-18th century. It was primarily a method, a procedure for solution; and as such always an algebraic method in contrast to geometrical, empirical or general inductive methods. As a subject in itself it was associated, and sometimes identified, with algebra, but more especially as we have seen, with those parts of algebra which involve infinitesimals and infinite series. To this extent analysis encompassed the differential and integral calculus.

The above summary also applies to the German language writings of the 18th century but here the usages were well organised. The words "Algebra" or "Buchstabenrechnung" (calculation with letters) were usually reserved for very elementary matters, rules of signs, notations, manipulation of fractions etc. The main part of algebra (equations, series, functions etc.) was "Analysis der endlichen Grössen" (analysis of finite quantities), while the differential and integral calculus was contained in the "Analysis der Unendlichen" (analysis of the infinite). These two phrases were common textbook titles, for example volumes with each of these titles form the third part of Kästner's Die mathematische Anfangsgründe. In Klügel's Mathematisches Wörterbuch (Klügel [2], Part I, 1803) the entry "Analysis" is separated into Analysis als wissenschaftliches System (analysis as a scientific system) and Analysis als Methode (analysis as a method), reflecting the ambiguity to

which we have already referred. In the former section analysis in a very general sense is described as the study of any kind of combination of quantities through calculation. In this sense it would include algebra as its first part. Then analysis in a "narrower and proper" sense is defined as "the science of the forms of quantities". (Klūgel added that this is sometimes called the theory of functions). In this sense he distinguishes algebra and analysis: the former considers quantities as known or unknown, the latter considers them as constant or variable. There then follows the division into "Analysis der endlichen Grössen" and "Analysis des Unendlichen" as described before. An alternative and quite common terminology for these finite and infinite kinds of analysis was "niedere Analysis" ("lower" or elementary analysis) and "höhere Analysis" (higher analysis) respectively. (See for example Rogg[1].)

By the end of the 18th century the algebraic and infinitesimal methods that had been used to solve the geometry problems of the 17th century had proved so enormously successful in a whole range of physical problems that, under the general heading "analysis", they had become the dominant part of mathematics. The intuitive concepts of space, time and motion, long associated with geometry and mechanics, were losing ground to the more formal manipulation of functions and equations by the methods of calculus. It was inevitable after the advent of analytic geometry that these developments should lead, during the 18th century, to analytical or "rational" mechanics. In the Preface to his Mécanique analytique (1788) Lagrange wrote:

.... No diagrams will be found in this work. The methods which I expound in it demand neither constructions nor geometrical or mechanical reasonings, but solely algebraic operations subject to a uniform and regular procedure. Those who like analysis will be pleased to see mechanics become a new branch of it .... (Translation as in Kline [1] p. 615)

The purely algebraic, or analytic, treatment of mechanics described in this quotation was evidence of the major transition taking place in the development of mathematics. This was not only the rise of analysis and its subsequent arithmetisation. It was the replacement of the empirical and intuitive elements in mathematics by more formal symbolic and arithmetic procedures. More fundamentally arithmetic (and thereby operations with symbols) was being freed from the need to be interpreted geometrically. The truth of Euclidean geometry was giving place to the truth of arithmetic. Geometry, however, did not decline, it developed enormously (and in its turn was finally freed from the need to be empirically interpreted) over the period of the "arithmetisation" of analysis. The latter was a huge and haphazard process, occurring to no-one suddenly and being advanced and retarded over many generations until there was a general and stable consensus.

After the work of Euler and the Bernoullis it was the French mathematicians of the late 18th century who contributed most to the prominence of analysis. By around 1800 analysis was not simply a new and fruitful

branch of mathematics, it had displaced geometry as the paradigm of mathematics. And this was understood, for example by Laplace, in terms of its superior generality and degree of abstraction. In his Exposition du système du monde (1796) Laplace writes:

The algebraic analysis soon makes us forget the main object [of our researches] by focussing our attention on abstract combinations and it is only at the end that we return to the original objective. But in abandoning oneself to the operations of analysis, one is led by the generality of the method and the inestimable advantage of transforming the reasoning by mechanical procedures to results often inaccessible to geometry. (Laplace [1], translation as in Kline [1] p. 615).

Confidence in the future of analysis was reflected in the great number of new and widely read works appearing soon after the French revolution. These included Lagrange's Théorie des Fonctions analytiques (1797, German 1798, 2nd ed. 1810), and Leçons sur le calcul des fonctions (1806), Traité élémentaire . . . . (1802, 2nd ed. 1806, English 1816, German 1817), Carnot's Réflexions sur la métaphysique du calcul. . (1797, German and English 1800, 2nd ed. 1813).

The main purpose of these works, and others like them, was to spread and teach the methods and achievements of the calculus. In most standard textbooks one could expect to find all the elementary processes and rules of differentiation and integration for a wide range of functions,

applications to finding maxima, minima and singularities of curves and surfaces (also their rectification and quadrature), the solutions to many ordinary and partial differential equations, and perhaps Lagrange's calculus of variations.

There were numerous ways of introducing the concepts of differential and of derivative. They either involved infinitesimals explicitly, the use of a limit without a proper arithmetic definition (though L'Huilier virtually had this correct), or the use of infinite series without a clear definition of convergence. None of these methods were regarded as wholly satisfactory though Lagrange's technique of assuming a Taylor series expansion for all functions enjoyed considerable, if short-lived, enthusiasm in the first decade or so of the 19th century. The works of Lacroix and Carnot mentioned above adopt a kind of amalgam of several of these methods. In spite of being a matter for concern, and numerous attempts to improve the relevant definitions, there was really little sense of "crisis". The foundations of analysis were not as significant to the mathematicians of the early 19th century as they have become in our eyes. Logical structure was secondary to truth. For although various peculiarities and paradoxes were known to arise from using infinitesimals and infinite series, these could cast no doubt at all on the truth of the main body of analysis. That was guaranteed both by its overall coherence and its overwhelming success in applications. The foundations were desirable not so much for the sake of truth but in order to conform the subject to the newly

developing ideal of mathematics as being independent of any intuitive appeal to such things as vanishing quantities, motion, or ideas borrowed from geometry.

#### 4.1.2. Bolzano's view of Analysis

Bolzano was fairly well acquainted with the mathematical literature of his time. In his papers of 1816 and 1817 there are over twenty references to important works on analysis which had been published within the previous twenty years. These included works by Lagrange, Lacroix, Gauss and Crelle. He also knew at least some of the works of Newton, Euler and D'Alembert. To a large extent then it was natural that he inherited the general views of the time on analysis that we have outlined in the previous section. There are a number of specific remarks which show that this was true. In BL (Preface) the differential and integral calculus are classified as higher analysis. (Bolzano says he regards this subject as containing "the most important discoveries in mathematics" (A495;DP, VI)). On the same page he explains the term "purely analytic" as being equivalent to "purely arithmetic, or algebraic" and says that a "purely analytic procedure" is,

one by which a certain function is derived from one or more other functions through certain changes and combinations which are expressed by a rule completely independent of the nature of the quantities designated.

The example is given of forming  $(1 + x)^n$  from  $(1 + x)$ .

Thus far this seems quite a straightforward interpretation of "analytic" in terms of algebraic operations. On closer inspection, however, there are some significant differences in Bolzano's view of analysis not only from the modern understanding but also from the views of his contemporaries. From our modern viewpoint what comes first logically in developing analysis, defining a domain of values such as the real number system, came about last of all historically. It is in consequence of a proper (analytic) definition of the number concept (as well as the continuity of a function) that we should regard the main result of RB, the intermediate value theorem, as a theorem of analysis. Bolzano regards the result, as was usual at the time, as part of "the theory of equations" (A431;RB, 1). Doubtless he would have regarded this theory of equations as part of analysis (*Analysis der endlichen Grössen*), but more because it is algebraic than because of the underlying limit concept. So in spite of the careful continuity definition (applied in BL and RB), the outlook and priorities are rather different from the viewpoint which emerged later in the century. It was regarded as more significant to find a way of solving an equation than to prove a property of continuous functions.

Concerning the way in which Bolzano's understanding of analysis differed from that of his contemporaries there are two points to be made: one a matter of substance, the other a matter of emphasis. The substantial matter is the categorical rejection in BL and DP of infinitesimals or infinitely

small quantities. He says that the usual definition of such quantities as "actually smaller than every . . . . . conceivable quantity" is "contradictory" (A269;BL, V and A497;DP, VIII). With specific reference to calculus Bolzano assumes "it must be known to everyone" that the rules of calculus "can be expressed in such a way that the concept of the infinitely small (which might perhaps often be associated with the symbols  $dx$ ,  $dy$ ,  $dz$  . . .) is completely avoided" (A495;DP, VI). By this time this was probably not even a minority view. It was quite popular to reject both infinitesimals and limit concepts. The full title of Lagrange's 1797 work was, "Théorie des fonctions analytiques contenant les principes du calcul différentiel, dégagés de toute considération d'infiniment petits, d'évanouissans, de limites et de fluxions, et réduits à l'analyse algébrique des quantités finies." And a substantial work by DuBourguet (1810, referred to in DP) is entitled, "Traité élémentaire du calcul différentiel et du calcul intégral, indépendans de toutes notions de quantités infinitésimales et de limites."

Bolzano himself was not in the tradition of these works; he preserved a clear and essential limit concept while rejecting the infinitely small. However, the latter was still used by several authors and was a matter of serious controversy. For example, J. Schultz in whom Bolzano had found an ally on several issues, is criticised (A497;DP, VIII) for his use of the infinitely small. Whereas for some authors it had been a characteristic of analysis since the 17th century that it involved the algebraic treatment of the infinitely



small, it is clear in Bolzano's early writings that for him such quantities had no proper place in any part of mathematics.

The other distinctive feature of Bolzano's thinking on analysis is the extent to which he dissociated the subject from geometry. For someone like Isaac Barrow in the 17th century the early problems of calculus were geometrical problems and they were most appropriately solved by purely geometrical means. On the Continent during the 18th century the algebraic formulation, aided by Leibniz's notation and the development of the function concept, passed from being a convenient description of a geometrical or mechanical situation to being an independent body of theory capable of geometrical or mechanical interpretation. A symbolism which began as a servant to geometry became, not its master, but independent and superior (in its generality) to geometry. To guarantee this independence it was therefore essential that analysis should borrow nothing from geometry unless it could be reformulated completely in arithmetic terms. Bolzano saw this clearly, and especially in terms of the generality of arithmetic and analysis. The quantities of geometry were "spatial quantities" and accordingly theorems about them were only a special case of more general theorems (A435;RB, 7).

The attitudes of late 18th century mathematicians to the relationship of analysis and geometry were various and muddled. The intermediate value theorem of RB provides a good example. It was widely accepted and used. It was clearly "true", but such clarity and truth was a product of the

geometrical interpretation and did not extend to the general functional formulation of the theorem. Kästner and Gauss saw the need to give a properly analytic proof but many others did not and seem to have been quite content with the appeal to geometric intuition. He is therefore far from being conventional when Bolzano emphasises repeatedly in RB Preface that the proof of the intermediate value theorem must not make use of concepts or methods borrowed from geometry. The title of RB (Purely analytic Proof...) reinforces this understanding of "analytic", and in the first paragraph of the Preface "a purely analytic truth" is contrasted with "a geometric consideration". The implication is that it is necessary to the proper sense of "analytic" that it implies "non-geometric". The arguments for this in RB are the matter of generality already mentioned (leading to logical circularity) and also the "genus argument" against using concepts from one kind of theory in another (A434;RB, 6; see 2.4.1.). In fact, in consequence of Bolzano's regard for "kinds" one might be left in doubt from reading RB alone whether he would actually endorse the application of algebra to geometry (or anything else), i. e. whether he would allow analytical geometry as valid mathematics! Such doubt is completely dispelled by, "the most general way of determining the nature of a spatial thing is to state certain equations between co-ordinates" (A495;DP, 6). Nevertheless the tension in these early works between the principles of genus on the one hand, and generality on the other, is never resolved. The genus principle invoked on several occasions with the phrase

μεταβασις εἰς ἄλλο γένος [transition to another kind] seems to imply mutual exclusion of theories and their respective concepts. The generality principle implies a one-way relationship of inclusion, or of the application of the more general to the less general. With DP the generality principle is clearly the dominant one; the rectification problems are geometrical but they are being solved by the powerful, more general methods of analysis. However, the main conclusion to which we wish to draw attention here is that in both RB and DP various considerations led Bolzano to emphasise more than many of his contemporaries that analysis derived its meaning partly in contrast to geometry, and that being analytic implied being non-geometric.

As we have seen in the previous two chapters Bolzano's main motivation in his mathematical work was the improvement of the foundational aspects of theories: the clarifying of concepts, and the provision of rigorous, appropriate proofs for the important theorems. It would naturally have been the foundational problems in analysis that most interested him and there was plenty of scope for his contributions. That he was well aware in 1810 of the continuing confusion in this area is clear from the following:

I do not want to mention anything here about the defects in the higher algebra and the differential and integral calculus. It is well known that up till now there has not been any agreement on the concept of a differential. Only at the end of last year the Royal Jablonosvky Society of Sciences at Leipzig gave as their prize-question the

discussion of different theories of the infinitesimal calculus and the decision as to which of these is preferable. (A96;BD, V)

That he says no more at this stage about the foundations of calculus is evidently because there were already more problems than he could solve to his satisfaction in the more elementary parts of mathematics.

#### 4.1.3. Bolzano's view of his works on analysis

The programme started in BD of re-organising mathematical theories (including the simplestones of arithmetic and geometry) did not progress far. Parts of the "zweyte Lieferung" (second issue) of BD were written but not published (see Bolzano [1] Vol. 2A5). There was no lack of enthusiasm on Bolzano's part, but to continue the work he clearly needed such enthusiasm or interest to be shown on the part of other mathematicians. Since this was not forthcoming in reviews or correspondence he decided to postpone the major work of BD and, as he candidly acknowledges, "make myself better known to the learned world by publishing some papers which, by their titles, would be more suited to arouse attention" (A455;RB, 27). He explains in the same passage that this also applies to BL and DP. There seems to have been some difficulty in finding publishers for these works and so in addition to the desire to obtain criticism and interest in his work there may also have been a simple commercial motive. At all events the topics of these analysis works were

chosen for publicity purposes—to gain attention. In this respect they were not conspicuously more successful than his earlier efforts.

There are few new results proved in these papers; their main purpose in each case is to give new proofs of well-known theorems that were essential to analysis. Bolzano regarded them each as being the first truly rigorous proof. This attention to the proofs of basic theorems was the result of fundamental conceptual requirements: the removal from analysis of ideas of infinity and infinitesimals, as well as the remnants of geometrical intuitions. These papers really represent just a few examples of how Bolzano would like to reorganise analysis. He describes BL as, "a sample of a new way of developing analysis" (A279;BL, XV).

The aim of attracting some attention by means of these analysis works was eventually achieved, but long after Bolzano's death. The first important recognition of Bolzano's work was in connection with BL in Hankel's article Grenze (Limit) in Ersch and Grüber's Allgemeine Encyklopädie in 1871. Thereafter there are regular references to Bolzano's early work on analysis in the literature - often, however, confined to footnotes. (For example, there are more than a dozen such footnote references to BL and RB in the Encyklopädie der mathematischen Wissenschaften between 1898 and 1916).

However, the modern recognition of Bolzano's work raises an important historical point. From Hankel's article in 1871 to the extracts in Birkhoff [1] commentators have been inclined to give particular credit to Bolzano

for matters which were not of great significance to him. We are thinking here of the proper arithmetic concept of limit and the concept of the convergence of infinite series. These concepts had been used in some form for a long time (see 4.2.2 and 4.3.2) and judging from other examples in his writings (e. g. A439, 448; RB, 11, 20; A557; DP, 44) Bolzano would not have been too modest to claim them as new and original if he had regarded them as such. He does not do so. Undoubtedly he had great confidence in these definitions; they satisfied his conceptual requirements, he knew they would be fruitful and effective in the development of analysis, but never does he claim them to be his own. Nor could this be explained by his trying to avoid disapproval at a time when, at least in some quarters, the limit concept was simply unfashionable - he could hardly disguise the fact that its use was fundamental to his approach in all three of his works. Thus in judging Bolzano's work it is worth distinguishing carefully between the definition of a concept (for which, in the case of limit Bolzano would not claim responsibility), and the liberation of that concept into effective use in a theory (which, armed with a sense of the modern value of the concept, we may rightly trace back to Bolzano). The effective use of a new concept requires the vision of an overall context or theory within which the new concept has clear connections with already well-established concepts of the theory, and consequently clear connections with the existing problems of the theory.

## 4.2. Infinitesimals and the Limit Concept

### 4.2.1. Introduction

In this chapter we are outlining the conceptual preliminaries to Bolzano's main proofs. These are the introduction of arbitrarily small quantities and their effective use in expressing the concept of limit, the precise statement of a convergence criterion for infinite series, and the definition of the continuity of a function.

From the modern point of view there is a clear logical priority among these concepts. The concept of limit should be defined first; this then forms an essential component in the definitions of convergence and continuity. Simple and obvious as this may appear today after a century of well-established use, it had taken about two centuries from the first emergence of these concepts until such an understanding as we now have came to be widely accepted and taught (roughly, from 1670 to 1870). This process of clarification was attended by two major influences. One was the general replacement of geometry by arithmetic and algebra as the new paradigm of mathematics; the other was the presence of infinitesimals and the apparent need to incorporate them into the wider arithmetisation programme which the success of calculus methods had accelerated. Bolzano's work on the foundations of analysis was naturally affected by the variety and confusion of the 18th century contributions.

#### 4.2.2. Infinitesimals and the Limit Concept before 1815

Infinitesimals did not enter mathematics with, or because of, the calculus. They were considered by Greek mathematicians in discussion of the infinite divisibility of space and time (e. g. Zeno's paradoxes). They were also in common use by 17th century geometers (e. g. Cavalieri) in solving quadrature and tangent problems. Infinitesimals, or infinitely small quantities, were fundamentally geometric in origin and use; they were typically thought of as measuring the magnitude of a point or the distance between two "adjacent" points.

The mathematical use of infinitesimals is sometimes traced back to Archimedes' use of the method of exhaustion (e. g. Leibniz [5]). But in at least some cases (e. g. Measurement of a Circle Prop. 1) this method does not, even implicitly, involve infinitesimals. It would be more accurate to trace their mathematical ancestry to the technique (sometimes called the "discovery method") in Archimedes' The Method, where a solid is regarded as consisting of infinitely many, infinitely thin, surfaces. Now although the Greeks could not have had an arithmetic concept of limit, they did understand what must be its immediate predecessor, the concept of arbitrarily close approach. It is this which is at the heart of the method of exhaustion. Thus both infinitesimals and the limit concept have their origins in geometry. From their association in Archimedes' work until well into the 19th century they were closely, and yet uneasily, linked with one another.



The 19th century is often regarded as a period of the "arithmetisation of analysis" (e. g. Klein [1]). A pre-requisite for this, the arithmetisation of arithmetic, had occurred about two centuries earlier. At the turn of the 17th century number was still typically regarded metrically in terms of geometric magnitude. The rise to predominance of arithmetic and algebra was not, of course, a co-ordinated plan. It is a pattern suggested to us in retrospect by events (usually publications) which in themselves were haphazard, expedient and perhaps of little significance in their own time. What is relevant here is how the three concepts, limit, convergence and continuity fitted into this pattern. One of the first arithmetic interpretations of limit was given by Wallis in 1655. He says, in reference to a certain (convergent) series, that by increasing its number of terms the difference of its value from a certain quantity "may be continually diminished so that it eventually becomes less than any arbitrarily assignable quantity" (Wallis [1]). This is emphatically not an arithmetic formulation of the modern limit concept as misleadingly suggested in Pringsheim [1] p.64, Kline [1] p. 963. It is only an essential preliminary step, the awareness that the notion of arbitrary close approach as it appears in the method of exhaustion could be usefully expressed in a purely arithmetic context. It was to be another major step requiring the concept of a variable, to come to the formulation and use of the modern symbolic definition of the limit concept.

As we said above, Wallis's description of the limit property is given

in the context of infinite series. The terms "convergence" and "divergence" are used for the first time by J. Gregory in 1668 (see Kline [1] p.461). The concepts were used only intuitively and vaguely. A function was still regarded (by Newton for example) as a curve and so continuity was still a geometrical matter.

The advent of the calculus methods initiated by Newton and Leibniz in the late 17th century focussed much more attention on infinitesimals or infinitely small quantities. The general methods they adopted required an arithmetic approach. The arithmetic treatment of the necessary infinitesimals seemed to be regulated more by the known results than by any consistent procedure. Inconsistencies and paradoxes were easily produced. Thereafter infinitesimals played a major and controversial role in the foundations of calculus. We are only concerned here with the various meanings that were given to infinitesimals and their relation to the limit concept.

Both Newton and Leibniz initially employed infinitesimals in defining and calculating derivatives, but they each later sought ways of avoiding reliance on them. In De Quadratura... (Newton [2] 1676, published 1704) Newton seeks to avoid infinitesimals entirely: the increment in  $x$  (denoted by "o") is allowed to approach zero through ordinary finite values and the ratio of this increment to a corresponding change in a function of  $x$  approaches the "ultimate ratio". The latter is clearly understood in terms of an intuitive notion of limit, not a substitution or calculation with zeros;

Ultimate ratios in which quantities vanish are not, strictly speaking, ratios of ultimate quantities, but limits to which the ratios of these quantities decreasing without limit, approach, and which, though they can come nearer than any given difference whatever, they can neither pass over nor attain before the quantities have diminished indefinitely.

(Newton [3] )

Leibniz, although giving his main emphasis to the methods and results of calculus (rather than its concepts), could not ignore the peculiar behaviour required of infinitesimals. In John Bernoulli's treatise of 1691 on the differential calculus (which was mainly material from Leibniz's lectures), he states as a postulate: "If a quantity is diminished or increased by an infinitely small quantity it is neither diminished nor increased" (Bernoulli, John [1] ).

At various times Leibniz offered all sorts of interpretations of infinitesimals, but his explanations were basically of two kinds. There was the interpretation as a variable (the "potentially" infinitely small), according to which describing a quantity as "infinitely small" was just a manner of speaking signifying that the quantity, "could be taken as small as one wishes" (Leibniz [6] p. 90).

And there was the interpretation as a constant, as something "less than any quantity" (Leibniz, [5] p. 322) which, even if a fiction, could be used as a tool, "as algebraists retain imaginary roots with great profit" (Leibniz, [7] p. 150).

These represent the main attitudes to infinitesimals throughout the 18th century. They were either completely replaced (e. g. by limits or by

the intuition of an instantaneous rate of change, so-called fluxions), or else they were tolerated. They were tolerated either as arbitrarily small, but ordinary, quantities, or as fictional "ideal" adjuncts to ordinary arithmetic. Generally speaking British mathematicians favoured the replacement, Continental mathematicians favoured the toleration, of infinitesimals. But their use was almost universally distrusted. Berkeley's criticisms in The Analyst... (1734) of infinitesimals and differentials had provoked a flurry of publications but no real answers. The attitude of John Landen in his Residual Analysis (1764) was typical: he said he wanted to develop calculus "without the aid of any foreign principle relating to an imaginary motion or incomprehensible infinitesimal" (Landen [1]). He effectively used a method of limits. The works of Fontenelle [1] (1727) and Carnot [1] (1797) were exceptional in maintaining the real existence of infinitesimals. In Wolff [2] p. 597-602 the infinitely small was said to be actually impossible but could be a convenient fiction useful for discovery. In his dissertation De vera infiniti notione (Kästner [4]), Kästner says that infinity and the infinitely small are not quantities but just express the possibility of unbounded increase or decrease. So to say that the last term of the series  $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \dots$  is 0 and its sum is 1 merely means that the terms decrease indefinitely and their sum approaches 1 indefinitely.

The standard phrase used to describe infinitesimals was "less than every assignable quantity". Thus D'Alembert in the Encyclopédie Méthodique..... under the general article Infini (Vol. 8, 1765) writes:

Infiniment Petit (Geom.) on appelle ainsi en Géométrie les quantitiés qu'on regarde comme plus petites que toute grandeur assignable. Nous avons assez expliqué au mot Différentiel ce que c'est que ces prétendues quantités, & nous avons prouvé qu'elles n'existent réellement ni dans la nature, ni dans les suppositions des Géometres.

Euler sensibly remarked that for a quantity to be less than every assignable quantity it must necessarily be zero. But then two zeros were said to be capable of either an arithmetic or a geometric ratio to one another. Thus the rejection of infinitesimals was only nominal, different kinds of zeros, or ratios between zeros, were introduced. (In Torrelli [1] there is an attempt to distinguish a geometrical and a metaphysical ratio of zeros). Schultz rejected the non-zero infinitely small as a mere fiction but then (Schultz [4]) described the differentials  $dx$ ,  $dy$  as zeros which are equal in quantity but different in quality.

In spite of the early British work on the limit concept (Wallis, Newton, Robins), perhaps the clearest 18th century advocates of the fundamental nature of the limit concept were d'Alembert and L'Huilier. D'Alembert's formulation in the Encyclopédie article Limite (Vol. 9 1765) was as follows:

Limite, s.f. (Mathémat). On dit qu'une grandeur est la limite d'une autre grandeur, quand la seconde peut approcher de la première plus près que d'une grandeur donnée, si petite qu'on la puisse supposer,

sans pourtant que la grandeur qui approche, puisse jamais surpasser la grandeur dont elle approche; ensorte que la différence d'une pareille quantité à sa limite est absolument inassignable.

... A proprement parler, la limite ne coïncide jamais ou ne devient jamais égale à la quantité dont elle est la limite; mais celle-ci s'en approche toujours de plus en plus, & peut differer aussi peu qu'on voudra.

There was still therefore the association of limit with the infinitely small through the notion of an "absolutely unassignable difference". The insistence on never reaching, or being equal to, the limit was probably due to the geometrical example of a limit (which immediately follows in the D'Alembert article) from a sequence of polygons inscribed in a circle. However, D'Alembert (in the same article) was perfectly clear on the place of the limit concept: "Le theorie des limites est la base de la vraie Metaphysique du calcul differentiel."

L'Huilier adopts a similar position in his paper Exposition élémentaire des principes des calculs supérieurs (L'Huilier [1]1787). He has a clear understanding that analysis can be rigorously founded on the method of limits but he advocates continuing to use the language of infinitesimals. His formulation of the limit concept is sound though rather restricted and still only verbal:

Given a variable quantity always smaller or greater than a proposed constant quantity but which can differ from the latter by any proposed quantity however small this constant quantity, is called the limit in greatness or in smallness of the variable quantity. (L'Huilier [1])

The variable in L'Huilier's definition is therefore not allowed to oscillate towards a limit.

The time was not yet ripe for these notions to be taken up and improved: there seems to have been no general feeling in the 18th century for the potential of the limit concept. L'Huilier's work was not widely read, and according to Boyer [1], D'Alembert's limit concept appeared to some as, "enmeshed in as dark a metaphysics as was that of the infinitely small." Indeed it appears to have been the popular opinion to be as deeply suspicious of limits as of infinitesimals. This distrust of the limit concept must have been further reinforced by the wide popularity and acclaim given to two works appearing in 1797. These were Lagrange's Théorie des fonctions analytiques... (Lagrange [3]) and Carnot's Réflexions sur la métaphysique du calcul infinitesimal (Carnot [1]). The former explicitly excludes limits and the latter emphasises the superiority of infinitesimal methods over the calculation of limits.

However, during the 18th century the concept of infinitesimal had been considerably refined. The varieties of interpretation had been subject to a gradual process of the survival of the fittest. Interpretations as special

sorts of non-zero constants, or special kinds of zeros had virtually become extinct. The general tendency was to interpret the phrase "infinitely small" as a manner of speaking which did not so much denote a special kind of quantity (e. g. one which is already "less than any assignable quantity"), but rather a quantity which could be taken as small as desired. Carnot, in the work mentioned above, uses language which combines both ideas ambiguously:

We will call every quantity which is considered as continually decreasing, (so that it may be made as small as we please, . . . .) an infinitely small quantity. . . .

. . . . quantities called infinitely small are never quantities actually nothing nor even quantities actually less than such and such a determinate magnitude, but merely quantities . . . . allowed to remain variable. . . . continually decreasing until they become as small as we wish. . . . (Carnot [1] p. 15)

There is clearly a problem here in the notion of a variable. Variable quantities are being regarded as special kinds of quantities of which the change or "continual decrease" is part of their nature rather than our choice. But Carnot was one of the last to use the term "infinitely small quantity" in such a definite, objective sense. After 1800, and even later in the 19th century on the Continent, the term "infinitely small" was often used, perhaps innocently, in the sense of "arbitrarily small". Thus for example, Cauchy in 1821 uses "infiniment petit" in this way in connection with his definition



of the continuity of a function  $f(x)$ :

on attribue à la variable  $x$  un accroissement infiniment petit  $\alpha, \dots$

la valeur numérique de la différence

$$f(x + \alpha) - f(x)$$

decroit indefiniment avec celle de  $\alpha$ . En autres termes, la fonction  $f(x)$  restera continue par rapport à  $x$  entre les limites données, si, entre ces limites, un accroissement infiniment petit de la variable produit toujours un accroissement infiniment petit de la fonction elle-même. (Cauchy [1]).

The limit concept as we have seen, made very little progress and gained no general approval during the 18th century. Its 19th century revival began with the work of Lacroix. In 1797 his Traité du calcul... appeared but as far as the foundations were concerned it was rather confused. He presented Lagrange's method of Taylor series expansions in terms of limits, but there was also reference to the limit of a divergent series and the derivative as a quotient of zeros. This was all much improved in the highly successful abridged version of 1802 (Traité élémentaire...). Here the method of Lagrange was dropped and the limit concept was clearly made basic. In the second edition of the Traité du calcul... (1810) there is an improved version of the limit concept. With reference to the example of a function  $\frac{ax}{x+a}$  he writes:

La différence entre  $a$  et la fraction proposée, étant exprimée en

général par

$$a - \frac{ax}{x+a} = \frac{a^2}{x+a},$$

devient d'autant plus petite que  $x$  est plus grand, et peut être rendue  
moindre qu'aucune grandeur donnée, quelque petite que soit cette  
grandeur; ensorte que le fraction proposée peut approcher de  $a$  aussi  
près que l'on voudra;  $a$  est donc la limite de la fonction  $\frac{ax}{x+a}$ ,  
relativement à l'augmentation indéfinie que peut recevoir  $x$ .

..... la fonction  $\frac{ax}{x+a}$ , quoique pouvant s'approcher indéfiniment  
de la limite  $a$ , ne saurait jamais l'atteindre et à plus forte raison la  
surpasser; mais ce serait à tort qu'on insérerait cette circonstance,  
comme une condition dans la définition générale du mot limite:.....

(Lacroix [1])

There are two improvements here on the limit concept of D'Alembert and  
L'Huillier. The function is allowed to reach its limit and it is made perfectly  
clear how the "variable quantity" varies.

The evolution of the limit concept was slow and tortuous; it was simply  
not easy to understand in arithmetical terms. R. Woodhouse (a Senior  
Wrangler and Fellow of Caius College Cambridge) wrote in 1803, "the def-  
inition of a limit is neither simple nor concise" Woodhouse [1]. The reason  
for this difficulty lay in the fact that the formulation of the concept was funda-  
mentally connected with deeply rooted geometrical ways of thought and

language. The main requirements for the limit concept were to develop a suitable notion of a variable, to find a suitable general symbolism for sequences and functions and to establish the existence of numerical limits (especially when these were irrational).

The method of exhaustion had employed only geometric objects: a changing one (e. g. a polygon of which the number of sides was repeatedly doubled) approached arbitrarily closely to a fixed one (e. g. a circle). In the 18th century the geometrical objects were replaced by the general concept of quantity (Grösse, grandeur). The basic form of all the limit definitions of this century was: if a variable quantity can approach arbitrarily close to (or differ by less than any given quantity from) a fixed quantity, then the latter is the limit of the former. A quantity, and certainly a variable quantity, would not easily be interpreted abstractly. After all, numbers do not change, they are what they are. It is typically things like distance, velocity and direction which change or vary. A variable quantity, even if denoted by a letter, was most naturally thought of in terms of the motion of a particle. In the 18th century (as opposed to the 20th) the letters of algebra needed to denote and if they were to be variables their denotation must vary. Thus a limit would be conceived as a boundary of whatever it was that was varying. And the language of "approach" and "variable" served to emphasise this. It was therefore significant when D'Alembert (and L'Huilier and Lacroix) made more use than there had previously been of the (arithmetic) term "difference".

Another important development in the language used to describe the limit concept was to add to the phrases about approach and difference the idea that the difference is a matter of choice. For example, "la fraction proposée peut approcher de  $a$  aussi près que l'on voudra" (Lacroix), and "... & peut différer aussi peu qu'on voudra" (D'Alembert). The arbitrarily close approach of polygons to a circle was not simply a circumstance set in motion by the repeated doubling of the number of sides, it was a process over which we have control and choice. To exhibit and express this choice a suitable symbolism and notation was important but still (1800) nowhere used or exploited. Such notation also required a clear understanding that every limiting process really involved two variables: the quantity approaching a limit and an independent variable of which the former is a function. Lacroix, at least, understood this when he refers explicitly (see above) to: "la limite... relativement à l'augmentation indéfinie que peut recevoir  $x$ ."

Finally, seeing the need to establish the existence of a numerical limit was surely the most difficult step in the full arithmetisation of this concept. Certainly up till about 1815 mathematicians had always taken refuge in geometrical ideas and analogies. It was assumed that if one could satisfactorily give numerical values to the variable quantity then there was also a numerical value for the limit. The gradual realisation in the 19th century that this was not so ushered in the final phase of the arithmetisation programme - the construction and definition of the real numbers.

### 4.2.3. Infinitesimals and the Limit concept in Bolzano's work

From the references which appear in BL, RB and DP it is clear that Bolzano was aware of most of the important authors and works on analysis in the late 18th century. He refers in some way to most of the authors mentioned in the previous section (with the main exception of Carnot). However, the selective summary in that section of views on infinitesimals and limits would probably not have appeared coherent or significant in Bolzano's time; it was inevitably assembled from a modern point of view. Certainly Bolzano's work on analysis was not in direct response to the situation and problems described there. As we have seen, the works of 1816 and 1817 concern particular, important theorems and Bolzano had at least half an eye on the publicity value of the topics chosen. They were thorough works but modest in both their size and aims.

One of the difficulties of describing and assessing the development of a concept, or a particular contribution to a concept, is that it may be used by an author and even refined in a variety of ways without it ever being explicitly mentioned or considered in itself. This is the case with Bolzano's work with the limit concept. We have already referred (4.1.3.) to the fact that the use of this concept was fundamental to Bolzano's approach in all his analysis works. But the concept is never actually referred to in either BL or RB. In DP there is only brief mention, in a geometrical context, of "the method of limits". A natural reason for the use of a concept preceding its mention is that it may

only be through its use in various contexts that the concept can be isolated at all and be distinguished by naming. This could not be the reason in the present case because the concept, even if controversial, was by then well known and Bolzano must have been aware of the fact. There are, however, several other reasons why he may have preferred not to refer explicitly to limits. Just because they were controversial meant that they were unsuitable from the point of view of his seeking to gain general approval for his works. Then, as far as BL was concerned, Bolzano wished to be as straightforward and simple as possible, he says in his concluding note (A424;BL, 144) that, "in the present work we have decided to proceed everywhere only from concepts which are already known and common, and to avoid all the more difficult innovations". The context here is primarily that of imaginary and irrational quantities but considering its unwelcome reception in some quarters the limit concept may also have been felt to be a "difficult innovation". Finally, and perhaps most importantly, is the fact of the very restricted context in which Bolzano uses the limit concept. There is no attempt at a general theory of convergence, continuity or differentiation, these concepts are employed only in their immediate application to the proofs of the binomial theorem, the intermediate value theorem and the formulae for rectification and quadrature.

The way the concept of limit is tacitly used in defining convergence and continuity will be considered in the next two sections; our purpose now is to explain the mechanics of how the notion of arbitrarily close approach

was expressed arithmetically in BL. Infinitesimals, in the sense of a peculiar kind of constant or zero, are emphatically rejected and their role in attempting to symbolise the notion of arbitrarily close approach is taken by what we may most suitably call "arbitrarily small quantities". We quote from BL:

Instead of the so-called infinitely small quantities I have also always made use, with the same result, of the concept of those quantities which can become smaller than any given quantity, or (as I sometimes call them to avoid monotony but less precisely) quantities which can become as small as desired... The requirement of conceiving a quantity (I mean a variable quantity) which can always become smaller than it has already been taken, and generally can become smaller than any given quantity, really contains nothing that anyone could find objectionable..... On the other hand the idea of a quantity which cannot only be assumed to be smaller but is really to be smaller than every quantity, not merely every given quantity but even every alleged, i. e. conceivable, quantity, is this not contradictory? Nevertheless this is the usual definition of the infinitely small. (A269;BL, V)

Bolzano is careful to explain what it is that he rejects under the name of "infinitely small quantity". He understands by this term a (non-zero) constant which is smaller than every conceivable quantity. As we saw in the previous section this meaning, common in the mid-18th century, still lingered into the 19th century but Bolzano was wrong to describe it at that time

as "the usual definition of the infinitely small". The interpretations as a variable, or else as a manner of speaking for "relatively negligible", were by then more common. His objection to the infinitely small is rather less emphatic than his clear rejection of the infinitely many (this was in the context of infinite series, see 4.3.3.). In the above quotation there is the rhetorical "... is this not contradictory?"; in DP we read, "those... who make use of the concept of the infinitely small can never avoid the suspicion of contradiction in the concept itself" (A497; DP, VIII). Certainly the concept is to be avoided (A495, DP, VI), but only in BL, XI(A275) does Bolzano actually commit himself to describing it as self-contradictory. Such a view had many predecessors and, of course, was very reasonable. However, hesitation in regarding infinitesimals as self-contradictory was also vindicated by the discovery of non-standard analysis that given the axiom of choice, or some equivalent, actual infinitesimals could consistently be adjoined to the real numbers.

Though not historically fair, Bolzano's characterisation of the infinitely small served well to show, by contrast, what he believes is sufficient for analysis, i. e. "quantities which can become smaller than any given quantity". These are, he says, variable quantities which can always become smaller than they have already been taken. There is no danger of self-contradiction here for "there are often such quantities in space as well as in time" (A269; BL, V). This was no new idea. Bolzano's definition applies perfectly to the difference



between a variable quantity and its limit as described by D'Alembert and Lacroix. It is important though that Bolzano recognises this as a distinctive use of ordinary quantities which successfully replaces the older use of infinitesimals. (By the 19th century some writers, e. g. Cauchy, were using the word "infinitesimal" for Bolzano's concept of arbitrarily small) His reference to arbitrarily small quantities as variable quantities (A269;BL, V) just reflected the use and confusion of the time. They were consistently used by Bolzano as what we should call arbitrarily chosen constants. Mention has already been made of the importance for the development of the concept of a variable that the value of a variable symbol should be understood as a matter of choice. Bolzano does this clearly, and when he says (BL, V as above p.194) that it is "less precise" to describe arbitrarily small quantities as "those which can become as small as desired", this does not stem from the extra emphasis on choice in the phrasing; it is simply more precise to actually say what is desired, i. e. to state, with a symbol, a "given quantity". In fact this distinction highlights what we regard as Bolzano's main contribution to the development of the limit concept: the facility with which he uses symbolism for arbitrarily small quantities. This was all that was required to take advantage of the limit concept but was lacking to any really effective degree in earlier authors.

The first example of this facility is in BL§12 (A292) where it is shown that if  $x$  is a proper fraction,

the notable circumstance occurs that the binomial series

$1 - x + x^2 - \dots \pm x^r$  can be brought as close to the value of  $(1 + x)^{-1}$  as desired merely by sufficiently increasing the number of its terms.

In order to keep this difference less than a quantity  $D$  a sufficient inequality is found for  $r$  in terms of  $D$ . (The working here is identical to that in a modern elementary analysis text) No doubt it had been known for a long time that this could be done but it was not common to actually go through the working and make the final inequality explicit. The effect of doing so was two-fold. On the one hand it emphasised that all the quantities involved were perfectly ordinary finite quantities subject to all the usual algebraic operations and that the entire argument was open to inspection. On the other hand it drew attention to the arbitrarily small quantities, i. e. to the difference between a "variable quantity" and some fixed quantity. Following the result just mentioned (BL§12) there is a series of paragraphs devoted to the treatment of such quantities. A conventional notation is introduced for quantities intended to be arbitrarily small (A295;BL§14);  $\omega$  and  $\Omega$  are used by Bolzano in much the same way as  $\epsilon$  is used today in elementary analysis. Then there are several lemmas which effectively provide short-cuts in working with  $\omega$ 's. The following are the main ones:

BL§15, 16: For fixed  $r$ ,  $(A + \omega) \pm (B + \omega^1) \pm (C + \omega^2) \pm \dots$   
 (A295)  $\pm (R + \omega^r) = A \pm B \pm C \pm \dots \pm R + \Omega.$

BLS17, 18:  $A \cdot \omega = \Omega$  and so  $(A + \omega) (B + \dot{\omega}) = A \cdot B + \Omega$ .  
(A296)

BLS19:  $\frac{A + \omega}{B + \dot{\omega}} = \frac{A}{B} + \Omega$ .  
(A296)

BLS27: If the quantities  $\omega, \dot{\omega}$  in the equation  $A + \omega = B + \dot{\omega}$   
(A307)  
can become as small as desired while A and B  
remain unchanged then it must be that  $A = B$  exactly.

The first three of these correspond naturally to limit theorems if we regard the A, B etc. occurring in them (as Bolzano did) as partial sums of infinite series. Adding suffices for clarity Bolzano's lemmas correspond respectively to:

- (i)  $\lim A_n + \lim B_n = \lim(A_n + B_n)$ .
- (ii)  $(\lim A_n) (\lim B_n) = \lim (A_n B_n)$
- (iii)  $\lim \frac{A_n}{B_n} = \frac{\lim A_n}{\lim B_n}$  (assuming non-zero denominators).

The fourth result quoted is rather different, the A and B are constants and are to be thought of as the result of a limiting process. What Bolzano needs from this lemma is equivalent to: if  $f(x) = g(x)$  for all x,  $\lim f(x) = \lim g(x)$  as x approaches a. It can be regarded as a direct arithmetic formulation of the method of exhaustion. Bolzano applies it in BLS28(A307), BLS64(A394) and RB57(A463). Thus in spite of the very specific context in which he is working Bolzano does develop what could be described as an elementary, but generally applicable, theory of limits. It is entirely arithmetic, it uses a systematic notation and all arguments can be explicitly followed through; nothing is left mysterious.

### 4.3. Infinite Series and Convergence

#### 4.3.1. Outline of Bolzano's work on Series

It was in connection with his treatment of series in BL and RB that the merit of Bolzano's contribution to analysis was first recognised. This recognition began with Hankel's article Grenze of 1871 (Hankel [1] ) where he credits Bolzano with, "the first strict development of series". We shall describe in this section the extent to which this was deserved.

Great care is needed in assessing Bolzano's work on this subject because the notion of convergence gained so much more significance in the later 19th century than it could have had to Bolzano. It is easy therefore to attribute to him a purpose and deliberation which he may not have had himself. For example, with respect to convergence, when considering his very successful treatment of the concept it is important to be aware that Bolzano nowhere actually uses the term "convergence" in BL or RB. The occurrence in A270; BL, VI is really a quotation but Bolzano's comment suggests he regards the term "convergent series" as meaning one with decreasing terms. This would certainly explain why he did not find the term useful. Bolzano himself saw the main significance of his work on series in the complete rejection (as he supposed) of the concept of infinity from analysis. Firstly, we shall state the main result of Bolzano's work on series, then give some sketch of the relevant earlier work, and finally return to Bolzano's papers for a more detailed assessment.

In RBSS6, 7(A462) there appears, for the first time in mathematical literature what is now usually known as the general principle of convergence. The partial sums of an arbitrary power series in  $x$  are denoted  $F_1 x, F_2 x, \dots, F_n x, \dots$ . Bolzano says,

we regard the quantities

$$F_1 x, F_2 x, F_3 x, \dots, F_n x, \dots, F_{n+r} x, \dots$$

as a new series (called the series of sums of the previous one).

Then there is the main theorem: If a series of quantities

$$F_1 x, F_2 x, F_3 x, \dots, F_n x, \dots, F_{n+r} x, \dots$$

has the property that the difference between its  $n$ th term  $F_n x$  and every later one  $F_{n+r} x$ , however far from the former this is, remains smaller than any given quantity if  $n$  has been taken large enough, then there is always a certain constant quantity, and indeed only one, which the terms of this series approach and to which they can come as near as desired if the series is continued far enough.

The principle is therefore not entirely general since it is applied only to power series. The variable  $x$  is assumed to remain constant throughout: there is no suggestion in BL or RB of uniform convergence. The particular value of Bolzano's formulation here is two-fold. Firstly, there is the realisation that the behaviour of a series is not determined merely by the behaviour of the terms, but by arbitrary (though finite) blocks of terms. Secondly, there is the clear claim, and the attempt to prove, that the property of

"bunching up" is sufficient for the series to approach a fixed quantity arbitrarily closely. This sufficiency proof requires a prior theory of real numbers for it to be complete. (We consider Bolzano's proof in detail on p.216 .) The principle behind this criterion for convergence had been suggested (in words not symbols) in BL, XIV(A278) but though it is often implicit it is never clearly formulated in BL where the emphasis is on the essentially finite treatment of all series. In fact it appears that Bolzano's introduction of the convergence principle in RB is more a consequence of his desire to avoid the infinite than a deliberate formulation of a fundamental concept. There is a sense then in which his success is accidental. It is ironic that Bolzano's work is not only earlier than that of Cauchy (to whom the convergence principle is usually attributed, the Cours d'Analyse appearing in 1821) but, as pointed out in Pringsheim [1]p. 79, it is also clearer since Cauchy still uses the ambiguous language of infinitesimals. But Bolzano's somewhat oblique treatment of convergence is not only to be explained in terms of his own priorities but also against the background of the varied 18th century attitudes to infinite series.

#### 4.3.2. Infinite Series and Convergence before 1815

One of the chief reasons for regarding the analysis of the 18th century as loose and ill-founded compared with the rigour of the 19th century is the very free use made then (especially by Euler) of divergent series. It has

been pointed out (e. g. Kline [1] p. 460) that the "correct" concept of convergence was touched on several times during the 18th century by different authors only to be subsequently ignored. The question of why the modern concept of convergence was not taken up and exploited at once is tempting to pursue but largely misconceived. The modern concept is inseparable now from such things as the theory of real numbers and the importance of uniform convergence for integration; it has a significance and attraction based on reasons which would be anachronisms in the 18th century. The concept at that time in its evolution had simply not proved useful enough, nor clear enough, to gain a central role in the treatment of infinite series. It will be valuable though to describe what place it did have and to consider why it developed so slowly. To do this we need some outline of the occurrence and use of infinite series up till 1800.

Two important problems of infinite series had already emerged clearly by the end of the 17th century. These were (i) how to find the sum of a given infinite series, and (ii) how to expand a given function into an infinite series (usually a power series). The first kind of problem typically arose in the 17th century from attempts to solve quadrature and rectification problems. The second problem was very important for applying calculus operations to trigonometric functions and even simple reciprocal functions. The urgent practical need here produced the binomial theorem for fractional and negative exponents (Newton 1676) and Taylor series (1717). The concept of convergence

was eventually crucial for both kinds of problem but initially it arose with the first problem, that of summation. Gregory of St. Vincent resolved the paradox of Achilles and the tortoise by summing an appropriate infinite geometric progression, and the meaning of the "sum" he expressed as follows:

the terminus of a progression is the end of the series to which the progression does not attain, even if continued to infinity, but to which it can approach more closely than by any given interval. (Gregory of St. Vincent [1])

Though somewhat muddled this contains the germ of the modern concept of convergence in terms of limit. The actual words "convergent" and "divergent" used by James Gregory seem to have been more a means of describing how a series reaches its sum than whether it has one. The point here is that infinity was regarded as a "number" that could be included in ordinary calculation. (For example, see Wallis Arithmetica Infinitorum 1655; he and John Bernoulli regarded infinitesimals as "real" numbers arising from  $\frac{1}{\infty}$ .) Thus many of the early discussions of convergence and divergence were in the guise of considerations as to whether the sum of a particular series was finite or infinite. There was no question then of a series failing to have a sum just because it became indefinitely large. This attitude was clearly an important factor in the slow development of a convergence criterion; while there was no great arithmetical distinction between finite and infinite number symbols there was no pressing need to distinguish series with finite and infinite sums.



Indeed the more urgent problem was to improve the rate of convergence for the sake of improving tables and practical applications.

The use of infinite number symbols and a kind of convergence criterion both appear in a paper by Euler on harmonic series in 1734 Euler [1]. On p. 119 of Reiff [1] it is said that this criterion coincides with the modern general convergence criterion but this is certainly an exaggeration of the passage he quotes and the example Euler gives. A more accurate comment (Pringsheim [1] p. 78 Note 150) is that what Euler actually offers here is a correct diver-  
gence criterion to the effect that a series diverges if  $\lim_{n \rightarrow \infty} |s_n - s_{n-1}| > 0$ . The example given by Euler is the series  $\frac{c}{a} + \frac{c}{a+b} + \frac{c}{a+2b} + \dots$  with  $i$ th term  $\frac{c}{a+(i-1)b}$ . (And here Euler is using  $i$  as a symbol for infinity!) Now if the series is continued to the term  $\frac{c}{a+(n,i-1)b}$  then the sum of the series from the  $(i+1)$ th term to the  $n$ ,  $i$ th term is smaller than  $\frac{(n-1)ic}{a+ib}$  but greater than  $\frac{(n-1)ic}{a+(n,i-1)b}$ . Since  $i$  is infinitely great this sum is less than  $\frac{(n-1)c}{b}$  and greater than  $\frac{(n-1)c}{nb}$ , therefore it is finite and the above series is divergent. Euler was inconsistent with respect to infinite series, sometimes advocating great caution with divergent series and sometimes committing the wildest excesses with such series. He was, like many others in the 18th century, strongly influenced by the work of Leibniz. In the work De seriebus divergentibus, 1754 Euler [2] (written partly in response to criticism from Nicholas Bernoulli), Euler discusses the arguments for and against the use of divergent series. Among reasons for their use he quotes

approvingly Leibniz's determination of the series  $1 - 1 + 1 - 1 + \dots$  as equal to  $\frac{1}{2}$ . Leibniz said that "a quantity for which there are equal grounds for ascribing two values must be taken equal to their arithmetic mean . . . . thus the nature of things here follows the same law of justice." (Leibniz [5] p. 386) He concluded, "although this kind of argument may seem more metaphysical than mathematical, it is nevertheless sound." Oscillating divergent series, like  $1 - 1 + 1 - 1 + \dots$ , caused much discussion as to what value should be attached to them, but it was assumed that they should have some value. In the end Leibniz's probability method was accepted by the Bernoullis, Euler (who used the method for many more complicated cases) and Lagrange. It was virtually unopposed throughout the 18th century and until Laplace disputed the method in Théorie de probabilité (1812). If one insists on associating a "sum" with a diverging series of this sort it is indeed a perfectly reasonable way of doing so. Unfortunately it had the unjustified, but plausible, effect of reinforcing confidence in the validity of binomial theorem expansions for unrestricted values of the variable. (Grandi had obtained  $1 - 1 + 1 - \dots = \frac{1}{2}$  by putting  $x = -1$  in the expansion of  $\frac{1}{1+x}$  (Grandi [1]), and Euler put  $x = 1$  in  $\frac{1}{1-x}$ .)

More important from our present point of view was the attitude to properly divergent series. James Bernoulli had proved in 1689 that the harmonic series was divergent (Bernoulli, James [1]), and he drew attention to the fact that the sum of a series whose "last" term vanishes can be

infinite. To most mathematicians in the 18th century this result not only seemed highly paradoxical but was almost a contradiction in terms. Convergence was generally taken to mean "having a finite sum" and a sufficient criterion was regarded as being that the terms decrease in magnitude to zero as a limit. The divergence of the harmonic series contradicted this and the result  $1 - 1 + 1 - 1 + \dots = \frac{1}{2}$  showed the criterion was not necessary for convergence. Euler, again under criticism from Nicholas Bernoulli, tried to distinguish the sum from the "value" of a series. The value of a series, he said, was the value of the expression from which the series originates (Euler [3]). Thus it was acknowledged that an infinite series cannot have a "sum" in the same sense as a finite series, we can simply associate a number, a value, with the series which may behave like a sum.

There had always been voices of caution over infinite series and during the 18th century attitudes tended to polarise into two camps. In the majority were those who felt that anything that can be written down and operated with formally (including  $\infty$ ) must mean something. But a minority were very conscious that an infinite series can never actually be written down or summed in the usual sense, and operating with such a series may be meaningless. Of course, both sides used the language of convergence and divergence as well as the  $\infty$  symbol. We shall mention a few of those who advocated caution. Varignon [1] said early in the 18th century that no series should be used without investigating its remainder. Nicholas Bernoulli pointed out in a

letter to Euler that a divergent series can never exactly represent the value of the quantity from which it arose since even if continued to infinity there is still an error. In Bernoulli, N.[1] he gives as an example that

$$\frac{1}{1-x} \text{ is not } = 1 + x + x^2 + \dots + x^\infty$$

but

$$\frac{1}{1-x} = 1 + x + x^2 + \dots + x^\infty + \frac{x^{\infty+1}}{1-x} .$$

D'Alembert was perhaps the strongest influence for the cause of convergence in the 18th century. In 1768 he wrote:

For me all reasoning with series which do not converge and which cannot be assumed to do so is always very suspect even when these reasonings agree with truths known otherwise. (D'Alembert [2] p.183)

Even in this second half of the century the infinity symbol was still used freely, D'Alembert wrote (in reference to a series used by Langrange) that one cannot ascribe convergence to the geometric series

$$\text{or}$$

$$e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} + \dots$$

$$e^{-x\sqrt{-1}} + e^{-2x\sqrt{-1}} + \dots$$

since neither  $e^{\infty\sqrt{-1}}$ , nor  $e^{-\infty\sqrt{-1}}$ , equals 0 (D'Alembert [3]). But what did D'Alembert mean by convergence? The Encyclopédie entry Convergent (Vol. 4, 1754) is very brief:

Convergent, adj. en Algèbre, se dit d'une série, lorsque ses termes vont toujours en diminuant. Ainsi  $1, \frac{1}{2}, \frac{1}{4}, \dots$ , & c est une série convergente.

But by the entry for Série (Vol. 15, 1765) this had slightly improved:

.... lorsque la suite ou la série va toujours en approchant de plus en plus de quelque quantité finie, & que par conséquent les terms de cette série, ou les quantités dont elle est composée, vont toujours en diminuant, on l'appelle une suite convergente, & si on la continue à l'infini, elle devient enfin égale à cette quantité.

Lagrange's Théorie des fonctions (1797) is sometimes hailed as marking the beginning of "the exact treatment of infinite series" (Reiff [1] p. 155).

This is on account of the careful treatment in the work of the remainder term for a Taylor series. However, Lagrange nowhere relates the behaviour of the remainder with the convergence of the series. The whole point for Lagrange was to be able to estimate the error in an approximation obtained from only a finite number of terms of a Taylor series. His notion of convergence is still quite naive and no improvement on that he expressed in 1770:

... in order for a series to be able to be regarded as really representing the value of a quantity sought it is necessary for it to be convergent at its extremity, that is to say, its last terms should be infinitely small so that the error can become less than any given quantity. (Lagrange [2] )

Lagrange's proof of convergence here is limited to showing that the terms of a series finally converge to zero. And in much of the work of Théorie des fonctions .... he introduces, and relies on, series without any

consideration of whether they converge or not. Hankel comments: "Naive trust in the good-nature of series had its last triumph with Lagrange." (Hankel [1] p. 209).

One of Bolzano's important reference works was Klügel's Mathematisches Wörterbuch. But there was little inspiration to be found there in the article Convergent. With the ambiguity between definition and criterion that was so common with regard to convergence we read:

A series is convergent if the successive terms become continually smaller. The sum of the terms then always approaches nearer to the value of the quantity which is the sum of the series continued to infinity. (Klügel [2] )

Even around 1800 when infinite series were well-established and becoming much more sophisticated the question of finding a general characteristic to distinguish convergent series had not become a widely recognised or urgent problem. There were two reasons. Firstly, the common use in the 18th century of infinity as a numerical value meant that the existence or meaning of an infinite series did not depend on its convergence. Secondly, the pragmatic policy of ignoring the absurdities and paradoxes that sometimes arose from divergent series and just using them whenever they could be useful was very successful in those areas where success was obvious and appreciated - the solving of practical problems. However, this neglect of the theoretical aspects of infinite series led to the most widely divergent attitudes

early in the 19th century.

We shall end this survey by mentioning two very different works which were each published in 1813. The first is by von Prasse, Institutiones Analyticae, described by Hankel as "standing at a high point in science in Germany". It is almost entirely concerned with the development and use of series but convergence is only spoken of once where it is remarked about a geometric series that, "one says it diverges if  $x > 1$  and in other cases it converges, these terms are carried over to other series." Prasse [1]. And that is all. In contrast there is the important paper by Gauss on the hypergeometric series:

$$1 + \frac{\alpha \beta x}{1 \cdot \gamma} + \frac{\alpha(\alpha+1)\beta(\beta+1)x^2}{1 \cdot 2 \gamma(\gamma+1)} + \dots \quad \text{Gauss [2]}.$$

He begins by considering the ratio of successive terms and dealing with the cases  $x < 1$  and  $x > 1$  by comparison with a geometric progression. The case  $x = 1$  occupies the major part of the paper with a detailed analysis of the possibilities for  $\alpha, \beta, \gamma$ . He develops the criteria for convergence in an ad hoc way just for this particular case but it is exhaustive and strict and he even considers the cases when  $x$  is complex. It has been said that here the convergence of an infinite series was investigated properly for the first time (Dunnington [1], Tropfke [1]). The claim is true but modest; the investigation is entirely concerned with this particular series. But no claims are made by Gauss about general criteria for use with any series.

### 4.3.3. Bolzano's work on Infinite Series and Convergence

Bolzano does not set out anywhere in BL or RB to give a theory of infinite series or convergence. What he says about these topics is entirely for the sake of the theorems he is engaged in proving: the binomial theorem and the intermediate value theorem. His approach to the infinite cases of the binomial series is however quite radical and is evidently intended to apply generally. He rejects the concept of an infinite series completely:

every assumption of an infinite series, as far as I see, is the assumption of infinitely many quantities and every attempted calculation of its value is therefore an attempted calculation of the infinite, a true calculus infinitesimalis. Therefore if one does not want to be involved in such things . . . . . then one must refrain altogether from the acceptance and calculation of infinite series. (A268;BL, IV)

Bolzano immediately goes on to speak of his replacement of infinitely small quantities by what we have called arbitrarily small quantities. Thus the reason for this ruthless approach to infinite series lies in the implicit assumptions about infinity which Bolzano believes are made in the usual use of such series. This refers both to the infinitely large ("the assumption of a sum of infinitely many quantities") and the infinitely small ("a true calculus infinitesimalis"). The connection between these concepts is not made very clear, it is presumably thought that the infinitely remote terms of a convergent series will have to be infinitely small. (Compare the quotation by Lagrange on



convergence, p.208). The consequent restriction to finite series does not mean the complete rejection of the general binomial theorem, although "it is certain that this [binomial] equation really only holds precisely if the exponent is a whole positive number" (A269;BL, V). But to deal with the cases of fractional or negative exponents Bolzano speaks of the binomial expression being "equal in a certain sense" (A272;BL, VIII) to a finite portion of its corresponding binomial series. By this he means that the difference between a binomial expression and its corresponding series can be made arbitrarily small by taking the series far enough. For example, the binomial series

$1 - x + x^2 - x^3 + \dots \pm x^r$  can be put equal only in a certain sense, to the true value of

$$(1 + x)^{-1} = \frac{1}{1 + x} = 1 - x + x^2 - x^3 + \dots \pm \frac{x^r}{1 + x}$$

i. e. if  $x^r - \frac{x^r}{1 + x}$  can become as small as desired by the increase in  $r$ , i. e. if  $x < \pm 1$ . (A272;BL, VIII, IX).

In fact, as mentioned in the section on limit (p.197) Bolzano shows explicitly (A292;BL§12) how to find the value of  $r$  which makes this difference less than a given quantity  $D$ . In §13 he remarks that for practical calculation these imprecise but arbitrarily close equations are just as good as an exact equation and therefore merit "careful attention". He now describes such an imprecise binomial equation as "valid in the sense that one considers added to one term [i. e. side] of the equation a quantity which can be smaller than any given quantity." This leads to the series of lemmas on arbitrarily small

quantities,  $\omega, \Omega$  etc. which correspond with results about limits. For the further results on the binomial theorem Bolzano effectively proves theorems of the form  $f(x) = p_r(x) + \omega_r(x)$  for all  $r$ , where  $f(x)$  is the binomial to be expanded,  $p_r(x)$  is a finite power series up to  $x^r$  and  $\omega_r(x)$  is the corresponding remainder. When the  $\omega_r(x)$  is arbitrarily small, there is a useful and valid case of the binomial theorem. We should now express this by saying that when  $\lim_{r \rightarrow \infty} \omega_r(x) = 0$ , we define the corresponding infinite series to mean  $\lim_{r \rightarrow \infty} p_r(x)$ ; in this context the limit would necessarily exist and equal  $f(x)$ . Bolzano's approach avoids the need to prove the existence of any limit but it has the great inconvenience that the binomial series (whenever it should be infinite) is not unique, it is any one of an (infinite) sequence of finite series. In spite of his repudiation of the infinite and of infinite series it is clear that Bolzano must assume not only the potential infinity of integers as exponents but also the infinity of arbitrarily small numbers as differences. The important feature of Bolzano's treatment which was distinctive and fruitful was that only finite quantities ever entered into calculations and all calculations were only finite.

The insistence on finite calculations, without the explicit use of the limit concept, led to rather elaborate circumlocutions in the formulation of many theorems. For example in BL§30(A315),

A series developed in powers of  $x$  is to be either completely equal, or at least come as near as desired if the number of its terms is

taken large enough, to the value of the complex function  $(1 + x)^n \dots$ ."

The same sort of long-winded description occurs in many results throughout the rest of the paper (e. g. BLSS33, 35, 41, 42, 43 etc. ) and its repetition must have emphasised, to Bolzano at least, the significance of arbitrarily close approximation. There seems to have been a kind of feedback here between the two aspects of infinite series mentioned earlier, i. e. direct summation of a series and the series development of a function. The binomial theorem is the development of a certain kind of function: the series was seen to be either exactly equal, or under suitable conditions arbitrarily close, to the function. Here the function comes first with the series derived from it and compared with it. In RB the priority is reversed and a general series is considered with a property (namely that remote finite "blocks" of the series become arbitrarily small) which ensures that the series comes arbitrarily close to some fixed quantity which was not previously known or given. We shall now indicate how the significance of considering finite "blocks" of a series was likely to have been suggested by Bolzano's work on the binomial theorem.

In the Preface of BL there is an informal description of the idea of the convergence principle which is used in RB. While considering proofs of the binomial theorem, "of which nothing could be criticised if they were not based on the inadmissible idea of infinite series " (A278;BL, XIV), Bolzano continues:

Series are obtained which are indeed equal to one another from their first term up to arbitrarily many terms but they then have just as

many unequal terms, so that in order to claim the equality of their value it is necessary to show that the sum of the unequal terms can become smaller than any given quantity if one makes the number of equal terms large enough.

This is not explained any more in the Preface, but Bolzano is almost certainly referring to the sort of induction step in BL which takes the form of proving that if the theorem holds for exponents  $p$  and  $q$  separately then it will also hold for the exponent  $p + q$ ; the  $p$  and  $q$  here are not just integers but arbitrary numbers (see A335-346; BLSS38-41). Bolzano proves this by taking initial finite blocks of the expansions of  $(1 + x)^p$  and  $(1 + x)^q$  (up to  $x^r$  and  $x^s$  respectively) and first showing that all terms of their product up to  $x^r$  or  $x^s$  (whichever is the smaller of  $r, s$ ) are identical with the corresponding terms of the binomial series for  $(1 + x)^{p+q}$ . But then, as explained in BIS39(A340), there are always a number of other terms, in fact as many as the greater of  $r, s$  which are not identical with the corresponding terms of the  $(1 + x)^{p+q}$  series: "these will be greater as one takes  $r$  and  $s$  greater . . . therefore however many terms in the product  $M$  correspond to the binomial series, at least as many also deviate from it." It is the behaviour of this finite block of terms from  $x^{r+1}$  (if  $r < s$ ) to  $x^{r+s}$  that determines, in the case of  $p$  or  $q$  not being integers, whether the two series may be said to be equal or not.

Only in the case when  $x$  is a proper fraction does the special circumstance occur that the unequal terms always become smaller and that

one can actually make the value of their sum as small as desired by the increase in  $r$  and  $s$ . (A341;BL, 61).

Although it appears here in the context of the equality of two series, this behaviour of the finite block of terms in the product from  $x^{r+1}$  to  $x^{r+s}$ , as  $r$  increases, is precisely what is described in the convergence criterion of RB§§6, 7(A462). The  $n$ th partial sum of a power series is denoted  $F_n x$  and, the difference between its  $n$ th term  $F_n x$  and every later term  $F_{n+r} x$  (no matter how far from that  $n$ th term) stays smaller than any given quantity if  $n$  has been taken large enough.

Given this condition, then the theorem of RB§7 states that there is a unique constant quantity which the partial sums approach arbitrarily closely. The argument here consists of four steps:

- (i) if such a constant quantity  $X$  exists it can be determined as accurately as desired;
- (ii) therefore the assumption of a quantity  $X$ , "contains no impossibility";
- (iii) therefore there is a real quantity  $X$ ;
- (iv) this quantity  $X$  is unique.

There can be no quarrel with the first, or the fourth step, Bolzano goes through the working correctly to show exactly how, using the assumptions, an  $F_n x$  can be found which differs from  $X$  by less than any given quantity  $d$ .

We could paraphrase the original statement of the theorem by saying that if a sequence of partial sums "bunches up" then it has a limit. Bolzano's first step shows that if the sequence has a limit (which will generally not coincide with any of the partial sums) then the value of this limit may be determined arbitrarily closely by purely internal inspection of the partial sums. But here the argument breaks down; it is hard to see how steps (ii) and (iii) follow in any way from what has gone before. In BG Bolzano had emphasised that the possibility of a concept must be proved before the concept can be used (A68, 71; BG, II§3, §7). In the passage from BD (A137;BD, 30) quoted on p. 55 he speaks of purely symbolic concepts (such as infinity and  $\sqrt{-1}$ ) which, if their use were shown to be consistent, could be adjoined to the "real" concepts of elementary mathematics. These ideas may have led Bolzano to believe that the possibility of the limit could be sufficient ground for its existence. And (i) may very reasonably have made him feel that the limit certainly should exist, or that no inconsistency would arise if it were assumed to exist. However, to the extent that we give Bolzano credit for recognising the subtlety of the need to prove the existence of the limit (and this, after all, is the main statement of the theorem RB§7), we must also admit that he fails to give this proof. He can hardly have been satisfied himself: the abrupt conclusion after the proof of (i) that "there is therefore a real quantity" is wholly disconnected and inadequate. It has no precedent in his other proofs except perhaps parts of the tentative BG, Part II, but there he often admits his own failures. RB

was intended to be something of a showpiece and presumably this may have prevented any admission of imperfection.

It has not been pointed out before in the literature on this part of Bolzano's work (4.3.4) that RB§7 is in fact just a formal statement of the convergence criterion, not its first discovery. We have indicated how it originates from the work in the early part of BL but it is also used there explicitly to prove the convergence of the exponential series. In BLS70(A411) it is correctly proved that,

$$\text{the series } 1 + \frac{1}{1.2} + \frac{1}{1.2.3} + \dots + \frac{1}{1.2.3\dots r}$$

can be continued so far that its increase for every further continuation remains smaller than any given quantity.

And he concludes,

..... it follows from this there would have to exist a certain constant quantity which this series steadily approaches and to which it can come so near that the difference is smaller than any given quantity.

It is tantalising in retrospect to consider how near Bolzano was, after proving property (i) (p.216) of what are now often called Cauchy sequences, to actually identifying real numbers with Cauchy sequences of rationals. In fact there is some evidence that Bolzano was aware that the result of RB§7 does depend on a "correct" concept or definition of number. The theorem of RB§7 is used crucially to prove RB§12, the original form of the Bolzano-Weierstrass theorem, and in his overall summary in the Preface of RB

Bolzano refers to the main existence result as follows: "Whence it follows for everyone who has a correct concept of quantity, [Grösse] that the idea of [such] an  $i, \dots$  is the idea of a real, i. e. actual, quantity." (A451;RB, 23)

Also, during the 1830's in the course of developing his own, rather complicated theory of real numbers Bolzano does refer to the inadequacy of his RB§7 proof (Bolzano [6]).

#### 4.3.4. Secondary Sources on Bolzano's work on Convergence

Unfortunately Bolzano's purpose of gaining some attention and support with the publication of BL and RB failed miserably. There is no definite evidence of either of these works having been read or appreciated by any mathematicians for about fifty years after their appearance.

There have been a number of articles on the question of whether Cauchy may have read and used Bolzano's ideas on convergence and continuity. Most notable in recent years have been the papers Grattan-Guinness [1], Freudenthal [1] and Sinaceur [1]. We do not propose to go into the details of the issue here. It will probably never be known for certain whether Cauchy, in writing his Cours d'Analyse, owed anything to Bolzano or not. Clearly this was possible but the strenuous special pleading employed by Grattan-Guinness to try and make it appear probable does not seem fair or profitable. The controversy is largely irrelevant from our present point of view, that of



simply understanding Bolzano's work on convergence, except that it has produced some serious misjudgements, by both sides, of the work in RB. We refer to these later.

Hankel's article Grenze was the first published and authoritative acknowledgement of Bolzano's mathematical work on infinite series. Obviously seeking to make amends for the unjust neglect of his subject Hankel speaks in glowing terms of the work in BL:

Bolzano's concepts of the convergence of series are altogether clear and correct, his operations with infinite series are all strictly proved. . . . Briefly, he possessed everything which puts him in this respect at equal eminence with Cauchy - only without the Frenchman's art of dressing-up his ideas and presenting them in the most attractive way. So Bolzano stayed unknown and would soon be forgotten; Cauchy was the fortunate one who was hailed as the reformer of the science and whose elegant writings spread quickly and widely. (Hankel [1] §19)

This is rather exaggerated. There are mistakes in Bolzano's work and it really bears little comparison with the prolific work of Cauchy. For example, Bolzano mistakenly claims that  $(1 + x)^n$  does not converge for  $x = 1$  and positive  $n$  (A271;BL, VII); in fact with  $x = 1$  it converges for all  $n > -1$ . What is striking about Hankel's article is the omission of any mention of RB. His eulogy of Bolzano is entirely based on the "finite" treatment of the general binomial theorem, which we regard here as only an intermediate

stage on the way to his real insight into how to treat convergence in RBSC, 7. As Stolz points out (Stolz [1]), the combination of Hankel's readiness to credit Bolzano and the fact that earlier in his article (Hankel [1] §§4-8) Hankel seems to think he is himself the first to establish the sufficiency of the Cauchy convergence criterion must mean that RB was inaccessible to him. This seems very odd because he can hardly have been ignorant of the work. It is predicted, though not by title, in BL itself and it appears in Rogg's Handbuch der mathematischen Literatur (1830), as well as in the first volume of the famous bibliography by Poggendorf published in 1863. Also, RB not only appeared separately in 1817 but it was published in 1818 in the Abhandlungen der königlichen böhmischen Gesellschaft der Wissenschaften.

The purpose of the paper Stolz [1] (1881) was, as he describes it, to clarify and summarise the definitions and propositions on the principles of analysis [Infinitesimalrechnung] which were peculiar to Bolzano in contrast to Cauchy. He believes that though Cauchy "founded analysis" Bolzano had "discovered some years earlier the basic concepts which in many ways agreed with those of Cauchy but which in important points were even better" (original emphasis in Stolz [1] p. 255). Among others he has sections on upper limits, convergence of series of real terms, differentiation of infinite series, and the rectification of curves. He makes use of the works BD, BL, RB, DP and the posthumous work Paradoxien des Unendlichen. The paper is brief with little commentary. The section IV on convergence mainly

consists of quotations from RB§§5, 6 and 7. Stolz is wrong to regard RB§5, 6 as an explanation of the necessity of the convergence criterion. Bolzano just shows there that a geometric progression with common ratio less than one, or a series whose terms are smaller than such a progression, will satisfy the criterion in RB§6.

Kolman, in his work on Bolzano originally written in 1955 follows Stolz in the error of supposing that in RB§5, 6 Bolzano is showing the necessity of his criterion (Kolman [1] p. 50). While admitting the proof of sufficiency is defective he says that, "it goes as far as possible without a theory of real numbers". This seems a rather pointless and biased claim in the absence of any justification.

For the sake of completeness we should also briefly mention the paper Wussing [1] which quotes both Bolzano's RB§7 and Cauchy's convergence definition for comparison. He adds almost no commentary except the remark, "Bolzano makes use - before Cauchy - of the Cauchy convergence principle, a necessary and sufficient criterion for convergence". He does not say Bolzano proves the necessity (contrary to Grattan-Guinness [2] p. 72, Note 12). However, this mistake is made categorically by Steele in his Introduction to Paradoxes of the Infinite (Steele [1] p. 29) and also by Sebestik in the introduction to his French translation of RB (Sebestik [1]). Steele actually summarises the proof but has obviously failed to see the point that the limit can be approximated to any accuracy by the  $F_n x$  alone.

There are two sources of commentary by Grattan-Guinness on Bolzano's work on series. These are the book Grattan-Guinness [2] and article Grattan-Guinness [1]; both appeared in 1970 and we shall denote them Foundations and New Analysis respectively. They contain a more detailed consideration of Bolzano's early work than had previously been given in any English publication. Two useful points that are made are to correct the error that RB§6 is about the necessity of the convergence criterion, and to state that RB was available in Paris, in the Bibliothèque Nationale, in 1818 (Foundations p. 76, 77). Unfortunately, however, much of the assessment of Bolzano is ill-judged. The extravagant claims made about his work are nevertheless relevant so they need some consideration. Grattan-Guinness's judgements seem to be affected by his idea of "limit-avoidance" as an understanding of the limit concept by which "we may move as close as we wish to the limit, while still avoiding the limit itself". And in New Analysis (p. 378) he says of limit-avoidance that,

the limiting value is defined by the property that the values in a sequence avoid that limit by an arbitrarily small amount when the corresponding parameter ... avoids its own limiting value.

The theory, or approach, of limit-avoidance is regarded as a profound distinguishing feature of a "complete reformulation of the whole of analysis" which was inaugurated by Bolzano and Cauchy (New Analysis p. 378). Limit-avoidance is meant to be the key to understanding Bolzano's definition of continuity (see p.238). In that context he writes:

This is the quintessence of his "pure analysis", for with it he started a revolution in approach to the subject: the arithmetisation of analysis by means of limit-avoidance techniques such as in his definition 3.3.

He used the approach consistently in his paper of 1817;... (Foundations, p. 55).

And again a little later:

Bolzano not only distrusted infinitesimals but also solved the problem of limits by introducing limit-avoidance. (Foundations, p. 56, original emphasis in both quotes)

Thus Grattan-Guinness regards limit-avoidance as something new with Bolzano (and Cauchy) and that it is consistently adopted by him. (This, incidentally, is one of his main arguments for Cauchy's plagiarising) But both these opinions are wrong. In the Sec. 4.2.2 we have quoted earlier examples of unambiguous concepts of limit-avoidance such as that of Newton and D'Alembert. And Bolzano (if he had used the term "limit" which he did not) must have allowed a sequence to achieve its limit because in RB<sup>5</sup> he regards finite series as examples which show that the convergence criterion is satisfied by at least some series. Clearly after a sufficient number of terms they actually equal the "constant quantity to which they can come as near as desired".

In fact the whole idea of limit-avoidance seems to be of doubtful value as a distinctive concept to apply to the development of limits. In many

definitions, such as those of L'Huilier and Cauchy, it is not specified whether or not the limit is allowed to be achieved. Not being excluded it is presumably therefore included - to the added generality of the concept. Yet Grattan-Guinness always seems to regard limit-avoidance as an advance; does he think it is exemplified in the modern concept of limit? It is all very peculiar.

In Foundations there is an account of the important paragraph RB§7. Having pointed out that Bolzano has phrased his theorem, "in a way which again uses limit-avoidance" (but does it?), he writes the following sentence:

Since the existence proof was prior to uniqueness, he assumed that he could take a variable value for the sum-function (varying, that is, independently of its being a function of  $x$ ), which could therefore be selected to be within an arbitrary degree of closeness to what he called the "true value" of itself. (Foundations p. 73)

This is completely wrong, as a reading of the proof (RB, 22) is enough to show. Bolzano starts his proof by saying that if the quantity claimed by the theorem was not assumed to be unique and invariable it could easily be chosen suitably - it could even be identified with the  $F_n, x$  terms. (This may seem silly or irrelevant but it is the way Bolzano does it.) Then he goes on to say that the assumption of an invariable quantity is not impossible because it can be arbitrarily closely approximated etc.

So much for the account in Foundations. In the New Analysis there is a section (2.2) on the convergence of series and the main point made

(p. 376) is that both Bolzano and Cauchy, "found a general condition for convergence in terms of the behaviour of  $s_{n+r} - s_n$  as  $n$  tends to infinity: a result of quite profound originality." We have pointed out above the importance of this treatment of finite blocks and how Bolzano may have been led to its general application through experience in BL with the finite treatment of the product of two binomial series. But it was not an original idea. We have quoted Euler's use of such finite blocks of an infinite series to obtain a divergence criterion (p. 204). (Grattan-Guinness refers to this paper himself in Foundations p. 75, Footnote 18.) There are many detailed errors about Bolzano's work in New Analysis which we shall not mention here: most of them are pointed out in Freudenthal [1].

We shall consider one further point from New Analysis because of its significance (if there were any truth in it) to our present treatment of Bolzano's analysis. Grattan-Guinness believes that between BL and RB there occurs the transition in Bolzano's thought between the "old" analysis and the "new" (New Analysis p. 384). Referring to Bolzano he writes there:

Thus in 1816, for example, before the flood of his own new thinking, he published a treatise on the binomial series in the style of the old analysis which is really quite remarkably uninteresting.

Now all three papers BL, RB and DP were almost certainly written within twelve months of each other (probably in 1814 or 1815 and then revised Sec. 1.3). And RB is said, in a footnote of BL (A312;BL, 32) to already be

written out ready for printing. There would therefore need to be very strong internal evidence to maintain the idea of any major transition in outlook. We can see no such evidence at all. On the contrary, as we have indicated, there seems to be a steady progression in the basic ideas about convergence and continuity (particularly the latter, see p.238).

The paper Freudenthal [1] is a lively rejoinder to Grattan-Guinness' New Analysis. The purpose of the paper is to discuss whether Cauchy plagiarised Bolzano, so much of the material is not immediately relevant to us. There are several corrections to New Analysis but in the Sec. 3 entitled "Bolzano's Pamphlet of 1817" Freudenthal makes an error himself. Referring to Bolzano he says (p. 379):

His terminology is unusual: a sequence of functions is called a veränderliche Grösse, and a single function a beständige Grösse. The Cauchy convergence criterion is formulated for a sequence, not of numbers, but of functions, and the property that is formulated, is, in fact, uniform convergence...

For the concept of the uniform convergence of a sequence of functions it is essential to quantify over the independent variable. There is no suggestion of such quantification in RBS6, 7 or anywhere else in RB. Nor can there be any doubt about it precisely because of the perfectly normal use of beständige Grösse (constant quantity) in RBS2 and RBS7. The limit of the sequence of function values,  $F_1x, F_2x, \dots, F_nx, \dots$ , say  $Fx$ , is a constant quantity



because  $x$  is being regarded as fixed; the sequence is point-wise convergent.

The partial sum  $F_n x$  (not the sequence) is a veränderliche Grösse (variable quantity), as  $n$  varies, but this term is only used in this context in RB§1.

Usually veränderliche Grösse is used for the independent variable  $x$  (e.g. RB§11).

The recent paper Kitcher [1] is by far the most substantial and interesting study of RB that has appeared in the literature. Here our interest is on the paper's Sec. III which is chiefly about the proof of RB§7. Kitcher's main point is, ostensibly, that the proof will not make sense (will be "utterly incomprehensible" and "hopelessly askew") if we view it as part of an attempt at arithmetising analysis. Instead, we are invited to view it "against the background of his [Bolzano's] ideal of algebraic analysis". In fact in this context the distinction seems more rhetorical than substantial for it is not clearly related to the remarks Kitcher makes in explaining the proof. He divides the argument into two parts:

[1] If the assumption that there is a constant quantity to which the sequence tends does not contain "anything impossible" then there is such a quantity.

[2] Since on the basis of that assumption we can determine the quantity as precisely as we like, there is nothing impossible contained in the assumption. (Kitcher [1] p.248)

These corresponds to the steps, (ii) implies (iii), and (i) implies (ii), in terms of our analysis of the argument above (p216). Kitcher says we

should consider [1] in the light of what he calls (and attributes to Bolzano, the "liberal approach to quantities". According to this, we may take expressions to denote (analytic) quantities provided that our assumptions are compatible with the laws of analysis. So consistency implies existence. Evidence that Bolzano did follow this liberal approach is then adduced from numerous references to Paradoxien des Unendlichen. Then it is easy to justify step [1] because a proposition is possible if and only if it is compatible with the conceptual truths of analysis. This argument is in accord with what we have said above (p.55 ) based on remarks made in BD. Our only criticism would be the strong and unqualified reliance Kitcher makes on the Paradoxien, a work which was not written (even then only in note form) until the last year of Bolzano's life in 1848. There are many drastic changes in this work from his earlier views (e. g. he there defends the existence and use of infinitely large and small quantities) and so it is quite unfounded to use it to support intricate steps in an argument made thirty years earlier.

As for step [2], this is admitted to be simply unjustified. A good example is given to prove this which we could paraphrase as follows. Suppose  $\pi$  were an algebraic number and consider a suitable Cauchy sequence (e. g. derived from inscribed polygons) which converges to  $\pi$ . The number defined by the properties of being algebraic and the limit of this sequence could be arbitrarily closely approximated to but to assume it exists would be contradictory. Kitcher adds the speculation that Bolzano may have argued in the

following way. Any incompatibility of the limit with the laws of analysis would be preserved by the approximation process, i. e. if the approximating quantities were compatible then so would be their limit. Thus the argument could be thought of as a kind of relative consistency proof. Such an idea is valuable because its plausibility shows that Bolzano may not just have been dissimulating in demonstrating the possibility of arbitrary approximation.

## 4.4. The Continuity of Functions

### 4.4.1. Introduction

The concept of function is more abstract and more recent than that of an infinite series and so the analytic concept of continuity had received less treatment before Bolzano's time than the concept of convergence. Accordingly there is less background material available or necessary for this topic. It will be convenient to deal with the background, Bolzano's own work and the secondary material all together, dividing the subject only into two sections dealing with the concepts of function and continuity respectively.

### 4.4.2 The Concept of Function

The idea of the value of one quantity depending on the value of one or more other quantities seems simple and must be extremely old. Yet the development of the mathematical concept of function was complicated and very slow. It first of all required the means to symbolise arithmetical dependence and then after long use of the resulting relations and equations there appeared the initial concept of function as any "analytic expression". The first explicit definition of function in this sense was actually published less than a hundred years before the time Bolzano was writing his early analysis works. It was given by John Bernoulli in 1718 (Bernoulli, John [2]).

Euler, his pupil, gives a similar definition in the Inroductio in analysin infinitorum (1748) which reads as follows:

A function of a variable quantity is an analytic expression composed in any way from this variable quantity and numbers or constant quantities.  
(Euler [4])

Euler's work in the 18th century was destined to give this new concept the fundamental place it was to occupy in mathematics. The time was ripe for its introduction and definition. As for infinite series, the calculus had stimulated work concerning all kinds of functions and the concept had been considered and some definitions attempted in the last decades of the 17th century. (Most notable here were the works Gregory J. [1](1667), Leibniz [8] (1673), John Bernoulli (Leibniz [1] p. 506, 507)(1698)). However, most mathematicians of the 17th century would have thought of a function primarily as a curve and to appreciate Bolzano's view-point it is worth remembering that the analytic concept of function was really an insight of the 18th century.

The word "analytic" here certainly implied "non-geometric" but it also had a positive, though variable, meaning. The conservative view was that an analytic expression was composed only from the four basic arithmetical operations together with taking roots. The inclusion of limiting or infinite processes was controversial and the idea of a completely arbitrary function was not to appear until Dirichlet [1] (1837). It is claimed in Youschkevitch [1] that Euler had a completely general concept of function as early as 1755

because in the Institutiones calculi differentialis (p. 4) there is the following account:

If some quantities so depend on other quantities that if the latter are changed the former undergo change, then the former quantities are called functions of the latter. This denomination is of the broadest nature and comprises every method by means of which one quantity could be determined by others. If, therefore,  $x$  denotes a variable quantity, then all quantities which depend upon  $x$  in any way or are determined by it are called functions of it.

The generality of this account all depends on what Euler means by "every method". He always in fact uses functions which are representable by power series. Lacroix in the Traité du Calcul... (1797 and 1810) says,

Toute quantité dont la valeur dépend d'une ou de plusieurs autres quantités, est dite fonction de ces dernières, soit qu'on sache ou qu'on ignore par quelles opérations il faut passer pour remonter de celles-ci à la première. (Lacroix [1] p.1)

And in Kline's judgement (Kline [1] p. 949) Lagrange uses the word "function" in the second edition of his Mécanique analytique (1811-15) to cover "almost any kind of dependence on one or more variables". On the other hand, Gauss, in his 1813 paper on hypergeometric series is obviously uneasy about allowing infinite processes in a function. He speaks of the series, "tamquam functio quatuor quantitatum  $\alpha, \beta, \gamma, x$  spectari potest" ("in as much as it can be

viewed as a function of the four quantities  $\alpha, \beta, \gamma, x''$ ) (Gauss [2]).

Thus even if the most general and abstract concept of function had not been appreciated by 1815 the use and notation of the concept of the usual algebraic and transcendental functions was well established and Bolzano assumes these to be well known. Functions for Bolzano are real valued but are not always assumed to be single-valued (A306;BL, §26). For the most part though he has in mind functions which are, or can be represented as, power series. Contrary to the claim of Kolman there is no discussion in RB (or in BL or DP) of the definition of the function concept. Kolman writes that,

... before he proceeds to the proof of the theorem [RB§15] Bolzano gives a logically strict definition of continuity which in turn is preceded by the definitions of variable quantity and function...

Bolzano defined function in this work as a dependence given by an arbitrary known or unknown law provided that to every value of one variable there corresponds a determinate value of the other. (Kolman [1] p. 46, 47)

There is, of course, no reference to this definition: it does not exist in the early work at all. Either Kolman was deliberately composing myths or he is confusing RB with the Functionenlehre to which he goes on to refer and which does contain a definition like that above (Bolzano [6]). The claim about defining variable quantities is even worse because his comments are virtually copied from Stolz who concocted a distinction of his own between freely

variable and continuously variable quantities. Kolman, however, does not mention Stolz and he omits the reference which Stolz gave to the (one) place in RB where the phrase "freely variable" occurs. We quote Stolz's original paragraph (Stolz [1] p. 257) headed, "II. Variable Quantities":

A quantity which can assume all possible values between two given values is called according to RB, p. 49 "freely variable", a quantity which without assuming all values nevertheless takes values which differ arbitrarily little from each of its values, is called "continuously variable" (c. f. RB, p. 11, 49).

It is sufficient to look at the references to RB(A439, 477) to see that the meaning of a "freely variable" quantity and the distinction given here between freely and continuously variable are entirely of Stolz's own construction.

#### 4. 4. 3. The Concept of Continuity

We have indicated the beginning of the 18th century as marking the transition from a geometric to an analytic concept of function. The corresponding transition from the spatial concept of the continuity of a curve to the purely arithmetical description of the continuity of a function lagged behind for about one hundred years. But it is perhaps misleading to speak of a "corresponding transition". While a function was thought of as a curve continuity was redundant: wherever there was a curve it was continuous.



Only with the increasing use of analytic expressions did the need arise to preserve and express the desirable properties of spatial curves.

In Volume 2 of Euler's Introductio... (p. 11) he divides both curves and their corresponding functions into continuous and discontinuous (or mixed) ones. But what Euler meant had nothing to do with what we now mean or what is intuitively meant by the word "continuous". A function was continuous in his sense over a certain domain if it has the same analytic expression, or equation, over the whole domain. It is discontinuous at a point where the form of its equation changes. Thus for Euler continuity meant something like uniformity. This thoroughly confusing language continued well into the 19th century. Its significance was partly sustained by the controversy (mainly between Euler and D'Alembert) over whether the initial form of a vibrating string can be given by a single expression or not. But as it was realised that the question of whether a particular curve or function can be represented by one or more equations is rather arbitrary, Euler's language for the distinction was gradually dropped in favour of its more significant modern sense.

The attempts of the late 18th and early 19th centuries to give a suitable analytic definition of continuity need much more investigation than will be given here. The available historical literature is remarkably scarce on any work prior to Bolzano. The ideas about continuity in Lacroix [2] Art. 60 and in the 1814 paper Cauchy [2] are relatively vague. In the Functionenlehre (Bolzano [6] p. 16) there is a note in which Bolzano mentions his criticisms

of ideas about continuity in Kästner [1], Fries [1], Eytelwein [1] and Lacroix [2]. Only the first of these had appeared when he was writing RB and he specifically mentions Kästner in connection with the intermediate value theorem. The main criticism (which is also made in RB, 12(A440) but not referring to anyone in particular) was that continuity of a function was defined as, or identified with, the property of taking all intermediate values between any two values of the function. Bolzano was emphatic that though this was a true theorem about continuous functions it would not suffice as a definition of continuity. This insight was obviously closely connected with his seeing the need for a proof of the intermediate value theorem. Bolzano's insistence on distinguishing clearly between a true property, and a correct definition, of continuity was significant. He was accustomed to giving examples, or counter-examples to illustrate and confirm distinctions (e.g. A467;RB, §10). From the absence of any such example here, and its intrinsic interest, it is reasonable to conclude that he did not know an example of a function to show that taking intermediate values is insufficient to ensure continuity. (All commentators who give an example mention the type of function given in Darboux [1] (1875) i. e.  $f(x) = \sin\left(\frac{1}{x}\right), x \neq 0, f(0) = 0$ . Is this really the first such example?) Bolzano was more likely to have been led to his definition, not for such technical reasons, but on primarily conceptual grounds. The taking of intermediate values was just a crude translation of the alien spatial intuition; it would not express the essence of the more general analytic concept.

The definition of continuity in RB is pre-figured by a clear but relatively informal and incidental account in BLS<sup>S</sup>29(A309). We discuss this important lemma later (5.2.3) but here we shall just quote what is said on BL, 34(A314) about continuity:

In fact a function is said to be continuous if the change which occurs for a certain change in the argument can become smaller than any given quantity if the change in the argument is taken small enough.

Bolzano proceeds to argue that, assuming there exist functions  $\overset{r}{F}x$  and  $\overset{r}{f}x$  each consisting of an arbitrary number of terms,  $r$ , then the equation  $\frac{\overset{r}{F}(x+\omega) - \overset{r}{F}x}{\omega} = \overset{r}{f}x + \Omega$  indicates that  $\overset{r}{F}x$  is continuous because  $\overset{r}{F}(x+\omega) - \overset{r}{F}x = \omega(\overset{r}{f}x + \Omega)$  can become smaller than any given quantity if (with the same  $r$  and  $x$ )  $\omega$  is taken small enough. Now to assume the existence of such an  $\overset{r}{f}x$  is to assume that  $\overset{r}{F}x$  is differentiable so this result amounts to the fact that differentiability implies continuity. The argument that follows to show that also  $\overset{r}{f}x$  must be continuous is wrong; it is falsely assumed that  $\frac{\overset{r}{F}(x-i+\omega) - \overset{r}{F}x}{\omega}$  may be made arbitrarily small by decreasing  $i$  and  $\omega$ . It is in the course of the main proof of BLS<sup>S</sup>29 that Bolzano appeals to the intermediate value theorem and refers to the forthcoming proof in RB. There is no further mention of continuity in BL.

In RB the question of continuity arises in the Preface in the course of criticism of earlier proofs of the intermediate value theorem. At RB, 11 (A439) Bolzano states his own definition:

According to a correct definition, the expression, that a function  $fx$  varies according to the law of continuity for all values of  $x$  inside certain limits\* means just that, if  $x$  is some such value the difference  $f(x + \omega) - fx$  can be made smaller than any given quantity provided  $\omega$  can be taken as small as we please. With notation I introduced in §14 of Binomische Lehrsatz etc. (Prague 1816) this is,  $f(x + \omega) = fx + \Omega$ .

The footnote \* reads:

There are functions which vary continuously for all values of their root, e.g.  $\alpha x + \beta$ . But there are others which are continuous only for values of their root inside or outside certain limits. Thus  $x + \sqrt{(1-x)(2-x)}$  is continuous only for values of  $x < +1$  or  $> +2$  but not for values between  $+1$  and  $+2$ .

Bolzano's definition is clear, original and suitably formulated in symbols for easy arithmetical application. The limit concept is not required explicitly, as with the convergence criterion, due again to the use of arbitrarily small quantities. This procedure is admirably practical. Continuity is used essentially in the main theorem RBS15(A479), and in RBS17(A484) it is straightforwardly proved that polynomials are continuous. In DPSS1-6(A514-520) a number of theorems are proved about continuous functions. The first main theorem, DPS2 is rather long-winded and imprecise but it is basically the statement that a continuous function of a continuous function is again continuous. There is a confusing ambiguity on the statement of the theorem.

The phrase in the first sentence, "those values of  $x$  which approach as close as desired to  $a$ " is taken to include  $a$  when  $f(x)$  is said to be continuous for this range. (At least, in the proof it is assumed that  $f(x)$  approaches  $f(a)$  arbitrarily closely.) But when  $X$  is said to be determined (as a function, say  $\varphi$ , of  $f(x)$ ) for this range of values of  $x$ ,  $a$  must be excluded. The point of the theorem is to show that  $X$  is also determined at  $x = a$  provided  $\varphi$  and  $f$  are continuous, i. e. that its value is  $\varphi\{f(a)\}$ . The proof given is straight-forward and correct. Stolz, in attempting to clarify the theorem as it stands gives a paraphrase (Stolz [1] p. 263) of both the proof and theorem which is muddled and incorrect. For example, Bolzano does not say in the theorem, "if  $X$  is defined and continuous at  $x = a$ ". Nor does Stolz point out what the theorem is really about. Kolman (following Stolz, as ever, without acknowledgement) does say in a footnote (Kolman [1] p. 53) that in modern terminology the theorem means  $\lim_{x \rightarrow a} F\{f(x)\} = F(\lim_{x \rightarrow a} f(x))$ ; though he should have written  $\varphi$  instead of  $F$  since he puts  $X = \varphi\{f(x)\}$ , (Stolz puts  $\varphi\{f(x)\} = Fx$ ). In DPSS3 Bolzano extends the result to the case when  $X$  is a continuous function of a finite or infinite number of continuous functions,  $f(x), \bar{f}(x), \bar{\bar{f}}(x), \dots$ . Thus the theorem is true in the finite case but not necessarily true in the infinite case. The proof is the same for either case and it is obviously wrong. It is assumed both that  $X$  may be an arbitrary function of  $f(x), \bar{f}(x), \bar{\bar{f}}(x), \dots$  etc. and that all but one of these functions can be held fixed so as to reduce the result to that of DPSS2. Then DPSS5, 6 are special cases of DPSS3.

Bolzano's primacy and success in the defining of continuity has often been acknowledged (e. g. Pringsheim [2] and Kline [1] p. 951), and so (less often) has the superiority of his formulation over that of Cauchy (e. g. Freudenthal [1] p. 380, see p. 188). Coolidge remarks on the continuity definition, "If Bolzano had done nothing else in mathematics, this alone would secure for him a place in the history of the subject." (Coolidge [1]) Inexplicably, Kitcher does not seem to have appreciated this part of Bolzano's work, "Given the unclarity of the notion of "continuous function" employed by Bolzano and his contemporaries." (Kitcher [1] p. 260)

Grattan-Guinness has claimed the continuity definitions as a great example of the accord between Cauchy and Bolzano, especially in exemplifying his idea of limit-avoidance. Having quoted Bolzano's continuity definition from RB, 11(A439) he reformulates it in two ways:

#### Definition 3.3

$f(x)$  is continuous at  $x = x_0$  if  $f(x_0 + \alpha) - f(x_0)$  is small when  $\alpha$  is small.

Let us reinterpret the definition as defining the limiting value  $f(x_0)$  of  $f(x_0 + \alpha)$  as  $\alpha$  tends to zero, rather than continuity. It is continuity that guarantees that as a matter of fact this limit exists, and to avoid confusion we shall denote it by the symbol  $B$  (which is unconnected with the symbol  $f(x)$ ) rather than  $f(x_0)$ . Then we have:

#### Definition 3.4

The function  $f(x_0 + \alpha)$  has a (unique) limit, of value  $B$ , as  $\alpha \rightarrow 0$  if

$f(x_0 + \alpha) - B$  is small when  $\alpha$  is small. In other words we may move as close as we wish to the limit  $B$ , while still avoiding the limit itself.

The full significance of Bolzano's definition 3.3 can only be grasped when seen in terms of the pattern of definition 3.4. Bolzano has defined continuity there but he has done it in a limit-avoiding way in terms of arithmetical subtraction of expressions. (Grattan-Guinness [2] p. 54, 55)

But the significance of limit-avoidance is still far from clear.

In Birkhoff [1] there is a section entitled, "Bolzano on Continuity and Limits". This only refers to RB and is mainly taken up with a modernised version of the convergence criterion  $RB\text{§}7$ . The continuity definition of RB Preface is stated but then instead of giving Bolzano's version of the intermediate value theorem (e. g.  $RB\text{§}15, \text{§}18$ ) a theorem is stated which appears on RB, 14(A442). This "hybrid" theorem belongs to nobody - it is an arithmetic form of an assumption Bolzano is in the course of criticising on the grounds that it is effectively equivalent to the theorem to be proved (see 5.3.1). It has been inadvertently mistaken for the main theorem of the paper, but Bolzano would never have used the phrase occurring in it, "the function vanishes or becomes infinite".

Undoubtedly Bolzano's great achievement in the continuity definition was to express what had been a spatial intuition in purely arithmetic terms. He seems to have been motivated to seek such a definition on conceptual

grounds: an analytic concept of function requires a purely analytic concept of continuity. He had arrived at the insight contained in his definition by the time of writing BL and it is reasonable to conjecture that it was suggested by the notion of arbitrarily close approach that had been so successful with infinite series. In contrast to his merely implicit treatment of convergence it is clear that Bolzano was well aware of the central importance to analysis of his concept of continuity.



## Chapter 5 : Analysis II

### 5.1. Introduction

In Chapter 4 we have considered Bolzano's treatment in his early analysis works of those concepts (namely those of limit, convergence and continuity) which have turned out to occupy a central place in modern analysis. As mentioned in 4.1.3 these concepts were not always those to which Bolzano himself attached particular significance in the course of working out his "new way of developing analysis" (A279:BL, XV). This is hardly surprising, their modern significance is a product of one hundred and fifty years of reinforcement through repeated refinement and generalisation. What matters here historically is to understand what was significant to Bolzano and see how it relates to what has become fruitful in later mathematics. A useful new concept often emerges, not intentionally or through direct effort, but rather in the course of pursuing some other goal. It is the recurrent theme of this thesis that Bolzano developed some important new concepts (or at least refinements of existing concepts) as a result of following his fundamental requirements for mathematical proofs and concepts.

In his early analysis works the main purpose was to give rigorous

proofs of certain fairly elementary results. In the present chapter we shall briefly examine each of these works in the light of this aim and show how the proofs they contain are related to Bolzano's general methodology. Attention to the background of each theorem is especially necessary here not only to provide the appropriate context but because Bolzano himself always begins with a thorough consideration of all the previous relevant work known to him. His concepts and ideas arose as much in criticism of earlier efforts as in original response to a problem. Accordingly each of the following main sections begins with an introduction containing background material with the emphasis on those authors mentioned by Bolzano. Then there follows an account and assessment of what each paper actually contains.

## 5.2. The Binomial Theorem and BL

### 5.2.1. Introduction

The original formulation of the binomial theorem for a rational exponent was given by Newton in a famous letter of 1676 published in Newton [4]. The first strict proof, with a proper consideration of the conditions for convergence and allowing for a complex variable, is generally acknowledged to have been given in Abel [1] (1826). The proofs attempted in the eighteenth century were either by means of calculus or by combinatorial methods. There was virtually no treatment of the two major problems of the convergence of an infinite series and of the meaning of an irrational exponent. For the most part these were not "problems" at all. Such questions did not arise when the main concern was the actual calculation of approximate values of the series for practical purposes. Typical of the better proofs at this time (e. g. those of Euler) was the remark that for the binomial series to be "suitable for calculation" the argument  $x$  must be a proper fraction. Bolzano's proof in BL was the first to take both the problems of convergence and of an irrational exponent seriously.

Fundamental as it was to many applications of calculus the binomial theorem was slow to be recognised in its own right and in the first half of the eighteenth century there were only a few attempts at a proof. The German term "binomische Lehrsatz" does not even appear in the Mathematisches Lexikon compiled by Wolff in 1716, though it is present in the edition

of 1747 (Wolff [4]). In Euler [5] (1749) the binomial theorem is introduced without explanation as a "theorema universale", it is used extensively but there is no attempt at a proof.

The early calculus proofs (for example those by Colson [1] (1736), and Maclaurin [1] (1742)) were, as pointed out in Pringsheim and Faber [1], both inadequate and circular. They were inadequate in assuming the existence of a power series development for  $(1+x)^n$  and only determining the coefficients. They were also circular because the binomial theorem was originally used in finding the derivatives of rational powers which were then being used to prove the binomial theorem.

A proof for a positive integer exponent was given in James Bernoulli [2] (1713) by considering combinations; it was given in Castillon [1] (1742) with the refinement of an inductive proof of the combinatorial argument. The same improvement is found in Kästner [5] (1745). For rational exponents a relatively satisfactory proof did not appear until Euler's paper of 1775 (Euler [2]). Here Euler denotes the series  $1 + nx + \frac{n(n-1)x^2}{2!} + \frac{n(n-1)(n-2)x^3}{3!} + \dots$  by  $[n]$ , so for positive integer  $n$ ,  $[n] = (1+x)^n$ . He then shows that  $[m] \cdot [n] = [m+n]$  just by considering the first few terms of each side, there is no induction. Then  $[m]^2 = [2m]$ ,  $[m]^3 = [3m]$  etc. and generally for any integer  $a$ ,  $[am] = [m]^a$ . Consequently for integer  $i$ ,  $[1] = \left[\frac{1}{2}\right]^2 = (1+x)^i$ , so  $\left[\frac{1}{2}\right] = (1+x)^{\frac{i}{2}}$  and generally  $\left[\frac{1}{a}\right] = (1+x)^{\frac{i}{a}}$ . In Pringsheim and Faber [1] this paper of Euler's is spoken of in glowing terms as the first "fully valid"

proof, significant for its new fruitful method of attending to the summation of the binomial series rather than the development of the function  $(1 + x)^n$ . But without any discussion of convergence, i. e. without any consideration of whether the symbol  $[n]$  above denotes a quantity at all, the proof can hardly be called "fully valid" or be regarded as satisfactorily dealing with the summation of series. It is a significant but modest paper. The work L'Huilier [2] (1795) gives a rather similar proof to that of Euler with the improvement of having a proper induction step for proving  $[m] \cdot [n] = [m + n]$ .

Between Euler's paper of 1775 and the end of the eighteenth century there appeared a considerable number of proofs of the binomial theorem for rational and negative exponents. Rarely is there a mention of convergence or of the case of an irrational exponent. The paper of Segner [1] is one of the best in this respect: he follows the same lines as Euler but notes that an irrational exponent can be regarded as a limiting case of rational exponents and that the variable will "in general" be smaller than 1. However, the absence of consideration of an irrational exponent would still not have been recognised as a defect. Apart from the practical irrelevance of such exponents which has already been noted, it seems that their theoretical possibility was often not considered. For example, in Klügel's Mathematisches Wörterbuch (Klügel [2] (1803)) under Binomischer Lehrsatz we read, "... 2. The formula is general, the exponent n may be an integer or fraction, positive or negative".

The notation  ${}^nS$ , for the binomial series denoted  $[n]$  by Euler, was probably first introduced by Busse (see Klügel [2] Vol. 1, p. 325) and it was widely adopted. Bolzano uses this notation in BL, XIV. There were no significant new contributions to the binomial theorem until Bolzano's BL.

Bolzano must have studied most of the previous proofs of the binomial theorem. In BL Preface he gives a detailed classification and criticism of the various methods that had been used. But he does this without giving any specific references. References were generally still rather haphazard in the mathematical literature of this period: there would have been a large number of such references and after all BL was intended to be a fairly elementary textbook. Bolzano lists about thirty authors known to him who had attempted a proof of the binomial theorem (A268;BL, IV), but he mentions no titles! In many cases it is quite clear which work Bolzano must have intended and since the list is fairly comprehensive (especially for the later eighteenth century) we give, in the next section, a probable list of Bolzano's sources. The list is certainly not exhaustive, for example, he does not mention significant proofs by Maclaurin and van Swinden. In fact Bolzano may have relied on one or two reference works. In Klügel [2] the article Binomischer Lehrsatz lists all but four of Bolzano's list with regard to authors whose works appeared before Klügel [2] was published (i. e. all but the last seven of Bolzano's authors). This work was probably Bolzano's main source; the references there are given accurately and in detail though there is very little comment

by Klügel. In order to give his criticism of previous methods of proof Bolzano must have studied the majority, if not all, of these references.

The criticism in BL Preface is in two parts. First he comments on misunderstandings of the meaning of the binomial theorem, then on the attempts to prove it. The former mainly concerns the rejection of infinite series and we have already dealt with this in Chapter 4. The previous proofs must all be defective, Bolzano says (A271;BL, VII), because none of them involves the condition  $x < \pm 1$ ; the proof should therefore hold generally but the result does not. The most basic fault was, of course, the lack of attention to convergence and Bolzano lists this first, albeit in his own rather elaborate way: the series are said to be equal when only the terms up to the  $r$ th term are shown to be equal, "beyond this  $r$ th term... the difference between the two series can perhaps never be reduced as much as desired" (A272;BL, VIII). The Euler and L'Huilier proofs of  $\binom{m}{r} \cdot \binom{n}{r} = \binom{m+n}{r}$  are evidently in mind here. There are three other main types of proof which Bolzano analyses: proceeding from a general form,  $(1+x)^n = A + Bx^\beta + Cx^\gamma + \dots$  and determining the  $\beta, \gamma, \dots, A, B, \dots$  (A273;BL, IX); proof by Taylor's theorem (A276;BL, XII); and finally an argument from interpolation in a geometric series (A277;BL, XIII). The last of these is briefly dismissed, and the main objection to using Taylor's theorem is that this is a much more difficult theorem than the binomial theorem and it can only be strictly proved by using the latter (see also A268;BL, IV). Thus here again (as in the geometrical

work) is an allusion to a correct ordering of theorems according to complexity. The method of assuming a general series for the binomial series and determining its coefficients and exponents is considered in detail. This, in principle, is the method Bolzano adopts in BL so his criticisms here are especially important as they indicate the errors which his own procedure attempts to avoid.

There are three steps that are criticised. First there is the argument from

$$(1 + x)^n = A + Bx^\beta + Cx^\gamma + \dots \quad (1)$$

to

$$n(A + Bx^\beta + \dots) = (1 + x) (\beta Bx^{\beta-1} + \gamma Cx^{\gamma-1} + \dots) \dots \quad (2)$$

This deduction is usually made either by differentiation (which Bolzano says is, "still based on the most shaky foundations" (A275;BL, XI)) or else the procedure of differentiation is imitated without explicit mention. He explains the latter as the use of divisors which are eventually put equal to zero. (He may also have had the so-called method of "residual analysis" in mind here, e.g. in Landen [1]). The second criticism is over the fact that having shown (1) implies (2) and finding the constants to satisfy (2) they then assume, at least tacitly, that (2) implies (1). Thirdly, Bolzano says that the exponents  $\beta, \gamma, \dots$  are often just assumed to be  $1, 2, 3, \dots$  without sufficient reason.

Several of the general criticisms levelled against the binomial theorem proofs, such as the arbitrary use of infinite series and the use of divisors



which are put equal to zero, are also held against the usual derivation of the exponential and logarithmic series. The treatment of these series as an application of the binomial theorem, forms a substantial part of BL (BL, 102-144).

Bolzano had two purposes in mind with BL apart from the aim of attracting some attention and response to his work. It was "a sample of a new way of developing analysis" (A279;BL, XV) and at the same time it was meant to be a new, substantial contribution to analysis in being the first really strict proof of the binomial theorem and associated results. Consequently it had to cater for two kinds of readers. As a way of developing analysis it was to be accessible to beginners and suitable as a textbook, but as a thoroughly strict proof it was to be complete and rigorous. To cope with this Bolzano indicates in the Preface (A280;BL, XVI) many paragraphs which can be omitted on a first reading. He also points out that the main proof is not nearly so long as might be supposed from the total number of pages (it occupies about 20 of the 144 pages). Apart from the style, which is far from terse, there are several reasons for the work's length. It is not only about the binomial theorem; it includes proofs of the multinomial theorem and the exponential and logarithmic series. There is explanation and motivation suitable for beginners who have not met any of these topics before. The concepts of convergence and differentiation which Bolzano regards either as novel, or unsatisfactorily explained in earlier texts, are treated on each occasion by his elaborate method of working explicitly with arbitrarily small quantities.

### 5.2.2. Bolzano's Sources on the Binomial Theorem

The list of authors which Bolzano gives (A268;BL, IV) follows no obvious order exactly, it is partly by nationality and partly chronological. We have followed his order and given, for each author, the work containing his most relevant contribution(s) to the binomial theorem. Where appropriate the reference to the relevant section of a large work has been given. Any work which is doubtful or has not been checked because of being unavailable has been prefixed by a question-mark. We have included references to the polynomial or multinomial theorem.

- Colson, J.                      The method of fluxions and Infinite series etc.  
translated from the original (I. Newton) with a perpetual comment. 1736, p. 309
- Horsley, S.                      Isaaci Newtoni Operaque exstant omnia commentariis illustrabat S. Horsley, 5 vols. 1779-85.  
Vol. I, p. 286, footnote.
- Simpson, T.                      A general method of exhibiting the value of an algebraic expression involving several radical quantities in an infinite series.... Phil. Trans. 1751, p. 20.
- Robertson, A.                      The binomial theorem demonstrated by the principles of multiplication. Phil. Trans. 1795, p. 298.

- Sewell, W.                      Newton's binomial theorem legally demonstrated by algebra. Phil. Trans. 1796, p. 382.
- Landen, J.                      A Discourse concerning the Residual Analysis...  
Londor. 1758.
- Clairaut, A. C.                Anfangsgründe der Algebra,... Berlin 1778  
Dritter Theil, XLVIII
- Aepinus, F. V. T.             Demonstratio generalis theorematis Newtoniani de binomio ad potentiam indefinitam elevando.  
Novi Comment. Petrop. 1760/1, Vol. VIII
- Castillon, J.                 A demonstration of the polynomium of Sir Isaac Newton. Phil. Trans. 1742, Vol. XLII, p. 91.
- L'Huillier, S.                Principiorum Calculi Differentialis et Integralis Expositio Elementaris... Tübingen, 1795.
- Lagrange, J. L.              Théorie des fonctions analytiques....  
Paris 1797, 1813, Paragraph 18,
- Kästner, A. G.                Demonstratio theorematis binomialis. Leipzig 1745.  
Theorema binomiale universaliter demonstratum,  
Göttingen 1758.
- Euler, L.                      Demonstratio theorematis Newtoniani de evolutione potestatum binomii pro casibus quibus exponentes non sunt numeri integri. Novi Comm. Petrop. 1775, Vol. XIX, p. 103.

- Euler, L. (continued) Nova demonstratio quod evolutio potestatum binomii  
Newtoniana etiam pro exponentibus fractis valent.  
Nova Acta Petrop., 1787, Vol. V., p.52.
- Segner, J. A. von Demonstratio universalis theorematis binomialis  
Newtoni. Nouv. Mém. de Berlin 1777, p.37.
- Scherfer=Scherffer, K. ? Institutionum analyticarum 1771-2.
- Karsten, W. J. G. ? Anfangsgründe der Mathematischen Wissenschaften  
3 vols. Rostock 1780.  
? Mathesis theoretica elematar ac sublimior.  
Rostock, 1760, p.567.
- Klügel, G. S. Mathematisches Wörterbuch. Vol. 1, 1803 Articles:  
Binomial-Coefficienten and Binomischer Lehrsatz.  
Bemerkungen über den Polynomischen Lehrsatz  
(In the collection by Hindenburg below).  
? Analytischer Trigonometrie (Anhang) Braunschweig,  
1770.
- Busse, F. G. von Elementarischer Beweis des allgemeinen binomischen  
Lehrsatzes, contained in Kleine Beyträge zu Math-  
ematik und Philosophie, Dessau 1785, p. 17.
- Pfaff, J. F. Peculiaris differentialia investigandi ratio ex  
theoria functionum deducta. Helmstadii, 1788 §XII.

- Pfaff, J. F. (continued) *Disquisitiones analyticae*, Helmstadil 1797,  
Vol. 3, p. 301. *De Theoremate polynomiiali.*
- Rothe, H. A. *Theorema binomiale ex simplicissimus analyseos  
finitorum fontibus universaliter demonstratum.*  
Leipzig, 1796.
- Hindenburg, K. F. editor of: *Der polynomische Lehrsatz, das wichtigste  
Theorem der Analysis. Sammlung combinatorisch-  
analytischer. Abhandlungen, 1796-1800, Vol. I.*
- Kaussler, C. F. translator of: *Vollständige Anleitung zur Algebra  
von L. Euler 3ter Teil, Anhang I: Allgemeiner  
Beweis der binomischen Lehrsatzes, 1796.*
- Schulz = ? Schultz, J. ? *Kurze Entwicklungen einiger der wichtigsten  
mathematischen Theorien, Königsberg, 1803.*
- Pasquich, J. ? *Anfangsgründe der gesammelten theoretischen  
Mathematik, 2 vols., 1812.*
- Rösling, C. L. ? *Grundlehren von den Formen, Differenzen, Differ-  
entialen und Integralien der Functionen, Erlangen, 1805.*
- Jungius, F. W. ? *Die Lehre von der Combination und Permutation  
usw. Berlin, 1806.*

Fischer, L. J. and

Krause, K. C. F. ? Lehrbuch der Combinationslehre und der Arithmetik.  
Dresden, 1812.

Crelle, A. L. ? Versuch eine rein analytischen usw. Darstellung  
der Rechnung mit veränderlichen Grössen usw.  
Göttingen, 1813.

Nordmann, G. ? Grundriss der Algebra. Leipzig, 1815.

### 5.2.3. Account and Assessment of BL

None of the results in BL was actually new but as a textbook treatment of the binomial theorem it was much more detailed and comprehensive than any of its predecessors. It contained several methods and proofs which were original and which had an important bearing on the foundations of calculus. In fact it is because Bolzano's methods effectively involve differentiating convergent power series from first principles (using arbitrarily small quantities instead of a limit operation) that the work is so long. The apparatus needed is elaborate to develop and cumbersome to use.

There are three main sections corresponding to the topics mentioned in the title. The binomial theorem occupies §§1-51, the polynomial theorem

is dealt with in §§52-59 and the exponential and logarithmic series are the subject of §§60-74. These sections can be further sub-divided as follows:

Binomial theorem	}	§§ 1-10	positive integer case introduced combinatorially and proved by induction
		§§ 14-22	properties of arbitrarily small quantities
		§§ 23-29	various lemmas, effectively on the differentiation of convergent series
		§§ 30-51	proof extended to positive and negative rationals and irrationals
Polynomial theorem		§§ 52-59	polynomial theorem in the form $(1 + \binom{n}{1}ax + \binom{n}{2}ax^2 + \dots + \binom{n}{m}ax^m)^n$
Exponential and logarithmic series	}	§§ 60-69	power series for exponential and logarithms
		§§ 70-74	definition of e and the use of natural logarithms

In the opening section Bolzano discusses the positive integer case of the binomial theorem and the combinatorial argument for the coefficient of the general term  $x^r$  in the expansion of  $(1+x)^n$ . The essence of the combinatorial argument, which by this time was fairly standard, is to show that

$\binom{n}{r+1} = \frac{n-r}{r+1} \cdot \binom{n}{r}$ : then since it is clear that  $\binom{n}{1} = n$ , it is also clear (by induction) that  $\binom{n}{r} = \frac{n(n-1)(n-2)\dots(n-r+1)}{r!}$ . Now Bolzano rejects

this argument, as a proof, in favour of a proof by induction, saying that the former, "does not seem to us a genuinely scientific proof since it derives the conclusion from an alien concept" (A287;BL, 7). It is not clear whether Bolzano realised that the combinatorial argument involves an "and so on" step which really needs induction. There are not two alternative methods of proof here. There is a proof by induction of a given formula and an explanation (logically unnecessary) of how one might discover the formula. But what is of interest here is that Bolzano rejects the combinatorial argument on conceptual grounds. It should therefore not even form part of a correct proof. Evidently he felt that an argument for counting possible choices was alien to the simple multiplication and addition of like terms involved in this case of the binomial theorem. This may seem rather far-fetched. After all, counting the combinations is simply a way of counting the like terms that will appear in the multiplication. Counting is thoroughly arithmetic and ideas of choice (or combination) are just auxiliary and a matter of convenience.

However, such plausible reasoning is neither relevant to Bolzano's claim nor



correct. To be alien on Bolzano's view a concept need not be from another part of mathematics, it is sufficient that it does not appear in the theorem being proved (see A15, 16; BG, VIII, IX). Some concept of choice or selection (i. e. the gathering together and counting of like terms) is essential to the combinatorial method and is irreducible: it is not explicit or essential in the statement of the theorem. We need to emphasise here that Bolzano does not reject or minimise the value of the combinatorial argument. He puts it first and spends a long time explaining it; he also says it makes clear the correctness of the series for positive integers. What he rejects is that such an argument can be part of the unique objective proof of this case of the binomial theorem. It is, of course, true that the combinatorial approach only has direct application to the positive integer case. The objective proof of a theorem should be that of the most general case (according to the principle stated in BD(A209;BD, 102) that proofs should always proceed from the general to the particular).

This example of Bolzano's application of his methodology highlights two points to which we shall return later. Bolzano gives only negative criteria for determining an objective proof (e. g. there is no positive reason given for supposing the subsequent proof by induction in BLS7 is a "genuine scientific proof"). Secondly, there is the tension between the uninformative, but preferred, proof by induction (A287; BL, §7) and the explanatory value of the rejected combinatorial argument. This tension is unexpected because it appeared in the geometry work, at least, that the proof which exhibited and

correct. To be alien on Bolzano's view a concept need not be from another part of mathematics, it is sufficient that it does not appear in the theorem being proved (see A15, 16; BG, VIII, IX). Some concept of choice or selection (i. e. the gathering together and counting of like terms) is essential to the combinatorial method and is irreducible: it is not explicit or essential in the statement of the theorem. We need to emphasise here that Bolzano does not reject or minimise the value of the combinatorial argument. He puts it first and spends a long time explaining it; he also says it makes clear the correctness of the series for positive integers. What he rejects is that such an argument can be part of the unique objective proof of this case of the binomial theorem. It is, of course, true that the combinatorial approach only has direct application to the positive integer case. The objective proof of a theorem should be that of the most general case (according to the principle stated in BD(A209;BD, 102) that proofs should always proceed from the general to the particular).

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followed the objective ground of a theorem should, in some sense, be the most explanatory.

Before the general case of the binomial theorem is considered there now follows a long section (BL, §§11-29) of preliminaries not specifically related to the binomial theorem. This is the most interesting section of the work from a modern point of view because here Bolzano develops in an original way, and in some detail, three very significant ideas. These are: the convergence of infinite series, the continuity of functions and the process of differentiation. The first two we have already discussed in Chapter 4. As with convergence, Bolzano nowhere says explicitly that he is defining the derivative of a power of  $x$  when he proves in BLS<sup>i</sup>23(A300) that  $\frac{(x + \omega)^n - x^n}{\omega} = nx^{n-1} + \Omega$ , but this would obviously have been recognised by any informed contemporary reader. Here, of course  $\omega$  and  $\Omega$  are the arbitrarily small quantities which Bolzano uses in place of a limit operation and which we have discussed in 4.2.3. Although differentiation plays a vital part in his proof of the binomial theorem and he proves in BLS<sup>S</sup>29(A309) that the derivative of a convergent power series is again a convergent series, he nowhere mentions differentiation in BL except to cast aspersions on its foundations and validity. Presumably he avoided such reference only because of the strong association at this time of calculus operations with the suspect concept of infinitesimals. (For example, see A495;DP, VI footnote) This avoidance is ironic in that Bolzano seems to have had the understanding and technique at his disposal

to have written a thoroughly rigorous account of differential calculus.

However, what is done in BL is tailored to the problem in hand and the derivative of  $x^n$  is first proved for  $n$  a positive or negative rational and finally for  $n$  an irrational. In the course of this proof Bolzano is careful only to use the binomial theorem in the positive integer case which he had already proved thus avoiding the circularity mentioned above (p.247 ). The main argument here for the rational case is clear and correct making essential use of the lemmas on arbitrarily small quantities in BL§§17, 19, 21. For the irrational case Bolzano says (A303;BL, 23) that, "it follows from the definition of the concept of an irrational power that the quantity  $a^{p/q}$  gives a value as close to that of  $a^n$  as desired if  $\frac{p}{q}$  is as close to the value  $n$  as desired." Neither an irrational number nor an irrational power had yet been strictly defined but as with the sufficiency argument of RB§7(A465;RB, 37) Bolzano assumes the existence of a quantity (here the  $a^n$  where  $n$  is irrational) provided it can be arbitrarily closely approximated to by quantities already considered as known or existing (here the  $a^{p/q}$  which, in general, will themselves be irrational). The argument here avoids the real issue but at least Bolzano recognises that there is a separate problem with the concept of an irrational power; this was a problem which even if recognised before was hardly ever acknowledged.

The lemma of BL, §29(A309) is the most significant of these preliminary results. In order to appreciate the statement of the lemma it is

necessary to realise that for the function  $\sum^r f(x)$  Bolzano has in mind (in modern terminology) the difference between the partial sum of a convergent series and the limit function of that series. The lemma states that if the value of  $\sum^r f(x)$  becomes arbitrarily small with increasing  $r$ , and a given value of  $x$ , then so does its derivative  $\sum^r f'(x)$ . This therefore justifies the term by term differentiation of convergent series. It is not definitely stated, but nevertheless very likely that Bolzano only considered the result for power series. He nowhere mentions, for example, the relevant contemporary problem of infinite trigonometric series. The proof, which is basically correct, assumes that the functions concerned are continuous and Bolzano makes this the occasion for defining, in a rather impromptu fashion, what is meant by a continuous function. This has been discussed in 4.4.3. An important gap in the proof, which is acknowledged in a footnote (A312:BL, 32), is the assumption of the intermediate value theorem; this was proved separately in RB.

In the latter part of the proof in BL, §29 Bolzano shows the important fact that, as we should now express it, differentiation is a mapping between continuous functions. The way in which it is shown that continuity is a necessary condition for a function to be differentiable also shows that Bolzano clearly understood at this time how his verbal definition of continuity was to be used precisely and formally. Just as we have claimed this as the first appearance of the precise continuity definition this must also be the first proof that differentiability of a function implies its continuity. (It is by now

fairly well known that Bolzano was also the first to provide a counter-example showing that continuity was insufficient for differentiability, see Bolzano [6] §75.)

The section BL§§30-51 is the centre-piece of BL and contains the main substance of the work: a detailed proof of the binomial theorem and its range of validity. There are some mistakes in the course of the proof (notably a serious error in the range of validity) and it is presented in a very long and complicated way. This is partly a result of the long-winded methods employed and partly it is the price to be paid for the unprecedented detail and thoroughness that Bolzano wanted to achieve. Even with the explanatory paragraphs such as §34 and §37 it would have been (and still is) quite difficult to follow the logical layout of Bolzano's procedure.

The proof really falls into two parts which could well be described today as proofs of uniqueness and of existence. In the first part, for which the key paragraphs are §§30, 32, 33, it is shown that if there is a power series in  $x$  the value of which, for given  $x$  and  $n$ , becomes arbitrarily close to value of  $(1+x)^n$ , then it must be the binomial series  $1 + nx + \frac{n(n-1)x^2}{2} + \dots + \frac{n(n-1)\dots(n-r+1)x^r}{r}$ . (As explained in 4.3.3 for Bolzano the "binomial series" for  $n$  is this finite series of arbitrary length. The "binomial equation" holds if the value, for given  $x$  and  $n$ , of the binomial series is arbitrarily close to the value of  $(1+x)^n$ .) The necessary conditions found in this part of the proof also serve to delimit the possible range of validity

of the binomial theorem by showing some values of  $x$  for which it cannot hold. The second part of the proof, for which the key paragraphs are BL§§38, 40, 41, shows that the binomial equation actually does hold for  $|x| < 1$  and for positive or negative and rational or irrational  $n$ .

The logical structure of the proof was a considerable achievement. In most, if not all, previous attempts to prove the binomial theorem only one of the above two parts was considered. For example, treatments in the works cited in our bibliography for BL by Simpson and Hindenburg deal only with the first part and remain silent or vague on the range of validity. The works by L'Huilier and Euler on the other hand start from the integer case of the theorem and really assume it will make sense in the rational case without any general derivation of its form. Also the details of both parts of the proof, supported by the lemmas we have described and the relatively rigorous treatment of convergence and differentiation, render Bolzano's work far superior to anything that had appeared earlier. Without analysing the details of this section in full we shall mention here certain points by way of explanation or criticism.

The argument of §30 can best be expressed in modern terminology in the identity,  $\frac{d}{dx} (1+x)^n = n(1+x)^{n-1} = \frac{xd}{dx} (1+x)^n$ , where in place of the function  $(1+x)^n$  is put the supposed power series development  $Ax^\alpha + \dots + Rx^\rho + \Omega$ . The resulting necessary condition on such a power series is that denoted  $\dagger$  on BL, 39(A319). By repeated differentiation of the series a more elaborate necessary condition is found in BL, §31 but this is only used for a

part of §35. Then in §32 the arguments for the exponents and coefficients of the series are given very thoroughly and clearly. The main point in this is that the condition of §30 is to hold "for all  $x$  smaller than a certain value", so it must be an identity (as proved in the lemma §28).

In contrast to most of the earlier work, the proof given in §35 is blatantly incomplete and wrong. It is claimed at the outset that,

the binomial series can never give the value of  $(1 + x)^n$  if  $x$  is  $>$  or even  $= \pm 1$ , unless at the same time  $n$  is either a whole positive number or zero (A327;BL, 47).

In fact, of course, the series for  $x = 1$  does converge to  $2^n$  for  $n > -1$ , and for  $x = -1$  it converges for  $n \geq 0$ . The idea used throughout §35 derives from §32 part 3, that the term  $\frac{n(n-1)\dots(n-r+1)}{r!} (n-r)Ax^r$  must become arbitrarily small if the binomial series is to converge. Bolzano tries to show that this will not happen for  $x > 1$  but to do so he is content to consider just the ratio of successive terms and to prove that this has modulus greater than 1. However, in general this is an alternating series and it is necessary to ensure that, if  $a_r$  is the above general term,  $\left| \frac{a_{r+1}}{a_r} \right| \rightarrow k$  where  $k > 1$ . Not surprisingly most of Bolzano's ratios in this proof actually have limit 1 and no inference is possible. In the very long calculation of the ratio  $a_{r+1}/a_r$  for positive  $n$  (case 2. on A330:BL, 50) there is a minus sign ignored half-way through the working and the case of  $x = -1$  is not properly considered at all.

In spite of these errors the work on the positive aspect of the binomial



theorem, that is that it does hold for  $x < 1$ , which occurs in the remainder of this section is perfectly correct. As we have indicated in 4.3.3 this section of Bolzano's work may have suggested to him the convergence criterion given in A462;RB, §6. In BL, §42(A348) he does rely on the remark in BL, §12 for the starting point of the induction for any negative integer. The various cases follow of  $n$  being a fraction of the form  $1/m$  (§43), any positive fraction (§44), negative fractions (§45) and finally irrational  $n$  (§46). The last-mentioned case is conspicuous by its presence at this stage in the history of the binomial theorem. But as with its appearance in the derivative definition of §23 we can really only credit Bolzano with its recognition. The "proof" for this case follows from the claim that "as a consequence of the concept of the symbol  $(1+x)^i$ , also  $(1+x)^{n/m}$  comes as close to the value  $(1+x)^i$  as desired" (A356;BL, 76).

There seems little of lasting interest in the remainder of BL that has not been dealt with in Chapter 4. Given the binomial theorem, the work on the polynomial theorem was quite standard and includes nothing more than was to be found, say, in the work Hindenburg [1]. It remains an historical curiosity that the series for  $a^x$  (§64) and  $e^x$  (§72) as well as the corresponding logarithmic functions (§§66, 72, 73) may be calculated, as Bolzano does here, either directly from the binomial theorem or by similar methods to those which he had used for the binomial theorem.

### 5.3 The Intermediate Value Theorem and RB

#### 5.3.1. Introduction

By the "intermediate value theorem" we shall mean the result that if a function  $f(x)$  of one real variable is continuous on the closed interval  $[a, b]$  and if  $f(a)$  and  $f(b)$  have opposite signs, then  $f(x)$  is zero for at least one  $x$  in the open interval  $(a, b)$ . This is sometimes actually called "Bolzano's Theorem", as for example in James [1] and Courant and Robbins [1]. The latter source wrongly regards the theorem as occurring in Bolzano's Paradoxien des Unendlichen (Bolzano [5]); it is only in RB that the theorem is stated and proved by Bolzano. Together with an early form of the Bolzano-Weierstrass theorem these are the major results proved in RB. Their proofs are achieved using the convergence criterion and the definition of continuity, both of which we have discussed fully in Chapter 4. Here we shall be concerned with the actual proofs, their technical significance and the way they are related to Bolzano's general principles. The Preface to RB is as long as the main part of the paper and explains in detail why Bolzano saw so clearly the need for purely analytic proofs; it is therefore worth separate consideration.

### 5.3.2. The Preface to RB

After having said on A432;RB, 4 that the intermediate value theorem had not yet attracted much attention Bolzano proceeds to describe five different types of proof that had been given. This is a slight rhetorical exaggeration since some only seem to occur once and they are not all distinct. Furthermore, they have in some cases been embellished to facilitate criticism which allows Bolzano to demolish each type of proof, mainly on the grounds of incorrect method, thus leaving the reader in a receptive state for Bolzano's own proof which contains, "I flatter myself, not a mere confirmation, but the objective justification of the truth to be proved" (A448;RB, 20).

Although Bolzano gives specific references to the previous work in the seven footnotes on A433;RB, 5 he does not indicate how these match up with his five types of proof. This avoids any definite charge of mis-representation though it is usually quite clear which work he has in mind. The kinds of proof, which are described at length (A434-448;RB, 6-20), may be summarised as follows:

- I. Purely geometrical proofs relying on the fact that a continuous curve joining two points, one above and one below the x-axis, must intersect the x-axis. This is said to be the most common kind of proof (which is probably true) but, curiously, it is not to be found in any of Bolzano's references.

- II. Proofs based on the "wrong" concept of continuity (namely that of a function taking all values between any two of its values, see 4.4.3) together with the concepts of time and motion. This must refer primarily to Lagrange [4] but also, to a lesser extent, to Lacroix [3] and Clairaut [1].
- III. Use of the principle that, "Every variable quantity can pass from a positive state to a negative one only through the state of being zero or infinite." This definitely refers to the proof in Kästner [3] where reference is made to the following geometrical illustration in Kästner [1] p.200. Consider a line rotating about a point not on the x-axis. Its intercept with the x-axis changes sign only by going through zero or being "infinite".
- IV. Use of the principle that there must be a "last" value for which the function is negative and a "first" for which it is positive. Though not quite fairly described, this almost certainly refers to the method used in Rösling [1] where the author does point out that his values a and b (where the transitions from negative values and into positive values occur) must differ by an arbitrarily small quantity; he does not use the terms "last" or "first".

V. Use of the fundamental theorem of algebra in the form stated at the beginning of RB Preface: "Every algebraic rational integral function of one variable quantity can be divided into real factors of first or second degree." This result is used at the outset in the "proof" in Klügel [2] Vol. 2, p.447 but in any case only a kind of converse of the intermediate value theorem would follow from the reasoning here.

Bolzano is conscious of the close relationship between the proposition which forms the title of RB (i. e. not the intermediate value theorem but the special case when the function is a polynomial function) and the fundamental theorem. At the beginning of the Preface to RB he says that the second and third proofs of the latter theorem given by Gauss (Gauss [3] and [4] 1816) "hardly leave anything to be desired", but in his discussion of case V. above he maintains that both these proofs actually depend on the RB title theorem. Thus the choice of subject for RB, in Bolzano's eyes, was strategic: it supplied the necessary missing link in the rigorous proof of the fundamental theorem as well as being essential to complete his own proof of the binomial theorem (A312;BL, 32). Moreover, from his account of the previous work it is easy to see why Bolzano should recognise the need for such a purely analytical proof. Almost every criticism that he makes of the previous proofs is directly related to one of his methodological principles. We list them

here in the order corresponding to the above types of proof I. - V.

- I. The geometrical proofs are clearly contrary to the principle of "conceptual correctness" (2.4.1), they are a "crossing to another kind" and it is logically circular to prove a result true of all quantities from one true of only spatial quantities (A434:RB, 6).
- II. The concepts of continuity (4.4.3) and of motion (2.4.1 and 3.1.2) have already been discussed. It is striking that in hardly any of the references Bolzano gives is the essential condition of the continuity of the function even mentioned. Bolzano's demand that "all characteristics of the subject must be used in any correct proof" (A213;BD, 106, see 2.4.3) ensured that this was not neglected in his own proof.
- III. Such a complex truth as the principle used here could not possibly be an axiom (2.4.5); it is actually equivalent to the theorem being proved.
- IV. The criticism here is a matter of plain mathematical fact.
- V. The fundamental theorem is a more complex truth than the RB title theorem and this determines their objective order of derivation, the former from the latter and not vice versa (A211;BD, 104 and 2.4.3).

Bolzano was not the only mathematician at his time to see the need, or at least the desirability, of purely analytic proofs for analytic results. For example, Gauss says in reference to his earlier proof Gauss [1], "that first proof depended, at least partially, on geometrical considerations while the one which I am embarking on here will rest on purely analytic

principles." (Gauss [2] §1) And as the survey of proof-types II.-V. shows, there were several authors who avoided geometrical methods for their proofs of the intermediate value theorem. However, we have seen that for Bolzano the need for analytical proofs was not just something he felt as a matter of taste or convenience. For a long time he had considered the whole question of what a mathematical proof should be and he was able to articulate many reasons for this need which were part of a wider, coherent view of all theoretical knowledge. This made his criticisms of past work into a powerful programme. In the present case it was not just that geometric proofs were inadequate, they were wholly irrelevant; analysis itself had to develop the means to answer its own problems. In the nature of things (as Bolzano saw them) this had to be possible.

After giving an account of the main proofs of RB we shall be able to make some assessment of the claims for their strictness and consider the secondary sources on RB.

### 5.3.3. The Main Proofs of RB

The two principal theorems of RB are the predecessor of the Bolzano-Weierstrass theorem in RB§12 (A469) and the intermediate value theorem in RB§15 (A479). For convenience these will be referred to as Theorem 1 and Theorem 2 respectively. Theorem 1 makes essential use of the convergence

criterion (4.3.3) and Theorem 2 depends on Theorem 1 and the definition of continuity which first appeared in BL but which is repeated in RB Preface (4.4.3). Their proofs in RB are quite long and to help clarify their structure, and their affinity with modern methods, we shall paraphrase the statement and proof of each theorem in modern language and style. The structure and all steps of the proofs remain exactly as in the original and the only change in notation is the introduction for Theorem 1 of a sequence of sets to avoid the recurring phrase "all  $x$  which are  $< \dots\dots$ ". In Theorem 2 we have given only the first case where  $\alpha$  and  $\beta$  are positive.

Theorem 1 If a property  $M$  holds for all numbers less than a certain number  $u$ , but not for all numbers in general, then there is always a greatest number  $U$  for which all numbers less than  $U$  have the property  $M$ .

Proof: By assumption there is some number  $D$  such that  $M$  does not hold for all numbers less than  $u + D$ . Let  $S_m = \{x: x < u + D/2^m\}$  for  $m = 0, 1, 2, \dots$  and consider which  $m$ , if any, is the smallest with the property that  $M$  holds for all members of  $S_m$ . If there is no such  $m$  then  $u$  itself is the number required. For given any larger number  $u + d$ , we have  $u + D/2^m < u + d$  for sufficiently large  $m$ , and if  $M$  applies to all numbers less than  $u + d$  then  $M$



applies to all members of  $S_m$  for such  $m$ . This contradicts the assumption that there is no such  $m$ .

Suppose now that  $m$  is the smallest integer for which  $M$  holds for all members of  $S_m$ . So  $M$  does not hold for all members of  $S_{m-1}$ . We now repeat the above set construction using  $u + D/2^m$  in place of  $u$ , and  $D/2^m$  (i. e. the difference between the upper bounds of  $S_m$  and  $S_{m-1}$ ) in place of  $D$ . Let  $S_{m,n} = \{x: x < u + D/2^m + D/2^{m+n}\}$  for  $n = 0, 1, 2, \dots$  and consider which  $n$ , if any, is the smallest with the property that  $M$  holds for all members of  $S_{m,n}$ . If there is no such  $n$  then  $u + D/2^m$  is the number required. Otherwise let  $n$  be the smallest integer such that  $M$  holds for all members of  $S_{m,n}$  but not for all members of  $S_{m,n-1}$ . The difference between the upper bounds this time is  $D/2^{m+n}$  and we repeat the process on this interval. Continuing in this way there are two possibilities:

(a) We come to a number of the form  $R = u + D/2^m + D/2^{m+n} + \dots + D/2^{m+n+\dots+r}$  such that  $M$  applies to all numbers less than  $R$  but does not apply to all numbers less than  $R + D/2^{m+n+\dots+r+s}$  for all  $s = 0, 1, 2, \dots$ . In this case  $R$  is the number required.

(b) There is no such number and the process continues indefinitely. Now the partial sums of the series  $u + D/2^m + D/2^{m+n} + D/2^{m+n+0} + \dots$  do not exceed the partial sums of the geometric progression  $u + D/2 + D/2^2 + D/2^3 + \dots$  with common ratio  $\frac{1}{2}$ . By the convergence criterion both series are convergent. If  $u + D/2^m + D/2^{m+n} + D/2^{m+n+0} + \dots$  converges to  $U$ , then  $U$  has the properties required. For if  $M$  did not hold for

any smaller number say  $U - \delta$ , then

$$U - \delta > u + D/2^m + \dots + D/2^{m+n+\dots+r} - \omega \text{ for arbitrarily}$$

small  $\omega$  and so,

$$U - [u + D/2^m + \dots + D/2^{m+n+\dots+r}] > \delta - \omega.$$

Now the left-hand side can be made as small as desired by increasing  $r$  but since  $\delta$  is fixed and  $\omega$  is arbitrarily small we have a contradiction.

Finally suppose that  $M$  also held for all numbers less than any number larger than  $U$  say  $U + \epsilon$ . By assumption  $M$  does not hold for all numbers less than  $u + D/2^m + D/2^{m+n} + \dots + D/2^{m+n+\dots+r}$  for any  $r$ . But for sufficiently large  $r$  the series is arbitrarily close to  $U$  and since the difference between  $D/2^{m+n+\dots+r}$  and  $D/2^{m+n+\dots+r-1}$  tends to zero as  $r$  increases, the sum  $u + D/2^m + D/2^{m+n} + \dots + D/2^{m+n+\dots+r-1}$  is arbitrarily close to  $U$ , so  $u + D/2^m + \dots + D/2^{m+n+\dots+r-1} < U + \epsilon$  which is a contradiction.

Theorem 2 If  $f x$  and  $\varphi x$  are continuous functions of  $x$  for all  $x$  between  $\alpha$  and  $\beta$  and if  $f \alpha < \varphi \alpha$  and  $f \beta > \varphi \beta$  then there is a value of  $x$  between  $\alpha$  and  $\beta$  for which  $f x = \varphi x$ .

Proof: Suppose  $\alpha$  and  $\beta$  are positive and  $\beta$  is the greater, so  $\beta = \alpha + l$  where  $l$  is some positive number.

(i) Since  $f\alpha < \varphi\alpha$  we must have  $f(\alpha + \omega) < \varphi(\alpha + \omega)$  for sufficiently small  $\omega$ . Because, being continuous for  $\omega < 1$ , we can make  $f(\alpha + \omega) - f\alpha (= \Omega$  say) and  $\varphi(\alpha + \omega) - \varphi\alpha (= \Omega'$  say) arbitrarily small and so

$$\begin{aligned} \varphi(\alpha + \omega) - f(\alpha + \omega) &= \varphi\alpha - f\alpha + \Omega' - \Omega \\ &= A + \Omega' - \Omega \end{aligned}$$

where by assumption  $A$  is positive and so for small enough  $\omega$  we can make  $\Omega' - \Omega$  so small that the right-hand side is positive, i. e.  $f(\alpha + \omega) < \varphi(\alpha + \omega)$ .

Now regard the relationship  $f(\alpha + \omega) < \varphi(\alpha + \omega)$  as a property  $M$  of  $\omega$ .  $M$  holds for all  $\omega$  less than a certain value and  $M$  does not hold for all  $\omega$  (e. g.  $\omega = 1$ ). So by Theorem 1 there is a value  $U$  which is the greatest number such that all  $\omega < U$  have the property  $M$ .

(ii)  $U$  must be between 0 and 1 since if it was equal to 1 then  $f(\alpha + \omega) < \varphi(\alpha + \omega)$  provided  $\omega < 1$ . But just as  $f\alpha < \varphi\alpha$  implies by continuity  $f(\alpha + \omega) < \varphi(\alpha + \omega)$  if  $\omega$  is small enough, so  $f(\alpha + 1) > \varphi(\alpha + 1)$  implies  $f(\alpha + 1 - \omega) > \varphi(\alpha + 1 - \omega)$  if  $\omega$  is small enough. Clearly  $U$  cannot be greater than 1 since then  $1 < U$  but  $f(\alpha + 1) > \varphi(\alpha + 1)$ . It is certainly positive so  $U$  lies between 0 and 1, and  $\alpha + U$  lies between  $\alpha$  and  $\beta$ .

(iii) We cannot have  $f(\alpha + U) < \varphi(\alpha + U)$  since this implies, as before,  $f(\alpha + U + \omega) < \varphi(\alpha + U + \omega)$  for small enough  $\omega$ , contradicting the property of  $U$  as the greatest number such that all numbers smaller than  $U$  have

property M. But neither can  $f(\alpha + U) > \varphi(\alpha + U)$  because then again we have  $f(\alpha + U - \omega) > \varphi(\alpha + U - \omega)$ , contrary to the property of U that numbers smaller than U have property M. So  $f(\alpha + U) = \varphi(\alpha + U)$  and the theorem is proved.

#### 5.3.4. Assessment of RB

On the assumption that Bolzano is working implicitly with what we now call real numbers we shall first prove that the theorem of RBS12 (or Theorem 1 of the previous section) implies the Bolzano-Weierstrass theorem. We take the latter in the form: a bounded sequence of real numbers contains a convergent subsequence. Let the property M in Theorem 1 be, "not a member of a sequence of real numbers A", the theorem then says: Given a non-empty set of real numbers A with a lower bound, there exists a greatest lower bound for A. Now let A be a non-empty bounded sequence of real numbers. Being bounded below it has a greatest lower bound say, h, but since A is also bounded above the set  $B = \{y: y \geq x, \forall x \in A\}$  is non-empty and bounded below so B has a greatest lower bound (say k) which, it is easy to see, must also be a least upper bound for A. If  $\{a_n\}$  is an increasing subsequence in A then for all  $\epsilon > 0$ , there is an N with  $a_n > k - \epsilon$  for all  $n > N$ . But  $a_n \leq k$  for all n so  $|k - a_n| < \epsilon$  for all  $n > N$  and so  $\{a_n\}$  converges to k. Similarly if  $\{a_n\}$  is a decreasing subsequence in A it converges to h. But any sequence has a monotonic subsequence (Scott

and Tims [1] p. 116) so the Bolzano-Weierstrass theorem is proved.

In the above formulation in terms of bounds it may be shown that Theorem 1 implies the statement that an increasing sequence bounded above must be convergent. This is proved in Scott and Tims [1] p. 129 where the latter result is taken as a "Fundamental Axiom" in place of a definition of real numbers. Naturally the convergence principle follows from this axiom and so Theorem 1 also implies the convergence criterion "proved" in RBS<sup>S</sup>7 (A463).

In the later Functionenlehre (Bolzano [6]) Bolzano actually uses the Bolzano-Weierstrass theorem and in Rootselaar [1] we read, "For this theorem [Bolzano-Weierstrass] Bolzano refers to his own work, in which up to now it has not been found." Since it follows so easily from RBS<sup>S</sup>12 this would certainly appear to be what Bolzano had in mind in this reference.

Theorem 1 is a result of central importance to the later development of real numbers and function theory in the nineteenth century. Its appearance and proof in 1817 is therefore of greater significance than simply being for the sake of Theorem 2. Bolzano was aware of its importance throughout mathematics (A476;RB, 48), though it would be interesting to know what applications he saw for it in "chronometry"! We do not know of any earlier statement of the theorem.

Given the convergence criterion of RBS<sup>S</sup>7 then the proof is perfectly sound and although long-winded in its expression it was no more so than was

normal at this time. The particular merits of the proof appear in its part 5 (which begins on A474;RB, 46) and they are:

- (i) the careful application of the convergence criterion (by comparison with a geometric series) to the series produced by the successive halving process;
- (ii) the detailed checking that the "sum"  $(U)$  of the series has the required property by the correct and perfectly modern use of arbitrarily small quantities.

It is an interesting question whether the method of proof which Bolzano adopts for Theorem 1 is original. Previous commentators have been rather unoriginal and unhelpful here. In a vague reference which does not specify RB but which probably refers to RBS<sup>12</sup>, Schwarz mentions "a method of proof devised [ersonnen] by Bolzano and developed further by Weierstrass" (Schwarz [1] 1872). This opinion that Bolzano was the originator of the method is quoted with approval in Stolz [1] p. 255, 258. G. Cantor denies this, but only says that the proof is "in essence very old" (G. Cantor [1]) and gives no specific references of work before RB. Cantor's remarks are repeated in Jourdain [1] and Coolidge [1] (where they are attributed to Jourdain). Even the Encyklopädie article in Vol. I, A5, 1 only refers to Cantor's article. The question is not clear cut and there is truth on both sides, although we feel that there is more to be said for Bolzano's originality here than against it. On Cantor's side the judgement in Kolman [1] p. 49 is the best that can be said, "The procedure by which Bolzano proves his theorem was already contained in an embryonic form in Euclid." There is a successive bisecting

process in Euclid's Elements X, 1 and examples of exhaustion methods in Book XII, but the "convergence" involved in Greek work relies on geometric intuition. We have no hesitation, on the other hand, in claiming that both of the points (i) and (ii) above were original with Bolzano. To the extent that the convergence criterion itself was original then (i) must have been so, and (ii) would not normally at this time have been dealt with arithmetically and precisely although a purely verbal description could hardly be sufficient in this case.

As for Theorem 2, there can be no doubt about the originality and rigour of Bolzano's work in this proof. It depends crucially on his definition of continuity which was so effective just because it was purely arithmetic. Bolzano had mastered the technique of applying the definition to present precise arguments about functions in a manner which would hardly seem out of place in a modern textbook. As with Theorem 1, an important merit of Theorem 2 is the generality of its formulation. We have referred to the fact that in previous proofs of the intermediate value theorem the continuity of the function is hardly ever mentioned. This was because the theorem was usually introduced and proved for the sake of finding approximate roots to polynomial equations. So the only case considered was that of a polynomial function and this was "obviously" continuous (Bolzano proves the fact in RBS17(A484)). It was because Bolzano had concentrated on the general case of any function (for which he points out on A443:RB, 15 Theorem 2 may

be false) that he was led to make a proper definition of continuity essential to the proof.

The main defect of Theorem 2 is the first paragraph of the proof (A479;RB, 51) in which  $f(x)$  and  $\varphi(x)$  are said to be compared with one another "simply in their absolute values". This is consistent with Bolzano's earlier use of the inequality sign but makes obvious nonsense of the theorem. Fortunately it does not affect the working of the proof but we do have to interpret the inequality sign in the normal modern sense. Bolzano cannot conveniently use the symbol consistently.

A further weakness, possibly related to the matter of symbolism, is that Bolzano shows no indication of being aware that the first "between" in his statement of Theorem 2 (A479;RB, 51) should include the end-points while in the second instance it should not (the sign  $\leq$  is nowhere used in Bolzano's early work).



## 5.4. The Rectification Problem and DP

### 5.4.1. Introduction

The problems with which Bolzano is dealing in DP, those of the rectification of curves, the complanation of surfaces and the cubature of solids, form a family of problems which began, for the Greeks, as a part of pure geometry and which has only received satisfactory treatment within a major new branch of twentieth century analysis, measure theory. In the course of this long transition the efforts to solve these problems have played a fruitful part in the progress of analysis. This was particularly true during the seventeenth century.

Bolzano's consideration of previous work on the problems ranges from Archimedes hypotheses to the very latest work on the subject by Crelle for the criticism of which an Appendix was added to the main work (A589;DP, 76). To put his comments into some context we shall give a very brief outline of the history of these problems up to Bolzano's time.

For the Greeks two magnitudes were comparable and had a ratio provided they were of the same kind. This was the case whenever a multiple of one exceeded the other (the so-called "axiom of Archimedes"). From this it was deduced that the repeated subtraction of at least half of any given magnitude would result in a magnitude smaller than any preassigned magnitude of the same kind (Euclid's Elements X, 1). This was the principle used in the method of exhaustion for the indirect proofs of various simple quadratures

such as the area of a circle or of a segment of a parabola. The area was approximated by a sequence of polygons and then it was shown, by contradiction, that the sequence could eventually be neither more nor less than the particular value of the area. Several cubature problems (such as volumes of revolution of conics) were also solved by this method but the Greeks did not succeed, as far as we know, with the rectification of any curves beyond the circle nor with the complanation of any surfaces beyond that of the sphere.

The first real advances in these problems came in the seventeenth century by means of important modifications to the method of exhaustion. The polygons were replaced by rectangular strips and there was a vital shift in the final step of the reasoning. In place of the reductio ad absurdum of the older method the appropriate infinite series was now considered and a crude limiting process used. The development of analytic geometry by Descartes and Fermat was, of course, crucial to formulating general methods of solution. It is ironic that at just about the time that Roberval had shown the length of the arch of a cycloid to be four times the diameter of the generating circle Descartes had written, "the ratios between straight and curved lines are not known, and I believe cannot be discovered by human minds." (Descartes [1] (1637) p. 91). The general rectification of the cycloid soon followed and then the rectification of an algebraic curve, the semi-cubical parabola, was given independently by Neile, van Heuratt and Fermat in the late 1650's. Fermat's method compared an arc with the circumscribing tangents drawn at its

end-points, while Neile used the fact that a small arc is virtually the hypotenuse of what is now called the "differential triangle". In modern notation this is to say,  $ds = \sqrt{dx^2 + dy^2}$  or  $ds/dx = \sqrt{1 + (dy/dx)^2}$ . After the advent of the general calculus methods of the 1660's the latter formula became well known and also quadratures were effected by the reverse of differentiation. Huygens had shown that the rectification of the parabola reduced to the quadrature of the hyperbola, but the rectification of the ellipse and hyperbola posed new problems for analysis which were not solved until the next century. Huygens had also been the first to find the surface area of a segment of a paraboloid of revolution. The special complanation and cubature problems of surfaces and volumes of revolution could immediately be solved as simple integrals. The more general surface integral and multiple integral formulae only appeared in the second half of the eighteenth century.

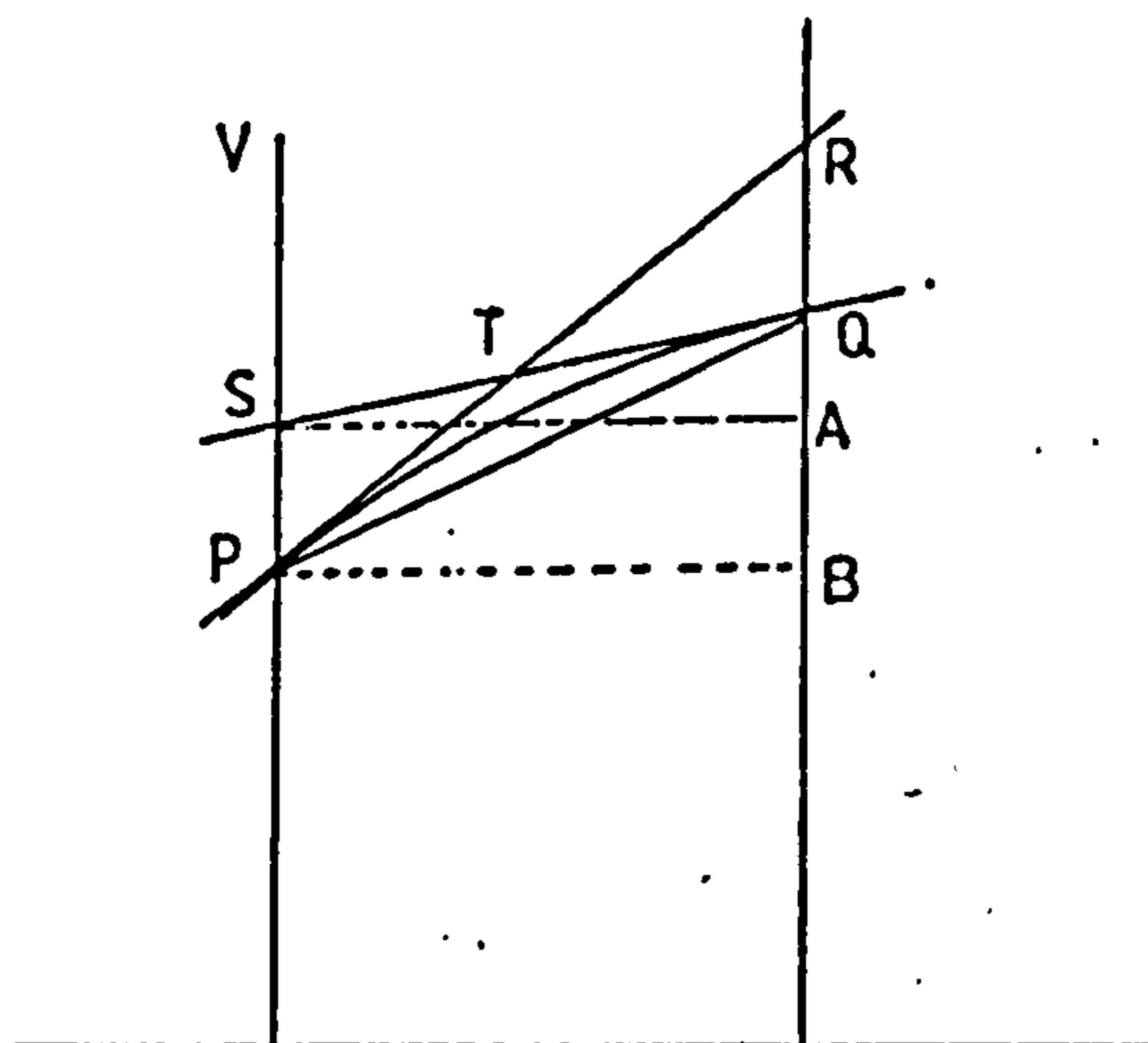
In the course of the Preface to DP Bolzano makes several specific references to the works of nearly twenty previous authors concerned with the rectification problems. This is less impressive than it appears. More than half of the references are to attempts to prove the hypotheses of Archimedes on curves and surfaces. (This does not refer to the "axiom of Archimedes", see A493;DP, IV and Note [1], A595.) Those references that really are on the rectification problem cover a very narrow spectrum of the work and do not include any of the authors we have mentioned above. However, Bolzano was surely right to stress the reliance made by most, if not all, methods of

rectification on the Archimedean assumptions. (This is a point that has been neglected by modern commentators on this subject)

As an example of the kind of approach Bolzano was familiar with we shall outline Lagrange's proof of the rectification formula. This is mentioned first among those in which at least the calculus part of the work is deemed satisfactory, (A499;DP, X). Lagrange begins (Lagrange [3] p.218):

For the solution of this problem we start from the principle of Archimedes, adopted by all geometers ancient and modern, according to which for two curved lines, or ones made up of straight parts, which are concave on the same side and have the same end-points, the one which encloses the other is the longer. From this it follows that an arc of a curve, which is all concave on the same side, is greater than its chord and at the same time less than the sum of the two tangents drawn at the ends of the arc and contained between these end-points and their point of intersection.

Lagrange continues with a lengthy verbal description of the following diagram (which Lagrange does not give).



For convenience we shall paraphrase the rest of the argument using this diagram. From the Archimedean principle,

$$PQ < \text{arc } PQ < PT + TQ.$$

Clearly,  $T\hat{P}V < Q\hat{P}V < T\hat{S}V$  and so,

$$SQ < PQ < \text{arc } PQ$$

and  $\text{arc } PQ < PT + TQ < PT + TR$

therefore  $SQ < \text{arc } PQ < PR$

Let the curve have equation  $y = fx$  and let the increase in  $x$  from  $P$  to  $Q$  be  $i$ ;  $f'x$  will be "the tangent of the angle under the tangent" at  $P$ . So  $RB = if'x$

and  $PR = i\sqrt{1 + (f'x)^2}$ , similarly  $SQ = i\sqrt{1 + [f'(x+i)]^2}$ . Put

$\phi x = \sqrt{1 + f'x}$  then, from the above, the arc length  $PQ$  lies between  $i \phi x$  and  $i \phi(x+i)$  for arbitrarily small  $i$ . If the length of the arc is given by the function  $\Phi x$ ,

$$i \phi x < \Phi(x+i) - \Phi x < i \phi(x+i)$$

and as it tends to zero this gives  $\Phi'x = \phi x$ , or  $\Phi x = \int \sqrt{1 + (f'x)^2} dx$ .

(Lagrange himself, of course, defined the derivative  $\Phi'x$  as a coefficient in a Taylor series, not as a limit, so the last step of his argument is slightly more complicated.)

An important criticism in DP Preface (which Lagrange's proof avoids) is liable to be misunderstood. In commenting on the continued misuse of infinitesimals Bolzano says:

Why does the length of an infinitely small arc only coincide with the

length of that straight line which goes through the ordinates which bound it if it has the direction of the chord or tangent, but not if it goes through the ordinates at some other kind of angle? (A497;DP, VIII)

The criticism is repeated in different ways on the two following pages. Vojtěch says that Bolzano's objection here "is, of course, wrong" (Bolzano [3] p.200 Note 45). But a consideration of the context shows that Bolzano is not disputing the correctness of the result concerned nor that it can be proved. He is disputing that it can possibly be correctly proved by using infinitesimals in the way, for example, that Schultz does. The arguments from infinitesimals or from the simultaneous vanishing of quantities are, as he quite correctly claims, completely arbitrary and vacuous.

After further criticism of the Archimedean hypothesis already mentioned, on the grounds that it cannot easily be modified to deal with the general case of space curves of double curvature, he returns to the above idea and suggests that a better hypothesis from which to prove the rectification formula might have been:

The relation of the length of an arc curved according to the law of continuity (whether simple or double) to its chord comes as close as desired to equality if the arc is taken as small as desired. (A503;DP, XIV)

This is interesting because it is the most common assumption made in modern proofs of the formula (e. g. Hardy [1]). It is, however, rejected by Bolzano as being in no way an axiom and requiring for its proof a result at least as strong as the rectification formula itself.

### 5.4.2. Account and Assessment of DP

The work DP is of rather a different character from the four other works we have considered. The content and the style are both more muddled, even the title of the work suggests this. Perhaps in his effort to gain attention Bolzano tried to include work to interest as many mathematicians as possible. A long initial section on the determination of functions is followed by geometrical definitions and comment interspersed between the main rectification proofs with no clear connection between the two. Because the material is so diverse some of the work has already been dealt with in earlier chapters as indicated in the following summary of the contents of DP.

§§ 1 - 10 (A514-533)	Various properties of continuous functions (See 4.4.3) Determination of functions from given sequences of functions using a process analogous to geometrical similarity.
§§ 11-31 (A533-557)	Definitions of line, straight line and determinable spatial object (See 3.4.2) Definition and distinction of length and distance (See 3.4.4) Application of similarity to lengths of general lines.
§§ 32-34 (A557-564)	Solution of the rectification problem.

§§ 35-49                      Definition, determination and properties of  
(A564-572)                      surfaces and the area of surfaces. (See 3.4.2)

§§ 50-51                      Solution of Complanation problem  
(A572-578)

§§ 52-60                      Definition, determination and properties of  
(A579-582)                      solids and the volume of solids. (See 3.4.2)

§§ 61-62                      Solution of Cubature problem  
(A582-587)

§ c3                              Analogy between main proofs  
(A587-589)

Bolzano's solution to the rectification problem is radical, original and exasperating. It contains some valuable and subtle insights into the concepts of line, length and function, but these are inextricably mixed up in the proofs with very dubious assumptions and rather vague concepts of similarity and "determination". The reasons for the radical approach are, as in all Bolzano's previous innovations basically to do with concepts. We have mentioned in the last section that Bolzano regarded the fact that the length of an arc approaches the length of its chord as the latter tends to zero, as a



theorem to be proved from the rectification formula rather than vice versa. One reason for this emerges from the remarkable comments he makes on the use of inscribed and circumscribed polygons for the quadrature of a plane surface. He says (A506;DP, XVII) that this method is not "scientific" because

the truths to be proved are not derived, by the method of limits, in the way they should be in a truly scientific proof - from the concepts of the thesis itself - but only through certain associated concepts that have been brought in here quite fortuitously (per aliena et remota).

Anybody should realise that those infinitely many regular polygons circumscribed around a circle and inscribed within it are completely alien objects if one wishes to find not their area but that of the circle itself.

Thus the method of limits was kept strictly within arithmetic, there was to be no "crossing to another kind" even by means of a limiting process. But what constitutes a "kind"? Bolzano nowhere attempts to discuss this, but his interpretation here seems impossibly strict. No doubt he regarded the length of a curve as an intrinsic property of the curve which does not require (and should not be given) a definition in terms of the length of straight lines. But this is no reason to prevent us using a limiting process from rectilinear figures in order to find the value of this length. In DP§19(A547) the length of a line in general is defined as a quantity derived from the nature of the line (with respect to a given unit of distance) and subject only to the natural

additive property (see 3.4.4).

For his alternative approach to rectification Bolzano makes use of a general concept of similarity in the form of the following theorem:

Lengths of lines which are similar to one another are in proportion to the lengths of other lines determined from them in a similar way.

(A554; DP, §30).

As shown by the proof of this theorem it does simply mean that in similar figures the ratios of corresponding lengths are equal. The basic idea of Bolzano's method is simple but his presentation, in DP, §32, and for the two-dimensional case given at the end of the Preface (A507), is rather confusing. We shall concentrate here on clarifying the latter version since it acts as a good model for all the principal proofs in DP and suffices to reveal the weaknesses of Bolzano's approach.

Let  $y = fx$  be the equation of a plane curve and let  $Fx$  be the arc length up to a given value  $x$ . The first part of the proof shows that  $dF/dx$  depends only on  $df/dx$  (or that  $df/dx$  determines  $dF/dx$ ). Then assuming the relationship between these derivatives is independent of the particular curve, Bolzano uses the case of a straight line to deduce the rectification formula. The main steps in the arguments are as follows:

- (i) As  $x$  increases by  $\Delta x$  the arc length increases by  $F(x + \Delta x) - Fx$  and this quantity is determined by all the ordinates of the curve over the interval  $\Delta x$ , i. e. by the values of  $f(x + m \Delta x)$  as  $m$  takes all values from 0 to 1.

(ii) The value of  $F(x + \Delta x) - Fx$  is therefore also determined by the values of  $f(x + m \Delta x) - fx$  for  $m \in [0, 1]$ .

(iii) If, for two or more curves, the values of  $\frac{f(x + m \Delta x) - fx}{m \Delta x}$  for  $m \in [0, 1]$  are the same (for each  $m$ ) then the curves are similar. Therefore the ratio  $\frac{F(x + \Delta x) - Fx}{\Delta x}$  will also be the same for these curves.

(iv) So the values  $\frac{f(x + m \Delta x) - fx}{m \Delta x}$  for  $m \in [0, 1]$  determine the value  $\frac{F(x + \Delta x) - Fx}{\Delta x}$ .

(v) This is true for arbitrarily small  $\Delta x$ , so,

$$df/dx \text{ determines } dF/dx .$$

(vi) For the straight line  $y = \alpha + \beta x$   $Fx = x \sqrt{1 + \beta^2}$

$$\text{so } dF/dx = \sqrt{1 + \beta^2} = \sqrt{1 + (df/dx)^2} \quad \text{so generally}$$

$$Fx = \int \sqrt{1 + (df/dx)^2} \quad dx \text{ as required.}$$

For part (vi) above Bolzano actually introduces another function  $y = \varphi x$  with length function  $\Phi x$  before putting  $\varphi x = \alpha + \beta x$ . He then says,

there is without doubt some corresponding law by which for all lines the functions  $Fx$  and  $\Phi x$  can be derived from the functions  $fx$  and  $\varphi x$  (A510; DP, XXI).

And in the more general case of a space curve  $y = fx$ ,  $z = \tilde{f}x$  in DP§32 we

have the same claim,

without doubt there is some general law by which for all lines the functions  $Fx$  and  $\Phi x$  are derivable from the nature of  $fx$ ,  $\bar{f}x$  and  $\varphi x$ ,  $\bar{\varphi}x$ . (A560;DP, 47)

This general law is likened to a kind of "higher rule of three" (A528;DP, 15); there is the vague suggestion that this could be viewed as a kind of extension of the similarity principle from elementary mathematics. But it is important to be clear here exactly what is proved by similarity and what is not. The only step in the proof outlined above where similarity is used is part (iii), and it forms a legitimate and correct step. The existence of a "general law" by which the length function is related to the curve function is simply an assumption. It is not claimed to have been proved, by similarity or in any other way. Bergmann is therefore wrong to say of Bolzano's similarity theorem DP§30 that,

It plays the same role in his proof as the usual method for solving the rectification problem of dividing the line into infinitely many small parts which are regarded as straight. (Bergmann [1] p. 188).

This is not so, Bolzano does not use similarity to make the transition between straight and curved lines. Instead it is precisely for this purpose that he uses the assumption of a general rule applying to both straight and curved lines. The lack of proof for this assumption is a major defect in Bolzano's method. Worse still, the assumption is not even plausible in the light of his

earlier objection to the standard limiting procedure on polygons. If the latter are indeed "alien" and of a different kind to curved lines then we should surely expect a different rule for expressing the length of a straight line from the rule for a curved line.

The work DP is the first occasion when Bolzano freely uses calculus notation though he feels the need to stress that this does not mean the implicit use of infinitesimals (A495;DP, VI). Du Bourguet's notation for partial derivatives is followed (A524:DP, 11) and Taylor series expansions are assumed for all functions without any justification. Possibly the latter assumption (which is unnecessary for the main proofs) was a concession to followers of Lagrange who wished to define the derivatives of a function by means of the Taylor series coefficients. Finally, we note the curious fact that the phrase, "every conceivable proper fraction including 0 and 1", is used repeatedly throughout DP in the sense of "every real value from 0 to 1" (e.g. A508;DP; XIX).

There is no doubt that the main strength of DP lies in the purely geometrical work it contains, so this brief assessment of the analysis work on rectification needs to be considered in conjunction with the relevant sections of Chapter 3 for a fair impression of the paper as a whole.

## Chapter 6 : Conclusion

### 6.1. The Mathematical Achievements

Bolzano's achievements in the five works considered here were the most significant parts of a much larger whole; namely the extensive notes and drafts on mathematical topics which Bolzano constantly worked on during his time at Prague University. (See especially Bolzano [1] Vol. 2B, 2/1, 2/2 and 2A5). His considerable contributions to geometry and analysis form, in each of these areas, a closely related system of concepts and theorems. It was not only Bolzano's method of working that promoted this unity (in his notes he constantly reverts to earlier problems and solutions, correcting and revising them), but it was also a natural consequence of his belief in the existence of a few fundamental simple concepts governing a particular subject (2.4.4). We shall summarise those achievements that can clearly be distinguished in Bolzano's early works and which were, in the sense explained below, original to Bolzano.

- (i) The complete reorganisation of the elementary geometry of points, lines and triangles, avoiding the use of the concepts of motion and the plane, and giving a central role to the concept of similarity (3.2).
- (ii) The analysis of the concept of the straight line resulting in important distinctions concerning distance, direction and order, together with their fundamental properties which need either to be proved from definitions or to be embodied in an axiom system. (3.3)
- (iii) Topological definitions of line, surface and solid together with various special cases of these. (3.4.2).
- (iv) The modern definition of the continuity of a function of one real variable and its use for various properties and theorems about continuous functions including: the derivative of a continuous function is continuous, a continuous function of a continuous function is continuous. (4.4.3).
- (v) The use (though not explicit definition) of the modern definition of the derivative of a function of one real variable. (5.2.3).
- (vi) The correct statement, and attempted proof, of the convergence criterion for an infinite series. (4.3.3).

(vii) An attempted proof of a general statement of the binomial theorem which in many respects was far superior to earlier efforts. (5.2.3).

(viii) The statement and proof (from the convergence criterion) of an original form of the Bolzano-Weierstrass Theorem. (5.3.3).

(ix) The proof (from result (viii)) of the intermediate value theorem. (5.3.3).

The claim that these results were original to Bolzano needs further explanation. The development of most major mathematical concepts involves a long evolutionary process which no individual can lay claim to have "achieved" personally. It is most likely, for example, that in the first decade of the nineteenth century Lagrange and Gauss had at least as clear an idea as Bolzano had of what should be meant by the continuity of a function and the convergence of an infinite series. But the development of ideas is a fitful, haphazard process and one thing that has often led to rapid advances in mathematics has been the formulation and use of an appropriate symbolism. In the present case, by referring to the originality of Bolzano we simply mean that it is in these five works of his that there appears for the first time in the mathematical literature essentially the same arithmetic formulation used for the concepts of convergence, continuity and derivative that



turned out later in the century to be acclaimed as securing the foundations of analysis. The same formulation of these concepts has not been improved in any fundamental way in our own day.

Although it appears that the time was ripe for the sort of breakthrough made by Bolzano's work, we have seen that it actually had a minimal influence on other mathematicians. By the time it was recognised in the literature (by Hankel, Stolz etc.) the same work had been done far more thoroughly by many other mathematicians. This does not detract from Bolzano's achievement and it remains of interest to consider why he, and no others at this time, was so successful.

## 6.2 The Significance of Bolzano's General Views

The views Bolzano expresses in the five early works on the general nature of mathematics are hardly sufficient to be called a "philosophy of mathematics". There is little or no discussion of the nature of mathematical knowledge or truth. But there is an important ideal of mathematical proof involving the principle of "conceptual correctness" (2.4.1). This principle acts as an effective method of criticising and improving proofs. Sufficient evidence has already been adduced in the previous chapters to show that it was this method of criticism, and therefore these general views on the nature of mathematics, which led, more or less directly depending on the case, to the achievements listed in the previous section. We mean by this that these views promoted the choice of suitable problems by showing where new proofs were needed and sometimes they indicated the lines of an appropriate solution. But they never, of course, constitute the solution, that was entirely a matter of mathematics. We shall attempt to clarify the matter a little further.

In order to substantiate the claim that general views on the nature of mathematics led to certain mathematical achievements we may proceed (in the case of Bolzano at least) in the following two stages. Firstly, it should be made clear that the mathematical results were closely related to the consideration and refinement of certain concepts. Secondly, it needs to be shown how the general views led to the consideration of these particular concepts and why they were modified or replaced in the particular way they

were. The first requirement is much the simpler and has already been carried out in the main chapters, but we shall summarise the situation here using the numbering of the results as in 6.1.

The work of (i) was explicitly the result of excluding the concepts of motion and the plane from elementary geometry. This also meant the exclusion of superposition arguments for congruence proofs. (ii) was precisely the result of analysing the concept of straight line and (iii) was related (as argued in 3.4. .) to the distinction of the concepts of length and distance. The important new formulations in (iv), (v) and (vi) arose, as described in Chapter 4, from the exclusion of all spatial intuitions as well as those of time and motion. This involved a refinement of the concepts of function, infinite series and continuity. A purely quantitative account was necessary in analysis and a pre-requisite for this was the conceptual distinction between something which was meant to be smaller than any arbitrary quantity and yet be non-zero (an infinitesimal), and an arbitrarily small quantity. The theorems of (vii), (viii) and (ix) all depend essentially on the new concepts defined in (iv), (v) and (vi).

A good deal of mathematics, in the past and present, seems to have been achieved without any apparent preoccupation with concepts. Why was it so important in the present case? To suggest an answer to this, and to prepare the way for the second task referred to above, some general remarks will be useful about this rather self-conscious, quasi-mathematical

activity of analysing mathematical concepts.

Before the rise of the axiomatic method in the nineteenth century mathematicians were inclined to regard their theorems as true in an absolute sense. The definition of complex concepts represented a true analysis of the concept into simple components. When axioms were stated they too were true expressions of relations between simple concepts. Usually such axioms were unmentioned and the relations of simple concepts were just assumed, with a varying degree of awareness that such assumptions were being made. The great majority of mathematical work before 1850 proceeded, very successfully, on such an intuitive use of concepts. This testifies to the extraordinary fruitfulness, and reliability over a wide range, of the concepts that had been acquired of number, function, curve, area and so on, including even the concept of infinitesimal. Such concepts were not static: they were constantly being affected by new theorems and distinctions, by practical problems and by the way they were passed on to new generations. Occasionally, however, mathematics itself produces problems of such difficulty and profundity that they provoke a sustained and deliberate consideration of the concepts concerned. In some sense the limits of the intuitive use of a concept have been reached and a new approach has to be made. The classic examples of this are the problems of irrationals and parallels, and the paradoxes arising from infinitesimals in the eighteenth century and from set theory at the turn of this century. The resolution of these problems produced new concepts, or

a new understanding of the place of earlier concepts, and these opened the way for large new areas of mathematics. The important point here is that these changes in concepts were made in direct response to a breakdown in the mathematics. They were mathematical remedies for mathematical problems.

Bolzano's attention to mathematical concepts was unusual in that it did not arise in the way we have just described. The issue is complicated because his work was the occasion of the vigorous rejection of infinitesimals and it therefore has the appearance of conforming to this pattern. But we have Bolzano's own word on many occasions that it was not so much particular mathematical problems that inspired his work as his inclination to philosophy in general and his views about proofs and concepts in particular. This is further confirmed by the fact that his views on proofs led to the rejection of methods which were not generally regarded as posing any mathematical difficulty. For example, he rejected the combinatorial argument for the positive integer case of the binomial theorem (p. 260), and the use of sequences of polygons for the quadrature of the circle (p. 291). There is also his postponement of the concept of the plane in elementary geometry (3.1.3).

Bolzano's general views formed a methodology which made strict demands on the concepts which could occur in proofs and the order in which they should occur (2, 4). These demands had the overall effect of purging mathematics of all empirical elements and thus rendering its concepts more

exact and susceptible to purely logical treatment. Accordingly Bolzano's work was chiefly centred on the replacement of all spatial and temporal intuitions in analysis and re-organisation of geometry. This completes our argument for the claim made at the beginning of this section.

The main conclusion of this thesis is therefore that it was largely in response to his general views about the nature of proof that Bolzano brought about some important developments in mathematical concepts. Furthermore, because these changes answered to some contemporary mathematical needs, particularly in analysis, they led directly to important and fruitful new mathematics.

The arguments in favour of Bolzano's general views and his requirements for proofs are unfortunately only vaguely indicated in the early works. There is an implicit appeal to preserving "natural kinds" (through his repeated use of the Greek prohibition against "crossing to another kind") and also to a hierarchy of concepts in which theorems involving "higher" concepts (i. e. more general concepts) may not be proved by making use of concepts from "lower" levels. But there is a tension between these two principles themselves in Bolzano's work. Both principles prohibit the use of geometry for analysis proofs, but why does the "natural kind" argument not prohibit analytical geometry? Many other questions arise about Bolzano's position. Is it possible to make sense of his idea of a unique, preferred proof for a theorem? (Are there, in any case, criteria for different proofs?) How does

this relate to Kitcher's suggestion of a theory of mathematical explanation? (see p. 260 and Kitcher [1] pp. 267-269). How do Bolzano's views relate to the modern conception of axioms and formal theories? These are questions which might be useful areas for further investigation.

Finally, it is worth noting that it has only been possible to complete the present study with any degree of conviction because of two fortunate characteristics in Bolzano himself. There was no lack at the beginning of the nineteenth century of philosophers ready to make remarks about mathematics and vice versa. What was unusual in Bolzano was that he had both the philosophical insight to see so strongly the need for important fundamental changes in mathematics and the mathematical expertise to carry out his own programme of conceptual refinement and produce valuable results. There can also be no doubt that many of the great mathematicians of that time were strongly motivated by conceptual, and even philosophical, considerations. Again, what was special in Bolzano's case was his willingness and ability to articulate these considerations so clearly and fully throughout his works. Thus in spite of the neglect of his work in the nineteenth century, and his consequent lack of influence, the positive relationship which we have described here between Bolzano's general views and his mathematical results render his work of substantial and enduring historical importance. It sheds another ray of light on the mystery of how change takes place in the realm of mathematics.

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